

# Removing user fees for facility-based delivery services: a difference-in-differences evaluation from ten sub-Saharan African countries

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## Key messages:

- Ghana, Kenya and Senegal have recently adopted policies that remove user fees for facility-based delivery services. There is little rigorous evidence of the impact of these policies on utilization of delivery services and no evaluations have examined effects on neonatal mortality.
- Using a difference-in-differences approach, we found evidence that removing delivery fees was associated with an increase in the proportion of births delivered in a health facility and a possible reduction in neonatal mortality.
- Evidence from this and other user fee policy evaluations will be useful to governments weighing the potential benefits of removing user fees for delivery services.

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## Abstract

**Background:** Several countries in sub-Saharan Africa have recently adopted policies that remove user fees for facility-based delivery services. There is little rigorous evidence of the impact of these policies on utilization of delivery services and no evaluations have examined effects on neonatal mortality rates (NMR). In this article, we estimate the causal effect of removing user fees on the proportion of births delivered in facilities, the proportion of births delivered by Caesarean section, and NMR.

**Methods:** We used data from Demographic and Health Surveys conducted in ten African countries between 1997-2012. Kenya, Ghana, and Senegal adopted policies removing user fees for facility-based deliveries between 2003-2007, while seven other countries not changing user fee policies were used as controls. We used a difference-in-differences (DD) regression approach to control for secular trends in the outcomes that are common across countries and for time invariant differences between countries.

**Results:** According to covariate-adjusted DD models, the policy change was consistent with an increase of 3.1 facility-based deliveries per 100 live births (95% CI: 0.9, 5.2) and an estimated reduction of 2.9 neonatal deaths per 1000 births (95% CI: -6.8, 1.0). In relative terms, this corresponds to a 5% increase in facility deliveries and a 9% reduction in NMR. There was no evidence of an increase in Caesarean deliveries. We examined lead and lag-time effects, finding evidence that facility deliveries continued to increase following fee removal.

**Conclusions:** Our findings suggest removing user fees increased facility-based deliveries and possibly contributed to a reduction in NMR. Evidence from this evaluation may be useful to governments weighing the potential benefits of removing user fees.

## Introduction

In 2010, nearly 3.1 million children died within the first four weeks of life and an estimated 278,000 women died during pregnancy or childbirth [1,2]. Over 98% of these deaths occur in developing countries, and the vast majority are preventable with effective low-cost interventions. The highest priority interventions to improve maternal and neonatal survival are those that can be provided by skilled attendants (midwives, nurse-midwives, doctors) at the time of delivery (e.g., proper hygiene during birth, identification and referral of cases that require emergency care) and through expanded coverage of emergency obstetric and neonatal care (EmONC, e.g., Caesarean delivery). It is estimated that universal coverage of skilled delivery care and access to EmONC could result in up to 74% fewer maternal deaths and 30-45% fewer neonatal deaths [3,4]. However, coverage is currently far from universal and significant barriers to increasing utilization of skilled delivery care and EmONC services remain.

User fees represent a major barrier to accessing essential maternal and newborn health services in low-income countries [5,6]. Although user fees were once believed to promote higher quality health services and provide an important source of revenue for resource-strained health systems, nearly all global health actors (e.g., intergovernmental organizations, non-governmental organizations, etc.) now agree that user fees represent an inefficient funding mechanism that negatively affects utilization of essential health services [7]. Recently, the World Health Organization and the World Bank, along with other international and community organizations and numerous heads of state, have endorsed prioritizing free health services for women and children at the point of service as a first step towards free universal health coverage [8]. Several countries in sub-Saharan Africa, including Burkina Faso, Ghana, Niger, Kenya, Burundi, and Senegal, have adopted policies that remove or substantially reduce user fees for delivery services [9,10].

A recent Cochrane review on the impact of user fees on access to health services found that abolishing user fees generally increases utilization of health services, although the quality of current evidence was deemed very low [11]. Numerous studies have reported increases in facility-based deliveries after the removal of user fees [12-16]. However, a 2013 systematic review concluded, “most studies to evaluate the impact of user fees on utilization of maternal health services employ poor methods and therefore cannot produce reliable estimates of effect” [17]. Previous studies have predominantly been small pre-post samples evaluating short-term uptake of health services within limited geographic areas. Comparison of outcomes in the same population before and after a policy change may be biased because it is impossible to disentangle the effects of the policy from underlying secular trends affecting the outcome. Quasi-experimental designs, such as difference-in-differences (DD), can be used to account for underlying

secular trends in the outcome by using a series of control countries to estimate the counterfactual outcome trajectories of the countries that adopted the policy [18]. In addition, there have been no population-based evaluations of the impact of a delivery fee exemption policy on maternal or neonatal outcomes. As other researchers have pointed out, short term increases in facility deliveries after a policy change may not necessarily translate into improvements in maternal and neonatal survival [12,19].

In this study, we took advantage of a natural experiment whereby three African countries (Kenya, Senegal, and Ghana) adopted policies that removed user fees for facility-based delivery services between 2003-2007. Using a series of control countries and a DD approach, we estimated the causal effect of the delivery fee policy change on facility-based deliveries per 100 live births, Caesarean deliveries per 1000 live births, and neonatal mortality rates (NMR, the number of deaths in the first month of life per 1000 live births). Although elective Caesarean sections are common in many parts of the world, the countries included in our analysis all have rates of Caesarean delivery below 6.5% [20]. The minimum rate recommended by the World Health Organization (WHO) is 5% and, as such, we make the assumption that Caesarean delivery in this context primarily reflects a life-saving obstetric procedure [21]. This is the first study, to our knowledge, to examine the effect of a delivery fee exemption policy on neonatal mortality and the first to use a quasi-experimental design to evaluate the effects of the policy change on the proportion of births delivered in a health facility and by Caesarean section.

## Methods

### Data

We used data from Demographic and Health Surveys (DHS) conducted between 1997-2012. The DHS are nationally representative household surveys that are repeated approximately every 5 years in order to monitor trends in population health in LMIC (<http://www.measuredhs.com/>). A household questionnaire provides information on the demographic, socioeconomic, and environmental conditions of each household surveyed. A gender-specific questionnaire, which is administered to all women age 15-49 who spent the night before the survey in each household, collects complete birth histories, including information on the use of maternal and child health services. We used available surveys that provided information on live births that occurred between 1995 and 2012.

### Measures

We examined the effect of a delivery fee exemption policy on three binary outcome measures: neonatal death, delivery by Caesarean section, and delivery in a health facility. Neonatal mortality was measured by an indicator of whether a child who was born alive died within the first month of life. Mothers reporting a deceased child were asked to report the age at death, in days if the death occurs in the first 30 days of life, in months between 1 and 23 months, and in years for deaths age 2 and older. We included data on neonatal deaths occurring up to 10 years prior to the date of each survey. Women were also asked whether each child was born by Caesarean section and the location of the birth. Self-reports of Caesarean delivery have been shown to be reliable [22] and the WHO endorses the use of Caesarean delivery rates as a marker for the availability and use of obstetric services in resource-poor settings where access to skilled obstetric and newborn care is limited [21]. For the outcome of health facility delivery, births that took place in a public or private health facility (hospital, health center, maternity, clinic) were coded as one and births that took place at home (either the woman's or someone else's) were coded as zero. Multiple births (twins, triplets) were considered as a single observation for the outcomes of facility delivery and Caesarean section. For most of the surveys, information on facility delivery and Caesarean section was available for births in the 5 years preceding the survey date. However, a few of the earlier surveys (Kenya 1999, Cameroon 1998, and Nigeria 1999) only collected this information for births up to 3 years before the interview date. Reports of Caesarean section among births that did not occur in health facilities were recoded as non-Caesarean deliveries. Information on Caesarean delivery was not collected in the 1997 Senegal DHS.

The exposure of interest is a variable indicating whether each live birth occurred after the adoption of a policy removing user fees for facility-based deliveries. The countries that passed a policy and thus contribute outcomes to the “intervention” group are Kenya, Senegal, and Ghana. Months were used as the time variable to allocate births before and after the policy. The policy adoption dates are shown in Figure 1 and a brief description of the policies is provided in Table 1. Information on the different policies was obtained from previous publications and government documents [16,23-25]. In Ghana and Senegal, the delivery fee exemption policy was implemented first in selected regions/provinces and subsequently rolled out to the rest of the country. In these countries we defined the exposure based upon region of residence. For example, births occurring after September 2003 in Ghana's Northern, Upper East, Upper West, or Central regions contributed outcomes to the intervention group, while births in the other 6 regions of Ghana contributed outcomes to the control group until April 2005 when the policy was adopted in these regions.

In addition to the three intervention countries, we included a set of control countries that did not pass a policy exempting user fees for deliveries during our study period. The following criteria was used in selecting the control countries: 1) at least two available DHS surveys covering the study time period, with the most recent study conducted in 2008 or later; 2) sub-Saharan African countries; 3) no evidence of major reforms affecting health care financing (e.g., Rwanda adopted a community based health insurance program, Burkina Faso subsidized delivery services by 80%); and 4) no evidence of pre-policy trends for outcomes that differed significantly from those of the intervention countries. The fourth criteria relates to the importance of selecting a control group that represents a good approximation of the counterfactual outcome trends for the intervention group, which is further discussed in the statistical analysis section below. Given the first three control selection criteria, we identified seven potential control countries: Cameroon, Congo (Brazzaville), Ethiopia, Gabon, Mozambique, Nigeria, and Tanzania. We retained Tanzania, Mozambique, and Ethiopia as potential controls even though policies exist (at least on paper) that exempt women from paying user fees for maternity care. As these countries experienced no major delivery fee policy changes over the study period they may provide good approximations of the counterfactual outcome trends for the intervention group. Moreover, there is ample evidence that the fee exemptions in these countries are not widely known about or enforced [26,27]. The availability of DHS birth history data for each country is shown in Figure 1.

We considered several covariates in our analyses: maternal age (<20 years, 20-35 years, 35+ years), urban/rural residence, parity (firstborn vs. other), maternal education (none, primary, secondary or higher), and household wealth. For household wealth, we used the continuous asset-based wealth index provided in the DHS, which is based on a set of variables related to household conditions (e.g., water source, sanitation facilities, electricity) and ownership of consumer goods (e.g., a bicycle, a telephone, a refrigerator) and is constructed for each survey using factor analysis [28]. We then generated wealth quintiles separately for each policy area used in the analysis, meaning that the wealth quintile is measured relative to other households within the same area. For analyses that used the household wealth variable we had to exclude births from the 1998 Nigerian survey because information on assets was not collected for use in estimating household wealth.

## Statistical Analysis

We used DD regression to estimate the causal effect of a policy change abolishing user fees on three outcomes: neonatal death, delivery by Caesarean section, and delivery in a health facility [18]. DD analysis

is used frequently in policy evaluations to compare outcomes before and after a policy change for a group affected by the change (intervention group) to a group not affected by the change (control group) [29,30]. Systematic reviews have found that quasi-experimental designs, including DD, mimic the results of experimental designs much better than traditional methods of controlling only for observed confounding via regression modeling [31,32].

To estimate the effect of policy change on neonatal mortality, we used a logistic regression model of the form:

$$\text{logit}[P(Y_{ict})] = \alpha + \beta \text{Policy}_{ct} + \gamma_t + \delta_c + \mathbf{X}_{ict}$$

where  $Y$  is an indicator of whether infant  $i$  died in the first month of life in area  $c$  at time  $t$ ,  $\text{Policy}_{ct}$  is a dummy variable indicating whether the birth occurred after the passage of a policy abolishing user fees,  $\gamma_t$  and  $\delta_c$  are vectors of fixed effects for birth year (2-year intervals) and area (country or sub-national area), respectively, and  $\mathbf{X}_{ict}$  is a vector of individual-level covariates. The coefficient of interest is  $\beta$ , which represents the change in log-odds of the outcome among those exposed to a reduced user fee policy compared to those not exposed. Area fixed effects control for any time-invariant characteristics of countries or sub-national regions (for the regions/provinces that were early or late adopters of the policy changes in Ghana and Senegal). For example, the area fixed effects will control for unmeasured differences between countries (e.g., political, economic, environmental) that have been shown to predict persistent differences in NMR [33]. Year fixed effects control for secular trends in the outcomes that are common across countries (e.g., declining rates of neonatal mortality across Africa [34]). Models for the two other outcomes were analogous to the above equation, where  $Y$  indicates whether infant  $i$  was delivered by Caesarean section or in a health facility. We estimated multivariable models that included covariates that may control for factors that could contribute to changes in the outcomes over time, including mother's age, mother's education level, urban/rural residence, and parity. To facilitate interpretation for all models and to assess differences on the absolute probability scale, we reported average marginal effects calculated from the logistic coefficients [35]. We adjusted standard errors for clustering by the primary sampling unit and performed all analyses using Stata version 12 (StataCorp, College Station, TX).

A main assumption of the DD model is that the outcome trend in the control group represents a good approximation of what the outcome trend in the intervention group would have been in the absence of the policy change (i.e., the counterfactual trend). Because we cannot observe the counterfactual trend, this assumption can be partially checked by ensuring that outcome trends are similar for the intervention and

control areas prior to introduction of the policy. We therefore examined pre-policy trends between the intervention and control countries, both graphically and using formal statistical tests. As the policies were passed at different times between 2003-2007, we used the period of 1995-2003 to represent the pre-policy time period across all countries. To formally test whether outcome trends were different between the intervention and control groups, we estimated multivariable logistic regression models that included an interaction term between birth year and country group (control or policy). We modeled trends using a linear term for birth year; however, we also compared these results with models including birth year fixed effects. This process was performed separately for each of the three outcomes in order to select a set of control countries that approximated average pre-policy outcome trends for the intervention countries.

The DD model also assumes that the policy is the only factor that affects trends in the outcomes between the intervention and control groups following the policy change. For example, if broad health reforms occurred around the time of the delivery fee policy change that had effects on reproductive health, our DD model might erroneously attribute these effects to the delivery fee policy change. Our use of multiple treatment and control groups and multiple pre- and post-intervention time periods helps to minimize this threat [36]. We also searched the literature for other major health policies and reforms occurring around the time of the user fee policy changes in the three intervention countries.

## Results

Table 2 presents country-specific descriptive statistics for our three outcomes in the pre-policy time period (1995-2003). While there were considerable differences in average levels for the three outcomes between countries, these baseline differences are accounted for by the area fixed effects in the DD analysis. Our main concern was to ensure approximately equivalent pre-policy outcome *trends* between the policy countries (i.e., countries that eventually pass a delivery fee exemption policy) and control countries. As such, for each outcome we attempted to select a set of control countries that would approximate the average trend for the policy countries. We estimated covariate-adjusted logistic regression models that included an interaction term between birth year and county group (policy or control) to assess whether there was formal statistical evidence that pre-policy trends differed significantly between the policy and control countries. Based on small interaction p-values suggestive of differential trends, we excluded Congo and Mozambique for the outcome of NMR, Nigeria for the outcome Caesarean delivery, and Nigeria, Gabon, Cameroon, and Ethiopia for facility-based delivery. In general, the policy countries experienced greater estimated increases in facility deliveries over the pre-policy time period than did most of the potential control countries, which would potentially violate the main assumption of our DD analysis. For



this reason, we excluded several countries from the control group for the facility delivery analysis. Once the control countries were selected, there was no evidence that trends between the policy and control groups were significantly different from each other. Interaction p-values testing for equality of trends between the policy and control countries were 0.41, 0.43, and 0.18 for facility delivery, Caesarean delivery, and NMR, respectively.

Table 3 presents the results of the DD analysis for the three outcomes. Estimates for the variable ‘Fee exemption policy’ are average marginal effects of the policy change on each outcome and can be interpreted as the difference in adjusted outcome proportions between the intervention (policy) and control groups in the post-policy period. The delivery fee exemption policy was associated with an increase of 3.1 health facility deliveries per 100 births (95% CI: 0.9, 5.2), adjusted for individual- and household-level covariates. The fully adjusted effect estimate of the policy on NMR was -2.9 neonatal deaths per 1000 live births (95% confidence interval (CI): -6.8, 1.0) suggesting a possible reduction in NMR, although the 95% confidence interval includes the null. We found no evidence the policy change was associated with an increase in Caesarean deliveries. Individual (e.g., maternal age, parity) and household (e.g., household wealth) characteristics showed expected associations with the outcomes, although their inclusion in the models did not have much impact on the estimated policy effects. This adds credibility to the assumption that, conditional on time and area fixed effects, the policy changes are exogenous. Lastly, our inferences were unaffected when we used a linear term for birth year instead of birth year fixed effects.

The results presented in Table 3 show average pooled effects for the three policy changes. However, in Table 2 we saw that pre-policy trends differed between the three countries, particularly with Senegal experiencing a more rapid increase in facility deliveries and reduction in neonatal mortality than Ghana and Kenya. To further investigate whether Senegal may have been driving our estimated average policy effects, we performed some sensitivity analyses (Table 1 in the Supplementary Material). Sensitivity Analysis 1 estimated the effect of the policies in Ghana and Kenya (excluding Senegal) on the proportion of deliveries in a health facility, finding an estimated increase of 3.8 facility deliveries per 100 live births (95% CI: 2.1, 5.6). We could not, however, identify an appropriate set of control countries to estimate the effect of the policy change in Senegal alone on facility deliveries. Similarly, in Sensitivity Analysis 2 we estimated the policy change in Senegal was associated with a reduction of 4.3 neonatal deaths per 1000 live births (95% CI: 0.1, 8.5)). In general, these two sensitivity analyses gave similar inferences compared to the pooled analyses.

We wanted to investigate the possibility that effects of the policy change might be more evident as time passed after the policy change, perhaps because it takes some time to fully implement the policy or for the public to become aware of the policy change. To do this we examined lag effects to see whether effects changed one year subsequent to the policy change. We also estimated lead-time effects to check that observed effects attributed to the policy were not present before adoption of the policy (i.e., that consequences did not happen before the cause) [18]. Lead-time effects were estimated for the three years prior to policy adoption. Examining lead-time and lag effects is a useful check for DD analysis because of the concern that other reforms or policies affecting the health sector in general may have affected trends in reproductive health services and neonatal mortality over the study time period. Figure 2 plots lead-time and lag effects for facility-based deliveries and NMR, estimated from covariate-adjusted DD models. There is evidence of an increase in the effect of the policy on facility deliveries one year following the policy change, as well as some evidence of a lag effect for NMR. The estimates show no effects in the three years before implementation of the policies for either facility deliveries or NNR. We also examined lead-time and lag effects for Caesarean deliveries, finding no evidence of an effect of the policy for any time point (not shown).

## Discussion

To our knowledge, this is the first paper to estimate the causal effect of removing or reducing user fees for delivery services on neonatal mortality and the first to use a DD design to evaluate the impact of the policy change on the proportion of births delivered in a health facility and by Caesarean section. Our evaluation found that the delivery fee policy change led to substantial increases in facility-based deliveries, and was consistent with a meaningful reduction in neonatal mortality. We also found evidence of stronger effects of the policy change on facility deliveries one year after the policy change. This seems to suggest the policy change took some time to be fully implemented, which is a plausible scenario for a large national-scale program such as a user fee exemption.

The direction of our results are consistent with previous evaluations that found increases in the proportion of deliveries in health facilities subsequent to adoption of a delivery fee exemption or reduction policy [12,13,16,19]. However, our estimate of the magnitude effect of the policy change on facility deliveries (an increase of 3.1 facility-based deliveries per 100 live births) is much smaller than increases reported in several previous evaluations that estimated single pre-post differences. Studies using household survey data from rural Burkina Faso, Uganda, and Ghana estimated increases of 35, 28 and 5-12 facility-based deliveries per 100 live births, respectively, following delivery fee exemption/subsidy policies [19,25,38]. In

the presence of increasing secular trends in the prevalence of facility-based delivery, single pre-post differences are likely to overestimate policy effects, especially when estimated over longer time periods. It is possible that we observed generally smaller effect estimates because our DD analysis controlled for common secular trends affecting trends in most countries. This is also supported by the substantial increases in the magnitude of our policy effect estimates for all outcomes when we did not include birth year fixed effects in our models. Without adjustment for secular trends, our policy effect estimates were: 8.6 facility deliveries per 100 births (95% CI: 6.0, 11.2), -9.5 neonatal deaths per 1000 live births (95% CI: -13.4, -5.6), and 1.3 Caesarean deliveries per 100 live births (95% CI: 0.9, 1.7). Our DD estimate was, however, similar in magnitude to that of a recent study that used time series analysis to account for secular trends [12]. The study found a 2.3% increase in facility deliveries after the free delivery policy in several predominantly rural districts of Ghana.

We did not find evidence that the delivery fee policy change was associated with an increase in the proportion of deliveries by Caesarean section. In contrast to normal delivery care with a skilled birth attendant (e.g., nurse, midwife), Caesarean delivery requires surgical skills and is most often limited to hospital settings. It has been shown that access to emergency obstetric care, including Caesarean section, is determined by a range of factors, including the availability, quality, and cost of health services [6,39]. It may be that geographical proximity to hospitals and quality of services available are more important determinants of Caesarean delivery than financial barriers imposed by user fees. Caesarean delivery was also a rare occurrence in the countries included in our analysis. In the majority of the countries, Caesarean delivery was less common than neonatal mortality. Furthermore, information on Caesarean section was only asked for births in the 3 or 5 years preceding each survey, so the sample size was considerably smaller than for neonatal mortality.

A major contribution of this study is that it is the first to estimate the causal effect of a delivery fee exemption policy on neonatal mortality. Several researchers have cautioned that increased utilization of services after a fee exemption policy may not necessarily translate into improved health outcomes and have endorsed further research to evaluate effects on morbidity and mortality [17,19]. Furthermore, a recent Cochrane review found there exists little quality evidence of the effects of removing user fees on health outcomes in low- and middle-income countries [11]. The review, however, did not include a recent randomized trial in Ghana that found removing out-of-pocket costs increased utilization of child health services but did not lead to any difference in mortality. Thus, although our estimated effects are imprecise and should be interpreted cautiously, we estimated that the introduction of a policy to remove delivery

fees was consistent with a 9% reduction in neonatal mortality, which is an encouraging finding and an important contribution to the literature on the effects of removing user fees for health services.

We used nationally representative data from five countries and a more rigorous analytical approach than previous evaluations to further understand the health and health service utilization effects of removing user fees for deliveries. Our results, however, should be considered in light of some important limitations of both the data and the analytical approach. First, all of our outcomes are self-reported and there is some concern about possible misclassification and recall bias. While self-reports of Caesarean section have been shown to have generally good reliability [22], self-reports of neonatal death are more of a concern [40]. However, we have no reason to believe that underreporting of early neonatal death or misclassification of neonatal death as stillborn would differ systematically between time periods before and after the policy change or between intervention and control areas.

In our analysis, we chose to combine multiple policy changes in order to strengthen the DD design and increase our sample size to permit investigation of neonatal mortality and Caesarean delivery. However, the delivery fee policy changes in Kenya, Ghana, and Senegal were similar but not identical, and as such it is valid to question whether pooling to obtain an average policy effect was appropriate. One difference among the policy changes is that in Ghana the policy extended to public, private and faith-based facilities, while in Senegal and Kenya the policy applied only to public health facilities. Furthermore, while the fee exemption policies in Senegal and Ghana included hospital care and Caesarean delivery, the initial free delivery policy in Kenya applied only to delivery services in lower-level facilities (e.g., health centers, dispensaries). When we performed a sensitivity analysis excluding Kenya as a policy country (Table 1 in the Online Supplementary Material, Sensitivity Analysis 3), our inference for the effect of the policy on Caesarean deliveries was unchanged (adjusted estimate of 0.9 caesarean deliveries per 1000 live births (95% CI: -4.8, 6.5)).

Lastly, the validity of our results depends to a large extent on how well the assumptions of the DD analysis were met. The DD set-up assumes the temporal trends in the outcomes for the control group represents a good approximation of the counterfactual trend for the intervention group. We performed checks of these assumptions, including examining trends and looking at lag and lead effects, which generally support our main results. The other major assumption is that no other factors differentially affected outcomes in the intervention and control areas at the time of the delivery fee exemption policy. To address this concern, we searched the literature for other major health policies and reforms occurring around the time of the user fee policy changes in the three intervention countries. One policy that is important to mention is the

National Health Insurance Scheme (NHIS) in Ghana, which began to be implemented in late 2005 [12]. The NHIS was designed to replace the practice of charging user fees at the point of service, with enrollment in the scheme estimated at 7% and 45% of the population in 2005 and 2008, respectively [37]. Ghana's free delivery policy was implemented prior to the NHIS and already exempted payment of user fees for delivery services and Caesarean sections. As such, we would not expect much additional effect on utilization of delivery services or neonatal mortality due to implementation of the NHIS. In 2008, the delivery fee exemption policy officially ended and was replaced by the NHIS; however, this could not affect our results as we only had DHS data from Ghana until 2008.

## Conclusions

We found evidence that more women accessed maternity services after they were made free, implying that user fees in health facilities were limiting demand for delivery services in our study population. This corroborates evidence from numerous low-income settings that cost is a significant barrier to increasing the use of maternal health services [5,6]. However, even after delivery services were made free, still fewer than 60% of women in our study gave birth in a health facility and average rates of Caesarean section remained below the recommended 5% minimum level. Moreover, we found no evidence that removing delivery fees increased rates of Caesarean section, a critically important intervention to save maternal and newborn lives. Thus, while our results are largely positive regarding the effects of free delivery services on utilization of facility-based care and neonatal mortality, user fees are by no means the only barrier to accessing essential delivery care and emergency obstetric care in resource-limited settings. Factors such as geographical access to facilities, quality of services, transportation costs, and cultural barriers also need to be simultaneously addressed in efforts to reduce maternal and neonatal mortality.

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**Table 1:** Description of delivery fee policy changes in Ghana, Senegal and Kenya

Country	Dates of policy adoption	Policy Details
Ghana	Sept 2003 (four most deprived regions (Northern, Upper East, Upper West, Central)) April 2005 (remaining 6 regions)	Free deliveries in public, private and faith-based health facilities. Covers all normal deliveries, management of assisted deliveries including Caesareans, and management of medical and surgical complications of delivery [25].
Senegal	Jan 2005 (five most deprived provinces (Kolda, Ziguinchor, Tambacounda, Matam and Fatick)) Jan 2006 (remaining provinces except Dakar)	Covers normal deliveries at health posts and health centers and Caesarean sections at district and regional hospitals. Only public sector (not private or traditional facilities) [16,25].
Kenya	July 2007	Free deliveries in all public dispensaries and health centers, including all supplies required for delivery. The policy did not initially cover delivery fees in district hospitals and thus did not apply to Caesarean sections [24].

**Figure 1: Dates of delivery fee exemption policies and birth history data availability by country.** Birth history data for all live births in the 5 years preceding each survey comes from Demographic and Health Surveys conducted between 1997 and 2012. Policies in Ghana and Senegal were passed first in selected regions/provinces and subsequently rolled out to the rest of the country.

[Figure 1]

**Table 2:** Pre-policy estimates of means and annual rates of change for NMR, Caesarean deliveries, and facility deliveries by country, Demographic and Health Surveys 1995-2003

Country	Neonatal deaths per 1000 births		Caesarean deliveries per 1000 births		Facility deliveries per 100 births	
	Mean (SE)	Annual Change (95% CI)	Mean (SE)	Annual Change (95% CI)	Mean (SE)	Annual Change (95% CI)
Policy countries						
Ghana	34.6 (1.8)	0.1 (-1.2, 1.5)	33.3 (2.7)	-0.0 (-1.9, 2.0)	40.9 (1.3)	0.4 (-0.5, 1.3)
Kenya	31.5 (1.5)	0.3 (-0.7, 1.3)	44.9 (2.7)	-0.3 (-2.3, 1.7)	40.7 (1.1)	0.0 (-0.7, 0.8)
Senegal	40.0 (1.5)	-1.4 (-2.3, -0.5)	17.7 (2.1)	0.4 (-2.5, 3.3)	53.1 (1.4)	1.9 (1.1, 2.6)
Control countries						
Cameroon	30.9 (1.3)	-1.0 (-1.9, 0.0)	20.3 (2.8)	-0.9 (-2.2, 0.3)	59.9 (1.7)	-0.1 (-1.2, 1.0)
Congo	31.2 (2.1)	-1.1 (-2.5, 0.4)	31.2 (3.6)	0.7 (-6.4, 7.7)	86.1 (1.5)	0.4 (-0.7, 1.4)
Ethiopia	41.4 (1.2)	-0.7 (-1.7, 0.2)	12.6 (1.1)	0.7 (-0.1, 1.5)	9.6 (0.6)	0.1 (-0.3, 0.5)
Gabon	22.0 (2.0)	0.1 (-1.1, 1.4)	44.7 (4.0)	-0.8 (-5.0, 3.4)	83.3 (1.4)	-1.0 (-1.7, 0.4)
Mozambique	37.7 (1.4)	-2.2 (-3.1, -1.3)	19.2 (1.5)	-0.5 (-1.6, 0.6)	52.9 (1.2)	0.8 (0.1, 1.5)
Nigeria	47.0 (1.4)	-0.7 (-1.6, 0.3)	21.5 (1.8)	-3.1 (-4.8, -1.3)	36.6 (1.3)	-0.3 (-1.2, 0.5)
Tanzania	30.8 (1.3)	-0.9 (-1.8, 0.0)	26.7 (2.0)	0.4 (-1.1, 2.0)	45.2 (1.2)	0.1 (-0.8, 1.0)
Policy countries	36.0 (0.9)	-0.6 (-1.2, -0.0)	33.3 (1.5)	-0.4 (-1.5, 0.6)	45.7 (0.8)	0.8 (0.5, 1.1)
Control countries <sup>a</sup>	38.7 (0.7)	-1.2 (-1.7, -0.7) <sup>b</sup>	21.3 (0.8)	0.0 (-0.5, 0.6) <sup>c</sup>	53.8 (0.9)	0.5 (0.1, 0.9) <sup>d</sup>
		P <sup>e</sup> =0.18		P <sup>e</sup> =0.43		P <sup>e</sup> =0.41

SE, Standard Error

<sup>a</sup> The group of control countries were selected to approximate average trends in the policy countries for each outcome.

<sup>b</sup> Excludes Congo and Mozambique

<sup>c</sup> Excludes Nigeria

<sup>d</sup> Excludes Nigeria, Gabon, Cameroon, and Ethiopia

<sup>e</sup> Interaction p-values to test for of equality of trends between policy and control countries. Estimated from covariate-adjusted logistic regression models that included an interaction term between birth year (linear term) and country group (i.e. policy or control countries).

**Table 3:** Estimated effect of the user fee policy change on facility-based deliveries, Caesarean deliveries, and neonatal mortality. Estimates are from difference-in-differences models that include area and birth year fixed effects. Demographic and Health Surveys, 1995-2012

	Health facility deliveries per 100 births (95% CI) (n=105,638)		Caesarean deliveries per 1000 births (95% CI) (n=166,662)		Neonatal deaths per 1000 births (NMR) (95% CI) (n=291,479)	
Fee exemption Policy	3.9 (0.8, 7.0)	3.1 (0.9, 5.2)	1.1 (-3.7, 5.9)	0.3 (-4.3, 4.8)	-3.0 (-7.2, 1.2)	-2.9 (-6.8, 1.0)
Maternal age						
<20 years		-3.6 (-4.5, -2.8)		-9.4 (-11.6, -7.3)		7.5 (5.4, 9.7)
20-34 years (ref)		-		-		-
35+ years		0.4 (-0.4, 1.2)		8.2 (4.6, 11.7)		11.9 (9.4, 14.4)
First birth		12.8 (12.0, 13.7)		22.8 (20.6, 25.0)		11.5 (9.5, 13.4)
Urban residence		15.6 (14.3, 17.0)		14.7 (12.1, 25.0)		-2.0 (-4.3, 0.4)
Education						
None (ref)		-		-		-
Primary		10.9 (9.9, 11.9)		13.9 (11.7, 16.2)		-3.4 (-5.4, -1.4)
Secondary or higher		21.6 (20.1, 23.0)		28.2 (24.8, 31.6)		-7.1 (-9.6, 4.7)
Wealth Quintile						
Poorest (ref)		-		-		-
2 <sup>nd</sup>		6.8 (5.6, 8.0)		3.7 (1.0, 6.4)		-0.6 (-3.0, 1.8)
Middle		14.9 (13.6, 16.3)		9.9 (6.9, 12.9)		2.6 (0.1, 5.2)
4 <sup>th</sup>		23.6 (22.2, 25.1)		14.1 (11.0, 17.1)		-1.8 (-4.4, 0.7)
Richest		35.1 (33.4, 36.9)		32.6 (24.8, 31.6)		-5.1 (-8.0, -2.1)

**Figure 2: Lead-time and lag effects for facility-based deliveries and NMR.** Point estimates represent effects of the delivery fee exemption policy change, estimated at the time of the policy adoption ( $t=0$ ) and for the three years before and one year after the policy change. Vertical bars represent 95% confidence intervals for the estimates.

[Figure 2]