IFAU
Institute for Evaluation of Labour
Market and Education Policy

Caring, Sharing and Childbearing

Essays on Labor Supply, Infant Health and Family Policies

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Abstract

Norén, A. 2017. Caring, Sharing, and Childbearing. Essays on Labor Supply, Infant Health, and Family Policies. *Economic studies* 173. 206 pp. Uppsala: Department of Economics, Uppsala University. ISBN 978-91-85519-80-4.

Essay I: I study the consequences on labor market outcomes and sick leave of having an elderly parent in need of care. Caring for an elderly parent may be associated with opportunity costs such as productivity loss on the labor market if informal caregivers are of working age. Using Swedish register data I compare the labor market outcome trajectories of adult children before and after their parent suffer a health shock. I find that employment and income of adult children are slightly reduced in the years leading up to the demise of their parent, but that the size of the impact is largest in the year, and the year after, parental demise. I also find that daughter's sick leave absence increases in the year that the parent dies. No effects on labor market outcomes are found from having a parent suffering stroke. Furthermore, I find no clear gender differences between sons and daughters in the impact of having a parent with increased care demand. Taken together, the results suggest that the opportunity costs of parental care need in the form of adverse labor market impacts are small.

Essay II (with Erik Grönqvist, Anna Sjögren and Helena Svaleryd): 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.

Essay III (with Erik Grönqvist, Anna Sjögren and Helena Svaleryd): 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.

Essay IV: Despite several policies aimed at increasing fathers' participation in the caring of children, Swedish mothers still use the bulk of the paid parental leave which may have several negative consequences for the family e.g. in terms of weaker labor market attachment for the mother. Division of parental leave is likely affected by how parents value the costs associated with parental leave. I investigate whether a reduction in the care burden, or a decreased non-monetary cost, of parental leave through the availability of childcare for older siblings affects how the leave is divided. The effect of access to childcare is evaluated by utilizing the regional heterogeneity of the implementation of a childcare reform in Sweden in 2002 that gave children of parents on parental leave with a younger sibling the right to stay in childcare. Results suggest that availability of childcare for an older sibling during parental leave does not impact the division of parental leave between mothers and fathers.

Keywords: Informal care, Elderly, Labor supply, Alcohol prevention, Brief intervention, AUDIT, Antenatal care, Child health, Childcare, Parental leave, Gender equality
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Stockholm, October 2017
Anna Norén

Contents

Introduction

At some point in life, most of us provide care of some sort to a family member. For most people, it starts with caring for one's children; for women already during pregnancy. A safe environment for the unborn child has been shown to be of great importance in shaping the offspring's opportunities.[1] Once the child is born, parents invest both time and energy into their child's upbringing and thereby creating the foundations for the child's skill formation.[2] Later in life, individuals may also have a responsibility of caring for their elderly parents. The term *sandwich generation* has been used to refer to the middle-aged generation supporting both their aging parents as well as their growing children. Since there is only 24 hours in the day there is a conflict of time between work and family, and providing care can therefore affect how much an individual chooses to work. Moreover, providing care may have important health implications for the caregiver as well as the person being cared for. In this thesis, I study how caring for a family member affects labor market and health outcomes of individuals in the situations just described.

In order to understand individuals' labor supply decisions, economists' standard model describes how an individual allocates time between work and leisure to maximize his or her utility. Gary Becker's theory of household production (Becker 1965) in addition stresses that the labor supply decision is rather the time allocation between work in the market, household work, and leisure. This theory provides a basis for understanding how family ties affect the optimal time allocation, since an important implication of this model is that an increase in care responsibilities reduces the amount of time spent on the labor market. Given the traditional roles within the family, Becker's models became especially useful for understanding female labor supply decisions. Becker (1981, enlarged ed., 1991) suggests that women's relatively higher productivity within the household implies that couples maximize their utilities when women specialize in household production.[3] As a result, women would be more inclined to provide care for both children and elderly parents. In order to enable the combination of labor market work and family life, paid parental leave and publicly provided childcare have become important parts

[1] This literature is reviewed in Almond and Currie (2011).

[2] Economic models such as e.g. Cunha and Heckman (2007) suggest that skill formation is a dynamic process in which genetic background and parental investment both strongly affect outcomes later in life.

[3] This is true given the assumption that men have a comparative advantage in market work.

of family friendly welfare policies. These policies have most likely been important for increasing female labor force participation and for gender equality in the labor market (see for example Waldfogel 2002; Björklund 2006; Lalive and Zweimüller 2009).

Even though effective family polices such as paid parental leave play a key role in increasing female labor force participation, there may also be negative consequences from extending already existing parental leave policies. Sweden has one of the world's most generous parental leave systems and is on the frontier of a gender neutral parental leave outtake. But since women still use the bulk of paid parental leave, this extends time away from the labor market with potentially negative consequences for their human capital accumulation (Gupta and Smith 2002) and creates a risk for statistical discrimination (Albrecht et al. 2003). It is therefore important to understand how polices affect the individual's labor supply in order to create effective welfare policies that enable the combination of work and family life and promote gender equality in the labor market.

A large body of economic literature is devoted to the understanding of the consequences of childbearing and the effects of caring for children on, especially female, labor force participation and the gender wage gap. A less explored determinant for labor supply decisions is the consequences of care-needing elderly parents. As recognized by Ettner (1995), although the decision to care for an elderly parent and the nature of such care may differ in many aspects to that of caring for a child, the influence of the time commitment may be similar with respect to its implications for labor market outcomes. Caring for an elderly parent may be fulfilling, but it could also be time consuming and mentally straining. This motivates the policy interest for whether caregivers incur opportunity costs in terms of productivity loss on the labor market and whether those costs differ between men and women.

Care provision also has consequences for the person being cared for. The quality of the care has important implications, especially in the case of a pregnant woman caring for her fetus. Pregnancy can be seen as the first part of parents' investments in their child's human capital accumulation (Cunha and Heckman 2007). In fact, advocates of the *fetal origins hypothesis* (Barker 1990; Almond and Currie 2011) would argue that the nine months in utero is the most important period in a person's life because it shapes future lifetime opportunities. A large body of literature has emerged in economics corroborating the importance of early life experiences for outcomes later in life. In a theoretical model, Cunha et al. (2006) show how human capabilities develop through investments in upbringing, learning and health. An implication from this model is that early childhood events can have long-term consequences and that early interventions in a child's life may have substantial impacts on health and economic outcomes later in life, especially when supporting disadvantaged groups. Awareness of the importance of the early environment has motivated policies promoting maternal health during pregnancy, such as

the establishment of universal maternity care in Sweden in the mid-20th century. Today almost all expecting mothers are enrolled in maternity care and antenatal clinics have a strategic position in detecting and preventing risks and conditions which can affect the development of the fetus. It is therefore of great importance to identify effective interventions in maternity care and to understand how preventive interventions against health hazards in utero affect the health and early development of children.

This thesis consists of four self-contained essays covering the different aspects of caring for a family member from these perspectives. **Essay 1** addresses the consequences of having a parent in need of care on the adult children's employment, income, and sick leave absence. The second and third essay analyze how policies within antenatal care in Sweden aimed at identifying and reducing risky alcohol consumption during pregnancy affect the health of children. **Essay 2** studies the effects of an alcohol prevention program in Swedish antenatal on the health of children. **Essay 3** focuses on a specific part of the program and studies how preventive interventions targeted at those identified as having risky alcohol consumption affects infant health. Finally, **Essay 4** focuses on how the decision-making process in the family is affected by family policies. I study how access to childcare for an older sibling during parental leave affects the division of paid parental leave between mothers and fathers.

A challenge when studying the relation between, for example, care provision and labor supply is that even if we observe a relation between the two, this does not necessarily say anything about whether care provision causes a change in labor supply. It may well be that persons with weaker labor market attachment are more likely to provide care or that other factors not observed affect both care provision and labor market attachment. In order to observe the causal effect of providing care we ideally would want to compare the outcome of the individual to the outcome had he or she not provided care. The problem is that we cannot observe this *counterfactual* outcome. During the last three decades, economists' have made use of the rapid development in quantitative methods for causal inference where exogenous variation generated by nature is used to mimic an experimental situation to create a credible estimate of the counterfactual outcome.[4] Understanding the direction of causality is key in order to draw accurate conclusions from research, and a central theme throughout this thesis is the aim of estimating causal effects.

[4] Angrist and Pischke (2010) summarize how the evolution of empirical tools within economics has transformed the field.

1 Parenthood, family friendly policies, and gender equality

Recent decades have witnessed an increase in the female labor force participation together with decreased fertility. This has motivated the interest for family friendly policies in order to maintain both high fertility rates and high female labor force participation. The Nordic countries have since long had generous family policies including paid and job-protected parental leave, and publicly funded childcare which has likely contributed to the high female labor force participation (see for example Waldfogel 2002; Björklund 2006; Lalive and Zweimüller 2009). On the other hand, it has been pointed out that too generous parental leave policies may hamper female labor force participation (Gupta and Smith 2002; Karimi et al. 2012). Since women are usually the parent most involved in child care, the benefits may discourage them from career commitment (Albrecht et al. 2003).[5] Moreover, the aim of family policies goes beyond enabling the combination of work and family life; there is an outspoken goal that they should promote gender equality. Several attempts have been made by the Swedish government to increase the father's share of the parental leave take-up through reserving benefit days for each parent, and by introducing tax credits to parents who share the leave equally. But this seem to have had limited or no effect on the division of the responsibility of child care in terms of parental leave and leave for care of sick children (see for example Karimi et al. 2012 and Ekberg et al. 2013).

In Essay 4 I study another potential determinant that could affect the division of the caring responsibility, namely the care burden during the parental leave. I evaluate whether making childcare available for children to parents who are on parental leave with a younger sibling affects the division of parental leave between mothers and fathers. Access to childcare for the older child could be an important relief in everyday life by decreasing the care burden and thus making parental leave more attractive. Making parental leave less demanding may therefore impact parents' willingness to stay at home, which in turn could have implications for the division of parental leave. The effect of access to childcare is evaluated by utilizing the regional heterogeneity of the implementation of a childcare reform in Sweden in 2002 that gave children of parents who are on parental leave with a younger sibling the right to stay in childcare. Already prior to this reform, some municipalities allowed older siblings to keep their spot in daycare which implies that although the reform was implemented at the same point in time throughout the country, it had different implications for different municipalities. This heterogeneity is used in a so called difference-in-differences strategy where the change in parental leave outtake before and after the reform in municipalities where

[5] This may also be also be reinforced by employers' lower expectations on female career commitment (Albrecht et al. 2003).

children could keep their spot in child childcare is compared to changes in parental leave outtake where they could not.

I find no evidence that the availability of childcare for an older sibling has an impact on father's take-up of parental leave for the second born child. Nor is there any evidence of the reform having an effect on the division of paid parental leave between the mother and father. There may be several reasons for this finding. It could be that mothers and fathers value the reduction in care burden equally in which case the division should be left unaffected. Another potential explanation is that other factors such as gender norms and monetary incentives outweigh the impact of decreased burden.

2 Early life care and health promoting policies

A large body of literature has emerged in economics emphasizing the importance of factors in early childhood for the evolution of human capabilities (see for example Cunha et al. 2006 and Cunha and Heckman 2007). Early life conditions and *in utero* environments have been shown to be of great importance for health and economic outcomes later in life (Currie 2009; Almond and Currie 2011), and this has motivated policies for promoting maternal health during pregnancy. Furthermore, given the overwhelming evidence of negative effects of alcohol exposure in utero (McBride 1961; von Lenz and Knapp 1962; Jones et al. 1973; Barker 1990), an important part of health promoting policies has involved public interventions and recommendations concerning pregnant women's alcohol consumption. Most expecting women are aware that excessive alcohol consumption during pregnancy can be harmful for the child. But since there is no established "safe level" of alcohol consumption during pregnancy, and since earlier correlational studies indicates an unclear relation between moderate alcohol consumption and birth outcomes, this raises the concerns for increased alcohol exposure in utero.

In Essays 2 and 3 (both co-authored with Erik Grönqvist, Anna Sjögren and Helena Svaleryd) we study the effects of a universal alcohol prevention program in Swedish antenatal care with the aim of detecting and preventing risky alcohol consumption during pregnancy. Within the program midwives screen pregnant women for alcohol consumption using the Alcohol Use Disorders Identification Test (AUDIT); a questionnaire developed by WHO to detect harmful alcohol consumption. Women who are identified as having risky alcohol consumption become subject to preventive intervention in the form of motivational interviewing (MI) techniques to modify behavior, and are – if necessary – remitted to other health care professionals or to the social services. In connection to the implementation of the program midwifes were trained in using AUDIT as well as in MI technique.

An important issue when assessing the effects of an alcohol prevention program is that those who receive treatment tend to have higher alcohol consump-

tion compared to women who do not receive treatment. Therefore it is likely that treated women may differ in dimensions, many of which most likely are unobservable, that may affect the health of both the mother and the child. This implies that simple comparisons of outcomes between children whose mother was part and was not part of the program run the risk of being biased. In the two papers we exploit quasi-experimental variations in the allocation of the program across mothers, generated through to the stepwise implementation of the program and through the prescribed intervention guidelines.

In the first of these essays (Essay 2), we evaluate the effects on child health from introducing the program at Swedish antenatal clinics. By exploiting the regional time variation in the implementation of the program across municipalities within the same county, we are able to identify the causal effect of the program. The stepwise introduction among antenatal clinics enables us to compare child outcomes for subsequent "cohorts" of mothers before and after the implementation of the program. However, since child outcomes can change over cohorts for many reasons apart from the introduction of screening, the changes in outcome is compared to the change in outcomes in other "control" municipalities in the same county where the program is not yet introduced, thus using a difference-in-differences strategy. To construct measures of child health we use information about prescription of pharmaceutical drugs and hospital care consumption. We find that introducing the screening and brief intervention program for alcohol in antenatal care improves infant health. The program lowers the probability that a child is prescribed a pharmaceutical drug during the first year of life by 8.4 percent relative to a population average, and lowers the probability of the child being admitted to hospital during their first year of life by 7.5 percent. Our results suggest that the program leads to behavioral changes among treated mothers beyond the pregnancy since the effects on hospitalizations are mainly driven by reductions in inpatient care due to injuries and conditions where hospitalization could have been avoided.[6] We also find evidence of reduced maternal smoking during pregnancy, and suggestive evidence of increased breastfeeding. The impact on child health and maternal behavior may be due to either one or a combination of the following factors: (i) structured alcohol screening with the AUDIT instrument; (ii) training of midwives in MI-techniques; (iii) targeted preventive brief intervention using MI for pregnant women with elevated alcohol risk.

In the second of these essays (Essay 3), we try to isolate the effects on infant health and maternal behavior from one specific component of the alcohol prevention program, namely the targeted brief intervention for pregnant women with elevated alcohol risk. To establish causality, we exploit the variation in alcohol prevention generated through the guidelines that prescribe when to implement the intervention: if a woman scores 6 or higher on the AUDIT ques-

[6]These conditions are avoidable in the sense that appropriate care and nutrition is likely to reduce their incidence.

tionnaire (which is scored on a 0-40 scale), this indicates a strong likelihood of harmful alcohol consumption and the midwife is instructed to initiate a brief intervention using motivational interviewing techniques with the aim of motivating and encouraging behavioral modification. Thus, the guidelines create a discrete increase in the probability of receiving treatment at AUDIT 6 which we exploit in a so called regression discontinuity (RD) framework (see for example Thistlethwaite and Campbell 1960; Lee and Lemieux 2010) where we compare women scoring just above (and therefore are subject to treatment) and just below the cutoff of 6. The RD strategy relies on the assumption that although the guidelines increase the probability of receiving treatment, there is little reason to expect a similar discrete change in the characteristics of the mothers scoring just above and below AUDIT 6. Pregnant women at the cutoff for treatment are very similar (almost identical) to women just below the cutoff, except for the higher probability of receiving the treatment.

We find that the targeted preventive MI-intervention has small to negligible average effect on birth weight of children whose mothers were treated. There is however indications of a larger effect in the lowest part of the birth weight distribution, which suggests that the intervention may have more important health effects for children at greater risk. We find no evidence of the intervention leading to more women breastfeeding or ceasing to smoke during the pregnancy. Since we do not have data on the same types of outcomes in the two studies, it is difficult to say with certainty whether it was the introduction of the program at large, or if it was the targeted brief intervention that generated the positive effect of the program observed in Essay 2.

3 Parental care and its opportunity costs

In most developed countries, the working-age population is shrinking in size relative to the number of elderly (OECD 2008). This is of concern to public policy since it imposes fiscal strains due to a larger share of the population not working, but also because future workers may face a higher personal cost of care for the elderly in the form of care-needing parents.[7] Informal care – organized within the family – could reduce the pressure on the health care systems, but there may be opportunity costs to informal care that are not fully taken into account, such as the reduction in labor supply and adverse health effects for the caregiver.

In Essay 1 I analyze how labor supply and sickness absence is affected by having an aging parent in need of care. Parental care need is identified by studying parents who suffered a stroke or who are in their final years of life. Stroke is a life-threatening condition that strikes suddenly and often leaves the

[7] Although increased longevity does not necessarily imply increased health care expenditure as older people also become healthier (Zweifel et al. 1999) it raises the demand for long-term care (Spillman and Lubitz 2000; Seshamani and Gray 2004).

patient with serious adverse effects. It has also been found that the care need is concentrated to the final years of life. Thus, both of these health events (stroke and being in the final years of life) can be expected to alter the care need of the parent. Since the primary caregiver of elderly is usually the spouse, I focus on lone elderly parents and study their adult children's income, employment, and sickness absence in the years before and after the parent becomes ill. The challenge in studying the relation between care provision and labor market outcomes is that there may be a selection of individuals with inferior labor market opportunities into care. To establish causality, and not mere correlation between having a parent in need of care and labor market outcomes, I use the within individual variation to eliminate the potential bias from unobserved characteristics affecting both the decision to care for the parent and labor market outcomes. The individual's labor market outcome at a time point when the need for care is assumed to be limited is used as counterfactual for the labor market outcome when the parent is in need of care. By including year fixed effects that are specific for the child's birth cohort, I control for changes over time in employment, income, and sickness absence. I utilize the variation between individuals, belonging to the same birth cohort, in the timing of the parent's ill health to identify the effect on adult children from having parents in their last years of life or having suffered stroke.

I find that having a lone parent in his or her final years of life has a small negative impact on the adult child's income. For sons, there is also a negative impact on employment prior to parental demise but for daughters this effect is not statistically significant. The negative impact on both employment and income is largest (and statistically significant for both sons and daughters) in the year that the parent dies and the years immediately after, which could be due to grief or realization of inheritance. I also find that the sick leave absence for daughters increases in the year that the parent dies. Overall however, the impact on labor market outcomes from having a parent in his or her final years of life is small. I find no evidence of an effect of having a lone parent suffering a stroke on income and employment of either sons or daughters. One interpretation of these findings is that stroke is such a severe health shock that it requires care from the formal caring institutions, leaving children's labor supply unaffected. There is however a short-term effect of parental stroke on sickness absence for daughters and suggestive evidence of an upward shift in sickness absence also for sons, which indicates that the child's own health may be affected by the severity of the health shock.

Contrary to what would be expected given earlier research on the consequences of informal care provision (see for example Ettner 1995 and Lilly et al. 2007), I find no differences between men and women (or between brothers and sisters) in the effects of having a parent in need of care as a result of stroke or being in the final years of life. If anything, the impact on income seems to be larger for sons, at least with respect to having a parent in its final years of life. There may be many reasons for this result. It may be that men are employed

to a larger extent implying that their work life is more affected, whereas informal care supply mainly affects women's leisure, or that women have higher productivity in combining work and family care. It may also be that previous studies have overestimated the effect of care provision for women by not considering that women with lower labor market attachment may be more likely to provide care.

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I. Sick of my Parents? Consequences of Parental Ill Health on Adult Children

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1 Introduction

The number of elderly is increasing in most developed countries implying that the working age population is shrinking in size relative to the number of retirees (OECD 2008). This is of concern to public policy since it imposes fiscal strains due to a larger share of the population not working. The working-age population may, in addition to the burden of financing an increasing public sector, also face a higher personal cost of care for the elderly in the form of care-needing parents.[1] Care for the elderly can either be supplied by the health care services or informally by families and relatives. Many countries promote informal care by offering financial support to the caregiver, but transfers rarely measure up to the time input of the caregiver (Wimo and Jönsson 2001). While informal care reduces the financial pressure on the health care system, there may be large opportunity costs associated with informal care provision when the caregiver is of working age. Such costs include productivity loss on the labor market, reduced labor supply, and adverse health effects of the caregiver (see for example Fevang et al. 2012; Bauer and Sousa-Poza 2015). Furthermore, there may be important gender differences in the response to parental care need. Women can be more adversely affected as they generally have the main responsibility for caring in the family (Ettner 1995; Carmichael and Charles 2003; Crespo and Mira 2014; Heitmueller and Inglis 2007). Increased care burden could thus add to the psychological pressure of the dual role sometimes referred to as the "double burden" of women (Bratberg et al. 2002). It is therefore important to study the consequences of informal care provision for the adult children and, in particular, whether the response to parental care need differs between men and women.

I analyze how employment, income, and sickness absence is affected by having an elderly parent in need of care. Care need is identified using two types of health shocks – stroke and being in the final years of life – where the need for care is elevated in the years preceding death and the in the years following stroke (Gerdtham and Jönsson 1990; Emanuel et al. 1999; Polder et al. 2006; Wolff et al. 2007; Yang et al. 2003; Meijer et al. 2011; Bugge et al. 1999; McCullagh et al. 2005). More specifically, I study the change in labor supply and sickness absence of adult children having parents in their last years of life or having suffered stroke. The impact of increased parental care need is studied separately for adult sons and daughters, and I also exploit within-family differences in the response to parental care need and compare brothers and sisters to see whether there are gender differences. Rather than observing informal care provision directly, I identify increased care need following a health shock. Having a parent in need of care can affect the adult child both as a result of actual informal care provision, and also as a result of other cir-

[1] Although an aging population not necessarily increases the demand for care since individuals also tend to get healthier (Zweifel et al. 1999; Yang et al. 2003), it raises the demand for long-term care (Spillman and Lubitz 2000; Seshamani and Gray 2004).

cumstances related to having a family member suffering a health shock such as wanting to spend more time with that person and worrying about his or her well-being. While the former can be described as caring *for* a person, the latter relates to caring *about* a person (Bobinac et al. 2010). In this study, I will refer to both these types of care using the term informal care provision.

A negative correlation between provision of informal care and labor supply is well established in the earlier literature (see Lilly et al. (2007) for a literature review). It is also suggested that informal caregiving can have further implications not only on labor supply, but also on the psychological and mental well-being (Bauer and Sousa-Poza 2015) as well as health (Schmitz and Stroka 2013) and life satisfaction of the caregiver (Leigh 2010). The challenge in estimating a causal relation between the adult children's provision of informal care and their labor market outcomes is that there is likely a selection of individuals with lower alternative costs on the labor market into parental care. If not taking the unobserved characteristics affecting both informal care provision and labor market attachment into account, the impact of having a parent in need of care may be overestimated. Much of previous literature on the relation between informal care and labor supply has not addressed these endogeneity problems while some studies have drawn on instrumental variables approaches to control for the endogeneity of caregiving. These earlier studies generally document a negative relation between informal care provision and labor market outcomes (Heitmueller and Inglis 2007; Bolin et al. 2008; Ettner 1995; Ciani 2012) where the impact varies with the intensity of the care (Lilly et al. 2007; Ettner 1996) and the consequences seem to be more severe for female caregivers (Heitmueller and Inglis 2007; Ettner 1995, 1996). Exceptions include Crespo and Mira (2014) and Meng (2013) who find only negligible impacts on labor market activity of the caregiver. However, since most of these studies rely on cross-sectional data and use information about parental health as instruments for the care need of the parent, the validity of these instrumental variable studies is questionable given the intergenerational transmission of health between parents and children (Björkegren et al. 2017).

There are also a few studies that more credibly utilize panel data to investigate the consequences of parental care. Using a difference-in-differences approach, Løken et al. (2016) study the related question of substitution between formal and informal care and find that expansion of formal care reduces work absence among middle-aged daughters. Spiess and Schneider (2003) use panel data with information about changes in informal care provision over time and find that care initiation results in fewer hours worked, and also Van Houtven et al. (2013) find negative effects on labor market outcomes of female care provision in the U.S using panel data. Similarly, using a panel survey of Australians Leigh (2010) finds a negative, but small, impact on labor force participation from initiating caregiving, and that this effect is much smaller compared to the association in a cross-section setting. The paper closest related to my study is Fevang et al. (2012) who use Norwegian register data and find that employ-

ment decreases and dependence on sickness insurance increases among adult children in the years immediately prior to the death of a parent.

I will add to this literature by identifying care need using two types of parental health shocks that are likely to cause different types of care needs due to the different courses of the disease. I study the effect of parental stroke and parental demise in Sweden. While the need for care increases instantly following a stroke, the care demand often increases gradually, and is concentrated to, the final years of a person's life. Therefore, the care needs caused by these two health shocks are likely to differ. The role of the formal care differs between the two types of health shocks as formal care is involved from the start in case of a stroke. The possibility to substitute formal care with informal care may also be different between the two health shocks since informal care can only substitute formal care that does not require medical skills.

In this paper, I exploit the within-individual variation to eliminate the bias from unobserved individual characteristics affecting both the decision of informal care provision and labor market outcomes. I take advantage of rich register data covering all Swedish residents which allows me to study the individual time path of labor market outcomes of adult children in the years before and after a parental health shock. Labor market outcomes and sickness absence in periods when the need for care is unaffected by the health shock is used as counterfactual. In this way, the endogeneity of care provision can be controlled for. In the first part of the analysis, I compare the adult child's labor market outcome and sickness absence before parental stroke to the period after the stroke, when the parent's need for care has increased. In the second part of the analysis, I compare the labor market outcomes and sickness absence of the adult child before the parent is in its final years of life to the years just before, and also after the parent has died when care need of the parent has ceased. In a separate analysis I will also focus on gender differences in the impact of parental care need by studying the income gaps and sick leave gaps of brothers and sisters from the same family. By looking within families I can control for observed and unobserved characteristics of the family that may influence the informal care provision. Moreover, I can control for potential endogeneity in the timing of a parental health shock that may arise due to children's investments in parental health.[2]

Two separate samples are used for the analyses consisting of children to parents who suffered stroke between the years 1995 and 2005, and children whose parents died between the years 1995 and 2008.[3] Using universal administrative Swedish registers between 1990 and 2010 I am able to track adult children's employment, incomes and sick leave absences over the years before

[2] See Section 3 for a more detailed description.

[3] Since the period of focus for studying the effects of parental care need takes place prior to death, a shorter period of outcomes post death is required. Thus, observations where parent died in 2006 to 2008 are included as well in order to get a larger sample, although this means I can only study the outcome of these children for a shorter period after demise.

and after the parental health shock. The main part of the analysis will focus on lone parents since the primary caregiver of married elderly is typically their spouse (Ulmanen and Szebehely 2014). The register data allows me to match lone elderly with their children and also to match siblings. In this way, I can explore whether there are differences in the response to parental care need between male and female siblings.

I find no evidence of an effect of having a lone parent suffering stroke on the labor market outcomes for either sons or daughters. These results are stable across subgroups of different individual characteristics of adult children, and there seem to be no difference in response between male and female siblings. One interpretation of these findings is that stroke is such a severe health shock that it requires care from the formal caring institutions, leaving the children's labor supply unaffected. Also, the substitutability between formal and informal care may be less feasible if the care following stroke requires medical skills. There is however a short term effect of parental stroke on sickness absence for daughters in the months just after parental stroke and also suggestive evidence of an upward shift in the sickness absence of sons, which indicates that the child's own health could be affected by having a parent suffering a severe health shock.

The results from the analysis of adult children having a lone parent in his or her final years of life suggest that there is no statistically significant impact on employment for daughters but a small marginally statistically significant negative impact for sons prior to parental demise. The negative impact on employment is largest in the year that the parent dies, and it continues to be deceased for both sons and daughters after parental demise. The results from studying income conditional on employment suggest that the income of children is reduced in the years leading up to the parent's death. As with employment, the reduction in income is largest in the year that the parent dies, and remains reduced for a couple of years after. I find that the reduction in income is more likely to be the result of having a parent in need of care rather than children reducing their behavior in anticipation of expected inheritance since the behavior after parental demise in a group where expectation of inheritance is less obvious (parent died suddenly) is similar to that in a group where parent did not die suddenly (and children therefore would have been able to adapt labor supply in anticipation of inheritance). Overall however, the impact on both employment and income is small suggesting that they are reduced by around 1 percent in the years around parental demise. Again, there seem to be no difference between male and female siblings within the family. If anything, the impact on income seems to be larger for sons. I also find an increase in sick leave absence of daughters in the year that the parent dies. Since no similar impact is found among daughters whose parents died suddenly, and if we assume that parents that die suddenly have lower care needs, this indicates that the increase in sick leave is not solely driven by grief but rather stems from having a parent in need of care.

The rest of the paper is organized as follows. Section 2 provides some background information about informal care provision in Sweden and how stroke and being in the final years of life affects the demand for informal care. Section 3 explains the identification and the empirical model to be estimated and Section 4 describes the data and provides some graphical evidence. Section 5 presents the results and finally, Section 6 concludes.

2 Background

The care need of an elderly parent is in some cases the result of a dramatic event such as stroke, but the care demand may also increase gradually as part of a natural aging process. How the adult children of care-needing parents are affected depends on the level of care required and on the availability of care from public or private sector[4] as well as the substitutability between formal and informal care. This section provides an overview of informal care provision in Sweden and how the two different types of health shocks affect the demand for care.

2.1 Informal care in Sweden

Around one fifth of the Swedish population provides informal care to a family member and the most common situation (around half of the cases) is that an adult child cares for his or her elderly parent (Socialstyrelsen 2014). Informal care provision may involve different activities but is usually oriented towards basic care and supervision as well as practical chores such as grocery shopping, cleaning, transportation and contacts with authorities, rather than medical attention. From an international point of view, Sweden along with the other Scandinavian countries has a comprehensive publicly funded system of care for older people. Municipalities are responsible for providing and financing both home-based care (home-help or home nursing) and institutional care facilities.[5] Services are granted based on care needs. During the last decades however, the amount of publicly provided care has been reduced and the requirements for being granted care has become stricter which, along with a shift from the more comprehensive institutional care to home-based care, has increased the demand for informal care provision provided by family members (Johansson et al. 2003; Ulmanen and Szebehely 2015). It has been estimated that 70 percent of the total care effort for elderly living in their own homes is

[4] Although there has been an increase during the last decade, the use of privately purchased services play a marginal role in Sweden. (Ulmanen and Szebehely 2015)

[5] In 2000, around 20 percent of the population aged 80 years and older received public home help services in their private homes and around 20 percent lived permanently in nursing homes (Larsson et al. 2006).

supplied by the elderly's next of kin (Johansson et al. 2003).[6] Moreover, earlier literature suggests that the primary caregiver of an elderly parent is usually the spouse (see for example Ulmanen and Szebehely 2014). Therefore, I will focus the analyses on lone parents.

How adult children respond to informal care demand may depend on many factors. Sometimes, informal care giving can be combined with work (at the expense of leisure) but sometimes it may require that the offspring cuts back on working hours.[7] A Swedish survey in 2013 reports that 13 percent of women and 8 percent of men in Sweden reduce the number of hours worked, quit their job or retire as a consequence of providing informal care (Ulmanen and Szebehely 2014). For family members of stroke patients specifically, around one in ten of those younger than 65 years of age report to have reduced the number of hours worked or retired one year after the stroke (The Swedish Stroke Register 2016). Although these surveys cannot control for the endogeneity of care provision, they support the hypothesis that there could be effects of informal care provision on labor market outcomes. Moreover, having a family member suffering from a life-threatening disease such as stroke can be a stressful experience that affects the psychological well-being of the adult child (Forsberg-Warleby et al. 2002; Jönsson et al. 2005) which in turn could spill over to a reduced labor market productivity (Bauer and Sousa-Poza 2015).

There may also be important gender differences in the response to parental care need. Traditionally, women have had the main responsibility for the care of family members. Earlier literature suggests that daughters are more likely to change their labor supply in response to parental care demand (Ettner 1995; Bolin et al. 2008). According to Szebehely (2005), receiving informal care from a daughter was twice as common as receiving care from a son. However, more recent Swedish survey data suggests that men and women are more equal in providing care, but that women provide more demanding care and are more likely to be affected psychologically (Ulmanen and Szebehely 2014). In this paper, the gender difference will be addressed in two different ways: men and women will be studied separately in the main analysis, and in an additional analysis I will compare the response to a parental health shock between brothers and sisters within the same family. Whereas the first strategy compares women and men on average, the latter directly compares gender differences within the family and may hence reveal potential differences in the expectations on sons and daughters in providing family care.

[6] Ulmanen and Szebehely (2014) find that 42 percent of the surveyed aged 45-66 care for a family member at least once a month and that this family member is most often an elderly parent.

[7] The decision making process and what motivates children to care for their parents is described in Heitmueller (2007) and in Fevang et al. (2012)

2.2 Informal care demand following stroke

Every year around 30 000 people suffer stroke in Sweden and it is the third most common cause of death, after myocardial infarction and cancer (Socialstyrelsen 2011). Moreover, stroke is the number one cause of impairment among adults and one third of the survivors are left with some type of disability (Socialstyrelsen 2017). Stroke is a "brain attack" and can be characterized as the blood flow to an area of the brain being cut off. As a result, brain cells are deprived of oxygen and begin to die. There are two main types of stroke: the most common is ischemic stroke where blood vessels in the brain are blocked by blood clots. The less common is the hemorrhagic stroke and this happens when a weakened blood vessel leaks or a brain aneurysm bursts, often resulting in death. How a patient is affected by the stroke depends on the amount of cell death and which part of the brain that is affected. Patients with more severe strokes may be permanently paralyzed; may suffer from balance- and mobility disorders; and may lose their ability to speak. The average age of a stroke patient is around 73 for males and around 78 for females. (Socialstyrelsen 2011; Swedish Heart-Lung Foundation 2016).

Stroke is not a random event. The risk of stroke varies with socioeconomic background (Peltonen et al. 2000) and there is also a genetic component (Kiely et al. 1993). The risk of suffering stroke increases exponentially with age (Asplund 2003) and the most predominant risk factors are hypertension (high blood pressure), smoking, diabetes, atrial fibrillation, and physical inactivity (O'Donnell et al. 2010). Nevertheless, the symptom onset is usually sudden[8] and stroke is an acute condition. Being struck by stroke is a life altering event with consequences not only for the patient but also for the family members of the patient. The recovery process and the rehabilitation after a stroke varies greatly but the largest regain of function usually occurs during the first weeks after the stroke (Ullberg et al. 2015). According to a follow-up survey of patients who suffered stroke one year earlier, one in six report being dependent on others to manage daily activities. For those aged 75 years and older the corresponding share was two thirds. Since most of the stroke survivors live in their own homes[9], the need for care in the home is large (Bugge et al. 1999; McCullagh et al. 2005). Around 40 percent of the one-year stroke survivors report being dependent on the care from family members such as their adult children (The Swedish Stroke Register 2016). It is therefore reasonable to assume that a parental stroke increases the need to care for the parent. This care is not limited to informal care in the sense of providing help for someone who is ill, but it also involves caring about the parent in a broader sense like worry-

[8] Sudden enough to be categorized into two-hour intervals during the day. Interestingly, the incidence of stroke is highest between 10:00 am and noon (Marler et al. 1989).

[9] One year after the stroke, around 90 percent of stroke patients are able to live in their own home whereas 10 percent live in institutional homes. The average number of days spent in hospital as the result of a stroke is around 10 days (Socialstyrelsen 2011).

ing about the parent's well-being and wanting to spend time together. Having a parent suffering stroke is thus used as a proxy for care need. The adult children of elderly parents are studied in the period leading up to the parental stroke, where the care needs of parents are assumed to be unaffected, and compared to the period after where the care need of the parent has increased as a result of the stroke.

2.3 Informal care demand in the final years of life

Following Fevang et al. (2012) the second health shock that is studied in this paper is death, or rather the final years of an elderly parent's life. Although it is often suggested that aging populations will increase health care expenditure, research shows that health care costs are primarily determined by proximity to death rather than age (Polder et al. 2006; Seshamani and Gray 2004; Zweifel et al. 1999).[10] Not only is the care demand increased as elderly parents approach their death, but it is often concentrated in the final years of life (Gerdtham and Jönsson 1990; Emanuel et al. 1999; Polder et al. 2006; Wolff et al. 2007; Yang et al. 2003). Time to death can therefore be used as an approximation of disability (Meijer et al. 2011). Moreover, having a parent in the final phase of life may also be associated with wanting to spend time together and with grief, both of which requires "mental attention". The potential impact on the child's labor market activity is therefore not limited to informal care provision only, but includes the consequences of these other aspects as well. Having a parent in the final phase of life is used as a proxy for this type of care need. The adult children of elderly parents are studied in the period before as well as the period leading up to parental demise, and in the period after where care need of the parent has ceased to exist. Since the initiation of increased care demand in the final years of life is less distinct (compared to the timing of e.g. stroke) it is more complex to determine when it is realistic to assume that the adult child is unaffected by the parent's increased care demand. In the analysis, it is assumed that there is no causal impact of having a parent in its final years of life more than eight years before parental demise.

3 Identification strategy

There are several challenges associated with estimating the effect of a parental health shock on labor market outcomes. To begin with, having a parent suffering from a health shock (stroke or being in the final years of life) at a certain point in the life course is not random in relation to the adult child's own health and labor market outcomes. The risk of stroke is related to lifestyle factors

[10] This is often referred to as the "red herring argument" (Zweifel et al. 1999).

such as diabetes and high blood pressure, which in turn is genetic. And although everyone dies at some point, those that have parents who die during a certain period are most likely different from adult children of the same age whose parent do not die during that period. It would thus be misleading to compare adult children that experience a parental health shock to those that do not. Therefore, I study only adult children whose parent is indeed struck by stroke or die during the period studied. Since the primary caregiver of elderly is usually their spouse (Ulmanen and Szebehely 2014) I will focus the analyses on children of lone parents.

Moreover, the timing of a parental health shock over the life course cannot be assumed to be uncorrelated with characteristics of the offspring. With a positive intergenerational correlation in health (Björkegren et al. 2017), healthy individuals can expect to have a parent suffering a health shock at higher own age than less healthy individuals. Since health itself is correlated with labor market performance (for literature reviews see e.g. Strauss and Thomas 1998, Smith 1999, and Deaton 2003), cross-section estimates of the effect of a parental health shock on labor market productivity would likely be biased. Furthermore, how an adult child reacts to a parental health shock and how likely they are to supply informal care depends on the individual's attachment to the labor market. Those with weaker labor market attachment and thus a lower alternative cost of supplying informal care are likely to respond more intensely to increased demand for informal care than others. If not addressed properly, the sorting of individuals into caregiving will result in biased estimates that overestimate the effect of caregiving.

I handle these selection problems by estimating individual fixed effects models that use only the within-individual variation in labor market outcomes and sickness-absence over time. The strategy is inspired by Fevang et al. (2012), and I utilize variation between individuals, of the same birth cohort, in the timing of the parental health shock to identify the effect of having a parent in need of care, controlling for time invariant individual characteristics. I assume that there is no impact from the parental health shock in the pre-treatment period, which occurs at different points in time depending on the type of health shock studied. That is, in the analysis of children to parents who suffer stroke, the pre-treatment period is the years before the parental stroke. In the analysis of children to parents in their final years of life, the pre-treatment period is the years more than eight years before the parental demise.

The main regression model is the following individual fixed-effects model and it is estimated separately for each sample and for sons and daughters[11]:

$$y_{i(c)t} = \alpha + \gamma_i + \lambda_{ct} + \sum_{k=t}^{5} \delta_k \mathbf{1}[t = k] + \varepsilon_{ict} \quad (1)$$

where $y_{i(c)t}$ is the labor market- and sickness absence outcomes for individual i, belonging to birth cohort c at time t and it is measured in three ways: a dummy for employment, log of annual income, and the number of days on sick-leave. The impact of the parental health shock is captured by the δ_k's and they measure the change in outcome compared to a reference level. That is, for the stroke sample the δ_k's (for $k = -4, -3, ..., 5$ where k is the number of years[12] away from the parental stroke) estimate the change in outcome at $t = k$ relative to the reference level, which is an average of all available years at least five years prior to stroke ($t \leq -5$). In the analysis of children with parents in their final years of life, the δ_k's (for $k = -8, -7, ..., 5$ where k is the number of years away from parental demise) estimate the change in outcome at $t = k$ relative to the reference level, which is an average of all available years at least nine years prior to stroke ($t \leq -9$). The care demand of the parent is expected to increase at different points in time in the two analyses. For children of parents suffering stroke, the effect of increased informal care demand is expected to happen after $k = 0$ i.e. after the parent suffers stroke. For children having a parent in his or her final years of life on the other hand, the effect of informal care demand is expected to take place in the years immediately before $k = 0$.

γ_i is a vector of individual fixed effects capturing the time invariant individual specific characteristics, $\mathbf{1}[\cdot] = 1$ if the expression in brackets is true, and zero otherwise, and ε_{ict} is an error term. Since I will study the time-path of the outcomes of adult children over several years, I need to control for changes in the outcome due to age. Moreover, there may be differences in the wage trajectories for different birth cohorts. To this end I will include a vector of birth cohort specific year effects, λ_{ct}, to control for time shocks so that it can vary by birth cohort (and gender). Throughout all estimations in the main analysis, the standard errors will be clustered at the individual level to account for potential within-individual correlation in the error terms.

Given the individual-level panel data structure and the difference in timing of the health shock between individuals, the identification strategy can be seen as a form of difference-in-differences approach. The main identifying assumption is that the timing of the parental health shock is exogenous, i.e. that the timing of the parental health shock is not correlated with expected changes in

[11] All the regressions are estimated using ordinary least squares. Ideally, one would want to use a fixed effects logit model for estimations on employment. However, due to the large number of fixed effects, such a model does not converge.

[12] Since sick-leave data is available on monthly level, I will also perform a short-run analysis where I study the period 12 months before and after the parent is struck by stroke.

offspring's labor market outcomes or sick leave that would have happened in absence of a parental health shock. Put differently, exogeneity of the timing of the parental health shock implies that I assume that the change in treatment status (having a parent in need of care or not) is uncorrelated with changes in the error term, and that the timing of the parental health shock is exogenous to child behavior. Specifically, I assume that children's investment in care for their elderly parent is exogenous to the timing of parent's health shock. If children's time investments in their parents make the parents healthier (Torssander 2013), the timing of the parental health shock may not be exogenous to the offspring's labor market outcomes. This assumption is probably more important when studying children of parents in thier final years of life since stroke is less likely to be affected by children's investments. When focusing on within-family impacts from a parental health shock in Section 5.4, this assumption can be relaxed.

In the stroke analysis, the pre-treatment period (all years prior to stroke) can be studied to test the assumption of parallel trends which in this case corresponds to no causal impact on the outcome variable before treatment. For the sample with children of parents in their final years of life, the distinction of the pre-treatment period is less precise and coincides with the reference level, but I assume no significant effects on the outcomes at least eight years away from the demise (that is for $k \leq -9$ which is the reference level, and $k = -8$).

A potential threat to identification in the analysis of children with parents in their final years of life is the fact that parental demise may imply a changed budget constraint due to inheritance (Elinder et al. 2012). Individuals may change their labor supply in response to an expected inheritance. It is therefore difficult to disentangle whether any effects on labor supply are the result of increased informal care demand or a changed budget constraint. One way to get at whether it is informal care provision that is driving the results is by comparing the labor market response after parental demise in a sample where parents die suddenly, and where the expectation of inheritance therefor is less obvious, to one where they do not. If the impact on income after parental demise is smaller in the sample where parents did not die suddenly this could indicate that the children, knowing they would be inheriting in the near future, adapted their labor supply in advance rather than after parental demise.

4 Data, descriptive statistics and descriptive graphics

In this study, two different samples are analyzed: one consisting of families where the elderly parent suffers from a stroke and one of families who lose an elderly parent. Several universal Swedish administrative registers are combined to create these samples.

The underlying population in the stroke sample consists of all adult children of stroke patients who suffer stroke between 1995 and 2005. Using reg-

ister information on all inpatient hospital episodes available from the Swedish National Board for Health and Welfare (NBHW), I sample all first-ever stroke patients who suffered stroke and survived for at least one month. A stroke patient is defined as an individual being admitted to hospital with the primary diagnosis of cerebrovascular disease.[13] The patient register contains detailed information on the admission date as well as diagnosis classified according to WHO's ICD9 and ICD10 classification system. The information is typically entered into the hospital administrative system at discharge and hospitals are obligated by law to report the data.

The underlying population in the sample of children with a parent in the final years of life consists of all adult children to parents who pass away between the years 1995 and 2008. From the Causes of Death Register held at NBHW I retrieve information on the date of death as well as the cause of death for all Swedish residents who die during these years. The information about cause of death will be used to distinguish whether the death was sudden.[14]

The stroke patients and the diseased individuals are linked to their family members using the Swedish population register from Statistics Sweden to create the two separate data sets: one of stroke families, and one of parental death families. The population register covers all persons born in Sweden and links individuals to their biological children. It also contains information on birth year and month as well as the birth order of children. I restrict attention to families where all children share the same biological mother and father. Moreover, since earlier literature suggests that the primary caregiver of an elderly parent is usually the spouse (see for example Ulmanen and Szebehely 2014), I distinguish between lone and non-lone parents. Lone parents are defined as individuals where the other parent of their children is dead at the time of the health shock.[15]

In order to avoid non-participation in the labor market due to higher education and retirement, I restrict the sample to include only observations when the child is between the ages 35 and 65. Moreover, because I want to ensure that there are observations of the outcomes prior to the parental health shock, I focus the analysis on individuals that I can follow at least five years before

[13] Specifically, patients admitted with the following ICD9 diagnosis are included: 433 – Occlusion and stenosis of precerebral arteries, 434 – Occlusion of cerebral arteries, and 434 – Occlusion of cerebral arteries. Patients with the following ICD10 diagnosis are included: I61.9 – Intracerebral hemorrhage, unspecified, I63.9 – Cerebral infarction, unspecified, and I64 – Stroke, not specified as hemorrhage or infarction.

[14] The definition of sudden death is taken from Andersen and Nielsen (2011) who defines sudden death as death caused by conditions with the following ICD codes: I22-I23 (acute myocardial infarction), I46 (cardiac arrest), I50(congestive heart failure), I60-I69 (stroke), R95-R97(sudden death from unknown causes), V00-V89 (traffic accidents), V90-V99 ,X00-X59, X86-X90 (other accidents and violence).

[15] This is measured with an error. I cannot observe if an elderly has re-married or is cohabiting with a partner who is not the parent of their child, as register data on civil status is only available for individuals aged 65 and younger.

parental stroke or nine years before parental death. The stroke sample consist of 99 116 adult children whose parent suffered stroke between the years 1995 and 2005.[16] The parental death sample consists of 984 054 individuals who lost a parent between the years 1995 and 2008.

I focus on three outcomes: employment, annual labor market income, and number of days with sickness benefit. Employment is defined as earning at least 165 000 annually which corresponds roughly to annual labor market income from full time work for those with minimum wages (Skedinger 2005). In order to capture impacts at the intensive margin, I also study income measured as annual income conditional on employment. I focus on employed individuals when studying income since those working fulltime are more likely to be affected by parental care needs because they face a time restriction in combining labor market work and parental care demands. However, restricting the outcome on employment implies that there could be compositional changes in the sample since only those who are employed will be studied in the analysis of impacts on income. This means that those who remain in the sample could be positively selected, implying that I possibly underestime the impact on income of parental care need. For robustness, I will therefore also study income conditional on the lower level of earning at least 20 000 SEK annually.[17]

For each adult child I retrieve information on labor market outcomes and socioeconomic background characteristics from register data held at Statistics Sweden based on administrative records and population censuses. Information on labor income stems from annual reports from employers to the Swedish tax authorities, reporting total annual income for declaration purposes.

Data on sickness absence is retrieved from the Social Insurance Agency (SIA) and contains information about start and end date of sickness-spells that are reimbursed by the SIA. People who work or are unemployed in Sweden are entitled to sickness benefits in case of own illness. When employed, the employer pays sick-pay from day 2 to day 14 (the first day is not replaced) of the sick spell. Thereafter, the SIA pays sickness benefits. For unemployed persons, the SIA pays sickness benefits already from day 2 and onward. The register contains data on sickness absence with sickness benefit from the SIA and I therefore study number of days on sick leave from the first day of week three in a sick-spell for employed individuals and from the second day in a sick-spell for unemployed individuals. Thus, for most individuals I am not able to study short-term sickness absence but focus rather on longer-lasting

[16] Individuals where both parents suffer stroke during the studied period are excluded.

[17] Given that it is preferable to study log income, I can only study annual incomes larger than 0.

sick absences.[18] I measure the number of days on sick-leave at monthly level but also at annual level to be comparable to the labor market outcomes.

Since I use data on sickness benefits from the SIA I will not capture the short absences from work (usually not more than 10 days) that are often granted by the employer through collective agreements to be able to e.g. attend a funeral or to care for a sick relative. Nor are benefits for care of closely related persons ("Närståendepenning") included.[19] This means that sickness absence studied in this paper mainly captures the adult child's own health.

Descriptive statistics of the samples are found in Table 1 which shows the summary statistics in the year of the parental health shock ($t = 0$). The number of observations per event year for the two samples (i.e. for $t = -9, -8, ..., 5$) is found in Table A1 in Appendix.

[18] Since this implies that the number of days on sickness benefit depends on whether or not the individual is employed, and because employment may be affected by informal care provision, this outcome may be endogenous. I therefor adjust the number of days on sickness absence for unemployed individuals by reducing them with 14 days, and reassuringly this does not change the results in the analysis.

[19] Benefit for care of closely related persons is a cash benefit paid by SIA for caring for a close relative who has a life-threatening condition.

Table 1. *Descriptive statistics of the studied samples in the year of the health shock, t=0*

	Stroke sample		Death sample	
	(1)	(2)	(3)	(4)
	All	Lone Parent	All	Lone Parent
Child's age at shock	50.22	51.63	52.26	54.18
	(5.441)	(5.412)	(7.356)	(6.744)
log Income (hundreds SEK)	8.007	8.001	8.045	8.039
	(0.373)	(0.368)	(0.384)	(0.380)
Employed (%)	69.16	67.35	65.9	63.5
	(46.18)	(46.90)	(47.4)	(48.2)
Sick days per yr.	19.20	20.46	16.85	17.64
	(67.63)	(69.78)	(62.86)	(64.39)
Parental age	79.89	81.47	82.46	84.46
	(5.765)	(5.489)	(7.348)	(6.488)
Year of shock	2000.1	2000.2	2003.5	2003.6
	(3.193)	(3.165)	(4.459)	(4.410)
Share w. paternal shock (%)	45.49	24.47	47.03	29.89
	(49.80)	(42.99)	(49.91)	(45.78)
Share w. university educ. (%)	37.94	36.12	37.53	36.12
	(48.52)	(48.04)	(48.42)	(48.03)
Living in same muni. (%)	50.95	51.38	52.06	52.28
	(49.99)	(49.98)	(49.96)	(49.95)
Singleton child (%)	15.16	16.09	14.52	15.93
	(35.87)	(36.75)	(35.23)	(36.60)
Sudden death (%)	-	-	15.71	17.41
	.	.	(36.39)	(37.92)
Observations	99116	44020	984054	613089

Note: Means of individual characteristics in the year that the parent suffers a stroke or dies. Standard deviations in parenthesis. The log annual income conditional on employment (deflated) is measured in hundreds SEK.

4.1 Descriptive evidence

Figure 1 illustrates the pooled cross-section relation between time distance to parental health shock and the log income and number of days on sickness absence for children for the two samples. The upper-left graph shows the time distance in years to a parental stroke and the annual log income (deflated and measured in hundreds SEK) for sons and daughters. The log income profiles of sons and daughters appear to be unrelated to the timing of a lone parent's stroke. The upper-right graph shows time distance to parental stroke in months and days of sickness benefit. It shows a jump in the number of sick days for daughters in the month right after the lone parent suffers stroke.[20] The increase in number of days continues for a few months after parental stroke. This is

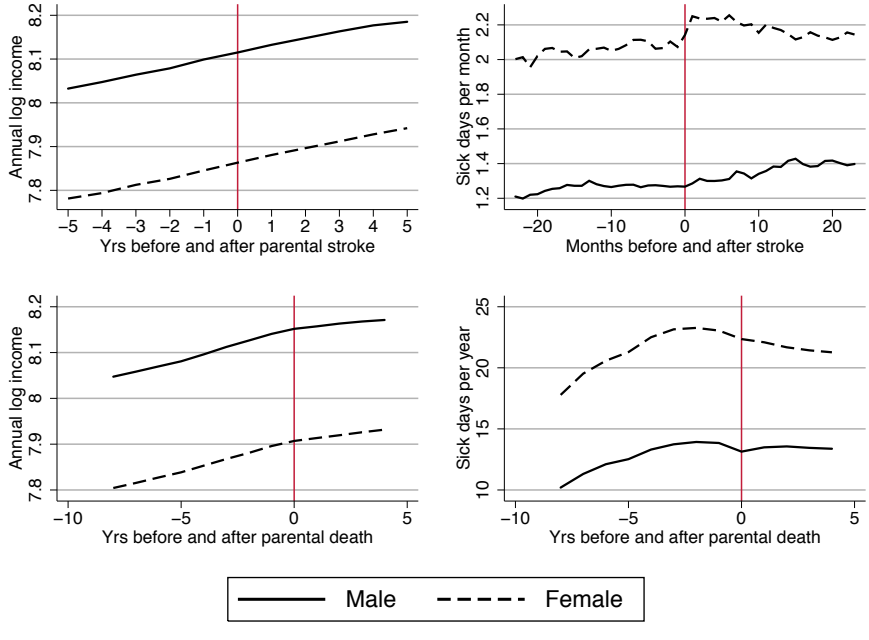
[20]The number of days on sick leave at annual level in the years before and after stroke is found in Figure A1 in Appendix.

indicative of increased care demand of the parent resulting in daughters being on sick leave from work.

In the lower-left graph, the relation between the time distance to parental demise in years and the log income of sons and daughters is illustrated. There is no apparent pattern of a decreased income due to increased care need in the final years of the parent. On the other hand, the increase in log income seems to be reduced slightly after the parent has died. The relation between time distance to parental demise and the number of days on sick leave at an annual level is shown in the lower-right graph of Figure 1. There is an increase in the number of sick days, peaking in the year before the parent dies, and is then reduced in the year that the parent dies and onward. This could suggest that children increase the number of sick days due to increased care demand of the parent, and when the parent has died and the care is no longer needed the number of days on sick leave decreases.

While Figure 1 shows the pooled cross-section relations between the timing of the parental health shock and the outcomes, they do not take the endogeneity of caregiving into account. In the next section the results when controlling for this endogeneity by using individual fixed effects estimations are presented.

Figure 1. Log income (deflated and measured in hundreds SEK) and sick leave absence in the years (and months) before and after parental stroke and parental demise



Note: Means of individual characteristics in the year that the parent suffers a stroke or dies. Standard deviations in parenthesis. The log annual income conditional on employment (deflated) is measured in hundreds SEK.

5 Results

This section presents the regression results on labor market outcomes and sick leave of having an elderly parent who suffers from a health shock. The estimated effects, separately by sons and daughters, are presented graphically with 95 percent confidence intervals represented by vertical bars.[21] First, I present the results of the individual fixed effects regressions for the parental stroke sample. Thereafter, I present the main results for the parental demise sample, followed by subgroup analyses. Finally, I explore whether there are gender differences within the family in the response to a parental health shock.

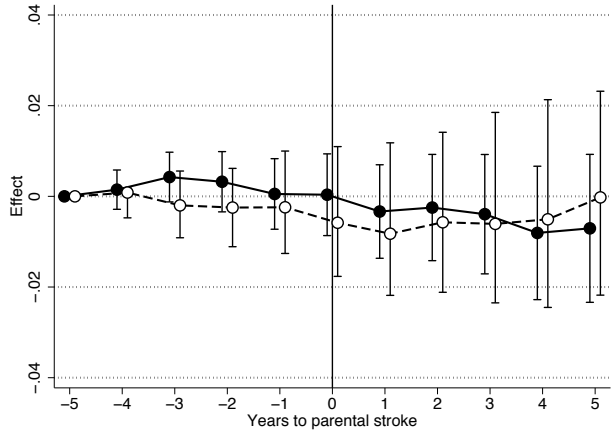
5.1 Main results - Parental stroke

Figure 2 shows the estimated effects of having a lone elderly parent suffering from stroke on employment and income. The point estimates in Figure 2a can be interpreted as percentage point change in employment relative to the reference level, which is all available years at least five years prior to parental stroke. The results suggest that there is no impact on employment of sons and daughters following a parental stroke; the estimates in the years following the stroke are small and not statistically significant. Similarly, Figure 2b indicates that there is no significant impact on the income conditional on employment following a parental stroke. By using the logarithm of the offspring's income, the impacts in Figure 2b can be interpreted as percentage change in income relative to the reference level. Reassuringly, there are no statistically significant effects from the parental stroke in the years prior to the stroke which suggests that the assumption of parallel trends is fulfilled.

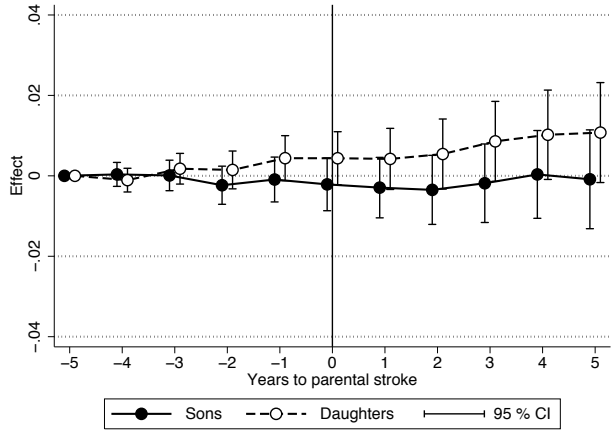
In Figure 2b, I focus on offspring's income conditional on employment since individuals who work full time are more likely to have their income affected because they need to combine labor market work and a parent with care needs. As discussed in Section 4, this implies that there may be a compositional change in the studied population since only those who are employed remain in the sample. Estimations using income conditional on earning the lower level of 20 000 SEK annually reassuringly show very similar results, apart from a small marginally statistically significant decrease in income for daughters in the year after parental stroke (these results are presented in Table A14 in the Appendix). Overall however, the results from this analysis confirm the conclusion that there are no impacts from parental stroke on income of adult offspring.

[21] Tables with the estimates for the corresponding graphs are found in the Appendix.

Figure 2. Individual fixed effects estimates of the effects of parental stroke on employment and log income conditional on employment, lone parents



(a) Employment



(b) Log income

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

The estimated impacts on the sick leave of the offspring are presented in Figure 3, where the first two graphs study the immediate response of a parental stroke of sons and daughters using monthly data[22], and the bottom graph studies the more long-term effect by studying sick leave at an annual level. In the monthly analysis, I have included children where both parents are alive at the time of the stroke because there may be short term adjustment effects in the months

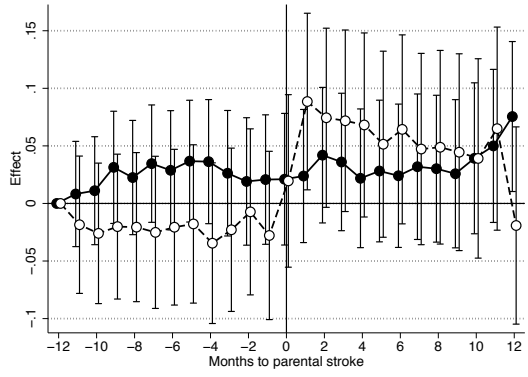
[22]Since I use data on sick leave absence I will only capture long term sick leave (at least 14 days) for employed individuals.

just after a stroke also among children of parents where the primary caregiver is likely the spouse of the stroke patient. The impact on the number of days on sickness benefit month – by – month one year before and after the stroke for this sample is presented in Figure 3a. The results indicate that the number of sick days per month increases significantly by around 0.1 day (or 8.8 percent) for daughters in the month following the stroke and that the number of days on sick leave is increased for up to four months after the parent suffers stroke (where point estimates in months 2-4 are significant at the 10 percent level). For sons, on the other hand, there seems to be no effect. When focusing on children of lone parents only, there is no statistically significant effect for sons or daughters, as seen in Figure 3b, but the point estimates for daughters follow the same pattern as in Figure 3a.

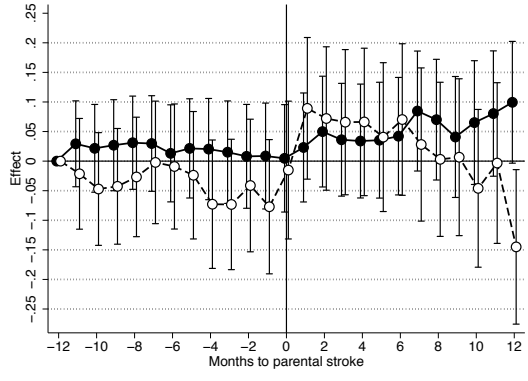
Figure 3c shows the more aggregated effects of having a parent suffer stroke for children of lone parents. The dependent variable is the total number of days on sick leave at an annual level. For sons, the number of days on sick leave increases significantly in the years after the parental stroke by around 2 days. This corresponds to an increase of 34 percent compared to the average level at least five years prior to stroke. Moreover, there is a positive impact for each of the studied years following the stroke, suggesting that there is an upward shift in sick leave in the post-stroke period. These results should however be interpreted with some caution as there seem to be significant positive pre-treatment estimates. Although the size of the estimates is larger in the post-stroke period, the significant effects on sick leave before the stroke has occurred suggest that sons' sick leave may also be affected by something other than a parental stroke, or that there are variations in sickness absences over time not captured by the cohort-specific time fixed effects. There are no significant effects on the sick leave at annual level for daughters. Again, there are significant pre-treatment effects suggesting that the assumption that any changes in the development of the offspring's sick leave may be due to increased care demand of the parent is violated.

Taken together, there seem to be no effect on children's labor market outcomes following a parental stroke apart from a temporary increase in sick leave in the months right after the stroke. Whether this increase in sick leave is due to the child's own illness or whether children use the sick leave in order to be able to take care of the parent is not clear from this analysis. It could be that a parental stroke comes as shock for the child causing mental suffering and the need to cope with the stress. Having a parent suffering a serious health event could also imply that the child would want to spend more time with the parent. The more long term analysis suggests that the temporary impact on sick leave for daughters is not substantial enough to be detectable at an annual level.

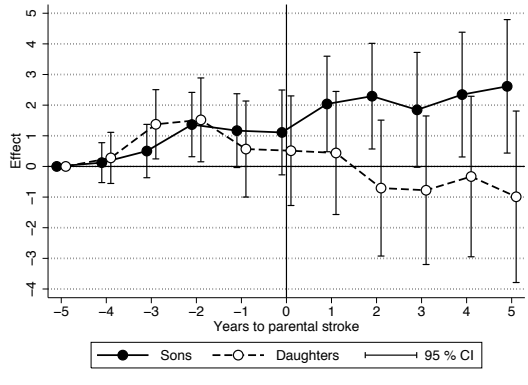
Figure 3. Individual fixed effects estimates of the effects of parental stroke on sick days at monthly and annual level



(a) Sick days per month, all parents



(b) Sick days per month, lone parents



(c) Sick days per year, lone parents

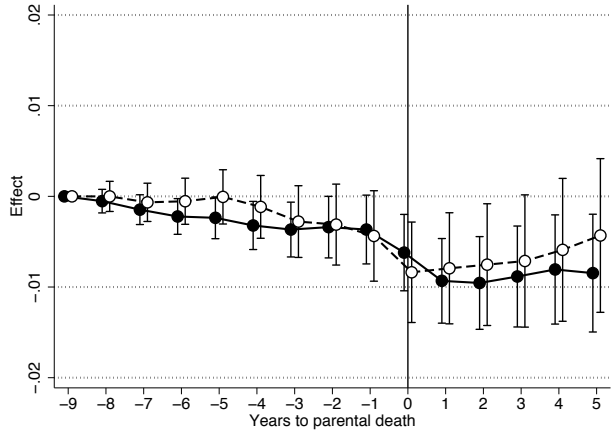
Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

5.2 Main results - Parent in final years of life

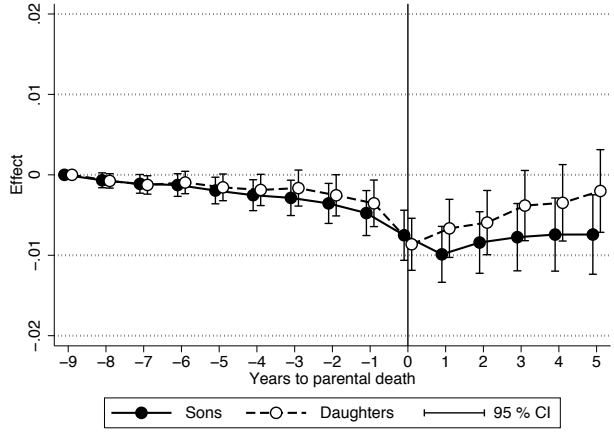
Figure 4 shows the estimates of the effect of having a lone parent in the final years of life and who dies at $t = 0$ on the employment and income of sons and daughters. Figure 4a shows that there is no statistically significant effect on employment prior to parental death for daughters relative to the reference level at least nine years prior to parental death. There is however a reduction in employment in the year that the parent dies and it continues to be significanly reduced in the following three years. For sons, there is a marginally statistically significant reduction in employment already seven years prior to the death of the parent, but the size of the point estimates is small and similar to that of daughters. Again, the reduction is larger in the year that the parent dies and onwards; the point estimate of -0.009 in $t = 1$ suggest that employment is reduced by almost 1 percentage point (or 1.2 percent relative to the average level at least nine years prior to parental death) the year after parental demise.

Figure 4b shows the estimated impacts on income conditional on employment for sons and daughters. The income is significantly reduced for sons in the final five years of the parent's life relative to the average income level at least nine years before the parent's death. In the year that the parent dies, the income is even more reduced, and it continues to be reduced throughout the studied period. The estimate of -0.008 at $t = 0$ suggests that the income is reduced by 0.8 percent compared to the level where it is assumed that the parent has limited or non-existent care needs. The negative impacts on daughter's income appear to be slightly smaller and are statistically significant only between t-2 and t+2. After that, the income of daughters picks up again.

Figure 4. Individual fixed effects estimates of the effects of parental demise on employment and log income conditional on employment, lone parents



(a) Employment



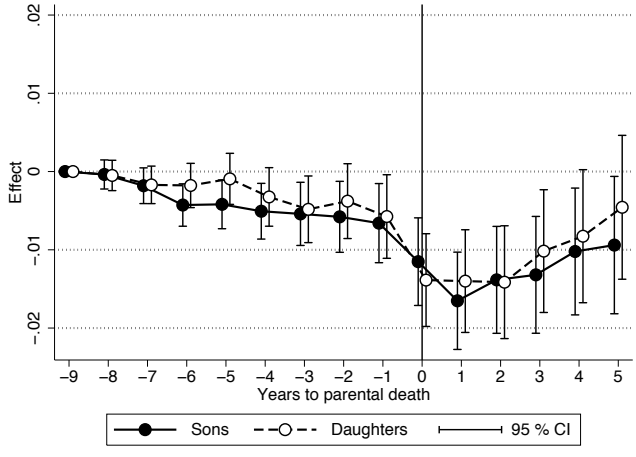
(b) Log Income

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

As discussed earlier, focusing on offspring's income conditional on employment implies that there may be a compositional change in the studied population. in order to see whether this has important implications for the impact on income, I also study the effect on log income conditional on earning at least 20 000 SEK annually. These results are presented in Figure 5 and they show that the size of the point estimates are somewhat larger than those presented in Figure 4b, suggesting that conditioning income on full employment most likely means that I underestimate the impact on income (results from this

analysis is also found in Table A14 in the Appendix). Still, the impacts follow a similar pattern as in Figure 4b, again with sons and daughters being similarly affected, and do not give reasons to revise the conclusion that the impact on income prior to parental demise is generally small.

Figure 5. Individual fixed effects estimates of the effects of parental demise on log income conditional on earning more than 20 000 SEK annually, lone parents



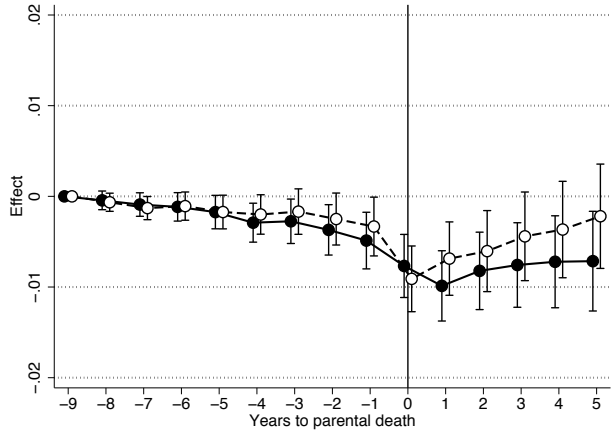
Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

As discussed earlier, it is difficult to disentangle the effects of informal caregiving from the effects of expected inheritance. A reduction in income prior to parental death could stem from the offspring adapting their labor supply to an expected increase in income from inheritance. One aspect in Figure 4b that speaks against this is that the reduction in income is larger when the parent has died which it would not be if children gradually reduce their income due to anticipated inheritance.[23] Another way to get at whether the reduction in income stems from parental care demand or anticipated inheritance is by comparing the behavior of the offspring after parental demise for groups where anticipation of inheritance is likely to differ. To this end, I separate the analysis according to whether the parent died suddenly (so that the anticipation of parental death is less obvious) or not. The drop in income after parental demise would arguably be the largest for those children where the death of the parent is less expected so that they had not been able to adjust their labor supply in anticipation of expected inheritance. Using the Causes of Death Register I can distinguish those parents that die suddenly according to the definition of sudden death in Andersen and Nielsen (2011), who characterize a sudden death as unexpected and the result of abrupt change in the person's clinical state. The

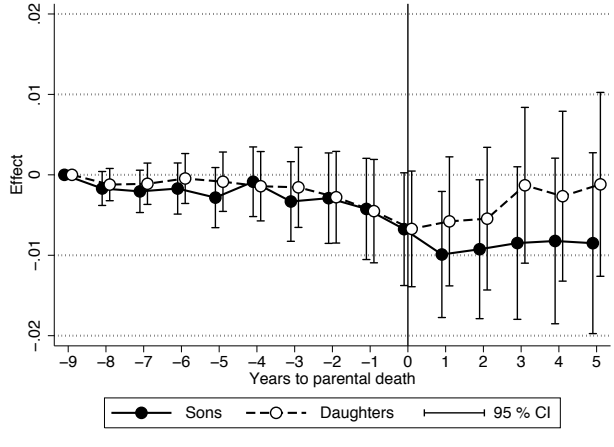
[23]This is true if individuals are able to borrow against future inheritance.

results on income separated according to whether the parent died suddenly or not is found in Figure 6. When comparing the estimates of the drop in income in the two samples (Figures 6a and b), the size of the drop is similar (point estimate is -0.01 at time t=1 for sons in both samples). That is, the response in income (conditional on employment) following the realization of a potential increase in income due to inheritance is equal for both types of deaths which it should not be had the offspring already adapted labor supply, and started to consume the inheritance already before parental demise. These findings are consistent with a reduction in labor supply due to increased informal care provision rather than intentional labor supply smoothing of the offspring.

Figure 6. Individual fixed effects estimates of the effects of parental demise on log income conditional on employment, lone parents: type of parental death



(a) Log income, Not sudden death



(b) Log income, Sudden death

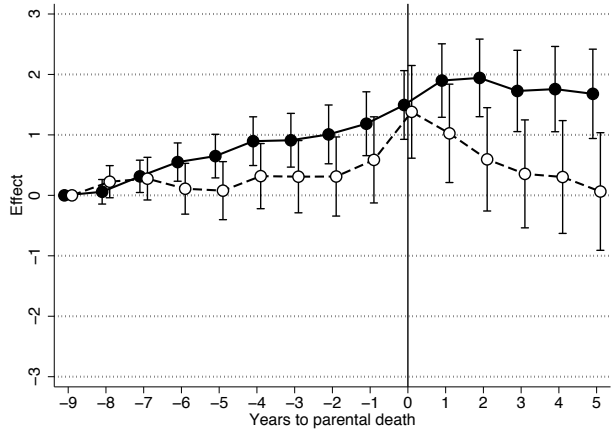
Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

Taken together, the results on labor market outcomes from having a parent in his or her final years of life suggest that although there is a small reduction in employment and income in the years around parental demise, the opportunity costs of parental care need for adult children in the form of adverse labor market outcomes are small. Employment and income are both reduced by less than 1 percent in the year prior to parental death relative to the average level at least nine years prior to parental demise. The reduction in employment and income after parental demise could be the result of grief or a reduction

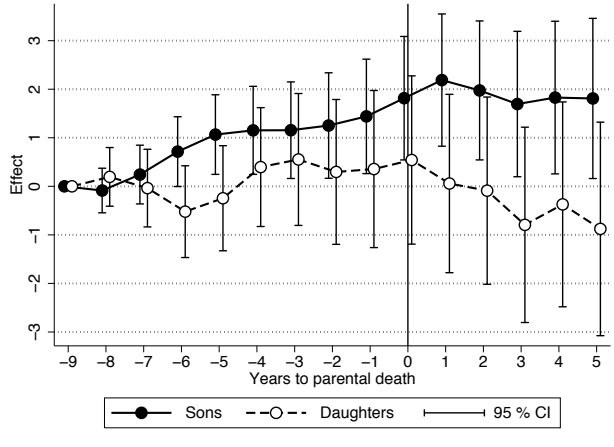
in labor supply due to realized inheritance. Results do however suggest that the small reduction in income prior to parental demise is likely the result of informal care provision rather than labor supply smoothing in expectation of inheritance. Interestingly, the estimated effects of parental demise for sons and daughters follow the same pattern, suggesting that there are no clear gender differences in the impact of parental demise on labor market outcomes.

Turning to the effects of having a lone parent in his or her final years of life on sickness absence, Figure 7 shows the estimated impact on the total number of days on sick benefit per year in the years leading up to and after parental demise. Figure 7a presents the results for the full sample of children of lone parents and shows that for sons there is a positive effect on the number of days on sick leave for almost all years prior to and following parental demise. This suggests that there seems to be an underlying trend in the number of sick days that my model does not capture and that the results for sons should be interpreted with some caution. For daughters, there is a significant increase of around 1.4 days (corresponding to almost 14 percent) in the year that the parent dies relative to the level at least nine years prior to parental demise, and it is increased also in the year after the parent has died. Although this could be a grieving effect since it coincides with parental demise, no similar increase is found when separately studying daughters whose parent dies suddenly, and who also likely mourn their parent (as seen in Figure 7b). If it can be assumed that the care need in the final year of life is larger for parents who do not die suddenly compared to those that do, this finding would indicate that the impact on sick leave absence is not solely driven by grief, but may be the result of increased care need in the final year of the parent's life. It should however be noted that parental care need would not be able to explain the increased level of sick leave that remains also the year after parental death.

Figure 7. Individual fixed effects estimates of the effects of parental demise on sickdays, lone parents



(a) Sickdays, All



(b) Sickdays, Sudden death

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

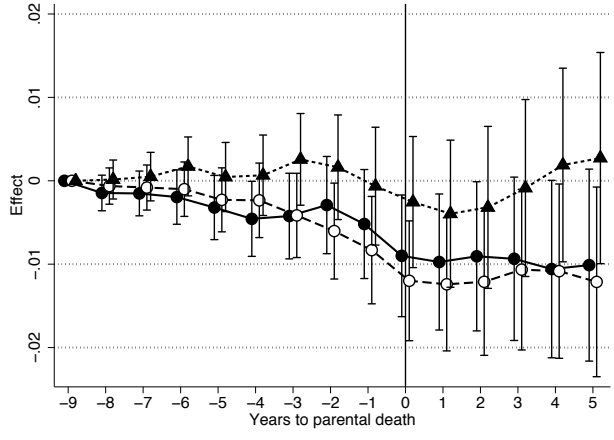
5.3 Subgroup analyses

The characteristics of the adult child may be associated with his or her labor market attachment as well as the sensitivity in the response to a parental health shock. To see whether there are heterogeneities in the impact on the child's income I analyze offsprings with different characteristics.

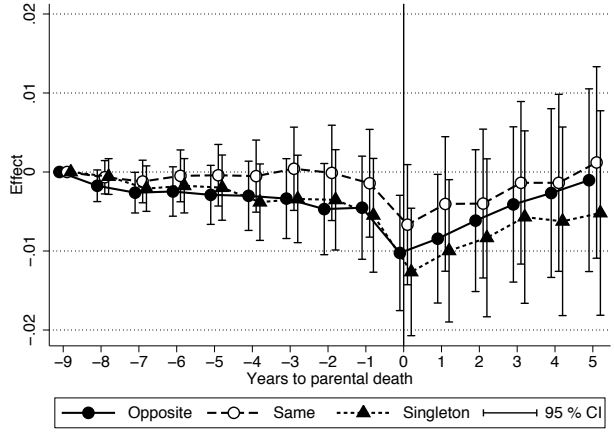
There may be differences in how the adult child is affected depending on whether he or she has siblings to share the informal care burden with. More-

over, it may also matter whether that sibling is male or female since earlier literature has suggested that daughters are more likely to care for their elderly parent (Szebehely 2005). Using a sample including only families with two adult siblings and with a single adult child, I estimate the impact of having a parent in his or her final years of life separately according to whether the adult child has a brother, a sister, or whether he or she has no siblings. Figure 8 shows the impact on income separately for sons and daughters of lone parents. The results indicate that the negative impact on income prior to lone parent's death is statistically signficant only for sons with a brother (Figure 8a) and that the point estimates are somewhat smaller for daughters with a sister, compared to other daughters (Figure 8b). One interpretation of these results is that the negative impact is found where there is no female sibling to share the burden with. Surprisingly, there is no impact on income for singleton men. Given that they have no sibling to share the care burden with, it would have been expected to see a larger impact for these men, similar to that found for singleton daughters. The corresponding analysis for the children of parents who suffer stroke (found in Figure A2 in Appendix) reveals no differences among different types of sibling constellations; there is no statistically significant effect of a parental stroke on income in either subsample.

Figure 8. Individual fixed effects estimates of the effects of parental demise on log income conditional on employment, lone parents: different sibling constellations



(a) Income of sons



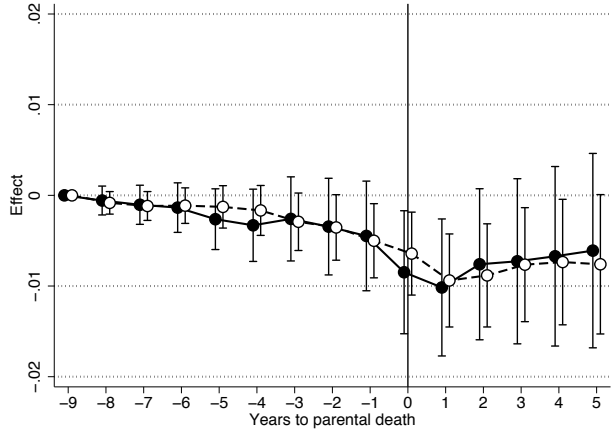
(b) Income of daughters

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

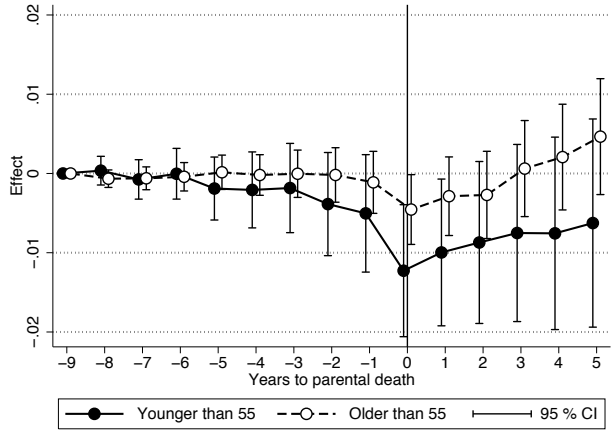
Another potentially important aspect that may impact how the offspring responds is at what point in the child's life course the parent suffers a health shock. If the health shock occurs at a point in time when the adult child is about to make career advancements, or if it happens when the adult child is deciding whether or not to remain on the labor market because of high own age, could have implications for the size of the impact on income. One way to study this is to split the sample according to the age of the offspring at the time of the parental health shock. I therefore separate the analysis according

to whether the adult child was older or younger than 55 at the time of the health shock. As seen in Figure 9a the point estimates of older and younger sons follow the same pattern. For daughters on the other hand, younger adult children seem to be driving the impact on income. The results could indicate that women who are in the middle of their career are more sensitive to circumstances in their private life. It should be noted that since the group of adult children who are below the age of 55 is smaller, the impact is more imprecisely measured for both sons and daughters and becomes noisy when moving further away from the reference level. None of the point estimates prior to parental demise in Figure 9b is statistially significant. For children of parents suffering stroke, there seem to be no difference depending on the child's own age at the time of the stroke. Results from this analysis are presented in Figure A3 in Appendix.

Figure 9. Individual fixed effects estimates of the effects of parental demise on log income conditional on employment, lone parents: different ages of the child



(a) Income of sons



(b) Income of daughters

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

The impact from a parental health shock could also differ depending on whether it is the mother or the father that is affected. For example, women who suffer stroke are often older and fare worse following the stroke (Glader et al. 2003). Moreover, women usually die at higher age and could therefore have different care demands compared to aging men. The results from the stroke sample when comparing maternal and paternal stroke (found in Figure A4 in Appendix) reveals no difference in the impact. For daughters, the impact of having a father in its final years of life is larger compared to having

a mother in its final years. For sons on the other hand, there is no difference in impact between having a lone mother or father (results are presented in Figure A7 in Appendix).

How the adult child's income is affected by the parent's health could also differ depending on whether he or she lives in the same municipality as the parent. On one hand, children living close to their parent may be more likely to provide informal care. On the other hand, living further away from the parent may imply that it takes more effort to provide the informal care. However, I find no difference in the impact from a parental health shock on income in either sample depending on whether the offspring and parent live in the same municipality or not (results are found in Figures A5 and A8 in Appendix). When comparing the difference in impact from a parental health shock depending on child's educational level, there is no difference in impact on income for sons. For daughters however, the point estimates are larger for women without post high school education in the years prior to parental demise, but they are not statistically significant (results found in Figures A6 and A9 in Appendix).

5.4 Gender differences within the family

In this section, I turn to the analysis of gender differences in the response to a parental health shock within the family. Whereas the previous analysis compares daughters to sons in general, the analysis in this section will compare a daughter to her brother. Differences in impact of the outcomes between sons and daughters within the family may reveal differences in the expectations of sons and daughters in providing family care. By looking at within-family differences in the outcomes I can control for observed and unobserved characteristics of the parent as well as inherited health and human capital that may influence informal care provision and labor market outcomes. Siblings share the same upbringing and, on average, 50 percent of their genes, and they are also affected by the exact same severity and type of parental health shock. Moreover, studying the within-sibling change in income implies that I control for offspring's investment in parental health before the health shock and thereby relax the assumption that children's investment in parental health is exogenous to timing of parent's health shock. I focus the analysis on a sample where each family consists of two children of opposite sex, and estimate the following model (which is inspired by Angelov et al. 2016):

$$\tilde{y}_{ijt} = \alpha + \lambda_t + \sum_{j}^{J} \alpha_j \mathbf{1}[t = k] + \tilde{x}'_{it}\beta + u_{ijt} \tag{2}$$

where $\tilde{y}_{ijt} = y_{bjt} - y_{sjt}$ is the within-sibling difference between (b)rother's and (s)ister's outcome for siblings i, j years away from the parental health shock

measured in calendar year t. $\tilde{x}$ is a vector of sibling differences in covariates measured prior to the health shock, $\mathbf{1}[\cdot] = 1$ if the expression in brackets is true, and zero otherwise, and u_{ijt} is an error term. The parameters of interest α_j for $j = -4, -3, ..., J$ in the stroke analysis, and $j = -8, -7, ..., J$ in the parental demise analysis, identify the impact of parental ill health on the sibling outcome difference up to J years after the health shock relative to the pre-health shock gender difference in income. I estimate equation 2 using birth cohort-specific calendar year fixed effects λ_t[24] and with controls for the within-sibling age difference and within-sibling difference in educations.[25]

The main identifying assumption is that the timing of the parental health shock is exogenous to changes in outcomes of the offspring. That is, conditional on any secular trends in the outcome (and the difference in pre-health shock covariates) the timing of the health shock cannot be related to expected future changes in the outcomes that would have happened in absence of the parental health shock.

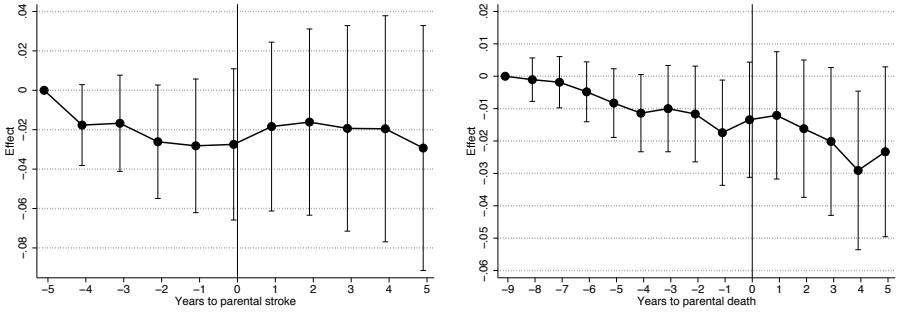
Figure 10 shows the results on the income gap of siblings whose lone parent suffer a health shock, with 95 percent confidence intervals represented by vertical bars. There is no statistically significant impact on the within-sibling income gap after the parent suffers stroke, as seen in Figure 10a. Figure 10b shows the results on the income gap of siblings whose lone parents are in their final years of life. The results indicate a small negative impact on the income gap between brothers and sisters of close to 2 percentage points (2 log points) in the year prior to parental death. This effect is small and, if anything, suggest that the negative impact on the son's income is larger compared to his sister's. Results when studying wage gaps using the lower income threshold of 20 000 SEK are found in Figure A11 in the Appendix and they show similar results as in Figure 10.

I also examine whether there are gender differences within siblings in the response of a parental health shock on the number of days on sick leave; these results are presented in Figure 11. In Figure 11a I show the results of estimating equation 2 using monthly data on the sibling gap in number of sick days for the children of parents suffering stroke. There is no significant effect on the within-sibling difference in monthly sickness absence found for either the full sample of adult children or when focusing on adult children of lone parent's only. In Figure 11b, I present the results on the sick gap between siblings whose lone parent is in his or her final years of life and, similarly, they suggest that there is no statistically significant effect on the sick gap between siblings.

[24] I use the birth year of the older sibling as the birth cohort.

[25] I also estimate equation 2 using calendar year fixed effects only and with birth cohort-specific year fixed effects but without controls siblings differences in covariates. These results, as well as those presented in Figures 10 and 11 are found in Tables A2, A3, A4, and A5 in Appendix.

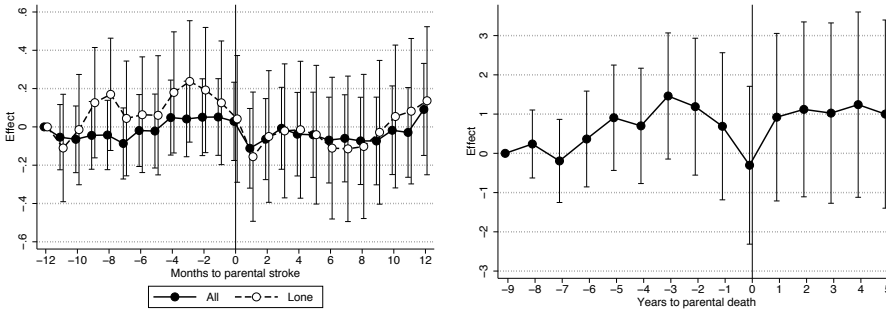
Figure 10. Yearly effects of parental health shock in $t = 0$ on the within-sibling change in income gap



(a) Effects of parental stroke (b) Effects of parental death

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the α_j's and the 95 percent confidence intervals are represented by the vertical bars.

Figure 11. Monthly and yearly effects of parental health shock in $t = 0$ on the within-sibling change in sick gap



(a) Monthly effects of parental stroke (b) Yearly effects of parental death

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the α_j's and the 95 percent confidence intervals are represented by the vertical bars.

6 Conclusion

In this paper I have studied the effects of parental stroke and parental demise on labor market outcomes and sick leave for adult sons and daughters. A large literature documents a negative relation between informal care provision and labor supply, but if not taking the endogeneity of caregiving into account the consequences of having a parent in need of care is most likely overestimated. I handle the selection into caregiving by estimating individual fixed effect models and utilizing the timing of a parental health shock to identify the effects of parental ill health on the outcomes of adult children.

I find that the income of both sons and daughters significantly decreases in the period from five to two years prior to parental demise. For sons, there is also a negative impact on employment prior to parental demise but for daughters this effect is not statistically significant. The negative impact could stem from informal care provision, but may also be the result of caring in a broad sense involving also the desire to spend time with the parent in his or her final years of life. The negative impact on both employment and income is largest in the year that the parent dies and the years immediately after, which could be due to grief or realization of inheritance. The results do however suggest that impacts on income stem from parental care demand rather than from the expectation of inheritance; when comparing the impact after parental demise between children to parents that die suddenly and those that do not, where the former arguably have less obvious reasons to expect inheritance, I find that the reduction in income is equal in size which it would not be had the offspring adapted labor supply and started consuming expected inheritance already before parental demise. Compared to the results found in Fevang et al. (2012), I find no statistically significant effect on employment for daughters prior to parental demise. Moreover, I find that having a parent in the final years of life has larger impacts on the child's income rather than employment. Nevertheless, I find that the size of the impact from parental demise on labor market outcomes is small, suggesting that employment and income is reduced by less than 1 percent from having a parent in his or her final years of life.

There are no effects on adult children's labor market outcomes following a parental stroke. Although surveys suggest that children do provide informal care for their stroke-suffering parents, the analysis in this study shows that this care provision does not affect the child's labor market activity. One reason for not finding any results on income from a parental stroke can be that since stroke requires medical attention from the beginning, the parent becomes a part of the public caring system which may ease the care giving burden on adult children. Also, the substitutability between formal an informal care may be restricted if the care that follows after a stroke requires medical skills. I find suggestive results of a temporary increase in sick leave for daughters in the months just after a parent suffers stroke. This could suggest that providing informal care to a lone elderly parent has a negative effect on adult daughter's own health. Another interpretation is that daughters use sick leave benefit as a way of reducing time spent on working in order to manage providing informal care. This temporary increase in sick leave for daughters is however not visible at an aggregated level. At annual level, there is rather an upward shift in sick days for sons in the years following the stroke. Conclusions from this aggregated analysis is however not certain since there seems to be a positive trend in the level of sick leave days for sons not captured by my model.

As for the analysis of the effects on sick leave of having a lone parent in its final years of life, the results suggest that daughter's sick leave absence increases in the year that the parent dies. Since this increase coincides with

the death of the parent, it could be the result of grief. However, since I find no similar increase for children whose parent's dies suddenly, the increase is not solely driven by grief. If it can be assumed that the care need of parents who die suddenly is lower than that of those who do not, the result would indicate that having a parent in need of care has negative impact on sickness absence, which extends also to the year after parental demise. Sons' sick leave absence increases throughout the studied period and the increase may therefore not be attributable to parental care needs.

I also analyze whether there are differences between brothers and sisters in the impact of a parental health shock. Contrary to surveys and previous research I find that, if affected at all, brothers' income decreases to a larger extent than daughters'. There may be many reasons for this result. First, it may be that men are employed to a larger degree and therefore have to cut back on worked hours to have time to care for their parent whereas women's informal care supply mainly affect their leisure. Second, if women have higher productivity in combining caring responsibilities with work, their labor supply may not be as affected. Moreover, the effects found in other studies may be overestimated if they have not considered the potential selection of women into caring.

Taken together, the results suggest that the opportunity costs of parental care need in the form of adverse labor market outcomes are small. Sweden has a comprehensive publicly funded system of care for older people, which is likely to limit the negative consequences on the labor market for adult children. By comparing the results of two different types of health shocks – one instant and severe shock requiring medical attention (stroke), and one with a more gradual development of care demand (being in the final years of life) – I find that the impact, albeit small, is more pronounced when care demand increases gradually and when care is not necessarily provided formally by the health care services.

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Appendix

Table A1. *Descriptive statistics of the studied samples*

	Stroke sample				Death sample			
	(1)	(2)	(3)	(4)	(5)	(6)	7)	(8)
	Obs	Sickdays	Obs	log Inc	Obs	Sickdays	Obs	log Inc
t=-9					489579	12.55	313283	7.93
						(54.43)		(0.35)
t=-8					489579	14.99	314329	7.95
						(59.64)		(0.36)
t=-7					489579	17.22	314548	7.97
						(64.33)		(0.36)
t=-6					489579	18.83	314076	7.98
						(67.35)		(0.37)
t=-5	44020	10.73	28184	7.93	489579	19.77	313487	7.99
		(50.08)		(0.35)		(68.74)		(0.37)
t=-4	44020	13.22	28372	7.94	489579	20.24	311275	8.02
		(56.19)		(0.35)		(69.75)		(0.37)
t=-3	44020	16.33	28461	7.95	489579	20.19	308081	8.03
		(62.45)		(0.36)		(69.74)		(0.38)
t=-2	44020	19.08	28395	7.97	489579	19.87	303515	8.05
		(67.89)		(0.36)		(69.17)		(0.38)
t=-1	44020	20.02	28324	7.99	489579	19.38	296770	8.06
		(69.09)		(0.37)		(68.28)		(0.38)
t=0	44020	20.46	28164	8.00	489579	18.15	286958	8.07
		(69.78)		(0.37)		(65.74)		(0.38)
t=1	43954	21.20	27831	8.02	438507	17.90	250890	8.07
		(71.66)		(0.37)		(65.80)		(0.38)
t=2	43757	20.99	27564	8.03	388831	17.05	216898	8.08
		(71.63)		(0.38)		(64.43)		(0.38)
t=3	43396	20.74	27016	8.04	339291	15.85	184178	8.08
		(70.50)		(0.38)		(61.93)		(0.38)
t=4	42822	20.80	26274	8.06	291312	14.59	153847	8.08
		(71.01)		(0.38)		(59.40)		(0.38)
t=5	38221	20.80	25507	8.07	246067	13.00	125755	8.09
		(71.01)		(0.38)		(55.60)		(0.38)

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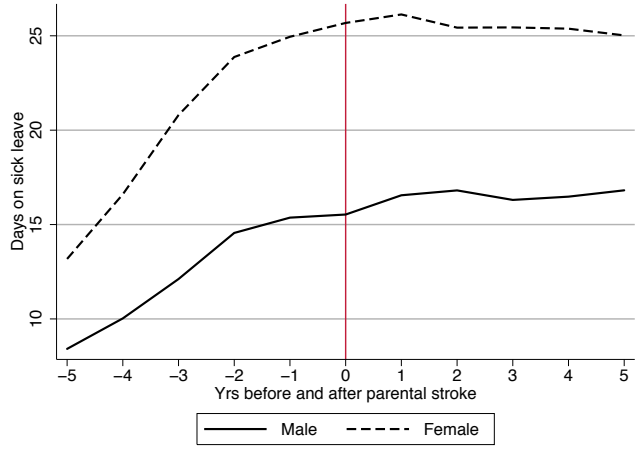
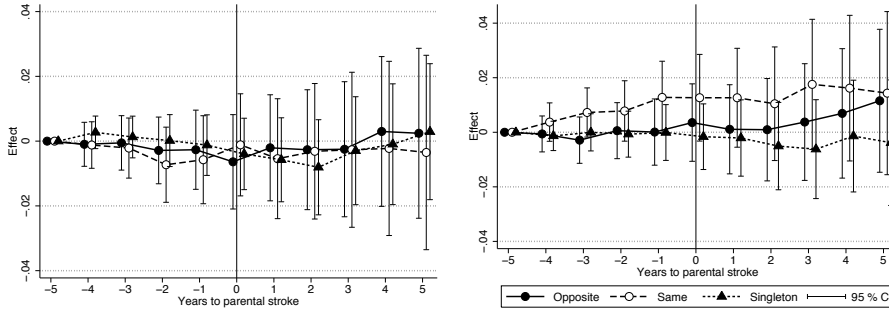


Figure A1. Sick leave absence in the years before and after parental stroke

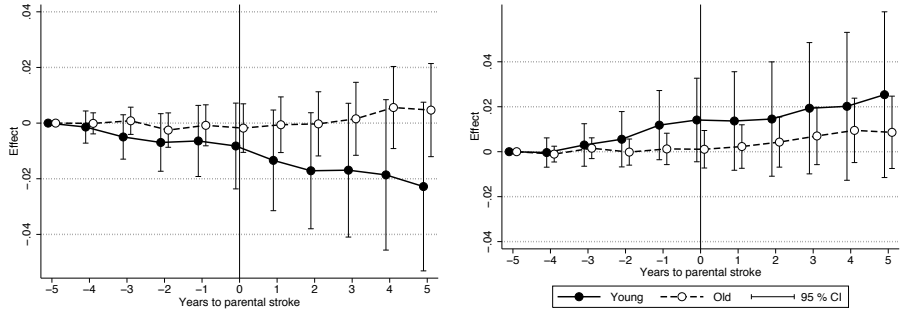
Figure A2. Individual fixed effects estimates of the effects of parental stroke on log income conditional on employment in different sibling constellations, lone parents



(a) Income of sons (b) Income of daughters

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

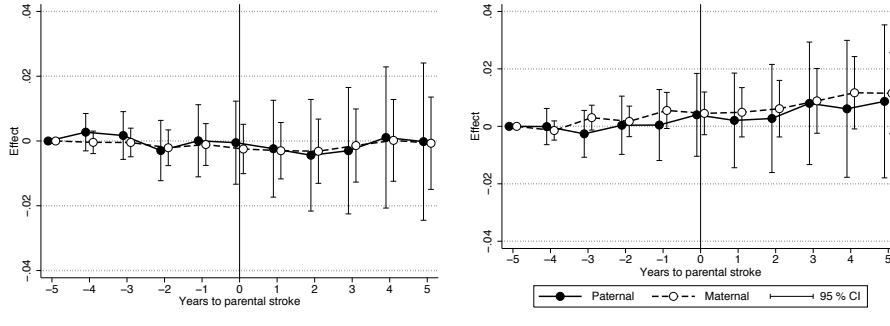
Figure A3. Individual fixed effects estimates of the effects of parental stroke on log income conditional on employment depending on whether the child is older than 50 or not, lone parents



(a) Income of sons (b) Income of daughters

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

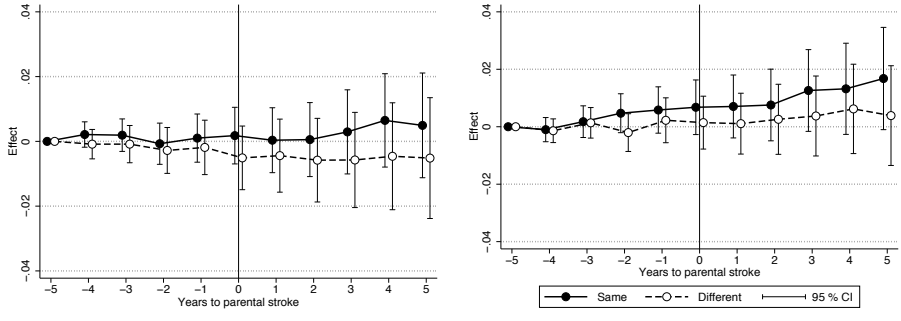
Figure A4. Individual fixed effects estimates of the effects of parental stroke on log income conditional on employment for maternal or paternal stroke, lone parents



(a) Income of sons (b) Income of daughters

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

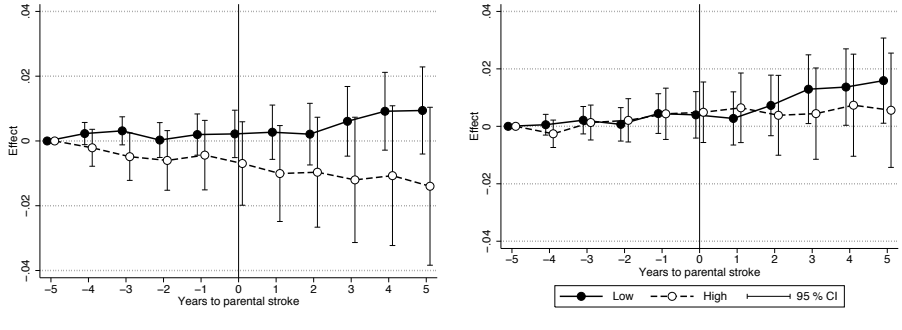
Figure A5. Individual fixed effects estimates of the effects of parental stroke on log income conditional on employment depending on whether the child and parent lives in the same municipality or not, lone parents



(a) Income of sons (b) Income of daughters

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

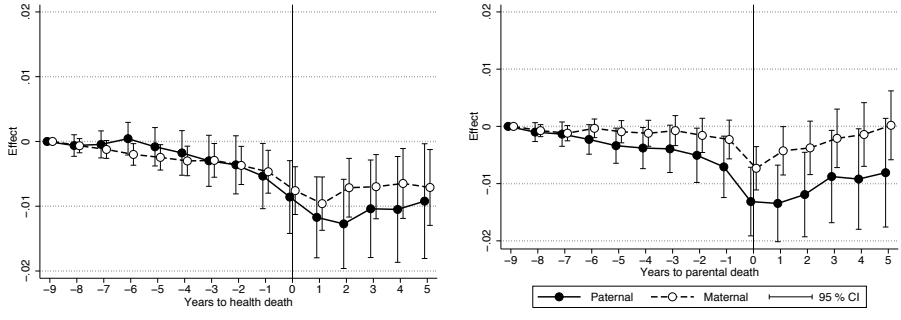
Figure A6. Individual fixed effects estimates of the effects of parental stroke on log income conditional on employment depending on whether the child has post high school education (high) or not (low), lone parents



(a) Income of sons (b) Income of daughters

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

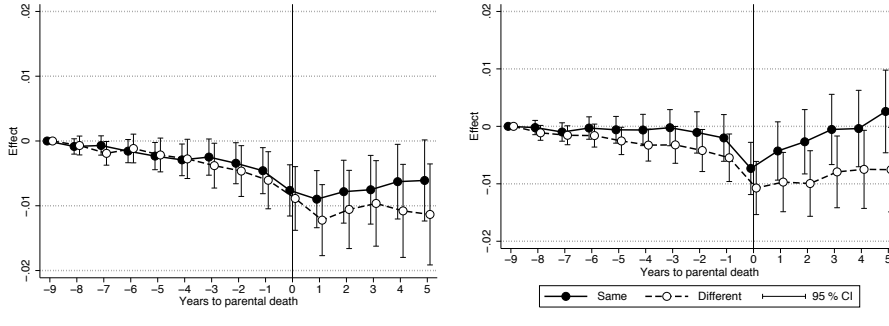
Figure A7. Individual fixed effects estimates of the effects of parental demise on log income conditional on employment for maternal or paternal death, lone parents



(a) Income of sons (b) Income of daughters

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

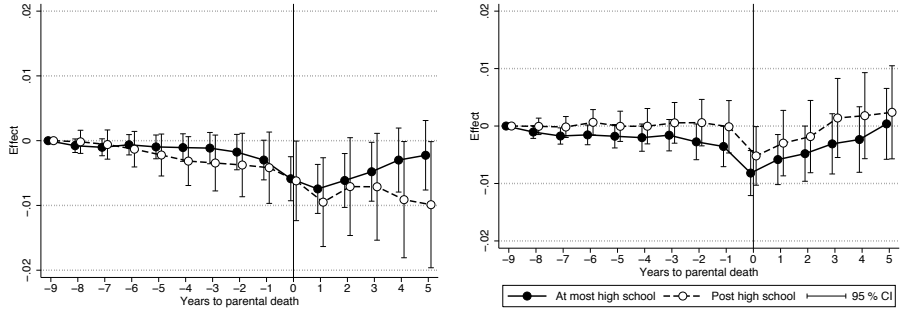
Figure A8. Individual fixed effects estimates of the effects of parental demise on log income conditional on employment depending on whether the child and parent lives in the same municipality or not, lone parents



(a) Income of sons (b) Income of daughters

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

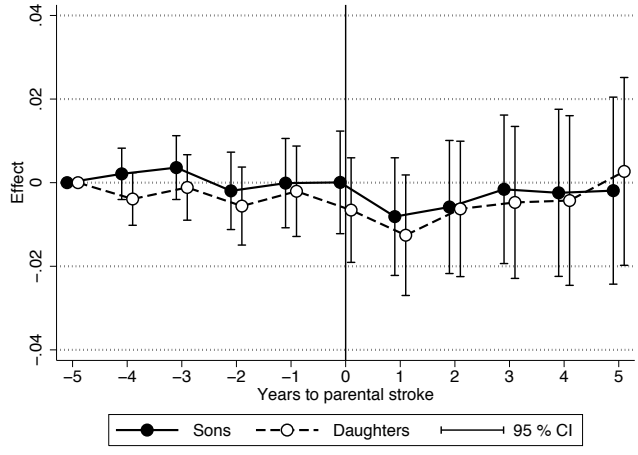
Figure A9. Individual fixed effects estimates of the effects of parental demise on log income conditional on employment depending on whether the child has post high school education (high) or not (low), lone parents



(a) Income of sons (b) Income of daughters

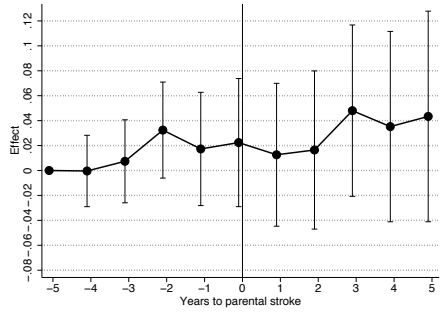
Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

Figure A10. Indiviudal fixed effects impacts on log income conditional on earning at least 20 000 SEK annually in the years before and after parental stroke

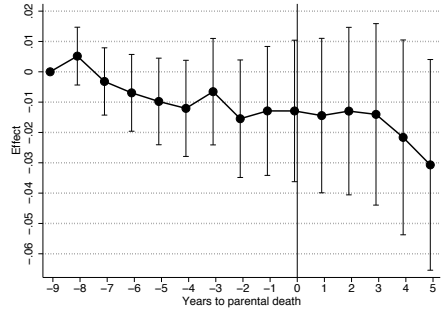


Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the δ_k's and the 95 percent confidence intervals are represented by the vertical bars.

Figure A11. Yearly effects of parental health shock in $t = 0$ on the within-sibling change in income gap using incomes above 20 000 SEK annually



(a) Effects of parental stroke



(b) Effects of parental death

Note: The figure displays the individual fixed effects estimates where each dot represents the point estimate for the α_j's and the 95 percent confidence intervals are represented by the vertical bars.

Table A2. *Yearly effects of parental stroke in $t = 0$ on the within-sibling change in income gap (log income conditional on employment)*

	(1)	(2)	(3)
	Yr FE	Yr*Coh FE	Controls
t=-4	-0.006	-0.016	-0.018*
	(0.010)	(0.011)	(0.010)
t=-3	-0.001	-0.011	-0.017
	(0.012)	(0.013)	(0.012)
t=-2	-0.013	-0.024	-0.026*
	(0.015)	(0.015)	(0.015)
t=-1	-0.014	-0.029	-0.028
	(0.017)	(0.018)	(0.017)
t=0	-0.015	-0.029	-0.027
	(0.019)	(0.020)	(0.020)
t=1	-0.003	-0.019	-0.018
	(0.022)	(0.023)	(0.022)
t=2	-0.003	-0.019	-0.016
	(0.024)	(0.025)	(0.024)
t=3	-0.006	-0.021	-0.019
	(0.026)	(0.028)	(0.027)
t=4	-0.004	-0.022	-0.020
	(0.029)	(0.031)	(0.029)
t=5	-0.013	-0.032	-0.029
	(0.031)	(0.033)	(0.032)
Constant	0.280***	0.309***	0.339***
	(0.009)	(0.040)	(0.037)
Year FE	Yes	Yes	Yes
Cohort*Year FE	No	Yes	Yes
Controls	No	No	Yes
N	29067	29067	28677
Clusters	3056	3056	2952

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A3. *Yearly effects of parental demise in $t = 0$ on the within-sibling change in income gap (log income conditional on employment)*

	(1)	(2)	(3)
	Yr FE	Yr*Coh FE	Controls
t=-8	0.002	-0.002	-0.001
	(0.004)	(0.004)	(0.003)
t=-7	0.002	-0.002	-0.002
	(0.004)	(0.004)	(0.004)
t=-6	-0.001	-0.005	-0.005
	(0.005)	(0.005)	(0.005)
t=-5	-0.004	-0.009*	-0.008
	(0.006)	(0.006)	(0.005)
t=-4	-0.006	-0.012*	-0.011*
	(0.006)	(0.006)	(0.006)
t=-3	-0.004	-0.011	-0.010
	(0.007)	(0.007)	(0.007)
t=-2	-0.005	-0.012	-0.012
	(0.008)	(0.008)	(0.008)
t=-1	-0.013	-0.019**	-0.017**
	(0.009)	(0.009)	(0.008)
t=0	-0.009	-0.015	-0.013
	(0.009)	(0.010)	(0.009)
t=1	-0.006	-0.013	-0.012
	(0.010)	(0.011)	(0.010)
t=2	-0.011	-0.018	-0.016
	(0.011)	(0.011)	(0.011)
t=3	-0.015	-0.021*	-0.020*
	(0.012)	(0.012)	(0.012)
t=4	-0.025*	-0.031**	-0.029**
	(0.013)	(0.013)	(0.012)
t=5	-0.019	-0.024*	-0.023*
	(0.014)	(0.014)	(0.013)
Constant	0.275***	0.315***	0.328***
	(0.003)	(0.014)	(0.014)
Year FE	Yes	Yes	Yes
Cohort*Year FE	No	Yes	Yes
Controls	No	No	Yes
N	304863	304863	301549
Clusters	27984	27984	27198

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A4. *Monthly effects of parental stroke in $m = 0$ on the within-sibling change in sick gap*

	All			Lone Parent		
	(1)	(2)	(3)	(4)	(5)	(6)
	Yr.FE	YrMo.FE	Controls	Yr.FE	YrMo.FE	Controls
m=-11	-0.055	-0.054	-0.054	-0.047	-0.048	-0.110
	(0.083)	(0.083)	(0.087)	(0.136)	(0.136)	(0.143)
m=-10	-0.080	-0.079	-0.065	0.027	0.024	-0.014
	(0.084)	(0.084)	(0.089)	(0.139)	(0.140)	(0.147)
m=-9	-0.057	-0.058	-0.044	0.168	0.163	0.126
	(0.085)	(0.086)	(0.090)	(0.140)	(0.140)	(0.147)
m=-8	-0.071	-0.072	-0.042	0.177	0.174	0.170
	(0.088)	(0.088)	(0.092)	(0.143)	(0.143)	(0.150)
m=-7	-0.104	-0.105	-0.087	0.060	0.058	0.044
	(0.090)	(0.090)	(0.095)	(0.146)	(0.146)	(0.153)
m=-6	-0.039	-0.040	-0.019	0.098	0.094	0.064
	(0.091)	(0.091)	(0.096)	(0.148)	(0.148)	(0.154)
m=-5	-0.032	-0.032	-0.022	0.131	0.126	0.060
	(0.093)	(0.093)	(0.098)	(0.151)	(0.151)	(0.159)
m=-4	0.053	0.052	0.049	0.259*	0.252	0.180
	(0.095)	(0.095)	(0.100)	(0.153)	(0.154)	(0.161)
m=-3	0.055	0.054	0.041	0.330**	0.326**	0.237
	(0.095)	(0.095)	(0.101)	(0.154)	(0.154)	(0.162)
m=-2	0.055	0.055	0.051	0.279*	0.277*	0.192
	(0.097)	(0.097)	(0.102)	(0.158)	(0.159)	(0.167)
m=-1	0.045	0.046	0.052	0.231	0.233	0.126
	(0.097)	(0.097)	(0.102)	(0.157)	(0.157)	(0.165)
m=0	0.020	0.021	0.029	0.128	0.131	0.042
	(0.099)	(0.099)	(0.104)	(0.161)	(0.161)	(0.169)
m=1	-0.110	-0.109	-0.112	-0.088	-0.086	-0.156
	(0.100)	(0.100)	(0.106)	(0.164)	(0.164)	(0.172)
m=2	-0.073	-0.070	-0.064	-0.005	-0.003	-0.050
	(0.102)	(0.102)	(0.108)	(0.167)	(0.167)	(0.175)
m=3	-0.012	-0.010	-0.007	0.031	0.035	-0.021
	(0.103)	(0.103)	(0.109)	(0.169)	(0.170)	(0.178)
m=4	-0.027	-0.023	-0.038	0.045	0.052	-0.015
	(0.105)	(0.105)	(0.111)	(0.172)	(0.173)	(0.182)
m=5	-0.001	0.003	-0.041	0.062	0.069	-0.041
	(0.107)	(0.107)	(0.114)	(0.174)	(0.175)	(0.185)
m=6	-0.028	-0.025	-0.069	-0.026	-0.023	-0.111
	(0.107)	(0.108)	(0.114)	(0.178)	(0.179)	(0.189)
m=7	-0.016	-0.014	-0.060	-0.020	-0.019	-0.115
	(0.109)	(0.109)	(0.116)	(0.183)	(0.183)	(0.194)
m=8	-0.032	-0.031	-0.073	-0.031	-0.031	-0.102
	(0.109)	(0.110)	(0.116)	(0.182)	(0.182)	(0.192)
m=9	-0.035	-0.033	-0.074	0.032	0.036	-0.028
	(0.110)	(0.110)	(0.117)	(0.181)	(0.181)	(0.191)
m=10	0.024	0.026	-0.018	0.120	0.122	0.054
	(0.111)	(0.111)	(0.118)	(0.180)	(0.180)	(0.190)
m=11	0.009	0.011	-0.029	0.149	0.152	0.082
	(0.112)	(0.112)	(0.119)	(0.183)	(0.183)	(0.194)
m=12	0.119	0.118	0.091	0.216	0.215	0.137
	(0.114)	(0.115)	(0.123)	(0.184)	(0.185)	(0.197)
Constant	-0.045***	-0.035**	-0.046***	-0.078***	-0.059***	-0.068***
	(0.016)	(0.015)	(0.016)	(0.024)	(0.022)	(0.025)
Year FE	Yes	No	No	Yes	No	No
YearMonth FE	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
N	2385720	2385720	2168928	989556	989556	901080
Clusters	10753	10753	9782	4418	4418	4025

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A5. *Yearly effects of parental demise in $t = 0$ on the within-sibling change in sick gap*

	(1) Yr FE	(2) Yr*Coh FE	(3) Controls
t=-8	0.024	0.134	0.224
	(0.415)	(0.416)	(0.442)
t=-7	-0.410	-0.204	-0.186
	(0.507)	(0.510)	(0.541)
t=-6	-0.055	0.221	0.364
	(0.583)	(0.588)	(0.623)
t=-5	0.306	0.656	0.892
	(0.638)	(0.646)	(0.685)
t=-4	0.213	0.585	0.456
	(0.701)	(0.712)	(0.757)
t=-3	1.096	1.453*	1.254
	(0.762)	(0.776)	(0.826)
t=-2	0.757	1.059	0.895
	(0.826)	(0.842)	(0.897)
t=-1	0.264	0.508	0.336
	(0.887)	(0.904)	(0.964)
t=0	0.908	1.012	0.692
	(0.923)	(0.943)	(1.004)
t=1	1.362	1.342	0.890
	(1.002)	(1.022)	(1.090)
t=2	1.748*	1.577	1.087
	(1.046)	(1.066)	(1.138)
t=3	1.714	1.419	1.001
	(1.079)	(1.100)	(1.173)
t=4	1.920*	1.499	1.226
	(1.108)	(1.130)	(1.205)
t=5	1.727	1.234	0.966
	(1.127)	(1.150)	(1.223)
Constant	-0.396***	-0.748	-0.248
	(0.112)	(0.524)	(0.489)
Year FE	Yes	Yes	Yes
Cohort*Year FE	No	Yes	Yes
Controls	No	No	Yes
N	745376	745376	671644
Clusters	39651	39651	35689

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A6. *Individual fixed effects estimates of the effects of parental stroke on log income conditional on employment and on employment, lone parents*

	Log Income		Employment	
	(1)	(2)	(3)	(4)
	Sons	Daughters	Sons	Daughters
t=-4	0.000	-0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.003)
t=-3	0.000	0.002	0.004	-0.002
	(0.002)	(0.002)	(0.003)	(0.004)
t=-2	-0.002	0.001	0.003	-0.002
	(0.002)	(0.002)	(0.003)	(0.004)
t=-1	-0.001	0.004	0.001	-0.002
	(0.003)	(0.003)	(0.004)	(0.005)
t=0	-0.002	0.004	0.000	-0.006
	(0.003)	(0.003)	(0.005)	(0.006)
t=1	-0.003	0.004	-0.003	-0.008
	(0.004)	(0.004)	(0.005)	(0.007)
t=2	-0.003	0.005	-0.002	-0.006
	(0.004)	(0.004)	(0.006)	(0.008)
t=3	-0.002	0.009*	-0.004	-0.006
	(0.005)	(0.005)	(0.007)	(0.009)
t=4	0.000	0.010*	-0.008	-0.005
	(0.006)	(0.006)	(0.008)	(0.010)
t=5	-0.001	0.011*	-0.007	-0.000
	(0.006)	(0.006)	(0.008)	(0.011)
Constant	8.038***	7.760***	0.883***	0.618***
	(0.008)	(0.003)	(0.008)	(0.008)
Cohort*Year FE	Yes	Yes	Yes	Yes
N	247844	190977	342080	323670
Clusters	19636	17349	22190	20865

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A7. *Individual fixed effects estimates of the effects of parental stroke on number of sick days per month*

	All		Widow	
	(1)	(2)	(3)	(4)
	Men	Women	Men	Women
m=-11	0.005	-0.025	0.025	-0.028
	(0.023)	(0.030)	(0.037)	(0.048)
m=-10	0.012	-0.024	0.022	-0.049
	(0.024)	(0.031)	(0.038)	(0.049)
m=-9	0.030	-0.023	0.025	-0.048
	(0.025)	(0.032)	(0.039)	(0.050)
m=-8	0.026	-0.018	0.037	-0.016
	(0.025)	(0.033)	(0.040)	(0.052)
m=-7	0.034	-0.022	0.029	0.001
	(0.026)	(0.034)	(0.041)	(0.053)
m=-6	0.031	-0.023	0.016	-0.007
	(0.027)	(0.035)	(0.042)	(0.054)
m=-5	0.035	-0.016	0.020	-0.022
	(0.027)	(0.035)	(0.043)	(0.055)
m=-4	0.035	-0.036	0.024	-0.076
	(0.028)	(0.036)	(0.044)	(0.055)
m=-3	0.023	-0.026	0.015	-0.079
	(0.028)	(0.036)	(0.044)	(0.056)
m=-2	0.022	-0.010	0.015	-0.041
	(0.028)	(0.037)	(0.045)	(0.057)
m=-1	0.021	-0.032	0.006	-0.078
	(0.029)	(0.037)	(0.046)	(0.058)
m=0	0.023	0.020	0.012	-0.010
	(0.029)	(0.038)	(0.047)	(0.060)
m=1	0.025	0.090**	0.024	0.088
	(0.030)	(0.039)	(0.047)	(0.061)
m=2	0.043	0.075*	0.051	0.069
	(0.030)	(0.040)	(0.048)	(0.062)
m=3	0.033	0.070*	0.037	0.071
	(0.031)	(0.040)	(0.049)	(0.063)
m=4	0.026	0.069*	0.041	0.069
	(0.031)	(0.041)	(0.049)	(0.064)
m=5	0.029	0.048	0.039	0.038
	(0.031)	(0.041)	(0.050)	(0.064)
m=6	0.024	0.066	0.044	0.071
	(0.032)	(0.042)	(0.051)	(0.065)
m=7	0.031	0.048	0.085	0.028
	(0.032)	(0.042)	(0.052)	(0.066)
m=8	0.032	0.048	0.070	0.001
	(0.033)	(0.043)	(0.052)	(0.067)
m=9	0.031	0.045	0.047	0.004
	(0.033)	(0.044)	(0.052)	(0.068)
m=10	0.043	0.033	0.069	-0.048
	(0.034)	(0.044)	(0.054)	(0.068)
m=11	0.049	0.064	0.083	-0.004
	(0.034)	(0.045)	(0.054)	(0.069)
m=12	0.075**	-0.021	0.100*	-0.147**
	(0.033)	(0.044)	(0.053)	(0.067)
Constant	0.009	0.048**	0.018	0.059**
	(0.016)	(0.021)	(0.022)	(0.027)
N	11638728	11058420	5217252	4924320
Clusters	50813	48303	22656	21364

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A8. *Individual fixed effects estimates of the effects of parental stroke on sick days at annual level*

	Annual Tot.	
	(1)	(2)
	Men	Women
Year -4	0.184	0.629
	(0.348)	(0.448)
Year -3	0.885*	1.211**
	(0.470)	(0.612)
Year -2	1.212**	0.800
	(0.559)	(0.737)
Year -1	0.937	-0.003
	(0.651)	(0.850)
Year 0	1.309*	0.898
	(0.746)	(0.967)
Year 1	2.245***	-0.478
	(0.834)	(1.075)
Year 2	1.921**	-1.159
	(0.919)	(1.185)
Year 3	2.134**	-1.132
	(1.003)	(1.290)
Year 4	2.320**	-0.748
	(1.089)	(1.398)
Constant	0.438	-2.116*
	(0.771)	(1.256)
N	332955	313308
Clusters	22656	21364

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A9. *Individual fixed effects estimates of the effects of having a lone parent in it's final years of life on log income conditional on employment and on employment*

	Income		Employment	
	(1)	(2)	(3)	(4)
	Sons	Daughters	Sons	Daughters
t=-8	-0.001	-0.001*	-0.001	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)
t=-7	-0.001*	-0.001**	-0.001*	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
t=-6	-0.001*	-0.001	-0.002**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
t=-5	-0.002**	-0.002*	-0.002**	-0.000
	(0.001)	(0.001)	(0.001)	(0.002)
t=-4	-0.003***	-0.002*	-0.003**	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)
t=-3	-0.003**	-0.002	-0.004**	-0.003
	(0.001)	(0.001)	(0.002)	(0.002)
t=-2	-0.004***	-0.003*	-0.003*	-0.003
	(0.001)	(0.001)	(0.002)	(0.002)
t=-1	-0.005***	-0.004**	-0.004*	-0.004*
	(0.001)	(0.001)	(0.002)	(0.003)
t=0	-0.008***	-0.009***	-0.006***	-0.008***
	(0.002)	(0.002)	(0.002)	(0.003)
t=1	-0.010***	-0.007***	-0.009***	-0.008**
	(0.002)	(0.002)	(0.002)	(0.003)
t=2	-0.008***	-0.006***	-0.010***	-0.008**
	(0.002)	(0.002)	(0.003)	(0.003)
t=3	-0.008***	-0.004*	-0.009***	-0.007*
	(0.002)	(0.002)	(0.003)	(0.004)
t=4	-0.007***	-0.003	-0.008***	-0.006
	(0.002)	(0.002)	(0.003)	(0.004)
t=5	-0.007***	-0.002	-0.008**	-0.004
	(0.003)	(0.003)	(0.003)	(0.004)
Constant	8.023***	7.724***	0.868***	0.591***
	(0.002)	(0.002)	(0.002)	(0.003)
Cohort*Year FE	Yes	Yes	Yes	Yes
N	3195130	2535584	4447489	4265636
Clusters	218139	196136	244841	232512

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A10. *Individual fixed effects estimates of the effects of having a lone parent in it's final years of life on log income condtional on employment, separated according to type of parental death*

	Not sudden death		Sudden death	
	(1)	(2)	(3)	(4)
	Sons	Daughters	Sons	Daughters
t=-8	-0.000	-0.001	-0.002	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
t=-7	-0.001	-0.001**	-0.002	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
t=-6	-0.001	-0.001	-0.002	-0.000
	(0.001)	(0.001)	(0.002)	(0.002)
t=-5	-0.002*	-0.002*	-0.003	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)
t=-4	-0.003***	-0.002*	-0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)
t=-3	-0.003**	-0.002	-0.003	-0.002
	(0.001)	(0.001)	(0.003)	(0.003)
t=-2	-0.004***	-0.003*	-0.003	-0.003
	(0.001)	(0.001)	(0.003)	(0.003)
t=-1	-0.005***	-0.003**	-0.004	-0.005
	(0.002)	(0.002)	(0.003)	(0.003)
t=0	-0.008***	-0.009***	-0.007*	-0.007*
	(0.002)	(0.002)	(0.004)	(0.004)
t=1	-0.010***	-0.007***	-0.010**	-0.006
	(0.002)	(0.002)	(0.004)	(0.004)
t=2	-0.008***	-0.006***	-0.009**	-0.005
	(0.002)	(0.002)	(0.004)	(0.005)
t=3	-0.008***	-0.004*	-0.008*	-0.001
	(0.002)	(0.002)	(0.005)	(0.005)
t=4	-0.007***	-0.004	-0.008	-0.003
	(0.003)	(0.003)	(0.005)	(0.005)
t=5	-0.007**	-0.002	-0.008	-0.001
	(0.003)	(0.003)	(0.006)	(0.006)
Constant	8.023***	7.725***	8.020***	7.719***
	(0.003)	(0.002)	(0.005)	(0.005)
Cohort*Year FE	Yes	Yes	Yes	Yes
N	2585380	2049674	609750	485910
Clusters	176611	158750	41528	37386

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A11. *Individual fixed effects estimates of the effects of having a lone parent in it's final years of life on sick leave*

	All		Sudden death	
	(1)	(2)	(3)	(4)
	Sons	Daughters	Sons	Daughters
t=-8	0.059	0.226*	-0.087	0.194
	(0.104)	(0.136)	(0.234)	(0.308)
t=-7	0.314**	0.276	0.242	-0.037
	(0.136)	(0.180)	(0.309)	(0.408)
t=-6	0.550***	0.110	0.714*	-0.522
	(0.162)	(0.214)	(0.367)	(0.482)
t=-5	0.649***	0.078	1.066**	-0.246
	(0.184)	(0.245)	(0.418)	(0.552)
t=-4	0.896***	0.318	1.153**	0.396
	(0.206)	(0.275)	(0.462)	(0.625)
t=-3	0.911***	0.309	1.156**	0.553
	(0.227)	(0.305)	(0.507)	(0.693)
t=-2	1.008***	0.312	1.252**	0.297
	(0.248)	(0.334)	(0.554)	(0.761)
t=-1	1.185***	0.587	1.440**	0.356
	(0.269)	(0.364)	(0.601)	(0.825)
t=0	1.495***	1.381***	1.815***	0.542
	(0.290)	(0.391)	(0.649)	(0.884)
t=1	1.899***	1.026**	2.188***	0.058
	(0.310)	(0.415)	(0.695)	(0.937)
t=2	1.943***	0.596	1.975***	-0.090
	(0.327)	(0.436)	(0.730)	(0.984)
t=3	1.727***	0.355	1.695**	-0.794
	(0.343)	(0.456)	(0.764)	(1.026)
t=4	1.758***	0.303	1.828**	-0.372
	(0.360)	(0.476)	(0.802)	(1.076)
t=5	1.679***	0.063	1.810**	-0.877
	(0.377)	(0.496)	(0.841)	(1.122)
Constant	1.114***	2.775***	0.785	3.962***
	(0.295)	(0.414)	(0.599)	(1.215)
N	4709334	4485667	899787	859002
Clusters	250738	238841	47795	45625

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A12. *Individual fixed effects estimates of the effects of parent in its final years of life on log income conditional on employment, lone parents, different sibling constellations*

	Sons			Daughters		
	(1)	(2)	(3)	(4)	(5)	(6)
	Opp. sex	Same sex	Singleton	Same sex	Opp. sex	Singleton
t=-8	-0.001	-0.001	0.000	-0.002*	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
t=-7	-0.002	-0.001	0.000	-0.003**	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
t=-6	-0.002	-0.001	0.002	-0.002	-0.000	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
t=-5	-0.003	-0.002	0.000	-0.003	-0.000	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
t=-4	-0.005**	-0.002	0.001	-0.003	-0.001	-0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
t=-3	-0.004	-0.004	0.003	-0.003	0.000	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
t=-2	-0.003	-0.006**	0.002	-0.005	-0.000	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
t=-1	-0.005	-0.008**	-0.001	-0.005	-0.001	-0.005
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
t=0	-0.009**	-0.012***	-0.003	-0.010***	-0.007*	-0.013***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
t=1	-0.010**	-0.012***	-0.004	-0.008**	-0.004	-0.010**
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
t=2	-0.009**	-0.012***	-0.003	-0.006	-0.004	-0.008
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
t=3	-0.009*	-0.011**	-0.001	-0.004	-0.001	-0.006
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
t=4	-0.011*	-0.011**	0.002	-0.003	-0.001	-0.006
	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)
t=5	-0.010*	-0.012**	0.003	-0.001	0.001	-0.005
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Constant	7.996***	7.994***	7.994***	7.690***	7.692***	7.707***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Cohort*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	619707	600353	479649	516213	460508	370535
Clusters	41883	40350	33275	39423	34997	29060

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A13. *Individual fixed effects estimates of the effects of parent in its final years of life on log income conditional on employment, lone parents, split according to age at parental demise*

	Sons		Daughters	
	(1)	(2)	(3)	(4)
	<55	≥55	<55	≥55
t=-8	-0.001	-0.001	0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
t=-7	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
t=-6	-0.001	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.002)	(0.001)
t=-5	-0.003	-0.001	-0.002	0.000
	(0.002)	(0.001)	(0.002)	(0.001)
t=-4	-0.003	-0.002	-0.002	-0.000
	(0.002)	(0.001)	(0.002)	(0.001)
t=-3	-0.003	-0.003*	-0.002	-0.000
	(0.002)	(0.002)	(0.003)	(0.002)
t=-2	-0.003	-0.004*	-0.004	-0.000
	(0.003)	(0.002)	(0.003)	(0.002)
t=-1	-0.004	-0.005**	-0.005	-0.001
	(0.003)	(0.002)	(0.004)	(0.002)
t=0	-0.008**	-0.006***	-0.012***	-0.005**
	(0.003)	(0.002)	(0.004)	(0.002)
t=1	-0.010***	-0.009***	-0.010**	-0.003
	(0.004)	(0.003)	(0.005)	(0.003)
t=2	-0.008*	-0.009***	-0.009*	-0.003
	(0.004)	(0.003)	(0.005)	(0.003)
t=3	-0.007	-0.008**	-0.008	0.001
	(0.005)	(0.003)	(0.006)	(0.003)
t=4	-0.007	-0.007**	-0.008	0.002
	(0.005)	(0.004)	(0.006)	(0.003)
t=5	-0.006	-0.008*	-0.006	0.005
	(0.005)	(0.004)	(0.007)	(0.004)
Constant	7.915***	8.010***	7.637***	7.712***
	(0.002)	(0.001)	(0.003)	(0.001)
Cohort*Year FE	Yes	Yes	Yes	Yes
N	1249390	1945740	976998	1558586
Clusters	87029	131110	79730	116406

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

Table A14. *Individual fixed effects estimates of the effects of a parental health shock on log income conditional on earning at least 20 000 SEK annualy, lone parents*

	Parental demise		Parental stroke	
	(1)	(2)	(3)	(4)
	Sons	Daughters	Sons	Daughters
t=-8	-0.000	-0.000		
	(0.001)	(0.001)		
t=-7	-0.002	-0.002		
	(0.001)	(0.001)		
t=-6	-0.004***	-0.002		
	(0.001)	(0.001)		
t=-5	-0.004***	-0.001		
	(0.002)	(0.002)		
t=-4	-0.005***	-0.003*	0.002	-0.004
	(0.002)	(0.002)	(0.003)	(0.003)
t=-3	-0.005***	-0.005**	0.004	-0.001
	(0.002)	(0.002)	(0.004)	(0.004)
t=-2	-0.006**	-0.004	-0.002	-0.006
	(0.002)	(0.002)	(0.005)	(0.005)
t=-1	-0.007**	-0.006**	-0.000	-0.002
	(0.003)	(0.003)	(0.005)	(0.006)
t=0	-0.012***	-0.014***	0.000	-0.007
	(0.003)	(0.003)	(0.006)	(0.006)
t=1	-0.017***	-0.014***	-0.008	-0.013*
	(0.003)	(0.003)	(0.007)	(0.007)
t=2	-0.014***	-0.014***	-0.006	-0.006
	(0.003)	(0.004)	(0.008)	(0.008)
t=3	-0.013***	-0.010**	-0.002	-0.005
	(0.004)	(0.004)	(0.009)	(0.009)
t=4	-0.010**	-0.008*	-0.002	-0.004
	(0.004)	(0.004)	(0.010)	(0.010)
t=5	-0.009**	-0.005	-0.002	0.003
	(0.004)	(0.005)	(0.011)	(0.011)
Constant	8.008***	7.545***	8.028***	7.666***
	(0.004)	(0.005)	(0.043)	(0.054)
Cohort*Year FE	Yes	Yes	Yes	Yes
N	3630603	3532779	281708	271279
Clusters	229848	220594	20836	19754

Note: Standard errors are clustered at parental level. * significant at 10%; ** at 5%; *** at 1%.

II. Sober Mom, Healthy Baby? Effects of Brief Alcohol Interventions in Swedish Maternity Care

Co-authored with Erik Grönqvist, Anna Sjögren, and Helena Svaleryd

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1 Introduction

Public interventions and recommendations concerning expecting women's alcohol consumption have long been part of national strategies to promote maternal and child health. This has been motivated by the insight that the fetus is not protected from harm in utero and by evidence of negative effects of alcohol exposure (McBride 1961; von Lenz and Knapp 1962; Jones et al. 1973; Barker 1990).[1] Ambiguous findings regarding the effects of moderate alcohol consumption during pregnancy have however lead to a questioning of strict recommendations to completely abstain from alcohol (see for example Oster 2013), and pregnant women do not always follow the recommendations. In spite of strict recommendations in Sweden, Göransson et al. (2003) find that about 30 percent of pregnant women reported using alcohol regularly, in an anonymous survey. Barry et al. (2009) report much lower figures for the US: 10-12 percent of pregnant women report drinking at all. Yet, this is of concern in view of a growing recent literature in economics showing that alcohol exposure in utero has causal adverse effects on health and human capital (see e.g. Wüst 2010; Zhang 2010; von Hinke et al. 2014; Nilsson 2017); in particular since Wüst and von Hinke are able to demonstrate that the ambiguous impact on child health of maternal wine or moderate alcohol consumption disappear when selection effects are accounted for.

In a report of the US National Task Force on Fetal Alcohol Syndrome and Fetal Alcohol Effect it is concluded that research on the effectiveness of universal prevention interventions to reduce alcohol related pregnancies or fetal alcohol spectrum disorders is insufficient, though Screening and Brief interventions are mentioned as promising strategies (Barry et al. 2009). Hence, it is of great importance to identify effective methods for preventing harmful fetal alcohol exposure, and more generally to find interventions that improve child health. It is also important to understand how 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.

[1] 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). The insight that the fetus is not protected from harm in utero has gained recognition since the 1960's. 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 et al. 1973) was important for establishing the vulnerability of the fetus. These and other findings lead Barker (1990) to formulate the Fetal origins hypothesis, which is discussed at length in Almond and Currie (2011). 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 et al. 2009; Currie et al. 2009; Currie et al. 2011; Currie and Walker 2011; Black et al. 2013), maternal health shocks (Almond 2006), malnutrition (Lindeboom et al. 2010; Almond and Mazumder 2011; Doblhammer et al. 2013), maternal stress (Currie and Rossin-Slater 2013; Lindo 2011), economic conditions (van den Berg et al. 2006; van den Berg et al. 2011), and alcohol (Wüst 2010; Zhang 2010; von Hinke et al. 2014; Nilsson 2017).

We exploit regional time variation 2004-2009 in the introduction of the Swedish Risk Drinking project in antenatal care. This is 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 behavior; and (iii) referral to treatment for those identified as needing more extensive treatment with access to specialist care.[2] The roll out of the program involved a major effort to train midwives in screening with AUDIT and 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 – as well as 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.

Interest in the 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 2017) 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. 1998; Abel and Hannigan 1995).

Interest in the effectiveness of this screening and BI program 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 cessation, drug or alcohol addiction and in promoting reductions in risky behaviours (Rubak et al. 2005). However, in reviewing a large number of reviews, O'Donnell et al. (2013) conclude that the evidence regarding interventions during pregnancy is yet rather weak.[3] Moreover, studies of large scale BI-programs in primary care for gen-

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

[3]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-Williams et al. 2015).

eral 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 health and maternal behavior.[4]

Due to timing constraints, not all antenatal clinics were able to introduce the program simultaneously (Socialstyrelsen 2008). This resulted in a staggered introduction of the screening and MI across antenatal clinics in Sweden so that similar mothers giving birth in the years 2003-2009 faced different screening and alcohol prevention regimes depending on where they lived and when they were pregnant. This allows us to estimate the effects of the program with a difference-in-differences strategy. We use rich administrative data on prescription drugs and hospital care consumption (including detailed information on chemical classification and diagnosis) to construct measures of health, for the universe of first born children in Sweden during the implementation 2004-2009. In an additional analysis we use a similar strategy to estimate the effects on self-reported maternal behaviors and child health exploring survey data collected by the midwifes covering 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. In particular, we find that screening lowered the probability of children being prescribed a pharmaceutical drug during their first year of life by 8.4 percent, and lowered the probability of 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, which would not have required hospitalization if the child had access to timely and effective preventive or primary care (e.g. asthma, diarrhea and infections). We find no effects on conditions that could be connected to congenital malformations or perinatal condition and complications at birth that would be associated with heavy alcohol exposure in early gestation. Neither do we find an effect on the sex ratio at birth nor do we find differential health effects by sex of the child. This pattern of results is consistent with the program having no influence on hazardous alcohol consumption in early gestation, which is what to expect given that it is administered towards the end of the first trimester. Instead, the results are consistent with the interpretation that the screening and brief alcohol intervention reduced alcohol exposure later in the pregnancy, leading to improvement in children's immune

[4]P. Nilsen et al. (2012) analyze maternal self-reported (but anonymous) drinking habits prepregnancy 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.

system. The effects on avoidable conditions and injuries, as well as effects on maternal smoking cessation also point to behavioral effects that extend beyond alcohol consumption and the duration of the pregnancy. Effects on smoking may partly be the result of the MI-training improving midwives general ability to support health promoting behaviors, not only behaviors related to alcohol. Smoking and alcohol consumption are however often complements as is found in Wüst (2010).

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 the 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 BI- program in Swedish antenatal care improved child health and maternal behaviors when implemented within the context of universally available antenatal care is an important argument for supporting such policy initiatives. The socioeconomic profile of the results also suggests that alcohol prevention in antenatal care contributes to closing socioeconomic gaps at birth. 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).

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) which 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 et al. 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; Clarke 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).

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 alcohol consumption and child health in a general population of mothers.[5] There are, however, a growing number of studies with well-identified causal effects utilizing sales restrictions to document the detrimental effects of maternal alcohol consumption on child outcomes at the population level (Zhang 2010, Fertig and Watson 2009, and Nilsson 2017).[6] 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.[7] Similarly, Fertig and Watson (2009) find that changes in state minimum drinking age laws in the US 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 (2017) 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 in the age 18-21 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) for boys.[8]

[5] See discussion in Nilsson (2017) for a discussion of the earlier mainly observational studies.

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

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

[8] 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 (2017). Catalano

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 individuals in the full population, 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 shows 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 easily 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

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.

the first visit, which occurs around week 8-12 of the pregnancy, is primarily to make a physiological assessment and to provide 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 to stop drinking alcohol during pregnancy.

In 2004 the Risk Drinking project was initiated in Swedish maternity care in response to 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 Källmén 2003). Since consumption of alcohol during pregnancy is influenced by established habits, changed consumption patterns in general, may have consequences for women's attitudes towards alcohol during pregnancy (Göransson et al. 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 risky 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 displaying a risky alcohol consumption pattern.[9]

AUDIT is a 10-item questionnaire, developed by WHO, covering three areas: consumption, addiction, and alcohol related damages (Babor et al. 2001).[10] The AUDIT instrument was adapted for use in antenatal clinics by asking, not about present but rather, about pre-pregnancy alcohol behavior, and was promoted as a pedagogic tool to be used at the woman's 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 display risky consumption patterns. 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.[11]

[9] MI is developed in Miller (1983) and Miller and Rollnick (1991).

[10] See Figure A4 in Appendix for the AUDIT questionnaire.

[11] See eg Handmaker and Wilbourne (2001).

One strength of the AUDIT protocol 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 lies in its implementation which is focused on women's alcohol consumption prior to pregnancy.[12] 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).

The AUDIT protocol grades 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 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).[13] If a woman scores a value of 6 or higher on AUDIT the midwife will immediately start a motivational BI with the aim of supporting modified behavior. The woman will also be invited 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 (Folkhälsoinstitutet 2014; Damström Thakker 2011; Västra Götalandsregionen 2008). Importantly, the intervention is aimed at motivating and encouraging behavioral modification rather than coercion or merely providing health information.

During the roll out of the Risk Drinking project in antenatal care midwifes were trained in using AUDIT as well as in MI technique. The training programs were organized by the coordinating midwives at the county level.[14] 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

[12] 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).

[13] 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).

[14] Together with Heads of Obstetrics, coordinating midwives in the counties are in charge of developing, implementing and evaluating local practice in the area of antenatal care and reproductive health.

were involved in these training programs and hence time constraints implied that it took some time to train midwifes in AUDIT and MI.[15] As a result the program was gradually adopted by antenatal clinics, where the exact timing depended on accessibility and scheduling possibilities among both participating clinics and by lecturers and instructors. By 2010, 92 percent of the clinics had introduced AUDIT and MI (Socialstyrelsen 2008).[16]

In an evaluation of the Risk Drinking project, the National Board of Public Health (Folkhälsoinstitutet 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 midwives 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).

4 Empirical strategy

To estimate the effects of a universal screening brief alcohol intervention program in antenatal care on infant health and maternal behavior, we use a difference-in-differences approach where we utilize the staggered implementation of AUDIT screening and MI 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), and hence the measures of health used in this study. We will therefore focus on within-county variation between municipalities in the timing of implementation to identify the effects of the 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.[17]

[15] In Figure A1 in Appendix we describe the gradual implementation of the AUDIT-MI-program.

[16] For a detailed account of the training program and implementation see eg Nilsen et al. (2011). Details about the implementation are also based on an interview with Kerstin Petersson, head administrator of the MHV-register and Coordinating midwife in Stockholm County, October 16, 2015.

[17] 33 municipalities are excluded from the analysis because the clinics within the municipality introduced the program in different years. The sample restrictions are discussed in Section 5.1.

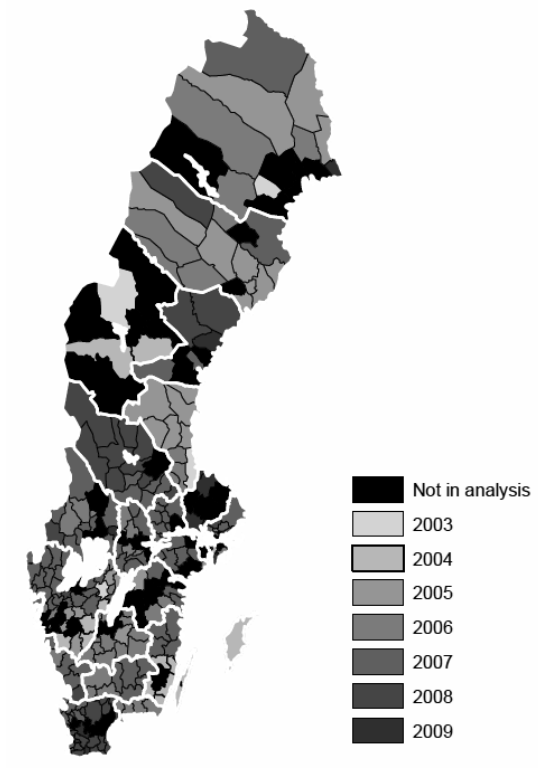

Figure 1. Regional implementation of the program by year

Mothers are regarded as treated by the program if they – during the first four months of the pregnancy – live in a municipality where the antenatal clinics have implemented the 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:

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

where y_{ickt} is the outcome of child i in county c in municipality k, year t. With γ_k being a vector of municipal fixed effects, and η_{ct} a vector of county specific time effects, the variations between municipalities within a county identify the effect. $Treatment_{kt}$ is an indicator taking the value 1 if the mother belongs to a clinic which has implemented the screening and BI program 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. $\mathbf{X}_i$ is a vector of controls for predetermined family characteristics. There is a social gradient both in child health (Cutler et al. 2008; Mörk et al. 2014) as well as in drinking and awareness of the detrimental effects of alcohol consumption during pregnancy (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, $\mathbf{K}_{kt}$. 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 in the municipality. 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 by 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 potentially select into clinics based on their alcohol prevention 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 municipalities where all clinics implemented screening and BI at the same time. The problem of varying screening practices, and the scope for clinic choice, is more pronounced in larger cities with several clinics and in section 6.6 we present sensitivity analyses with regard to excluding these municipalities.

Another potential threat to the identification strategy is that mothers who were 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 later and at a slower pace than the antenatal care clinics. In 2006, the fraction of child health nurses who had received at least some training in prevention of risky alcohol consumption was 52 percent, substantially lower than the corresponding fraction of midwives which was 88 percent. In addition, the midwives typically had received more training. By 2009, two thirds of midwives and one third of child health nurses had received at least three days of training (Folkhälsoinstitutet 2010).

4.1 Expected effects of the program

In order to assess through which mechanisms a screening and brief alcohol intervention program for pregnant women affects infant health we analyze heterogeneities by different domains of infant health, by sex of the child and by socioeconomic status of the mother. 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).[18] 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).

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 investigated screening and BI program 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 sex ratios at birth or gender heterogeneities. To capture effects of fetal exposure throughout the pregnancy we instead study effects on *infections* which may be a consequence of increased sensitivity or reduced immune function related to birth weight and fetal growth (Gauthier 2015). In addition, we study the most common diagnoses leading to hospitalization among infants, 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.[19]

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).[20]

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[18] 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 boy-to-girl-ratio and a sex gap in health at birth to the favour of girls. This is consistent with the findings of Nilsson (2017). 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.

[19] When using hospital admissions as outcome we combine we combine respiratory diagnoses (which include both admissions for asthmatic problems, croup, RS-virus and throat infections) and admissions for eye and ear infections.

[20] 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 lists diagnoses groups and the ICD codes included as well as the ATC codes for the categories of drugs.

The program was designed to better detect at risk mothers. It is well known that 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; von Hinke et al. 2014). Differential effects by maternal age and education may thus pick up heterogeneous impact of the program due to heterogeneities in risk of alcohol exposed pregnancies as well heterogenous responses at given risk levels.

Although the program was focused on alcohol prevention, it is possible that other behaviors are affected. We therefore also study effects on smoking and breastfeeding, which could be a consequence of reduced alcohol consumption, since alcohol and cigarettes are often consumed together, while mothers may be reluctant to breastfeed when they have been drinking. However, breastfeeding and smoking effects could also be spill-overs of MI training to other areas of health promotion if the midwives' ability to successfully promote behavioral change is not limited to alcohol.

5 Data

In the main analyses we combine data from administrative registers – e.g. the Population register, the Hospital Discharge register and the Prescription Drug register – with antenatal clinic level survey data on the implementation of the program from the Swedish Maternal Health Care Register. We describe these data below. In auxiliary analyses we also make use of 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 want to avoid information given during earlier pregnancies to influence the results. Moreover, given the possibility that the program may affect 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 and whose mothers reside 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 based on administrative records and population censuses; specifically: educational attainment, annual labor income, age, and municipality of residence. The information on educational attainment is based on a 3-digit code, corresponding to 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 by the Swedish Maternal Health Care Register. The register 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. The register is based on a local organization of participating antenatal clinics. Participation by these facilities is not mandatory, yet in 2008 compliance was 89 percent. Since the register was initiated from within the profession and is used to benchmark quality and 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. Structured screening was first introduced as a part of the studied program and using structured screening 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 AUDIT screening from 2005 and 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 the program in 2004 and no alternative, structured screening methods were in use.[21] 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 Swedish Maternal Health Care Register, 72 municipalities have multiple clinics. Among municipalities with multiple units, 29 munici-

[21] 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 noted (i) that only 2 percent of the clinics were using such methods in 2003 as can be seen in Figure A1 in Appendix, 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.

palities 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 midwife usually occurs around week 8-12, screening is 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 later 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 (over night) 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 chemi-

cal classification of the drug according to WHO's ATC system.[22] Pharmacies have strong incentives to report sales in order to get reimbursed from the public drug benefit. By using information from the ICD and ATC classification we define hospitalizations and drug prescriptions for different conditions and events of ill-health as described in Section 4.1 (see Table A1 in Appendix for exact ICD10 and ATC codes).

Information from the Hospital Discharge register is available for the whole implementation period 2003-2009. Information on drug prescriptions is available only 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 displays 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 shows 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 decreased somewhat whereas the share of children getting drugs prescribed has been rather constant over the period (see Figure A2 and Figure A3 in Appendix). 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.

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

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

Note: This table presents the means of variables included in the main analysis with standard deviation in parenthesis.

5.4 AUDIT scores, maternal characteristics, behaviors and child outcomes

Before proceeding to the analysis we characterize how maternal characteristics, health behaviors and child health relate to AUDIT scores. Table 2 presents statistics for first time mothers 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 Swedish Maternal Health Care Register for the period 2010-2014; that is, when the studied program is 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 pregnant women have elevated AUDIT scores of 6 or above at their sign in visit. Women with high AUDIT scores are younger than the average pregnant woman, and are more likely to have just compulsory education. The fraction of non-Nordic immigrants with an elevated AUDIT score is lower than among women in general.

About half of the first time pregnant women say they are in good or excellent health and 25 percent have normal BMI at registration. A remarkable difference between the different groups of women is that 24 percent of women with AUDIT ten or above smoked at registration while the corresponding fraction for low-AUDIT women was only 4 percent. This pattern also persists

during pregnancy. Moreover, we see that fewer women with elevated AUDIT breastfeed fully or partially when the child is a month old.

Table 2. *Characteristics and behavior sign-in visit AUDIT score 2010-2014*

	AUDIT 0-5	AUDIT 6-9	AUDIT≥10
Characteristics of woman			
Age	29.1	27.7	26.3
Young (<25)	0.21	0.32	0.47
Old (>34)	0.16	0.10	0.08
University education	0.50	0.37	0.21
Compulsory education	0.047	0.057	0.161
Non-nordic immigrant	0.15	0.04	0.04
In good health at registration	0.49	0.50	0.47
BMI normal at registration	24.3	24.5	24.5
Smoking at registration	0.038	0.104	0.235
In good health during pregnancy	0.50	0.50	0.48
Smoking in week 32	0.026	0.071	0.183
Breastfeeding at 1 month	0.87	0.85	0.79
Observations	118496	11863	2256

Note: This table presents the means of variables based on individual level data from the Maternity Health Care Register for the period 2010-2014 (standard deviation in parenthesis).

6 Results

We present the results of estimating the effect of implementing a screening and brief intervention alcohol prevention program in antenatal care on children's health. First we present results on the probability that the child is prescribed a drug or is admitted to hospital during the first years of life. Then we present results relating to 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, and thereafter we analyze socioeconomic outcomes of parents. Finally we present some robustness checks.

6.1 The effect of the program on child health

The first two columns in Panel A of Table 3 show the effect of the program 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 the 1 percent level and suggests that children of treated 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

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 per capita, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

who were not treated by the program. Columns 1 and 2 in Panel B show that the program also reduces the probability that a child is 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 189 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 concentrated 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 the first year of life, choosing the model with control variables as our main specification.

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. Children of treated women have significantly lower probability of being prescribed drugs against infections, but for drugs for respiratory conditions we find no effect. The estimated effect on antiinfectives is 4.4 percentage points, or 20 percent, suggesting that children of treated mothers may have a stronger immune system or that they are less exposed to infections. Increased susceptibility to infections through a weaker immune system is a potential consequence of poor nutrition due to impaired placental function caused by alcohol exposure (Burd et al. 2007).

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 eye and ear infections and respiratory conditions. The next two columns are hospitalizations in diagnoses where admissions are avoidable, 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: avoidable hospitalizations are reduced by 3.9 percentage points, or 24 percent, while injuries are reduced by 42 percent. The point estimates for perinatal, eye and ear 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, 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.

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

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 per capita, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%.

6.3 Heterogenous 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 significant. 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 of 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 again the effect of the program is accounted for by children of fathers with low income (See Table A3 in Appendix).

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 children of young mothers and significant at the 10-percent level, but not statistically different from the effect of the program on children of 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, where low income (and younger) families are more inclined to seek hospital care for their children while pharmaceutical drugs prescriptions is the affected margin for children of older mothers. Similarly, we also find that effects on prescriptions are larger in municipalities where alcohol sales are below average (See Table A4 in Appendix).[23]

[23] In Table A4 in Appendix 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.

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-value 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-value difference	0.651		0.010		0.735	
Observations	61858	46704	59764	48798	73596	34966
Municipalities	231	231	231	231	231	231
Mean of outcome	198.137	175.149	195.301	179.600	187.086	190.844

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 per capita, 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%.

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 month) 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 2017).

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 our effects on health stem from reductions in moderate consumption.

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-value 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 per capita. Columns 1-4 also include the sex of the child. * Significant at 10%; ** at 5%; *** at 1%.

6.5 Socio-economic outcomes of parents

The objective of the Swedish maternity care system is to monitor the health of the mother and of the 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 parents are likely to be affected. 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 thus also have longer run consequences for the health and welfare of parents.

In Table 7 we therefore analyze effects on socio-economic outcomes such as family stability and the likelihood of receiving social assistance (SA). Social assistance is strictly means tested at the household level and conditional on the recipient household having no alternative sources of income or assets to sell in order support themselves. The result in column 1 shows 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 the likelihood of the parents receiving any social assistance during the calendar year after the pregnancy; even if one of the parents is on parental leave, a family can receive social assistance if the money does not last a full month until the next parental benefit payment. The result in column 2 suggests that being subjected to the program reduced the probability of mothers being social assistance recipients with 0.8 percentage points, which corresponds to a 14 percent reduction at the mean. This result is robust to controlling for social assistance the year before the pregnancy in column 3. For fathers, we also find negative point estimates; the effect becomes significant in column 5 when controlling for fathers social assistance before the pregnancy. In order to corroborate the results on social assistance we (in columns 6-7) estimate the impact of the program on the

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

	Cohabiting	SA recipient year after pregnancy				SA recip. year before pregnancy	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Program	-0.003	-0.008**	-0.009**	-0.004	-0.008**	0.004	0.005
	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
SA recipient			0.248***		0.345***		
before pregnancy			(0.008)		(0.007)		
Sample	All	Mother	Mother	Father	Father	Mother	Father
Observations	103649	103649	99217	103482	101333	99477	101715
Municipalities	231	231	231	231	231	231	231
Mean of outcome	0.089	0.057		0.045		0.053	0.054

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 per capita, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%.

likelihood of receiving social assistance pre-pregnancy. The point estimates are positive and insignificant in this placebo analysis.

We also analyze if there is any direct effects on mothers health. The results presented in Table A5 in Appendix show no effect of the program on drug prescriptions to mothers' or on hospitalizations during the first year after giving birth, but show suggestive evidence (signtificant at 10 percent) that hospitalizations in the longer run is reduced. For fathers there is no effect on our health outcomes (See Table A6 in Appendix).

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 are treated 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: these municipalities are defined as treated when the largest clinic in the municipality introduces the program. Adding these municipalities lowers the estimates but they are still statistically significant. Next we instead include the years when the program was introduced. The results in columns 4 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

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

	Drug prescriptions (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 per capita, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%.

hospital is no longer statistically significant. While weakening the results, the underlying pattern stays the same when relaxing these sample restrictions.

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 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 where the implementation is mainly centered to a few years, the pre-effects become 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 becomes 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 more negative than the year before implementation and marginally significant. It needs to be pointed out that estimates away from the implementation year are based on an imbalanced sample of municipalities, because data availability on drug prescriptions is limited to the post 2005-period. Hence, some caution is warranted when interpreting the effect sizes for drug prescriptions. For hospital admissions, in column 2, we again find a treatment-effect of the same order of magnitude as in the baseline results (in Table 3), while the parameter for

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

	Drug prescriptions (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	72724	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 per capita, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%.

pre-treatment outcomes two years before implementation and earlier is positive but insignificant. It is worth noting that the sample period for this analysis is longer. These results are largely consistent with the parallel trends assumption, even if they are not conclusive for drug prescriptions where the sample period is restrictive.

We also assess the parallel trends assumption by re-estimating our baseline model for infant hospitalization using admissions during the first year of life for children born 6 years earlier in the same municipality as the outcome.[24] The result from this placebo analysis using the population of first-born children born between 1997 and 2002 is 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.

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 time restrictions in the training of midwives. 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

[24]This placebo is not possible for drug prescriptions since the drug data is only available from 2005.

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%.

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, compared to the mean, are about 4 percent more likely to implement the program 2007 or later.

Based on results from an events-study approach, a placebo analysis of a previous time period and on an analysis where we attempt to predict the timing of implementation, our over all assessment is that the data supports a causal interpretation of our results. Some caution is warranted as regards the results for drug prescriptions since data availability restricts our ability to draw firm conclusions.

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

The results found so far suggest that introducing screening and BI for alcohol at the antenatal clinics affect child health and maternal behaviors, and that the effects extend beyond the birth of the child. To further understand these behavioral changes we explore additional information from survey data 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 whether the pregnancy ended in a miscarriage. This is the same data as used in Section 5.4, but for the 2003-2008 period that we use here the

registration practices were less developed, so the data suffers from some misreporting and problems with missing data (coverage varies across questions).

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 are able to motivate reductions in risky alcohol consumption, but also other behaviors which have adverse effects on the child, such as smoking.

This survey data allows us to link women to the antenatal clinic they are registered at. We can thus estimate the effect of the program 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 program was introduced since it is not clear who was screened. Women are considered treated if they are registered at a clinic which has implemented program. We do not capture all women as not all clinics report information to the Swedish Maternal Health Care Register. The data, nevertheless cover a substantial fraction of first time mothers; 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:

$$y_{iact} = \alpha + \beta Treatment_{at} + \gamma_a + \eta_{ct} + \mathbf{K}_{kt}\lambda + \varepsilon_{iact} \tag{2}$$

where y_{iact} is the outcome of child/mother i at antenatal clinic a in county c 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. 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, $\mathbf{K}_{kt}$. However, as we are not able to link the individual level survey data to population registers, we are unable to control for background characteristics of the parents and the birth month of the child. 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 clinic level. 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 woman smoked at registration in week 8-12 but not in week 32 (*quit smok-*

ing) 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 the program affected behavior in a positive direction we expect smoking to decrease and the likelihood of breastfeeding to increase. However, we do not expect miscarriages to 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 infant health and maternal alcohol consumption, 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 the program induced more women to cease smoking. The probability to quit smoking between registration and week 32 is increased by 0.6 percentage points, corresponding to 25 percent at the mean. Since 7.5 percent of the women smoked at registration, this implies an 8 percent decrease in smoking. Very few pregnant women take up smoking during pregnancy; in column 2 we see that the share who do is reduced by 0.02 percentage points. This implies a reduction by 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 from women registering at clinics without the program in the sense that they were as likely to smoke or have stopped smoking before the initial visit at the clinic.[25]

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

[25]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. 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*

	Quit smoking between registration and week 32 (1)	Start smoking between registration and week 32 (2)	Breastfed at 1 month (3)	Mis-carriage (4)	Smoke at registration (5)	Quit smoking between 3 months before pregnancy (6)
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	132135	132135	116372	133860	134077	133938
Mean of outcome	0.0239	0.0045	0.8889	0.0054	0.0748	0.1131

Note: Standard errors in parenthesis, clustered at clinic level. All models include clinic and county-year fixed effects, and controls for municipal unemployment level, municipal level of alcohol sales per capita, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%.

8 Conclusion

Most expecting women are aware that excessive alcohol consumption during pregnancy can be harmful for the child. But changing consumption patterns with a shift towards more daily 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 screening and brief alcohol intervention program 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 effects of the program on infant health.

We find that introducing screening and brief intervention for alcohol in antenatal care improves infant health. The program lowers the probability that a child is prescribed a pharmaceutical drug during the first year of life by 8.4 percent relative to the population average, and lowers 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 drug prescriptions and 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 had 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. Effects on hospitalization 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 an improved home environment. The reductions in drug prescriptions are mainly related to infections, which would suggest that the impact of screening may also run through improved fetal conditions throughout the pregnancy. Still it is difficult to rule out that this also stem from improved care and attention after birth. We also find that the program reduced social assistance dependency. Moreover we find that the program reduced smoking. The results suggest, overall, that the program led to behavioral changes among treated mothers and that these effects persist after the birth of the child.

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 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 are shown to be an effective instrument to modify maternal behavior.

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Appendix

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, 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 pharmaceuticals in chapter R
Antiinfectives	=1 if prescribed pharmaceuticals in chapter J

Table A2. *Effects of the program on drug prescription and hospital admission during the first year of life excluding events within 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 per capita, 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-value difference	0.955		0.005	
Obsevations	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 per capita, 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, 2012 AUDIT 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 AUDIT score	Below median AUDIT score	Above median alcohol cons.	Below median alcohol cons.
P-value 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 AUDIT score	Below median AUDIT score	Above median alcohol cons.	Below median alcohol cons.
P-value 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 per capita, 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 prescriptions (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 per capita, 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 prescriptions (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)	(3.321)
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 per capita, 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*

	Quit smoking between registration and week 32	Start smoking between registration and week 32	Breastfed at 1 month	Mis-carriage	Smoke at registration	Quit smoking between 3 months before pregnancy
	(1)	(2)	(3)	(4)	(5)	(6)
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	83717	83717	83717	83717	83717	83717
Mean of outcome	0.0273	0.0051	0.8666	0.0055	0.0876	0.1190

Note: Standard errors in parenthesis, clustered at clinic level. All models include clinic and county-year fixed effects, and controls for municipal unemployment level, municipal level of alcohol sales per capita, and sex of the child.* Significant at 10%; ** at 5%; *** at 1%.

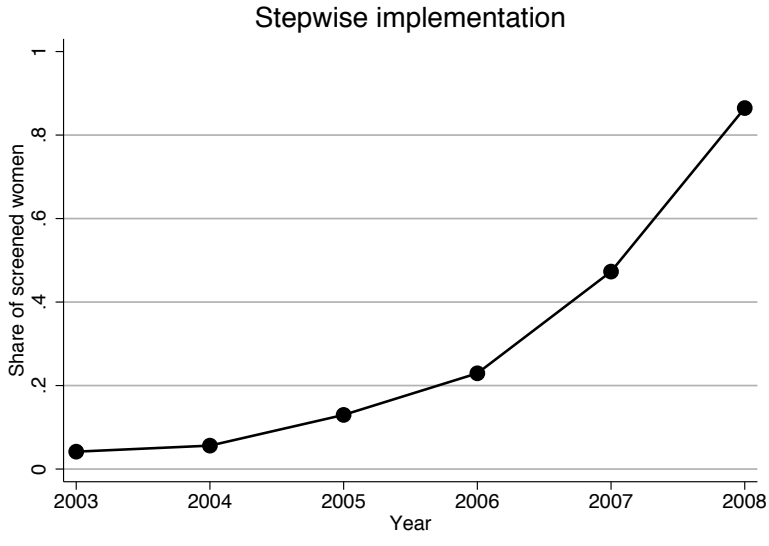

Figure A1. Share of clinics with a structured working methods to detect women with risky alcohol consumption 2003-2008

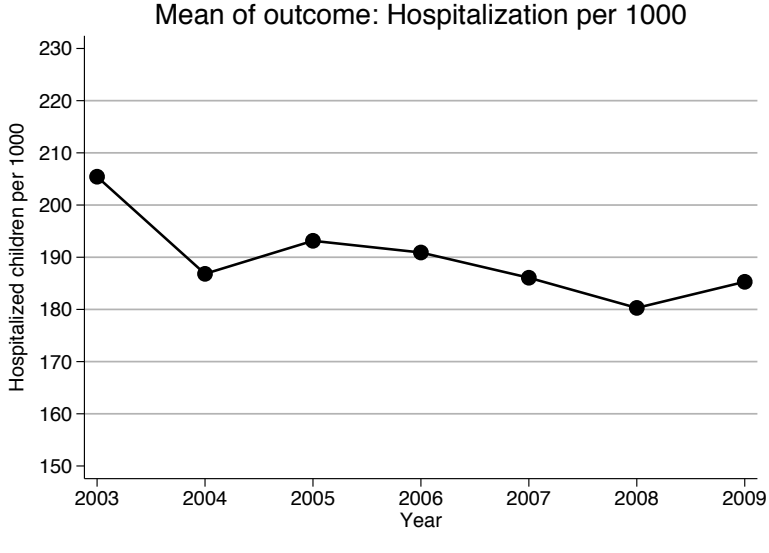

Figure A2. Share of children hospitalized during first year of life 2003-2009

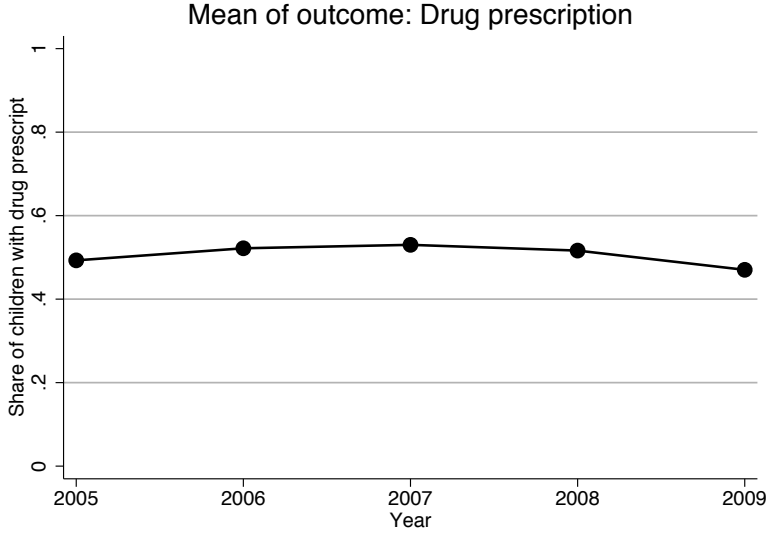

Figure A3. Share of children with drug prescription during first year of life 2005-2009

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	

Figure A4. AUDIT questionnaire – Source: Babor et a. (2001)

III. Auditing Mothers: The Effect of Targeted Alcohol Prevention on Infant Health and Maternal Behavior

Co-authored with Erik Grönqvist, Anna Sjögren, and Helena Svaleryd

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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 regression discontinuity design (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 preg-

nancy, 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).

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

As an adaption to the maternity care setting, women are asked about pre-pregnancy, 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 data is registered manually by midwives in the maternity clinic and coverage of individual data varies in the studied period from 81 percent to 89 percent. Since the register was initiated from within the profession and is used by the maternity clinics for benchmarking quality and to compare procedures, the in-

centives 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.

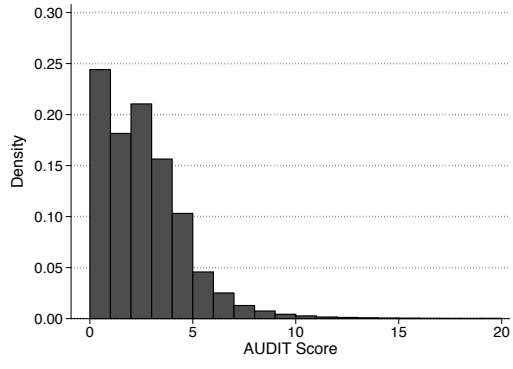


Figure 1. Distribution of AUDIT score 0-20

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

[2]Petersson et al. (2014) find 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.

weight. Examining the predetermined characteristics reveals that women with AUDIT score 0 is a selected group.

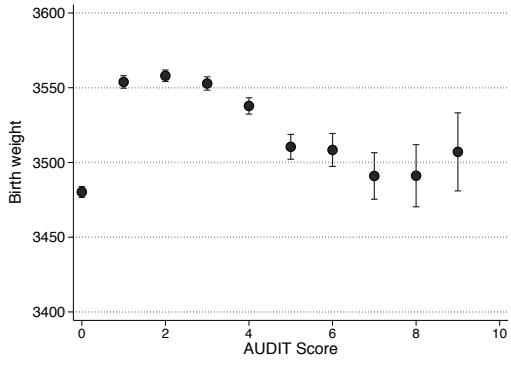


Figure 2. AUDIT score by birth weight

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 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*

	(1) ALL (0-9)	(2) AUDIT 0	(3) AUDIT 1-5	(4) 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 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 that 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 ma-

ternity 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:

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

where

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

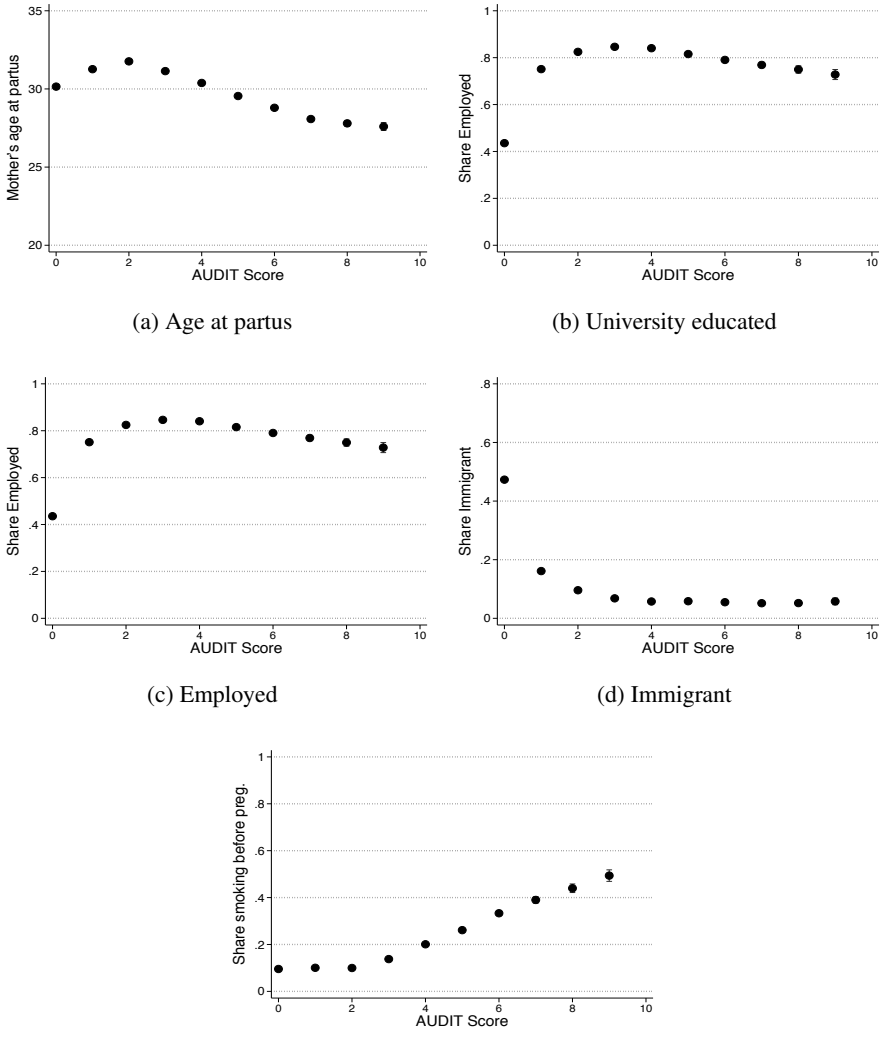
[4] As an alternative, we have also estimated the effect using a local linear regression over the AUDIT range 2-9 weighted using a triangular kernel. The triangular kernel assigns linearly decreasing weights to observations on each side of the treatment cutoff. The results from these estimates are presented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

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 score 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, and maternity clinic fixed effects 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.

Figure 3. AUDIT score by maternal characteristics



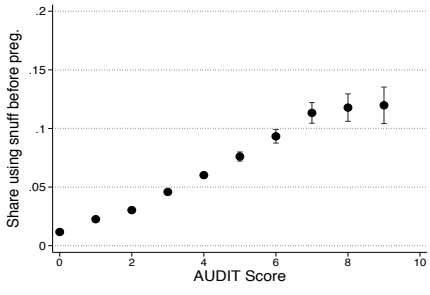
(a) Age at partus

(b) University educated

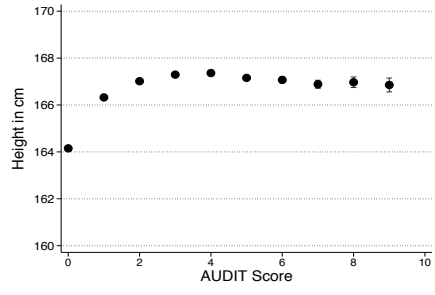
(c) Employed

(d) Immigrant

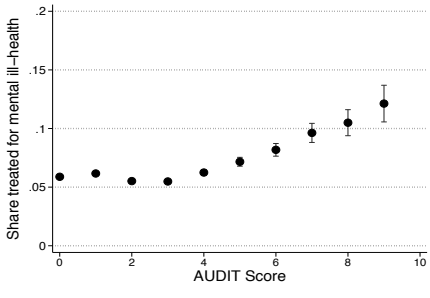
(e) Smoke



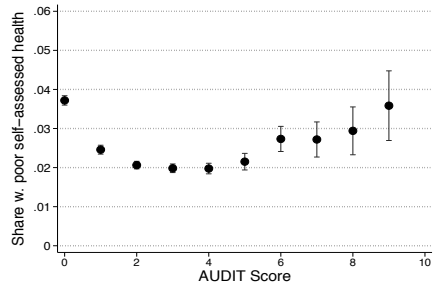
(f) Snuff



(g) Height



(h) Treated for mental illness



(i) Poor self-assessed health

Figure 3. (continued) 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.

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 threshold 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 for 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 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]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%.

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.

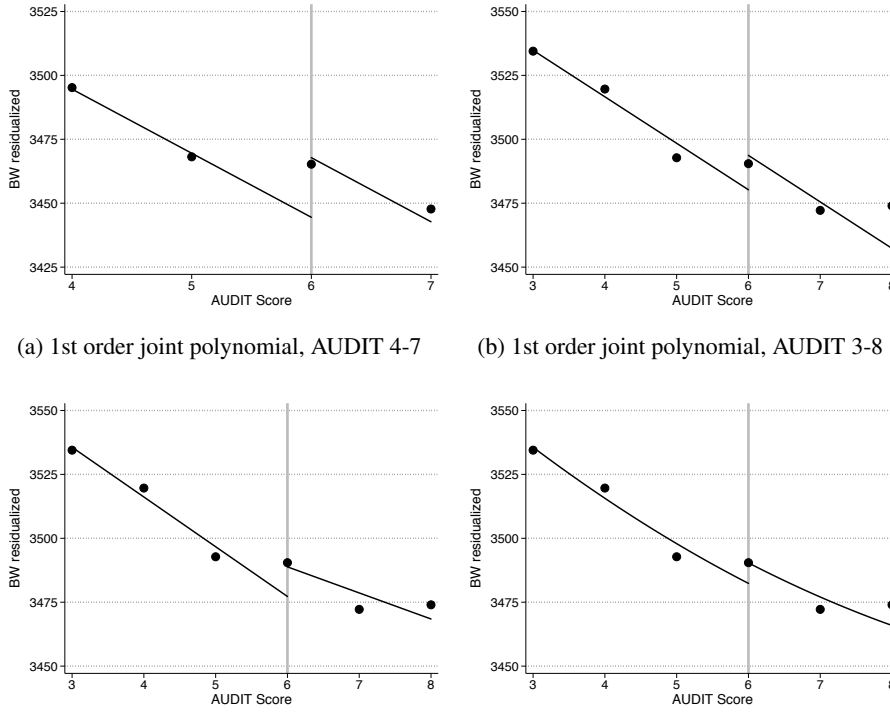
5.1 Birth weight

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 A.2 in Appendix.

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



(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

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 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

[7]Results from estimating the models without control variables show similar results and are reported in Tables A.2 to A.4 in Appendix.

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.

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≥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%.

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 illhealth, 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.

[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≥6	26.281**	3.450
	(10.605)	(11.961)
R-squared	0.028	0.024
Panel B: Extended controls 2		
AUDIT≥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 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%.

Effects across the distribution of birth weight

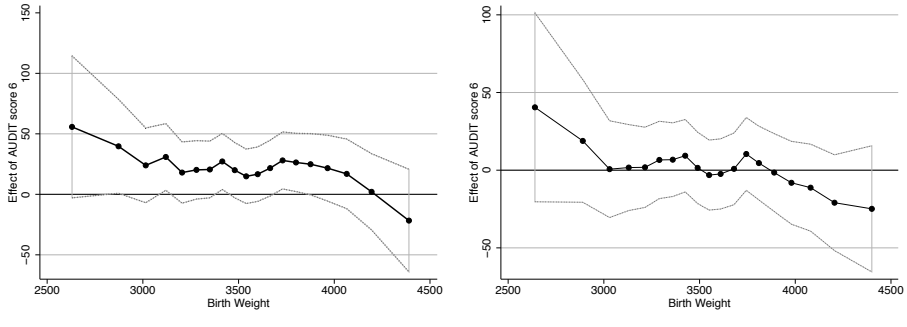
Even if the estimated average effect suggests that the intervention has a small to negligible impact on birth weight, this can mask larger impacts in different parts of the weight distribution. The effects may well be larger for children with elevated risk; for example children whose health is more susceptible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

Figure 5. Unconditional quantile effects of passing the threshold to AUDIT 6 on birth weight



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

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.

[9]The estimates from the unconditional quantile regressions for different quantiles are found in Table A.6 in Appendix.

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

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 report 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.

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%.

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 must be interpreted with care.

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 midwives, using 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 has 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 is 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 Conclusion

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) find 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

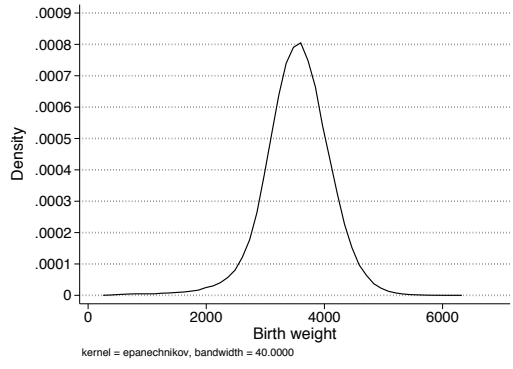


Figure A1. Kernel density of birth weight

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)	(2)	(3)	(4)
	Birth Weight	Birth Weight	Birth Weight	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*

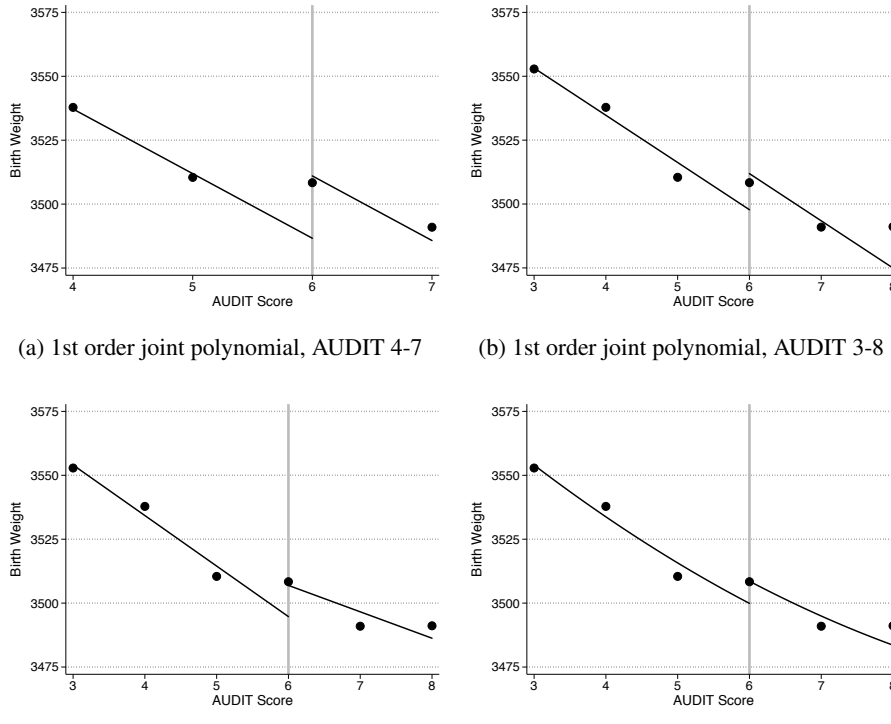
	5th	10th	25th	50th	75th	90th	95th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Range 4-7, 1st joint							
AUDIT≥6	55.745*	39.681**	17.995	14.880	24.882*	2.068	-21.709
	(29.928)	(19.797)	(12.913)	(11.473)	(12.905)	(16.082)	(21.646)
Observations	73,185	73,185	73,185	73,185	73,185	73,185	73,185
R-squared	0.009	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

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



(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

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 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≥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%.

IV. Childcare and the Division of Parental Leave

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1 Introduction

Sweden has one of the world's most generous parental leave systems. The primary purpose of the parental leave insurance is to enable parents to combine work and family life. Sweden is on the frontier of gender neutral parental leave outtake and there is an outspoken policy goal of equal child care responsibility between parents. Despite this, Swedish women still use the bulk of the parental leave. In 2012, women used 76 percent of the total paid parental leave days (Swedish Social Insurance Agency 2013). An unequal responsibility for the care of children may have several consequences for the family, some of which may be regarded as adverse. It is a likely candidate for the explanation of the earnings gap between men and women (Gupta et al. 2008; Lundberg and Pollak 2007; Albrecht et al. 2015). The father's involvement in childcare has also been highlighted as an important component of child development (see for example Tamis-LeMonda and Cabrera 2002). Furthermore, common for many of the OECD countries is that sickness absence rates are higher for women than for men which may be the result of women taking a more active part in child care during the first years (Angelov et al. 2013). Deepening our understanding of spouses' decisions of time allocation in the household is important so as to aid policies directed toward a more equal responsibility for the care of children.

In this paper, I will evaluate whether making childcare available for children of parents on parental leave due to the birth of a younger sibling has an impact on the division of parental leave between mothers and fathers. Until recently, access to childcare in Sweden was mainly reserved for children of working parents or parents who study. But after the implementation of a reform on January 1st 2002, Swedish municipalities were obligated to offer childcare for at least 15 hours per week to all children aged 1-5, including those whose parents were either unemployed or on parental leave with a younger sibling.[1] Prior to this reform, some municipalities already allowed older siblings to keep their spot in daycare. This heterogeneity in the implementation of the reform is exploited in a difference-in-differences approach to evaluate the effects of gaining the possibility to keep the older sibling in childcare. I will estimate a reduced form model in which parents before and after the reform in municipalities where children could keep their spot will be compared to parents before and after the reform in municipalities were they could not keep their child in pre-school[2] prior to the reform.

The knowledge about what motivates fathers to increase their responsibility for the care of their children is limited. Several attempts have been made by the Swedish government to increase the father's share of the parental leave by reforming the parental leave regulations through reserving benefit days for each

[1] The reform had additional components which will be described in section 3.

[2] In Sweden, pre-school is integrated into childcare. The terms childcare, pre-school and daycare will be used interchangeably throughout the paper.

parent, and by introducing tax credits to parents who share the leave equally. But this seem to have had limited or no effect on the division of the responsibility of child care (Ekberg et al. 2013; Eriksson 2005; Karimi et al. 2012; Swedish Social Insurance Agency 2010). Another potential determinant that could affect the division of the caring responsibility is the care burden during the parental leave. Being on parental leave can be demanding, especially when there are older siblings to look after as well. Access to childcare for the older child could therefore be an important relief in everyday life by decreasing the care burden and thus make parental leave more attractive. This could have implications for the division of parental leave.

If access to childcare makes the parental leave less demanding, this could impact the way that parents value the leave and hence how it is divided. In which direction the division would be affected is ambiguous. Given that mothers take the bulk of the child care responsibility they are likely to be less sensitive to changes in the cost of parental leave, whereas fathers could be more sensitive to such changes. A less demanding parental leave could increase the father's share if fathers regard the parental leave as relatively more attractive. On the other hand, mothers may also be sensitive to changes in the cost of parental leave and if they value the reduction in care burden higher it could increase their parental leave outtake. It could also be that both parents value the reduction in the care burden equally, in which case the reform would leave the division of the leave unaffected.

I find no evidence of availability of childcare for an older sibling during parental leave having an effect on the father's take-up of parental leave for the second born child. Difference-in-differences estimates of the effect of the reform on fathers' parental leave are small and not statistically different from zero. Nor is there any evidence of the reform having an effect on the division of parental leave between the mother and the father.

Making parental leave less demanding may also have implications for the health of the parents. Caring for only one child during parts of the day may decrease the amount of stress during parental leave and hence have a positive effect on parents' health. On the other hand, children in childcare are most likely subject to increased probability of attracting infections that could be transferred to other members of the family. Keeping an older sibling in childcare could therefore have a negative impact on parental health. In an additional analysis, this paper investigates whether access to childcare during parental leave has an effect on the number of days on sick leave during the infant's first year of life. Results suggest that there is no effect of access to childcare during parental leave on mother's and father's sick leave absence during the first year of the infant's life.

The rest of the paper is organized as follows. In the following section, I provide a short overview of earlier literature on family policies and discuss potential channels through which childcare may affect the division of parental leave. Section 3 summarizes family policies in Sweden and describes the re-

form used for identification. In Section 4, I describe the empirical strategy and Section 5 describes the data. Section 6 reports the main results, and finally Section 7 concludes.

2 Access to childcare and the division of parental leave

This section begins with a short overview of earlier literature on the effects of family policies, followed by a discussion of the potential mechanisms through which access of childcare can affect the division of parental leave.

2.1 Previous literature

Parents' decision of whether or not to stay at home and care for their child is certainly affected by access to childcare. There are several studies documenting the importance of childcare for child care decisions. Most of the literature in economics on the role of access to childcare has focused on maternal employment (for a review, see Waldfogel 2002). The impact of universal childcare on children's cognitive development has also received attention, and both positive and negative effects have been identified (Baker et al. 2005). Anderson and Levine (1999) study how child care decisions are affected by the costs of childcare; their results suggest that there is a negative relationship between the price of childcare and female labour supply. Since childcare in Sweden is subsidized, the Swedish context is different. Using the exogenous variation in childcare prices that resulted from a reform in Sweden in the early 2000's Lundin et al. (2008) find that reduced childcare prices do not seem to affect female labour supply. A related study by Vikman (2010) exploits another part of the same reform and finds that availability of childcare increases the probability of leaving unemployment.

As with earlier literature on effects of childcare, the focus of studies on parental leave policies has mainly been on the effects on female labour supply, fertility (see for example Lalive and et al. 2013; Lalive and Zweimüller 2009; Schönberg and Ludsteck 2007; Björklund 2006), and children's scholastic performance (Liu and Nordström Skans 2010). It has been suggested that a generous parental leave system has contributed to the relatively high labour force participation of women found in most Nordic countries. Recently however, potential drawbacks of a generous parental leave system have been pointed out. Since women use the bulk of the leave, increased durations of paid parental leave extend women's time away from the labour market which may have a negative effect on their career possibilities (see for example Gupta and Smith 2002; Albrecht et al. 2003; Karimi et al. 2012; Albrecht et al. 2015).

Evidence on effects of parental leave policies on the allocation of time within the household in a Swedish context is limited (Ekberg et al. 2005; Eriksson 2005; Karimi et al. 2012; Duvander and Johansson 2012). Ekberg

et al. (2013) study the effects of a reform of parental leave in Sweden that reserved parental leave days for the father. Despite increasing the father's share, there is no evidence of behavioral effects in the household. In 2008, a gender equality bonus was also introduced which gives tax credits to parents who share the leave equally. This reform does however not seem to have affected the division of parental leave (Duvander and Johansson 2012; Swedish Social Insurance Agency 2010; Swedish Social Insurance Agency 2014).

2.2 Mechanisms

Although policies aimed at increasing fathers' participation in the caring of children show no behavioral effects on the time allocation within the household, little is known about potential effects of changes in the burden – or non-monetary costs – of taking care of children on the division of parental leave. Access to pre-school for older siblings during parental leave can be regarded as a decreased burden for the parent on leave since there is one less child to look after during parts of the day. It gives the opportunity to focus on the infant and perhaps also makes the leave less time intensive. If the older sibling(s) can stay in childcare, the non-monetary cost of being at home with the infant is reduced. Whether and how this will affect the division of parental leave depends on how each parent value the cost reduction and on the spouses' bargaining power within the household.

If both parents value the non-monetary cost reduction equally, which would be the case if they for example find it equally burdensome being on parental leave, the division could be left unaffected. There may however be differences in the sensitivity to changes in the costs of parental leave between mothers and fathers. Given that mothers use the larger part of the leave they may be less sensitive, whereas fathers who use little leave may be more sensitive, to changes in the costs of parental leave. If fathers value the cost reduction more than mothers the fathers may use more leave and hence impact the division. Furthermore, if both parents value the reduction in the care burden equally the division may still be affected via an unequal bargaining power within the household. If fathers have a higher household bargaining power due to a larger share of the income, a decreased burden of the leave could imply that fathers use this to increase their share of the paid parental leave. Gender norms may however also impact the bargaining power when time allocation within the household is negotiated. There may be norms and beliefs about who is more suitable to care for children (Swedish Social Insurance Agency 2013; Dahl 2014). If women are the main caretaker in the family this may increase her bargaining power with respect to child care. A reduction in the non-monetary cost of parental leave can thus also increase the length of the maternal leave and leave fathers' parental leave unaffected.

3 Family policies in Sweden

One of the cornerstones in family policy in Sweden is the subsidized publicly provided childcare. A large share of Swedish children attends pre-school. In 2001 which is the year prior to the reform that will be studied in this paper, 43.3 percent of all one-year-old children and 79.3 percent of all two-year-old children in Sweden attended pre-school (Swedish National Agency for Education 2002). Another important part of family policies in Sweden is the parental leave insurance system. Up until January 2002, parental leave benefit was given for 450 days per child and one month was reserved for each parent. For parents of children born from January 1st 2002 and onward, parental leave benefit was extended to 480 days per child and a second month was reserved for each parent. The latter part of this extension of the parental leave is often referred to as the second "daddy-month reform".[3] [4]

The number of calendar days that are used for parental leave is different from the number of days available with parental leave benefit. The leave can be extended by extracting the benefit for only shares of the days or not using any benefit on some days. Therefore, the number of calendar days that an individual has been on parental leave can be different from the total number of days with parental leave benefit. The focus of this paper is whether access to childcare for the older sibling(s) affects the division of time spent at home with the second born child. The measure of parental leave outtake of interest is therefore the one that resembles time spent at home as closely as possible. Parents who extend time at home by using shares of the day could potentially be masked if shares of days were used to calculate the net total parental leave outtake. On the other hand, since the parental leave periods can be split into several smaller blocks of extracting benefit, the length of the total period that the parent spent at home is not clear from the register data. In this paper the number of days, regardless of the share, with parental leave benefit will be used to calculate the parental leave outtake. Although it may underestimate total time spent at home with the child, this measurement will serve as a good proxy for time spent on parental leave.

When the parental leave benefit was introduced in 1974, men used 0.5 percent of all days. Since then mens' share has increased to around 23 percent in 2010 (Duvander and Johansson 2012). During the infant's first years, mothers' outtake dominates. The fathers use around 9 percent of the total parental leave during a child's first year, and have used around 17 percent when the child turns two. There are large differences in fathers' parental leave outtake

[3] The first daddy-month reform that reserved days for the father was implemented in 1995.

[4] The second daddy-month reform occurred at the same time as the reform studied in this paper but was implemented similarly across all municipalities in Sweden. Given the assumption that the daddy month reform affected fathers in different municipalities in the same way, the time fixed effects will net out the impact of the daddy month reform from the estimate of the effect of access to childcare and the simultaneity of the two reforms will not matter (see section 4 for further discussion).

and around 25 percent of the fathers have not used any leave at all during the child's first two years. Around 12 percent of families in Sweden have a gender neutral parental leave outtake, where both parents use between 40-60 percent of the total number of days (Dahl 2014). Moreover, both mothers and fathers use less leave with the second born child compared to the first born. This is most likely due to changes in economic circumstances when a family grows and that younger siblings usually starts pre-school at a lower age (Dahl 2014).

3.1 The childcare reform

Since 1995, Swedish municipalities are obligated to offer a spot in pre-school to children whose parents are either working or studying. The decision whether or not to offer a spot in pre-school to children of unemployed parents or parents on parental leave was however until 2002 decided locally in each municipality. In the end of the 1990's only one in four municipalities allowed children of parents on parental leave to remain in pre-school. As part of the many steps taken by the government to make childcare a part of the educational system, several new policies were implemented under a Swedish childcare reform called *Maxtaxa och allmän förskola m m* in order to make public childcare available to all children. The reform was introduced between 2001 and 2003 and consisted of four parts. The first part, implemented in July 2001, made it mandatory for municipalities to offer childcare to children of unemployed parents. The second part, introduced in January 2002, introduced a cap on childcare prices. The final part of the reform was implemented in January 2003 and introduced universal free childcare to all four- and five-year-old children. The reform analyzed in this study is the third part of the reform which, as of January 1st 2002, gave children of parents who were on parental leave with a younger sibling the right to a pre-school spot for at least 15 hours a week for the older kid. Since this part of the reform was implemented simultaneously to a drop in childcare prices resulting from the second part of the "Maxtaxa"-reform the effects of increased availability of childcare for the older sibling can be confounded by the reduction in childcare prices. Not only were parents on parental leave with an older sibling able to keep their child in pre-school after the reform, but it also became cheaper after January 1 2002. However, since childcare prices were reduced in all municipalities at the same point in time, the effect of the reduction in childcare prices can be controlled for by including time fixed effects in the estimations if we assume that the level of the price reduction was uncorrelated with the availability of childcare prior to the reform. To address this I also present estimates of the effect of the reform where I control for childcare prices before and after the reform in a robustness analysis (see section 6.3).

Access to childcare for children to parents on parental leave does not necessarily imply that more children attended daycare since pre-school is not

mandatory; it only gave parents the possibility to keep the older child in pre-school.[5] However, the Swedish National Agency for Education (NAE) concludes that the reform led to more frequent participation in pre-school of children with parents on parental leave. In 1999 26 percent of all 1-5 year olds with parents on parental leave attended pre-school, whereas in 2002 the share was 47 percent. NAE also concludes that the share of 1-5 year olds with a parent on parental leave who were at home with the parent decreased from 70 percent to 48 percent between the years 1999 and 2002. (Swedish National Agency for Education 2002)

In the spring of 2001, NAE conducted surveys among all Swedish municipalities to document the availability of childcare. Among several questions, they asked whether parents who already had a child in pre-school could keep their spot if the parents went on parental leave. By grouping the municipalities according to the answer to this question I construct a treatment group of municipalities; those that did not offer childcare prior to the reform, and a control group; those that already before the reform offered childcare to children of parents on parental leave. In some municipalities before the reform the older sibling could remain in pre-school, but only for a limited number of months. If the number of months was restricted to three months or less I group the municipality as belonging to the treatment group, and as control group otherwise. The amount of hours per week that the child is allowed to remain in pre-school also differs across municipalities both before and after the reform. Most common after the reform is that the child can stay for at most 15 hours per week, but in some municipalities the child can stay for between 20 and 30 hours per week. The grouping in treatment and control only considers whether the child could keep his or her spot at all. Eight municipalities are dropped as they did not answer the survey. Table 1 lists the number of municipalities in each category. Figure A1 in the Appendix shows a map over Sweden and how treatment and control regions are located.

Table 1. *Municipality groups*

Treatment group (Childcare was not available before reform)	204
Control Group (Childcare was available before the reform)	77
No answer	8

Source: Swedish National Agency for Education (NAE).

[5]Recall that since childcare is heavily subsidized in Sweden compared to many other countries, keeping an older sibling in childcare during parental leave is less of a financial strain for the family.

4 Empirical strategy

There are several methodological challenges in assessing the effects of access to pre-school on the division of parental leave. First and foremost, there could be a selection problem. If parents that are more concerned with a gender neutral parental leave outtake request for the older sibling to remain in pre-school to a higher extent than other parents, any differences found would potentially be the result of selection of certain types of parents into pre-school. This implies that the direction of causality between childcare and gender neutral parental leave cannot be distinguished. Another problem is that there is no available individual data on pre-school attendance. Ideally, one would like to estimate the effect of pre-school attendance of the older sibling on the division of parental leave for the younger sibling.

I utilize the pre-school reform in January 2002 to address these methodological challenges. In some municipalities before the reform, there was no possibility to select into pre-school as children of parents on parental leave were not able to keep their spot. The identification strategy exploits the fact that the reform, although implemented at the same point in time throughout the country, had different implications for different municipalities since some offered childcare already prior to the reform. This heterogeneity in the implications of the reform will be used in a difference-in-differences setting. I use the location of where the family lives and the timing of the birth of the second child as determinants for whether the older sibling had access to childcare or not. Because I have no individual level data on which children attends pre-school, I instead estimate a reduced form effect. In order to draw causal conclusions from the difference-in-differences estimation, we must assume that treatment is exogenous against other trends in the municipalities. The composition of individuals is assumed to remain unchanged before and after the reform. The identification strategy relies on the assumption that trends in the outcome - conditional on observable pre-determined covariates - should be the same for all regions absent of treatment. This assumption is tested in a placebo analysis which investigates whether trends in the outcome were the same in treatment- and control municipalities before the implementation of the reform (Angrist and Krueger 1999).

The way that my treatment and control groups are constructed will imply that municipalities in the control group give access to childcare all the time whereas municipalities in the treatment group will supply treatment (i.e. childcare) after the reform date in January 2002. Parents before and after the reform in municipalities where children could keep their spot in pre-school will be compared to parents before and after the reform in municipalities where they could not. The difference-in-differences estimation equation is given by:

$$y_{ist} = \alpha + \lambda_s + \lambda_t + \delta(T_s * d_t) + \mathbf{X}_{ist}\beta' + \varepsilon_{ist} \quad (1)$$

where y_{ist} is the outcome (the division of parental leave) for individual i in municipality s in year t. λ_s is a set of municipality fixed effect included to capture time-invariant differences in parental leave outtake between municipalities. λ_t is a set of year dummy variables controlling for time shocks that commonly influence parental leave outtake in Swedish municipalities. One example of such a shock is the introduction of the second daddy-month reform which reserved an additional month of the parental leave benefit to each parent. However, since the reform was implemented simultaneously throughout the country and given the assumption that this shock affected fathers in different municipalities similarly, the effect of the daddy-month reform will be controlled for by the year-fixed effects. There may however be differences in how fathers reacted to the second daddy-month reform across municipalities that are correlated with the implementation of the childcare reform. If this is true I cannot separate the effect of the second daddy-month reform from the effect of access to childcare. The estimates of the effect of access to childcare in the analysis in this paper would then have to be interpreted as an interaction effect of the two reforms. This would imply that any effects of the reform found in this study could have been different had it not been for the simultaneous implementation of the second daddy-month reform. T_s is an indicator for whether the municipality in which the individual lives changed its access to childcare as a result of the reform or not and d_t is a dummy for post-reform years. δ is the variable of interest and captures the effect of the treatment, which is defined as living in a municipality that did not offer childcare before the reform and having a second child post reform date (i.e. an interaction of a dummy for whether the municipality was affected by the reform or not and a dummy for post-treatment period or not).

Additionally, X_{ist} is a vector of controls for predetermined individual characteristics of the parents and of the children which vary within the municipality. Different characteristics of the family can affect the division of parental leave (see Swedish Social Insurance Agency 2013; Dahl 2014). I will therefore include controls for family characteristics such as age of the parents, age of the older sibling, parental educational level, and whether the parents are married or not. A control for parental leave take-up for the first born child is also included (see section 5.2 for further discussion). I have also included a control for annual municipal unemployment to capture changing economic circumstances within the municipality over time. Furthermore, monthly fixed effects for the timing of the birth of the younger child are included since there may be seasonal effects in the parental leave outtake. Throughout all estimations, the standard errors will be clustered at the municipal level to address the potential within-municipality correlation in estimated standard errors.

Even if the reform can only impact the leave with the second born child, information on the leave with the first born child is available. This gives the opportunity to look at changes between children of the same parents and hence net out unobserved individual parental characteristics. That is, unobserved

differences in e.g. fathers' tendency to take parental leave can be controlled for. Note that this does not contribute to the identification of the treatment effect but may contribute to the precision. In the analyses, controls for parental leave with the first born child will be included to capture unobservable family characteristics.

5 Data and parental leave measurements

5.1 Data

The data used in this study resides from several data registers. Using the multi-generational register, family members are identified. The register covers all individuals born in Sweden and links individuals to their biological mother and father. The register also contains information on year and month of birth. In this way, older siblings with the same biological mother and/or father can be identified. Based on this information, a sample is created consisting of parents who had their second child between January 1998 and March 2005[6], and where an older sibling was in pre-school age (1-5 years old) at the time of the birth of the infant. Observations that cannot be linked to an older sibling, observations where the biological parents differ or where birth order of children differ between parents, and twins are excluded from the analysis.[7]

The data on the families is matched with the population register (called Louise) which contains annual individual level data on background variables such as educational attainment and annual labor income as well as demographic variables such as age and municipality of residence. Most of the parental characteristics will be measured using values of the variable in the year of birth of the second born child; this includes parental education, whether they are married or not, age, and country of birth. Income is measured the year prior to the birth of the second born child since most mothers use the first part of the leave and therefore have reduced income the same year as the birth of the child.

Data on parental leave take-up resides from the National Social Insurance Agency and includes information on the number of calendar days that parental leave benefit was lifted for each child and parent. Since the interest of the

[6] I unfortunately only have access data on the personal identifier for children born up onto April 1st 2005 in the multi-generational register, which is required in order to be able to match the parental leave data with each child. To compensate for any seasonal effects that may result from including children born only in the first quarter of 2005 I will include monthly fixed effects in the estimations.

[7] According to the multigenerational register there are 286 326 second born children born between January 1998 and march 2005. Approximately 44 000 are not the father's second born child and are therefore dropped. An additional 12 000 observations are dropped since they cannot be matched with their older sibling. Finally, 20 000 observations are dropped because the older sibling is more than five years old at the time of the birth of the infant.

paper is the division of parental leave during the first period of the infant's life, I only consider parental leave outtake during the infant's first two years. This is firstly because parental leave usually refers to the time that parents stay at home with a child before it starts pre-school (which usually happens at the age of 1-2 years), and secondly because leave that is lifted when the child is older than two is usually used to extend holidays and vacations and therefore has little implications for the gender neutrality of the care of the child (Dahl 2014). Total parental leave for each parent is measured by adding the number of calendar days that they lifted the benefit respectively during a two year period after the birth of the child. As mentioned earlier, parental leave can be extended by lifting the benefit for only parts of day but I will only consider the number of calendar days that any benefit was registered. The three sources of information can be linked on an individual level, since all Swedish residents have a unique identity number that defines them in all contacts with the authorities.

In the main analysis families where both parents are born outside of Sweden are excluded since these families are less likely to have a gender equal division (Swedish Social Insurance Agency 2013). Because very little is known about the driving forces behind the division of paid parental leave between mothers and fathers, this paper will focus on a more homogeneous sample of parents as a starting point to investigate any potential effects of the reform. Results of estimations where families with immigrant background are included can be found in Appendix.

Descriptive statistics of the families included in the analyses are found in Table 2. Means and standard deviations (the latter in parenthesis) are reported for treatment and control municipalities, before and after the implementation of the reform. The final column shows the difference-in-differences on the characteristics of the families. All covariates except for age of older sibling and maternal education are balanced between treatment and control. The significant difference, although small, found in the age of the older sibling and on maternal education shows the importance of controlling for these covariates in the regressions. Separate analyses depending on age difference of the siblings and on maternal education will also be performed in a sub-group analysis.

Table 2. *Descriptive statistics of parental and child characteristics in treatment and control groups before and after reform*

	Treatment		Control		All
	(1)	(2)	(3)	(4)	(5)
	Pre2001	Post2001	Pre2001	Post2001	DD
Mother's age	29.83	30.60	30.84	31.63	-0.008
	(4.004)	(4.050)	(4.143)	(4.107)	(0.043)
Father's age	32.03	32.70	32.87	33.58	-0.029
	(4.484)	(4.473)	(4.651)	(4.600)	(0.050)
Age of sibling	2.662	2.654	2.663	2.622	0.032***
	(0.865)	(0.895)	(0.870)	(0.876)	(0.012)
Mother w. high school educ. (%)	56.69	45.85	47.17	37.38	-0.011**
	(49.55)	(49.83)	(49.92)	(48.38)	(0.005)
Father w. high school educ. (%)	58.22	51.22	47.51	39.88	0.005
	(49.32)	(49.99)	(49.94)	(48.97)	(0.005)
Mother w. university educ. (%)	36.32	40.53	46.31	49.76	0.009*
	(48.09)	(49.10)	(49.86)	(50.00)	(0.005)
Father w. university educ. (%)	31.49	33.04	43.62	45.32	-0.000
	(46.45)	(47.04)	(49.59)	(49.78)	(0.005)
Married (%)	47.07	43.28	51.91	48.35	-0.002
	(49.91)	(49.55)	(49.96)	(49.97)	(0.005)
Father's income (thousands SEK)	235.5	256.0	261.7	285.0	-2.854
	(146.7)	(168.1)	(217.2)	(240.3)	(2.742)
Mother's income (thousands SEK)	106.2	120.8	118.1	134.2	-1.432
	(90.24)	(99.20)	(111.3)	(123.4)	(1.430)
Municipal unemp. (%)	4.237	3.622	4.238	3.822	-0.002
	(1.571)	(1.180)	(1.537)	(1.033)	(0.001)
Observations	42876	40005	34981	33517	151332

Note: Means of variables in the used data set. Standard errors in parenthesis. DD estimates are from running equation 1 without any controls for predetermined characteristics.* Significant at 10%; ** at 5%; *** at 1%.

5.2 Measuring the division of parental leave

The outcome of interest is the division of parental leave outtake for the second born child while the older sibling is in pre-school age. Division of parental leave can be measured in several ways. One way to investigate whether the reform has an impact on the division of the leave is to focus on the father's parental leave outtake. Since women use the main part of the leave, parental leave policies aimed at increasing gender neutrality in the parental leave have focused on fathers' outtake. If the reform has a positive impact on fathers' parental leave, this most likely implies a more gender neutral outtake. Another way to investigate the division of parental leave is to look at whether the father's share of the total number of parental leave days is affected. This implies including the mother's parental leave in the outcome. As a first outcome I use the sum of father's parental leave days during the second child's first two years, $D_{F,2}$. In the analyses, fathers' parental leave with his first born child $D_{F,1}$ is included as a control variable to capture unobservable family characteristics. Figure 1 shows the distribution of the father's parental leave days with the first and second born child during the

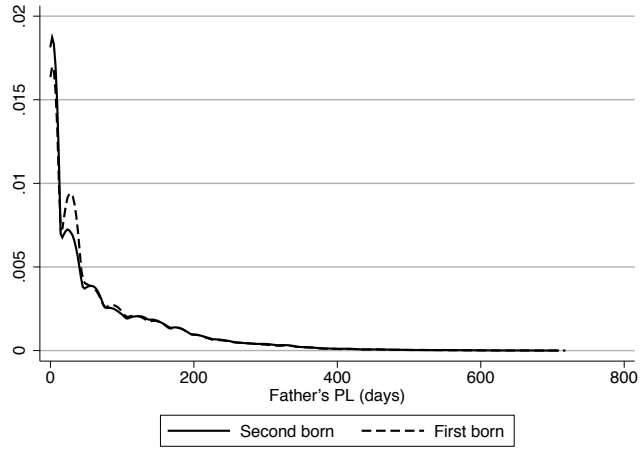

Figure 1. Fathers' parental leave days, first and second child

child's first two years. The figure shows that many fathers take no or very little parental leave, but also that there are some fathers who take a substantial leave.

As a second outcome I use the fathers' share of the total parental leave days during the child's first two years of life, thus relating the father's leave to the mother's. The father's share of the total parental leave days with the second child is calculated as:

$$FS_2 = \frac{D_{F,2}}{D_{M,2} + D_{F,2}} \tag{2}$$

where $D_{M,2}$ is the number of days that the mother used during the first two years after the birth of the second child. Information on the share of the leave with the first born child, FS_1, will be included as a control in the analysis to capture unobservable family characteristics.

Table 3 presents the summary statistics of the parental leave outtake for fathers and mothers with their second born and first born children, and the father's share of the total parental leave with the second born and first born child. The fathers' parental leave outtake is higher in control regions compared to treatment regions. Over time the fathers' share of the leave increases in both treatment and control regions. This is most likely a response to the daddy-month reform implemented January 1st 2002. This is also evident looking at pre- and post means for the number of days used by the father. This consequence of the daddy-month reform will be captured by the year-fixed effects. Graphs of the level of father's parental leave outtake in treatment and control group before and after the implementation of the reform can be found in Appendix, Figures A2 and A3.

Table 3. *Descriptive statistics of parental leave outtake, second and first born child*

	Treatment		Control	
	(1)	(2)	(3)	(4)
	Pre2001	Post2001	Pre2001	Post2001
Father's PL, 2nd child ($D_{F,2}$)	51.78	84.09	58.25	92.61
	(80.72)	(96.74)	(84.74)	(98.02)
Father's share, 2nd child (FS_2)	0.100	0.154	0.115	0.174
	(0.153)	(0.171)	(0.167)	(0.179)
Father's PL, 1st child ($D_{F,1}$)	59.19	78.35	63.80	85.27
	(81.00)	(92.43)	(84.38)	(94.94)
Father's share, 1st child (FS_1)	0.114	0.146	0.126	0.162
	(0.149)	(0.167)	(0.163)	(0.178)
Mother's PL, 2nd child ($D_{M,2}$)	450.9	441.2	441.6	429.4
	(122.5)	(125.6)	(126.4)	(127.5)
Mother's PL, 1st child ($D_{M,1}$)	443.2	443.8	434.5	432.4
	(115.0)	(125.2)	(120.0)	(130.0)
Observations	42876	40005	34981	33517

5.3 Measuring parental health

Parental health will be measured as the number of days on sick leave absence during the first year after the child is born. Data on parental sick leave resides from National Social Insurance Agency and contains information on dates and the number of days on sick leave benefits for the Swedish population. If a person becomes sick while being on parental leave he/she has to report sick to the Social Insurance Agency in order for the other parent to be able to care for the child and receive benefit.[8] As opposed to when a person becomes sick while working, there is no period of sick pay when a person is on parental leave.[9] After a first unpaid day, sick leave benefit is paid by the Social Insurance Agency. Hence, sickness absence during parental leave is likely reported to the authorities at an early stage and also shorter sickness spells will be visible in the data. It should be noted however that sick leave absence during parental leave is only a proxy for parental health. Many sickness episodes are probably not reported and only illness that makes the caring parent unable to care for the infant will be captured. The father's and the mother's number of days on sick leave will be used as outcomes in separate analyses. I will include the number of days on sick leave during the first year after the birth of the first born child as a control variable for the mothers and fathers respectively. Descriptive statistics of days on sick leave benefit during

[8] If the child is below eight months of age, the other parent can use parental leave days to stay at home with the child if the main caretaker reports sick. Once the child has turned 8 months old, temporary parental benefit can be used.

[9] In Sweden, employers are obligated to pay sick pay to employees who cannot work due to illness for the first 14 days. As of the 15th sick day, the employee can instead receive sickness benefits from the Social Insurance Agency.

Table 4. *Descriptive statistics of days on sick leave during second born child's first year of life*

	Treatment		Control	
	(1)	(2)	(3)	(4)
	Pre2001	Post2001	Pre2001	Post2001
Sickdays Mother	3.459	4.255	3.187	3.712
	(17.43)	(20.73)	(17.16)	(19.54)
Sickdays Father	3.401	3.250	2.958	2.918
	(20.16)	(20.09)	(18.88)	(19.92)
Share of mothers ever sick	0.165	0.178	0.144	0.152
	(0.371)	(0.383)	(0.351)	(0.359)
Share of fathers ever sick	0.0641	0.0558	0.0566	0.0483
	(0.245)	(0.230)	(0.231)	(0.214)
Observations	42876	40005	34981	33517

the second born child's first year of life can be found in Table 4. There are no apparent differences between treatment and control municipalities in the number of sick days for parents. The share of mothers that are ever sick is somewhat higher in treatment municipalities, and the share of mothers ever sick is generally three times higher than the share of fathers ever sick.

6 Results

This section presents the regression results of the effect of childcare availability on the different parental outcomes. First, the results of the difference-in-difference analysis on the father's parental leave outtake are presented, followed by the results of the effect on the division of the leave between mothers and fathers. In section 6.3, a sensitivity analysis is conducted by estimating placebo regressions as well as investigating whether the reform impacts fertility decisions of families. In section 6.4, a summary of the results from heterogeneity analyses is presented. Finally, section 6.5 presents the estimates of the difference-in-differences analysis on the effect of the reform on parental health.

6.1 Father's leave

Table 5 reports the results of the difference-in-differences models using the fathers' parental leave outtake as outcome. Controls for parental and child characteristics are included in all estimations: parental education, age of parents and of older sibling at the time of the birth of the second born child, birth month dummy variables for the second born child, and a control for municipal

unemployment. The first two columns of Table 5 use the full sample. Both estimates are negative suggesting that access to childcare may decrease the father's parental leave take-up. None of the estimates are however statistically significant. When fathers' tendency to take parental leave also is considered in the model, the estimate is closer to zero. The point estimate of paternal leave with the first child is positive suggesting that there is a positive correlation between the leave with the first and the second child.

Although the reform was intended to be implemented on January 1st 2002, I have limited information on the implementation process as I rely on survey data prior to the reform to create treatment and control groups. Since children are born throughout the year, I have families in the treatment group that are potentially both treated and untreated. If for example the younger child is born in October 2001 and the parents are on parental leave with this child for a year, the parental leave spell overlaps both pre and post reform periods, but is categorized as only untreated in my data. Furthermore, it could be that childcare centers knowing that they shortly will be obligated to care for the older sibling, allows the child to stay already before implementation of the reform. In an attempt to deal with this problem, I have re-estimated the model on a subset of the sample where I exclude families where the younger child is born between July and December 2001. Throughout, this sample will be referred to as the one without unclearly treated children. The estimates of the reform effects on father's parental leave outtake using this subsample are found in columns 3 and 4. Compared to Columns 1 and 2, the estimates of the effect of the reform are smaller and again not statistically significant.

The size of the point estimates of the effect of the reform are small. If it were to be interpreted, the estimate of -0.64 in Column 4 would suggest that access to childcare during parental leave reduces the father's parental leave outtake by a little more than a half day. Given the average of 68.8 days of paternal leave with the second born child in the sample, the estimate would correspond to a reduction in paternal leave days by a little less than 1 percent. The lower bound of the point estimate is -3.17 which would correspond to a reduction in paternal leave by 3.2 days or by 4.6 percent. Taken together, the results in Table 5 give no (clear) evidence of access to childcare during parental leave having an impact on the father's parental leave outtake.

Table 5. *Difference-in-differences estimates of the effect of the reform on father's parental leave outtake*

	Full Sample		Excl. unclear treat.	
	(1)	(2)	(3)	(4)
	$D_{F,2}$	$D_{F,2}$	$D_{F,2}$	$D_{F,2}$
Treatment	-1.900	-1.067	-1.474	-0.636
	(1.332)	(1.147)	(1.495)	(1.289)
$D_{F,1}$		0.431***		0.430***
		(0.005)		(0.005)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	139971	136719	131470	128366

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates of the effect of access to childcare for the older sibling during parental leave on the father's parental leave outtake with the younger child. The father's parental leave outtake is measured by the number of days on parental leave during the child's first two years of life, $D_{F,2}$. A control for the father's leave with the first born child is included in column 2 and 4, $D_{F,1}$. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment. In columns 3 and 4, children born just prior to the implementation of the reform are excluded.

6.2 Division of the leave

The main research question posed in this paper is whether access to childcare during parental leave affects the division of the leave. Although the previous analysis shows no clear evidence of the reform having an impact on father's take-up, it may still have affected the division between the parents. I therefore turn to the second outcome looking at the division of the parental leave. Table 6 presents the estimates of the difference-in-differences estimations using the father's share of the total leave with the second born child (FS_2) as outcome variable. Again, I have estimated the model described in equation 1 with the full sample (column 1 and 2) as well as with the sample where children with unclear treatment are excluded (column 3 and 4).

The estimates in columns 1 and 3 are negative, suggesting that access to childcare during parental leave decreases the father's share of the total leave with the second born child. The estimates are however small and not statistically significant. In columns 2 and 4 I control for the father's share of the leave with the first born child. This reduces the size of the estimate of the effect of

the reform. If it were to be interpreted, the estimate of -0.001 in the fourth column suggests that the father's share of the leave with the second born child is reduced by 0.1 percentage points. Given an average of father's share of around 13.3 percent this would correspond to a reduction of 0.75 percent which is not much, especially not if it were to be translated into days. Similarly to the results in Table 5, the estimates of the control for the leave with the first child are positive and significant suggesting a positive correlation between fathers' share of the leave with the first child and the second.

In order to create the measure of the division of parental leave, I have incorporated mothers' parental leave in the outcome. If the mother's parental leave is affected by the reform, this will in turn affect the division of the leave without necessarily affecting the father's leave. It may therefore be informative to analyze effects on mothers' parental leave separately. Estimates of the difference-in-differences estimations using the mother's parental leave with the second born child as outcome are found in Appendix, Table A1. None of the estimates are statistically significant. There is no evidence of access to childcare during parental leave having affected mothers' parental leave either. Additionally, Table A2 in Appendix shows that there is no effect of access to childcare on the total number of days on parental leave with the second born child.

To sum up, access to childcare during parental leave for an older sibling seem to have had no impact on the division of the parental leave with the second born child. The difference of the division of parental leave with the second born child before and after the reform for families in treated municipalities is not different from the difference of the division of parental leave in families in the control municipalities, where childcare was available at all times.

Table 6. *Difference-in-differences estimates of the effect of the reform on the division of parental leave*

	Full Sample		Excl. unclear treat.	
	(1)	(2)	(3)	(4)
	FS_2	FS_2	FS_2	FS_2
Treatment	-0.003	-0.002	-0.003	-0.001
	(0.002)	(0.002)	(0.003)	(0.002)
FS_1		0.420***		0.420***
		(0.005)		(0.005)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	139141	135361	130686	127086

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates of the effect of access to childcare for the older sibling during parental leave on the division of parental leave with the younger child. The division is measured by father's share of the total number of days on parental leave with the second born child, FS_2. A control for the father's share of the leave with the first born child is included in column 2 and 4, FS_1. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment. In columns 3 and 4, children born just prior to the implementation of the reform are excluded.

6.3 Robustness

As a way of testing the parallel trends assumption I perform a placebo test where I estimate the model again but this time rolling back the timing of the treatment and use only pre-reform data. The results of the placebo tests where fictitious reforms occur in 2000 or 2001 are presented in Table 7 and Table 8 respectively. Controls for parental and child characteristics are included in all estimations and the full sample is used in all estimations. The first two columns of Tables 7 and 8 use father's days with the second born child as outcome variable whereas the last two columns use the father's share with the second born child as outcome variable. The estimates of the effects of the fictitious reforms are not statistically significant in either of the estimations and they are also closer to zero compared to estimates in Tables 5 and 6. This suggests that the assumption of parallel trends is fulfilled.

As mentioned, access to childcare during parental leave was introduced at the same point in time as a reduction in childcare prices occurred due to an-

other part of the "Maxtaxa"-reform.[10] If the opportunity to keep an older sibling in childcare is uncorrelated with the level of the price change in the municipality, the simultaneity of the reforms will not be a problem. Given that the reduction in prices affected all municipalities, any effects of a price reduction will be netted away by the time fixed effects. There may however be a concern that the reduction in childcare prices is correlated with the opportunity to keep the children in childcare prior to the reform. Municipalities that allowed children of parents on parental leave to stay in childcare prior to the reform may also have been more generous in terms of charging for childcare. This would imply that the price reduction of childcare could be relatively higher in treated municipalities compared to control municipalities after the reform. The estimated effects of the reform would then potentially capture not only the effect of access to childcare but also the effect of a price reduction. If this is the case I would be overestimating the effect of access to childcare since some of the effect may actually be attributed to a price reduction. (Conversely, the effect of access to childcare could be dampened if municipalities that allowed children of parents on parental leave to stay in childcare prior to the reform were less generous in terms of charging for childcare.) Furthermore, the change in prices may also have led to a change in the type of families that put their first child in childcare. Families who regarded childcare as too expensive prior to the reform and therefore cared for their child at home may have placed their child in pre-school when the prices were reduced. If those families that avoided the more expensive childcare have different preferences for division and length of parental leave in general, this may bias the estimates.

In order to check whether the estimates are sensitive to changes in childcare prices, I have estimated the effect of access to childcare controlling for the price level before and after the implementation of the reform. In the estimations I control for the price by including a variable with information on the prices for childcare in each municipality in 1999 (pre-reform) and in 2003 (post-reform).[11] The results of this robustness analysis are found in Table 9. The estimates are somewhat larger (more negative) in all columns, but remain statistically insignificant and of small size. The effect of access to childcare does not seem to be biased by changes in childcare prices.

Moreover, access to childcare during parental leave could potentially affect fertility decisions of parents. If the burden during parental leave is reduced, parents that value this relief highly may be more likely to have a second child.

[10] Recall that childcare prices are subsidized in Sweden. In 2001, prior to the reduction in childcare prices, the cost for childcare was around 10 percent of the net household income for an average family. After the reform, the cost of childcare for an average family was around 4 percent of the net income. (Lundin et al. 2008)

[11] Data on childcare prices are collected by NAE and are given by different types of households. I use prices for households that most closely resemble the families in the analysis, namely those consisting of one child in pre-school age with parents living together, one working full time and the other working part-time, and where both parents have around average income.

If this is true, the composition of families with two children after the reform may be different than it would have been, had it not been for the availability of childcare. This would violate the assumption that the composition of parents in treatment and control groups remains unchanged over time. In order to test whether the reform impacts fertility decisions I have estimated the difference-in-differences regression using the probability of having a second child within two years or within three years after the first child as outcome variables. The results of these estimations can be found in Table A12 in Appendix. There is no evidence of the reform affecting fertility decisions of the parents; neither of the estimates of the effect of access to childcare on the probability of having a second child within two or three years are statistically significant.

Table 7. *Placebo estimates of the effect of the reform on father's parental leave outtake, fictitious reform in 2000*

	(1)	(2)	(3)	(4)
	$D_{F,2}$	$D_{F,2}$	FS_2	FS_2
Treatment in 2000	-1.440	-1.024	-0.00326	-0.00249
	(1.168)	(1.139)	(0.00223)	(0.00220)
$D_{F,1}$		0.415***		
		(0.00720)		
FS_1				0.421***
				(0.00796)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	72568	70318	72190	69706

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the placebo difference-in-differences estimates. The father's parental leave outtake is measured using two outcomes, $D_{F,2}$, and FS_2. A control for the father's take-up and his share of the leave with the first born child is included in column 2 and 4, $D_{F,1}$, and FS_1 respectively. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment.

Table 8. *Placebo estimates of the effect of the reform on father's parental leave outtake, fictitious reform in 2001*

	(1) $D_{F,2}$	(2) $D_{F,2}$	(3) FS_2	(4) FS_2
Treatment in 2001	0.992	0.240	0.00166	0.000870
	(1.342)	(1.249)	(0.00276)	(0.00281)
$D_{F,1}$		0.415***		
		(0.00720)		
FS_1				0.421***
				(0.00796)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	72568	70318	72190	69706

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the placebo difference-in-differences estimates. The father's parental leave outtake is measured using two outcomes, $D_{F,2}$, and FS_2. A control for the father's take-up and his share of the leave with the first born child is included in column 2 and 4, $D_{F,1}$, and FS_1 respectively. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment.

Table 9. *Difference-in-differences estimates of the effect of the reform controlling for childcare prices*

	Full Sample		Excl. unclear treat.	
	(1)	(2)	(3)	(4)
	$D_{F,2}$	FS_2	$D_{F,2}$	FS_2
Treatment	-1.234	-0.003	-0.817	-0.002
	(1.123)	(0.002)	(1.259)	(0.002)
Childcare price	-0.003	-0.000**	-0.003*	-0.000**
	(0.002)	(0.000)	(0.002)	(0.000)
$D_{F,1}$	0.430***		0.430***	
	(0.005)		(0.005)	
FS_1		0.420***		0.420***
		(0.005)		(0.005)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	136574	135217	128234	126954

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates of the effect of access to childcare for the older sibling during parental leave on the father's parental leave outtake with the younger child. The father's parental leave outtake is measured using two outcomes, $D_{F,2}$, and FS_2. Controls for the father's leave with the first born child is included in all estimations. Included in all estimations is also a control for the price level in childcare in each municipality before and after the reform. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment. In columns 3 and 4, children born just prior to the implementation of the reform are excluded.

6.4 Sub-group analyses

Since there may be heterogeneity in the treatment effect, I have analyzed different sub-samples of the population. It has been suggested that certain parental characteristics are associated with differences in the tendency to divide the leave equally (Swedish Social Insurance Agency 2013). Equal division between parents is more common where both the mother and father have high income and are highly educated (Dahl 2014). The age of the older sibling could also matter: older siblings may have had more time to settle in in daycare the older they are, and it may thus be more burdensome to keep these children at home. Furthermore, pre-school attendance is likely to be higher the older the sibling is. There may also be differences in the effect of the reform among municipalities in city regions compared to rural municipalities. Finally,

there are differences in the amount of hours per week that the older siblings is allowed to stay in childcare. In most municipalities the older sibling may only stay for 15 hours per week whereas in other municipalities they may stay longer. The effect of the reform can therefore be more pronounced in regions where the sibling is allowed to stay for larger parts of the day. The results of the heterogeneity analysis are found in Appendix. All but one estimate are statistically not significant and should therefore be interpreted cautiously as they are imprecisely measured. Taking point estimates at face value may however provide some information on differences in impact between different types of families and regions.

The point estimates of the treatment effect on the division of the leave are close to zero in all estimations in Tables A3-A9 in Appendix. Point estimates of the father's parental leave are positive for families where the mother or the father has university education (Tables A3 and A4) and where the mother or the father has an income above the median in the sample (Tables A5 and A6). Since equal division is more common among both high earning and highly educated parents, this is what might be expected. The estimates are however small and and not statistically significant. The point estimate of the father's parental leave is positive for families where the older sibling is older than 2.5 years of age at time of the birth of the younger sibling, and negative for families with a young older sibling (Table A7). In the latter group, some parents may still have parental leave days left with the first born child and pre-school attendance is likely to be lower, which would imply a weaker first stage (keeping child in childcare due to the reform) for this group. Again, the estimates are not statistically significant. The point estimate is positive (and larger) for families living in a non-city region, and statistically significant for the division of the leave (Table A8). However, since the estimate is only weakly significant, it is difficult to draw conclusions from this result. Finally, the estimate of the father's parental leave is positive for municipalities where the children can stay for larger parts of the day and negative where they can only stay for 15 hours per week (Table A9).

6.5 Parental health

Reduced care burden during parental leave may have implications on other outcomes than the utilization of parental leave days. In the following section I present the results for whether access to childcare during parental leave has an effect on the utilization of sick leave insurance. In the sample, the average number of days on sick leave during the infant's first year of life is 3.7 days for mothers and 3.2 days for fathers. Around 16 percent of the mothers and 6 percent of the fathers in the sample use sick leave benefit some time during the child's first year of life. Results of the difference-in-differences estimation on the effect of access to childcare during parental leave on sickness absence

of the mother and the father can be found in Table 10. All estimations include controls for parental and child characteristics as well as a control for the parent's number of days on sick leave during the first year of the first born child's life. This control is positive and significant in all estimations indicating a positive correlation between sickness during parental leave with the first child and the second. None of the estimates of the effect of the reform in Table 10 are statistically significant. Hence I find no evidence of an effect of access to childcare during parental leave on parental health.

Table 10. *Difference-in-differences estimates of the effect of the reform on parental health*

	Full Sample		Excl. unclear treat.	
	(1)	(2)	(3)	(4)
	Sickdays Mother	Sickdays Father	Sickdays Mother	Sickdays Father
Treatment	0.170	-0.127	0.282	-0.132
	(0.245)	(0.189)	(0.239)	(0.185)
Mother Sickdays 1'st	0.206***		0.200***	
	(0.013)		(0.012)	
Father Sickdays 1'st		0.080***		0.081***
		(0.006)		(0.006)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	151332	151332	142499	142499

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates of the effect of access to childcare for the older sibling during parental leave on the health of the mother and the father. Health is measured by the number of days on sick leave benefit during the younger sibling's first year of life. A control for the parent's number of days on sick leave with the first born child is included in all estimations. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment. In columns 3 and 4, children born just prior to the implementation of the reform are excluded.

7 Conclusion

In this paper I have studied whether access to childcare for an older sibling during parental leave affects the father's parental leave outtake and the division of paid parental between mothers and fathers. The effects of a childcare reform in January 2002 that gave children of parents on parental leave with a younger sibling the right to a spot in childcare for an older sibling is evaluated using difference-in-differences. While the reform only affected the leave with the second born child, I still have information on the parental leave with the first born child, and I have therefor investigated whether the reform affects the division of the parental leave controlling for unobserved family characteristics. By controlling for the parental leave outtake with the first born child unobservable family characteristics could be netted away. Focusing on fathers' parental leave, the reform does not seem to have affected their outtake. The estimate

is small, suggesting a reduction in parental leave days by a half day or a reduction by 1 percent, and not statistically significant. Similarly, when turning to whether the reform affected the division of the leave, the estimates of the effect of the reform are small and not statistically significant.

Access to childcare was granted in treated municipalities at the same point in time as the introduction of the second daddy month reform. Given the assumption that fathers were affected similarly by the daddy month reform across municipalities, the difference-in-differences strategy will net out any effects of that reform. It could however be the case that there is an interaction effect between the two reforms. It should therefore be kept in mind when interpreting the results of the analysis that they may have looked different had it not been for the simultaneous implementation of the daddy month reform.

There may be many reasons for why families do not react more strongly to a decreased non-monetary cost of parental leave. Firstly, if the mother and father value the non-monetary cost reduction equally the division between the parents would remain unchanged. Secondly, it could be that families do not care about the aspect of care burden. Other factors, such as gender norms and monetary incentives, may outweigh the impact of a decreased burden. The treatment may therefor not be strong enough. Thirdly, it could be that parents do not realize that access to childcare will imply a decrease in the non-monetary cost of parental leave. Since the reform only comes into play with the second born child, families have no prior experience to compare with. Finally, it could be that access to childcare implies no reduction of the burden during parental leave. Most municipalities have restricted the number of hours that the older sibling can spend in childcare per day and if picking up and dropping off the child interferes with the planning of the day, it need not imply a reduction in care burden.

This paper also analyzes whether access to childcare during parental leave has an effect on mother's and father's number of days on sick leave benefit during the infant's first year of life. I find no evidence of the reform having an effect on sick leave absence during parental leave. Additional reasons for this, apart from the abovementioned reasons, may be that diseases during parental leave requiring the other parent to step in are of a severe type. If this is the case, a reduction in care burden during parts of the day will not matter and access to childcare would therefore not impact the sick leave of either parent. It could also be that having one child in daycare increases the risk of infections which would counteract any positive effects of a reduction in the care burden.

Several measures have been taken by the Swedish Government to increase equal sharing of parental leave between mothers and fathers. Although Swedish fathers have increased their share of the parental leave during the last decades, mothers remain the primary caregiver during a child's first years. It is therefore of importance to investigate what could motivate families to increase equal sharing of paid parental leave. In this paper, I have studied whether decreased burden, or a reduction in non-monetary costs of parental

leave, affects the division of paid parental leave. The results from the analyses give no evidence of access to childcare having an effect on the father's parental leave outtake or on the division of the parental leave between the mother and the father.

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Appendix

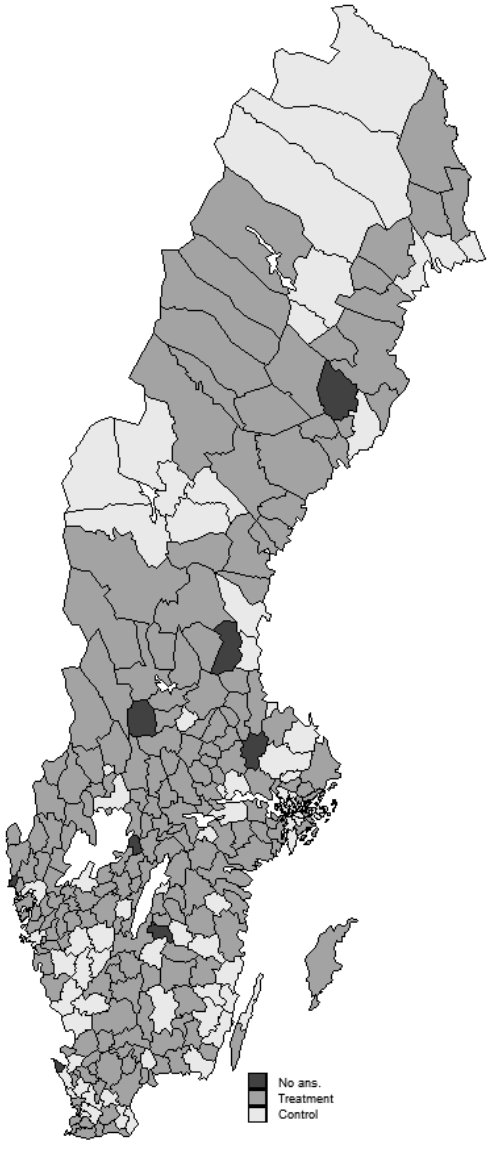

Figure A1. Treatment and control municipalities

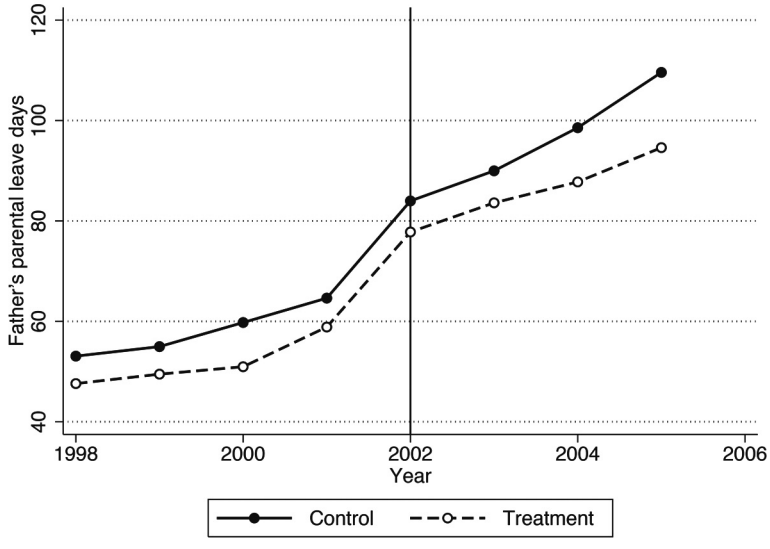

Figure A2. Fathers' parental leave days in treatment and control municipalities before and after the reform

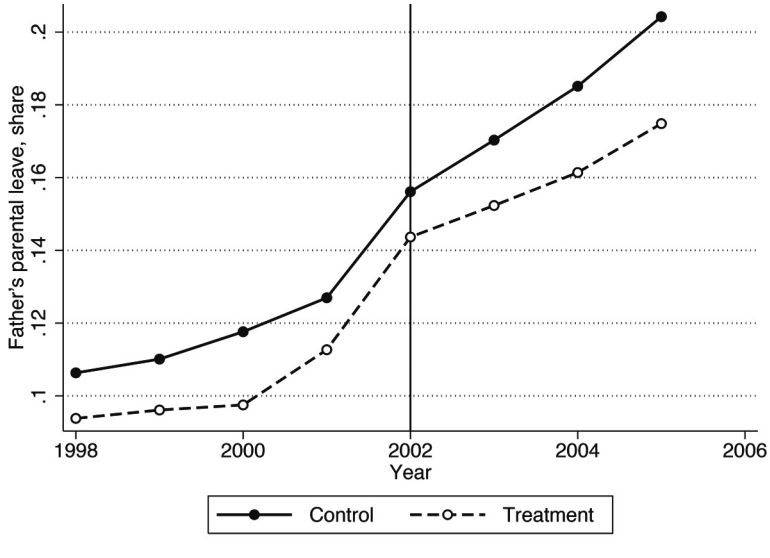

Figure A3. Fathers' share of parental leave days in treatment and control municipalities before and after the reform

Table A1. *Difference-in-differences estimates of the effect of the reform, mother's parental leave*

	Full Sample		Excl. unclear treat.	
	(1)	(2)	(3)	(4)
	$D_{M,2}$	$D_{M,2}$	$D_{M,2}$	$D_{M,2}$
Treatment	1.646	0.731	1.560	0.651
	(1.633)	(1.463)	(1.777)	(1.576)
$D_{M,2}$		0.440***		0.440***
		(0.003)		(0.003)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	150575	150180	141532	141152

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates of the effect of access to childcare for the older sibling during parental leave on the mother's parental leave outtake with the younger child. The mother's parental leave outtake is measured by the number of days on parental leave during the child's first two years of life, $D_{M,2}$. A control for the father's leave with the first born child is included in column 2 and 4, $D_{M,1}$. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment. In columns 3 and 4, children born just prior to the implementation of the reform are excluded.

Table A2. *Difference-in-differences estimates of the effect of the reform, total parental leave*

	Full Sample		Excl. unclear treat.	
	(1)	(2)	(3)	(4)
	Total Days 2'nd	Total Days 2'nd	Total Days 2'nd	Total Days 2'nd
Treatment	-0.423	-0.070	-0.304	0.028
	(1.744)	(1.662)	(1.773)	(1.675)
Total Days 1'st		0.386***		0.387***
		(0.005)		(0.004)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	143814	136102	135100	127770

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates of the effect of access to childcare for the older sibling during parental leave on the total parental leave outtake of mother and father with the younger child. The total parental leave outtake is measured by adding the number of days on parental leave during the child's first two years of life of the mother and father, $D_{F,2} + D_{M,2}$. A control for the total leave with the first born child is included in column 2 and 4, $D_{F,1} + D_{M,1}$. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment. In columns 3 and 4, children born just prior to the implementation of the reform are excluded.

Table A3. *Difference-in-differences estimates of the effect of the reform, splitting the sample according to mother's educational level*

	At most highschool		University	
	(1)	(2)	(3)	(4)
	$D_{F,2}$	FS_2	$D_{F,2}$	FS_2
Treatment	-0.041	0.001	1.669	0.001
	(1.427)	(0.003)	(1.355)	(0.003)
$D_{F,1}$	0.432***		0.424***	
	(0.007)		(0.006)	
FS_1		0.433***		0.402***
		(0.008)		(0.007)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	67262	66704	55518	54895

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates splitting the sample according to mothers' educational level. The father's parental leave outtake is measured using two outcomes, $D_{F,2}$, and FS_2. A control for the father's take-up and his share of the leave with the first born child is included in column 2 and 4, $D_{F,1}$, and FS_1 respectively. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment.

Table A4. *Difference-in-differences estimates of the effect of the reform, splitting the sample according to father's educational level*

	At most highschool		University	
	(1)	(2)	(3)	(4)
	$D_{F,2}$	FS_2	$D_{F,2}$	FS_2
Treatment	-0.081	0.001	1.889	0.001
	(1.281)	(0.002)	(1.611)	(0.003)
$D_{F,1}$	0.422***		0.436***	
	(0.006)		(0.007)	
FS_1		0.419***		0.414***
		(0.007)		(0.008)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	74081	73546	48668	48023

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates splitting the sample according to fathers' educational level. The father's parental leave outtake is measured using two outcomes, $D_{F,2}$, and FS_2. A control for the father's take-up and his share of the leave with the first born child is included in column 2 and 4, $D_{F,1}$, and FS_1 respectively. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment.

Table A5. *Difference-in-differences estimates of the effect of the reform, splitting the sample according to father's income level*

	Below median		Above Median	
	(1)	(2)	(3)	(4)
	$D_{F,2}$	FS_2	$D_{F,2}$	FS_2
Treatment	0.508	0.003	-0.490	-0.001
	(1.742)	(0.003)	(1.510)	(0.003)
$D_{F,1}$	0.414***		0.453***	
	(0.007)		(0.007)	
FS_1		0.401***		0.440***
		(0.007)		(0.008)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	51589	50995	60267	59813

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates splitting the sample according to fathers' income level in the sample. The father's parental leave outtake is measured using two outcomes, $D_{F,2}$, and FS_2. A control for the father's take-up and his share of the leave with the first born child is included in column 2 and 4, $D_{F,1}$, and FS_1 respectively. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment.

Table A6. *Difference-in-differences estimates of the effect of the reform, splitting the sample according to mother's income level*

	Below median		Above median	
	(1)	(2)	(3)	(4)
	$D_{F,2}$	FS_2	$D_{F,2}$	FS_2
Treatment	0.459	0.004	0.077	-0.001
	(1.529)	(0.003)	(1.474)	(0.003)
$D_{F,1}$	0.477***		0.397***	
	(0.007)		(0.007)	
FS_1		0.492***		0.361***
		(0.008)		(0.009)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	50528	49769	61386	61104

Note: Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates splitting the sample according to mothers' income level in the sample. The father's parental leave outtake is measured using two outcomes, $D_{F,2}$, and FS_2. A control for the father's take-up and his share of the leave with the first born child is included in column 2 and 4, $D_{F,1}$, and FS_1 respectively. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment.

Table A7. *Difference-in-differences estimates of the effect of the reform, splitting the sample according to age difference between siblings*

	Young Sibling		Older Sibling	
	(1)	(2)	(3)	(4)
	$D_{F,2}$	FS_2	$D_{F,2}$	FS_2
Treatment	-2.181	-0.004	0.948	0.002
	(1.672)	(0.003)	(1.642)	(0.003)
$D_{F,1}$	0.475***		0.384***	
	(0.006)		(0.007)	
FS_1		0.484***		0.360***
		(0.006)		(0.007)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	63737	63177	64629	63909

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates splitting the sample according to the age difference of siblings. Older siblings are at least 2.5 years older and young sibling less than 2.5 years older. The father's parental leave outtake is measured using two outcomes, $D_{F,2}$, and FS_2. A control for the father's take-up and his share of the leave with the first born child is included in column 2 and 4, $D_{F,1}$, and FS_1 respectively. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment.

Table A8. *Difference-in-differences estimates of the effect of the reform, splitting the sample according to type of municipality*

	City Region		Not City Region	
	(1)	(2)	(3)	(4)
	$D_{F,2}$	FS_2	$D_{F,2}$	FS_2
Treatment	-0.582	-0.002	2.883	0.006*
	(1.367)	(0.003)	(1.842)	(0.003)
$D_{F,1}$	0.431***		0.427***	
	(0.006)		(0.008)	
FS_1		0.420***		0.419***
		(0.006)		(0.009)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	82850	81955	45516	45131

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates splitting the sample according to the type of municipality that the family lives in. Municipalities are categorized into different types by the Swedish Association of Local Authorities and Regions (SKL). The father's parental leave outtake is measured using two outcomes, $D_{F,2}$, and FS_2. A control for the father's take-up and his share of the leave with the first born child is included in column 2 and 4, $D_{F,1}$, and FS_1 respectively. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment.

Table A9. *Difference-in-differences estimates of the effect of the reform, splitting the sample according to whether the child could stay for 15 or 30 hours per week*

	Stay short		Stay long	
	(1)	(2)	(3)	(4)
	$D_{F,2}$	FS_2	$D_{F,2}$	FS_2
Treatment	-0.428	-0.00179	1.038	0.00515
	(1.559)	(0.00287)	(1.814)	(0.00366)
$D_{F,2}$	0.429***		0.434***	
	(0.00512)		(0.0124)	
FS_1		0.417***		0.428***
		(0.00531)		(0.00924)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	94692	93845	30450	30046

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates splitting the sample according to whether the older sibling could stay for more than 15 hours per week or not. The father's parental leave outtake is measured using two outcomes, $D_{F,2}$, and FS_2. A control for the father's take-up and his share of the leave with the first born child is included in column 2 and 4, $D_{F,1}$, and FS_1 respectively. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment.

Table A10. *Difference-in-differences estimates of the effect of the reform on father's outtake, including immigrated parents*

	Full Sample		Excl. unclear treat.	
	(1)	(2)	(3)	(4)
	$D_{F,2}$	$D_{F,2}$	$D_{F,2}$	$D_{F,2}$
Treatment	-1.455	-0.992	-1.008	-0.541
	(1.178)	(1.011)	(1.282)	(1.106)
$D_{F,1}$		0.423***		0.423***
		(0.004)		(0.005)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	151996	147406	142691	138337

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates of the effect of access to childcare for the older sibling during parental leave on the father's parental leave outtake with the younger child. The father's parental leave outtake is measured by the number of days on parental leave during the child's first two years of life, $D_{F,2}$. A control for the father's leave with the first born child is included in column 2 and 4, $D_{F,1}$. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment. In columns 3 and 4, children born just prior to the implementation of the reform are excluded.

Table A11. *Difference-in-differences estimates of the effect of the reform on division, including immigrated parents*

	Full Sample		Excl. unclear treat.	
	(1)	(2)	(3)	(4)
	FS_2	FS_2	FS_2	FS_2
Treatment	-0.003	-0.002	-0.002	-0.001
	(0.002)	(0.002)	(0.003)	(0.002)
FS_1		0.410***		0.411***
		(0.005)		(0.005)
Controls	Yes	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Observations	150740	145030	141506	136103

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates of the effect of access to childcare for the older sibling during parental leave on the division of parental leave with the younger child. The division is measured by father's share of the total number of days on parental leave with the second born child, FS_2. A control for the father's share of the leave with the first born child is included in column 2 and 4, FS_1. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the second born child, are included: mother's and father's education, the age of the mother and father, whether they are married or not, age of older sibling, birth month fixed effects for the second born child, and a control for municipal unemployment. In columns 3 and 4, children born just prior to the implementation of the reform are excluded.

Fertility

This section presents the regression results of the effect of childcare availability on the probability of having a second child within two years or within three years after the birth of the first child. These analyses use a sample of first born children born between 1998 and 1999 and between 2002 and 2003. These first born children and their parents are matched with a younger sibling, if they have one. The outcome variables are dummy variables taking the value 1 of the first born child has a sibling born within two years or within three years, and 0 otherwise. In the main analyses of this paper, treatment status of the families depends on the timing of the birth of the second child. Since we can no longer use the timing of the birth of the second child, the analyses presented here assume that the parental leave with younger siblings born within two or three years after the first born children born between 1998 and 1999 will take place before the implementation of the reform. Parental leave with younger sibling of first born children born between 2002 and 2003 will

however take place after the implementation of the reform. We can therefore compare these families before and after the implementation of the reform in treatment and control municipalities to investigate whether the reform affects fertility decisions of the families. Since parental leave spells of younger sibling of first born children born between 2000 and 2001 may overlap the reform date, these observations are excluded in the analysis. Table 22 presents the difference-in-differences estimates of the effect of the reform. Standard errors, clustered at the municipal level, are given in parenthesis. None of the estimates of the effect of the reform are statistically significant, suggesting that the reform has no effect on fertility decisions in this sample.

Table A12. *Difference-in-differences estimates of the effect of the reform on timing of second child*

	98-99 vs. 02-03		98 vs. 02
	(1)	(2)	(3)
	Within 3 yrs	Within 2 yrs	Within 3 yrs
Treatment	-0.003	0.003	
	(0.007)	(0.004)	
Treatment			-0.008
			(0.010)
Controls	Yes	Yes	Yes
Municipal Dummies	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Observations	121611	121611	60335

Note: Standard errors in parenthesis, clustered at municipality level. * significant at 10%; ** at 5% ; *** at 1%. This table presents the results of the difference-in-differences estimates of the effect of access to childcare for the older sibling during parental leave on fertility decisions of the family. Fertility is measured by a dummy variable taking the value 1 if the family has a second child within two years after the birth of the first child, and 0 otherwise $Within2yrs$, or a dummy variable taking the value one if the family has a second child within three years, and 0 otherwise $Within3yrs$. The model includes municipality fixed effects and year fixed effects. The following controls for parental and child characteristics, measured at the time of birth of the first born child, are included: mother's and father's education, the age of the mother and father, and whether they are married or not.