

# Removing Delivery Fees and Caesarean Section Use in Ghana: A Study Protocol for a Quasi-Experimental Difference-in-Differences Analysis

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## Study protocol

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# Abstract

## Background

Ghana's Delivery Fee Exemption Policy (DFEP) removed user fees for all childbirth services in four pilot regions (Northern, Upper East, Upper West, and Central) starting in September 2003, before the policy's national expansion in April 2005. Its effects on facility delivery are documented, but no causally identified estimate of its specific effect on caesarean section (CS) utilization that exploits Ghana's own phased regional rollout exists. Whether fee removal improves uptake of this resource-intensive intervention is unknown. This pre-registered study evaluates the DFEP through four complementary strands: a pilot-phase difference-in-differences estimate for CS, mechanism and equity (distributional) analyses, and a national scale-up analysis. It also offers a replicable template for DHS-based evaluation of maternal health financing reforms.

## Methods

A retrospective quasi-experimental difference-in-differences (DiD) design will compare CS utilisation before and after the DFEP pilot phase (September 2003 to March 2005) between the four pilot regions and the six non-pilot regions. Data are the Ghana Demographic and Health Survey (GDHS) Individual Recode files for 2003 and 2008. Birth-level cohorts are constructed from DHS century-month codes, with the pre-policy period defined as January 1998 to December 2002 and the pilot-exposed period as September 2003 to March 2005. The primary outcome is caesarean section; secondary outcomes are facility-based delivery and skilled birth attendance. The DiD estimator will be obtained from a survey-weighted linear probability model with region fixed effects. Because the design has only 10 regional clusters, inference will use a wild-cluster bootstrap with Webb weights and exact randomisation inference. Equity will be examined through triple-difference models, and pre-specified robustness checks include a placebo pre-trend test using the 1998 GDHS and a relative-magnitudes sensitivity analysis.

## Discussion

This is a study protocol; no analysis has been undertaken, and no results are reported. By exploiting Ghana's phased regional rollout within a within-country DiD design, the protocol sets out a causally defensible approach to estimating the DFEP's effect on caesarean section utilisation and its distribution, and a replicable template for evaluating maternal health financing reforms using DHS data. The design is powered only for large effects so that the estimate will be reported as precision-bounded.

## Registration:

Open Science Framework (OSF); <https://doi.org/10.17605/OSF.IO/FR75Z>.

## Background

# Global maternal mortality and the role of emergency obstetric care

Maternal mortality remains one of the largest sources of preventable death among women of reproductive age, and the burden is concentrated in low-income settings. An estimated 260,000 women died from maternal causes globally in 2023, a 41% reduction from 443,000 deaths in 2000. Yet the world remains far off track to meet the Sustainable Development Goal (SDG) 3.1 target of a maternal mortality ratio (MMR) below 70 per 100,000 live births by 2030 [1]. Progress has also stalled. Between 2016 and 2020, the global MMR stagnated or worsened in most regions, and only two regions recorded significant declines [2, 37]. Sub-Saharan Africa carries a disproportionate share of this burden, accounting for approximately 70% of all maternal deaths worldwide and recording an MMR of over 450 per 100,000 live births, more than 150 times the level in high-income countries [37]. Haemorrhage, hypertensive disorders, sepsis, and indirect obstetric causes remain the leading causes of preventable maternal death, and most deaths occur in the postpartum period [3]. Because the majority of these deaths can be averted by timely emergency obstetric care, their persistence in sub-Saharan Africa reflects a failure of access rather than an absence of clinical knowledge.

## Caesarean section as a marker of access and inequality

At the population level, caesarean section rates of 10% to 15%, the range WHO has long regarded as sufficient, are associated with lower maternal and neonatal mortality, and rates below 10% indicate unmet need for emergency obstetric surgery [4]. More recent analyses refine this range, with rates of 9% to 19% associated with mortality reductions, and no additional benefit observed beyond that range [5]. Globally, 21.1% of births occurred by CS in 2015. Sub-Saharan Africa averaged approximately 5%, well below the lower threshold and indicative of widespread unmet surgical need, compared with 42.8% in Latin America and the Caribbean; global CS rates are projected to reach 28.5% by 2030, with unmet need and overuse expected to coexist [6]. These averages conceal large within-country inequalities. In low- and middle-income countries, CS rates are nearly five times higher in the wealthiest than in the poorest quintile, and substantially higher in private than in public facilities [7, 8]. Ghana shows both ends of this problem. CS prevalence rose from 4.4% in 2003, below the recommended lower threshold at the time of the Delivery Fee Exemption Policy and indicating unmet need, to 20.1% by 2022, which exceeds the upper bound of the recommended range [9, 10]. The same country has therefore moved from unmet need to possible overuse within two decades, which raises questions about both equitable access and medically unnecessary surgery.

## Access barriers to caesarean section

Both demand-side and supply-side barriers constrain access to CS in low-income settings. On the demand side, direct costs at the point of care (facility fees, drug and consumable charges, and informal

payments) deter poor and rural women from seeking facility delivery, even in emergencies [11–13]. Even where formal fee-exemption policies exist, women continue to incur unofficial out-of-pocket costs such as transport, drugs, food, and informal payments, which remain a substantial deterrent to care-seeking among rural and poor households [12, 14]. On the supply side, shortages of trained obstetricians and anaesthetists, limited theatre capacity, and unreliable supply chains for surgical materials restrict the availability of CS services even where women do seek care [15]. The Three Delays Framework identifies the financial and geographic barriers that delay the decision to seek care (delay 1) and the decision to reach a facility able to provide it (delay 2) as primary determinants of preventable maternal death [16]. Fee removal directly targets the first delay, but its effect on the second and third delays is limited when supply-side constraints are not addressed simultaneously.

## User fee removal and maternal health in sub-Saharan Africa

User fees, which are direct charges levied at the point of care, were introduced across many low- and middle-income countries (LMICs) during the 1980s and 1990s as part of structural adjustment programmes, on the assumption that modest charges would raise revenue without substantially reducing utilisation [17]. Evidence accumulated over the following decades contradicted this assumption. User fees were repeatedly shown to reduce service utilisation, particularly among the poorest households, to widen socioeconomic inequities in access, and to delay care-seeking to the point of preventable death [11–13]. The case for abolishing user fees in maternal care gained ground through the 2000s, and several countries in sub-Saharan Africa introduced exemption policies for antenatal care, facility delivery, and emergency obstetric services.

The evidence on user fee removal and maternal health outcomes has expanded over the past decade. In a systematic review of 20 studies, Dzakpasu et al found evidence of an increase in facility-based delivery following fee removal. However, the evidence on maternal and neonatal outcomes was limited and generally of low quality [18]. A recent systematic review of government-led free CS policies across 28 LMICs found that fee removal consistently increased CS uptake but frequently failed to ensure equity or financial protection, with informal costs and delayed reimbursements limiting its impact and catastrophic expenditure persisting for 12% to 43% of households despite formal exemptions [14]. A more recent systematic review of quasi-experimental studies, which provides the evidence base for the present study, found that free healthcare policies consistently improved access to antenatal care, facility-based delivery, and skilled birth attendance, but that effects on CS utilisation and mortality were heterogeneous [19]. Equity outcomes were also inconsistent: some policies narrowed socioeconomic disparities in access, while others widened them by disproportionately benefiting women who already had greater capacity to reach care.

Evidence on the specific effect of fee removal on caesarean section utilisation has increased in recent years, but remains limited compared with the broader fee removal literature. In a quasi-experimental evaluation of free CS policies in Benin and Mali, a study found a significant increase in CS rates after fee exemption, particularly among women with no education and those living in rural areas [20]. That study

is the most rigorous prior evidence on this question in West Africa, but it is confined to countries with CS-specific exemption policies. Ghana's DFEP, by contrast, exempted all deliveries from fees, including caesarean section, and therefore offers a distinct natural experiment.

Quasi-experimental evidence on broader fee-removal policies, which exempt all deliveries rather than caesarean section alone, has generally found gains in facility delivery without a matching rise in caesarean use. In a difference-in-differences analysis of Demographic and Health Survey data from ten sub-Saharan African countries, including Ghana, over the 2003 to 2007 period, McKinnon et al. found that fee removal raised facility-based delivery but produced no detectable increase in caesarean sections [21]. A difference-in-differences study of Zambia's staggered fee abolition reached the same conclusion for caesarean section and for neonatal mortality, with facility-delivery gains emerging only about two years after the reform and only where the quality of care was adequate [22]. A quasi-experimental analysis of user-fee reforms in five sub-Saharan countries including Ghana likewise found only a 0.7 percentage-point change in caesarean use, concentrated among poor, non-educated, and rural women [23]. These are the closest methodological precedents for the present analysis, yet each either pools Ghana with other countries or studies a different setting. Hence, none exploits the within-Ghana variation created by the phased regional rollout of the DFEP.

## **Ghana's Delivery Fee Exemption Policy, 2003 to 2007**

In September 2003, the Government of Ghana introduced the Delivery Fee Exemption Policy (DFEP) to remove user fees for all childbirth services at accredited public, mission, and private health facilities. The policy covered normal vaginal deliveries, assisted deliveries, caesarean sections, and the management of obstetric complications. It was one of the most comprehensive maternal fee exemption policies in sub-Saharan Africa at the time [24].

Implementation was phased by region. Between September 2003 and March 2005, the DFEP was piloted in four regions, namely Northern, Upper East, Upper West, and Central, which were selected based on their historically high poverty, low facility delivery, and limited access to skilled obstetric care. These are among Ghana's most economically marginalised regions, and their selection for early implementation reflected an explicit equity objective. From April 2005, the policy was extended to all ten regions to provide free delivery care nationwide. It was discontinued in June 2007 and replaced by a different maternal health financing arrangement [36]. Unlike the later maternal health reforms incorporated into Ghana's National Health Insurance Scheme (NHIS), the DFEP exempted deliveries without any enrolment requirement, so that access did not depend on prior registration or documentation [25]. The DFEP should not be conflated with the later 2008 'Free' Maternal Health Care Policy delivered through the National Health Insurance Scheme, which required enrolment and has been evaluated separately [39]. An early household survey of the DFEP in the Central and Volta regions found that out-of-pocket payments for delivery, including caesarean section, fell after the exemption but that better-off households benefited more than the poorest. This study examines that equity question directly for the pilot regions [38].

Although the DFEP's effects on facility delivery and skilled birth attendance have been evaluated [24, 25], no study has rigorously assessed its specific effect on CS utilisation using a causal quasi-experimental design that exploits the phased regional rollout. The existing evaluations rely mainly on descriptive or non-experimental approaches that compare aggregate regional or national statistics over time without controlling for confounding or testing counterfactual assumptions. The phased regional rollout, in which four pilot regions received the policy before national expansion, creates the kind of quasi-experimental variation suited to a difference-in-differences analysis that can yield credible causal estimates.

## Evidence gap and study justification

Two evidence gaps motivate this study. First, no causally identified estimate of the DFEP's effect on CS utilisation that uses Ghana's own regional variation exists. The available literature documents trends in facility delivery and antenatal care uptake but does not establish whether the removal of fees improved access to the most resource-intensive obstetric intervention. This matters because CS depends on both financial access and functional facility infrastructure, including operating theatres, anaesthesia, blood products, and trained personnel, and may therefore respond differently to fee removal than lower-intensity services. Second, no study has examined the distributional effects of the DFEP on CS. It is therefore unknown whether any gain in access was shared across wealth groups and geographic areas or concentrated among women who were already advantaged.

The quasi-experimental evidence isolating caesarean section as an outcome of fee removal in sub-Saharan Africa remains small, and none of these studies identifies the effect of the DFEP itself [21–23]. By applying to birth-level GDHS data a pre-registered set of analyses that exploits Ghana's phased regional rollout, combining a within-country pilot-phase difference-in-differences with mechanism, equity and scale-up analyses, this study offers, to our knowledge, the first within-country evaluation of the DFEP and caesarean section, together with a replicable template, adding new evidence to health financing policy in Ghana and to the wider literature on user fee reform in low-income settings.

## Conceptual framework

The conceptual framework draws on three established theoretical models to describe the pathways through which fee removal may influence caesarean section utilisation: Andersen's Behavioural Model of Health Services Use [26], Donabedian's Structure-Process-Outcome framework [27], and the Three Delays Framework [16]. Together, these models capture both the demand-side and supply-side determinants of CS access. The hypothesised causal sequence is as follows. The DFEP reduces financial barriers (a predisposing and enabling determinant in Andersen's model) by removing direct costs at the point of care. This demand-side stimulus is expected to increase the uptake of facility-based delivery and skilled birth attendance, which are essential preconditions for accessing CS. These intermediate outcomes inform the probability of CS utilisation, the primary outcome of interest. The translation of increased utilisation into improved CS access is, however, moderated by supply-side conditions such as facility readiness, theatre capacity, trained personnel, and logistics, which constitute

the structural dimension in Donabedian's framework. Equity is treated as a cross-cutting dimension, and the distributional impact of the policy is assessed across wealth, education, and geographic groups using the approach of Mills et al. [28]. These pathways are summarised in Fig. 1.

## Objectives, research questions, and hypotheses

The overall goal is a pre-registered, causally informed evaluation of Ghana's 2003 to 2007 Delivery Fee Exemption Policy in relation to caesarean section that integrates four complementary analyses of equal standing: the pilot-phase difference-in-differences, the mechanism (pathway) analysis, the equity (distributional) analysis, and the national scale-up analysis, together with a replicable template for DHS-based evaluation of maternal health financing reforms. Within this design, the pilot-phase difference-in-differences provides the causal estimate of the effect on caesarean section, interpreted as precision-bounded given the limited power of the four-cluster pilot (see Sample size and statistical power). The specific objectives are to estimate this effect and to determine whether it differed across household wealth quintiles, maternal education levels, and urban or rural residence, using birth-level GDHS data.

The study addresses four research questions:

- (1) Did the DFEP pilot phase (September 2003 to March 2005) significantly increase the probability of CS delivery among women in pilot regions compared with women in non-pilot regions?
- (2) What are the pathway effects of the DFEP on facility-based delivery and skilled birth attendance as potential mechanisms linking fee removal to CS utilisation?
- (3) Did the national rollout of the DFEP (April 2005 to June 2007) affect CS utilisation across all regions?
- (4) Did the effect of the DFEP on CS utilisation differ by household wealth quintile, maternal education, or urban or rural residence?

The study tests the following hypotheses.  $H_0$ : there is no significant difference in CS utilisation between pilot and non-pilot regions following the introduction of the DFEP pilot phase;  $H_1$ : the DFEP pilot phase significantly increased CS utilisation in pilot regions relative to non-pilot regions, after controlling for pre-existing regional differences and temporal trends. A second null hypothesis holds that there is no significant association between facility-based delivery and CS utilisation in the context of DFEP exposure. A third null hypothesis holds that the effect of the DFEP on CS utilisation does not differ significantly across household wealth quintiles, maternal education levels, or areas of residence.

## Methods

### Study design and policy context

This study will use a retrospective quasi-experimental difference-in-differences (DiD) design to estimate the causal effect of the DFEP pilot phase on CS utilisation. Quasi-experimental designs are the standard approach for evaluating health policy interventions in the absence of randomisation, particularly for social policies aimed at vulnerable populations, where withholding treatment for a control group would be ethically impermissible [20, 29]. The DiD estimator will compare changes in CS utilisation over time between pilot regions (treatment group) and non-pilot regions (comparison group), thereby controlling for time-invariant regional characteristics and national secular trends that might otherwise confound observed differences.

The phased implementation of the DFEP, with four regions receiving the policy in September 2003 and the remaining six in April 2005, generates the treatment variation required for the DiD design. The pilot phase is the primary period of analytical interest because it is the only phase during which a valid within-country comparison group (the non-pilot regions) exists. The national rollout phase (April 2005 to June 2007) will be analysed separately as a policy scale-up analysis and framed as descriptive rather than causal, since no untreated comparison group is available during that period. This two-stage approach is consistent with the recommendation that DiD designs with variation in treatment timing be analysed by exposure cohort, to avoid bias from heterogeneous treatment effects across exposure windows [30]. Figure 2 summarises the design, the analytic variables, and their interrelationships.

## Justification of study period

The analytical requirements of the DiD design drive the choice of GDHS 2003 and GDHS 2008. The 2003 GDHS, conducted before the DFEP was implemented, provides the pre-policy baseline. The 2008 GDHS, conducted after the policy ended, captures outcomes for births occurring during both the DFEP pilot phase (September 2003 to March 2005) and the national rollout phase (April 2005 to June 2007). More recent Ghana DHS waves (2014 and 2022) are not suitable because, by 2005, all regions were exposed to the policy, and by 2008 the DFEP had been replaced by a new maternal health financing programme integrated into the NHIS. The pairing of 2003 and 2008 is therefore the only one that captures both a clean pre-policy baseline and births occurring solely under the DFEP.

## Data sources and sample

The analysis will draw on the Individual Recode (IR) files from the 2003 and 2008 Ghana Demographic and Health Surveys. Both are nationally representative household surveys with a two-stage stratified cluster sampling design: enumeration areas (clusters) are first selected with probability proportional to size from a master sampling frame derived from the Ghana Population and Housing Census, and households within selected clusters are then drawn by systematic random sampling. All women aged 15 to 49 years who are permanent residents or who slept in the household the night before the interview are eligible for an individual interview. The 2003 GDHS collected data from approximately 6,251 households and 5,691 women; the 2008 GDHS from approximately 11,778 households and 4,916 women. Applying the birth-cohort restrictions described below will reduce the analytic sample well below these raw counts. Table 1 sets out the anticipated construction of the analytic sample and the expected cell sizes,

including the small pilot-phase exposed cell (approximately  $n = 351$ ), which is expected to be a first-order constraint on statistical precision and motivates the design-based inference described below.

## Sample weighting

Both GDHS datasets use complex sampling designs with stratification and clustering, which require survey-weighted analysis to produce nationally representative estimates and appropriate standard errors. Sample probability weights will be derived from the GDHS sampling weight variable V005, divided by 1,000,000 to normalise, and applied at all stages of analysis. Variable V021 defines the primary sampling unit (PSU), and variable V022 defines stratification. In Stata, the survey design will be declared as `svyset V021 [pweight=V005/1000000], strata (V022)`, and all subsequent estimation commands prefixed with `svy:`. This follows DHS analytical guidelines and established practice in DHS-based policy evaluation [29].

## Analytic sample and birth-cohort construction

Although GDHS maternal health indicators are conventionally described as referring to births in the five years before the survey, the analyses will be conducted at the birth level and timed using the DHS century month code (CMC) variable B3, which records the year and month of each birth. This is essential for the DiD design because classifying births by survey wave alone (Pre = 2003, Post = 2008) would conflate the DFEP pilot phase, the national rollout phase, and the post-DFEP period within the 2008 survey, yielding a biased and uninterpretable composite treatment effect. The birth-cohort windows are defined as follows.

Pre-policy sample (GDHS 2003): births from January 1998 to December 2002 (CMC 1177 to 1236) will be included; all births occurring in 2003 (CMC  $\geq 1237$ ) will be excluded. This ensures a clean baseline unaffected by the transitional implementation of the DFEP, which began in September 2003.

Policy-exposed sample (GDHS 2008): DFEP pilot-phase births from September 2003 to March 2005 (CMC 1245 to 1263) and national rollout births from April 2005 to June 2007 (CMC 1264 to 1290) will be included. All 2003 births before September (CMC  $< 1245$ ) will be excluded to avoid overlap with the pre-policy baseline, and all births from July 2007 onwards (CMC  $\geq 1291$ ) will be excluded to prevent contamination from post-DFEP maternal health financing reforms.

The unit of analysis is the most recent live birth reported by each woman, because the DHS individual maternal health module (M-series variables) collects detailed delivery information only for the most recent birth. This restriction ensures comparability across survey rounds and prevents artificial over-representation of women with multiple births in the analytic window. Variable definitions are pre-specified in this protocol and will be harmonised across the 2003 and 2008 GDHS IR files using the DHS Standard Recode Manual (Version V).

Table 1  
Construction of the analytic sample, GDHS 2003 and 2008.

| Restriction step                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                      | GDHS 2003 pre-policy (n) | GDHS 2008 policy-exposed (n) | Notes                        |
|---------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|--------------------------|------------------------------|------------------------------|
| All women 15–49 interviewed                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                           | 5,691                    | 4,916                        | IR file                      |
| With a dated most recent live birth                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                   | 3,992                    | 3,299                        | Most recent birth (M-series) |
| Most recent birth in pre-policy window (Jan 1998–Dec 2002)                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                            | 2,331                    | —                            | CMC 1177–1236                |
| Most recent birth in DFEP-exposed window (Sep 2003–Jun 2007)                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                          | —                        | 1,301                        | CMC 1245–1290                |
| of which pilot phase (Sep 2003–Mar 2005)                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                              | —                        | 351                          | CMC 1245–1263                |
| of which national rollout (Apr 2005–Jun 2007)                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                         | —                        | 950                          | CMC 1264–1290                |
| Primary DiD analytic sample (pre-policy + pilot phase)                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                                | 2,682                    |                              | Pooled 2003 + 2008 pilot     |
| <p><i>Note. CMC = century month code (DHS variable B3). Cell counts are anticipated figures based on published GDHS reports and the window definitions; final counts will be confirmed after data extraction. The analytic sample is expected to comprise approximately 881 (pilot) and 1,450 (comparison) pre-policy births and 130 (pilot) and 221 (comparison) pilot-phase births; because pilot-region caesarean rates are low before the policy, the treated pilot-phase cell is expected to contain only a small number of caesarean events (of the order of two to four), the binding constraint on statistical power.</i></p> |                          |                              |                              |

## Operational definitions and variables

Caesarean section (CS), the primary outcome, is delivery of the most recent live birth by surgical caesarean section (M17); it is interpreted as an indicator of access to emergency obstetric surgery.

Facility-based delivery (FBD; M15 codes 21–36) and skilled birth attendance (SBA; derived from M3A–M3F) are secondary pathway outcomes analysed descriptively.

The treatment and time indicators are Pilot (region of residence among Northern, Upper East, Upper West, or Central; V024) and Post (births during the DFEP pilot period).

Covariates are maternal age (V012), parity (V201), education (V106), household wealth quintile (V190), and urban or rural residence (V025).

Region fixed effects will be included. Table 2 lists all analytic variables, their GDHS source, coding, and role. Table 3 sets out the exact recoding rules and the derived variable names to be used in the analysis.

Table 2  
Analytic variables, GDHS 2003 and 2008.

| Variable                 | GDHS source | Type / role           | Coding                                                | Notes                                  |
|--------------------------|-------------|-----------------------|-------------------------------------------------------|----------------------------------------|
| Sample weight            | V005        | Survey design         | V005/1,000,000 = pweight                              | Applied via svyset                     |
| Primary sampling unit    | V021        | Survey design         | Cluster identifier                                    | SEs clustered at PSU level             |
| Strata                   | V022        | Survey design         | Regional stratum                                      | Used in svyset                         |
| Birth CMC                | B3          | Policy timing         | Century month code → year/month                       | Constructs Post and phase indicators   |
| Post (pilot)             | Derived     | Core DiD              | 1 = birth Sep 2003–Mar 2005; 0 = 1998–2002            | Main Post indicator                    |
| Pilot region             | V024        | Core DiD              | 1 = Northern/Upper East/Upper West/Central; 0 = other | Treatment indicator                    |
| DiD interaction          | Derived     | Core DiD              | Post × Pilot                                          | Primary estimand ( $\beta_3$ )         |
| Caesarean section (CS)   | M17         | Primary outcome       | 1 = CS; 0 = vaginal/other; 9 → missing                | Most recent live birth                 |
| Facility delivery        | M15         | Pathway (descriptive) | 1 = inrange(M15,21,36); 0 = home/other                | Not a causal mediator                  |
| Skilled birth attendance | M3A–M3F     | Pathway (descriptive) | 1 = M3A/M3B/M3C/M3F = 1; 0 otherwise                  | Derived variable                       |
| Maternal age             | V012        | Covariate             | Continuous (years)                                    | Non-linearity assessed                 |
| Parity                   | V201        | Covariate             | Continuous (children ever born)                       | Associated with CS need                |
| Education level          | V106        | Covariate / modifier  | 0 none;1 primary;2 secondary;3 higher                 | Also heterogeneity analysis            |
| Wealth quintile          | V190        | Covariate / modifier  | 1–5 (poorest–richest)                                 | Also equity analysis                   |
| Residence type           | V025        | Covariate / modifier  | 1 = urban; 2 = rural                                  | Also geographic heterogeneity          |
| Survey year              | V007        | Wave identifier       | 2003 or 2008                                          | Pools datasets; not the Post indicator |

*Note. All variables harmonised across GDHS 2003 and 2008 IR files using DHS Standard Recode Manual (Version V). PSU = primary sampling unit.*

Table 3

Operational recoding and derived analytic variables (GDHS 2003 and 2008 Individual Recode files). Derived names correspond to the variables to be used in the analysis.

| #  | Variable (role)                     | GDHS variable | Recoding rule                                           | Derived name |
|----|-------------------------------------|---------------|---------------------------------------------------------|--------------|
| 1  | Caesarean section (primary outcome) | M17           | Caesarean = 1; other mode = 0                           | cs           |
| 2  | Facility-based delivery (mediator)  | M15           | Codes 21–36 = 1; home/other = 0                         | fbd          |
| 3  | Skilled birth attendance (mediator) | M3A–M3F       | Doctor/midwife/nurse = 1; else = 0                      | sba          |
| 4  | Pilot region (treatment)            | V024          | Northern, Upper East, Upper West, Central = 1; else = 0 | pilot        |
| 5  | Post period (time)                  | B3 (CMC)      | Birth Sep 2003–Mar 2005 = 1; Jan 1998–Dec 2002 = 0      | post         |
| 6  | Policy exposure (DiD term)          | derived       | post × pilot                                            | did          |
| 7  | Maternal age                        | V012          | Years (continuous)                                      | age          |
| 8  | Parity                              | V201          | Number of births (continuous)                           | parity       |
| 9  | Maternal education                  | V106          | None / primary / secondary or higher                    | educ         |
| 10 | Household wealth                    | V190          | Quintiles 1–5                                           | wealth       |
| 11 | Residence                           | V025          | Urban = 1; rural = 0                                    | urban        |
| 12 | Survey weight                       | V005          | V005 / 1,000,000                                        | wt           |
| 13 | Primary sampling unit               | V021          | Cluster identifier                                      | psu          |
| 14 | Stratification                      | V022          | Sample strata                                           | strata       |

## Difference-in-differences specification

The primary causal analysis estimates the effect of the DFEP pilot using the model  $CS_{it} = \beta_0 + \beta_1 \cdot Post_{it} + \beta_2 \cdot Pilot_{it} + \beta_3 \cdot (Post_{it} \times Pilot_{it}) + \beta_4 \cdot X_{it} + \gamma_r + \varepsilon_{it}$ , where  $CS_{it}$  is a binary indicator equal to 1 if woman  $i$  in region  $r$  with a birth at time  $t$  delivered by caesarean section (M17);  $Post_{it}$  equals 1 for births during the DFEP pilot period and 0 for pre-policy births;  $Pilot_{it}$  equals 1 for the four pilot regions and 0 otherwise;  $\beta_3$  is the difference-in-differences estimator and the primary quantity of interest;  $X_{it}$  is the covariate vector; and  $\gamma_r$  are region fixed effects. The primary estimand is the effect of pilot exposure on the unconditional probability of caesarean section among all births; any facility-conditional quantity will be treated as a separate descriptive analysis, since conditioning on facility delivery, which is itself affected by the policy, would introduce post-treatment selection. The difference-in-differences estimator is summarised schematically in Table 4. Because the policy can raise caesarean use both by increasing facility births

and by improving access to indicated surgery among facility births, an increase in the unconditional rate should not, on its own, be read as improved access to medically indicated caesarean section; the facility-conditional and mechanism analyses are intended to help separate these channels.

Because CS is a binary outcome, the primary specification will use a Linear Probability Model (LPM), which yields the DiD coefficient directly as an absolute percentage-point change and is unbiased in expectation for causal inference with experimental variation. Because CS is a rare outcome, fitted probabilities will be checked for out-of-range values, and logistic regression will be estimated as an alternative functional form with adjusted risk differences reported [20]. Figure 3 illustrates this identification logic. The linear probability model is the primary specification for the main difference-in-differences because it returns the estimand directly as an absolute percentage-point change; logistic regression, reported as average adjusted risk differences, is the functional-form reconciliation and is treated as primary for the triple-difference equity models, where thin subgroup cells make the linear model unstable. Anticipated event counts per subgroup cell will be reported so that the estimability of the three-way interaction can be judged.

Table 4

*Difference-in-differences estimator (schematic). Cells A–D are mean caesarean-section rates; the difference-in-differences estimate is the lower-right cell ( $\beta_3$  in the estimating equation).*

| <b>Pilot regions (Northern, Upper East, Upper West, Central)</b> | <b>Pre-policy (Jan 1998–Dec 2002)</b> | <b>Pilot-exposed (Sep 2003–Mar 2005)</b> | <b>Difference (Post – Pre)</b> |
|------------------------------------------------------------------|---------------------------------------|------------------------------------------|--------------------------------|
|                                                                  | <b>A</b>                              | <b>B</b>                                 | <b>B – A</b>                   |
| <b>Non-pilot regions (six)</b>                                   | C                                     | D                                        | D – C                          |
| <b>Difference (Pilot – Non-pilot)</b>                            | A – C                                 | B – D                                    | DiD = (B – A) – (D – C)        |

## Inference with a small number of clusters

Treatment is assigned at the regional level, and the design contains only 10 regional clusters, of which 4 are treated. Conventional cluster-robust standard errors are unreliable in this setting. Inference on the DiD coefficient will therefore rely primarily on a wild cluster bootstrap, clustered at the regional level, using Webb weights in place of Rademacher weights because ten clusters admit too few distinct Rademacher draws for reliable inference [32, 33]. In addition, exact randomisation (permutation) inference will be conducted by reassigning pilot status to every possible combination of four of the ten regions, yielding an exact distribution of the DiD estimate under the null against which the observed estimate is compared. These design-based procedures, rather than model-based standard errors, will be the primary basis for inference. Exact permutation inference is treated as the primary basis for the headline test, and the wild cluster bootstrap as a complementary check.

## Sample size and statistical power

The pilot phase has only four treated regions and a small number of exposed births, so statistical power is a central design consideration. Applying the birth-window definitions to the GDHS Individual Recode files, the analytic sample is expected to comprise about 881 pilot and 1,450 comparison births in the pre-policy window, and 130 pilot and 221 comparison births in the pilot-phase window. Caesarean rates in the pilot regions were low before the policy, on the order of 1–2%, consistent with published GDHS regional estimates. The treated pilot-phase cell will therefore contain only a few caesarean events. The smallest detectable effect was examined using a Monte Carlo simulation that reproduces this structure: ten regional clusters with four treated, the expected cell sizes above, baseline caesarean rates of about 1 to 2% in the pilot regions and 5 to 6% in the comparison regions, and a common secular rise of about one percentage point. Inference was by exact permutation across the ten regions; the reference set of 210 possible assignments ( $C(10,4)$ ) gives a smallest attainable two-sided p-value of about 0.01. Under these assumptions, the design has approximately 80% power only for a very large effect, of the order of ten to twelve percentage points, and less than 25% power for effects of four percentage points or smaller. This limitation follows from a rare outcome measured across four small, deprived clusters. It is stated here in advance rather than discovered after analysis. The pilot-phase difference-in-differences can detect only large effects; it cannot distinguish the small effects reported in earlier cross-country studies [21, 23] from no effect. The pilot-phase estimate is therefore treated as precision-bounded and reported with its design-based confidence interval whether or not it reaches significance, and greater weight is placed on the pre-specified equity, mechanism and national-rollout analyses and on the value of the design as a transparent and replicable template. Final cell sizes and the minimum detectable effect will be reconfirmed on the realised analytic sample.

## **National rollout analysis**

The national rollout phase (April 2005 to June 2007), during which the DFEP was operational in all ten regions, will be analysed separately, using only births from the GDHS 2008. Because no within-country untreated comparison group exists during this period, these estimates will be interpreted as policy scale-up effects rather than causal contrasts, using a before-and-after within-country design, and framed as supporting descriptive evidence for the pilot-phase findings. Because this before-and-after comparison lacks a contemporaneous untreated group, it is reported as descriptive scale-up evidence only and is not interpreted causally.

## **Mechanism analysis**

To understand the pathways through which the DFEP may have affected CS, two intermediate outcomes will be analysed using the same DiD specification: facility-based delivery (M15) and skilled birth attendance (M3 series). These analyses will be descriptive rather than causal mediation analyses, given the limitations of DHS data for formal mediation inference. Because a CS can only occur if a woman delivers in a facility with a skilled provider, an observed change in CS without a corresponding change in facility delivery or SBA would point to supply-side rather than financial-access pathways.

## **Heterogeneity and equity analysis**

Heterogeneity will be assessed with a triple-difference model that interacts the DiD estimator with each equity indicator (household wealth quintile V190, education V106, and residence V025):  $CS_{ir} = \beta_0 + \beta_1 \cdot Post_t + \beta_2 \cdot Pilot_r + \beta_3 \cdot (Post_t \times Pilot_r) + \beta_4 \cdot Subgroup_i + \beta_5 \cdot (Post_t \times Subgroup_i) + \beta_6 \cdot (Pilot_r \times Subgroup_i) + \beta_7 \cdot (Post_t \times Pilot_r \times Subgroup_i) + \beta_8 \cdot X_{ir} + \gamma_r + \varepsilon_{ir}$ . The coefficient  $\beta_7$  on the three-way interaction is the parameter of interest; the full set of lower-order terms is included because a valid triple-difference requires them. This retains the full analytic sample and maximises power rather than estimating separate models within subgroups, and stratified estimates will also be presented descriptively. These analyses will follow the equity framework of Mills et al and methods used in comparable analyses of wealth-related inequality in reproductive health care in Ghana [28, 31].

## Measurement error and misclassification

Three sources of measurement error bear on the estimates, and each is expected to work against, rather than in favour of, a positive finding. Mode of delivery is self-reported: caesarean status (M17) is generally well recalled. Still, any residual misclassification is unlikely to differ systematically between pilot and non-pilot regions, so it would not bias the difference-in-differences estimate toward the null. Exposure timing depends on the birth date recorded in the century month code, and heaping or recall error in reported birth months could misassign a small number of births across the pre-policy and pilot windows; the pre-specified alternative-window analysis gauges how far the estimate moves under different boundaries. Treatment is assigned by region of usual residence rather than region of delivery, so women who cross regional boundaries to give birth are misclassified with respect to policy exposure. Because three of the four pilot regions (Northern, Upper East and Upper West) border other regions and neighbouring countries, cross-boundary delivery is plausible and need not be symmetric, so the direction of any resulting bias cannot be signed in advance. We will quantify the extent of out-of-region delivery where DHS variables permit and, as a pre-specified sensitivity analysis, re-estimate the model excluding districts adjacent to regional boundaries. The facility-only restriction and the exclusion of the Central Region provide further checks on the primary estimate's sensitivity to these concerns.

## Robustness and sensitivity analyses

Several pre-specified robustness checks will assess the stability of the primary estimates:

- The analytic sample will be restricted to facility births only, to assess whether the DFEP effect on CS is driven by increased facility attendance or by a change in CS conditional on facility arrival.
- Logistic regression models will be estimated as an alternative functional form, with predicted probabilities reported as adjusted risk differences.
- The Central Region will be excluded from the pilot classification and the main model re-estimated, to test sensitivity to heterogeneity in pilot-region health system capacity.
- The analytic sample will be re-estimated excluding districts adjacent to regional boundaries, to assess sensitivity to misclassification of treatment status among women who delivered outside their region of residence.

- A wild-cluster bootstrap with Webb weights, clustered at the regional level, will yield inference that is robust to the small number of clusters [32, 33].
- Alternative birth-window definitions will be examined by extending the pre-policy window to 1996–2002 and the policy window to include all DFEP births, excluding those from June 2007.
- Exact randomisation (permutation) inference will reassign pilot status across all combinations of four of the ten regions to obtain the null distribution of the DiD estimate.

No post hoc sensitivity analyses will be conducted based on the primary results.

## Parallel trends assumption: justification and assessment

The validity of the DiD design rests on the parallel trends assumption: absent the DFEP, CS trends would have evolved similarly in pilot and non-pilot regions. The single pre-policy DHS wave in the main window precludes formal statistical testing, but several arguments support plausibility. First, before September 2003, no fee-exemption policy for delivery care existed in any region; fees were charged uniformly nationwide, removing a major source of differential pre-trend, and there is no documentation of region-specific obstetric reforms in the pre-policy period. Second, pilot regions were selected for high deprivation and low access to skilled care, not for pre-existing improvements in CS uptake, making endogenous placement (Ashenfelter’s dip) unlikely. Third, baseline comparisons in the GDHS 2003 will be used to assess whether, despite differences in absolute CS rates across regions, the key CS determinants (maternal age, parity, antenatal care-seeking) are comparable between pilot and non-pilot regions. Fourth, any residual violation from unobserved regional divergence is argued to bias the DiD coefficient toward zero, because the more-deprived pilot regions faced greater structural barriers that financial access alone would not remove.

Because only one pre-policy survey falls within the main analytic window, two empirical assessments will supplement these arguments. First, a placebo pre-trend test will add the 1998 GDHS and compare the change in the outcome and pathway measures between the 1998 and 2003 surveys, both of which predate the policy; under parallel trends, pilot and comparison regions should show no differential change across these two pre-policy periods. Second, robustness to residual violations will be assessed with the relative-magnitudes sensitivity analysis of Rambachan and Roth, which bounds the treatment effect under the restriction that any post-policy violation is no larger than a specified multiple of the pre-policy violation estimated from the 1998-to-2003 comparison [34]. This design follows guidance for difference-in-differences studies with a single pre-intervention period [30, 35]. Although only one pre-policy survey falls within the main analytic window, the 2003 GDHS records births back to 1998, so annual caesarean-section rates for 1998 to 2002 can be constructed for the pilot and comparison region groups and inspected for parallelism before the policy; under the identifying assumption, these region-group series should not diverge across the pre-policy years. This graphical, event-study-style pre-trend provides multiple pre-policy slopes against which to assess the assumption and supplements the single pre-post placebo. Because the 1998 GDHS does not contain a comparable asset-based wealth index, that wave is used only for the outcome and pathway pre-trend and not for any equity placebo. Because

these annual pre-policy points are derived from retrospectively reported births within a single survey, they share survey-level recall and are indicative rather than a formal statistical test.

## **Statistical methods and software**

Descriptive analyses will report means and proportions with 95% confidence intervals, stratified by pilot or non-pilot status and survey wave, and a baseline characteristics table will compare socio-demographic characteristics across pilot and non-pilot regions before the DFEP. A graph of annual CS trends by regional group and birth year will provide an informal pre-trend assessment. Variable management, data cleaning, and all regression analyses will be carried out in the latest version of Stata, with survey estimation commands prefixed by `svy:`. Model code will be maintained to reproduce all tables and figures from the raw DHS IR files. Statistical significance will be assessed at the 5% level, but conclusions will emphasise effect sizes, confidence intervals, and plausibility within the conceptual framework rather than binary p-value thresholds.

## **Pre-specification and analytical discipline**

Primary and secondary outcomes, covariates, subgroup analyses, and robustness checks are fully pre-specified in this protocol and registered before data analysis. No changes to the analytic plan will be made based on observed results, thereby guarding against outcome switching and selective reporting. The pilot-phase difference-in-differences on caesarean section is the single primary test; the mechanism, equity (triple-difference) and national-rollout analyses are secondary and exploratory, are not adjusted for multiplicity, and are interpreted with corresponding caution.

## **Data management, security, and reproducibility**

DHS data are used under a formal data agreement with the DHS Program (ICF International). Data will be stored on password-protected institutional servers accessible only to approved project team members, with raw, cleaned, and output files held in separate version-controlled directories. Analysis scripts will reproduce all outputs from the raw inputs. No individual-level data will be published; only aggregated statistics, model coefficients, and predicted probabilities will be reported. Scripts and non-identifiable outputs will be archived for at least ten years after publication. The phased implementation and monitoring and evaluation plan is summarised in Table 5.

Table 5  
Protocol implementation, monitoring and evaluation.

| Phase I — Data acquisition & preparation                                                                                                                                                                                | Phase II — Analysis & inference                                                                                                                                                                                               | Phase III — Robustness, reporting & dissemination                                                                                                                                                                     |
|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|-----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| DHS data-access agreement (ICF); download GDHS 2003 & 2008 (and 1998 for the placebo); construct birth cohorts via century month code (B3); harmonise variables (Recode Manual v5); declare the survey design (svyset). | Baseline balance and descriptive trends; survey-weighted linear-probability difference-in-differences with region fixed effects; mechanism analysis (facility delivery, skilled attendance); triple-difference equity models. | Placebo pre-trend (1998 GDHS); Rambachan–Roth bounds; Central-region exclusion; alternative birth windows; logistic-model check; STROBE-guided reporting; policy brief; thesis submission; peer-reviewed publication. |
| Range and consistency checks on derived variables; document data provenance and versioning.                                                                                                                             | Small-cluster inference: wild cluster bootstrap (Webb weights) and exact permutation across the ten regions.                                                                                                                  | Independent re-run of analysis scripts to reproduce every table and figure from the raw data.                                                                                                                         |

*Monitoring and evaluation applies across all three phases: adherence to the pre-registered OSF analysis plan (no outcome-switching), supervisory review by the academic supervisor, version-controlled analysis scripts, and data handling under the DHS Program terms of use.*

## Study status and timeline

This is a study protocol; no data analysis has been undertaken at the time of submission. The Ghana Demographic and Health Surveys of 2003 and 2008 were designed and conducted by the DHS Program, and the datasets are pre-existing, publicly available secondary data that were not collected for this study. At the time of submission, the analytic sample has not yet been extracted, and no statistical analysis has been performed; accordingly, no results are reported. The complete analytical plan, including the primary outcome, secondary outcomes, covariates, subgroup analyses, and robustness checks, was pre-specified and registered on the Open Science Framework before any analysis (registration DOI: 10.17605/OSF.IO/FR75Z). The study will proceed strictly in accordance with this registered plan, and any departure from it will be reported transparently. This is an analysis pre-registration on the Open Science Framework rather than a clinical-trial registration, as the study is observational.

## Discussion

### Principal anticipated contributions

This protocol sets out a pre-registered, multi-strand evaluation of Ghana's 2003 Delivery Fee Exemption Policy in relation to caesarean section. It combines a within-country pilot-phase difference-in-differences with mechanism, equity and national scale-up analyses, and a replicable template for DHS-based policy evaluation. By exploiting the phased regional rollout, the pilot-phase difference-in-differences holds national-level confounders fixed and addresses a persistent evidence gap: whether universal fee

exemption translates into caesarean access. The accompanying strands address the pathways, the distribution of any effect, and the national scale-up. Because the pilot phase is powered only for large effects, no single strand is treated as definitive; the contribution lies in the integrated, transparent evidence they provide together. The systematic evidence reviewed by Taadi et al found consistent improvements in facility delivery and skilled attendance after fee removal but inconsistent effects on CS, suggesting that financial access may be necessary but not sufficient for CS uptake [19]. A significant positive effect on the unconditional caesarean rate would be consistent with improved access to facility-based delivery and potentially to emergency obstetric surgery. However, distinguishing a genuine gain in access to indicated caesarean section from a compositional shift toward facility births would require facility-conditional and mechanism analyses. A null or negligible effect would instead point to binding supply-side constraints (theatre availability, surgeon supply, anaesthesia capacity). It would be consistent with the null caesarean findings of the closest cross-country difference-in-differences evaluations [21–23], which this better-identified within-country design can test.

## **Anticipated analytic outputs**

The study will produce several outputs. The first is a causally identified estimate of the effect of the DFEP pilot phase on CS utilisation, expressed as an adjusted percentage-point change with design-based confidence intervals. The analysis will also show whether the effect differed by household wealth, maternal education, and residence, and will provide descriptive evidence on the facility-delivery and skilled-attendance pathways. Finally, the study will yield a replicable quasi-experimental template for DHS-based evaluation of maternal health financing policies, and a policy brief for decision-makers.

## **Equity considerations**

The equity analysis will determine whether the distributional effects of the DFEP were progressive or regressive. Universal fee exemption policies do not automatically benefit the poorest households, particularly for resource-intensive services that require geographic access and facility readiness in addition to financial access [28]. If the DFEP raised CS rates disproportionately among wealthier or urban women, this would suggest that fee removal without complementary supply-side investment widens rather than narrows socioeconomic inequity in obstetric care, with direct implications for the design of Ghana's NHIS and similar programmes across sub-Saharan Africa.

## **Methodological contribution**

The DHS-based DiD approach used here, which includes birth-level cohort construction, region fixed effects, survey-weighted estimation, design-based inference for few clusters, and a pre-registered analysis plan, advances beyond the descriptive and non-experimental approaches that have characterised previous evaluations of Ghana's fee policies, and beyond the cross-country designs that could not isolate the DFEP. Findings will be interpreted in the context of Ghana's health system, including the later integration of maternal health benefits into the NHIS and the sustainability problems associated with delayed provider reimbursement [14, 29]. This transparent, reproducible template is pre-registered,

design-based and equity-oriented, and applies to other maternal health financing reforms that can be evaluated with DHS data.

## Strengths and limitations

The design is appropriate for estimating average treatment effects at the regional level. Still, it cannot identify individual-level treatment receipt, since DFEP exposure is defined at the regional birth-cohort level. The most important limitations are the small number of treated clusters (four of ten regions) and the small treated pilot-phase sample. About 130 births fall in the four pilot regions, within a pilot-phase window of roughly 351 births, and the caesarean rate is low. Together, these leave the design powered only for large effects, of the order of ten to twelve percentage points. Inference will therefore use the wild cluster bootstrap and exact permutation rather than model-based standard errors, and the estimate will be reported as precision-bounded. The study's contribution therefore rests as much on its pre-registered transparency, its distributional equity analysis, and its value as a replicable template as on a definitive causal test of the average effect. A synthetic-control comparison for the pilot regions was considered but not adopted, given the same small-cluster constraint. A single pre-policy wave in the main window precludes a conventional event-study, and will be addressed through the 1998-GDHS placebo pre-trend test and the Rambachan–Roth sensitivity bounds. DHS records the region of usual residence rather than the region of delivery, introducing measurement error in the treatment indicator for women who crossed regional boundaries, which will be examined in a sensitivity analysis. Finally, the study examines CS utilisation, not the quality, appropriateness, or outcomes of CS; this matters because Ghana's national CS rate has since risen to 20.1% by 2022, exceeding the WHO upper threshold, with wealth, education, and urban residence significant predictors of CS use nationally [9, 10].

## Conclusions

This protocol sets out a rigorous, pre-registered, within-country evaluation of Ghana's Delivery Fee Exemption Policy and caesarean section, integrating a pilot-phase difference-in-differences analysis with mechanism, equity, and national scale-up analyses. The pilot-phase estimate is powered only for large effects and is therefore reported as precision-bounded rather than as a definitive test; the study's contribution lies in the coherence and transparency of these complementary analyses and in a replicable template for evaluating maternal health financing reforms with DHS data, including whether their benefits reach the women who need them most.

## Dissemination

Findings will be disseminated through peer-reviewed publication and will follow the STROBE reporting guidelines, adapted for quasi-experimental studies. Results will be shared with the Ghana Health Service, the Ministry of Health Ghana, and academic and policy stakeholders through conference presentation and an accessible policy brief. As part of the PhD thesis submission at the University of Central

Nicaragua, a complete report of findings will be submitted to the thesis supervisory committee. Analytical outputs, including do-files and log files, will be archived and made available for independent verification on reasonable request.

## Abbreviations

ANC  
antenatal care  
CI  
confidence interval  
CMC  
century month code  
CS  
caesarean section  
DFEP  
Delivery Fee Exemption Policy  
DHS  
Demographic and Health Survey  
DiD  
difference-in-differences  
FBD  
facility-based delivery  
GDHS  
Ghana Demographic and Health Survey  
IR  
Individual Recode  
LMIC  
low- and middle-income country  
LPM  
Linear Probability Model  
MMR  
maternal mortality ratio  
NHIS  
National Health Insurance Scheme  
OSF  
Open Science Framework  
PSU  
primary sampling unit  
SBA  
skilled birth attendance

SDG

Sustainable Development Goal

STROBE

Strengthening the Reporting of Observational Studies in Epidemiology

WHO

World Health Organization

## Declarations

Ethics approval and consent to participate

This study is a secondary analysis of de-identified data from the 2003 and 2008 Ghana Demographic and Health Surveys (GDHS). The survey procedures and questionnaires were reviewed and approved by the ICF Institutional Review Board (IRB), which ensures compliance with the United States Department of Health and Human Services regulations for the protection of human subjects (45 CFR 46), and by the national ethics review authority in Ghana, which ensures compliance with national laws and norms. Informed consent was obtained from all respondents by the DHS Program at the time of data collection. Documentation of this ethical review has been obtained from the DHS Program and is provided as a supplementary file. The authors accessed the data under a formal data-use agreement with the DHS Program (ICF International); all datasets are fully de-identified, with displaced geographic coordinates, and no contact with participants occurred. As this is a secondary analysis of existing, publicly available, de-identified data, no additional ethical approval was required, and the study was conducted in accordance with the principles of the Declaration of Helsinki.

## Consent for publication

Not applicable.

## Availability of data and materials

The GDHS Individual Recode datasets are available from the DHS Program (<https://dhsprogram.com>) upon registration and approval of a data access request. Analytic do-files and non-identifiable outputs will be made available by the corresponding author on reasonable request. The power-simulation code is provided as Additional file 1 and deposited with the OSF registration.

## Competing interests

The authors declare that they have no competing interests.

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This study received no specific grant from any funding agency in the public, commercial, or not-for-profit sectors.

# Authors' contributions

PKT conceived the study, designed the analytical plan, and drafted the protocol. POA contributed to the study design and critically revised the protocol for methodological and intellectual content. Both authors read and approved the final manuscript.

# Use of large language models / generative AI

Generative artificial intelligence tools were used to assist with formatting, reference styling, and language editing during manuscript preparation. The authors are responsible for the study design, the analytical plan, and all intellectual content, and have reviewed and verified the final text.

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## Figures

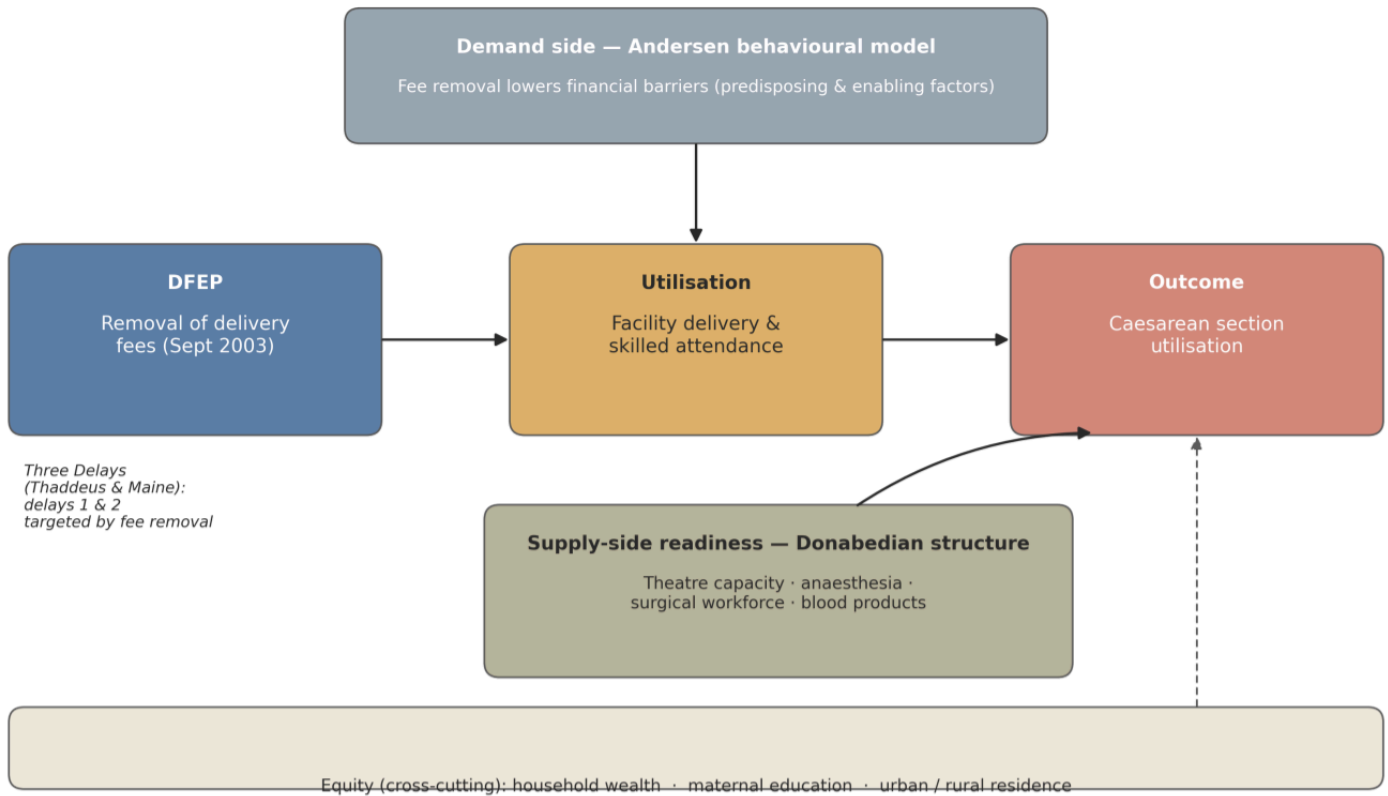


**Figure 1**

Conceptual framework showing the hypothesised pathways from delivery-fee removal to caesarean section utilisation. Fee removal (demand side, Andersen) raises facility delivery and skilled attendance, which are preconditions for caesarean section; supply-side readiness (Donabedian) moderates the translation of utilisation into caesarean access; equity dimensions cut across all pathways.

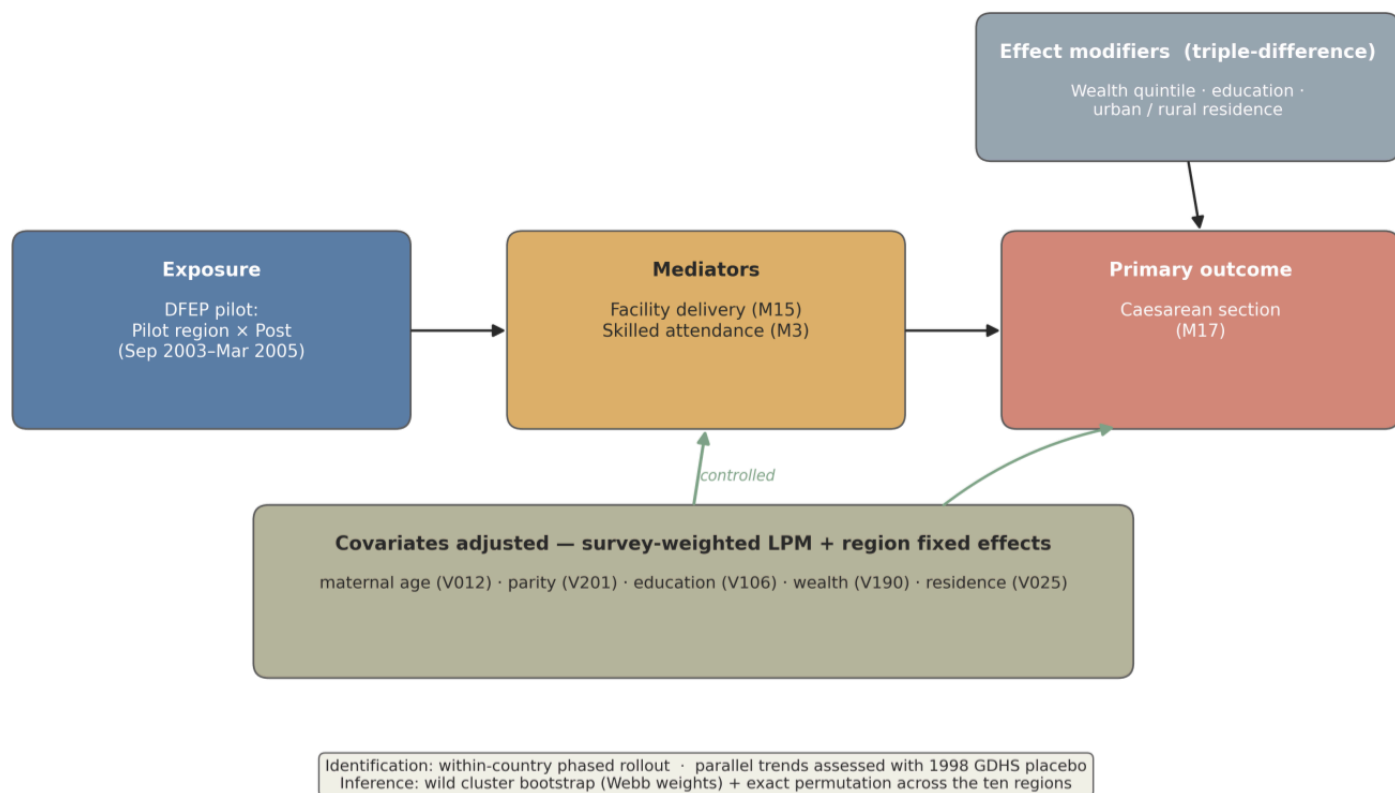


**Figure 2**

Study design showing related variables and confounders. The difference-in-differences exposure (pilot region × post period) acts through facility-based delivery and skilled attendance on the primary outcome, caesarean section (M17). Individual covariates are adjusted in a survey-weighted linear probability model with region fixed effects, and wealth, education and residence enter the triple-difference as effect modifiers.

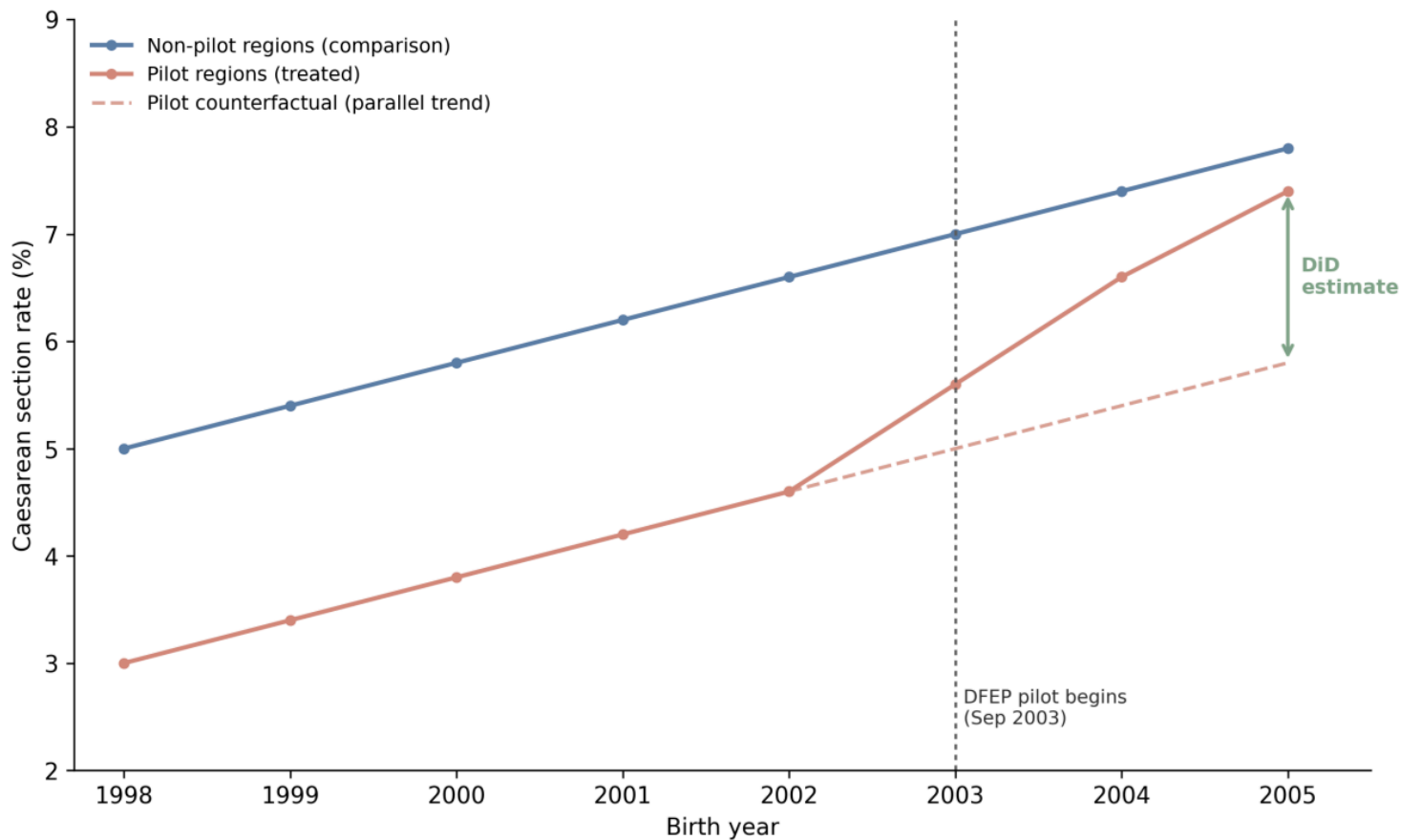


**Figure 3**

Illustrative difference-in-differences logic. Pilot and non-pilot regions follow parallel caesarean-section trends before the policy; the difference-in-differences estimate is the gap between the observed pilot trend and its counterfactual after the pilot begins. The figure depicts the identification strategy and does not present study results.

## Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- [DHS5EthicsApproval.pdf](#)