Free Health Care for Children and Child Health: Evidence from Senegal*

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May 5, 2026

Preliminary and Incomplete.

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Abstract

This paper evaluates Senegal's national free child care policy, introduced in October 2013 to eliminate user fees for children under five in public health facilities. Using nationally representative Service Provision Assessment (SPA) and Demographic and Health Survey (DHS) data, I first document policy implementation in practice. Nearly all public facilities report offering free care, yet only about 44 percent of eligible children are reported as covered in household surveys, with large regional disparities. I then estimate the impact of the policy on financial protection and treatment provision during sick-child visits using a difference-in-differences design comparing outcomes in public and non-public facilities before and after the reform. Out-of-pocket expenditures decline by 2,023 FCFA, the probability that visit costs exceed the daily international poverty line falls by 26.4 percentage points, and the likelihood that caregivers receive take-home medication rises by 26.5 percentage points. The unchanged mix of presenting symptoms suggests that lower costs and higher treatment provision reflect improved access to care for children with similar underlying conditions, rather than a shift in the types of cases presenting for care. Finally, I examine mortality effects by constructing a child-month panel from DHS birth histories and estimating difference-in-differences models that exploit variation in baseline geographic access to public facilities. The monthly probability of death falls by 0.34 percentage points for children living within 5 kilometers of a public facility relative to lower-access areas (95% CI: $-0.0055, -0.0013$), a decline of roughly 31 percent relative to the pre-policy mean. Mortality reductions are suggestively larger among children from poorer households, rural areas, and families where mothers have no primary education. These results suggest that fee removal improved financial protection and treatment provision and translated into gains in child survival where children lived in closer proximity to public health facilities. Persistent gaps between statutory eligibility and reported coverage point to caregiver awareness and geographic access as the central constraints on the effective reach of the policy.

*I am grateful to Jessica Cohen, David Cutler, and Kevin Croke for their guidance and feedback as members of my thesis committee, and to Khadidja Ngom for helpful comments on earlier drafts. Any errors are my own.

This research was supported by the Social Equity and Health Equity (SEHE) Stipend from the Harvard Kennedy School.

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1 Introduction

Child mortality has declined substantially across Sub-Saharan Africa over the last two decades, yet the region continues to experience the highest under-five mortality rates globally (UNICEF, 2025). In Senegal, under-five mortality fell from 129 deaths per 1,000 live births in 2000 to 39 in 2023 (UN IGME, 2023), but remains above the Sustainable Development Goal (SDG) target of 25 by 2030 (United Nations, 2015). Preventable infectious conditions such as diarrheal disease, malaria, pneumonia, and complications related to malnutrition remain the leading causes of child death (World Health Organization, 2023; UNICEF, 2025). Many of these deaths could be averted with timely access to treatment: care for these conditions is both clinically effective and low-cost, yet financial barriers, geographic distance, and uneven service quality continue to limit care-seeking among the poorest households (Najjuuko et al., 2025; Avelino et al., 2025; Simmons et al., 2021; Kruk et al., 2017; Leslie et al., 2017).

Financial barriers are widely recognized as a major constraint on healthcare utilization and an important contributor to preventable child mortality (Ridde and Morestin, 2011; Dupas, 2011; Simmons et al., 2021; Najjuuko et al., 2025). In response, many low- and middle-income countries have eliminated user fees for maternal and child health services as part of broader universal health coverage (UHC) strategies (Mathauer et al., 2017; Hanson et al., 2022). Removing fees can expand access to essential healthcare by relaxing liquidity constraints and reducing financial risk for households. Whether this improves welfare, however, depends on whether induced utilization is concentrated in high-value or low-value care. Fee removal raises welfare when it brings children with undertreated illness into care; it generates smaller gains, or may even reduce welfare, when additional utilization yields limited health returns or strains supply-constrained systems (Dupas and Miguel, 2017; Baicker et al., 2015; Hendren and Sprung-Keyser, 2020).

Recent systematic evidence finds that eliminating fees for child health services is generally associated with improvements in healthcare access, health outcomes, and financial protection and, in some contexts, greater equity (Dehnavi et al., 2025). However, this literature remains limited in several important respects. Drawing on 38 studies across 16 countries, Dehnavi et al. (2025) show that the existing evidence is concentrated in a small number of settings, most notably Burkina Faso, and that only 16 studies meet high standards of causal identification. Relatively few examine mortality outcomes directly, and the existing evidence rarely speaks to the mechanisms connecting fee removal to health gains: in particular, how implementation gaps, geographic access, and household awareness shape effective coverage.

Existing studies also show substantial heterogeneity in impacts across contexts. Studying Thailand's 2001 universal coverage reform, Gruber et al. (2014) find that expanding access

to publicly financed healthcare substantially reduced infant mortality and eliminated the gap between poorer and richer provinces. Evidence from health insurance expansions likewise suggests that the benefits of removing financial barriers depend on local conditions and baseline access to care. For example, Conti and Ginja (2023) study the expansion of Mexico's Seguro Popular program and find limited overall mortality effects but a 10 percent reduction in infant mortality in poorer municipalities, where baseline health needs and barriers to care were greatest. Both studies, however, examine large-scale insurance expansions in middle-income countries. Evidence from low-income Sub-Saharan African settings, where supply constraints, geographic barriers, and information frictions are more severe, remains sparse. This paper provides causal evidence from one such setting and shows that the health returns to fee removal depend critically on whether households can translate formal eligibility into effective access to care.

This paper evaluates Senegal's national free child care policy for children under five, introduced on October 1, 2013. The policy eliminated user fees for essential child health services delivered in public facilities and represents a central pillar of Senegal's universal health coverage strategy, covering children under five, who constitute roughly 12% of the total population in 2023 (ANSD, 2024). Despite its scale, the policy has received no prior causal evaluation.

The analysis proceeds in three steps. First, I document stylized facts on policy implementation using nationally representative Service Provision Assessment (SPA) and Demographic and Health Survey (DHS) data spanning 2008 to 2019. Facility survey data show that participation is widespread among public facilities, with nearly all reporting that they offer free care for children under five, while private and NGO facilities rarely do so. At the same time, household survey data indicate that reported coverage among eligible children is substantially lower and varies sharply across regions. These patterns point to the role of information frictions and geographic disparities in limiting effective coverage, even when formal facility participation is near-universal.

Second, I estimate the causal impact of the policy on financial protection and treatment provision during sick-child visits using SPA facility exit interviews. The empirical strategy compares outcomes in public and non-public facilities before and after the policy introduction in a difference-in-differences framework. Because the policy applies only to services delivered in the public sector, non-public facilities provide a natural comparison group. This design allows me to test whether the removal of user fees reduced out-of-pocket expenditures and changed the care children received during illness visits.

Third, I examine whether the policy translated into improvements in child survival using DHS birth histories. I construct a child-month panel that tracks mortality risk over the

first five years of life and estimate difference-in-differences models that compare changes in mortality after the policy between areas with higher and lower baseline geographic access to public health facilities. The main specification defines high access as residing within 5 kilometers of the nearest public facility. This strategy tests whether the survival gains from fee removal were larger where children faced lower geographic barriers to care.

The results show that the policy substantially improved financial protection and treatment provision during sick-child visits. Out-of-pocket expenditures declined by 2,023 FCFA in public facilities relative to non-public facilities after the reform, and the probability that visit costs exceed the daily international poverty line fell by 26.4 percentage points. The policy is also associated with a 26.5 percentage point increase in the likelihood that caregivers received take-home medication during the visit. These changes occurred without meaningful shifts in the case mix of presenting symptoms, suggesting that the reform primarily changed the cost and treatment of illness episodes rather than the composition of cases arriving at facilities.

The mortality analysis also points to meaningful health gains. In the main specification, the policy reduced the monthly probability of death by 0.34 percentage points for children living within 5 kilometers of a public facility relative to children in lower-access areas (95% CI: $-0.0055, -0.0013$). Given a pre-policy mortality hazard of 1.09 percent per month in low-access clusters, this implies a decline of about 31 percent. A similar estimate emerges when access is defined using distance to the nearest public health center or hospital. Subgroup analyses further suggest that mortality gains were at least as large, and in some cases larger, among children in poorer households, rural areas, and families in which mothers had no primary schooling, although confidence intervals overlap across groups.

This paper contributes to three strands of literature. First, a large literature examines the effects of user fee removal on healthcare utilization and health outcomes in low- and middle-income countries (Dupas, 2011; Dupas and Miguel, 2017; Powell-Jackson et al., 2014; Gruber et al., 2014; Tanaka, 2014; Dehnavi et al., 2025). Within this literature, evidence from Sub-Saharan Africa remains concentrated in Burkina Faso, and causal evidence on mortality effects is rare. This paper contributes by providing the first causal evaluation of a national free child care program in Senegal, documenting impacts on financial protection, treatment provision, and child survival. Second, a related literature studies the welfare effects of health subsidies in settings characterized by demand- and supply-side frictions (Dupas and Miguel, 2017; Baicker et al., 2015; Sautmann et al., 2025). Within this literature, I contribute evidence that fee removal primarily expanded access to clinically indicated care, with the case mix of presenting symptoms remaining stable after the reform, consistent with underuse rather than overuse as the binding constraint. Third, a growing literature examines deter-

minants of child survival and the returns to early childhood health interventions (Almond and Currie, 2011; Conti and Ginja, 2023). Within this literature, I show that the survival gains from fee removal depend on geographic access to care, with effects concentrated among children in higher-access areas and suggestively larger among children from disadvantaged households, pointing to access and information as central constraints on the effectiveness of free-care policies.

The remainder of the paper proceeds as follows. Section 2 describes the setting. Section 4 describes the data and summary statistics. Section 5 outlines the empirical strategy. Section 6 presents the results. Section 7 interprets the findings and discusses policy implications. Section 8 concludes.

2 Institutional background

In October 2013 the Government of Senegal introduced a policy providing free health care for children under five. The policy was implemented as part of the national strategy for Universal Health Coverage (Couverture Maladie Universelle, CMU), which aims to expand financial protection and access to health services for vulnerable populations (Ministère de la Santé et de l'Action Sociale, 2013). The policy targets children aged 0–59 months. Its stated objective was to reduce child mortality, which stood at 72 deaths per 1,000 live births at the time of introduction (Santé Tropicale, 2013).

The benefit package covered services across all levels of the public health system. At health posts, the policy covered consultations and vaccinations. At health centers, coverage extended to consultations, vaccinations, and hospitalization for up to seven days. At hospitals, the policy covered emergency consultations and cases referred from lower-level facilities. In January 2014 the program expanded to include generic medicines.

The initiative was initially placed under the Ministry of Health (MOH). In 2015, the government established the National Agency for Universal Health Coverage (ANACMU) as a separate entity to oversee implementation (Daff et al., 2020). The Agency functions as the insurer, while the MOH, which manages public providers, acts as the service provider. Providers are reimbursed through fixed payments per episode of care: approximately 1,400 CFA francs (about $2.30) at health posts, 4,500 CFA francs (about $7.50) at health centers, and 2,000 CFA francs (about $3.30) for emergency consultations or referred cases at hospitals (Agence de la Couverture Maladie Universelle, 2018). Access to benefits requires documentation of the child's age and identity, including a child health booklet, birth certificate, vaccination card, or other civil registry document (Senegal Services, 2023).

The policy was introduced in a health system with limited insurance coverage and high

financial barriers to care. Prior to the launch of UHC policies in 2013, formal insurance was restricted to roughly 20% of the population employed in the formal sector or as civil servants (Magazine de l'Afrique, 2015). Community-based health insurance schemes existed but reached only a small share of the population. As a result, health care was largely financed through out-of-pocket payments, which accounted for approximately 58% of total health expenditures in 2013 (Ministère de la Santé et de l'Action Sociale, 2015). Typical costs in public facilities included about 2,000 CFA francs (about $3.30) for a pediatric consultation and approximately 3,000 CFA francs per day (about $5.00) for hospitalization (World Health Organization Africa, 2013). With nearly 50% of the population living below the poverty line, even these modest fees represented a substantial financial burden for many households (Magazine de l'Afrique, 2015).

Geographic barriers compound these financial constraints. In 2013, the average distance to the nearest health center was 23 km (Khaliloulah, 2017). As a result, transportation costs to a facility could equal or exceed the cost of care itself. Free care therefore offers limited benefit to households that cannot reach a public facility.

The program expanded rapidly after its introduction. During the first three years, it financed approximately three million episodes of sick-child care (Medang, 2016). Between 2015 and 2023, the government allocated an estimated $31.7 million to the initiative, representing about 21% of national UHC expenditures (Faye, 2023).[1] Children under five constitute roughly 12% of the population, making this one of the largest subsidy programs in the country (ANSD, 2024).

Empirical evidence on the effects of Senegal's free child care policy remains limited. Faye (2022) examines inequalities in the use of free health services for children under five using survey data. The study finds that utilization of free services remains uneven across socioeconomic groups, suggesting that fee removal alone does not eliminate disparities in access to care. Pouye et al. (2023) examine the relationship between the policy and child undernutrition and healthcare utilization using household survey data. The analysis documents improvements in service utilization and some reduction in inequality in access to care between 2012 and 2017, though the empirical approach does not support causal inference. Neither study provides causal estimates of the policy's effects on financial protection or child health outcomes.

This paper contributes to the literature by providing causal evidence on the impact of Senegal's free child care policy. Because the policy was implemented nationwide, identification relies on variation in geographic access to public health facilities. Before describing the

[1] These figures reflect financing through the national health insurance agency and exclude broader Ministry of Health spending on infrastructure, personnel, and facility operations.

data and empirical strategy in detail, I outline a conceptual framework that formalizes the trade-offs involved in such a program.

3 Conceptual framework: welfare effects of untargeted free care

User fee removal can affect welfare through several channels. Following the literature on health insurance and public subsidies (Dupas and Miguel, 2017; Hendren and Sprung-Keyser, 2020; Baicker et al., 2015), the welfare effects of untargeted free care depend on whether the policy expands use of services whose marginal social benefits exceed their marginal social costs.

In low-income settings, user fees may generate underuse of high-value care. Households often face liquidity constraints, limited information about treatment benefits, and uncertainty about service quality. These frictions can lead caregivers to delay or forgo clinically effective treatment (Dupas, 2011; Kremer and Glennerster, 2011). When underuse is the binding constraint, lowering prices expands use of services that generate large health returns.

At the same time, standard models of moral hazard predict that lower prices may also increase use of services with limited clinical value. More recent work emphasizes that the welfare consequences of increased utilization depend on the health productivity of care, not utilization alone (Baicker et al., 2015). Fee removal generates smaller welfare gains when additional utilization is concentrated in low-value services or when service quality is weak.

Several features of child health in low-income countries suggest that underuse of effective care is the more relevant concern. Many leading causes of child mortality, including diarrheal disease, pneumonia, malaria, and neonatal complications, are highly responsive to timely access to low-cost interventions (World Health Organization, 2023). Experimental evidence shows that subsidies for childhood illness treatment increase care-seeking primarily when treatment is clinically indicated, with relatively small increases in unnecessary care (Dupas and Miguel, 2017; Sautmann et al., 2025). Evidence from Ghana finds that removing user fees increases utilization and improves health outcomes among children with higher baseline health risk, with limited responses among healthier children (Powell-Jackson et al., 2014). Supply-side constraints such as medicine shortages and reimbursement delays may further limit excessive utilization, though they also reduce the effectiveness of coverage expansion.

In the context of Senegal's policy, fee removal is most likely to improve welfare when it expands access to effective treatment for common childhood illnesses. The magnitude of these gains depends on whether households can translate formal eligibility into effective access to care. Limited awareness of eligibility, geographic distance to facilities, and variation in service readiness may therefore limit the extent to which free care improves health outcomes.

The next section describes the data and presents descriptive evidence on the gap between statutory eligibility and effective coverage.

4 Data and descriptive analysis

4.1 Data sources

The analysis draws on two nationally representative survey systems implemented under the Demographic and Health Surveys (DHS) Program: the Demographic and Health Surveys (DHS) and the Service Provision Assessments (SPA). These sources provide complementary information on household characteristics, child health outcomes, healthcare utilization, and facility-level service delivery.

DHS, 2008–2019. The Senegal DHS provides repeated cross-sections on fertility, mortality, and household socioeconomic conditions (ANSD and ICF, 2020a). The surveys use a stratified two-stage cluster sampling design in which census enumeration areas are selected as primary sampling units and households are then randomly sampled within each cluster.

I use three files. The children's recode (KR) and household member recode (PR) files are used to characterize caregiver-reported coverage of the free child care policy and household socioeconomic characteristics. The birth recode (BR) file contains complete birth histories for all children ever born to interviewed women, including month and year of birth, survival status, and age at death. I use these histories to construct the child-month mortality panel described in Section 5.2. The analytical sample is restricted to children born between 2008 and 2019 to maintain comparability across cohorts and to exclude births potentially affected by the COVID-19 pandemic.

SPA, 2012–2019. The SPA surveys provide nationally representative assessments of health facility readiness and service delivery (ANSD and ICF, 2020b). I rely primarily on sick-child exit interviews with caregivers seeking care for children under five. These interviews record services provided, medications dispensed, and out-of-pocket payments associated with the visit. I also use facility audit data to classify providers as public or private. This distinction is central to the empirical strategy: the free care policy applied to services delivered in public facilities but not to those delivered in private or NGO facilities.

4.2 Sample summary statistics

Table 1 reports pooled descriptive statistics for the SPA and DHS analytic samples.

SPA sample. The SPA exit interview sample includes 7,474 sick-child visits between 2012 and 2019. The average child is 1.73 years old, and 47 percent are female. Most visits occur in

the public sector: 87 percent of children are seen at public facilities, with the remainder split between NGO or faith-based facilities (8 percent) and private facilities (4 percent). Among visit types, 69 percent take place at health posts, 21 percent at health centers, and 10 percent at hospitals. Fifty-six percent of visits occur in urban areas. Across all facilities, 88 percent report offering free care for children under five, though this variable is only observed for the 3,696 visits with linked facility audit data. Thirty-four percent of visits involve some out-of-pocket expenditure, with average spending of 590 FCFA and a standard deviation of 1,820 FCFA, reflecting a highly skewed distribution.

DHS persons and children samples. The DHS persons sample covers approximately 552,000 individuals and is used to document reported coverage under the free child care policy. Overall, 16 percent report any form of health insurance. Among the full sample, only 8 percent report coverage specifically under the free child care policy. The DHS children's sample includes 87,538 children under five, with an average age of 1.97 years. Thirty percent live in urban areas and 28 percent of mothers have completed primary school. Among children with utilization and insurance data, 55 percent visited a health facility in the past year, 49 percent report coverage under some form of health insurance, and 45 percent report coverage under the free child care policy specifically.

DHS births sample. The birth histories provide information on 310,005 births between 2008 and 2019. Twelve percent of births with recorded weight are classified as low birthweight, and 3 percent are twin births. Thirty-one percent of births occur in urban areas and 82 percent in areas classified as low malaria risk. Forty-seven percent of children belong to households in the bottom two wealth quintiles, and 22 percent of mothers have completed primary school. These patterns reflect the considerable heterogeneity in baseline health risk and socioeconomic conditions across the sample.

Table 1. Summary statistics for SPA facility-linked and DHS analytic samples.

	Mean	SD	N
SPA Sample			
Child age (years)	1.73	1.35	7,271
Female child	0.47	0.50	7,469
Any OOPE incurred	0.34	0.47	7,471
OOPE (thousands of FCFA)	0.59	1.82	7,441
Urban residence	0.56	0.50	7,474
Facility type: health post	0.69	0.46	7,474
Facility type: health center	0.21	0.41	7,474
Facility type: hospital	0.10	0.30	7,474
Facility ownership: public	0.87	0.33	7,474
Facility ownership: private	0.04	0.21	7,474
Facility ownership: NGO/faith-based	0.08	0.27	7,474
Facility offers free child care	0.88	0.32	3,696
DHS Persons' Sample			
Age (years)	22.28	19.59	551,910
Female child	0.53	0.50	552,294
Age under 5	0.17	0.38	551,910
Urban residence	0.36	0.48	552,316
Covered by any health insurance	0.16	0.36	163,564
Covered by free child care	0.08	0.26	163,564
DHS Children's Sample			
Age (years)	1.97	1.42	87,538
Female child	0.49	0.50	87,538
Urban residence	0.30	0.46	87,538
Mother completed primary school	0.28	0.45	87,533
Visited a health facility in the past year	0.55	0.50	23,301
Covered by any health insurance	0.49	0.50	23,007
Covered by free child care policy	0.45	0.50	23,007
DHS Births Sample			
Age (years)	10.20	7.22	310,005
Female child	0.49	0.50	310,005
Birth order	3.35	2.24	310,005
Twin birth	0.03	0.17	310,005
Low birthweight	0.13	0.34	42,130
Born after October 2013	0.10	0.30	310,005
Child alive	0.88	0.33	310,005
Age at death (years)	1.94	3.31	37,651
Urban residence	0.31	0.46	310,005
Lives in low malaria zone	0.82	0.39	310,005
Wealth index: Q1–Q2	0.47	0.50	310,005
Mother's current age	35.30	7.84	310,005
Mother's age at child's birth	25.58	6.44	310,005
Mother completed primary school	0.22	0.42	309,997

Notes: The table reports means, standard deviations, and sample sizes for variables used in the analysis. The SPA sample is drawn from sick-child exit interviews conducted in health facilities between 2012 and 2019. The DHS persons and children samples are used to characterize insurance coverage and household socioeconomic conditions. The DHS births sample is constructed from complete birth histories and forms the basis for the child mortality analysis. Sample sizes vary across variables due to item nonresponse and differences in the subsamples for which specific variables are collected. *Abbreviations*: OOPE = out-of-pocket expenditure. FCFA = West African CFA franc. LBW = low birthweight.

4.3 Stylized facts on the free child care policy

This section documents patterns in the supply-side availability of free child care and demand-side awareness of the policy.

4.3.1 Facts from SPA facility and exit interview data

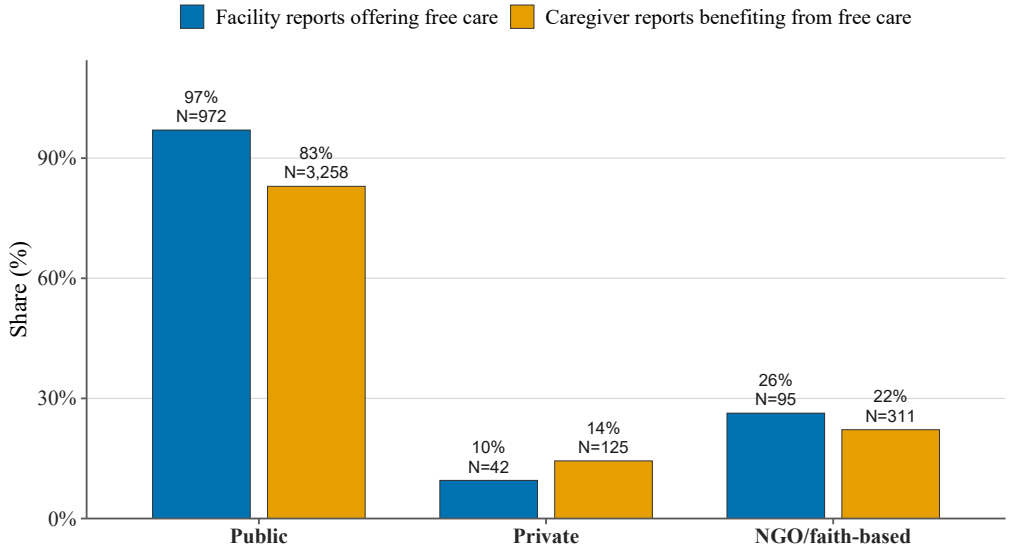
Fact 1: Public facilities are far more likely to report offering free child care than private or NGO facilities

Figure 1 compares facility reports of offering free child care with caregiver reports of benefiting from free care during a sick-child visit, by facility management authority. Facility reports are drawn from the SPA facility module, available starting in 2016. Caregiver reports are drawn from SPA sick-child exit interviews.

The two measures tell a consistent story. Among public facilities, 97 percent report offering free child care, and 83 percent of caregivers visiting these facilities report benefiting from free care during the visit. In contrast, only 12 percent of private facilities and 26 percent of NGO or faith-based facilities report offering free child care, and caregiver reports in these facilities are similarly low (14 and 22 percent, respectively). The policy therefore operates almost entirely through the public sector.

The two measures need not match exactly. Facility reports capture whether the facility offers free child care services in general, while caregiver reports reflect whether a specific visit was provided free of charge. Some services during a visit may fall outside the policy package, and caregivers may not always be aware that a service was covered. The broad alignment between the two measures nonetheless suggests that facility reports of policy participation are credible.

Figure 1. Offer of free child care by facility management authority



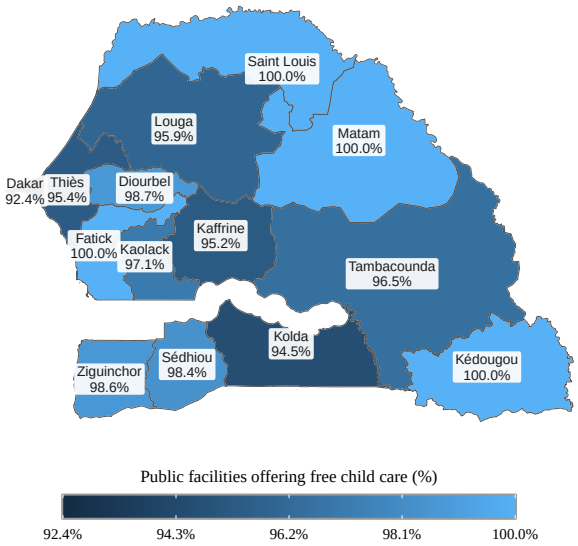
Notes: The figure compares facility reports of offering free child care with caregiver reports of benefiting from free care during sick-child visits, by facility management authority. Facility reports are based on the SPA facility module (available from 2016) and indicate whether the facility offers free child care services. Caregiver reports are based on SPA sick-child exit interviews and indicate whether the caregiver reported benefiting from free care during the visit. Sample sizes are reported above each bar.

Fact 2: Public facility participation is near-universal across all regions

Figure 2 shows the share of public facilities reporting that they offer free child care, by region.

Reported availability exceeds 92 percent in every region. The lowest rate is in Dakar (92.4 percent), and five regions—Fatick, Saint Louis, Matam, Kédougou, and Ziguinchor—report 100 percent participation. There is little regional disparity in facility-level adoption. The policy appears to have been widely implemented across the public health system.

Figure 2. Share of public facilities offering free child care, by region



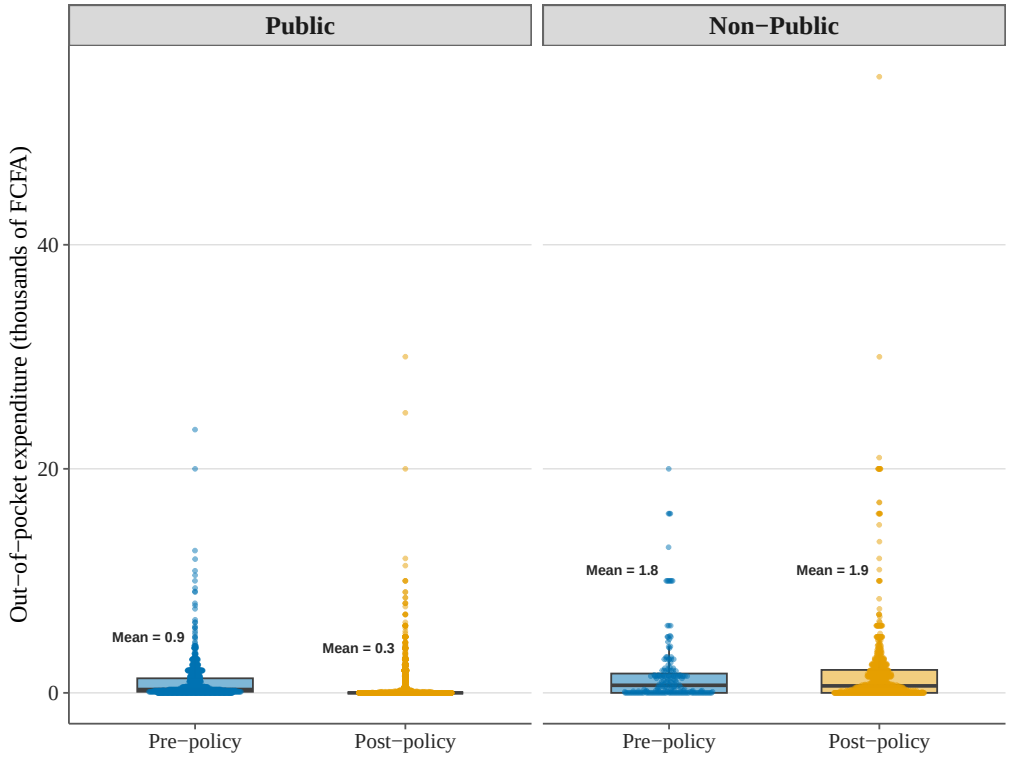
Notes: The figure shows the share of public health facilities reporting that they offer free child care services, by region. The sample is restricted to public facilities. Reports are based on the SPA facility module. Percentages and facility counts are reported above each bar.

Fact 3: Out-of-pocket expenditures in public facilities shift markedly toward zero after the policy

Figure 3 shows the distribution of out-of-pocket expenditures (OOPE) for sick-child visits from SPA exit interviews, by facility management authority and by whether the visit occurred before or after October 2013.

In public facilities, the distribution shifts markedly toward zero after the reform. Mean OOPE falls from approximately 900 FCFA before the policy to 300 FCFA afterward, and a large share of post-policy observations are concentrated at zero. In non-public facilities, the distribution remains largely stable: mean OOPE is approximately 1,800 FCFA before the reform and 1,900 FCFA afterward. The contrast across sectors is consistent with the policy operating primarily through the public health system, and provides the basis for the difference-in-differences strategy described in the next section.

Figure 3. Distribution of out-of-pocket expenditures for sick-child visits, by facility management authority and period



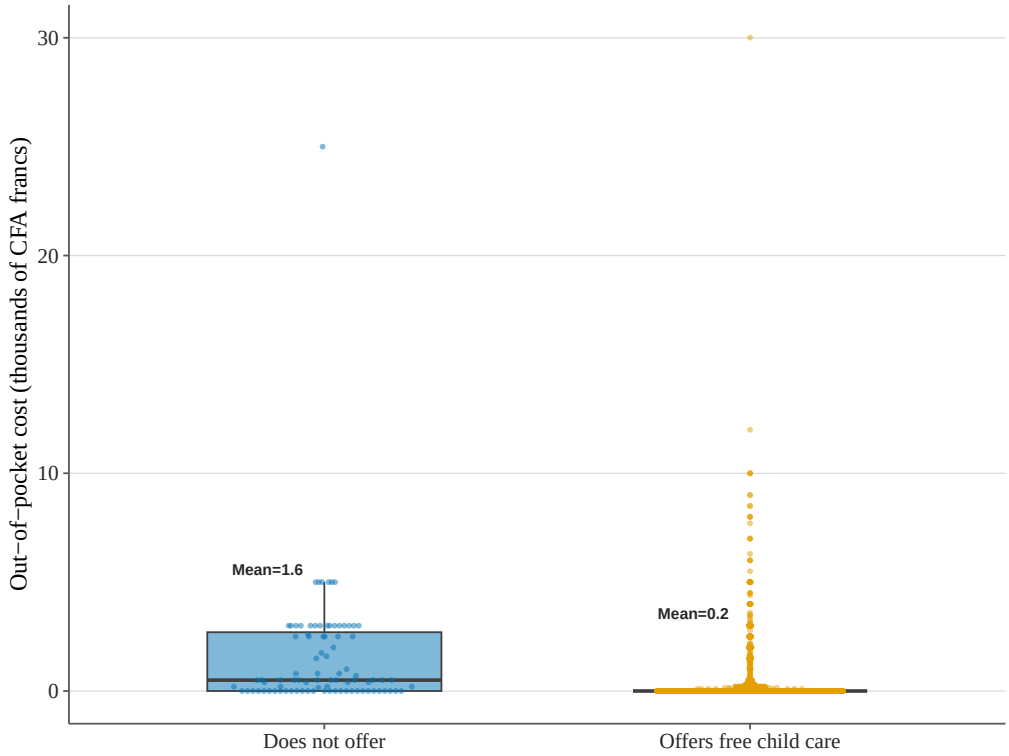
Notes: The figure displays out-of-pocket expenditures (thousands of FCFA) for sick-child visits reported in SPA exit interviews, grouped by facility management authority (public versus non-public) and by whether the visit occurred before or after the October 2013 policy. Each point corresponds to a visit. Boxes indicate the interquartile range, and annotated values report mean expenditures for each group.

Fact 4: Facility reports of policy participation correspond closely to caregiver payment patterns

Figure 4 compares the distribution of OOPE at public facilities that report offering free child care with those that do not.

Among public facilities reporting free care, OOPE is heavily concentrated at zero, with a mean of approximately 250 FCFA. Among those not reporting free care, expenditures are more dispersed and the mean is approximately 1,560 FCFA. This correspondence between facility self-reports and observed payment patterns suggests that facilities accurately report their participation in the program.

Figure 4. Distribution of out-of-pocket expenditures by free child care offer status (public facilities)



Notes: The figure displays out-of-pocket expenditures (thousands of FCFA) for sick-child visits at public health facilities, grouped by whether the facility reports offering free child care. Each point represents a visit. Boxes indicate the interquartile range, and annotated values report mean expenditures for each group.

4.3.2 Facts from DHS data

Fact 5: Household reports of coverage follow the age eligibility rule but remain far from universal

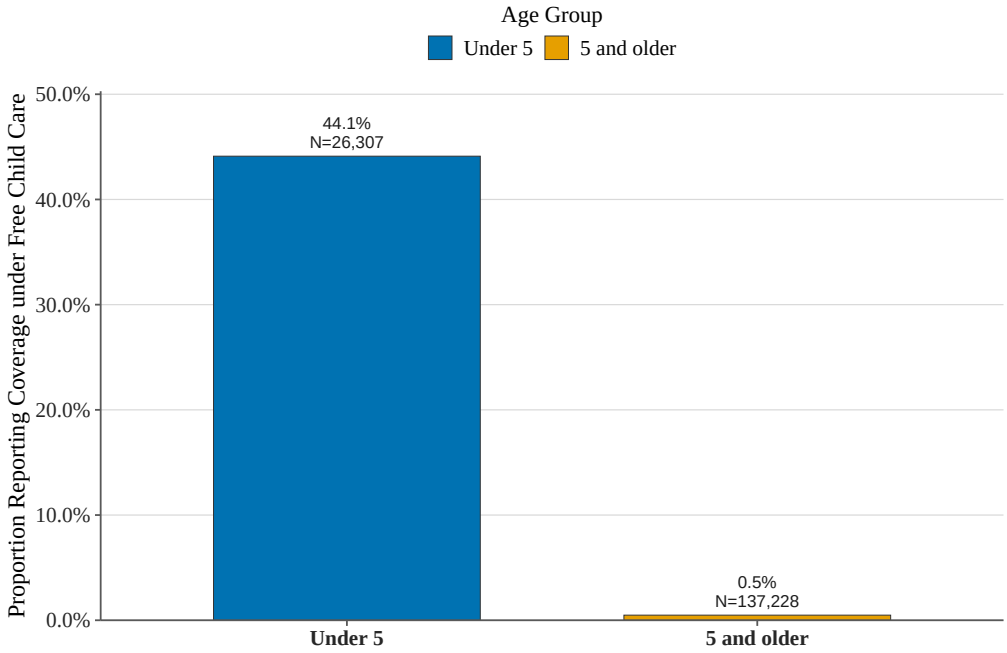
Figure 5 shows reported coverage under the free child care policy in DHS household surveys, separately for children under five and individuals aged five or older.

Reported coverage closely follows the policy's age eligibility cutoff. Among children under five, 44 percent of caregivers report that the child is covered by the program. Among individuals aged five or older, the rate is essentially zero (0.5 percent). This pattern confirms that reported coverage reflects knowledge of the policy rather than general awareness of any insurance scheme.

At the same time, coverage among eligible children is far from universal. The 44 percent reported in household surveys stands in sharp contrast to the near-universal facility participation documented in the SPA data and the high share of caregivers reporting free care once

they reach a public facility. This gap suggests that many caregivers are unaware of their child's eligibility prior to seeking care, and may only learn of it upon arrival at a facility.

Figure 5. Reported coverage under the free child care policy, by age group



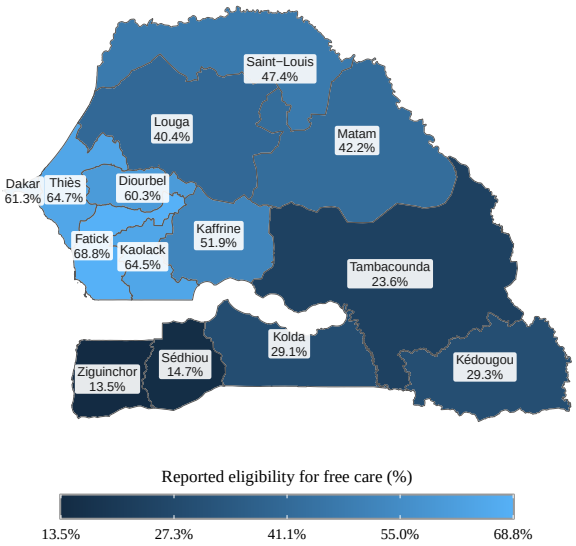
Notes: The figure shows the share of individuals reporting coverage under the free child care policy, based on the KR and PR modules of the Senegal DHS. Bars are shown separately for children under five and individuals aged five or older. Sample sizes are reported above each bar.

Fact 6: Reported coverage among eligible children varies sharply across regions

Figure 6 maps caregiver-reported coverage among children under five across Senegal's regions.

The geographic pattern contrasts sharply with the near-uniform facility participation documented in Fact 2. Reported household coverage ranges from 13.5 percent in Ziguinchor and 14.7 percent in Sédhiou to 68.8 percent in Fatick and 64.7 percent in Thiès. The regions with the lowest reported coverage—Ziguinchor, Sédhiou, Kolda, Kédougou, and Tambacounda—are concentrated in the south and southeast of the country, where malaria incidence is highest and socioeconomic conditions are generally weakest. These areas also tend to have lower caregiver education levels, lower household wealth, and more limited geographic access to facilities. The pattern suggests that awareness of eligibility, rather than formal availability of free care, is a central constraint on effective coverage.

Figure 6. Reported free-care coverage among children under five, by region



Notes: The figure shows the share of caregivers reporting that their child under five is covered by the free child care policy, by region, based on the DHS KR and PR modules. Shading reflects the proportion of eligible children whose caregivers report coverage.

These six facts document a clear gap between supply-side implementation and demand-side awareness. Public facilities nearly universally report offering free child care, and out-of-pocket payments fall sharply in these facilities after the reform. Yet fewer than half of eligible children's caregivers report knowing that their child is covered, and awareness varies substantially across regions. This gap between formal eligibility and effective coverage motivates the empirical strategy in the next section, which estimates the causal effects of the policy on financial protection, treatment provision, and child mortality.

5 Empirical strategy

This study evaluates the impact of Senegal's October 2013 free child care policy using two difference-in-differences designs. The first exploits variation in facility ownership to estimate effects on financial protection and treatment provision. The second exploits spatial variation in geographic access to public facilities to estimate effects on child mortality.

5.1 Financial protection and treatment provision

The free care policy applied only to services delivered in the public health system. This generated a differential change in the cost of care between public and non-public facilities after October 2013. I exploit this institutional feature in a difference-in-differences design

that compares outcomes for children seen at public facilities to those seen at private facilities, before and after the policy.

The estimating equation is:

$$Y_{ift} = \gamma \, \text{Public}_f + \beta \, (\text{Public}_f \times \text{Post}_t) + \delta_t + \varepsilon_{ift}$$

where Y_{ift} denotes an outcome for child i seen at facility type f in year t. The indicator Public_f equals one if the facility belongs to the public health system, and Post_t equals one for observations after October 2013. The coefficient γ captures the time-invariant level difference between public and non-public facilities. The coefficient β captures the differential change in outcomes at public facilities after the policy, which is the parameter of interest. Visit year fixed effects δ_t account for common temporal shocks. Standard errors are clustered at the facility level.

The analysis uses SPA exit interviews from sick-child visits and examines three sets of outcomes. First, I measure out-of-pocket expenditures (OOPE) for the visit, expressed in thousands of FCFA. I also construct an indicator equal to one if reported OOPE exceeds the applicable daily international poverty line threshold. This threshold corresponds to the World Bank's extreme poverty line: \$1.90 per person per day (2011 PPP) during 2012–2016 and \$2.15 per person per day (2017 PPP) from 2017 onward (World Bank, 2022). After converting to FCFA using year-specific exchange rates, the threshold ranges from approximately 1,000 FCFA per day in 2012–2013 to about 1,225 FCFA per day in 2020. This indicator captures whether a visit imposes a potentially burdensome cost on the household.[2]

Second, I examine whether the case mix of presenting symptoms changed after the policy. I analyze caregiver-reported symptoms recorded during the exit interview, including fever in the last two days, cough or difficulty breathing, vomiting, and watery stools in the last two days. These outcomes capture the conditions that prompted the visit and allow me to assess whether the composition of cases presenting for care shifted following the reform.

Third, I examine whether the provider dispensed take-home medications to the caregiver during the visit, as a measure of treatment provision.

Private-for-profit facilities serve as the primary comparison group because they are expected to continue charging for services throughout the study period. Non-public providers such as NGO or faith-based facilities could in principle also serve as controls. However, descriptive evidence suggests that many of these facilities already provided meaningful sub-

[2] A common approach in the health economics literature is to measure financial burden using catastrophic health expenditure indicators, which require information on both health spending and household income or consumption. These data are not available in the SPA exit interviews. I therefore use the daily poverty-line threshold as a benchmark for visits that may represent a substantial financial burden.

sidized care prior to the policy, and their pricing may have evolved in response to the broader UHC environment. Private-for-profit facilities have stronger incentives to maintain user fees and therefore provide a more stable counterfactual. As a robustness check, I re-estimate the main specifications using an expanded comparison group that includes all non-public providers.

The identifying assumption is that, absent the policy, outcomes in public and private facilities would have followed parallel trends. Following the recommendation in Baker et al. (2025) to begin with unconditional parallel trends before introducing covariates, the baseline specification includes no individual-level controls. Covariates would be added if conditional parallel trends were more plausible than unconditional parallel trends; in practice, the event-study estimates presented in Section 6 support the unconditional version of the assumption. A potential threat is that contemporaneous supply-side changes, such as increased resource allocation to public facilities, could confound the estimates. To assess this, I estimate event-study specifications that examine pre-policy outcome trends. Because the SPA surveys begin in 2012, the pre-policy window is short. Bins are defined by interview date relative to October 2013, and some early bins contain few observations, limiting precision in those periods. Given the small SPA sample, I do not estimate heterogeneous treatment effects for these outcomes.

5.2 Mortality effects

5.2.1 Construction of the child-month mortality panel

I construct a pseudo-panel at the child-month level using DHS birth history data. The DHS collects complete birth histories for women aged 15–49, recording the month and year of each birth and, where applicable, the child's age at death in months. I transform these histories into a child-month panel covering the first 59 months of life. Each child contributes one observation per month lived until death or censoring at age 59 months or at the survey date. A binary indicator records whether the child dies in a given month. This structure links mortality risk directly to the timing of the October 2013 policy.

If fee removal improved child survival, the effects should be larger where children faced lower geographic barriers to reaching care. I therefore estimate mortality effects by exploiting spatial variation in baseline geographic access to public health facilities.

5.2.2 Measuring geographic access

The ideal measure of baseline geographic access would draw on a complete historical facility list with precise geocoordinates for all public facilities operating before October 2013, linked

to each child's residence during the first five years of life. Neither is fully observed in practice. Senegal does not have a historical master facility list with complete geocoordinates for the pre-reform period, and the DHS does not record children's residential histories.

I therefore construct a proxy for baseline geographic access by combining a nationally assembled facility database with DHS cluster coordinates. I use the public-sector facility database compiled by Maina et al. (2019), which draws on Senegal's 2011 national health facility census and reflects the public facility network up to approximately 2013. The database includes 1,347 public facilities: 1,231 health posts (91.4 percent), 87 health centers (6.5 percent), and 29 hospitals (2.2 percent). For the subset of facilities missing coordinates in the Maina et al. (2019) database, I supplement with the consolidated facility list assembled by Gueye et al. (2024), which integrates multiple secondary sources and Ministry of Health records. Of the 91 facilities missing coordinates, I recover geocoordinates for 65, leaving 26 without coordinates.

I link the facility data to DHS cluster coordinates and compute the distance from each cluster to the nearest public facility. This is my primary access measure. Because 91 percent of facilities in the network are health posts, this measure primarily captures access to frontline primary care. I also construct an alternative measure equal to the distance to the nearest public health center or hospital, which captures access to facilities equipped to provide more advanced curative and referral care.

Table 2 reports summary statistics for both measures. The average distance to the nearest public facility is 4.08 kilometers (SD: 4.23). Using the main 5-kilometer threshold, 71.6 percent of children are classified as living in high-access clusters; 52.4 percent live within 3 kilometers and 86.2 percent within 8 kilometers. For the alternative measure, the average distance to the nearest health center or hospital is 16.40 kilometers (SD: 15.73), and 31.1 percent of children live within 5 kilometers of such a facility. Figure 7 plots the corresponding distributions.

These access measures are subject to two sources of measurement error. First, cluster coordinates reflect the caregiver's residence at the time of survey rather than the child's residence throughout the first five years of life. Second, DHS cluster coordinates are randomly displaced to protect respondent confidentiality: urban clusters by up to 2 kilometers, and rural clusters by up to 5 kilometers for most clusters and up to 10 kilometers for a small share (Demographic and Health Surveys Program, 2022). Both sources introduce noise in the classification of geographic access. To the extent that misclassification is random, the resulting attenuation biases estimated treatment effects toward zero rather than generating spurious effects.

To further limit this concern, I restrict the mortality analysis to children born between

2008 and 2019, relying mostly on DHS waves for which displaced coordinates are constrained within second administrative boundaries (departments). This restriction reduces, though does not eliminate, the noise introduced by displacement.

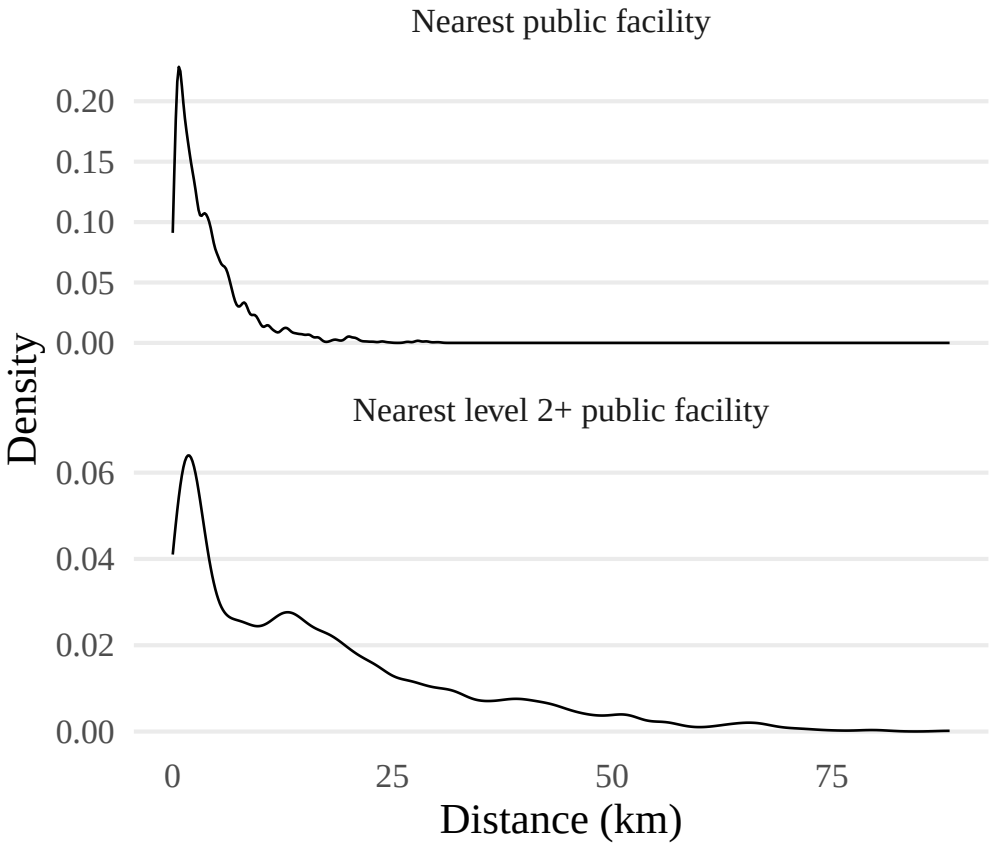
In the main analysis, high geographic access is defined as residing within 5 kilometers of the nearest public facility. This threshold is widely used in the health access literature as a benchmark for reasonable physical access to primary care in low-income settings (Croke et al., 2020). Robustness checks use alternative thresholds of 3 and 8 kilometers.

Table 2. Summary statistics for measures of distance to the nearest public healthcare facility

Variable	Statistic
Unique children	88,247
Distance to nearest public facility (km)	4.08 (4.23)
Within 3 km of nearest public facility	46,207 (52.4%)
Within 5 km of nearest public facility	63,141 (71.6%)
Within 8 km of nearest public facility	76,087 (86.2%)
Distance to nearest level 2+ public facility (km)	16.40 (15.73)
Within 3 km of nearest level 2+ public facility	22,266 (25.2%)
Within 5 km of nearest level 2+ public facility	27,474 (31.1%)
Within 8 km of nearest level 2+ public facility	34,396 (39.0%)
Number of child-month observations per child	5.35 (4.87)

Notes: Summary statistics for geographic access measures, computed using one observation per child in the DHS birth panel. Distance to the nearest public facility is measured in kilometers from the DHS cluster to the nearest public health facility in the completed facility database based on Maina et al. (2019). Level 2+ facilities include health centers and hospitals. Continuous variables are reported as mean (standard deviation). Indicator variables are reported as count (percentage).

Figure 7. Distribution of distance to the nearest public facility



Notes: The figure plots the distribution of distance to the nearest public facility of any level and to the nearest public facility at the health center level or above. Distances are measured in kilometers from DHS cluster coordinates to the completed public facility database based on Maina et al. (2019).

5.2.3 Estimating mortality effects

Using the child-month panel, I estimate the following difference-in-differences specification:

$$Y_{icats} = \gamma\,\text{HighAccess}_c + \beta\big(\text{HighAccess}_c \times \text{Post}_t\big) + \delta_t + \alpha_a + \psi_s + \varepsilon_{icats}$$

where Y_{icats} is an indicator equal to one if child i in cluster c dies during age-month a, calendar period t, and survey wave s. The indicator HighAccess_c equals one if the child's cluster lies within 5 kilometers of the nearest public health facility, and Post_t equals one for calendar months at or after October 2013. The coefficient γ captures time-invariant differences in baseline mortality between high- and low-access clusters. The coefficient β is the parameter of interest: the differential change in the monthly probability of death in high-access clusters relative to low-access clusters following the policy. Calendar month-year fixed

effects (δ_t) absorb common temporal shocks to child mortality, including secular trends and seasonal disease patterns, that affect all clusters equally in a given period. Age-in-months fixed effects (α_a) absorb the biological age profile of under-five mortality, which is invariant to calendar time. Survey wave fixed effects (ψ_s) account for differences in sampling across DHS rounds. The Post_t main effect is subsumed by δ_t. Standard errors are clustered at the DHS cluster level. This approach follows Croke et al. (2020) and is similar in spirit to Tanaka (2014).

I also examine an alternative specification using proximity to a health center or hospital rather than to any public facility. For the main access measure, robustness checks use thresholds of 3 and 8 kilometers. The 3-kilometer cutoff provides a stricter definition of physical access but is more susceptible to misclassification from DHS coordinate displacement: urban clusters may be displaced by up to 2 kilometers and rural clusters by up to 5 kilometers, so children near the 3-kilometer boundary may be assigned to the wrong group. The 8-kilometer threshold is less sensitive to this concern. Comparing estimates across thresholds therefore helps assess sensitivity to the choice of cutoff in the presence of spatial measurement error.

In addition to estimating average treatment effects, I examine heterogeneous effects across several dimensions of vulnerability, including birth weight, maternal education, urban versus rural residence, malaria transmission zone, and household wealth. Children of less educated mothers may benefit more from fee removal if education affects information about services or care-seeking behavior. Children in rural areas may see larger effects because geographic barriers are greater outside urban centers. Children in high malaria zones may benefit more if improved access increases treatment of malaria-related illness. Poorer households may respond differently if they face residual costs not covered by the policy, such as transportation, diagnostics, or services outside the benefit package.

As an additional falsification test, I assign placebo implementation dates within the pre-policy period and re-estimate the model for each admissible date. Placebo dates are restricted to those allowing at least six months of observations on each side of the assigned cutoff. The identification strategy is supported if the distribution of placebo estimates is centered near zero and rarely approaches the magnitude of the main estimate.

6 Results

6.1 First-stage evidence

6.1.1 Out-of-pocket expenditures

Figure 8 plots event-study estimates for out-of-pocket expenditures (OOPE) and the probability that visit costs exceed the daily poverty-line threshold. Table 3 reports the corresponding difference-in-differences estimates.

The event-study coefficients show little evidence of differential pre-trends between public and private-for-profit facilities prior to October 2013. Pre-policy estimates are generally small, close to zero, and statistically indistinguishable from zero, supporting the parallel trends assumption.

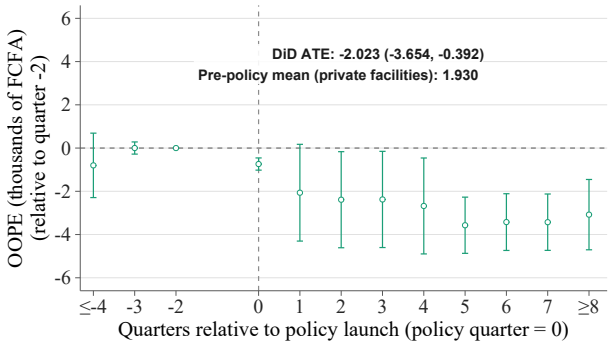
After the policy, financial burden falls sharply in public facilities relative to private-for-profit facilities. The magnitude of the decline increases over time in both panels, suggesting the policy's effects emerged gradually. One interpretation is that facilities adjusted operations over time as revenue from user fees declined. This pattern is also consistent with the phased expansion of the benefit package, including the addition of generic essential medicines in January 2014.

Turning to the average treatment effect estimates, the policy reduced OOPE by 2.023 thousand FCFA (95% CI: [−3.654, −0.392]), equivalent to approximately $3.40 per visit. Relative to the pre-policy mean of 1.930 thousand FCFA among clients of private-for-profit facilities, this corresponds to a decline of roughly 105 percent. [3] The policy also reduced the probability that visit costs exceeded the daily poverty-line threshold by 26.4 percentage points (95% CI: [−0.427, −0.102]). Against a pre-policy probability of 0.343 in private facilities, this implies a reduction of approximately 77 percent. These estimates indicate that the policy substantially reduced both the level of out-of-pocket spending and the incidence of financially burdensome visits for households seeking care for sick children.

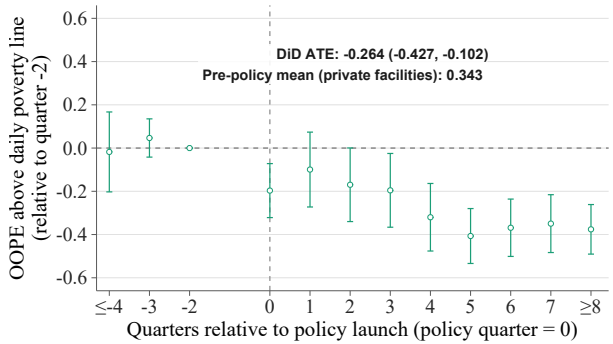
[3] The pre-treatment mean OOPE in private facilities is modest by the standards of urban anecdotal benchmarks, reflecting several patterns in the data. First, many private facilities in lower-income regions are small practices staffed by a single nurse or health worker, and their charges are correspondingly low: mean OOPE among uninsured clients ranges from approximately 7,100 FCFA in Dakar and 4,300 FCFA in Thiès to below 1,000 FCFA in Kolda and Ziguinchor, consistent with private facilities adjusting charges to local purchasing power and facility size. Second, approximately 42 percent of clients visiting private facilities in the pre-policy period report health insurance coverage, compared with 15 percent among public-facility clients, which may pull down reported out-of-pocket costs among some insured clients. However, insurance coverage does not always translate directly into zero out-of-pocket costs at the point of care: in many cases, insured households pay out of pocket at the time of the visit and subsequently seek reimbursement from their insurer, so the relationship between insurance status and reported OOPE is not mechanical. The pooled mean therefore reflects a national sample weighted toward lower-income regions where both facility size and charges tend to be lower.

Figure 8. Event-study estimates for financial outcomes of sick-child visits

(a) Out-of-pocket expenditure (thousands of FCFA)



(b) Expenditure above the daily poverty-line threshold



Notes: The figure plots event-study estimates from difference-in-differences specifications using sick-child exit interviews from the Senegal SPA. Each coefficient traces the differential evolution of outcomes in public facilities relative to private-for-profit facilities over time, with the period immediately preceding the policy as the omitted category. Panel (a) reports estimates for out-of-pocket expenditure, measured in thousands of FCFA. Panel (b) reports estimates for an indicator equal to one if reported expenditure exceeds the applicable daily poverty-line threshold for that year, corresponding to the World Bank extreme poverty line ($1.90 per person per day during 2012–2016 and $2.15 from 2017 onward, converted to year-specific FCFA values). All specifications include visit-year fixed effects. Standard errors are clustered at the facility level. Because the SPA begins in 2012, the pre-policy window is short and some early event-time bins contain few observations. *Abbreviations*: OOPE = out-of-pocket expenditure; FCFA = West African CFA franc.

Table 3. Effect of exposure to free care on OOPE

	Dependent variable:	
	OOPE (thousands of FCFA)	OOPE above daily poverty line
Post-policy × Public	-2.023 (-3.654, -0.392)	-0.264 (-0.427, -0.102)
Pre-policy mean (private facilities)	1.930	0.343
Observations	6,827	6,827
Clusters	1,916	1,916
R^2	0.108	0.082

Notes: The table reports difference-in-differences estimates for financial outcomes using sick-child exit interviews from the Senegal SPA. The coefficient on *Post-policy* × *Public* captures the differential change in outcomes for sick-child visits in public facilities after the October 2013 policy, relative to private-for-profit facilities. Column (1) uses out-of-pocket expenditure for the visit, measured in thousands of FCFA, as the dependent variable. Column (2) uses an indicator equal to one if reported expenditure exceeds the applicable daily poverty-line threshold for that year. Private-for-profit facilities serve as the comparison group because they are more likely to have continued charging patients throughout the study period and therefore provide a more stable counterfactual than NGO, nonprofit, or faith-based facilities. All specifications include visit-year fixed effects. Confidence intervals in parentheses. Standard errors are clustered at the facility level. *Abbreviations*: OOPE = out-of-pocket expenditure; FCFA = West African CFA franc.

6.1.2 Changes in the case-mix of presenting symptoms

I next examine whether the composition of sick-child visits changed after the policy. Lower user fees may encourage caregivers to seek care for symptoms that previously did not prompt a facility visit, shifting the symptom profile of presenting cases. Figure 9 plots event-study estimates for four commonly reported symptoms. Table 4 reports the corresponding difference-in-differences estimates.

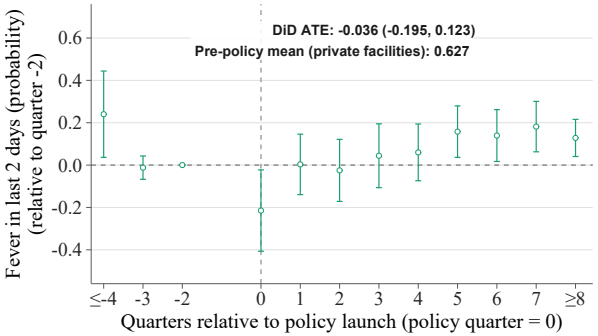
Pre-policy coefficients are generally small and statistically indistinguishable from zero across all four symptoms, consistent with parallel trends in the composition of presenting cases prior to the reform.

The post-policy estimates show limited and statistically imprecise changes in most symptoms. The estimated effects for fever in the last two days (-0.036; 95% CI: $[-0.195, 0.123]$) and watery stools in the last two days (0.080; 95% CI: $[-0.025, 0.185]$) are not distinguishable from zero. The estimate for cough or difficulty breathing (0.124; 95% CI: $[-0.001, 0.249]$) is also statistically imprecise, with a confidence interval that just includes zero. The probability of vomiting is the only estimate that excludes zero (0.096; 95% CI: $[0.002, 0.190]$), though the lower bound is close to zero and the effect is small relative to a pre-policy mean of 0.284 in private facilities.

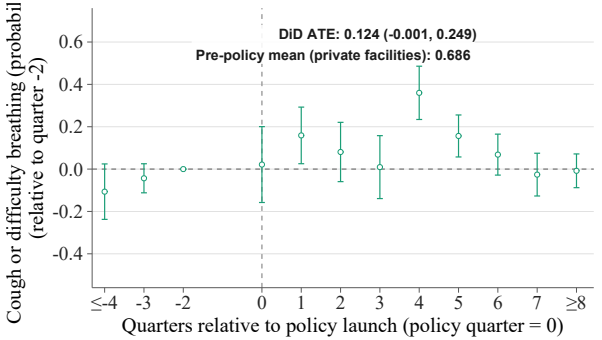
The case mix of presenting symptoms is therefore broadly stable across public and private-for-profit facilities after the policy. Large compositional shifts in the types of cases arriving at facilities are unlikely to explain the reductions in out-of-pocket expenditures documented above. The next subsection examines whether the policy affected the treatment provided during sick-child visits.

Figure 9. Event-study estimates for the case-mix of presenting symptoms

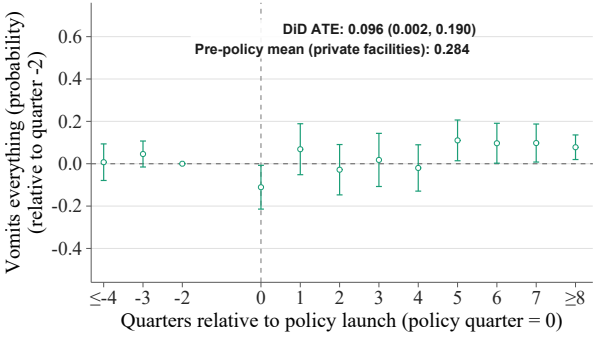
(a) Fever in the last two days



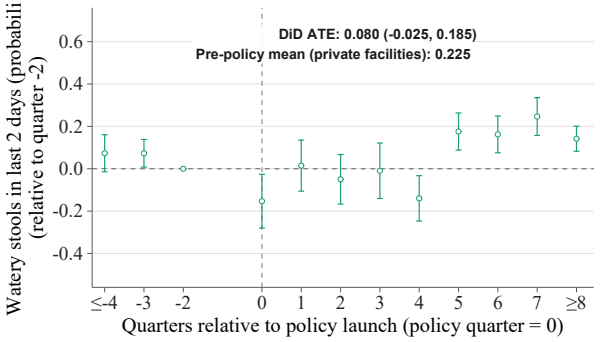
(b) Cough or difficulty breathing



(c) Vomits everything



(d) Watery stools in the last two days



Notes: The figure plots event-study estimates from difference-in-differences specifications using sick-child exit interviews from the Senegal SPA. Each coefficient traces the differential evolution of the probability that a symptom was reported in public facilities relative to private-for-profit facilities over time, with the period immediately preceding the policy as the omitted category. Outcomes correspond to caregiver-reported symptoms recorded during the sick-child visit: fever in the last two days, cough or difficulty breathing, vomiting everything, and watery stools in the last two days. All specifications include visit-year fixed effects. Standard errors are clustered at the facility level.

Table 4. Effect of exposure to free care on the case-mix of presenting symptoms

	Dependent variable:			
	Fever	Cough/diff breath	Vomits everything	Watery stools
Post-policy × Public	-0.036 (-0.195, 0.123)	0.124 (-0.001, 0.249)	0.096 (0.002, 0.190)	0.080 (-0.025, 0.185)
Pre-policy mean (private facilities)	0.627	0.686	0.284	0.225
Observations	6,851	6,853	6,854	6,852
Clusters	1,915	1,916	1,917	1,917
R^2	0.013	0.004	0.005	0.007

Notes: The table reports difference-in-differences estimates for caregiver-reported symptoms using sick-child exit interviews from the Senegal SPA. The coefficient on *Post-policy* × *Public* captures the differential change in the probability that each symptom was reported during visits to public facilities after the October 2013 policy, relative to private-for-profit facilities. Columns correspond to four symptoms: fever in the last two days, cough or difficulty breathing, vomiting everything, and watery stools in the last two days. All specifications include visit-year fixed effects. Confidence intervals in parentheses. Standard errors are clustered at the facility level.

6.1.3 Provision of treatment

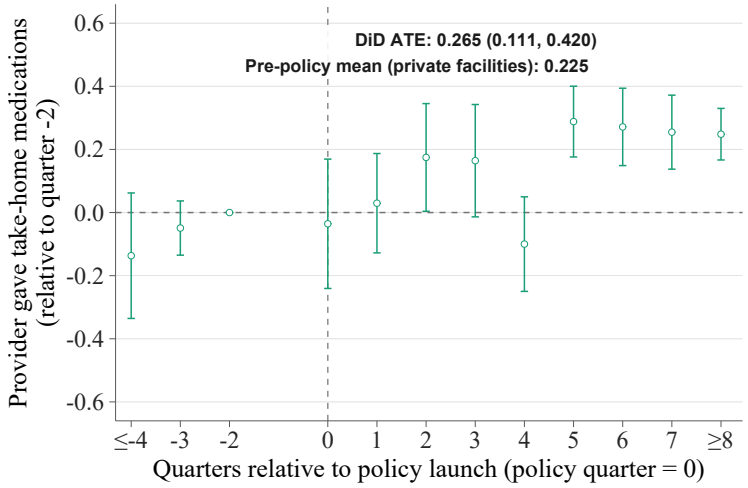
I next examine whether the policy affected treatment provision during sick-child visits. Figure 10 plots event-study estimates for whether the provider dispensed take-home medications to the caregiver. Table 5 reports the corresponding difference-in-differences estimate.

Pre-policy coefficients are close to zero and statistically indistinguishable from zero, consistent with parallel pre-trends in treatment provision across facility types.

After October 2013, the probability of receiving take-home medications rises sharply in public facilities relative to private-for-profit facilities. The event-study estimates show a sustained increase beginning shortly after the policy launch and persisting in subsequent periods. The difference-in-differences estimate confirms this pattern: the policy increased the probability that caregivers received take-home medications by 0.265 (95% CI: [0.111, 0.420]). Against a pre-policy mean of 0.225 in private facilities, this corresponds to an increase of roughly 118 percent.

The first-stage results therefore indicate that the policy substantially reduced financial barriers to care and increased the likelihood that children received medications during sick-child visits, without meaningfully changing the composition of presenting cases. The next subsection assesses whether these findings are sensitive to the choice of comparison group.

Figure 10. Event-study estimates for provision of take-home medications during sick-child visits



Notes: The figure plots event-study estimates from a difference-in-differences specification using sick-child exit interviews from the Senegal SPA. The outcome is an indicator equal to one if the provider dispensed take-home medications to the caregiver during the visit. Each coefficient traces the differential evolution of treatment provision in public facilities relative to private-for-profit facilities over time, with the period immediately preceding the policy as the omitted category. All specifications include visit-year fixed effects. Standard errors are clustered at the facility level.

Table 5. Effect of exposure to free care on the likelihood of medication provision

	Dependent variable: Provider gave take-home medications
Post-policy × Public	0.265 (0.111, 0.420)
Pre-policy mean (private facilities)	0.225
Observations	6,855
Clusters	1,917
R^2	0.106

Notes: The table reports a difference-in-differences estimate for treatment provision using sick-child exit interviews from the Senegal SPA. The outcome is an indicator equal to one if the provider dispensed take-home medications to the caregiver during the visit. The coefficient on *Post-policy* × *Public* captures the differential change in treatment provision in public facilities after the October 2013 policy, relative to private-for-profit facilities. Private-for-profit facilities serve as the comparison group because they are more likely to have continued charging patients throughout the study period and therefore provide a more stable counterfactual than NGO, nonprofit, or faith-based providers. All specifications include visit-year fixed effects. Confidence intervals in parentheses. Standard errors are clustered at the facility level.

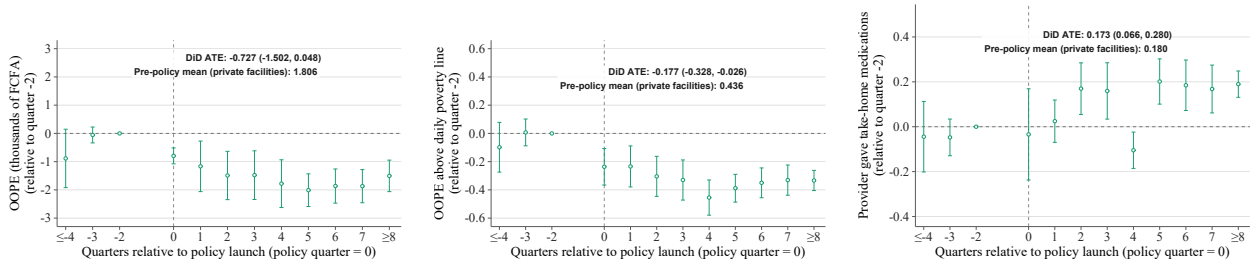
6.1.4 Robustness: expanded non-public control group

As a robustness check, I expand the comparison group to include nonprofit, NGO, and faith-based facilities alongside private-for-profit providers. Figure 11 presents the corresponding event-study estimates, and Table 6 reports the difference-in-differences estimates. Pre-policy trends are broadly parallel between public facilities and the expanded control group, supporting the validity of the design under this alternative comparison.

The results are directionally consistent with the main specification. The probability that visit costs exceeded the daily poverty-line threshold fell by 17.7 percentage points (95% CI: [−0.328, −0.026]), and the probability of receiving take-home medications increased by 17.3 percentage points (95% CI: [0.066, 0.280]). The estimate for mean OOPE is −727 FCFA but is not statistically distinguishable from zero (95% CI: [−1.502, 0.048]).

The magnitudes are somewhat smaller than in the main specification, which is consistent with the expectation that nonprofit, NGO, and faith-based providers already offered subsidized care prior to the policy and therefore provide a less stark counterfactual. The direction and pattern of effects are nonetheless robust to this alternative comparison group definition. The next section examines whether the policy translated into improvements in child survival.

Figure 11. Event-study estimates: robustness test with expanded non-public control group



Notes: The figure plots event-study estimates for the robustness specification in which the control group includes all non-public facilities: private-for-profit, nonprofit, NGO, and faith-based providers. Each coefficient traces the differential evolution of outcomes in public facilities relative to the expanded non-public control group, with the period immediately preceding the policy as the omitted category. The three panels show effects on mean out-of-pocket expenditure (thousands of FCFA), the probability that expenditure exceeds the daily poverty-line threshold, and the probability that the provider dispensed take-home medications. All specifications include visit-year fixed effects. Standard errors are clustered at the facility level. *Abbreviations*: OOPE = out-of-pocket expenditure; FCFA = West African CFA franc.

Table 6. Effect of exposure to free care on OOPE and treatment for sick child care: expanded non-public control group

	Dependent variable:		
	OOPE (thousands of FCFA)	OOPE above daily poverty line	Provider gave take-home medications
Post-policy × Public	-0.727 (-1.502, 0.048)	-0.177 (-0.328, -0.026)	0.173 (0.066, 0.280)
Pre-policy mean (private facilities)	1.806	0.436	0.180
Observations	7,426	7,426	7,456
Clusters	2,078	2,078	2,079
R^2	0.088	0.116	0.107

Notes: The table reports difference-in-differences estimates from the robustness specification in which the control group includes all non-public facilities: private-for-profit, nonprofit, NGO, and faith-based providers. The coefficient on *Post-policy* × *Public* captures the differential change in outcomes in public facilities after the October 2013 policy, relative to the expanded non-public control group. Columns correspond to mean out-of-pocket expenditure (thousands of FCFA), an indicator for expenditure above the daily poverty-line threshold, and an indicator for whether the provider dispensed take-home medications. Pre-policy means are reported for the expanded non-public control group. All specifications include visit-year fixed effects. Confidence intervals in parentheses. Standard errors are clustered at the facility level. *Abbreviations*: OOPE = out-of-pocket expenditure; FCFA = West African CFA franc.

6.2 Mortality effects

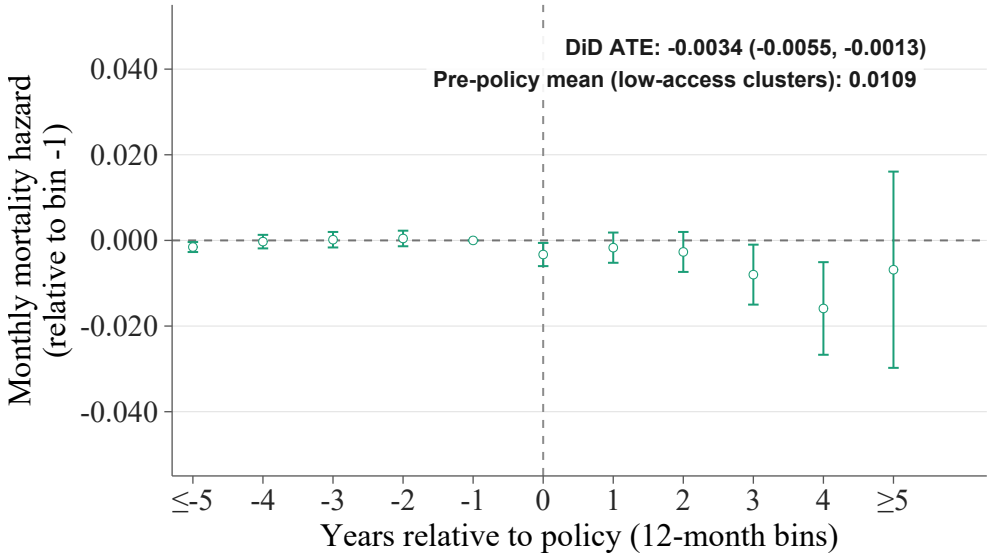
6.2.1 Main specification: 5-kilometer access threshold

I next examine whether the free care policy affected child mortality. The analysis compares children in clusters located within 5 kilometers of the nearest public health facility to those in lower-access clusters. Figure 12 presents event-study estimates, and Table 7 reports the corresponding difference-in-differences estimate.

Pre-policy coefficients are generally close to zero and statistically indistinguishable from zero, showing no evidence of differential mortality trends between high- and low-access areas before the reform. After October 2013, the estimates turn negative, indicating a sustained decline in the monthly mortality hazard among children in higher-access areas.

The difference-in-differences estimate is -0.0034 (95% CI: $[-0.0055, -0.0013]$), implying a reduction of 0.34 percentage points in the monthly probability of death for children in higher-access areas relative to lower-access areas. Against a pre-policy baseline mean of 0.0109 among low-access clusters, this corresponds to a decline of roughly 31 percent in the monthly mortality hazard. This result is consistent with the first-stage evidence: where geographic access was sufficient for households to act on the subsidy, the policy reduced out-of-pocket costs and increased treatment provision, and these changes translated into measurable gains in child survival.

Figure 12. Event-study estimates of the effect of free care on the monthly child mortality hazard



Notes: The figure plots event-study estimates from a difference-in-differences specification examining the effect of the free care policy on child mortality. The analysis uses a child-month panel constructed from DHS birth histories. The outcome is an indicator equal to one if a child dies in a given month. High access is defined as residing within 5 kilometers of the nearest public health facility. Event time is measured in 12-month bins relative to the October 2013 policy, with the year immediately preceding the policy as the omitted category. The specification includes fixed effects for calendar month, child age in months, and survey year. Standard errors are clustered at the DHS cluster level.

Table 7. Effect of exposure to free care on child mortality

	Dependent variable: Monthly mortality hazard
Post-policy × High access	-0.0034 (-0.0055, -0.0013)
Pre-policy mean (low-access clusters)	0.0109
Observations	470,852
Clusters	2,048
R^2	0.063

Notes: The table reports a difference-in-differences estimate of the effect of the free care policy on child mortality using a child-month panel constructed from DHS birth histories. The dependent variable is an indicator equal to one if the child dies in a given month. The coefficient on *Post-policy* × *High access* captures the differential change in the monthly mortality hazard for children in clusters within 5 kilometers of the nearest public health facility after the October 2013 policy, relative to children in lower-access clusters. The specification includes fixed effects for calendar month, child age in months, and survey year. Confidence intervals in parentheses. Standard errors are clustered at the DHS cluster level.

6.2.2 Alternative access thresholds

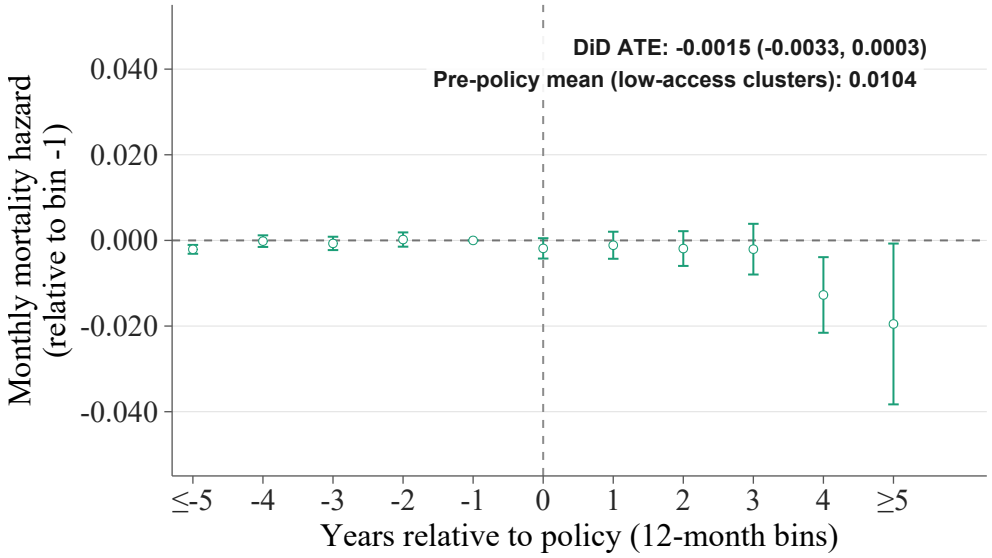
I assess whether the mortality results depend on the definition of high geographic access. In addition to the main 5-kilometer threshold, I consider a stricter 3-kilometer threshold and a broader 8-kilometer threshold. The 3-kilometer cutoff is close to the median distance to the nearest public facility in the sample, but estimates based on this threshold should be interpreted with caution given the random displacement of DHS cluster coordinates. The 8-kilometer threshold tests whether the mortality effects extend beyond children living in the immediate vicinity of a facility.

Figure 13 presents event-study estimates under the 3-kilometer threshold. Pre-policy coefficients are close to zero, with no evidence of differential mortality trends before the reform. Post-policy estimates are negative but less precisely estimated than in the main specification. The difference-in-differences estimate is -0.0015 (95% CI: $[-0.0033, 0.0003]$), implying a reduction of 0.15 percentage points in the monthly mortality hazard relative to a pre-policy mean of 0.0104 in low-access clusters, or roughly 14 percent. The point estimate is not statistically distinguishable from zero. The attenuation relative to the main specification is consistent with greater susceptibility to misclassification from coordinate displacement at tighter thresholds.

Figure 14 presents estimates under the 8-kilometer threshold. Pre-policy coefficients again show no evidence of differential trends. The difference-in-differences estimate is -0.0035 (95% CI: $[-0.0063, -0.0008]$), implying a reduction of 0.35 percentage points relative to a pre-policy mean of 0.0114, or roughly 31 percent. This estimate is similar in magnitude to the main result and statistically distinguishable from zero, indicating that the mortality effects are not limited to children living within 5 kilometers of a facility.

The pattern across thresholds is therefore informative. The 3-kilometer estimate is attenuated and imprecise, which is consistent with measurement error from coordinate displacement at tight cutoffs. The 5- and 8-kilometer estimates are similar in magnitude and both statistically significant, suggesting the main result is robust to reasonable variation in how geographic access is defined. The next subsection assesses robustness to an alternative measure of geographic access based on proximity to higher-level public facilities.

Figure 13. Event-study estimates of the effect of free care on the monthly child mortality hazard: 3-kilometer threshold



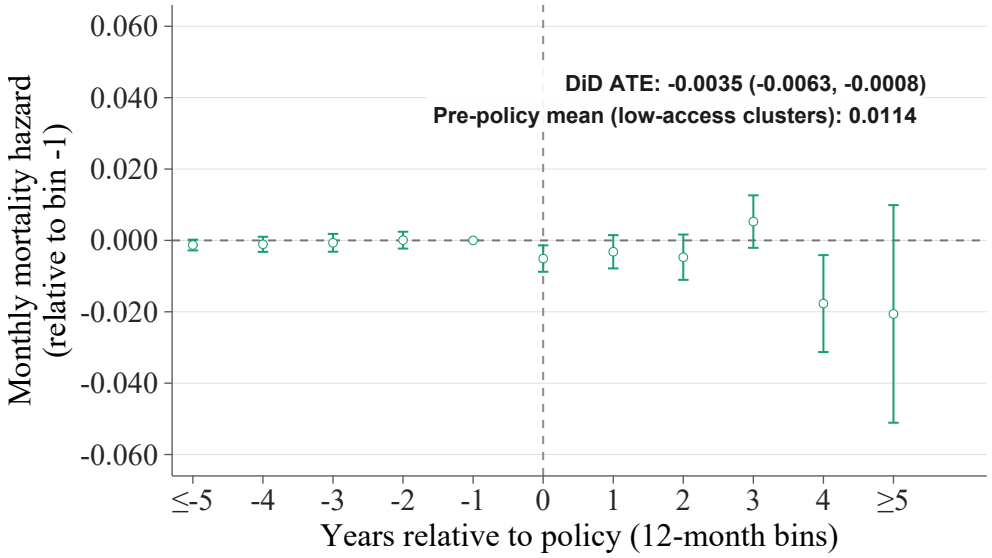
Notes: The figure plots event-study estimates from a difference-in-differences specification examining the effect of the free care policy on child mortality, using a 3-kilometer threshold to define high geographic access. The analysis uses a child-month panel constructed from DHS birth histories. The outcome is an indicator equal to one if a child dies in a given month. Event time is measured in 12-month bins relative to October 2013, with the year immediately preceding the policy as the omitted category. The specification includes fixed effects for calendar month, child age in months, and survey year. Standard errors are clustered at the DHS cluster level. Estimates based on this threshold should be interpreted with caution given the random displacement of DHS cluster coordinates.

Table 8. Effect of exposure to free care on child mortality: 3 km threshold

	Dependent variable: Monthly mortality hazard
Post-policy × High access	-0.0015 (-0.0033, 0.0003)
Pre-policy mean (low-access clusters)	0.0104
Observations	470,852
Clusters	2,060
R^2	0.063

Notes: The table reports a difference-in-differences estimate of the effect of the free care policy on child mortality using a 3-kilometer threshold to define high geographic access. The dependent variable is an indicator equal to one if the child dies in a given month. The coefficient on *Post-policy* × *High access* captures the differential change in the monthly mortality hazard for children in clusters within 3 kilometers of the nearest public facility after the October 2013 policy, relative to lower-access clusters. The specification includes fixed effects for calendar month, child age in months, and survey year. Confidence intervals in parentheses. Standard errors are clustered at the DHS cluster level. Estimates should be interpreted with caution given the random displacement of DHS cluster coordinates.

Figure 14. Event-study estimates of the effect of free care on the monthly child mortality hazard: 8-kilometer threshold



Notes: The figure plots event-study estimates from a difference-in-differences specification examining the effect of the free care policy on child mortality, using an 8-kilometer threshold to define high geographic access. The analysis uses a child-month panel constructed from DHS birth histories. The outcome is an indicator equal to one if a child dies in a given month. Event time is measured in 12-month bins relative to October 2013, with the year immediately preceding the policy as the omitted category. The specification includes fixed effects for calendar month, child age in months, and survey year. Standard errors are clustered at the DHS cluster level.

Table 9. Effect of exposure to free care on child mortality: 8 km threshold

	Dependent variable: Monthly mortality hazard
Post-policy × High access	-0.0035 (-0.0063, -0.0008)
Pre-policy mean (low-access clusters)	0.0114
Observations	470,852
Clusters	2,060
R^2	0.063

Notes: The table reports a difference-in-differences estimate of the effect of the free care policy on child mortality using an 8-kilometer threshold to define high geographic access. The dependent variable is an indicator equal to one if the child dies in a given month. The coefficient on *Post-policy* × *High access* captures the differential change in the monthly mortality hazard for children in clusters within 8 kilometers of the nearest public facility after October 2013, relative to lower-access clusters. The specification includes fixed effects for calendar month, child age in months, and survey year. Confidence intervals in parentheses. Standard errors are clustered at the DHS cluster level.

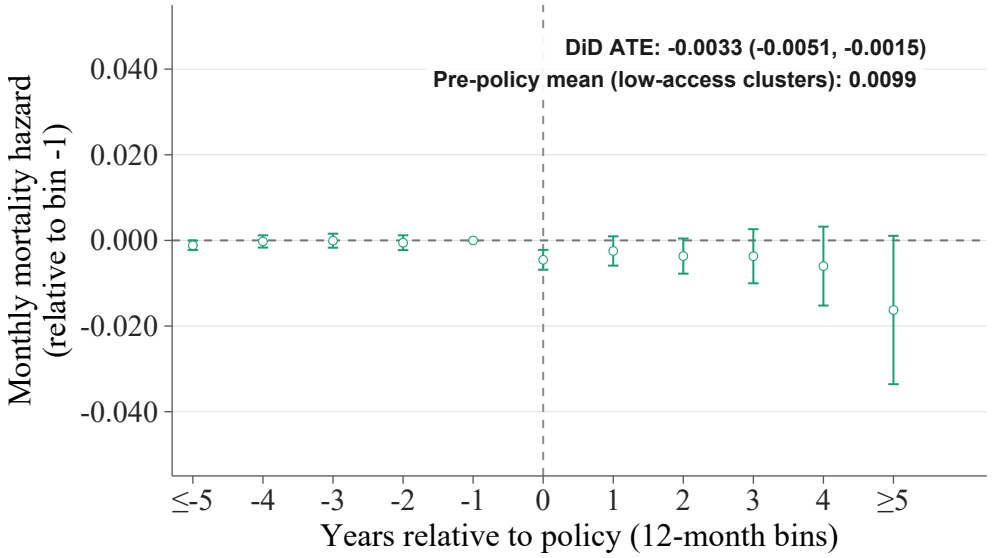
6.2.3 Alternative distance measure: proximity to higher-level public facilities

I next assess whether the mortality results are sensitive to the measure of geographic access. The main specification uses distance to the nearest public facility of any level, which primarily captures proximity to health posts given the composition of Senegal's public facility network. I consider an alternative measure based on distance to the nearest public health center or hospital. This captures a stricter notion of access, since higher-level facilities are better equipped to provide curative and referral care.

As in the main specification, I define high access as residing within 5 kilometers of the relevant facility. Figure 15 presents the event-study estimates, and Table 10 reports the difference-in-differences estimate. Pre-policy coefficients are close to zero, with no evidence of differential mortality trends before the reform. After October 2013, the estimates turn negative. The difference-in-differences estimate is -0.0033 (95% CI: $[-0.0051, -0.0015]$), implying a reduction of 0.33 percentage points in the monthly mortality hazard. Against a pre-policy mean of 0.0099 in low-access clusters, this corresponds to a decline of roughly 33 percent.

This alternative measure is nested within the main one: children within 5 kilometers of a health center or hospital are also within 5 kilometers of any public facility, but not the reverse. In the sample, 31.1 percent of children are classified as high access under both measures, while an additional 40.4 percent are classified as high access only under the broader measure. The similarity of the estimates across the two measures suggests that the mortality gains were not limited to areas with nearby higher-level facilities. Proximity to the broader primary-care network, dominated by health posts, was sufficient for fee removal to translate into improvements in child survival. The next subsection presents placebo tests using pre-policy data.

Figure 15. Event-study estimates of the effect of free care on the monthly child mortality hazard: proximity to public health centers and hospitals



Notes: The figure plots event-study estimates from a difference-in-differences specification examining the effect of the free care policy on child mortality. High access is defined as residing within 5 kilometers of the nearest public health center or hospital. The analysis uses a child-month panel constructed from DHS birth histories. The outcome is an indicator equal to one if a child dies in a given month. Event time is measured in 12-month bins relative to October 2013, with the year immediately preceding the policy as the omitted category. The specification includes fixed effects for calendar month, child age in months, and survey year. Standard errors are clustered at the DHS cluster level.

Table 10. Effect of exposure to free care on child mortality: proximity to public health centers and hospitals

	Dependent variable: Monthly mortality hazard
Post-policy × High access	-0.0033 (-0.0051, -0.0015)
Pre-policy mean (low-access clusters)	0.0099
Observations	470,852
Clusters	2,060
R^2	0.063

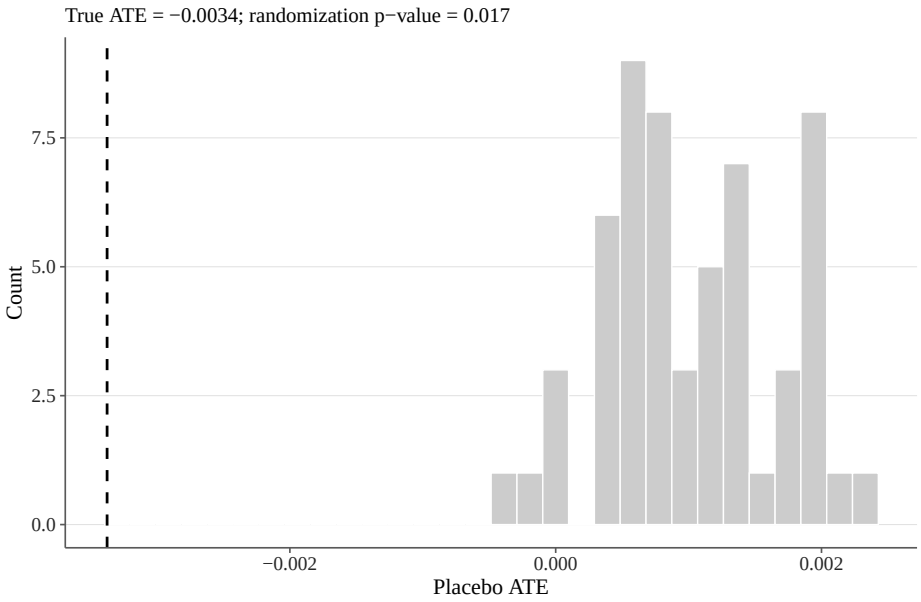
Notes: The table reports a difference-in-differences estimate of the effect of the free care policy on child mortality using proximity to the nearest public health center or hospital to define geographic access. The dependent variable is an indicator equal to one if the child dies in a given month. The coefficient on *Post-policy* × *High access* captures the differential change in the monthly mortality hazard for children in clusters within 5 kilometers of the nearest public health center or hospital after October 2013, relative to lower-access clusters. The specification includes fixed effects for calendar month, child age in months, and survey year. Confidence intervals in parentheses. Standard errors are clustered at the DHS cluster level.

6.2.4 Placebo policy dates

To assess whether the estimated mortality effects could arise from spurious correlations with underlying trends, I conduct placebo exercises using only pre-policy observations. I assign placebo implementation dates within the pre-policy sample and re-estimate the baseline difference-in-differences specification for each admissible date. Placebo dates are restricted to those allowing at least six months of observations on each side of the assigned cutoff, yielding 57 valid dates.

Figure 16 shows the resulting distribution of placebo estimates. The distribution is centered near zero, and the true estimated effect of -0.0034 lies in the far left tail. None of the 57 placebo estimates are more negative than the true estimate, corresponding to a randomization p-value of 0.017. The observed mortality effect is therefore unlikely to reflect chance correlations with underlying mortality dynamics. The next subsection examines heterogeneity in the mortality effects across population subgroups.

Figure 16. Distribution of placebo treatment effects for the monthly child mortality hazard



Notes: The figure shows the distribution of difference-in-differences estimates from placebo exercises conducted using pre-policy observations only. Placebo implementation dates are assigned within the pre-policy sample and the baseline model is re-estimated for each admissible date. Only dates allowing at least six months of observations on each side of the assigned cutoff are retained, yielding 57 valid placebo dates. The vertical line denotes the estimated treatment effect from the true October 2013 policy implementation.

6.2.5 Heterogeneity in mortality effects

I next examine whether the mortality effects vary across subgroups defined by socioeconomic conditions, epidemiological environment, and baseline vulnerability. I estimate sepa-

rate difference-in-differences specifications by birth weight, maternal education, malaria risk zone, household wealth, and place of residence. Figure 17 presents the subgroup-specific event-study estimates, and Figure 18 summarizes the corresponding difference-in-differences estimates. Pre-policy coefficients are generally close to zero across subgroups, with no clear evidence of differential pre-trends.

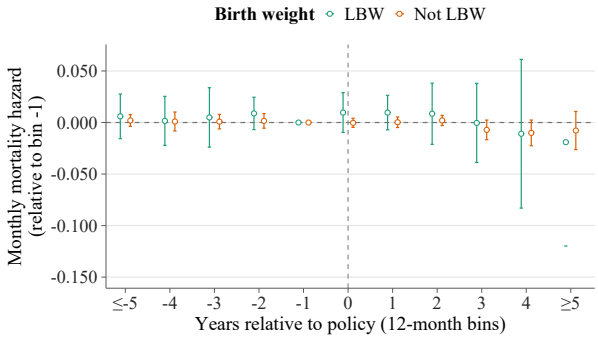
The most consistent differences appear across socioeconomic groups. The estimated mortality reduction is larger among children whose mothers have no primary education (-0.0032; 95% CI: [-0.0056, -0.0008]) than among children of mothers who completed at least primary school (-0.0024; 95% CI: [-0.0064, 0.0017]). A similar pattern holds across wealth groups: the estimate for children in the poorest two quintiles is -0.0033 (95% CI: [-0.0058, -0.0008]), while the estimate for children in higher wealth quintiles is smaller and imprecise (-0.0012; 95% CI: [-0.0079, 0.0055]). By place of residence, the effect is negative and statistically significant in rural areas (-0.0028; 95% CI: [-0.0052, -0.0005]) and close to zero in urban areas (0.0025; 95% CI: [-0.0103, 0.0154]). These patterns are consistent with the policy generating larger gains where financial barriers to care were more binding.

The estimates also vary by malaria environment. The effect is larger in low-risk zones (-0.0067; 95% CI: [-0.0102, -0.0031]) than in moderate- or high-risk zones (-0.0027; 95% CI: [-0.0052, -0.0001]). One interpretation is that in higher-malaria settings, supply-side constraints such as congestion, medicine shortages, or quality limitations dampen the health returns to fee removal. Both estimates are negative, however, and confidence intervals overlap, so this difference should be interpreted cautiously.

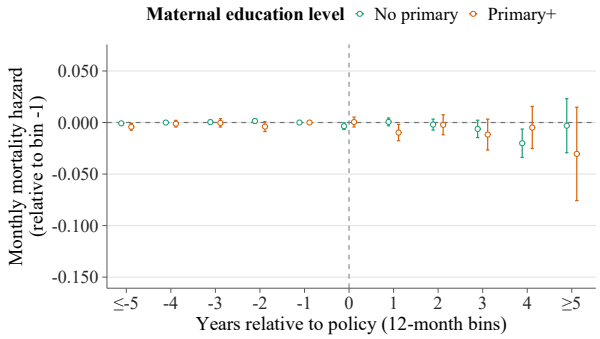
There is little evidence that the policy produced larger mortality gains among low-birth-weight children. The estimate for this group is small and very imprecise (0.0009; 95% CI: [-0.0159, 0.0178]), reflecting the substantially smaller sample size for low-birth-weight births. These results are not informative about heterogeneity by biological vulnerability.

The subgroup estimates do not suggest the policy disproportionately benefited children already better positioned to access care. Point estimates are more favorable for children in poorer households, rural areas, and families where mothers have no primary education, which is more consistent with an equalizing effect on child survival. Confidence intervals overlap substantially across all subgroups, however, and these results should be read as suggestive rather than definitive. In addition, subgroup characteristics are measured at the time of survey rather than at birth, so these estimates reflect differences across families as observed at interview and not necessarily across fixed baseline characteristics.

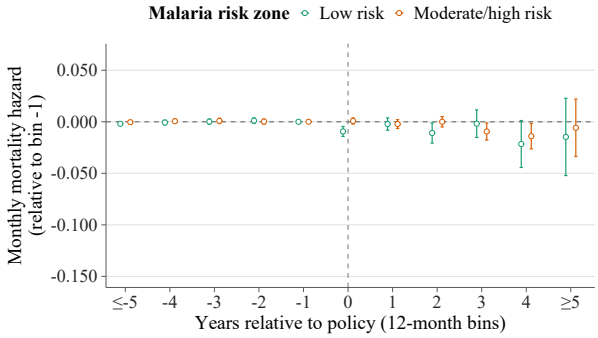
Figure 17. Event-study estimates of the effect of free care on the monthly child mortality hazard, by subgroup



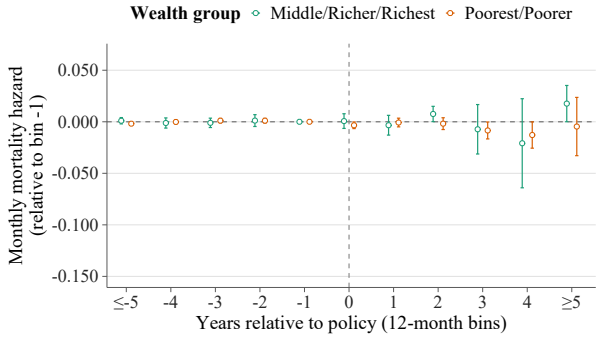
(a) Birth weight



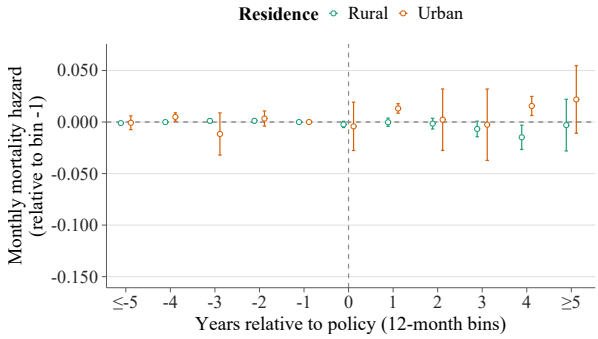
(b) Maternal education



(c) Malaria risk zone



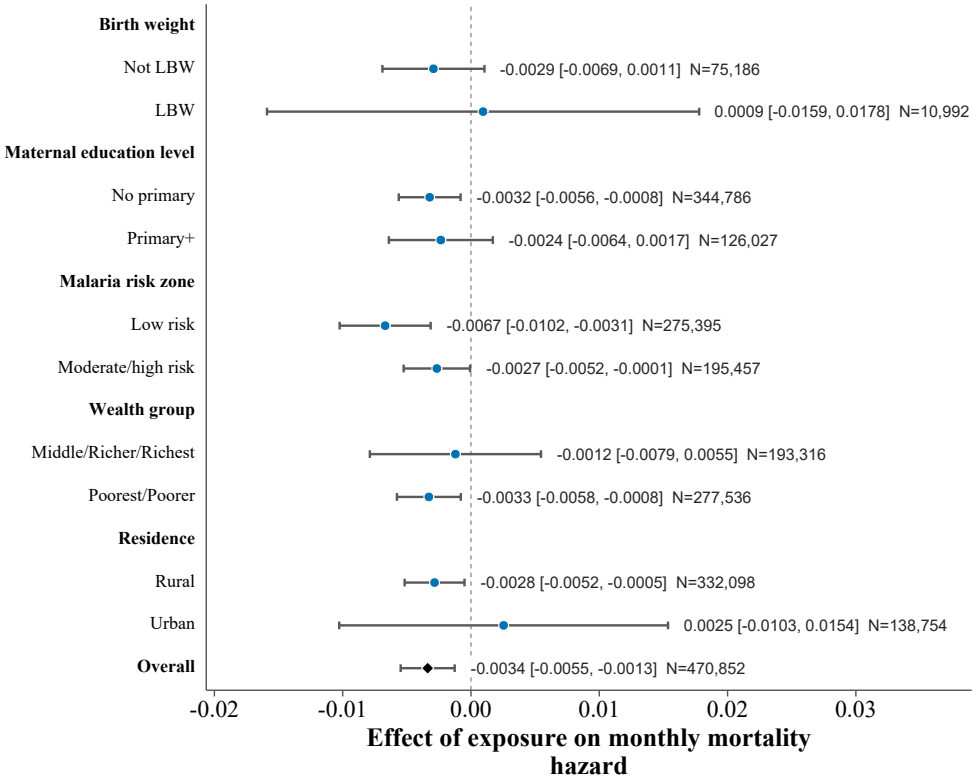
(d) Household wealth



(e) Place of residence

Notes: The figure plots event-study estimates from difference-in-differences specifications examining heterogeneity in the mortality effects of the free care policy. The outcome is an indicator equal to one if the child dies in a given month. Each panel reports estimates for the indicated subgroup. Event time is measured in 12-month bins relative to October 2013, with the year immediately preceding the policy as the omitted category. All specifications include fixed effects for calendar month, child age in months, and survey year. Standard errors are clustered at the DHS cluster level.

Figure 18. Difference-in-differences estimates of the effect of free care on the monthly child mortality hazard, by subgroup



Notes: The figure reports difference-in-differences estimates of the effect of the free care policy on the monthly probability of child death, by subgroup. Each estimate corresponds to the coefficient on *Post-policy* × *High access* from a specification using the same fixed effects as the main analysis: calendar month, child age in months, and survey year. Error bars denote 95 percent confidence intervals. Standard errors are clustered at the DHS cluster level. Sample sizes are reported to the right of each estimate.

7 Discussion

This paper evaluates the impact of Senegal's free health care policy for children under five. The policy was broadly implemented in the public sector, with participation exceeding 92 percent of public facilities in every region. Yet fewer than half of eligible children's caregivers report knowing that the child is covered, and reported coverage is lowest in the southern regions where health needs are greatest. The causal estimates show that the policy reduced out-of-pocket expenditures by 2,023 FCFA per visit, lowered the probability that visit costs exceeded the daily poverty line by 26.4 percentage points, and increased the likelihood of receiving take-home medications by 26.5 percentage points, without shifting the case mix of presenting symptoms. The mortality analysis shows a 31 percent decline in the monthly mortality hazard for children in higher-access areas, with similar results when access is

defined using distance to health centers and hospitals. Mortality reductions are larger among children from poorer households, rural areas, and families where mothers have no primary education, though confidence intervals overlap across subgroups.

These findings speak to the three strands of literature highlighted in the introduction. Within the literature on user fee removal in low- and middle-income countries, this paper provides the first causal evaluation of a national free child care program in Senegal, adding mortality evidence from a West African setting outside Burkina Faso where such evidence remains sparse. The reductions in out-of-pocket spending and treatment provision improvements align with the general pattern documented across settings, while the mortality results extend the literature into an outcome domain that few existing studies address causally. Within the literature on welfare effects of health subsidies, the stable case mix of presenting symptoms provides direct evidence that fee removal expanded access to clinically indicated care, consistent with underuse rather than overuse as the binding constraint (Sautmann et al., 2025; Powell-Jackson et al., 2014). Within the literature on child survival and early childhood health interventions, the results show that the health returns to fee removal depend on geographic access to care: effects are concentrated in higher-access areas and are suggestively larger among children from disadvantaged households, pointing to access and information as the central constraints on translating formal entitlement into survival gains (Conti and Ginja, 2023).

The subgroup pattern is more consistent with an equalizing effect on child survival than with a scenario in which fee removal mainly benefits better-positioned populations. In that respect, the findings are closer in spirit to Gruber et al. (2014), who show that Thailand's universal coverage reform reduced infant mortality and narrowed disparities across provinces. The mortality results also add to the limited causal evidence on health outcomes in the systematic review by Dehnavi et al. (2025), who find that few existing studies meet high standards of causal identification and that mortality outcomes in particular remain understudied. At the same time, the Senegal results fall short of a strong equalizing effect. Sharp regional gaps in reported household coverage, especially in poorer southern regions, suggest that awareness and effective take-up remained uneven. Differences in quality of care, staffing, transport infrastructure, and referral capacity likely limited the extent to which formal eligibility translated into survival gains in more disadvantaged settings.

Three policy implications follow. First, legal entitlement alone does not guarantee effective coverage. Expanding caregiver awareness of eligibility is central to increasing the policy's reach, particularly in poorer and more remote areas. Second, despite documented implementation problems including reimbursement delays, stock-outs, staffing shortages, and weaknesses in quality of care (Faye, 2022), the policy generated meaningful improvements in

financial protection, treatment provision, and child survival where access was more favorable. Third, if equity in child health is a policy objective, fee removal needs to be complemented by investments that expand geographic access and improve quality of care, especially in rural areas.

These points are relevant in light of ongoing reforms that seek to shift financial and managerial responsibility for the policy toward community-based health insurance schemes (Agence de la Couverture Maladie Universelle, 2017; Daff et al., 2020). Such a transfer risks jeopardizing the health gains documented here. Community-based health insurance schemes in Senegal face well-documented operational and financial viability challenges (Rouyard et al., 2022), and moving toward capitation-based reimbursement could weaken provider incentives for service provision if payment rates are not well aligned with the costs of care. Any reform of the financing architecture should therefore be evaluated not only on fiscal grounds but also on its implications for effective coverage, provider behavior, and quality of care.

A back-of-the-envelope calculation helps quantify what is at stake. Using pre-policy monthly death rates from the DHS panel—0.00868 in high-access areas and 0.01087 in low-access areas—and the finding that 71.6 percent of children reside within 5 kilometers of a public facility, approximately 66.8 percent of under-five deaths are estimated to occur in high-access areas. Applying this share to UNICEF estimates of total under-five deaths in Senegal over 2015–2023, which range from 25,980 in 2015 to 19,854 in 2023 and sum to approximately 207,000 over the period (UNICEF, 2024), yields roughly 138,000 deaths in high-access areas. The estimated 31 percent reduction in monthly mortality in these areas implies approximately 43,000 lives saved between 2015 and 2023. Against the 18.1 billion FCFA allocated to the program over that period (Faye, 2023), this corresponds to a cost of roughly 422,000 FCFA (approximately $690) per life saved. From the perspective of ANACMU as the national insurer responsible for financing the free child care benefit, this figure suggests that the program represents a highly cost-effective use of public funds.[4]

Several limitations warrant discussion. First, the study relies on repeated cross-sectional survey data rather than longitudinal follow-up of children or facilities. The difference-in-differences design adjusts for time-invariant differences in composition across groups and common trends over time, but time-varying factors correlated with the October 2013 policy launch that affect public and private facilities differentially remain a concern. Second, the SPA data include relatively few pre-policy observations and visits to private facilities, limiting

[4]This calculation assumes that under-five mortality is distributed across areas in proportion to pre-policy death rates, that the estimated mortality reduction applies uniformly throughout the post-policy period, and that the DiD estimate captures the full mortality effect of the policy in high-access areas. It also does not account for broader program costs beyond the ANACMU budget, such as facility-level inputs and Ministry of Health expenditures on infrastructure and personnel.

statistical power for some comparisons. Third, geographic access measures are based on DHS cluster locations rather than exact household residence, and cluster coordinates are randomly displaced for confidentiality, introducing measurement error particularly at tighter distance thresholds. Fourth, subgroup characteristics are measured at the time of survey rather than at birth, so heterogeneity estimates reflect differences across families as observed at interview rather than across fixed baseline traits. Fifth, selective migration and differential fertility could in principle confound the mortality results. If poorer or sicker households relocated toward public facilities after the policy, the post-policy composition of high-access clusters would shift toward higher-risk children, biasing the mortality estimates toward zero. If free care visits increased exposure to family planning services and reduced high-risk births in high-access areas, some of the estimated mortality reduction could reflect compositional change in births rather than survival gains. Both mechanisms require differential dynamics across access groups that are absent in the pre-policy period, which is inconsistent with the flat pre-policy event-study estimates; to the extent they operate post-policy, they are more plausibly mechanisms through which the policy improved child survival than independent confounders. Finally, other contemporaneous policies could have affected child health during the study period. For these to explain the main mortality results, they would need to generate differential changes across areas with different baseline access to care. The first-stage evidence on out-of-pocket spending and treatment provision makes the free care policy a plausible central mechanism.

The evidence indicates that Senegal's free child care policy improved financial protection and treatment provision and reduced child mortality where access to care was sufficient for households to act on the subsidy. The main shortfall is not formal facility participation but the incomplete translation of entitlement into effective coverage. Future work should examine how reimbursement systems, service readiness, and provider behavior shape the health returns to user fee removal, and how those factors interact with the geographic and informational constraints that limit effective coverage in settings like Senegal.

8 Conclusion

This paper studies Senegal's policy of free health care for children under five using facility and household survey data. The policy was broadly implemented in public facilities and substantially reduced out-of-pocket spending during sick-child visits. It also increased the likelihood that caregivers received take-home medications. Household survey data reveal a large gap between formal eligibility and effective coverage: fewer than half of eligible children's caregivers report that the child is covered, and reported coverage varies sharply

across regions.

The mortality analysis shows that the policy reduced child mortality in areas with better baseline access to public health facilities. Removing user fees can generate meaningful health gains, but only where children can reach care and households are aware that care is free. The results point to a practical limit of fee removal as a stand-alone reform: financial protection matters, but its effects depend on the reach and functioning of the health system.

The central lesson for Senegal and for similar settings is that legal entitlement is not sufficient. Free-care policies need to be accompanied by efforts to expand geographic access, improve caregiver awareness of eligibility, and strengthen the quality of care, especially in rural and underserved areas. Without those complementary investments, gains in utilization and financial protection are less likely to translate into the full potential improvements in child survival.

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