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From Fees to Free: User Fee Removal, Maternal Health Care Utilization and Child Health in Zambia

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Abstract

Despite recent progress, about 295,000 women in the World still die each year from pregnancy-related causes, and about 4.1 million children die before reaching the age of one. 99% of these deaths occur in developing countries. In 2006 the Zambian government removed user fees in public and mission health facilities in 54 out of 72 districts, and then extended this policy to rural parts of unaffected districts in 2007. I exploit the staggered implementation of the policy to assess its impact on maternal health care utilization and child health outcomes. Using a difference-in-differences estimation strategy, I find a 43% increase in the probability to give birth in a medical facility following the removal and a 36% increase in the probability of being assisted by a skilled birth attendant during childbirth. These positive effects decrease with household's distance from the nearest health facility. In terms of child health, chronic malnutrition decreased by 8% and the abolition of user fees reduced newborn mortality risk only for those living close to a health facility providing essential emergency obstetric care and child health services. Access improved but returns to formal health services remained rather limited, highlighting the importance of addressing supply-side constraints to generate substantial gains in population health.

JEL Codes – I12, I18, J13, O12, O15

Keywords – *Free health care, Childbirth conditions, Child health, User fees, Zambia, Difference-in-differences*

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1. Introduction

Access and returns to formal health services are critical elements in the ongoing debate on the relative effectiveness of demand- and supply-side interventions in improving population health in low-income countries. Despite a still tragically high incidence of preventable, premature deaths, there is little rigorous empirical evidence on whether removing user fees effectively helps increase health care utilization and ultimately improve population health [Dzakpasu et al., 2014; Lagarde and Palmer, 2011; Hatt et al., 2013].

An extensive empirical literature has established that even small prices may drastically deter individuals' willingness to invest in their health. However, it focuses almost exclusively on health products that can be directly used at home (e.g. Ashraf et al., 2010; Cohen and Dupas, 2010; Cohen et al., 2015; Spears, 2014). Evidence concerning the impact of reducing fees for health services in public amenities is more scarce [Kremer and Glennerster, 2011], despite the fact that curative out-of-pocket health expenditures may represent 10% of total household's budget [Dupas, 2011].

Theoretically, the effects of user fee removal are unclear, especially in low-income countries. On the one hand, removing user fees may encourage health care utilization and improve population health if individuals were kept out of good-quality health services for financial reasons. On the other hand, many factors beyond user fees may discourage individuals from seeking care, including low quality of care, health staff absenteeism [Banerjee et al., 2008; Chaudhury and Hammer, 2004], distance from health facilities [Thornton, 2008] and imperfect information on the benefits and costs of health investments [Banerjee et al., 2015; Jalan and Somanathan, 2008; Rhee et al., 2005]. The removal of user fees may have exacerbated some of them, such as health staff workload, informal fees and medical supplies shortages [Hatt et al., 2013; Meessen et al., 2011; Nabyonga-Orem et al., 2011]. Overall, the removal of user fees at the point of services might thus not be sufficient to reduce the marginal cost of consultation below the perceived marginal benefit associated with.[1] Moreover, the impact on health will depend not only on the price sensitivity of health care use but also on the impact of health facility visits on health. If removing user fees only leads to a drop in households out-of-pocket health expenditures without any effect on individuals' health, it should cast some doubts about the appropriateness of such an expensive policy.

Existing studies generally point to an increase in health care utilization [Bagnoli, 2019; Fitzpatrick and Thornton, 2018; Friedman and Keats, 2019b; Leone et al., 2016; Masiye et al., 2010; McKinnon et al., 2015a,b; Powell-Jackson et al., 2014; Ridde et al., 2013] and to a decline in household out-of-pocket health expenditures [Powell-Jackson et al., 2014; Ridde et al., 2015] after a reduction of

[1] A full conceptual framework is provided in Appendix A.

user fees. The impact on health outcomes has received much less attention and evidence is much more mixed. Exceptions include Tanaka [2014] who finds a significant improvement of child's nutritional status after the removal of user fees in South Africa and McKinnon et al. [2015b], who find no change in neonatal mortality risk following user fee removal for facility-based deliveries in Kenya, Ghana and Senegal. Fitzpatrick [2018] finds that free caesearan sections and deliveries have resulted in a decrease in maternal mortality and a stagnant or increased neonatal mortality risk in Sub-Saharan Africa depending on the specification used. Finally, Friedman and Keats [2019b] show that making facility births free in Ghana has had no effect on newborn mortality, but has lead to substantial reduction in infant mortality risk and improvement of child nutritional status later in life.

This paper sheds new light on the extent to which abolition of user fees affects maternal health care utilization and child health in a resource-limited setting. It also investigates how physical access to health amenities as well as quality of care shape the effectiveness of such a policy. Zambia constitutes an interesting framework to study these questions. User fees were removed in government-run and mission primary health facilities from April 2006 in 54 districts out of 72, and then in rural areas of previously unaffected districts one year later, in July 2007. Using birth history from four waves of nationally representative Demographic and Health Surveys reported by more than 18,900 mothers, I exploit this staggered adoption in a difference-in-differences framework.

The impact of this policy change has been explored in three recent papers. Chama-Chiliba and Koch [2016] find no effect of the April 2006 removal on deliveries in public facilities, but part of their control group was already exposed to free primary health care at survey time through the second wave of removal. Lépine et al. [2018] find no impact of the April 2006 removal on health care utilization but a strong short-term reduction in out-of-pocket health expenditures. Finally, Hangoma et al. [2018] assess the long-term effects of the policy and find a significant increase in health care utilization but no impact on average out-of-pocket health expenditures. None of these papers investigate how these effects depend on other supply-side factors, nor the resulting impact on health outcomes.

Looking at the effect on child health is important for several reasons. First, under-five children were in theory already covered by targeted fee exemptions since 1995. However, targeted exemptions were poorly implemented in practice, so that one can reasonably expect that under-five children have directly benefited from the 2006 policy change in terms of access to health services, and potentially, health status. Second, even if under-five children were perfectly covered by fee exemptions, they may have been adversely affected by the extension of free health care to the rest of the population. For instance, the increase in health care utilization may trigger supply-side constraints that may result in a deterioration of health services quality. Third, delivery conditions have been shown to be a strong predictor of newborns' survival chances both in developed [Daysal et al., 2015; Lazuka, 2018] and developing countries [Okeke and Chari, 2018] with important long-term effects on individuals' health [Ahsan et al., 2020; Friedman and Keats, 2019a,b; Lazuka, 2018], including in terms of child nutritional status. For instance, institutional deliveries may result in more interactions with postnatal health services providers and higher child health investments early in life. Fourth, if parents visit health facilities more frequently as a result of the policy, they will be more regularly exposed to health workers, and potentially to prevention messages. Finally, households may benefit from additional resources as they no longer have to pay for primary health services after the removal. This might indirectly affect child health through an income effect. For instance, these resources might be reallocated to invest in preventive health products and to increase food consumption. Overall, it appears that from a theoretical point of view the effect of removing user fees on child health is of interest but is far from clear-cut and must be empirically assessed.

This paper makes several contributions to the literature. First, I find a large and sustained change in maternal health care utilization, with a 43 percent increase in the probability to give birth in a medical facility after the removal, a result confirmed by the concurrent work of Lagarde et al. [2021]. Second, I assess the final impact of this reform on child health outcomes. Chronic malnutrition decreased by 8 percent after the policy change, but this positive effect is only showing up for at least 12 months of exposure to free health care. There is however no evidence that user fee removal led to a change in average infant mortality risk, a result which is not driven by potential fertility or selection effects set off by the policy. Finally, I use unique administrative data from the national census of health facilities to further investigate how such policy's effects vary with physical access to health amenities and the quality of health services available locally. I uncover important heterogeneity. As expected, the positive effect on delivery conditions fade with distance from the nearest health facility, highlighting the importance of considering physical access when estimating the returns to such a policy. While there is no discernible effect on child mortality on average, newborn mortality risk did decrease in the direct vicinity of qualified health centers. These findings have important implications for policy makers. They illustrate a twin challenge: making health services both financially accessible and of better quality for all. In particular, returns to formal health services appear to be limited without a sufficient quality of care.

The remainder of the paper is organized as follows. Section 2 provides some background on the Zambian health system and the policy of user fee removal. Section 3 presents the data as well as the empirical strategy. Results are presented in Section 4. Section 5 discusses the results and concludes.

2. Policy Background

Despite having one of the continent's fastest growing economies between 2000 and 2010, Zambia is also one of the poorest and more unequal country in Sub-Saharan Africa. According to the World Development Indicators in 2006, the year of policy adoption, more than 60% of the population lived with less than 1.90 dollars per day. About two-thirds of the poor were located in rural areas of the country, a situation that has barely changed since then. Life expectancy at birth stood at 50 years and the average fertility rate was 5.7 births per woman. The same year, 75% of all deaths were due to communicable diseases or maternal, perinatal and nutritional conditions, which are mostly preventable causes of death. In particular, maternal and neonatal disorders represented 7.2% of all deaths occurring in the country in 2006, a share that increased to 8.8% in 2017 [Global Burden of Disease, 2018].

2.1. Zambian Health System

Health care provision in Zambia is organized through a three-tier referral system. The first level provides primary health care services and includes health posts, health centers as well as district hospitals. The second level of care corresponds to provincial and general hospitals, while the third one comprises central hospitals and the National University Teaching Hospital. In 2006, 85% of the 1,327 health facilities in the country were government-run, 9% were private facilities and the remaining 6% were mission facilities which are publicly-supported [Chankova and Sulzback, 2006]. As many countries in Sub-Saharan Africa, Zambia faces an important health workers shortage. In 2006, there were only 649 doctors, 6,096 nurses and 2,273 midwives in the country [WHO, 2018]. For a corresponding population of 12.4 million, it gives a density of 7.3 health care professionals per 10,000 inhabitants, far below the World Health Organization's recommendation of 22.8 per 10,000 [WHO, 2006].

2.2. The User Fee Removal Policy

After independence, one of the top priorities of the newly elected government was to improve health equity throughout the country between racial groups. From 1964, health care was provided free of charge at public health facilities. In 1993, during a period of structural adjustments, the government of Zambia decided to introduce user fees at all levels of care to raise additional resources for the health sector, strucked by severe economic difficulties. A flat user fee was set by each health facility with the local community and District Health Office, depending on the ability to pay of the population living in its catchment area [Carasso et al., 2010]. Targeted fee exemptions were then introduced in 1995 for children below the age of five and the elderly (65 years old and above), antenatal care as well as chronic diseases, but were poorly implemented in practice [Masiye et al., 2010]. However, delivery services were not exempted from payment [Chama-Chiliba and Koch, 2016]. In a study by Cheelo et al. [2010], the average user fee charged for deliveries in a rural district of the North-Western province prior to user fee removal lied between 10,000 and 20,000 Zambian Kwachas (US$ 2.84 and US$ 5.68 in 2006), that is 15.5% to 31% of the average monthly per capita income in this province in 2006.[2]

In January 2006, the Zambian President announced that user fees were to be abolished for registration, consultation, outpatient and inpatient care, treatment, as well as diagnostic services in all publicly-supported primary health facilities of rural areas as a first step towards universal access to health services [Ministry of Health, 2007]. Facilities had to provide free health services to all individuals living in their catchment area, except foreigners. Patients referred to higher levels of care continued to be exempt from paying user fees. From April 1st, 2006, user fees were removed in government-run and mission facilities in 54 districts classified as rural but not in the 18 districts designated as urban.

One year after, in 2007, the government redefined eligibility criteria to extend the policy to rural areas of previously unaffected districts. From July, 1st publicly-supported facilities located more than 15 kilometers away from the administrative center of urban districts, and more than 20 kilometers away in urban districts located along the line of rail (the major Zambian railway) started to provide free primary health care. Such areas were previously excluded from the policy, despite levels of deprivation and poverty equivalent to rural districts.

User fees were finally removed in urban areas of urban districts from 2012, making primary health care free in publicly-supported health facilities throughout the country from this date (see Appendix B for a timeline of the policy implementation).

[2] Ngulube and Carasso [2010] note that traditional healers are not necessarily cheaper than formal care but are generally more flexible on payment.

3. Data and Estimation

3.1. Data

3.1.1. Individual-level Data and Outcome Variables

I use four waves of microdata from the nationally representative Zambia Demographic and Health Surveys (DHS) conducted in 1996, 2001, 2007 and 2013. Appendix C provides a description of the sampling frame. Within sampled households, all women aged 15-49 who were either permanent residents of the household or visitors present on the night before the survey were eligible for survey interview. The DHS collect data on birth history,[3] with detailed information on delivery conditions for births that occurred during the last five years preceding the survey, as well as maternal and under-five health, including anthropometric measurements and child death history.

Place of birth and the presence of a skilled birth attendant during childbirth constitute our main indicators of delivery conditions. Other things being equal, the removal of user fees may stimulate the demand for health services, including delivery services, through a reduction of the marginal cost of doing so. In that case, one should observe an increase in the probability of delivering in a publicly-supported facility. If health workers absenteeism did not increased dramatically as a result of the policy, a higher share of births should in turn be assisted by a skilled birth attendant.[4] I also explore the effect on postnatal check-ups, which gives an insight on the quality of care received by women, but this information is only available in the last three survey waves.

Child health is proxied by anthropometric indicators and child mortality. Anthropometric indicators refer to being stunted (height-for-age z-score$<$-2), severely stunted (height-for-age z-score$<$-3) or wasted (weight-for-height z-score$<$-2). Stunting and wasting are often referred as indicators of chronic and acute malnutrition respectively, and are strong predictors of overall health and mortality among under-five children. It is estimated that in 2006, malnutrition was the main cause of 2.6% of infant deaths in Zambia [Global Burden of Disease, 2018] in addition to being a serious compounding factor in other causes of child mortality. In all waves, anthropometric measures were taken for survivors: of the 23,128 under-five children alive at survey time (90.1% of the original sample), 92.9% were measured (Appendix Table C3).[5] I show further below that my

[3] Interviewers ask women to report only live births. Very limited information on miscarriages, abortions and stillbirths is available and was not collected in 1996 and 2001.

[4] Note that women may have difficulty in accurately reporting whether the attendant was qualified, and in particular distinct cadres of skilled birth attendants [Radovich et al., 2019].

[5] 2.3% were not measured because they were not present during interview, 0.7% refused to be measured and 4.1% missed anthropometric measurements because they were sick or for an unknown reason. Appendix Table C3 decomposes these figures by survey wave. Among children measured, some have anthropometric indicators

results on nutritional status are not driven by selective mortality using both inverse probability weighting and a semi-parametric approach based on survival probabilities. Premature deaths are measured by deaths at birth as well as neonatal and infant mortality risks, which correspond to the probability for a child to die before reaching the age of 28 days and one year respectively.[6] Infant mortality risk is highly concentrated within the first days of life, when newborn survival is strongly related to delivery conditions. In my sample, about a third of neonatal deaths occurred on the day of birth and more than three quarters within the first week of life. A large part of these deaths is due to labour and delivery complications, such as birth asphyxia which accounts for a quarter of neonatal deaths and one-third of deaths in the first week of life in the country in 2006.[7] The presence of a skilled birth attendant may help manage such complications in a life-saving way. Hence, by improving mothers' access to skilled birth attendants, the removal of user fees may have resulted in lower mortality risks and better health among newborns.

Finally, I explore the effect on health investment in children, proxied by whether child's vaccinations against polio, measles, diphtheria, pertussis, tetanus and tuberculosis were up-to-date at survey time. One can reasonably expect that a more regular exposure to health workers following the policy can affect household's health-related decisions, including for preventive investments.

3.1.2. Assignment to treatment

To identify individuals' district of residence, I obtained from the DHS the name of the district for each household surveyed in the first two waves, and made use of the geographic coordinates of each cluster for the last two. Since administrative boundaries changed after 1996, with some old districts splitting into several new ones, I use a consistent definition of district boundaries over time which respects the staggered implementation of the removal.[8]

Based on the progressive roll-out of the policy, I define three groups, two being affected from

considered as biologically implausible by the World Health Organization: height-for-age z-score below -6 or above 6 for stunting, and weight-for-height z-score below -5 or above 5 for wasting [WHO, 2019]. The corresponding 1.8% and 1.9% of measured children falling outside these intervals, respectively, are dropped from the analysis. Results do not change if these children are kept in the sample.

[6] One concern that arises when using retrospective data is measurement error due to recall bias. I argue that recall bias can be considered low in this setting since the birth and death of a child are milestones in a woman's life, and the recall period of five years is relatively short. However, mothers may have rounded up child's age at death, leading to mismeasurement in child mortality. I show as a robustness checks that my results are not sensitive to age-heaping.

[7] Author's calculation from the Global Burden of Disease [2018] data (accessible from `http://ghdx.healthdata.org/gbd-results-tool`).

[8] 43 districts did not change over time, 10 districts split into 21 new ones with exactly the same exposure to the policy (i.e. for example a given old district split into two districts which were equally affected by the policy in 2006) and four districts split into eight districts with different treatment status, which hinders accurate assignment of the corresponding DHS 1996 households to treated and non-treated areas. Thus, 911 births reported in 1996 are excluded from the analysis. It gives a total of 43+10+8=61 harmonized districts, of which 43 are rural and 18 are urban.

different dates, and one being the control group. The first treatment group (T1) consists of individuals living in rural districts where user fees were removed as of April 2006. The second one (T2) corresponds to individuals living in rural areas of urban districts, affected from July 2007. Finally, the control group (C) refers to individuals living in urban areas of urban districts which were not affected by the policy until 2012 (Appendix Figure C1). To very precisely determine the treatment status of an individual living in a rural area of an urban district, one would need to know both to which health facility's catchment area she belongs and the geographical coordinates of the corresponding health facility to compute the distance from the district administrative center. Such information is unfortunately not available. Thus, I consider as exposed to the second wave of user fee removal individuals from urban districts who reside in an area classified as rural by the DHS. I show as a robustness check that the results do not change when using a finer assignment to treatment based on the eligibility criteria defined above for households sampled in 2007 and 2013.

I restrict my sample in three ways. First, I exclude children born before 1993 since they were already exposed to a policy of free health care. Second, I drop children born in 2012 or later since there is no more control group as the policy was extended throughout the country from this date. Finally, I exclude visitors since we do not observe their district of residence.[9] The analytical sample consists of 25,678 live births reported by 18,903 mothers, with reliable anthropometric information for 91.3% of children alive at survey time.

3.1.3. Health Facility Census

I complement the DHS with facility-level data obtained from the national Zambian Health Facility Census conducted in 2005. Precise information on the geographic coordinates, physical infrastructures, equipment, services offered and head count of health workers were collected from all public and mission health facilities.

I use the straight-line distance from each DHS cluster surveyed in 2007 or 2013 to the nearest health facility as a proxy for travel time.[10] This distance varies from 53 meters to 40.7 kilometers. On average, households are located 6 kilometers away from their closest health facility. To ensure respondents' confidentiality, the DHS randomly displace cluster location[11] (see Appendix C, section

[9] The DHS define visitors as individuals who are not usual residents of the household, that is who usually do not live and eat with the household's members, but who stayed in the household the night before the interview [ICF, 2012]. Following this definition, 2.7% of all eligible adults interviewed in the four DHS survey waves I use are considered as visitors, similar to what is observed in the national census data from 1990, 2000 and 2010 (2.5%).

[10] Results from Masiye et al. [2010] suggest that 92% of Zambians seek care at the nearest health facility to their home.

[11] Urban clusters are randomly displaced within a radius of 2 kilometers around their true location. Rural clusters are randomly displaced within a radius of 5 kilometers around their true location, and up to 10 kilometers for a further 1% of them.

5 for more details), creating a measurement error in the distance to the nearest facility which generates an attenuation bias [Arbia et al., 2015]. Corresponding point estimates thus represent lower bounds of the true effects of distance to the nearest health facility on delivery and health outcomes.

Beyond monetary cost and distance, quality of health services available locally may play a crucial role for parents in non-emergency situations when deciding where to give birth or whether to seek care for their child and, if so, where. In particular, perceived quality might be a key determinant of such decisions, and quality itself may improve newborns' survival chances. I construct an indicator for the local availability of essential care based on Gabrysch et al. [2011]. It measures the provision of emergency obstetric care and child health services by a publicly-supported health facility within a radius of five kilometers around each enumeration area.[12] One concern is that such data is only available for the year 2005: new facilities may have opened while others may have closed. To limit this problem, I alternatively restrict my sample to births occurring three and four years around the census date as a robustness check. Conclusions presented below remain unchanged.

3.2. Summary Statistics

Columns 1-2 in Table 1 show the summary statistics before the policy implementation for children from rural districts (T1) and urban areas (C). Changes in demographic characteristics and outcome variables after the removal are presented in columns 3-4. Column 5 reports p-values obtained when comparing these changes. Columns 6-11 replicate this analysis for rural areas of urban districts (T2) affected one year apart.

Affected areas and the control group are significantly different before the policy change. In particular, children from affected areas have on average a mother less educated than their urban counterparts, a higher probability to be born at home and without the help of a skilled birth attendant. They also have a worse nutritional status, with a probability of being stunted 35% higher, and are 65% to 71% more susceptible to be severely stunted. Such baseline differences are not a threat to identification, which relies on the parallel trend assumptions.[13]

[12] This proxy is an indicator variable which takes the value of one if there is at least one publicly-supported health facility within five kilometers (1) providing at least 4 out of 6 basic emergency obstetric care signal functions (injectable antibiotics, injectable oxytocics, injectable anticonvulsants, manual removal of placenta, manual removal of retained products, assisted vaginal delivery), (2) offering referral services for obstetrics emergencies with a vehicle or using communication tools, (3) having at least a midwife or a doctor present or on call 24/7, (4) having at least two registered health professionals, including one on duty at the time of the census, and (5) performing resuscitation of newborns, growth monitoring, deworming, infant feed counseling, as well as case management of diarrhea, dehydratation and pneumonia, zero otherwise. Only 12% of the 1,274 publicly-supported health facilities present in the census meet these criteria, and 24% of households in my sample live within five kilometers of at least one of them.

[13] Conclusion remain the same when the estimation strategy outlined below is combined with matching (see robustness checks).

Table 1. Extensive summary statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Before		Change after 2006		ΔT1 = ΔC	Before		Change after 2007		ΔT2 = ΔC
	T1	C	ΔT1	ΔC	*p*-value	T2	C	ΔT2	ΔC	*p*-value
Panel A. *Demographic characteristics*										
Mother's age at childbirth	26.29	25.63	0.73	0.37	0.085	26.22	25.65	0.84	0.38	0.134
Mother's number of years of education	4.53	7.28	0.74	0.90	0.099	4.59	7.32	0.46	0.95	0.001
Girl	0.50	0.50	-0.01	0.00	0.760	0.51	0.50	0.00	0.00	0.892
Multiple birth	0.04	0.04	-0.01	-0.02	0.028	0.04	0.04	-0.01	-0.02	0.228
Preceding birth interval	36.33	39.83	0.86	4.35	0.000	36.23	40.01	-1.25	4.59	0.000
Panel B. *Childbirth conditions*										
Assisted by a health professional	0.33	0.80	0.22	0.06	0.000	0.29	0.80	0.17	0.07	0.000
Assisted by a doctor	0.01	0.08	0.02	0.00	0.021	0.01	0.08	0.01	0.01	0.963
Assisted by a nurse or midwife	0.31	0.77	0.21	0.05	0.000	0.27	0.77	0.17	0.06	0.000
Assisted by a clinical officer	0.01	0.01	-0.01	0.00	0.008	0.01	0.01	0.00	0.00	0.155
Assisted by a traditional birth attendant	0.16	0.03	0.06	0.01	0.000	0.21	0.04	0.09	0.01	0.000
Assisted by a relative or no one	0.52	0.17	-0.28	-0.07	0.000	0.50	0.16	-0.26	-0.07	0.000
Delivered at home	0.67	0.20	-0.25	-0.06	0.000	0.70	0.20	-0.20	-0.07	0.000
Delivered in a public health facility	0.33	0.71	0.25	0.13	0.000	0.29	0.72	0.20	0.12	0.000
Delivered in a government-run facility	0.24	0.70	0.28	0.13	0.000	0.26	0.71	0.21	0.13	0.000
Delivered in a mission health facility	0.08	0.01	-0.02	0.00	0.008	0.03	0.01	-0.01	0.00	0.185
Delivered in a private health facility	0.00	0.09	0.00	-0.06	0.000	0.01	0.09	0.00	-0.06	0.000
Panel C. *Child mortality*										
Death at birth	0.01	0.01	0.00	0.00	0.374	0.01	0.01	0.00	0.00	0.681
Neonatal mortality risk	0.03	0.04	-0.01	-0.01	0.523	0.03	0.04	-0.01	-0.01	0.347
Infant mortality risk	0.09	0.09	-0.04	-0.03	0.660	0.08	0.09	-0.03	-0.03	0.703
N	9,815	3,810	16,199	5,949	22,148	2,549	4,161	3,530	5,949	9,479
Panel D. *Child nutritional status*										
Stunted	0.54	0.40	-0.10	-0.04	0.000	0.55	0.40	-0.11	-0.04	0.007
Severely stunted	0.28	0.17	-0.08	-0.02	0.000	0.29	0.17	-0.10	-0.03	0.000
Wasted	0.06	0.06	-0.01	0.00	0.291	0.06	0.06	-0.01	0.01	0.303
N	5,788	2,211	13,544	4,842	18,386	1,795	3,029	2,942	4,842	7,784

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Unweighted statistics. The first two columns report summary statistics for live births occurring before April 2006 (Panels A, B and C) or children measured in a survey wave prior to the removal (Panel D) in rural districts (T1) and control areas (C). Columns 3 and 4 show changes in average characteristics observed in the aftermath of the policy in both groups. Column 5 reports *p*-values computed from crude differences-in-differences in which I compare changes over time in areas affected from April 2006 versus control areas. Columns 6 to 10 replicate this analysis for areas affected from July 2007 (T2). Control areas correspond to urban areas unaffected by the policy until 2012. In Panel D, the sample is restricted to children alive at survey time. Note that column 2 reports summary statistics for children of the control group born (Panels A, B and C) or observed (Panel D) up to April 2006, while column 7 reports summary statistics for children of the control group born (Panels A, B and C) or observed (Panel D) up to July 2007.

Delivery conditions and child anthropometric indicators changed significantly differently in affected and unaffected areas in the aftermath of the policy. The probability of being assisted by a skilled birth attendant during childbirth or to give birth in a publicly-supported facility increased significantly faster in affected areas than in control ones. We also observe a much steeper decline in risk of stunting in affected areas. However, I cannot detect any difference in child mortality risk, both in level before the removal and in changes after it. I show further below that when taking into account year of childbirth- and area-specific effects, demographic characteristics of mothers from affected areas changed in a way that is similar to those from unaffected ones.

3.3. Estimation Strategy

Taking advantage of the progressive roll-out of user fee removal across the country, I employ a difference-in-differences design and estimate the average effect of the policy from the following equation:

$$y_{imta} = \alpha + \gamma Exposed_{ta} + X'_{imta}\Gamma + \delta_a + \delta_t + \varepsilon_{imta}$$

where y_{imta} is the outcome of interest for child i of mother m who lives in area a, and t the time period relevant for the outcome being investigated. It will refer to year of childbirth when looking at retrospective childbirth outcomes and child mortality, and to survey year when looking at anthropometric indicators measured at survey time. Areas are the geographic unit at which the policy has been rolled out. Each area corresponds either to a rural district, the rural area of an urban district or the urban area of an urban district, which gives a total of 79 areas. δ_a denotes area fixed effects, which take into account any time-invariant area-specific factors such as risks of diseases,[14] and δ_t time fixed effects, which control for area-invariant time-specific factors such as macroeconomic conditions common to all areas in the country. The independent variable of interest, $Exposed_{ta}$, is an indicator variable taking the value of one if user fees were removed in area a at time t, zero otherwise. A positive γ would indicate an average increase in the outcome of interest after user fee removal in affected areas relative to unaffected ones.[15] X_{imta} is a set of covariates including a dummy for high-risk multiple pregnancy, as well as mother's year of birth for childbirth conditions, mother's year of birth and child's sex when looking at child mortality,

[14] Due to data limitations, I do not observe the effective area of birth of each child. A mother may have migrated since then or may have given birth in an area different from the one where she currently lives. Hence, her current area of residence might not be the same as the one where she gave birth. I can only partially deal with this issue by restricting my sample to mothers who already lived there before childbirth, leaving the results unchanged (see robustness checks).

[15] Note that for anthropometric outcomes, γ combines the effect of a difference in exposure status (children measured in 2007) and the effect of a difference in length of exposure to the policy (children measured in 2013 and 2014) since the policy has been extended to the entire country from 2012.

and child's sex and age dummies[16] for anthropometric outcomes.[17]

Recent advances in econometric theory show that the two-way fixed effects (TWFE) estimator with staggered treatment adoption may yield to biased estimates in presence of heterogeneous treatment effects (e.g. de Chaisemartin and D'Haultfœuille 2020, 2021; Goodman-Bacon 2021). Goodman-Bacon [2021] shows that the TWFE difference-in-differences estimator is a weighted average of all possible two groups-two periods difference-in-differences estimators. In particular, already-treated units act as a control group for not-yet treated units, which is problematic under time-varying treatment effects. The resulting bias then feeds through to γ based on the weight attached to such two groups-two periods comparisons. In our context, the problem arises when rural districts are used as a control group in the two groups-two periods difference-in-differences that estimates the effect of removing user fees in rural areas of urban districts. Based on the Bacon decomposition [Goodman-Bacon, 2021], I find that it accounts for less than 5% of the point estimates obtained with the TWFE estimator, which primarily rely on the comparison of the treated groups (T1 and T2) with the never-treated one (83 to 96% depending on the outcome).

I overcome this issue in two ways. First, I separately estimate the effect in the two treatment groups using only the never-treated (urban areas) as the control group. It has the advantage to allow for the estimation of phase-specific effects of the policy and to check whether the policy had the same effects in both types of treated areas. Second, I use the estimator developed by de Chaisemartin and D'Haultfœuille [2021] which is unbiased in the presence of heterogeneous treatment effects. As expected, conclusions remain unchanged with this alternative estimator.[18]

To take into account serial correlation and to avoid overrejection of the null hypothesis of no effect, robust standard errors are clustered at the area level [Bertrand et al., 2004; Cameron and Miller, 2015] in all specifications. This is an intention-to-treat estimate since some health workers in rural areas may have decided to still charge fees on patients despite the law, and some patients living in urban areas might have received health care in an affected area despite the limitation of the policy to individuals living in the catchment area of affected facilities. Moreover, some individuals supposed to be treated may not have benefited from the policy because of remoteness of health facilities in rural areas. Hence, compliance with the policy is likely to be imperfect.

[16] Alternatively, controlling for a cubic relationship with age in months leaves the results unchanged (available upon request).

[17] Results are virtually unchanged when controlling for a full set of maternal covariates which are not included in the main specification due to endogeneity issues (see robustness checks).

[18] Note that my results are also robust to other estimators proposed by Callaway and Sant'Anna [2020], Borusyak et al. [2021], and Gardner [2021]. Results available upon request.

3.4. Parallel Trends Assumption

Change over time in outcomes of interest in urban areas is used to estimate the unobserved counterfactual change for rural areas had user fees not been abolished. The key identifying assumption here, known as the parallel trends assumption, is that in absence of the policy both rural and urban areas would have experienced the same trends in the outcomes of interest. It implies that in absence of the policy, area-specific confounders must be time invariant and time-specific confounders must be common across treated and untreated areas [Angrist and Pischke, 2009]. It cannot be tested since it would require to observe the average post-treatment outcomes in treated areas in absence of the treatment. One can assess the plausibility of this assumption by checking pre-treatment trends in outcomes between treated and untreated areas, conditional on the covariates included in the estimation. If trends were parallel in pre-treatment periods, then we might expect trends to have remained the same in post-treatment periods had user fees not been removed.

3.4.1. Graphical Evidence

First, I present graphical evidence of parallel pre-treatment trends. Figures reported in Appendix D1 plot the raw and conditional pre-treatment trends in outcomes of interest. Until the removal of user fees, the different outcomes follow similar trends in affected and unaffected areas. After it, the figures show an increase in maternal health care utilization and a decrease in chronic malnutrition in affected areas.

3.4.2. Event-Study Specification

I also formally test for differential pre-trends between affected and unaffected areas using an event-study design. For this, I modify the equation above to include leads and lags of the time variable interacted with the indicator for whether fees were removed or not in area a (see Appendix D). By doing so, I can check for diverging trends prior to policy implementation and assess the timing of the policy's effects. Point estimates and 95 percent confidence interval are reported in Figures 1 and 3.[19] Compared to unaffected ones, results suggest that affected areas did not exhibit a significantly different pattern prior to user fee removal, whatever the outcome considered.[20]

[19] Similar figures for rural districts and rural areas of urban districts separately are presented in Appendix Figures D2 to D5.

[20] I do not investigate the effect of the policy on prenatal visits since treated and control districts were already on different slopes before the removal of user fees (available upon request), which prevents the causal interpretation of the corresponding point estimates. This is not surprising since prenatal visits were made free of charge in 1995 and increased gradually over time. Moreover, attendance was already high before the removal with 96% of

3.4.3. *Placebo Tests*

Finally, I implement a broad set of placebo tests where I compare unexposed children from both types of areas. For this, I drop children born in the aftermath of the policy and use the full set of lags of the real implementation date as starting points of a series of fictitious policies. Then, I run difference-in-differences regressions using the newly defined implementation dates. If affected and unaffected areas were on similar slopes before the removal, point estimates from these regressions should be statistically insignificant and close to zero. This is what I find, as reported in Appendix Figure D6. Here again, it fails to reject the null of pre-treatment parallel trends between affected and unaffected areas: only 9 point estimates out of 255 are marginally significant at the five percent level.

All together, these results strongly support the identifying assumption, and thus the causal interpretation of my results.

4. Results

4.1. Effect on Maternal Health Care Utilization

4.1.1. *The probability to give birth in a publicly-supported health facility increased sharply after the removal*

Table 2 reports the average effect of the policy on place of delivery. The result suggests a sharp increase in medical deliveries, which is significant at less than 0.1 percent. The user fee removal led to a rise of 13.9 percentage points in the probability to give birth in a medical facility, a 43 percent increase relative to the pre-policy mean (Panel A, Column 1). This result is confirmed when the potential bias introduced by heterogeneous treatment effects is taken into account (Panel B, Column 1). I then estimate the effect separately for rural districts and rural parts of urban districts (Panels C and D). Rural districts exhibit a stronger effect of the policy, but relative to the pre-policy mean the results remain similar. This result echoes the one from Hangoma et al. [2018], who find an increase of overall utilization of care following the removal. However, point estimates from the event-study suggest that this increase did not materialize right after the removal (see

women making at least one prenatal visit and 72% at least four prenatal visits. The results remain unchanged when I control for having done at least four prenatal visits in the estimation, and when I control for a linear time trend interacted with the share of pregnancies for which at least four prenatal visits have been done within the area of residence, before policy implementation. This is not done in the main specification due to the endogenous nature of prenatal visits. Results are available upon request.

Figure 1), a result consistent with Lépine et al. [2018] and Chama-Chiliba and Koch [2016] who respectively find no effect on health care utilization and deliveries in public facilities in the very short-term.

Table 2. The effect of user fee removal on childbirth conditions

	(1)	(2)	(3)	(4)	(5)	(6)
	Institutional delivery	Type of health facility		Assisted by a		
		Public	Private	Health worker	Doctor	Nurse or Midwife
Panel A. *Average effect of user fee removal*						
Affected by the policy	0.139***	0.108***	0.031	0.114***	0.004	0.120***
	(0.024)	(0.034)	(0.022)	(0.023)	(0.011)	(0.024)
Mean before policy	0.323	0.319	0.004	0.318	0.013	0.300
R^2	0.224	0.202	0.244	0.218	0.045	0.208
N	25,485	25,485	25,485	25,580	25,580	25,580
Panel B. *Average effect of user fee removal using de Chaisemartin and D'Haultfœuille [2021] estimator*						
Affected by the policy	0.126**	0.131***	−0.005	0.110**	0.002	0.128**
	(0.053)	(0.049)	(0.019)	(0.051)	(0.019)	(0.054)
Mean before policy	0.323	0.319	0.004	0.318	0.013	0.300
N	25,485	25,485	25,485	25,580	25,580	25,580
Panel C. *Effect in rural districts*						
Affected from 2006	0.165***	0.131***	0.034	0.136***	0.005	0.143***
	(0.024)	(0.034)	(0.025)	(0.024)	(0.012)	(0.024)
Mean before policy	0.330	0.326	0.004	0.325	0.013	0.306
R^2	0.229	0.205	0.252	0.222	0.048	0.211
N	21,974	21,974	21,974	22,063	22,063	22,063
Panel D. *Effect in rural parts of urban districts*						
Affected from 2007	0.132***	0.106**	0.026	0.104**	−0.007	0.111**
	(0.046)	(0.051)	(0.024)	(0.043)	(0.013)	(0.045)
Mean before policy	0.297	0.291	0.006	0.291	0.015	0.274
R^2	0.292	0.259	0.268	0.295	0.057	0.270
N	9,431	9,431	9,431	9,442	9,442	9,442

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a childbirth. The table reports the average (Panels A and B) and phase-specific effect (Panels C and D) of user fee removal on the probability to give birth in a health facility (Column 1), in a publicly-supported health facility (Column 2), in a private one (Column 3), to give birth with a skilled birth attendant (Column 4), with a doctor (Column 5) and with a nurse or a midwife (Column 6). Each coefficient is from a different regression. All regressions control for area and year of childbirth fixed effects, as well as mother's year of birth and a dummy for multiple births.
* $p<.10$; ** $p<.05$; *** $p<.01$

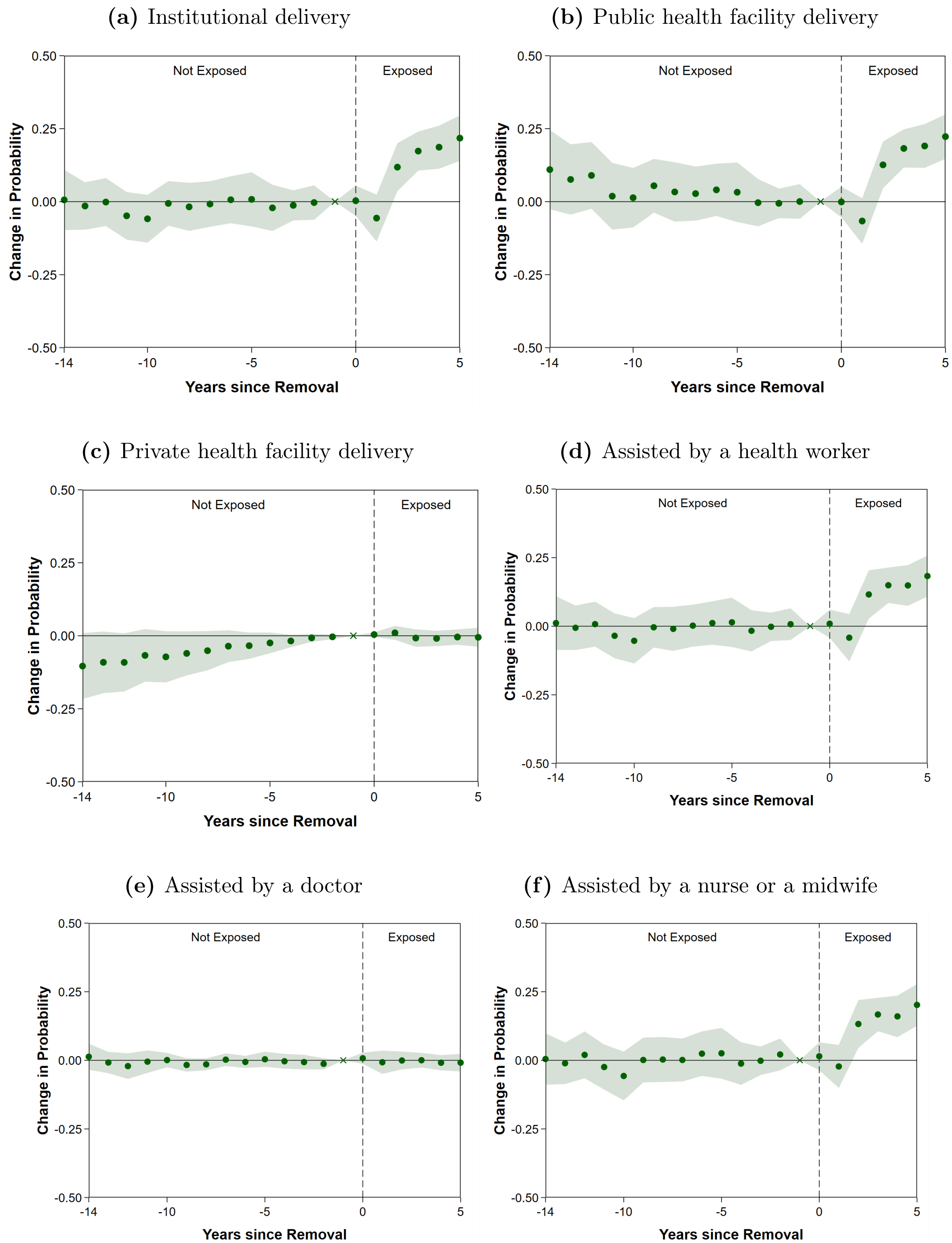


Figure 1. Event study estimates of the effect of user fee removal on childbirth conditions

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: These figures show the coefficients for interaction terms between time dummies and treatment status obtained from an event-study specification. Year of implementation is normalized to zero. In addition to area and year of childbirth fixed effects, the covariates include mother's year of birth and a dummy for multiple pregnancy. Shaded areas represent 95% confidence intervals with robust standard errors clustered at the area level. The omitted category is the last pre-treatment time dummy. Outcomes of interest are dummies equal to one if mother gave birth (a) in a health facility, (b) in a public or mission health facility, and (c) in a private health facility, in presence of (d) a health worker, (e) a doctor, and (f) a nurse or a midwife, zero otherwise.

Columns 2 and 3 report the effect in publicly-supported and private facilities. The aggregate effect found in column 1 is exclusively driven by deliveries in publicly-supported health facilities, with a strong and sustained effect over time (see Figure 1), which is reassuring since the policy change only applies to this type of facility. One might be concerned if the removal of user fees only causes mothers to switch from the private sector to the public one and did not reach those delivering at home without a skilled birth attendant. This is not the case here since the overall utilization of health facilities increases and there is no effect on private facilities.

4.1.2. A higher share of childbirths were assisted by a skilled birth attendant after the removal

Given the high increase in institutional deliveries I find, one can reasonably expect to observe an increase in the share of births assisted by skilled birth attended, unless health worker absenteeism dramatically increased as a result of the policy. Table 2 reports the results for medical assistance received during childbirth. Column 4 indicates a 11.4 percentage points increase in the probability of giving birth with the help of a skilled birth attendant in affected areas. Compared to the pre-policy mean, it represents a 36 percent increase (Panel A). This result remains remarkably stable when the potential bias arising from heterogeneous treatment effects is considered (Panel B). It also suggests a stronger effect in rural districts (42 percent increase, Panel C) than in rural parts of urban districts (36 percent increase, Panel D). The pattern presented in Figure 1 is striking: before policy implementation, there is no differential trend between rural and urban areas, whereas after the removal, the probability of being assisted by a skilled birth attendant increased significantly faster in rural areas than in unaffected ones.

In both treatment groups, the effect is however exclusively driven by deliveries with a nurse or a midwife (Column 6), while the probability of being assisted by a doctor remains close to zero (Column 5). This is consistent with the high concentration of doctors in cities and urban areas.

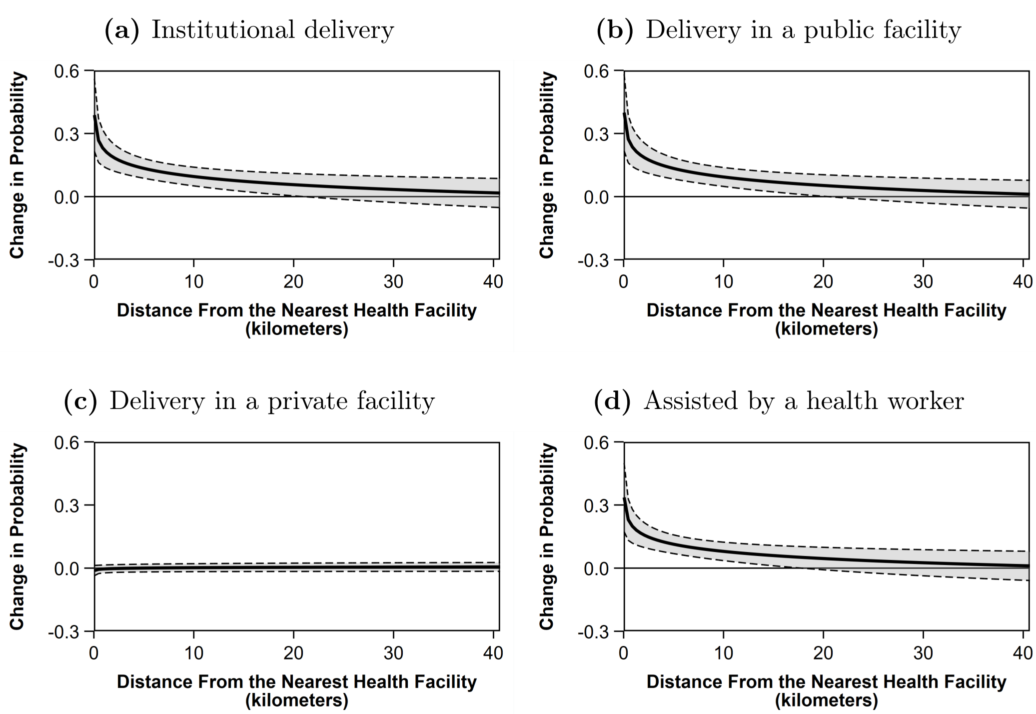
Finally, I investigate the effect on postnatal check-ups received by the mother after delivery. I find that the removal of user fees has increased the probability for mothers to receive a postnatal check-up in the first 24 hours after giving birth. The results indicate a significant 7.8 percentage points increase in rural districts, and a nearly identical effect in rural parts or urban districts, though not significant perhaps due to a smaller sample size (see Appendix E). While this result suggests that more women benefited from postnatal check-ups, it should also be interpreted in light of the large increase in institutional deliveries I found (+43%). The large discrepancy in the two effects suggests a poor quality of care since many women who delivered in health facilities did not benefit from a medical check-up that could have prevented postpartum complications.

This could explain, at least to some extent, why there is no discernible trend break in maternal mortality ratio around the policy (Appendix Figure F1).

4.1.3. These positive effects decrease with distance from the nearest facility

The policy may have had heterogeneous effects with respect to the physical access of households to public health facilities. In particular, it may have benefited more those living near a health facility. This is exactly what I find. To investigate this, I use the log of the straight-line distance between each DHS cluster position and the nearest publicly-supported health facility as a proxy for travel time, interacted with exposure to free health care. Figure 2 plots the corresponding marginal effect of the policy on childbirth conditions outcomes. As expected, the positive effect on childbirth conditions decreases as the distance increases.

I find no differential effect of the policy on childbirth conditions with respect to the local availability of a qualified health facility (results not shown available upon request). This is not surprising for at least two reasons. First, parents may not be aware of the effective quality of services offered in all health facilities near their home. Second, even if they are, a spontaneous, non-planned delivery may force them to go to the nearest one, whatever the perceived quality.



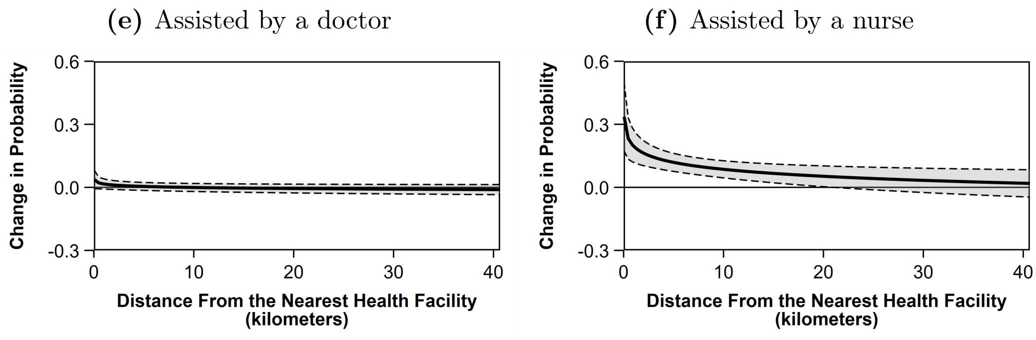


Figure 2. Marginal effect of exposure to the policy on childbirth conditions depending on distance from the nearest health facility

Source: Author's calculations from DHS 2007 and 2013.
Notes: The figures plot the marginal effect of the policy change depending on the distance from the nearest health facility. Distance corresponds to the straight-line distance between each DHS cluster from the 2007 and 2013 survey waves and the nearest publicly-supported health facility from the 2005 Health Facility Census. Each figure is from a separate estimation where the distance is log-transformed and interacted with exposure to the policy. Control variables include area and year of childbirth fixed effects, as well as mother's year of birth and a dummy for multiple births. Shaded areas represent 95% confidence intervals with robust standard errors clustered at the area level.

4.2. Effect on Child Health

4.2.1. Chronic malnutrition decreased after the removal of user fees

The average effect of the policy on anthropometric indicators is presented in columns 1 to 3 of Table 3. The results clearly indicate a positive effect of free primary health services on child nutritional status with a significant 8 percent reduction in the prevalence of stunting. The effect is even stronger on severe stunting, with a 15 percent reduction relative to the pre-policy period (Panel A).[21] This is in line with Bagnoli [2019] and Friedman and Keats [2019b] who respectively find a significant and positive effect of health insurance and free deliveries on child height-for-age z-score in Ghana. These results are encouraging since childhood stunting is a strong marker of recurrent and severe infections with long-lasting effects on health, and is commonly used as a proxy for healthy growth. Conclusion remains the same when using the de Chaisemartin and D'Haultfœuille [2021] estimator (Panel B).

There is no discernible effect on acute malnutrition. This is not surprising since wasting does not reflect the cumulative effects of poor health conditions but is rather the result of a rapid deterioration in nutritional status over a short period of time, probably independent of a regular access to formal care.

[21] Importantly, these results are not driven by a differential seasonality effect in the measurement of anthropometric indicators across treatment groups.

Table 3. The effect of user fee removal on child health

	(1) Stunted	(2) Severely stunted	(3) Wasted	(4) Death at birth	(5) Neonatal mortality	(6) Infant mortality
Panel A. *Average effect of user fee removal*						
Affected by the policy	−0.044**	−0.043***	−0.004	0.002	0.003	−0.004
	(0.021)	(0.015)	(0.011)	(0.003)	(0.005)	(0.008)
Mean before policy	0.545	0.286	0.063	0.009	0.033	0.086
R^2	0.086	0.066	0.022	0.017	0.029	0.036
N	21,106	21,106	21,065	25,678	25,265	19,173
Panel B. *Average effect of user fee removal using de Chaisemartin and D'Haultfœuille [2021] estimator*						
Affected by the policy	−0.094**	−0.028	0.009	−0.002	0.019	0.052
	(0.042)	(0.034)	(0.033)	(0.006)	(0.020)	(0.040)
Mean before policy	0.545	0.286	0.063	0.009	0.033	0.086
N	21,106	21,106	21,065	25,678	25,265	19,173
Panel C. *Effect in rural districts*						
Affected from 2006	−0.055**	−0.052***	0.001	0.004	0.004	−0.005
	(0.022)	(0.015)	(0.013)	(0.003)	(0.005)	(0.009)
Mean before policy	0.544	0.283	0.063	0.009	0.033	0.087
R^2	0.083	0.063	0.022	0.019	0.030	0.037
N	18,206	18,206	18,159	22,148	21,785	16,486
Panel D. *Effect in rural parts of urban districts*						
Affected from 2007	−0.073***	−0.070***	−0.008	0.002	0.005	0.000
	(0.022)	(0.020)	(0.013)	(0.003)	(0.007)	(0.012)
Mean before policy	0.548	0.294	0.063	0.009	0.033	0.081
R^2	0.088	0.065	0.020	0.018	0.026	0.032
N	7,708	7,708	7,682	9,479	9,344	7,163

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a child. The sample is restricted to children alive at survey time in columns 1 to 3 (anthropometric indicators). The table reports the average (Panels A and B) and phase-specific effect (Panels C and D) of user fee removal. *Stunted* (respectively *Severely stunted*) is a dummy which equals one if the height-for-age ratio is at least two (respectively three) standard deviations below WHO z-score, zero otherwise. *Wasted* is a dummy equals to one if the weight-for-height ratio is at least two standard deviations below WHO z-score, zero otherwise. In columns 4, 5 and 6, the dependent variable is respectively a dummy which equals one if the child died at birth, within her first 28 days of life and before reach the age of one, zero otherwise. Each coefficient is from a different regression. All regressions control for area and time fixed effects, a dummy for multiple births and child's sex. Columns 1 to 3 also control for child's age dummies, and columns 4 to 6 for mother's year of birth. Time fixed effects correspond to survey years in columns 1 to 3, and to years of childbirth in columns 4 to 6. For neonatal and infant mortality, children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group.
*$p<.10$; **$p<.05$; ***$p<.01$

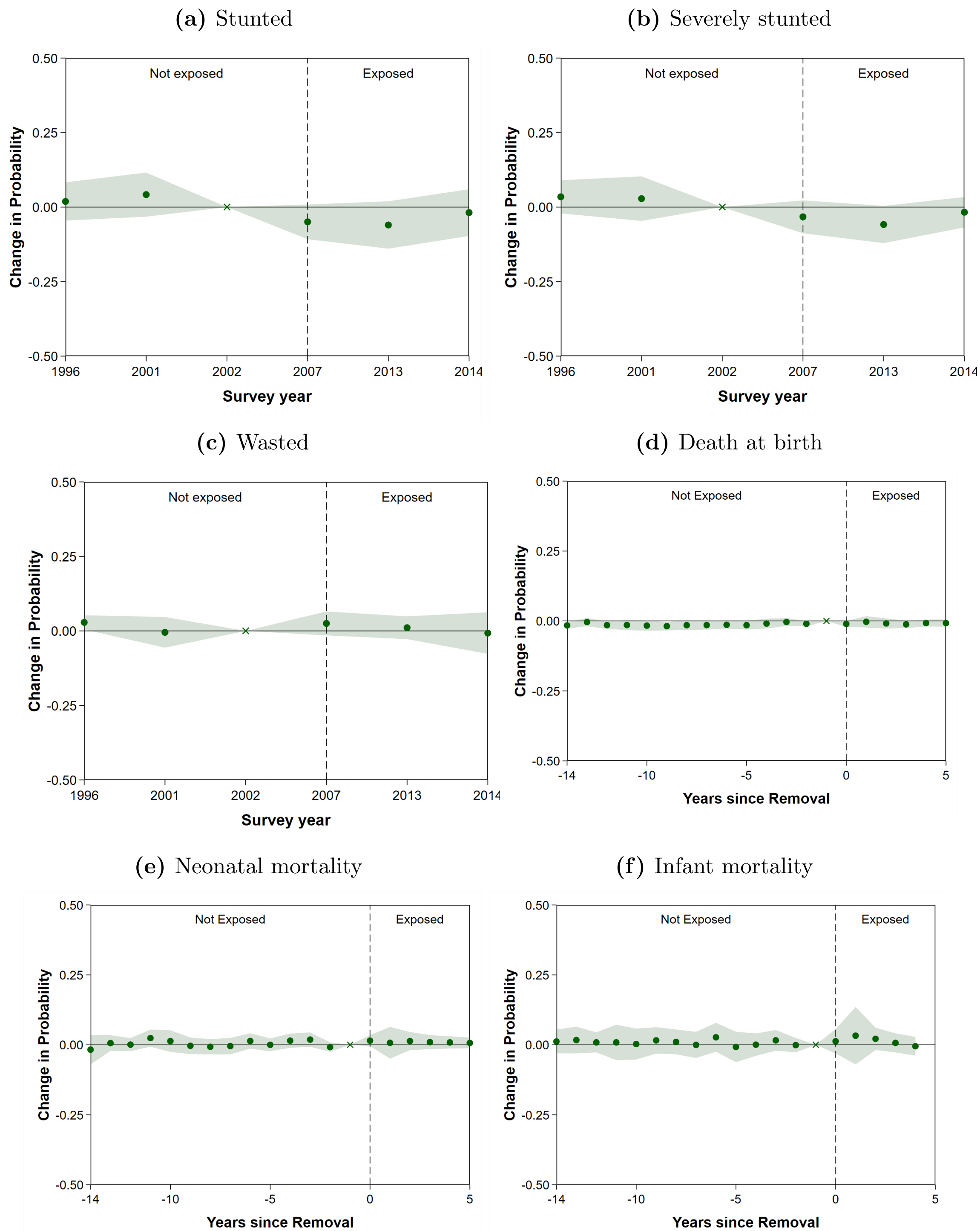


Figure 3. Event study estimates of the effect of user fee removal on child health

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: These figures show the coefficients for interaction terms between time dummies and treatment status obtained from an event-study specification. Year of implementation is normalized to zero. In addition to area and year of childbirth fixed effects, the covariates include a dummy for multiple pregnancy and child's sex. Regressions for anthropometric outcomes also control for child's age dummies, and regressions for mortality outcomes for mother's year of birth. Shaded areas represent 95% confidence intervals with robust standard errors clustered at the area level. The omitted category is the last pre-treatment time dummy. Outcomes of interest are dummies equal to one if child is (a) stunted (height for age z-score<-2), (b) severely stunted (height for age z-score<-3), and (c) wasted (weight for height z-score<-2), or died (d) at birth, (e) within her first 28 days of life, and (f) before reaching the age of one year, zero otherwise. Note that for anthropometric outcomes, points estimates for 2013 and 2014 should be interpreted as the effect of a difference in length of exposure to free health care, since the policy was extended throughout the country from 2012. For neonatal and infant mortality, children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group. Hence, it is not possible to assess the effect on infant mortality for children born in 2011.

Since analysis of child anthropometric indicators is solely based on survivors' measurement at time of survey, one potential threat to identification for these outcomes is selection due to endogenous mortality. In particular, survivors may be stronger than those who died prematurely. However, such selection effect is unlikely to play a role here as we do not observe any effect of the policy on child mortality outcomes. Nonetheless, I test the robustness of my results by considering selection through survival in two ways.[22] First, I use an inverse probability weighting method to weight observations according to predicted survival probabilities at survey time. Second, following Cosslett [1991], I use a semi-parametric approach by including one indicator variable for each centile of predicted survival probabilities as additional control variables. In both cases, point estimates remain extremely similar (results reported in Appendix Table H1).

I also investigate the effect of removing user fees on health investment in children through vaccination. In particular, I check whether child's vaccinations against polio, measles, diphtheria, pertussis, tetanus and tuberculosis were up-to-date at survey time. Results are presented in column 4 of Appendix Table E1. I find no discernible effect of the policy on child's vaccination.

4.2.2. The decrease in chronic malnutrition only occurs after a certain duration of exposure to the removal of user fees

Duration of exposure to the policy may drive the average treatment effect I found on anthropometric outcomes. One can reasonably expect that children should benefit more if they have been exposed longer to free primary health care. To investigate any heterogeneous effect of the duration of exposure, I change the $Exposed_{ta}$ term in my equation for a continuous measure of exposure based on date of measurement, date of birth and date of user fee removal in area a. This measure ranges from 0 to 59 months of exposure. Results are reported in Table 4. I find that being exposed to the policy for an additional month significantly reduces chronic malnutrition (Columns 1 and 3) but has no impact on the risk of being wasted (Column 5). However, such effects may require a minimum duration of exposure to manifest. This is exactly what I find (Columns 2, 4 and 6). For instance, results suggest that children need to be exposed to the policy for more than 12 months for their risk of being severely stunted to shrink.

[22] I do not implement the standard Heckman procedure since the predictors of the selection equation and the main equations are the same.

Table 4. The effect of the length of exposure to user fee removal on child nutritional status

	(1)	(2)	(3)	(4)	(5)	(6)
	Stunted		Severely stunted		Wasted	
Linear duration of exposure (in months)	−0.001***		−0.001***		0.000	
	(0.000)		(0.000)		(0.000)	
Duration of exposure (in months):						
]0, 12]		−0.011		−0.018		0.011
		(0.030)		(0.024)		(0.013)
]12, 24]		−0.036		−0.050***		−0.002
		(0.024)		(0.015)		(0.011)
]24, 36]		−0.033		−0.050**		−0.008
		(0.028)		(0.021)		(0.016)
]36, 48]		−0.080***		−0.066***		−0.001
		(0.026)		(0.019)		(0.017)
]48, 59]		−0.070***		−0.057***		−0.001
		(0.025)		(0.017)		(0.015)
Mean before policy	0.545	0.545	0.286	0.286	0.063	0.063
R^2	0.086	0.086	0.066	0.066	0.022	0.022
N	21,106	21,106	21,106	21,106	21,065	21,065

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a child. The sample is restricted to children alive at survey time. The table reports the average effect of the length of exposure to the user fee removal on anthropometric indicators. *Stunted* (respesticely *Severely stunted*) is a dummy which equals one if the height-for-age ratio is at least two (respectively three) standard deviations below WHO z-score, zero otherwise. *Wasted* is a dummy equals to one if the weight-for-height ratio is at least two standard deviations below WHO z-score, zero otherwise. All regressions control for area and survey year fixed effects, a dummy for multiple births, as well as child's sex and age dummies.
* $p<.10$; ** $p<.05$; *** $p<.01$

4.2.3. Child mortality risk is only affected near health facilities providing essential emergency obstetric care and child health services

The results for child mortality outcomes appear in columns 4 to 6 of Table 3. For neonatal and infant mortality I dropped children who did not reach the corresponding age at survey time to avoid censoring bias, and by 2012 since the policy was then extended to the control group. All point estimates are precisely estimated and very close to zero, suggesting that on average the removal of user fees has had no impact on child mortality, regardless of the definition considered and whatever the distance from the nearest health facility as shown in Figure 4.

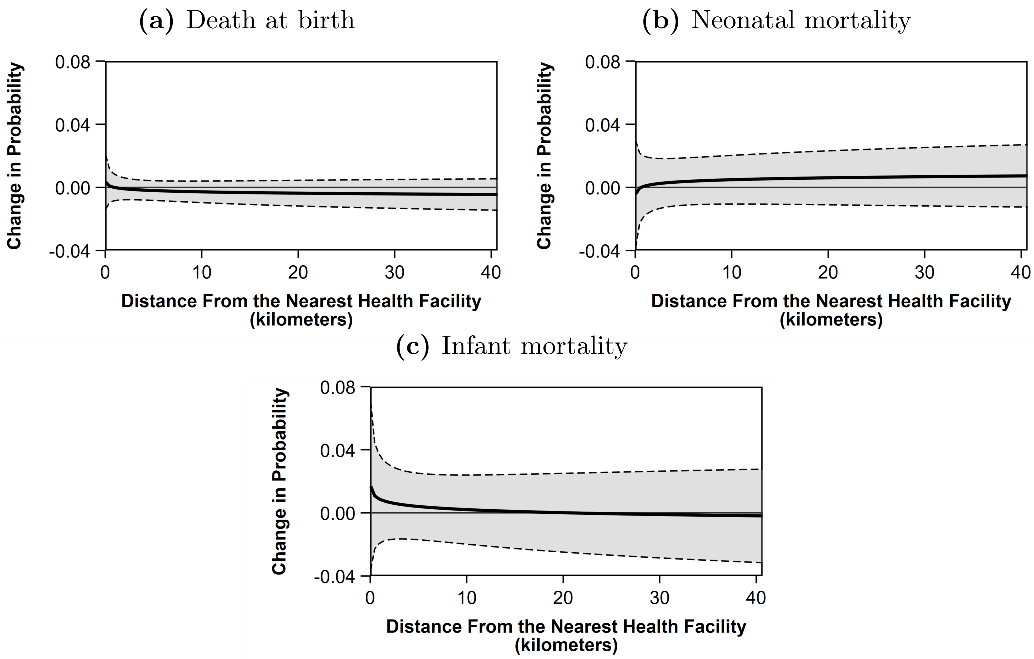


Figure 4. Marginal effect of exposure to the policy on child mortality outcomes depending on distance from the nearest health facility

Source: Author's calculations from DHS 2007 and 2013.
Notes: The figures plot the marginal effect of the policy change depending on the distance from the nearest health facility. Distance corresponds to the straight-line distance between each DHS cluster from the 2007 and 2013 survey waves and the nearest publicly-supported health facility from the 2005 Health Facility Census. Each figure is from a separate estimation where the distance is log-transformed and interacted with exposure to the policy. Control variables include area and year of childbirth fixed effects, as well as mother's year of birth, a dummy for multiple births and child's sex. For neonatal and infant mortality, children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group. It is not possible to investigate such heterogeneous effects for anthropometric outcomes since clusters from the pre-policy period (surveyed in 1996 and 2001) were not georeferenced, so that all surviving children georeferenced were measured after the policy change. Shaded areas represent 95% confidence intervals with robust standard errors clustered at the area level.

Next, I examine whether the absence of an effect on child mortality outcomes masks any heterogeneity with respect to the presence of a publicly-supported facility providing emergency obstetric care and child health services locally. Point estimates are reported in Table 5. I find that the policy has led to a decline in newborn mortality risk at birth within affected areas for households living close to a qualified health facility relative to both those that are not (-0.011, significant at the 5% level) and those located in an unaffected area (-0.012, significant at the 5% level). Conclusions remain the same when using a restricted sample around the census date. It suggests that combined with an improved physical access to essential health services for maternal and child care, such as management of obstetric emergencies and resuscitation of newborns, removing user fees can be an effective way to reduce child mortality risk. This result echoes the one from Bagnoli [2019] in Ghana, who observes a positive effect of health insurance only for children living in regions with a high-quality of care. This is also consistent with Godlonton and Okeke [2016] who find that the increase in institutional births following a ban on informal health providers in Malawi was accompanied by a reduction in newborn mortality only for households close to a high-quality health facility.

Table 5. The effect of user fee removal on child mortality depending on the local availability of a qualified publicly-supported health facility

	(1) Death at birth	(2) Neonatal mortality	(3) Infant mortality
Panel A. *Average effect of user fee removal - Whole sample*			
Affected by the policy	−0.001	0.006	0.007
	(0.003)	(0.008)	(0.011)
Qualified health facility within 5 km	0.008	0.010	0.008
	(0.006)	(0.008)	(0.012)
Affected by the policy × Qualified health facility within 5 km	−0.011**	−0.020**	−0.021
	(0.005)	(0.009)	(0.016)
Mean before policy	0.011	0.030	0.062
R^2	0.022	0.031	0.033
N	14,267	13,969	10,350
Panel B. *Average effect of user fee removal - Sample: ± 4 years around 2005 facility census*			
Affected by the policy	0.001	0.010	0.014
	(0.004)	(0.009)	(0.012)
Qualified health facility within 5 km	0.009	0.011	0.006
	(0.007)	(0.009)	(0.013)
Affected by the policy × Qualified health facility within 5 km	−0.011*	−0.027**	−0.022
	(0.007)	(0.012)	(0.014)
Mean before policy	0.011	0.030	0.062
R^2	0.025	0.035	0.035
N	9,019	8,944	7,716
Panel C. *Average effect of user fee removal - Sample: ± 3 years around 2005 facility census*			
Affected by the policy	0.002	0.006	0.024
	(0.005)	(0.011)	(0.019)
Qualified health facility within 5 km	0.009	0.003	0.003
	(0.008)	(0.011)	(0.015)
Affected by the policy × Qualified health facility within 5 km	−0.014*	−0.034*	−0.044
	(0.008)	(0.018)	(0.037)
Mean before policy	0.011	0.030	0.062
R^2	0.029	0.040	0.037
N	6,415	6,340	5,112

Source: Author's calculations from DHS 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a childbirth. The table reports the average effect of user fee removal on child mortality outcomes estimated on the whole sample (Panel A), and alternatively for children born ± 4 years (Panel B) or ± 3 years around the 2005 Health Facility Census (Panel C). All regressions control for area and time fixed effects, as well as mother's year of birth, a dummy for multiple births, child's sex and the log of the straight-line distance between each DHS cluster from the 2007 and 2013 survey waves and the nearest publicly-supported health facility. A health facility is considered *qualified* if it provides a set of essential emergency obstetric and child health services, based on the 2005 Health Facility Census. See footnote 12 for a full description of this indicator. For neonatal and infant mortality, children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group. It is not possible to investigate such heterogeneous effects for anthropometric outcomes since clusters from the pre-policy period (surveyed in 1996 and 2001) were not georeferenced, so that all surviving children georeferenced were measured after the policy change.
*$p<.10$; **$p<.05$; ***$p<.01$

4.3. Selection Issues and Fertility Behaviors

The null effect on child mortality may be explained either by a real absence of effect,[23] or by selection effects and fertility decisions induced by the policy change.

4.3.1. Selection into Pregnancy and Composition Effects

One potential threat to identification is that demographic characteristics of mothers may have changed in a different way in affected and unaffected areas after the removal. In case of selection into pregnancy, specific women may react to the policy by having more babies. A related concern is that we can only observe childbirths and children from mothers who survived until survey time. The policy change may have helped high-pregnancy related risks women survive by reducing their risk of maternal death. In that case, affected and unaffected areas may have diverged in post-treatment periods not only in terms of policy implementation but also in terms of characteristics of women giving birth. If such women also tend to have babies with poor health outcomes, my results may underestimate the gains in terms of child health.

In Appendix Table G1, Panel A, I test the absence of compositional changes in affected areas relative to unaffected ones by estimating difference-in-differences regressions with maternal characteristics as dependent variables. Results suggest no composition effect in women giving birth, which strengthens the causal interpretation of my results. Then, I replicate this analysis separately for births in a publicly-supported facility (Panel B) and home births (Panel C) to check if the average characteristics of women giving birth in each type of delivery place changed differently in affected and unaffected areas after the removal. The average education level of mothers giving birth in a publicly-supported health facility decreased in affected areas relative to unaffected areas, and women who persist in giving birth at home despite the policy change become relatively older in affected areas than in unaffected ones.

4.3.2. Selection into Medical Delivery

The policy may have failed to reach households with the higher maternal and child mortality risks, for which returns to formal health services are potentially high. For example, there might be a selection effect into medical delivery within affected areas in case of imperfect information concerning the policy. In particular, more educated women may have a better access to information and a higher capacity to ensure the removal of user fees. If such women also have *ex-ante* relatively

[23] Due for example to an insufficient or a drop in quality of public health services, or because mortality is an indicator too extreme to capture the health improvements brought about by free health care.

low-mortality risk babies, the probability to deliver in a health facility may increase without any effect on child mortality.

I explore this possibility in two ways. In Panel A of Appendix Table G2, I interact exposure to the policy with mother's education. I find that more educated mothers did not respond more to the policy than others. Then, I interact exposure to the policy with an indicator for whether mothers have already experienced an infant death before childbirth, and can thus be considered at risk.[24] Risky mothers benefited from the policy as much as the non-risky ones, which suggests no advantageous selection within affected areas (Appendix Table G2, Panel B).

In addition, I find no heterogeneous effect according to household's material wealth except for child nutritional status, for which the policy has essentially benefited the poorest (Appendix Figure G3). The same conclusion applies when focusing on rural districts only. However, within rural areas of urban districts, the removal of user fees has essentially benefited the poorest, including in terms of access to delivery services.

4.3.3. Selection into Live Birth

The policy may have led to selection into live birth if improvement in delivery conditions helped fragile babies survive to childbirth. If these babies would not have survived in absence of the policy, then the probability to observe weaker, high-child mortality risk babies increases in affected areas relative to unaffected ones. In such a case, my sample of exposed children would be negatively selected, potentially leading to underestimate the gains in child health.

I test this assumption in two ways. First, using information from the reproductive calendar collected in the 2007 and 2013 DHS survey waves, I create a new database where each observation is now a pregnancy occurring during the last five years preceding the survey, whatever its final outcome, and not necessarily a live birth. I find no effect of the policy on the risk of stillbirth (Appendix Table G4, Column 1). Second, I check this assumption by looking at the gender composition of live births after the policy implementation. Male fetuses are commonly known to be biologically weaker and more susceptible to disease and premature death than female ones due to sex differences in genetic makeup. This is particularly true in Sub-Saharan Africa, even when controlling for the preconception environment [Pongou, 2013]. A recent meta-analysis finds a risk of stillbirth 10 percent higher for males fetuses than for females, a pattern consistent across countries of different income groups [Mondal et al., 2014]. Hence, if the policy has helped fragile babies survive, one should observe an increase in the proportion of male births in affected areas

[24] In my sample, 24% of non-first born children have at least one elder sibling who died before reaching the age of one.

relative to unaffected ones. However, I find no evidence of such an effect (Appendix Table G4, Column 3).

4.3.4. *Fertility*

Couples may have changed their fertility decisions in response to the removal. By lowering the direct cost of having a child, the policy change may have induced parents to have more children with shorter birth intervals in a way that increases child mortality risk [Molitoris et al., 2019]. Such fertility decisions are likely to be endogenous. In particular, it may be influenced by unobserved characteristics at the household level including parents' preferences, and motivated by a replacement effect in case of child death [Bhalotra and van Soest, 2008; Hossain et al., 2007].

To take it into account, I restrict my sample to first born children and conclusions remain unchanged (Appendix Table G5).[25] I then explore the effect on birth spacing and find that the policy has not resulted in riskier birth intervals (Columns 1 and 2, Appendix Table G7).[26] As an alternative test for a fertility effect, I transform my cross-sectional individual data into a panel at the area $\times$ birth date $\times$ survey year level and find no aggregate effect of the policy change on number of reported births per 1,000 surveyed mothers (Column 3, Appendix Table G7).

4.4. Robustness Checks

Contamination effects - One concern is that some individuals living in control areas might have benefited from the policy if they seek care in a treated one. In such a case, point estimates will likely represent lower bounds of the true effects. Using data from the 1998 Living Conditions Monitoring Survey, Lépine et al. [2018] have identified three urban districts (Kasama, Mazabuka and Mongu) in which a significant part of the population (12% to 25%) declared seeking care in a rural district. People living in such districts might have benefited from the policy while they should not. Conclusions remain unchanged when these three districts are excluded from the analysis (Appendix Figure H3). In the same way, those living in control enumeration areas spatially close to a treated one could have benefited from it. I show that conclusions are robust to

[25] Point estimates are even higher than for the full sample, which is not surprising since parents may be more concerned with their first birth and cannot rely on their own past experience as parents when choosing where to give birth, a result consistent with Sialubanje et al. [2015]. Point estimates by rank of birth plotted in Appendix Figure G6 suggest that the positive effects of user fee removal fade away with birth rank. However, this gradient is less pronounced once mother's education level and wealth are taken into account. This is consistent with the fact that children with a high birth rank are reported by mothers on average less educated than the other ones, and are observed in the poorest households.

[26] The World Health Organization recommends a birth interval of at least 24 months after a live birth to prevent maternal, perinatal and infant disorders [WHO, 2007].

the exclusion of control enumeration areas located less than five kilometers away from an affected one (Appendix Figure H4).

Migration - Due to data limitation, the effective area of birth of each child is unknown. Of the 25,678 births occurring during the last five years preceding the survey, 85% occurred in the same locality as the place of residence.[27] It is not possible to track where the remaining 15% of births occurred: in another district, or in another place within the same district. This figure overestimates the share of births for which treatment status might be incorrectly assigned, as place of birth and place of residence can be different but in the same area, or in different areas but with the same treatment status. Overall, it suggests that migration should not drive my results. This is exactly what I find. I show that dropping mothers who have migrated since childbirth leaves the results unchanged (Appendix Figure H3).

Other robustness checks - Further robustness checks are performed and extensively discussed in Appendix H. Most importantly, the results are qualitatively unchanged if I include additional control variables and district-specific linear time trends. Point estimates remain also very similar when I use an alternative definition of exposure to the policy based on geographic coordinates. I also test the robustness of my results to combining difference-in-differences with several matching approaches, which leaves the conclusions unchanged.[28]

5. Discussion and Concluding Remarks

This paper offers new evidence on the extent to which the removal of user fees affects demand for curative health services and child health in a resource-limited setting. Exploiting variation in the timing of the abolition of user fees across districts of Zambia, this paper points to large and sustained positive effects of the policy on maternal health care utilization and delivery conditions.

However, these effects did not materialize immediately after the removal. This might be explained by several factors specific to the reform [Carasso et al., 2010]. First, it was announced suddenly by the incumbent President during a pre-election period. While this suddenness is an advantage to identify the policy's impacts, it left little time and capacity to precisely design the reform, to anticipate its effects and finally to provide adequate resources to facilities. Second, and related to this, the implementation rules of the policy were unclear and changed at the last time, causing confusion at the district and facility levels during the first months following the official removal date. In particular, it was unclear where user fees should be abolished. Third, health facilities

[27] For sake of comparison, I find a very similar figure with the national census data from 1990, 2000 and 2010 (86.9% of children were born in their current district of residence).
[28] Results available upon request.

were initially seriously under-compensated for the loss of user fee revenues, leading to the discontinuation of several health services. The replacement grant was initially based on projected loss of revenues based on fees collected prior to the removal but was seriously underestimated. Major delays in disbursement of compensation fund were also observed, with facilities receiving it 8 to 12 months after the removal. Last, the important shortage of essential drugs and medical supplies, as well as the inefficient allocation of funds in 2006 in favor of hospitals, which has resulted in a 40% drop in districts primary health services funding [Carasso et al., 2010], have certainly limited the effectiveness of the reform in its early stages. Hadley [2011] suggests that even when drugs were available, they were not used efficiently. While necessary, compensation for the loss of user fee revenues and the increased workload should not mask earlier, deeper problems such as health workers shortage and lack of equipment.

This paper also finds that the removal of user fees drastically reduced child chronic malnutrition but only for those exposed at least 12 months to free health care. There is no discernible impact on child mortality, a result which is not driven by selective fertility, nor by a selection effect into live births. A potential explanation of this limited effects on child health is a drop in quality of health services after the reform. Due to data limitations it cannot be tested directly, but several pieces of evidence suggest a drop in effective quality after user fees were removed while perceived quality remained stable or even improved [Masiye et al., 2010]. Overall, these results are in line with a broad set of empirical studies looking at the effect of free health care or health insurance, which find an increase in health care utilization but no or limited effect on health, both in low- (Ansah et al., 2009; Powell-Jackson et al., 2014; and Erlangga et al., 2019 for a review) and high-income countries [Card et al., 2004; Chen et al., 2007; Finkelstein and McKnight, 2008], even if evidence is more mixed for the latter.

This paper suffers from several caveats I wish to stress here. First, some individuals living in unaffected areas may have sought care in an affected one. While I cannot completely rule out this possibility, I show that the results remain the same when districts with potentially significant contamination effects are excluded, and when control areas close to treated ones are removed. Moreover, in some facilities informal payments may have been introduced or increased following the removal of user fees. For these reasons, results presented in this paper must be interpreted as lower bound estimates of the true effects of the policy in an ideal framework with perfect compliance and enforcement of the policy change. Second, due to data limitations, it is not possible to study the effect of the policy on maternal mortality. However, there is suggestive evidence that there was no compositional change in mothers reporting births after the policy change, and there is no distinguishable trend break in the national maternal mortality ratio after the removal (Appendix Figure F1). Finally, the absence of effect on child mortality does not imply that free health care is ineffective in improving child health, and so for several reasons:

mortality is certainly an extreme indicator of child health conditions, and too blunt a measure to reflect health improvements associated with free health care; I find encouraging results for chronic malnutrition, one of the leading causes of child morbidity and mortality; and the removal of user fees may have impacted other important health issues not explored due to data limitations such as medical treatment of malaria.

This paper contributes to the ongoing debate on the relative effectiveness of demand- and supply-side interventions in improving population health in low-income countries. It suggests that removing user fees is a good way to stimulate individuals' demand for curative health services but is clearly not sufficient *per se* to generate huge gains in individuals' health. If access improved, returns to formal health services are limited. Health care quality appears as a crucial piece of the puzzle since child mortality risk only decreased in the vicinity of qualified health centers. These conclusions have important policy implications for population health. They call for massive efforts to improve the capacity of such health care systems to provide financially accessible, high-quality health services to all.

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Supplementary Material

for

From Fees to Free: User Fee Removal, Maternal Health Care Utilization and Child Health in Zambia

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Appendix A. Conceptual Framework

Consider individuals that maximize their utility function over their health stock, determined by their current investment in health as well as their stock of health during the previous period of time. These individuals are subjected to a budget constraint which depends on their resources and a vector of prices, including the prices of preventive and curative health investments. We assume that individuals value health in itself since, other things being equal, they prefer to be healthy than sick, and thus invest in their health if they have the opportunity to do it. For example, individuals may decide whether to invest in and use a mosquito net to protect against malaria, whether to be vaccinated, whether to seek care and, if so, when and from which health provider [Dupas, 2011]. We also assume that individuals are not covered by a health insurance scheme, since only 4% of Zambians had a health insurance during the period studied in this paper.

To decide whether to invest in their health, individuals compare the marginal benefit with the marginal cost of doing such an investment, given their actual health stock.[29] It follows a demand for health products and services which depends negatively on the price, as is usual. For instance, numerous empirical studies have found a high price elasticity of demand for health products (e.g. Ashraf et al., 2010; Cohen and Dupas, 2010), suggesting that household's health-related decisions may be very sensitive to price. It could partly explain the large differences in maternal health care utilization and nutritional status by wealth level observed in Zambia before the removal of user fees,[30] both nationally and within rural and urban areas (Appendix Table C5). Other things being equal, including the perceived quality of care, if households were kept out of health services for financial reasons, a fall in price of health services should lead to a higher demand for health care.

By lowering the marginal cost of health investments, user fee removal will *ceteris paribus* increase individuals' demand for health care and result in better health. Moreover, resources released by fee exemption may be reallocated within households towards other virtuous practices such as

[29] In particular, if their health stock reaches its maximum level, then the marginal benefit will be zero for curative care but positive for preventive care. Indeed, the objective of preventive investments is to reduce the likelihood of adverse health shocks in the future, with benefits that extend far beyond the current period of time.

[30] For instance, 63% of childbirths were assisted by a health professional among the richest 50%, compared with 24% only among the poorest 50% (42% vs. 23% in rural districts, 36% vs. 22% in rural areas of urban districts, and 91% vs. 69% in urban districts).

higher nutritional intakes or investment in preventive health products such as mosquito nets and vaccination. If this is the case, then we should observe an increase in individuals' health following the removal of user fees in health facilities.

However, we might expect to find no evidence of such an effect for different reasons. First, if quality of care was initially too low, encouraging health facility visits at a reduced cost may not translate into health gains for users. I investigate whether this is the case by looking at the heterogeneous effect of removing user fees depending on the quality of health services available locally.

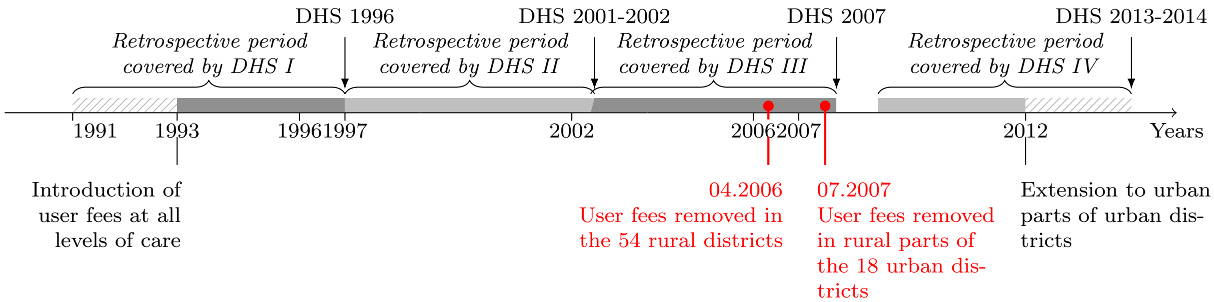
Second, the overall increase in health services utilization after user fee removal may have led to a deterioration of health care quality because of insufficient funding and human resources to compensate for the increase in utilization [Meessen et al., 2011]. This drop in quality may affect one's health stock in the short term when perceived quality has not changed but effective quality of services offered already did.[31] In the medium- to long-term, individuals may react and reduce their demand for health care in the public sector since the drop in quality potentially lowers the marginal benefit of investing in their health and increases the marginal cost of doing so. If quality of delivery care becomes equivalent at home and in a health facility, then we would only observe a price effect on demand for health care without any impact on health outcomes.

Third, the loss of user fee revenue in health facilities and the increased workload may have encouraged health workers not to spread information about the removal and to charge informal fees on users [Hatt et al., 2013; Nabyonga-Orem et al., 2011]. In that case, user fee removal will not (or not fully) translate into lower out-of-pocket health expenditures for households, hence reducing the expected higher health services utilization and potential health gains. Hangoma et al. [2018] find a reduction in the probability of incurring any spending after the removal in Zambia.

Fourth, other barriers may discourage individuals from seeking care, like health staff absenteeism [Banerjee et al., 2008; Chaudhury and Hammer, 2004], distance from health facilities [Thornton, 2008] or imperfect information on the benefits and costs of health investments [Rhee et al., 2005; Jalan and Somanathan, 2008; Banerjee et al., 2015]. The removal of user fees at the point of services might thus not be sufficient to reduce marginal cost of health investment below the perceived marginal benefit associated with, leaving individuals' demand for health services unchanged. In particular, I explore how physical access to health amenities shapes the effect of removing user fees.

[31] For example, demand for health care cannot be fulfilled, longer waiting times induce higher risk of birth asphyxia, or going to a health facility may raise one's risk of contracting a disease if sanitary conditions have worsened.

Appendix B. Timeline of the policy and survey waves



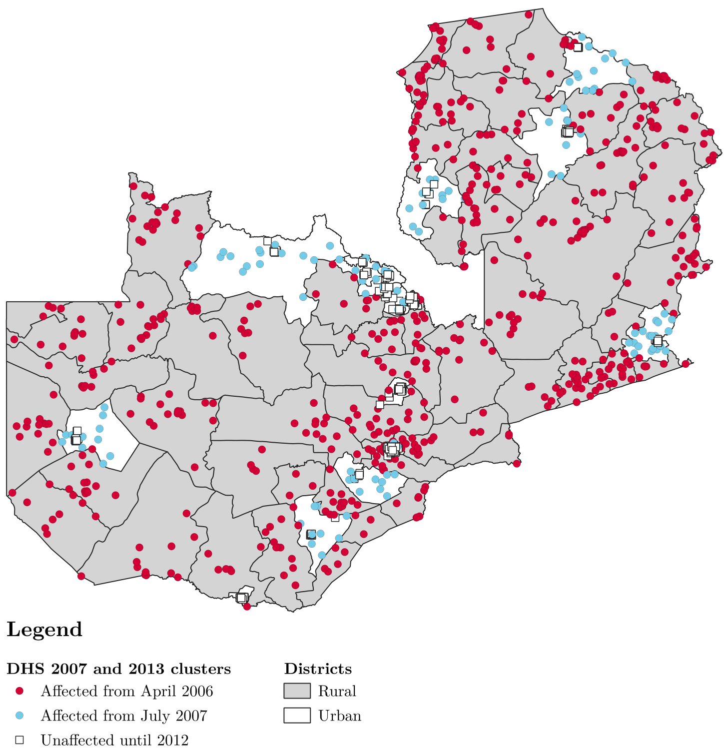
Appendix C. Demographic and Health Surveys

C.1. Sampling frame

The Demographic and Health Surveys sample design is based on a two-stage sampling procedure. First, enumeration areas, known as clusters, are selected from a sampling frame corresponding to a list of enumeration areas from the most recent national population census. The sampling frame is stratified by province and urban/rural areas within each province, and enumeration areas are randomly selected using a probability proportional to size method. Then an exhaustive listing of all the households present in each cluster is carried out. Second, 20 to 30 households per cluster are randomly selected with equal probability. Within sampled households, all women aged 15-49 who were either permanent residents of the household or visitors present on the night before the survey were eligible for survey interview. Sample design and questionnaires are standardized across survey waves which allows for pooled cross-section analysis.

C.2. Spatial roll-out of the policy

Figure C1. Map of districts and DHS clusters



Source: Author based on DHS 2007 and 2013.
Notes: The map shows the 72 districts of Zambia according to the 2006 classification of the Government. Grey areas are rural districts and white areas represent urban districts. Cluster surveyed in the Demographic and Health Surveys were not georeferenced in 1996 and 2001. Hence, the map only reports DHS 2007 and 2013 clusters. Red dots correspond to clusters located in districts where user fees were removed from April 2006. Blue dots denote clusters located in rural areas of urban districts where user fees were removed from July 2007. White squares represent clusters located in urban areas of urban districts, where user fees were maintained until 2012.

C.3. Distribution of birth year

Table C2. Distribution of live births by year of birth and treatment status

	Rural districts	Rural areas of urban districts	Urban areas of urban districts	N
1993	747	98	361	1,206
1994	794	109	368	1,271
1995	815	116	347	1,278
1996	628	95	260	983
1997	627	165	230	1,022
1998	748	229	279	1,256
1999	815	255	274	1,344
2000	884	280	294	1,458
2001	858	230	270	1,358
2002	399	136	178	713
2003	772	186	266	1,224
2004	796	159	283	1,238
2005	739	209	320	1,268
2006	824	210	304	1,338
2007	432	82	141	655
2008	116	24	74	214
2009	1,712	310	582	2,604
2010	1,754	312	568	2,634
2011	1,739	325	550	2,614
N	16,199	3,530	5,949	25,678

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013. *Notes:* Based on my sample, DHS 1996 was collected from 1996-07-18 to 1997-01-06, DHS 2001-2002 from 2001-11-08 to 2002-06-03, DHS 2007 from 2007-04-03 to 2007-10-08, and DHS 2013 from 2013-08-20 to 2014-04-16. Few households are surveyed in the last days of each wave, and by construction there is no data for the last three months of 2007 nor for the first eight months of 2008, which explains the lower sample sizes for these two years. A higher number of households was sampled in 2013 than in the two previous waves. See Appendix B for a timeline of the policy change and periods covered by the different survey waves.

C.4. Sample sizes and anthropometric measurement

Table C3. Sample sizes

	1996	2001	2007	2013	Whole sample
Panel A. *Child mortality analysis*					
Sample size	4,734	6,677	6,201	8,066	25,678
% of children alive at survey time	0.868	0.872	0.913	0.934	0.901
Panel B. *Childbirth conditions*					
Sample size - Place of delivery	4,724	6,634	6,163	7,964	25,485
↳ % births with missing information	0.002	0.006	0.006	0.013	0.008
Sample size - Assistance received	4,727	6,665	6,182	8,006	25,580
↳ % births with missing information	0.001	0.002	0.003	0.007	0.004
Panel C. *Child anthropometric measurement*					
Among survivors % measured	0.940	0.945	0.927	0.913	0.929
Among survivors % not present	0.036	0.016	0.009	0.032	0.023
Among survivors % refused	0.009	0.003	0.013	0.004	0.007
Among survivors % sick or other	0.015	0.036	0.051	0.051	0.041
Sample size after cleaning of height-for-age z-score†	3,813	5,375	5,086	6,832	21,106
Sample size after cleaning weight-for-height z-score†	3,801	5,396	5,068	6,800	21,065

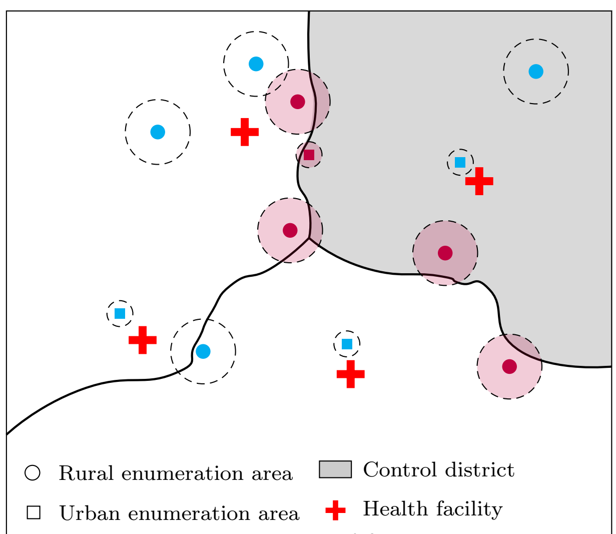
Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: The table reports the different sample sizes for child mortality (Panel A), childbirth conditions (B) and child nutritional status (Panel C) analysis.
Based on weight and height measures, I compute anthropometric z-scores.
† Among children measured, I exclude from the analysis the ones with biologically implausible z-scores values according to the World Health Organization, ie. height-for-age z-score below -6 or above 6 for stunting, and weight-for-height z-score below -5 or above 5 for wasting [WHO, 2019]. They represent 1.8% and 1.9% of measured children, respectively.

C.5. Scrambling procedure and geographic coordinates

For confidentially reasons, geographic coordinates have been randomly displaced by the DHS. Urban clusters are randomly displaced within a radius of 2 kilometers around their real location, creating a positional error ranging from a minimum of 0 and a maximum of 2 kilometers. For rural clusters, the maximum displacement increases to 5 kilometers, and up to 10 kilometers for a further 1% of them. See DHS methology for geographic data for additional information.

This may create measurement errors and an attenuation bias since households may have been assigned to the wrong district and potentially to the wrong treatment status. This is a problem only for those enumeration areas located near the boundary of a district which has not the same treatment status as the assigned one. In Fig. C4, I show the case where an urban cluster and four rural ones (in red) are potentially assigned to the wrong treatment status as their (unknown) exact location can be either in a treated or a control area.

Figure C4. Random displacement of enumeration area and treatment status



Notes: Large circles represent a radius of five kilometers around rural clusters and small circles a radius of two kilometers around urban clusters.

C.6. Pre-treatment differences in childbirth conditions and child health by wealth level

Table C5. Pre-treatment differences in childbirth conditions and child health outcomes between the poorest 50% and the richest 50%

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	National level			Rural districts T1			Rural areas of urban districts T2			Urban areas of urban districts C		
	Poorest 50%	Richest 50%	*p*-value (1)=(2)	Poorest 50%	Richest 50%	*p*-value (4)=(5)	Poorest 50%	Richest 50%	*p*-value (7)=(8)	Poorest 50%	Richest 50%	*p*-value (10)=(11)
Panel A. *Childbirth conditions*												
Assisted by a health professional	0.24	0.63	0.000	0.23	0.42	0.000	0.22	0.36	0.000	0.69	0.91	0.000
Assisted by a doctor	0.01	0.05	0.000	0.01	0.02	0.000	0.00	0.03	0.000	0.05	0.11	0.000
Assisted by a nurse or midwife	0.23	0.60	0.000	0.22	0.40	0.000	0.21	0.34	0.000	0.67	0.87	0.000
Institutional delivery	0.25	0.63	0.000	0.24	0.43	0.000	0.23	0.36	0.000	0.69	0.91	0.000
Delivered in a public health facility	0.24	0.58	0.000	0.24	0.42	0.000	0.23	0.36	0.000	0.69	0.73	0.002
Delivered in a private health facility	0.00	0.05	0.000	0.00	0.01	0.000	0.00	0.01	0.095	0.00	0.18	0.000
Panel B. *Child mortality*												
Death at birth	0.01	0.01	0.894	0.01	0.01	0.335	0.01	0.01	0.165	0.01	0.01	0.786
Neonatal mortality risk	0.03	0.03	0.942	0.04	0.03	0.084	0.04	0.03	0.337	0.04	0.03	0.360
Infant mortality risk	0.09	0.09	0.957	0.09	0.09	0.849	0.08	0.08	0.725	0.10	0.08	0.020
N	7,995	7,946	15,941	4,959	4,856	9,815	1,295	1,254	2,549	1,925	1,885	3,810
Panel C. *Child nutritional status*												
Stunted	0.57	0.46	0.000	0.57	0.52	0.000	0.59	0.51	0.001	0.44	0.36	0.000
Severely stunted	0.31	0.22	0.000	0.31	0.26	0.000	0.32	0.27	0.013	0.19	0.15	0.008
Wasted	0.06	0.06	0.244	0.07	0.06	0.027	0.06	0.07	0.485	0.07	0.06	0.417
N	4,691	4,611	9,302	2,933	2,855	5,788	882	913	1,795	1,074	1,137	2,211

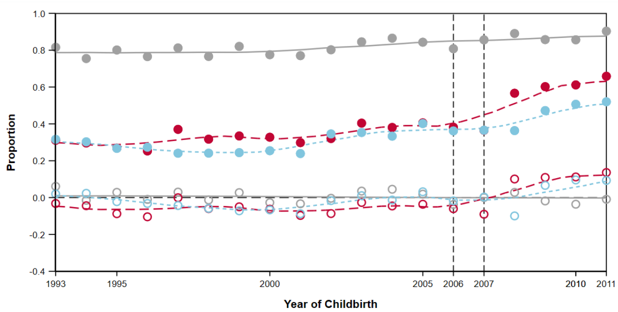
Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Unweighted statistics. The table reports summary statistics for live births occurring before the removal of user fees (Panels A and B) or children measured in a survey wave prior to the removal (Panel D), disaggregated between the poorest 50% and the richest 50%. This analysis is carried out at the national level (columns 1-2), and within each treatment groups (T1, T2 and C). Columns 3, 6, 9 and 12 report *p*-values associated with the comparison between the poor and the rich. In Panel D, the sample is restricted to children alive at survey time.

Appendix D. Parallel trends and event-study analysis

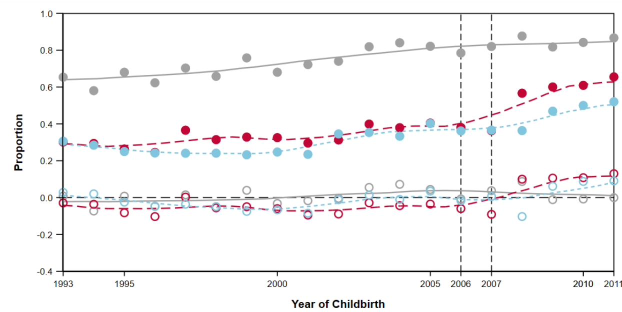
D.1. Trends in outcomes of interest

Figure D1. Trends in condition of childbirth and child mortality

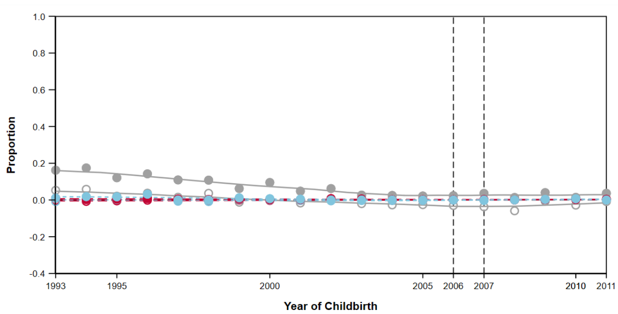
(a) Institutional delivery



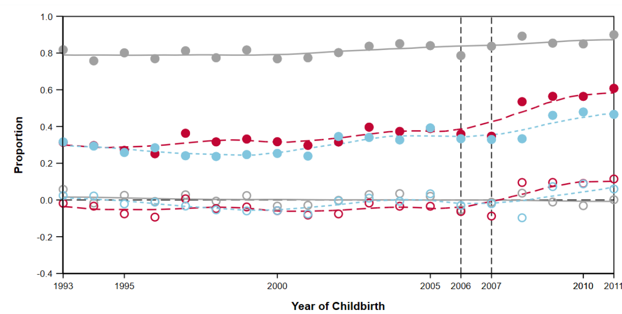
(b) Delivery in a public facility



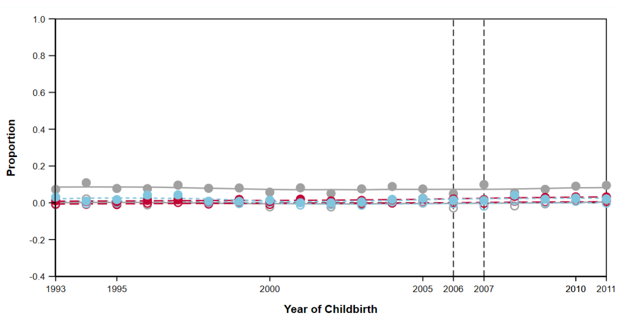
(c) Delivery in a private facility



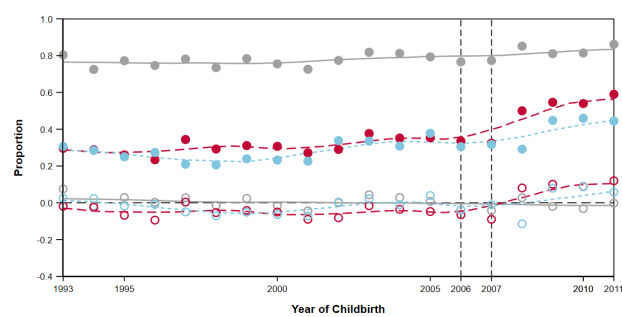
(d) Assisted by a health worker



(e) Assisted by a doctor



(f) Assisted by a nurse



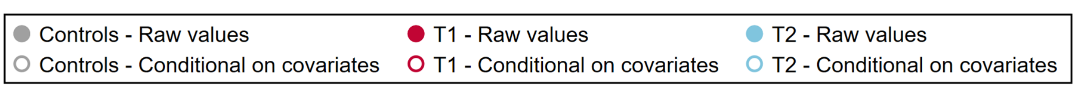
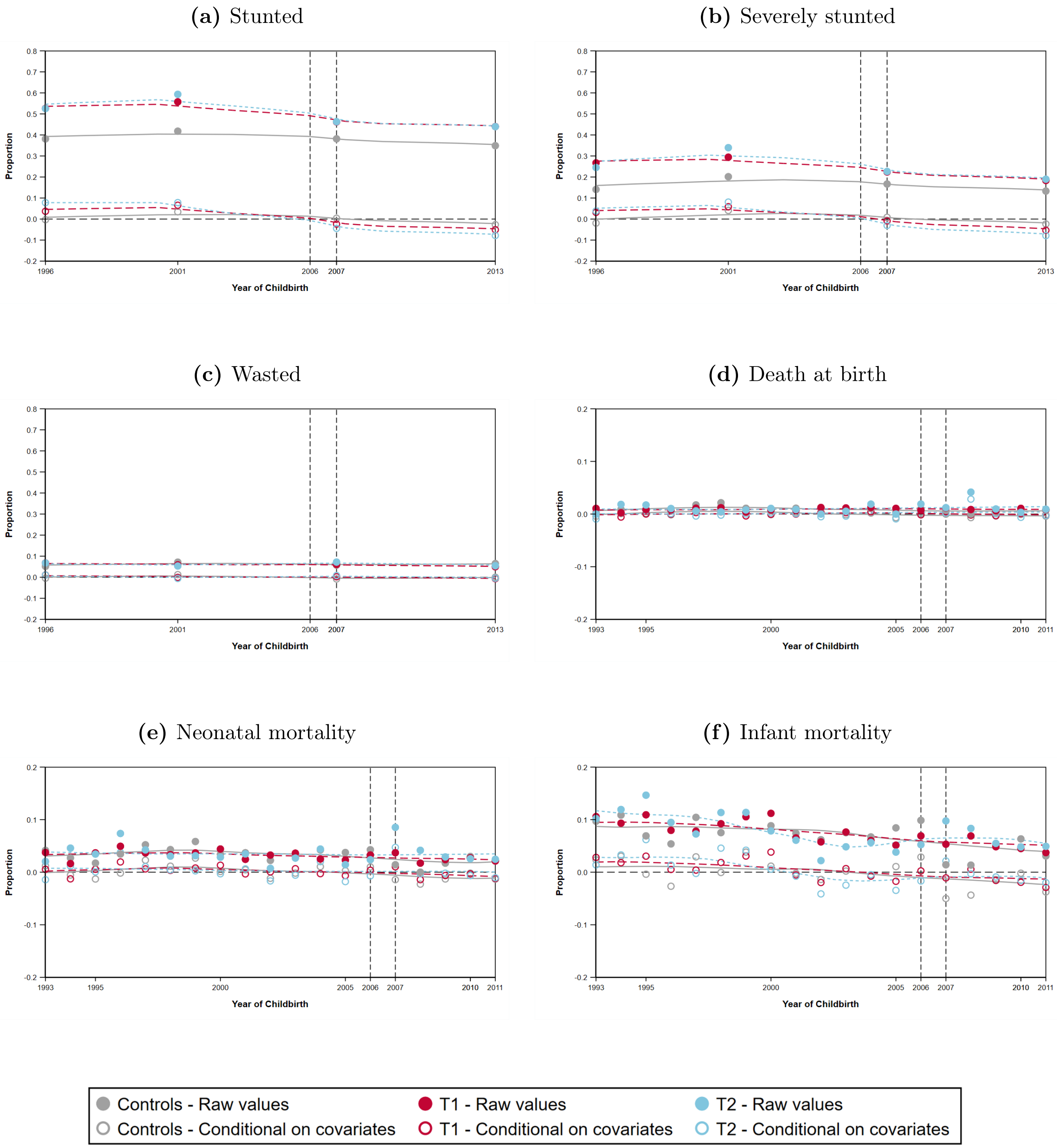


Figure D1 (*continued*). Trends in condition of childbirth and child health outcomes



Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: The figures plot the raw trends of each outcome for affected (T1 for rural districts, T2 for rural areas of urban districts) and unaffected areas (controls). It also reports the residual trends after controlling for all covariates and area fixed effects, as in the main specification. The vertical dashed lines indicate the starting date of the policy in rural districts (April 2006) and in rural areas of urban districts (July 2007). For anthropometric indicators, the vertical dashed line indicates the survey year from which children measured in affected areas were exposed to the policy.

D.2. Event-study specification

To investigate the timing of policy's effects, I use an event-study design where I include leads and lags of exposure to the policy, allowing for year-specific effects of the removal on the outcomes of interest:

$$y_{imta} = \alpha + \sum_{\tau=-L}^{-2} \beta_\tau \mathbb{1}_{t=\tau} \times Removed_a + \sum_{\tau=0}^{K} \gamma_\tau \mathbb{1}_{t=\tau} \times Removed_a + X'_{imta}\Gamma + \delta_a + \delta_t + \varepsilon_{imta}$$

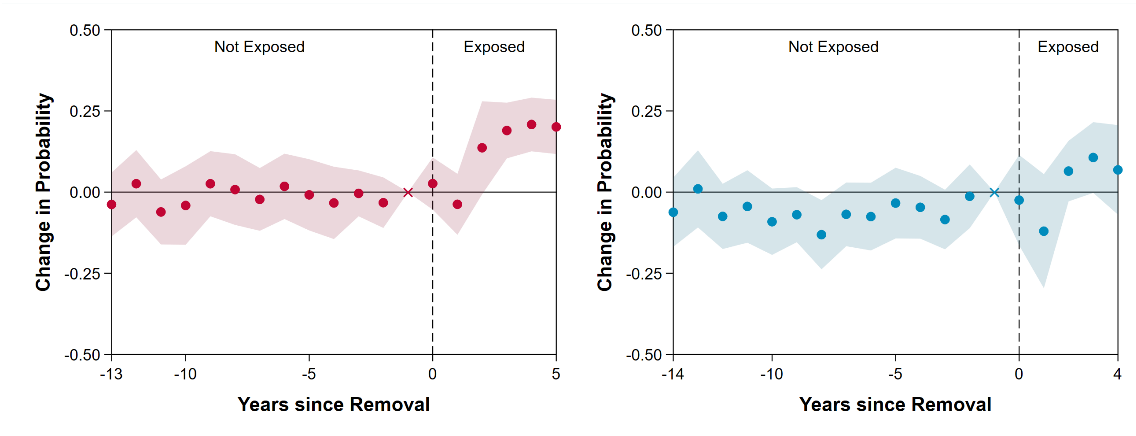
where L and K are the total number of pre-removal and post-removal periods, respectively. $Removed_a$ is an indicator variable taking the value of one if user fees were removed in area a, zero otherwise. Since user fees were removed in different years in rural districts and rural areas of urban districts, I normalise the year of policy implementation to 0. Hence, $\tau \geqslant 0$ denotes post-treatment periods and $\tau \leqslant -1$ pre-treatment periods. As usual in this kind of specification, the omitted one is the last pre-treatment period, that is $\tau = -1$. γ_τ now indicates the policy's effect τ years after its implementation, while β_τ corresponds to the policy's effect τ years before its implementation, relative to the last pre-treatment period.

This event-study design allows one to assess the effect of user fee removal over time by looking at γ_τ, and to formally test the parallel pre-trends between treatment groups. If trends are parallel before the removal, then the coefficients β_τ should not be significantly different from zero.

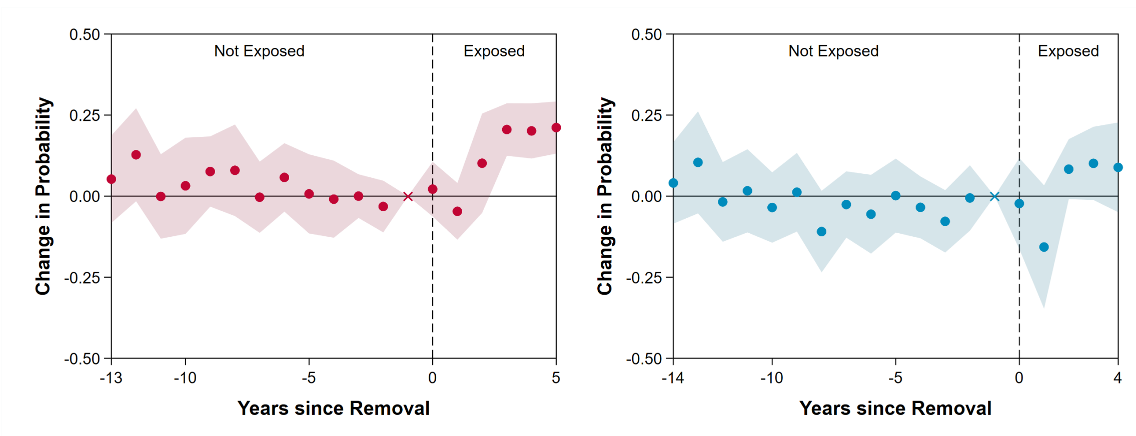
Figures D2 to D5 plot the corresponding point estimates and 95 percent confidence intervals for the outcomes of interest for rural districts and rural areas of urban districts separately. It provides strong evidence of absence of differential trends between affected and unaffected areas prior to the removal, which supports the identifying assumption and the causal interpretation of my results.

Figure D2. Event study estimates of the effect of user fee removal on place of delivery for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side)

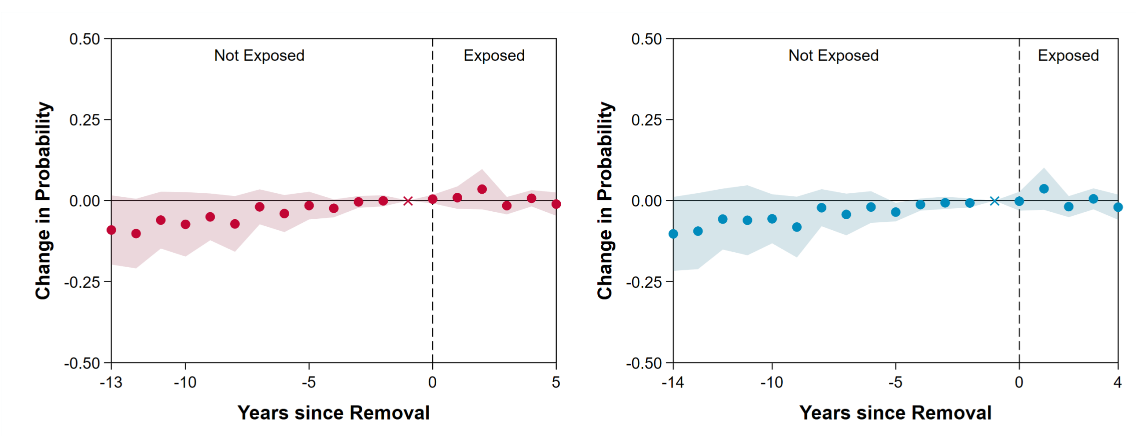
(a) Institutional delivery



(b) Public health facility delivery



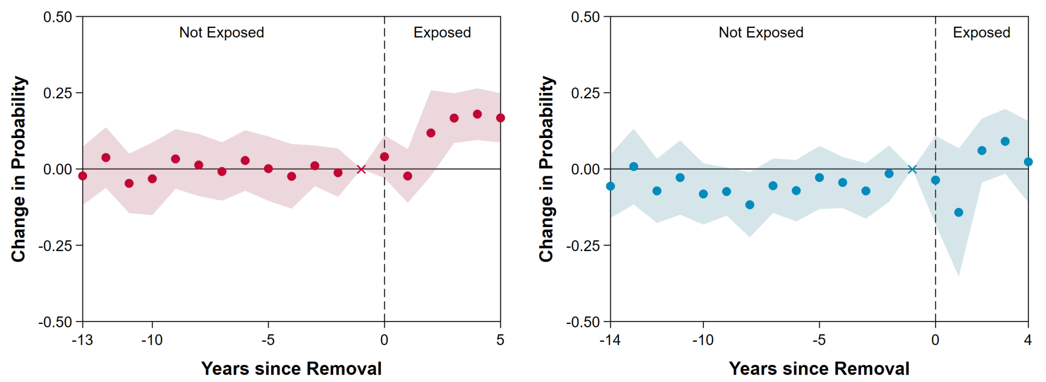
(c) Private health facility delivery



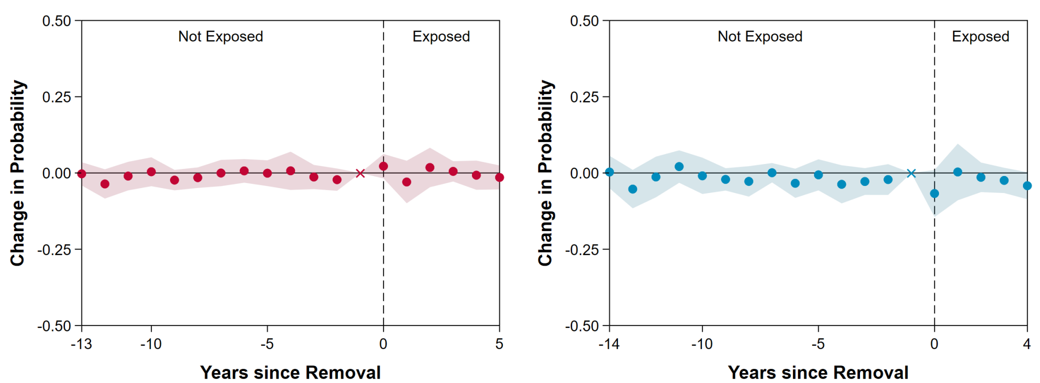
Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: These figures show the coefficients for interaction terms between time dummies and treatment status obtained from an event-study specification for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side). Year of implementation is normalized to zero. In addition to area and year of childbirth fixed effects, the covariates include mother's year of birth and a dummy for multiple pregnancy. Shaded areas represent 95% confidence intervals with robust standard errors clustered at the area level. The omitted category is the last pre-treatment time dummy. Outcomes of interest are a dummy equals to one if mother gave birth (a) in a health facility, (b) in a public or mission health facility, and (c) in a private health facility, zero otherwise.

Figure D3. Event study estimates of the effect of user fee removal on assistance received during childbirth for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side)

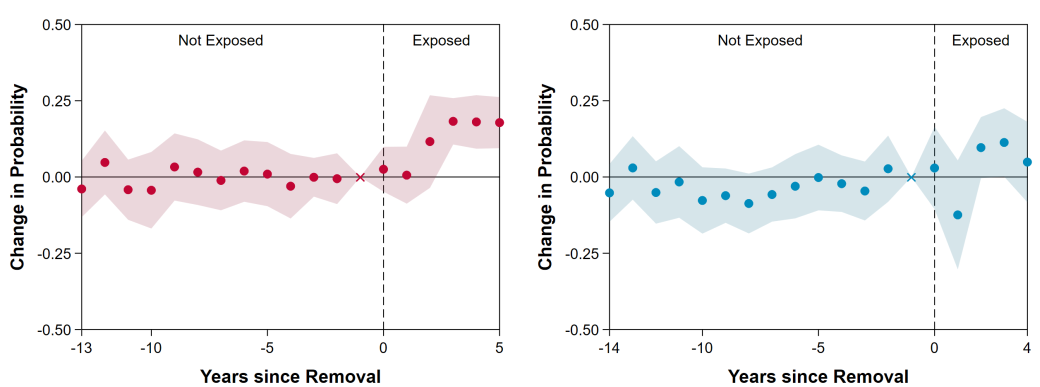
(a) Assisted by a health worker



(b) Assisted by a doctor



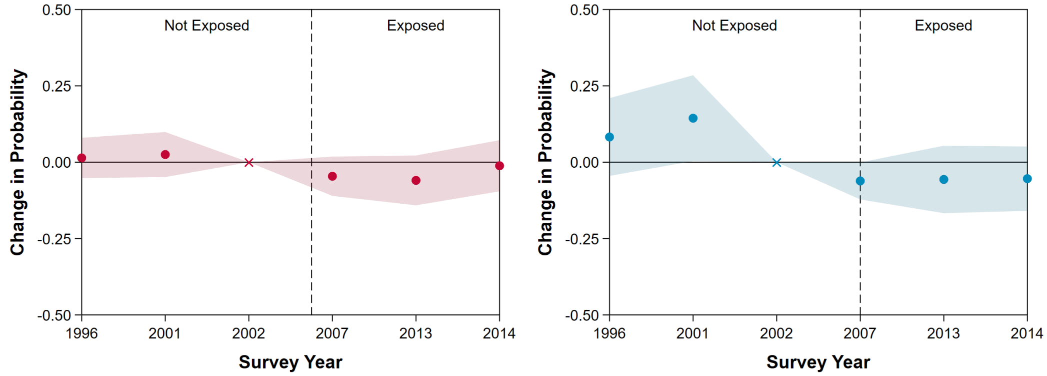
(c) Assisted by a nurse or a midwife



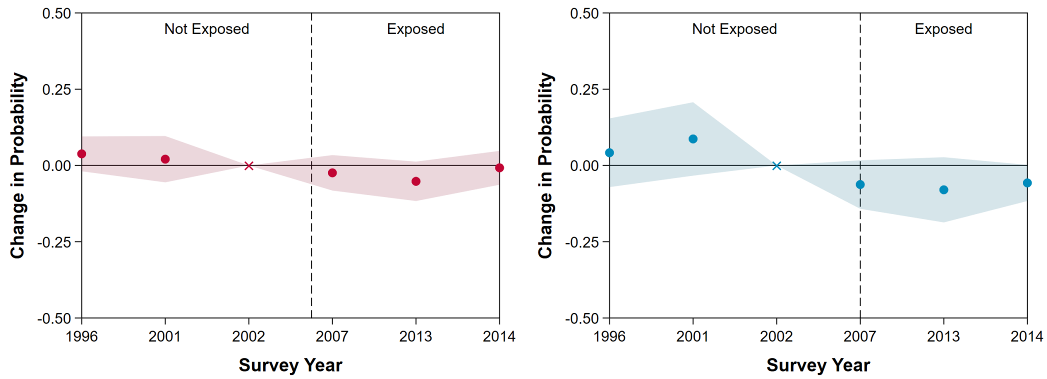
Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: These figures show the coefficients for interaction terms between time dummies and treatment status obtained from an event-study specification for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side). Year of implementation is normalized to zero. In addition to area and year of childbirth fixed effects, the covariates include mother's year of birth and a dummy for multiple pregnancy. Shaded areas represent 95% confidence intervals with robust standard errors clustered at the area level. The omitted category is the last pre-treatment time dummy. Outcomes of interest are a dummy equals to one if mother gave birth in presence of (a) a health worker, (b) a doctor, and (c) a nurse or a midwife, zero otherwise.

Figure D4. Event study estimates of the effect of user fee removal on nutritional status for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side)

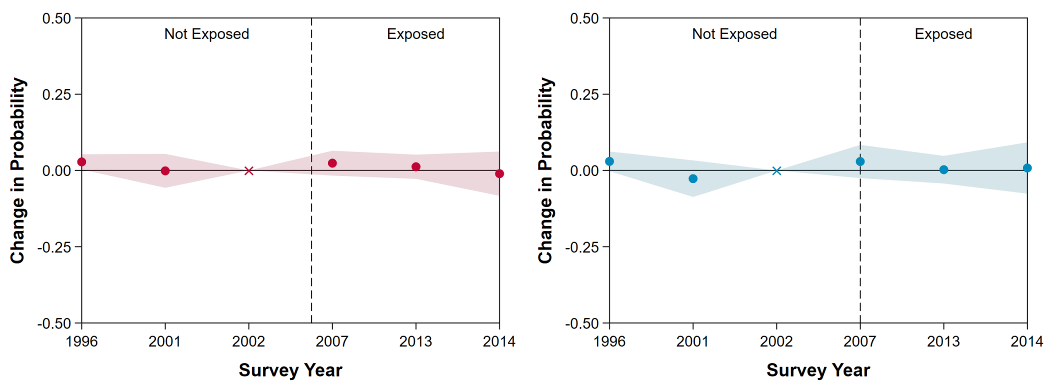
(a) Stunted



(b) Severely stunted



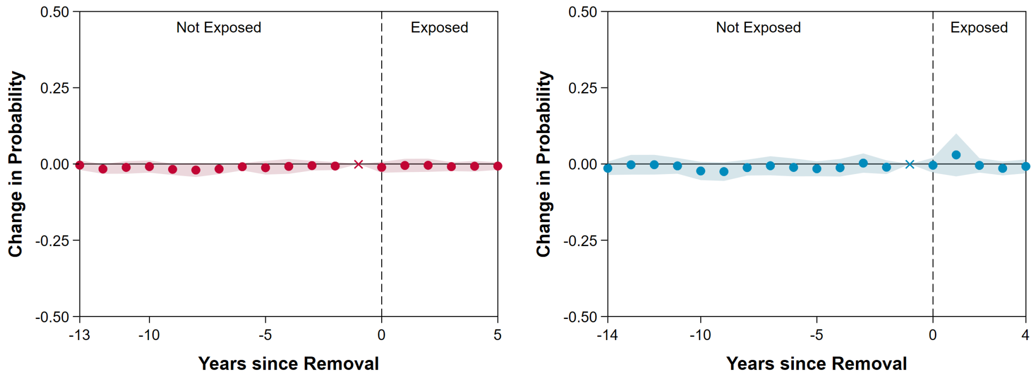
(c) Wasted



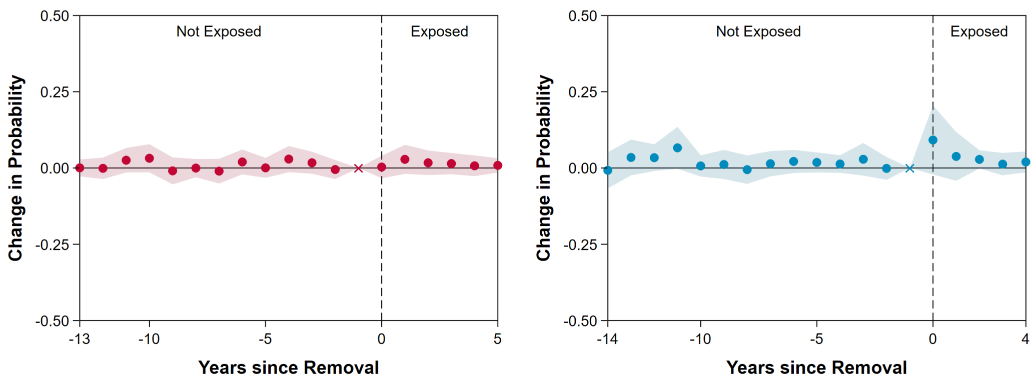
Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: These figures show the coefficients for interaction terms between time dummies and treatment status obtained from an event-study specification for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side). In addition to area and survey year fixed effects, the covariates include a dummy for multiple pregnancy, child's age dummies and child's sex. Shaded areas represent 95% confidence intervals with robust standard errors clustered at the area level. The omitted category is the last pre-treatment time dummy (the survey year 2002). Outcomes of interest are a dummy equals to one if child is (a) stunted (height for age z-score$<$-2), (b) severely stunted (height for age z-score$<$-3), and (c) wasted (weight for height z-score$<$-2), zero otherwise.

Figure D5. Event study estimates of the effect of user fee removal on child mortality for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side)

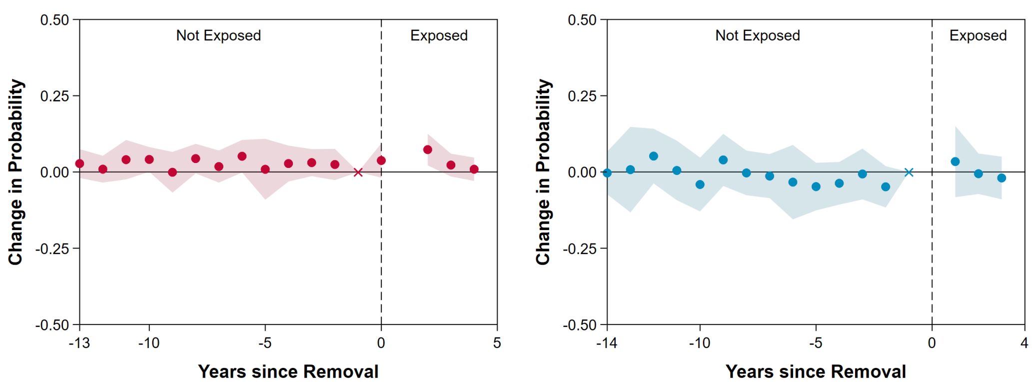
(a) Death at birth



(b) Neonatal mortality



(c) Infant mortality



Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: These figures show the coefficients for interaction terms between time dummies and treatment status obtained from an event-study specification for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side). Year of implementation is normalized to zero. In addition to area and year of childbirth fixed effects, the covariates include mother's year of birth, a dummy for multiple pregnancy and child's sex. Shaded areas represent 95% confidence intervals with robust standard errors clustered at the area level. The omitted category is the last pre-treatment time dummy. Outcomes of interest are a dummy equals to one if child died (a) at birth, (b) within her first 28 days of life, and (c) before reaching the age of one year, zero otherwise. For neonatal and infant mortality, children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group. Hence, it is not possible to assess the effect on infant mortality for children born in 2007 and 2011.

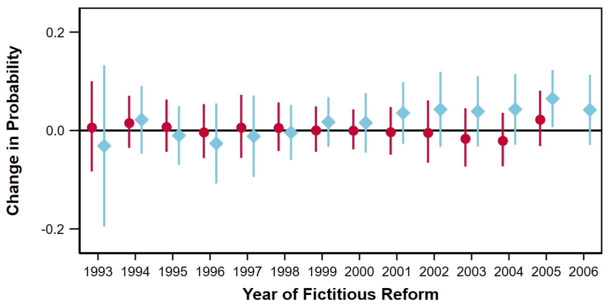
D.3. Placebo tests

I conduct a broad set of placebo tests to check for the existence of differential trends between affected and unaffected areas before the removal by comparing unexposed children from affected and unaffected areas. Children born after the beginning of the policy are dropped. I create fictitious removal policies for each year prior to the real implementation date. Affected and unaffected areas remain the same. For each fictitious policy, I estimate a difference-in-differences regression. The independent variable of interest is an interaction of a new indicator variable for post-fictitious removal observations and an indicator taking the value of one if user fees were removed in area a, zero otherwise.

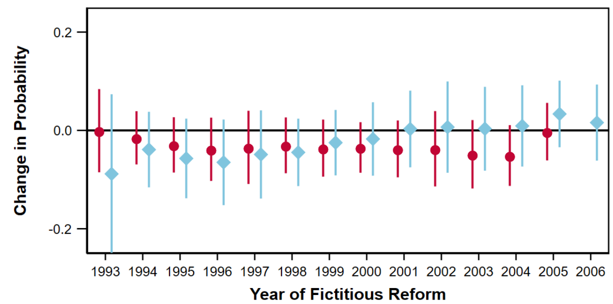
One should observe no effect of these fictitious policies on the different outcomes of interest in absence of differences in trends before the reform. Appendix Figure D6 plots the point estimates and 95 percent confidence intervals from these regressions. Only 9 point estimates out of 255 are marginally significant at the 5% level, which strongly support the identifying assumption.

Figure D6. Placebo tests

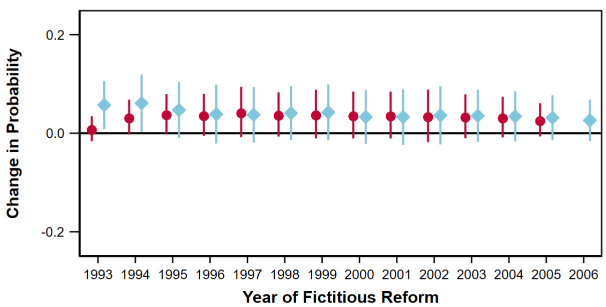
(a) Institutional delivery



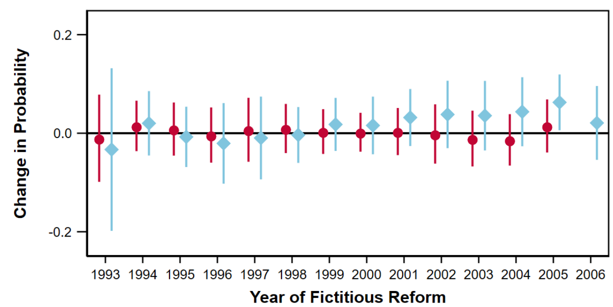
(b) Delivery in a public facility



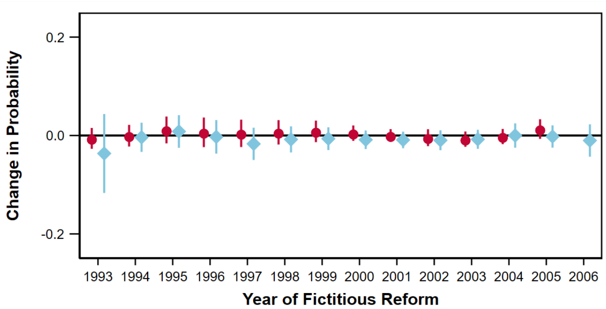
(c) Delivery in a private facility



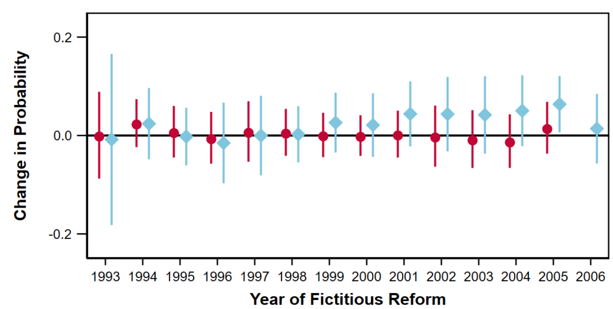
(d) Assisted by a health worker



(e) Assisted by a doctor



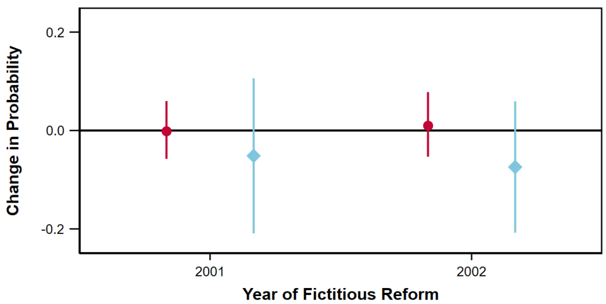
(f) Assisted by a nurse



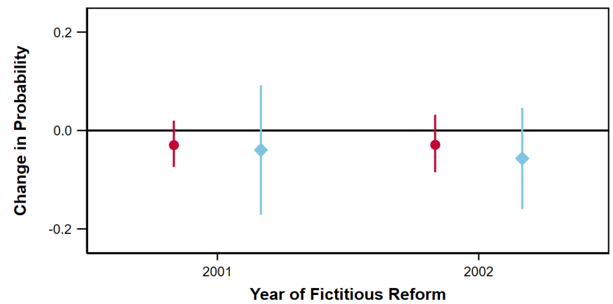
Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Each point corresponds to a separate difference-in-differences estimation for fictitious years of implementation of the policy in rural districts (red dots) and rural areas of urban districts (blue diamond). The sample is restricted to non-exposed children: childbirths and anthropometric measurements occurring after the real removal of user fees are dropped. Control variables include area and time fixed effects, as well as mother's year of birth and a dummy for multiple births. The lines represent 95% confidence intervals with robust standard errors clustered at the area level.

Figure D6. Placebo tests

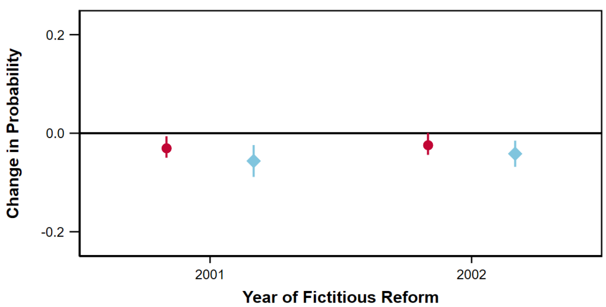
(a) Stunted



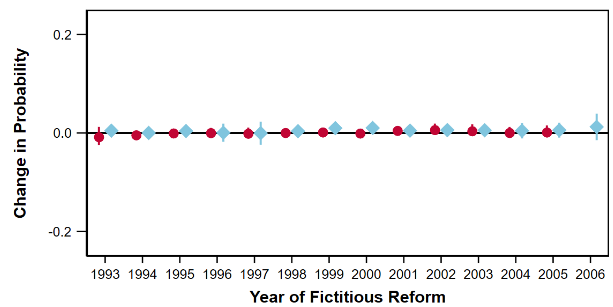
(b) Severely stunted



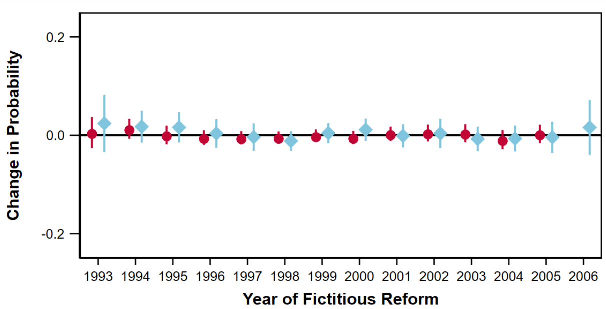
(c) Wasted



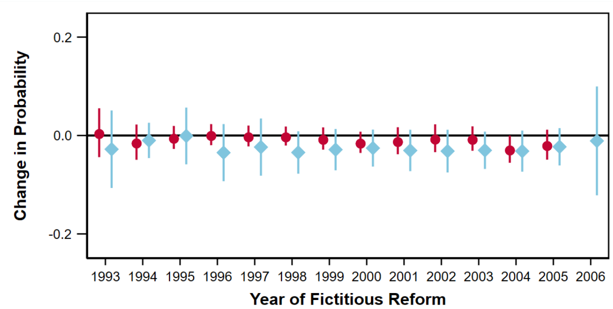
(d) Death at birth



(e) Neonatal mortality



(f) Infant mortality



Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Each point corresponds to a separate difference-in-differences estimation for fictitious years of implementation of the policy in rural districts (red dots) and rural areas of urban districts (blue diamond). The sample is restricted to non-exposed children: childbirths and anthropometric measurements occurring after the real removal of user fees are dropped. Control variables include area and time fixed effects, child's sex and a dummy for multiple births. Additional controls include mother's year of birth for mortality outcomes, and child's age dummies for anthropometric outcomes. The lines represent 95% confidence intervals with robust standard errors clustered at the area level.

Appendix E. Postnatal check-ups and child vaccination

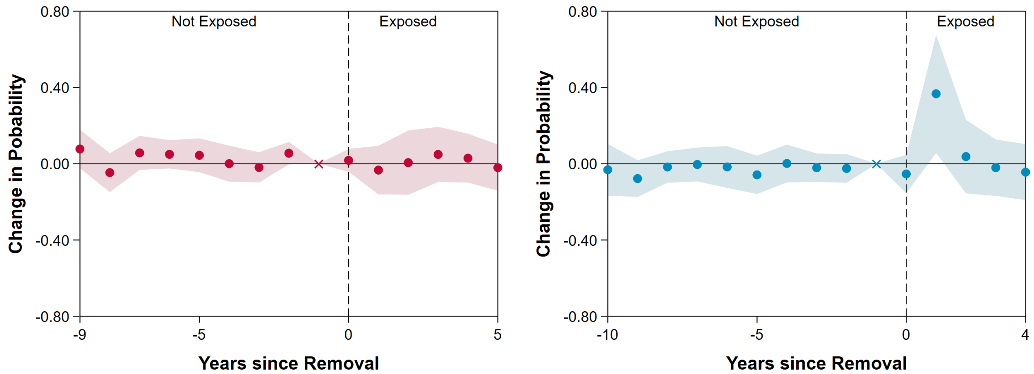
Table E1. The effect of user fee removal on postnatal check-up

	(1)	(2)	(3)	(4)
	Mother's health was checked			
	immediately	within one hour	within one day	Child up-to-date vaccinations
	after delivery			
Panel A. *Average effect of user fee removal*				
Affected by the policy	−0.011	−0.013	0.061*	−0.009
	(0.028)	(0.029)	(0.035)	(0.017)
Mean before policy	0.087	0.137	0.271	0.481
R^2	0.076	0.106	0.180	0.311
N	12,346	12,346	12,346	23,075
Panel B. *Average effect of user fee removal using de Chaisemartin and D'Haultfœuille [2021] estimator*				
Affected by the policy	0.029	0.009	0.101*	0.008
	(0.035)	(0.044)	(0.059)	(0.031)
Mean before policy	0.087	0.137	0.271	0.481
N	12,346	12,346	12,346	23,075
Panel C. *Effect in rural districts*				
Affected from 2006	−0.014	−0.003	0.078*	−0.002
	(0.031)	(0.032)	(0.039)	(0.018)
Mean before policy	0.098	0.143	0.284	0.485
R^2	0.072	0.106	0.176	0.308
N	10,573	10,573	10,573	19,910
Panel D. *Effect in rural parts of urban districts*				
Affected from 2007	0.008	−0.044	0.080	−0.059
	(0.059)	(0.061)	(0.070)	(0.045)
Mean before policy	0.057	0.120	0.231	0.471
R^2	0.092	0.113	0.216	0.349
N	4,922	4,922	4,922	8,500

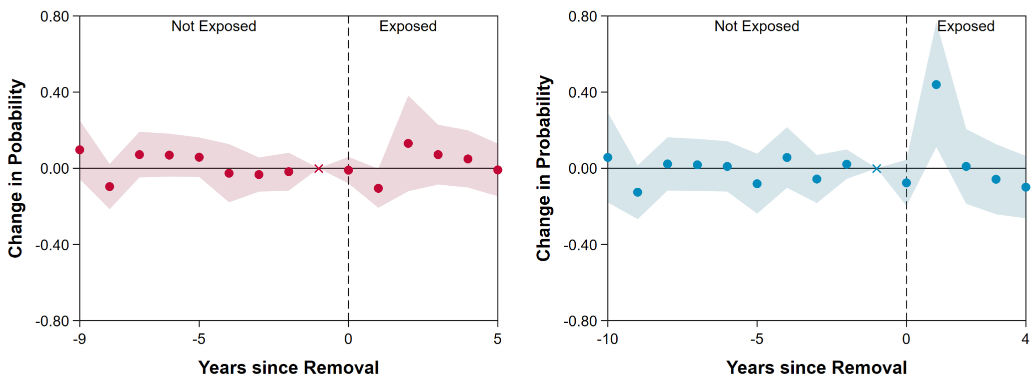
Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a childbirth. The table reports the average (Panels A and B) and phase-specific effect (Panels C and D) of user fee removal on postnatal care received by mothers immediately (column 1), within one hour (column 2) or within one day (column 3) after delivery, and the probability that child's vaccination is up to date at survey time (column 4). Information about the timing of postnatal care is only available for the last birth of each mother, and is not present in DHS 1996. Each coefficient is from a different regression. All regressions control for area and time fixed effects, and a dummy for multiple births. Columns 1 to 3 also control for mother's year of birth, and column 4 for child's sex and age dummies. Time fixed effects correspond to years of childbirth in columns 1 to 3, and to survey years in column 4. *$p<.10$; **$p<.05$; ***$p<.01$

Figure E2. Event study estimates of the effect of user fee removal on postnatal check-up for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side)

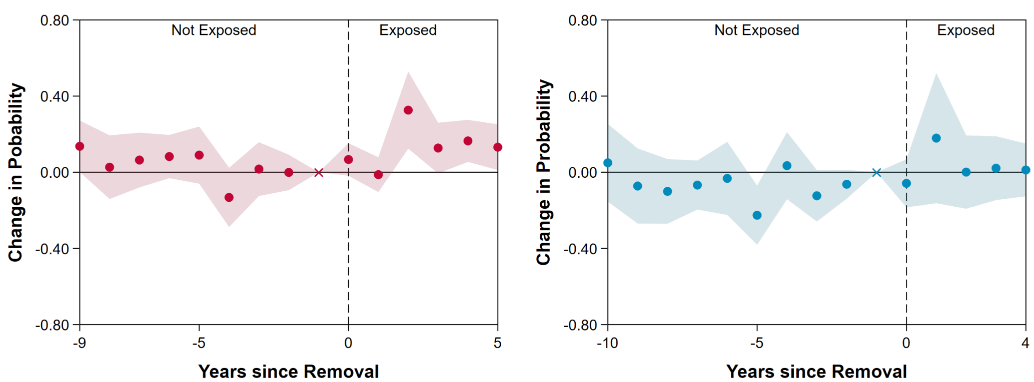
(a) Immediately after delivery



(b) Within one hour after delivery

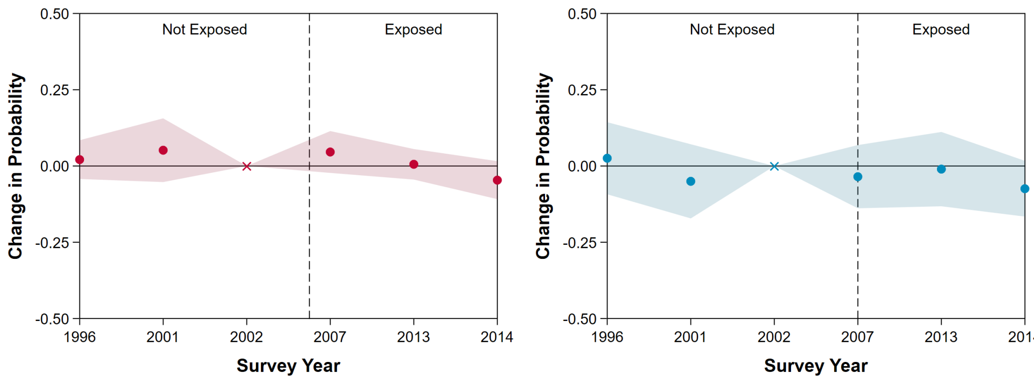


(c) Within one day after delivery



Source: Author's calculations from DHS 2001, 2007 and 2013.
Notes: These figures show the coefficients for interaction terms between time dummies and treatment status obtained from an event-study specification for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side). Year of implementation is normalized to zero. In addition to area and year of childbirth fixed effects, the covariates include mother's year of birth and a dummy for multiple pregnancy. Shaded areas represent 95% confidence intervals with robust standard errors clustered at the area level. The omitted category is the last pre-treatment time dummy. Outcomes of interest are a dummy equals to one if mother's health was checked (a) immediately, (b) within one hour, and (c) within one day after delivery, zero otherwise. This information is not available in DHS 1996 and is only reported for the last birth of each mother.

Figure E3. Event study estimates of the effect of user fee removal on the probability that child's vaccinations are up to date at survey time for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side)

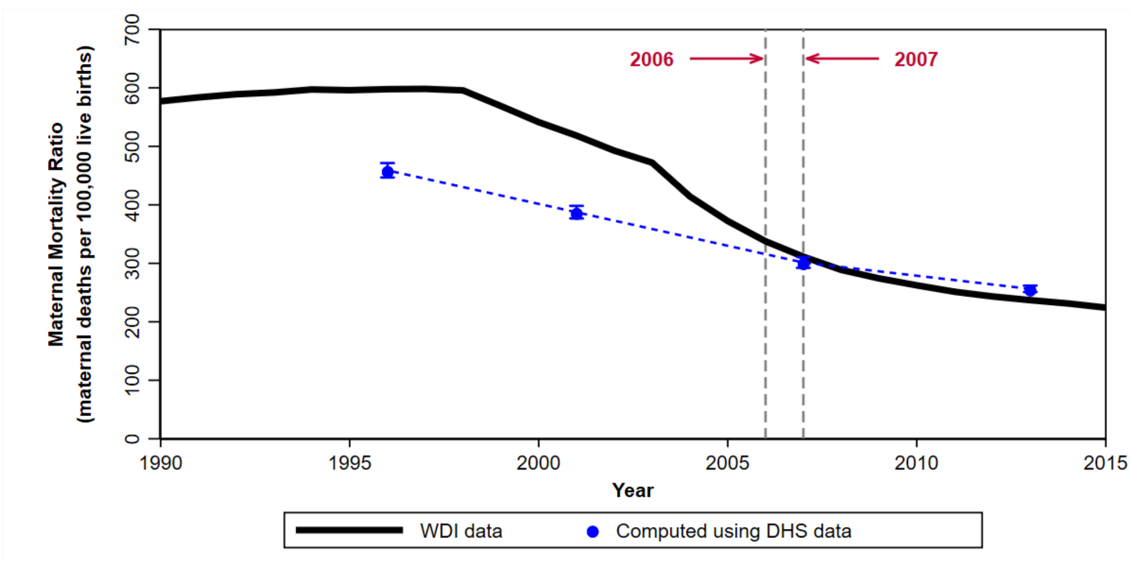


Source: Author's calculations from DHS 2001, 2007 and 2013.
Notes: These figures show the coefficients for interaction terms between time dummies and treatment status obtained from an event-study specification for areas affected from April 2006 (left-hand side) and those affected from July 2007 (right-hand side). In addition to area and survey year fixed effects, the covariates include a dummy for multiple pregnancy, as well as child's sex and age dummies. Shaded areas represent 95% confidence intervals with robust standard errors clustered at the area level. The omitted category is the last pre-treatment time dummy. The outcome of interest is a dummy equals to one if child's vaccinations are up to date for polio, measles, Diphtheria-Pertussis-Tetanus (DPT) and the Bacillus Calmette–Guérin (BCG) vaccine against tuberculosis, depending on child's age and the immunization schedule, and zero otherwise.

Appendix F. Evolution of the aggregate maternal mortality ratio

Due to data limitations, it is not possible to study the effect of the policy on maternal mortality. The Demographic and Health Surveys collect information on maternal mortality but only for the siblings of surveyed women. However, it does not gather information on where the women's siblings lived, making the assignment to treatment impossible. Instead, Appendix Figure F1 plots the estimated national maternal mortality ratio from the World Development Indicators over the 1990-2015 period. It also reports the national maternal mortality ratio computed with the DHS data.

Figure F1. Evolution of Maternal Mortality Ratio in Zambia since 1990



Source: World Development Indicators (WDI) and author's calculations from DHS 1996, 2001, 2007 and 2013

Notes: The figure shows the maternal mortality ratio estimates from the World Development Indicators and the raw values obtained from the DHS.

Appendix G. Compositional changes, selection effects and fertility

G.1. Selection into pregnancy and compositional changes in mothers giving birth

Table G1. The average effect of user fee removal on mothers' characteristics for different samples of births

	(1) Years of education	(2) At least one child born before first cohabitation	(3) Never in union	(4) Age at childbirth	(5) Already experienced infant death	(6) Polygamous	(7) Wealth Index
Panel A. *Average effect of user fee removal on characteristics of women giving birth*							
Affected by the policy	−0.227*	−0.001	0.000	0.265	−0.010	0.005	0.030
	(0.130)	(0.006)	(0.010)	(0.228)	(0.014)	(0.011)	(0.065)
Mean before policy implementation	4.538	0.028	0.053	26.275	0.304	0.180	−0.397
R^2	0.181	0.022	0.047	0.012	0.033	0.067	0.529
N	25,653	24,037	25,677	25,678	22,279	21,154	25,678
Panel B. *Average effect of user fee removal on characteristics of women giving birth in a publicly-supported health facility*							
Affected by the policy	−0.529***	0.003	0.001	−0.167	0.005	0.011	−0.015
	(0.173)	(0.009)	(0.014)	(0.245)	(0.014)	(0.012)	(0.065)
Mean before policy implementation	5.797	0.038	0.082	25.767	0.262	0.141	−0.147
R^2	0.134	0.024	0.046	0.016	0.032	0.065	0.467
N	12,512	11,405	12,527	12,527	10,084	9,925	12,527
Panel C. *Average effect of user fee removal on characteristics of women giving birth at home*							
Affected by the policy	−0.089	−0.002	0.001	1.034***	0.009	0.000	0.020
	(0.164)	(0.009)	(0.014)	(0.368)	(0.024)	(0.026)	(0.050)
Mean before policy implementation	3.941	0.024	0.039	26.505	0.322	0.197	−0.521
R^2	0.117	0.026	0.058	0.019	0.034	0.061	0.427
N	12,467	11,976	12,474	12,475	11,587	10,624	12,475

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.

Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a childbirth. The table reports the average effect of user fee removal on different maternal characteristics for the full sample of births (Panel A), births in a publicly-supported health facility (Panel B) and home births (Panel C). These characteristics are: the number of years of education of the mother, a dummy which equals one if she had at least one child born before cohabitation (zero otherwise), a dummy which equals one if she has never been in union (zero otherwise), her age at childbirth, a dummy which equals one if she already experienced an infant death before the removal of user fees (zero otherwise), a dummy which equals one if she belongs to a polygamous household (zero otherwise), and an index of material wealth computed by the DHS. Each coefficient is from a different regression. All regressions control for area and year of childbirth fixed effects. In column 5, the sample is restricted to children whose mother had already experienced a childbirth before policy implementation. In column 6, the sample is restricted to mothers in union or living with a man at survey time.

* $p<.10$; ** $p<.05$; *** $p<.01$

G.2. Selection into medical deliveries and heterogeneous treatment effects

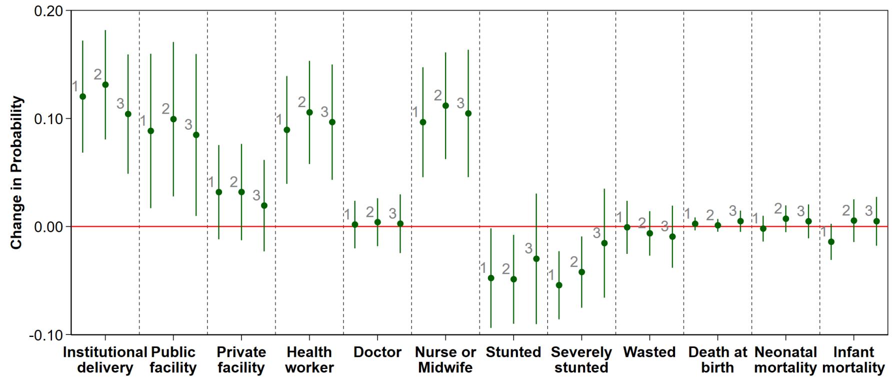
Table G2. Heterogeneous effects of the policy according to mother's education and past experience of infant deaths

	(1) Institutional delivery	(2) Public facility	(3) Private facility	(4) Health worker	(5) Doctor	(6) Nurse or midwife	(7) Stunted	(8) Severely stunted	(9) Wasted	(10) Death at birth	(11) Neonatal mortality	(12) Infant mortality
Panel A. *Heterogeneous effects according to mother's number of years of education*												
Affected by the policy	0.148***	0.107***	0.040*	0.116***	0.015	0.113***	−0.068**	−0.056***	−0.003	0.004	0.004	−0.012
	(0.030)	(0.035)	(0.024)	(0.028)	(0.014)	(0.027)	(0.026)	(0.019)	(0.013)	(0.003)	(0.006)	(0.010)
Years of education	0.037***	0.034***	0.002***	0.037***	0.006***	0.034***	−0.013***	−0.009***	−0.001*	0.000	0.000	−0.002**
	(0.003)	(0.003)	(0.001)	(0.003)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Affected by the policy × Years of education	0.000	0.002	−0.002**	0.001	−0.002	0.003	0.004*	0.002	0.000	0.000	0.000	0.002
	(0.004)	(0.004)	(0.001)	(0.004)	(0.001)	(0.004)	(0.002)	(0.002)	(0.001)	(0.000)	(0.001)	(0.001)
Mean before policy	0.323	0.318	0.004	0.318	0.013	0.299	0.545	0.286	0.063	0.009	0.033	0.086
R^2	0.277	0.250	0.246	0.272	0.053	0.255	0.091	0.070	0.022	0.017	0.029	0.036
N	25,462	25,462	25,462	25,556	25,556	25,556	21,092	21,092	21,050	25,653	25,240	19,156
Panel B. *Heterogeneous effects according to mother's past experience of infant deaths*												
Affected by the policy	0.113***	0.085**	0.028	0.090***	0.013	0.095***	−0.047**	−0.041**	−0.006	0.003	0.008	0.004
	(0.026)	(0.035)	(0.021)	(0.025)	(0.011)	(0.025)	(0.023)	(0.016)	(0.011)	(0.003)	(0.007)	(0.012)
Already experienced infant death	−0.040***	−0.030***	−0.010**	−0.039***	0.002	−0.036***	−0.005	−0.015*	−0.008	0.007***	0.018***	0.032***
	(0.010)	(0.010)	(0.004)	(0.010)	(0.003)	(0.010)	(0.013)	(0.009)	(0.005)	(0.002)	(0.005)	(0.007)
Affected by the policy × Already experienced infant death	0.002	−0.005	0.007**	−0.002	−0.004	−0.005	0.012	0.019	−0.004	0.004	−0.003	0.001
	(0.017)	(0.017)	(0.003)	(0.016)	(0.006)	(0.015)	(0.019)	(0.014)	(0.009)	(0.004)	(0.007)	(0.011)
Mean before policy	0.302	0.297	0.005	0.297	0.010	0.281	0.537	0.279	0.062	0.008	0.029	0.081
R^2	0.218	0.194	0.278	0.213	0.048	0.205	0.085	0.065	0.022	0.021	0.038	0.040
N	20,129	20,129	20,129	20,206	20,206	20,206	17,034	17,034	16,990	20,287	19,945	15,116

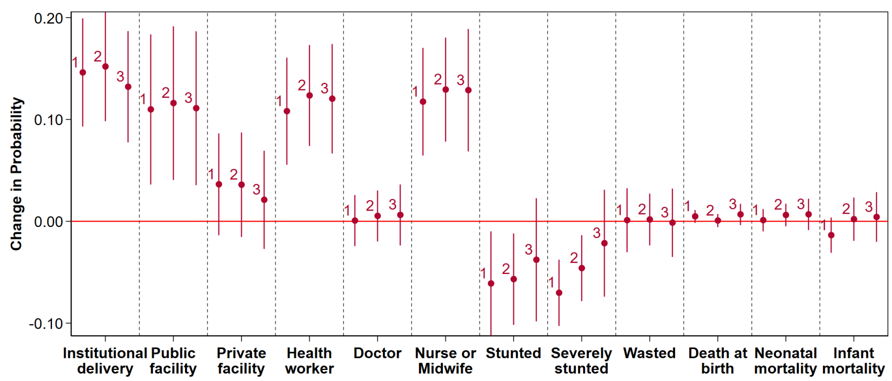
Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a childbirth (columns 1 to 6) or a child (columns 7 to 12). The table reports the heterogeneous effects of the policy according to mother's number of years of education (Panel A) and past experience of infant deaths (Panel B) on the different maternal health care utilization (columns 1 to 6) and child health (columns 7 to 12) outcomes. Each coefficient is from a different regression. All regressions control for area and time fixed effects, and a dummy for multiple births. Columns 1 to 6 also control for mother's year of birth, columns 7 to 9 for child's sex and age dummies, and columns 10 to 12 for mother's year of birth and child's sex. For neonatal and infant mortality, children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group.
*$p<.10$; **$p<.05$; ***$p<.01$

Figure G3. Heterogeneous effects of the policy according to tercile of material wealth

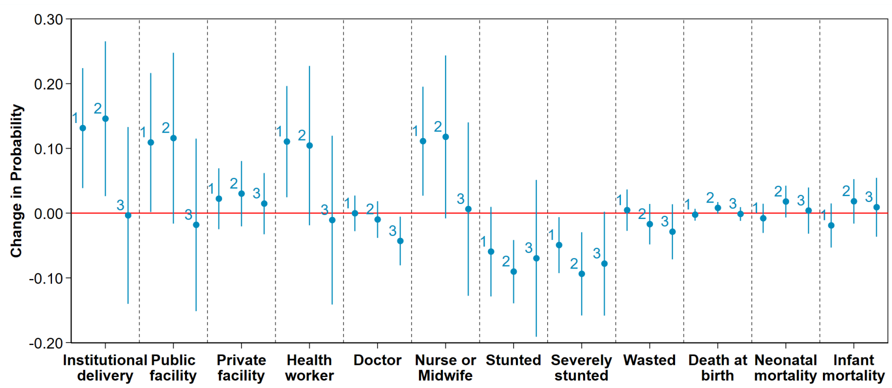
(a) Heterogeneous effects of user fee removal



(b) Heterogeneous effects in rural districts



(c) Heterogeneous effects in rural parts of urban districts



Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: The figures plot the total effect of the removal of user fees on the corresponding outcome for each tercile of material wealth specified. *1* corresponds to the poorest third of households, *2* to the middle third, and *3* to the richest third. All regressions control for area and year of childbirth fixed effects, as well as a dummy for multiple birth and terciles of material wealth. Regressions for delivery conditions and child mortality outcomes also control for mother's year of birth. Child's sex and age dummies are included as additional control variables when looking at child nutritional outcomes, and child's sex is controlled for when looking at child mortality outcomes. The lines represent 95% confidence intervals with robust standard errors clustered at the area level.

G.3. Selection into live birth

Table G4. The effect of user fee removal on stillbirths, miscarriages/abortions and live births composition

	(1) Stillbirth	(2) Miscarriage/Abortion	(3) Male birth
Panel A. *Average effect of user fee removal*			
Affected by the policy	0.001	−0.001	0.003
	(0.005)	(0.009)	(0.011)
Mean before policy	0.012	0.033	0.496
R^2	0.011	0.022	0.006
N	14,337	14,913	25,678
Panel B. *Average effect of user fee removal using de Chaisemartin and D'Haultfœuille [2021] estimator*			
Affected by the policy	0.009	−0.018	0.026
	(0.017)	(0.022)	(0.038)
Mean before policy	0.012	0.033	0.496
N	14,337	14,913	25,678
Panel C. *Effect in rural districts*			
Affected from 2006	0.004	−0.004	0.008
	(0.005)	(0.010)	(0.012)
Mean before policy	0.011	0.034	0.497
R^2	0.012	0.022	0.005
N	12,956	12,956	22,148
Panel D. *Effect in rural parts of urban districts*			
Affected from 2007	−0.004	0.011	0.007
	(0.006)	(0.013)	(0.016)
Mean before policy	0.012	0.031	0.490
R^2	0.020	0.035	0.012
N	5,411	5,411	9,479

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a pregnancy (columns 1 and 2) or a live birth (column 3). The table reports the average (Panels A and B) and phase-specific effect (Panels C and D) of user fee removal on the probability of stillbirth (column 1), of miscarriage or abortion (column 2) and on the sex ratio for live births (column 3). Due to data limitations, miscarriages and abortions cannot be distinguished. Each coefficient is from a different regression. All regressions control for area and year of childbirth fixed effects, as well as mother's year of birth. Column 3 also controls for a dummy for multiple births, which is not possible in columns 1 and 2. The sample used in columns 1 and 2 corresponds to all pregnancies that occurred during the last five years preceding the 2007 and 2013 survey waves, whatever their final outcome. This information is not available for the 1996 and 2001 survey waves.
$^{*}p<.10$; $^{**}p<.05$; $^{***}p<.01$

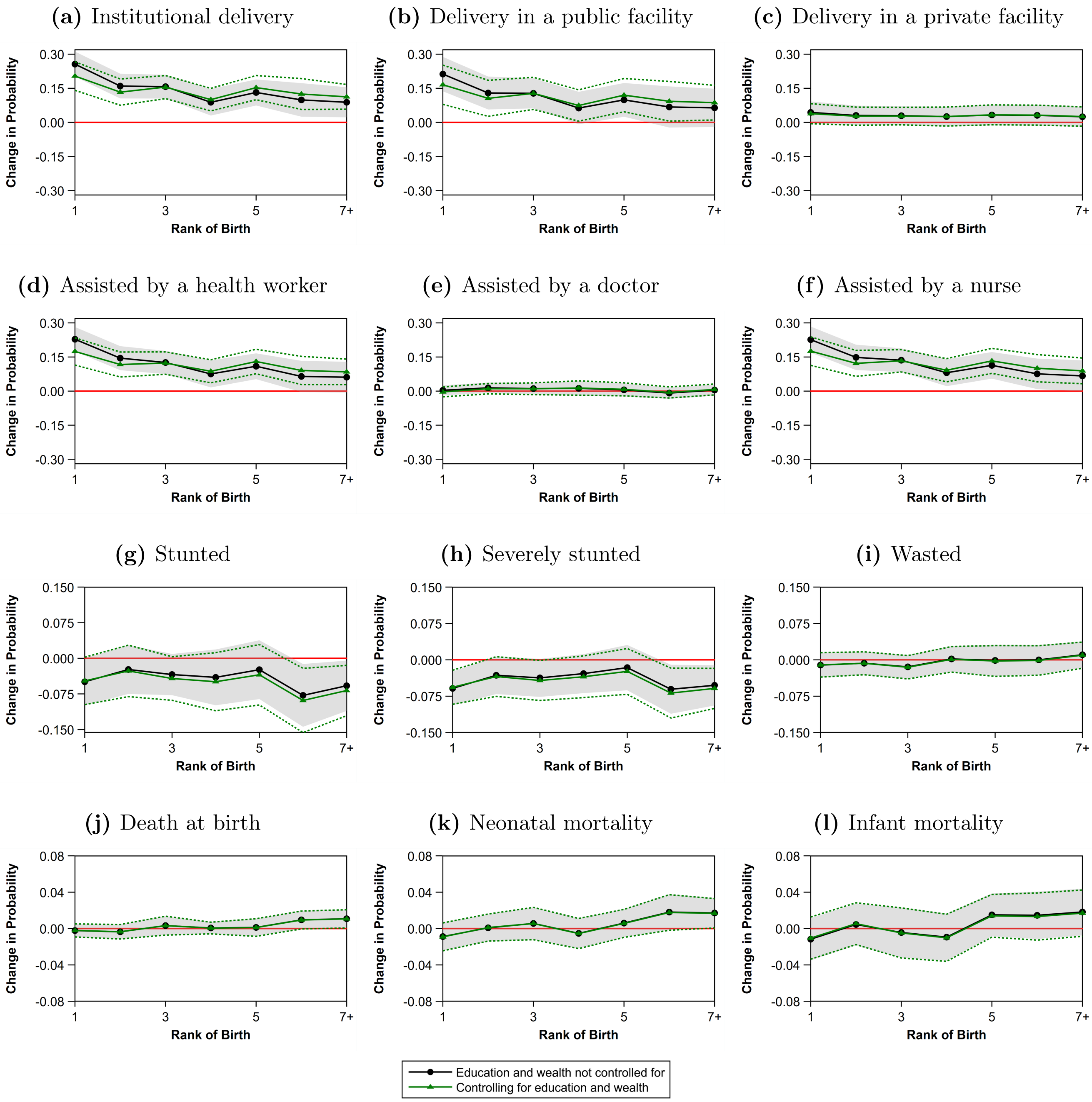
G.4. Fertility and heterogeneous effects according to rank of birth

Table G5. The effect of user fee removal when the sample is restricted to first born

	(1) Institutional delivery	(2) Public facility	(3) Private facility	(4) Health worker	(5) Doctor	(6) Nurse or midwife	(7) Stunted	(8) Severely stunted	(9) Wasted	(10) Death at birth	(11) Neonatal mortality	(12) Infant mortality
Panel A. *Average effect of user fee removal*												
Affected by the policy	0.240***	0.211***	0.029	0.214***	−0.006	0.220***	−0.045	−0.068**	−0.005	−0.006	−0.016	−0.029
	(0.034)	(0.041)	(0.021)	(0.034)	(0.019)	(0.037)	(0.039)	(0.031)	(0.018)	(0.006)	(0.011)	(0.017)
Mean before policy	0.411	0.407	0.004	0.405	0.027	0.375	0.581	0.316	0.066	0.012	0.048	0.109
R^2	0.256	0.235	0.165	0.244	0.079	0.221	0.108	0.095	0.045	0.040	0.039	0.065
N	5,356	5,356	5,356	5,374	5,374	5,374	4,072	4,072	4,075	5,391	5,320	4,057
Panel B. *Effect in rural districts*												
Affected from 2006	0.235***	0.203***	0.032	0.204***	−0.009	0.215***	−0.074**	−0.096***	0.009	−0.003	−0.016	−0.028
	(0.036)	(0.044)	(0.023)	(0.037)	(0.021)	(0.040)	(0.036)	(0.028)	(0.020)	(0.006)	(0.012)	(0.017)
Mean before policy	0.421	0.418	0.003	0.417	0.028	0.388	0.586	0.313	0.065	0.012	0.051	0.109
R^2	0.251	0.229	0.174	0.238	0.080	0.214	0.107	0.092	0.041	0.045	0.042	0.066
N	4,706	4,706	4,706	4,723	4,723	4,723	3,585	3,585	3,580	4,738	4,675	3,546
Panel C. *Effect in rural parts of urban districts*												
Affected from 2007	0.296***	0.275***	0.021	0.270***	0.007	0.252***	−0.067	−0.069	−0.020	−0.010	−0.004	−0.058**
	(0.068)	(0.075)	(0.022)	(0.065)	(0.026)	(0.066)	(0.073)	(0.052)	(0.023)	(0.008)	(0.017)	(0.027)
Mean before policy	0.371	0.363	0.008	0.357	0.025	0.326	0.564	0.327	0.071	0.010	0.035	0.106
R^2	0.311	0.275	0.203	0.310	0.106	0.267	0.100	0.084	0.051	0.068	0.048	0.067
N	2,258	2,258	2,258	2,262	2,262	2,262	1,713	1,713	1,708	2,268	2,240	1,699

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a childbirth (columns 1 to 6) or a child (columns 7 to 12). The sample is restricted to first born children. The table reports the average (Panel A) and phase-specific effect (Panels B and C) of user fee removal on the different maternal health care utilization (columns 1 to 6) and child health (columns 7 to 12) outcomes. Each coefficient is from a different regression. All regressions control for area and time fixed effects, and a dummy for multiple births. Columns 1 to 6 also control for mother's year of birth, columns 7 to 9 for child's sex and age dummies, and columns 10 to 12 for mother's year of birth and child's sex. For neonatal and infant mortality, children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group.
*$p<.10$; **$p<.05$; ***$p<.01$

Figure G6. Heterogeneous effects according to child's rank of birth



Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: The figures plot the point estimates from interaction terms between a dummy for exposure to user fee removal and a set of dummies for child's rank of birth. Each figure is from a separate estimation. Control variables include area and time fixed effects, and a dummy for multiple births. Additional controls include mother's year of birth for childbirth conditions, mother's year of birth and child's sex for mortality outcomes, and child's sex and age dummies for anthropometric outcomes. For neonatal and infant mortality, children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group. Green triangles represent the point estimates obtained with the same set of covariates, as well as mother education level and household material wealth. Shaded areas (resp. green dotted lines) represent 95% confidence intervals for point estimates obtained with the initial set of covariates (resp. point estimates obtained with the additional covariates) with robust standard errors clustered at the area level. Similar figures for both rural districts and rural areas or urban districts separately are available upon request.

Table G7. The effect of user fee removal on fertility behaviors

	(1) Preceding birth interval	(2) Preceding birth interval < 24 months	(3) Number of reported births per month per 1,000 surveyed mothers
Panel A. *Average effect of user fee removal*			
Affected by the policy	−2.602***	0.005	1.437
	(0.701)	(0.013)	(2.085)
Mean before policy	36.313	0.169	30.025
R^2	0.098	0.033	0.354
N	20,286	20,286	9,907
Panel B. *Effect in rural districts*			
Affected from 2006	−2.728***	0.008	2.825
	(0.728)	(0.013)	(1.904)
Mean before policy	36.334	0.170	28.825
R^2	0.099	0.035	0.286
N	17,409	17,409	8,423
Panel C. *Effect in rural parts of urban districts*			
Affected from 2007	−4.779***	0.015	0.817
	(1.173)	(0.020)	(5.574)
Mean before policy	36.233	0.166	36.707
R^2	0.127	0.043	0.435
N	7,211	7,211	3,703

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a birth (columns 1 and 2) or an area × birth date × survey year cell (column 3). The table reports the average (Panel A) and phase-specific effect (Panels B and C) of user fee removal on preceding birth interval in months (column 1), a dummy for preceding birth interval being less than 24 months (column 2) and number of reported births per month per 1,000 mothers surveyed (column 3). All regressions control for area and year of childbirth fixed effects. Columns 1 and 2 also control for a dummy for multiple births and column 3 for birth month fixed effects.
*$p<.10$; **$p<.05$; ***$p<.01$

Appendix H. Sensitivity analysis

H.1. Correction for selective mortality

Table H1. The effect of user fee removal on anthropometric indicators when taking into account potential selective mortality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Child is alive at survey time	—— Stunted ——		— Severely stunted —		—— Wasted ——	
Panel A. *Average effect of user fee removal*							
Affected by the policy	−0.020	−0.044**	−0.045**	−0.044***	−0.041**	−0.005	−0.002
	(0.105)	(0.021)	(0.022)	(0.016)	(0.016)	(0.011)	(0.011)
Mean before policy	0.881	0.545	0.545	0.286	0.286	0.063	0.063
R^2	0.056	0.087	0.092	0.068	0.072	0.023	0.027
N	25,651	21,080	21,080	21,080	21,080	21,039	21,039
Panel B. *Effect in rural districts*							
Affected from 2006	−0.021	−0.055**	−0.059**	−0.054***	−0.053***	0.000	0.002
	(0.107)	(0.023)	(0.023)	(0.015)	(0.016)	(0.013)	(0.013)
Mean before policy	0.881	0.544	0.544	0.283	0.283	0.063	0.063
R^2	0.056	0.084	0.091	0.066	0.071	0.023	0.028
N	22,138	18,199	18,199	18,199	18,199	18,152	18,152
Panel C. *Effect in rural parts of urban districts*							
Affected from 2007	−0.096	−0.074***	−0.070***	−0.072***	−0.071***	−0.007	−0.005
	(0.197)	(0.022)	(0.023)	(0.021)	(0.021)	(0.013)	(0.013)
Mean before policy	0.886	0.549	0.549	0.295	0.295	0.063	0.063
R^2	0.058	0.090	0.099	0.067	0.076	0.022	0.036
N	9,429	7,661	7,661	7,661	7,661	7,636	7,636
Model	Logit	OLS	OLS	OLS	OLS	OLS	OLS
Inverse Probabilty Weighting	No	Yes	No	Yes	No	Yes	No
Semi-parametric approach	No	No	Yes	No	Yes	No	Yes

Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a child (column 1) or a surviving child (columns 2 to 7). The table reports the average (Panel A) and phase-specific effect (Panels B and C) of user fee removal on the probability of being alive at survey time (column 1), stunted (columns 2 and 3), severely stunted (columns 4 and 5) and wasted (columns 6 and 7). All regressions control for area and time fixed effects, a dummy for multiple births and a dummy for girls. Column 1 also controls for mother's year of birth, and columns 2 to 7 for child's age dummies. The inverse probability weighting method consists in weighting observations according to the predicted survival probabilities at survey time obtained from column 1. The semi-parametric approach follows Cosslett [1991] by including one indicator variable for each centile of predicted survival probabilities obtained from column 1 as additional control variables.
* $p<.10$; ** $p<.05$; *** $p<.01$

H.2. Recall bias and age heaping

There is two possible source of measurement error due to recall bias. First, mothers may under-report births and child deaths. I argue that recall bias can reasonably be considered low in this setting since a child's birth and death are milestones in a woman's life, and the recall period of five years is relatively short. Another reason is that the retrospective birth history questionnaire of the DHS is very precise and interviewers are asked to check the accuracy of reported births with respect to the rest of the survey. Second, mothers may have rounded up child's age at death, leading to a mismeasurement in child mortality outcomes. To check the sensitivity of my results to age-heaping, I use an expanded definition of neonatal and infant mortality: neonatal mortality is now defined as the probability to die within the first month of life instead of 28 days, and infant mortality now includes children who are reported to be dead at the age of one. Results barely changes with these new definitions (Appendix Table H2).

Table H2. The effect of user fee removal on child mortality when allowing for age-heaping

	(1) Died within first month of life	(2) Died within first year of life
Panel A. *Average effect of user fee removal*		
Affected by the policy	0.005	−0.005
	(0.005)	(0.009)
Mean before policy	0.039	0.096
R^2	0.032	0.036
N	25,265	19,173
Panel B. *Effect in rural districts*		
Affected from 2006	0.007	−0.005
	(0.005)	(0.009)
Mean before policy	0.039	0.096
R^2	0.033	0.037
N	21,785	16,486
Panel C. *Effect in rural parts of urban districts*		
Affected from 2007	0.007	−0.010
	(0.007)	(0.014)
Mean before policy	0.037	0.096
R^2	0.025	0.033
N	9,344	7,163

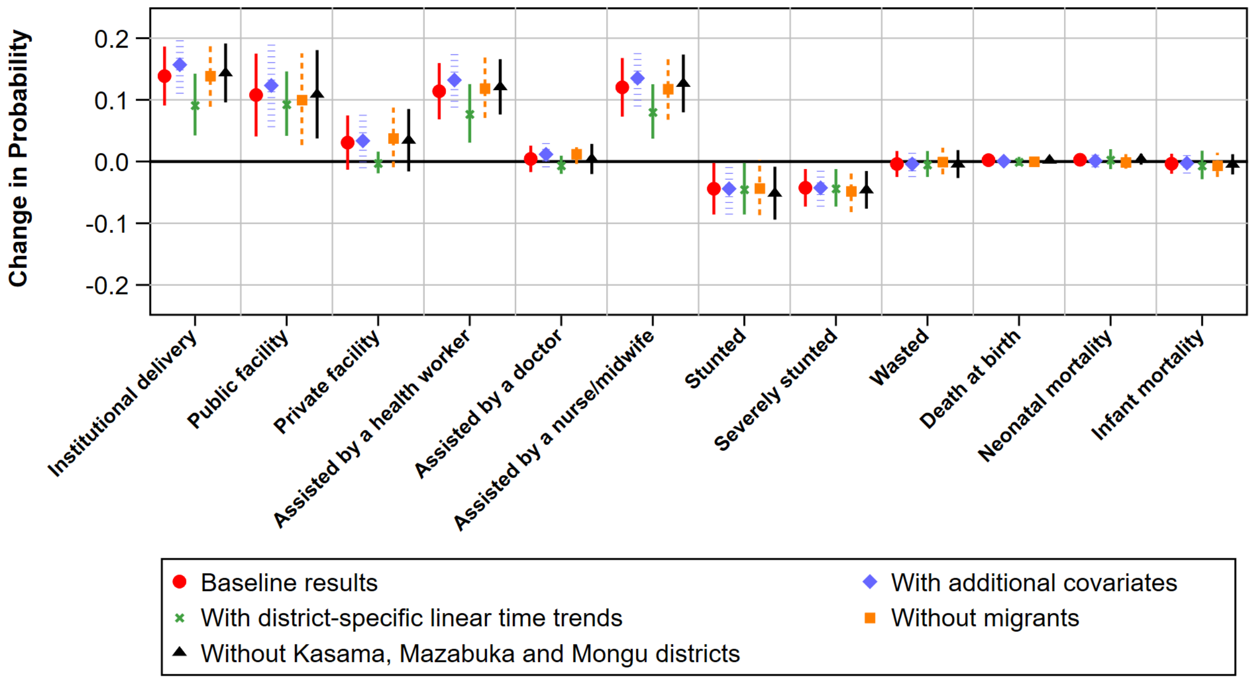
Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: Robust standard errors clustered at the area level reported in parentheses. The unit of observation is a child. The table reports the average (Panel A) and phase-specific effect (Panels B and C) of user fee removal on the probability for a child to die within her first month of life (column 1) and within her first year of life (column 2). Each coefficient is from a different regression. All regressions control for area and year of childbirth fixed effects, as well as mother's year of birth, a dummy for multiple births and child's sex. Children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group.
*$p<.10$; **$p<.05$; ***$p<.01$

H.3. Additional control variables and treatment assignment

Additional covariates - Results remain similar when I include a full set of maternal covariates as additional control variables. I also present results when controlling for district-specific linear time trends. While these trends may pick up at least part of the effect, it gives some insight about the robustness of the results. As expected, point estimates are smaller in magnitude but conclusions remain similar (Appendix Figure H3).

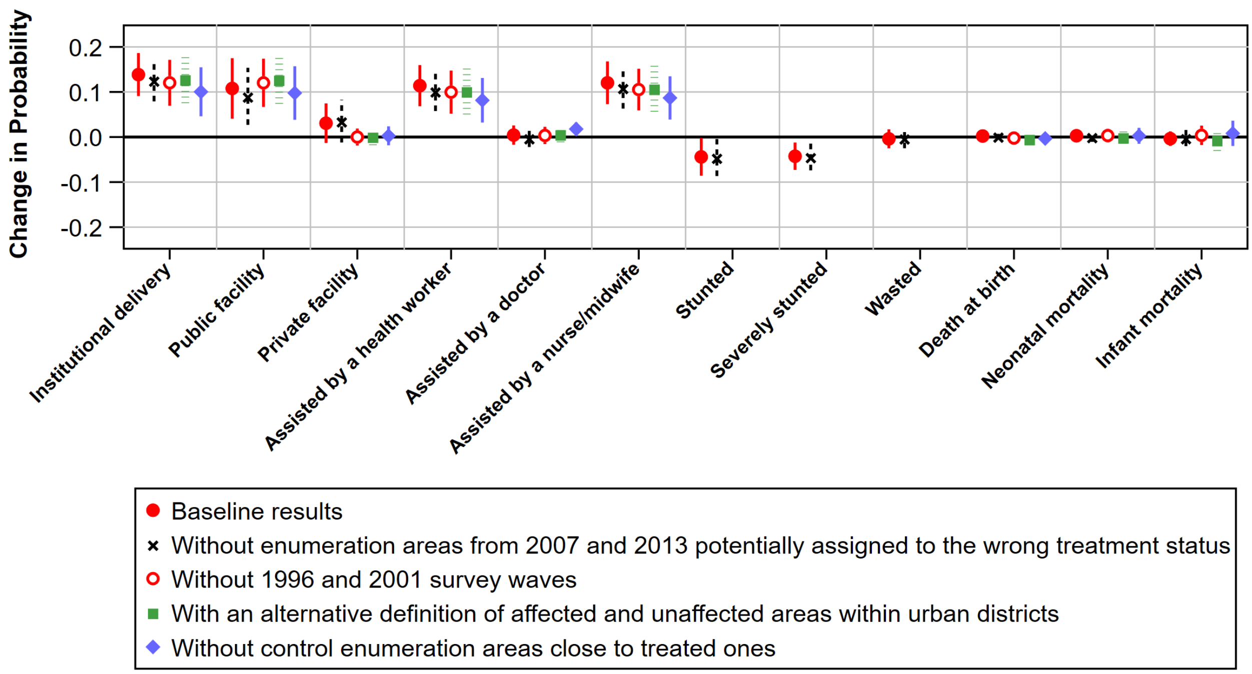
Sensitivity to treatment assignment - I now check the sensitivity of my results to a finer assignment to treatment within urban districts by applying the eligibility criteria provided by the government. This refinement is not possible for the 1996 and 2001 survey waves since GPS coordinates are not available. I now consider as affected from July, 1st 2007 those individuals whose nearest health facility is located more than 15 kilometers away from the administrative center of the district, and 20 kilometers away for districts located along a railway. For this, I use the GPS coordinates of all publicly-supported health facilities collected during the 2005 Health Facility Census. Results barely change when using this new definition of treatment areas (Appendix Figure H4). Due to the scrambling procedure applied by DHS on GPS coordinates for confidentiality reasons, households surveyed in 2007 and 2013 may have been assigned to the wrong treatment area (see Appendix C, section 5 for more details). This is a problem only for those enumeration areas located near the boundaries (2 kilometers or less for urban enumeration areas and 5 kilometers or less for rural enumeration areas) of a district which has not the same treatment status as the actual one. 98 enumerations areas are concerned. The results are not significantly different when I excluded the corresponding 1,298 births from the analysis (see Appendix Figure H4).

Figure H3. Robustness checks: alternative specifications and sample



Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: The figure shows the different point estimates obtained for each outcome from alternative specifications and when removing migrants from the analysis. Each point corresponds to a separate difference-in-differences estimation. The red dots plot the baseline results presented in Tables 2 and 3. Blue diamonds correspond to a specification with additional covariates, namely mother's year of birth by child's year of birth fixed effects, mother's number of years of education, religion and a set of dummies for rank of birth. Green crosses correspond to point estimates when controlling for district-specific linear time trends. Orange squares show the effect of user fee removal when excluding children whose family have migrated since their birth. Black triangles plot point estimates when excluding the three urban districts identified by Lépine et al. [2018] as having a significant part of their population that declare seeking care in a rural district. In all specifications, control variables include area and time fixed effects, as well as a dummy for multiple births. Additional controls include mother's year of birth for childbirth conditions, mother's year of birth and child's sex for mortality outcomes, and child's sex and age dummies for anthropometric outcomes. For neonatal and infant mortality, children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group. The lines represent 95% confidence intervals with robust standard errors clustered at the area level. Similar figures for both rural districts and rural areas of urban districts separately are available upon request.

Figure H4. Robustness checks: alternative samples



Source: Author's calculations from DHS 1996, 2001, 2007 and 2013.
Notes: The figure shows the different point estimates obtained for each outcome for different samples. Each point corresponds to a separate difference-in-differences estimation. The red dots plot the baseline results presented in Tables 2 and 3. Black crosses show the results when the DHS 2007 and 2013 samples are restricted to under-5 children living in a rural enumeration area located more than 5 kilometers away from the boundaries of a district with another treatment status (more than 2 kilometers away for children living in an urban enumeration area). For the rest of the results presented in this figure, GPS coordinates from DHS 2007 and 2013 are used. Hence, DHS 1996 and 2001 are left apart and it is no longer possible to assess the effect of the policy on anthropometric indicators since there is no more pre-policy period for these outcomes measured at survey time. Red circles correspond to the results obtained without DHS 1996 and 2001. These are the benchmark for the last two sets of results presented here. Green squares correspond to point estimates when using an alternative criteria to classify enumeration areas within urban districts as affected or not. Individuals affected from 2007 are those living in an urban district and whose nearest health facility is located more than 15 kilometers away from the administrative center of the district and 20 kilometers away for districts located along the line of rail. Unaffected ones are those living within these radiuses. Blue diamonds plot point estimates when control enumeration areas located less than 5 kilometers away from an affected enumeration area are dropped. In all specifications, control variables include area and time fixed effects, as well as a dummy for multiple births. Additional controls include mother's year of birth for childbirth conditions, mother's year of birth and child's sex for mortality outcomes, and child's sex and age dummies for anthropometric outcomes. For neonatal and infant mortality, children who did not reach the corresponding age at survey time are dropped to avoid censoring bias, and those who did not reach this age by 2012 are also excluded since the policy was then extended to the control group. The lines represent 95% confidence intervals with robust standard errors clustered at the area level. Similar figures for both rural districts and rural areas of urban districts separately are available upon request.

Appendix References

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