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Access to Abortion, Investments in Neonatal Health, and Sex-Selection: Evidence from Nepal

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Abstract

Every year, over 9 million children die under the age of five in developing countries, where the abortion regime is generally very restrictive. Evidence from the United States suggests that abortion liberalization may be a powerful policy tool in the fight against mortality in early life. In this paper, I consider the impact of providing affordable, legal abortion facilities in the high-fertility, high-mortality context of Nepal, on pregnancy outcomes, antenatal and perinatal health inputs, neonatal mortality, and sex-selection. In order to exploit geographical and time variation in coverage, I combine fertility histories with a unique data set recording geo-referenced coordinates and registration dates of newly introduced legal abortion centers. Consistent with the prediction that proximity to a legal abortion center reduces the cost of abortion, I find that the probability of a pregnancy ending in a live birth decreases by 8.1 percent, for a given mother. However, there is no evidence that improved access to abortion increases observed investments in antenatal and prenatal care or unobserved investments favorable to neonatal survival. Access to these legal, first-trimester abortion centers does not appear to have led to more sex-selective terminations.

JEL codes: I12, J13, J16

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1 Introduction

Abortion liberalization took place at the beginning of the 1970s in the United States. The impact of this reform on the "quality" of the next generation has attracted substantial academic interest, and evidence was found that the liberalization favored, amongst others, higher birth weight (Grossman & Joyce (1990); Gruber et al. (1999)), neonatal survival (Grossman & Jacobowitz 1981), and infant survival (Gruber et al. 1999). Grossman & Jacobowitz (1981) even conclude that "the increase in the legal abortion rate is the single most important factor in reductions in both white and nonwhite neonatal mortality rates" between 1964 and 1977 (p.695), dominating not only the other public policies considered in their analysis, but also improvement in maternal schooling and poverty reduction.

The causal nature of the estimates reported in the early literature is unclear, and the findings of more recent, better controlled, studies are mixed (Gruber et al. (1999); Currie et al. (1996)). Indeed, Gruber et al. (1999) find compelling evidence of positive selection on living conditions following abortion legalization, but their results for infant mortality and birth weight are less robust. Furthermore, findings by Currie et al. (1996) do not support the hypothesis that abortion funding restrictions have a negative impact on birth weight. However, the debate started in the context of developed countries such as the United States raises the question of whether improved access to abortion may contribute significantly to enhancing child health in the developing world, where 9.1 million children still die under the age of five every year (Loaiza et al. 2008). Most of these deaths occur in Sub-Saharan Africa and South Asia where, contrary to Nepal, abortion is generally not legal without restriction as to reason,[1] and safe abortion is only available at high expense. It is particularly important to shed light on the potential role of abortion reform in decreasing neonatal mortality, as there has been less success over the years in reducing the incidence of deaths in the first four weeks of life, which now constitute over a third of all child deaths (Lawn et al. 2005).

There are two main channels through which reducing the psychological or financial cost of abortion can affect average child outcomes. The first is a "behavioral" channel, through which parents can terminate a pregnancy if it is untimely or has other characteristics that parents find undesirable (i.e., through selection on *pregnancy* characteristics including fetal health), and substitute investments in child health for quantity of children (Becker & Lewis (1973); Willis (1973)).[2] The second is a parental "composition" or "parental selection" effect, which occurs if the abortion price shock disproportionately reduces the birth rate amongst parents who have systematically worse or systematically better child outcomes. The first of these two effects is unambiguously positive, but the

[1] Cape Verde and South Africa are the only exceptions outside Nepal (Center for Reproductive Rights 2008).

[2] This behavioral effect corresponds to what Pop-Eleches (2006) refers to as the "unwantedness" effect (p.747).

sign of the second effect is unclear, and so both can go in opposite directions, as dramatically illustrated by the abortion ban introduced in the late 1960s in Romania. In this case, Pop-Eleches (2006) shows that, overall, educational and labor market outcomes improved after the ban, but that they worsened once a range of parental socioeconomic characteristics are controlled for, because urban, better-educated women used abortion more frequently before the ban. It is therefore important to empirically distinguish between these two channels.

Many countries facing high child mortality today, and where abortion reform may have the largest effects, are also characterized by a degree of son-preference, so that gender-specific concerns arise with respect to abortion liberalization. The main concern is that abortion liberalization may increase sex-selective abortions. According to Lin et al. (2008), this was the case in Taiwan, where abortion was liberalized up to the 24th week of gestation, and so could easily be combined with sex-detection. In Nepal, however, during the period covered by the data used in this paper, legal abortion centers were only authorized to carry out first trimester abortions. Sex-detection technology reliable under 12 weeks of gestation is costly and not widely available in this country, so that access to legal abortion centers may in fact decrease sex-selection if some women substitute early, legal abortions for illegal ones.

Few studies have shed light on the impact of abortion laws on health in early life for countries outside the United States. Lin et al. (2008) study the impact of abortion liberalization in the mid-1980s in Taiwan. In the absence of within-country variation in exposure to this legal change, they focus on sex-differentiated effects of the reform, e.g., the change in neonatal mortality of girls relative to boys. They find that, in Taiwan, the liberalization of abortion increased sex ratios (defined as the ratio of male to female births), reduced female neonatal mortality relative to boys, but had no effect on sex-differential antenatal care and infant mortality. In a study focussing on educational and labor market outcomes, Pop-Eleches (2006) also shows graphical evidence of an increase in low birth weight and infant mortality following an unexpected abortion *ban* in Romania, which led to a near doubling of the fertility rate between 1966 and 1967.

In this paper, I consider the impact of providing affordable, legal abortion facilities in the high-fertility, high-mortality context of Nepal, on pregnancy outcomes, antenatal and perinatal health inputs, neonatal mortality, and sex-selection. In order to exploit geographical and time variation in coverage, I combine fertility histories from the 2006 Nepal Demographic and Health Survey (DHS), administrative data on registration dates of all legal abortion centers corresponding to the period covered by this DHS, and GIS coordinates of each of these abortion centers, based on unique data collected purposefully for this study. Contrary to previous analyses, I identify the within-mother, behavioral response to improved access to abortion by comparing siblings conceived before and after the opening of a legal abortion center nearby, in a difference-in-difference setting. Therefore, I can first control for-, and then analyze changes in the composition of mothers along unobservable characteristics.

Consistent with the prediction that proximity to a legal abortion center reduces the cost of abortion, I find that a pregnancy is less likely to result in a live birth when it occurs closer to a legal abortion center. However, there is no evidence that improved access to abortion reduces neonatal mortality. Similarly, improved access to abortion does not appear to increase observed investments in antenatal and perinatal care. These results add to the doubts shed in Currie et al. (1996) on the empirical link between abortion reform and health in early life, and more generally, on the existence of a quantity-quality trade-off relevant to health up to the first month of life.

I do not find support for the hypothesis that legal abortion centers in Nepal have led to more sex-selective terminations. Indeed there is some suggestive evidence that improved access to early abortions in a regulated environment may reduce sex-selection.

The rest of the paper is organized as follows. Section 2 reviews the literature, Section 3 gives useful background on the abortion reform in Nepal, Section 4 describes the data and empirical strategy, Section 5 reports within-mother estimates, Section 6 analyzes the sensitivity of these results to alternative specifications, Section 7 studies compositional effects, and Section 8 concludes.

2 Summary of the Literature

In this section, I first review the literature on the impact of access to abortion on pregnancy outcomes, before turning to the effect on child health and, finally, on sex-selection.

In the US, the total number of abortions has been found to increase with legalization (Ananat et al. 2009) and to decrease with MEDICAID funding restrictions (Levine et al. 1996) and terrorist attacks against abortion clinics (Jacobson & Royer 2011). In Romania, the abortion ban introduced in 1966 resulted in a large increase in birth rates in the short run (Pop-Eleches 2006), whilst the lifting of the ban in 1989 had the reverse effect, albeit less marked (Pop-Eleches 2010). It is interesting to note, however, that demographers have estimated that about two thirds of legal abortions following legalization in the US replaced illegal ones (Tietze (1973); Sklar & Berkov (1974)).

Several studies have documented the correlation between access to abortion and child outcomes outside the United States.[3] Dytrych et al. (1975) for the Czech Republic and

[3]For conciseness, I do not review here studies comparing children according to whether or not their mothers reported them to have been unwanted or unintended. Rosenzweig & Wolpin (1993) and Joyce et al. (2000) illustrate the doubts regarding the causal nature of the relationships estimated in this literature. Rosenzweig & Wolpin (1993) show that ex-post unwantedness is affected by child endowments and is systematically higher than ex-ante unwantedness, and both this study and that of Joyce et al. (2000) find that the relationship between wantedness and investments in child quality, including prenatal and infant care, is not robust to the inclusion of controls for parental characteristics.

Bloomberg (1980*a*) and Bloomberg (1980*b*) for Sweden have compared outcomes of children whose mothers were denied legal abortion with a control group of children whose mothers did not request abortion. The treated and control groups in these studies are likely to differ in a number of ways that matter for the outcomes of interest, but the authors find that pregnancy outcomes, including height and weight at birth, were no worse for children whose mothers were refused a legal abortion. However, these children were significantly more likely to perform less well at school and to have adverse behavioral outcomes and poor mental health later in life.

The econometric literature on the impact of abortion on early health outcomes in the United States, where most research has concentrated, can be divided into three main groups.

Studies belonging to the first group regress the neonatal mortality rate on aggregate regressors, including some measure of the abortion rate at the county-, health area- or state-level, using cross-sectional data (Grossman & Jacobowitz (1981); Corman & Grossman (1985); Joyce (1987)). These studies find that increases in the abortion rate lead to a large and significant decrease in neonatal mortality. However, there are doubts as to whether the inclusion of lagged neonatal mortality (Grossman & Jacobowitz (1981); Corman & Grossman (1985)) or attempts at instrumenting the abortion rate with socio-economic indicators, family planning and abortion availability (Joyce (1987)) satisfactorily remove concerns over the endogeneity of the abortion rate.

The second group consists of cross-sectional, microeconometric analyses based on structural models aimed at testing whether unobserved attributes of mothers who abort and selection on fetal health lead to a positive selection effect on prenatal care and/or birth weight (Grossman & Joyce (1990); Joyce & Grossman (1990)). Findings suggest that such positive selection indeed occurs, but these results rely on strong identifying assumptions since it has to be assumed that some variables (e.g., availability of abortion providers) are uncorrelated with prenatal care and birth weight over and above their effect on the likelihood of an abortion.

Finally, the third and most recent group adopts a quasi-experimental approach. In an individual random effects model using longitudinal data, Currie et al. (1996) find that more deprived women were less likely to give birth in states where laws restricting MEDICAID funding of abortion were passed and enforced, compared to states where similar laws were passed but could not be implemented due to (arguably exogenous) court rulings. However, they find no difference in birth weight between the two groups of states, thus concluding that abortion funding restrictions had no effect on birth weight. Currie et al. (1996) control for a number of observable maternal characteristics, and so their findings suggest that the behavioral response to a restriction in abortion funding is weak at best. Gruber et al. (1999) exploit the fact that some US states legalized abortion before the Roe v. Wade supreme court ruling of 1973 that legalized abortion in the whole country. In a state panel analysis based on census data and vital statistics, they find robust evidence that state average child living conditions improved following legalization, but infant survival and birth weight are only found to improve in a subset of specifications. Their results therefore show a clear pattern of positive selection on observable parental characteristics, but their estimates for infant mortality and birth weight cannot be clearly interpreted as *parental* selection, since these outcomes are also influenced by changes in parental behavior.

To the best of my knowledge, only one econometric study has shed light on the impact of the abortion regime on health in early life for a country outside the United States, or on the sex ratio. Lin et al. (2008) study the impact of abortion liberalization in the mid-1980s in Taiwan. In the absence of within-country variation in exposure to this legal change, they focus on sex-differentiated effects of the reform, e.g., the change in neonatal mortality of girls relative to boys. Using natality files from Taiwan, they find that the liberalization of abortion up to the 24th week of gestation nearly accounts for the entire increase in sex ratios in the country during the period covered (1982-89), and that it reduced female neonatal mortality relative to boys, but had no effect on sex-differential antenatal care and infant mortality.

Two recent studies have analyzed the impact of improved access to *sex-detection technology* on female foeticide in India and China and found that the spread of ultrasound technology dramatically increased sex ratios in both countries (Chen et al. (2010); Bhalotra & Cochrane (2010)).

But contrary to China, India, and Taiwan, there is no systematic evidence of sex selection in Nepal, although the country is usually considered at risk of anti-girl child bias and sex-selection (Craft (1997); Oster (2005); Hesketh & Xing (2006)), and there are concerns that sex-selective abortion may become more prevalent with the recent change in abortion regime (CREHPA 2007).

A simple graph representing sex ratios at birth by parity over time based on DHS data suggests that sex selection *is* taking place in Nepal (Figure 1). With improved access to sex-detection tests, and more especially in the context of South Asia, to ultrasound technology, sex-selective abortions should increase. The pattern observed in Figure 1, i.e., an increase in the proportion of boys over time at higher parities, is consistent with this hypothesis. [4]

In the case of Nepal during the period covered here, legal abortion centers were only

[4] If mothers prefer sons, then they may be more likely to report an additional son than an additional daughter, and so survey-based sex ratios by parity may reflect this sex-specific reporting bias rather than genuine sex selection. For the pattern in Figure 1 to be due to sex-specific reporting bias, women with more recent births of a given parity would have to exhibit stronger sex-specific reporting bias. This would be the case if younger women had a stronger preference for sons. This is not very plausible, however, since a least squares regression of the woman's self-reported ideal number of sons on her age at interview and her self-reported ideal total number of children shows that older women report significantly higher ideal male-to-female offspring ratios. Estimation results not reported here are available from the author.

authorized to carry out first trimester abortions. There is evidence that this restriction held in practice. Out of the 4,245 clients of legal abortion centers interviewed in MHP & CREHPA (2006), no woman reports having received an abortion after 12 weeks (p. 16). Although women may under-report the gestational length of aborted pregnancies, the absence of any reported post-first trimester abortion gives support to the idea that the regulations were generally observed.

In some specific cases, sex-prediction techniques can predict gender accurately during the first trimester. Chorionic villus sampling is an expensive method but is available in neighboring India and is normally performed around the 10th week of gestation (Retherford & Roy 2003). More common, relatively inexpensive ultrasound techniques may also be able to predict gender accurately at 12 weeks of gestation with good quality equipment and experienced technicians (Retherford & Roy (2003); Efrat et al. (2006)), and there is anecdotal evidence that sex-determination tests are sometimes carried out at the end of the first trimester in Nepal (CREHPA 2007). But to the extent that sex-detection technology reliable under 12 weeks of gestation is costly and not widely available in this country, access to legal, first-trimester, abortion centers is unlikely to have contributed to biasing sex ratios.

3 Abortion Reform in Nepal

Nepal has been known for its high level of maternal and child mortality, as well as its particularly harsh stance on abortion. In 2001-2005, neonatal mortality was 3.3 percent, under-5 mortality 6.1 percent and the maternal mortality ratio for 199-2005 was estimated at 281 deaths per 100,000 live births (MOHP and New Era and Macro International Inc. 2007). Until the 2002 abortion law reform, women who aborted were not uncommonly sent to prison for infanticide (Ramaseshan 1997).

In March 2002, the House of Representatives passed a law authorizing abortion on demand up to the 12th week of pregnancy, in case of conception by rape or incest up to the 18th week, and at any time during pregnancy on specific grounds (e.g., on the advice of a medical practitioner, or to preserve the health of the mother, or in case of foetal impairment) (MHP et al. 2006). This came into effect upon signature by the king on 27th September 2002 (10/06/2059 in the Nepalese calendar, which will be used throughout the empirical analysis as this is the calendar used in the Demographic and Health Survey of the country).

The legislative reform voted in March 2002 encompasses a broader range of women rights issues. In particular, it also improves women's property rights through both inheritance and marriage and entitles her to part of the husband's property in case of divorce.[5]

[5]The civil code amendment establishes a wife's equal right to her husband's property immediately after marriage, rather than after she reaches 35 years of age or has been married for 15 years as before.

These further changes in the law may improve neonatal health in their own right, through an increase in the mothers' negotiation power within the household.[6] However, nearly 18 months went by between the enactment of the law and the opening of the first legal abortion service in March 2004 (12/2060 in the Nepalese calendar). An abortion service is legal if both the provider and the facility are approved by the health authorities. Facilities need to apply for approval or "registration", and training is publicly funded for public facilities.[7].

As many as fifty out of 75 districts had at least one listed legal abortion service (called Comprehensive Abortion Care or CAC center) by March 2005, and by July 2006, 68 out of 75 had one such service (MHP et al. 2006). Figure 2 illustrates the geographical expansion of these centers. Between March 2004 and February 2005, 7,496 women received CAC services, and this number has grown exponentially to reach a total of 43,400 by the end of April 2006 (MHP et al. 2006). DHS data collection took place between January and July 2006. The last 28 months preceding completion of the 2006 Nepal DHS therefore coincided with a period of geographical expansion in access to legal abortion, which I exploit in order to estimate the effect of improved access to affordable, safe abortion.

The cost of an abortion in a legal center ranges from Rs800 to Rs2000 (USD11.33 to USD28.33) (MHP & CREHPA 2006). This can be compared with a mean household income of Rs51,978 per annum in the last living standards survey (Central Bureau of Statistics (2004), p.37). It is difficult to compare the cost of a legal abortion with that of an illegal abortion due to lack of data. The five case studies in MHP et al. (2006) indicate very varied costs for illegal abortions (Rs200, Rs500, Rs700, Rs3000, Rs8000). Taken together, the information in MHP et al. (2006) suggests that the cost of an abortion in a CAC center is higher than illegal alternatives at the low end of the scale (e.g., village abortionist using traditional methods), but much cheaper than illegal abortions carried out in modern facilities. The financial cost of abortion is only part of its total cost, which also includes psychological and health costs. Given that the inexpensive options for illegal abortions are not infrequently life-threatening, post-abortion complications common, and that costs incurred for post-abortion emergency care are high (Rs2000 to Rs5000 in the case studies in MHP et al. (2006)), it seems reasonable to hypothesize that access to CAC centers reduces the perceived cost of abortion. Results in Section 5.1 confirm this hypothesis.

[6] See Hoddinott & Haddad (1995) and Duflo (2003) for examples of the literature on the impact of female bargaining power on child health.

[7] The majority of legal abortion centers opened as of the last pregnancy in the DHS are government-run (62 percent). 29 percent of these centers are run by NGOs, whilst only 9 percent are private sector facilities.

4 Data and Estimation Framework

4.1 Data

4.1.1 Individual Data

As in many developing countries, in Nepal only a minority of births (35 percent of recent births according to MOHP and New Era and Macro International Inc. (2007)), let alone pregnancies, are recorded in official logs, and so one has to rely on survey data. Demographic and Health Surveys have been carried out in a number of developing countries as part of the Measure DHS project, a worldwide USAID-funded project aimed essentially at providing detailed, reliable information on fertility, family planning, maternal and child health and mortality.

The third and latest DHS carried out in Nepal took place in 2006. It collected data from a nationally representative sample of women aged between 15 and 49. Respondents were asked about their entire fertility history, including dates of all births and deaths of any liveborn child and dates of start and end of all other pregnancies. The questionnaires contain a number of probes for these, and enumerators are specifically trained to ensure that this information, that is central to the survey, is reliable [8]. This allows one to create a panel dataset where mothers are the cross-sectional units and pregnancies the "longitudinal" unit, as in Bhalotra & van Soest (2008).

Due to the retrospective nature of the data, there may be measurement error in the dependent variable. Births and deaths of children are key events in the life of a woman, and should therefore be prone to less recall bias than economic variables such as income. Beckett et al. (2001) find that recall error in fertility histories is not an issue for live born children, except for some age heaping (e.g., rounding at one year old for children who die when 11 or 13 months old). As a consequence, I allow for age heaping such that the neonatal mortality indicator switches on for children who were reported to be up to one month old at the age of death.[9] I also address this issue by restricting the analysis to children born no longer than 15 years before the date of the interview. Note that since legal abortion centers opened within less than 3 years of the survey date, reporting error in dates of birth and pregnancy loss, which are used to create the treatment variable, should be minimal where it matters for the definition of treatment status.

Data on pregnancies that do not result in a live birth are prone to more measurement error, especially in the form of underreporting (Beckett et al. 2001). Comparisons between survey and administrative data in the US have shown that induced abortions there are largely underreported (Jones & Darroch Forrest 1992). In the 10 years preceding the 2006 Nepal DHS, 9.7 percent of pregnancies were reported not to end in a live birth,

[8]See MOHP and New Era and Macro International Inc. (2007) for more information.

[9]Strictly speaking, neonatal mortality relates to mortality in the first 4 weeks of life.

including only 2.4 percent reported induced abortions (MOHP and New Era and Macro International Inc. 2007). In the DHS, women were asked to report each of their pregnancies in turn, and, one by one, whether the baby was "born alive, born dead, or lost before birth". If they answered either of the two last options, the respondents were then asked about the month and year the pregnancy ended and its duration. Only then were they asked whether they or someone else had done "something to end this pregnancy" (MOHP and New Era and Macro International Inc. 2007). It is therefore likely that overall fetal loss is less underreported than induced abortions in the data at hand, and so I focus on the impact of access to abortion on the overall probability that a pregnancy ends in a live birth. I also provide a robustness check relying only on live birth data, as described in Section 4.2.

In addition to the fertility histories obtained from the women interviewed, the DHS also collected more detailed information on prenatal and perinatal care for the subsample of children born up to five years before the date of the interview. Prenatal care variables were only collected for the last birth (if it occurred within five years of the interview), which precludes the use of mother fixed effects techniques.

Finally, the DHS also collected GPS coordinates for each sample cluster (which are 260 in total), so that it is possible to compute the distance between the place of residence of women in the DHS and each CAC center.[10]

Adjustments made to the original sample of 28,740 pregnancies are as follows. I keep only singletons as is common practice in demography because multiple births can bias estimates (thus dropping 434 pregnancies), I restrict the sample to children born no longer than 15 years before the date of the interview to reduce recall error (9590 pregnancies), and keep only pregnancies that could have been carried to term by the time of interview (thus excluding 81 pregnancies occurring within 9 months of the survey date). Finally, in order to limit measurement error in the measure of access to abortion at the time of conception, I drop 596 pregnancies of mothers who do not normally reside in the place where they are interviewed, and children conceived before their mother moved to the place where she is interviewed (2787 pregnancies).

The final pregnancy sample thus comprises 15252 pregnancies of 5944 mothers. The largest sample used in the gender analysis only comprises pregnancies that ended in a live birth (13958), whilst the largest sample used in the neonatal mortality analysis (13863) further excludes 95 live born children who had not been born for at least a complete month at the time of interview, and so were potentially not fully exposed to the risk of neonatal death.

Most regressions presented in the paper have fewer observations because the sample is restricted to pregnancies with at least another sibling in the sample, for comparability

[10]Publicly available locations for this survey are "scrambled", which may lead to attenuation bias. GPS coordinates used here are un-displaced coordinates.

between within-mother and within-cluster estimates. In low-fertility countries, the requirement that individuals have at least another sibling in the sample is quite restrictive. However, in the present sample of mothers, the average number of live births per mother is 3.5, and so restricting this sample to mothers with at least two children is likely to yield results that are valid for a large share of the population. This restriction will however disproportionately exclude younger women, who have only recently initiated childbearing. In order to ascertain that this restriction is not driving my results, where relevant I present results for both the within-mother sample and the whole sample.

4.1.2 Abortion Services Data

Dates of CAC registration were obtained from official government records provided by the Ministry of Health and Population, who also provided contact details for each center. One hundred and forty one CACs were registered by June 2006, of which 101 were registered by the start of the last pregnancy recorded in the DHS that could have been carried to term by the time of interview (i.e., conceived at least 9 months before). Except for 2 of the 141 CACs, one which could not be reached, and one that did not appear to have ever existed, all were surveyed.

A telephonic survey of all CAC facilities registered by June 2006 was carried out by the Center for Research on Environmental, Health and Population Activities (CREHPA) to obtain data on the precise location of each CAC. The information on CAC location was then used by the GIS Society of Nepal to map the facilities' GIS coordinates.

The survey also collected qualitative data on distance traveled by CAC clients, in order to inform the choice of a distance cut-off to define the catchment area of abortion centers. More specifically, the following two questions were put to CAC representatives: "In your opinion, how far does your average patient travel from to get an abortion?" and "In your opinion, what is the furthest distance that your clients travel to get an abortion?". Excluding one outlier, the mean answer to the first (second) question was about 16 (86) kilometers, based on 136 (134) non-missing answers.

In order to shed some light on the determinants of the timing of the opening of CACs, the survey asked whether there was "a reason why this CAC center opened on (reported date) rather than opening a few months before or after?" and, to probe negative answers to this question, whether "it would have been possible to open the CAC center" at the reported date minus three months. Follow-up questions then asked about the reason(s) why this was the case, and of the 117 out of 139 surveyed centers for whom respondents said there was a reason (only 1 "did not know"), 91 said it was because they were awaiting official registration, and 68 said it was because they were waiting for a trained abortionist to be transferred to their facility. Only 5 reported different reasons. Although applications for CAC registration are likely to be at least partially determined by demand (and thus caused by characteristics of local women), this suggests that the timing of opening is more exogenous.

Finally, respondents were asked a series of questions aimed at establishing whether the opening of CAC centers could have led not only to a decrease in the cost of abortion, but also improved access to other services relevant for fertility and early life health. Out of 139 abortion centers, 114 opened in facilities that already existed and 32 started offering post-abortion care services, which contributes to the decrease in the cost of abortion. Interestingly, only 8 started offering maternal and newborn health services, and only 6 of these centers and another 2 started offering contraception services. The estimated effect of proximity to a CAC center is therefore unlikely to be biased by the simultaneous provision of other health services.

4.2 Estimation Framework

An inspection of Figure 2 shows that the area surrounding the capital Kathmandu was better supplied with CAC centers at first, whilst access to CACs was slower to arrive to mountainous regions and to some districts very affected by the civil conflict that took place between 1996 and 2006 (Rukum, Rolpa). Table 1 confirms that women who live nearer a legal abortion center are on average wealthier, better educated, more urban, live in less mountainous regions, and report lower ideal numbers of sons and daughters. I address this heterogeneity in a number of ways. First, I use either maternal- or cluster-fixed effects estimation, which controls for time-invariant unobserved heterogeneity of women in localities gaining access to CACs. Table 9 shows that, controlling for location fixed effects and pregnancy variables, mothers of children conceived nearer a legal center are no different to the rest of the sample along these characteristics. Second, I include a treatment-specific quadratic time trend allowing for the possibility that women gaining CAC access also experience a different trend in the outcome variable. Third, I present results of a placebo experiment in which the treatment dummy is not based on actual registration dates, but these dates minus 12 months. Fourth, I present results for the sample of mothers who gain access to a CAC at some point during the period covered by the data. Fifth, I estimate a variant of the main specification in which I control for the number of civil-conflict related deaths in the district by the time of conception and during pregnancy.

I start by focussing on the "behavioral" effect of improved access to abortion by estimating maternal fixed-effects equations of the form:

$$\begin{aligned} y_{imct} = {} & \beta_0 + \beta_A A_{ct} + X_{imct}\beta_X + Y_t\beta_Y \\ & + \beta_t Trend_{ct} + \beta_{t2} Trend^2_{ct} + M_m + u_{imct} \end{aligned} \tag{1}$$

Where i indexes pregnancies, m indexes mothers, c indexes DHS cluster, t indexes date of birth. A_{ct} is a treatment dummy for "access to a legal abortion center" defined below, y_{imct} is, in turn: a dummy equal to one if a reported pregnancy leads to a live birth, and zero otherwise; a dummy equal to one if the index child dies by age one month, and zero otherwise; measures of antenatal and perinatal care; a dummy equal to one if the index child is female, and zero otherwise. X_{imct} is a vector of child-specific regressors, namely: pregnancy order, age of mother at conception and its square, number of siblings alive at the time of conception, gender (for explained variables defined over the sample of pregnancies carried to term), and calendar month of birth dummies.[11] Y_t is a set of conception year dummies, M_m a set of individual maternal effects, u_{imct} an error term assumed independent between clusters but not necessarily within cluster, and $(\beta_0, \beta_A, \beta_X, \beta_Y, \beta_t, \beta_{t2})$ are parameters to be estimated. $Trend_{ct}$ is a linear trend specific to areas with access to a CAC center at some point during the period covered by the data, and is included in all baseline specifications except for outcomes variables only defined for recent births.

A_{ct} is equal to one if pregnancy i occurring to mother m at date t (defined by month and year) starts at a time when the mother lives close to a legal abortion center. This treatment variable thus varies by locality (i.e., DHS cluster) and month of conception. In the main set of results, closeness to a CAC center is defined as being no further than the median distance to the nearest center for pregnancies conceived after the registration of the first CAC, namely 28.6 kilometers. This choice appears reasonable in view of the distance traveled by abortion clients according to the survey of CAC facilities (see Section 4.1.2). It is somewhat arbitrary, but a range of sensitivity checks are provided in Section 6, namely: regressions where A_{ct} is replaced by three treatment variables corresponding to the different quartiles of the distribution of distance to the nearest CAC center, an alternative specification including A_{ct} as well as a continuous variable equal to distance to the nearest registered center at the time of conception, and point estimates for (76) variants of Equation 1 in which the cut-off used to define A_{ct} corresponds to the 5th percentile of the distribution of distance to the nearest CAC center, then the 6th percentile, etc... until the 80th percentile.[12]

As long as there is no selection into being conceived near a CAC center on *time-varying* factors affecting the outcome of interest after controlling for maternal time-invariant characteristics, pregnancy characteristics X_{imct} and a quadratic trend specific to locations in the catchment area of a CAC, β_A identifies the *behavioral* effect of the decrease in the cost of abortion, i.e., net of changes in the composition of mothers. For instance, when

[11] When a gender control is included in the regression, the estimated treatment effect is net of any effect mediated by gender. Excluding this control does not affect my conclusions.

[12] If the magnitude of the treatment effect is constant and the catchment area is misspecified, the effect of CAC centers is underestimated. This is the case either because women outside the assumed catchment area are treated but included in the control group (when the hypothesized radius is shorter than the true one) or because untreated observations are considered treated (when the hypothesized radius is too long). However, if the magnitude of the treatment effect decreases with distance to CAC and the size of the catchment area is assumed to be smaller than it is in reality, the treatment effect is overestimated.

y_{imct} is a neonatal mortality indicator, β_A identifies the effect due to changes in parental investments in neonatal health and in the ability of parents to select on fetal health.

A reduction in the cost of abortion could lead to more pregnancies if abortion is substituted for contraception. In addition, as clarified by Ananat et al. (2009), parents have a more complete information set when faced with the choice of whether or not to abort (e.g., they may know the gender of the fetus) than at the time they decide whether or not to use contraception. Following Ananat et al. (2009), the potential cost of an abortion can be seen as the cost of purchasing the option of giving birth, once additional information has been gathered. As the cost of abortion falls, there may be an increase in the probability of a pregnancy, whilst the likelihood of a birth conditional on pregnancy decreases, and so the birth rate may decrease or not, depending on whether or not marginal pregnancies match the number of marginal births. But in either case, the "quality" of the next cohort should increase.

To check the robustness of my findings on pregnancy outcomes to the misreporting of fetal loss, I estimate the impact of the decrease in the cost of abortion on the unconditional probability of giving birth, *using only data on live births.* A decrease in this unconditional probability would indicate that the additional number of abortions is larger than any increase in the number of pregnancies.

More specifically, I estimate the probability, in any given month, for a woman to become pregnant with a child who will be born alive. This is done by nesting the data on live births reported in the DHS within a panel defined by mothers as cross-sectional units and century months as longitudinal units, and estimating the following equation:

$$\begin{aligned} C_{mct} = {} & \gamma_0 + \gamma_A A_{ct} + X_{mct}\gamma_X + T_t\gamma_T \\ & + \gamma_t Trend_{ct} + \gamma_{t2} Trend_{ct}^2 + M_m + \nu_{mct} \end{aligned} \tag{2}$$

Where t now indexes century month (i.e., month 3 of year 2060). C_{mct} is a dummy equal to one if a live born child was conceived in month t by mother m, A_{ct} is a treatment dummy equal to one if the mother is in the catchment area of a CAC center in month t, X_{mct} is a vector comprising maternal age and its square, potential birth order (if a child was conceived at date t by mother m and was carried to term), a dummy equal to one if mother m was married at date t, a dummy equal to one if either the mother or her husband were sterilized at date t, and a dummy equal to one if the mother is pregnant with a future live born child during the index month or has given birth during that month. T_t is a vector of century months fixed effects, and ν_{mct} a residual term that is assumed to be uncorrelated between clusters, but not necessarily within cluster.

The linear probability model (LPM) is preferred despite the binary nature of most explained variables considered in this paper because one cannot obtain the marginal effect of regressors in the conditional logit model without making arbitrary assumptions regarding the value of the fixed effects, which are not estimated in the conditional logit. Whilst the linear approximation is straightforward for binary outcomes that are strongly balanced in the sample (e.g., gender), it is necessary to confirm that the sign and significance of the LPM findings hold when using conditional logit, which I do in Section 6.

Parental composition effects of improved access to abortion are analyzed in Section 7. I first investigate explicitly differences in behavioral responses to proximity to a CAC center by estimating variants of Equation 1 in which y_{imct} is an indicator for a pregnancy ending in a live birth, and the treatment dummy, as well as all other regressors, are interacted with maternal characteristics of interest, namely, in turn, asset ownership and education. I then test whether women with certain attributes which are more or less favorable to child health are more likely to be represented amongst mothers who give birth after having experienced a decrease in the cost of abortion. This is done by comparing the maternal fixed effects estimates with cluster fixed-effects estimates obtained by estimating models of the form:

$$\begin{aligned} y_{imct} = \delta_0 + \delta_A A_{ct} + X_{imct}\delta_X + Y_t\delta_Y \\ + \delta_t Trend_{ct} + \delta_{t2} Trend_{ct}^2 + C_c + \mu_{imct} \end{aligned} \tag{3}$$

where C_c is a set of cluster effects. If mothers who respond more to legal abortion centers have characteristics (other than place of residence) systematically correlated with the outcome of interest (e.g., neonatal mortality and child gender), then δ_A will differ from β_A. I also estimate variants of Equation 3 with added maternal characteristics to shed light on the respective role of compositional changes in observable and non-observable characteristics.

Summary statistics for all the variables used in the regressions can be found in Tables 2 and 3. Table A-1 provides further detail of variable construction.

5 Behavioral Response to Proximity to a Legal Abortion Center

5.1 Effect on Pregnancy Outcomes and Fertility

Before turning to the impact of improved access to abortion on investments in neonatal health, it is important to check that the opening of the legal abortion centers has indeed decreased the cost of abortion.

Table 4 contains mother-fixed effect estimates of the impact of access to a legal abortion center on pregnancy outcomes and fertility. Column 1 presents estimates of Equation 1 where the dependent variable is a dummy equal to one if the pregnancy results in a live birth, and Column 4 reports estimates of Equation 2. Proximity of a CAC center at the time of conception reduces the probability of a pregnancy ending in a live birth by 7.4 percentage points (8.1 percent of the mean), and the probability of conceiving a child who will be carried to term in any given month decreases by 0.4 percentage points (20 percent of the mean) for a month in which the woman has a CAC nearby. Both findings are significant at the 1 percent significance level.

Other results are consistent with the expectation that the likelihood of unintended pregnancy loss decreases with pregnancy order. An additional sibling alive at the time of conception decreases the probability of a live birth by 12.6 percentage points, confirming that a sizeable share of pregnancy loss is due to induced abortions. Findings for the unconditional probability of a live birth (Column 4) are as expected: as a given women has more live births she becomes less likely to have any more; the probability that she gives birth increases between 15 and 30 years old, but then decreases as she becomes older; being married has a huge positive effect on the likelihood of having a child; and reported sterilization of either partner suppresses the probability of a conception.[13] Being pregnant with another future live birth or having just delivered has a large effect (about 31 percent of the mean), but not as large as sterilization because this indicator is equal to zero when women are pregnant with a child who was not born alive.

A somewhat surprising finding is that of a U-shape relationship (with a minimum at 38) between maternal age and the likelihood of giving birth, conditional on being pregnant (Column 1). This is more likely to be due to differential reporting of fetal loss than biology. This pattern is, for instance, compatible with underreporting of fetal loss at the two extremes of the reproductive period.

The results reviewed so far present evidence that having a legal abortion center nearby decreases the likelihood of giving birth for a given woman, conditional on being pregnant or not. The next section investigates whether this leads to an increase in the "quality" of children through changes in parental investments in health inputs up to the neonatal period.

5.2 Effect on Neonatal Health

Results in Table 5 indicate that children who were conceived closer to a legal abortion center are not significantly less likely to die by age one month compared to their siblings (Column 2). If there were a degree of substitution of gender selection to discrimination on neonatal health inputs, then we would expect the distribution of quality amongst girls who are effectively born to shift by a larger amount than amongst boys. I test this hypothesis by interacting all regressors with a dummy for female gender. However, there is no evidence of gender-differentiated effects on neonatal mortality (Column 3).

[13] 2.8 percentage points correspond to more than 100 percent of the raw probability of conception in any given month.

Other results are consistent with the well-known female advantage in neonatal survival. There is an inverted U-shaped relationship between maternal age at conception and neonatal mortality (with a maximum at 20 years old). The probability of neonatal mortality decreases with pregnancy order, but increases with the number of siblings alive at the time of conception.

I also estimate the impact of the reduction in the cost of abortion on size at birth, which is available for births occurring up to five years before the survey. Birth weight is one of the most commonly used indicators of health at birth. In Nepal however, over 80 percent of births take place at home, and so children for whom we have birth weight data are a very selected sample. Instead, I use the information provided by another DHS question, namely one asking women whether, at birth, the child was "very large", "larger than average", "average", "smaller than average", or "very small", in order to create a dummy equal to one if the child is said to have been smaller than average or very small, and zero otherwise. Access to a CAC has a negative but insignificant effect on small size at birth.

Even in Nepal, neonatal mortality is a relatively uncommon phenomenon (affecting 4.2 percent of births in the sample), and so the present sample size may not suffice to identify the effect of legal abortion centers precisely enough. But the data also allow me to estimate the effect of access to abortion on neonatal health inputs with much higher variation in the data.

For recent births, the DHS provides data on antenatal and perinatal care. Antenatal and breastfeeding information is only available for the latest birth if it occurred in the five years preceding the survey, which prevents estimation by maternal fixed effects. Instead, I present within-cluster estimates described in Equation 3, including controls for maternal characteristics (Table 6), for four key antenatal care variables: the number and timing of antenatal checks, the number of tetanus injections received by the mother whilst pregnant, and whether the mother took iron/folic acid tablets. I also estimate the impact of having a CAC nearby on help with delivery, delivery place, and on the timing of first breastfeeding.[14] Delivery help and delivery place data are available for all births in the five years before the survey, and so for these variables I also present maternal fixed effects estimates, although mothers with more than one pregnancy in the five years preceding the survey may have characteristics not shared by the majority of the population (Table A-2).

There is no indication of increased investments in antenatal or perinatal care. All treatment effects are statistically insignificant, except for a marginally significant increase in the probability of delivering at home in the mother fixed-effects specification (Table A-2). If anything, the signs of the estimates go in the direction of *lower* investments in

[14]Breastfeeding is virtually universal in Nepal, and so I focus here on the impact on timing rather than on whether or not breastfeeding occurs. CAC centers do no affect the likelihood of breastfeeding either.

antenatal and perinatal care.

One additional effect of access to abortion that has received less attention in the literature operates through changes in cohort size (Pop-Eleches (2006); Ananat et al. (2009)). Indeed, there may be *less* pressure on maternal and child health facilities after the arrival of legal abortion facilities. Or one could wonder whether CAC centers also provided antenatal care services. The telephonic survey of CAC facilities indicates that this was not the case (see Section 4.1.2). In both cases, the risk would be to overestimate the positive contribution of CAC centers to neonatal health. This adds support to the conclusion that improved access to abortion did not lead parents to increase investments in neonatal health.

5.3 Effect on Gender Selection

Table 7 shows estimates of the impact of access to a CAC on the likelihood that the index child is a girl. There appears to be a significant, positive effect on the likelihood of giving birth to a female child within mother (Column 1). As discussed in the next section, and contrary to the findings regarding pregnancy outcomes and neonatal mortality, this result is sensitive to alternative definitions of the CAC catchment area, and so caution should prevail in interpreting it. As would be expected from improved access to abortion in the first trimester only, proximity to a CAC center has *not* increased sex selection against girls, and this conclusion is robust to changes in specification.[15] What is less clear is whether or not access to CAC centers has *decreased* the likelihood of a sex-selective abortion.

One channel through which this could be the case is through substitution of early, gender-blind, abortions in legal facilities to later, illegal abortions with sex-selection. Women with strong son preference are unlikely to be affected by improved access to early abortion, and women with no taste for sons have no desire to sex-select. So, for these two groups of women, access to CAC centers should have no effect on sex ratios. On the contrary, women who are near-indifferent between aborting a male fetus and aborting a female fetus may move away from sex-selective abortions when the cost of first-trimester abortion decreases. A first test for this hypothesis can be performed by regressing the following variant of Equation 1:

$$\begin{aligned} y_{imct} = {} & \beta'_0 + \beta'_{A1}A_{ct} + \beta'_{A2}A_{ct} \times NoBoys_{imct} + \beta'_{A3}A_{ct} \times TooFewBoys_{imct} \\ & + \beta'_{No}NoBoys_{imct} + \beta'_{TooFew}TooFewBoys_{imct} + X'_{imct}\beta'_X + Y'_t\beta'_Y \\ & + Trend'_{ct}\beta'_t + Trend'^2_{ct}\beta'_{t2} + M_m + u'_{imct} \end{aligned} \tag{4}$$

[15] Note that this finding sheds new light on the absence of gender-differentiated effects on neonatal mortality: absent increased sex-selection, there is no reason to expect female neonatal mortality to decrease relative to that of boys.

where $NoBoys_{imct}$ is a dummy equal to one if the mother has no sons alive at the time of conception of the index child, and zero otherwise, and $TooFewBoys_{imct}$ is a dummy equal to one if the mother has fewer sons alive at the time of conception of the index child than she reports as her ideal number of sons, and zero otherwise. $X', Y', Trend', Trend'^2$ correspond to the original set of regressors $X, Y, Trend, Trend^2$ along with their interaction with $NoBoys_{imct}$ and with $TooFewBoys_{imct}$. This specification produces treatment effects for 4 different subgroups, characterized by different degrees of son preference, as summarized in Table 8.

Column (2) of Table 7 shows estimates of Equation 4. Given the comparatively small sample of treated children, it is not surprising to obtain results that are not conclusive when splitting treated observations into different subgroups. Although not statistically significantly different, the point estimates and p-values for each of the four above cases are in line with the expectation that the effect of access to a CAC on gender is larger for pregnancies with some son preference compared both to those with no or very high son preference. In order to increase the precision of the estimates for the treatment interaction terms, one can choose to restrict all other coefficients to be equal across observations, and thus exclude the interaction terms from $X', Y', Trend', Trend'^2$. This is done in Column (3), which reinforces the conclusion that the effect on gender in Case 3 is larger than that in Case 4, and that in Case 3 is larger than that in Case 1, but it is impossible to reject that Case 2 = Case 3. Self-reported fertility preferences are very imperfect measures of fertility preferences, not least because these are influenced by the respondents' fertility history. However, these results give some support to the hypothesis that some women who were near-indifferent between a sex-selective abortion and a gender-blind abortion may have substituted away from the former due to the decrease in the cost of first-trimester abortion.

Other results suggest that, irrespective of access to CAC centers, there is either sex-selection or differential underreporting by gender in the data. Indeed, results in Column (1) show a strongly negative correlation between the number of siblings alive at the time of conception and the likelihood that the child is a girl, and some negative correlation between high pregnancy order and the likelihood that the child is a girl.

6 Robustness of the Behavioral Response Estimates

I first investigate selection into treatment on observable characteristics by estimating Equation 3 on the sample of pregnancies, but defining y_{imct} as, in turn: indicators of maternal socioeconomic status, caste, fertility preferences, and knowledge of abortion legality and of where to get an abortion (Table 9). These indicate that, after controlling for location fixed effects, mothers who become pregnant near a CAC center are similar to the mothers of control pregnancies. Importantly, they are no more likely to say that abortion is legal when asked about it, which suggests that the estimated treatment effect on the likelihood of a pregnancy being carried to term is not driven by mothers of treated pregnancies being more likely to know about the change in the law and thus being more likely to *report* a fetal loss.

Table 10 reports results of a number of robustness checks.

Panel A contains estimates of Equation 1 augmented with a variable equal to the linear distance to the nearest CAC at the time of conception. In Panel B, the binary treatment is replaced with 3 dummies corresponding to the three first quartiles of distance to a CAC center. The omitted category therefore includes pregnancies that occur before the first center opened and those occurring in the fourth quartile of the distance distribution, i.e., more than 52.6 kilometers away. In both cases, the probability for a pregnancy to end in a live birth decreases with proximity to a CAC center (noting the joint significance of the coefficients on A_{ct} and on the linear distance to the nearest CAC in Panel A). In either case, distance to a CAC center does not affect neonatal mortality, similar to the baseline results. The relationship between distance to the nearest CAC and female gender is non-monotonic, with the largest effect observed between about 13 and 29 kilometers. This sheds doubt on whether access to these centers indeed decreases sex-selection, or whether the observed correlation is spurious. Further sensitivity checks echoing these findings are represented in Figure 3, representing point estimates and corresponding 95 percent confidence intervals for each cut-off distance between the 5th percentile and the 80th percentile of the distance to nearest CAC distribution, with and without the quadratic treatment-specific trend. Note that the pattern illustrated in Figure 3 does not necessarily imply that the gender effect obtained in the main regression is a spurious one, since only some women using CAC centers may be substituting away from sex-selection (i.e., those with only moderate preference for a son). The pattern observed here could arise in the presence of a specific type of correlation between distance to the nearest CAC and son preference.

Panel C shows estimates obtained for a control experiment such that the placebo treatment dummy is not based on the actual CAC registration dates, but on the actual dates minus 12 months. It has a zero effect on all three outcomes, which gives support to the interpretation of previous findings as causal in the sense that, if these were due to some time-varying omitted factors, then the simulated treatment would tend to capture the same omitted factors.

Panel D shows estimates of Equation 1 for the restricted sample of women who gain access to an abortion center at some point during the period covered by the data. These confirm the robustness of the estimates in the main specifications.

Panel E contains the results obtained from estimating variants of Equation 1 including two additional controls, namely (i) the cumulated number of conflict casualties in the index child's district at the time of conception and (ii) the average monthly number of conflict-related casualties during pregnancy.[16] The estimates of the effect of access to a CAC center is largely unchanged, confirming the robustness of my findings to controlling for differences in conflict intensity.

Panel F presents maternal fixed effects estimates of the outcome of interest on the treatment dummy and year fixed effects, excluding all other covariates. The results are very similar to the baseline model.

Finally, Panel G reports conditional logit estimates, which show that the sign and significance of the linear fixed effects estimates hold when the binary nature of the explained variable is taken into account.

7 Compositional Effects

In Columns (2) and (3) of Table 4, I investigate differences in behavioral responses to proximity to a CAC center by estimating variants of Equation 1 in which y_{imct} is an indicator for a pregnancy ending in a live birth, and all regressors are interacted with household wealth indicators (Column (2)) or maternal education indicators (Column (3)). Although point estimates tend to be smaller for poor, uneducated women, the results do not suggest a clear pattern as to the observable characteristics of women who respond more to the decrease in the cost of abortion. Splitting treated pregnancies into sub-groups decreases precision, and so it is not possible to reject equality of treatment effects across groups.[17]

For outcomes relevant to children actually born, such as neonatal mortality and gender, the difference between within-mother and within-cluster estimates sheds light on the potentially heterogeneous response to the lower cost of abortion by parents who differ in unobserved determinants of neonatal survival and child gender. The last three columns of Table 5 are cluster fixed-effects estimates of the effect of access to an abortion center on neonatal mortality. These estimates are very similar to within-mother estimates, suggesting that there are no important compositional effects. Results in Columns (6) are obtained when controls for maternal characteristics are added to the specification in Column (4). The treatment effect is little affected, suggesting that changes in maternal composition are small for both observable and unobservable characteristics. Note also the similarity of estimates based on the sample of children with at least one sibling in the

[16] Conflict variables are derived from monthly conflict-related deaths per district of Nepal over the entire conflict period, namely 1996-2006, as collected by the Informal Sector Service Centre (INSEC, Nepal). For a detailed analysis of the impact of conflict on fetal and child health, see Valente (2011).

[17] Another test of changes in the observable characteristics of parents of *born* children consists of estimating Equation 3 on the sample of *live births*, with indicators of maternal socioeconomic status on the left-hand-side. Regressions of this type on indicators for wealth, education, caste and religion do not suggest any such selection.

data (Column (4)) and in the whole dataset (Column (5)), confirming that within-mother estimates are unlikely to be driven by unobserved characteristics of parents with more than one child in the sample.

The last three columns of Table 7 are cluster fixed-effects estimates of the effect of access to an abortion center on female gender. The within-cluster estimates in Columns (4) (without maternal controls) and (6) (with maternal controls) are smaller in magnitude than the within-mother estimates, suggesting that parents who use CAC centers are overall more likely to have daughters (or at least to report having had a daughter). This is consistent with the idea that parents with very strong son preference do not use CAC centers. Within-cluster estimates based on the whole sample (Column (5)) are again smaller than those obtained with the sample of siblings and statistically insignificant (Column (4)). One plausible explanation for the difference in estimates between these two samples is that the siblings sample disproportionately excludes first pregnancies, at which parity sex-selection does not normally occur.

8 Conclusion

Abortion liberalization is believed to have had a sizeable impact on various aspects of human capital in the United States, where most research has concentrated. However, reliable econometric evidence of the impact of improved access to abortion on health in early life is scant, not unanimous, and the estimated effects are difficult to interpret insofar as they encapsulate both behavioral responses of given parents and aggregated effects due to changes in the socioeconomic composition of births due to heterogenous responses to abortion reform.

This paper uses new data on the geographical spread of legal abortion centers in Nepal in order to estimate the impact of improved access to abortion on fertility, investments in health up to the first month of life, and sex-selection, with particular emphasis on distinguishing the within-mother or "behavioral" effect of abortion reform from its effect on parental composition. Consistent with the prediction that proximity to a legal abortion center reduces the cost of abortion, I find that the probability of a live birth conditional on conception decreases by 7.4 percentage points (8.1 percent of the mean), for a given mother. This suggests that, even in developing countries where illegal abortions may be thought to be more common than in developed countries, access to legal abortions contributes to further birth control.

However, there is no evidence that improved access to abortion increases observed investments in antenatal and perinatal care, or that it increases unobserved behaviors favorable to neonatal survival. There is no evidence either of changes in average parental characteristics leading to better average health outcomes, net of potential changes in average location characteristics.

These results add to the doubts shed in Currie et al. (1996) on the empirical link between abortion reform and health in early life and more generally, on the ubiquity of a quantity-quality trade-off (Black et al. (2005); Angrist et al. (2010)).

In Nepal, abortion is available on demand during the first trimester, and although it is still legal at a later stage if approved by a medic, or in case of fetal health impairment, it was not allowed in any of the legal abortion centers opened during the period covered by the data. Although sex-selective abortion is forbidden by law, there is a concern that improved access to abortion may increase (male) sex ratios in countries such as Nepal. However, contrary to findings in Lin et al. (2008) for Taiwan, where sex-selective abortion was not prohibited and the emphasis was not on first-trimester abortions but on abortions up to the 24th week of gestation, access to legal abortion centers in Nepal does not appear to have led to more sex-selective pregnancy terminations. On the contrary, there is limited evidence that it may have led to a decrease in sex-selective abortions, which could be due to the substitution of first-trimester legal abortions to illegal abortions at a later gestational stage, and in environments where the legal ban on sex-selective abortion is less likely to be obeyed.

Several explanations can account for the lack of increased investments in neonatal health observed here. Parents may not perceive antenatal care and other healthy behaviors that matter for neonatal mortality as investments in their child's health, but rather as investments in the mother's health. There may also be barriers to access to antenatal care and delivery facilities preventing parents from adjusting their behaviors. Future research considering investments in child quality beyond the neonatal period may find an effect, since the range of decisions over which the abortion price shock can have an impact increases with the age of the child.

Table 1: Mean Characteristics of Mothers, by Distance to a Legal Abortion Center at the End of the Survey Period

	(1) >28.6 kms of CAC[a]	(2) <=28.6 kms of CAC[a]
=1 if first (lowest) wealth quintile	0.415	0.231
=1 if second wealth quintile⁻	0.235	0.220
=1 if third wealth quintile	0.158	0.202
=1 if fourth wealth quintile	0.139	0.199
=1 if fifth (highest) wealth quintile	0.052	0.148
=1 if no education	0.834	0.689
=1 if Hindu	0.908	0.869
=1 if Buddhist⁻	0.067	0.063
=1 if Other Religion	0.025	0.068
=1 if Brahmin	0.476	0.324
=1 if Madhesi	0.059	0.117
=1 if Dalit⁻	0.131	0.148
=1 if Newar⁻	0.042	0.037
=1 if Janajati⁻	0.285	0.312
=1 if Muslim	0.006	0.045
=1 if other caste	0.001	0.017
=1 if Urban	0.137	0.224
Altitude	982.081	758.016
	(844.341)	(709.537)
Age at interview	32.587	31.807
	(7.353)	(7.184)
Ideal number of girls	1.032	0.962
	(0.538)	(0.500)
Ideal number of boys	1.514	1.433
	(0.689)	(0.738)
Live births at interview	4.225	3.886
	(2.123)	(2.009)
Observations	841	3471

Source: Nepal DHS 2006 and Valente (2010). Figures obtained using one observation per mother. Based on the sample of mothers with at least two pregnancies in the data after the adjustments described in Section 4.1.1. [a] Refers to distance to the closest CAC center opened by the date of DHS interview minus nine months. All differences are statistically significant at 5 percent or less except for variables marked with a ⁻ sign. Standard deviations for non-binary variables are in parentheses.

Table 2: Summary Statistics

	(1) Pregnancies starting >28.6 kms of CAC			(2) Pregnancies starting <=28.6 kms of CAC		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
Pregnancy characteristics						
=1 if Born alive	0.918		14519	0.853		733
=1 if Self-reported induced abortion	0.016		14519	0.045		733
=1 if CAC<=28.6km at conception	0		14519	1		733
=1 if CAC<= 28.6 km by date of interview minus 9 months	0.790		14519	1		733
as above × linear trend	6.603	4.8903	14519	15.386	0.4872	733
as above × linear trend^2	67.508	68.7449	14519	236.969	15.1027	733
Nepali year of conception	2054.4	4.0022	14519	2061.4	0.4872	733
Maternal age at conception	24.231	5.9453	14519	23.888	5.8000	733
=1 if first pregnancy[a]	0.216		14519	0.243		733
=1 if second pregnancy	0.214		14519	0.273		733
=1 if third pregnancy	0.178		14519	0.177		733
=1 if fourth pregnancy	0.132		14519	0.126		733
=1 if fifth pregnancy and above	0.260		14519	0.181		733
Siblings alive at conception	1.793	1.6722	14519	1.532	1.5692	733
Calendar month of conception:						
Baisakh (April to May)[a]	0.093		14519	0.102		733
Jestha	0.088		14519	0.100		733
Asadh	0.081		14519	0.094		733
Shrawan	0.074		14519	0.093		733
Bhadra	0.073		14519	0.072		733
Ashoj	0.088		14519	0.098		733
Kartik	0.086		14519	0.087		733
Mangshir	0.084		14519	0.085		733
Poush	0.089		14519	0.071		733
Magh	0.083		14519	0.059		733
Falgun	0.078		14519	0.070		733
Chaitra	0.083		14519	0.070		733
Maternal Characteristics						
=1 if no education[a]	0.735		14519	0.533		733
=1 if primary education	0.139		14519	0.210		733
=1 if secondary education	0.113		14519	0.220		733
=1 if higher education	0.013		14519	0.037		733
=1 if Hindu[a]	0.874		14519	0.889		733
=1 if Buddhist	0.064		14519	0.056		733
=1 if Other Religion	0.061		14519	0.055		733
=1 if first (lowest) wealth quintile[a,b]	0.289		14519	0.226		733
=1 if second wealth quintile	0.220		14519	0.199		733
=1 if third wealth quintile	0.189		14519	0.194		733
=1 if fourth wealth quintile	0.181		14519	0.206		733
=1 if fifth wealth quintile	0.121		14519	0.175		733
=1 if Brahmin[a]	0.354		14519	0.299		733
=1 if Madhesi	0.105		14519	0.112		733
=1 if Dalit	0.148		14519	0.175		733
=1 if Newar	0.036		14519	0.035		733
=1 if Janajati	0.306		14519	0.322		733
=1 if Muslim[c]	0.037		14519	0.044		733
=1 if other caste	0.014		14519	0.014		733
Outcomes defined for live births only						
=1 if Female child	0.495		13333	0.501		625
=1 if Neonatal mortality[d]	0.042		13321	0.015		542

Source: Nepal DHS 2006 and Valente (2010). Sample of pregnancies after the adjustments detailed in Section 4.1.1. [a]Omitted category. [b]Wealth quintiles as provided in the DHS data, based on quality of housing and ownership of household goods, using principal component analysis. [c]Muslim is counted as an "ethnicity" in the Nepali DHS. [d]Neonatal mortality is only defined for children who were born at least one whole month before the interview.

Table 3: Summary Statistics, Additional Variables Collected for Recent Births

	(1) Pregnancies starting >28.6 kms of CAC			(2) Pregnancies starting <=28.6 kms of CAC		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
Delivery characteristics						
=1 if Helped by doctor or nurse[a]	0.154		4188	0.223		637
=1 if No delivery help[a]	0.082		4188	0.068		637
=1 if Delivery at home[a]	0.839		4188	0.777		637
=1 if Small baby[a]	0.198		4186	0.194		635
Antenatal care (ANC)						
Number of antenatal care visits[b]	2.351	2.2560	2907	2.675	2.0233	636
Number of tetanus injections[b]	1.471	1.1550	2907	1.553	0.9681	636
=1 if iron/folic tablets[b]	0.528		2908	0.687		636
=1 if 1st visit in 1st trimester[b]	0.249		2906	0.265		635
Hours old at first breastfeeding[b,c]	8.300	21.8048	2886	7.900	18.3140	631

Source: Nepal DHS 2006 and Valente (2010). Variables available only for children born no more than 5 years before the survey, either for all of these children ([a]), or only the last birth ([b]). These variables are only defined over the sample of children born alive.[c] Defined over the sample of breastfed children.

Table 4: Effect of Access to an Abortion Center on Pregnancy Outcomes and Fertility, Within-Mother Estimates

Explained variable	(1) =1 if Live Birth	(2) =1 if Live Birth	(3) =1 if Live Birth	(4) =1 if Conception Leading to Live Birth
=1 if CAC(<=28.6km at conception)	-0.0737***			-0.0041***
	(0.0272)			(0.0012)
=1 if CAC×=1 if 1st wealth quintile		-0.0289		
		(0.0453)		
=1 if CAC×=1 if 2nd wealth quintile		-0.0428		
		(0.0526)		
=1 if CAC×=1 if 3rd wealth quintile		-0.1294**		
		(0.0511)		
=1 if CAC×=1 if 4th wealth quintile		-0.0368		
		(0.0812)		
=1 if CAC×=1 if 5th wealth quintile		-0.2322*		
		(0.1198)		
=1 if CAC×=1 if no education			-0.0580*	
			(0.0299)	
=1 if CAC×=1 if 1ary education			-0.1330*	
			(0.0681)	
=1 if CAC×=1 if 2ary education			-0.0963	
			(0.0673)	
=1 if CAC×=1 if 3ary education			-0.2552	
			(0.2544)	
CAC-specific linear trend	-0.0141**	-0.0070	-0.0096	-0.0011
	(0.0058)	(0.0082)	(0.0060)	(0.0007)
CAC-specific linear trend^2	0.0007*	0.0004	0.0006	0.0000
	(0.0004)	(0.0005)	(0.0004)	(0.0000)
Maternal age at conception	-0.0375***	-0.0137	-0.0246*	0.0181***
	(0.0117)	(0.0192)	(0.0134)	(0.0008)
Maternal age at conception^2	0.0005***	0.0003	0.0003*	-0.0003***
	(0.0001)	(0.0002)	(0.0002)	(0.0000)
=1 if second pregnancy	0.1134***	0.0762***	0.0898***	
	(0.0139)	(0.0214)	(0.0154)	
=1 if third pregnancy	0.2038***	0.1292***	0.1619***	
	(0.0231)	(0.0335)	(0.0252)	
=1 if fourth pregnancy	0.2705***	0.1747***	0.2099***	
	(0.0315)	(0.0442)	(0.0340)	
=1 if fifth pregnancy and above	0.3857***	0.2566***	0.3073***	
	(0.0391)	(0.0540)	(0.0414)	
=1 if potential 2nd live birth				-0.0593***
				(0.0015)
=1 if potential 3rd live birth				-0.1001***
				(0.0019)
=1 if potential 4th live birth				-0.1307***
				(0.0023)
=1 if potential 5th live birth and above				-0.1664***
				(0.0027)
Siblings alive at conception	-0.1256***	-0.0951***	-0.1007***	
	(0.0108)	(0.0149)	(0.0105)	
=1 if married				0.0536***
				(0.0021)
=1 if sterilized				-0.0281***
				(0.0010)
=1 if pregnant with live birth during index month[a]				-0.0065***
				(0.0005)
Constant	1.3751***	1.1830***	1.1546***	-0.2011***
	(0.1817)	(0.1794)	(0.1790)	(0.0156)
Panel variable	Mother	Mother	Mother	Mother
Year FE	Yes	Yes	Yes	No
Calendar month dummies	Yes	Yes	Yes	No
Century month dummies	No	No	No	Yes
Full set of interactions[b]	n/a	Yes	Yes	n/a
No. of Observations	13620	13620	13620	594543[c]
No. of Mothers	4312	4312	4312	4292[d]
Max. Observations per Mother	10	10	10	183
Min. Observations per Mother	2	2	2	10
Clusters	260	260	260	260
R-squared	0.0557	0.0893	0.0958	0.0225
P-val F-test Equal CAC Effects		0.3094	0.6192	

Source: Author's calculations using Nepal DHS 2006 and Valente (2010). * p<0.10, ** p<0.05, *** p<0.01. Cluster-correlated robust standard errors in parentheses. [a] This specification only uses data on live births, and so this dummy is equal to zero when the mother reports being pregnant but the pregnancy is not carried to term. [b] Full set of interactions between all explanatory variables in Column (1) and wealth or education indicators. The coefficients reported in Columns (2) and (3) for control variables correspond to the first wealth quintile or the "no education" category, respectively. Coefficients on interaction terms between control variables and indicators for the other wealth or education categories are not reported for conciseness. [c] Number of mothers×months, see Section 4.2. [d] 20 mothers do not have any live birth.

Table 5: Effect of Access to an Abortion Center on Size at Birth and Neonatal Mortality

	Mother Fixed Effects			Cluster Fixed Effects		
Explained variable	=1 if small baby		= 1 if Neonatal Death			
	(1)	(2)	(3)	(4)	(5)	(6)
=1 if CAC(<=28.6km at conception)	-0.0426	-0.0128		-0.0143	-0.0132	-0.0157
	(0.0429)	(0.0173)		(0.0134)	(0.0105)	(0.0133)
=1 if CAC×=1 if male child			-0.0203			
			(0.0246)			
=1 if CAC×=1 if female child			-0.0117			
			(0.0278)			
=1 if female child	0.0682***	-0.0101**	-0.0920	-0.0077**	-0.0066**	-0.0075**
	(0.0184)	(0.0046)	(0.1041)	(0.0035)	(0.0033)	(0.0035)
Maternal age at conception	-0.0129	0.0124	0.0085	-0.0141***	-0.0121***	-0.0137***
	(0.0562)	(0.0090)	(0.0101)	(0.0032)	(0.0029)	(0.0032)
Maternal age at conception^2	-0.0001	-0.0003***	-0.0002	0.0002***	0.0002***	0.0002***
	(0.0009)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Second pregnancy	-0.1148**	-0.0909***	-0.1011***	-0.0080	-0.0027	-0.0082
	(0.0504)	(0.0100)	(0.0144)	(0.0069)	(0.0058)	(0.0069)
Third pregnancy	-0.1670**	-0.1642***	-0.1733***	-0.0026	0.0009	-0.0034
	(0.0828)	(0.0176)	(0.0211)	(0.0090)	(0.0078)	(0.0092)
Fourth pregnancy	-0.1776	-0.2277***	-0.2400***	0.0049	0.0060	0.0039
	(0.1131)	(0.0250)	(0.0301)	(0.0112)	(0.0101)	(0.0114)
Fifth pregnancy and above	-0.2182	-0.3190***	-0.3322***	0.0037	0.0047	0.0021
	(0.1423)	(0.0318)	(0.0375)	(0.0123)	(0.0111)	(0.0124)
Siblings alive at conception	0.1135***	0.1279***	0.1260***	0.0026	0.0025	0.0029
	(0.0410)	(0.0081)	(0.0088)	(0.0028)	(0.0025)	(0.0028)
CAC-specific linear trend		0.0099	0.0101	0.0055	0.0052	0.0053
		(0.0065)	(0.0064)	(0.0053)	(0.0050)	(0.0053)
CAC-specific linear trend^2		-0.0004	-0.0004	-0.0002	-0.0003	-0.0002
		(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Constant	0.4277	0.0262	0.0710	0.2748***	0.2329***	0.2528***
	(0.9284)	(0.1343)	(0.1462)	(0.0477)	(0.0422)	(0.0483)
Panel variable	Mother	Mother	Mother	Cluster	Cluster	Cluster
Full set of interactions[a]	n/a	n/a	Yes	n/a	n/a	n/a
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Maternal characteristics[b]	No	No	No	No	No	Yes
No. of Pregnancies	2428[c]	12092	12092	12092	13863	12092
No. of Groups (Mothers or Clusters)	1160	4035	4035	260	260	260
Max. Pregnancies per Group	3	9	9	150	151	150
Min. Pregnancies per Group	2	2	2	2	5	2
Clusters	235	260	260	260	260	260
R-squared	0.0331	0.0810	0.0843	0.0100	0.0084	0.0118
P-val $\beta_{male} = \beta_{female} = 0$			0.6285			

Source: Author's calculations using Nepal DHS 2006 and Valente (2010). * p<0.10, ** p<0.05, *** p<0.01. Cluster-correlated robust standard errors in parentheses. [a] Full set of interactions between all explanatory variables in Column (2) and a female gender indicator. The coefficients reported in Columns (3) for control variables are for boys. Coefficients on interaction terms between control variables and the female gender indicator are not reported for conciseness. [b] Maternal characteristics are indicators for maternal education, asset ownership quintile, religion, and caste/ethnicity as listed in Table 2, and excluding the first variable listed in each category. [c] Information on size at birth only collected for pregnancies occurring within 5 years of the survey.

Table 6: Effect of Access to an Abortion Center on Investments in Prenatal and Neonatal Health, Within-Cluster Estimates

Explained variable	(1) Number of ANC checks[a]	(2) =1 if 1st trimester check[a]	(3) Number of Tetanus Injections[a]	(4) =1 if iron/folic tablets[a]	(5) =1 if Medical help with delivery[b]	(6) =1 if No assistance with delivery[b]	(7) =1 if Delivery at home[b]	(8) Hours old at 1st breastfeeding[a]
=1 if CAC<=28.6km at conception	-0.1853	-0.0369	-0.0613	-0.0037	-0.0086	0.0064	0.0254	-1.2920
	(0.1173)	(0.0277)	(0.0654)	(0.0290)	(0.0178)	(0.0156)	(0.0185)	(1.2432)
=1 if female child	-0.0018	-0.0149	-0.0027	-0.0103	-0.0056	0.0020	0.0045	0.2131
	(0.0607)	(0.0140)	(0.0338)	(0.0149)	(0.0081)	(0.0073)	(0.0088)	(0.6499)
Maternal age at conception	0.1382***	0.0041	0.0925***	0.0245**	0.0134*	0.0005	-0.0178**	0.0455
	(0.0438)	(0.0101)	(0.0248)	(0.0096)	(0.0073)	(0.0067)	(0.0077)	(0.7045)
Maternal age at conception squared	-0.0023***	-0.0000	-0.0017***	-0.0004***	-0.0001	0.0000	0.0002	0.0020
	(0.0008)	(0.0002)	(0.0005)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0115)
=1 if second pregnancy	-0.1937*	-0.0172	-0.0344	-0.0188	-0.1097***	0.0106	0.1152***	-4.8331***
	(0.1055)	(0.0264)	(0.0544)	(0.0220)	(0.0169)	(0.0100)	(0.0176)	(1.3492)
=1 if third pregnancy	-0.4434***	-0.0427	-0.0748	-0.0531*	-0.1403***	0.0169	0.1568***	-2.4175
	(0.1309)	(0.0336)	(0.0735)	(0.0286)	(0.0208)	(0.0172)	(0.0225)	(1.6426)
=1 if fourth pregnancy	-0.5607***	-0.0731*	-0.1178	-0.0590	-0.1615***	0.0183	0.1804***	-4.3658**
	(0.1673)	(0.0413)	(0.0869)	(0.0364)	(0.0246)	(0.0257)	(0.0274)	(1.9129)
=1 if fifth pregnancy and above	-0.2505	-0.0107	0.0470	0.0020	-0.1326***	0.0338	0.1604***	-4.6492*
	(0.1987)	(0.0493)	(0.1075)	(0.0458)	(0.0279)	(0.0297)	(0.0306)	(2.3929)
Siblings alive at conception	-0.1831***	-0.0287***	-0.0835***	-0.0374***	-0.0186***	0.0147*	0.0183***	-0.0803
	(0.0380)	(0.0093)	(0.0229)	(0.0099)	(0.0052)	(0.0080)	(0.0057)	(0.4268)
Constant	0.4317	0.1362	0.3619	0.1180	-0.0682	0.0349	1.1109***	7.6417
	(0.6196)	(0.1419)	(0.3354)	(0.1273)	(0.0970)	(0.0821)	(0.1029)	(9.3756)
Panel variable	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster	Cluster
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Maternal characteristics[c]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Clusters	259	259	259	259	259	259	259	259
No. of Pregnancies	3543	3541	3543	3544	4825	4825	4825	3517
R-squared	0.1797	0.0714	0.0844	0.1300	0.1412	0.0423	0.1241	0.0189

Source: Author's calculations using Nepal DHS 2006 and Valente (2010). * p<0.10, ** p<0.05, *** p<0.01. Cluster-correlated robust standard errors in parentheses. [a] Data only collected for a mother's last child, if born up to five years before the survey. [b] Data only collected for children born up to five years before the survey. [c]Maternal characteristics are indicators for maternal education, asset ownership quintile, religion, and caste/ethnicity, as listed in Table 2, and excluding the first variable listed in each category.

Table 7: Effect of Access to an Abortion Center on Gender

	Mother Fixed Effects			Cluster Fixed Effects		
Explained variable	=1 if Female Child					
	(1)	(2)	(3)	(4)	(5)	(6)
=1 if CAC(<=28.6km at conception)	0.1163**	0.0441	0.1584***	0.0785*	0.0428	0.0788*
	(0.0480)	(0.0720)	(0.0549)	(0.0402)	(0.0340)	(0.0402)
=1 if CAC×=1 if no alive brothers		-0.0611	-0.1383**			
		(0.1067)	(0.0663)			
=1 if CAC×=1 if fewer sons than ideal		0.0892	-0.0220			
		(0.1044)	(0.0657)			
CAC-specific linear trend	-0.0082	0.0021	-0.0004	-0.0134	-0.0131	-0.0139
	(0.0152)	(0.0166)	(0.0157)	(0.0109)	(0.0094)	(0.0110)
CAC-specific linear trend^2	0.0005	-0.0001	0.0001	0.0007	0.0007	0.0007
	(0.0009)	(0.0011)	(0.0009)	(0.0006)	(0.0005)	(0.0006)
Maternal age at conception	0.0137	0.0045	-0.0230	-0.0026	-0.0031	-0.0017
	(0.0224)	(0.0236)	(0.0208)	(0.0069)	(0.0062)	(0.0070)
Maternal age at conception squared	-0.0001	0.0001	0.0006**	0.0001	0.0001	0.0000
	(0.0003)	(0.0003)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
Second pregnancy	-0.0285	0.1291**	-0.1573***	-0.0280*	-0.0263**	-0.0284*
	(0.0209)	(0.0590)	(0.0203)	(0.0149)	(0.0127)	(0.0149)
Third pregnancy	-0.0416	0.0610	-0.2289***	-0.0149	-0.0170	-0.0157
	(0.0342)	(0.0604)	(0.0343)	(0.0208)	(0.0182)	(0.0208)
Fourth pregnancy	-0.0791*	0.0435	-0.2782***	-0.0160	-0.0196	-0.0172
	(0.0447)	(0.0669)	(0.0442)	(0.0260)	(0.0228)	(0.0260)
Fifth pregnancy and above	-0.0888	0.0058	-0.2951***	0.0045	-0.0005	0.0029
	(0.0544)	(0.0687)	(0.0558)	(0.0280)	(0.0246)	(0.0281)
Siblings alive at conception	-0.0637***	-0.1233***	-0.1623***	-0.0077	-0.0048	-0.0079
	(0.0136)	(0.0166)	(0.0157)	(0.0061)	(0.0056)	(0.0061)
=1 if no alive brothers		-0.4259	-0.4859***			
		(0.2618)	(0.0178)			
=1 if fewer sons than ideal		0.0842	-0.3243***			
		(0.2819)	(0.0187)			
Constant	0.3329	0.6679*	1.6225***	0.5950***	0.5638***	0.5935***
	(0.3440)	(0.3645)	(0.3173)	(0.0948)	(0.0866)	(0.0970)
Panel variable	Mother	Mother	Mother	Cluster	Cluster	Cluster
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Maternal characteristics[a]	No	No	No	No	No	Yes
Full set of interactions[b]	n/a	Yes	No	n/a	n/a	n/a
No. of Pregnancies	12196	12196	12196	12196	13958	12196
No. of Groups (Mothers of Clusters)	4073	4073	4073	260	260	260
Max. Pregnancies per Group	9	9	9	150	151	150
Min. Pregnancies per Group	2	2	2	2	5	2
Clusters	260	260	260	260	260	260
R-squared	0.0153	0.2184	0.1911	0.0036	0.0031	0.0044
P-val $\beta'_{A1} + \beta'_{A2} = 0$		0.8840	0.8037			
P-val $\beta'_{A1} + \beta'_{A3} = 0$		0.1365	0.0380			
P-val $\beta'_{A1} + \beta'_{A2} + \beta'_{A3} = 0$		0.2821	0.9727			

Source: Author's calculations using Nepal DHS 2006 and Valente (2010). * p<0.10, ** p<0.05, *** p<0.01. Cluster-correlated robust standard errors in parentheses. [a]Maternal characteristics are indicators for maternal education, asset ownership quintile, religion, and caste/ethnicity as listed in Table 2, and excluding the first variable listed in each category. [b] Full set of interactions between all explanatory variables in Column (1) and indicators for no alive brothers and fewer sons than ideal, except for the control variable for pregnancy of order 5 and above. This control variable is interacted with neither $NoBoys$ nor $TooFewBoys$ as there are too few such pregnancies occurring at $NoBoys = 1$ or $TooFewBoys = 1$. The coefficients reported in Column (2) for control variables are for children for whom $NoBoys = 0$ and $TooFewBoys = 0$. Coefficients on interaction terms between control variables and $NoBoys$ or $TooFewBoys$ are not reported for conciseness.

Table 8: Indicators of Preference for a Son with Within-Mother Variation

Case	Intensity of Son Preference	Coefficient	Relevant subgroup
1	No preference	$\beta'_{A1} + \beta'_{A2}$	No son, ideal sons = 0
2	No preference	β'_{A1}	At least one son, >= ideal sons
3	Some preference	$\beta'_{A1} + \beta'_{A3}$	At least one son, < ideal sons
4	High preference	$\beta'_{A1} + \beta'_{A2} + \beta'_{A3}$	No son, ideal sons > 0

Relevant subgroup refers to sons alive at the time of conception of the index child and self-reported ideal number of sons.

Table 9: Selection on Observable Characteristics

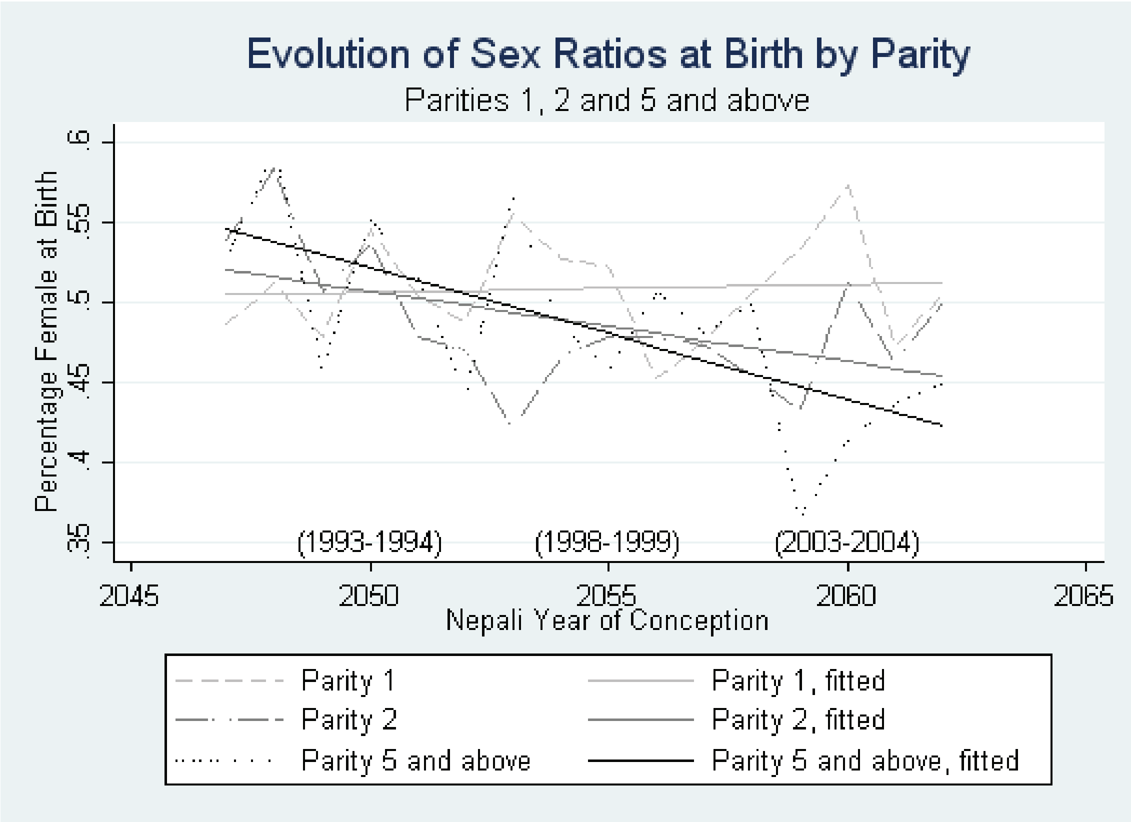
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Explained variable	=1 if says abortion legal	=1 if knows where to get an abortion					
PANEL A: Knowledge of Abortion Facts							
=1 if CAC<=28.6km at conception	-0.0209	-0.0393					
	(0.0251)	(0.0252)					
PANEL B: Wealth Quintile							
	=1 if First (Poorest)	=1 if Second	=1 if Third	=1 if Fourth	=1 if Fifth		
=1 if CAC<=28.6km at conception	0.0136	0.0108	-0.0090	-0.0011	-0.0143		
	(0.0221)	(0.0269)	(0.0243)	(0.0207)	(0.0157)		
PANEL C: Caste/Ethnicity							
	=1 if Brahmin	=1 if Madhesi	=1 if Dalit	=1 if Newar	=1 if Janajati	=1 if Muslim[a]	=1 if Other
=1 if CAC<=28.6km at conception	-0.0169	-0.0127	-0.0001	-0.0136	0.0368*	0.0090	-0.0026
	(0.0226)	(0.0153)	(0.0229)	(0.0089)	(0.0200)	(0.0075)	(0.0064)
PANEL D: Maternal Education and Religion							
	=1 if No Education	=1 if Primary	=1 if Secondary	=1 if Higher	=1 if Hindu	=1 if Buddhist	=1 if Other Religion
=1 if CAC<=28.6km at conception	-0.0249	0.0276	-0.0079	0.0051	-0.0060	-0.0001	0.0061
	(0.0261)	(0.0218)	(0.0225)	(0.0080)	(0.0172)	(0.0123)	(0.0116)
No. of Pregnancies	13620	13620	13620	13620	13620	13620	13620

Source: Author's calculations using Nepal DHS 2006 and Valente (2010). * p<0.10, ** p<0.05, *** p<0.01. Within-cluster estimates. Cluster-correlated robust standard errors in parentheses. Estimates based on the pregnancy sample. [a] Muslim is considered an "ethnicity" in the Nepali DHS.

Table 10: Robustness Checks, Within-Mother Estimates

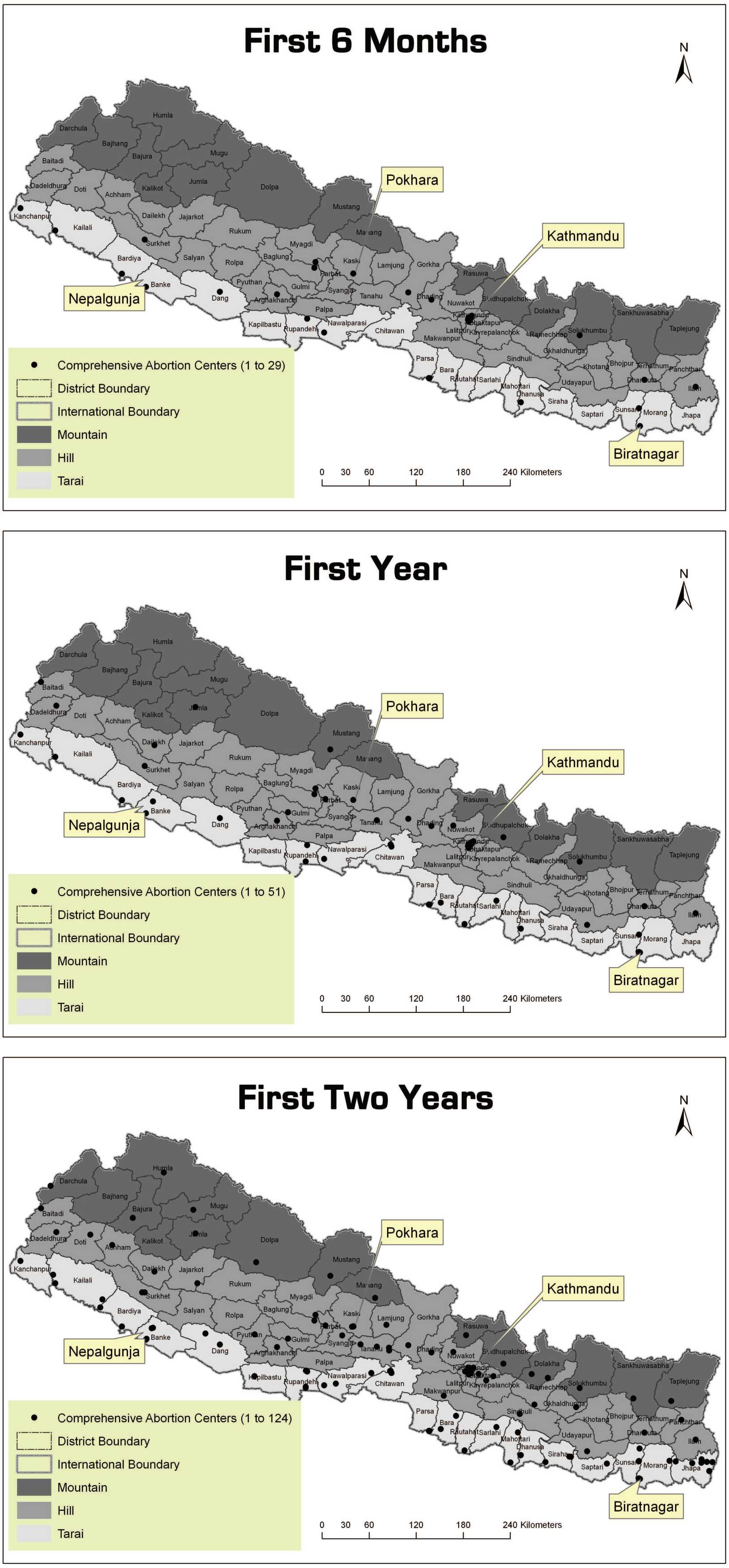
Explained variable	(1) Live Birth (Conditional)	(2) Neonatal Mortality	(3) Female Child
PANEL A: Inclusion of Linear Distance			
=1 if CAC<=28.6km at conception	-0.1299***	-0.0307	0.0731
	(0.0452)	(0.0283)	(0.0728)
=1 if CAC<=28.6km at conception x distance to nearest CAC	0.0039	0.0012	0.0029
	(0.0024)	(0.0015)	(0.0033)
No. of Pregnancies	13620	12092	12196
No. of Mothers	4312	4035	4073
R-squared	0.0562	0.0811	0.0154
F-test both treatment variables =0	0.0093	0.5519	0.0255
PANEL B: Distance Quartiles			
=1 if 0-13.4kms to CAC (1st quartile)	-0.1347***	-0.0126	0.0502
	(0.0363)	(0.0249)	(0.0714)
=1 if 13.4-28.6kms to CAC (2nd quartile)	-0.0731**	0.0120	0.1227*
	(0.0330)	(0.0233)	(0.0638)
=1 if 28.6-52.6kms to CAC (3rd quartile)	-0.0534*	0.0293	-0.0510
	(0.0319)	(0.0267)	(0.0635)
No. of Pregnancies	13620	12092	12196
No. of Mothers	4312	4035	4073
R-squared	0.0565	0.0813	0.0156
F-test all quartiles=0	0.0030	0.3834	0.0281
F-test difference between quartiles	0.0917	0.2360	0.0130
PANEL C: Placebo Experiment			
=1 if conceived up to 12 months before CAC<=28.6km	-0.0115	0.0214	0.0438
	(0.0206)	(0.0144)	(0.0407)
No. of Pregnancies	13620	12092	12196
No. of Mothers	4312	4035	4073
R-squared	0.0543	0.0804	0.0145
PANEL D: Sample Restricted to Women Who Gain Access to CAC During Data Period			
=1 if CAC<=28.6km at conception	-0.0806**	-0.0119	0.1021*
	(0.0311)	(0.0198)	(0.0537)
No. of Pregnancies	10828	9593	9684
No. of Mothers	3471	3244	3277
R-squared	0.0628	0.0814	0.0158
PANEL E: Controls for Conflict Intensity			
=1 if CAC<=28.6km at conception	-0.0727***	-0.0097	0.1165**
	(0.0271)	(0.0174)	(0.0483)
No. of Pregnancies	13620	12092	12196
No. of Mothers	4312	4035	4073
R-squared	0.0558	0.0819	0.0154
PANEL F: No Controls Except Year Fixed Effects			
=1 if CAC<=28.6km at conception	-0.0622**	-0.0235	0.1282***
	(0.0252)	(0.0172)	(0.0448)
No. of Pregnancies	13620	12092	12196
No. of Mothers	4312	4035	4073
R-squared	0.0066	0.0080	0.0099
PANEL G: Conditional Logit			
=1 if CAC<=28.6km at conception	-0.6921**	-0.4605	0.4749**
	(0.3045)	(0.6891)	(0.1969)
No. of Pregnancies	3468^a	1823^a	9235^a
No. of Mothers	913^a	470^a	2840^a
Pseudo R-squared	0.2176	0.3466	0.0183

Source: Author's calculations using Nepal DHS 2006 and Valente (2010). * p<0.10, ** p<0.05, *** p<0.01. Cluster-correlated robust standard errors in parentheses. The regression corresponding to Panel C does not include a treatment-specific trend. The regression corresponding to Panel E includes two additional regressors compared to Equation 1: (i) cumulated number of conflict casualties in the index child's district at the time of conception and (ii) the average monthly number of conflict-related casualties during pregnancy. ^{a} Conditional logit estimates only include observations with within-mother variation in outcomes.



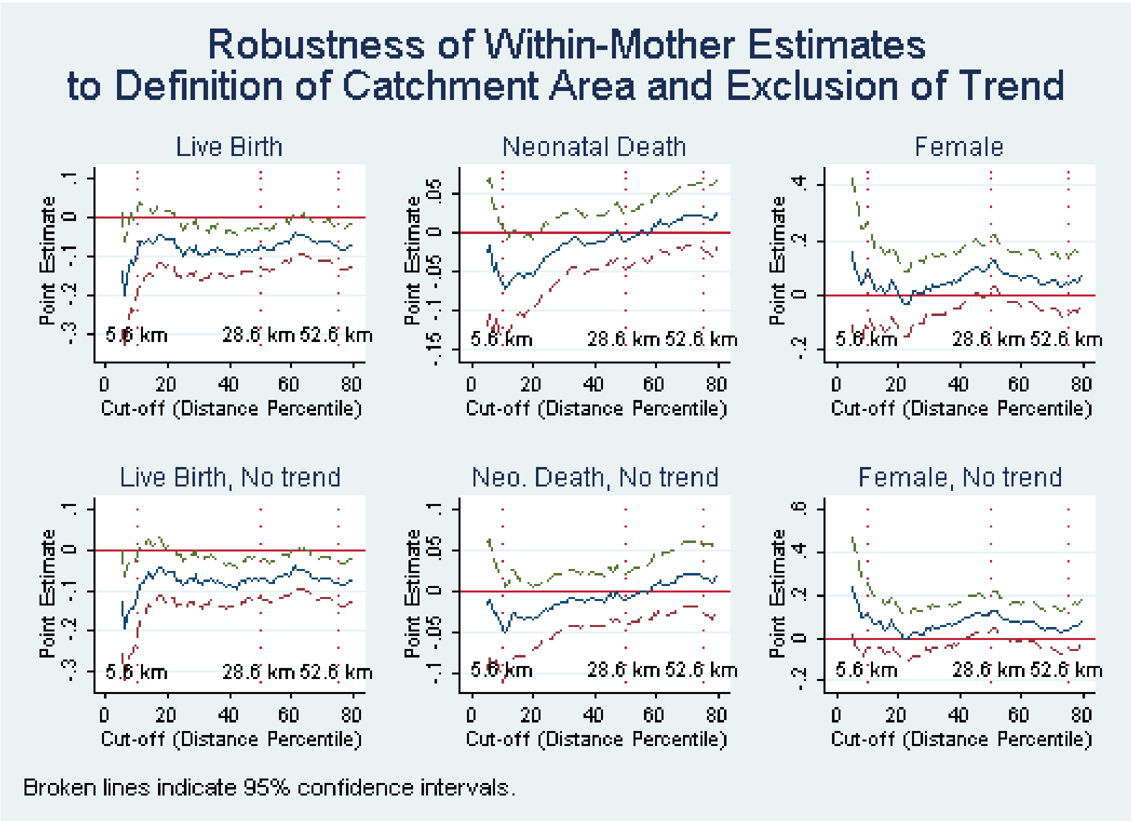
Source: Author's calculations using Nepal DHS 2006

Figure 1: Evolution of Sex Ratios in Nepal



Source: Valente (2010)

Figure 2: Registered Comprehensive Care Centers by Time since First Registration



Source: Author's calculations using Nepal DHS 2006 and Valente (2010). Estimates obtained by regressing Equation 1, with and without $Trend_{ct}^{(2)}$, and with alternative definitions of A_{ct}

Figure 3: Robustness to Changes in Treatment Cut-Off

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Table A-1: Detail of Definitions of Selected Variables

Variable	Definition
Explained Variables	
=1 if born alive	Binary indicator defined over the sample of all reported pregnancies. It is equal to one if the mother answered "born alive" when asked whether the baby was "born alive, born dead, or lost before full term", and zero otherwise.
=1 if live birth in 9 months	Binary indicator equal to one if a women reports a live birth occurring 9 months later, defined for all months covered by the fertility histories and in which respondents were at least 15 years old and already lived in the place where they are interviewed.
=1 if neonatal mortality	Binary indicator equal to one if the child is born alive but dies at age 0 or 1 month, and zero otherwise. Set to missing if the child was born on the month of interview or the previous month to ensure full exposure to neonatal death risk.
=1 if small baby	Binary indicator equal to one if the mother answers that, at birth, the child was "smaller than average" or "very small", and zero if they say that the child was "very large", "larger than average", or "average".
=1 if Helped by doctor or nurse	Binary indicator equal to one if the mother reports help by a doctor, nurse or midwife during delivery, and zero if other or no help.
=1 if No delivery help	Binary indicator equal to one if the mother reports receiving no help of any kind during delivery, and zero if she reports some help.
=1 if Delivery at home	Binary indicator equal to one if the child was delivered at the mother's or someone else's home, zero if somewhere else.
=1 if says abortion legal	Binary indicator equal to one if the respondent answers "yes" when asked whether abortion is legal in Nepal, zero otherwise.
Number of antenatal visits	Number of times the mother reports receiving antenatal care (ANC) for the index pregnancy.
Number of tetanus injections	Number of times the mother reports receiving an anti-tetanic injection for the index pregnancy.
=1 if iron/folic tablets	Binary indicator equal to one if, when shown iron/folic tablets, the mother reports having received or bought any during the index pregnancy.
=1 if 1st visit in 1st trimester	Binary indicator equal to one if the mother reports first receiving antenatal care in the first trimester, zero if no ANC or started later.
Hours old at first breastfeeding	Number of hours after birth the mother reports first putting the child to breast.
Explanatory Variables	
Pregnancy characteristics	
=1 if conceived within 28.6 kms of CAC	Binary indicator equal to one if, at the date of conception, the ground distance between the sample cluster in which the mother lives and the closest legal abortion center registered by this date is no more than 28.6 kms. The month of conception is defined as the birth month minus 9 for live born children, and as the date of end of pregnancy minus the duration of gestation for pregnancies not ending in a live birth.
=1 if CAC within 28.6 km by date of interview minus 9 months	Binary indicator equal to one if, at the date of the last potential conception resulting in a live birth by the interview data, the sample cluster in which the mother lives was no further than 28.6 kms from the closest legal abortion center registered by this date.

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Variable	Definition
Maternal age at conception	Integer number of years between the mother's month of birth and the month of conception of the index child.
=1 if no alive brothers	Binary indicator equal to one if, in the month of conception, the mother had no son alive.
=1 if fewer sons than ideal	DHS respondents are first asked how many children they would have in their entire life if they could choose (asking them to imagine going back to before they had any children if they have some already). They are then asked how many of those they would like to be boys, girls, or either. This binary indicator is equal to one if, in the month of conception, the mother had fewer alive sons than the number she reports as ideal, and zero otherwise.
Maternal Characteristics	
=1 if ith wealth quintile	Binary indicators for wealth quintiles as provided in the DHS based on a principal component analysis of (i) ownership of consumer items such as television, bicycle, car, and (ii) dwelling characteristics including source of drinking water, sanitation and type of housing materials.
Caste indicators	The 2006 Nepal DHS contains 96 ethnicity categories. Here they are grouped in 7 following Bennett, L. and Ram Dahal, D. and Govindasamy, P. (2008), namely: Brahmin or Chhetri, Tarai/Madhesi Other Castes, Dalit, Newar, Janajati, Muslim and Other.

Table A-2: Effect of Access to an Abortion Center on Assistance with Delivery, Within-Mother Estimates

Explained variable	(1) =1 if Medical help with delivery	(2) =1 if No assistance with delivery	(3) =1 if Delivered at Home
=1 if CAC<=28.6km at conception	-0.0248	-0.0085	0.0527*
	(0.0263)	(0.0223)	(0.0271)
=1 if female child	0.0155	0.0055	-0.0143
	(0.0144)	(0.0099)	(0.0143)
Maternal age at conception	-0.0002	-0.0144	-0.0431
	(0.0330)	(0.0288)	(0.0351)
Maternal age at conception squared	0.0000	0.0003	0.0006
	(0.0005)	(0.0004)	(0.0005)
=1 if second pregnancy	-0.0902**	0.0029	0.1234***
	(0.0363)	(0.0212)	(0.0391)
=1 if third pregnancy	-0.0934	0.0150	0.1672***
	(0.0601)	(0.0401)	(0.0637)
=1 if fourth pregnancy	-0.0746	0.0318	0.1734*
	(0.0840)	(0.0604)	(0.0888)
=1 if fifth pregnancy and above	-0.0517	0.0959	0.1961*
	(0.1035)	(0.0892)	(0.1086)
Siblings alive at conception	-0.0213	0.0487**	-0.0032
	(0.0258)	(0.0239)	(0.0267)
Constant	0.1656	0.1599	1.4439**
	(0.5415)	(0.4672)	(0.5855)
Panel variable	Mother	Mother	Mother
Year FE	Yes	Yes	Yes
Calendar month dummies	Yes	Yes	Yes
No. of Pregnancies	2431	2431	2431
No. of Mothers	1161	1161	1161
Max. Pregnancies per Mother	3	3	3
Min. Pregnancies per Mother	2	2	2
Clusters	235	235	235
R-squared	0.0485	0.0385	0.0362

Source: Author's calculations using Nepal DHS 2006 and Valente (2010). * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Cluster-correlated robust standard errors in parentheses. Data only collected for children born up to five years before the survey. Sample only includes children with another sibling with delivery information.