Effectiveness of antimalarial interventions in Nigeria: Evidence from facility-level longitudinal data

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Objective: To evaluate the effectiveness of a program of antimalarial interventions implemented in 2010-2013 in Niger State, Nigeria.

Data Sources: Utilization reports from 99 intervention and 51 non-intervention health facilities from the Niger State Malaria Elimination Program, supplemented by data on facility-level characteristics from the Niger State Primary Health Care Development Agency and Local Government Malaria Control units.

Study Design: Estimated with mixed-effects negative binomial modeling, a difference-in-differences method was used to quantify the impact of the program on the number of febrile illness cases and confirmed malaria cases. Potential confounding factors, non-stationarity, seasonality, and autocorrelation were explicitly accounted for.

Data Extraction Methods: Data were retrieved from hard copies of utilization reports and manually inputted to create a panel of 5550 facility-month observations.

Principal Findings: The program was implemented in two phases. The first phase (August 2010-June 2012) involved the provision of free artemisinin-based combination therapies, long-lasting insecticidal nets, and intermittent preventive treatments. In the second phase (July 2012-March 2013), the program introduced an additional intervention: free parasite-based rapid diagnostic tests. Compared to the pre-intervention period, the average number of monthly febrile illness and malaria cases increased by 20.876 ($P < 0.01$) and 22.835 ($P < 0.01$) in the first phase, and by 19.007 ($P < 0.05$) and 19.681 ($P < 0.05$) in the second phase, respectively. The results are consistent across different evaluation methods.

Conclusions: This study suggests that user-fee removal leads to increased utilization of antimalarial services. It motivates future studies to cautiously select their investigative methods.

KEYWORDS

difference-in-differences, malaria, Nigeria

1 | INTRODUCTION

With the number of global malaria-attributable deaths estimated at 438 000 in 2015, malaria is a major public health concern.¹ The disease imposes a burden on the household and the economy, leading, *inter alia*, to a higher probability of catastrophic expenditures

and a lower level of human capital.² Despite the fact that malaria is treatable and related services are available at a low cost, antimalarial services remain generally underutilized.¹

In resource-poor settings, several barriers exist. Examples include stock outages of antimalarial commodities,³ prohibitively costly distance to the nearest provider,⁴ the relative shortage of

health personnel in malaria control,³ and social insensitivities in the delivery of antimalarial services.⁵ One of the most important barriers remains financial;^{6,7} poorer households are unable to obtain antimalarial services, despite their availability, physical accessibility, and social acceptability.

To improve access to antimalarial services, many governments have lifted financial barriers and removed user fees. While most studies find a statistically significant association between user-fee removal and increased utilization of antimalarial services,^{3,6–8} effectiveness of the policy is still regarded as inconclusive.⁹

Given that the overwhelming methodology of choice is difference-in-differences (DD) with routine facility data,^{7,9–11} shortcomings of the recent literature may be identified. They call into question the credibility of previously estimated results. First, the period within which user-fee removal is evaluated in existing studies has been considered too short. The limited data length implies that only immediate, but not longer-term, impacts are captured, leading to an imperfectly informed policy decision.^{9,11,12} Second, a relatively weak research design has been used. Difference-in-differences estimates in existing studies are likely to be biased, given the baseline non-comparability of intervention vis-à-vis control groups.^{9,11,13,14} Finally, statistical analyses performed in earlier studies have been too simplistic. Given the increased use of longitudinal data, it has been demonstrated that existing studies have inadequately tackled the issues of non-stationarity, autocorrelation, and seasonality, which could lead to incorrect statistical inferences.^{9,15,16}

This study evaluates a large-scale program that lifted user fees for antimalarial services at selected health facilities in 2010–2013 in Niger State, Nigeria—a country that has been harshly affected by the disease. The program was split into two phases, with the second phase introducing an additional intervention that was complementary to the first phase. Addressing the above shortcomings explicitly, the evaluation is performed separately for both phases. It is hypothesized that (a) the program was conducive to increased utilization of antimalarial services overall, and (b) the impacts were more pronounced in the second phase, given the complementarity of the interventions.

2 | METHODS

2.1 | Study settings

With 95 percent of the general population at risk and 50 percent of the adult population undergoing at least one episode annually, Nigeria carries the heaviest malaria burden in the world.¹⁷ Situated in the North Central region and considered the largest state in the country, Niger State is no exception. Malaria accounts for 65 percent of all outpatient visits and constitutes a leading cause of child and maternal mortality in the state.¹⁸

The Niger State government has implemented numerous measures to battle malaria. Through nationwide health care reforms, malaria treatment in public facilities has been subsidized since 2010.¹⁸

In August 2010–March 2016, with assistance from international donors, the Niger State government was able to adopt the WHO's guidelines for malaria control,³ and implement a program of antimalarial interventions, providing free antimalarial services and commodities at selected health facilities.

During the first phase in August 2010–June 2012, two types of interventions were provided.¹⁸ The first intervention was malaria case management (MCM), which entailed a prompt provision of artemisinin-based combination therapies (ACTs) to febrile patients. The second intervention was the provision of long-lasting insecticidal bed nets (LLINs) and intermittent preventive treatments of malaria (IPTs) for pregnant women. With a view to preventing (rather than treating) malaria, pregnant women who visited a supported facility would be given an LLIN during their first antenatal visit as well as the first dose of sulfadoxine-pyrimethamine at 16 weeks and the second dose at 22 weeks.

In July 2012, the program expanded and included an additional intervention: parasite-based rapid diagnostic tests (RDTs). The purpose was to screen out non-malarial febrile illnesses, confirming treatment needs. This intervention and the first two interventions were implemented together from July 2012 to March 2016.

A total of 375—out of 1585—government health facilities across all 25 local government areas (LGAs) were included in the program.¹⁸ The State Malaria Control Program and the Ministry for Local Government jointly undertook the selection. Examples of selection criteria included the fact that the facility had to (a) have antenatal services, (b) have an efficient system for drug storage, and (c) never have had prior interventions.¹⁹ The implementation was orchestrated at the state level. Selected facilities were provided with technical assistance and fully subsidized antimalarial commodities, which were delivered directly to them.¹⁸ They were not offered monetary incentives to participate in the program.

2.2 | Data

Data were drawn from three independent sources. Facility characteristics were retrieved from the Niger State Primary Health Care Development Agency and Local Government Malaria Control units. Utilization data were taken from monthly reports at the Niger State Malaria Elimination Program (SMEP), which, during the data-collection period, were available only in hard copy. (They were later digitized in 2016, yet the digital version has incomplete records and contains data from 2013 onward only—after the interventions had been introduced—making it unsuitable for pre-post evaluations.) Under the supervision of SMEP staff, data were manually inputted, with facility names de-identified prior to analyses.

The final sample was determined, based on data completeness and a sampling of representative LGAs. Following the SMEP's definition of LGA-specific burden of febrile illnesses, that is, the percentage of febrile patients (ie, those whose body temperature exceeds 37.8 degrees Celsius for at least 3 days) to the population within 1 year, the LGAs in Niger State were classified into three groups.²⁰ The first group had a 0–10 percent (low) burden, consisting of four

TABLE 1 Selected local government areas in the sample

LGA	Burden of febrile illnesses	Geopolitical zone	Facility types		Total
			Intervention	Non-intervention	
Bida	Low	South	11	0	11
Shiroro	Low	East	15	13	28
Lapai	Medium	South	15	5	20
Bosso	Medium	East	15	3	18
Kontagora	Medium	North	15	10	25
Agwara	High	North	13	11	24
Paikoro	High	East	15	9	24
N			99	51	150

LGAs from two geopolitical zones (Niger South and Niger East Senatorial Districts). The second group had an 11-20 percent (medium) burden, comprising 18 LGAs from three zones (Niger South, Niger East, and Niger North). The final group included three LGAs from two zones (Niger East and Niger North), with an above 20 percent (high) burden. Finally, seven LGAs were selected from the three groups and, within each group, from each of the geopolitical zones. In cases where there was more than one LGA within each group/geopolitical zone subcategory, the LGA was randomly chosen. Details of the selected LGAs are provided in Table 1.

The selected LGAs were similar and different at the same time. While they had in common a mixture of primary and secondary health facilities and a perennial malaria transmission pattern, they were different in terms of spoken languages and ways of life. For example, among the medium-burden group, Bosso was inhabited by a Gbagyis-speaking tribe, Kontagora by a Hausas-speaking tribe, and Lapai by a Nupes-speaking tribe, representing the three major tribes in the state. No two LGAs in the study were completely alike, and it is argued that, together, the seven LGAs were representative of the state.

The sample was composed of all facilities with complete data (N = 150) in the seven LGAs. It included 99 facilities that received support from the program and consequently did not charge user fees for antimalarial services ("intervention facilities," henceforth) as well as 51 facilities that did not receive such support ("non-intervention facilities," henceforth). For each facility, 37 months of data (March 2010-March 2013) were collected, amounting to a panel of 5550 facility-month observations. The sample constituted 29 percent of facilities with complete monthly reports; only 523 out of 1585 government health facilities in Niger State reported data on a regular basis. Given the variation in terms of location, size, and operational level of facilities included, it is argued that the sample was reasonably representative of facilities at the state level.

2.3 | Empirical approach

The outcomes of interest include the number of febrile illness cases and the number of confirmed malaria cases, treated at a government

health facility in a month. The first outcome captures the extent to which antimalarial services were accessible. It does not necessarily imply actual utilization, but rather addresses how the Niger population acted upon (as opposed to ignored) their suspicion of malaria. The second outcome indicates actual utilization of antimalarial services.

Consistent with existing studies,^{13,21,22} the evaluation method for this study is difference-in-differences (DD). Based on mixed-effects negative binomial modeling,¹⁰ the preferred specification is given by:

$$Y_{ijt} = \beta_0 + \beta_1 \text{INT}_{ij} + \beta_2 \text{POST}_t + \beta_3 \text{INT}_{ij} * \text{POST}_t + X'_{ijt} \gamma_1 + \text{LGA}'_j \gamma_2 + \alpha_1 \text{TREND}_t + \text{MTH}'_t \alpha_2 + \alpha_3 \text{PEAK}_t + u_{ij} + \varepsilon_{ijt} \quad (1)$$

Subscripts i , j , and t represent facility, LGA, and month, respectively. Y_{ijt} denotes the outcome of interest for facility i in LGA j at month t . INT_{ij} is equal to 1 if facility i was an intervention facility, and 0 otherwise. POST_t is equal to 1 in the months where the interventions were implemented, and 0 otherwise. $\text{INT}_{ij} * \text{POST}_t$ is the interaction term. X_{ijt} represents a vector of facility characteristics. LGA_j denotes a vector of indicator variables representing each of the LGAs. TREND_t is a linear time trend that starts from 1 at the beginning of the data-collection period and increases by 1 with each passing month until the end of the data-collection period (37). MTH_t is a vector of indicator variables representing each of the 12 months in the calendar year. PEAK_t is an indicator variable that takes the value of 1 for July-November entries and 0 otherwise. The definition is based on the 2013 report of the Niger State Ministry of Health,¹⁸ which suggests that malaria transmission in the area has a seasonal peak in July-November. u_{ij} represents facility-level random intercepts and ε_{ijt} the idiosyncratic error term. β , γ , and α are the parameters to be estimated. β_3 captures the DD (program) effect.

Different analyses can be performed. The data-collection period can be sectioned into the following:

- Period 1 (March-July 2010) where none of the facilities provided antimalarial services for free;

- Period 2 (August 2010-June 2012) where ACTs (MCM), LLINs, and IPTs were provided free-of-charge at intervention facilities; and
- Period 3 (July 2012-March 2013) where RDTs were added to the package of free antimalarial interventions at intervention facilities.

The panel of 37 months is sufficiently long to capture impacts of (a) the free provision of ACTs, LLINs, and IPTs, comparing Period 2 against Period 1, and (b) the free provision of the entire package of antimalarial interventions (inclusive of ACTs, LLINs, IPTs, and RDTs), comparing Period 3 against Period 1. Altogether, four DD estimates are derived in the analyses below, two for each of the outcome variables.

The preferred specification is deliberate. The inclusion of the first three variables in Equation 1 (INT_{ijt} , $POST_t$, and $INT_{ijt} * POST_t$) follows the standard DD approach. The other explanatory variables need further explanation. Vector X_{ijt} represents time-specific facility characteristics. It includes (a) whether facility i provided primary or secondary care, (b) the number of beds, (c) the number of skilled health personnel, (d) the ratio of female skilled health personnel, (e) the number of support staff, (f) distance from the facility to the farthest community served, and (g) distance from the facility to the nearest free-flowing water source. These variables are intentionally

sourced for three reasons. First, they represent confounding factors that can be used to account for baseline differences between intervention and non-intervention facilities and for the non-random assignment of the intervention status. Second, as the unit of analysis is facility, it is important to view the outcome variables as outputs of a firm and to specify the regression to reflect the firm's production function. Here, the specification includes proxies of capital and labor inputs, as well as measures of technical capacity, representing factors of production. Finally, X_{ijt} is needed to encapsulate the multidimensionality of access to health care.⁴ In light of the fact that the intervention status (INT_{ijt}) captures the affordability dimension of access, X_{ijt} addresses the dimensions of social acceptability and physical accessibility through the following variables respectively: the ratio of female skilled health personnel and distance to the farthest community served.

The inclusion of the LGA fixed effects (LGA_j) is consistent with existing studies.²³⁻²⁵ They are used to capture unobserved, time-invariant area characteristics and used as the offset for incidence rates, accounting for the varying size of the target population across LGAs.^{10,26}

The variables $TREND_t$, MTH_t , and $PEAK_t$, as well as the facility-level random intercepts (u_{ij}), are intended to reflect the repeated

TABLE 2 Descriptive statistics of outcome variables and difference-in-differences (DD) estimates without covariates

Facility type/period	Number of monthly febrile illness cases		Number of monthly malaria cases	
	Intervention facilities	Non-intervention facilities	Intervention facilities	Non-intervention facilities
Panel A: descriptive statistics of outcomes of interest across periods				
Entire sample period (37 months)	116.362 (126.267)	69.860 (121.786)	87.491 (97.348)	52.974 (90.479)
Period 1: January-July 2010 (Pre-intervention period)	66.162 (84.308)	50.616 (91.385)	45.701 (51.601)	37.749 (68.702)
Period 2: August 2010-June 2012 (Post-intervention: Phase 1)	116.426 (119.579)	71.142 (127.840)	88.458 (92.612)	53.152 (90.880)
Period 3: July 2012-June 2013 (Post-intervention: Phase 2)	144.086 (151.384)	77.277 (119.824)	108.237 (119.064)	60.978 (98.820)
Difference: period 2 vs Period 1	50.265*** [5.658]	20.526*** [8.440]	42.757*** [4.301]	15.403*** [6.035]
Difference: period 3 vs Period 1	77.925*** [7.368]	26.661*** [8.632]	62.536*** [5.624]	23.229*** [6.971]
Panel B: mean comparison between facility types				
Difference-in-differences: period 2 vs Period 1	29.739*** [1.746]		27.354*** [1.482]	
Difference-in-differences: period 3 vs Period 1	51.264*** [2.902]		39.307*** [2.391]	

Notes: Standard deviations are in parentheses and standard errors in square brackets. Mean differences in Panel A are based on t tests, assuming equal variances. The DD estimates in Panel B are based on t tests, assuming equal variances, and do not account for any covariates.

*10% level of significance, **5% level of significance, ***1% level of significance.

measurement of the outcome variables across time. $TREND_t$ accounts for non-stationarity of the outcome variables. MTH_t and $PEAK_t$ address the seasonality of malaria transmission that is specific to Niger State. Finally, u_{ij} tackles the problem of autocorrelation, allowing for temporal variations of the outcomes at the facility level. The inclusion of a linear trend^{24,26–28} and month fixed effects,^{23,26,29} and the use of mixed-effects modeling^{10,26–28,30} are observed in other policy-evaluation studies.

Altogether, the DD specification in this study improves the validity and the precision of the estimated program effects. The addition of X_{ijt} and LGA fixed effects minimizes estimation bias and offers a contribution over existing studies using facility-level data, which do not always account for confounding factors.^{8,21} The incorporation of $TREND_t$, MTH_t , $PEAK_t$, and u_{ij} ensures that appropriate statistical inferences can be made, addressing non-stationarity, seasonality, and autocorrelation that have been largely ignored in the literature.¹⁶

2.4 | Justification of the econometric model

The negative binomial model is carefully selected. It is compared against alternative count-data models, and proves to be most appropriate for this study. The negative binomial model has been shown to outperform log-linear OLS^{7,16,31} and regular Poisson models in cases of over-dispersion;^{29,32} Table 2 (explained below) shows that, for both outcomes of interest, the variances (SD-squared) are much larger than the means. It is also favored over zero-inflated models, following the fact that zero observations make up <5 percent of the sample. Finally, the negative binomial model is preferred to the overdispersed Poisson model. Deviance and AIC statistics of the two models, re-estimated as generalized linear models, were calculated, based on the preferred specification (without the hierarchical structure of the data). For the overdispersed Poisson model, standard errors were scaled based on the square root of Pearson chi-square-based dispersion. For the negative binomial model, standard errors were scaled based on the estimated alpha. The diagnostic statistics (unreported) were consistently lower for the negative binomial model. The exercise suggests that the negative binomial model provides a better fit.

2.5 | Justification of the evaluation method

This study employs the difference-in-differences (DD) design, which is compared against two alternative evaluation methods. The first method is interrupted time series (ITS), consistent with studies that evaluate national policy changes using routine facility data.^{29,31,33} In contrast with DD, the ITS design typically considers only the intervention group, using the pre-intervention period as the reference. The program effect is measured as a shift in the outcomes *among* the intervention group that is distinct from the pre-intervention trend. Nevertheless, as shown in Appendix S1, in this paper, the regression is run separately on both the intervention and the non-intervention groups to strengthen the ITS analysis, consistent with Zuckerman et al³³ and suggestions made by Lagarde.¹⁶ The idea is to compare

whether a change in the outcome variables can be more evidently observed among the intervention than the non-intervention group. The purpose of exploring the ITS design in addition to DD is to provide robustness checks. The DD design has indeed been considered the more rigorous approach, as it explicitly incorporates the non-intervention group, thereby having better degrees of freedom and more generalizability.³⁰

The second method is comparative/controlled interrupted time series (CITS). The method has been proved to be valid and precise,^{24,27,28,30} and is considered more flexible than DD,^{25,27,30} rendering it superior under certain conditions. More specifically, while the DD method assumes that outcome trends are similar, that is, “common,” between the intervention and the non-intervention groups, the CITS method relaxes the assumption and allows those trends to diverge.^{27,30} The CITS design is considered more appropriate if trends of the outcomes of interest statistically differ between the intervention and the non-intervention groups. The DD design is preferred otherwise.

Results from the ITS and the CITS methods are shown in Tables A.1 and A.2 in Appendix S1, respectively. Incidence rate ratios (IRR) are reported based on mixed-effects negative binomial modeling. Table A.1 reveals that the ITS results are qualitatively similar to the DD results discussed below, demonstrating robustness of the preferred model. Table A.2 shows that DD is preferred to CITS; the diverging trend hypothesis does not hold statistically, leading to overfitting under CITS.³⁰ An explanation why CITS performs less well here compared to other studies^{24,27,28,30} is perhaps the fact that the number of pre-intervention data points in this study is too small (five data points: March–July 2010) to properly establish pre-intervention trends and observe trend divergence.^{16,25,27}

2.6 | Justification of the methods to improve statistical inference

This study improves the statistical inference of the DD estimates, addressing seasonality, non-stationarity, and autocorrelation through the use of month fixed effects, a linear time trend, and mixed-effects modeling, respectively. While the method used to incorporate seasonality here is consistent with existing studies,^{10,26,29} the literature differs on how to deal with non-stationarity and autocorrelation. To capture non-stationarity, time fixed effects have alternatively been used^{21,23,25} and, in certain instances, they have been shown to outperform a linear trend in terms of model fit.^{21,25} Also, to correct for the presence of first-order autocorrelation, clustered Huber-White standard errors have been employed, instead of mixed-effects modeling.^{15,21,23,33} Since it is not clear how these methods should be compared in practice,²⁵ Table A.3 in Appendix S1 offers results from negative binomial DD regressions, using time fixed effects and standard errors clustered at the LGA level.^{15,21,23} The results reveal a pattern that is similar to the preferred estimates below, and show that using time fixed effects does not statistically lead to a higher log-likelihood value, compared to a linear trend. Consistent with the ITS and CITS exercises, the use of alternative methods to account

for non-stationarity and autocorrelation reinforces the conclusions of this paper, illustrating their robustness.^{25,27}

3 | RESULTS

3.1 | Descriptive statistics

Table 2 presents descriptive statistics of the outcome variables: febrile illness cases and confirmed malaria cases. The table is divided into two panels. Panel A shows the averages and standard deviations of the outcome variables in the intervention and the non-intervention groups in four periods: the entire data-collection period of 37 months, as well as the above-defined Periods 1, 2, and 3. Within each group, mean differences between (a) Period 2 and Period 1 and (b) Period 3 and Period 1 are provided and tested using *t* tests with an equal-variance assumption. Panel B presents differences of the mean differences between the intervention and the non-intervention facilities. They are equivalent to difference-in-differences estimates, with covariates unaccounted for. Statistical significance of these estimates is based on equal-variance *t* tests.

According to Table 2, the antimalarial interventions seemed to be effective in increasing the number of febrile illness cases and confirmed malaria cases. The introduction of free ACTs, LLINs, and IPTs was associated with an average increase of 29.739 and 27.354 cases of febrile illness and malaria per facility per month, respectively. The addition of free RDTs led to a larger increase of 51.264 and 39.307 cases respectively. Nevertheless, these estimates are only preliminary, as they do not account for facility and LGA characteristics.

Table 3 shows descriptive statistics of facility characteristics. The intervention and the non-intervention groups are compared, using *t* tests. On average, the proportion of secondary-care facilities was 6.8 percent lower among the intervention group. The numbers of beds, skilled health personnel, and support staff were also lower, while the ratio of female skilled health personnel and distance to the nearest source of water were larger among the intervention group. Altogether, the table suggests that the intervention facilities provided less advanced care, were smaller in size, were further away from a mosquito-breeding site, and had a larger percentage of female health personnel. These differences are significant at the 1-5 percent level.

3.2 | Difference-in-differences evaluation

Based on the preferred specification, the results are provided in Table 4. The table is split into two panels, with Panels A and B representing each of the outcome variables. Column I refers to the comparison between Periods 2 and 1, capturing the impacts of the first phase of the program. Column II refers to the comparison between Periods 3 and 1, showing the impacts of the second phase. The bottom part of the table explicitly discusses the empirical approach. The DD estimates in Table 4 are based on the interaction term in Equation 1 ($INT_{ij} * POST_t$). The incidence rate ratios (IRR) (provided in the same fashion as previous studies^{10,26}) and the marginal effects calculated against the predicted number of events, conditional on the random intercepts, are shown. All regression equations pass the Wald test, and the likelihood ratio (LR) tests justify the random-intercepts model. Overall, Table 4 offers support for the

TABLE 3 Descriptive statistics of facility-level characteristics

Variables	Nature of variables	Intervention facilities	Non-intervention facilities	Mean difference
Primary care = 1 [Excluded category]	Time-invariant	0.990 (0.100)	0.922 (0.269)	0.068*** [0.005]
Secondary care = 1	Time-invariant	0.010 (0.100)	0.078 (0.269)	-0.068*** [0.005]
Number of beds	Time-invariant	3.611 (6.172)	5.212 (11.869)	-1.601*** [0.242]
Number of skilled health personnel	Time-varying (monthly)	5.372 (7.646)	5.873 (13.482)	-0.501** [0.284]
Ratio of female skilled health personnel	Time-varying (monthly)	0.548 (0.369)	0.492 (0.390)	0.057*** [0.011]
Number of support staff	Time-varying (monthly)	3.201 (4.023)	6.541 (23.084)	-3.340*** [0.392]
Distance to farthest community served in km	Time-invariant	7.545 (6.612)	7.497 (4.082)	0.048 [0.166]
Distance to the nearest source of water in km	Time-invariant	4.091 (4.239)	3.764 (4.256)	0.327** [0.120]
N		3663	1887	

Notes: Standard deviations are in parentheses and standard errors in square brackets. Mean differences are based on *t* tests, assuming equal variances. *10% level of significance, **5% level of significance, ***1% level of significance.

TABLE 4 Effects of antimalarial interventions: difference-in-differences (DD) estimates from mixed-effects negative binomial regressions

DD Estimates	First phase of intervention (Period 2 vs period 1) (I)	Second phase of intervention (Period 3 vs period 1) (II)
Panel A: Outcome variable = Number of febrile illness cases		
IRR [95% CI]	1.222*** [1.079, 1.383]	1.184** [1.016, 1.379]
Marginal effects	20.876*** [6.856]	19.007** [8.926]
Log likelihood	-21 397.153	-10 865.192
LR test	1965.550***	750.150***
Panel B: Outcome variable = Number of confirmed malaria cases		
IRR [95% CI]	1.265*** [1.102, 1.451]	1.228** [1.042, 1.446]
Marginal effects	22.835*** [7.523]	19.681** [8.417]
Log likelihood	-20 131.286	-10 247.998
LR test	2260.570***	805.290***
N	4200	2100
Bias reduction	Facility characteristics and LGA fixed effects	
Seasonality	Month fixed effects and seasonal peak indicator	
Non-stationarity	Time trend	
Autocorrelation	Facility-level random intercepts	

Notes: The 95% confidence intervals of the IRRs are shown in square brackets in the same row. Standard errors of the marginal effects are in square brackets in the row below. Marginal effects are calculated against the predict counts (number of febrile illness and confirmed malaria cases), conditional on the random intercepts. LR test refers to the likelihood ratio test against a regular negative binomial model of the same specification without facility-level random intercepts.

*10% level of significance, **5% level of significance, ***1% level of significance.

interventions. The DD impacts are positive and statistically significant; with the removal of user charges, facilities were able to provide care for more patients with symptoms of malaria. Focusing on the marginal effects, the introduction of free ACTs, LLINs, and IPTs was associated with an increase, at the 1 percent level, of 20.876 and 22.835 cases of febrile illness and malaria per month, respectively. The expansion to include RDTs led to an increase, at the 5 percent level, of 19.007 and 19.681 cases of febrile illness and malaria per month, respectively.

4 | CONCLUSIONS AND DISCUSSION

This study evaluates the impact of free antimalarial interventions, namely ACTs, LLINs, and IPTs provided in August 2010-June 2012 (ie, the first phase of the program) and the addition of RDTs from July 2012 onwards (ie, the second phase) in Niger State, Nigeria. The outcomes of interest include the number of febrile illness cases and confirmed malaria cases. Based on 37 months of data from 99 intervention and 51 non-intervention facilities, this study uses the difference-in-differences (DD) method estimated with a negative binomial model with facility-level random intercepts. The preferred specification accounts not only for facility and LGA characteristics to minimize bias, but also for non-stationarity, seasonality, and autocorrelation to improve statistical inferences. The results suggest

that the interventions were effective in stimulating health care utilization among malaria-potential (febrile) and malaria-confirmed patients. Compared to the pre-intervention period, the first phase of the program was associated with an increase of 20.876 and 22.835 cases, and the subsequent addition of RDTs led to an increase of 19.007 and 19.681 cases of febrile illness and malaria per month, respectively.

The fact that the overall program led to increased utilization of antimalarial services is consistent with the demand theory. Considering that the probability of obtaining free services among febrile patients (who may or may not have had malaria) was higher and that the expected cost was lower at an intervention than a non-intervention facility, it is unsurprising that the number of febrile illness and confirmed malaria cases was higher among intervention facilities.

The fact that the impacts were less strong in the second phase requires further explanation. The demand theory suggests that the impacts should be stronger in the second phase. The addition of free RDTs should increase utilization of RDTs, through the own-price effect, and that of ACTs, LLINs, and IPTs, through the cross-price effect (given the complementarity of these commodities), leading to a greater number of febrile illness and confirmed malaria cases. Nevertheless, the demand theory makes the *ceteris paribus* assumption: that all things remain constant. The assumption does not hold in dynamic settings. Empirical evidence has indicated that,

if implemented properly for a sufficiently long period, antimalarial interventions can interrupt malaria transmission, reducing the incidence rate and, consequently, the number of malaria-related facility visits over time.^{8,11,13} The introduction of free RDTs, in particular, has been associated with a lower rate of clinical services utilization, with non-malaria cases being more effectively screened out.¹⁰ The decline in the utilization rate at a later stage can also be attributed to a lower level of enthusiasm among potential patients, with social mobilization efforts dwindling down as the program progresses.²⁶ It is argued that the impacts observed in the second phase here were a likely result of the above counteracting effects combined.

There are limitations to the study. First, the study may still be subjected to estimation bias. There may have been observed and unobserved qualities of the selected facilities—unrelated to the interventions—that drove the results. Observed characteristics among the intervention and the non-intervention groups clearly differed, as demonstrated by Tables 2 and 3. Unobserved differences may have also existed. The literature suggests that there are time-varying determinants of antimalarial services utilization that are difficult to account for. Examples include quality of care provided at health facilities,^{9,13,26} as well as the level of protective immunity^{11,12} and exposure to malaria vectors¹¹ among different populations in different areas, all of which may change after the introduction of a large-scale intervention program and may impact the program effects.

However, an attempt has been made to reduce estimation bias in this paper. In contrast with existing studies,^{21,31} the preferred specification here includes explanatory variables that reflect the selection criteria and control for (observed) baseline differences between intervention and non-intervention facilities. The inclusion of facility characteristics and LGA fixed effects (Table 4) not only improves the fit, but also suppresses the estimates (Table 2), which may be taken as a sign that bias weakens.¹⁴ It is worth noting also that an imperfect study design is common among evaluation studies;^{9,14} it is often infeasible to undertake a study that randomly assigns interventions into some sites (thereby circumventing selection bias) while ignoring others that may also need them.

Also, the data used may be subjected to sample selection bias. As mentioned earlier, only 523 out of 1585 health facilities reported utilization data consistently. There may have been characteristics that systematically differentiated reporting from non-reporting facilities. Moreover, there was likely some reporting bias. Among facilities that reported utilization data, there may have been a non-monetary incentive to misreport or be selective in what was reported. These issues may potentially bias the estimates upward.

Nevertheless, it should be emphasized that most data are imperfect. This can be generalized to individual-level data, which are typically considered superior in terms of sample size and breadth of information contained therein. Specifically, individual-level data are susceptible to at least two types of bias: (a) recall bias, as the information is extracted from subjectively answered questions,²² and (b) response bias, as the survey may not be well understood by the respondents. Albeit also imperfect, facility-level data can be used to complement individual-level data. An advantage of facility-level data

is that they are collected through standard-format reports, making the information acquisition process more objective and perhaps less prone to measurement error.

Despite its limitations, the study has many contributions. First, this study adds to the literature of user-fee removal. It carefully encompasses several dimensions of health care access in the analyses, for example, the number of skilled health personnel and geographical distance between the facility and the farthest community served, so that the affordability dimension can be more effectively investigated. The results suggest that the removal of facility-levied fees can lead to increased utilization of antimalarial services, even after controlling for other dimensions of access to health care.

The study also contributes methodologically. As discussed in the Methods section and shown partly in Appendix S1, this study explores an array of evaluation methods (DD, ITS, and CITS) and regression models (overdispersed Poisson and negative binomial, with time trends vs time fixed effects, with random intercepts vs clustered standard errors). It illustrates that, despite quantitative differences, the results are consistent. In so doing, this study motivates future studies to cautiously select their methodology and double-check whether their conclusions are robust across methodological options. Compared to existing studies using facility data that typically have a smaller sample size, this study also collects a long panel of data (N = 5550), which allows for a more thorough investigation of temporal changes. Finally, this study provides results that are grounded on a robust empirical analysis, accounting for facility characteristics and area fixed effects to reduce estimation bias and addressing inference concerns associated with longitudinal data, namely non-stationarity, seasonality, and autocorrelation. The methodological contributions are deliberately undertaken to make up for the imperfect study design.

The quantification of more accurate estimates would require a more comprehensive dataset that includes also non-reporting facilities and a prospective evaluation design that allows for a cleaner, more randomized distinction between the intervention and control groups. This will need to be left with future studies.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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