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ABSTRACT

The Effects of Incentivizing Early Prenatal Care on Infant Health*

We investigated the effects of the timing of early prenatal care on infant health by exploiting a reform that required expectant mothers to initiate prenatal care during the first ten weeks of gestation to obtain a one-time monetary transfer paid after childbirth. Applying a difference-in-differences design to individual-level data on the population of births and fetal deaths, we identified small but statistically significant positive effects of the policy on neonatal health. We further provide suggestive evidence that improved maternal health-related knowledge and behaviors during pregnancy are plausible channels through which the reform might have affected fetal health.

JEL Classification: I120, I180, J130

Keywords: prenatal care, neonatal health, conditional cash transfers, prenatal care timing

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1. Introduction

Prenatal care is a central component of public policy since it targets both maternal and child health. Its main goals include improving maternal health knowledge, preventive monitoring of pregnancy, and early diagnosis of infections and fetal abnormalities, as well as their potential treatment. Specific regulations regarding the quality, content, and timing of medical care during pregnancy differ considerably across countries (Bernloehr et al., 2005); however, there seems to be a consensus that prenatal care should begin as early as possible (Alwan et al., 2016). For example, the World Health Organization (2016) recommends its initiation at the latest in the 12th week of gestation, arguing that the first trimester of pregnancy lays the foundations for a child's future development. Specifically, visits during the first trimester have long been endorsed as a means to prevent problems that may occur later in the pregnancy and to advise expectant mothers on beneficial behaviors, such as eliminating smoking and alcohol (Alexander and Korenbrot, 1995; Currie and Grogger, 2002). However, to date, we do not know much about the causal link between the exact timing of prenatal care and child health and in particular, about shifting its initiation to earlier in pregnancy.

Most economists who study the link between prenatal care and birth outcomes draw on the theoretical framework by Grossman (1972), where prenatal care enters the "infant health production function" simultaneously with maternal health endowments, behaviors, and other inputs. Thus, a key empirical challenge is the potential endogeneity of the demand for prenatal care, which is likely correlated with unobserved maternal characteristics and other parental inputs that simultaneously affect infant health. Early empirical research addressed this endogeneity problem and estimated the production functions using a two-stage approach that used a set of individual-level and local-area instrumental variables (IV), such as goods prices, health infrastructure, public expenditures, and labor market conditions (e.g., Rosenzweig and Schultz, 1983 or Grossman and Joyce, 1990). These studies generally find that prenatal care moderately improves neonatal health. More recent literature has increasingly used quasi-experimental designs that exploit sources of exogenous variation from changes in Medicaid policies (e.g., Joyce 1999; Gray, 2001; Currie and Grogger, 2002) or bus strikes (Evans and Lien, 2005). These studies focus primarily on socioeconomically disadvantaged mothers and the effects of extended access to prenatal care and its utilization, rather than the timing of its commencement. Evans and Lien (2005) provide suggestive evidence that a loss of a prenatal visit early on in the pregnancy is more detrimental than visits lost later on, but their delineation happens only at the 6th month of pregnancy. Generally, the literature is silent on the possible gains from shifting prenatal care initiation to much earlier stages in the pregnancy. Furthermore, there is little causal evidence on the effects of prenatal care or its timing from outside the U.S., in particular for countries with universal health care insurance systems that provide all mothers eligibility for free prenatal care.[1]

Recent literature reviews support these notions and conclude that we still know relatively little about the causal effect of prenatal care on child outcomes, especially the role of its timing (Almond and Currie, 2011; Currie and Rossin-Slater, 2015; Corman et al., 2019). On the other hand, prenatal care should be viewed as one of the earliest investments in a child's development, and growing evidence documents that health status at birth has long-term consequences for socioeconomic outcomes, such as education (Almond and Currie, 2011; Figlio et al., 2014), employment and wages (Black et al., 2007), mortality (Bharadwaj et al., 2017), and other adult outcomes (Almond et al., 2018). The question of whether the timing of prenatal care causally affects neonatal health becomes particularly important in light of self-productivity of human capital and dynamic complementarities of skills (Cunha and Heckman, 2007).

From a policy perspective, parallel to a discussion about the effects of prenatal care is the question of whether and how governments can encourage mothers to begin care as early as possible. Two commonly suggested policy tools that could help to promote such behavior are information and monetary incentives. In the former case, governments or NGOs provide information to the public in the interest of changing a suboptimal behavior. For example, it has been shown that such interventions can be successful in educational (Wiswall and Zafar, 2015) and health (Keskin et al., 2017) decisions. Providing only information or guidance, however, may not be sufficient since its reception and understanding may vary across different educational or socioeconomic groups (de Walque, 2010; Lange, 2011), or it simply may not induce the expected behavioral change (Prina and Royer, 2014).[2] Monetary incentives, in turn, have gained popularity in the developing world as an effective policy tool (Lagarde et al., 2007), and more recently, they have started being employed in developed countries (Fryer, 2017).[3] Their main premise is a contract between a principal (government) and the agent (recipient) that payment will be made upon fulfilling a priori specified conditions.

[1] There is also some evidence on the effectiveness of prenatal care access in lower income countries e.g., Gajate-Garrido (2013) for Philippines or Gonzalez and Kumar (2018) for Mexico. In the context of the U.S., Yan (2020) shows that within-sibling variation in prenatal care utilization is predictive of birth outcomes. However, none of these studies focus on the timing of prenatal care.
[2] Buckles and Guldi (2017) suggest that information can be effective if it is provided to doctors rather than patients.
[3] Related more directly to birth outcomes, there is evidence from variation in taxation and benefits availability that expectant mothers respond to financial incentives when it comes to scheduling births (e.g., Dickert-Conlin and Chandra, 1999; Gans and Leigh, 2009; Tamm, 2012; Schulkind and Shapiro, 2014; Lalumia et al., 2015; Brunner and Kuhn, 2015; Borra et al., 2019).

In this paper, we evaluate the effects of a novel policy implemented in 2009 in Poland, which directly speaks to the questions of whether the timing of prenatal care matters and whether mothers respond to financial incentives. Specifically, the reform required mothers to obtain a medical certificate of having the first prenatal visit by the 10th week of gestation in order to be entitled to a one-time cash transfer paid after childbirth. Before the reform, no such certification was required, making the benefit unconditional. Given the universal coverage by public health insurance, the reform did not affect the eligibility for or the access to prenatal care itself. Rather, for many mothers, it effectively shifted the timing of their first prenatal visit towards earlier in the pregnancy. Polish parliament passed the legislation on 6th December 2008 but the law was only effective for births on or after 1st November 2009. This unique hold-up in policy implementation allows us to separately identify the effects of providing unconditional public information versus a joint effect of monetary incentive and public information about the benefits of early initiation of prenatal care.

To estimate effects of the reform on infant health, we used data on all births and fetal deaths in Poland between 2006 and 2011 paired with a difference-in-differences identification strategy. In that, we compare mothers whose conceptions happened shortly before and shortly after the policy-relevant cutoffs to mothers who conceived in the same weeks but in nonreform years. Overall, we observed statistically significant positive effects of the reform on neonatal outcomes, such as decreased fetal deaths and increased birth weight. On the other hand, we did not identify economically meaningful effects on gestational age. We show that these results are not driven by potential selection issues and that the estimates are remarkably robust to alternative econometric specifications and sample restrictions. Due to the sequential nature of the legislation, we further document that financial conditionality is essential to generating the observed gains in neonatal health. Using auxiliary survey data, we also document postreform improvements in maternal knowledge and behaviors related to drinking and smoking during pregnancy, which are consistent with the observed gains in neonatal health.

These findings contribute to the existing literature in multiple ways. First, we show that the timing of prenatal care, even relatively early in pregnancy, causally affects infant health. This is different from prior quasi-experimental work that focused on access to prenatal care and its utilization per se rather than the timing of its initiation. Thus, our results provide a careful population-level validation of the prevalent medical recommendations (e.g., by the WHO). Second, we document that health improvements from prenatal care can be achieved by introducing conditionality to already existing universal family benefits. In that, our policy in question is fiscally neutral and does not require any additional funding. Moreover, we show that public information stemming from the passage of the reform is itself insufficient to affect birth outcomes, and conditionality of the cash transfer is a critical piece of the legislation that leads to improvements in neonatal health. To the best of our knowledge, this distinction between public information and conditional cash transfer has not been previously studied in the context of prenatal care and infant health. Finally, by exploring the potential channels behind the uncovered benefits of early prenatal care, we found improvements in maternal health-related knowledge and less hazardous behaviors during the pregnancy as plausible mechanisms.

2. INSTITUTIONS

2.1 THE HEALTH CARE SYSTEM AND PRENATAL CARE IN POLAND

According to the Polish Constitution, everyone has a right to have access to health care and citizens are granted equal access to the publicly funded health care system that is based on a mandatory insurance scheme.[4] Premiums are deducted from personal income, while those who do not work, irrespective of the reason, are fully subsidized by the state. Doctors working in the public health care system are state employees, and their wages are to a large degree centrally set. Medical education is provided free of charge, although the number of available slots for each cohort is limited. The majority of doctors work simultaneously in the public system and run their own private practices, where patients pay out of pocket. In parallel, a limited number of private health care centers provide services almost exclusively to patients having an add-on private insurance. Due to universal coverage by the mandatory public health care insurance and an insufficient supply of doctors, in particular specialists, rationing of care in the public sector occurs through queuing and gatekeeping. Importantly, from the perspective of access to prenatal care, during the study period, a referral from a primary care doctor was not needed to visit an OB/GYN specialist.

The excess demand in the public sector creates market space for private health care provisions. The private market has steadily grown from approximately 30 to 35 percent, as a fraction of total health care expenditures between 2003 and 2010. For comparison, during the same period, the total health care expenditure more than doubled (Łysoń, 2012). The private health care system comprises primarily services paid out-of-pocket, which are theoretically available to everyone (approximately 70 percent of private health care spending in 2010), and private insurance add-ons, which refer to employer-sponsored insurance and are mostly limited to employees of large private firms (approximately 20 percent of private health care spending

[4] Additional details on the Polish health care system are provided in Girouard and Imai (2000) or Nieszporska (2017).

in 2010). According to survey data from March 2009, less than 10 percent of households had an add-on private insurance, but approximately 54 percent reported some out-of-pocket expenditures on health care services during the past three months. The average monthly spending of 175 PLN (approximately 55 USD at that time) accounted for approximately 6.4 percent of the net household income (Diagnoza Społeczna, 2009).[5] Even though the private health care market has been growing during our study period, it was still uncommon to utilize it for more complex medical procedures (e.g., birth or cardiac surgery), and its primary focus was on consultations with specialists, dental treatments, tests, and specialized diagnostics not covered or heavily rationed by the public system.

The standards of health care services are set and monitored by the Ministry of Health. In particular, prenatal care standards relevant for our study were issued in 2004 (Dziennik Ustaw, 2004) and stipulated eight routine consultations with a doctor or a midwife. Each of the visits included monitoring of maternal weight gain, blood pressure, general and gynecological examination, pregnancy risk assessment, and promotion of a healthy lifestyle. In addition, these standards required several basic blood morphology tests, five fetal heart function assessments, three ultrasound screenings, and a test for gestational diabetes. The first visit was recommended to occur during the first ten weeks of gestation. Generally, care during delivery and the postpartum period is also of high quality, and almost all births (99.4 percent in 2008) are delivered in a hospital and attended by skilled healthcare personnel (UNICEF, 2020). Comparing information on deliveries in publicly funded clinics in 2009 (earlier data are not available) with the number of births from the Central Statistical Office for the same year implies that nearly 90 percent of deliveries occur within the public healthcare system (NFZ, 2020) and the remaining 10 percent in private hospitals. The same data reveal that in 2009, 27 percent of births in public hospitals were delivered via caesarian section, and the median duration of a birth-related hospital stay was four days.

According to UNICEF indicators (UN IGME, 2019), the neonatal mortality rate in Poland in 2010 was 3.6 per 1000 live births, which was somewhat higher compared to other developed Western European countries, e.g., U.K. (3.0), Germany (2.3), Spain (2.1) or Sweden (1.6), but lower than in the U.S. (4.0) or Russia (4.5). Likewise, the same data suggest that the mortality of children aged 5-14 does not depart from averages observed in other developed countries. Therefore, Poland should be viewed as a developed, middle-income country with a

[5] The Diagnoza Społeczna ("Social Diagnosis") is a large biannual survey of a representative sample of the Polish population. Recently, the data have been used e.g., by Becker et al. (2020).

high quality of health care, and thus our results could be generalizable to similar countries with publicly funded health care systems.

2.2 "Baby Bonus" and the Introduction of a Prenatal Care Requirement

In February 2006, the Polish government introduced a new one-time post-birth monetary transfer, which aimed at reversing a historically negative fertility trend. The payment amounted to 1,000 PLN (ca. 318 USD or 262 EUR at the time), which was substantial and constituted approximately 40% of average gross monthly earnings. This "baby bonus" was paid upon application and eligibility was universal, i.e., independent of family income or any other conditions. For low-income parents, there was an independent means-tested allowance of an additional 1,000 PLN. In July 2007, the universal "baby bonus" was subject to slight modifications, which extended the eligibility to adoptive parents and prolonged the application period from 3 to 12 months after birth or adoption.

A substantial change was subsequently passed on 6th December 2008 to promote the early utilization of prenatal care. The new law stipulated that parents of newborns who applied for the "baby bonus" on or after 1st November 2009 needed to document that a mother's first prenatal visit was at the latest during the 10th week of gestation. The law defined a prenatal visit as a preventive consultation by a primary health care physician, a specialist in obstetrics/gynecology, or a hospital treatment during pregnancy.

The reform effectively introduced a financial penalty for not using early prenatal care and was motivated by an insufficient utilization of health services early in pregnancy, as well as by poor knowledge about healthy behaviors during pregnancy. For example, in the mid-2000s only 58.6 percent of women in urban and 46.0 percent in rural areas had their first prenatal visit during the first trimester of pregnancy (Kornas-Biela, 2012). These early utilization rates did not seem to improve over timer since in 1997 they likewise were at 60.4 percent and 44.3 percent in urban and rural areas, respectively (Druki Sejmowe, 1998). Moreover, about one-third of new mothers declared that they smoked cigarettes or drank alcohol during pregnancy (Kornas-Biela, 2012). For comparison, since the 1990s, the trend in early prenatal care utilization was increasing in the U.S., and in 2003, almost 85% of births were to women who received first-trimester care (Corman et al., 2019), and most European countries had first-trimester rates of above 90% in the early 2000s (EURO-PERISTAT, 2008).[6]

[6] In 2004, the rates of first trimester prenatal care were as follows: Germany - 93.9%, France - 95%, Italy - 94.5%, Finland - 95.9%, and Sweden - 91.5%. Among countries of the former Soviet Bloc these numbers were: Czech Republic - 92.5%, Latvia - 91.8%, Lithuania - 74.5%, and the Slovak Republic - 79.5%.

The 10th-week requirement was passed unexpectedly as part of a law package that primarily focused on future maternity leave regulations.[7] As an approximation for the public interest, as well as information about its prenatal care component and changed eligibility criteria, Figure A1 in the Appendix illustrates the development of the Google search index for "baby bonus" and "week of pregnancy" in Poland. The two vertical solid lines correspond to December 2008 and November 2009 – months when the reform was signed into law and implemented, respectively. The times series depicts no substantial increase in the search volume for "baby bonus" after the law passage in December 2008. Conversely, the searches for "week of pregnancy" exhibit a sharp uptick right after the announcement of the new regulation.

Although the 10th-week threshold was linked to standards of prenatal care already issued by the Ministry of Health in 2004 (Dziennik Ustaw, 2004), it became a controversial regulation from the beginning and was often criticized as too restrictive. One particular concern was related to the ability of the publicly financed, free of charge, health care system to cope with an expected sudden increase in the demand for early prenatal visits. Indeed, aggregate data do not exhibit any notable changes in the number of general practitioners or gynecologists and obstetricians under contract to the public health care system in 2008 or 2009 (see Figure A2 in the Appendix). Thus, opponents argued that the reform could prolong waiting lines within the public system, which would prevent some mothers from obtaining the required certificate on time. To alleviate these concerns, the law permitted the qualifying visit to be with a primary health care provider, thus relieving excess demand for specialist visits within the public system. Irrespective of the provider, however, capacity constraints in the timely access to the first doctor visit could have been particularity relevant for low-income mothers who were unable to afford any out-of-pocket medical expenses.[8] Otherwise, longer waiting periods in the public system would induce women to initiate the required early prenatal care in a private practice and to switch to a public health care facility as soon as possible.

After a few sensational media reports on mothers who lost the "baby bonus" due to the reform, on 5th March 2010, parliament passed an amendment to the legislation relaxing the eligibility criteria for several months. Specifically, between 31st March 2010 and 31st December 2011, a medical certificate of at least one prenatal visit with a medical doctor or a midwife, irrespective of its timing, was sufficient to be eligible for the "baby bonus". Furthermore,

[7] Specifically, the law passed on 6th December 2008 regulated an extension of maternity leave duration of 20 weeks for singleton births by two additional weeks starting from 1st January 2010 and two further weeks starting from 1st January 2012. More specific regulations applied to multiple births. The law also enacted parental leave reserved for fathers of one week starting from 1st January 2010 and two weeks starting from 1st January 2012.

[8] Approximate cost of a private visit during this time period was 80 to 100 PLN (36 to 45 USD at that time) and thus less than 10% of the "baby bonus".

mothers who lost eligibility between 1st November 2009 and 30th March 2010 due to a lack of the required medical certificate could reapply for the "baby bonus". In January 2012, the tighter criterion of the first prenatal visit being at the latest in the 10th gestational week was reinstated. Finally, in 2013, a major reform abolished the universality of the "baby bonus" by introducing an income threshold for eligibility, which suppressed the share of eligible families to approximately 75%. Irrespective of this change, the payment of the "baby bonus" remained conditional on the first prenatal visit occurring by the 10th week of gestation at the latest. Our focus in this paper is on the law change passed on 6th December 2008 and implemented on 1st November 2009, which was intended to promote early utilization of prenatal care.

While the policymakers motivated the 10th-week threshold with a late initiation of early prenatal care, there are only limited data on the timing of the first prenatal care visit in Poland. Unlike in the U.S. natality files, Polish birth certificates do not include any information on the timing of prenatal care initiation. Therefore, it is not possible to estimate by how many weeks expectant mothers potentially sped up their first visit due to the reform. To shed some light on the question to what extent pregnant women actually complied with the new regulation, we needed to rely on aggregate statistics on first-trimester initiation rates (i.e., up to 14 completed weeks of gestation) from other sources. Figure 1 summarizes all available information from the publicly funded health care system. The data show that before the reform, on average, only 54% of expectant mothers initiated prenatal care during the first trimester of pregnancy and that this share was higher in urban than in rural areas. In contrast, in 2012, the average early initiation rate reached 79%, suggesting an increase of approximately 25 percentage points after the reform. It is important, however, to keep in mind that these numbers do not cover visits with private health care providers, which account for approximately 20 percent of prenatal care visits in the first trimester.[9]

An indirect source of information on prenatal care utilization in the postreform period in both the public and private sector is the take-up rate of the "baby bonus". Figure A3 presents official governmental statistics on this issue. Panel A reports the annual number of live births and bonus payments made, and panel B documents the ratio of paid allowances to births in a

[9] There are no data on private visits, but the almost universal coverage rate of government payments after introduction of the conditionality in 2009 depicted in Figure A3 paired with approximately 80 percent early prenatal care utilization in the public insurance sector depicted in Figure 1 leads us to believe that about 20 percent of women used private providers for their first prenatal visit after the reform. As we note in Section 2.1, at the same time, approximately 10% of households had an add-on private insurance, which fits the evidence that, in 2009, nearly 10% of women gave birth in private clinics. These women would also most likely use private providers for prenatal care from the beginning of the pregnancy. The remaining 10% needed to pay out of pocked for each private prenatal visit and most likely switched to the public system as soon as possible, at the latest for delivery.

given year. Not surprisingly, between 2006 and 2008, the take-up rates of the unconditional transfer were close to 100 percent.[10] The coverage rate, however, remained remarkably stable in 2009 and 2010, when the eligibility was tied to a medical certificate of prenatal care. The decrease from 99.5 percent in 2008 to 99.1 percent in 2009 corresponds to approximately 1700 mothers who might have lost the benefit due to stricter legislation but in 2010, the coverage reached 99.9 percent again.[11] Based on this evidence, we conclude that the vast majority of mothers complied with the 10th-week threshold and that the utilization of early prenatal care reached almost 100 percent during the postreform period.

3. DATA

We used individual-level data from the Polish natality files for the years 2006-2011. The Central Statistics Office of Poland collects information on all of the approximately 400,000 live births and fetal deaths that occur in Poland each year from the original birth certificates. Given that fetal deaths are tracked after 20 weeks of gestation, the birth registry covers pregnancies that lasted at least 20 weeks.[12] The data files contain detailed demographic information on the mother, such as age at delivery, place of residence, number of previous births, marital status, education, and source of income. Children's characteristics comprise birth date, birth weight, gestational length, and gender, but, unlike for example, U.S. birth records, there is no information on prenatal care utilization in the data. Importantly though, we do observe the exact day of birth and completed weeks of gestation, so that we can estimate the day of conception with a weekly accuracy. We first compute the actual number of completed weeks spent by a child in utero by subtracting two weeks from the gestation length because it is measured from the first day of a woman's last menses and not from the day of conception, which typically

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[10] In 2006, the coverage rate exceeded 100% because upon its introduction, the benefit was paid retrospectively to parents of children born after 9th November 2005. Likewise, the excess rate in 2011 reflects the retrospective payments to mothers who initially lost eligibility in the preceding years and after relaxing the criteria, could re-apply for the benefit.

[11] The sharp decline in utilization observed between 2012 and 2013 reflects an introduction of income eligibility criteria for "baby bonus", thus changing it from universal to means tested.

[12] We were unable to observe pregnancies that terminated earlier either by a spontaneous or induced abortion. Early spontaneous abortion is associated with congenial defects rather than with socioeconomic factors (e.g. Currie and Grogger, 2002). In Poland, since 2000, their annual number was estimated at around 41,000, which translates to about 11% of pregnancies (UNDP, 2007). Induced abortions are generally prohibited in Poland, except in three cases: if the pregnancy is a result of rape or incest, if the pregnancy threatens a woman's life, or if the fetus is irreparably damaged. Even then, an abortion needs to occur no later than in the 12th week of gestation, and a doctor might refuse to perform an abortion if it is not reconcilable with their conscience. Consequently, the number of registered abortions in Poland is negligible. For example, there were 499 induced abortions in 2008 (BIP, 2017), compared to 416,437 births (0.1% of births). International organizations estimate the number of illegal abortions in Poland in a range of 1.5-5% of births (Zięba, 2006), but there are no reliable numbers on this issue. Nevertheless, various validity tests that we performed in Section 4.2 do not support the idea that selective abortion might be a threat to our empirical strategy. Furthermore, Figure A4 shows that official statistics on abortions and miscarriages do not exhibit any discontinuous patterns in years when the 10th week threshold was binding (i.e., 2009 and 2010).

occurs two weeks later. Then, we convert the period in utero into days (i.e., multiply the weeks by seven) and subtract it from a child's birth date to calculate the day of conception.[13]

Our outcomes of interest captured different aspects of neonatal health. Specifically, we considered a child's birth weight, which is a common measure of the underlying health of newborns and is predictive of long-term health and economic outcomes (Almond and Currie, 2011). To ease the interpretation, we calculated a natural logarithm of birth weight (in grams). Following previous studies, we also included an indicator for low birth weight (less than 2500 grams) as a separate outcome since it is a leading risk factor for infant mortality, and those infants who survive are at elevated risk for many long-term health conditions and developmental disabilities (Corman et al., 2019). Babies with a low birth weight also tend to be born prematurely, which motivates us to consider gestational age (in weeks) as an additional measure of health at birth. Furthermore, given that early prenatal care might decrease the risk of fetal death (Currie and Grogger 2002), we likewise examined this margin of neonatal health. To address concerns of multiple hypothesis testing and to increase statistical power, we also estimated a summary index of neonatal health, which was constructed using the first component of a principal components analysis (PCA) based on the following variables: an indicator for fetal death, birth weight in grams, and gestational age in weeks. We document its details in Table A1 in the Appendix.[14]

We restricted our sample to singleton births (97.5%) as multiple births are born at lower birth weight for reasons unrelated to prenatal care (e.g., Figlio et al., 2014).[15] Furthermore, we focused on mothers between 15 and 50 years old at birth (99.99%) and omitted a small group (0.14%) of foreign residents. Finally, given the reform's timing, we zoomed in on children conceived at most up to four weeks before and up to four weeks after 6th December 2008 or 8th February 2009, and exactly the same weeks in the surrounding years 2005/6 – 2009/10.

Table 1 shows the summary statistics for our primary estimation samples, which we explain in further detail in Section 4. For comparison, column 1 shows the means for all live and stillbirths in Poland between 2006 and 2011. Panel A presents maternal background characteristics, and in the full sample, they are on average approximately 27 years old at the time of conception and for approximately 50 percent of them, it is the first birth. The vast

[13] Consequently, we assume that a delivery occurred exactly on the next day following the last day of the reported week of gestation, but our results are substantively unchanged if we alternatively assume a mid-week delivery in the following gestation week (i.e., if we add three days to the period in utero measured in days). We present these results in Section 7.

[14] We obtained nearly identical results when we alternatively constructed a summary health index by aggregating the z-scores of the three outcomes as e.g., Kling et al. (2007).

[15] Our results were nearly identical when we expanded the sample to include multiple births, and the policy itself did not address mothers of singletons and multiple births differently.

majority, 80 percent, of them are married, and almost 70 percent participate in the labor force. At the same time, over a quarter of the mothers in this data set have not finished high school, and more than 40 percent resided in rural areas. Importantly, these descriptive statistics are very comparable across columns in Table 1, suggesting that a subset of mothers that we are using in our main analysis is broadly representative of the population of all births. In panel B, we present our outcomes of interest. Children in our dataset weighed on average 3377 grams at the time of birth, and the incidence of births below 2500 grams was 4.8 percent. This is comparable to other developed settings, e.g., in Florida mean birth weight for singletons born between 1992 and 2002 was 3342 grams (Figlio et al., 2014). We also observed a fetal death rate of 0.4 percent out of 2,327,942 births recorded between 2006 and 2011. We come back to differences in outcomes across various columns in Table 1 below when we describe our empirical strategy.

4. METHODS

4.1 EMPIRICAL STRATEGY

We used the law change passed on 6th December 2008 in Poland as a source of exogenous variation in the timing of the first prenatal care visit and estimated its effects on neonatal health. Specifically, we exploited the fact that depending on the date of conception, the reforms quasi-randomly assigned women to the new requirement of the first prenatal visit during the first ten weeks of gestation. Figure 2 summarizes the legislative timeline and treatment status based on the date of conception.

The hold-up in implementation of the reform provides us with two thresholds along the conception date that we examined using a difference-in-differences (DD) design. The first threshold, 6th of December 2008, refers to the passage of the law. The latter cutoff, 8th of February 2009, is determined by the day of the law's implementation (i.e., 1st November 2009), which applied to births, and the common assumption for establishing the expected birth date is that typically a fetus spends 38 weeks in the uterus if we count from the day of conception.[16] Thus, for women who conceived a child on 8th February 2009, a physician typically calculates an expected due date on 1st November 2009, when the new law became binding. We used the date of conception instead of the birthdate to determine the cutoff because the actual day of birth might be endogenous if, for example, the reform affected the duration of pregnancy. The first cutoff induced exogenous variation in public information about the importance of early

[16] Note that common medical standards count the duration of a pregnancy from the first day of a woman's last menstrual cycle, which typically gives a 40-week gestational period. Nevertheless, a conception is possible only after ovulation, which usually occurs two weeks later, thereby giving the fetus typically only 38 weeks in the womb.

prenatal care, while the second was related to real monetary losses in the case of noncompliance with the policy. Having established the two cutoffs, we used a child's date of conception inferred from the data to determine whether and how its mother was affected by the reform.[17]

Consequently, we considered three groups of mothers: those who conceived until 6th December 2008; those who conceived between 6th December 2008 and 7th February 2009; and finally, those who conceived on 8th February 2009 or later. The first group of mothers was not affected by any part of the reform. The second group most likely knew that change in legislation was passed, as evidenced by the Google trends depicted in Figure A1; however, they also anticipated that most likely they would not be financially affected as their expected due dates were still before 1st November 2009. Nevertheless, these mothers might have positively reacted to the information about the benefits of early prenatal care steaming from the law passage spread by the media and the government. This creates a unique treatment group that potentially experienced a public information shock but remained unaffected by the conditionality of the cash transfer. Since the actual length of pregnancy is uncertain (e.g., Jukic et al., 2013), mothers with calculated due dates approaching 1st November 2009 might have also changed their behavior regarding prenatal care to avoid the risk of losing the "baby bonus". Therefore, we expect that compliance with the new law among this group was increasing along the conception date, where mothers who conceived a few weeks after 6th December 2008 comply solely due to the information channel, while those who conceived a few weeks before 8th February 2009 also complied to avoid financial losses. Finally, the third group of women comprises those with expected due dates on or after 1st November 2009 who knew that they would need to provide an appropriate prenatal care certificate to obtain the monetary transfer from the government after birth.

In our empirical analysis, we estimated two sets of regressions intended to uncover either the joint effect of information and cash transfer or just the impact of public information. In the first case, we used both cutoffs and a donut-hole sample that compared health outcomes of children conceived shortly (i.e., up to four weeks) before 6th December 2008 and shortly (i.e., up to four weeks) after 8th February 2009 relative to children conceived in the same weeks in surrounding years. In the second instance, using a difference-in-discontinuities setting, we compared the outcomes of children conceived up to four weeks before and after 6th December 2008 relative to children conceived in the same weeks in surrounding years.[18]

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[17] As we describe in Section 3, we infer a child's date of conception from its exact day of birth and the actual gestational age at birth.

[18] Using a similar difference-in-discontinuities setting, one could also consider exploring the 8th February 2009 cutoff to isolate the effect of the monetary transfer conditional on information. Since the actual length of pregnancy

Irrespective of the exact setting, we estimated the following equation:[19]

$$Y_{it} = \alpha + \beta Reform_{it} + l_c + f_{dy} + d_{dw} + d_{dw} \times c + \gamma X_i + \varepsilon_{it} \quad (1)$$

where Y_{it} is an outcome of a child i conceived on a given day t. Our explanatory variable of interest is the dummy variable $Reform_{it}$, which is equal to one if a child was conceived on or after the particular cutoff day during the reform winter of 2008/9 (i.e., up to four weeks after 6th December 2008 or 8th February 2009). Our regressions further include the following fixed effects: cohort for each winter between 2005/06 and 2009/10 (l_c), day of year taking values from one to 365 for non-leap years (f_{dy}), and day of week taking seven values (d_{dw}). The cohort fixed effects (l_c) adjusted for any cohort-specific effects being common to children conceived during a particular winter, such as the business cycle. The day of year (f_{dy}) and day of week (d_{dw}) fixed effects captured any unobserved effects associated with conceptions occurring during holidays, on weekends, or any specific day within a year more generally. For example, they account for the fact that our estimation samples might comprise holidays (e.g., Christmas or New Year's Eve) and that both our policy cutoffs were weekend days. The day of year fixed effects also flexibly isolate any seasonality effects that are common across cohorts. Therefore, we did not need a separate dummy variable to control for unobserved differences between children conceived in the weeks before versus after a particular cutoff. Our econometric specification, which closely follows Gans and Leigh (2009) and Borra et al. (2019), also includes an interaction term between the day of week dummies and cohort. We further included a set of control variables, X_i, accounting for various sociodemographic characteristics, such as maternal age at conception (linear and squared), dummies for her marital status, education, source of income, province of residence, rural areas, number of previous births, and child's gender.[20] Finally, ε_{it} is an error term, and we applied day-level clustered standard errors that adjust for correlation among children conceived on any particular day.

is uncertain, however, women who conceived a few weeks before 8th February 2009 most likely complied with the new regulation not only because of the law's announcement but also to avoid financial losses in case their birth should occur later than expected. Thus, they can be considered as likely treated by both the information and conditionality of the monetary transfer. For this reason, we view our donut-hole difference-in-differences as less problematic from interpretation standpoint given that it contrasts individuals who were not affected by the reform at all with those who were affected by both of its elements.

[19] A similar strategy has been used, e.g., by Lalive and Zweimüller (2009), Dustmann and Schönberg (2012), Cygan-Rehm (2016), and Huebener et al. (2019), to evaluate the impacts of parental leave reforms. Given that we use daily-level data, our model specification is particularly close to Borra et al. (2019), who examined the health consequences of speeding up births for non-medical reasons.

[20] The reform we are studying in this paper coincided with the global financial crisis but this is unlikely to confound our treatment effects since we focus on births around a relatively narrow time bandwidth surrounding the policy cutoffs. Nevertheless, in Section 7, we show that our primary results remained unchanged if we additionally controlled for local unemployment rates, which serve as a proxy for the potential impact of the recession. Furthermore, our period of analysis also covers some changes in maternity leave duration, but none of these

4.2 IDENTIFICATION

The key identifying assumption is that net of any potential seasonal effects captured by the fixed effects outlined above, there are no other factors beyond the policy change that differentially affected children conceived before and after the policy-related cutoffs. This implies that the treatments by public information and conditional cash transfer were "as good as" randomly assigned by the reform and solely conditional on the conception date. We performed various validity checks to assess the plausibility of this assumption.

First, we examined whether children born up to four weeks before and after the policy-related cutoffs differed in their sociodemographic backgrounds. Table 1 reports summary statistics and formally tests this balancing assumption. Columns 2 and 3 present sample means for children born up to four weeks before and up to four weeks after the 6th December 2008 cutoff. Panel A reveals that mothers birthing right before and right after the day when legislation was voted in are comparable in their background characteristics. Columns 5 and 6 show the corresponding sample means for the policy implementation threshold on 8th February 2009, and here, likewise, we did not observe any striking differences between the two groups of mothers. In columns 4 and 7, we formally tested whether the covariates were balanced across the cutoffs by estimating difference-in-differences regressions similar to equation 1 but replacing the outcome variable with a particular maternal characteristic and excluding the vector of control variables X_i. For each regression, we report the point estimates on the reform indicator and the corresponding p-values.[21] Column 9 presents an analogous analysis for our donut-hole difference-in-differences sample that excludes the period between 6th December and 8th February in conception-years 2005/6 to 2009/10 from the analysis.

Results in panel A reveal that, irrespective of the exact cutoff or samples, the treatment and control groups were balanced on almost all covariates. A single statistically significant difference was that mothers conceiving after 6th December 2008 were slightly less likely to be high school graduates; however, this imbalance is small in magnitude and becomes insignificant if we adjust the p-values for multiple hypothesis testing. Additionally, at the bottom of panel A, we report F-statistics from three F-tests where in each estimation sample, we regressed the

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reforms coincided with the cutoffs for the 10th week threshold along the conception date. Nonetheless, given the recent evidence that a longer leave may reduce maternal stress during pregnancy (Rossin-Slater, 2018), in a robustness test in Section 7, we directly controlled for this potential confounder, and our results remained unchanged. We were able to include these controls only because there is variation in maternity leave provisions across birth orders. Otherwise, any changes in maternity leave duration would be collinear with cohort fixed effects.

[21] Columns 4, 7, and 9 present regression adjusted differences and are thus numerically different from the raw differences that can be computed based on their respective treatment-control dyads in the preceding columns.

treatment indicator on a full set of background characteristics and tested their joint significance. These F-statistics likewise point to the conclusion that maternal characteristics are not systematically related to the reform indicator. The descriptive statistics for neonatal outcomes presented in panel B preview the results of our main analysis where we mostly compare children who did not receive any treatment (those conceived before 6th December 2008) with those who were treated by both public information and conditionality of the benefit (those conceived after 8th February 2009). The difference-in-differences estimates in column 9 suggest that both policy components together generated statistically significant gains in infant health.

The balancing tests presented in Table 1 argue against the concern that the reform spawned differential sorting of mothers across thresholds. For example, the law announcement on 6th December 2008 could have sped up the decision to have a child if some prospective mothers wanted to avoid the stricter eligibility rules for the "baby bonus" by bearing a child before the law went into effect. If true, then we would also expect a sudden increase in the number of conceptions right after the announcement date. Such immediate effects are unlikely as a healthy, fertile couple typically needs three to six months to conceive a child when actively trying (e.g., González, 2013), whereas the time window to avoid the stricter eligibility rules was limited to two months (between 6th December 2008 and 8th February 2009). Nevertheless, to mitigate this concern, we tested for discontinuous changes in the number of pregnancies initiated around both 6th December 2008 and 8th February 2009 by estimating the following equation:

$$C_{tc} = \alpha + \beta Reform_{tc} + l_c + f_{dy} + d_{dw} + d_{dw} \times c + \varepsilon_{tc} \qquad (2)$$

which mimics specification from equation (1) but replaces the dependent variable with a (log) number of conceptions occurring on day t in a given winter (cohort) c. Thus, to estimate this equation, we aggregated individual-level data to daily level observations using the inferred day of conception. In this case, $Reform_{tc}$ is an indicator equal to one for conceptions taking place on or after 6th December 2008 (or 8th February 2009), and β captures any excess daily conceptions occurring shortly after compared to shortly before these dates net of any date-specific conception patterns differenced out by comparison with surrounding years. If the reform announcement accelerated conceptions, we expect β to be positive and statistically significant.

Panels A and B in Table 2 present the results of this analysis for four different samples where the time span around the cutoff varies between one and four weeks. Irrespective of the exact sample chosen, the estimates corresponding to 6th December 2008 cutoff imply between

3 and 6 additional conceptions per day, on average, in days following the reform announcement. This effect is not only statistically insignificant but also small in magnitude given the sample means of over 1000 conceptions per day. This is confirmed by the specifications using the log number of conceptions as an outcome, which imply effect sizes between 0.4 and 1.4 percent. These coefficients are insignificant and much smaller in magnitude than the shifts in births due to financial incentives reported by Borra et al. (2019) who found effect sizes in the range of 11 to 38 percent. Panel B shows that there are also no statistically significant or sizable changes in the number of conceptions after 8th February 2009.[22]

Another potential composition effect could emerge if mothers with due dates around 1st November 2009 sped up a delivery (e.g., by labor induction or caesarian section) to apply for the "baby bonus" without the additional requirement mandated by the new law. We tested for such endogenous shifting of births by estimating the following equation:

$$B_{tc} = \alpha + \beta Reform_{tc} + l_c + f_{dy} + d_{dw} + d_{dw} \times c + \varepsilon_{tc} \qquad (3)$$

which is very similar to equation (2), but the dependent variable here is a (log) number of births occurring on day t in a given winter (cohort) c. Thus, for estimations, we aggregated the data to daily level observations using the day of birth. In this case, $Reform_{tc}$ is an indicator equal to one for births taking place on or after 1st November 2009, and thus β captures any changes in the number of deliveries occurring right after compared to right before the reform's implementation day and net of any date-specific birth patterns differenced out by comparison with surrounding years. If the reform's implementation endogenously sped up deliveries, we would expect β to be negative and statistically significant.

Panel C in Table 2 presents the regression results for four bandwidths around the 1st November 2009 cutoff. If anything, the point estimates imply an increased mass of deliveries right after the new law became effective, which contradicts the prediction outlined above. Although all estimates are positive, they are quantitatively small and never statistically significant. In fact, using 95 percent confidence intervals, we can rule out negative effect sizes

[22] Note that any increases in the number of conceptions could be not only due to endogenous delays but also due to early prenatal care improving the survivability of fetuses beyond the 20th week of gestation, which is the relevant threshold for filing a birth certificate (i.e., being recorded in the data). Fewer early deaths of the "marginal" fetuses would bias our main results in Section 5 downwards because of the negative compositional effect on health outcomes.

of more than -1.7 percent.[23] Using the natural log of the number of births as a dependent variable leads to similar conclusions.

Our difference-in-differences approach also requires that, absent the policy change, conceptions and births around the cutoffs would have followed similar seasonality patterns as their counterparts for the nonreform years. Only under this assumption, we can net out seasonality using double differencing. We present graphical evidence in favor of this assumption in Figure A5, which shows that the number of conceptions and births in reform years tracks very closely with those in the surrounding years. Although there are clear seasonality patterns for all years – particularly related to Christmas holidays and New Year's Day – we did not observe any discontinuities across years around our cutoffs of interest.

Generally, these various validity checks mitigated our concerns that the reform's passage and its final implementation might have led to endogenous sorting across the thresholds or scheduling births before the new law became effective. Furthermore, when analyzing our primary outcomes, we show that the results are invariant to including a rich set of control variables, which additionally supports a quasi-experimental design.

5. EFFECTS OF THE REFORM ON NEONATAL HEALTH

5.1 MAIN RESULTS

We begin with estimating the total effect of the policy change, i.e., of public information and conditionality of the financial benefit jointly, by using a donut-hole difference-in-differences design. In that, we compared the health outcomes of children conceived up to four weeks before 6th December 2008 and up to four weeks after 8th February 2009 relative to children conceived in the same weeks in surrounding years. Each panel in Table 3 shows the results for one of the following outcomes: summary health index at birth (standardized), probability of fetal death, the log of birth weight, probability of low birth weight, and gestational age in weeks. The outcomes in panels B, C, and D are multiplied by 100 to increase visibility and enhance interpretation. In each case, we present the point estimate on the reform indicator across different sample bandwidths from one to four weeks around the relevant cutoffs, as well as with and without individual-level controls.

We observed that the reform significantly increased the summary health index at birth by 2.0 to 3.5 percent of a standard deviation. Although the effect size varied somewhat across the different bandwidths, the point estimates were statistically indistinguishable from each other

[23] We are less concerned about the upper bound on these estimates since positive effects may imply that an announcement of the reform might have prolonged the average duration of the pregnancy, which we view as a positive outcome. We investigated this issue in more detail in Section 5.1.

as their 95 percent confidence intervals largely overlap. For the other outcomes, the estimates imply nontrivial reductions in the probability of fetal death of up to almost 40 percent relative to the sample mean. We also identified significant gains in birth weight of approximately 0.3 to 0.8 percent (or 9 to 21 grams) and corresponding reductions in the probability of low birth weight from 7 to 18 percent relative to the sample mean. On the other hand, we did not find any meaningful effects on gestational age, and based on 95 percent confidence intervals, we ruled out positive effects larger than 0.08 weeks (or 0.2 percent) and negative effects larger than 0.01 weeks (or 0.04 percent).[24] Thus, we conclude that the reform led to statistically significant improvements in neonatal health, which were, however, not solely attributable to extended gestational age. The results for all outcomes are invariant to including a rich set of control variables, which supports a quasi-experimental design.

The hold-up in the implementation of the reform allowed us to estimate the effect of the information channel separately from the conditionality of the cash transfer. In that, we explored the policy signing cutoff and compared individuals with and without information on the government's intentions. The results are documented in Table 4, which mimics the structure of Table 3. Here, we did not find robust evidence for positive effects of the public information about the benefits of early prenatal care steaming immediately from the law passage and the subsequent media coverage. In fact, in the narrower samples, some of the estimates for the effects on health index, birth weight, or gestational age were wrong-signed but were generally small and virtually never statistically significant. For example, the statistically significant at 10 percent level (p-value of 0.098) estimate for gestational age in column 2 implies a trivial effect size of -0.1 percent or about one-quarter of a day. Akin to prior results, control variables do not change the results. When we focused on the +/- 4 weeks sample, all coefficients were signed in the expected direction, some were statistically significant, and those tended to converge to the estimates reported in Table 3. This makes sense given how the reform was implemented and the fact that compliance with the new legislation must have been increasing along the conception date. In that, compared to the narrower sample, when we used wider bandwidths, we potentially had more mothers who started complying with the new requirement to avoid financial penalties (Figure 2). Overall, we conclude that the direct benefits of the information

[24] We further investigated effects on log gestational age and an indicator for premature birth, but these results are qualitatively similar to what we present in Table 3 for weeks of gestation. At the same time, we observed reductions in post-term deliveries, but these estimates are only statistically significant in the one- and two-week bandwidth samples.

from the law announcement and its subsequent media coverage are minuscule compared to having both information and conditionality of the transfer.

5.2 DISCUSSION OF THE MAGNITUDES

All coefficients presented above are reduced-form and correspond to intention-to-treat (ITT) effects of incentivizing prenatal care during the first ten weeks of gestation. To assess the magnitude of our estimates, we first compared them to reduced-form impacts of other programs affecting neonatal health from related literature. For instance, Rossin-Slater (2013) found that access to the Women, Infants, and Children (WIC) program in a neighborhood increased average birth weight by 0.8 percent, while Hoynes et al. (2015) showed that an expansion in Earned Income Tax Credit (EITC) payments reduced the incidence of low birth weight by up to 5.2 percent.[25] Almond et al. (2011) further documented that the introduction of Food Stamps led to about a 1% decrease in the fraction of births below 2,500 grams. Our estimates are also in line with Borra et al. (2019), who found that abolishing the Spanish "baby bonus" significantly increased the number of early inductions of births, which led to birth weight reductions of up to 15 grams. Thus, we view our reduced-form estimates as not only plausible but also comparable in magnitude to what was found in previous studies on other determinants of neonatal health.

To compare our results with earlier research on prenatal care, which typically focused on the availability of first-trimester care in the U.S., we rescaled our reduced-form effects by the change in first-trimester rates, which is discussed in Section 2. Recall that the Polish reform likely induced at least a 25-percentage-point increase in the use of first-trimester care. Thus, scaling up our reduced-form estimates by factor 0.4 yields back-of-the-envelope estimates of the treatment on the treated (TOT) effects of increasing the utilization of early prenatal care by ten percentage points.

Consequently, our results imply that a 10-percentage-point increase in early prenatal care leads to birth weight gains of up to 0.32 percent and reductions in the probability of birth weight below 2,500 grams by up to 0.37 percentage points or by 7.3 percent relative to the sample mean. The latter estimate appears large, but it is not out of the realm of possibility. It also makes sense if earlier interventions matter more than later ones (Cunha and Heckman, 2007). For example, using Medicaid expansions that increased eligibility for free prenatal care, Currie and Gruber (1996) observed reductions in the incidence of low birth weight of up to 5.3

[25] Using the same reform, however, Dench and Joyce (2020) documented that EITC might not be causally linked to neonatal health.

percent for a 10-percentage-point increase in Medicaid take-up due to extensions targeting at low-income groups.[26] Gray (2001) demonstrated a slightly larger impact using the variation in Medicaid reimbursement rates for prenatal services paid to the physicians, and his estimates implied that a 10-percentage-point increase in prenatal care reduces the relative risk of low birth weight by 6.6 percent. This is very close to our largest estimate, and similar to our results, he did not identify any significant effects on gestational age.

While most previous studies have focused on birth weight effects for infants born alive, we also found significant effects on fetal deaths, which are often associated with maternal complications of pregnancy, such as problems with amniotic fluid levels or blood disorders. If these conditions are detected early enough, the death of the fetus can be often prevented by proper treatment during pregnancy or emergency delivery (Currie and Grogger 2002). Our results imply that a 10-percentage-point increase in early prenatal care utilization reduces the relative risk of fetal death by 10 to 15 percent. For comparison, Currie and Grogger (2002) observed substantially larger effects using various administrative measures that increase eligibility for Medicaid and reduce nonprice barriers to prenatal care utilization in the U.S. Their results suggested a 40-percent reduction in fetal deaths among black mothers if prenatal care increased by 10 percentage points. The results for whites were less conclusive, but some of the effects are even higher in magnitude.

Taking a longer-term perspective, using the estimates from Figlio et al. (2014) and Black et al. (2005), our results for birth weight would suggest up to 0.16 percent of a standard deviation improvement in cognitive development at school or a 0.32 percent increase in adult income. Compared to other human capital inputs, such as being assigned to a high value-added teacher (Chetty et al., 2014), we view these potential gains from early prenatal care as modest. Moreover, given that our scaling exercise does not consider any potential speedups of the first visit within the first trimester (e.g., from 12th to 10th week of gestation), we likely underestimate the actual shift in the timing of prenatal care initiation due to the reform. Thus, our TOT estimates should be treated as an upper bound on the true effect of speeding up the first prenatal visit. Nevertheless, the small to moderate gains in neonatal health are certainly policy-relevant given the fiscal neutrality of the Polish reform.

[26] Overall, for targeted changes applied to low-income groups, they identified a 2.6 percent reduction in the incidence of low birth weight if eligibility for Medicaid increased by 10 percentage points. Since the take-up was not full but rather about 49 percent, however, this effectively implies a 5.3 percent decrease in the incidence of low birth weight.

Our results suggest that financial conditionality baked into the reform was essential to generating the observed gains in neonatal health. Before the reform, however, the state was paying the unconditional benefits, and thus, this monetary dimension allowed us to compute willingness to pay for prenatal health. Previous research on tax incentives and birth timing suggests that parental private willingness to pay is in the range of 1.2 to 3.1 USD per gram of birth weight (Schulkind and Shapiro, 2014), and these findings have been confirmed using an experimental design (Clarke et al., 2017). Our birth weight estimates suggest gains of between 9 and 21 grams due to the reform, which should be further scaled by a factor of four since we expect that only 1 in 4 mothers shifted their prenatal care earlier as explained above, while the transfer had a value 1,000 PLN (or 318 USD at the time). Based on these numbers, an implied willingness to pay for birth weight ranged from 3.8 to 8.8 USD per gram. These estimates are between previous estimates for private willingness to pay and public willingness to pay stemming from programs such as WIC or Food stamps (Clarke et al., 2017). Overall, we view our estimates from an incentivized prenatal health intervention in Poland as remarkably close to those in prior literature where either private or public monetary valuation of prenatal health was assessed.

6. HETEROGENEITY

Prior research suggests substantial heterogeneity in the potential gains from prenatal care (e.g., Currie and Grogger, 2002; Conway and Deb, 2005; Abrevaya and Dahl, 2008; Sonchak, 2015) so that most effects are concentrated among socioeconomically disadvantaged groups (e.g., blacks, unmarried teen mothers, or school dropouts). Although our policy in question was universal rather than targeted, for lower-income mothers, the "baby bonus" constituted a relatively larger boost to their incomes compared to higher-income mothers. Thus, we might expect a bigger change in the utilization of early prenatal care by the 10^{th}-week threshold among socioeconomically disadvantaged groups, which should be reflected by disproportionately better birth outcomes.

We present heterogeneity results by maternal education, employment, and place of residence in Table A2 in the Appendix. Additionally, we also stratify by maternal age and birth order, hypothesizing that first-time and teen mothers might benefit relatively more from early prenatal care due to its focus on promoting knowledge about the course of pregnancy and healthy prenatal behaviors. The analysis in Table A2 is based on our largest donut-hole estimation sample that yielded the most conservative estimates in Table 3. In each panel, we extended the primary specification from equation 1 with an interaction term between the reform indicator and a maternal characteristic of interest. We executed the analysis in the way where arguably the most disadvantaged group represents a reference category, and the interaction terms yielded the excess losses or gains relative to this reference group. For comparison, in panel A, we repeat our primary analyses for the entire sample from Table 3.

In panel B, we observed some evidence for an educational gradient in the reform's effects on neonatal health. Specifically, the reduced probability of fetal deaths is primarily driven by mothers with less than a high school education. We observed similar patterns for the remaining outcomes, but the differences were less pronounced and were usually statistically insignificant. The estimates by maternal employment (Panel C) and for rural and urban areas (Panel D) did not yield any notable differences across these groups, but we did find some significant heterogeneity by maternal age at conception (Panel E). Specifically, teenage mothers contributed to the decrease in the probability of a low birth weight, but these benefits vanished for children of mothers who gave birth later in life. Finally, we did not observe any significant differences between first-born and higher-order children (Panel F). If anything, the point estimates suggest that higher-order births experience slightly improved birth outcomes over first-borns. Given that higher-order children generally receive relatively less prenatal care (e.g., Brenøe and Molitor, 2018), it could be that the reform incentivized particularly non-first-time mothers to use early prenatal care, but we cannot verify this conjecture with the available data.

Earlier research by Abrevaya and Dahl (2008) suggests that the largest beneficial effects of prenatal care might be concentrated among fetuses most at risk for adverse birth outcomes. Therefore, we also considered the potential heterogeneity in the treatment effects along the distribution of (log) birth weight and our summary health index using unconditional quantile regressions, summarized in Figure A6. We obtained strikingly large point estimates at the very bottom of the birth weight distribution. Beyond that, however, the point estimates were generally indistinguishable from the average effects and remained relatively constant and mostly statistically significant over the entire distribution. We observed a nearly identical pattern when looking at the distribution of the summary health index.

Taken together, our heterogeneity analysis indicates slightly larger effects among children from socioeconomically disadvantaged backgrounds and those at high risk for adverse birth outcomes. However, the premiums for those more vulnerable children appear limited, and our estimates are generally underpowered to detect statistically significant differences across most groups and outcomes. Thus, these results should be treated with caution.

7. ROBUSTNESS

So far, we documented that our main results should not be confounded by changes in the composition of births or conceptions around the policy-relevant cutoffs and that they are robust to including a rich set of background characteristics. To further strengthen the causal interpretation of our estimates, Table 5 provides additional robustness checks. Similar to the heterogeneity analysis, we focused on the +/- 4 weeks bandwidth since these were the most conservative estimates in Table 3; however, the results for the other bandwidths were likewise stable and, akin to the main results, larger in magnitude. For comparison in panel A of Table 5, we repeated our main estimates from Table 3. We also added, in column 6, an indicator for a child's date of birth being an odd number (even number is the reference category) as a placebo outcome, which we discuss in more detail below.

Regarding potential confounding factors, we are not aware of any other contemporaneous policy changes that could have differentially affected neonatal or maternal health for conceptions up to four weeks before 6th December 2008 and up to four weeks after 8th February 2009. Nonetheless, the timing of the latter cutoff coincides with the introduction of the "baby bonus" three years earlier on 9th February 2006. Since the transfer was always paid postpartum it should not have affected birth outcomes of children included in our pre-policy control group. To mitigate any remaining concerns about a potentially confounding impact of this earlier reform, in panel B, we excluded children conceived up to four weeks before 6th December 2005 and up to four weeks after 8th February 2006 (i.e., 2005/2006 birth cohort).[27] The results were very similar to our preferred specification.

Following Borra et al. (2019), in the main analysis, we included both pre- and post-reform cohorts as a control group to difference out seasonality effects. This approach increases precision (due to a larger sample) and is not problematic so long as the seasonality patterns remain the same across cohorts, which we have documented in Figure A5. Nevertheless, in panel C, we present our results after excluding the post-reform cohort 2009/2010 to mitigate the concern that seasonality could have changed during the post-reform period.[28] Again, our results remained substantively unchanged.

In the main specification, we used the policy announcement – 6th December 2008 – as a delineation determining the treatment through the public information channel because

[27] We also checked that introduction of the "baby bonus" did not lead to additional conceptions by conducting an analysis akin to what we presented in Table 2 but for a 9th February 2006 cutoff. None of the estimates were statistically significant nor were they consistently signed.

[28] This exercise further alleviates a concern that our estimates are confounded by any anticipatory effect of the policy relaxation passed on 5th March 2010 as it is theoretically possible that women who conceived a child shortly after 8th February 2010 could have already suspected that the 10th week requirement might be repealed soon.

mothers who conceived on or after this cutoff date knew about the new regulation from the very beginning of the pregnancy. However, mothers who were below the 10th week of gestation on 6th December 2008 found out about the policy sometime during the first ten weeks, and theoretically, they could have changed their behavior due to this information. If this is the case, our control group includes some potentially treated women. To mitigate this concern, in panel D, we backdate this cutoff by 10 weeks (to 27th September 2008) so that no women in the control group could have initiated their prenatal care according to the new requirement in response to the policy announcement. Our conclusions remained unchanged, and the estimates were very similar to those from our preferred specification.

In Panel E, we applied a different assumption for computing the exact conception date. As explained in Section 3, we can calculate the date of conception with a weekly accuracy. For the main analysis, we assumed that each pregnancy ends on the next day following the completed gestational age in weeks, but this implies a certain degree of measurement error in our treatment variable. Thus, in panel E, we alternatively added three days to each pregnancy duration, thereby assuming a mid-week delivery in the last (noncompleted) gestational week. These estimates, except for the probability of fetal death, remained very similar to our main results.

We also investigated whether our estimates were sensitive to the inclusion of flexible time trends in conception date, which are meant to capture any trends in neonatal health over time, e.g., due to advances in the quality of health care or the accuracy of pregnancy tests. In panel F, we present estimates, including quadratic terms. This alternative specification yields very similar findings, and if anything, the estimates become more precise. While not shown in the table, we obtained very similar results when we only included a linear trend in conception date or added higher-order polynomials (up to fourth-order).

As mentioned in Section 2, the 10th-week threshold for the first prenatal visit was passed in a bundle of laws that primarily regulated future maternity leave provisions, which became effective in January 2012. While these regulations happened beyond the sample period, our analysis covers some earlier changes in maternity leave. It is highly unlikely, however, that these earlier reforms confound our results because none of them coincided with the cutoffs for the 10th-week threshold along the conception date. We tested this conjecture in Panel G by including indicators for the expected maternity leave duration at birth (between 16 and 22 weeks), which is based on a mother's expected due date. Our main results remained unchanged.

Another concern is that the reform in question was implemented in the year of the Great Recession. For example, using Icelandic data, Olafsson (2016) shows that exposure to the Great

Recession during the first trimester in utero had detrimental consequences for newborns' health. On the other hand, Poland has weathered the crisis remarkably well and was the only economy in the European Union that avoided recession in its aftermath. Moreover, even our widest bandwidth (+/- 4 weeks) does not cover children conceived in the immediate postcrisis period as for each included cohort, we began with conceptions in the second week of November (four weeks before 6th December) and stopped at conceptions in the first week of the following March (four weeks after 8th February). Our DD strategy should also flexibly capture any cohort-specific effects. Nevertheless, to account for any differential effects of the crisis across regions, in Panel H, we additionally controlled for the monthly unemployment rate at the province level. As expected, given the empirical design, our results were not sensitive to the inclusion of this additional variable.

To validate our estimates, we also executed two placebo analyses. First, in panel I, we challenged the internal validity of our estimates by evaluating the effects of a placebo reform within the same difference-in-differences design. In that, we moved back the two reform-related cutoffs by six months to 6th June 2008 and 8th August 2008. In support of our identifying assumption, these placebo estimates were all close to zero and statistically insignificant. Furthermore, in all cases but for gestational age, where our main results were the weakest, the 95 percent confidence interval of the placebo estimate excluded our preferred estimates from Table 3. Second, throughout the table in column 6, we estimate placebo effects on the likelihood of being born on an odd-numbered day, which should not plausibly be affected by the reform. Irrespective of the exact model specification or sample restrictions, none of the estimates in the last column were statistically significant, and they were all minuscule relative to the mean.

Finally, in Table A3, we present results from an event study analysis, which allows estimation of the effects beyond the narrow windows around the reform cutoffs and testing of the internal validity by investigating the pre-trends. Irrespective of the outcome, there were no clear pre-reform patterns, and we did not observe any sizable or statistically significant coefficients in the pre-period either. The only exception was a single coefficient on the probability of low birth weight; however, this estimate strongly departs from its surrounding coefficients, indicating it is likely a statistical aberration. On the other hand, in the post-period, and especially in the weeks after the reform's implementation, we observed mostly statistically significant estimates that mimicked our findings from Table 3. A notable exception to this pattern represents the effect on fetal mortality, which was concentrated in the first three weeks after the reform's implementation. For the remaining outcomes, the effects persist well beyond our preferred estimation windows from Table 3.

In summary, we conclude that our results are not sensitive to excluding particular cohorts from the control group, correcting for the potential measurement error in the assignment of the treatment, or changing the preferred model specification. Likewise, the data do not exhibit any worrisome differential pre-trends. Furthermore, we did not find any quantitatively meaningful estimates using fake cutoff dates or placebo outcomes. These additional results support the causal interpretation of our main estimates from Section 5.1.

8. POTENTIAL MECHANISMS

To shed more light on the potential channels through which the reform might have positively affected neonatal health, we relied on auxiliary data from the Polish Pregnancy-related Assessment Monitoring System (Pol-PrAMS). The Pol-PrAMS is a survey conducted during one defined week of the year in all Polish hospitals among new mothers in the first few days following childbirth (typically 1-4 days postpartum).[29] Importantly, it includes a set of questions on health-related knowledge and behaviors during pregnancy (Wojtyła and Wojtyła-Buciora, 2016). The first wave took place in June 2009, i.e., more than six months after the reform was passed. Importantly, however, the vast majority of interviewed mothers conceived their newborn in September and the first half of October 2008, i.e., about two months before the law's announcement. Thus, we view the behavior of these mothers early in pregnancy as unaffected by the reform.[30] In 2010, the data were collected in the second week of August, which implies that the second survey covers mothers who conceived in November 2009, i.e., shortly after the 10th-week threshold became a binding requirement for "baby bonus" eligibility. We also included two later surveys from November 2011 and March 2012.

These additional data allow us to look at changes in maternal health-related knowledge and behaviors using a before-after design where we compare mothers surveyed in 2009 (control group) to those surveyed in the years 2010-2012 (treatment group). We estimated the following equation:

$$Y_i = \alpha + \sum_{k=2010}^{2012} \beta_k Post_i^k + \gamma X_i + \varepsilon_i \qquad (4)$$

where Y_i is an outcome of a woman i, while the post-reform changes of interest are captured by a series of three dummy variables $Post_i^k$ which are each equal to one if a woman was interviewed in the 2010 to 2012 surveys, respectively. If the reform altered maternal behaviors

[29] Because some hospitals and some mothers refused to participate in the study, the final sample covers approximately 80% of women who gave birth in Poland on the survey day each year (ca. 3,000 mothers).
[30] These mothers could not have reacted by altering their early prenatal care in response to the reform; however, it is possible that they reported better knowledge and behaviors after birth because they were affected by changing social expectations due to public discussion related to the reform. Thus, if anything, these estimates should be treated as the lower bound of changes in behavior and knowledge.

and knowledge, then we would expect statistically significant coefficients on β_k. Furthermore, if the estimates are indeed driven by the reform rather than just secular trends in these outcomes, we would expect all three coefficients on the $Post$ dummies to be similar in magnitude.[31]

We did two things to ameliorate, at least to some extent, concerns that the results were driven by other changes over time. First, we estimated both the unconditional changes, as well as a specification that controls for a rich set of maternal sociodemographic characteristics (included in X). Specifically, similar to our main analysis for neonatal outcomes, we controlled for maternal age, marital status, education, province of residence, town size, number of previous births, and child's gender. These covariates are themselves strong and statistically significant predictors of health knowledge and behaviors. Second, we used a placebo outcome – exposure to smoking at work – that arguably should not have been affected by the policy (or at least not to the same extent as other analyzed outcomes) but has been shown to affect birth outcomes (Bharadwaj et al., 2014; McGeary et al., 2020). Thus, if we found any significant changes in exposure to smoking at work over time, they should reflect general trends in healthy behaviors, which would imply that our before-after comparisons for other outcomes are most likely confounded.

Results are presented in Table 6. The outcomes in columns (1) to (5) are associated with health-related knowledge, while those in columns (6) to (8) with risky behaviors during pregnancy. Column (9) investigates passive smoking at the workplace, which we view as a placebo outcome. All outcomes are indicator variables. Panel A shows the means of the outcomes for mothers who conceived in the pre-reform period. Thus, for example, we observe that before the reform, approximately 88 percent of mothers knew that small alcohol amounts during pregnancy are harmful for maternal and fetal health, and 95 percent knew that large alcohol amounts are detrimental. Most but not all were aware that active (97 percent) and second-hand smoking (92 percent) are also harmful. Furthermore, nearly 13 percent of mothers surveyed in 2009 admitted to alcohol consumption and 17 percent to smoking during pregnancy.

Panel B shows the before-after differences compared to the baseline mean without any additional covariates, and the estimates remained remarkably stable when we added a rich set of demographic controls in Panel C. Generally, in columns 1 to 5, we found statistically significant improvements in the awareness that alcohol consumption and smoking are harmful

[31] From an identification standpoint, to interpret β_k as causal effects, we needed the outcomes to be the same in 2009 and in later years absent the reform requiring the 10th week prenatal care certification. In that, no other factors affecting the outcomes could have changed across these years.

to maternal and a newborn's health. This appears to be accompanied by reductions in admitted drinking and smoking during pregnancy documented in columns 6 to 8. Such changes are consistent with increased exposure to early prenatal care, which usually focuses on educating women about the course of pregnancy and on promoting healthy behaviors.[32] Importantly, the estimates are very similar across all post-reform years, which mitigates the concern that we are picking up secular improvements in health knowledge and behaviors. Furthermore, in column 9, we did not observe any sizable or statistically significant changes in exposure to passive smoking at work – even though mothers reported knowing about its harmful effects – which likewise suggests that our estimates do not simply reflect confounding trends in improved healthy behaviors over time.

Assuming that the estimates in Table 6 do indeed reflect changes induced by the 10th-week policy, improved maternal behavior during pregnancy seems to be a candidate channel through which the reform affected neonatal health. This would be in line with prior work on the detrimental effects of smoking and drinking in pregnancy on birth outcomes (Abrvaya, 2006; Fertig and Watson, 2009; Bharadwaj et al., 2014).[33] Specifically, our results in column 7 imply that the rate of maternal smoking during pregnancy dropped by, at most, 6.9 percentage points. Extrapolating this estimate through the lens of Abrevaya (2006) would imply increases in birth weight in the range of 9 to 13 grams and decreases in low birth weight rates in the range of 0.17 to 0.21 percentage points. The former estimates are on the lower end of our effects in Table 3, while the latter estimates are approximately 50 percent smaller than the smallest coefficients reported in Table 3. Our point estimate on a decrease in drinking during pregnancy is at most 6.2 percentage points (column 6 in Table 6), which when paired with small effect sizes reported by Fertig and Watson (2009) can explain relatively little of the potential effect of a decrease in drinking on observed improvements in birth outcomes. Nonetheless, we view both of these channels as plausibly jointly contributing to the observed gains in health at birth.

Overall, the magnitudes implied by the extrapolation exercise seem reasonable as it is inconceivable that all benefits of early prenatal care for birth outcomes accrue solely through reductions in drinking and smoking. Although we caution readers about drawing strong causal conclusions based on the estimates in Table 6, they are remarkably consistent, irrespective of the exact outcome or specification. Thus, we view them as providing suggestive evidence that

[32] Here we refer to the applicable Polish standards of prenatal care services (Dziennik Ustaw, 2004).

[33] Nilsson (2017) provides compelling evidence from Sweden that prenatal alcohol consumption negatively affects the fetus thought adulthood and leads to worse labor market and educational outcomes. Although we cannot observe long-term outcomes in our data, this evidence suggests that our policy in question may have positive effects on children's success in adulthood.

early promotion of healthy behaviors during pregnancy might represent a plausible, but not necessarily exclusive, channel through which the reform improved newborns' health.

9. Conclusions

This paper investigated whether the timing of prenatal care initiation matters for neonatal health. Specifically, we evaluated a policy intended to speed up the first prenatal care visit in Poland, which has a universal and publicly funded health care system. The reform forced expectant mothers to begin prenatal care during the first ten weeks of gestation in order to be eligible for a one-time monetary transfer after childbirth, which was before that time an unconditional benefit. Administrative data on prenatal care utilization and governmental payments suggested that the vast majority of mothers complied with the stricter eligibility criteria, and the reform increased the utilization of early prenatal care by at least 25 percentage points.

Applying a differences-in-differences design to individual-level register data on the population of births and fetal deaths, we found statistically significant positive effects of the policy on neonatal health. These benefits were generated by mothers who were treated both by the conditionality of the monetary transfer and the public information about the importance of early prenatal care spread by the media and the government due to the law passage. In contrast, by exploring the hold-up in policy implementation, we did not identify any statistically significant or economically meaningful effects from the public information channel alone. Our estimates imply moderate gains in birth weight and significant decreases in the likelihood of fetal death. From the perspective of mixed findings on the benefits of prenatal care itself (Corman et al., 2019), our results are promising as the reform only shifted the timing of the first prenatal visit rather than affecting the extensive margin of healthcare utilization. We also found some suggestive evidence that the beneficial effects might disproportionately accrue to more disadvantaged mothers. Finally, using auxiliary survey data, we showed that improved maternal health knowledge and less risky behaviors during pregnancy might be plausible, though not necessarily exclusive, mechanisms underlying the positive effects of early prenatal care initiation on neonatal health.

In summary, our results suggest that children in utero may benefit from earlier initiation of prenatal care. Moreover, it is important to keep in mind that these public investments might have additional impacts far beyond neonatal health documented in this paper. For example, Miller and Wherry (2019) demonstrated that children who gained Medicaid coverage in the womb and during infancy exhibited better health and educational outcomes as adults. Related research also found that first-trimester care positively affects maternal health-related behaviors measured postpartum (e.g., Conway and Kutinova, 2006; Reichman et al., 2010), while most recent results suggests that benefits of improved prenatal health persist even beyond the treated generation (East et al., 2019). From the policy perspective, it is also important to highlight that the Polish legislation was fiscally neutral and did not require any additional transfers from the state as it built upon an existing birth-related benefit. Therefore, even small to moderate gains in neonatal health should be viewed as welfare improving.

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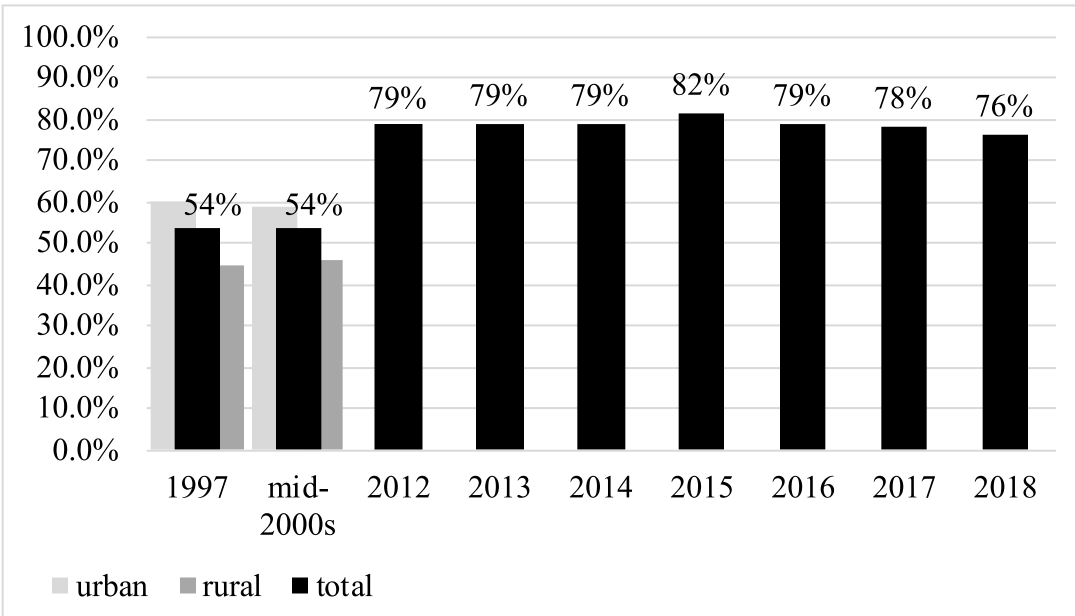
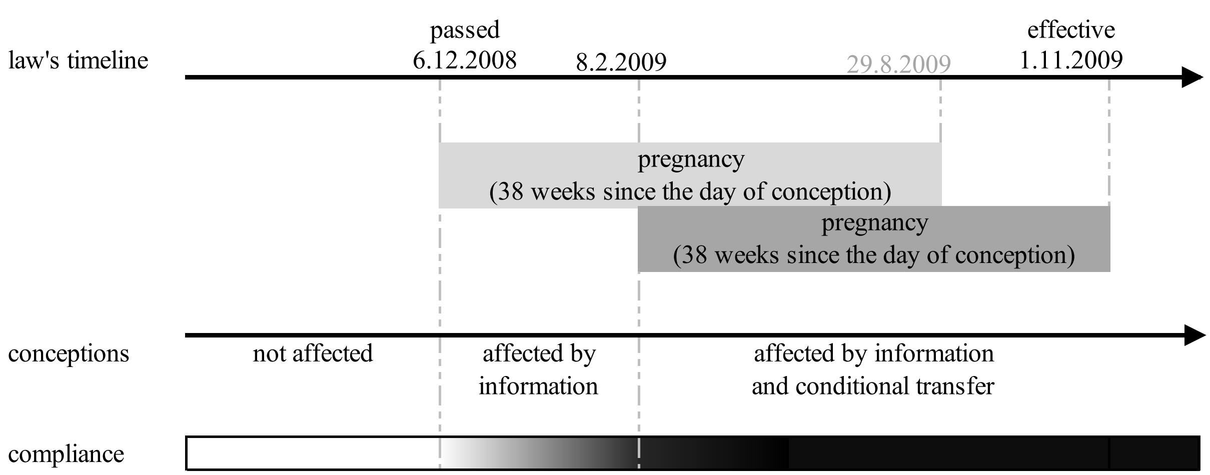
Figures and Tables

Figure 1: Rates of Prenatal Care Initiation in the First Trimester



Note: This figure presents aggregate statistics on rates of prenatal care initiation in the first trimester. The data is limited to visits contracted by the public health care insurance system. The numbers for 1997, are from a governmental report (Druki Sejmowe, 1998); for mid-2000s, from Kornas–Biela (2012). The data from 2012 onwards is provided by the Healthcare Information Systems Center (CSIOZ) of the Ministry of Health.

Figure 2: Timeline of the Law Change



Note: Own illustration. This figure presents the timing of the reform.

Table 1: Descriptive Statistics and Balance of Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Cutoff: 6th December 2008			Cutoff: 8th February 2009			Donut-hole sample +/- 4 weeks	
	All births	4 weeks before	4 weeks after	D-in-D [p-value]	4 weeks before	4 weeks after	D-in-D [p-value]	Mean	D-in-D [p-value]
				Panel A. Background characteristics					
Age at conception	27.146	26.997	27.049	-0.011	27.989	28.046	0.038	27.268	0.037
				[0.781]			[0.397]		[0.371]
First birth	0.511	0.524	0.520	0.003	0.519	0.518	-0.003	0.520	-0.004
				[0.486]			[0.453]		[0.181]
Less than high school	0.269	0.250	0.256	0.003	0.253	0.255	0.000	0.274	0.002
				[0.377]			[0.895]		[0.511]
High school graduate	0.359	0.359	0.353	-0.010	0.356	0.353	-0.001	0.361	-0.005
				[0.002]			[0.880]		[0.119]
In labor force	0.679	0.707	0.704	0.003	0.702	0.703	-0.001	0.678	-0.004
				[0.371]			[0.656]		[0.136]
Married	0.799	0.799	0.795	-0.002	0.792	0.789	0.003	0.797	0.000
				[0.491]			[0.389]		[0.995]
Divorced	0.027	0.027	0.027	0.000	0.029	0.030	0.001	0.027	0.002
				[0.775]			[0.477]		[0.180]
Widowed	0.003	0.003	0.003	0.000	0.003	0.003	0.000	0.003	0.000
				[0.517]			[0.936]		[0.837]
Rural residence	0.417	0.411	0.412	-0.002	0.405	0.405	0.001	0.415	0.001
				[0.616]			[0.838]		[0.736]
F-statistic				1.430			0.270		0.980
				Panel B. Neonatal outcomes					
Health index (PCA)	0.000	-0.007	0.001	0.007	0.004	0.006	0.008	-0.005	0.020
				[0.391]			[0.351]		[0.018]
Fetal death (×100)	0.415	0.432	0.347	-0.089	0.409	0.318	-0.105	0.438	-0.117
				[0.042]			[0.038]		[0.022]
Birth weight (grams)	3376.980	3366.124	3368.030	-1.860	3373.216	3373.491	1.415	3367.558	9.728
				[0.671]			[0.738]		[0.033]
Low birth weight (×100)	4.844	5.068	4.788	-0.260	4.934	4.920	-0.105	5.053	-0.391
				[0.083]			[0.568]		[0.014]
Gestational age (weeks)	39.166	39.179	39.187	0.017	39.195	39.183	0.007	39.184	0.025
				[0.254]			[0.686]		[0.130]
Observations	2,327,942	32,639	32,833	313,251	28,394	27,966	284,083	296,828	296,828

Note: Column 1 presents means for the universe of births between 2006 and 2011. Columns 2 and 3 present descriptive statistics for children conceived +/- 4 weeks around 6th December 2008 cutoff while columns 5 and 6 present descriptive statistics for children conceived +/- 4 weeks around 8th February 2009 cutoff. Columns 4 and 7 show the coefficients on an indicator variable that equals to one if a child was conceived in the 4 weeks after the cutoff in the reform year 2008/09 estimated for each listed variable separately as an outcome. The p-value of a significance test is in square brackets and each cell represents a separate linear regression. F-tests in panel A report the value of F-statistics from regressions of the reform indicator on all the covariates listed in this panel. Column 8 presents the mean values for the donut difference-in-differences sample while column 9 presents the balancing test for this sample. Note that some college is reference category in education, not in labor force is a reference category for employment, single mother is a reference category for civil status while urban is a reference category for place of residence. P-values are based on heteroskedasticity robust standard errors.

Table 2: Effects of the Reform Passage and Implementation on Number of Conceptions and Births

	(1) +/- 1 week	(2) +/- 2 weeks	(3) +/- 3 weeks	(4) +/- 4 weeks
	Panel A. 6th December 2008 cutoff (conceptions)			
Number of conceptions	6.321 (17.036)	3.982 (13.609)	2.762 (11.209)	6.223 (10.763)
Mean of Y	1057.375	1057.527	1074.071	1106.156
Log number of conceptions	0.014 (0.015)	0.011 (0.012)	0.005 (0.010)	0.004 (0.009)
Observations	70	140	210	280
	Panel B. 8th February 2009 cutoff (conceptions)			
Number of conceptions	19.929 (22.604)	1.625 (14.508)	-3.667 (11.179)	10.316 (10.425)
Mean of Y	1001.75	1007.152	1010.048	1016.621
Log number of conceptions	0.018 (0.022)	0.001 (0.014)	-0.002 (0.011)	0.011 (0.010)
Observations	70	140	210	280
	Panel C. 1st November 2009 cutoff (births)			
Number of births	43.357 (27.309)	28.750 (21.404)	15.726 (16.838)	19.179 (13.228)
Mean of Y	1011.75	1005.625	1006.423	1012.393
Log number of births	0.040 (0.026)	0.026 (0.021)	0.017 (0.017)	0.020 (0.013)
Observations	70	140	210	280

Note: Panels A and B show the coefficient on a Reform dummy from equation (2) obtained for the cutoffs of 6th December and 8th February, respectively. Panel C shows the coefficient on a Reform dummy from equation (3). The dependent variable is the (log) daily number of conceptions (Panels A and B) or births (Panel C). Each cell represents a separate linear regression and unit of observation is a day. Heteroskedasticity robust standard errors in parentheses. The estimation samples include all conceptions/births up to four weeks before and up to four weeks after a particular cutoff in years 2005/6 - 2009/10. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

Table 3: Effects of the Reform on Neonatal Health

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	+/- 1 week		+/- 2 weeks		+/- 3 weeks		+/- 4 weeks	
A. Health	0.034***	0.035***	0.031***	0.031***	0.021**	0.021**	0.020**	0.021**
index	(0.012)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)	(0.008)	(0.008)
Mean of Y	0.000		0.000		0.000		0.000	
B. Fetal death	-0.178**	-0.173**	-0.154**	-0.148**	-0.156***	-0.153***	-0.117**	-0.116**
(×100)	(0.085)	(0.085)	(0.059)	(0.059)	(0.059)	(0.058)	(0.051)	(0.050)
Mean of Y	0.465		0.462		0.450		0.453	
C. Log birth	0.767***	0.799***	0.524**	0.539**	0.319	0.332*	0.345**	0.346**
weight (×100)	(0.212)	(0.212)	(0.214)	(0.218)	(0.196)	(0.201)	(0.166)	(0.169)
Mean of Y	810.394		810.431		810.487		810.455	
D. Low birth	-0.937***	-0.925***	-0.636***	-0.622***	-0.347*	-0.352*	-0.391**	-0.399**
weight (×100)	(0.154)	(0.154)	(0.194)	(0.195)	(0.184)	(0.184)	(0.158)	(0.158)
Mean of Y	5.099		5.125		5.117		5.129	
E. Gestational	0.034	0.034	0.042**	0.041**	0.025	0.027	0.025	0.028*
age (weeks)	(0.023)	(0.023)	(0.020)	(0.020)	(0.020)	(0.019)	(0.016)	(0.016)
Mean of Y	39.180		39.187		39.192		39.189	
N	74,084		148,843		222,848		296,828	
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note: Each cell is based on a separate regression and displays the coefficient on a Reform dummy from equation (1). Individual controls include maternal age at conception (linear and squared), dummies for her marital status, education, source of income, province of residence, rural areas, number of previous births, and a child's gender. Standard errors in parentheses are clustered at the level of conception day. The estimation samples include all conceptions up to four weeks before 6th December in years 2005 - 2009 and up to four weeks after 8th February in years 2006 - 2010. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

Table 4: Effects of the Reforms' Information Component on Neonatal Health

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	+/- 1 week		+/- 2 weeks		+/- 3 weeks		+/- 4 weeks	
A. Health index	-0.008	-0.010	-0.007	-0.007	0.002	0.001	0.007	0.007
	(0.012)	(0.012)	(0.010)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Mean of Y	0.000		0.000		0.000		0.000	
B. Fetal death	-0.084	-0.073	-0.051	-0.048	-0.091*	-0.089*	-0.089**	-0.088**
(×100)	(0.058)	(0.057)	(0.059)	(0.059)	(0.050)	(0.050)	(0.044)	(0.044)
Mean of Y	0.475		0.457		0.449		0.456	
C. Log birth	-0.134	-0.155	-0.201	-0.145	-0.082	-0.048	0.038	0.051
weight (×100)	(0.217)	(0.217)	(0.171)	(0.170)	(0.173)	(0.175)	(0.152)	(0.151)
Mean of Y	810.413		810.416		810.471		810.517	
D. Low birth	-0.294	-0.244	-0.068	-0.067	-0.083	-0.083	-0.260*	-0.263*
weight (×100)	(0.192)	(0.189)	(0.181)	(0.182)	(0.163)	(0.164)	(0.149)	(0.148)
Mean of Y	4.990		5.063		5.053		5.026	
E. Gestational age	-0.033	-0.039*	-0.011	-0.015	0.008	0.004	0.017	0.015
(weeks)	(0.020)	(0.021)	(0.018)	(0.018)	(0.017)	(0.017)	(0.015)	(0.015)
Mean of Y	39.183		39.186		39.191		39.190	
N	74,796		149,695		228,204		313,251	
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note: Each cell is based on a separate regression and displays the coefficient on a Reform dummy from equation (1). The estimation samples include all conceptions up to four weeks before and up to four weeks after 6th December in years 2005 - 2009. Individual controls include maternal age at conception (linear and squared), dummies for her marital status, education, source of income, province of residence, rural areas, number of previous births, and a child's gender. Standard errors in parentheses are clustered at the level of conception day. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

Table 5: Effects of the Reform on Neonatal Health: Sensitivity Analysis

	(1) Health index	(2) Fetal death (×100)	(3) Log birth weight (×100)	(4) Low birth weight (×100)	(5) Gestational age (weeks)	(6) Odd number DOB (×100)
A. Baseline	0.021**	-0.116**	0.346**	-0.399**	0.028*	-0.032
	(0.008)	(0.050)	(0.169)	(0.158)	(0.016)	(1.630)
Mean of Y	0.000	0.453	810.455	5.129	39.189	51.144
Observations			296,828			
B. Excluding 2005/06 cohort	0.022**	-0.125**	0.357**	-0.467***	0.030*	-0.028
	(0.008)	(0.052)	(0.166)	(0.154)	(0.016)	(1.999)
Mean of Y	0.000	0.437	810.585	5.071	39.179	51.337
Observations			240,365			
C. Excluding 2009/10 cohort	0.017**	-0.118**	0.299*	-0.346**	0.019	-0.182
	(0.008)	(0.052)	(0.164)	(0.161)	(0.016)	(2.058)
Mean of Y	0.000	0.453	810.455	5.129	39.189	51.144
Observations			236,006			
D. Lagging control group by 10 weeks	0.020***	-0.130**	0.340**	-0.347**	0.013	0.471
	(0.007)	(0.050)	(0.142)	(0.154)	(0.015)	(1.773)
Mean of Y	0.000	0.430	810.648	4.985	39.202	50.983
Observations			290,769			
E. Assuming mid-week delivery	0.019**	-0.058	0.369**	-0.376**	0.030*	-0.378
	(0.008)	(0.051)	(0.170)	(0.158)	(0.016)	(1.563)
Mean of Y	0.000	0.457	810.466	5.105	39.189	50.775
Observations			294,451			
F. Adding quadratic trends	0.024***	-0.108**	0.403**	-0.497***	0.038**	0.123
	(0.009)	(0.054)	(0.178)	(0.166)	(0.017)	(1.773)
Mean of Y	0.000	0.453	810.455	5.129	39.189	51.144
Observations			296,828			
G. Adding maternity leave controls	0.021**	-0.116**	0.346**	-0.399**	0.028*	-0.032
	(0.008)	(0.050)	(0.169)	(0.158)	(0.016)	(1.630)
Mean of Y	0.000	0.453	810.455	5.129	39.189	51.144
Observations			296,828			
H. Adding regional unemployment	0.021**	-0.112**	0.388**	-0.405**	0.023	-0.013
	(0.009)	(0.054)	(0.189)	(0.179)	(0.018)	(1.552)
Mean of Y	0.000	0.453	810.455	5.129	39.189	51.144
Observations			296,828			
I. Placebo reform (6 months earlier)	0.001	0.019	-0.100	0.109	0.019	-0.224
	(0.008)	(0.050)	(0.147)	(0.157)	(0.016)	(0.843)
Mean of Y	0.000	0.391	811.145	4.688	39.147	50.651
Observations			297,435			

Note: Each cell is based on a separate regression and displays the coefficient on a Reform dummy from equation (1). Samples include all conceptions up to four weeks before 6th December in years 2005 - 2009 and up to four weeks after 8th February in years 2006-2010. All regressions include individual level controls: maternal age at conception (linear and squared), dummies for her marital status, education, source of income, province of residence, rural areas, number of previous births, and a child's gender. Panel B excludes conceptions from 2005/06 cohort while panel C excludes conceptions from 2009/10 cohort. Panel D lags the threshold by 10 weeks to exclude any partially treated women from the control group. Panel E assumes a mid-week delivery in the last (non-completed) gestational week. Panel F includes quadratic trends in conception date. Panel G adds maternity leave controls while panel H adds regional unemployment controls. Panel I presents placebo estimates when we move the two cutoffs back by six months to 6th June 2008 (from 6th December 2008) and 8th August 2008 (from 8th February 2009). Standard errors in parentheses are clustered at the level of conception day. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

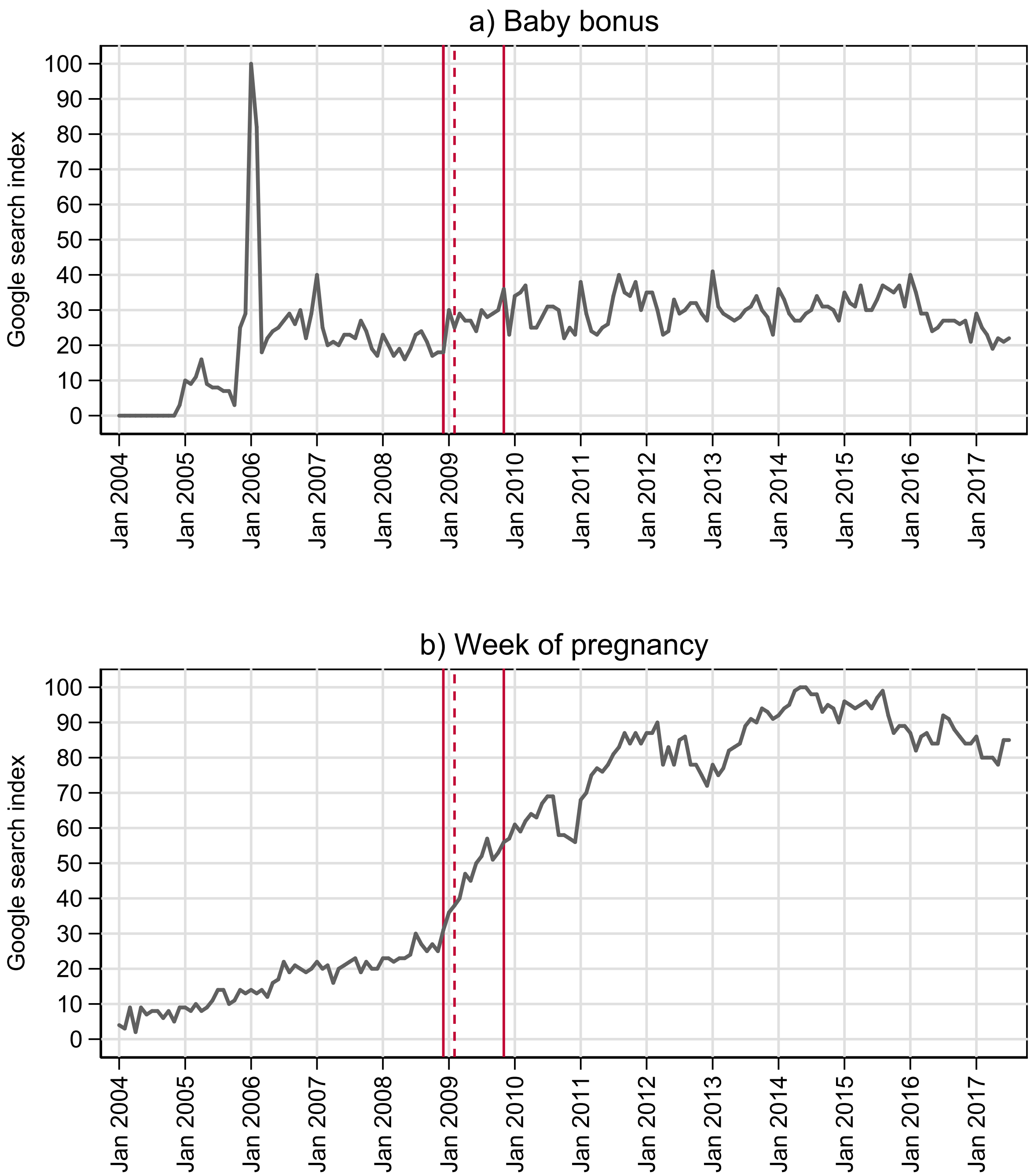
Table 6: Exploring potential mechanisms: Maternal health-related knowledge and behaviors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Health-related knowledge					Health-related behaviors during and post pregnancy			
	Small alcohol amounts harmful	Large alcohol amounts harmful	Active smoking harmful	Passive smoking harmful	Knows all of this	Alcohol during pregnancy	Smoking during pregnancy	Smoker postpartum	Exposure to smoking at work
	Panel A. Baseline June 2009 mean (pre-reform period)								
	0.877	0.946	0.969	0.924	0.832	0.129	0.173	0.091	0.114
	Panel B. Unconditional differences between 2009 and subsequent years								
Post 2010	0.096***	0.050***	0.025***	0.070***	0.138***	-0.062***	-0.066***	-0.027***	0.002
	(0.007)	(0.005)	(0.004)	(0.005)	(0.008)	(0.008)	(0.010)	(0.008)	(0.009)
Post 2011	0.107***	0.052***	0.028***	0.073***	0.150***	-0.054***	-0.041***	-0.026***	-0.004
	(0.007)	(0.004)	(0.003)	(0.005)	(0.008)	(0.009)	(0.010)	(0.008)	(0.010)
Post 2012	0.103***	0.051***	0.027***	0.071***	0.144***	-0.055***	-0.059***	-0.035***	-0.012
	(0.007)	(0.004)	(0.004)	(0.005)	(0.008)	(0.008)	(0.010)	(0.007)	(0.009)
	Panel C. Differences between 2009 and subsequent years conditional on covariates								
Post 2010	0.096***	0.050***	0.025***	0.069***	0.137***	-0.061***	-0.069***	-0.030***	0.003
	(0.007)	(0.005)	(0.004)	(0.005)	(0.008)	(0.008)	(0.009)	(0.007)	(0.009)
Post 2011	0.109***	0.052***	0.029***	0.073***	0.152***	-0.055***	-0.052***	-0.034***	-0.001
	(0.007)	(0.004)	(0.004)	(0.005)	(0.008)	(0.009)	(0.010)	(0.008)	(0.010)
Post 2012	0.104***	0.052***	0.029***	0.071***	0.146***	-0.055***	-0.063***	-0.037***	-0.003
	(0.007)	(0.005)	(0.004)	(0.005)	(0.008)	(0.009)	(0.009)	(0.007)	(0.009)
Observations	9,372	9,372	9,372	9,372	9,372	9,372	9,372	9,372	8,540

Note: Panel A presents mean values of outcome variables in the pre-reform period while panels B and C present regression output where treatment variables of interest are indicators for survey years 2010, 2011, and 2012, respectively. In these regressions data from 2009 survey serve as a reference category. Regressions in panel B do not include any additional controls while regressions in panel C control for maternal age at delivery (indicators in three-year intervals), dummies for her marital status, education, province of residence, town size, number of previous births, and a child's gender. Heteroskedasticity robust standard errors in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

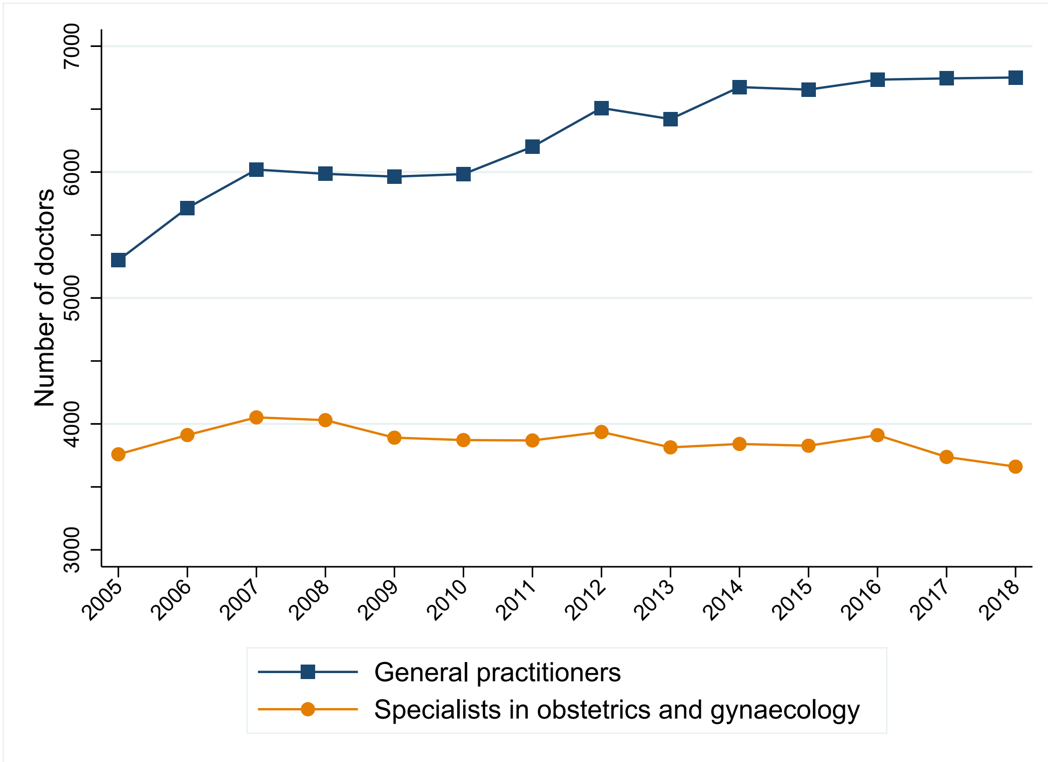
A Supplemental Figures and Tables

Figure A.1: Internet Searches for "Baby Bonus" and "Week of Pregnancy"



Note: Google Trends available online at https://trends.google.com/trends/explore [Last accessed: 01.08.2019]. The figures show monthly Google search index for the Polish keywords "becikowe" and "tydzień ciąży", respectively. The search region was restricted to Poland and the search period to 01.01.04 - 31.07.17. The vertical solid lines mark December 2008 and November 2009, respectively. The vertical dashed line marks February 2009.

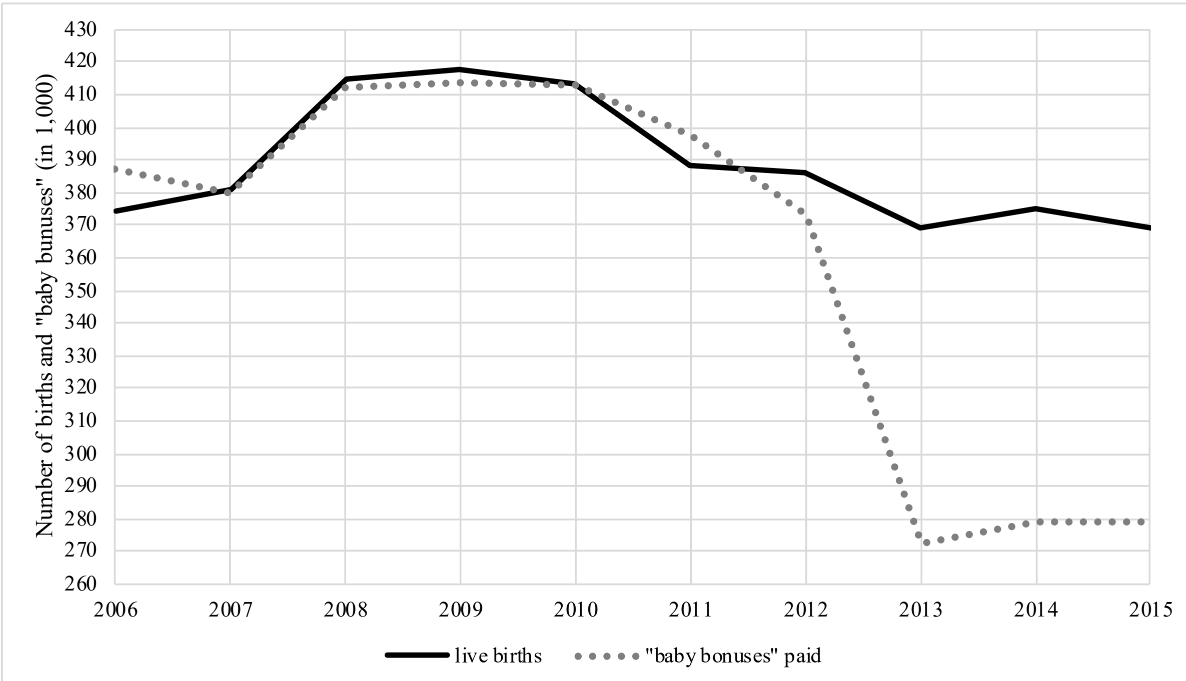
Figure A.2: Number of General Practitioners as well as Gynaecologists and Obstetricians



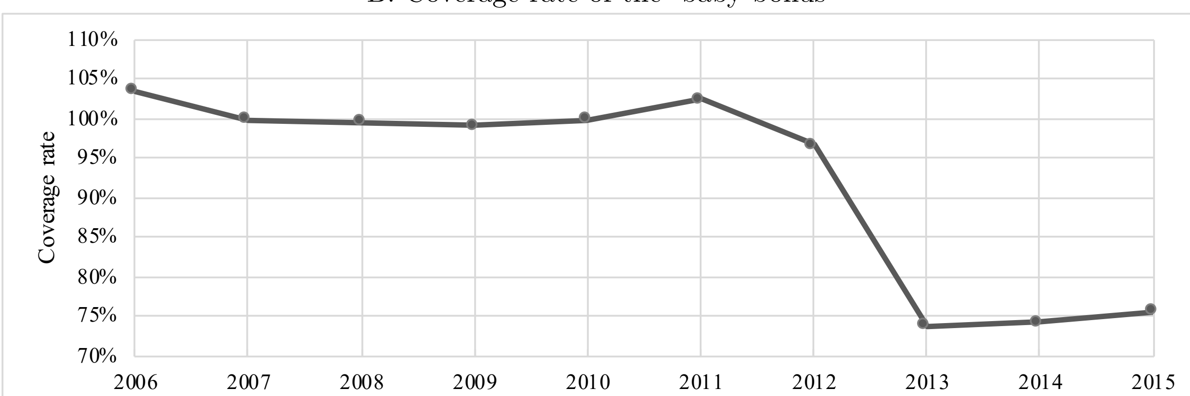
Note: This figure presents aggregate statistics on number of general practitioners as well as OB/GYN specialists in Poland between 2005 and 2018. This data is limited to doctors under contract to the public health care insurance system.
Source: The Central Statistics Office of Poland, Basic Data on Health Care (various years).

Figure A.3: Take up of the "Baby Bonus" Between 2006 and 2015

A. Absolute number of births and “baby bonuses” paid



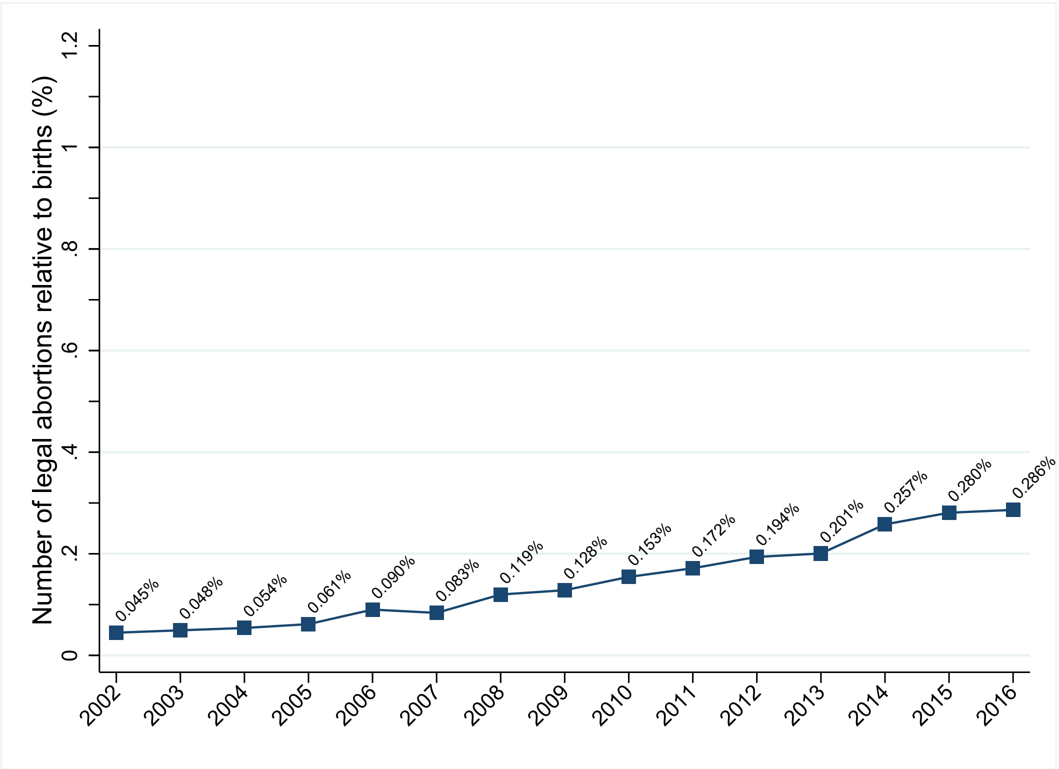
B. Coverage rate of the “baby bonus”



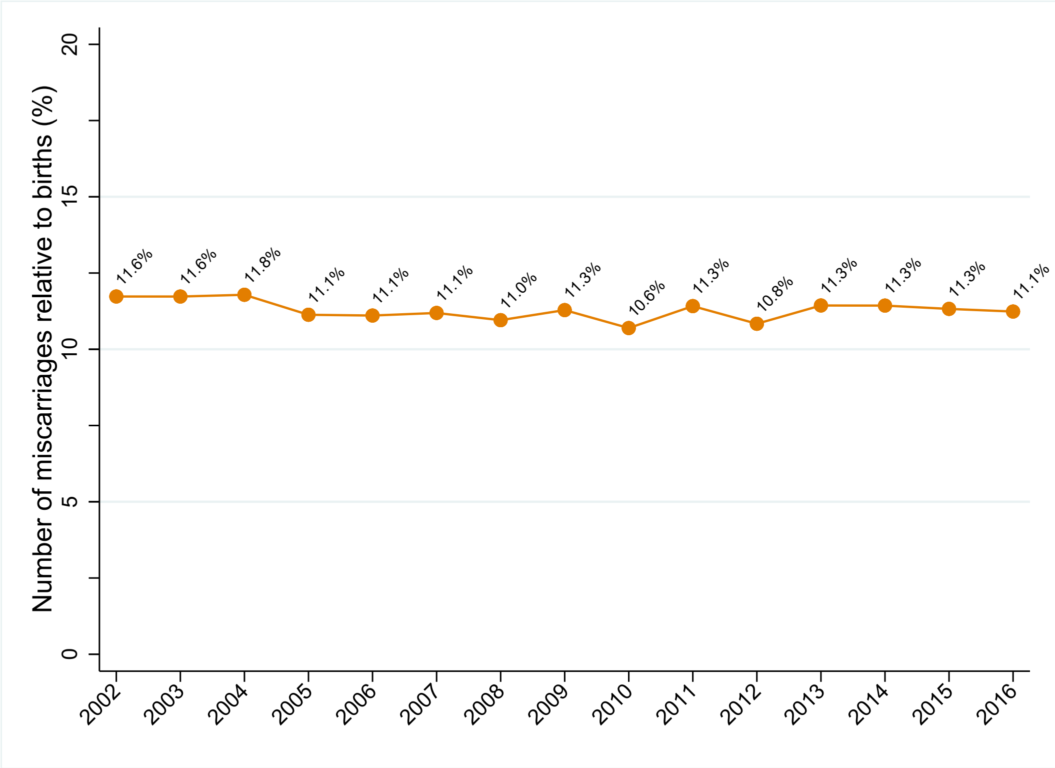
Note: This figure presents aggregate statistics on all births and number of benefits paid based on annual data from The Ministry of Family, Labour and Social Policy. The coverage rate is a ratio of the number of “baby bonuses” to the number of all births in a given year.

Figure A.4: Legal Abortions and Miscarriages between 2002 and 2016 in Poland

A. Legal abortions

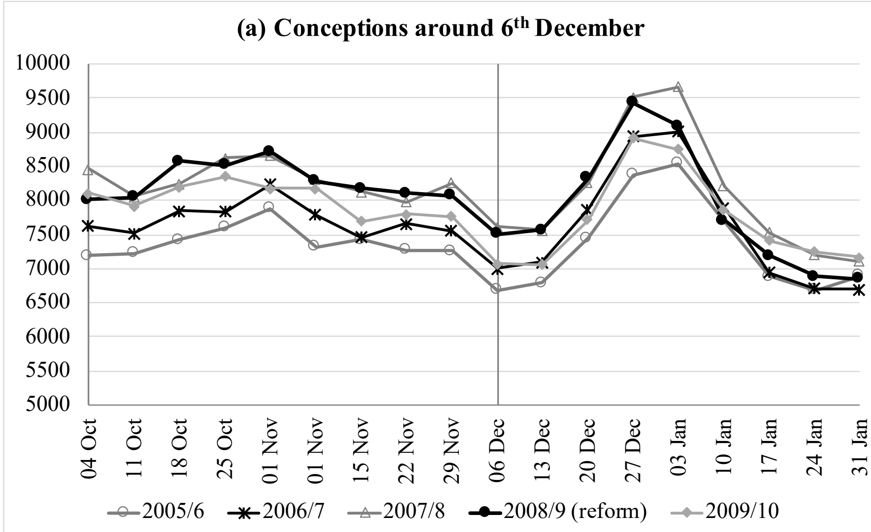


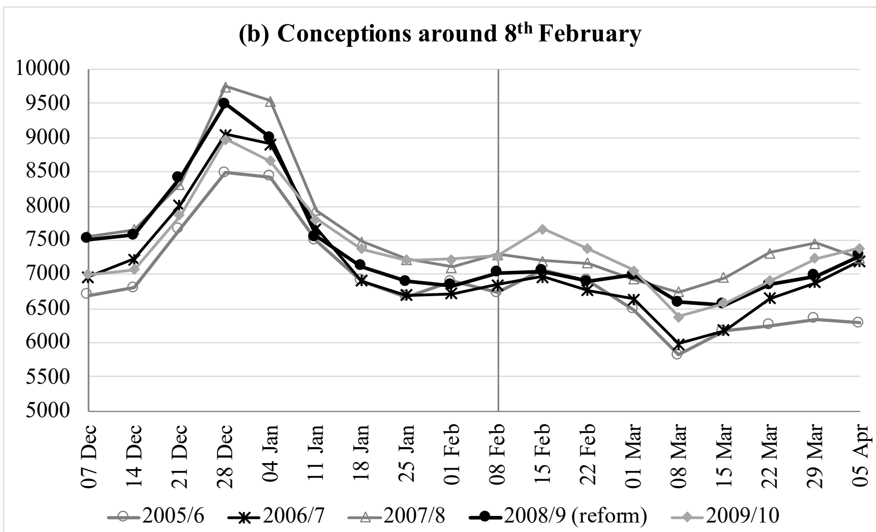
B. Miscarriages

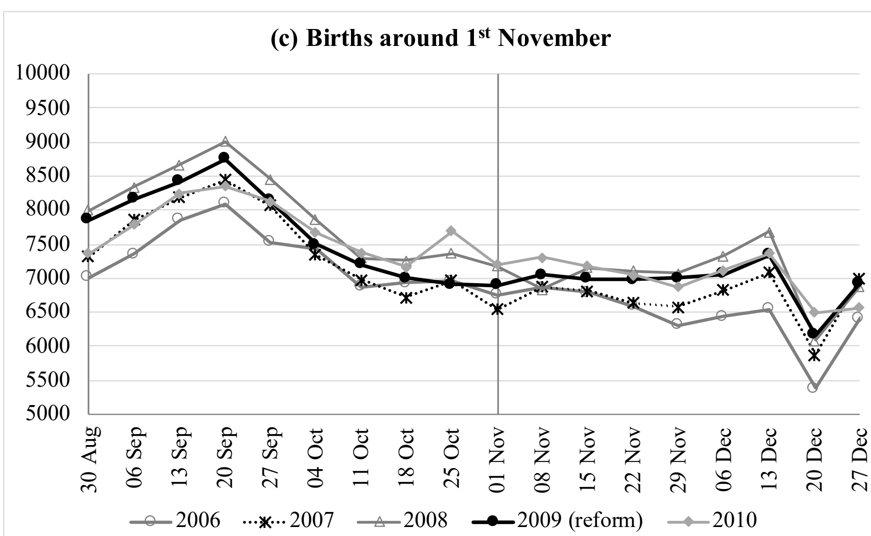


Note: This figure presents aggregate statistics on number of legal abortions and miscarriages relative to live births between 2002 and 2016 in Poland.
Source: Centrum Systemow Informacyjnych Ochrony Zdrowia (CSIOZ).

Figure A.5: Trends in Conceptions and Births

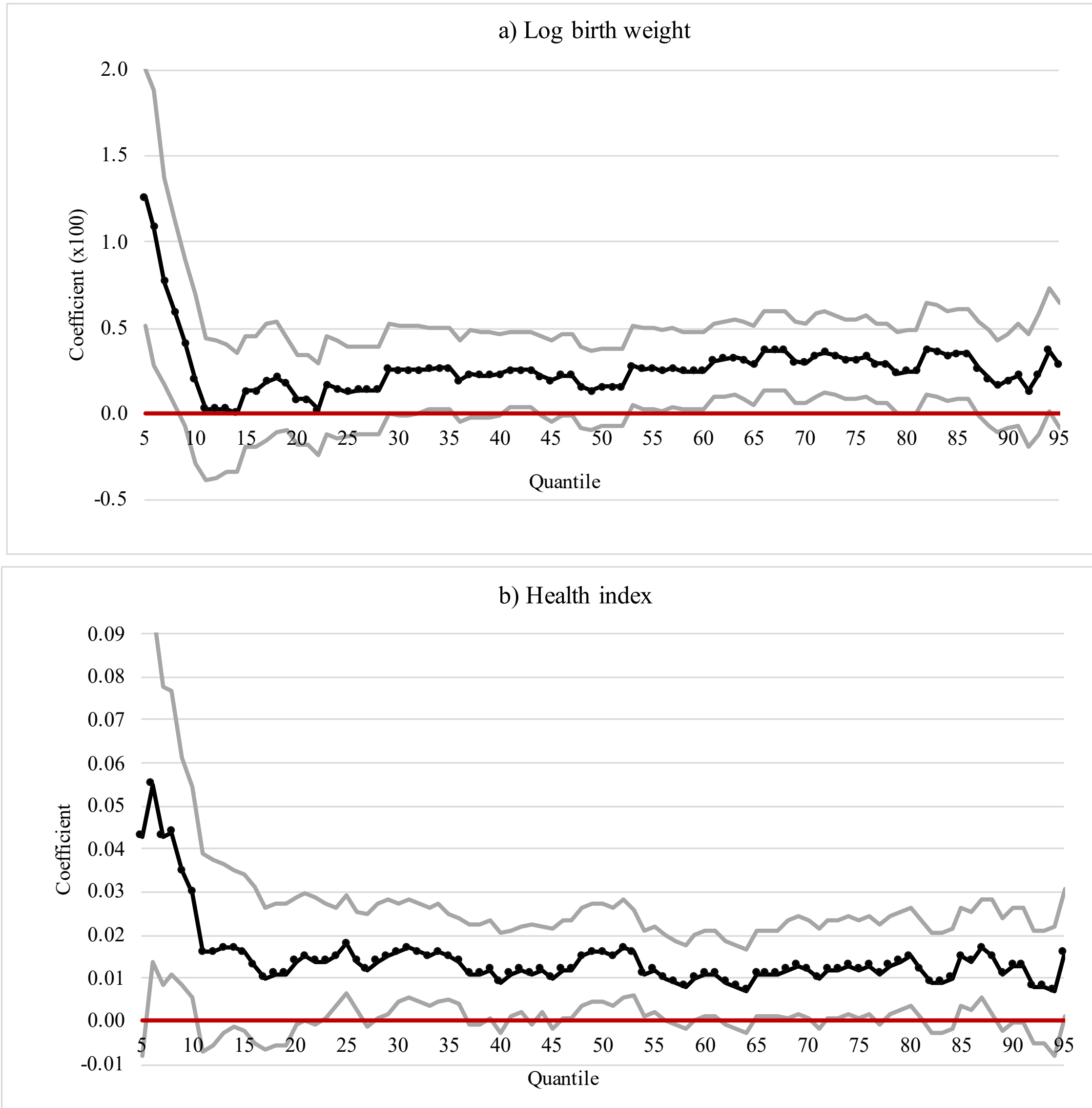






Note: This figure plots the weekly number of conceptions and births across years around three cutoffs: 6th of December, 8th of February, and 1st of November. Conception date is calculated as the day of birth minus the actual completed gestational weeks converted into days. The x-axis shows the starting days of seven-day periods.

Figure A.6: Effects of the Reform on Neonatal Health: Quantile Regressions



Note: The estimation samples include all conceptions up to four weeks before 6th December in years 2005 - 2009 and up to four weeks after 8th February in years 2006 - 2010. These figures plot unconditional quantile regression estimates, from RIF regressions (Firpo et al., 2009) implemented using the rifreg command in Stata, on the reform dummy as defined in equation (1). Solid black line depicts the point estimate while gray lines present 90% confidence intervals. Panel A presents estimates for log birth weight while panel B for health index. Control variables include maternal age at delivery (linear and squared), dummies for her marital status, education, source of income, province of residence, rural areas, number of previous births, and a child's gender.

Table A.1: Construction of Principal Components Health Index

	First component	Second component
Fetal death (indicator)	-0.389	0.918
Birth weight (grams)	0.643	0.328
Gestational age (weeks)	0.660	0.221
Eigenvalue	1.708	0.877

Summary statistics for the first component

Mean	0.000
Standard deviation	1.307

Note: This table reports the results of a principal components analysis of an indicator for fetal death, birth weight in grams, and gestational age in weeks. The eigenvectors associates with the first and second components are reported, as well as their associated eigenvalues. The bottom panel reports summary statistics of the health index, defined as the first component of the principal components analysis, for the overall sample.

Table A.2: Heterogeneity in the Effects of the Reform on Neonatal Health

	(1) Health index	(2) Fetal death (×100)	(3) Log birth weight (×100)	(4) Low birth weight (×100)	(5) Gestational age (weeks)
	Panel A: Baseline specification				
Reform	0.020**	-0.117**	0.345**	-0.391**	0.025
	(0.008)	(0.051)	(0.166)	(0.158)	(0.016)
Mean of Y	0.000	0.453	810.455	5.129	39.189
	Panel B: Interaction with education				
High school dropout	0.040***	-0.234***	0.636**	-0.617	0.049
(reference)	(0.015)	(0.078)	(0.286)	(0.380)	(0.032)
Reform × High	-0.027	0.183*	-0.450	0.497	-0.031
school graduate	(0.019)	(0.109)	(0.375)	(0.496)	(0.040)
Reform × Some	-0.027*	0.136*	-0.334	0.108	-0.023
college and above	(0.016)	(0.080)	(0.317)	(0.403)	(0.035)
	Panel C: Interaction with labor force participation				
Not in labor force	0.025*	-0.126*	0.426*	-0.702**	0.038
(reference)	(0.014)	(0.071)	(0.253)	(0.284)	(0.031)
Reform × Working	-0.006	0.014	-0.114	0.431	-0.013
	(0.014)	(0.074)	(0.255)	(0.314)	(0.033)
	Panel D: Interaction with place of residence				
Rural (reference)	0.025**	-0.115*	0.479**	-0.428*	0.022
	(0.012)	(0.067)	(0.229)	(0.227)	(0.024)
Reform × Urban	-0.006	-0.002	-0.223	0.049	0.011
	(0.011)	(0.065)	(0.234)	(0.272)	(0.027)
	Panel E: Interaction with maternal age at conception				
Teen birth	0.056*	-0.239*	1.272*	-2.407***	0.074
(reference)	(0.032)	(0.132)	(0.654)	(0.700)	(0.071)
Reform × Non-teen	-0.037	0.130	-0.974	2.113***	-0.048
birth	(0.033)	(0.140)	(0.656)	(0.690)	(0.071)
	Panel F: Interaction with birth order				
First birth	0.014	-0.069	0.259	-0.351*	0.018
(reference)	(0.011)	(0.069)	(0.211)	(0.211)	(0.020)
Reform × Non-first	0.014	-0.098	0.180	-0.099	0.021
birth	(0.013)	(0.081)	(0.249)	(0.252)	(0.028)
N			296,828		

Note: Each column in each panel is based on a separate regression without controls based on the donut-hole sample where we include conceptions up to four weeks before 6th December in years 2005-2009 and up to four weeks after 8th February in years 2006-2010. It displays the coefficient on a Reform dummy from equation (1), which is labelled as reference category group comparison between treated and control individuals, as well as its interactions with demographic characteristics. Standard errors in parentheses are clustered at the level of conception day. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

Table A.3: Robustness: Event study analysis

	(1) Health index	(2) Fetal death (×100)	(3) Log birth weight (×100)	(4) Low birth weight (×100)	(5) Gestational age (weeks)
13-15 weeks prior to reform	-0.014	0.024	-0.239	0.073	-0.013
	(0.010)	(0.064)	(0.174)	(0.176)	(0.018)
10-12 weeks prior to reform	-0.001	-0.063	-0.106	-0.048	0.003
	(0.010)	(0.057)	(0.198)	(0.178)	(0.017)
7-9 weeks prior to reform	0.009	-0.072	0.198	-0.346*	0.006
	(0.011)	(0.052)	(0.203)	(0.181)	(0.019)
4-6 weeks prior to reform	-0.003	-0.046	0.011	0.015	-0.012
	(0.010)	(0.060)	(0.193)	(0.189)	(0.018)
1-3 weeks prior to reform			Reference period		
1-3 weeks after reform signing	0.003	-0.092	-0.013	-0.105	0.007
	(0.010)	(0.056)	(0.183)	(0.174)	(0.018)
4-6 weeks after reform signing	0.016*	-0.096*	0.205	-0.141	0.034*
	(0.010)	(0.051)	(0.193)	(0.204)	(0.018)
7-9 weeks after reform signing	0.017	-0.024	0.411*	-0.350*	0.018
	(0.011)	(0.061)	(0.220)	(0.214)	(0.019)
1-3 weeks after reform implementation	0.032***	-0.202***	0.510**	-0.491**	0.041**
	(0.011)	(0.059)	(0.211)	(0.205)	(0.020)
4-6 weeks after reform implementation	0.022***	-0.015	0.352*	-0.258	0.055***
	(0.010)	(0.065)	(0.209)	(0.203)	(0.018)
7-9 weeks after reform implementation	0.019*	0.003	0.356*	-0.421**	0.043**
	(0.011)	(0.057)	(0.210)	(0.201)	(0.020)
10-12 weeks after reform implementation	0.013	0.006	0.297	-0.476**	0.030
	(0.012)	(0.071)	(0.222)	(0.193)	(0.021)
13-15 weeks after reform implementation	0.019*	0.046	0.545***	-0.456**	0.035*
	(0.010)	(0.072)	(0.204)	(0.200)	(0.019)
Reference period mean	0.000	0.431	810.645	5.014	39.180
N			1,459,814		

Note: Each columns is based on a separate regression with controls and the sample includes conceptions up to 15 weeks before and up to 24 weeks after 6th December in years 2005-2010. The 24-week post-period is divided into 9-week period between signing and implementation of the reform and 15-week period post implementation. Event-time dummies are defined based on 3-week intervals and a period of 1 to 3 weeks prior to reform signing serves as a reference period. Individual controls include maternal age at conception (linear and squared), dummies for her marital status, education, source of income, province of residence, rural areas, number of previous births, and a child's gender. Standard errors in parentheses are clustered at the level of conception day. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.