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The Impact of Reproductive and Birth Technologies on Health, Fertility and Labor Outcomes

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To my beloved husband Otto

and our families

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Contents

Introduction

Access to reproductive and birth technologies

Access to reproductive and birth technologies are essential for human welfare. While family planning dates back to around 1550 BCE (O'Dowd and Philipp, 2000), recent technological innovation has spurred the opportunity to plan and optimize fertility. These innovations consist of a broad range of technologies including modern contraception, induced abortion, assisted conception (here referred to as "reproductive technologies") as well as numerous medical procedures and treatments developed for use during pregnancy and child birth (here referred to as "birth technologies"). Over the last century, the rapid advancement in these innovations, has had an astonishing impact on demographic patterns, increased freedom of choice and economic liberation of women (Bailey and Lindo, 2017). While several reproductive and birth technologies have been available for decades, many women and children lack access, especially in low income countries. Even though great efforts are made in procuring access to reproductive and birth health care by global health institutions, local governments and NGOs, large differences, both across and within countries, still remain (The World Health Organization, 2015).

The aim of this thesis is to assess the impact of access to reproductive and birth technologies on a range of socioeconomic outcomes for women and children. The outcomes of interest, summarizing the topic of this thesis, are fertility, health and behavior in the household and the labor market. The three chapters of the thesis build on the applied economic literature on fertility, health and labor. Despite the rapid progress in the development of reproductive and birth technologies, multiple questions remain unanswered regarding the consequences of these technologies as well as how improved access can decrease the gaps in health between individuals. My research contributes knowledge in this area and in particular on how medical interventions and technologies can contribute to narrowing gaps with the emphasis on the causal- and long-run impacts including both health indicators and labor market outcomes.

Reproductive technologies have contributed to a large worldwide reduction in total fertility from 5 children per woman in the 1960s to 2.5 in 2015 (The World Bank, 2015). The use of modern contraceptives in industrialized countries has increased from only 9% during the 1960s to nearly 60% in the 2000s (Grimes et al., 2006). Nevertheless, there is still a large unmet need of contraception, which contributes to high rates of unintended pregnancies. Globally, unintended pregnancies lead to an estimated 46 million induced abortions each year (Van Lerberghe et al., 2005), many of which are done in clandestine and under unsafe circumstances (Grimes et al., 2006). Legal restrictions are viewed as a barrier to safe abortion (Grimes et al., 2006). Starting from the 1970s and onwards, local- and nation-wide policy changes have increased access to elective abortions worldwide. These reforms have been documented to have substantial impacts on women, children and families (Ananat et al., 2009; Bailey, M. J., 2013; Mitrut and Wolff, 2011; Pop-Eleches, 2005; Pop-Eleches, C., 2010), including impacts on total fertility and fertility timing (Ananat et al., 2007; Clarke, 2017; Gruber et al., 1999; Guldi, 2008; Valente, 2014); women's labor market outcomes (Angrist and Evans, 1996; Mølland, 2016), the composition of children and living conditions (Mitrut and Wolff, 2011; Pop-Eleches, C., 2010), as well as female empowerment (Oreffice, 2007). However, the access to reproductive health rights has not remained unchallenged over the course, which is reflected in multiple efforts in raising barriers to accessing safe abortions (Bitler and Zavodny, 2001; Cunningham et al., 2017; Fischer et al., 2017; Joyce and Kaestner, 1996; Lu and Slusky, 2016).

Reproductive technologies provide women with not only the ability to prevent childbearing, they also assist individuals with involuntary infertility as well as providing women the opportunity to postpone childbearing. An estimated 10-15% of all couples worldwide suffer from involuntary infertility. Assisted reproductive technology (ART) has greatly improved over the last decade with the introduction of in vitro fertilization (IVF). The share of births facilitated by assisted conception through IVF has increased rapidly, now exceeding 3% in several industrialized countries (de Mouzon et al., 2010). Yet research shows adverse perinatal and neonatal outcomes among children born after IVF compared with children born after unassisted conception (Kalra and Barnhart, 2011). Improvements in individual and aggregate health for children born after IVF are therefore paramount and will be magnified as rates of IVF use continue to increase worldwide.

The notion of reproductive health also includes access to care during pregnancy and childbirth for assuring maternal and child health. Despite Cesarean section (C-section) being the most common surgical procedure in industrialized countries, many questions remain regarding the causal and long-term impact on child health and maternal health, future fertility and labor market outcomes. The use of C-section is widely recognized by the medical society as a life saving measure for mother and child for many conditions during pregnancy and birth. However, for low-risk births, C-section may lead to higher morbidity and mortality than vaginal birth (Clark et al., 2008). The global use of C-section has dramatically increased from the 1970s until today, with rates that cannot solely be attributed to demographical changes (Betran et al., 2015) or parental preferences (Kozhimannil et al., 2013), suggesting that supply-side incentives are affecting the usage of C-section (Halla et al., 2016; Kozhimannil et al., 2013), with sub-optimal use in settings such as the US (Currie, J. and MacLeod, W. B., 2013). The implications of C-section are not fully understood. To quote the World Health Organization: “The effects of Cesarean section rates on other outcomes, such as maternal and perinatal morbidity, pediatric outcomes, and psychological or social well-being are still unclear. More research is needed to understand the health effects of Cesarean section on immediate and future outcomes.”

This thesis contributes to a large and growing literature within the bio-medical, sociological, legal and economics fields. Fertility, health and family economics are widely recognized topics within the discipline of economics and date back to the theoretical work on fertility, population and economic development by Thomas Robert Malthus (Heckman, J.J, 2015). A landmark contribution to this field (and to economics in general), is the pioneering work on the economics of fertility and family by Gary Becker, who created a foundation to a significant body of research on human capital, health, and labor (Heckman, J. J., 2015). Alongside, and as an extension of this strand of literature, came a new era of empirical work, facilitated by better access to micro-data and methods (Heckman, J.J, 2015).

Today, the body of literature on fertility, labor market responses, the importance of early life conditions and health for later life outcomes continues to grow (Almond et al., 2017). Indeed, during recent years, the importance of early life investment has been strongly emphasized within economics. Many studies show that early childhood conditions have a strong and long term impact on various socioeconomic outcomes such as cognitive and non-cognitive skills, health, and income (Almond and Currie, 2010; Barker, 1995; Currie and Vogl, 2012; Cutler and Meara, 2000; Heckman, 2007). In particular, improved neonatal care (Almond et al., 2010; Bharadwaj et al., 2013; Daysal, 2015; Daysal et al., 2013) has been shown to have a long-lasting impact on health and educational performance later in life (Almond and Mazumder, 2011). Contributing to this literature, I want to highlight how access to technologies and information regarding fertility and reproductive health affect a large range of outcomes regarding the welfare of women and children directly and by extension, the welfare of all.

Summary of the thesis

My dissertation consists of three independent empirical papers on fertility, child health, and maternal health and labor market responses. The overall contribution of my thesis is to assess the causal and long-run impacts of access to reproductive and birth technologies on a range of socioeconomic outcomes for women, children and families. I use quasi-experimental methods and detailed administrative data that allow for isolating the effects of policy reforms on outcomes, in both high-income and emerging economies. In the first chapter, I examine the causal and long-term impact of an increase in planned C-sections for high-risk births on multiple socioeconomic outcomes including health, future fertility and labor responses. In the second chapter, the impact of an IVF reform mandating single embryo transfer as default procedure providing a negative fertility shock is analyzed. Finally, in the third chapter, we examine the effect from abortion legalization in Mexico City on fertility and female empowerment.

In chapter one, “Cesarean Section for High-Risk Births: Short- and Long-Term Consequences for Breech Births”, I study the causal impact of Cesarean section (C-section) on health, subsequent fertility and labor market outcomes for “at risk” births. This particular high-risk group consists of breech births, where the fetus is presented with its head upward instead of downward. The causal impact of C-section is captured by exploring an information shock to the medical society in Sweden, on the benefits of planned C-section for breech births, which led to an increase in planned C-sections by 23% for this group. By employing a pre-post analysis, I examine both the reduced form and 2SLS effects from the rise in C-sections using detailed Swedish register data, combining birth records, in-patient records and labor market registers. I find that an increase in C-sections for breech births led to strong improvements in child health in both the short and long run, as indicated by higher Apgar scores at birth and fewer nights hospitalized for children ages 1-7. In terms of maternal outcomes, no significant impact on maternal health at birth or future births or labor market outcomes is found. However, the estimated impact on future fertility suggests a potential negative impact on subsequent fertility. The contribution of the study is twofold: first, I address the issue of endogeneity estimating the causal impact of an increase in C-sections. Second, compared to previous studies, I emphasize the long-run impact focusing on a broader set of outcomes including maternal labor market outcomes and subsequent birth and fertility outcomes.

Chapter two, “Multiple Births, Birth Quality and Maternal Labor Supply: Analysis of IVF Reform in Sweden”, (with co-authors Sonia Bhalotra, Damian Clarke and Mårten Palme)

continues on the theme of fertility, health and maternal labor responses. Studying a policy change in Sweden that mandated single embryo transfer (SET) instead of double embryo transfer (DET), we assess the impact of SET as the default IVF procedure on rate of twin births, child and maternal outcomes. By using detailed Swedish register data for the time period 1998-2007, we examine the impact of switching from DET to SET on a broad set of outcomes using a difference-in-differences approach. The new policy led to a precipitous drop in twin births by 63%. We find large positive effects of the SET reform on child health and maternal labor outcomes, which narrow the differences in health between IVF and non-IVF births by 53%, and differences in the labor market outcomes of mothers three years after birth by 85%. For first time mothers it also narrowed the gap in maternal health between IVF and non-IVF births by 36%. Our study makes a number of important contributions. First, we employ a more robust empirical design for eliciting the causal impacts and by estimating the effects using detailed register data for a long time period. Second, the previous literature does not take into account the impacts of SET on longer term health or maternal labor market outcomes. The findings of this study have important implications for other countries considering policy reform similar to that implemented in Sweden.

Focusing on fertility and women's health rights in an emerging economy, in the third chapter, "The Impact of Abortion Legalization on Fertility and Female Empowerment: New Evidence from Mexico" (with co-author Damian Clarke), the impact of a large-scale, free, elective abortion program in Mexico City in 2007 is assessed. This reform is unique by its kind, considering the Latin American context, which exhibits some of the world's most conservative laws on abortion and other reproductive technologies. We document that abortion legalization in Mexico City led to a legislative backlash in 18 other Mexican states which constitutionally altered penal codes to raise barriers in accessing abortions. We explore this dual policy environment by using a difference-in-differences approach and entropy weighting for estimating the causal impact of progressive and regressive abortion reform on both fertility and female empowerment. Using administrative birth data, our findings suggest that progressive abortion laws, and thus access to free and safe elective abortion services, leads to lower fertility, especially among young women. In addition, by using survey data, we study the impact on women's participation in household decision making and find it to increase with access to abortions. This result is in line with economic theory and empirical results on female empowerment in a developed-country setting. The results do not suggest any reverse relationship between amendments to more conservative abortion laws and fertility or female empowerment. By analyzing mechanisms using evidence from a panel of women, we find no evidence indicating that the results are driven by changes in sexual behavior, altered knowledge, or use of contraception, thus suggesting that improved access to abortion is the main channel through which fertility and empowerment is affected. The evidence in this paper suggests that abortion legalization in the context of an emerging economy has strong and rapid impacts on political behavior, aggregate fertility patterns and household decision-making.

Chapter I

CHAPTER 1

Cesarean Section for High-Risk Births: Short- and Long-Term Consequences for Breech Births

Abstract

Cesarean sections (C-sections) are the most commonly performed surgical procedures in industrialized countries. While they can be potentially lifesaving in cases of high-risk pregnancies, as with any surgical procedure, they can pose complications, and little is known about their long-term consequences for the mothers and children involved. In this paper, I use a sample of "at-risk" births—namely, breech births, in which the fetus is presented with its head upward instead of downward—to study the causal impact of C-sections on the health of infants and on the health, subsequent fertility, and labor market outcomes of mothers. Because selection into C-section may be endogenous, I exploit an information shock to doctors in 2000, in which a new study about the benefits of planned C-sections for breech births led to a sharp 23% increase in planned C-sections. This increase occurred across the board: I find no evidence of a shift in the composition of women receiving C-sections following the shock. I then use this information shock in a reduced form pre-post analysis and as an instrument for C-sections in a 2SLS analysis of Swedish birth, in-patient, and labor market register data associated with births taking place between 1997 and 2003. I find that an increase in C-sections among breech births led to strong improvements in child health originating from both short- and long-term improvements, as indicated by higher Apgar scores at birth and fewer nights hospitalized during ages 1-7. The estimates suggest that the medical intervention almost completely narrowed the gap in health between breech and cephalic (normal position) births. I find no significant impact on maternal health at birth or subsequent births, nor on maternal labor market outcomes. Though marginally insignificant, estimates suggest a potential negative impact on future fertility.

Keywords: fertility, maternal health, child health, birth technology, labor market response

JEL Codes: J13, I11, I12, I38, J24.

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1 Introduction

There are large disparities in early-life health both within and across countries. These disparities stem from a multitude of factors, including health at birth, parental investments, and childhood environment (Haenfler and Johnson, 2002). It is widely recognized that early life conditions have long-lasting impacts on future socioeconomic outcomes (Almond et al., 2017). Given that investments in early stages play a greater role in the production of human capital than do investments later in life, medical intervention at birth could function as an efficient means for narrowing gaps in later life outcomes (Almond and Currie, 2010; Cunha and Heckman, 2007). While many medical interventions early in life, especially among high-risk births, are considered to improve immediate health and long term health and educational performance (Almond et al., 2010; Bharadwaj et al., 2013, 2017; Breining et al., 2015; Cutler and Meara, 2000; Daysal, 2015), the returns to care of low risk births may be less clear (Almond and Doyle, 2011). In light of the potential negative consequences of increases in medicalization of childbirth (Costello and Osrin, 2005) and because of the rapid increase in medical spending on infants compared with older individuals (Cutler and Meara, 2000), a better understanding of medical interventions at birth is important.

A large proportion of children in OECD countries begin their lives through a medical intervention, as around 28% of all births are delivered via Cesarean section (C-section). This intervention is traditionally prescribed for high-risk pregnancies, especially in cases of breech births (i.e., with the head facing upward instead of downward). In most populations, 3-4% of all babies are presented in breech position at term (Herbst and Thorngren-Jerneck, 2001). While the reasons for term breech presentation are unknown, it is associated with poorer birth outcomes compared with the normal (cephalic) fetal position (Hofmeyr et al., 2001).[1] Breech presentation births are riskier, since a fetus positioned with its head upward experiences a more difficult passage through the birth canal and is thus more likely to suffer from complications including oxygen deficiency during a vaginal delivery (Kotaska et al., 2009).

The global incidence of C-section has dramatically increased from the 1970s until today, with C-section rates now exceeding 30% in many countries, including Australia, China, Italy, and the United States (Gibbons et al., 2010), and with rates up to 50% in countries such

[1] The risk of breech presentation is associated with higher maternal age, multiple births, preterm delivery, contracted pelvis, uterine anomalies, and placenta previa (a condition where the placenta covers the uterus). There are also a number of pregnancy complications that are associated with breech presentation: short umbilical cord, fetal malformation, oligohydramnios (too much amniotic fluid in the uterus) and hydramnios (too little amniotic fluid in the uterus), (Leyon and Hagberg, 2008).

as Brazil, Iran, and Mexico (Temmerman, 2016). Because the strong increase in C-section rates worldwide cannot be attributed solely to demographic or maternal health changes, this suggests that C-sections also are performed for nonmedical reasons, possibly driven by supply-side incentives which lead to suboptimal use of the procedure (Betran et al., 2015; Currie and MacLeod, 2008; Currie, J. and MacLeod, B., 2017; Halla et al., 2016; Johnson and Rehavi, 2016). The use of C-section is widely recognized by the medical society as a lifesaving measure for mother and child when medically indicated. However, for low-risk births, C-section may lead to higher morbidity and mortality than vaginal birth (Clark et al., 2008). C-section delivery is also associated with adverse health outcomes for subsequent pregnancies (Daltveit et al., 2008) and lower future fertility (Gurol-Urganci et al., 2013; O'Neill et al., 2013).[2]

Despite C-section being the most common surgical procedure in industrialized countries, many questions remain regarding the causal and long-run impact on the health of children and on the health, future fertility, and labor market outcomes of mothers. These questions can be difficult to assess not only because of the lack of detailed data on long-term outcomes, but also because the choice of delivery mode is endogenous to maternal and child outcomes, with any preexisting conditions likely being correlated with the outcomes of the procedure. This study overcomes both of these issues and aims to causally identify the impact of C-section on maternal and child outcomes among high-risk births consisting of breech births. To obtain reliable information on both long- and short-run outcomes, I use Swedish register data. To overcome the intrinsic endogeneity issues, I explore an exogenous increase in C-section attributed to an information shock to the medical establishment on the benefits of C-section among term breech births.[3] Specifically, I use a pre-post analysis wherein the timing of the birth, before or after the information shock, creates a sharp discontinuity in the probability of planned C-section amongst breech births, allowing me to capture the causal impact of the procedure on subsequent maternal and child outcomes.

[2]The negative association between C-section and future fertility has still not been fully explained but has been posited to be due to factors such as physiological channels (Hurry et al., 1984), psychological channels (Lobel and DeLuca, 2007; Rowlands and Redshaw, 2012), and maternal preferences (Bhattacharya et al., 2006; Norberg and Pantano, 2016; Tollånes et al., 2007). C-section could affect future fertility outcomes for a number of reasons. First, complications from the surgery procedure may cause involuntary infertility (biological channels) (Hurry et al., 1984). Second, the time for recovery from C-section compared with vaginal birth is usually longer. Third, if C-section is considered more traumatic than vaginal delivery, then the psychological cost of childbearing increases with C-sections, reducing the willingness of mothers to have subsequent births (psychological channels) (Lobel and DeLuca, 2007; Rowlands and Redshaw, 2012). Fourth, maternal preferences may also be a contributing factor (Bhattacharya et al., 2006; Norberg and Pantano, 2016; Tollånes et al., 2007). In contrast, Smith et al. (2006) find that the negative association between C-section and future fertility is strongly diminished when controlling for maternal characteristics.

[3]This approach follows other studies using a medical information shock as a source of variation in treatment (Anderberg et al., 2011; Jensen and Wüst, 2015; Price and Simon, 2009).

The information shock I study in this paper occurred in August 2000, at the annual meeting of the Swedish College of Obstetricians and Gynecologists. During this meeting, preliminary results from a large-scale international randomized control trial by Hannah et al. (2000), called the Term Breech Trial, were presented (Alexandersson et al., 2005).[4] The new evidence suggested that planned C-section delivery should be the preferred delivery mode for singleton term breech births. This led to a substantial and immediate increase in planned C-sections for breech births in Sweden (Herbst, 2005) as well as in multiple industrialized countries (Sharoni et al., 2015).[5] This information shock has had a strong and long-lasting impact on medical practice worldwide. To quote Glezerman (2006), "Rarely in medical history have the results of a single research project so profoundly and so ubiquitously changed medical practice as in the case of this publication (TBT)." This was also the case for Sweden, which exhibited a stark rise in planned C-sections for breech births, from 47% to over 60% between 2000 and 2001.

This paper makes important contributions to the previous literature regarding the impact of C-sections on child and maternal outcomes in high-risk births.[6] First, most previous studies, particularly those in the biomedical literature, suffered from endogeneity issues, small sample sizes, or both.[7] The information shock to the medical society in 2000 allows me to credibly identify the causal impact of C-section, since using Swedish register data provides a much larger sample size and is unique in that it allows visibility into the universe of breech births, alongside a rich set of maternal covariates. Second, previous studies with a causal interpretation either focus on short- and mid-run health outcomes (Jensen and Wüst, 2015) or are limited to maternal outcomes regarding future fertility and labor market outcomes without being able to examine the intermediary effects on health outcomes for both mother and child (Halla et al., 2016).[8] By exploring exogenous variation in C-section and detailed Swedish register data, this

[4]Preliminary results from a retrospective cohort study by Herbst and Thorngren-Jerneck (2001) on the benefits of planned C-section were also presented at the annual meeting in 2000. For a detailed discussion of evidence based changes in delivery mode due to the Term Breech Trial, see Alexandersson et al. (2005).

[5]These countries include Denmark, Australia, UK, Netherlands, Malaysia, Finland, and Saudi Arabia (Sharoni et al., 2015).

[6]This study also relates to the literature on C-section and incentives and information (Currie, J. and MacLeod, B., 2017; Johnson and Rehavi, 2016), as well as Borra et al. (2014), which studies the effect of removing child benefits on timing of births (scheduling of induced labor and C-section). The elimination of child benefits led to a rise in low birthweight children and neonatal mortality.

[7]A study with a more robust design is Norberg and Pantano (2016). Using multiple data sources and estimation techniques, they find a negative association between C-section and future fertility (corresponding to a reduction by 17%), which is at least partly attributed to maternal preferences.

[8]Jensen and Wüst (2015) use the publication of the term breech trial as exogenous variation in C-section (see Section 2). Halla et al. (2016) use exogenous variation in emergency C-section, originating from supply-side incentives to accelerate deliveries across weekdays, to assess the effect on fertility in Austria. Their findings suggest that emergency C-section at first birth reduces fertility by approximately 17% and causes a temporary rise in maternal employment such that income increases by 14%. Card et al "The Health Effects of Cesarean Section: Evidence From the First Year of Life", examine the short-run impact of C-section on child health.

study considers a broader set of outcomes and a longer time horizon compared with previous studies. To the best of my knowledge, this is the first study to analyze the causal impact of C-section on outcomes such as child health up to age 7, maternal health outcomes during any subsequent pregnancy, and labor market outcomes including income from both sickness and parental benefits. Thus, relative to previous studies, this paper provides a more extensive analysis by studying the impact of C-section on both short- and long-run impacts on child health and maternal health, future fertility, and labor market responses. Third, the analysis in this paper sheds light on possible heterogeneous effects across socioeconomic status and health indicators, which is important given that breech births constitute a particular high-risk group, implying a possible social gradient in the response to altered delivery mode.

The results from this study show that the information shock led to a substantive and significant increased use of planned C-section deliveries by 11 percentage points among singleton breech births at term, roughly corresponding to a 20% increase. This increase was not restricted to any particular group of women with breech births, but was found among all women below age 35, all educational levels, and both normal-weight and overweight women. No change in delivery mode was found for pregnancies with normal fetal position. Importantly, I find no evidence of changes in the composition of mothers receiving a planned C-section or in the proportion of breech births being reported. Likewise, I find no other discontinuities when examining placebo dates.

The reduced form estimates from the pre-post analysis suggest that the information shock improved child health among breech births, as measured by a summary index of various short- and long-term health measures by 0.104 standard deviations,[9] almost completely closing the gap in child health between breech and normal-position births. This increase in child health is driven by both improvements in health at birth and during childhood, indicated by higher Apgar scores,[10] and fewer nights hospitalized during ages 1-7. Thus, the increase in C-sections improved child health in both the short and the long run. While the beneficial impacts of the shock on children are clear, there appears to be little impact on mothers. I find no significant impact of the information shock on maternal health at birth or at subsequent births. I also find an insignificant negative impact on future fertility (in terms of both the total number of future births and the probability of no future birth) in most specifications. However, because

[9] Child health is measured by a summary index according to Anderson (2008) consisting of Apgar score, infant mortality, and hospitalization during the first year of life and ages 1-7.

[10] Apgar stands for appearance, pulse, grimace response, activity, respiration. This score is an assessment made by a physician 1, 5, and 10 minutes after birth. A score of 10 indicates perfect health and 1 extremely poor health.

these estimates are consistent across specifications and are occasionally marginally significant, I cannot rule out a potential negative impact. Furthermore, I do not find any effects on labor market outcomes for the mother when analyzing income from labor earnings, sickness and parental benefits.

In addition to the reduced form analysis, I estimate a two-stage least squares model using the information shock as an instrumental variable for planned C-section. While the reduced form analysis provides the causal impact of the information shock on all breech births, the two-stage least squares estimates provide the causal impact of C-sections that are generated by the information shock. That is, this analysis gives us the local average treatment effect of C-section on compliers. The two-stage least squares estimates suggest that the change from vaginal delivery to planned C-section improved child health by 0.93 standard deviations, which is driven by an increase in Apgar score of 0.58 units and by 5.9 fewer hospital nights.

The results are robust to a number of sensitivity checks, including alternative specifications (using quadratic trends, cubic trends and triangular kernel and a smaller window of time), non-weighted indices, and a difference-in-differences design using births with normal fetal position (cephalic births) as controls.

In summary, this study suggests that the increase in planned C-sections for breech births improves child health. However, it appears to have limited consequences for maternal health and labor market outcomes, but a possible negative effect on future fertility. These findings should be particularly relevant for countries with low rates of planned C-section for breech births. Singleton breech births at term represent a reasonably large share (3-4%) of all births. Thus, interventions that can improve health among this high-risk group are important. The results from these exercises show that the gap in child health between this risk group and normal births nearly vanishes in the face of this medical intervention. Moreover, this study contributes to the general literature on the causal effects of C-section, and although it focuses primarily on breech births, the results may be of interest for other high-risk groups as well.

The paper is structured as follows. Section 2 presents a description of breech birth in the Swedish context and outlines the information shock to the medical society. In Sections 3 and 4, the data and empirical strategy are described. Sections 5 and 6 present and discuss the results. Section 7 concludes the study.

2 Background

2.1 Breech presentation and delivery mode

Multiple biomedical studies suggest that planned C-section deliveries reduce the risk of perinatal and neonatal morbidity and mortality in breech births (Betran et al., 2015; Cheng and Hannah, 1993; Gifford et al., 1995; Herbst, 2005; Herbst and Thorngren-Jerneck, 2001). However, this association does not necessarily provide a causal interpretation because of the correlation between the choice of delivery and birth outcomes (Hannah et al., 2000). This research position is reflected in the lack of consensus within the medical society on the optimal delivery mode for breech births (Glezerman, 2006; Goffinet et al., 2006; Sharoni et al., 2015; Turner, 2006).

A milestone within the medical literature regarding delivery mode for breech birth deliveries at term was the publication of the Term Breech Trial by Hannah et al. (2000). This large-scale international randomized controlled trial was conducted across 121 hospitals in 26 countries, covering 2,088 women randomly assigned to either a planned C-section or planned vaginal delivery.[11] The results showed that perinatal and neonatal mortality as well as severe neonatal morbidity were significantly lower in breech births delivered with planned C-section (1.6%) than with planned vaginal delivery (5.0%). Moreover, the reduction in mortality and morbidity risks were higher in countries with already low neonatal death rates. No significant differences in maternal mortality or severe maternal morbidity were found between planned C-section and planned vaginal delivery. The study was terminated prematurely because of findings of statistical differences in perinatal outcomes between the two groups, making it unethical to continue the randomization (Hannah et al., 2000). While the Term Breech Trial had a strong and long-lasting impact on medical care in multiple countries (Sharoni et al., 2015), the results of the study did not remain unchallenged. Strong criticism has been directed toward the implementation of the trial by Glezerman (2006) and Turner (2006).[12]

Two follow-up studies were conducted two years after the Term Breech Trial, assessing the impact of planned C-sections for breech births on child outcomes (Whyte et al., 2004) and maternal outcomes (Hannah et al., 2004). These studies were conducted using survey data collected from questionnaires directed to the mother and child from a subsample of women from the Term Breech Trial sample.[13] No significant impact was found on either child health

[11] Women were eligible if the fetus was a singleton, was alive and had a weight below 4,000 grams at gestational age 37 weeks or beyond. Births with known fetal anomalies were excluded from the trial.

[12] Glezerman (2006) argues that most cases of the neonatal mortality and morbidity are unrelated to the delivery mode.

[13] The current study differs from these previous studies in that rich register data on long-term outcomes beyond

(Whyte et al., 2004) or maternal health or fertility (Hannah et al., 2004).[14] These results were surprising with regard to the Term Breech Trial, which showed a strong positive health effect for babies delivered by planned C-section.

A more recent study supporting the findings of the Term Breech Trial is Jensen and Wüst (2015), which examines the impacts of the Term Breech Trial on child and maternal health in Denmark. By using the Term Breech Trial as exogenous variation in the likelihood of C-section among high-risk pregnancies, Jensen and Wüst (2015) find that child health improves. The results suggest that C-section among breech presentation pregnancies is associated with 4 percentage points higher Apgar score, 6 percentage points lower probability of an Apgar score below 7 and approximately seven fewer visits to the doctor over the first three years of life. They also find that C-section does not affect maternal health other than increasing hospitalization by 2.3 days.[15]

2.2 The Swedish context

In Sweden, prenatal care is provided free of charge and includes ultrasounds, physical examinations, and sampling of biomarkers, as well as birth classes.[16] While midwives usually carry out these tasks, pregnant women may also access care by an obstetrician or gynecologist (OB/GYN), which is a necessity for prescription medications or medical procedures. Most women have their first visit at a maternity unit during gestational weeks 6-12. From week 20, women are advised to have monthly checkups until week 30, after which biweekly checkups are advised (Vårdguiden, 2017). Prenatal care attendance in Sweden is very high. Only 0.4% of all women visit less than three times, and only 9.4% register later than gestational week 15 (Buekens et al., 1999).

Near the end of the pregnancy, around weeks 36-37, the fetal position is examined by a midwife at the prenatal care unit. If the fetal position is suspected to be breech or deviates from normal presentation in some other way, the woman is referred to a specialist maternity

two years are further explored for both mother and child. A longer time period is of interest because the average birth spacing in many countries including Sweden is over 3 years.

[14]Evidence on long-term impact of breech presentation is scarce. One exception is Mackay et al. (2015), who show that vaginal delivery among breech presentation pregnancies led to lower Apgar scores at birth and lower educational attainment.

[15]The increased probability of C-section occurred only among second time mothers (or subsequent pregnancies) since C-section among first-time mothers with breech births was already the default delivery mode in Denmark when the Term Breech Trial was published. For this reason, first-time mothers are omitted from their analysis, thereby excluding more than 50% of their sample. In Sweden, however, the main increase in C-section occurred among first-time mothers.

[16]All residents in Sweden are guaranteed access to public health care, which is primarily provided by the county councils (*Landsting*) and funded by central and local taxation. Only 2.5% of all residents have taken out private health insurance (Anell, 2008).

care unit, where the OB/GYN tries to manually turn the baby into a cephalic position using a procedure called "external cephalic version". If this is successful (and if the baby stays in cephalic position until delivery) vaginal delivery is attempted following the normal procedures. Approximately 50% of all external cephalic versions are unsuccessful. In such cases, a planned C-section is usually scheduled 7-10 days before the expected day of delivery (based on the date of last period and ultrasound examination). Vaginal delivery can be attempted if certain criteria are fulfilled, including normal fetal growth, pelvis size, spontaneous start of delivery, and abundant amniotic fluid. However, not all breech presentations are identified prior to birth. If a breech position is discovered at the time of delivery, the decision-making process is similar to that when discovered before (Karolinska Universitetssjukhuset, 2016).

2.3 Information shock to the medical society

In 2000, new scientific evidence became available, suggesting that planned C-section is the preferred delivery mode for singleton breech presentation births at term. In Sweden, the debate within the medical society on the preferred delivery mode for breech births began before the publication of the Term Breech Trial, at the annual meeting of the Swedish College of Obstetricians and Gynecologists (SFOG) in August 2000 (Alexandersson et al., 2005). The annual meeting started with a symposium on "Term breech: C-section or vaginal delivery?"[17] where preliminary results from the Term Breech Trial by Hannah et al. (2000) were presented as evidence in favor of planned C-section as preferred delivery mode.

The next piece of evidence was presented by Herbst and Thorngren-Jerneck (2001), consisting of preliminary results from a cohort study in Sweden, suggesting that planned C-section is the preferred delivery mode from breech births at term.[18] Because of the new evidence presented at the annual meeting, the Swedish medical society of perinatal medicine organized an extra meeting in Stockholm in December 2000 together with other medical societies of perinatal medicine from Scandinavia.

Although no new guidelines were issued, multiple sources suggest that the dissemination of new evidence on preferred delivery mode by Hannah et al. (2000) and Herbst and Thorngren-

[17] The symposium, "Sätesändeläge i fullgången graviditet-kejsarsnitt eller vaginal förlossning", was chaired by Professor Ingemar Ingemarsson and consisted of several lecturers on the topic of preferred delivery mode according to evidence, based knowledge. The internal newsletter of SFOG "Medlemsbaldet nr 4, 2000" includes a detailed description of the SFOG annual meeting and the symposium.

[18] Herbst and Thorngren-Jerneck (2001) find that babies delivered by planned vaginal delivery had lower Apgar scores (3-5%) and exhibited higher neonatal neurological morbidity (3%). Another study by Herbst (2005) on perinatal and infant mortality among babies in breech presentation at term in Sweden, using the Medical Birth Registry for the period 1991-2000, finds that breech babies delivered by C-section exhibit lower perinatal and infant mortality.

Jerneck (2001) caused a strong increase in planned C-sections.[19] First, an immediate poll of SFOG members after the symposium on breech births at the annual meeting showed an increased support for planned C-section compared with vaginal birth.[20] Second, the data show an evident pattern of altered Swedish obstetric practice regarding delivery mode among breech presentation births attributed to new evidence-based recommendations (Alexandersson et al., 2005), consistent with many other industrialized countries at this time (Sharoni et al., 2015).

In Figure 1, trends in C-section among breech births at term are presented. The trends show a sharp increase in the rate of planned C-section, from approximately 47% in 2000 to over 60% in 2001 (Figure 1b). The trends at the monthly level (see Figure 2) display how C-section increased after the annual meeting in August 2000. Based on residency, trends in probability of C-section for breech births at term are presented by county in Figures A1, A2 and A3. The increase is the sharpest in some of the most populated counties. From 2001 onward, approximately 70% of breech-presentation pregnancies have been delivered by planned C-section in Sweden. During this period, the prevalence of breech births is constant (see Figures 3 and 4) and no increase in C-section for normal-position births can be found either by graphical examination (see Figure 5) or in the regression analysis (see columns 3-4 in Table 1).

Since the information shock took place at an internal medical gathering rather than in the public media, the increase in C-section can be thought of as mainly supply side driven. That is, a woman giving birth to a baby in breech presentation at term after the annual meeting in 2000 was more likely to be recommended a planned C-section than a vaginal birth. While there are regional differences in planned C-section, residential sorting due to demand for a planned C-section is less likely given that expectant mothers learn late in the pregnancy (weeks 36-37) about the fetal position of the child, thereby making it difficult to plan ahead. Given that information on internal hospital routines can be hard to access as an outsider and that expectant mothers in Sweden have a limited ability to choose the hospital at which to give birth, it is thus reasonable to believe that the increase in C-sections can be attributed to supply-side change due to the new evidence provided to the medical society. Finally, in the Swedish context, there is no (known) financial incentive for the individual doctor to choose one specific delivery

[19]National guidelines for specific selection criteria on mode of delivery for breech presentation pregnancies have been available in Sweden since 1974. During the 1980s and 1990s, studies from several countries (including Sweden) on preferred delivery mode for breech presentation pregnancies showed increasing support for planned C-section. However, the evidence was not conclusive, which led to different medical practices across Swedish hospitals (Herbst, 2005).

[20]Nearly 50% of the OB/GYNs at the annual meeting voting in favor of routinely planned C-section. When the same question was asked, but with the stipulation that the OB/GYNs should imagine that the patient was a family member or oneself, two-thirds voted in favor of routine planned C-section (internal newsletter of SFOG, "Medlemsbaldet nr 4, 2000").

mode over another.

3 Data

3.1 Data description

I use Swedish administrative population-level data to study the impacts of C-section on child and maternal outcomes. I use data for all births in Sweden between 1973 and 2011 from cohorts born between 1940 and 1985 (including their children and parents) which are identified via the Swedish Multi-generational Registry (Flergenerationsregistret) and the Swedish Medical Registry (Svenska födelseregistret). Based on this sample, covering more than 98% of all births in Sweden during 1973-2011, multiple data registries on health and labor market outcomes are combined, and data are complete for the period 1991-2011.

Information on pregnancy and birth outcomes is obtained from the Swedish Medical Birth Registry. The Swedish Medical Registry is provided by the National Board of Health and Welfare and contains information on all births in Sweden since 1973 (beyond 22 weeks of gestational age and including both stillbirths or live births). This registry provides detailed information on pregnancy, delivery and postpartum conditions, including maternal characteristics (maternal age, height, and weight), previous health conditions (diabetes, asthma, and epilepsy) and pregnancy behavior (tobacco usage and prenatal visits). In addition, it also provides extensive data on perinatal and neonatal outcomes for the child, including fetal position, gestation, birth weight, health at birth, malformation, surgeries, and medical diagnoses and treatments. There is detailed information, for each birth, regarding medical interventions during delivery such as C-section, induction of labor, and operative procedures such as the use of forceps and vacuum extraction.

I particularly focus on *delivery mode*, whether vaginal delivery or C-section delivery. As C-section can be either a planned or emergency surgical procedure, information about the indication (whether a planned or emergency C-section) for the procedure is of interest. The birth registry lacks detailed information about the indication for C-section before 2000. Instead, it provides information on whether delivery started or ended with C-section. For this reason, deliveries that started with C-sections are used as a proxy for planned C-section, and deliveries that end with C-sections (after attempting vaginal delivery) are used as a proxy for emergency C-section. Deliveries of term births that start with C-section are considered a good proxy for planned C-section (Källén et al., 2005).

In order to define treatment status, I need to identify whether the birth occurred before or after the information shock. Information on the exact date of birth is unfortunately not available. As an approximation for date of birth, I use the discharge date from the maternity unit minus the number of average hospital nights for corresponding delivery mode. In the year 2000, the number of nights spent at the hospital after delivery by C-section was, on average, four nights, compared with two nights for vaginal delivery (The Swedish National Board of Health and Welfare, 2003). There are other important variables for this analysis. These include *fetal position*, in which breech birth is defined as complete, frank, or footling breech,[21] *Apgar score at 5 minutes*, indicating the general health condition of the newborn baby five minutes after the delivery, and maternal complications postpartum, which are identified via the ICD-10 classification system. In particular, complications include diagnoses of *postpartum hemorrhage* (severe blood loss) and *maternal sepsis* (infection).[22]

Data on hospitalization are obtained from the National Patient Registry, provided by the National Board of Health and Welfare. Using this registry, I obtain information on inpatient care at all Swedish hospitals since 1987, including length of each hospital stay. Because of data availability, I use the mother's discharge date from the maternity unit, via the Medical Birth Registry, as a proxy for the date of delivery. Thus, for the mother, I can observe hospitalization only after readmission to the hospital. Mortality data are identified using *the Cause of Death Registry*, which is provided by the National Board of Health and Welfare and includes information on all deaths of registered residents in Sweden since 1961. The diagnoses of causes of death are coded according to the ICD system.

Data on labor market outcomes are obtained from the Longitudinal Integration Database for Health Insurance and Labor Market Studies (LISA), which is provided by Statistics Sweden and contains annual information on education and earnings for all individuals above age 16 starting from 1991. To assess the impact of birth technology on labor market responses, I focus on the following variables: *income from gainful employment*, defined as total annual gross earnings (in cash) and net income from active business; *income from parental leave*, defined as the total annual income from parental leave (this includes income from parental allowance, temporary parental leave, and child care allowance); *income from sick leave*, defined as the total annual income resulting from illness, injury, or rehabilitation (including a sick pay period of 14

[21] ICD-10: O80.1, O83, O64.1, P03.0, or codes defined by Swedish Medical Birth Registry: MAG00, MAG03, MAG10, MAG11, MAG20, or MAG96.

[22] ICD10 codes for sepsis: O85, O86, O860, and O861. ICD10 codes for postpartum hemorrhage: O678, O72, O720, O721, O721A, O721B, O721X, O722, O723, and DR029.

days); *income from unemployment benefit*, defined as the total annual income from unemployment benefits. All income variables are expressed by annual amount of 100 SEK. Education is measured by the highest level of educational attainment (levels 1 to 7).[23]

3.2 Main outcome variables and multiple hypothesis testing

The outcomes are chosen based on data availability and to enable comparisons to previous results (Hannah et al., 2000, 2004; Herbst, 2005; Herbst and Thorngren-Jerneck, 2001; Hofmeyr et al., 2001; Jensen and Wüst, 2015). Since I test a large number of outcome variables, the analysis is prone to type 1 errors. To account for this potential issue, I compute summary indices as suggested by Anderson (2008), combing multiple outcomes into one measure for child health, maternal health at birth, maternal health at subsequent birth, and labor market outcomes. The indices are computed as follows: The direction of each outcome is oriented in the same direction, such that a higher value indicates a better outcome. All outcomes are standardized, subtracting the mean and dividing it by the standard deviation of the control group. For each category of interest, an index is created using the standardized variables weighted by the inverse of the covariance matrix. This means that variables with lower correlation with the other variables within the category provide new information and will therefore obtain a higher weight than variables with high correlation.[24] The index is computed in such a way that mean in the control group is zero with standard deviation one. The following indices are computed:

1. *Child health index*: Apgar score (scale 1-10, positively coded), Apgar score below 7 (negatively coded), infant mortality (negatively coded), nights hospitalized (inpatient admission overnight) within the first year of life (negatively coded), and between ages 1 and 7 (negatively coded).

2. *Maternal health index*: Maternal sepsis (negatively coded) and postpartum hemorrhage (negatively coded), number of nights hospitalized postbirth (inpatient admission overnight within one year of birth, negatively coded).

3. *Maternal health at subsequent birth index*: Maternal sepsis (negatively coded) and postpartum hemorrhage (negatively coded), number of nights hospitalized postbirth within

[23]Level 1 is primary education less than 9 years, level 2 is primary education of 9 years, level 3 is secondary education at most 2 years, level 4 is secondary education of 3 years, level 5 is tertiary education less than 3 years, level 6 is tertiary education 3 years or more, and level 7 is graduate studies.

[24]See Anderson (2008).

one year from birth (negatively coded), and emergency C-section (negatively coded) at subsequent birth.

4. *Maternal labor market index*: Annual labor income (in 100 SEK is positively coded), parental benefits (negatively coded), and sickness benefits (negatively coded).

In addition, fertility outcomes using a fixed time period of 8 years after birth are analyzed focusing on the total number of future births, a binary measure of any future birth, and birth spacing. Finally, effects on income from gainful employment, sickness benefits, and parental benefits are analyzed separately.[25] The issue of multiple comparisons is further addressed by controlling for *false discovery rates (FDR)*, which is the proportion of type I errors. Corrected p-values are estimated using the step-up procedure suggested by Benjamini and Hochberg (1995). While FDR has the disadvantage of a higher number of false positives than with alternative methods (for example, family wise error rate), the advantage is that it yields higher power.[26]

3.3 Sample and descriptive statistics

For this analysis, data from multiple registers are combined for the time period August 1997 to August 2003 (i.e., 36 months before and after the information shock). The sample consists of 522,606 births. I further restrict the sample to mothers with a singleton birth, in which the fetus is presented in breech position at term (gestational age equal to 37 weeks or above).[27] Multiple births and preterm births are omitted from the analysis since the information shock of preferred delivery mode considers only singleton breech births at term. I cannot observe whether external cephalic version was attempted. Thus, my sample consists of fetuses in breech presentation, in which births with successful external cephalic version are implicitly omitted from the sample. The final sample of breech babies covers 13,208 births (of which 34 are stillbirths and 1,107 are babies with malformations).[28]

To illustrate how breech births are related to normal (cephalic) births at term, I present summary statistics in Table 2 of unconditional means and standard deviations in child and

[25]Since a significant proportion of the sample earns an income of zero, I use an inverse hyperbolic sine transformation $\log(y_i + (y_i^2 + 1)^{1/2})$ analogue to Burbidge et al. (1988). This transformation has an analogous interpretation to the standard logarithmic transformation (of the percentage change in income) but is defined at zero.

[26]Bonferroni corrected p-values, the most conservative alternative, will also be reported for comparison.

[27]According to the Swedish National Board of Health, breech presentation is identified by maternal diagnosis by ICD-10 codes O80.1, O83, O64.1, and P03.0. Breech implies breech or footling position.

[28]During this period, there are 406,448 singleton term births with normal presentation. Subsamples of different time periods around the information shock are also used for the analysis. I exclude births with no information on year of birth or date of discharge (577 observations) as well as observations without information on gestational age (48 observations), since these variables are pertinent for defining treatment status.

maternal characteristics among breech births (columns 1-3) and normal position births (columns 4-6).[29] A t-test of differences in means between breech and normal births is presented (column 7) together with its p-values (column 8). This table shows a clear pattern in that singleton babies presented in breech at term tend to have poorer health outcomes at birth than babies in normal presentation. On average, birth weight is 250 grams lower for babies in breech presentation and gestational age is one week shorter. Breech babies are less likely to be male (0.46 compared with 0.51), suggesting negative selection of male fetuses in utero[30] and more likely to suffer from fetal malformation (8% compared with 3%). Apgar score is lower in absolute terms as well as for the dichotomous measures of low health at birth (below score 7). The infant mortality rate is higher for breech babies, at 3.2 deaths per 1,000 live births, compared with normal position babies, at 1.3. Other health indicators show a similar pattern in which babies in breech presentation exhibit inferior health compared with those in normal presentation. These differences may not be due only to breech position but also to delivery mode and underlying maternal characteristics. The differences in child health remain when comparing child health between breech vaginal birth and normal vaginal birth.[31]

Maternal health outcomes show a similar pattern of adverse health and obstetric outcomes. A striking difference between breech and normal position births is the delivery mode. Among breech births, planned C-section delivery is the more common method (55.5%) compared with emergency C-section (26.6%). In comparison, among normal position births, 3.9% of all deliveries are planned C-sections, and 4.5% are emergency C-sections. Mothers with breech births have higher educational attainment and higher annual labor income (114,792 SEK compared with 100,000 SEK) prior to birth, but no statistical differences are seen for the amount of sickness benefits prior to birth. This suggests that women having breech births are not disadvantaged in terms of education and income compared with mothers with normal position births. Finally, in panel D, the indices confirm the summary statistics presented, showing that child and maternal health are poorer among breech births (by 0.039, -0.02, -0.01) compared with normal position births (0.106, 0.016, 0.157). This is, however, not the case for the labor market index, which exhibits better outcomes (0.106) compared with normal births (-0.003).

Birth outcomes, delivery mode, and fertility outcomes for first-time mothers with term breech singleton births are compared before and after the information shock in Table 3.[32]

[29] The samples of normal position births include singleton births at term (born in week 37 or later).

[30] For example male fetuses are less likely to survive under distress (Almond and Mazumder, 2011).

[31] These differences also remain when holding gestational age constant, regressing breech status on child health. These results are available on request.

[32] This simple pre-post comparison does not account for trends and covariates, which will follow in the empirical

Columns 1 and 2 display variable averages, 36 months before and after the annual meeting in August 2000, respectively, and a t-test of the difference between means with p-values in brackets below, is presented in column 3. In Panel A, the summary statistics document a strong increase in all C-sections, which can be attributed mainly to the strong increase in planned C-sections, from 47% to 65% compared with the smaller and less significant decrease in emergency C-sections from 29% to 27%. The induction of labor decreased from 2% to 0.8%, which is consistent with a higher use of C-section deliveries. Child health outcomes are presented in Panel B and suggest an overall improvement in health. Health at birth measured by the absolute level of Apgar score as well as a dichotomous measure of low Apgar score (below 7) is improved. While the infant mortality rate and number of hospital nights are lower in the post period, these differences are not significantly different from zero. There is no clear pattern of changes in maternal health at birth, maternal health at subsequent births or fertility outcomes within a fixed time period of eight years,[33] presented in Panels C, D, and E. Maternal labor market outcomes appear to be improved, presented in Panel F, when comparing unconditional means. The summary indices are presented in Panel G.

These outcomes for the full sample are further explored by graphical examination, which confirms the summary statistics presented in Table 3. Trends in delivery mode and child and maternal outcomes among breech births at term are presented in Figures 1, 2, 6, 7, and 8. These graphs show monthly level (as well as yearly level for C-section) trends around the information shock, where 0 is the month of information shock. The red vertical dashed line indicates the month of the information shock, and each dot represents the average rate of C-section on a monthly basis. Figures 1 and 2 show a sharp increase in C-section among breech births at term, which is driven by an increase in planned C-section deliveries compared with emergency C-sections.[34] The proportion of breech presentation births during this period appears to be constant across the cutoff, which is important for the identification strategy used in this project. Moreover, no discontinuous increase in any type of C-section can be detected among normal position births (see Figure 5).[35] Thus, these graphs confirm the summary statistics.

analysis.

[33]It should be noted that all subsequent pregnancies are likely to be endogenous to the first birth. See Section 6 for a longer discussion.

[34]The proportion of emergency C-sections are presented in Figure A4.

[35]The regression analysis confirms this finding and is further elaborated in Tables 1 and 5.

4 Empirical analysis

The aim of this paper is to study the causal impact of C-section on child and maternal outcomes. The intrinsic endogeneity problem when studying this relationship is that delivery mode tends to be correlated with prebirth characteristics of mother and child. Thus, a simple correlation between outcomes and delivery mode will suffer from selection bias—it is likely that important, but unobservable, prebirth maternal and child characteristics differ systematically between C-section births and other births. For example, such maternal characteristics may involve unobserved preferences, behavior, and health conditions that can affect both delivery mode and outcomes. To overcome these issues, I use an identification strategy based on a pre-post design. In short, I compare outcomes among breech births occurring before the information shock with outcomes among breech births occurring after the information shock. Thus, births delivered before the information shock function as a control group for births delivered after. The key identifying assumption required for this empirical strategy to be valid is that the information shock is exogenous to the timing of the birth. In other words, I can identify the causal impact of an increased proportion of C-sections if prebirth maternal and child characteristics are constant before and after the information shock.

I start by examining the impact of the information shock on delivery mode, obstetric care, and outcomes for singleton breech births at term, which is estimated according to Equation 1 using ordinary least squares model.

$$Y_{it} = \alpha_1 + \alpha_2 InfoShock_t + f(t) + \boldsymbol{X}_{it}\delta + \epsilon_{it} \tag{1}$$

The outcome variables are denoted by Y across individual i and time t. The variable $InfoShock_t$ is a binary variable equal to one if birth occurs after the 25 of August 2000 and zero if birth occurs before.[36] Split time trends $f(t)$ are included, consisting of a first-order polynomial of normalized daily calendar time away from the information shock in August 2000, allowing for different slopes across the cutoff. The calendar time is normalized such that the cutoff date, 25 of August 2000, is zero where treatment is positive to the right of this threshold. By including $f(t)$, I allow for different trends (slopes) before and after the information shock. In certain specifications, a full set of child and maternal characteristics $\boldsymbol{X}_{it}$, consisting of binary measures of birth order, maternal age, county of residency, quarter of birth, nationality (born in Sweden

[36]Date of birth is not available. Instead, I use the discharge date as an approximation for date of birth. More information is available in Section 3.

or not), tobacco usage during the first trimester, sex of the baby, educational attainment, mean income, and income from sickness benefits before giving birth, is included.[37] The idiosyncratic error term is denoted ϵ_{it} clustered on the discrete values of the assignment variable, day-month-year, suggested by Lee and Card (2008).[38] Robustness checks are conducted with respect to the choice of polynomials, kernel, and time period.

The effect of planned C-section can be captured by a two-stage least squares model (2SLS) using the information shock as an instrumental variable (IV) for planned C-section. The IV strategy is analogue to a fuzzy regression discontinuity design (Lee and Lemieux, 2010) using calendar time as the running variable. The estimated effect is thus the local average treatment effect (LATE) (Angrist and Pischke, 2009), which can be interpreted as the average treatment effect on "compliers"—that is, the breech presentation births delivered by C-section due to altered routines following the information shock. The IV analysis is described by Equation 2, where the relationship between various outcomes Y_i and delivery mode, which can be attributed to the information shock, is established.

$$Y_{it} = \beta_1 + \beta_2 P(\widehat{\text{C-section}}_{it} = 1) + g(t) + \boldsymbol{X}_{it}\theta + \xi_{it} \tag{2}$$

The predicted likelihood of C-section due to the information shock is expressed by $P(\widehat{\text{C-section}}_{it} = 1)$ and estimated according to Equation 1. Trends, time-varying controls, and the error term are handled analogously to Equation 1, such that each variable included in the first stage is included in the second stage. The regression, expressed by Equation 2, is estimated by a linear probability model using a 2SLS method where the estimated effect can be interpreted as the local average treatment effect on compliers.

In addition to the date of birth being independent of the information shock, the IV strategy is valid provided that the following assumptions are satisfied (Angrist and Pischke, 2009): First, the *exclusion restriction* implies that the information shock affects outcomes only via a higher likelihood of having had a C-section and not other medical practices and treatments. Second, the instrument must be *relevant* such that the information shock is strongly correlated with the adaptation of a new delivery practice—that is, C-section among breech pregnancies. Finally, *monotonicity* implies that the information shock should have either a positive or zero treatment effect (such that C-section is more likely after the information shock but never less likely).

[37] Information on birth hospital or birth county is unfortunately not available; instead the baby's registered county of residence is used as an approximation for birth county.

[38] There is an ongoing debate about clustering when using time as the running variable; see Hausman and Rapson (2017). The results are robust to alternative clustering on the level of the mother.

While we can test the assumption of the existence of a strong and significant first stage and monotonicity, we cannot test whether the exclusion restriction is satisfied. It is possible that the information shock led to increased awareness of the risks associated with breech births, resulting in improved care for all breech births in terms of not only more planned C-sections but also other treatments.[39] Maternal selection is an additional potential issue for identification. These potential issues are further discussed in section 5.1.

5 Results

5.1 Effects on C-section and obstetric care

The sharp increase in planned C-sections among breech births is presented visually in Figures 1 and 2, and in regressions estimated according to Equation 1 presented in Table 1 (columns 1-2). The results indicate that the information shock to the medical society had a strong significant impact of approximately 11 percentage points on the probability of planned C-section among breech births. The estimate and precision remain very robust to the inclusion of maternal and child characteristics such as maternal weight, height, nationality, tobacco use during the first trimester, and sex of the baby, as well as age, birth order, birth-quarter, and county fixed effects.[40] F statistics for each regression are presented in Table 1. The F statistics with and without controls are 39.4 and 39.1 (see columns 1-2), respectively.[41] Thus, the results imply that there is a strong significant effect of the information shock to the medical society on the proportion of C-sections among breech births corresponding to a 23% increase when compared with the mean of the dependent variable in the pretreatment period.

While an increase in planned C-sections among breech presentation births is expected, there should be no impact on the proportion of planned C-sections among births with normal fetal position.[42] This is tested analogously to breech births and presented in Table 1 (columns 3-4). The estimates are both statistically insignificant and small in magnitude, with a F statistics of 0.4 and 0.2. Hence, the results show no indication of altered delivery mode among normal position births. These results suggest not only that there were no changes in delivery mode

[39] For instance, I cannot observe whether breech births were attended by more midwives and senior OB/GYNs or received improved care in general.

[40] As birth timing is not exogenous (Quintana-Domeque et al., 2016), seasonality is controlled for using birth-quarter fixed effects.

[41] An indicator of having a weak instrument is an F statistics below 10, which is perceived as a “rule of thumb” as suggested by Staiger and Stock (1994).

[42] Analogous to the sample of singleton term breech births, the sample of normal position births excludes preterm births ($<$ gestational week 37), multiple births, and births in fetal positions other than prostrate neck or head presentations.

among normal position births but also that the rise in C-sections among breech births did not crowd out C-sections for nonbreech births (due to constraints in the surgical team at the hospital).[43]

Panel A of Table 4 demonstrates the robustness of the first-stage results to alternative functional form and time period. These specifications include quadratic trends (column 1), cubic trends (column 2),[44] a triangular kernel that places more weight on observations close to the cutoff and less on those farther away (column 3) and alternative sample of a shorter time period of a 12-month window before and after the information shock (column 4) as well as a shorter time period of a 300-day window (column 5).[45] A full set of fixed effects and maternal and child characteristics are included. These estimates of 0.11-0.13 are very similar to the baseline estimate of 0.11 with an F statistic above 10.

To get a better understanding of the effect of the information shock, the impact on supplementary or intermediate medical interventions during delivery for breech births is examined and presented in Panel B, Table 4. The results are estimated analogously to the baseline specification, expressed by Equation 1, with a full set of covariates and fixed effects. The results imply that the information shock had no statistically significant impact on emergency C-sections (column 1), indicating that the rise in planned C-sections originated from women who would otherwise have given birth by vaginal delivery. There is no significant impact on induced labor (column 2) or the usage of forceps or vacuum extractor (column 4) but a strong significant increase in the use of spinal anesthesia (column 3), which has a similar estimate (0.13) to the increase in planned C-sections (0.11). The strong increase in the usage of spinal anesthesia is an automatic response to the increase in C-sections, since spinal anesthesia is routinely used during planned C-section. Finally, the likelihood of having an episiotomy,[46] a surgical procedure used at vaginal birth (column 5), drops by 5.7 percentage points, which is expected, since this procedure is not necessary during a C-section.

In Sweden, a planned C-section due to a breech pregnancy is usually scheduled 7-10 days before the expected date of delivery (determined by last date of menstruation and ultrasound). This means that having a planned C-section may decrease the gestational length, which could

[43] Similarly, the information shock suggests that planned C-section was the preferred delivery mode for singleton breech birth at term only, which is why we expect to see no impact on either breech twins or preterm breech. Consistent with this, no significant impact is found for either of these two groups. These results are presented in Table A1.

[44] There is an ongoing debate regarding the use of higher polynomials greater than two; see Gelman and Imbens (2014) for a detailed discussion.

[45] Three hundred days is the optimal bandwidth that minimizes the mean squared error suggested by Calonico et al. (2014).

[46] Surgical incision made in the perineum to widen the opening of the vagina for a faster delivery.

affect the fetal growth in the late stage of the pregnancy (Borra et al., 2014).[47] In Panel C, I investigate how the information shock affected gestational age in weeks (column 1), birth weight in kilograms (column 2), head circumference in centimeters (column 3), and length of the baby in centimeters (column 4). I find that gestational age decreases by nearly 0.136 weeks, and the baby's length decreases by 0.124 centimeters on average. This result should be considered when investigating the impact of planned C-section on child health.

Validity of the first stage

There are a number of potential threats to the identification. First, a potential issue is that the information shock could have led to changes in the frequency of breech births being reported. Similarly, if there were fewer attempts to turn the fetus to a normal position (external cephalic version) due to the information shock, the proportion of breech births would increase and possible selection issues could arise. By examining the proportion of breech births around the time of the shock I may alleviate this concern. In Figures 3 and 4, the proportion of breech births is presented. The trend in the proportion of breech births exhibits a highly constant development over time including the time of the information shock in 2000. Additionally, this is formally tested in Table 5, which confirms that the proportion of breech births remained unchanged at the time of the information shock.[48] Moreover, a McCrary test shows no evidence of a discontinuity in the number of breech births at the time of the information shock. The McCrary regression result for the information shock is -0.023 (se 0.13) and is visually presented in Figure A5.

Second, the absence of maternal selection to C-sections is important for the validity of the instrumental variable approach as well as for the interpretation of the reduced form treatment effect. Maternal selection to C-section can be both demand and supply driven and it may be difficult to distinguish between the two. However, demand-driven selection—that is mothers' demand for C-section after having accessed new information on the preferred delivery mode for breech births—can be considered less likely given that the information shock was targeting OB/GYNs during an internal meeting and, to the best of my knowledge, not announced to the public media. Also, the medical society reacted to preliminary findings of two studies, not

[47] Birth weight (as well as other growth indicators) is positivity correlated with later-life outcomes including education and income (Bharadwaj et al., 2017).

[48] Manipulation of the running variable is less likely for several reasons: the fertility decision was made before any knowledge of the information shock was available: it is unlikely that women would be able to delay or move the delivery to an earlier date: the preliminary results presented to the medical society were not announced in the public media.

yet available to the public. Breech presentation is discovered late in the pregnancy, making it difficult to plan ahead such that residential sorting can be considered less likely. Nevertheless, maternal selection is further examined.

To investigate maternal selection, a balancing test of covariates across the discontinuity is conducted, testing for compositional changes by running regressions with maternal characteristics as the outcome variables and the information shock as the explanatory variable. The results are presented in Panel A, Table 6 and suggest that for breech mothers, there was no significant change in observable maternal characteristics such as age, height, weight, educational attainment, labor income, or sickness benefits before birth at the time of the information shock.[49] I also test for compositional changes among maternal characteristics on mothers receiving a planned C-section, presented in Panel B, Table 6. Similarly, no significant impact on maternal characteristics is observed across the information shock. In addition, I regress the likelihood of having a planned C-section on a fully interacted model, in which the maternal characteristics are interacted with the treatment status (post information shock). The results are presented in Table A2 and show that none of these interactions are significantly different from zero except for height. The conclusion from these exercises is that based on observable characteristics, I find no persistent evidence in favor of changed maternal characteristics. This can alleviate concern to some extent regarding both selection and demographic changes at the time of the information shock.

Finally, I conduct placebo regressions for examining discontinuities at other points in the distribution of the running variable. More specifically, by using a bandwidth of 12 months before and after the placebo date, I examine whether there are any discontinuities in the proportion of planned C-section on 25 of August in one to three years before or after 2000. By doing this, I also check for seasonality in planned C-section to rule out that planned C-sections usually increase during this time of year. The results are presented in Table 7 and show no signs of significant changes in the probability of planned C-section at any of the placebo dates.

Heterogeneous effects of the first stage

In this section, I examine heterogeneous effects of the information shock on the probability of planned C-section for different subgroups of birth order, age interval, educational level, and

[49] Since fertility choices are made nine months before delivery, it is unlikely that the information shock would have caused demographic changes.

body mass index (BMI) classification.[50] To understand the treatment effect, it is important to analyze which women (with breech births) were more likely to have a C-section due to the information shock—in other words, to identify whether the marginal woman can be confined to a specific high- or low-risk type indicated by observable characteristics.

Heterogeneous effects of the information shock across birth order, age, BMI, and socioeconomic status (SES) indicated by educational level are examined and presented in Table 8.[51] The heterogeneous analysis across birth order, presented in Panel A, Table 8 (columns 1-3), shows a positive significant impact on both first- and second-time mothers. The treatment effects across age groups are presented in Panel A, Table 8 showing a significant impact for all age groups except for women age 35 and over (columns 4-8).

The treatment effects across educational levels is presented in Panel B, Table 8. The estimates indicate that the treatment effect is significant across all educational levels (columns 1-3), primary, secondary and tertiary education, with estimates of 0.090, 0.107, and 0.144, respectively. While the magnitudes across educational levels differ slightly, with higher likelihood of planned C-section for women with more education, these differences are statistically different only for mothers with secondary and tertiary education. Finally, the treatment effects across BMI classification levels are presented in Panel B, Table 8.[52] These results suggest that the treatment effect is largest among women in the normal weight range and overweight women (columns 5-6). For obese women no significant impact is found, which is expected, since these women are likely to "always-takers". An increase in the probability of planned C-section is identified for first- and second-time mothers, all educational levels, women under age 35, and both normal and overweight women. This indicates that a broad category of women had a planned C-section due to the information shock.

The results suggest that the information shock had less impact on C-section among women at risk (older women and obese women). These women already exhibit a higher rate of planned C-section of approximately 60% and are more likely to be always-takers. Finally, these results suggest that the monotonicity assumption is satisfied, implying that the information shock had either a positive or null effect but never a negative effect on the probability of planned C-section.

[50] BMI classification according to World Health Organization (2000): underweight <18.5, normal range 18.5 to 24.9, overweight 25 to 29.9 and obese ≥ 30.

[51] These observables are chosen because of data availability and because birth order, age, BMI, and SES are considered important determinants for delivery mode (Ecker et al., 2001; Sebire et al., 2001; Sheiner et al., 2004).

[52] The number of underweight women was only 292, so they are not reported separately. The estimated impact on planned C-section among underweight women is 0.067, which is not statistically significant.

5.2 Effects on health, fertility and labor market outcomes

Baseline results

i) Reduced form analysis

The baseline results on child and maternal health, subsequent fertility outcomes, and labor market outcomes are presented in Table 9. For each specification, I present estimates for the full sample and first-time mothers. The reason for focusing on first-time mothers, in addition to the full sample, is that future fertility outcomes could be endogenous to the first birth. The effect of the information shock is estimated according to Equation 1 and includes a full set of covariates and fixed effects.

Starting with the impact on child health presented in Table 9, the reduced form effect of the information shock suggests a significant increase in child health for the full sample of 0.104 standard deviations (SD) (Panel A, column 1). For first-time mothers, the estimate is 0.087 SD (Panel B, column 1). The health outcomes are further scrutinized separately and presented in Panel A, Table 10. When looking at the separate outcomes of the child index, there seems to be a consistent pattern of improved infant and child health by higher Apgar score, lower probability of low Apgar score, lower infant mortality, and fewer nights hospitalized within the first year of life and during ages 1-7. Yet the only significant effects (below 5% significance level using conventional p-values and below 10% using FDR), seem to originate from a higher level in absolute Apgar score by 0.07 unit change (column 1) and lower number of nights hospitalized during ages 1-7 by 0.67 nights (column 5). This result is interesting, since it suggests that the information shock improved both short- and long-term health for children.

Turning to the effects on maternal health, presented in Table 9, the results for the full sample and first-time mothers do not show any significant impact on maternal health (column 2, Panels A-B). The maternal health outcomes are presented separately in Panel B, Table 10. While these separate estimates indicate a lower risk of sepsis and postbirth hospital nights (re-admission), none of these effects are significantly different from zero, meaning that we cannot infer any significant impact on maternal morbidity. For women having at least one more birth within eight years after her breech birth, potential effects on maternal health outcomes are analyzed in Table 9 (column 3). The estimated effect on maternal health at subsequent births suggests no significant impact for either the full sample or first-time mothers. When examining each outcome separately in Panel C, Table 10, no significant impact can be found for any of the outcomes.

The impact on future fertility outcomes is investigated and presented in Table 9 (columns 4-6). These estimates suggest a negative but insignificant impact on future fertility measured by total number of future births and a binary measure of the probability of not having another birth. Compared with the mean of the outcome in the pretreatment period, the estimates suggest a reduction in fertility by 4.5%, but insignificantly so. No significant impact is found for birth spacing.

The effects on the labor market index are explored and presented in Table 9 (column 7).[53,54] No significant impact is found for either the full sample (Panel A) or for first time mothers (Panel B). In Table 10, the incomes from labor earnings, sickness benefits, and parental benefits within five years after giving birth are examined separately.[55,56] The results suggest that there was no significant impact on any of the labor market outcomes. In addition to using the average impact within five years from giving birth, event studies are carried out, examining the impact for each year separately, one through five years after the information shock, presented in Figure A6. Similarly to the previous results, no significant effect is found on any of the labor market outcomes for either the full sample or first-time mothers.

The results are further analyzed using alternative functional form and period presented in Table 11, such that all results are reestimated using quadratic and cubic trends (Panels A-B), triangular kernel (Panel C), and for a smaller window of 12 months before and after the information shock (Panel D). These results show that the estimates on child health (ranging from 0.117 to 0.194 SD) are consistent across specifications (and to the baseline specification of 0.104 SD). Similarly to the baseline results, the impacts on fertility, maternal health, birth spacing, and labor market outcomes remain insignificantly different from zero regardless of the specification.

ii) 2SLS results

The reduced form estimates capture the overall impact of the information shock on all breech births. In addition, I want to capture the causal impact of C-section using the information

[53] C-section alone does not qualify a woman for sickness benefits from the Swedish Social Insurance Agency. Moreover, the level of sickness and parental benefits depend on labor income (individually set in proportion to the labor earnings the year before giving birth). However, all Swedish residents receive a minimum amount of benefits when sick or becoming a parent.

[54] Three sources of income are analyzed: labor income, income from sickness benefits and income from maternity leave. Unfortunately, data on labor supply (e.g., working hours) are not available. Income data are available only on an annual basis. I am currently in the process of accessing new data with more suitable labor market outcomes to study the full implications from the increase in C-sections.

[55] Similar results are found for three, five and seven year averages after giving birth.

[56] All income variables are transformed using inverse hyperbolic sine transformation, providing a similar interpretation as log transformation of percentage change.

shock as an instrument. Provided that the exclusion restriction is satisfied, in other words, that the information shock affects outcomes only through higher likelihood of C-section, the impact of C-section attributed to the shock (the effect on complying women) can be estimated using 2SLS. The 2SLS model is estimated according to Equation 2 and is presented in Table 12.

The 2SLS results for the full sample (Panel A) suggest that child health significantly improved by 0.93 SD for the full sample of those who obtained a C-section due to the information shock and by 0.68 SD for first-time mothers. If we compare the estimated effects on child health to the gap in health between breech and normal position births in the pretreatment period, having a planned C-section would improve health beyond this gap.

The 2SLS estimates are presented in Table 13, for each outcome separately, showing that the improvement in child health is mainly driven by higher Apgar score (Panel A, column 1) by 0.58 units and fewer nights spent at the hospital (Panel A, column 5) by 5.9 nights, both significant below a 5% level when considering the conventional p-values. When correcting for false discovery rate, however, none of the estimates remain significant. Nevertheless, the estimates are all large in magnitude and show positive improvements in child health for each separate outcome.

No significant effects are found for maternal health at birth or at any subsequent births, future fertility, or maternal labor market outcomes for either the full sample or first-time mothers (Panels A-B, Table 12, columns 2-7). Similarly, no significant impact on any separate outcome is found, as presented in Table 13. Nonetheless, the fact that the magnitude is large—that is, no precisely estimated zero effects—makes the interpretation difficult. A longer discussion is provided in Section 6. These results are insensitive to choice of polynomial, kernel, and time period, as presented in Table A3, with the exception of estimated impact on the child health index, Panel D (column 1), which exhibits a larger magnitude than the baseline.

Alternative strategy

As an alternative approach for dealing with issues including other interventions occurring at similar time for all births, demographic changes, correlation between season of birth, and maternal and child characteristics, I report the Difference-in-Difference (DiD) estimates, comparing outcomes for breech births with those for normal position births before and after the information shock. While there are multiple plausible identification strategies, I conduct a DiD analysis to examine the consistency of the estimates across models.

The DiD is estimated according to:

$$Y_{it} = \gamma_1 + \gamma_2(Breech \times InfoShock)_{it} + \gamma_3 Breech_i + \pi_t + \boldsymbol{X}_{it}\mu + \varepsilon_{it} \quad (3)$$

in which $Breech$ is a binary variable equal to one if breech and zero if singleton normal position birth at term. γ_2 is the parameter of interest (DiD estimate), capturing the relative change in outcomes for breech births compared with normal position births due to the information shock. The interaction term $Breech \times InfoShock$ is equal to one if a birth is a breech birth born post the information shock and zero otherwise. π indicates day-month-year fixed effects, accounting for time factors. The vector of maternal and child control variables $\boldsymbol{X}_{it}$ and the error term ε are handled analogously to Equation 1.

Additionally, I combine the IV strategy with the DiD approach using the information shock as an instrument for planned C-section and normal position births as a control group.[57] When conducting a pre-post analysis, a possible scenario that would invalidate the chosen identification includes unobserved factors affecting the outcomes as well as treatment. Under the assumption of parallel trends of breech and normal births, the causal impact of the information shock can be captured using a DiD approach. In a pre-post analysis, other interventions are a possible threat to the identification. For instance, if there was a general change in obstetric care, in addition to the information shock, for all births, a DiD approach would account for this.[58] Under the assumption of common trends in the pretreatment period, the DiD approach should provide results consistent with those of the pre-post analysis.

The trust we can invoke in DiD estimates depends on whether the identifying assumption of parallel trends is satisfied. To explore the plausibility of the parallel trend assumption, I test for differences in the pretreatment trends in the outcome variables by conducting multiple event studies. For the event studies, I fully interact a binary indicator of breech presentation with the years before and after the information shock such that each coefficient represents an interaction term between year and breech birth. The year of treatment, 2000, is the omitted base category following general convention. The results are presented in Figures A7 and suggest a highly significant sharp increase in C-sections as well as significant improvements in child health after the information shock but no impact on any other outcomes. Importantly, the event studies also suggest that pretrends in the outcome variables are not significantly different between breech

[57] The DiD results can be compared with the reduced form baseline results (Table 14), and the DiD-IV approach with the IV baseline results (Table 15), provided that the time period is the same across specifications.

[58] The protocol and internal newsletter from SFOG's annual meeting in 2000 do not suggest that any other delivery practices were discussed or changed for either breech or normal births.

and normal positions. That is, out of eight outcomes, only one coefficient for one outcome (maternal health) is significantly different from zero. Hence, these tests indicate that there are common trends in the outcome variables in the pretreatment period.

The DiD estimates are presented in Table 14, suggesting a strong significant increase in probability of C-section by 15.7 percentage points for the full sample (Panel A, column 1) and by 17.4 percentage points for first-time mothers (Panel B, column 1). These estimates are somewhat higher than those of the pre-post analysis (11 percentage points). The impact on child health suggests a positive impact of 0.094 SD for the full sample (Panel A, column 2) and 0.1 SD for first-time mothers (Panel B, column 2). The overall results support the findings in the baseline model. The magnitude of the child health estimates is similar to that of the pre-post analysis but the DiD estimates exhibit a slightly higher effect size and precision for first-time mothers. Unlike the pre-post analysis, a marginal significant negative impact on fertility is found for the full sample as well as first-time mothers, by 0.019 and 0.026 births, respectively, which corresponds to a reduction in future fertility by 2-3% when compared with the mean of the dependent variable. Similarly to the pre-post analysis, no significant impact is found for birth spacing, maternal health, or labor market outcomes. In addition, I include breech-specific pretrends, Table A4, which shows similar results to the DiD estimates without the trends but with slightly larger estimates for child health (0.139 and 0.117 SD) and fertility outcomes (-0.042 and -0.044).

Turning to the combined 2SLS model and DiD estimates presented in Table 15, in which the information shock is used as an instrument for planned C-section and normal position births are used as a control group, these estimates suggest that planned C-section strongly improves child health by 0.6 SD for the full sample and 0.58 SD for first-time mothers. As with the baseline 2SLS results, the magnitude of the estimates is large and suggests that planned C-section would more than compensate for the difference between breech and normal positions in the pretreatment period. There is also a marginally significant negative impact on the number of subsequent births (0.12 for the full sample and 0.15 for first-time mothers), suggesting a reduction in future fertility (by 19% and 14%, respectively, when compared with the control mean).

In summary, using an alternative estimator of DiD, with normal position births as a control group, confirms the findings of the pre-post analysis while providing slightly more precision.

5.3 Additional robustness and sensitivity

A number of robustness and sensitivity checks are conducted for the baseline reduced form results. I consider alternative dates for the information shock using alternative cutoffs, including the date of publication of the Term Breech Trial and the date of the extraordinary Nordic OB/GYN meeting held following the publication of the Term Breech Trial.[59] To do this, I first remove observations between the annual meeting of SFOG in August and each of the alternative dates separately, since these births are at least partially treated in line with the documented response from the medical society according to Alexandersson et al. (2005). First, I use the date of the publication of the Term Breech Trial by Hannah et al. (2000), presented in Panel A, Table A5, and show that the results are robust but with a slightly lower estimate for child health of 0.08 SD (compared with the baseline of 0.10). Then I use the date of the extra OB/GYN meeting held in December 2000, presented in Panel B, which suggests that the results are similar to the baseline results but with a marginally significant negative estimate for the number of future births by 0.042.

Because of data availability, I use the discharge date from the maternity unit minus the number of average hospital nights for the corresponding delivery mode (four nights for C-section and two nights for vaginal delivery) as an approximation of the date of birth. This procedure may, however, result in a measurement error. To deal with this potential issue, I exclude a small window across the information shock, dropping births one week before and after the shock. The results are presented in Panel C, Table A5, and are similar to the baseline results, showing a positive significant impact on child health by 0.097 SD (compared with the baseline 0.104 SD).

The trends in C-section by county in Figures A1, A2 and A3, suggest variation in the response to the information shock across counties. This leads to the question of whether there is in fact a stronger effect in counties that exhibit the greatest increase in C-sections. To address this question first, I omit all but five of the largest counties,[60] which exhibited the greatest increase in C-sections. The results are presented in Panel D Table A5 and provide similar estimates to the baseline results (child health 0.131). These results suggest that even in counties with a greater increase in C-sections, the impact on maternal health and labor market outcomes remains insignificant. Second, I control for county-specific split trends. The results are presented in Panel E, Table A5 and show that the results are robust to the inclusion

[59] An extraordinary Nordic OB/GYN meeting was held in December 2000 in Stockholm as a result of the publication of the Term Breech Trial by Hannah et al. (2000) (Alexandersson et al., 2005).

[60] Stockholm, Västragötaland, Skåne, Jönköping, and Halland.

of trends but with higher precision for the fertility outcome, showing a marginally significant negative impact on the number of future total births by 0.046.

Finally, when constructing the summary index according to Anderson (2008), more weights are attached to variables with less correlation to the other variables within that category (that is, variables with "new information" are given more emphasis). The results are reestimated for the indices but now summarized using uniform weights and presented in Panel A, Table A6. Using equal weights provides results similar to the baseline results. For child health, the estimated effect by 0.087 SD is slightly smaller compared with the baseline (0.104 SD). Turning to the variables constructing the index, if binary measures of hospitalization (equal to one if hospitalized at least one night and zero otherwise) are used instead of continuous measures (number of nights hospitalized), how would it affect the estimates? The results, presented in Panel B, Table A6, are similar to the baseline results but with a slightly smaller estimated effect of 0.081 for the child health index. When further examining the binary measures of hospitalization, presented in Panel C, Table A6, the results yield smaller estimates with less precision for child health compared with the main results (see Table 10). That is, it is the number of total hospital nights that is affected by the information shock rather than the likelihood of being hospitalized at least one night.[61]

6 Discussion and interpretation of the results

Previous studies have suggested that health at birth for breech babies is improved when delivered by planned C-section (Hannah et al., 2000; Herbst, 2005; Herbst and Thorngren-Jerneck, 2001; Jensen and Wüst, 2015). In line with this literature, the findings in this study suggest that child health among singleton breech births is improved by planned C-section as the delivery mode. The improvements in child health stem from higher Apgar scores and also a decrease in the number of nights hospitalized during ages 1-7, implying both short- and long-term positive impacts on birth and childhood health. The long-term impact, up to age 7, is an important aspect not previously studied. To interpret the impact of the reduced form estimates on a general index of child health, planned C-section closes the gap in child health between children in breech and normal positions by nearly 100%.[62]

[61]Further analysis on the impact of the information shock on nights hospitalized during childhood is provided in Panel A, Table A7, which shows a negative and significant impact of nights hospitalized in ages 0-3 and onward among breech babies. No impact (as expected) is found among normal position babies.

[62]The unconditional difference in means in the child health index between breech and normal position births, before the information shock, is approximately 0.1 SD.

The 2SLS estimates suggest that planned C-section among breech births leads to higher Apgar scores by the magnitude of a 0.58 unit increase (a magnitude similar to that of Jensen and Wüst (2015))[63] and 5.9 fewer nights hospitalized during ages 1-7. In terms of economic cost per child, 5.9 fewer hospital nights would, save, on average, 14,400 SEK per night and 84,960 SEK in total. Regarding the improvements in Apgar score, while it is hard to evaluate the economic meaning of a 0.58 unit increase in Apgar score in the absolute level, the Apgar score is a good predictor of health during infancy and childhood, including infant mortality and neurological disorders during childhood (Casey et al., 2001; Li et al., 2011; Moster et al., 2001). There is a positive association between Apgar score and cognitive development (Odd et al., 2008). Figlio et al. (2014) find that a 1-unit increase in Apgar score maps to 0.8 SD higher average test scores in reading and math.

In line with the findings of previous studies, Hannah et al. (2000) and Jensen and Wüst (2015), I find no significant impact on maternal health at birth.[64] Moreover, I find no evidence of a significant impact on maternal health at future births, which is of particular interest, since these outcomes have not previously been examined causally. However, one has to consider that any future birth is endogenous to the previous, such that the impact on health outcomes at future birth is relevant to a selected sample and may not reflect the effect of planned C-section if all women had a future birth. While I find no evidence of a significant impact on maternal health at birth or future births, the magnitudes of the estimates, and the 2SLS estimates in particular, are not precisely estimated zeros. Although I cannot infer whether precision would increase with a larger sample size, the estimates are sensitive across specifications and never marginally significant, which suggests that the estimates may not be interpreted as different from zero.

The results regarding future fertility (in terms of both the total number of subsequent births and the probability of not having another birth within a fixed time period of eight years) suggest a negative impact on future fertility, yet not significantly different from zero. These estimates are, however, consistent across most specifications and marginally significant in some specifications (i.e, when controlling for county-specific pretrends, using alternative dates of the information shock and in the DiD model). That is, we cannot rule out that there may be a negative impact on subsequent fertility. While a null result on future fertility is in line with

[63] The 2SLS estimate for Apgar score is 0.42 in Jensen and Wüst (2015).

[64] Jensen and Wüst (2015) find that C-section leads to a longer hospital stay after giving birth by 2.4 nights but has no impact on infections or other complications. Because of data availability, my outcome consists of readmission to the hospital within the first year of birth, and thus my result on hospitalization cannot be compared with that of Jensen and Wüst (2015).

Hannah et al. (2004), these results are not completely comparable since my time period is four times longer than that of Hannah et al. (2004), who examine fertility within two years from giving birth. Likewise, the study by Halla et al. (2016) examines the impact of emergency C-section for low-risk births, which not only applies to a different population but also considers a different treatment (emergency C-section is associated with greater risks for mother and child compared with planned C-section).

Finally, I find no impact on maternal labor market responses, which may be for multiple reasons. Halla et al. (2016) hypothesize that lower future fertility is the main channel explaining their findings of higher female labor market participation after having a C-section in the Austrian setting. My results, however, do not suggest a highly significant impact on future fertility. Even if there were a negative impact on future fertility, it does not necessarily translate into a positive impact on labor outcomes because of differences in a potential “childbearing penalty” on the Swedish labor market compared with the Austrian setting.

The results from this exercise should be interpreted with regard to the risk margin of women delivering with planned C-sections due to the information shock. Among singleton breech births, 47% were already being delivered with planned C-sections before August 2000. Therefore, the marginal births were most likely not the highest risk births since the proportion of planned C-sections among obese or older women was already high, and no significant effect of the information shock was found on these groups. It is therefore noteworthy that the impact on child health is substantial—especially since the increase in planned C-sections appears to be attributed to fewer vaginal births rather than emergency C-section.

Regarding external validity, when interpreting the results, one should also consider the fact that breech births constitute a particular high-risk group. The effects from marginal planned C-sections are compared with those of high-risk vaginal births within this group, which may not be generalizable to births with normal presentation. While we cannot extrapolate the results regarding improved child health from more planned C-sections to normal births, it is plausible that medical interventions improving health at birth could have long-term consequences for child health (in terms of lower morbidity). Finally, breech births constitute a fairly large group across most populations worldwide, which can be easily identified. Breech births are a continuous high-risk group in need of extra medical interventions. Yet the preferred delivery mode continues to be a controversial topic with substantial variation in planned C-sections across industrialized countries. The findings in this paper are thus policy relevant, suggesting that countries with lower proportions of planned C-sections among breech births could improve child health.

By doing a simple back-of-the-envelope calculation, the costs of having a planned C-section can be explored. In Sweden, the average cost of a planned C-section ranges from 54,135 to 88,635 SEK, depending on how complicated the procedure is compared with 30,984 to47,572 SEK per vaginal birth.[65] The average cost of inpatient care per hospital night for children is 14,400 SEK. While switching from vaginal birth to planned C-section would increase the cost at birth (by 12,000 SEK per night), taking into account the reduction in hospitalization during childhood plus the average number of extra hospital nights for mothers who had a C-section, would save as much as 19,897 to 54,397 SEK per birth.[66]

7 Conclusion

In this study, I have presented evidence that an increase in planned C-sections, among the high-risk group of breech births, can lead to significant improvements in child health without affecting maternal health at birth or any subsequent births or labor outcomes, but with a potential negative impact on future fertility. To overcome the intrinsic endogeneity issue of selection into C-section, I use exogenous variation from an information shock of new scientific evidence to the medical society. This shock led to a precipitous rise in planned C-sections for breech births by 23%. By using detailed Swedish register data for the time period 1997-2003, I use this shock in a reduced form pre-post analysis and as an instrumental variable in a 2SLS model. The detailed Swedish register data enables me to examine the impact of planned C-section on a broader set of outcomes not previously examined as well as for a longer time period.

The increase in planned C-sections appears to originate from fewer vaginal births rather than emergency C-sections. No impact on planned C-sections is found for normal position births. Importantly for identification, I find no evidence of any changes in composition among mothers receiving planned C-sections following the information shock. Moreover, I find no change in the proportion of breech births reported (i.e., no manipulations) or any discontinuities at other placebo dates.

The reduced form results show that the increase in planned C-section, following the information shock, closed the gap in child health between breech and normal presentation births

[65]Information regarding average costs can be found in Table A8. The cost of each procedure depends on whether the birth is complicated. There is no standard rate for a breech vaginal birth. However, a breech vaginal birth is considered complicated and in need of extra resources such as a senior OB.

[66]Since I cannot measure whether planned C-sections leads to longer hospitalization for mothers, I use the average hospital stay for a C-section, which is two nights longer for a C-section than for a vaginal birth.

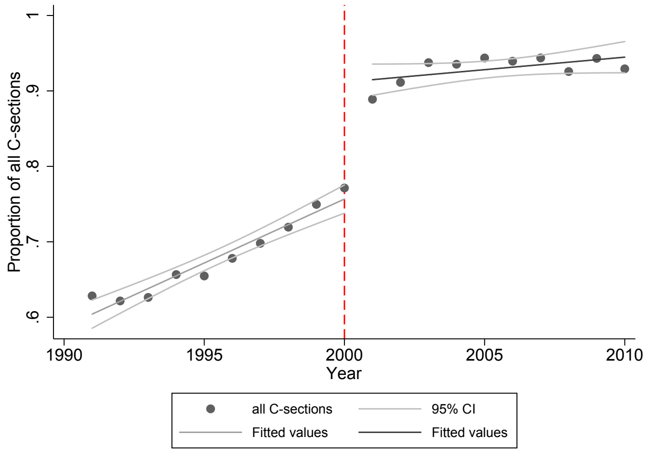
nearly completely. Using a general index on health outcomes, child health is improved by 0.10 SD. These improvements are found both immediately at birth (higher Apgar score by 0.07) and during childhood (fewer nights hospitalized, during ages 1-7 by 0.67). Turning to the 2SLS estimates, planned C-section (among complying mothers) leads to a strong improvement in the child health index by 0.93 SD. This improvement includes a 0.58 unit increase in Apgar score and a reduction in hospital stay by nearly 6 nights. No significant impact was found for maternal labor market outcomes, maternal morbidity at birth, or maternal morbidity at any future births. Although the estimates on future fertility are insignificant in most specifications, because the estimates are consistent across specifications and methods, and in some specifications marginally significant, a potential reduction in future fertility cannot be ruled out. These results are robust to a number of robustness and sensitivity checks, including alternative specifications (using quadratic trends, cubic trends, triangular kernel, and a smaller window of time), nonweighted indices, and the use of DiD design using births with normal fetal position as controls.

This study shows how increased use of C-section among breech births can improve child health in both the short and long run, implying that improved health at birth has a lasting impact during childhood.

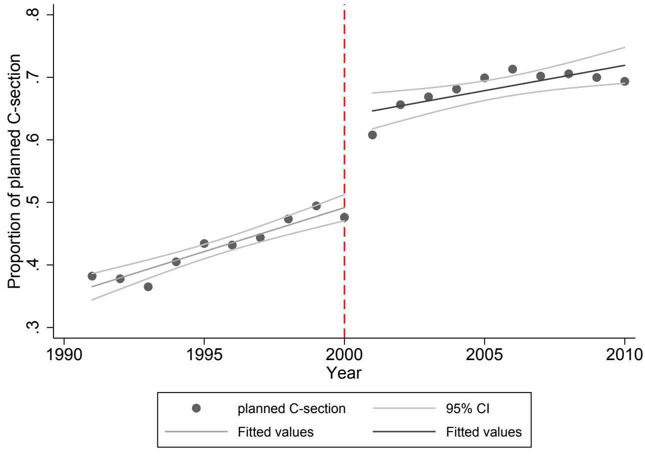
8 Figures and Tables

Figure 1: Yearly trends in C-sections for breech births

(a) Yearly data, all C-sections



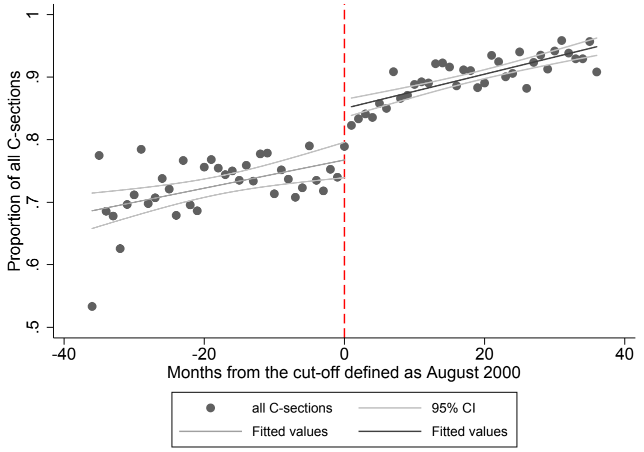
(b) Yearly data, planned C-section



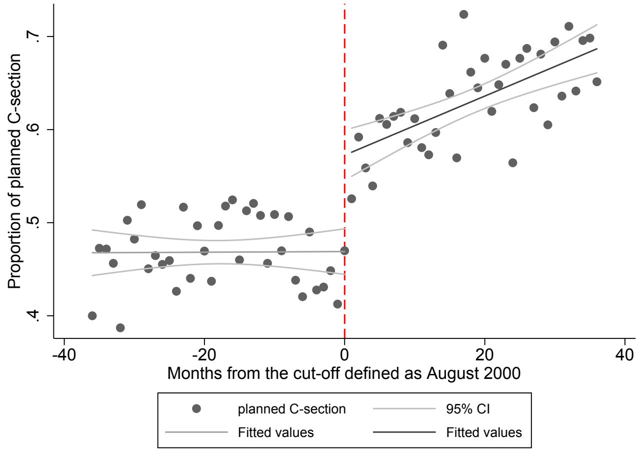
Note to Figure 1: The data are obtained from the Swedish Medical Birth Registry. Annual trends in all C-sections and planned C-section among singleton breech births at term ($\geq$37 gestational weeks) are presented in Figures 1a and 1b. The red vertical line indicates the date of the information shock to the Swedish medical society.

Figure 2: Monthly trends in C-sections for breech births

(a) Monthly data, all C-sections

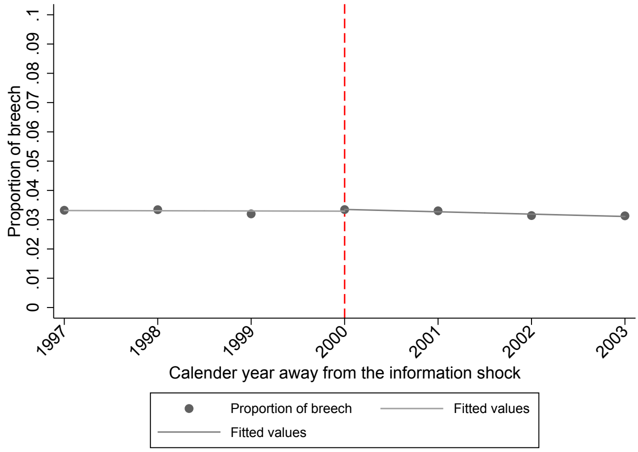


(b) Monthly data, planned C-section



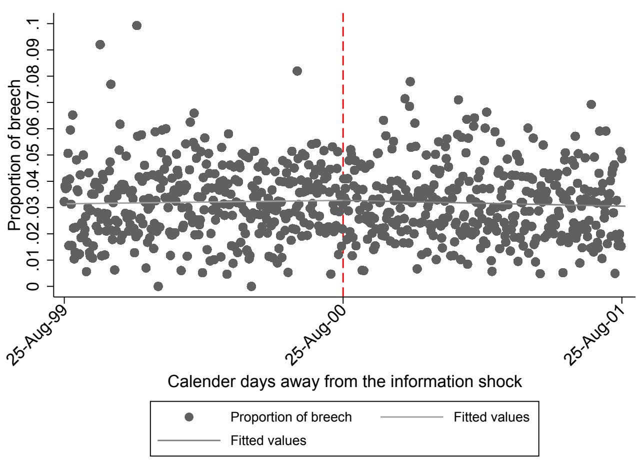
Note to Figure 2: The data are obtained from the Swedish Medical Birth Registry. Monthly trends in all C-sections and planned C-section among singleton breech births at term ($\geq$37 gestational weeks) are presented in Figures 2a and 2b. The red vertical line indicates the date of the information shock to the Swedish medical society.

Figure 3: Annual trend in the proportion of breech births



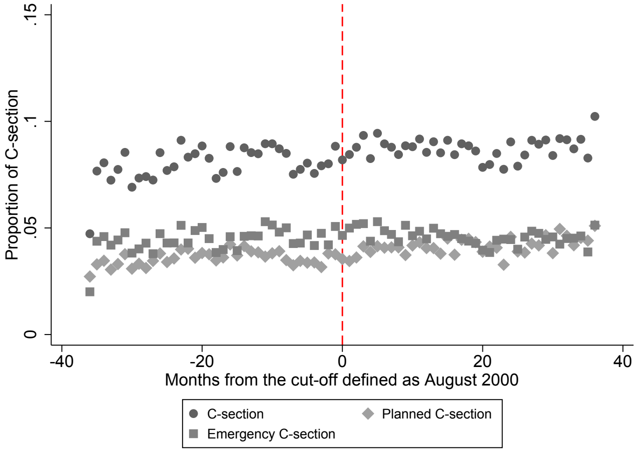
Note to Figure 3: The data are obtained from the Swedish Medical Birth Registry. Annual trend in the proportion of singleton breech births at term (≥37 gestational weeks) is presented in Figure 3. The red vertical line indicates the date of the information shock to the Swedish medical society.

Figure 4: Daily trend in the proportion of breech births



Note to Figure 4: The data are obtained from the Swedish Medical Birth Registry. Daily trend in the proportion of singleton breech births at term (≥37 gestational weeks) is presented in Figure 4. The red vertical line indicates the date of the information shock to the Swedish medical society.

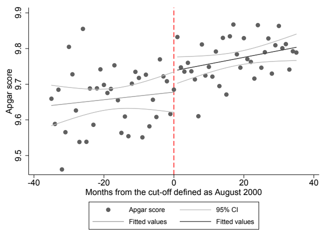
Figure 5: Monthly trends in C-sections for normal position births



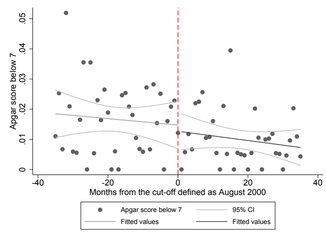
Note to Figure 5: The data are obtained from the Swedish Medical Birth Registry. Monthly trends in emergency and planned C-sections among singleton normal position (cephalic) births at term ($\geq$37 gestational weeks) are presented in Figure 5. The red vertical line indicates the date of the information shock to the Swedish medical society.

Figure 6: Child health outcomes

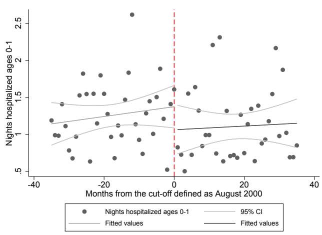
(a) Apgar score



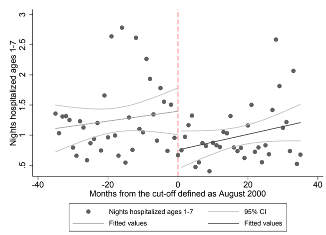
(b) Apgar score below 7



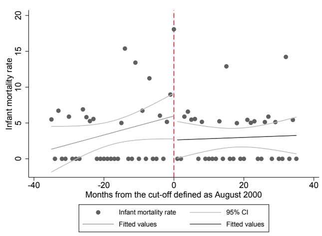
(c) Nights hospitalized ages 0-1



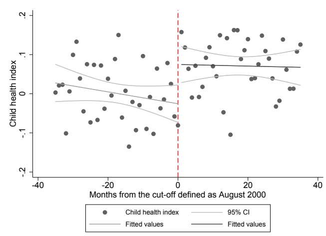
(d) Nights hospitalized ages 1-7



(e) Infant mortality rate



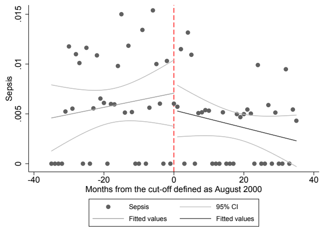
(f) Child health index



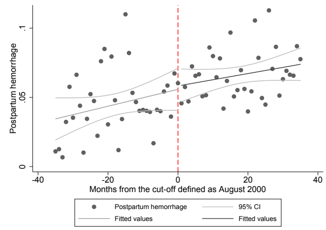
Note to Figure 6: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies. Monthly trends in child health outcomes among singleton breech births at term ($\geq$37 gestational weeks) are presented in Figure 6. The red vertical line indicates the date of the information shock to the Swedish medical society.

Figure 7: Maternal health and labor outcomes

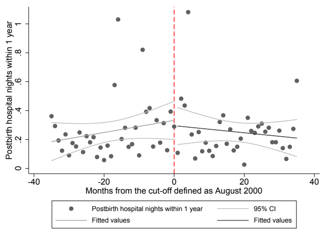
(a) Maternal sepsis



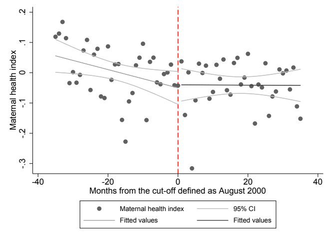
(b) Postpartum hemorrhage



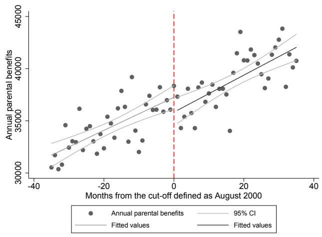
(c) Postbirth hospitalization



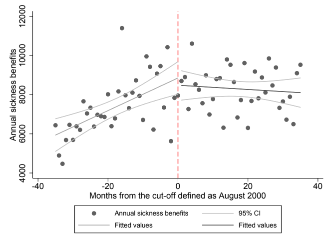
(d) Maternal health index



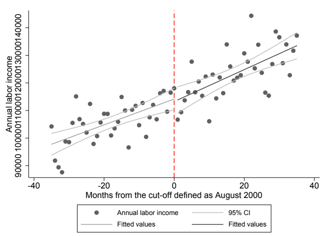
(e) Annual income from parental benefits



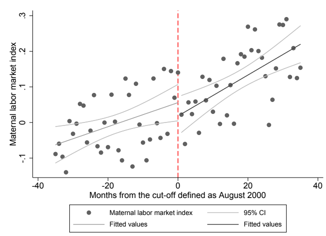
(f) Annual income from sickenss benefits



(g) Annual income from labor earnings



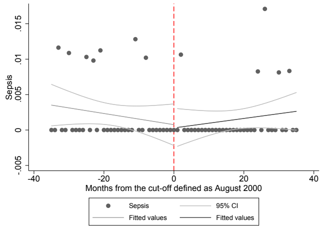
(h) Maternal labor index



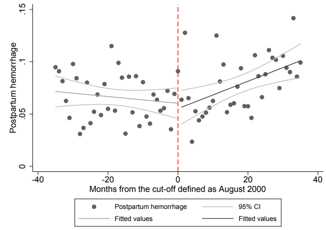
Note to Figure 7: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies. Monthly trends in maternal health outcomes among singleton breech births at term ($\geq$37 gestational weeks) are presented in Figure 7. The red vertical line indicates the date of the information shock to the Swedish medical society.

Figure 8: Trends in maternal health outcomes at subsequent births and subsequent fertility

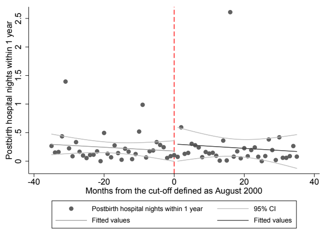
(a) Maternal sepsis



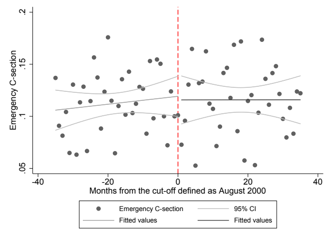
(b) Postpartum hemorrhage



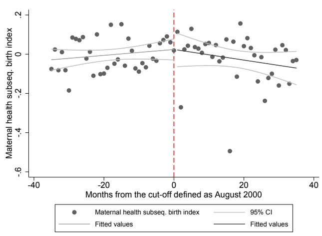
(c) Postbirth nights hospitalized



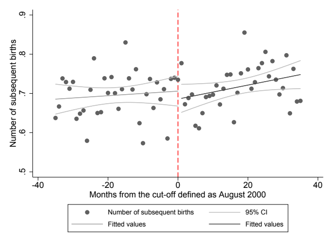
(d) Emergency C-section



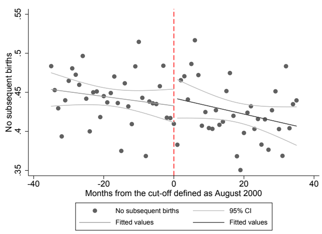
(e) Maternal health index



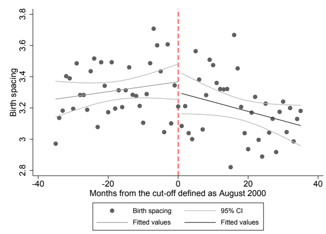
(f) Number of subseq. births within 8 years



(g) No more births within 8 years



(h) Birth spacing



Note to Figure 8: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies. Monthly trends in maternal outcomes, among mothers with a previous breech birth, are presented in Figure 8. The red vertical line indicates the date of the information shock to the Swedish medical society.

Table 1: Impact of information shock on the probability of C-section

	Breech Births		Normal Position Births	
	(1) Planned C-section	(2) Planned C-section	(3) Planned C-section	(4) Planned C-section
Information shock	0.113***	0.112***	0.001	0.001
	(0.018)	(0.018)	(0.001)	(0.001)
Mother weight		0.001***		0.001***
		(0.000)		(0.000)
Mother height		-0.004***		-0.002***
		(0.001)		(0.000)
Malformation		-0.029*		0.013***
		(0.016)		(0.002)
Smoking 1st trimester		0.015		0.004***
		(0.014)		(0.001)
Male		0.011		0.002***
		(0.008)		(0.001)
Native		0.059***		0.003***
		(0.013)		(0.001)
Income		0.000**		0.000*
		(0.000)		(0.000)
Sickness benefits		0.000***		0.000***
		(0.000)		(0.000)
Fixed effects	NO	YES	NO	YES
F-stat	39.42	39.12	0.45	0.20
R^2	0.029	0.063	0.000	0.024
Obs	13,208	13,208	406,448	406,448
Mean of dep. var.	0.555	0.555	0.039	0.039

Note to Table 1: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Each column presents a separate regression with OLS estimates of the impact of the information shock on the probability of planned C-section for breech births (columns 1-2) and for normal position (cephalic) births (columns 3-4). Only singleton births at term (≥37 gestational weeks) are considered for analysis. Linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in columns 2 and 4. Standard errors are clustered at day-month-year level. * p<0.1, ** p<0.05, *** p<0.01.

Table 2: Summary statistics

	Breech births			***Normal position (cephalic) births***			t-test	
	mean	sd	N	mean	sd	N	t-test	p-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Child characteristics and birth outcomes								
Birth weight (kg)	3,386.615	481.453	13,173	3,634.138	493.203	405,432	56.730	0.000
Gestation (week)	38.690	1.211	13,208	39.723	1.292	406,448	90.656	0.000
Male	0.458	0.498	13,208	0.515	0.500	406,446	12.894	0.000
Malformation	0.084	0.277	13,208	0.031	0.172	406,448	-34.145	0.000
Apgar score	9.718	0.851	13,108	9.766	0.695	403,893	7.719	0.000
Apgar score below 7	0.013	0.114	13,108	0.008	0.088	403,893	-6.834	0.000
Infant mortality rate	3.180	56.303	13,208	1.351	36.727	406,448	-5.517	0.000
Nights hospitalized ages 0-1	1.180	6.529	13,158	0.818	5.600	405,271	-7.252	0.000
Nights hospitalized ages 1-7	1.102	6.773	13,158	0.918	6.971	405,271	-2.983	0.003
Panel B: Maternal birth and fertility outcomes								
Planned C-section	0.555	0.497	13,208	0.039	0.193	406,448	-278.354	0.000
Emergency C-section	0.266	0.442	13,207	0.045	0.208	406,447	-113.897	0.000
Induction of labor	0.018	0.134	13,208	0.100	0.300	406,448	31.250	0.000
Sepsis	0.005	0.069	13,208	0.001	0.036	406,448	-10.805	0.000
Hemorrhage	0.056	0.230	13,208	0.053	0.223	406,448	-1.793	0.073
Hospital nights (readmission)	0.254	2.489	12,779	0.260	3.917	388,643	0.162	0.871
Sepsis, future	0.073	0.260	7,585	0.052	0.221	205,664	-8.225	0.000
Hemorrhage, future	0.002	0.041	7,585	0.001	0.027	205,664	-3.146	0.002
Hospital nights, future (readmission)	0.220	3.071	7,308	0.213	2.328	194,918	-0.239	0.811
Emergency C-section, future	0.114	0.317	7,583	0.041	0.198	205,633	-30.700	0.000
Number of children	2.281	0.933	13,208	2.536	1.062	406,448	27.195	0.000
Number of of subseq. births	0.708	0.724	13,208	0.647	0.760	406,448	-9.205	0.000
No future births	0.432	0.495	13,208	0.500	0.500	406,448	15.340	0.000
Birth spacing (years)	3.244	1.610	7,585	3.373	1.751	205,664	6.299	0.000
Panel C: Maternal characteristics								
Age	29.876	4.897	13,208	29.610	4.998	406,445	-6.029	0.000
Height	166.442	6.262	12,318	166.432	6.245	376,157	-0.176	0.860
Weight	67.084	12.420	11,739	67.381	12.466	357,865	2.544	0.011
BMI	24.196	4.215	11,437	24.323	4.230	348,132	3.146	0.002
Hypertensia	0.002	0.048	13,208	0.002	0.043	406,448	-1.019	0.308
Smoke 1st trimester	0.116	0.321	12,515	0.113	0.317	382,778	-1.052	0.293
Smoke 3rd trimester	0.073	0.260	8,906	0.081	0.273	281,611	2.708	0.007
Education	4.405	1.365	13,162	4.345	1.387	404,697	-4.895	0.000
Labor income	1,147.921	826.686	13,080	1,000.657	782.827	402,500	-21.135	0.000
Sickness benefits	27.491	94.403	13,080	28.246	83.931	402,500	1.008	0.313
Parental benefits	72.926	131.792	13,080	117.363	152.925	402,500	32.839	0.000
Panel D: Index								
Child health index	0.039	0.931	13,208	0.106	0.724	406,448	10.386	0.000
Maternal health index	-0.020	0.995	13,208	0.016	1.083	406,448	3.844	0.000
Maternal health at sub. birth index	-0.010	1.033	7,585	0.157	0.780	205,664	18.009	0.000
Maternal labor index	0.106	1.062	13,191	-0.003	1.043	405,999	-11.757	0.000

Note to Table 2: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry and Longitudinal Integration Database for Health Insurance and Labor Market Studies. The sample includes singleton term ($\geq$37 gestational weeks) births presented in breech position (columns 1-3) and normal position (cephalic) (columns 4-6) for the time period August 1997 to August 2003. Mean values(columns 1 and 4), standard deviations (columns 2 and 5), t-test (column 7), and p-values (column 8) are displayed.

Table 3: Summary statistics, before and after

	Before	After	t-test
A. Delivery mode			
Planned C-section	0.472	0.649	-16.481
	(0.499)	(0.477)	[0.000]
Emergency C-section	0.290	0.273	1.761
	(0.454)	(0.446)	[0.078]
Induction of labor	0.021	0.008	5.216
	(0.144)	(0.088)	[0.000]
B. Child health outcomes			
Birth weight (kg)	3348.861	3333.626	1.484
	(475.887)	(455.227)	[0.138]
Gestation (weeks)	38.859	38.553	11.602
	(1.284)	(1.115)	[0.000]
Apgar score	9.650	9.793	-7.644
	(0.988)	(0.703)	[0.000]
Apgar score below 7	0.021	0.008	4.666
	(0.142)	(0.092)	[0.000]
Infant mortality rate	3.106	2.725	0.321
	(55.656)	(52.140)	[0.748]
Nights hospitalized ages 0-1	1.136	1.055	0.599
	(5.624)	(6.535)	[0.549]
Nights hospitalized ages 1-7	1.146	0.927	1.545
	(7.788)	(4.894)	[0.122]
C. Maternal health outcomes			
Sepsis	0.005	0.003	1.237
	(0.072)	(0.058)	[0.216]
Hemorrhage	0.039	0.055	-3.460
	(0.193)	(0.227)	[0.001]
Postbirth hospital nights (readmission)	0.262	0.251	0.181
	(3.082)	(2.656)	[0.857]
D. Maternal health outcomes at subseq. birth			
Sepsis	0.064	0.082	-2.707
	(0.245)	(0.274)	[0.007]
Hemorrhage	0.002	0.002	0.221
	(0.045)	(0.042)	[0.825]
Post-birth hospital nights (readmission)	0.246	0.210	0.425
	(2.917)	(3.587)	[0.671]
Emergency C-section	0.121	0.117	0.521
	(0.326)	(0.321)	[0.603]
E. Fertility outcomes			
Number of future births	0.957	0.954	0.176
	(0.697)	(0.684)	[0.860]
No future births	0.238	0.239	-0.082
	(0.426)	(0.426)	[0.935]
Birth spacing (years)	3.201	3.070	3.470
	(1.579)	(1.443)	[0.001]
F. Labor market outcomes			
Labor income	1051.855	1258.342	-10.717
	(768.581)	(955.763)	[0.000]
Sickness benefits	63.842	73.234	-3.149
	(125.486)	(143.190)	[0.002]
Parental benefits	380.435	429.682	-11.491
	(191.588)	(196.572)	[0.000]
G. Index			
Child health index	-0.000	0.080	-3.951
	(1.000)	(0.840)	[0.000]
Maternal health index	0.000	-0.032	1.485
	(1.000)	(0.977)	[0.138]
Maternal health at sub. birth index	-0.000	-0.018	0.708
	(1.000)	(1.069)	[0.479]
Maternal labor index	-0.000	0.215	-9.048
	(1.000)	(1.140)	[0.000]
Observations	3,863	4,403	

Note to Table 3: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies. The sample includes singleton term ($\geq$37 gestational weeks) breech births for first-time mothers before (25 August 1997- 25 August 2000) and after (26 August 2000- 25 August 2003) the information shock. Mean values are displayed, with standard deviations below in parentheses in columns 1-2. T-statistics are reported in column 3, with p-values below in square brackets.

Table 4: Impact of information shock

	Panel A: Effects on planned C-section, alternative spec.				
	(1) Quadratic trends	(2) Cubic trends	(3) Triangular kernel	(4) 1 year	(5) 300-day window
Information shock	0.122***	0.120***	0.114***	0.127***	0.107***
	(0.026)	(0.023)	(0.019)	(0.030)	(0.033)
Fixed effects	YES	YES	YES	YES	YES
F-stat	21.41	26.47	34.51	17.68	10.32
R^2	0.062	0.062	0.063	0.071	0.081
Obs	13,208	13,208	13,194	4,305	3,614
Mean of dep. var.	0.555	0.555	0.542	0.520	0.518

	Panel B: Effects on obstetric outcomes				
	(1) Emergency CS	(2) Induction of labor	(3) Spinal anesthesia	(4) Forceps/vacuum	(5) Episiotomy
Information shock	-0.016	-0.008	0.130***	-0.001	-0.057***
	(0.016)	(0.005)	(0.017)	(0.002)	(0.009)
Fixed effects	YES	YES	YES	YES	YES
F-stat	1.07	2.57	59.69	0.28	36.81
R^2	0.023	0.015	0.120	0.006	0.054
Obs	13,207	13,208	13,208	13,208	13,208
Mean of dep. var.	0.266	0.018	0.639	0.002	0.044

	Panel C: Effects on birth outcomes			
	(1) Gestation	(2) Birth weight	(3) Head circumference	(4) Length
Information shock	-0.136***	-18.337	-0.028	-0.124*
	(0.044)	(16.923)	(0.053)	(0.074)
Fixed effects	YES	YES	YES	YES
F-stat	9.70	1.17	0.28	2.85
R^2	0.042	0.115	0.091	0.130
Obs	13,208	13,173	12,437	12,653
Mean of dep. var.	38.690	3,386.615	35.387	49.410

Note to Table 4: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term (≥37 gestational weeks) are considered for analysis. In Panel A, each column presents a separate OLS regression of the impact of the information shock on the probability of planned C-section including quadratic trends (column 1), cubic trends (column 2), a triangular kernel (column 3), a sample with a time period of 12 months (column 4), and a sample with a time period of a 300-day window before and after the information shock (column 5). In Panel B, each column presents a separate OLS regression of the impact of the information shock on the probability of emergency C-section (column 1), probability of induced labor (column 2), probability of spinal anesthesia (column 3), probability of using forceps or vacuum extractor (column 4) and probability of episiotomy (column 5). In Panel C, each column presents a separate OLS regression of the impact of the information on gestational age (column 1), birth weight (column 1), head circumference (column 3), and length of the baby (column 4). In panels B and C, linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * p<0.1, ** p<0.05, *** p<0.01.

Table 5: Proportion of term singleton breech births

	Proportion of breech births	
	(1)	(2)
	proportion breech	proportion breech
Information shock	0.0008	0.0001
	(0.0012)	(0.0013)
Fixed effects	NO	YES
R^2	0.003	0.058
Obs	2,191	2,191
Mean of dep. var.	0.032	0.032

Note to Table 5: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. The proportion of breech births is collapsed to daily level. Each column presents a separate OLS regression of the impact of the information shock on the proportion of breech births. Fixed effects including age, weight, height, education, and sickness benefits are included in column 2. Linear split time trends are included in each regression. Standard errors are clustered at day-month-year level. * p<0.1, ** p<0.05, *** p<0.01.

Table 6: Maternal characteristics

	Panel A: *Maternal characteristics*					
	(1) Age	(2) Weight (kilograms)	(3) Height (centimeters)	(4) Education	(5) Labor income	(6) Sick benefits
Information shock	0.219	0.340	0.020	0.053	0.028	-0.002
	(0.162)	(0.450)	(0.230)	(0.048)	(0.040)	(0.074)
Fixed effects	YES	YES	YES	YES	YES	YES
F-stat	1.84	0.57	0.01	1.21	0.50	0.00
R^2	0.160	0.024	0.008	0.058	0.026	0.112
Obs	13,208	11,739	12,318	13,162	13,072	13,080
Mean of dep. var.	29.876	67.084	166.442	4.405	7.462	1.490

	Panel B: *Maternal characteristics, C-section*					
	(1) Age	(2) Weight (kilograms)	(3) Height (centimeters)	(4) Education	(5) Labor income	(6) Sick benefits
Information shock	-0.067	0.589	0.221	0.100	-0.002	-0.093
	(0.224)	(0.634)	(0.332)	(0.064)	(0.051)	(0.102)
Fixed effects	YES	YES	YES	YES	YES	YES
F-stat	0.09	0.86	0.44	2.40	0.00	0.83
R^2	0.158	0.031	0.015	0.055	0.029	0.130
Obs	7,330	6,539	6,857	7,307	7,266	7,269
Mean of dep. var.	30.085	67.397	166.309	4.429	7.499	1.578

Note to Table 6: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. In Panel A, each column presents a separate OLS regression of the impact of the information shock on maternal characteristics such as age (column 1), weight (column 2), height (column 3), educational attainment (column 4), average labor income 5 years prior to birth (column 5), and average sickness benefits 5 years prior to birth (column 6). In Panel B, each column presents a separate OLS regression of the impact of the information shock on maternal characteristics among women delivering with planned C-section: age (column 1), weight (column 2), height (column 3), educational attainment (column 4), average labor income 5 years prior to birth (column 5), and average sickness benefits 5 years prior to birth (column 6). All income variables are transformed using inverse hyperbolic sine transformation with an interpretation analogue to the standard logarithmic transformation. Linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 7: Placebo discontinuities

	(1) Planned C-section	(2) Planned C-section	(3) Planned C-section	(4) Planned C-section	(5) Planned C-section	(6) Planned C-section
25-Aug-97	0.0504					
	(0.0360)					
25-Aug-98		0.0180				
		(0.0384)				
25-Aug-99			-0.0634			
			(0.0399)			
25-Aug-01				0.0502		
				(0.0353)		
25-Aug-02					0.0116	
					(0.0366)	
25-Aug-03						-0.0277
						(0.0351)
R^2	0.057	0.061	0.059	0.057	0.047	0.046
Obs	4,267	4,204	4,266	4,450	4,726	4,952
Mean of dep. var.	0.453	0.478	0.473	0.615	0.656	0.667

Note to Table 7: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. Using a bandwidth of 12 months before and after, each column represents a regression examining possible discontinuities in planned C-section using placebo dates of 25 August 1997-1999 and 2001-2003. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics and binary variables for missing values are included in each regression. Linear split time trends are included in each regression. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 8: Heterogeneous effects

Panel A:	*Birth order*			*Age groups*			
	(1) OLS Birth order 1	(2) OLS Birth order 2	(3) OLS Birth order ≥ 3	(4) OLS Ages < 25	(5) OLS Ages 25-29	(6) OLS Ages 30-34	(7) OLS Ages ≥ 35
Information shock	0.128***	0.089**	0.078	0.110**	0.126***	0.128***	0.055
	(0.022)	(0.037)	(0.051)	(0.049)	(0.029)	(0.030)	(0.042)
F-stat	33.83	5.87	2.31	5.07	18.96	18.32	1.73
R^2	0.062	0.072	0.083	0.068	0.062	0.061	0.069
Obs	8,266	3,311	1,631	1,794	4,522	4,577	2,315
Mean of dep. var.	0.566	0.547	0.513	0.537	0.529	0.564	0.602

Panel B:	*Education*			*BMI classification*		
	(1) OLS Primary education	(2) OLS Secondary education	(3) OLS Tertiary education	(4) OLS BMI < 25	(5) OLS BMI 25-29.9	(6) OLS BMI ≥ 30
Information shock	0.090***	0.107***	0.144***	0.109***	0.180***	0.080
	(0.033)	(0.028)	(0.032)	(0.023)	(0.037)	(0.061)
F-stat	7.54	14.59	20.31	22.77	23.10	1.72
R^2	0.071	0.058	0.077	0.064	0.067	0.118
Obs	3,761	5,409	3,992	7,553	2,778	1,106
Mean of dep. var.	0.542	0.562	0.558	0.550	0.554	0.613

Note to Table 8: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. In Panel A, *Birth order*, each column presents a separate OLS regression of the impact of the information shock on the probability of planned C-section across birth order: first-time mothers (column 1), second birth (column 2), and third or higher-order births (column 3). In Panel A, *Age groups*, each column presents a separate OLS regression of the impact of the information shock on the probability of planned C-section for the following age groups: ages $<$ 25 (column 4), ages 25-29 (column 5), ages 30-34 (column 6) and ages $\geq$ 35 (column 7). In Panel B, *Education*, each column presents a separate OLS regression of the impact of the information shock on the probability of planned C-section for the following educational levels: primary education (column 1), secondary education (column 2), and tertiary education (column 3). In Panel B, *BMI classification*, each column presents a separate OLS regression of the impact of the information shock on the probability of planned C-section for the following BMI levels: normal weight (column 4), overweight (column 5), and obese (column 6). Linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * p<0.1, ** p<0.05, *** p<0.01.

Table 9: Reduced form: Impact on child and maternal outcomes

	Panel A: *Full sample*						
	(1) Child health index	(2) Maternal health index	(3) Maternal health at sub. birth index	(4) Number of future births	(5) No more births	(6) Birth spacing	(7) Maternal labor index
Information shock	0.104***	0.018	0.030	-0.033	0.019	-0.014	-0.022
	(0.034)	(0.036)	(0.042)	(0.022)	(0.014)	(0.076)	(0.034)
R^2	0.036	0.021	0.018	0.263	0.318	0.072	0.219
Obs	13,208	13,208	7,585	13,208	13,208	7,585	13,191
Control mean	-0.00	0.00	0.00	0.70	0.44	3.32	0.00
Control sd	1.00	1.00	1.00	0.73	0.50	1.66	1.00

	Panel B: *First-time mothers*						
	(1) Child health index	(2) Maternal health index	(3) Maternal health at sub. birth index	(4) Number of future births	(5) No more births	(6) Birth spacing	(7) Maternal labor index
Information shock	0.087**	0.000	0.013	-0.044	0.023	-0.041	0.020
	(0.043)	(0.046)	(0.044)	(0.031)	(0.019)	(0.077)	(0.043)
R^2	0.024	0.017	0.018	0.099	0.119	0.050	0.207
Obs	8,266	8,266	6,365	8,266	8,266	6,365	8,253
Control mean	-0.00	0.00	-0.00	0.96	0.24	3.20	-0.00
Control sd	1.00	1.00	1.00	0.70	0.43	1.58	1.00

Note to Table 9: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. In Panel A, for the full sample, each column presents a separate OLS regression of the impact from information shock, on child health index (column 1), maternal health index (column 2), maternal health at subsequent birth index (column 3), number of subsequent births within 8 years from breech birth (column 4), probability of no more births within 8 years (column 5), birth spacing within 8 years (column 6), and labor market index (column 7). In Panel B, the effects (similar to those in Panel A) on first-time mothers are presented. Linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * p<0.1, ** p<0.05, *** p<0.01.

Table 10: Reduced form - Impact on child and maternal separate outcomes

Panel A: Child health

	(1) Apgar score	(2) Apgar < 7	(3) Infant mortality rate	(4) Hospital nights ages 0-1	(5) Hospital nights ages 1-7
Information shock	0.067**	-0.003	-3.286	-0.309	-0.668***
	[0.031]	[0.004]	[2.185]	[0.228]	[0.226]
FDR p-value (treat)	0.246	0.832	0.528	0.528	0.048
Bonferroni p-value (treat)	0.492	1.000	1.000	1.000	0.048
R^2	0.025	0.010	0.019	0.039	0.015
Observations	13,108	13,108	13,208	13,158	13,158
Mean of dep. var.	9.718	0.013	3.180	1.180	1.102

Panel B: Maternal health

	(1) Postbirth hospitalization	(2) Sepsis	(3) Hemorrhage
Information shock	-0.065	-0.001	0.001
	[0.104]	[0.003]	[0.008]
FDR p-value (treat)	0.832	0.832	0.938
Bonferroni p-value (treat)	1.000	1.000	1.000
R^2	0.006	0.011	0.025
Observations	12,779	13,208	13,208
Mean of dep. var.	0.254	0.005	0.056

Panel C: Maternal health at subsequent birth

	(1) Emergency C-section	(2) Postbirth hospitalization	(3) Hemorrhage	(4) Sepsis
Information shock	-0.013	0.056	-0.005	-0.001
	[0.015]	[0.101]	[0.012]	[0.002]
FDR p-value (treat)	0.832	0.832	0.832	0.832
Bonferroni p-value (treat)	1.000	1.000	1.000	1.000
R^2	0.028	0.009	0.020	0.007
Observations	7,583	7,308	7,585	7,585
Mean of dep. var.	0.114	0.220	0.073	0.002

Panel D: Maternal labor outcomes

	(1) Labor income	(2) Parental benefits	(3) Sickness benefits
Information shock	0.017	-0.009	-0.116
	[0.060]	[0.028]	[0.080]
FDR p-value (treat)	0.832	0.832	0.528
Bonferroni p-value (treat)	1.000	1.000	1.000
R^2	0.251	0.181	0.086
Observations	13,191	13,191	13,191
Mean of dep. var.	7.106	6.392	3.236

Note to Table 10: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. For the full sample, each column presents a separate OLS regression of the impact of the information shock on child health outcomes (Panel A), maternal health outcomes (Panel B), maternal health outcomes at subsequent births (Panel C), and maternal labor market outcomes (Panel D). All income variables are transformed using inverse hyperbolic sine transformation with an interpretation analogue to the standard logarithmic transformation. Linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. FDR corrected p-values and Bonferroni-corrected p-values are reported for each estimate.

Table 11: Reduced form: Polynomials, kernel, and period

	Child health index	**Maternal health index**	**Maternal health at sub. birth index**	**Number of future births**	**No more births**	**Birth spacing**	**Maternal labor index**
Panel A: *Quadratic trends*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Information shock	0.131**	-0.080	0.052	-0.046	0.028	-0.007	-0.032
	(0.051)	(0.055)	(0.063)	(0.034)	(0.021)	(0.111)	(0.051)
R^2	0.036	0.022	0.018	0.263	0.318	0.073	0.219
Obs	13,208	13,208	7,585	13,208	13,208	7,585	13,191
Panel B: *Cubic trends*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Information shock	0.126***	-0.059	0.053	-0.049	0.028	0.007	-0.027
	(0.045)	(0.048)	(0.054)	(0.030)	(0.019)	(0.099)	(0.045)
R^2	0.036	0.022	0.018	0.263	0.319	0.073	0.219
Obs	13,208	13,208	7,585	13,208	13,208	7,585	13,191
Panel C: *Triangular kernel*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Information shock	0.117***	-0.018	0.046	-0.034	0.020	-0.003	-0.023
	(0.038)	(0.039)	(0.044)	(0.024)	(0.015)	(0.082)	(0.037)
R^2	0.039	0.021	0.021	0.267	0.320	0.077	0.214
Obs	13,194	13,194	7,576	13,194	13,194	7,576	13,177
Panel D: *1 year*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Information shock	0.194**	0.008	0.041	-0.025	0.029	-0.076	-0.017
	(0.081)	(0.063)	(0.098)	(0.049)	(0.031)	(0.164)	(0.071)
R^2	0.048	0.031	0.043	0.287	0.331	0.098	0.201
Obs	4,305	4,305	2,435	4,305	4,305	2,435	4,297

Note to Table 11: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. For the full sample, each column presents a separate OLS regression of the impact of the information shock on the probability of planned C-section on each outcome analogue to the baseline results (in Table 1), including quadratic trends (Panel A), cubic trends (Panel B), a triangular kernel (Panel C), and a sample with a time period of a 1-year window before and after the information shock (Panel D). Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 12: 2SLS estimates: Impact on child and maternal outcomes

	Panel A: *Full sample*						
	(1) Child health index	(2) Maternal health index	(3) Maternal health at sub. birth index	(4) Number of future births	(5) No more births	(6) Birth spacing	(7) Maternal labor index
Planned C-section	0.927***	0.164	0.266	-0.291	0.173	-0.127	-0.193
	(0.320)	(0.317)	(0.374)	(0.204)	(0.131)	(0.660)	(0.299)
Obs	13,208	13,208	7,585	13,208	13,208	7,585	13,191
Control mean	-0.00	0.00	0.00	0.70	0.44	3.32	0.00
Control sd	1.00	1.00	1.00	0.73	0.50	1.66	1.00
	Panel B: *First-time mothers*						
	(1) Child health index	(2) Maternal health index	(3) Maternal health at sub. birth index	(4) Number of future births	(5) No more births	(6) Birth spacing	(7) Maternal labor index
Planned C-section	0.678**	0.003	0.104	-0.341	0.182	-0.330	0.158
	(0.335)	(0.352)	(0.353)	(0.246)	(0.150)	(0.612)	(0.336)
Obs	8,266	8,266	6,365	8,266	8,266	6,365	8,253
Control mean	-0.00	0.00	-0.00	0.96	0.24	3.20	-0.00
Control sd	1.00	1.00	1.00	0.70	0.43	1.58	1.00

Note to Table 12: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. In Panel A, each column presents a separate 2SLS regression of the impact of planned C-sections on child health index (column 1), maternal health index (column 2), maternal health at subsequent birth index (column 3), number of subsequent births within 8 years from breech birth (column 4), probability of no more births within 8 years (column 5), birth spacing within 8 years (column 6), and labor market index (column 7). In Panel B, the effects (similar to those in Panel A) on first-time mothers are presented. Linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 13: 2SLS estimates: Impact on child and maternal separate outcomes

Panel A: Child health

	(1) Apgar score	(2) Apgar < 7	(3) Infant mortality rate	(4) Hospital nights ages 0-1	(5) Hospital nights ages 1-7
Planned C-section	0.580**	-0.030	-29.251	-2.715	-5.878***
	[0.269]	[0.036]	[19.740]	[2.009]	[2.190]
FDR p-value (treat)	0.236	0.831	0.529	0.529	0.109
Bonferroni p-value (treat)	0.471	1.000	1.000	1.000	0.109
Observations	13,108	13,108	13,208	13,158	13,158
Mean of dep. var.	9.718	0.013	3.180	1.180	1.102

Panel B: Maternal health

	(1) Postbirth hospitalization	(2) Sepsis	(3) Hemorrhage
Planned C-section	-0.556	-0.010	0.005
	[0.887]	[0.022]	[0.070]
FDR p-value (treat)	0.831	0.831	0.938
Bonferroni p-value (treat)	1.000	1.000	1.000
Observations	12,779	13,208	13,208
Mean of dep. var.	0.254	0.005	0.056

Panel C: Maternal health at subsequent birth

	(1) Emergency C-section	(2) Postbirth hospitalization	(3) Hemorrhage	(4) Sepsis
Planned C-section	-0.117	0.506	-0.044	-0.007
	[0.133]	[0.900]	[0.101]	[0.013]
FDR p-value (treat)	0.831	0.831	0.831	0.831
Bonferroni p-value (treat)	1.000	1.000	1.000	1.000
Observations	7,583	7,308	7,585	7,585
Mean of dep. var.	0.114	0.220	0.073	0.002

Panel D: Maternal labor outcomes

	(1) Labor income	(2) Parental benefits	(3) Sickness benefits
Planned C-section	0.151	-0.077	-1.030
	[0.531]	[0.252]	[0.736]
FDR p-value (treat)	0.831	0.831	0.529
Bonferroni p-value (treat)	1.000	1.000	1.000
Observations	13,191	13,191	13,191
Mean of dep. var.	7.106	6.392	3.236

Note to Table 13: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. For the full sample, each column presents a separate 2SLS regression of the impact of planned C-section on child health outcomes (Panel A), maternal health outcomes (Panel B), maternal health outcomes at subsequent births (Panel C) and maternal labor market outcomes (Panel D). All income variables are transformed using inverse hyperbolic sine transformation with an interpretation analogue to the standard logarithmic transformation. Linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. FDR corrected p-values and Bonferroni-corrected p-values are reported for each estimate.

Table 14: Difference-in-differences estimates

Panel A: *Difference-in-difference estimates, full sample*

	(1) Planned C-section	(2) Child health index	(3) Maternal health index	(4) Maternal health at sub. birth index	(5) Number of future births	(6) No more births	(7) Birth spacing	(8) Maternal labor index
Information shock	0.157***	0.094***	-0.007	-0.021	-0.019*	0.010	-0.009	-0.019
	(0.009)	(0.023)	(0.023)	(0.032)	(0.011)	(0.007)	(0.037)	(0.016)
Breech	0.437***	-0.100***	-0.043**	-0.217***	-0.025***	0.008	0.053*	0.024**
	(0.007)	(0.018)	(0.018)	(0.023)	(0.008)	(0.005)	(0.028)	(0.011)
R^2	0.175	0.016	0.004	0.007	0.260	0.307	0.077	0.195
Obs	419,656	419,656	419,656	213,249	419,656	419,656	213,249	419,190
Control mean	0.05	-0.00	0.00	0.00	0.65	0.50	3.37	-0.00
Control sd	0.21	1.00	1.00	1.00	0.76	0.50	1.75	1.00

Panel B: *Difference-in-difference estimates, first-time mothers*

	(1) Planned C-section	(2) Child health index	(3) Maternal health index	(4) Maternal health at sub. birth index	(5) Number of future births	(6) No more births	(7) Birth spacing	(8) Maternal labor index
Information shock	0.174***	0.100***	0.005	-0.012	-0.026*	0.017*	-0.010	-0.010
	(0.011)	(0.027)	(0.023)	(0.037)	(0.016)	(0.009)	(0.038)	(0.020)
Breech	0.452***	-0.067***	0.016	-0.242***	-0.032***	0.008	0.051*	0.050***
	(0.008)	(0.021)	(0.018)	(0.027)	(0.012)	(0.007)	(0.029)	(0.014)
R^2	0.318	0.013	0.005	0.009	0.089	0.110	0.040	0.207
Obs	178,723	178,723	178,723	142,780	178,723	178,723	142,780	178,512
Control mean	0.03	-0.00	0.00	-0.00	1.03	0.21	3.13	-0.00
Control sd	0.17	1.00	1.00	1.00	0.72	0.41	1.55	1.00

Note to Table 14: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton births at term ($\geq$37 gestational weeks) are considered for analysis. In Panel A, each column presents a separate difference-in-differences regression of the impact of information on planned C-section (column 1), child health index (column 2), maternal health index (column 3), maternal health at subsequent birth index (column 4), number of subsequent births within 8 years from breech birth (column 5), probability of no more births within 8 years (column 6), birth spacing within 8 years (column 7), and labor market index (column 8). In Panel B, the effects (similar to those in Panel A) on first time mothers are presented. Day-month-year, maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 15: Difference-in-differences and 2SLS estimates

	(1) Child health index	(2) Maternal health index	(3) Maternal health at sub. birth index	(4) Number of future births	(5) No more births	(6) Birth spacing	(7) Maternal labor index
Panel A: *Difference-in-difference estimates, full sample*							
Planned CS	0.600***	-0.047	-0.128	-0.121*	0.065	-0.054	-0.122
	(0.148)	(0.148)	(0.191)	(0.073)	(0.046)	(0.226)	(0.101)
Breech	-0.362***	-0.023	-0.160	0.028	-0.020	0.077	0.077
	(0.079)	(0.080)	(0.102)	(0.038)	(0.024)	(0.123)	(0.052)
Obs	419,656	419,656	213,249	419,656	419,656	213,249	419,190
Control mean	-0.00	0.00	0.00	0.65	0.50	3.37	-0.00
Control sd	1.00	1.00	1.00	0.76	0.50	1.75	1.00
Panel B: *Difference-in-difference estimates, first-time mothers*							
	(1) Child health index	(2) Maternal health index	(3) Maternal health at sub. birth index	(4) Number of future births	(5) No more births	(6) Birth spacing	(7) Maternal labor index
Planned CS	0.578***	0.028	-0.067	-0.149*	0.095*	-0.056	-0.060
	(0.153)	(0.133)	(0.211)	(0.090)	(0.054)	(0.216)	(0.113)
Breech	-0.328***	0.003	-0.212*	0.035	-0.035	0.076	0.077
	(0.086)	(0.075)	(0.115)	(0.050)	(0.030)	(0.121)	(0.061)
Obs	178,723	178,723	142,780	178,723	178,723	142,780	178,512
Control mean	-0.00	0.00	-0.00	1.03	0.21	3.13	-0.00
Control sd	1.00	1.00	1.00	0.72	0.41	1.55	1.00

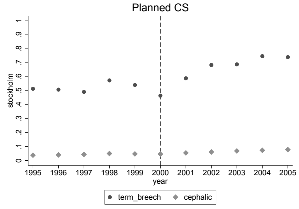
Note to Table 15: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton births at term ($\geq$37 gestational weeks) are considered for analysis. In Panel A, each column presents a separate difference-in-differences-2SLS regression of the impact of planned C-sections on child health index (column 1), maternal health index (column 2), maternal health at subsequent birth index (column 3), number of subsequent births within 8 years from breech birth (column 4), probability of no more births within 8 years (column 5), birth spacing within 8 years (column 6), and labor market index (column 7). In Panel B, the effects (similar to those in Panel A) on first-time mothers are presented. Day-month-year, maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Appendices

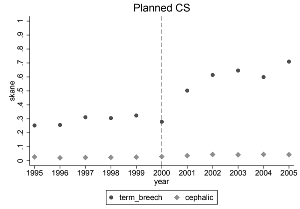
A1 Figures and tables

Figure A1: Trends in planned C-sections by county

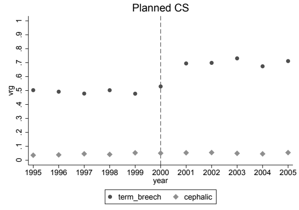
(a) Stockholm, 21.3%



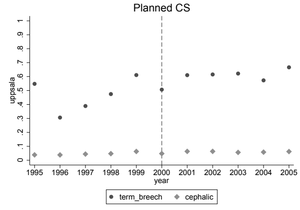
(b) Skåne region, 10.5%



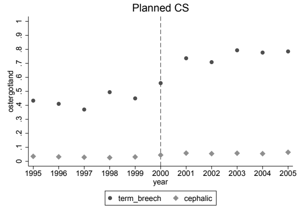
(c) Västra Götaland region, 9.2%



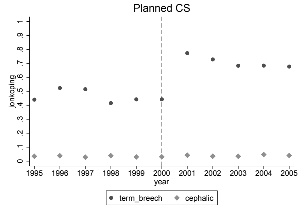
(d) Uppsala, 3.9%



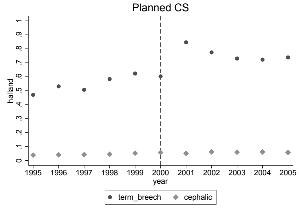
(e) Östergötland, 3.8%



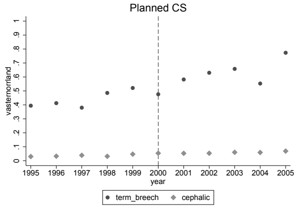
(f) Jönköping, 3.2%



(g) Halland, 3.1%



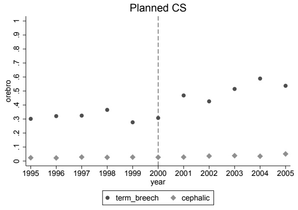
(h) Västernorrland, 3.1%



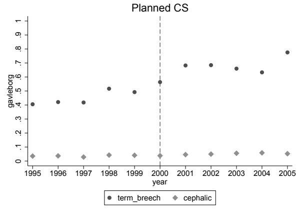
Note to Figure A1: The data are obtained from the Swedish Medical Birth Registry. Annual trends in planned C-section among singleton breech births at term (≥37 gestational weeks) per county are presented in Figure A1. The red vertical line indicates the date of the information shock to the Swedish medical society.

Figure A2: Trends in planned C-sections by county

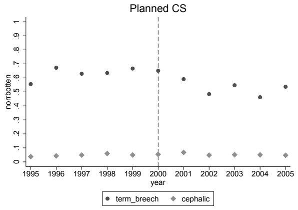
(a) Örebro, 2.8%



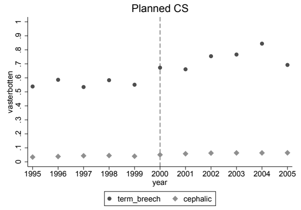
(b) Gävleborg, 2.8%



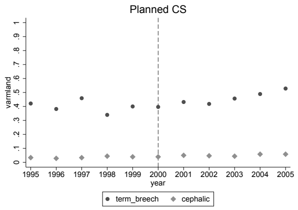
(c) Norrbotten, 2.5%



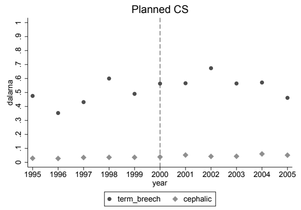
(d) Västerbotten, 2.4%



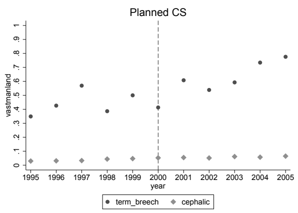
(e) Värmland, 2.3%



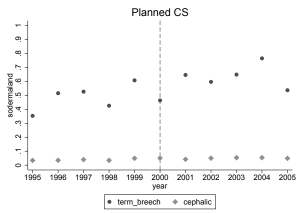
(f) Dalarna, 2.3%



(g) Västmanland, 2.1%



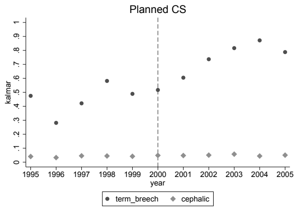
(h) Södermaland, 2.1%



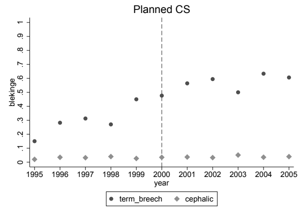
Note to Figure A2: The data are obtained from the Swedish Medical Birth Registry. Annual trends in planned C-section among singleton breech births at term ($\geq$37 gestational weeks) per county are presented in Figure A2. The red vertical line indicates the date of the information shock to the Swedish medical society.

Figure A3: Trends in planned C-sections by county

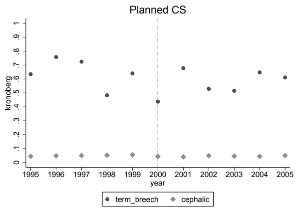
(a) Kalmar, 2.1%



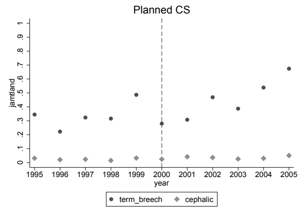
(b) Blekinge, 1.7%



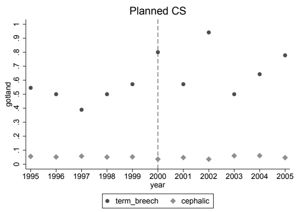
(c) Kronoberg, 1.4%



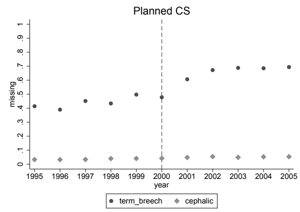
(d) Jämtland, 1.3%



(e) Gotland, 0.7%

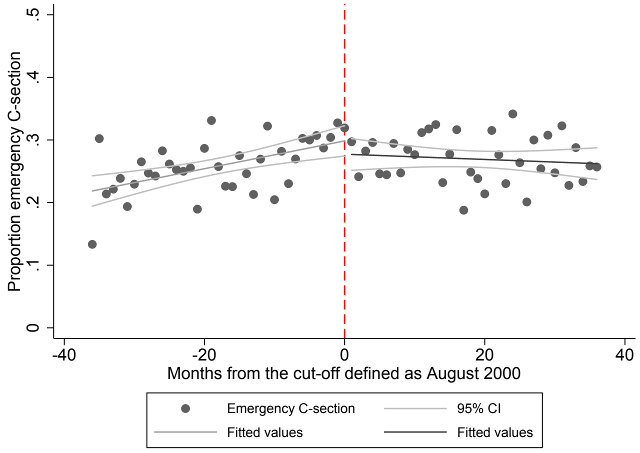


(f) Missing, 16.4%



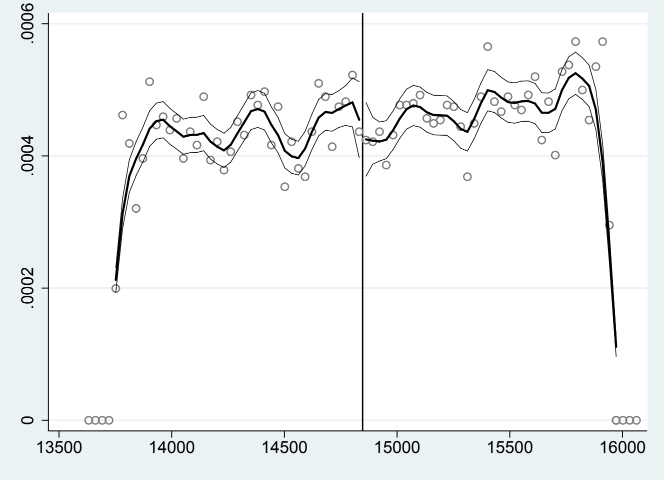
Note to Figure A3: The data are obtained from the Swedish Medical Birth Registry. Annual trends in planned C-section among singleton breech births at term (≥37 gestational weeks) per county are presented in Figure A3. The red vertical line indicates the date of the information shock to the Swedish medical society.

Figure A4: Trends in emergency C-section



Note to Figure A4: The data are obtained from the Swedish Medical Birth Registry. Monthly trend in emergency C-section among singleton breech births at term (≥37 gestational weeks) is presented in Figure A4. The red vertical line indicates the date of the information shock to the Swedish medical society.

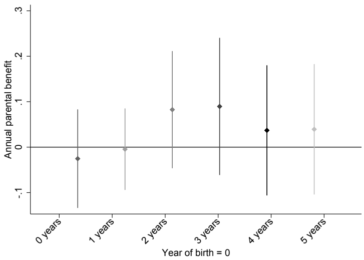
Figure A5: McCrary density test plot



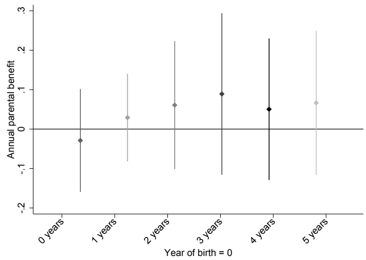
Note to Figure A5: The data are obtained from the Swedish Medical Birth Registry for the time period 25 August 1997 to 25 August 2003. McCrary density test of sorting across the event date (McCrary, 2008). The vertical line indicates the date of the information shock to the Swedish medical society.

Figure A6: Labor market outcomes, event study

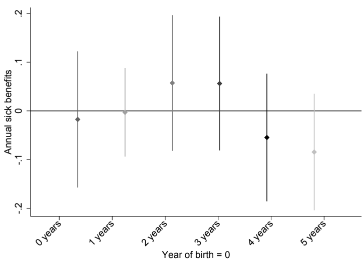
(a) Parental benefits, full sample



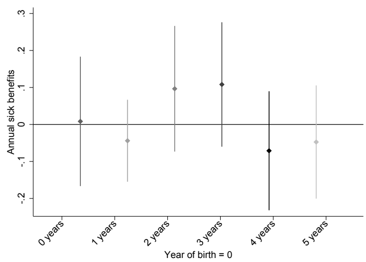
(b) Parental benefits, first birth



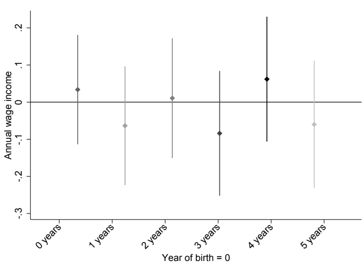
(c) Sickness benefits, full sample



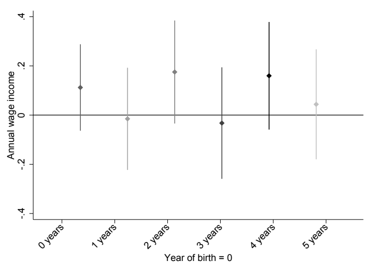
(d) Sickness benefits, first birth



(e) Labor income, full sample



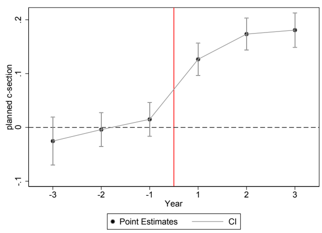
(f) Labor income, first birth



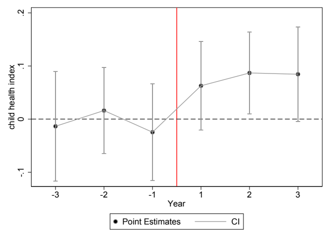
Note to Figure A6: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton births at term ($\geq$37 gestational weeks) are considered for analysis. The coefficient plots of the 2SLS estimates on labor market outcomes for each year separately, 1,2...5 years after giving birth (year 0). The effects on income from parental benefits are presented in figure A6a for the full sample and figure A6b for first-time mothers. The effects on income from sickness benefits are presented in figure A6c for the full sample and figure A6d for first-time mothers. The effects on labor income are presented in figure A6e for the full sample and figure A6f for first-time mothers. Maternal age, birth order, month, year and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level.

Figure A7: Difference-in-differences estimates, event studies

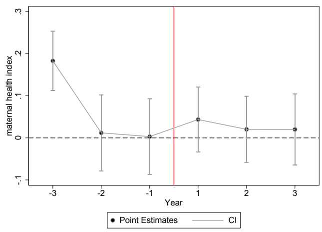
(a) Planned C-section



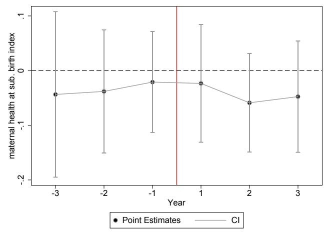
(b) Child health index



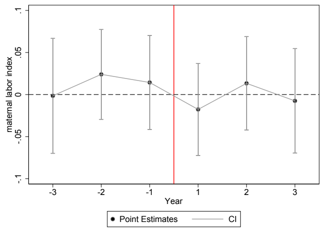
(c) Maternal helath index



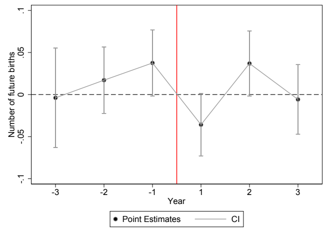
(d) Maternal health at subseq. birth index



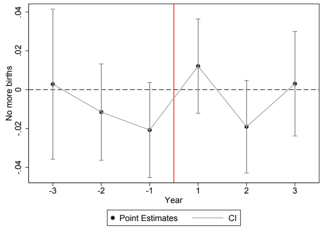
(e) Maternal labor market index



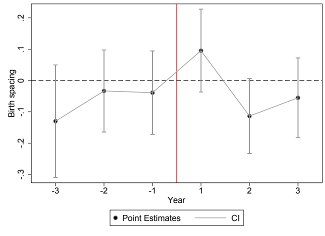
(f) Number of subseq. births within 8 years



(g) No subseq. births within 8 years



(h) Birth spacing within 8 years



Note to Figure A7: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton births at term ($\geq$37 gestational weeks) are considered for analysis. Each figure presents coefficients of interactions between each year and breech births. The red vertical line represents the year of the information shock, which is the omitted category. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level.

Table A1: Preterm and twin breech births

	Breech preterm		**Breech twin**	
	(1) Planned C-section	(2) Planned C-section	(3) Planned C-section	(4) Planned C-section
Information shock	-0.014	-0.018	0.050	0.044
	(0.031)	(0.031)	(0.037)	(0.037)
Mother weight		0.003***		0.002***
		(0.001)		(0.001)
Mother height		-0.003**		-0.004***
		(0.001)		(0.002)
Malformation		0.022		0.053
		(0.024)		(0.035)
Smoking 1st trimester		-0.044**		0.002
		(0.021)		(0.028)
Male		-0.024*		-0.023
		(0.013)		(0.015)
Native		-0.001		-0.023
		(0.024)		(0.026)
Income		-0.000		0.000*
		(0.000)		(0.000)
Sickness benefits		0.000*		0.000**
		(0.000)		(0.000)
Fixed effects	NO	YES	NO	YES
F-stat	0.19	0.33	1.85	1.41
R^2	0.001	0.042	0.008	0.067
Obs	4,967	4,967	4,263	4,263
Mean of dep. var.	0.333	0.333	0.376	0.376

Note to Table A1: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only breech births are considered for analysis. Each column presents a separate regression with OLS estimates of the impact of the information shock on the probability of planned C-section for preterm breech births (columns 1-2) and twin breech births (columns 3-4). Linear split-breech specific trends are included in all regressions. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in columns 2 and 4. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A2: Compositional changes in maternal characteristics

	Planned C-section
Information shock	-1.0743***
	(0.2629)
Mother weight	0.0016***
	(0.0006)
Mother height	-0.0084***
	(0.0013)
Mother age	0.0061***
	(0.0017)
Education	-0.0038
	(0.0054)
Income	0.0000
	(0.0000)
Sickness benefits	0.0001
	(0.0001)
InfoShock × motherWeight	-0.0001
	(0.0008)
InfoShock × motherHeight	0.0075***
	(0.0016)
InfoShock × motherAge	-0.0013
	(0.0023)
InfoShock × educBefore	0.0047
	(0.0072)
InfoShock × incomeBefore	-0.0000
	(0.0000)
InfoShock × sickBefore	0.0000
	(0.0001)
InfoShock × dTBT	0.0001***
	(0.0000)
dTBT	-0.0000
	(0.0000)
R^2	0.063
Obs	11,087

Note to Table A2: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. This table presents a regression of the likelihood of having a planned C-section on a fully interacted model, in which the maternal characteristics are interacted with the treatment status (post information shock). Linear split-breech specific trends are included in all regressions. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * p<0.1, ** p<0.05, *** p<0.01.

Table A3: 2SLS: Polynomials, kernel, and period

	Child health index	Maternal health index	Maternal health at sub. birth index	Number of future births	No more births	Birth spacing	Maternal labor index
Panel A: *Quadratic trends*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Planned CS	1.070**	-0.656	0.489	-0.373	0.230	-0.069	-0.262
	(0.452)	(0.470)	(0.636)	(0.291)	(0.181)	(1.039)	(0.417)
Obs	13,208	13,208	7,585	13,208	13,208	7,585	13,191
Panel B: *Cubic trends*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Planned CS	1.044***	-0.489	0.482	-0.404	0.230	0.066	-0.223
	(0.404)	(0.415)	(0.530)	(0.261)	(0.162)	(0.897)	(0.375)
Obs	13,208	13,208	7,585	13,208	13,208	7,585	13,191
Panel C: *Triangular kernel*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Planned CS	1.026***	-0.153	0.417	-0.300	0.176	-0.030	-0.198
	(0.352)	(0.344)	(0.418)	(0.217)	(0.137)	(0.735)	(0.322)
Obs	13,194	13,194	7,576	13,194	13,194	7,576	13,177
Panel D: *1 year*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Planned CS	1.807**	0.071	0.416	-0.234	0.266	-0.768	-0.160
	(0.917)	(0.584)	(1.011)	(0.473)	(0.311)	(1.650)	(0.660)
Obs	4,305	4,305	2,435	4,305	4,305	2,435	4,297

Note to Table A3: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. For the full sample, each column presents a separate 2SLS regression of the impact of the information shock on the probability of planned C-section on each outcome analogue to the baseline results (in Table 12), including quadratic trends (Panel A), cubic trends (Panel B), a triangular kernel (Panel C), a sample with a time period of a 1-year window before and after the information shock (Panel D). Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A4: Difference-in-differences estimates, with trends

Panel A: *Difference-in-difference estimates, full sample*

	(1) Planned C-section	(2) Child health index	(3) Maternal health index	(4) Maternal health at sub. birth index	(5) Number of future births	(6) No more births	(7) Birth spacing	(8) Maternal labor index
Information shock	0.112***	0.139***	0.022	0.007	-0.042*	0.022	0.005	-0.027
	(0.018)	(0.050)	(0.048)	(0.057)	(0.022)	(0.014)	(0.077)	(0.032)
Breech	0.429***	-0.147***	-0.078**	-0.195***	-0.028*	0.005	0.109**	0.024
	(0.013)	(0.041)	(0.034)	(0.039)	(0.016)	(0.010)	(0.055)	(0.023)
R^2	0.175	0.016	0.004	0.007	0.259	0.306	0.077	0.194
Obs	419,656	419,656	419,656	213,249	419,656	419,656	213,249	419,190
Control mean	0.05	-0.00	0.00	0.00	0.65	0.50	3.37	-0.00
Control sd	0.21	1.00	1.00	1.00	0.76	0.50	1.75	1.00

Panel B: *Difference-in-difference estimates, first-time mothers*

	(1) Planned C-section	(2) Child health index	(3) Maternal health index	(4) Maternal health at sub. birth index	(5) Number of future births	(6) No more births	(7) Birth spacing	(8) Maternal labor index
Information shock	0.126***	0.117**	-0.000	-0.012	-0.044	0.019	-0.043	0.017
	(0.022)	(0.059)	(0.047)	(0.065)	(0.031)	(0.019)	(0.077)	(0.040)
breech	0.439***	-0.085*	0.007	-0.200***	-0.041*	0.012	0.113**	0.037
	(0.016)	(0.048)	(0.032)	(0.042)	(0.022)	(0.014)	(0.056)	(0.028)
R^2	0.319	0.012	0.004	0.008	0.088	0.109	0.040	0.207
Obs	178,723	178,723	178,723	142,780	178,723	178,723	142,780	178,512
Control mean	0.03	-0.00	0.00	-0.00	1.03	0.21	3.13	-0.00
Control sd	0.17	1.00	1.00	1.00	0.72	0.41	1.55	1.00

Note to Table A4: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton births at term ($\geq$37 gestational weeks) are considered for analysis. In Panel A, each column presents a separate difference-in-differences regression of the impact of the information shock on planned C-section (column 1), child health index (column 2), maternal health index (column 3), maternal health at subsequent birth index (column 4), number of subsequent births within 8 years from breech birth (column 5), probability of no more births within 8 years (column 6), birth spacing within 8 years (column 7), and labor market index (column 8). In Panel B, the effects (similar to those in Panel A) on first-time mothers are presented. Linear split-breech specific trends are included in all regressions. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A5: Robustness and sensitivity

	Planned C-section	Child health index	Maternal health index	Maternal health at sub. birth index	Number of future births	No more births	Birth spacing	Maternal labor index
Panel A: *Date of the publication of TBT as cutoff*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information shock	0.118***	0.081**	0.023	0.037	-0.037	0.021	0.009	-0.015
	(0.019)	(0.037)	(0.039)	(0.042)	(0.023)	(0.015)	(0.080)	(0.035)
R^2	0.063	0.036	0.021	0.017	0.262	0.319	0.073	0.221
Obs	12,888	12,888	12,888	7,400	12,888	12,888	7,400	12,871
Panel B: *Date of the SFOG meeting as cutoff*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information shock	0.128***	0.090**	0.052	0.025	-0.042*	0.019	0.064	-0.026
	(0.020)	(0.038)	(0.036)	(0.048)	(0.024)	(0.016)	(0.085)	(0.038)
R^2	0.062	0.036	0.024	0.017	0.263	0.320	0.074	0.225
Obs	12,515	12,515	12,515	7,192	12,515	12,515	7,192	12,498
Panel C: *Remove 1 week before and after the shock*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information shock	0.118***	0.097***	0.021	0.025	-0.033	0.021	-0.013	-0.022
	(0.018)	(0.034)	(0.036)	(0.043)	(0.022)	(0.015)	(0.076)	(0.034)
R^2	0.062	0.036	0.022	0.018	0.262	0.318	0.072	0.219
Obs	13,120	13,120	13,120	7,534	13,120	13,120	7,534	13,103
Panel D: *Keeping the 5 largest counties*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information shock	0.131***	0.131***	-0.032	0.033	-0.017	0.031	-0.050	-0.029
	(0.026)	(0.045)	(0.057)	(0.059)	(0.033)	(0.022)	(0.112)	(0.052)
R^2	0.078	0.027	0.033	0.033	0.260	0.319	0.075	0.237
Obs	6,224	6,224	6,224	3,621	6,224	6,224	3,621	6,216
Panel E: *County-specific pretrends effects*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information shock	0.113***	0.107***	-0.021	0.019	-0.046*	0.025	0.043	-0.034
	(0.020)	(0.036)	(0.039)	(0.046)	(0.024)	(0.016)	(0.082)	(0.036)
R^2	0.074	0.041	0.033	0.023	0.270	0.324	0.082	0.286
Obs	11,005	11,005	11,005	6,328	11,005	11,005	6,328	10,995

Note to Table A5: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. For the full sample, each column presents a separate OLS regression of the impact of the information shock on the probability of planned C-section on each outcome analogue to the baseline results (in Table 1, using the date of the publication of the Term breech Trial as cutoff (Panel A), using the date of the extra SFOG meeting as cutoff (Panel B), removing observations 1 week before and after the shock (Panel C), using only the five largest counties (Panel D), and including county-specific pretrends effects (Panel E). Linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * p<0.1, ** p<0.05, *** p<0.01.

Table A6: Robustness index

	Panel A: *Index, uniform weights*			
	(1) Child health index	(2) Maternal health index	(3) Maternal health at sub. birth index	(4) Maternal labor index
Information shock	0.087**	0.027	0.003	-0.020
	(0.035)	(0.036)	(0.040)	(0.033)
R^2	0.036	0.020	0.012	0.233
Obs	13,083	12,779	7,308	13,191

	Panel B: *Index, binary measures of hospitalization*		
	(1) Child health index	(2) Maternal health index	(3) Maternal health at sub. birth index
Information shock	0.081**	-0.002	0.057
	(0.035)	(0.035)	(0.043)
R^2	0.046	0.026	0.022
Obs	13,208	13,208	7,585

	Panel C: *Hospitalization, binary measures*			
	(1) Hospitalized ages 0-1	(2) Hospitalized ages 1-7	(3) Hospitalization (readmission)	(4) Hospitalization (readmission), future birth
Information shock	-0.014	-0.017	0.003	-0.007
	(0.013)	(0.014)	(0.009)	(0.010)
R^2	0.036	0.026	0.010	0.015
Obs	13,158	13,158	12,779	7,308

Note to Table A6: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton breech births at term ($\geq$37 gestational weeks) are considered for analysis. For the full sample, each column presents a separate OLS regression of the impact of the information shock on the probability of planned C-section on the indices using uniform weights (Panel A), and binary outcomes for hospitalization (Panel B). In Panel C, the effects on the probability of hospitalization (at least one night) for children ages 0-1 and 1-7 are presented in columns 1-2, respectively. The probabilities of hospital readmission postbirth for mothers after the breech birth and at subsequent birth are presented in columns 3 and 4, respectively. Linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * p<0.1, ** p<0.05, *** p<0.01.

Table A7: Robustness hospitalization

	Panel A: *Hospital nights, breech births*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ages 0-1	ages 0-2	ages 0-3	ages 0-4	ages 0-5	ages 0-6	ages 0-7
Information shock	-0.309	-0.430	-0.479*	-0.477	-0.707**	-0.828**	-0.893**
	(0.228)	(0.264)	(0.283)	(0.294)	(0.319)	(0.337)	(0.358)
F-stat	1.83	2.65	2.86	2.62	4.92	6.03	6.20
R^2	0.039	0.037	0.036	0.034	0.034	0.035	0.035
Obs	13,158	13,158	13,158	13,158	13,158	13,158	13,158
Mean of dep. var.	1.180	1.500	1.716	1.853	2.002	2.112	2.190
	Panel B: *Hospital nights, normal position (cephalic) births*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ages 0-1	ages 0-2	ages 0-3	ages 0-4	ages 0-5	ages 0-6	ages 0-7
Information shock	0.005	0.010	0.011	0.017	0.010	0.004	0.015
	(0.040)	(0.048)	(0.053)	(0.057)	(0.061)	(0.064)	(0.066)
F-stat	0.02	0.04	0.04	0.09	0.03	0.00	0.05
R^2	0.016	0.015	0.014	0.014	0.013	0.013	0.013
Obs	405,271	405,271	405,271	405,271	405,271	405,271	405,271
Mean of dep. var.	0.818	1.076	1.234	1.361	1.475	1.573	1.658

Note to Table A7: The data are obtained from the Swedish Medical Birth Registry, Patient Registry, Death Registry, and the Longitudinal Integration Database for Health Insurance and Labor Market Studies, for the time period 25 August 1997 to 25 August 2003. Only singleton births at term (≥37 gestational weeks) are considered for analysis. For the sample of breech births (Panel A), each column presents a separate OLS regression of the impact of the information shock on the number of nights hospitalized during ages 0-1 (column 1), ages 0-2 (column 2), ages 0-3 (column 3), ages 0-4 (column 4), ages 0-5 (column 5), ages 0-6 (column 6), and ages 0-7 (column 7). In Panel B, the effects (similar to those in Panel A) for a sample of normal position (cephalic) births are presented. Linear split time trends are included in each regression. Maternal age, birth order, birth-quarter and county fixed effects, time-varying maternal and child characteristics, and binary variables for missing values are included in each regression. Standard errors are clustered at day-month-year level. * p<0.1, ** p<0.05, *** p<0.01.

Table A8: Costs and rates

Delivery modes			
Code	Delivery mode	Cost (SEK)	
P05C	Vaginal complicated	47,572	
P05E	Vaginal uncomplicated	30,984	
P01A	C-section extremely complicated	88,635	
P01C	C-section complicated	68,503	
P01E	C-section uncomplicated	54,135	
Hospitalization			
In-patient case		Average hourly rate (SEK)	per night(SEK)
Care at maternity unit		500/h	12,000
Neonatal care, gestation >36		540/h	12,960
Children's hospital		600/h	14,400

Note to Table A8: Data obtained from Karolinska Universitetssjukhuset Huddinge.

Chapter II

CHAPTER 2

Multiple Births, Birth Quality and Maternal Labor Supply: Analysis of IVF Reform in Sweden

with Sonia Bhalotra (University of Essex), Damian Clarke (Universidad de Santiago de Chile) and Mårten Palme (Stockholm University)

Abstract

In this study we examine the passage of a reform to in-vitro fertilization (IVF) procedures in Sweden in 2003. Following publication of medical evidence showing that pregnancy success rates could be maintained using single rather than multiple embryo transfers, the single embryo transfer (SET) was mandated as the default IVF procedure. Using linked registry data for the period 1998-2007, we find that the SET reform was associated with a precipitous drop in the share of multiple births of 63%. This narrowed differences in health between IVF and non-IVF births by 53%, and differences in the labor market outcomes of mothers three years after birth by 85%. For first time mothers it also narrowed the gap in maternal health between IVF and non-IVF births by 36%. Our findings imply that more widespread adoption of SET could lead to massive gains, reducing hospitalization costs and the foregone income of mothers and improving the long-run socioeconomic outcomes of children. This is important given that the share of IVF facilitated births exceeds 3% in several industrialized countries and is on the rise.

Keywords: IVF, fertility, maternal health, neonatal health, career penalty, human capital formation
JEL Codes: J13, I11, I12, I38, J24.

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1 Introduction

In-vitro fertilization (IVF) is a landmark innovation within assisted reproductive technologies (ART), assisting involuntary infertility as well as providing women with the opportunity to postpone childbearing. Similar to the introduction of the pill, the legalization of abortion and the availability of long-acting reversible contraceptives (Bailey and Lindo, 2017), IVF has contributed to the economic liberation of women (Abramowitz, 2014; Abramowitz, J., 2017; Kroeger and La Mattina, 2017; Rainer et al., 2011). Since its advent in the late 1970s, and tracking significant advances in rates of female labor market participation and contraceptive availability, uptake of this technology has increased steadily over time. As of 2012, more than 5 million children have been born as a result of IVF (Zegers-Hochschild et al., 2009), and the share of all births owing to IVF now exceeds 3% in many industrialized countries (de Mouzon et al., 2010).

However, there are substantial costs associated with IVF. In addition to costs of the procedure, estimated to range from 40,000 to over 200,000 USD per IVF birth in the US (Bitler, 2008), there are costs arising from adverse pregnancy outcomes (Sazonova et al., 2011) and adverse birth outcomes (Kalra and Barnhart, 2011). Women conceiving through IVF treatment are more likely to suffer from complications including hypertension, hemorrhage and emergency C-section. Children born of IVF are more likely to be preterm, be presented in breech position, have low birth weight and have lower Apgar scores at birth. This implies additional costs of neonatal and maternal health care that are potentially large (Almond et al., 2010) and, over and above, with expected long-run costs in terms of lower cognitive skills, educational attainment, income and life expectancy among IVF births (Behrman and Rosenzweig, 2004; Bhalotra et al., 2017; Bharadwaj et al., 2013; Black et al., 2007; Oreopoulos et al., 2008).

The main reason that IVF is associated with adverse pregnancy and birth outcomes is that IVF births are 10 to 15 times more likely to be multiple births (Kalra and Barnhart, 2011; Karlström and Bergh, 2007) and multiple births are associated with a higher risk of maternal and neonatal health problems (Bergh et al., 1999; Hall, 2003). For instance, between 2004 and 2005, the rate of twin births among IVF pregnancies was 30% in the United States and 21% in Europe, compared to approximately 1.6% among non-IVF pregnancies (Maheshwari et al., 2011). The reason that IVF births are so much more likely to be multiple births is that IVF has typically involved multiple embryo transfers to increase the chances of success. However, following advances in IVF technology, success rates with a single embryo transfer

(SET) have more or less converged to success rates obtained with the transfer of two embryos (see section 2.3). In light of evidence of this in medical studies, in January 2003, the Swedish National Board of Health and Welfare mandated a SET as the default IVF procedure. Exceptions were allowed as detailed in section 2.3.

Sweden was the pioneer. In July 2003, Belgium followed suit. In 2010, Turkey and Quebec implemented a similar reform. However, as we write in 2017, most IVF treatments in the United States, the United Kingdom and other countries continue to involve a double embryo transfer (DET). Using population registers for Sweden for 1998-2007, we compare outcomes for IVF vs non-IVF births before and after the SET reform, tracing impacts of the reform on indices of child and maternal health and maternal labor market outcomes in the years following birth. We document a post-reform drop in the probability that an IVF birth is a twin of 63%. We identify significant improvements in child health and maternal labor market outcomes overall and, among first-time mothers, also significant improvements in maternal health. We estimate that the reform narrowed the gap between IVF and non-IVF births by 53% in an index of child health and 85% in an index of maternal labor market outcomes. Among first-time mothers, the child health gap narrows by 58%, the maternal labor outcomes gap by 96% and the gap in an index of maternal health by 36%. The increase in child health is evident in indicators of child health, including fetal growth indicated by birth weight, length, head circumference, longer gestational age and fewer complications such as breech presentation, emergency C-section and hospitalization. The improvement in maternal labor markets outcomes is driven by higher labor incomes within three years of birth, and lower sickness benefits.[1] These are intent to treat estimates since there was not perfect compliance with SET. We adjust for selection into IVF and, conditional on IVF, for selection into SET. We allow that omitted trends are different for IVF vs non-IVF births. Since we have many outcomes, we check robustness of the estimates to adjusting for multiple hypothesis testing.

The documented improvements in maternal and child health flow directly from the reduction in the share of multiple births, consistent with the evidence cited above. The improvements in maternal labor supply are likely to flow from both the direct effect of an increase in the share of uniparous pregnancies, and the associated improvements in maternal and newborn health. Evidence of the impacts of fertility on the labor market outcomes of mothers is mixed (see, for example, Adda et al. (2017); Browning (1992); Lundborg et al. (2014)). Among reasons for

[1] This is a marker of lower sickness so although we do not find no average impacts on the maternal health index, we note that this is some evidence of improved health.

the evidence being ambiguous are that extensive margin fertility tends to have larger impacts on wages (Lundborg et al., 2014), that many studies use the occurrence of a twin birth as an instrument for fertility but twin births occur disproportionately to healthier women who may have unobservable characteristics that predispose them towards stronger labor market performance (Bhalotra and Clarke, 2016), or that the setting matters and the opportunity cost of women's time varies with the level of economic development (Aaronson et al., 2017).[2] Studies that instrument fertility with twins effectively model the impact of one unexpected child at any parity, while Lundborg et al. (2014) leverage quasi-random variation in IVF success rates so they effectively model the impact of an IVF birth at first parity and as this is often a twin, often this involves the number of children jumping from 0 to 2. Like (Lundborg et al., 2014) we study an IVF sample but our quasi-experiment (the SET reform) effectively delivers a discontinuous change in the number of children in the opposite direction, from 2 to 1. Although this is not discussed, previous studies are effectively modeling not only the impact of an increase in the number of births but also the occurrence of twin births which we know are less healthy. Similarly, the impact of a decrease in fertility in our study is combined with impacts of an improvement in the health of the child. Using the procedure in Gelbach (2016), we estimate that only a small portion of the improvement in the mother's labor market outcomes can be attributed to improvements in maternal and child health.

The Swedish SET reform has been analyzed in the biomedical literature but many of these studies suffer from small samples and/or are unable to identify causal effects of SET (Karlström and Bergh, 2007; Lundin and Bergh, 2007; Saldeen and Sundström, 2005; Sazonova et al., 2011; Thurin et al., 2004). The closest relatives of our study in the economics literature are Bitler (2008) and Lundborg et al. (2014). Bitler (2008) analyzes the negative impact of infertility treatment mandates in the United States on birth outcomes, underlining the costs of the rising share of twin births. Lundborg et al. (2014), discussed above, examine the impact of fertility among IVF users on female labor supply in Denmark. Our paper is similar insofar as it investigates variation in fertility brought about by IVF technologies, but we consider variation in the *number* of births subject to successful IVF treatment, while they study variation in the success of IVF treatment. We gain exogenous variation in the number of births in the IVF sample from introduction of the SET reform, while they demonstrate that variation in IVF success rates is orthogonal to the observed characteristics of women. Our purpose is to analyze

[2]This is a large literature and there are other variations including whether the outcome of interest is labor supply or earnings, and the horizon over which it is measured.

thoroughly the impacts of the SET mandate on health and labor market outcomes, contributing to the case for its more widespread adoption while theirs is to re-visit the classical question of whether fertility influences women's labor supply.

The findings of this study have important implications for other countries considering policy reform similar to that initiated by Sweden in 2003. While there has been a shift from DET to SET in multiple countries including Scandinavia and Belgium, and Turkey, DET or higher order embryo transfers are still prevalent in most other countries including the US and the UK. For example, only 10% of all embryo transfers were single transfers in 2008 in the US (Practice Committee of the Society for Assisted Reproductive Technology and Practice Committee of the American Society for Reproductive Medicine and others, 2012). There appears to be a lack of information on the advantages of SET among couples seeking IVF treatments and this may be a function of the financial incentives of insurance companies (Pinckney-Clark et al., 2016). Countries with mostly private funding and/or insurance systems appear to have a harder time implementing SET (Karlström and Bergh, 2007). Our findings shed light on the gains from SET not only in terms of child health but also in terms of labor market outcomes for the mother.

This paper is structured in the following way. Section 2 presents a description of IVF in a Swedish context and the implementation of the SET policy. In Sections 3 and 4, respectively, the data and empirical strategy are described. Section 5 presents the results. In Section 6 the findings are discussed, concluding the study.

2 Background

2.1 IVF treatments in Sweden

In Sweden, all residents (registered in the population registry) have access to heavily subsidized health care provided by both private and public health care providers and IVF treatments are covered under certain conditions discussed below.[3] Health care in Sweden is mainly funded by tax revenues and only 2% of all residents have private health insurance (Anell, 2008). IVF procedures are primarily regulated under the law on genetic integrity.[4] Access and funding varies across counties, with local county council boards being responsible for the setting and

[3]For most medical services, there is a small fee until the patient reaches the maximum amount of 1100 SEK (approximately 110 USD) annually. Above this fee the health care services have usually no additional costs.

[4]In Swedish: "Lag (2006:351) om genetisk integritet m.m.". Other aspects relating to IVF treatment such as establishing parenthood and defining and protecting patient rights are regulated in other laws, including the Children and Parents' Code (Föräldrabalk (1949:381)) and the Health and Medical Services Act (Hälso-och sjukvårdslag (1982:763)).

implementation of rules and requirements of IVF in their jurisdiction (Vårdguiden, 2017). For example, the maximum maternal age for government funded IVF differs across regions in the country. To take two cases, in Örebro county the upper age limit is 43 while the limit is 37 in Norrbotten county (Alm, 2010). The provision of IVF is allowed in private and public regimes, subject to the approval of the Health and Social Care Inspectorate (Inspektionen för vård och omsorg, IVO).[5]

The Swedish Association of Local Authorities and Regions provides guidelines for IVF treatments, including eligibility guidance although the local health care provider is responsible for making sure that these requirements are met and enforcement is not strict (SKL, 2016). The criteria laid down are as follows. First, the couple undergoing treatment should be in a stable union, either legally married or co-habitating for at least two years, although starting in 2016 single women are also allowed to access publicly funded IVF treatment. Second, the woman should have no previous children, either biological or adopted. Third, a medical assessment of the woman should be completed to confirm that her body mass index (BMI) is within the normal range, that there is no evidence of risky behavior such as smoking and use of alcohol and other drugs/narcotics, and that the county specific age restriction is met. Maternal age for starting the first treatment should be below age 40 and any remaining embryos/egg cells should be transferred before age 45. The age of the man should lie between 25 and 56 years. Fourth, three rounds of treatment (follicle aspiration) should be offered to each couple, and any remaining embryos and eggs of good quality should be frozen. Finally, additional conditions including mental and physical illness or disability are to be considered before offering treatment.

2.2 Number of embryo transfers and pregnancy success

During the 1980s and early 1990s, IVF had relatively low delivery rates per treatment. Therefore, multiple embryos were usually transferred in order to maximize the probability of a successful pregnancy (Karlström and Bergh, 2007). While this improved success rates it also raised the share of multiple births among IVF relative to non-IVF births. In response to multiple births exhibiting worse neonatal outcomes (Bergh et al., 1999), in the early 1990s Swedish clinics implemented a voluntary shift from triple embryo transfer (TET) to double embryo transfer (DET). This reduced triple births drastically towards zero without lowering the delivery rate but there remained a high prevalence of twins among IVF births throughout the 1990s (Karlström

[5]IVF using donated gametes is only permitted in publicly funded university hospitals under the law (2013:1147). For donated gametes an extraordinary assessment is required according to law (2016:18), with requirements similar to an adoption process.

and Bergh, 2007). It was not until 2003 that the single embryo transfer (SET) was introduced as a mandate.

The implementation of SET was a response to medical evidence that pregnancy success rates of IVF could be maintained with SET. The pioneering study was by Vilska et al. (1999), who looked at elective SET cases in Finland. Their evidence was broadly supported by one of the largest randomized control trials in this domain, with over 660 participants, conducted at multiple centers in Scandinavia (Thurin et al., 2004). This study showed that the success of IVF was maintained with SET under certain circumstances, namely when the woman was below 36 years and had at least two embryos of good-quality. They found that the cumulative rate of live births was not significantly different between elective SET (38.8%) and DET (42.9%), this being the probability of at least one live birth following transfer of one fresh embryo (under SET), and if needed, a subsequent transfer of a frozen embryo. Other randomized control trials with smaller samples, and subsequent observational studies provided broadly similar results (Criniti et al., 2005; Gerris et al., 2001; Karlström and Bergh, 2007; Lukassen et al., 2005; Lundin and Bergh, 2007).

2.3 The SET reform

On January 1 2003, the Swedish National Board of Health and Welfare issued new provisions and general guidelines for IVF, mandating that the routine procedure should be to transfer one embryo at a time in IVF treatments, with exceptions allowed for women with a low perceived risk of twinning. In particular, women with low embryo quality, those aged above 38 years and/or those women with more than three previously failed IVF cycles were still allowed double embryo transfer, provided that they were informed about the potential risks for the mother and child of undergoing a DET (Saldeen and Sundström, 2005).[6] The SET reform was motivated to improve birth outcomes and lower costs of neonatal care. Previous studies of SET suggested it achieved its goal of lowering costs per birth. One study estimates that costs six months following birth fell from approximately 160,000 to 90,000 Euros (Thurin et al., 2004) and another estimated that reduced maternal and neonatal hospital stays saved 10,000 Euros per birth (Lukassen et al., 2005).

Although exceptions were permitted, the reform generated a sharp increase in the share of IVF treatments that involved a single embryo transfer, from 30% to 70% within 24 months (see

[6]See also the 2003 provisions and general guidelines for IVF from the Swedish National Board of Health and Welfare: *Socialstyrelsens föreskrifter och allmänna råd om assisterad befruktning, SOSFS 2002:13.*

Figure 1a). The pregnancy success rate among IVF users was maintained at about one-quarter (Karlström and Bergh, 2007); also see Figure 2a. The number of IVF treatments performed is smooth around the cut-off (Figure 2b), although Figure 1b shows a slight decrease in the proportion of IVF births following the reform.[7] We shall formally test for discontinuities in the proportion of IVF births and deliveries per transfer (success rates) (see Section 5.2). Over this period there were no other changes in the IVF treatment procedure with respect to medication, technique or equipment (Saldeen and Sundström, 2005).[8]

3 Data and descriptive statistics

3.1 Data

We use Swedish administrative data to examine the impact of the SET reform. In particular, we use the Swedish Multi-Generational Registry (Flergenerationsregistret) provided by Statistics Sweden, which contains all registered people in Sweden after 1961. Our baseline sample consists of cohorts born between 1940 and 1985 including their children and parents. Based on these individuals, we select all women giving birth during 1998-2007 and their children, which are identified via the Swedish Medical Birth Registry. This sample constitutes approximately 98% of all births in Sweden during this period.

The Swedish Medical Birth Registry (Medicinska Födelseregistret) is provided by the National Board of Health and Welfare and contains detailed information on all pregnancies occurring since 1973 that led to a childbirth (at greater than 22 weeks of gestation) in Sweden. The information is provided by all Swedish prenatal care units, maternity clinics and neonatal care units, and contains extensive information regarding pregnancy, delivery, and health of the newborn child (including both stillborn and live births). These data include information on birth outcomes and child characteristics such as delivery mode, parity, multiplicity of births,

[7] See Figure A1 which shows a gradually rising trend in the proportion of IVF births and the share of twin births in all (IVF and other) births. Trends in the proportions of each type of ART procedures are presented in Figure A1, which shows that IVF is the only ART procedure exhibiting a strong trend.

[8] There is one exception. In January 2003, coincident with the SET reform, there was a change in regulation (*Socialstyrelsens föreskrifter och allmänna råd om assisterad befruktning* SOSFS 2002:13) that allowed donated eggs or sperm to be used in IVF treatments, although subject to an extensive assessment of the couple's medical, psychological and socio-economic characteristics, similar to those in an adoption process (Socialstyrelsen, 2016). Also the amendment allowing donated gametes was restricted to publicly funded university hospitals. In 2002, only 19 IVF cycles using donated egg cells were attempted resulting in 6 live births (Socialstyrelsen, 2006). While the number of IVF cases with donated eggs cells has increased (from 19 cycles in 2003 to 401 cycles in 2010, resulting in 86 live births), the share of IVF births using donated eggs cells is only 2% of all IVF births (Socialstyrelsen, 2013).

fetal position, gender, gestational age, birth weight, length, head circumference, Apgar score[9] measured at 1, 5 and 10 minutes, malformations and severe maternal complications.[10]

The Medical Birth Registry also contains detailed information on maternal characteristics such as age, number of previous births, weight, height, chronic diseases, and tobacco consumption. It also contains information on prenatal conditions and treatments such as the use of fetal diagnosis service and pregnancy complications (diagnosis and procedures). Since 1995, the Medical Birth Registry collects information on fertility treatments including standard IVF, Intra cytoplasmic sperm injection (ICSI), surgical procedures and ovarian stimulation, when resulting in a successful pregnancy delivered after week 22. During the period of 1998-2007, 21,783 babies born after IVF are registered in the Medical Birth Registry. Out of those, 20% are twin births.

We combine the Medical Birth Registry with the National Patient Registry in order to obtain data on the number of nights spent in hospital by the mother and child (inpatient care), as well as the Cause of Death Registry to obtain information on mortality. Both registries are provided by the National Board of Health and Welfare. Finally, administrative data on income and educational attainment of mothers is obtained from the Social Insurance Agency (Försäkringskassan) and the Swedish Agency for Innovative Systems (LISA), provided by Statistics Sweden. This information includes income from gainful employment, parental benefits and sickness benefits[11] as well as the highest level of education of each woman.[12]

3.2 Main outcome variables, data limitation and multiple hypothesis testing

In analyzing the effect of the reform we focus on mother and child health outcomes, and maternal labor market outcomes. As discussed above, child health outcomes include measures frequently used in the economic literature on early-life human capital (for example Apgar, birth

[9] Apgar score, measured 5 minutes after birth, stands for "appearance, pulse, grimace, activity, respiration" and is a five-criterion evaluation method, indicating the general health condition of the newborn baby 1, 5 and 10 minutes after the delivery.

[10] Severe maternal complications include postpartum hemorrhage (severe blood loss) and maternal sepsis (infection). Sepsis is defined as "infection of the genital tract occurring at any time between the rupture of membranes or labor, and the 42nd day postpartum, of which two or more of the following are present: pelvic pain, fever 38.5 C or more, abnormal vaginal discharge, abnormal smell of discharge, and delay in the rate of reduction of size of uterus (less than 2 cm a day during the first 8 days)" by the WHO (Bamfo, 2013).

[11] These variables are measured (respectively) as: total annual gross earnings in cash and net income from active business; total annual income from parental leave including income from parental allowance, temporary parental leave and child care allowance; total annual income caused by illness, injury and/or rehabilitation including a sick pay period of 14 days.

[12] This is a categorical measure from level 1-7. Level 1 is primary education less than 9 years, level 2 is primary education of 9 years, level 3 is 2 or fewer years of secondary education, level 4 is 3 years of secondary education, level 5 is fewer than 3 years of tertiary education, level 6 is 3 or more years of tertiary education and level 7 is graduate-level studies.

weight, length, head circumference and gender, infant mortality, and mortality under the age of 5) (Almond et al., 2017; Björkegren et al., 2016), as well as an additional set of rich measures available in Swedish registry data (malformation, breech presentation at birth, nights hospitalized during the first year of life and during years 1-4). For mothers, we examine a series of variables capturing health at the time of child birth, and potential results of child birth on subsequent health. These are the use of emergency C-section (a C-section after attempting vaginal delivery), maternal sepsis, postpartum hemorrhage, hypertension and nights hospitalized during the first year after delivery. Finally, we examine maternal labor market outcomes, namely income from gainful employment, parental benefits and sickness benefits in expressed in real terms using 1980 consumer price index and 100s SEK. We use an inverse hyperbolic sine transformation $\log(y_i + (y_i^2 + 1)^{1/2})$, given that a non-negligible portion of women have zero income in the years considered, and this measure can be interpreted in a similar way to a log transformation, while also being defined at zero.

The Medical Birth Registry contains all births delivered after the 22$^{\text{nd}}$ week of gestation (plus 0 days). As such, *unsuccessful* IVF treatments are not observed. For this analysis we focus on births that are the product of IVF procedures including standard IVF and IVF with ICSI. However, based on official usage figures, the Medical Birth Registry only contains approximately 70-90% of all IVF births occurring during this period, misclassifying 10-30% of all IVF births as non-IVF births. This means that the control group consisting of non-IVF births are "contaminated" by a small number of IVF births. We return to this point in section 5.3, and document that even under conservative assumptions it is likely to cause only a very small attenuation of estimated reform impacts.

When examining the impact of the SET reform on child and maternal outcomes we are interested in multiple outcomes to capture child or maternal well-being. We are thus faced with a problem of multiple-inference and risk over-rejecting null-hypotheses (i.e. an inflated rate of type I errors). We address this issue using two different approaches. First, we create summary indices for child health, maternal health and maternal labor outcomes separately. By doing so we decrease the number of hypotheses tested to a single outcome for each class of outcome variables. These indices are constructed as per Anderson (2008) by first ensuring that variables are consistently measured so that more positive values imply a positive change,[13] and then all variables are standardized by subtracting the mean and dividing it by the standard deviation

[13]For example, when considering the variables birth weight and premature, prematurity is multiplied by -1 so that both birth weight and "not premature" refer to positive health measures at birth.

of the variable in the control group. Finally, indices are created using a weighted average of the standardized variables of interest. Each variable is weighted by the inverse of the covariance matrix among the full set of variables so that those contributing the most linearly independent information receive a higher weight in the index.

Secondly, we adjust p-values by controlling for the false discovery rate (the proportion of type I errors in all significant findings) among all variables examined, using a step-up procedure described by Benjamini and Hochberg (1995). This method has the advantage of greater power compared to other approaches but at the cost of allowing for the false rejection of null-hypothesis (Anderson, 2008). We also report considerably more demanding Bonferroni (1935) corrected p-values, which controls for the Family Wise Error Rate, and thus sets the size of each test to avoid falsely rejecting *any* hypothesis.

3.3 Sample and summary statistics

We consider all twin and singleton births conceived between 1998 and 2007. We remove the small proportion of triplet and higher order births (516 births in all) given that these are a particularly extreme and uncommon outcome. This period consists of 60 months before and after the definition of the new SET guidelines relating to embryo transfer procedures. During this period there are 21,783 births following IVF recorded in the Medical Birth Registry and 916,110 non-IVF births.

Table 1 displays summary statistics for maternal and child characteristics for IVF (columns 1-3) and non-IVF births (columns 4-6). We report t-tests for the equality of means of each variable between IVF and non-IVF births in column 7 and p-values in column 8. As expected, the rate of twin births is significantly higher among IVF births: approximately 20% of IVF births result in twins compared to 2.5% among non-IVF births. Based on observable characteristics and outcomes, women conceiving using IVF are different to women with unassisted conception along multiple dimensions. First, women conceiving with IVF are much more likely to suffer from pregnancy and birth complications such as hypertension, maternal diabetes, postpartum hemorrhage, maternal sepsis and emergency C-sections compared to non-IVF mothers. For example, mothers conceiving with IVF had double the risk of postpartum hemorrhage (12%) and emergency C-section (16%) compared to non-IVF mothers. IVF mothers are also more likely to be hospitalized the year after giving birth. Second, women conceiving with IVF are somewhat taller and have a slightly higher weight. Moreover, IVF mothers are on average older (age 33) than non-IVF mothers (age 30). The age distribution of IVF and non-IVF mothers is presented in Figure 7. IVF mothers have higher education attainment based on the categorical measure available (4.7 compared to 4.5) and higher labor income (average annual income before birth is 65.4 TSEK compared to 42.5 TSEK).[14] However, women conceiving with IVF receive more sickness benefits before giving birth, of 1.9 TSEK compared to 1.4 TSEK and are more likely to suffer from diseases like ulcerative colitis. Moreover, behavioral differences are found between women conceiving with IVF and without IVF: IVF mothers are more than 50% less likely to smoke cigarettes during the first (4.2% versus 9.6%) and third (2.4% versus 6.6%) trimester. These differences in means suggest that high SES mothers (higher education and non-smokers) select into IVF but also that women conceiving with IVF are more likely to suffer from chronic diseases and receive sickness benefits.

Differences in means between children born following IVF and non-IVF are displayed in Panel B, and show a similar pattern of poorer health outcomes. IVF children have lower Apgar scores (9.63 versus 9.73) as well as a higher likelihood of having Apgar scores below 7. Mortality rates (infant and under-5) is higher among children born after IVF with means of 4.5 and 5.6 compared to 2.7 and 3.4 for children with an unassisted conception. Other important health indicators show a similar pattern, including shorter gestation by nearly a week, lower birth weight by 300 grams, smaller head circumference and shorter length at birth. Malformations, breech presentation, neonatal hospitalization and hospitalization during ages 1-4 are higher among children born after IVF. No statistical difference is observed in the sex ratio.[15]

Previous studies suggest that twin births are a major contributor to the observed differences in outcomes between children born following IVF and those following unassisted conceptions (Kalra and Barnhart, 2011). However, singletons born after IVF have also been shown to exhibit poorer health outcomes compared to non-IVF births (Pinborg et al., 2013). Similarly, as documented in Table 2, we observe significant differences between singletons born after IVF and those not following IVF (in the pre-reform period). These differences are however smaller. Singletons born after IVF weigh 100 grams less than non-IVF births and exhibit alleviated risk of mortality and hospitalization. This provides suggestive evidence that a significant part of the differences in poorer health outcomes between IVF and non-IVF births is due to the higher prevalence of multiple births.

[14] Expressed in real terms using 1980 consumer price index.

[15] Male fetuses are less resilient to more demanding conditions in utero (Almond and Mazumder, 2011).

3.4 Descriptive statistics of the SET reform

Trends in the rates of twin births among IVF and non-IVF conceptions are displayed in Figure 3. A clear drop in rates of twin birth from 30% to 13% is seen among IVF births in line with the SET reform in 2003, while no similar change in rates of twinning are observed for non-IVF births during this period. The improvements in child health for children born after IVF is seen across multiple outcomes and displayed in Figures 4 and 5. These graphs show a clear pattern of improved health for children born after IVF compared to non-IVF children when considering birth weight, gestation, length, head circumference and Apgar score as well as reductions in mortality and the probability of hospitalization within one year of birth. Trends in maternal health and labor market outcomes are presented in Figure 6, and show a similar pattern of improved outcomes for the likelihood of emergency C-section as well as somewhat decreased sickness benefits and higher labor income.

A before and after comparison of differences in means in outcome variables for IVF births during 1998-2007 is presented in Table A2 and confirms the observed trends. Means are reported, with standard deviations below each mean, and an associated t-test in column 3. As in the graphical evidence, significant differences in means around the reform are found for multiple child outcomes including birth weight, gestational age, length and head circumference as well as a lower probability of mortality. In terms of maternal outcomes, higher labor income and lower sickness benefits are observed following the SET reform, and health improvements are observed, for example a lower prevalence of emergency C-sections.

4 Empirical strategy

We estimate the impact of the SET reform using the following difference-in-differences (DiD) specification:

$$Y_{it} = \alpha + \beta_1 (PostSET \times IVF)_{it} + \beta_2 IVF_i + \boldsymbol{X}_{it}\delta + \alpha_c + \pi_t + \varepsilon_{it}. \qquad (1)$$

This exploits both variation in IVF usage and reform timing, comparing outcomes for IVF and non-IVF births prior to and posterior to the January 1, 2003 policy change. The dependent variable Y_{it} refers to a birth or maternal outcome for birth i in year t, and IVF_i refers to the IVF status of each birth (1 if IVF was used, or 0 otherwise). The parameter of interest is β_1, capturing the change in outcomes for IVF births relative to non-IVF births after relative to before the reform was implemented. While obstetric outcomes among IVF births are expected to be better post-SET, IVF children will nevertheless tend to have poorer obstetric outcomes than children born following an unassisted conception (Sazonova et al., 2011). Our estimates will allow us to capture not only the SET-led improvements in IVF outcomes but also the extent to which SET led to a convergence of outcomes from IVF with outcomes from non-IVF. Here $PostSET$ is a binary variable based on estimated date of *conception*: all births estimated to have been conceived after January 1 2003 are assigned as $PostSET = 1$.[16] Rather than include the uninteracted $PostSET$ term in the regression, we include a series of year fixed effects π_t to flexibly control for any time varying unobservables that may have evolved in a manner similar to the reform. County-specific fixed effects α_c capture time-invariant geographical variation in the outcomes.

In some specifications, we additionally include maternal and birth characteristics, denoted $\boldsymbol{X}$. These include age and pregnancy order fixed effects, maternal height and weight before pregnancy, nationality (a binary variable for having been born in Sweden or not), whether the mother smoked during the first trimester of pregnancy, and the mother's educational level, sickness benefits and labor income averaged over the 3 years prior to birth. The idiosyncratic error term is denoted by ε, and is clustered on the mother. We estimate equation 1 using OLS.

The identifying assumption is that in the absence of the SET reform, outcomes associated with IVF and non-IVF births would have followed similar trends over time. In order to test the plausibility of this assumption we estimate an event study, interacting the "treatment" indicator (IVF) with a binary variable for each year prior and posterior to the reform date. The specification we estimate is:

$$Y_{it} = \alpha + \sum_{k \in \ell} \gamma_k (IVF_i \times \mathbb{I}\{Year_t = SET + k\}) + \beta IVF_i + \boldsymbol{X}_{it}\delta + \alpha_c + \pi_t + \nu_{it}, \qquad (2)$$

where $\ell = \{-4, -3, -2, 0, \ldots, 4\}$ and the year before the SET reform, 2002, is omitted as a base category. Equation 2 is similar to equation 1 except that instead of defining differences around a single post-SET binary variable we allow the difference between IVF and non-IVF births to vary year on year. If IVF and non-IVF outcomes exhibit differential pre-trends then this will be evident in a test of the lagged coefficients.

We estimate the reduced form impact of the SET reform, that is, the average treatment

[16] Conception date is computed by subtracting the gestational days from the date of birth analogous to Currie and Schwandt (2013). Although date of birth is not available in our data set, we use the discharge date for the maternity unit.

effect among all IVF births (the intent to treat estimate). While the substantive change brought about by SET is a reduction in twinning and this is the main mechanism for impacts on outcomes, maternal selection into SET may also play a role. We expect positive selection into SET since women perceived to have a low risk of twinning (older women and/or multiple previous failed IVF cycles) were allowed to elect for DET following the SET reform. Positive selection is a concern as it will tend to lead to overestimation of the improvements in child and maternal outcomes. To account for selection into SET we estimate a specification that conditions upon mother fixed effects. However, only approximately 50% of all IVF mothers in the sample have more than one birth. We therefore show results on the restricted sample of women with and without mother fixed effects. This way we can isolate changes in the estimates arising from selection of a sample of women with at least two births from changes in the estimates associated with selection into SET. As one check on the twinning channel, we estimate a regression of the outcomes Y_{it} on whether the birth is a twin birth, instrumenting the indicator for a twin birth with an indicator for whether the birth occurred post-SET. This provides a local average treatment effect (LATE) for SET compliers.

We will subject the estimates to a number of robustness checks. We will discuss the fact that SET was mandated two years earlier in one county and re-estimate the model excluding this county. We also re-estimate it excluding the two years during which we see a gradual increase in the share of SET births among IVF mothers, so that identification comes from a sharp discontinuity in this share. We will investigate changes in the composition of mothers selecting into IVF treatment after SET, although this is accounted for by the main effect of IVF in our specification.[17] We will investigate heterogeneity in impacts of the reform by mother characteristics. Of particular interest, we will show all results for all women and then again for first-time mothers (44% of the sample).

5 Results

5.1 Twin births

Table 3 presents the impact of the SET reform on the likelihood of a twin as opposed to a singleton birth. For the full sample (columns 1-2) we estimate a reduction in the share of twins among IVF births of 16.8 to 17.3 percentage points (pp), depending on whether we do or do

[17]As explained in the Data section, our data do not allow us to investigate differences in characteristics of women who post-SET end up electing for SET vs DET, but we discussed above how we account for selection into SET among IVF users.

not control for the mother's characteristics. Estimates for first-time mothers (columns 3-4) are very similar at 17.7 pp, which is not sensitive to controls for woman characteristics. Using the twin rate among IVF births in the pre-treatment period of approximately 27 %, our estimates indicate that the SET reform narrowed the gap in twinning between IVF and non-IVF births by about 63%. To account for omitted trends that are specific to IVF outcomes, we include IVF-specific split linear time trends (columns 1 and 3) and IVF-specific (global) linear time trends (columns 2 and 4) (see Table A3). The general pattern of the results is maintained but the reduction in twinning is closer to 13 pp rather than about 17 pp.

We investigate impacts of SET on twinning by sub-groups identified by birth order, the mother's age at treatment, her education and her BMI; see Table 4. We see a statistically significant reduction in the share of twins among IVF births in every sub-group except for women 40 years and older. As these women have a lower probability of twinning (see Panel B of the table: the probability is 14% compared with about 27% on average), they were probably exempt from SET. Estimates by birth order show that the impact of SET is smaller for births of order 3 or higher, estimates by age show that the impact of SET on twinning is hump-shaped in age, being smaller for women under 25 and women 40 or older. There are no significant differences by the woman's education, but impacts are smaller for women with low BMI relative to other women.

5.2 Child and maternal outcomes

Table 5 presents estimates of the impact of the SET reform on child health, maternal health and maternal labor market outcomes for the full sample (columns 1-3) and first-time mothers (columns 4-6). We identify a significant improvement in the index of health of 0.189 standard deviations (SD), which is similar for first-time mothers and all mothers. This implies that the SET reform reduced the health gap between IVF and non-IVF children of -0.355 SD by 53%. The impact of SET on the maternal health index falls just short of significance in the sample of all mothers but it is statistically significant for first-time mothers, for whom health improves by 0.056 SD. This narrows the gap between mothers with IVF and non-IVF births by 36% in this group (observe that the gap is in fact similar for first-time mothers and other mothers). Maternal labor market outcomes within 3 years of birth improve by 0.106 SD for the full sample and by 0.156 for first-time mothers. Consistent with extensive margin fertility (the first birth) having larger impacts on labor market outcomes, the IVF/non-IVF gap in labor market outcomes is larger for first-time mothers (-0.163) than for all mothers (-0.125). The estimates suggest that SET narrowed the gap by 85% for the full sample, and nearly closed it for first-time mothers.

We present 2SLS estimates using the passage of the SET reform to instrument the likelihood of giving birth to a singleton in Table 6. We thus examine the (SET mediated) impact of having a singleton birth rather than a twin birth on child health, maternal health and maternal labor market outcomes. This is the local average treatment effect on compliers: women who had a singleton birth because they had IVF treatments after the policy change, but who would have had twins if SET were not the default policy. In columns 1-2, the first stage results are presented, which show the strong reduction in twinning for the full sample and first-time mothers, with F-statistics far exceeding typical weak instrument thresholds. For the full sample, the 2SLS estimates suggest a strong and significant impact of having a singleton child on the child's own health (1.1 SD of the index) as well as on maternal labor market outcomes (0.63 SD of the index). For first-time mothers, the 2SLS estimates suggest that having a singleton birth compared to twin births causes a strong positive impact on child health (1.02 SD), maternal health (0.31 SD) and labor outcomes (0.87 SD).

Examining each child health outcome separately in Table 7, for the full sample (Panel A) and first-time mothers (Panel B), we find that the improvement in child health is driven by multiple factors. In particular, the reform led to an increase in the average absolute Apgar score (column 1), a lower probability of having an Apgar score below 7 (column 2), increased birth weight (column 3), increased length of the baby at birth (column 4), a larger head circumference (column 5), longer gestation (column 6), declines in infant and under 5 mortality (columns 8 and 9), a lower likelihood of breech presentations (column 10) and a lower probability of hospitalization during first year of life (column 11). We find no effect on the child's gender, rates of malformation or hospitalization during ages 1-4. The magnitudes of these effects are large. For example, average birth weight increases by 175 grams, closing the gap between IVF and non-IVF babies by 57%. Similarly the gestational age increased by more than half a week following the reform, closing the gap by 52%. Changes in birth length and head circumference also reduced the IVF–non-IVF differential by 50%. These findings are of interest given the well-documented causal relationships between birth weight, gestational age, length and head circumference with later life outcomes (Bharadwaj et al., 2013; Björkegren et al., 2016). To account for multiple hypotheses when examining individual child-health components, we correct the *p*-values with a false discovery rate as well as Bonferroni correction. Even when correcting *p*-values with the Bonferroni correction—a particularly demanding test—highly significant effects on Apgar score, birth weight, length, gestation, head circumference, hospitalization and breech presentation remain. Similar results are found for first-time mothers but with slightly larger effects.

We estimate the impact of the SET reform at particular points in the distribution of birth weight and gestational length that are commonly used in the literature, presented in Table 8. Results are presented for the full sample in Panel A and first-time mothers in Panel B. We focus on these weights/dates given their importance in the targeting of medical resources based on (arbitrary but commonly used) treatment cut-offs (Almond et al., 2010; Bharadwaj et al., 2013). For the full sample, the impact on the likelihood of being born with a weight below 1500 grams (very low birth weight), is large and negative, at 1.2 pp, and for a weight below 2500 grams (low birth weight) is a 6.8 pp reduction (column 2). This corresponds to a decrease of 60% when compared to the rate of low birth weight babies born via IVF before the reform. Similarly, the probability of preterm delivery before week 28 (column 3), 32 (column 4) and 37 (column 5) decreases by 0.5, 1.3 and 8.3 pp, closing the gap by around 50% in each case. Importantly, these results demonstrate that average impacts on birth weight and gestation are not driven only by changes on the upper quintiles of outcome distributions. Very similar results are found for first-time mothers (Panel B).

Turning to maternal outcomes, the results for each separate health outcome from the maternal health index are presented in Table 9. The results are presented for the full sample (Panel A) and first-time mothers (Panel B). These results demonstrate no significant impact on complications such as postpartum hemorrhage (column 2), maternal sepsis (column 3), post birth hospitalization (column 4), or hypertension (column 5) for either the full sample (Panel A) or for first-time mothers (Panel B). Unsurprisingly, we do observe a significant negative impact on the likelihood of engaging in an emergency C-section at birth (column 1), and this impact closes the gap between IVF and non-IVF births by 42% for the full sample and 60% for the first-time mothers. The negative impact on emergency C-section remains highly significant when correcting for multiple hypothesis testing using a FDR and Bonferrroni correction.

We document the impact on each labor market outcome separately in Table 10, again presented separately for the full sample (Panel A) and first-time mothers (Panel B). We have transformed each income variable using hyperbolic sine transformation, so each coefficient can be interpreted as a percentage change. Here we observe that income from parental benefits decreases by 3.4%, sickness benefits decline by 43.6% and labor income increases by 8.5%. These findings suggest that in the three years following the birth of a child, IVF mothers have significantly higher labor earnings and lower usage of benefits and transfers. Correcting for multiple hypotheses tests renders no longer a significant effect on parental benefits and labor income when using the conservative Bonferroni correction (but not when referring to q-sharpened p-values). For first-time mothers, a similarly strong decrease in sickness benefits is found by 46.0%, but with no significant impact on labor income or parental income within 3 years after giving birth.

5.3 Identifying assumptions

Parallel trends and event studies

As is standard in difference-in-differences analyzes, correctly identifying the impact of the reform requires a parallel trends assumption. In this case, we must assume that trends in outcomes among IVF and non-IVF women would have evolved similarly over time in the absence of the reform. While this assumption cannot be tested directly, we can partially test its plausibility using event studies to examine the evolution of outcomes in the IVF and non-IVF groups in the pre-reform period.

In Figure 4, we present trends in birth weight, gestational age, length, head circumference and Apgar score, which show a clear improvement following the SET reform (the reform date is indicated by the red vertical line). Trends in mortality and hospitalization exhibit larger variation but also show an improvement following the 2003 reform. No apparent decrease is seen in malformations and hospitalization during ages 1-4. While some outcomes exhibit larger variation, overall, these graphs suggest approximately parallel trends in the outcome variables prior to the reform by simple visual inspection. While trends in maternal labor market outcomes i.e. income, parental, and sickness benefits appear to be parallel in the pretreatment period depicted in Figure 6, trends in maternal health exhibit large variation making it hard to assess the presence of common trends in the pre-treatment period presented in Figure 6.

To examine if the assumption of parallel trends is satisfied, we formally test this by IVF and reform lags and leads, as per equation 2. By allowing for a more flexible model we can infer trends in the pre-treatment period as well as whether the effect is persistent in the post-reform period. The event studies are presented in Figure 8, and confirm previous findings of a sharp and persistent decline in twin births (Figure 8a). Similarly, we find a strong increase in both child health (Figure 8b) and maternal labor market outcomes (Figure 8d) but no impact on maternal health (Figure 8c). Event studies for each component in the indices are presented in Figures A2, A3, and A4. In terms of pre-trends, twin births exhibit two coefficients in the pre-reform period which are significantly different from zero, suggesting some fluctuations in the pre-reform period. The maternal labor market index is not significantly different from zero in the pre-reform period suggesting that we cannot reject the absence of parallel trends in labor market outcomes prior to the SET reform. The event study of the child health index also suggests parallel trends in the pre-reform period.

We further examine if the results are robust to the inclusion of linear time trends, presented in Table A4, using both split trends (allowing for different slopes across the time of the reform) for IVF and non-IVF births (Panel A) and global trends for IVF and non-IVF births (Panel B). For the full sample, the results are consistent with the baseline results but with somewhat smaller coefficients for child health index and maternal labor index (of 0.137 and 0.061 respectively). The exception is the maternal health index, which suggests a significant positive effect of the magnitude 0.106 SD. For first-time mothers, the results are similar to the baseline, but with somewhat smaller magnitude for child health and labor outcomes (0.097 and 0.105 SD) and larger magnitude for maternal health. In Panel B, the results when including IVF specific linear trends are presented for the full sample (columns 1-3) and show a similar result to the baseline result for child health but with a smaller and less statistically significant impact on the maternal labor market index and with a positive impact on the maternal health index. For first-time mothers, results are similar to the baseline but with a stronger impact on the maternal health index. These results show that the estimated impact of SET is overall robust to the inclusion of trends but with the exception for maternal health outcomes, which indicates a positive significant impact when controlling for trends.

Compositional changes in IVF mothers and maternal selection to SET

The proportion of IVF births has increased since the 1990s, tracking changes in technologies, costs, and availability of IVF (see Figure 1b). It is likely that the composition of mothers using IVF also changed throughout this time. In order for this to invalidate our identification strategy, the composition of mothers must evolve differently for IVF and non IVF users around the date of the SET reform. To further explore this, we examine possible compositional changes using observable maternal characteristics including age, height, weight, education, labor and sickness benefits prior birth, nationality, smoking, asthma, epilepsy and ulcerative colitis. That is, we perform a balancing test of covariates across the time of the reform by running regressions using maternal characteristics as outcome variables and the SET reform as the explanatory variable.

The results are presented in Table 11, and show a significant impact on three outcomes out of eleven. These variables are: age (column 1), education (column 4), and smoking (column 8). While the magnitude of the coefficient on age is rather small, the magnitude of the estimates for education calls for closer consideration. It is hard to assess how a potential change in "quality" in mothers have evolved over time. These estimates suggest that mothers are both slightly older and somewhat more educated but also slightly more likely to be smokers.

We investigate the influence of this on our estimates of the impacts of SET using three complementary approaches. First, we re-estimate the baseline results including linear trends in maternal characteristics and IVF-status in each model. In Table 12, the estimates are presented for the full sample (columns 1-3) and first-time mothers separately (columns 4-6). For the full sample, the results show a positive and significant impact on child and labor market outcomes, as in the baseline specification. The estimate for child health (0.149 SD, column 2) is similar to the baseline estimate but for the maternal labor market index the estimated coefficient is smaller (0.059 SD, column 3). In contrast to the baseline results, a positive and significant effect is found for maternal health (column 2) of approximately 0.112 SD, which suggests that maternal health may have improved by the reform. For first-time mothers, the result is similar to the baseline but with a somewhat smaller magnitude in child health (0.122 SD, column 4) and somewhat larger impact on maternal health (0.105 SD column 6).

Since we can only control for trends in observable characteristics, we also examine estimates based on mother fixed effects. These will control for all unobservable time-invariant characteristics of mothers. To implement this model we need to restrict the sample to women with at least two pregnancies.[18] Approximately 50% of all IVF mothers have more than one pregnancy, and as such, the use of mother FEs excludes half of the sample. Since mothers with two pregnancies may not be representative of all mothers, we examine characteristics of IVF mothers with one pregnancy versus two pregnancies, and these are presented in Table A5. These mothers differ across multiple dimensions, for example, IVF mothers with only one birth have a higher risk of complications than those with two births. For this reason, we estimate the model without mother fixed effects on the reduced sample before we introduce the fixed

[18]We have shown that the SET reform led to a highly significant decline in twin births among a broad category of women e.g. all education levels, BMI classifications, parity and ages except for women older than 39. This suggests that compliance with the SET reform is higher among younger women. Provided that child health is negatively correlated with rising maternal age, this selection could bias our estimates upwards. In addition, there is likely to be maternal selection based on unobservable characteristics, where mothers with multiple previously failed IVF cycles are less likely to comply with SET, and are at a greater risk of adverse birth outcomes. That is, maternal selection into SET could potentially lead to biased estimates, and in particular, may cause *upwardly* biased estimates of the improvements in child health and maternal labor market outcomes.

effects. We can then assess how the coefficients change with the sample independently of how they change with controls for unobserved mother-level heterogeneity.

The results are presented in Table 13 and show the impact of the SET reform on twin rates (column 1), child health (column 2), maternal health (column 3) and labor market outcomes (column 4). In Panel A, the impact of SET on the selected sample of IVF mothers excluding mother fixed effects are displayed, and show that the coefficients are similar to the baseline results. In Panel B, results are presented including mother fixed effects. Again, similar results to the baseline results are observed but with somewhat lower precision when including mother fixed effects. We continue to observe large and significant effects of the SET reform even when only examining *within*-mother variation in IVF laws. The impact of the SET reform is estimated to increase child health and labor market attachment of women following birth.

To assess the magnitude of a potential selection bias, we postulate that selection based on observable explanatory variables provide information on selection on unobservables as suggested by Altonji et al. (2005). We consider the magnitude of omitted variable bias needed to eliminate the impact of the SET reform, by computing the ratio of how large the covariance between unobservables and the SET reform and the covariance between observables and the SET reform must be to explain away the impact of SET.[19] The more the inclusion of controls is affecting the coefficient indicating the treatment effect, the larger the potential bias is and vice versa. A large ratio indicates that it is less likely that omitted variables would explain away the impact of the reform. Table 14 presents the results without including maternal controls. The coefficients are very similar with and without controls, which is indicated by the ratio between the two regression models presented in the bottom row. This simple exercise suggests that, given the limited selection on observables (showed by the ratio) we may assume that selection on unobservables is equally limited.

5.4 Mechanisms

We examine potential mechanisms through which maternal labor market outcomes are affected by examining the importance of different components of the reform. We consider the negative fertility shock as a direct effect of the reform and child and maternal health as indirect effects. As a mechanism test we include child and maternal health outcomes, which themselves are outcomes of the reform and therefore "bad controls" (Angrist and Pischke, 2009). However,

[19] This can be computed by using the estimates from OLS regressions both with and without controls: $\frac{\alpha_{controls}}{\alpha_{nocontrols}-\alpha_{controls}}$. For a more detailed discussion see Bellows and Miguel (2009).

they will provide us with information on how much the reform is affecting maternal labor outcomes when improvements in child and maternal health are controlled for. To do this we adopt the conditional decomposition proposed by Gelbach (2016). We are interested in how much the estimated effect of the SET reform is affected by including maternal and child health indicators, $\beta^{uconditional} - \beta^{conditional} = \delta$, in which β^{uc} indicates the unconditional specification excluding child and maternal health and β^{c} expresses the conditional specification including child and maternal health. We can augment this expression by

$$\begin{aligned} \beta_{labor}^{uconditional} - \beta_{labor}^{conditional} \\ &= \Gamma_{labor}^{childhealth}\beta_{labor}^{childhealth} + \Gamma_{labor}^{maternalhealth}\beta_{labor}^{maternalhealth} \\ &= \delta_{labor}^{childhealth} + \delta_{labor}^{maternalhealth} = \delta_{labor} \end{aligned} \tag{3}$$

where Γ represents each estimate of the SET reform (postSET$\times$IVF) for each potential mechanism as the outcome variable. The coefficient β indicates the estimate of the potential mechanisms as explanatory variables in the full specification with maternal labor outcomes as the dependent variable. The conditional contribution of each component is given by δ, which is computed by multiplying Γ with β.

In Table 15, the potential mechanisms of child and maternal health are included in the baseline specification, Equation 1. Table 15 presents the impact of the SET reform on child health (column 1), maternal health (column 2) and maternal labor index including the potential mechanisms of child and maternal health (column 3). To see how each of the components are affecting the maternal labor index, the Gelbach decomposition is presented in column 4. The decomposition shows that the impact of changes in child health owing to the reform explains only 0.005 of the improvements in the maternal labor market index, with an even smaller value of 0.002 owing to changes in maternal health. The total explained difference is 0.007 of the total 0.099 SD of improvements in the maternal labor market index following from the SET reform. This suggests that the drop in fertility is the main contributor to improvements in maternal labor outcomes.

5.5 Additional robustness and sensitivity

Additional robustness checks are presented in Table 16. First (Panel A), we remove births from the two years prior to the SET reform (2001-2002) in order to account for a potential gradual increase in SET, which may bias our estimates downwards because of a partially contaminated control group. The impact of SET on twin birth (column 1), child health index (column 2), maternal health index (column 3) and maternal labor index (column 4) shows a similar coefficient and effect size to the baseline results. In Panel B, we remove the region of Skåne because of a regional rule mandating SET as the default starting in 2001 in this region. The results remain largely similar to the baseline results when removing this region.

In 2005, Sweden started to offer same-sex couples publicly funded access to fertility treatments including IVF. Previous literature suggests that same-sex couples exhibit a higher socioeconomic status (Ahmed et al., 2011a,b). Their children, however, exhibit somewhat worse birth outcomes in terms of lower birth weight, when compared with children born to heterosexual couples (Aldén et al., 2017). While the number of children born to lesbian parents during 1995-2010 is only 750, we further examine a potential impact of this legislative change. We restrict our sample to conceptions occurring during 1998-2004, and re-estimate our baseline model. The result is presented in Panel C in Table 16. The results are similar to the baseline results but with somewhat smaller estimates of -15 pp for twin births, 0.154 SD for child health and 0.067 SD for maternal labor index.

As discussed previously, the Medical Birth Registry correctly identifies approximately 70% of all IVF births based on reported usage in aggregate national IVF data. We may be concerned that approximately 30% of IVF births are mis-reported, and are incorrectly reported as non-IVF births, thus contaminating the control group. In practice, given that the size of the "treatment" group is much smaller than the size of the "control" group, even if the reform's impact was very-large, the 30% of mis-classified IVF births will be unlikely to impact averages in the control group in a substantive way. To see this, consider that the number of observed IVF births in the Medical Birth Registry is 21,783, and the number of non-IVF births is 916,110. Inflating the number of IVF births from 70 to 100% suggests that there are 9,356 IVF births incorrectly classified as non-IVF births. This is only slightly over 1% of the entire group of births assumed to be non-IVF births. We provide additional discussion, as well as a calculation of the (small) magnitude of any expected attenuation, in Appendix A2.

6 Conclusion

The invention of IVF allowed radical changes in the fertility behavior of some women and families, providing the opportunity to postpone childbearing, as well as assisting involuntary childlessness. However, there are also well-documented immediate and long-run costs associated with IVF-usage. As well as the direct financial costs of procedures, IVF births have been documented to be considerably more likely to suffer from adverse health outcomes when compared to non-IVF births (Saldeen and Sundström, 2005). The adverse health outcomes following IVF are mainly attributed to the increased likelihood of multiple births. Twin births are widely documented as a major risk factor for mothers and children, for example, given the alleviated risk of preterm birth, low birth weight, fetal malformation and complicated delivery (Gelbaya et al., 2010). In particular, premature delivery is associated with higher mortality as well as long term adverse effects on neurological development (Gelbaya et al., 2010).

In this study we document the causal impact of a reform mandating single embryo transfer (SET) for IVF treatments on a broad set of child and maternal health and labor market outcomes. Using rich Swedish registry data for the time period 1998-2007, we find that the SET reform led to a sharp drop in rates of twin births: by over 60% for women under age 40. Reduced rates of twinning are observed for a broad category of women across birth order, education level and BMI classification. We find a highly significant and sizeable impact of the SET reform on child health as measured by Apgar score, gestational age, birth weight, length and head circumference. These findings are important given the well-known links between human capital at birth, and outcomes across the entire life-course including cognitive and non-cognitive ability, educational attainments, health and life expectancy. Moreover, our results suggest a decrease in complications of labor such as breech presentation at birth, emergency C-sections and reduced usage of neonatal hospitalization. Overall, our estimates suggest that the adoption of SET as the official IVF procedure reduced the health differential between IVF and non-IVF births by over 50%.

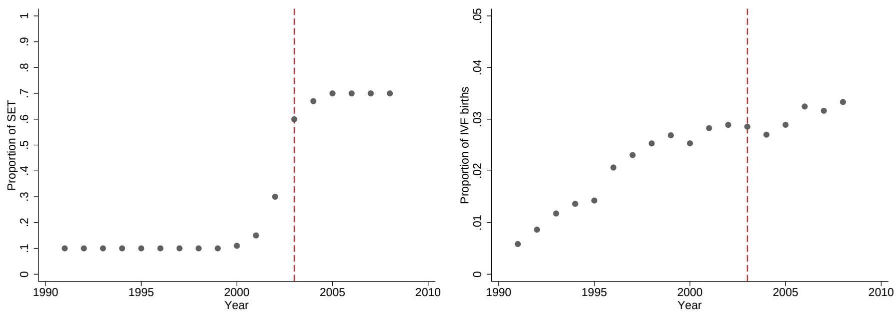
We also find that the adoption of SET resulted in a sizable and significant impact on maternal labor outcomes. The positive impact on labor market outcomes originate from a reduction in the usage of sickness benefits, and increased labor income in the period three years subsequent to birth. A significant impact on maternal health is found for first-time mothers, closing the gap in maternal health between IVF and non-IVF mothers by 36%.

The large magnitude of health benefits from SET is not limited to the children, mothers and families, but will also have a positive effect on the health care system and social safety net. Improvements in health at birth and during gestation will have follow-on effects, reducing demand for prenatal, obstetric and neonatal care. The SET reform is likely to reduce the long-term costs associated with IVF procedures, but at the immediate cost of less choice for women and couples seeking IVF. However, given that the delivery rate was unchanged despite the shift from DET to SET, this suggests there is no fertility-cost to the increased health of the child at birth. One reason why multiple embryo transfer is still common in many industrialized countries is likely due to financial incentives, jeopardizing the health of children and mothers (Karlström and Bergh, 2007). IVF has increased rapidly since the 1980s and is now a key feature of the reproductive landscape and is likely to increase further in the near future. Any improvements in individual and aggregate health due to the adoption of SET as a default IVF option will be magnified as rates of IVF use increase.

7 Figures and tables

Figure 1: Trends in SET and proportion of IVF births

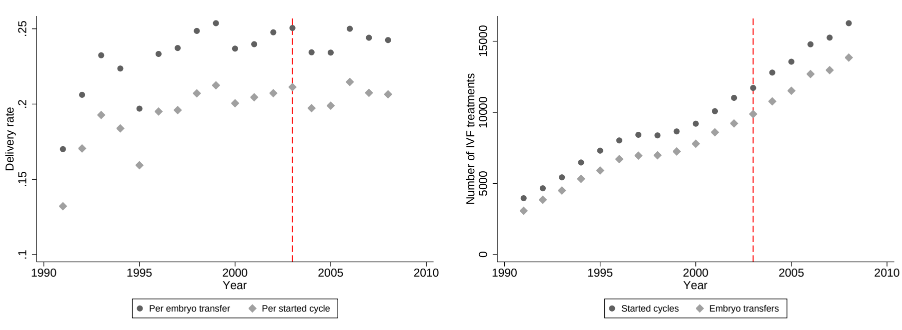
(a) Single embryo transfer **(b)** Proportion of IVF births



Annual trends in SET and proportion of IVF births are based on aggregated data collected from annual reports by the Swedish National Board of Health and Welfare and presented in Figures 1a and 1b. The red vertical line indicates the year of the SET reform.

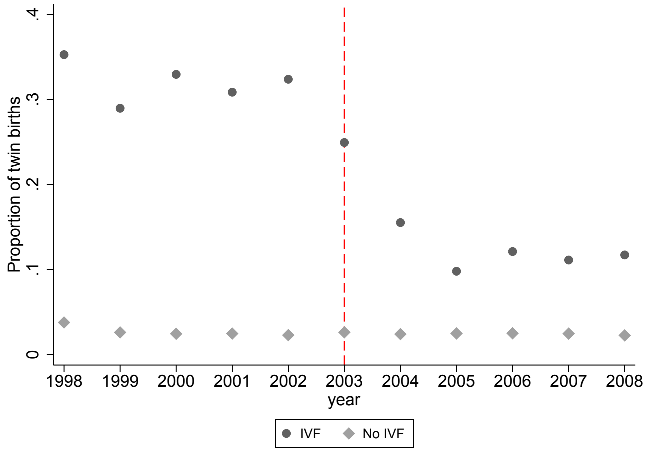
Figure 2: Trends in delivery rate and IVF treatments

(a) Deliveries per embryo transfer and cycle **(b)** Number of IVF treatments



Annual trends in deliveries per transfer/cycle and the number of IVF treatments are based on aggregated data collected from annual reports by the Swedish National Board of Health and Welfare and presented in Figures 2a and 2b. The red vertical line indicates the year of the SET reform.

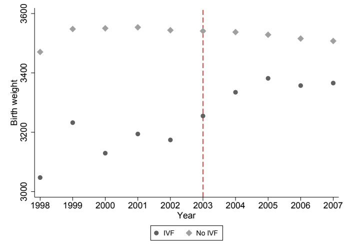
Figure 3: Trends in twin rates



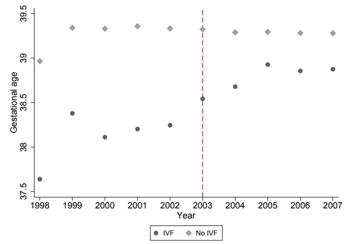
Annual trends in twin births with and without IVF conception, using data obtained from the Swedish Medical Birth Registry, are presented in Figure 3. The red vertical line indicates the year of the SET reform.

Figure 4: Child health outcomes

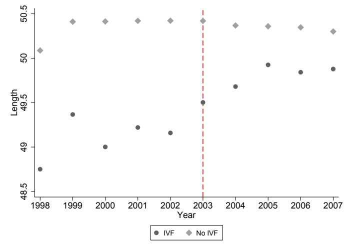
(a) Birth weight



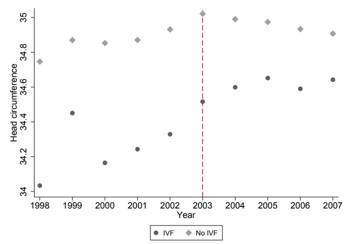
(b) Gestational age



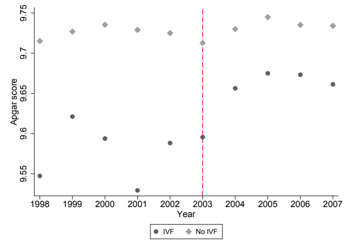
(c) Length



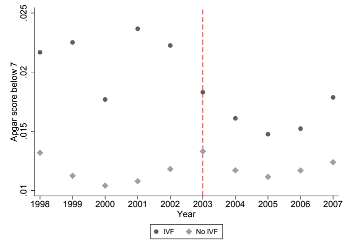
(d) Head circumfernce



(e) Apgar score



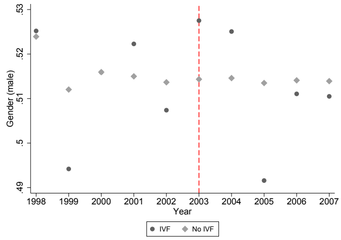
(f) Apgar score below 7



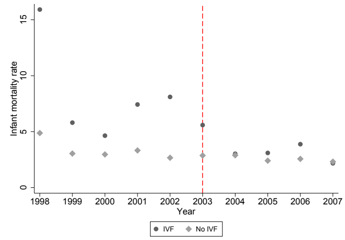
Annual trends in child health outcomes with and without IVF conception, using data obtained from the Swedish Medical Birth Registry and Patient Registry, are presented in Figures 4a to 4f. The red vertical line indicates the year of the SET reform.

Figure 5: Child health outcomes

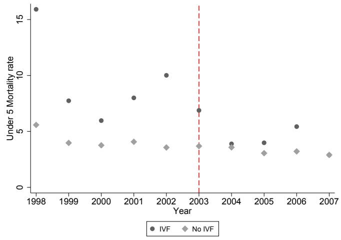
(a) Gender (male)



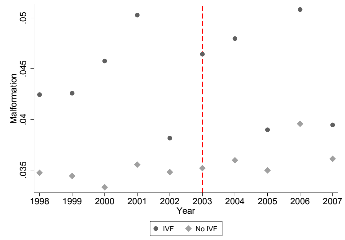
(b) Infant mortality



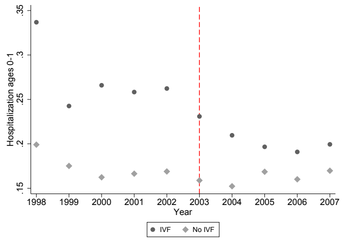
(c) Under 5 mortality



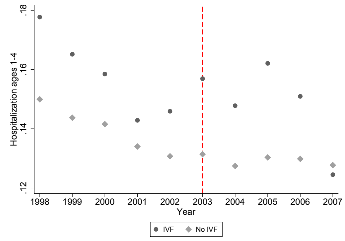
(d) Malformation



(e) Hospitalization ages 0-1

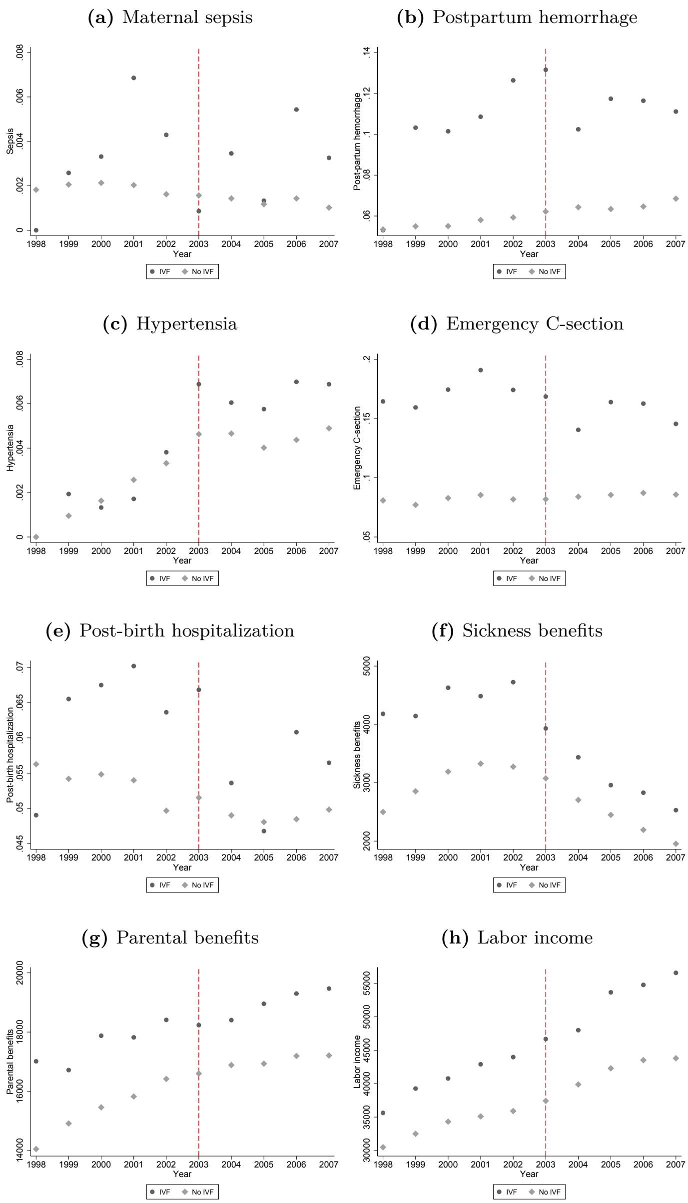


(f) Hospitalization ages 1-4



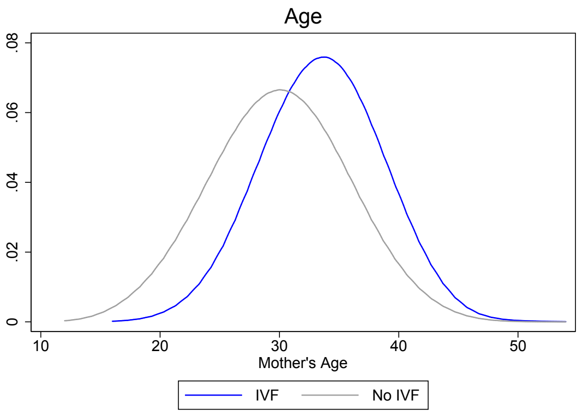
Annual trends in child health outcomes with and without IVF conception, using data obtained from the Swedish Medical Birth Registry and Patient Registry, are presented in Figures 5a to 5f. The red vertical line indicates the year of the SET reform.

Figure 6: Maternal health and labor outcomes



Annual trends in maternal health outcomes with and without IVF conception, using data obtained from the Swedish Medical Birth Registry and Patient Registry, and Longitudinal integration database for health insurance and labor market studies (LISA) are presented in Figures 6a to 6h. The red vertical line indicates the year of the SET reform.

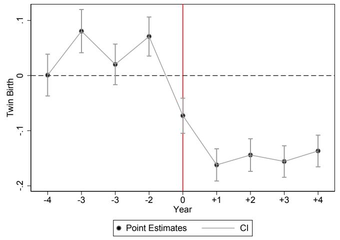
Figure 7: Age distribution among IVF and non IVF-mothers



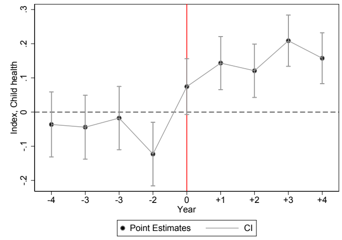
The data are obtained from the Swedish Medical Birth Registry for the time period 1998-2007. Figure 7 displays the age distribution among IVF and non-IVF mothers.

Figure 8: Event studies: main results

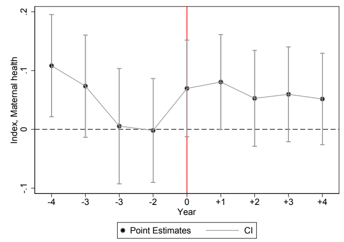
(a) Twin birth, full sample



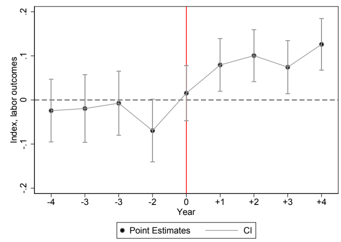
(b) Child health index, full sample



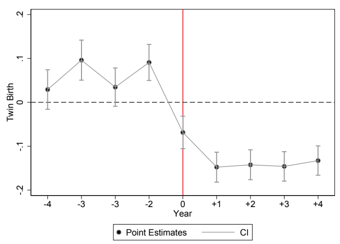
(c) Maternal health index, full sample



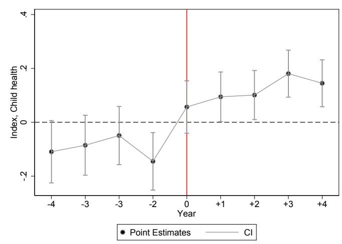
(d) Labor market index, full sample



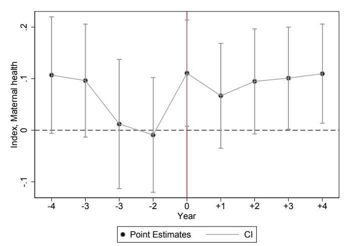
(e) Twin birth, first-time mothers



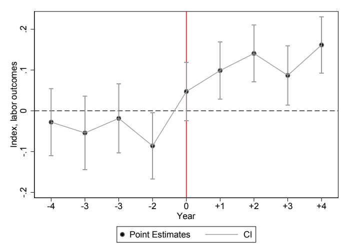
(f) Child health index, first-time mothers



(g) Maternal health index, first-time mothers



(h) Labor market index, first-time mothers



The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each figure presents coefficients of interactions between each year and IVF births. The red-vertical line represents the year of the SET reform using the previous year as the omitted category. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother.

Table 1: Summary statistics

	IVF			*Non-IVF*			T-test	
	Mean	SD	N	Mean	SD	N	T-test	P-values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Maternal outcomes and characteristics								
Twin birth	0.199	0.400	21783	0.025	0.155	916110	-154.479	0.000
Emergency C-section	0.162	0.368	21782	0.084	0.277	916102	-40.785	0.000
Maternal sepsis	0.003	0.056	21783	0.002	0.039	916110	-6.041	0.000
Postpartum hemorrhage	0.115	0.319	21783	0.062	0.242	916110	-31.613	0.000
Post-birth hospitalization	0.060	0.238	21197	0.051	0.220	867145	-6.195	0.000
Age	33.554	4.206	21783	30.005	5.062	916108	-102.639	0.000
Height	167.347	6.300	21364	166.393	6.350	852598	-21.680	0.000
Weight	68.890	12.163	20375	67.872	13.001	814764	-11.055	0.000
BMI	24.595	4.119	20126	24.509	4.430	800040	-2.727	0.006
Asthma	0.068	0.252	21783	0.068	0.252	916110	-0.044	0.965
Ulcerative colitis	0.011	0.102	21783	0.006	0.076	916110	-9.164	0.000
Epilepsy	0.004	0.063	21783	0.004	0.067	916110	1.024	0.306
Hypertension	0.005	0.070	21783	0.004	0.060	916110	-3.273	0.001
Smoke 1st trimester	0.042	0.201	21408	0.096	0.295	858730	26.692	0.000
Smoke 3rd trimester	0.024	0.153	19334	0.066	0.248	758490	23.439	0.000
Education	4.697	1.319	21741	4.456	1.402	910516	-25.111	0.000
Labor income	65.409	41.438	21755	42.538	33.165	906747	-99.866	0.000
Sickness benefits	1.969	5.376	21755	1.401	4.063	906747	-20.216	0.000
Parental benefits	2.143	4.504	21755	4.515	5.919	906747	58.685	0.000
Maternal health index	-0.249	1.260	21783	0.002	0.997	916110	36.555	0.000
Maternal labor index	0.179	1.096	21765	0.001	0.999	915012	-26.017	0.000
Panel B: Child outcomes and characteristics								
Apgar score	9.630	0.973	21513	9.731	0.810	907738	17.986	0.000
Apgar score ≤ 7	0.018	0.133	21513	0.012	0.108	907738	-8.355	0.000
Birth weight	3,291.359	712.527	21689	3,533.785	586.532	913011	59.831	0.000
Gestational age (weeks)	38.616	2.656	21766	39.313	1.955	915126	51.515	0.000
Head circumference	34.519	1.982	20412	34.928	1.682	872547	34.133	0.000
Length (centimeters)	49.601	3.212	21162	50.378	2.572	903283	43.145	0.000
Gender (male)	0.515	0.500	21765	0.514	0.500	915104	-0.028	0.978
Breech presentation	0.093	0.291	21766	0.038	0.191	915126	-41.754	0.000
Malformation	0.045	0.207	21766	0.035	0.185	915126	-7.311	0.000
Infant mortality rate	4.545	67.264	21783	2.713	52.012	916110	-5.099	0.000
Under 5 mortality rate	5.601	74.630	21783	3.433	58.491	916110	-5.367	0.000
Hospitalization ages 0-1	0.223	0.416	21766	0.164	0.370	915126	-23.131	0.000
Hospitalization ages 1-4	0.147	0.354	21766	0.131	0.338	915126	-6.553	0.000
Child health index	-0.276	1.242	21783	0.004	0.995	916110	40.737	0.000

Note to Table 1. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies. The sample includes IVF and non-IVF births for the time period 1998-2007. Mean values, standard deviations, observations, t-test with related p-values are displayed.

Table 2: Summary statistics, twins and singletons

	Twins				*Singletons*			
	IVF	*Non-IVF*	*Difference*		*IVF*	*Non-IVF*	*Difference*	
	Mean (1)	Mean (2)	T-test (3)	P-values (4)	Mean (5)	Mean (6)	T-test (7)	P-values (8)
Apgar score	9.429	9.476	1.796	0.072	9.651	9.733	8.157	0.000
Apgar score below 7	0.033	0.033	-0.115	0.909	0.015	0.011	-3.317	0.001
Stillbirth	0.006	0.006	-0.020	0.984	0.003	0.003	-0.356	0.722
Birth weight	2,563.517	2,597.222	2.450	0.014	3,470.749	3,571.744	13.813	0.000
Gestational age (weeks)	36.171	36.239	1.034	0.301	39.237	39.418	7.609	0.000
Head circumference	33.158	33.146	-0.243	0.808	34.814	34.947	6.194	0.000
Length (centimeters)	46.716	46.776	0.773	0.440	50.301	50.506	6.470	0.000
Gender (male)	0.499	0.505	0.522	0.602	0.518	0.515	-0.438	0.661
Breech presentation	0.266	0.268	0.269	0.788	0.050	0.031	-8.350	0.000
Malformation	0.049	0.042	-1.735	0.083	0.043	0.034	-3.543	0.000
Infant mortality rate	13.016	14.852	0.712	0.476	3.873	2.667	-1.821	0.069
Under 5 mortality rate	14.132	16.450	0.857	0.392	5.487	3.484	-2.645	0.008
Hospitalization ages 0-1	0.393	0.367	-2.493	0.013	0.194	0.162	-6.849	0.000
Hospitalization ages 1-4	0.166	0.154	-1.528	0.126	0.149	0.136	-2.996	0.003
Child index	-1.066	-1.021	1.298	0.194	-0.113	0.023	11.000	0.000
N	2,689	10,638			6,197	417,656		

Note to Table 2. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies. The sample includes IVF and non-IVF births, for the pre-treatment period 1998-2002. Mean values, t-tests and p-values are presented for singleton and twin births.

Table 3: Probability of twinning

	Full sample		*First-time mothers*	
	(1) *Twin birth*	(2) *Twin birth*	(3) *Twin birth*	(4) *Twin birth*
postSET*IVF	-0.173***	-0.168***	-0.177***	-0.177***
	(0.007)	(0.007)	(0.009)	(0.009)
IVF	0.276***	0.268***	0.274***	0.268***
	(0.006)	(0.006)	(0.007)	(0.007)
Mother weight		0.000***		0.000***
		(0.000)		(0.000)
Mother height		0.000***		0.000***
		(0.000)		(0.000)
Smoking 1st trimester		0.003***		-0.001
		(0.001)		(0.001)
Native		-0.000		0.004***
		(0.001)		(0.001)
Labor income		0.000		0.000*
		(0.000)		(0.000)
Sickness benefits		0.000		0.000
		(0.000)		(0.000)
Fixed effects	NO	YES	No	YES
Controls	NO	YES	No	YES
R-Squared	0.038	0.062	0.064	0.067
Observations	937893	937893	414182	414182
Mean of dep. var.	0.029	0.029	0.029	0.029
Control mean	0.027	0.027	0.027	0.027
Control sd	0.163	0.163	0.162	0.162

Note to Table 3. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on the probability of twin birth for the full sample (columns 1-2) and first-time mothers (columns 3-4). In columns 1 and 3, controls are excluded. In columns 2 and 4, a full set of maternal and child controls and fixed effects are included (age, birth order, country fixed effects along with maternal characteristics such as weight, height, smoking, and binary variables for missing values). Estimated date of conception fixed effects are included in all regressions. Standard errors are clustered on the mother. * p<0.1, ** p<0.05, *** p<0.01.

Table 4: Probability of twinning, heterogeneous effects

Dependent variable: twin birth

Panel A Sub-Sample:	Birth order			Age groups				
	(1) Birth order 1	(2) Birth order 2	(3) Birth order ≥ 3	(4) Ages < 25	(5) Ages 25-29	(6) Ages 30-34	(7) Ages 35-39	(8) Ages > 39
IVF × postSET	-0.178***	-0.160***	-0.0950***	-0.108**	-0.223***	-0.200***	-0.128***	-0.0235
	(0.0087)	(0.0153)	(0.0329)	(0.0456)	(0.0182)	(0.0113)	(0.0125)	(0.0231)
IVF	0.268***	0.276***	0.212***	0.174***	0.308***	0.288***	0.235***	0.140***
	(0.0074)	(0.0129)	(0.0264)	(0.0361)	(0.0152)	(0.0096)	(0.0105)	(0.0192)
Observations	414182	341724	181347	132592	290706	331919	153399	29275
R^2	0.066	0.045	0.047	0.085	0.067	0.079	0.104	0.254
Mean of dep. var.	0.0290	0.0278	0.0264	0.0162	0.0242	0.0316	0.0401	0.0373

Panel B Sub-Sample:	Education			BMI classification			
	(1) Primary education	(2) Secondary education	(3) Tertiary education	(4) Under weight BMI < 18.5	(5) Normal weight BMI 18.5-24	(6) Overweight BMI 25-29	(7) Obesity BMI ≥ 30
IVF × postSET	-0.146***	-0.162***	-0.183***	-0.0940**	-0.179***	-0.151***	-0.151***
	(0.0154)	(0.0116)	(0.0123)	(0.0479)	(0.0098)	(0.0148)	(0.0224)
IVF	0.266***	0.257***	0.275***	0.207***	0.270***	0.270***	0.236***
	(0.0118)	(0.0098)	(0.0108)	(0.0382)	(0.0084)	(0.0123)	(0.0195)
Observations	236541	383284	312432	19433	498958	211379	90396
R^2	0.089	0.067	0.078	0.344	0.069	0.088	0.123
Mean of dep. var.	0.0291	0.0280	0.0294	0.0226	0.0272	0.0301	0.0300

Note to Table 4. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on the probability twin birth for different sub-samples of birth order, age groups, education attainments and BMI classifications. All regressions including county, estimated date of conception, birth order and age fixed effects and binary variables for missing values. Standard errors are clustered on the mother. * p<0.1, ** p<0.05, *** p<0.01.

Table 5: Effects of SET on child and maternal outcomes

	Full sample			*First-time mothers*		
	(1) Child health index	(2) Maternal health index	(3) Maternal labor index	(4) Child health index	(5) Maternal health index	(6) Maternal labor index
postSET*IVF	0.189***	0.032	0.106***	0.184***	0.056**	0.156***
	(0.019)	(0.020)	(0.016)	(0.022)	(0.023)	(0.019)
IVF	-0.355***	-0.165***	-0.125***	-0.319***	-0.156***	-0.163***
	(0.016)	(0.015)	(0.013)	(0.018)	(0.018)	(0.016)
R-Squared	0.019	0.023	0.196	0.021	0.027	0.198
Observations	937893	937893	936777	414180	414180	413652
Mean of dep. var.	-0.003	-0.003	0.005	-0.003	-0.004	0.006
Control mean	0.000	-0.000	-0.000	0.000	0.000	-0.000
Control sd	1.000	1.000	1.000	1.000	1.000	1.000

Note to Table 5. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on child health index (columns 1 and 4), maternal health index (columns 2 and 5), maternal labor market index (columns 3 and 6). Panel A presents estimates for the full sample and Panel B a sub-sample of first-time mothers. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 6: Effects of SET on child and maternal outcomes, 2SLS estimates

	Full sample				*First-time mothers*			
	First stage	*Second stage*			*First stage*	*Second stage*		
	(1) Singleton	(2) Child health index	(3) Maternal health index	(4) Maternal labor index	(5) Singleton	(6) Child health index	(7) Maternal health index	(8) Maternal labor index
postSET*IVF	0.168***				0.177***			
	(0.007)				(0.009)			
Singleton		1.122***	0.186	0.630***		1.022***	0.308**	0.868***
		(0.106)	(0.115)	(0.095)		(0.115)	(0.126)	(0.113)
IVF	-0.268***	-0.055***	-0.120***	0.041**	-0.268***	-0.040**	-0.076***	0.071***
	(0.006)	(0.019)	(0.022)	(0.017)	(0.007)	(0.020)	(0.023)	(0.020)
F-stat	515.2				413.4			
Observations	937893	937893	937893	936777	414182	414182	414182	413654
Mean of dep. var.	0.971	0.000	-0.000	-0.000	0.971	-0.000	0.000	0.000
Control mean	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Control sd	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000

Note to Table 6. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Column 1 presents an OLS regression of the probability of having a singleton birth after SET for the full sample and column 5 the probability for first-time mothers. Columns 2-4 present estimates for the full sample and columns 6-8 a sub-sample of first-time mothers. Each column presents a separate 2SLS regression of the impact of having a singleton birth due to SET on child health (columns 2 and 6), maternal health (columns 3 and 7) and maternal labor market outcomes (columns 4 and 8). A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 7: Effects of SET on child health

Panel A: *Full sample*

	(1) Apgar score	(2) Apgar score <7	(3) Birth weight	(4) Birth lenght	(5) Head circumfernce	(6) Gestation	(7) Male	(8) Infant mortality rate	(9) Under five mortality	(10) Breech position	(11) Child hospitalization ages 0-1	(12) Child hospitalization ages 1-4	(13) Fetal malformation
postSET*IVF	0.061***	-0.005**	175.119***	0.605***	0.235***	0.539***	0.005	-2.993***	-3.492***	-0.036***	-0.045***	-0.002	-0.001
	(0.014)	(0.002)	(11.343)	(0.052)	(0.032)	(0.045)	(0.007)	(1.062)	(1.163)	(0.004)	(0.006)	(0.005)	(0.003)
IVF	-0.082***	0.006***	-307.348***	-1.121***	-0.547***	-1.037***	-0.003	3.748***	4.457***	0.063***	0.088***	0.024***	0.007***
	(0.011)	(0.002)	(9.283)	(0.042)	(0.026)	(0.037)	(0.005)	(0.932)	(1.017)	(0.004)	(0.005)	(0.004)	(0.002)
FDR p-value (Treat)	0.000	0.067	0.000	0.000	0.000	0.000	0.304	0.013	0.012	0.000	0.000	0.753	0.517
Bonferroni p-value (Treat)	0.000	1.000	0.000	0.000	0.000	0.000	1.000	0.211	0.182	0.000	0.000	1.000	1.000
R-Squared	0.021	0.008	0.093	0.075	0.057	0.024	0.004	0.006	0.005	0.012	0.019	0.009	0.005
Observations	930302	930302	935714	925477	894087	937893	937870	937893	937893	937893	937893	937893	937893
Mean of dep. var.	9.729	0.012	3528.062	50.360	34.918	39.297	0.514	2.755	3.483	0.039	0.165	0.132	0.036
Control mean	9.730	0.012	3530.472	50.367	34.922	39.303	0.514	2.750	3.478	0.039	0.165	0.131	0.036
Control sd	0.813	0.108	589.322	2.584	1.687	1.968	0.500	52.371	58.871	0.193	0.371	0.338	0.185

Panel B: *First-time mothers*

	(1) Apgar score	(2) Apgar score <7	(3) Birth weight	(4) Birth lenght	(5) Head circumfernce	(6) Gestation	(7) Male	(8) Infant mortality rate	(9) Under five mortality	(10) Breech position	(11) Child hospitalization ages 0-1	(12) Child hospitalization ages 1-4	(13) Fetal malformation
postSET*IVF	0.074***	-0.005**	185.631***	0.646***	0.242***	0.591***	0.010	-3.127***	-3.541***	-0.031***	-0.049***	0.002	-0.003
	(0.017)	(0.002)	(13.625)	(0.063)	(0.039)	(0.055)	(0.009)	(1.181)	(1.313)	(0.005)	(0.008)	(0.006)	(0.004)
IVF	-0.074***	0.004**	-293.378***	-1.120***	-0.525***	-1.074***	-0.008	3.261***	3.989***	0.057***	0.087***	0.018***	0.008***
	(0.014)	(0.002)	(11.176)	(0.052)	(0.032)	(0.046)	(0.007)	(1.050)	(1.155)	(0.004)	(0.006)	(0.005)	(0.003)
FDR p-value (Treat)	0.000	0.067	0.000	0.000	0.000	0.000	0.304	0.013	0.012	0.000	0.000	0.753	0.517
Bonferroni p-value (Treat)	0.000	1.000	0.000	0.000	0.000	0.000	1.000	0.211	0.182	0.000	0.000	1.000	1.000
R-Squared	0.023	0.012	0.066	0.063	0.045	0.027	0.009	0.010	0.010	0.014	0.029	0.014	0.010
Observations	411406	411406	413106	407269	393470	414180	414167	414180	414180	414180	414180	414180	414180
Mean of dep. var.	9.671	0.015	3431.773	50.133	34.739	39.299	0.515	2.764	3.530	0.052	0.168	0.132	0.038
Control mean	9.671	0.015	3434.312	50.143	34.744	39.309	0.515	2.767	3.529	0.051	0.168	0.132	0.038
Control sd	0.898	0.123	588.440	2.687	1.774	2.127	0.500	52.527	59.304	0.220	0.373	0.339	0.192

Note to Table 7. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on child health outcomes. Panel A presents the results for the full sample and Panel B for a sub-sample of first-time mothers. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Both FDR and Bonferroni corrected p-values are reported in addition to the conventional. Standard errors are clustered on the mother. * p<0.1, ** p<0.05, *** p<0.01.

Table 8: Effects of SET on birth weight and gestational age

	Panel A: *Full sample*				
	(1) Birth weight <1500 grams	(2) Birth weight <2500 grams	(3) Gestation <28 weeks	(4) Gestation <32 weeks	(5) Gestation <37 weeks
postSET*IVF	-0.012***	-0.068***	-0.005***	-0.013***	-0.083***
	(0.003)	(0.006)	(0.002)	(0.003)	(0.007)
IVF	0.020***	0.112***	0.008***	0.025***	0.157***
	(0.002)	(0.005)	(0.001)	(0.003)	(0.006)
FDR p-value (Treat)	0.000	0.000	0.009	0.000	0.000
Bonferroni p-value (Treat)	0.000	0.000	0.125	0.000	0.000
R-Squared	0.010	0.021	0.012	0.012	0.018
Observations	935714	935714	937893	937893	937893
Mean of dep. var.	0.008	0.042	0.003	0.009	0.113
Control mean	0.007	0.042	0.003	0.009	0.112
Control sd	0.086	0.200	0.052	0.095	0.315
	Panel B: *First-time mothers*				
	(1) Birth weight <1500 grams	(2) Birth weight <2500 grams	(3) Gestation <28 weeks	(4) Gestation <32 weeks	(5) Gestation <37 weeks
postSET*IVF	-0.015***	-0.078***	-0.006***	-0.016***	-0.091***
	(0.003)	(0.007)	(0.002)	(0.004)	(0.008)
IVF	0.023***	0.120***	0.010***	0.027***	0.156***
	(0.003)	(0.006)	(0.002)	(0.003)	(0.007)
FDR p-value (Treat)	0.000	0.000	0.009	0.000	0.000
Bonferroni p-value (Treat)	0.000	0.000	0.125	0.000	0.000
R-Squared	0.015	0.024	0.017	0.017	0.022
Observations	413106	413106	414180	414180	414180
Mean of dep. var.	0.010	0.055	0.004	0.012	0.126
Control mean	0.010	0.054	0.003	0.011	0.124
Control sd	0.097	0.226	0.059	0.106	0.330

Note to Table 8. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on child health outcomes regarding low birthweight and prematurity. Panel A presents the results for the full sample and Panel B for a sub-sample of first-time mothers. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Both FDR and Bonferroni corrected p-values are reported in addition to the conventional. Standard errors are clustered on the mother. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 9: Effects of SET on maternal health

Panel A: *Full sample*

	(1) Emergency C-section	(2) Hemorrhage	(3) Maternal sepsis	(4) Hospital re-admission	(5) Hypersention
postSET*IVF	-0.020***	-0.001	0.000	-0.004	0.001
	(0.006)	(0.005)	(0.001)	(0.004)	(0.001)
IVF	0.048***	0.039***	0.001	0.014***	-0.001
	(0.005)	(0.004)	(0.001)	(0.003)	(0.001)
FDR p-value (Treat)	0.000	0.282	0.645	0.362	0.532
Bonferroni p-value (Treat)	0.003	1.000	1.000	1.000	1.000
R-Squared	0.040	0.011	0.006	0.009	0.007
Observations	937884	937893	937893	888342	937893
Mean of dep. var.	0.085	0.063	0.002	0.051	0.004
Control mean	0.084	0.063	0.002	0.051	0.004
Control sd	0.278	0.242	0.039	0.220	0.060

Panel B: *First-time mothers*

	(1) Emergency C-section	(2) Hemorrhage	(3) Maternal sepsis	(4) Hospital re-admission	(5) Hypertension
postSET*IVF	-0.029***	-0.008	-0.001	-0.005	0.001
	(0.007)	(0.006)	(0.001)	(0.005)	(0.001)
IVF	0.048***	0.041***	0.001	0.014***	-0.001
	(0.006)	(0.005)	(0.001)	(0.004)	(0.001)
FDR p-value (Treat)	0.000	0.282	0.645	0.362	0.532
Bonferroni p-value (Treat)	0.003	1.000	1.000	1.000	1.000
R-Squared	0.041	0.017	0.011	0.014	0.012
Observations	414177	414180	414180	409619	414180
Mean of dep. var.	0.123	0.074	0.002	0.053	0.003
Control mean	0.122	0.072	0.002	0.053	0.003
Control sd	0.327	0.259	0.047	0.225	0.054

Note to Table 9. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on maternal health outcomes. Panel A presents the results for the full sample and Panel B for a sub-sample of first-time mothers. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Both FDR and Bonferroni corrected p-values are reported in addition to the conventional. Standard errors are clustered on the mother. * p<0.1, ** p<0.05, *** p<0.01.

Table 10: Effects of SET on labor market outcomes

	Panel A: *Full sample*		
	(1) Sickness benefits	(2) Labor income	(3) Parental benefits
postSET*IVF	-0.436***	0.085**	-0.034*
	(0.057)	(0.037)	(0.017)
IVF	1.269***	0.014	0.072***
	(0.043)	(0.030)	(0.013)
FDR p-value (Treat)	0.000	0.297	0.744
Bonferroni p-value (Treat)	0.000	1.000	1.000
R-Squared	0.105	0.276	0.088
Observations	936777	936777	936777
Mean of dep. var.	5.102	10.027	10.155
Control mean	5.094	10.017	10.152
Control sd	4.204	3.242	1.236

	Panel B: *First-time mothers*		
	(1) Sickness benefits	(2) Labor income	(3) Parental benefits
postSET*IVF	-0.460***	0.051	0.008
	(0.067)	(0.042)	(0.021)
IVF	1.337***	0.137***	0.050***
	(0.050)	(0.032)	(0.016)
FDR p-value (Treat)	0.000	0.297	0.744
Bonferroni p-value (Treat)	0.000	1.000	1.000
R-Squared	0.122	0.262	0.111
Observations	413654	413654	413654
Mean of dep. var.	5.127	10.170	10.191
Control mean	5.115	10.157	10.187
Control sd	4.191	2.977	1.286

Note to Table 10. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on maternal labor market outcomes. Panel A presents the results for the full sample and Panel B for a sub-sample of first-time mothers. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Both FDR and Bonferroni corrected p-values are reported in addition to the conventional. Standard errors are clustered on the mother. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 11: Maternal composition

Panel A: *Full sample*

	(1) age	(2) weight (kg)	(3) height (cm)	(4) education	(5) labor income	(6) sick benefits	(7) native	(8) smoke	(9) asthma	(10) epilepsy	(11) u. colitis
IVF*postSET	0.190***	-0.086	0.003	0.107***	0.032	-0.005	-0.000	0.008**	0.003	0.000	-0.000
	(0.063)	(0.195)	(0.099)	(0.020)	(0.033)	(0.059)	(0.005)	(0.003)	(0.004)	(0.001)	(0.002)
IVF	4.446***	1.798***	0.799***	0.059***	0.845***	0.814***	0.031***	-0.050***	-0.006*	-0.001	0.005***
	(0.050)	(0.156)	(0.079)	(0.017)	(0.026)	(0.045)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
R^2	0.193	0.025	0.010	0.058	0.060	0.122	0.042	0.021	0.009	0.004	0.005
Obs	937,891	835,139	873,962	932,257	928,502	906,708	937,393	880,138	937,893	937,893	937,893
Mean of dep. var.	30.087	67.897	166.417	4.462	10.227	2.870	0.813	0.095	0.068	0.004	0.006

Panel B: *First-time mothers*

	(1) age	(2) weight (kg)	(3) height (cm)	(4) education	(5) labor income	(6) sick benefits	(7) native	(8) smoke	(9) asthma	(10) epilepsy	(11) u. colitis
IVF*postSET	0.148*	-0.132	-0.076	0.115***	0.045	0.042	-0.009	0.007*	0.005	0.001	0.000
	(0.076)	(0.231)	(0.117)	(0.024)	(0.038)	(0.066)	(0.006)	(0.004)	(0.005)	(0.001)	(0.002)
IVF	4.789***	2.114***	0.853***	0.057***	0.868***	0.853***	0.032***	-0.054***	-0.008**	-0.001	0.004***
	(0.059)	(0.180)	(0.091)	(0.019)	(0.029)	(0.050)	(0.005)	(0.003)	(0.004)	(0.001)	(0.001)
R^2	0.083	0.020	0.012	0.028	0.023	0.017	0.024	0.020	0.014	0.009	0.010
Obs	414,181	368,213	384,525	412,106	407,728	392,747	413,965	387,840	414,182	414,182	414,182
Mean of dep. var.	28.253	66.777	166.623	4.616	10.469	1.401	0.834	0.088	0.075	0.004	0.006

Note to Table 11. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Panel A presents the results for the full sample and Panel B for a sub-sample of first-time mothers. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on maternal age (column 1), weight (column 2), height (column 3), education (column 4), labor income prior birth (column 5), sickness benefits prior birth (column 6), nationality (Swedish) (column 7), smoking (column 8), asthma (column 9), epilepsy (column 10) and ulcerative colitis (column 11). Birth number, county and conception date fixed effects are included in each regression. Standard errors are clustered on the mother. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 12: Effects of SET on child and maternal outcomes, controlling for trends in IVF-specific maternal characteristic

	Full sample			*First-time mothers*		
	(1) Child health index	(2) Maternal health index	(3) Maternal labor index	(4) Child health index	(5) Maternal health index	(6) Maternal labor index
postSET*IVF	0.149***	0.112***	0.059**	0.122***	0.120***	0.105***
	(0.038)	(0.039)	(0.028)	(0.044)	(0.046)	(0.035)
IVF	-1.066***	0.139	-1.133***	-0.927***	0.536*	-0.627**
	(0.270)	(0.282)	(0.228)	(0.309)	(0.320)	(0.270)
R-Squared	0.021	0.027	0.263	0.024	0.033	0.266
Observations	777363	777363	776837	337118	337118	336913
Mean of dep. var.	0.004	-0.001	0.031	0.001	-0.004	0.046
Control mean	0.000	-0.000	-0.000	0.000	-0.000	0.000
Control sd	1.000	1.000	1.000	1.000	1.000	1.000

Note to Table 12. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Columns 1-3 present estimates for the full sample and columns 4-6 a sub-sample of first-time mothers. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on child health index (columns 1 and 4), maternal health index (columns 2 and 5), maternal labor market index (columns 3 and 6). All regressions include IVF-specific maternal characteristic time trends (for education, labor and sickness benefits, nationality, previous health conditions and behavior and age), a full set of fixed effects (conception date, birth order and county) and binary variables for missing values. Standard errors are clustered on the mother. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 13: Sample of mothers with more than one pregnancy: mother fixed effects

	Panel A: *Mother fixed effects excluded*		
	(1) Child health index	(2) Maternal health index	(3) Maternal labor index
postSET*IVF	0.150***	-0.010	0.118***
	(0.026)	(0.025)	(0.020)
IVF	-0.336***	-0.121***	-0.107***
	(0.020)	(0.017)	(0.016)
R-Squared	0.014	0.015	0.206
Observations	735771	735771	735165
Mean dep. var.	-0.001	-0.002	0.003
Control mean	0.000	0.000	0.000
Control sd	1.000	1.000	1.000

	Panel B: *Mother fixed effects included*		
	(1) Child health index	(2) Maternal health index	(3) Maternal labor index
postSET*IVF	0.118**	-0.033	0.064**
	(0.059)	(0.057)	(0.028)
IVF	-0.149***	-0.016	-0.073***
	(0.049)	(0.044)	(0.023)
R-Squared	0.608	0.667	0.896
Observations	735771	735771	735165
Mean dep. var.	-0.001	-0.002	0.003
Control mean	0.000	0.000	0.000
Control sd	1.000	1.000	1.000

Note to Table 13. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007 for a selected sample of mothers with more than one pregnancy. Columns 1-3 present estimates for the full sample and columns 4-6 a sub-sample of first-time mothers. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on child health index (column 1), maternal health index (column 2), maternal labor market index (column 3). Panel A presents estimates excluding mother fixed effects and Panel B including mother fixed effects. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother. * p<0.1, ** p<0.05, *** p<0.01.

Table 14: Effects of SET on child and maternal outcomes, excluding controls

	Full sample			*First-time mothers*		
	(1) Child health index	(2) Maternal health index	(3) Maternal labor index	(4) Child health index	(5) Maternal health index	(6) Maternal labor index
postSET*IVF	0.202***	0.033*	0.118***	0.189***	0.051**	0.157***
	(0.019)	(0.020)	(0.017)	(0.022)	(0.023)	(0.020)
IVF	-0.398***	-0.268***	0.093***	-0.338***	-0.239***	0.021
	(0.016)	(0.015)	(0.013)	(0.018)	(0.018)	(0.016)
R-Squared	0.006	0.006	0.032	0.012	0.011	0.029
Observations	937893	937893	936777	414180	414180	413652
Mean of dep. var.	-0.003	-0.003	0.005	-0.003	-0.004	0.006
Control mean	0.000	-0.000	-0.000	0.000	0.000	-0.000
Control sd	1.000	1.000	1.000	1.000	1.000	1.000
Altonji et al 2005 Ratio	14.5	32.0	8.8	36.8	-11.2	156.0

Note to Table 14. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Columns 1-3 present estimates for the full sample and columns 4-6 a sub-sample of first-time mothers. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on child health index (columns 1 and 4), maternal health index (columns 2 and 5), maternal labor market index (columns 3 and 6). Maternal and child controls are excluded. Standard errors are clustered on the mother. * p<0.1, ** p<0.05, *** p<0.01.

Table 15: Gelbach decomposition

	(1) Child health index	(2) Maternal health index	(3) Maternal labor index	(4)
postSET*IVF	$\Gamma^{childhealth} = 0.189$***	$\Gamma^{maternalhealth} = 0.032$	0.099***	
			(0.016)	
$\beta_{labor}^{childhealth} = childhealthindex$			0.029***	
			(0.001)	
$\beta_{labor}^{maternalhealth} = maternalhealthindex$			0.050***	
			(0.001)	
$\Gamma_{labor}^{childhealth} \times \beta_{labor}^{childhealth}$				$\delta_{labor}^{childhealth} = 0.005$***
				(0.001)
$\Gamma_{labor}^{maternalhealth} \times \beta_{labor}^{maternalhealth}$				$\delta_{labor}^{maternalhealth} = 0.002$
				(0.001)
Total explained difference				0.007***
				(0.001)

Note to Table 15. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother. * p<0.1, ** p<0.05, *** p<0.01. Γ represents each estimate of the SET reform (postSET×IVF) for each potential mechanism as the outcome variable. β indicates the estimate of the potential mechanisms as explanatory variables in the full specification with maternal labor as the outcome variable. The conditional contribution of each component is given by δ, which is computed by multiplying Γ with β.

Table 16: Robustness: additional sensitivity

	Panel A: *Removing 2001-2002*			
	(1)	(2)	(3)	(4)
	Twin birth	Child health index	Maternal health index	Maternal labor index
postSET*IVF	-0.168***	0.177***	0.002	0.100***
	(0.010)	(0.024)	(0.024)	(0.020)
IVF	0.267***	-0.342***	-0.135***	-0.121***
	(0.009)	(0.021)	(0.021)	(0.018)
R-Squared	0.055	0.019	0.024	0.197
Observations	754464	754464	754464	753583
Mean of dep. var.	0.028	-0.003	-0.004	0.005
Control mean	-0.000	-0.000	-0.000	-0.000
Control sd	1.000	1.000	1.000	1.000

	Panel B: *Removing region of Skåne*			
	(1)	(2)	(3)	(4)
	Twin birth	Child health index	Maternal health index	Maternal labor index
postSET*IVF	-0.165***	0.188***	0.027	0.107***
	(0.008)	(0.020)	(0.020)	(0.016)
IVF	0.265***	-0.348***	-0.158***	-0.122***
	(0.007)	(0.016)	(0.016)	(0.013)
R-Squared	0.063	0.019	0.024	0.196
Observations	854191	854191	854191	853191
Mean of dep. var.	0.029	-0.003	-0.003	0.005
Control mean	-0.000	-0.000	-0.000	-0.000
Control sd	1.000	1.000	1.000	1.000

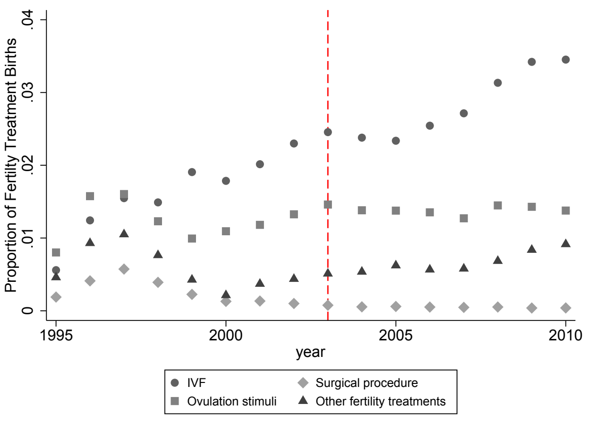
	Panel C: *Removing 2005-2007*			
	(1)	(2)	(3)	(4)
	Twin birth	Child health index	Maternal health index	Maternal labor index
postSET*IVF	-0.150***	0.154***	0.044*	0.067***
	(0.009)	(0.024)	(0.025)	(0.019)
IVF	0.267***	-0.357***	-0.170***	-0.114***
	(0.006)	(0.016)	(0.015)	(0.013)
R-Squared	0.076	0.021	0.023	0.186
Observations	631952	631952	631952	631184
Mean of dep. var.	0.029	-0.002	-0.002	0.002
Control mean	0.000	0.000	0.000	0.000
Control sd	1.000	1.000	1.000	1.000

Note to Table 16. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on the probability of twin birth (column 1), child health index (column 2), maternal health index (column 3), and maternal labor market index (column 4). In Panel A, the time period 2001-2002 is omitted. In Panel B, the region of Skåne is omitted and in Panel C, the time period 2005-2007 is omitted. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother. * p<0.1, ** p<0.05, *** p<0.01.

Appendices

A1 Figures and tables

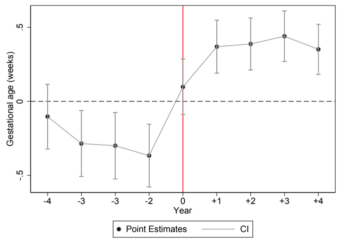
Figure A1: ART treatments



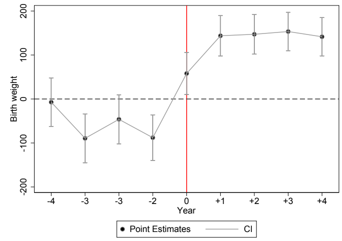
The data are obtained from the Swedish Medical Birth Registry. Trends in different ART treatments are presented in Figure A1. The red-vertical line represents the year of the SET reform.

Figure A2: Child health outcomes

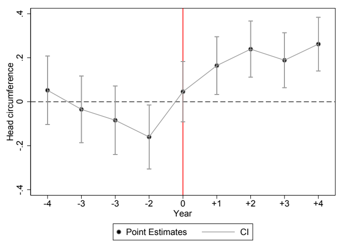
(a) Gestational age



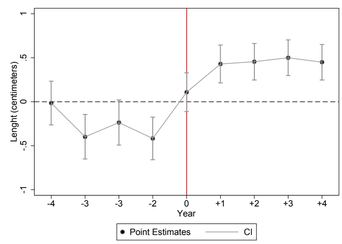
(b) Birth weight



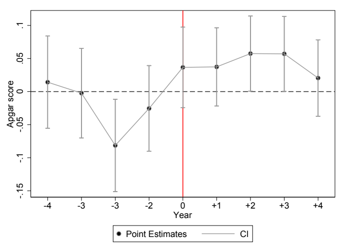
(c) Head circumference



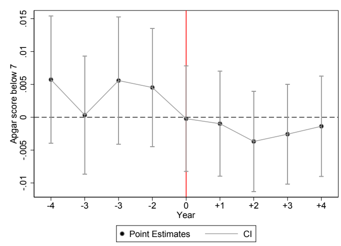
(d) Length



(e) Apgar score



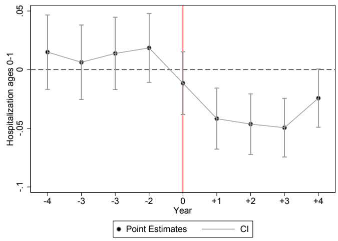
(f) Apgar score below 7



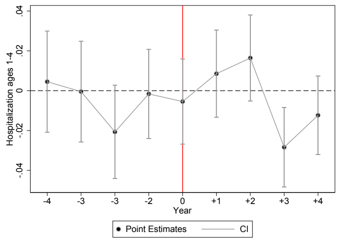
The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each figure presents coefficients of interactions between each year and IVF births. The red-vertical line represents the year of the SET reform using the previous year as the omitted category. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother.

Figure A3: Child health outcomes

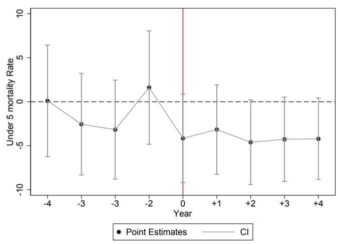
(a) Hospitalization ages 0-1



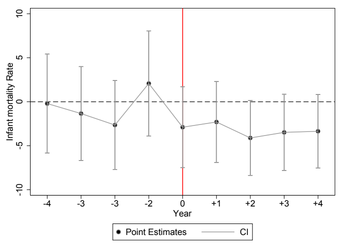
(b) Hospitalization ages 1-4



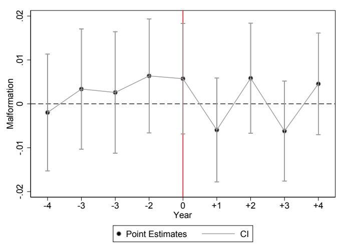
(c) Under 5 mortality rate



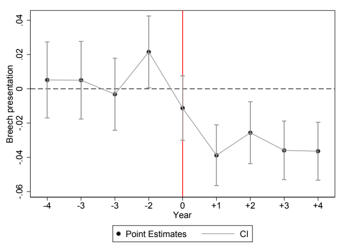
(d) Infant mortality rate



(e) Malformation

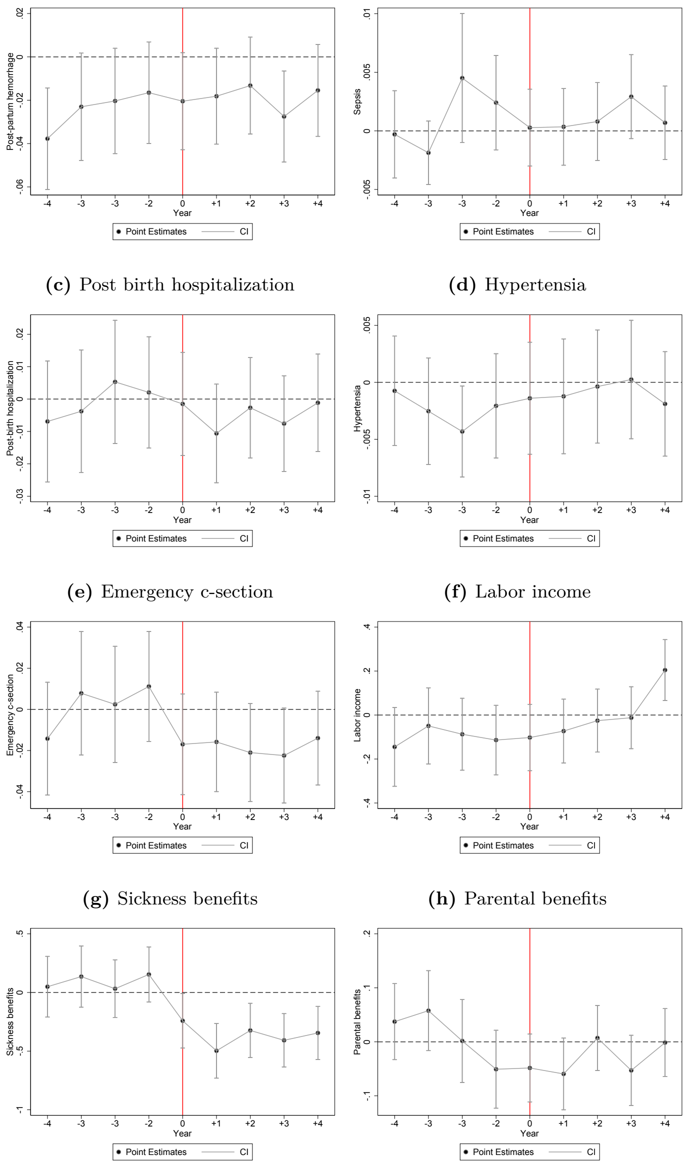


(f) Breech presentation



The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each figure presents coefficients of interactions between each year and IVF births. The red-vertical line represents the year of the SET reform using the previous year as the omitted category. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother.

Figure A4: Maternal health and labor outcomes



(a) Post-partum hemorrhage **(b)** Maternal sepsis **(c)** Post birth hospitalization **(d)** Hypertensia **(e)** Emergency c-section **(f)** Labor income **(g)** Sickness benefits **(h)** Parental benefits

The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each figure presents coefficients of interactions between each year and IVF births. The red-vertical line represents the year of the SET reform using the previous year as the omitted category. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother.

Table A1: Impact on proportion of IVF births, deliveries per transfer and number of IVF treatments

	(1) Proportion of IVF births	(2) Delivery rate	(3) Started IVF cycles
postSET	-0.002*	0.000	120.733
	(0.001)	(0.009)	(311.976)
Trend	0.001***	-0.000	813.327***
	(0.000)	(0.001)	(60.911)
R^2	0.847	0.056	0.992
Obs	11	11	11
Mean of dep. var.	0.029	0.244	11975.636

Note to Table A1. Aggregated data on proportion of IVF births, deliveries per transfer and number of IVF treatments are collected from annual reports by the Swedish National Board of Health and Welfare for the time period 1998-2008. Each column presents a separate OLS regression with the impact of the SET reform on the proportion of IVF births (column 1), deliveries per transfer (column 2), and number of treatments (column 3). All regressions include a linear time trend. Robust standard errors in parenthesis. * p<0.1, ** p<0.05, *** p<0.01.

Table A2: Summary statistics, before-after comparison

	(1) Before SET reform (Jan 1998 - Dec 2002)	(2) After SET reform (Jan 2003 - Dec 2007)	(3) T-test
Twin birth	0.303	0.128	32.426
	(0.459)	(0.334)	[0.000]
Emergency C-section	0.173	0.154	3.641
	(0.378)	(0.361)	[0.000]
Maternal sepsis	0.003	0.003	0.700
	(0.059)	(0.054)	[0.484]
Postpartum hemorrhage	0.111	0.118	-1.550
	(0.314)	(0.322)	[0.121]
Post-birth hospitalization	0.065	0.057	2.349
	(0.247)	(0.232)	[0.019]
Hypertension	0.003	0.006	-3.086
	(0.056)	(0.078)	[0.002]
Labor income	42.432	53.064	-18.401
	(36.319)	(45.336)	[0.000]
Sickness income	4.390	2.919	18.153
	(6.857)	(5.092)	[0.000]
Parental income	17.858	19.448	-13.297
	(8.023)	(9.096)	[0.000]
Apgar score	9.585	9.660	-5.588
	(1.019)	(0.939)	[0.000]
Apgar below 7	0.021	0.016	2.446
	(0.142)	(0.126)	[0.014]
Birth weight	3197.993	3355.470	-16.090
	(754.170)	(675.004)	[0.000]
Gestational age (weeks)	38.309	38.827	-14.225
	(2.890)	(2.459)	[0.000]
Head circumference	34.348	34.631	-9.961
	(2.036)	(1.938)	[0.000]
Length (centimeters)	49.269	49.824	-12.365
	(3.376)	(3.077)	[0.000]
Gender (male)	0.512	0.516	-0.622
	(0.500)	(0.500)	[0.534]
Breech presentation	0.115	0.078	9.270
	(0.319)	(0.269)	[0.000]
Malformation	0.045	0.045	-0.009
	(0.207)	(0.207)	[0.992]
Infant mortality rate	6.640	3.101	3.817
	(81.218)	(55.607)	[0.000]
Under 5 mortality rate	8.103	3.877	4.109
	(89.654)	(62.146)	[0.000]
Hospitalization ages 0-1	0.254	0.201	9.219
	(0.435)	(0.401)	[0.000]
Hospitalization ages 1-4	0.154	0.141	2.637
	(0.361)	(0.348)	[0.008]
Maternal health index	-0.257	-0.244	-0.715
	(1.229)	(1.282)	[0.474]
Maternal labor index	-0.401	-0.190	-12.399
	(1.351)	(1.154)	[0.000]
Child health index	-0.057	0.342	-26.882
	(1.090)	(1.070)	[0.000]
Observations	8,886	12,897	

Note to Table A2. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies. The sample includes IVF births, for the time period 1998-2007. Mean values with standard deviations below, t-tests and p-values are presented for the pre-reform period January 1998-December 2002 (column 1) and post-reform period January 2003-December 2007 (column 2).

Table A3: Probability of twinning per birth, including trends

	Full sample		*First-time mothers*	
	(1) *Twin birth*	(2) *Twin birth*	(3) *Twin birth*	(4) *Twin birth*
postSET*IVF	-0.132***	-0.129***	-0.131***	-0.129***
	(0.015)	(0.014)	(0.017)	(0.017)
IVF	0.263***	0.249***	0.252***	0.246***
	(0.012)	(0.008)	(0.014)	(0.010)
Mother weight	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Mother height	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Smoking 1st trimester	0.003***	0.003***	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Native	-0.000	-0.000	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Labor income	0.000	0.000	0.000*	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Sickness benefits	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
IVF specific split time trends	Yes	NO	YES	NO
IVF specific global time trends	NO	YES	No	YES
R-Squared	0.062	0.062	0.068	0.068
Observations	937893	937893	414182	414182
Mean of dep. var.	0.029	0.029	0.029	0.029
Control mean	0.027	0.027	0.027	0.027
Control sd	0.163	0.163	0.162	0.162

Note to Table A3. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on the probability of twin birth for the full sample (columns 1-2) and first-time mothers (columns 3-4). In columns 1 and 3, an IVF specific split linear time trend is included and in columns 2 and 4, an IVF specific (global) linear time trend is included. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A4: Effects of SET on child and maternal outcomes, including trends

Panel A: IVF specific split linear time trends	*Full sample*			*First-time mothers*		
	(1) Child health index	(2) Maternal health index	(3) Maternal labor index	(4) Child health index	(5) Maternal health index	(6) Maternal labor index
postSET*IVF	0.137***	0.106***	0.061**	0.097**	0.111**	0.105***
	(0.038)	(0.037)	(0.029)	(0.043)	(0.044)	(0.036)
IVF	-0.357***	-0.224***	-0.131***	-0.292***	-0.217***	-0.159***
	(0.029)	(0.028)	(0.022)	(0.033)	(0.033)	(0.028)
R-Squared	0.019	0.023	0.196	0.021	0.027	0.198
Observations	937893	937893	936777	414180	414180	413652
Mean of dep. var.	-0.003	-0.003	0.005	-0.003	-0.004	0.006
Control mean	0.000	-0.000	-0.000	0.000	0.000	-0.000
Control sd	1.000	1.000	1.000	1.000	1.000	1.000
Panel B: IVF specific linear time trends	*Full sample*			*First-time mothers*		
	(1) Child health index	(2) Maternal health index	(3) Maternal labor index	(4) Child health index	(5) Maternal health index	(6) Maternal labor index
postSET*IVF	0.130***	0.099***	0.054*	0.094**	0.102**	0.099***
	(0.037)	(0.037)	(0.028)	(0.042)	(0.044)	(0.036)
IVF	-0.327***	-0.197***	-0.100***	-0.277***	-0.178***	-0.136***
	(0.022)	(0.022)	(0.017)	(0.024)	(0.025)	(0.021)
R-Squared	0.019	0.023	0.196	0.021	0.027	0.198
Observations	937893	937893	936777	414180	414180	413652
Mean of dep. var.	-0.003	-0.003	0.005	-0.003	-0.004	0.006
Control mean	0.000	-0.000	-0.000	0.000	0.000	-0.000
Control sd	1.000	1.000	1.000	1.000	1.000	1.000

Note to Table A4. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies for the time period 1998-2007. Columns 1-3 present estimates for the full sample and columns 4-6 a sub-sample of first-time mothers. Each column presents a separate OLS regression with DiD estimates of the impact of the SET reform on child health index (columns 1 and 4), maternal health index (columns 2 and 5), maternal labor market index (columns 3 and 6). Panel A includes IVF specific split linear time trends and Panel B includes IVF specific (global) linear time trends. A full set of maternal controls and fixed effects are included in all regressions (as described in Table 3). Standard errors are clustered on the mother. * p<0.1, ** p<0.05, *** p<0.01.

Table A5: Summary statistics, IVF mothers with 1 or more than 1 birth

IVF mothers with:	*≥ 1 birth*	*Only 1 birth*	*Difference*	
	(1)	(2)	(3)	(4)
	Mean	Mean	T-test	P-values
Twin birth	0.123	0.221	-22.558	0.000
Planned C-section	0.145	0.168	-5.475	0.000
Emergency C-section	0.108	0.204	-22.884	0.000
Maternal sepsis	0.002	0.004	-3.358	0.001
Postpartum hemorrhage	0.091	0.129	-10.318	0.000
Post-birth hospitalization	0.057	0.061	-1.411	0.158
Age	33.552	33.369	3.555	0.000
Weight (kilograms)	167.335	167.358	-0.300	0.764
Height (centimeters)	68.608	68.883	-1.809	0.070
BMI	24.511	24.584	-1.411	0.158
Asthma	0.060	0.074	-4.643	0.000
Ulcerative colitis	0.010	0.011	-0.741	0.459
Epilepsy	0.004	0.005	-1.479	0.139
Hypertensia	0.005	0.005	0.167	0.868
Smoking 1st trimester	0.044	0.041	1.383	0.167
Smoking 3rd trimester	0.028	0.023	2.843	0.004
Education	4.691	4.731	-2.539	0.011
Labor income	58.220	70.097	-24.686	0.000
Sickness benefits	2.041	1.566	7.289	0.000
N births	18334	11154		
N mothers	9931	9831		

Note to Table A5. The data are obtained from the Swedish Medical Birth Registry, Swedish National Patient Registry and the Longitudinal integration database for health insurance and labor market studies. The sample includes IVF mothers for the time period 1998-2007. Mean values, t-test and p-values for the t-tests are displayed.

A2 Measurement of IVF usage

A number of methodologies exist to consider mis-reporting of treatment variables (Horowitz and Manski, 1995), or selection into treatment (Alderman et al., 2011; Lee, 2009). The case we are concerned with is relatively simple, as we are concerned only with a mis-classification of treated units to be included as part of the control group. Given our application, in general, we are likely to under-estimate the effect size by a small amount. To see why, we provide some simple algebra considering the difference between a DiD estimator where all treated units are correctly classified: $\widehat{\beta}_1$, and an estimator where some portion of treated units are mis-classified as controls $\widehat{\widetilde{\beta}}_1$. These estimators can, respectively, be written as:

$$\widehat{\beta}_1 = (\bar{Y}_{T1} - \bar{Y}_{C1}) - (\bar{Y}_{T0} - \bar{Y}_{C0}),$$

where $\bar{Y}_{T1}$ refers to average outcomes among treated following treatment, $\bar{Y}_{C1}$ refers to average outcomes among controls following treatment, and $\bar{Y}_{T0}$ and $\bar{Y}_{C0}$ are the same values prior to treatment. The biased estimator, on the other hand, is:

$$\widehat{\widetilde{\beta}}_1 = (\bar{Y}_{T1} - \bar{\tilde{Y}}_{C1}) - (\bar{Y}_{T0} - \bar{\tilde{Y}}_{C0}),$$

where now $\bar{\tilde{Y}}_{C1}$ includes a small portion of the incorrectly classified treated units, and similarly for $\bar{\tilde{Y}}_{C0}$. In particular,

$$\bar{\tilde{Y}}_{C1} = \frac{T_{C1}}{T_{C1} + T_{mc^1}} \bar{Y}_{C1} + \frac{T_{mc^1}}{T_{C1} + T_{mc^1}} \bar{Y}_{T1}.$$

Here T_{C1} refers to the total number of control units in period 1, and T^1_{mc} refers to the total number of mis-classified treated units included as controls following treatments. A similar value is defined for $\bar{\tilde{Y}}_{C0}$. It is worth noting here that $\bar{\tilde{Y}}_{C1}$ will equal the true value $\bar{Y}_{C1}$ in two circumstances: either if T^1_{mc} is zero (and there is no mis-classification), or if $\bar{Y}_{C1} = \bar{Y}_{T1}$ and so mis-classification does not matter. Now, we can calculate the bias in the diff-in-diff estimate as the difference between the true value $\widehat{\beta}_1$ and the observed value with misclassification $\widehat{\widetilde{\beta}}_1$. This is calculated as:

$$\begin{aligned}
Bias(\widehat{\beta}_1) &= \widehat{\beta}_1 - \widehat{\widetilde{\beta}}_1 = (\bar{\tilde{Y}}_{C1} - \bar{Y}_{C1}) - (\bar{\tilde{Y}}_{C0} - \bar{Y}_{C0}) \\
&= \left(\frac{T_{C1}}{T_{C1} + T^1_{mc}} \bar{Y}_{C1} + \frac{T^1_{mc}}{T_{C1} + T^1_{mc}} \bar{Y}_{T1} - \bar{Y}_{C1} \right) - \\
&\quad \left(\frac{T_{C0}}{T_{C0} + T^0_{mc}} \bar{Y}_{C0} + \frac{T^0_{mc}}{T_{C0} + T^0_{mc}} \bar{Y}_{T0} - \bar{Y}_{C0} \right) \\
&= \left(\frac{T^1_{mc}}{T_{C1} + T^1_{mc}} \bar{Y}_{T1} - \frac{T^1_{mc}}{T_{C1} + T^1_{mc}} \bar{Y}_{C1} \right) - \\
&\quad \left(\frac{T^0_{mc}}{T_{C0} + T^0_{mc}} \bar{Y}_{T0} - \frac{T^0_{mc}}{T_{C0} + T^0_{mc}} \bar{Y}_{C0} \right)
\end{aligned} \tag{4}$$

If we are further willing to assume that the misclassification of treatment units is constant over time (in our setting, that IVF births are constantly under-reported by 30%), this can be further simplified to:

$$Bias(\widehat{\beta}_1) = \frac{T_{mc}}{T_C + T_{mc}} [(\bar{Y}_{T1} - \bar{Y}_{C1}) - (\bar{Y}_{T0} - \bar{Y}_{C0})]. \tag{5}$$

This simple bias formula thus suggests that misclassification will bias the estimate by the true diff-in-diff estimate, scaled by a parameter capturing the degree of mis-classification of the control group. In our case, given that this proportion $\frac{T_{mc}}{T_C+T_{mc}}$ is small, biases in estimates will also be small. And indeed, we can provide a back-of-the-envelope calculation of this bias using the observed values in the data. Assuming that the proportion of mis-classified IVF births is constant over time, we have that $\frac{T_{mc}}{T_C+T_{mc}} = \frac{9{,}336}{916{,}110} = 0.0102$. Now, for the case of birth weight, we can approximate the bias using values from the data as:

$$\begin{aligned} Bias(\widehat{\beta}_1^{BW}) &= \frac{T_{mc}}{T_C + T_{mc}}[(\bar{Y}_{T1} - \bar{Y}_{C1}) - (\bar{Y}_{T0} - \bar{Y}_{C0})] \\ &= 0.0102 \times [(3200 - 3550) - (3400 - 3530)] = -2.244 \end{aligned} \tag{6}$$

In this case, we estimate that the bias in the estimate of SET is likely to be around 2 or 3 grams. When compared to the original estimate from table 8 of 176 grams, we see that this suggests a (relatively) quite small attenuation of estimated effects.

Chapter III

CHAPTER 3

The Impact of Abortion Legalization on Fertility and Female Empowerment: New Evidence from Mexico

with Damian Clarke (Universidad de Santiago de Chile)

Abstract

We examine the effect of a large-scale, free, elective abortion program implemented in Mexico City in 2007. This reform resulted in a sharp increase in the request and use of early term elective abortions. We document that this localized reform resulted in a legislative backlash in 18 other Mexican states which constitutionally altered penal codes to increase sanctions on abortions. We take advantage of this dual policy environment to estimate the effect of progressive and regressive abortion reform on fertility and women's empowerment. Using administrative birth data we find that progressive abortion laws reduce rates of child-bearing, particularly among young women. Additionally, the reform is found to increase women's role in household decision making—an empowerment result in line with economic theory and empirical results from a developed-country setting. We however find little evidence to suggest that the resulting regressive changes to penal codes have had an inverse result over the time period studied. In turning to mechanisms, evidence from a panel of women suggests that results are directly driven by increased access to abortion, rather than changes in sexual behavior, contraceptive use or contraceptive knowledge.

Keywords: fertility, female empowerment, abortion legalization, Mexico
JEL Codes: J13, I15, I18, O15.

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1 Introduction

Laws codifying access to abortion date from as far back as the early 20$^{\text{th}}$ century (Doan, 2007). However the issue of abortion legalization remains a highly controversial social topic, with considerable variation in the availability and legality of elective abortion world-wide. From the 1970s onwards a number of large-scale reforms have increased access to elective abortion, and these have been documented to have considerable impacts on the life courses of women, children and families (Ananat et al., 2009; Bailey, M. J., 2013; Mitrut and Wolff, 2011; Pop-Eleches, 2005; Pop-Eleches, C., 2010). However, legislation, both *de jure* and *de facto* has also lead to a tightening of access to elective abortion in a number of contexts. At least in the USA, recent work has shown that these restrictions lead to reductions in the use of abortions (Cunningham et al., 2017; Grossman et al., 2017), and corresponding increases in fertility (Fischer et al., 2017).

Despite, the political and legal complexities of abortion reform, the decisions taken by national and local governments in setting these policies have important long and short-run welfare implications. As well as impacts on total fertility and fertility timing (Ananat et al. (2007); Gruber et al. (1999); Guldi (2008); Valente (2014); among many others),[1] access to abortion has been documented to impact women's labor market outcomes (Angrist and Evans, 1996; Mølland, 2016), the composition of children as well as their living circumstances (Mitrut and Wolff, 2011; Pop-Eleches, C., 2010), and women's bargaining power (Oreffice, 2007).

In this study, we examine the effect of a sharply defined local abortion reform in Mexico City and document the effect of free access to legal and safe abortion services on fertility, sexual behavior and female empowerment. We combine the state-level variation over time resulting from this natural experiment with high quality vital statistics data on 23 million births. This reform—the so called legal interruption of pregnancy (or ILE for its name in Spanish)—was of considerable importance. During the pre-reform period of 2001-2007 a total of 62 legal abortions (available in restrictive conditions) were performed in Mexico City. Following the 2007 reform, more than 90,000 women accessed safe legal abortion between 2008 and 2012.

Abortion laws are determined at the state level in Mexico, where Mexico City (also known as the federal district of Mexico or Mexico D.F.) has its own legislative assembly. The ILE reform provided all women who reside in Mexico City with access to legal and safe abortion procedures, free of charge and for any reason, during the first trimester of pregnancy (Becker, 2013). The

[1] An overview of the impacts of abortion policies on fertility in a range of contexts can be found in (Clarke, 2017).

law was a radical change from previous legislation in Mexico City, and also compared to the rest of the states of Mexico, where abortion is still banned in all but the extreme circumstances of rape, to save the mother's life, or in cases of severe fetal malformation. Moreover, by legalizing abortion, Mexico City distinguishes itself from nearly all other countries in Latin America and the Caribbean which remain highly restrictive in their policies related to elective abortion (Fraser, 2015).[2] The passing of the ILE reform resulted in a swift backlash, with 18 states following the announcement of the ILE reform by constitutionally modifying their penal codes to increase the harshness of the treatment of suspected abortions. We construct a database recording the precise date for each of these law changes by piecing together dates from published constitutional decrees for each state, resulting in a time and state-varying measure of changes in abortion laws.

This reform thus provides a unique opportunity to examine simultaneous expansions and contractions of abortion policies. While much of the existing literature on the impact of abortion—and contraceptive policies more generally—focuses on expansions in access, there are a number of papers which focus on the contractions in policies. These include historical restrictions in Romania (Pop-Eleches, C., 2010), the impact of parental consent or notification laws targeted at adolescents in the USA (Bitler and Zavodny, 2001; Joyce and Kaestner, 1996), and recent contractions in availability of providers due to state-specific legislation in the USA (Cunningham et al., 2017; Fischer et al., 2017; Lu and Slusky, 2016). However, the ILE reform in Mexico DF and resulting spate of constitutional changes increasing the harshness of sentencing of illegal abortion provides the opportunity to examine the impact of a contemporaneous series of restrictive and permissive abortion policies in a single country and time.

This study adds to the existing literature by providing evidence on the effect of abortion legalization absent simultaneous changes in other major contraceptive laws and reforms.[3] And as described above, we take advantage of an idiosyncratic policy environment in which regressive changes in abortion laws in multiple and geographically disperse areas followed a large progressive change, allowing for the separate identification of the effects of both a loosening and tightening of abortion legislation. By combining rich administrative data with panel data following women on either side of abortion reforms we are able to test a number of exist-

[2]According to the most recent United Nations figures (United Nations, 2014), Mexico is one of only three countries in the Latin America and Caribbean region (along with Uruguay and Guyana) to be classified as the "Least restrictive" in abortion policy, implying that abortions are permitted for economic or social reasons upon request.

[3]In Mexico, the country under study, contraception has been legal and freely provided by the government since a constitutional declaration in 1974.

ing hypotheses relating to abortion reforms. We begin by testing whether—as in the existing literature—abortion reforms have immediate and important effects on fertility. Then we test the hypothesis that fertility reform, and abortion reform in particular, will increase female empowerment within the household (Chiappori and Oreffice, 2008). While this has been documented to hold historically in the USA (Oreffice, 2007), no similar evidence exists for an emerging economy, despite considerable interest in women's well-being and empowerment in literature on economic development (Baird et al., 2014; Duflo, 2012).[4] Indeed, earlier influential theoretical work of Akerlof et al. (1996) suggests that under certain circumstances, namely males being less likely to enter marital unions following abortion availability, the direction of a reform's effect on empowerment may even be negative for women.

By combining state by time variation provided by the ILE reform and the follow-on regressive law changes with rich administrative and panel data, we estimate a difference-in-differences effect of the reform on rates of fertility, and various measures of women's empowerment. We document that the progressive reform resulted in a sharp decline in fertility, particularly among young women, and an increase in measures of women's empowerment. These results are found to hold up to an event-study analysis, state-of-the-art correction for multiple hypothesis testing, and a number of placebo tests. We also document that effects and significance levels are largely unchanged when estimating using an entropy matching technique to form a more comparable quasi-control group for difference-in-difference estimates. The estimated effects on fertility are large, and in line with results documented in the developed-country literature. We estimate that the ILE reform resulted in a 3.7% reduction in fertility among all women, and a 6.9% reduction among adolescents. Moreover, we do not find evidence to suggest that the effect on fertility can be attributed to changes in other contraceptive use, nor do we find links between the abortions and contraceptive knowledge or altered sexual behavior.

Turning to empowerment, we estimate that the abortion reform made women approximately 10% more likely to report being involved in a series of important decisions within her household. No similar results were found for women older than fertile age at the date of the reform, in line with placebo tests laid out in Oreffice (2007). However, we find little evidence to suggest that the reverse was true with regressive abortion reforms. The tightening of laws to increase punitive treatment of abortion was not shown to increase rates of birth, nor decrease rates of

[4] A range of work exists showing links between fertility choices, gender preferences, and women's empowerment. For example, Becker (1999) demonstrates gender differentials between desired fertility and contraceptive use. More recent evidence from a randomized controlled trial by Ashraf et al. (2014), shows that when women are able to conceal contraceptive use from their husbands, fertility declines.

women empowerment. We suggest that this may be because regressive constitutional changes had little effect on rates of self-administered abortion, which often occur privately, without any formal medical intervention (Lara et al., 2011). And unlike other restrictions in abortion policy studied in the economic literature (Fischer et al., 2017; Joyce and Kaestner, 1996; Pop-Eleches, C., 2010), the prevailing policies prior to the legislative changes in Mexico were already restrictive.

This paper joins a number of studies on Mexico's ILE reform, spread across a range of fields including law (Johnson, 2013), public health (Becker, 2013; Contreras et al., 2011; Mondragón y Kalb et al., 2011; Schiavon et al., 2010), medicine (Madrazo, 2009), and demography (Gutierrez-Vazquez and Parrado, 2015).[5] The present paper, however is the first to harness the full power of vital statistics data, the first to collect and combine the ILE reform with the regressive law changes following this reform, and the first to consider how women's empowerment, as well as fertility declines, may be affected by abortion reform in Mexico. All in all, the paper provides strong evidence that abortion reform in an emerging economy leads to rapid and discernible changes in political behavior, aggregate fertility rates, and individual empowerment within households.

2 Unintended pregnancies, the Mexican context and the ILE reform

Globally, unintended pregnancies lead to approximately 46 million induced abortions each year, accounting for around 50% of the world-wide total of all (induced) abortions (Van Lerberghe et al., 2005). Induced abortion is a procedure or medical treatment for terminating pregnancy, and while induced abortion under appropriately supervised settings is considered one of the safest medical procedures in modern medicine, unsafe abortion is associated with substantially increased risks of severe morbidity and mortality.[6] Breathtaking figures suggest that world-wide,

[5]In examining the abortion reform and fertility outcomes, Gutierrez-Vazquez and Parrado (2015) use national vital statistics to examine the effect on fertility across ages. Due to the use of a limited amount of data and limitations inherent in the empirical design one cannot assign a causal interpretation to the results with confidence. More specifically, only a limited amount of data is used comparing outcomes between three different years (1990, 2000 and 2010).

[6]Induced abortions in a safe setting are carried out by professional health care providers in safe environment and in line with evidence based medicine. The procedure generally depends on gestational length of pregnancy. A safe induced abortion usually entails either a surgical operation or medical procedure. During a surgical operation, the products of conception are removed from the womb. The medical procedure is a non-invasive procedure that causes contractions of the womb, terminating the pregnancy. Medical abortion procedures are safer and more cost-efficient compared to other methods for first trimester abortions. It is common that the patient self-administers the medical abortion at home (Kulier et al., 2007). Induced abortion under safe conditions exhibits a mortality rate below 1 per 100,000 procedures (Grimes, 2005).

unsafe abortions may result in as many as 8 maternal deaths per hour (The Lancet, 2009). By the best available estimates, 13% of all maternal deaths are due to complications surrounding clandestine and unsafe abortion, with these numbers being much higher in certain regions and groups (WHO, 2011).

The highest estimated rate of unsafe abortion occurs in the Latin America and Caribbean region. Each year, an estimated 4.2 million unsafe induced abortions are carried out, accounting for 12% of all maternal deaths in the region (WHO, 2011). This region also exhibits some of the world's most conservative laws on abortion (United Nations, 2014). Prior to the legalization of abortion in Mexico City in 2007, and in line with nearly all other countries in the region, Mexico had very strict legal restriction on access to abortion.

Fertility and the Mexican context. Between the years 1975 and 2015, the fertility rate in Mexico declined rapidly from roughly 6 children per woman to approximately 2.2 children per woman. This major shift in fertility can be partially attributed to changes in access to modern contraceptive methods in the country (Juarez et al., 2013). In 1975, the Mexican government passed the General Population Law, which obliged the government to supply family planning services and provide contraceptives via the public health care sector free of charge. In 1995, family planning services were decentralized to the state level, where different states fund family planning to various degrees, possibly making family planning services differentially available across states. Although 67% of all women of childbearing age in Mexico report using modern contraceptive methods (and 5% use traditional and less efficient methods), it is estimated that more than half of all pregnancies are unintended.[7] Estimates suggest that up to 54% of these unintended pregnancies are terminated (Juarez et al., 2013).

Mexico consists of 32 federal entities, 31 of which are federal states plus the federal district of Mexico (also known as Mexico D.F. or Mexico City). In addition to the national constitution, each of the 32 federal entities has its own state or local constitution, defined by its own legislative power. Abortion laws in all of Mexico are determined at the state level (Becker, 2013). Mexico City contains approximately 8% of the entire population (8.9 million of Mexico's 119.5 million inhabitants according to 2015 estimates) and, since 2007, is the only state that allows for elective abortion during the first trimester.

[7] Modern contraceptives are condoms, oral or/injectable/implants of hormones preventing ovulation, IUD, sterilization and emergency contraception. Traditional or less efficient methods are calendar method or rhythm method, coitus interrupts, herbs or teas. For a detailed account of modern and traditional methods, see for instance Hubacher and Trussell (2015).

Legal restrictions and induced abortions. Prior to the reform in Mexico City, abortion laws were quite uniform across the 32 federal entities of Mexico. Induced abortion continues to be considered a criminal offense with the risk of up to 30 years imprisonment in many states, and legal abortion was only permitted in the limited cases of rape, threat to the life of the mother, or severe malformation of the fetus. In practice, even in these limited cases, legal abortion has been described by human rights organizations as extremely difficult to access due to rigid legal barriers (Juarez et al., 2013). In the densely populated Mexico City, only 62 abortions were legally performed during 2001-2007 (Becker, 2013).

The estimated rate of induced abortions for Mexico in 2006 was 33 abortions per 1,000 women of fertile age (Juarez et al., 2008), which is considered high internationally (Becker, 2013). As a substitute to legal options, abortions were performed in clandestine and often unsafe settings. In 2006 alone, medical records from public hospitals show that an estimated 150,000 women in Mexico were treated for abortion-related complications (Juarez et al., 2008). The most common method of induced abortion is believed to be the abortifacient drug Misoprostol, which despite the strict legal restrictions in Mexico, has been available in pharmacies since 1985 (Lara et al., 2011).[8] Despite the fact Misoprostol and other abortifacients formally require a doctor's prescription in Mexico, studies show that abortifacients are frequently sold over the counter without prescription (Lara et al., 2011). While a safe and well recognised method for induced abortion when appropriately taken, instructions on dosage and usage of Misoprostol is generally not available at pharmacies, leading to considerable risks when self administered (see for example Grimes (2005)).

Due to the high number of unsafe abortions as well as a growing movement for women's reproductive health rights and a coalition of pro-choice NGOs, the legislative assembly of the Federal District of Mexico City voted to legalize elective abortion (termed legal interruption of pregnancy, or ILE for its name in Spanish) on April 24, 2007, reforming Articles 145-148 of the penal code of Mexico City, and Article 14 of the Health Code. These reforms were signed into law the following day, and published in the official Gazette of the Federal District on April 26, 2007 (Ciudad de México, 2007). A broader discussion of the reform's social and legal setting is provided in Kulczycki (2011); Madrazo (2009), Blanco-Mancilla (2011) and Johnson (2013). This immediately permitted women above the age of 18 to request legal interruption

[8] Misoprostol (sometimes referred to as Cytotec, Arthrotec, Oxaprost, Cyprostol, Mibetec, Prostokos or Misotrol) is one of the recommended substance for induced abortion by the WHO (Lara et al., 2011). Misoprostol is a prostaglandin with the original purpose of curing gastric ulcers. It is also utilized for OB/GYN reasons such as induced abortion, post abortion procedures and induced labor for delivery (Kulier et al., 2007).

of pregnancy at up to 12 weeks of gestation without restriction. Access for minors requires parental or guardian consent. Under this law, induced abortion was made legal in both the public and private health care sectors.

Implementation of the ILE reform 2007. Immediate implementation was made possible by collaboration between the Ministry of Health of Mexico City, members of the health department and international NGOs, which had thoroughly designed a program for public provision of abortion services called the "the ILE program" and its implementation even before the law was passed (Singh et al., 2012). As such, abortion services were made available via the public health care hospitals immediately after the law was passed in April 2007, although with lower capacity and efficiency compared to current conditions. Abortion services were also quickly available in the private health care sector (Blanco-Mancilla, 2011). Additionally, under this law sexual education in schools was improved, and post-abortion contraceptives were made freely available directly from the health clinics which provided abortions (Contreras et al., 2011). Records from public hospitals show that the demand for post-abortion contraceptives is high (approximately 82% of all women accept contraceptives) and that prevalence of repeated abortion procedures are low (Becker, 2013). On August 29, 2008 the decision to pass the ILE law was ratified by the Supreme Court of Mexico, making Mexico City, together with Cuba and Uruguay, the most liberal jurisdiction in terms of abortion legislation in the entire Latin American and Caribbean region (Fraser, 2015).

Under the ILE program, women above the age of 18 with residency in Mexico City can access abortion services free of charge at a selected number of public health clinics operated via the Ministry of Health in Mexico City (MOH-DF).[9] Women with residency outside Mexico City can also access the public provision of abortion through MOH-DF but are charged with a sliding fee scale determined with regard to the woman's socioeconomic background. In 2010, 74% of all women who received an abortion through the public health care sector were women living in Mexico City, 24% were living in the state of Mexico (which shares a border with Mexico City) and 2% were living in other states (Mondragón y Kalb et al., 2011).

Figures from the Secretary of Health's administrative data suggest that abortions were used by women of all ages, though were disproportionately sought by younger (21-25 year-olds) and older women (36 year-olds and above), with lower rates of abortion among 26 to 35 year

[9]The public health care sector in Mexico is divided at both federal and state level, where the Ministry of Health (MOH) in Mexico City provides abortion procedures at a selected number of MOH-DF hospitals. Other MOH facilities (federally or state funded) are not legally required to provide abortion procedures.

olds. The proportion of all births by age and all abortions in public health clinics by age is presented in appendix figure A1. Approximately half of the abortions were sought by unmarried women (45.5% to single women, and 4.1% to divorced women), with the remainder nearly evenly split between married women, or those in a stable union. Information regarding the extent to which women below the age of 18 have access to abortion services is relatively scarce. However, according to a qualitative study by Tatum et al. (2012), the law on parental consent may be differentially enforced depending on the caregiver. While Public Hospitals require parental consent, only one out of three abortion providers in private health clinics require parental consent (Schiavon et al., 2010).

Accessibility and utilization of legally induced abortions. Information regarding the private provision of abortion services is limited due to a lack of supervision of the private market for legal abortion services (Becker, 2013). Despite the fact that safe abortion, at no or low cost, is provided by the public health system in Mexico City, women do seek abortion services within the private sector. A descriptive study by Schiavon et al. (2012) suggests that private abortion services are provided at high costs (157–505 US dollars) and that the quality of care is inferior to that in the public sector, given that the less safe and efficient "dilation and curettage" is used as the main method in the private sector (71%). A suggested explanation for the high rates of usage of private care relates to beliefs that the overall quality is higher in the private health sector (Schiavon et al., 2012).

Records from public hospitals show that during the year of 2007, when the reform was implemented, more than 7,000 abortion procedures were performed at 14 selected MOH-DF clinics. Over the years, the MOH-DF abortion program expanded its services and became more efficient in meeting the high demand for elective abortion. The MOH-DF program offers both surgical and medical abortion procedures and is the main provider of medical abortion (Winikoff and Sheldon, 2012).[10] The large shift from 25% of all abortion procedures being medical in 2007 to as much as 74% in 2011 have played a key part of meeting the demand (Becker, 2013). As of 2012, approximately 90,000 abortions were carried out at the MOH-DF clinics (Becker, 2013).

Post-April 2007 policy environment Almost immediately following Mexico D.F.'s ILE reform, a number of states began a series of counter-legislations to change the respective sections of their penal codes, defining the beginning of human life as occurring at conception. Often,

[10] Medical abortion procedures constitutes 66 percent of all abortion procedures in the MOH-DF program where Misoprostol was the main regimen until 2011 when Mifepristone was introduced.

these legal responses directly referenced Mexico D.F.'s ILE reform.[11] Even in cases where they did not directly refer to the ILE reform, it seems highly likely that the reform was a defining factor. For example, in the 20 years prior to the ILE reform there had been only two constitutionally defined changes to the articles relating to abortion in the penal codes of all states of Mexico (Gamboa Montejano and Valdés Robledo, 2014), compared to 18 changes between June 21, 2008 and November 17, 2009. Importantly, these reforms all changed the status of abortion from an act which was penalized according to specific articles of the penal code into a homicide, with considerably more severe sanctions of up to 30 years imprisonment.

In figure 1 we display the geographical distribution of law changes (progressive, regressive or neutral) over the period under study. The only progressive reform refers to Mexico D.F.'s ILE reform, while 18 states made regressive changes after the initial reform. We have compiled on a state-by-state basis the exact dates the reforms were passed into law, and these are displayed in table 1. To the best of our knowledge, there exists no centralized record of the dates and laws which were altered in the post ILE era, and as such we compiled these from our reading of legal source documents. In section 4 of this paper we return to how we use the state and time variation of this law in our identification strategy.

3 Data

3.1 Birth records from INEGI

To examine the effects of abortion reforms on fertility, we use vital statistics on all births registered in Mexico for the time period 2002-2011. The data is provided by the National Institute of Statistics and Geography (INEGI for its name in Spanish) and covers 23,151,080 live births among women aged 15-44. Vital statistics for births in Mexico are compiled by INEGI based on birth registries completed by each parent or guardian at the civil registry, rather than being based on birth certificates issued at hospitals (as is the case, for example with the National Vital Statistics System in the USA and in various developing and emerging economies, like Chile and Argentina). Using data from the 2010 census and birth records up until 2009, a recent (backward looking) analysis suggests that 93.4% of all births in Mexico were registered within 1 year of birth of the child, and in total, 94.2% of births are eventually registered at the national level (Instituto Nacional de Estadística y Geografía, 2012). The birth

[11]For example, the constitutional decree issued by the state of Nayarit when changing their penal code explicitly refers to the changes in the penal and civil code of Mexico D.F. (p. 14) (Gobierno de Nayarit, 2009).

register is released once per year, containing all births *registered* in that year, as well as the year the birth occurred. In order to avoid problems of under-reporting, differential reporting over time, and double-reporting, we collate all birth registers between 2002-2014, and then keep all births registered within 3 years of the date of birth.[12] This implies that we only have complete birth registers based on birth years up to (and including) 2011. While these birth registers are not universal, they are recognized as being considerably better than many other registry systems in developing economies. On average, dated estimates suggest that across all developing countries 41% of births are unregistered, and this figure for Latin America alone is 14% (UNICEF, 2005). As we discuss in later sections of this paper, unregistered births will only be a problem if rates of birth registration change differentially between regions of Mexico over the period under study. Empirical evidence on changes in birth records between 1999 and 2009 do not suggest a strong relationship between reform and non-reform areas, and changes in rates of coverage (Instituto Nacional de Estadística y Geografía, 2012).

For our principal analysis, we focus on all births occurring to women aged between 15 and 44 years of age who reside in each of Mexico's 32 states. Data from the birth registers is aggregated by each age group, state, and year, resulting in a total of 9,600 cells (years$\times$states$\times$age). The INEGI Birth Register contains information about the date of birth, actual birthplace and the official residency of the mother. In addition, information on maternal characteristics such as age, total fertility, educational attainment, marital status and employment status are recorded.

Summary statistics for birth data (as well as state-specific time-varying controls), are provided in table 2. Rates of birth are presented separately for Mexico D.F. (the principal reform state), states which went on to pass regressive reforms, and states which left un-altered their constitutions. We provide country averages in column 4, which agree with international calculations (The World Band, 2015). Summary statistics show that rates of birth in Mexico D.F. are lower than rates of birth in the rest of the country, and broadly comparable among regressive and non-regressive reform states. In principal analyses we capture difference in levels among states by state fixed effects, and examine robustness of our results to entropy weighting which matches on pre-reform birth rates.

[12]This allows us to record births even when they are registered months after birth (up to 36 months following the birth). Considering additional registration lags results in virtually unchanged estimates, as nearly all ever-registered births are registered within 3 years of birth. This is very similar to the methodology employed by Mexico's population authority in their calculation of official demographic trends (Consejo Nacional de Población, 2012).

3.2 Survey data from the Mexican family life survey (MxFLS)

In order to examine female empowerment and potential mechanisms through which the reform may have affected fertility, we use longitudinal data on household decision-making and contraceptive use and knowledge from the Mexican Family Life Survey (MxFLS). The MxFLS is a nationally and regionally representative longitudinal data set that follows the Mexican population over time, covering various topics regarding the well-being of individuals including information on household decision-making and reproductive health. The MxFLS dataset is publicly available, developed and operated by the Iberoamerican University (UIA) and the Center for Economic Research and Teaching (CIDE) and also supported by multiple institutions in both Mexico (INEGI and National Institute of Public Health) and the USA (Duke University and Universities of California, Los Angeles). The survey was conducted in three waves during 2002-2003, 2005-2006 and 2009-2012.

The sample used for the analysis of household decision-making consists of a panel of 5,816 unique women living in a household together with their spouse or partner and who completed the household module. The module on household decision-making includes questions on which household members decide on children's health and education, major household spending, labor market participation and contraceptive use, among other things. In table 3, summary statistics regarding women's participation in household decision-making processes are presented, separated by their region of residence. The averages in participation are presented again separately for Mexico D.F (column 1), states which went on to pass regressive laws (column 2), states which left their constitutions un-altered (column 3) and the averages for the full country (column 4). Panel A displays decision-making for women aged 15-44 (fertile age) and Panel B for women above age 44. We employ this split into fertile and non-fertile age women in a placebo test discussed in section 4. The summary statistics show that women with residency in Mexico City are on average more likely to participate in household decisions compared to women in the rest of the country. A similar pattern can be found across age groups, where women aged 15-44 appear to play more of a role in decisions within the household compared to women above age 44.

Finally, we use the reproductive health module from the MxFLS which collects information on contraceptive knowledge and usage as well as information on sexual behavior such as the number of sexual partners. This sample consists of a panel of women aged 15-44 who completed the reproductive health questionnaire resulting in a total of 5,404 women. We return to use these data in tests of behavior following abortion reforms. Summary statistics for reproductive health across regions are provided as an online appendix (table A1) and show that average knowledge of at least any kind of modern contraceptive methods are generally high across all regions, while the average usage of any kind of contraceptives and modern contraceptives are higher in Mexico City compared to other states.

3.3 Additional data sources

We collect a number of additional (time-varying) controls measured at the level of state and year. This includes the population of women (variation by age, state and year) from the National Population Council of Mexico (CONAPO), socioeconomic variables including illiteracy, schooling, and access to health insurance from the National Institute for Federalism and Municipal Development (INAFED) and the National Education Statistical Information System (SNIE), and data on the municipal-level roll-out of the national health insurance program *Seguro Popular*[13] from the INEGI data bank. Socioeconomic data and measures of *Seguro Popular* coverage vary by state and year. These are merged by year and state to the birth data discussed in section 3.1, and are included as time-varying controls in certain regression specifications.

4 Empirical strategy

4.1 Estimating effects on fertility

The impact of the abortion reform is evaluated by using the sub-national variation in abortion laws, and thus the access to legal and safe abortion procedures, resulting from the ILE reform. Given the temporal- and geographical-variation in availability of free legal abortions, and resulting regressive law changes, we estimate the following difference-in-differences (DiD) specification:

$$\ln(Birth)_{ast} = \beta_0 + \beta_1 \text{ILE}_{s,t-1} + \beta_2 \text{Regressive}_{s,t-1} + \boldsymbol{X}_{st}\boldsymbol{\delta} + \alpha_s + \nu_t + \pi_a + \lambda_s \cdot t + \varepsilon_{ast}. \quad (1)$$

Here the outcome variable of interest is the natural logarithm of the total number of births for women of age a in state s and year t. We are interested in two quasi-treatment variables, each

[13] Mexico's General Health Law underwent a major reform in 2003, which intended to provide 50 million Mexican citizens lacking social security with subsidized and publicly financed health insurance. The core of this reform was the health insurance program *Seguro Popular* (SP). The "People's Insurance" or *Seguro Popular* was launched in 2002, offering health service free of charge or subsidized to those without formal health insurance. By 2005, two years before the reform, all 32 states had enrolled in the SP program (Knaul et al., 2007).

of which are determined by the official residency of the woman. The first, indicated by $\text{ILE}_{s,t-1}$ is a variable that takes the value of one in Mexico City nine months after the ILE reform was adopted in order to compensate for the lag caused by the pregnancy length (assuming 40 weeks of gestation), and zero otherwise.[14] The second dependent variable of interest, $\text{Regressive}_{s,t-1}$ is defined in a similar way, however taking the value of one in those states which passed regressive laws in response to the ILE reform at least 9 months after each law was passed. As discussed in section 2, a non-negligible proportion of all elective abortions were accessed by women with residency in the neighboring state of Mexico. Thus, to ensure that any potential spillover effects of the reform are not included as part of the quasi-control group, we always separately control for this with a dummy for the post-reform period in Mexico State.

The difficulty in evaluating effects of these new laws lies in the fact that legislative changes are often endogenously determined. That is, abortion legalization is likely to be correlated with observed and unobserved characteristics of Mexico City and, similarly for the regressive reform states. Even though the distribution of treatment is non-random, the inclusion of state (α_s), year (ν_t) and age (π_a) fixed effects allows us to estimate the impact of the reform in a DiD setting. Under the parallel-trends assumption that in the absence of the reform treated and untreated states would have followed similar trends over time, DiD gives the causal impact of the reform on outcome variables. We examine the veracity of this assumption in following sections including estimating a full event study for the effect of the ILE reform. In certain specifications, we include a set of state-level time-varying controls $\boldsymbol{X}_{st}$, and also allow for differential linear time trends in each state over time, captured by the $\lambda_s \cdot t$ term. The idiosyncratic error term ε_{ast} is clustered at the state level in order to allow for autocorrelation of unobserved shocks within states over time,[15] and age by state by year cells are weighted by the number of births occurring to women of that age in that state and year (see for example Dell (2015) for a discussion based on a similar structure).

In our main specification, births are measured as the log number of total births occurring in each cell. While births can be measured in a number of ways, including counts, gross fertility

[14] We choose the most conservative definition of the post-treatment period starting in January 2008 and onwards for our baseline specification.

[15] This is the generally accepted method in a DiD model (Bertrand et al., 2004). However, there is a potential inconsistency in the standard error caused by serial correlation when the time period is long and numbers of groups (i.e. states) are small (Bertrand et al., 2004). A likely outcome in these circumstances is underestimated standard errors leading to falsely significant DiD estimates. This raises concern, since the number of clusters in our case are 32, which is slightly below commonly accepted "rule of thumb" thresholds for consistent estimation of standard errors (Angrist, J. D. and Pischke, J., 2009; Cameron and Miller, 2015). One suggested way of dealing with this problem is to use wild bootstrapped standard errors (Bertrand et al., 2004; Cameron and Miller, 2015), and as such, we also examine our main specifications using wild bootstrapped standard errors and show that these results are consistent with our baseline results.

rate and total fertility rate (which we report in the online appendix), we prefer the natural logarithm of the number of births for a number of reasons. First, we lack micro-data registers of population in each year and are constrained to demographic projections based on the census, quinquennial surveys, migration, births and deaths (Consejo Nacional de Población, 2012). Second, we estimate regressions with log births using OLS. Without the log normalization of births, regression residuals are not normally distributed, and predicted values are at times negative. Taking the log transformation allows us to resolve these issues in our case. Nevertheless, we consistently report alternative specifications and measures in appendix tables.

In equation 1, all states which were not affected by either the ILE reform or resulting regressive changes are considered as part of the quasi-control group. Given the considerable heterogeneity across the country, both within and between urban and rural districts, this may result in a quasi-control group which is considerably different from the quasi-treatment groups. While our difference-in-difference study will pick up any difference in levels, nevertheless we may be concerned that heterogeneity between groups drives the results, rather than the reform itself. In order to temper these concerns, we provide additional estimates of equation 1, however this time using entropy balancing to determine an optimal quasi-control group. Entropy-balancing, from Hainmueller (2012), is a technique designed to optimize covariate balance between two groups. This technique, increasingly used in economic applications (for example Stanton and Thomas (2016)) matches the moments between samples of desired covariates.[16] In order to apply this to our DiD methodology we calculate entropy weights matching only on *pre*-reform birth rates of births between states. As well as documenting graphical effects of the reform under entropy matching, we can then replicate our findings from equation 1 with the optimal weights, to examine whether our earlier effects are driven by a non-ideal control group.

4.2 Estimating effects on individual and household behavior

After documenting the effect of various reforms on fertility outcomes at a state level, we then go on to estimate their effect on individual behaviors collected from the MxFLS data. Given that the MxFLS follows women and families over time, this allows for the construction of a panel overlapping the full sets of reforms on each side. When turning to behavioral outcomes, this

[16]The logic of this methodology is similar in style to synthetic control methods described in Abadie et al. (2010). However, we prefer entropy weighting rather than synthetic control methods as synthetic control estimates require that outcomes in the treated state are contained in a convex hull of outcomes in potential control states. Given that the number of births in Mexico D.F. is higher than the number of births in states in the rest of the country, this convex hull assumption often is not met, and as such, entropy weights are consistently preferred in these alternative specifications.

leads to the following specification:

$$Behavior_{ist} = \alpha_0 + \alpha_1 \text{ILE}_{st-1} + \alpha_2 \text{Regressive}_{st-1} + \mu_i + \phi_t + \boldsymbol{X}_{it}\boldsymbol{\delta} + \eta_{ist} \qquad (2)$$

As before, ILE and Regressive are dummy variables indicating whether the woman i in question was exposed to either type of reform in the previous period. Once again these are measured at the level of state of residence (which is where the woman is interviewed in her household). Our outcome of interest in this case is *Behavior*, which measures a series of behaviors of interest, both in terms of empowerment within the household, and reported sexual behavior. Given the panel data setting and three rounds of data, we control for household-specific fixed effects (μ_i), and survey wave fixed effects (ϕ_t). Our coefficients of interest are thus the effect of having been exposed to the reform, *conditional* on all observable and unobservable household-specific invariant factors which are absorbed in the fixed effect.

For our tests described in equation 2, there are various *Behavior* indicators which were (ex-ante) defined as outcomes of interest. This implies running multiple regressions on our treatment indicators, leading to a well known problem of testing multiple hypotheses with a single reform. If we were to naively estimate multiple regressions and examine the test statistic relating to α_1 and α_2 at a fixed significance level in each one, we would be at risk of incorrectly over-rejecting null hypotheses after the first test. In order to account for this, we efficiently (both statistically and computationally) fix the level of the family wise error rate (FWER) of these tests. We follow a step-wise testing algorithm proposed by Romano and Wolf (2005); Romano, J. P. and Wolf, M. (2005), which updates the proposed multiple hypothesis testing algorithms of Bonferroni (1935) and Holm (1979). Fixing the FWER instead of a significance level of each individual hypothesis means that we will no longer be propense to overcommit type I errors. A full discussion of the Romano-Wolf step-down technique and the resulting p-values is provided in appendix A2.

The hypotheses of interest tested in equation 2 relate to well-known (theoretical) results suggesting that empowerment of women will respond to changes in birth control technologies (Chiappori and Oreffice, 2008). In order to allay concerns that any results may represent a general change of empowerment of all women, and identification concerns that empowerment may be the cause, rather than the result, of the reform, we propose two placebo tests. The first placebo test consists of re-estimating equation 2, however in place of using fertile aged women, estimate the effects on women who are no longer of fertile age, and hence no longer benefit from any additional bargaining power gained on the marriage market. This placebo test is suggested by Oreffice (2007), who we follow here.[17] The second test is an identification test, and consists of estimating the same model using only pre-reform waves of the MxFLS. Given that we have two waves of pre-reform data, we can re-estimate equation 2, where in place of the actual reform dates, we use placebo dates between the first and second survey round which were entirely before the actual reforms took place. In each case, the ILE and Regressive variables are defined for the same states, however in the second pre-treatment period. If any changes in empowerment do actually flow from the reform, we should see that these placebo reforms have no effect on empowerment, suggesting that parallel trends between treated and non-treated areas existed before implementation. As is the case with the main specifications, in all cases where multiple hypotheses are tested, we efficiently correct for over-rejection fixing the FWER using the Romano-Wolf procedure.

5 Results

5.1 Fertility

Table 4 presents results of the DiD model described in equation 1. The first three columns display the pooled effect of the reforms on women of all ages, while columns 4-6 present the same specifications for teenage women only (ages 15-19). These results suggest, first, that the legalization of abortion in Mexico D.F. caused a large and statistically significant reduction in rates of births, both for all women, and for teenage women. The estimated coefficient on ILE Reform for all women fluctuates between a reduction of births by 2.2% ($p < 0.05$) to a reduction by as much as (a marginally significant) 3.8% when including state-specific linear trends and time-varying controls.[18] When considering only the effect of passing the ILE reform on teenage motherhood, we find larger effects, of a magnitude comparable to international evidence (Ananat and Hungerman, 2012; Bailey, 2006; Guldi, 2008; Valente, 2014). The baseline (uncontrolled) DiD effect is estimated as a 5.3% reduction in rates of teen pregnancy, with estimates as high as a 7.0% reduction when accounting for time-varying controls and allowing for state-specific

[17]The results of Chiappori and Oreffice (2008) also suggest an alternative placebo test. Their proposition 5 suggests that all women gain bargaining power following abortion legislation, with the exception of cases in which there is a shortage of marriageable men. They suggest that this situation occurs in certain groups, such as low-skilled or minority women. As such, we replicate our tests using low-educated and minority groups, as a potential alternative placebo.

[18]Percentage change in births based in coefficients in the log model are interpreted as $\exp(\hat{\beta}_1) - 1$. The coefficients can be approximately interpreted as the proportional reduction in rates of birth, but when we refer to them in the text we will always perform the exponential transformation to refer to exact changes.

linear trends. The magnitude and direction of this effect is virtually identical to that found by Pop-Eleches (2005) following the lifting of abortion restrictions in Romania.

The estimates corresponding to the effect of constitutionally *tightening* policies relating to abortion appear to be largely of the reverse direction, however never at a statistically significant level. When considering the effect of "Regressive Law Changes" in table 4 we see that these are associated with small positive coefficients for all women (ranging from a 0.1% to a 1% increase in rates of births), though always imprecisely estimated. For teenage women the evidence is once again imprecise, suggesting that if anything, the effect of regressive laws are too small to be statistically indistinguishable from zero.

Our principal specification uses population-weighted cells by age, state and year, so results are interpreted as the effect on births per woman. In appendix table A2 we see that the negative effect of the ILE reform on fertility is largely unchanged when considering unweighted results which give equal weight to age by state cells.[19] Similarly, we find that when replicating this specification using cluster-robust wild bootstrapping (refer to appendix table A3), estimates are largely unchanged: the effect of the ILE reform is found to be negative and statistically distinguishable from zero, while we can never reject the null that the resulting regressive policy changes have had any significant effect of birth rates for all women, or young women. When examining results by age we see that results are largely driven by younger and older women (refer to appendix table A4), and are substantively similar when instead of using log(births) as the outcome variable of interest, we use the birth rate based on an estimated population in the denominator (refer to appendix table A5).

5.2 Validity of difference-in-differences strategy

The validity of the previous results rely fundamentally on the validity of a parallel-trends assumption for the DiD specification. We examine this assumption formally in figure 2 with the plotting of an event study examining the effect of the ILE reform on rates of birth. In this plot we fully interact a dummy of residing in Mexico D.F. with the years preceding and posterior to the reform. The coefficients on these variables then allow us to compare changes in levels of births in D.F. compared with changes in levels in the rest of the country, with respect to an arbitrary base year.[20] We follow the general convention of omitting the year that the reform

[19] We note that in this specification, when not including any controls on regressive changes are actually negative and at times significant, but when full controls are included, results are once again insignificant when examining regressive changes in abortion laws.

[20] Crude trends of numbers of births in Mexico D.F. and the rest of the country are displayed in appendix figure A3. If we fit simple time trends on either side of the initial reform for each group we observe that each trend is

was implemented as the omitted base category, as the effect on fertility should begin in the first post-reform year. The rest of the specification follows equation 1 precisely including the use of time-varying controls, fixed effects, and clustered standard errors.

If the estimated reduction in fertility from table 4 is indeed due to the effect of the reform rather than capturing prevailing differences in trends between quasi-treatment and quasi-control areas, we should see that differences in trends emerge only *after* the implementation of the reform. We see precisely this pattern in figure 2, where we display the event study for women of all ages (a similar result holds when considering only adolescent fertility, and is displayed in appendix figure A2). In the 5 pre-reform periods, there are no statistically significant differences between quasi-treatment and quasi-control compared to the prevailing difference in the year when the reform was implemented. However, a sharp reduction in fertility appears in Mexico D.F. in the first post-reform year, leveling off at approximately -5% in the following 3 years. This provides support of the parallel trend assumption, as any confounding factors which could explain the reform's effect on fertility must have emerged over exactly the same time-period of the reform, rather than as pre-existing differential trends. The magnitude of the dynamic effects also matches up quite well with actual usage figures of abortions in public health clinics, which reached a plateau two years after the reform's implementation.

5.3 Using entropy balancing to examine estimate validity

The results described in sections 5.1 and 5.2 provide convincing evidence that the ILE reform produced a significant reduction in rates of birth, especially among younger women. However, in the analysis up to this point, the reform area (Mexico D.F.) was compared to all untreated areas of the country, regardless of differential state-level characteristics. Given the heterogeneity between (and within) Mexican states, we examine the robustness of these findings to a potentially more comparable quasi-control group. In order to do so, we use an entropy weighting procedure described by Hainmueller (2012). This allows us to match states based on pre-reform rates of fertility, and examine how these pre-matched states evolve once the reform has been implemented.[21]

negative, and that the change to more negative in Mexico D.F. following the reform is larger than the change in the rest of the country. Of course, these trends do not necessarily imply a causal relationship, which is why we estimate DiD models and event studies.

[21] The logic of entropy weighting shares certain characteristics with the synthetic control method for difference-in-differences of Abadie et al. (2010). However, entropy weighting does not rely on a convex hull assumption within states over time, meaning that even if Mexico D.F. has higher rates of fertility over the period under study, we can apply entropy matching using pre-reform birth rates. For this reason, we prefer entropy matching over synthetic control methods.

In figure 3 we observe that entropy matching provides an appropriate pre-trend balance between Mexico D.F. and the matched rest-of-Mexico sample. Graphically, we observe that even when demanding that states are matched on pre-trends and levels of fertility, rates of birth in Mexico D.F. decline faster and by a greater amount after the reform than in the matched but untreated states. Similar results are presented by age-group in appendix figure A4, and we observe that (up to the age of 35 at least), a similar dynamic is observed.

We examine these results in a regression framework in table 5. In these specifications we use the weights estimated from the entropy matching process to replicate specification 1. We find, reassuringly, that results are qualitatively similar. For all women the effect of the reform is estimated to vary from -2.4% (baseline DiD) to -3.2% (including controls and state-specific linear trends), though when including state-specific trends the result is no longer estimated with sufficient precision to reject a null hypothesis of a zero effect. When turning to teenage births, however, we are able to reject a null of no effect for both the baseline and the trend with control model. In this case, our estimated effects are slightly smaller than when we use the full-Mexico quasi control group, though the results still suggest a quantitatively considerable effect, varying from a 4.4% to 5.2% reduction in teen births.

5.4 Mechanisms: Availability, education, or behavior

Along with the law change legalizing access to abortion, the ILE reform included additional components relating to sexual education and disbursement of additional contraceptives in clinics (refer to section 2 for a full discussion). In order to examine the channels through which the reform affected fertility: whether it be only access, or a combination of access with behavioral change, we turn to a dataset which allows us to observe (self-reported) behavior more directly. We use the MxFLS data which follows women over time, and has survey rounds both before and after the fertility reforms of interest. To examine the potential effect of the other aspects of the reform (sexual education and alternative contraceptives), we estimate equation 2, which allows for individual specific fixed-effects given the panel data nature of the MxFLS data used.

We examine the effect of abortion reform on all available measures of contraceptive use (whether using any contraceptive or using modern contraceptives), the number of reported sexual partners and whether the respondent reports having knowledge of modern contraceptive methods. In this case, as we are regressing multiple outcome measures on an identical series of reform variables, as discussed above it is well-known that classical tests will lead to over-rejection of null hypotheses of a zero effect of the reform (Bonferroni, 1935; Holm, 1979). To correct for a higher likelihood of committing type I errors, we estimate p-values using Romano and Wolf (2005); Romano, J. P. and Wolf, M. (2005)'s step down method. This penalizes p-values to account for multiple hypothesis testing, and does so in an efficient way which allows for arbitrary correlations between outcome variables. In appendix A2 we provide a full discussion of our implementation of this multiple hypothesis testing method.

We present results of these regressions in table 6. In general, we find very little evidence to suggest that the results of the abortion reform flow from an increase in *other* contraceptive knowledge in reform areas, or change in risky sexual behavior as a result of the reform. We find quite close to zero effects for change in contraceptive use and knowledge, and an insignificant reduction in the number of sexual partners reported. In all cases, these results are insignificant at the 10% level when using both traditional and Romano-Wolf corrected p-values, though as expected, p-values are lower when failing to account for multiple testing. When we replicate these results using a repeated cross-section of women rather than household fixed-effects in a panel setting, we reach similar conclusions that the ILE reform does not operate with alternative contraception or information channels, suggesting that the ILE reform's effect is largely due to the sharp increase in utilization of abortion services (see appendix table A6 for the cross-sectional replication). Similarly, we do not find that regressive changes in abortion laws cause women to seek additional information or be more likely to use contraceptives, or change sexual behavior as proxied by the number of sexual partners compared to areas which were not subject to a regressive reform. Overall, like the case of the fertility results described in previous subsections, these results suggest that regressive reforms themselves are not sufficient to result in easily perceptible changes in fertility behavior.

5.5 Female empowerment

Table 7 presents results of the reform's effect on women's reported empowerment within the household. Here we once again estimate specification 2 using MxFLS panel data. Table 7 suggests that, as in developed countries (Oreffice, 2007), so in an emerging economy setting (progressive) abortion reform increases women's bargaining power within the household. In column 6 of this table we present a panel-data regression of an aggregate empowerment index on reform indicators. This aggregate indicator, a sum of all ex-ante defined measures of women's empowerment in the household variables, takes a more positive value when women report having a greater role in decisions relating to their behaviors, or investments in their children. Following the ILE reform in Mexico D.F. the average value of this index for women was found to increase by substantially more than that for women in other parts of the country (we discuss two placebo tests relating to these results in the following paragraphs). The effect size is significant: on average, the sum of all empowerment variables increased by 10% of its baseline value when comparing between reform and non-reform areas. However, we find very little evidence to suggest that the regressive changes in abortion laws was sufficient to harm women *when considering intra-household outcomes only.* The estimated effect on the aggregate index was found to be positive, small, and statistically indistinguishable from zero following regressive law changes.

In additional columns of table 7 we examine each item of the index separately, where in each case a higher value for the variable indicates that the woman is more likely to take part in the respective decision in her household. As before, given that multiple hypotheses are tested, p-values are corrected using Romano and Wolf (2005); Romano, J. P. and Wolf, M. (2005)'s stepdown procedure. With one exception, we see that for all outcomes considered, the reform's effect is to increase empowerment compared to non-reform areas. However, among the five elements, the largest and most statistically significant effect is found on women reporting to be more likely to participate in decisions regarding investments in their children. The coefficient on taking part in a child's educational decisions is found to be statistically significant, even when correcting for multiple testing. Remaining variables, while signed in a way which suggests increasing empowerment, are not statistically significant based on Romano-Wolf p-values.

These results, while suggestive, may capture many other underlying changes in empowerment across districts within Mexico which are unrelated to fertility reform. We provide an additional test of whether these results may flow from the fertility reform using a placebo group in which we estimate the same specification, however this time comparing women *above* fertile age in reform and non-reform areas. This type of test follows discussion in Oreffice (2007), who argue that empowerment effects should be observed among fertile aged couples, but not older couples (for example, see p. 114 in Chiappori and Oreffice (2008) who also refer to the tests of Oreffice (2007)). In table 8 we present results of the effect of the reform on women who are no longer of fertile age. As in the empirical work of Oreffice (2007), we find no evidence to suggest that the reform increases empowerment among women who are aged 45 or above. Indeed, among the aggregate index and all elements of the index, both for the ILE reform and regressive reform states, only one significant effect was found, and it was a significant *negative* effect on participation in large expenditures. These placebo tests lead credence to the interpretation that abortion reform increases empowerment among women of fertile age as, if anything, empowerment was weakly decreasing in Mexico D.F. among women over the ages of 45.

Finally, we may be concerned that rather than being a result of the reform, women's empowerment may have been (part of) the cause of the reform. If this is the case, rather than our results indicating that contraceptive reform increased empowerment in Mexico D.F. we would be capturing causality that runs in the opposite direction. Fortunately, given our panel-data setting with two pre-reform periods, we can test this formally to see if empowerment changes emerge pre- or post-reform. The logic of this test is similar to typical tests of Granger (1969) causality. In table 9 we estimate a placebo specification where we remove the third round of survey data, and define the reform variables as if any reforms occurring between the second and third survey wave had occurred between waves 1 and 2 of the survey. In this case, any significant estimated effects of the reforms will indicate a pre-existing difference in trends among reform and non-reform states, rather than a direct effect of the reform itself. Once again, we find little—or no—evidence to suggest that this was the case. Among both the empowerment index and the elements of the index, no statistically significant effects are found (when appropriately adjusting for multiple hypothesis testing). While some individual elements have non-negligible but insignificant point estimates, the impact on the aggregate index is a quite tightly estimated zero (up to two decimal places).

6 Conclusion

The passing of the ILE reform in Mexico D.F. provided an unprecedented case among Latin American countries, and joined very few large scale reforms of abortion in developing and emerging countries world-wide. Given continual social and economic discussion of the tightening and loosening of abortion policy in many contexts, the passing of this reform allows for an important examination of the broad scope of potential effects. This paper allows us to test the impact of state-specific expansions of abortion policies, and also joins recent work including Lu and Slusky (2016) and Fischer et al. (2017) which examines the impact of regional contractions in abortion policies. The legislative environment following the ILE reform in Mexico D.F. provides an uncommon example of nearly simultaneous expansions and contractions of the availability of, and risk of accessing, elective abortions in a single country.

In this paper we document that, first, the passing of the ILE reform lead to immediate changes in policy which affected women even in states considerably separated from Mexico D.F. We generate a database of regressive law changes relating to abortion which precisely captures these *policy* changes, and allow for us to capture both the effects of the ILE reform, and resulting legislative changes on a state-by-state basis. Second, we show that as documented extensively in the USA and in a number of lower and middle income countries, the legalization of abortion does lead to a reduction in fertility, and that this reduction is particularly noteworthy for younger women. Had the abortion law not been passed in Mexico D.F., we estimate that fertility would have been approximately 7% higher among adolescents, which is equivalent to 4 additional births per 1,000 15-19 year olds. For means of comparison, in the 14 years from 2000 to 2014, the adolescent fertility rate in the whole country has fallen from approximately 80 per 1,000 teens to 63.5 per 1,000, or a reduction of 15.5 births per 1,000 women (The World Band, 2015). We document that this effect appears to be driven by access to legal abortion, and find little evidence to suggest that it leads to large changes in sexual behavior, contraceptive knowledge, or contraceptive use. Finally, we document that in the context of Mexico, large-scale abortion reform brings with it increases in women's empowerment within the household, finding that empowerment changes accrue to fertile aged women rather than older women, as proposed in formal economic models of fertility reform (Chiappori and Oreffice, 2008; Oreffice, 2007). Unlike recent evidence from the USA, we do not find statistically appreciable impacts of the tightening of restrictions on accessing elective abortion. However, the context studied is quite different to recent evidence from the USA (and other studies examining contractions of abortion availability world-wide). In the case of Mexico, contractions focus on the demand for, rather than supply of, elective abortions, and start from an already highly legislated setting where abortions are penalized by law.

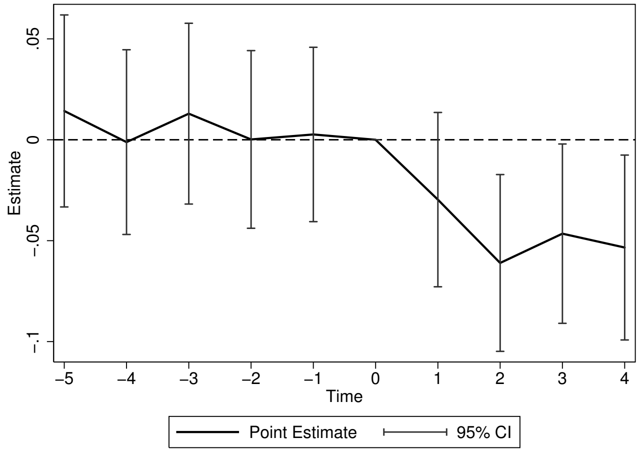
All in all, this paper provides additional evidence of the potential scope of legalized abortion, even in a late-adopting setting. Although many countries, particularly in the developed world, do allow access to legal abortion, the lessons from this case are relevant to many countries in the developing world which currently do not allow abortion in any circumstance, or only under a very limited set of conditions. At present, approximately 25% of the world's population lives in a place where abortion is not legal, suggesting that future reforms could be responsible for (further) demographic transition, empowerment, and the additional benefits that accrue from women playing a larger role in household decisions.

7 Figures and tables

Figure 1: Geographical Distribution of State Law Changes (post August-2007)

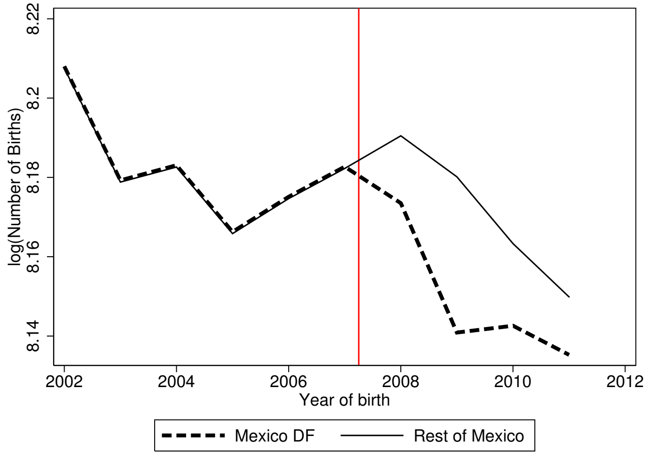
NOTES: The August 2007 ILE reform occurred in Mexico D.F. (yellow). Resulting (regressive) reforms in other states are indicated in red, with states highlighted in blue indicating that no law change occurred between 2007 and 2016.

Figure 2: Event Study Estimates of ILE Reform



NOTES: Event study estimates and confidence intervals interact the presence of legalized abortion with lags and leads. Each lag/lead is a yearly estimate, and year 0 (2007) is the omitted base year.

Figure 3: Births using Entropy Weights Based on Pre-Reform



NOTES: Trends in log(Births) for Mexico D.F. and an aggregate trend for the rest of Mexico are displayed. The aggregate trend is calculated using entropy weighting (Hainmueller, 2012). Weights are constructed based on *pre*-reform birth rates between treated and non-treated areas. The vertical red line displays the date of the law change.

Table 1: Constitutional Changes Following Mexico DF's ILE Reform

State	Reform Date	Constitutional Decree	Article in Question
Baja California	Dec 26, 2008	Decree 175	7
Chiapas	Jan 20, 2009	Decree 139	178
Chihuahua	Jun 21, 2008	Decree 231-08	143
Colima	Nov 25, 2009	Decree 296	187
Durango	May 31, 2009	Decree 273	350
Guanajuato	May 26, 2009	Dictamen 836	158
Jalisco	Jul 02, 2009	Decree 22361	228
Morelos	Dec 11, 2008	Decree 1153	115
Nayarit	Jun 06, 2009	Decree 50	335
Oaxaca	Sep 11, 2009	Decree 1383	312
Puebla	Jun 03, 2009	SPI-ISS-27-09*	136
Querétaro	Sep 18, 2009	P. O. 68‡	339
Quintana Roo	May 15, 2009	Decree 158	92
San Luis Potosí	Sep 02, 2009	Decree 833	128
Sonora	Apr 06, 2009	Law 174	265
Tamaulipas	Dec 23, 2009	Decree LX-1850	356
Yucatán	Aug 07, 2009	Decree 219	389
Veracruz	Nov 17, 2009	G. L. 155‡	150

NOTES: All states which formally altered their constitutions following Mexico DF's ILE reform are indicated above. Constitutional decree refers to the law composed to alter the state constitution, and article in question refers to the article altered in the constitution or penal code which was altered by the decree. Dates, decrees and articles are collated by the authors from various state government sources. The official document approving each decree and its associated date is available in a zipped folder on the authors' websites.

* Decrees or official newspapers for the State of Puebla could not be located by the authors. The date and article in question is suggested by Gamboa Montejano and Valdés Robledo (2014).

‡ P. O. refers to the official newspaper where laws are published in Querétaro, and G. L. refers to the same newspaper in Veracruz. The law was published without number (pp. 9857-9859) in P. O. 68 and in G. L. 155 (pp 2-5) in Querétaro and Veracruz respectively.

Table 2: State and Maternal Characteristics (Birth Data)

	(1) Mexico City	(2) Regressive States	(3) Rest of Mexico	(4) Full Country
ILE Reform	0.400	0.000	0.000	0.013
	(0.491)	(0.000)	(0.000)	(0.111)
Regressive Law Change	0.000	0.226	0.000	0.134
	(0.000)	(0.418)	(0.000)	(0.341)
Illiteracy	2.415	7.435	8.900	7.828
	(0.259)	(3.992)	(5.543)	(4.735)
People aged 6-14 with no schooling	2.954	5.122	5.504	5.197
	(0.152)	(1.188)	(2.086)	(1.632)
No Health Coverage	39.228	39.072	43.958	40.909
	(4.357)	(12.970)	(17.128)	(14.698)
Seguro Popular	0.625	0.746	0.742	0.741
	(0.463)	(0.370)	(0.363)	(0.371)
Birth Rate (All)	64.738	88.246	87.745	86.025
	(33.552)	(47.809)	(48.068)	(47.305)
Birth Rate 15-19	56.500	76.481	78.216	75.673
	(30.215)	(40.534)	(40.562)	(40.251)
Birth Rate 20-24	99.412	141.671	141.880	138.321
	(2.676)	(15.313)	(12.711)	(17.952)
Birth Rate 25-29	92.580	127.298	127.876	124.484
	(6.178)	(16.968)	(13.572)	(18.012)
Birth Rate 30-34	76.904	90.752	90.447	89.373
	(10.155)	(18.504)	(17.557)	(17.979)
Birth Rate 35-39	40.845	47.316	45.461	46.002
	(11.689)	(15.433)	(14.488)	(14.879)
Birth Rate 40-44	9.295	14.296	12.326	13.060
	(5.507)	(8.803)	(7.810)	(8.307)
States × Year	300	5700	3600	9600
Total Births	1,505,790	12,729,949	8,921,380	23,157,119

NOTES: Data on fertility and maternal characteristics is obtained from INEGI and covers all births among women aged 15-44 during the time period 2002-2011. Data on state level education and health care is obtained from the National Institute for Federalism and Municipal Development and the National Education Statistical Information System (respectively) for the same period. Mean values are displayed, with standard deviations below in parentheses. Regressive states are those which ever had a regressive law change posterior to 2008, and so regressive law change is the proportion of all years in these states which follow a law change. Similarly, ILE Reform refers to the proportion of years in Mexico D.F. which follow the implementation of the ILE Reform

Table 3: Summary Statistics, Household Decision Making, MxFLS

Elements and index	(1) Mexico City	(2) Regressive States	(3) Rest of Mexico	(4) Full Country
	Panel A: Women aged 15-44			
Child Education	0.929	0.898	0.882	0.893
	(0.258)	(0.303)	(0.323)	(0.309)
Child Health	0.895	0.903	0.880	0.894
	(0.307)	(0.297)	(0.325)	(0.308)
Expenditures	0.723	0.681	0.667	0.678
	(0.449)	(0.466)	(0.471)	(0.467)
Work	0.892	0.779	0.761	0.778
	(0.311)	(0.415)	(0.427)	(0.416)
Contraception	0.863	0.833	0.854	0.842
	(0.345)	(0.373)	(0.354)	(0.365)
Index	4.302	4.094	4.044	4.085
	(0.945)	(1.081)	(1.111)	(1.088)
Observations	172	4769	3234	8175
	Panel B: Women above age 44			
Child Education	0.442	0.464	0.475	0.466
	(0.499)	(0.499)	(0.499)	(0.499)
Child Health	0.503	0.496	0.492	0.495
	(0.502)	(0.500)	(0.500)	(0.500)
Expenditures	0.726	0.675	0.674	0.678
	(0.448)	(0.469)	(0.469)	(0.467)
Work	0.885	0.818	0.797	0.816
	(0.321)	(0.386)	(0.402)	(0.388)
Contraception	0.400	0.362	0.408	0.380
	(0.492)	(0.481)	(0.492)	(0.485)
Index	2.956	2.814	2.846	2.834
	(1.366)	(1.409)	(1.425)	(1.411)
Observations	112	3690	2178	5980

NOTES: Data on household decision making and sexual behavior is obtained from the Mexican Family Life Survey (MxFLS), which was conducted in 2002-2003, 2005-2006 and 2009-2012. In panel A, summary statistics of household decision making for women aged 15-44 are presented and for women above age 44 in panel B. Mean values are displayed with standard deviations in parentheses. Regressive states are those which ever had a regressive law change posterior to 2008.

Table 4: The Effect of the ILE Reform and Resulting Law Changes on log(Births)

	All Women			Teen-aged Women		
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Birth)	ln(Birth)	ln(Birth)	ln(Birth)	ln(Birth)	ln(Birth)
ILE Reform	-0.022**	-0.028	-0.038*	-0.053***	-0.058**	-0.070**
	[0.010]	[0.019]	[0.020]	[0.016]	[0.029]	[0.029]
Regressive Law Change	0.001	0.004	0.010	-0.007	0.001	0.013
	[0.006]	[0.008]	[0.009]	[0.009]	[0.011]	[0.012]
Constant	5.537***	0.080	-7.458	5.443***	-12.660	-31.098
	[0.016]	[12.536]	[19.697]	[0.021]	[16.900]	[26.589]
Observations	9600	9600	9600	1600	1600	1600
State and Year FEs	Y	Y	Y	Y	Y	Y
State Linear Trends		Y	Y		Y	Y
Time-Varying Controls			Y			Y

NOTES: Difference-in-differences estimates of the reform on rates of births are displayed. Standard errors clustered by state are presented in parentheses. All regressions are weighted by population of women of the relevant age group in each state and year. ***p-value<0.01, **p-value<0.05, *p-value<0.01.

Table 5: The Effect of the ILE Reform and Resulting Law Changes on log(Births) (Entropy Weighting)

	All Women			Teen-aged Women		
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Birth)	ln(Birth)	ln(Birth)	ln(Birth)	ln(Birth)	ln(Birth)
ILE Reform	-0.024*	-0.024	-0.032	-0.044***	-0.043	-0.052**
	[0.013]	[0.023]	[0.024]	[0.015]	[0.027]	[0.023]
Regressive Law Change	0.002	0.000	0.009	-0.018*	-0.008	0.010
	[0.009]	[0.012]	[0.011]	[0.010]	[0.014]	[0.014]
Constant	5.277***	-3.530	-19.594	5.342***	-20.413**	-43.749*
	[0.036]	[6.193]	[17.869]	[0.029]	[10.064]	[25.561]
Observations	9600	9600	9600	1600	1600	1600
State and Year FEs	Y	Y	Y	Y	Y	Y
State Linear Trends		Y	Y		Y	Y
Time-Varying Controls			Y			Y

NOTES: Specifications replicate those in table 1, however now using entropy re-weighting to balance pre-reform trends in births as described in Hainmueller (2012). Standard errors clustered by state are presented in parentheses. ***p-value<0.01, **p-value<0.05, *p-value<0.01.

Table 6: The Effect of the Abortion Reform on Reported Sexual Behaviour (Panel Specification)

	(1) Modern Contracep Knowledge	(2) Any Contraception	(3) Modern Contraception	(4) Num of Sex Partners
ILE Reform	0.002	-0.012	-0.013	-0.111
	(0.276)	(0.914)	(0.901)	(0.776)
	[0.693]	[0.933]	[0.993]	[0.993]
Regressive Law Change	-0.009	0.041	0.014	0.267
	(0.304)	(0.492)	(0.814)	(0.064)
	[0.600]	[0.760]	[0.833]	[0.220]
Observations	10007	10007	10007	10007
R-Squared	0.889	0.568	0.558	0.531
Mean of Dep Var	0.999	0.569	0.610	1.418

Each column presents a seperate regression of a contraceptive or sexual behaviour variable on abortion reform measures, house-hold fixed effects, year fixed effects and time-varying controls. In order to correct for Family Wise Error Rates from multiple hypothesis testing, we calculate Romano and Wolf (2005) p-values, using their Stepdown methods. Romano-Wolf p-values are presented in square brackets, and traditional (uncorrected) p-values are presented in round brackets. Significance stars refer to significance at 10% (*), 5% (**) or 1% (***) levels, and are based on Romano-Wolf p-values.

Table 7: The Effect of the Abortion Reform on Women's Empowerment in the Household

	Individual Elements					Index
	(1) Child Educ	(2) Child Health	(3) Expenditure	(4) Work	(5) Contracep	(6)
ILE Reform	0.139**	0.076	0.194	-0.001	0.066	0.474**
	(0.012)	(0.346)	(0.059)	(0.994)	(0.369)	(0.028)
	[0.047]	[0.740]	[0.213]	[0.993]	[0.587]	
Regressive Law Change	-0.071	-0.008	0.138	0.050	-0.039	0.071
	(0.128)	(0.809)	(0.022)	(0.355)	(0.503)	(0.619)
	[0.407]	[0.787]	[0.100]	[0.720]	[0.747]	
Observations	8175	8175	8175	8175	8175	8175
R-Squared	0.604	0.571	0.520	0.570	0.536	0.593
Mean of Dep Var	0.874	0.873	0.678	0.770	0.850	4.044

NOTES: Each column presents a seperate regression of an empowerment variable or the empowerment index including house-hold fixed effects, year fixed effects and time-varying controls. In order to correct for Family Wise Error Rates from multiple hypothesis testing, we calculate Romano and Wolf (2005) p-values, using their Stepdown methods. Romano-Wolf p-values are presented in square brackets, and traditional (uncorrected) p-values are presented in round brackets. Significance stars refer to significance at 10% (*), 5% (**) or 1% (***) levels, and are based on Romano-Wolf p-values.

Table 8: Placebo Test of the Effect of the Reform on Women's Empowerment (Women Aged 45+)

	Individual Elements					Index
	(1) Child Educ	(2) Child Health	(3) Expenditure	(4) Work	(5) Contracep	(6)
ILE Reform	0.053	-0.024	-0.334**	-0.057	0.083	-0.279
	(0.611)	(0.837)	(0.007)	(0.616)	(0.562)	(0.337)
	[0.953]	[0.847]	[0.027]	[0.827]	[0.960]	
Regressive Law Change	-0.098	-0.041	-0.171	0.013	0.118	-0.179
	(0.194)	(0.578)	(0.140)	(0.885)	(0.268)	(0.465)
	[0.533]	[0.820]	[0.500]	[0.900]	[0.547]	
Observations	5980	5980	5980	5980	5980	5980
R-Squared	0.674	0.683	0.540	0.529	0.607	0.676
Mean of Dep Var	0.463	0.497	0.668	0.791	0.380	2.799

NOTES: For full notes refer to table 7. Regression results presented here are estimated as in table 7, however now the sample consists of married women *above* fertile age (45 years and above).

Table 9: Identification Test of the Effect of the Reform on Women's Empowerment (Pre-Reform)

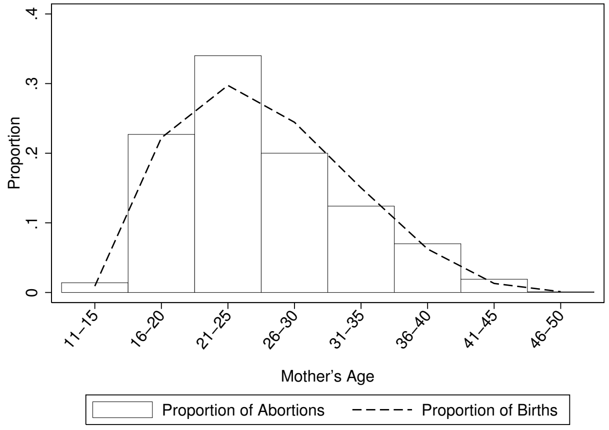
	Individual Elements					Index
	(1) Child Educ	(2) Child Health	(3) Expenditure	(4) Work	(5) Contracep	(6)
ILE Reform	-0.043	0.095	0.255	-0.299	-0.006	0.002
	(0.815)	(0.547)	(0.028)	(0.050)	(0.972)	(0.996)
	[0.973]	[0.907]	[0.153]	[0.180]	[0.960]	
Regressive Law Change	-0.008	0.016	0.077	-0.076	-0.112	-0.103
	(0.887)	(0.774)	(0.180)	(0.117)	(0.057)	(0.493)
	[0.900]	[0.940]	[0.473]	[0.387]	[0.267]	
Observations	3538	3538	3538	3538	3538	3538
R-Squared	0.768	0.783	0.708	0.676	0.722	0.768
Mean of Dep Var	0.507	0.546	0.668	0.782	0.381	2.883

NOTES: For full notes refer to table 7. This placebo test uses only the two pre-reform rounds, and defines as a placebo treatment group residents of Mexico D.F. in round two. A similar defintion is used to create the placebo Regressive Law Change group based on residents of regressive states, prior to the implementation of the reform.

Appendices

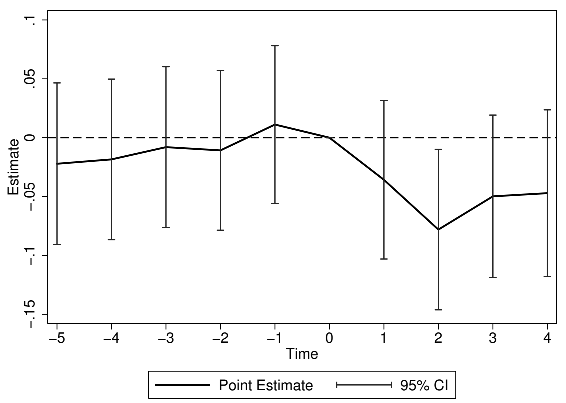
A1 Tables and figures

Figure A1: Birth and Abortion Descriptives: Mexico



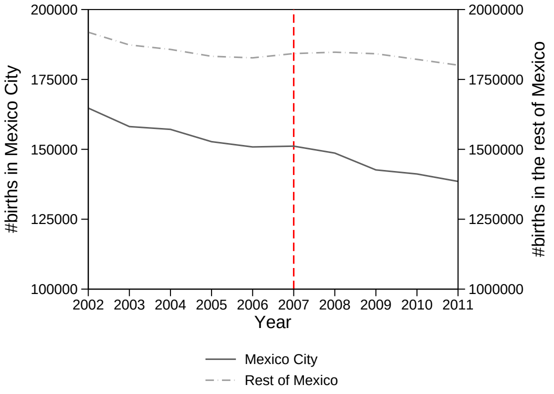
NOTES: Total births are plotted between 2002 and 2011. Abortions are plotted from the date of reform (April 26, 2007) until 2011. The total quantity of births is 23.2 million (all of Mexico), and total abortions are 69,861 (Mexico City only). Births are calculated from administrative data (INEGI) and abortions from administrative data (Secretary of Health, Mexico DF).

Figure A2: Event Study Estimates of ILE Reform (15-19 Year-olds)



NOTES: Event study estimates and confidence intervals interact the presence of legalized abortion with lags and leads. Each lag/lead is a yearly estimate, and year 0 (2007) is the omitted base year.

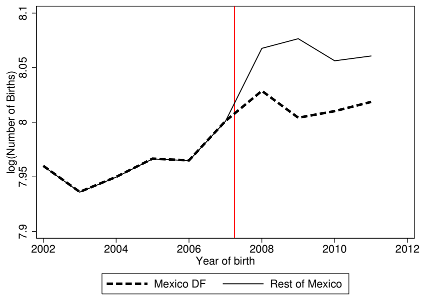
Figure A3: Crude Birth Trends: Mexico D.F. and Rest of Mexico



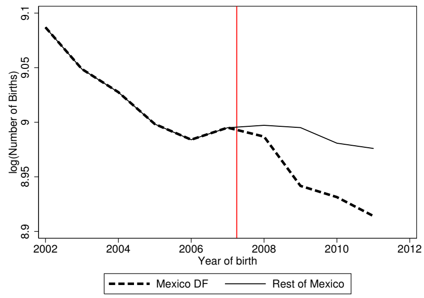
NOTES: Total births in each state are calculated from INEGI microdata registers based on state of residence of the mother.

Figure A4: Age-Specific Entropy-Weighted Trends in Births

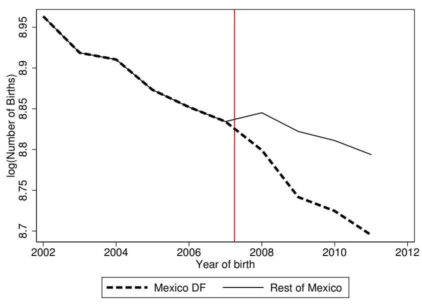
(a) 15-18 Year-olds



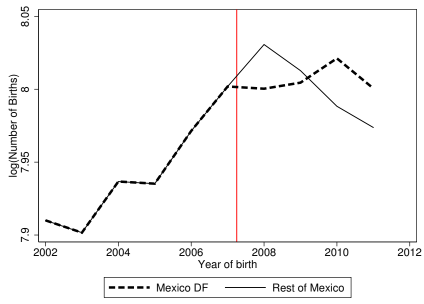
(b) 19-24 Year-olds



(c) 25-34 Year-olds



(d) 35-39 Year-olds



NOTES: Refer to notes to figure 3. Entropy weights are calculated use pre-reform birth trends for the age group in each particular figure only.

Table A1: Summary Statistics, Reproductive health, MxFLS

	(1) Mexico City	(2) Regressive States	(3) Rest of Mexico	(4) Full Country
	Women aged 15-44			
Knowledge of contraceptives	0.993	0.996	1.000	0.997
	(0.084)	(0.061)	(0.011)	(0.051)
Use modern method	0.653	0.565	0.565	0.570
	(0.477)	(0.496)	(0.496)	(0.495)
Use any method	0.661	0.610	0.602	0.610
	(0.474)	(0.488)	(0.489)	(0.488)
Number of sex partners	1.767	1.392	1.453	1.437
	(1.474)	(1.225)	(1.335)	(1.286)
Observations	226	5758	4023	10007

NOTES: Data on household decision making and sexual behavior is obtained from the Mexican Family Life Survey (MxFLS), which was conducted in 2002-2003, 2005-2006 and 2009-2012. Mean values are displayed with standard deviations in parentheses. Regressive states are those which ever had a regressive law change posterior to 2008.

Table A2: Unweighed Estimates of the Effect of Reforms on log(Births)

	All Women			Teen-aged Women		
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Birth)	ln(Birth)	ln(Birth)	ln(Birth)	ln(Birth)	ln(Birth)
ILE Reform	-0.034*	-0.029	-0.038	-0.062***	-0.054	-0.067
	[0.018]	[0.033]	[0.034]	[0.024]	[0.043]	[0.043]
Regressive Law Change	-0.012*	-0.008	-0.004	-0.021**	-0.020*	-0.010
	[0.007]	[0.009]	[0.009]	[0.009]	[0.011]	[0.011]
Constant	5.603***	-1.954	-14.568	5.458***	-12.332	-44.087**
	[0.013]	[8.298]	[17.004]	[0.014]	[10.628]	[21.644]
Observations	9600	9600	9600	1600	1600	1600
State and Year FEs	Y	Y	Y	Y	Y	Y
State Linear Trends		Y	Y		Y	Y
Time-Varying Controls			Y			Y

NOTES: Regressions replicate table 4, however using unweighted age by state by year cell. ***p-value<0.01, **p-value<0.05, *p-value<0.01.

Table A3: Replicating Fertility Results with Wild Cluster Bootstrapping

	All Women			Teen-aged Women		
	(1) ln(Birth)	(2) ln(Birth)	(3) ln(Birth)	(4) ln(Birth)	(5) ln(Birth)	(6) ln(Birth)
ILE Reform	-0.034‡‡	-0.029‡‡	-0.038‡‡	-0.062‡‡	-0.054‡‡	-0.067‡‡
	[-0.061,-0.012]	[-0.039,-0.021]	[-0.051,-0.028]	[-0.087,-0.037]	[-0.065,-0.042]	[-0.082,-0.054]
Regressive Law Change	-0.012	-0.008	-0.004	-0.021	-0.020	-0.010
	[-0.047,0.018]	[-0.024,0.007]	[-0.016,0.009]	[-0.058,0.016]	[-0.049,0.007]	[-0.033,0.014]
Observations	9600	9600	9600	1600	1600	1600
State and Year FEs	Y	Y	Y	Y	Y	Y
State Linear Trends		Y	Y		Y	Y
Time-Varying Controls			Y			Y

NOTES: Results replicate unweighted difference-in-difference estimates of the effect of reforms on rates of birth, however now using wild bootstrapped standard errors in place of analytical standard errors clustered at the level of the state. Point estimates are presented, along with 95% confidence intervals of these estimates in parentheses. ‡‡ Significant at the 95% level.

Table A4: The Effect of Abortion Reform on log(Births) by Age

	(1) Ages 15-19	(2) Ages 20-24	(3) Ages 25-29	(4) Ages 30-34	(5) Ages 35-39	(6) Ages 40-44
ILE Reform	-0.070**	-0.013	0.011	-0.062***	-0.047*	-0.036
	[0.029]	[0.012]	[0.013]	[0.016]	[0.024]	[0.041]
Regressive Law Change	0.013	0.009*	0.011*	0.023***	-0.002	0.004
	[0.012]	[0.005]	[0.006]	[0.007]	[0.011]	[0.019]
Constant	-31.098	19.381*	-6.635	10.290	-8.623	-37.297
	[26.589]	[11.712]	[13.172]	[16.744]	[25.512]	[43.923]
Observations	1600	1600	1600	1600	1600	1600

NOTES: All specifications include age, state and year fixed effects state-specific linear trends, and time varying controls (ie, the specification in columns (3) and (6) of table 4.) Standard errors clustered by state are presented in parentheses. All regressions are weighted by population of women of the relevant age group in each state and year. ***p-value<0.01, **p-value<0.05, *p-value<0.01.

Table A5: Replication Results Using Birth Rates instead of log(Birhts)

	(1) Ages 15-19	(2) Ages 20-24	(3) Ages 25-29	(4) Ages 30-34	(5) Ages 35-39	(6) Ages 40-44	(7) All Ages
ILE Reform	-3.942*	-2.380	0.375	-3.975***	-2.525**	-0.220	-2.334*
	[2.132]	[1.723]	[1.558]	[1.371]	[0.988]	[0.624]	[1.252]
Regressive Law Change	0.912	1.479**	1.004	1.666***	0.176	0.077	0.936*
	[0.858]	[0.728]	[0.683]	[0.613]	[0.446]	[0.286]	[0.544]
Observations	1600	1600	1600	1600	1600	1600	9600

NOTES: Regressions results using rates of birth are displayed. All specifications include age, state and year fixed effects, state-specific linear trends, and time varying controls (ie,the specification in columns (3) and (6) of table 4.) Standard errors clustered by state are presented in parentheses. All regressions are weighted by population of women of the relevant age group in each state and year. ***p-value<0.01, **p-value<0.05, *p-value<0.01.

Table A6: The Effect of the Abortion Reform on Reported Sexual Behavior (Repeated Cross-Section Specification)

	(1) Modern Contracep Knowledge	(2) Any Contraception	(3) Modern Contraception	(4) Num of Sex Partners
ILE Reform	-0.011	-0.050	-0.057	-0.111
	(0.513)	(0.579)	(0.520)	(0.675)
	[0.967]	[0.773]	[0.873]	[0.693]
Regressive Law Change	-0.002	0.093**	0.065	0.150
	(0.815)	(0.008)	(0.065)	(0.106)
	[0.873]	[0.020]	[0.233]	[0.213]
Observations	10007	10007	10007	10007
R-Squared	0.037	0.027	0.029	0.033
Mean of Dep Var	0.999	0.569	0.610	1.418

NOTES: Each column presents a seperate regression of a contraceptive or sexual behaviour variable on abortion reform measures, year fixed effects and time-varying controls. In order to correct for Family Wise Error Rates from multiple hypothesis testing, we calculate Romano and Wolf (2005) p-values, using their Stepdown methods. Romano-Wolf p-values are presented in square brackets, and traditional (uncorrected) p-values are presented in round brackets. Significance stars refer to significance at 10% (*), 5% (**) or 1% (***) levels, and are based on Romano-Wolf p-values.

A2 Correction for FWER using Romano and Wolf's stepdown procedure

We are interested in testing K hypotheses regarding the effect of the reforms on particular indicators. As we are running multiple hypothesis tests, the probability of falsely rejecting a null given that it is true is high. If we set the accepted type I error rate for each individual hypothesis as α, the likelihood of rejecting at least one hypothesis incorrectly would be equal to $1-(1-\alpha)^K$ (assuming independent hypotheses). For a type I error rate of $\alpha = 0.05$ per individual hypothesis and $K = 5$ hypotheses, the likelihood of falsely rejecting at least 1 null is thus equal to $\alpha_K = 0.226$.

In order to proceed with testing we thus aim to fix the Family Wise Error Rate (FWER), rather than the probability of type I errors for each hypothesis individually. This FWER is the probability of making at least one type I error in the family of K hypotheses, and we would like to fix this value at $\alpha_K = 0.05$. The classical multiple hypothesis correction of Bonferroni (1935) suggests simply adjusting a constant correction to inflate all p-values associated with each of the K tests, however as is well-known, this testing procedure is overly conservative, resulting in low power (Romano, J. P. and Wolf, M., 2005). A more powerful series of tests which both fix the FWER and have greater power are step-down methods, first proposed by Holm (1979). We follow a step-wise testing procedure which is more powerful in terms of type II errors than classical multiple hypothesis testing procedures given that it accounts for dependence between hypothesis tests. This stepdown procedure from Romano and Wolf (2005), is being increasingly used in empirical economics, see for example Savelyev and Tan (2015).

We implement the "Studentized StepM Method" described in Romano, J. P. and Wolf, M. (2005) (p. 1252). Specifically, we proceed following the steps below, where the computationally intensive steps 1 and 2 need only be estimated once. We have released along with this paper a program (rwolf) and documentation which performs these calculations in Stata.

1. Estimate the K models associated with each of the K hypotheses and calculate the t-statistics associated with each hypothesis as $t_k = (\widehat{\beta}_k - \beta_k^0)/se(\widehat{\beta}_k)$. Rank the absolute value of the t_k-statistics, and take the highest t-statistic to indicate the variable of interest for testing

2. Estimate $B = 150$ bootstrap replications of each of the K models, storing the t-statistic associated with each of the K tests for each of the B trials, resulting in $t_{k,b}$ t-statistics. Also calculate the (bootstrap) standard error for each variable using the distribution of parameters across each of the B bootstrap samples for a particular k.

3. For the variable of interest for testing, form the null distribution of t-statistics by taking the maximum t-statistic for each of the B bootstrap replications among all of the potential donor variables. The null distribution is defined as $t_k^{null} = |(\max(t) - \overline{\max(t)})/se(\widehat{\beta}_k)|$

4. Calculate the Romano Wolf p-value by comparing t_k from step one with t_k^{null} from step 3. Store this p-value as the p-value corresponding to this variable.

5. Remove this variable from the list of variables to test, and remove the bootstrap replications associated with this variable from the pool of t-values for the null distribution. The variable with the next highest t-statistic from 1 now becomes the variable of interest for testing, and the donor variables consist of this and the remaining variables to be tested. If there remain variables to test, return to step 3. Otherwise, end.

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