Unlocking Health Potential: Effects of Free Maternal and Child Health Program*

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We investigate the health impacts of Nigeria's free maternal and child health program (FMCHP) using a difference-in-differences empirical framework. Results show reduced under-five mortality and increased birth weights, with effects more pronounced in disadvantaged populations. Access to healthcare facilities enhances improvements in child mortality. Increased demand for institutional and preventive care likely drives these outcomes. The FMCHP may have prevented 405,425 under-five deaths over six years. Our findings suggest that improving access to institutional healthcare during pregnancy can enhance maternal and child health outcomes in areas with low healthcare utilization. These conclusions are robust to various empirical checks.

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1 Introduction

Can the reduction of medical costs incentivize the utilization of institutional healthcare? How does this effect differ based on access to health facilities? In the absence of supply-side changes, does congestion at health facilities worsen? Can community outreach help inform potential beneficiaries of medical cost reduction programs about their benefits?

As progress on improving child and maternal health across the world has stalled, increasing attention is being paid to programs that reduce medical costs. One such program is user fee elimination (World Health Organization , WHO). While the canonical economics models of demand for health predict an increase in healthcare demand when its price decreases, they fail to account for the broader social context (Grossman, 1972, 2000). These models assume that there are no capacity constraints in institutional healthcare. In a setting where the supply of healthcare does not respond to a decline in prices, congestion at existing health facilities may worsen (Kondo and Shigeoka, 2013). This may induce the relatively better-off population to seek healthcare at higher-quality institutions which in turn would worsen the existing socioeconomic inequities in health outcomes. One way in which agents in these models may form their preferences about health and consequently determine their healthcare demand is through existing norms and perceptions about healthcare institutions. Thus, policies that aim to reduce imperfections in the knowledge about healthcare institutions, such as community outreach and frequent interactions with medical providers, may engender health preferences. Despite the apparent importance of out-of-pocket (OOP) cost elimination programs, the existing literature can be characterized as conflictory at best. The conflicting evidence might be a result of insufficient statistical power to detect the effect on child mortality (Kremer et al., 2023).

In this paper, we study the health impacts of a free maternal and child health program (FMCHP). Our setting is Nigeria, a West African country with some of the highest rates of child mortality and very low levels of institutional deliveries (see Figure A1). The policy we examine is rolled out in a staggered fashion across twelve out of 36 states in the country. Each phase is initiated in six states. The first phase starts in October 2008 and the second phase in December 2009. FMCHP aims to provide free access

to primary health facilities along with referral to general hospitals for complicated pregnancies. While pregnant mothers are eligible beneficiaries from the time of pregnancy confirmation to six weeks after the delivery, their children can avail of the policy benefits till their fifth birthday. The FMCHP puts a great deal of emphasis on community outreach and engagement to refine the existing perceptions and norms related to institutional healthcare.

We leverage multiple data sources to evaluate the health effects of FMCHP exposure on mothers and their children. For almost all our outcome variables, we use three rounds of Demographic and Health Surveys (DHS). The respondents in these rounds are surveyed between 2003 and 2018. Although initially envisaged to be restricted to certain local government areas (LGA) in the exposed states, the program eventually enrolls all pregnant women as long as they can establish their residency in the state. Thus, we treat all women and children residing in a state where the policy is rolled out to be potential beneficiaries. These data also provide us with individual and household characteristics information that we use for various heterogeneity analyses. Additionally, information on demand for institutional and preventive care is used to examine potential mechanisms driving the estimated changes in health outcomes. We complement DHS data with the data on the universe of health facilities in Nigeria to examine if access to such facilities exacerbates the health effects for the mother and her child due to FMCHP exposure.

To uncover causal estimates of FMCHP exposure on maternal and child health outcomes, we use a mother fixed-effects empirical specification. This approach allows us to account for time-invariant mother-level characteristics that determine exposure to FMCHP and at the same time influence health outcomes. Through a series of empirical checks, we establish the validity of our research design. In particular, we show that our estimates are not subject to trends in the outcome variables that differ across the births that are and are not exposed to the FMCHP (see Figure A6). Our estimates are also robust to accounting for the staggered roll out of FMCHP where a conventional two-way-fixed-effects (TWFE) empirical specification is prone to "forbidden" comparisons between earlier and later treated units and heterogeneous treatment effects.

We find that children born after the FMCHP roll out are more likely to survive beyond their fifth birth-

day and also weigh more at birth. The decline in under-five mortality is large at approximately 14% of the pre-treatment mean for the treated group. Suggestive evidence for improvements in infant mortality along with some child development indicators, i.e., underweight and wasting is also found. The dynamics of improvement in child mortality accord well with the anecdotal evidence suggesting funding exhaustion in the last couple of years of FMCHP implementation as the decline in child mortality during these years is statistically less precise.

FMCHP may have reduced the existing disparities in child mortality. Our results differ significantly and are more pronounced for relatively disadvantaged subpopulations. The decline in under-five mortality is concentrated in those households that are in the bottom half of the wealth distribution, follow Islam, or reside in northern parts of the county. Steeper declines in under-five mortality among poorer groups are significant, given Nigeria's high obstetric care costs. Our estimates show households spend about 26% of their annual per-capita expenditure on emergency obstetric care.[1]

It might be the case that having access to health facilities accentuates the impacts of FMCHP exposure on child health outcomes. To shed more light on such pathways, we bring to the fore novel data on the universe of health facilities in Nigeria. Using these data we can ascertain the exposure of the respondents to various healthcare facility parameters. Our estimates show that reduction in child mortality is more pronounced in the states that are in the top half of health facilities, health personnel, and hospital beds per-capita distributions. Furthermore, improvements in child health outcomes are concentrated in states that are in the top half of the pediatric facilities per-capita distribution. We establish that congestion does not change after the FMCHP roll out as women who reside in states with relatively more health facilities that are privately owned, and hence with a possibility to reallocate their place of healthcare, are not more likely to deliver their child at such facilities.

An increase in the demand for institutional care as evidenced by an increase in such care during delivery and postnatal care is a likely mechanism leading to improvements in under-five mortality. We find that after FMCHP is rolled out, institutional delivery increases which is accounted for by one-to-one

[1] Average total expenditure of $243.8 on emergency obstetric care is derived from Adamu Aisha et al. (2013). From Nigeria General Household Survey Wave One (2010-2011), we estimate that an average household expends $981 per-capita annually.

substitution away from home delivery. Furthermore, post-FMCHP deliveries are also more likely to be assisted by health personnel (doctor, nurse, or midwife). We also show that conditional on postnatal care uptake, such care is more likely to be provided by a health personnel post-FMCHP. Pre-delivery interaction through institutional and health personnel provided antenatal care along with dietary supplement receipt during pregnancy may have led to increased institutional delivery post-FMCHP. An increase in diagnostic services uptake during pregnancy post-FMCHP lends further credence to pre-delivery interaction with medical providers as a causal pathway that leads to an increase in demand for institutional care.

Finally, we also establish that community health personnel provided antenatal care may have led pregnant women to seek institutional antenatal care and be willing to deliver their children at a health institution post-FMCHP. In the backdrop of FMCHP emphasizing community outreach and engagement to reduce the knowledge gap of women, these estimates allow us to trace out a causal pathway leading to improved child mortality beginning with actions that aim to educate pregnant women about the benefits of institutional care. We are unaware of an existing study that uncovers a similar causal pathway associated with programs that aim to improve maternal and child health outcomes in a developing country and argue that it is one of the novel contributions of this work.

Improvements in child mortality potentially through increased demand for institutional care take place without an increase in the number of primary health facilities in treated states relative to control states. This suggests that before FMCHP there was substantial slack capacity[2] which was unused due to prohibitively expensive institutional healthcare. We find evidence for both significant slack capacity before and reduction of financial barriers in accessing institutional care after the FMCHP roll out. We also show that increased preventive and not acute care may lead to a reduction in the likelihood of the child dying before their fifth birthday. Children born after the FMCHP roll out are more likely to have been vaccinated while they are not more likely to seek treatment for acute ailments like fever or diarrhea.

[2]Slack capacity is defined as the difference between the annual number of births in the state that a health institution can service and the number of institutional births during the same year in that state. More information on slack capacity and its extent is provided in Section 5.4.

We subject our estimates to a back-of-the-envelope welfare analysis. We find that FMCHP may have saved 405,425 child deaths before the age of five over six years. Using the program cost measures, we document a $203.71 expense to avert the death of a child before they turn five. This cost estimate is equivalent to approximately 4% of annual average household expenditure in Nigeria. Our cost estimates while not being directly comparable, are only a fraction of the welfare costs incurred under Medicaid in the United States in the 1960s and 1970s (Goodman-Bacon, 2018).

We contribute to the existing literature examining the health impacts of medical cost reduction programs on multiple dimensions. While the existing literature uses empirical specifications that do not explicitly account for time-invariant unobservable characteristics of the pregnant women, our empirical specifications leverage changes in the exposure to FMCHP for different births experienced by the same women (Renard, 2022). As we leverage data from multiple rounds of DHS, we also examine a much larger number of health outcomes than the existing studies (Fitzpatrick, 2018; Tanaka, 2014). This helps in studying multiple dimensions of health that might be impacted by FMCHP exposure. We also make progress on rationalizing conflicting evidence on health outcome changes due to user fee removals.[3] In particular, we highlight the role of access to health facilities in improving maternal and child health outcomes. Our work also signifies the role played by community outreach in informing pregnant women about their own and their children's health. We shed light on potential mechanisms leading to improvements in maternal and child health outcomes highlighting the role of an increase in demand for institutional care and preventive care. Finally, to the best of our knowledge, not only do we provide an estimate of the number of lives saved by FMCHP, but we are also the first to estimate the cost of averting the death of a child before they turn five due to user fee removals in a Sub-Saharan Africa context.

[3] Fitzpatrick (2018) document an increase in cesarean sections, a decline in maternal mortality, and stagnation or worsening neonatal mortality in Sub-Saharan Africa. On the contrary, McKinnon et al. (2014) find that there is no effect of user fee removal for facility-based delivery but a decline in neonatal mortality emanating from an increase in facility-based delivery. Using a randomized control trial that removed user fees for children under the age of five in Ghana, Ansah et al. (2009) found no effect on various health outcomes of the exposed children. Leveraging variation in the access to free healthcare arising due to historical segregation in South Africa, Tanaka (2014) show that children exposed to user fee removal have better nutritional outcomes later in life. Utilizing several policy changes in Ghana, Friedman and Keats (2024) highlights the role of antenatal care, monetary costs of institutional deliveries, and post-natal care in reducing child mortality.

The rest of the paper proceeds as follows. We provide a brief background in Section 2. Discussion of empirical framework and data is in Section 4. Main results, robustness checks, heterogeneities, mechanism analysis, and welfare estimates are presented in Section 5. Section 6 concludes.

2 Background

In this section, we provide a brief discussion of the healthcare landscape in Nigeria. Our focus while providing this discussion, is centered around maternal and child health. Additionally, we also provide important and relevant details about the Free Maternal and Child Health Program (FMCHP).

2.1 Healthcare in Nigeria

Maternal mortality in Nigeria remains an area of concern, with an estimated 814 maternal deaths per 100,000 live births.[4] Also, Nigerian women face a significantly higher lifetime risk of maternal death compared to women in developed countries, with a 1 in 22 chance of dying during pregnancy-related events. Additionally, infant and under-five mortality rates have remained steady over the past five years, with 74 infant deaths and 117 under-five deaths per 1,000 live births.[5] These depressing statistics underscore Nigeria's urgent need for improved maternal and child health services.

Out-of-pocket (OOP) healthcare spending remains Nigeria's primary financing mechanism for health services. Across the years, OOP health expenditure consistently accounts for a substantial portion of total health spending, ranging from 60% to 75% (Aregbeshola and Khan, 2021). Reliance on OOP payments leads to significant challenges, including limited financial risk protection, unaffordability of healthcare services, and inadequate coverage across the population. Indeed, data from the demographic and health surveys (DHS) in Nigeria show that more than 8% of respondents report financial or cost

[4]More details of maternal and child mortality in Nigeria at https://data.worldbank.org/indicator/SH.STA.MMRT?locations=NG.

[5]This is more pronounced for children from the poorest households as they are twice as likely to die before their first or fifth birthday compared to children from the wealthiest households. This figure is drawn from information available at https://www.unicef.org/nigeria/media/1636/file/Nigeria-equity-profile-health.pdf.pdf.

as an impediment for not delivering at a health institution (see Figure A2). Furthermore, when the respondent is sick, cost or financial reasons make them less likely to seek healthcare in at least 60% of such instances (see Figure A3).

2.2 Free Maternal and Child Health Program (FMCHP)

Due to mounting debt problems in the 2000s, Nigeria was granted debt relief by the Paris Club under the World Bank's Heavily Indebted Poor Countries (HIPC) initiative. The relief was conditional as a significant proportion of it was to be allocated towards improving the welfare of vulnerable groups. Recognizing the poor health profile of the Nigerian population, especially women and children, the government strategically directed part of the HIPC funds towards health investments as part of their commitment to achieving the Millennium Development Goals (MDGs) (Onwujekwe et al., 2019). Free Maternal and Child Health Program (FMCHP) is part of this initiative.

The FMCHP is a pilot program rolled out in two phases in twelve states in Nigeria. Phase one of the program started in October 2008, and phase two in December 2009. Figure A4 presents the timeline of the program. In Figure A5, we plot the boundaries of the states that constitute the treatment and control groups. Both phases ended in 2015[6]. The program was designed by the National Health Insurance Scheme (NHIS), a social health insurance program managed by the Federal Government of Nigeria to provide a complementary funding source to the health sector and improve healthcare access to most Nigerians.

According to Onwujekwe et al. (2016a), the program received about 13 billion *Naira*. The expenditure on the program is approximately 10% of the federal government health expenditure in 2005, which was 130.76 billion *Naira*. The program envisaged to provide free healthcare services to $600,000$ women and children concurrently in each phase. Each phase of the program focuses on six states, providing healthcare coverage to a maximum of $100,000$ individuals per state at any time totaling 1.2 million

[6]The program initially planned to have more than two phases. However, the third phase did not start or materialize due to the lack of funds allocated to the program beginning in 2013 (Onwujekwe et al., 2016a).

individuals (FMoH, 2010). The program selects states from each of the six geopolitical zones of the country and does not consider the disease burden or mortality rate in the selection of beneficiary states for program roll out (Briscombe and McGreevey, 2010; Onwujekwe et al., 2016a).

The program provides access to essential primary maternal and child healthcare in selected states. These services include seeing a doctor for an ailment, vaccinations, and getting referred to general hospitals for complications that primary healthcare cannot address (Mohammed and Dong, 2012). The policy focuses on eliminating financial barriers to access to health care for over 1.2 million pregnant women and children under five years of age. By providing a free maternal and child healthcare pilot program, the program aims to reduce the national out-pocket expenses from 66 to 60 percent, a target set out in the National Strategic Health Development Plan (NSHDP) (FMoH, 2010). FMCHP also introduced interventions to facilitate the use of institutional healthcare. A key component of these interventions was community outreach aimed at informing and reducing the knowledge gap among potential beneficiaries (Ezenwaka et al., 2022; Onwujekwe et al., 2019).

Women may enroll from when their pregnancy is confirmed until six weeks after giving birth. On the other hand, children may register from birth until age five. Initially, phase one of the project limited the enrollment of individuals to only three LGAs. However, this restriction was later relaxed, starting with Oyo State, due to difficulties NHIS officials and HMOs encountered in achieving the target of $100,000$ enrollees within such a limited pool. Expanding the number of LGAs to six was deemed necessary to facilitate achieving enrollment goals. Anecdotal evidence suggests that a similar situation arose in other states where FMCHP was rolled out (Green, 2012).

Our treatment group is the twelve states where FMCHP is rolled out in either phase (Dasgupta et al., 2022). Due to multiple reasons, we define treatment at the state level. First, we lack information on the identity of the LGAs that experienced the FMCHP roll out. Moreover, across LGA migration within the same state is high in Nigeria with almost non-existent across state migration (Ekpenyong et al., 2019, 2022). As was discussed previously, low enrollment in the program may have led to women from all LGAs within the state to avail of free healthcare services. The FMCHP includes social accountability initiatives, such as Health Facility Committees (HFCs), that monitor the eligibility for

receiving free maternal and child healthcare services. A proof of tax payment restricts the use of free healthcare services to the residents of the states (Ogbuabor and Onwujekwe, 2018). Taken together, women residing in the state with an FMCHP roll out may have used the healthcare services provided by the program.

3 Data

To uncover the causal effect of the Free Maternal and Child Health Program (FMCHP) on health outcomes, we use multiple data sources. In this section, we describe each data source and provide descriptive statistics for the analytical sample.

3.1 Demographic and Health Surveys

We use multiple rounds of Demographic and Health Surveys (DHS) to obtain data on the outcomes of interest as well as other covariates that we use in our various specifications and for uncovering the potential mechanisms driving the main results. We combine three DHS waves: three, four, and five. The third survey wave was conducted in 2008 while the following two waves were conducted in 2013 and 2018, respectively. For our main estimation sample, we exclude all the observations for which the birth year is after 2015 as FMCHP ceased to operate after 2015.

DHS is a nationally representative survey that provides detailed information on maternal health outcomes for women aged 15 to 49. Furthermore, for all the births that the surveyed women experienced in the five years preceding the survey, detailed information on the health outcomes of the offspring is also provided. From DHS we use multiple child health outcome variables. We examine if FMCHP affects under-five and infant mortality along with child development indicators of the child being underweight, stunted, or wasted. Further, we make use of the birth weight information provided for each child who is five years or younger at the time of the survey.[7] Under-five mortality refers to the event

[7] As we discuss in Section 5, birth weight information is not available for all children who are born in the five years

of the child dying before their fifth birthday and infant mortality refers to the event of a child dying before their first birthday.[8] Using the birth history of each woman surveyed, we can obtain information on child survival for all births experienced by the respondent women.

A child is classified as being underweight if their weight-for-age z-score is below two standard deviations of World Health Organization (WHO) child growth standards. A child is classified as stunted and wasted if their height-for-age and weight-for-height z-scores are below two standard deviations of WHO child growth standards, respectively. We note that wasting results due to acute inadequate undernutrition while stunting is a consequence of chronic undernutrition (Caulfield et al., 2006; Hoddinott et al., 2013). Being underweight, on the other hand, encompasses both stunting and wasting.

DHS also provides detailed information on the individual and household characteristics. This allows us to undertake heterogeneity analysis along with studying potential mechanisms that drive the change in health outcomes documented in Section 5. Two potential mechanisms that may impact child health outcomes that we focus on are the demand for institutional care and preventive care. We describe the construction of the variables used for the mechanism analysis when we discuss these estimates in Section 5.

3.2 Descriptive Statistics

We present descriptive statistics and balance of covariates in the pre-treatment and post-treatment periods in Table A1. Each observation is a mother-child pair. The post-treatment period is determined by the timing of FMCHP roll out across states. For states where FMCHP is rolled out in the first phase, all births after October 2008 are classified as occurring in the post-treatment period. On the other hand, all

preceding the survey date.

[8] Under-five mortality might be subjected to right censoring as some of the children that are part of our estimation sample have not yet turned five. Approximately 80% of child deaths in our estimation sample happen before children turn two. When we drop children who are younger than two years of age, our estimates are extremely similar to the baseline estimate reported in Table A3. The point estimate from this considerably reduced analytical sample is -0.022 with a standard error of 0.011. Furthermore, estimates from the specification in Equation 2 estimated through the Cox proportional hazard model suggest a marked decline in the under-five mortality with a hazard ratio estimate of 0.868 and associated standard error 0.036.

births after December 2009 in the states where FMCHP is rolled out in the second phase are classified as post-treatment.

In Table A1, we also present the difference between the treatment and control births in the pre-treatment period. In the last column of the table, we provide a difference-in-differences (D-I-D) estimate. We emphasize that this is the difference in the unconditional means of the variables between treatment and control births across post- and pre-treatment periods. The estimates in Table A1 show that the states where FMCHP is rolled out are negatively selected on the health outcomes in the pre-treatment period. The treatment group states have higher rates of child mortality and have worse child development indicators relative to the control group states. Figure A6 suggests that under-five mortality is trending similarly across control and treatment states before the FMCHP roll out.

Our empirical strategy investigates whether these differences in the health outcomes between treatment and control states persist after the roll out of FMCHP. The last column of Table A1 suggests that for many of the child health outcomes, there is a marked improvement in the treatment group in the post-treatment periods relative to pre-treatment periods compared to the control group. In the next section, we examine if we can impart a causal interpretation to the improvement in these health outcomes of the children potentially exposed to the FMCHP.

4 Empirical Strategy

This paper aims to identify the causal effect of the Free Maternal and Child Health Program (FMCHP) on child and maternal health outcomes. Our data precludes us from estimating the average treatment effect on the treated (ATT) as we lack information on the takeup of FMCHP by the respondents. Our empirical strategy, therefore, estimates intention-to-treat (ITT) effects. In the case of universal takeup, our ITT estimates will coincide with the ATT as long as the identifying assumptions of the empirical framework are satisfied. We return to these assumptions below and discuss why they are likely to be satisfied in our setting.

As is discussed extensively in Section 2, all pregnant mothers residing in a state where FMCHP is rolled out are eligible to be policy beneficiaries. Therefore, we designate all births that happen after the FMCHP is rolled out in mothers' residence state to be potential beneficiaries of the policy.

Our main empirical specification is the following.

$$y_b = \alpha_{m(b)} + \alpha_{MoB(b),YoB(b)} + \beta \left[\mathbb{1}\left\{ Treat_{s(b)} \right\} \times \mathbb{1}\left\{ Post_{MoB(b),YoB(b)} \right\} \right] + \mathbf{X}_b \gamma + \varepsilon_b \tag{1}$$

In Equation 1, y_b is the outcome for birth b experienced by mother m. Equation 1 also controls for observable characteristics of the birth, denoted by $\mathbf{X}_b$. The characteristics that we account for are birth order, an indicator for whether the birth is a multiple birth, and an indicator for the sex of the child. In Equation 1, we also control for month-year fixed-effects corresponding to the date-of-birth. These are denoted by $\alpha_{MoB(b),YoB(b)}$, where $MoB(b)$ is the month and $YoB(b)$ is the year in which the mother gives birth. These fixed-effects account for time-specific unobservable shocks that are common across treated and control states.

The empirical specification in Equation 1 also controls for fixed-effect specific to each mother. We denote these fixed-effects as $\alpha_{m(b)}$ in Equation 1. These fixed-effects account for time-invariant unobservable characteristics of the mother such as their health behaviors that do not differ across multiple births experienced by the mother. Including these fixed-effects implies that the identification of the ITT effect is based on differential exposure to FMCHP across pregnancies of the same mother.

ε_b is the idiosyncratic error terms that we cluster at the local government area-level to account for correlation across mothers in the same local government area (Abadie et al., 2022; MacKinnon et al., 2023).[9]

The parameter of interest in Equation 1 is β. The estimate of β is the marginal effect of FMCHP

[9]We do not cluster standard errors at the state-level as there are too few clusters for the inference with this level of clustering to be reliable. However, with the wild cluster bootstrap inference, the p-value for the null hypothesis test of the β in Equation 1 to be equal to zero is 0.076. Abadie et al. (2022) show that when cluster sizes are large and treatment varies within clusters, cluster standard errors can be considerably larger than necessary. Given that we use all the births from the three DHS survey rounds, the loss of statistical precision for the under-five mortality variable might be driven by the standard error inflation causes identified by Abadie et al. (2022).

exposure on the outcome of interest. It is the marginal effect of the interaction of an indicator for the birth b occurring in the treatment state, $\mathbb{1}\left\{Treat_{s(b)}\right\}$, and an indicator for the birth b occurring in the post-treatment period, $\mathbb{1}\left\{Post_{MoB(b),YoB(b)}\right\}$. As we are unable to ascertain individual takeup, this marginal effect is an estimate of the ITT effect of FMCHP exposure.[10]

We note that the specification in Equation 1 is akin to a difference-in-differences empirical framework. Consequently, to interpret estimates from this specification causally, we need certain assumptions to be satisfied. First, we need to ensure that the outcomes for the treatment and control births are trending similarly in the pre-treatment period.

To this end, we present event-study estimates in Figure A7. The event-study estimates are from the estimation of a specification similar to Equation 1 where we replace the interaction of the indicator for the birth occurring in a treatment state and the indicator for the birth occurring in the post-treatment period with multiple indicators for each period pre- and post-treatment in our analytical sample. All the estimates are relative to the period immediately preceding the first post-treatment period which is denoted by zero in both figures. This specification is as follows.

(3)

$$y_b = \alpha_{m(b)} + \alpha_{MoB(b),YoB(b)} + \sum_{i=-7, i\neq -1}^{4} \beta_i \left[\mathbb{1}\left\{Treat_{s(b)}\right\} \times \mathbb{1}\left\{YoB\,(b) - TreatYear_{s(b)} = i\right\}\right] + \mathbf{X}_b\gamma + \varepsilon_b$$

[10] For some outcome variables, we are unable to leverage variation in FMCHP exposure for the same mother across multiple pregnancies experienced by her. This primarily happens because the conditional distribution of such outcome variables after accounting for mother fixed-effects is almost degenerate. Furthermore, for some outcome variables, we observe information either for the last pregnancy experienced by the mother or we define the outcome variable at the mother-level. For such outcome variables, we estimate the following specification.

(2) $$y_b = \alpha_{s(b)} + \alpha_{MoB(b),YoB(b)} + \beta\left[\mathbb{1}\left\{Treat_{s(b)}\right\} \times \mathbb{1}\left\{Post_{MoB(b),YoB(b)}\right\}\right] + \mathbf{X}_b\gamma + \mathbf{X}_{m(b)}\delta + \varepsilon_b$$

Equation 2 differs from Equation 1 in that we replace mother fixed-effects with mothers' residence state fixed-effects, $\alpha_{s(b)}$, and include a vector of mother controls. The vector of mother controls includes indicators for mother's month-year-of-birth, indicator variables for religion, and number of household members. This vector of mother controls is denoted as $\mathbf{X}_{m(b)}$ in Equation 2. All other parameters in Equation 2 are the same as that in Equation 1.

We emphasize that specification in Equation 2 does not account for selection into program uptake by exposed mothers. Furthermore, this specification allows for mothers who experienced only one pregnancy in the five years preceding the survey date to be part of the analytical sample. Mothers who experience multiple births within five years before the survey date are likely to follow short interpregnancy intervals leading to adverse maternal and child health outcomes (Nnaji et al., 2022; Yaya et al., 2020). This suggests that mothers that constitute the analytical sample for specification in Equation 1 are selected negatively on health outcomes. Thus, we expect the estimates from Equation 2 to be biased upwards. Indeed for the comparable outcomes, estimates from Equation 1 are smaller in magnitude than the estimates from Equation 2 (see Table A3).

In Equation 3, all the parameters are the same as in Equation 1, except that the single interaction of the indicator for the treatment status of the state and the indicator for the post-treatment period is replaced with the interaction of treatment status of the state indicator, $\mathbb{1}\left\{Treat_{s(b)}\right\}$, and the indicator for the difference between the birth year, $YoB(b)$, and the treatment year for the birth state, $TreatYear_{s(b)}$, to be from -7 to 4. In Equation 3, we denote this interaction as $\left[\mathbb{1}\left\{Treat_{s(b)}\right\} \times \mathbb{1}\left\{YoB(b) - TreatYear_{s(b)} = i\right\}\right]$.

In Equation 3, $\beta_i, \forall i \in \{-7,..,4\}, i \neq -1$ is relative to β_{-1} which is the marginal effect of FMCHP exposure on outcome variable in the year immediately preceding the first year of treatment, $i = 0$. The outcomes for the treatment and control births are trending similarly in the pre-treatment period if $\beta_i = 0, \forall i \in \{-7,-5,..,-2\}$ in Equation 3.

For child outcomes, in Figure A7 we observe that except for the child being underweight, all other outcomes are trending similarly for children in the control and treatment group before FMCHP is rolled out. Going forward, we report the estimates for an indicator for the child being underweight but do not interpret these estimates. For the other four child outcomes, we note that the $p-value$ of the joint test for all pre-treatment parameters to be zero is above 0.10. Thus, we do not reject the null hypothesis that these outcomes are trending similarly for the children in treatment and control states in the pre-treatment period.

In addition to the outcomes for treated and control children trending similarly in the pre-treatment period, we also need to ensure that there is no anticipation for the mothers who eventually are exposed to FMCHP. We verify that there is no change in fertility due to exposure to FMCHP.[11] We further demonstrate that there are no abrupt changes in the number of new primary health facilities that begin operation in the treated states after FMCHP is rolled out (see Figure A8).[12] Thus, there is neither anticipation on the part of healthcare suppliers nor they are incentivized to begin operation in the aftermath of the FMCHP roll out.

[11] The point estimate from the specification in Equation 2 where the dependent variable is respondents' realized fertility is 0.0116 with a standard error 0.019667.

[12] The point estimate on the interaction of post-treatment period indicator variable and treatment state indicator is 2.77 with a standard error of 4.96. This specification has the number of health facilities in the state as the dependent variable with the fixed-effects for state and year-month.

Further, we also establish that there is no significant change in the composition of mothers in the treatment and control states after FMCHP is rolled out. We estimate specification in Equation 2 for multiple outcome variables. Table A2 presents these estimates. The first outcome variable that we examine is the education of mothers. We create an indicator variable for whether the mother reports having no education. Point estimate in column (1) of Table A2 does not suggest that mothers in states with FMCHP are more likely to report no education attainment in the period after FMCHP is rolled out relative to the period before the roll out when compared to their counterparts during the same period in states where FMCHP is not rolled out.

We reach the same conclusion for the other three variables examined in column (2) to column (4) of Table A2. The first of these variables is an indicator variable for whether the household wealth index is in the top two quintiles of wealth distribution.[13] The point estimate in column (2) of Table A2 is small in magnitude and not statistically significant.

The last two columns of Table A2 are indicator variables for whether the mother reports being currently married and whether her spouse has no educational attainment. For both these indicator variables, we do not find any evidence that the mothers in treated states differ statistically significantly from their counterparts in control states in the period post-FMCHP roll out relative to the period before the roll out of FMCHP. Taken together results from Table A2 establish that our empirical design is not plagued by changes in the composition of mothers in the states where FMCHP is rolled out after the policy is initiated.[14]

Finally, to the best of our knowledge, there is no other policy that was rolled out during our sample period that specifically targeted pregnant mothers and their children in states where FMCHP is rolled out in either phase. It is often the case that health policies in Nigeria are operationalized for the entire country concurrently (Ezenwaka et al., 2022). Our empirical design secures contamination from the

[13] DHS construct household wealth index by taking into account household's asset ownership, materials used for housing construction along with information on access to sanitation and water access facilities.

[14] DHS provides information on whether respondent reports being away from the current residence for more than a month during the preceding 12 months from the survey date. When we restrict the analytical sample to those respondents who don't report such short-term migration, our estimates are larger in magnitude than the baseline estimates reported in Table A3. The point estimate from the specification in Equation 1 is -0.032 with a standard error of 0.0093. Therefore, it is unlikely that our control group is contaminated by the strategic relocation of the unexposed respondents.

effects of such policies as they are likely to affect treatment and control births similarly.

Note that the specification in Equation 1 is a two-way-fixed-effects (TWFE) specification. Recent literature has highlighted that in the presence of staggered treatment adoption along with dynamic and heterogeneous treatment effects, TWFE estimates are biased (de Chaisemartin and D'Haultfœuille, 2022; Roth et al., 2023). This bias arises due to "forbidden" comparisons where an earlier treated unit serves as a counterfactual unit for a later treated unit. As FMCHP is rolled out across Nigerian states over two phases, the specification in Equation 1 is potentially subject to this bias.

In Figure A7 we also present estimates from the estimator proposed in Sun and Abraham (2021). This estimator removes contamination arising due to "forbidden" comparisons. Outcome variables that are trending in parallel in the periods before treatment with the TWFE estimator continue to do so even when using the proposed estimator of Sun and Abraham (2021). The $p-value$ shows that for all child health outcomes, there is evidence for outcome variables trending in parallel for births before and after the FMCHP roll out. We continue to observe that for the indicator variable of whether the child is underweight, there is evidence for outcomes to be trending differentially for the treated and control group before the FMCHP roll out. We also emphasize that there is significant overlap in the confidence intervals of estimates from TWFE and Sun and Abraham (2021) estimator. We cannot reject the null hypothesis that the two estimates are the same.

5 Results

In this section, we present results for various outcomes discussed in Section 3.1. We report estimates from the specification in Equation 1.[15] Through multiple empirical checks, we establish the robustness of our main results. We then examine if the main effects vary across different subpopulations. We end this section by highlighting mechanisms that may lead to documented changes in health outcomes due to Free Maternal and Child Health Program (FMCHP) exposure.

[15] While reporting estimates from the specification in Equation 2, we note the change in estimated specification.

5.1 Main Results

We begin by examining child health outcomes. Figure 1 reports event-study estimates for under-five mortality and birth weight from the specification in Equation 3. We observe that relative to children born before FMCHP is rolled out, children born after the policy roll out are less likely to die before age five. Quantitatively the decline in the under-five mortality is large at approximately 14% of the pre-treatment mean in the treated group. Children born after the FMCHP is rolled out in their residence state relative to children born before the policy roll out have higher birth weight.[16]

For both outcomes, we are not able to reject the null hypothesis that all of the pre-treatment parameter estimates are zero.[17] Under-five mortality declines from the second post-treatment period and this decline persists for the rest of the post-treatment period albeit with some loss of statistical precision in the last two post-treatment periods. We note that during these two post-treatment periods, FMCHP had started to experience funding issues. As not all children have their weight recorded at birth, the period estimates for birth weight are statistically imprecise. Nonetheless, all the point estimates are positive and when we aggregate all the post-treatment periods, we document a statistically significant increase in the birth weight of children potentially exposed to FMCHP. This conclusion, however, should be interpreted with caution as not all children are weighed at birth.

While the point estimates from the specification in Equation 2 in Table A3 suggest a decline in infant mortality due to FMCHP exposure, this change is not statistically significant even when we account for selection into FMCHP. For child development indicators, the specification that does not account for selection into FMCHP suggests improvements in some development indicators (underweight and wasting). This conclusion does not hold up when examining the specifications with mother fixed-effects. Similarly, increased stunting after the FMCHP roll out does not persist after we account for selection into FMCHP. Overall, these estimates show that FMCHP exposure reduces under-five mortality and increases birth weight with some suggestive evidence of improvement in child development

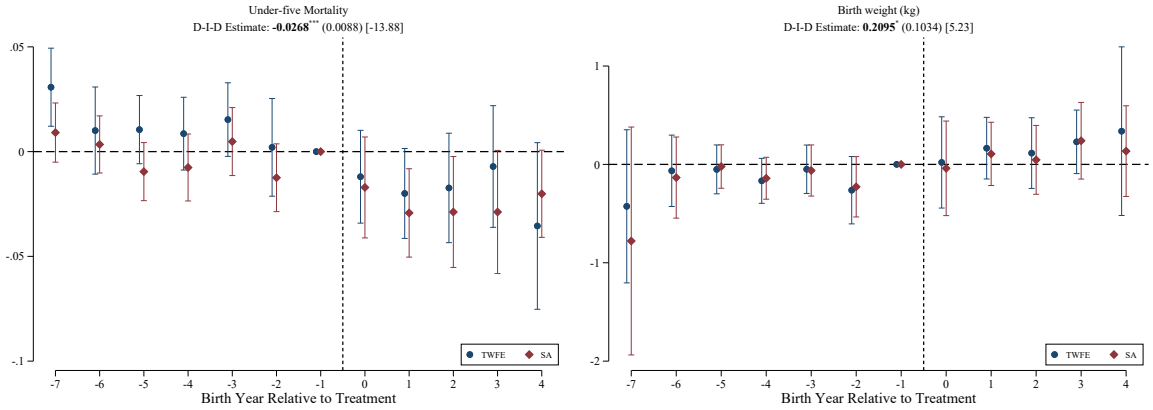
[16] Table A3 reports point estimates from specification in Equation 1 and Equation 2.

[17] p-value from a Wald test of the null hypothesis that all pre-treatment estimates are zero for under-five mortality is 0.209 and for birth weight is 0.354.

Figure 1: Event-study Estimates: Child Health Outcomes

(a) Under-five Mortality

(b) Birth Weight



Note: The figure presents event-study estimates and 95% confidence intervals from a specification with mother and year-month-of-birth fixed-effects. This specification is presented in Equation 3. This specification also includes an indicator for multiple births, birth order, and an indicator for the sex of the child. All estimates are relative to the period immediately before the first year of treatment. Estimates are from TWFE and interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

indicators.[18]

In the next subsection, we present various empirical checks to establish the robustness of the conclusions in this subsection. We establish that our main findings are unaltered when we change the estimating specification, modify the analytical sample, or use alternate FMCHP exposure measures, in addition to other modifications to our baseline empirical framework.

5.2 Robustness Checks

We next turn to establish the robustness of our estimates presented in Table A3. We conduct a series of robustness checks in Table A4.

The first column of Table A4 replicates column (4) of Table A3. In the second column, we drop all birth-specific control variables. The point estimate in the second column is larger than the point

[18] In Section 6, we present back-of-the-envelope estimates on how many deaths for children under the age of five from 2008 to 2015 in the states where FMCHP is rolled out are averted due to FMCHP. In that section, we also provide an estimate of the cost of averting each death of a child under the age of five.

estimate in the first column. We can reject the null hypothesis that the two point estimates are not statistically distinguishable from each other (p-value: 0.08).

In the next two columns, we change the analytical sample. In column (3), we drop the two most populated states: Lagos and Abuja. We add births that happen post-2015 in column (4) to the analytical sample. Neither of these changes to the estimation sample alters the improvement in under-five mortality with none of the differences being statistically significantly different from zero (p-value: 0.18).

Column (5) of Table A4 adds Local Government Area (LGA) linear time trends to the specification in Equation 1. This allows LGAs to trend differently over the sample period. If the specification in Equation 1 is unable to account for time-varying unobservables at the LGA-level, then we expect the main effects to be biased. Reassuringly, the point estimate when allowing for LGA time trends is unaltered (p-value: 0.22) lending credence to the assumption of the absence of time-varying unobservables that may conflate our point estimates.

In the next two columns of Table A4, we alter the definition of FMCHP exposure. In column (6), we classify all births to be exposed to FMCHP based on their date-of-conception (DoC). We define date-of-conception as nine months before the reported date-of-birth of the child. The point estimate with this redefined FMCHP exposure for under-five mortality is not statistically different from the baseline point estimate (p-value: 0.45).

We define exposure to FMCHP in column (7) based on the child's date-of-birth and program caps. As we discuss extensively in Section 2, children are entitled to the FMCHP benefits from conception to five years post-birth. Mothers, on the other hand, benefit from FMCHP during their entire duration of pregnancy and six weeks post-delivery. Based on the date-of-birth of the child we cap the program exposure to these maximum durations if the mother and child could have benefited for more months than the program maximum. For all other mother and child pairs, the exposure months are defined as the months for which FMCHP is in operation in their residence state. The point estimate in column (7) suggests that each additional month of FMCHP exposure reduces under-five mortality.

The worsening of statistical precision for child health outcomes in column (7) of Table A4 might be

due to measurement error induced by using a uniform gestation length of nine months. Existing work demonstrates that gestation length can differ across pregnant mothers by more than one month (Jukic et al., 2013). This suggests that in the absence of precise gestation length information, our approach to defining program maximum duration imparts significant measurement error in the FMCHP exposure. Thus, attenuation of point estimates and slight loss of statistical precision in column (7) of Table A4 can be explained by this classical measurement error.

In column (8) to column (10) of Table A4, we constrain the size of the estimation sample. Column (8) drops all mothers who are above the age of 40 at the time of birth of their child. Column (9), on the other hand, restricts the treatment states to only those who experienced FMCHP roll out in the second phase in 2009. In column (10), we drop all states from the estimation sample that may have had an operational FMCHP before 2008.[19] The estimate from the sample which excludes all states where FMCHP is rolled out in the first phase is larger in magnitude. Indeed, we can reject the null hypothesis that the estimate is the same as that reported in the first column (p-value: 0.00). Improvement in child health from the estimation sample where mothers over the age of 40 are excluded is not statistically significantly different from that reported in the first column of Table A4 (p-value: 0.58). Under-five mortality estimate is not qualitatively altered when we constrain the estimation sample by dropping the states that may have had an operational FMCHP before 2008.[20]

Column (11) drops all first order births from the estimation sample. While the point estimate from this higher order births sample is larger in magnitude, it is not statistically significantly different from the baseline point estimate reported in the first column (p-value: 0.58). Finally, in column (12) we restrict the analytical sample to first order and singleton births only. The estimates in this column are from the specification in Equation 2. The point estimate for under-five mortality is qualitatively similar to the baseline point estimate and we are unable to reject the null hypothesis that the two are not statistically distinguishable from each other (p-value: 0.50).

[19] We have not been able to ascertain whether some states had an operational FMCHP before the policy that we have examined in this paper. However, existing work suggests that some states may have had FMCHP in place before 2008. Identification of such states is based on Okonofua et al. (2011).

[20] The point estimates for under-five mortality differ across the two estimation samples and we can reject the null hypothesis that the two estimates are not different (p-value: 0.02).

In Figure A9, we show that our main results are not driven by birth at a particular birth order. For none of the subsamples, we can reject the null hypothesis of equality of estimates between the baseline sample and the subsample of concern.[21]

Overall results in this subsection show that exposure to FMCHP leads to a decline in under-five mortality for potentially exposed children. This conclusion is robust to multiple sensitivity checks related to empirical specification, analytical sample, and treatment variable definitions. In the next subsection, we examine if FMCHP affects various population subgroups differentially.

5.3 Heterogeneity

In this subsection, we present estimates for various subpopulations that constitute the estimation sample. We start by stratifying the estimation sample by various socioeconomic characteristics of the respondents. Then, we make extensive use of the health facility registry data to study if differential access to health facilities exacerbates how FMCHP affects the under-five mortality of children potentially exposed to FMCHP. Wherever necessary, we also report p-value for the Wald test of the null hypothesis of marginal effect not differing across the population subgroups.

Figure 2 presents event-study estimates from Sun and Abraham (2021) for various subpopulations.[22] The first panel report estimates by whether the household is classified as having below or above median wealth level.[23] Relatively poorer households see improvement in under-five mortality of children potentially exposed to FMCHP. Indeed the decline in the under-five mortality is concentrated only in the relatively less affluent subgroup (p-value: 0.02).

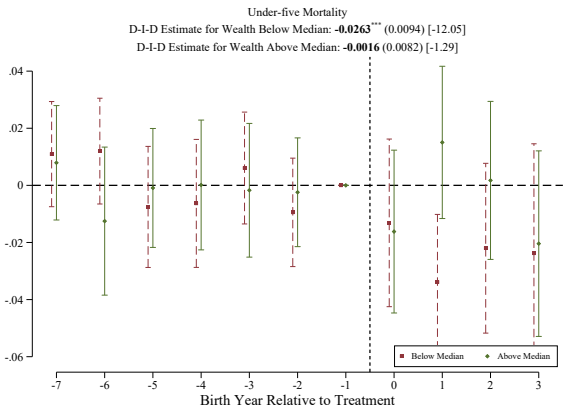
[21] Excluding all the households that have more than one child that could have potentially benefited from the FMCHP results in qualitatively similar estimates to those from the baseline specification and analytical sample. A child is a potential beneficiary of the FMCHP if their age during the post-treatment period is five or less. Restricting the analytical sample by excluding such households reduces the estimation sample by up to 8%. The point estimate and associated standard error for under-five mortality are -0.02536 and 0.0067, respectively.

[22] In Table A5, we report estimates from specification in Equation 1 for all these subpopulations. For the subpopulations not reported in Figure 2, we do not observe statistically significant differences in under-five mortality across the two population subgroups.

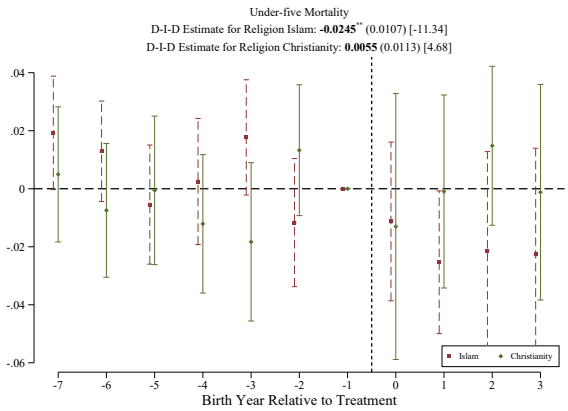
[23] We note that the classification of households in these two categories is based on the wealth distribution in the round that they were surveyed.

Figure 2: Event-study Estimates: Heterogeneity by Socioeconomic Characteristics

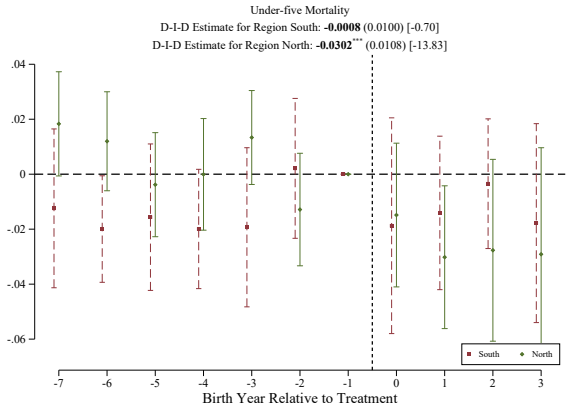
(a) Wealth



(b) Religion



(c) Region



Note: The figure presents event-study estimates and 95% confidence intervals from a specification with mother and year-month-of-birth fixed-effects. This specification is presented in Equation 3. This specification also includes an indicator for multiple births, birth order, and an indicator for the sex of the child. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. All subpopulations are constructed using the reported data in the DHS. Wealth distribution in the respective survey round is used to designate a household to be either below or above the median of the wealth distribution. Households are designated to be residing in the southern region if they reside in Oyo, Osun, Ekiti, Ondo, Edo, Anambra, Enugu, Ebonyi, Cross River, Akwa Ibom, Abia, Imo, Rivers, Bayelsa, Delta, Lagos, or Ogun. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

The second panel report estimates for subpopulations defined by respondents' religion. We classify respondents as following either Islam or Christianity based on their reported religion.[24] Existing work provides evidence that households following Islam are less likely to vaccinate their children and more likely to refuse cesarian-section (Antai, 2009; Ugwu and de Kok, 2015). FMCHP exposure leads to a lower likelihood of death before the fifth birthday of the child born to women following Islam. Indeed, the decline in under-five mortality for the women who follow Islam is statistically different from those women who follow Christianity (p-value: 0.01).

In the last panel of Figure 2, we stratify the estimation sample by whether the household resides in the northern or southern region of Nigeria. Existing evidence points to wide socioeconomic disparities between these two regions of the country. The northern part of Nigeria is disadvantaged over multiple dimensions related to human development, including access to healthcare services (Abegunde et al., 2015; Adeyanju et al., 2017; Ajisegiri et al., 2022; Gage et al., 2016). These healthcare services disparities reflect a higher concentration of hospitals and formal health structures in the southern region despite the northern region having a larger population (Abubakar et al., 2022; Birchall, 2019). Post the FMCHP roll out, the northern part of Nigeria experiences improvements in child health outcomes. Indeed, our point estimates are statistically significant at the conventional levels of statistical significance only for the northern part of the country. We note that children are more likely to survive beyond their fifth birthday if they are potentially exposed to FMCHP and reside in the northern region of Nigeria. We can also reject the null hypothesis that the estimates for under-five mortality are the same for the northern and southern regions of Nigeria (p-value: 0.02).

Overall, estimates in Figure 2 suggest that FMCHP may have reduced the existing disparities in the child health outcomes for the relatively worse-off subpopulation (those households who are relatively poorer, reside in the northern part of Nigeria or follow Islam).

In Table A6, we also study the heterogeneity of main effects by the marital status of the respondent. In the first two columns, we examine if the improvement in health outcomes differs across married and un-

[24] Due to extremely small sample sizes which may lead to incorrect inferences, we do not report estimates for respondents who neither follow Islam nor Christianity.

married respondents. We are unable to reject the null hypothesis that there is no statistically significant difference between the two population subgroups (p-value: 0.35). We also reach the same conclusion when the estimation sample is stratified by whether the respondent reports ever being divorced (p-value: 0.22). The next six columns examine if the main effects differ by whether the respondents' household is polygamous. We are unable to reject the null hypothesis that the child health outcomes differ between polygamous and monogamous households (p-value: 0.83). The last four columns of Table A6 then restrict the analytical sample to those households who are polygamous. For under-five mortality, improvements are seen for polygamous households following Islam but the difference is not statistically different from their non-Islam household counterparts (p-value: 0.25). For polygamous households who are relatively less affluent, potential exposure of children to FMCHP improves the likelihood of them surviving beyond their fifth birthday. Indeed, we can reject the null hypothesis that this effect is the same as that for relatively richer households (p-value: 0.05).

Figure 3 presents estimates for subpopulations constructed using the Nigeria Health Facility Registry (HFR) data.[25] The estimates in this figure are from specification in Equation 3 estimated using the interaction weighted estimator in Sun and Abraham (2021). Since the finest geographical unit observed in the DHS is the state, we are only able to categorize the population based on facilities in the state.[26] Furthermore, only primary facilities are used to derive these subpopulations.[27] This is because FMCHP envisaged providing free healthcare in such facilities (Onwujekwe et al., 2019). As facility exposure may change depending on whether the state rolls out FMCHP or not, we rely on facility data before 2008 during which the first phase of FMCHP started. To designate the respondents to be residing either below or above the median of the distribution of the measure per-capita used for stratification, we use population counts at the state-level from the 2006 Nigeria Population and Housing Census.

The first panel of Figure 3 examines if the reduction in under-five mortality differs across states with below or above median per-capita number of health facilities after FMCHP is rolled out relative to the pre-roll out period. We observe that for states with a relatively higher number of facilities per-

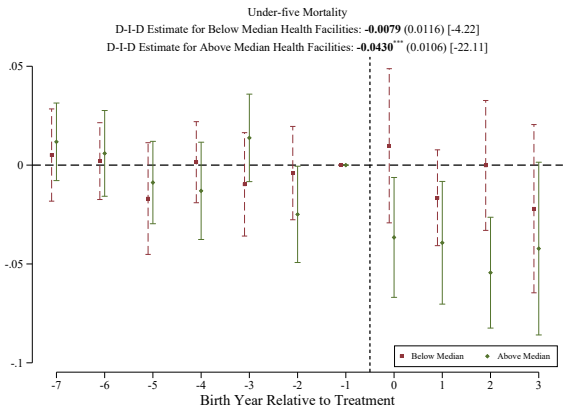
[25] In Table A7, we report estimates from specification in Equation 1 for the four subpopulations reported in Figure 3.

[26] While DHS provides local government area (LGA) codes, there is no concordance between the LGA codes and names. Therefore, we are unable to ascertain the residence LGA for respondents that constitute our estimation sample.

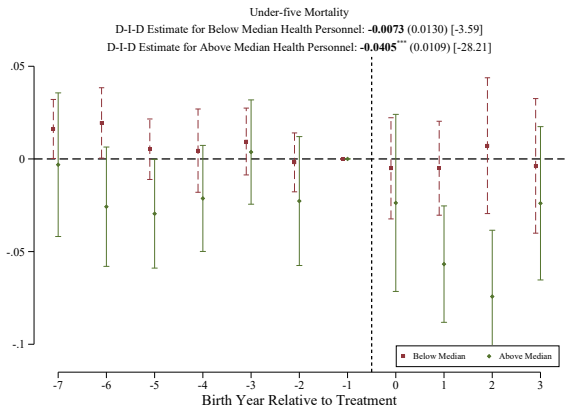
[27] Figure A10 presents the spatial distribution of "primary" health facilities derived from HFR data.

Figure 3: Event-study Estimates: Heterogeneity by Health Facility Access

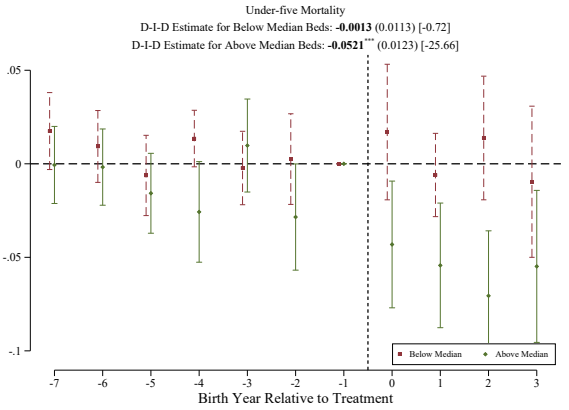
(a) Health Facilities Per-capita



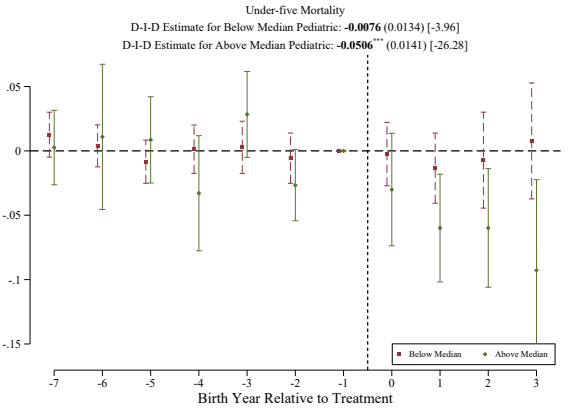
(b) Health Professionals Per-capita



(c) Health Facility Beds Per-capita



(d) Pediatric Health Facilities Per-capita



Note: The figure presents event-study estimates and 95% confidence intervals from a specification with mother and year-month-of-birth fixed-effects. This specification is presented in Equation 3. This specification also includes an indicator for multiple births, birth order, and an indicator for the sex of the child. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. All subpopulations are constructed using the Nigeria Health Facility Registry (HFR) data. Only primary facilities are used to construct subpopulations. Health personnel consist of doctors, nurses, and midwives. A facility is designated to provide pediatric services if they report providing any pediatric care service. Population data at the state-level is derived from the population and housing census conducted in 2006. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

capita, relative to births that occur before FMCHP is rolled out, births occurring after the roll out are less likely to result in the death of the child before their fifth birthday. Furthermore, we can reject the null hypothesis that the estimates are the same across the two subgroups based on the number of health facilities per-capita in the state (p-value: 0.01). Indeed, quantitatively the decline in under-five mortality in states that provide more access to primary health facilities is substantial at more than 22% of the pre-treatment mean for the treated group.

We reach similar conclusions for subpopulations defined based on access to health personnel (doctors, nurses, and midwives) per-capita in the residence state. These estimates are reported in the second panel of Figure 3. Offspring of pregnant mothers residing in states with more access to health personnel are more likely to survive beyond their fifth birthday after potential exposure to FMCHP. We can reject the null hypothesis that the two estimates are the same across the two population subgroups (p-value: 0.07).

Lending further credence to the conclusion that more access to healthcare services accentuates the effect of FMCHP exposure on child health outcomes are estimates reported in the third panel of Figure 3. In this panel, we stratify the population by whether the residence state of the mother and child is either below or above the median per-capita number of beds in the primary health facilities. We find that children born to mothers residing in states with FMCHP roll out are less likely to die before their fifth birthday if the state is above the median of the number of beds per-capita distribution. We can reject the null hypothesis that the estimates are not statistically different across the two subpopulations (p-value: 0.00).

The last panel of Figure 3 presents estimates from subpopulations that have access to either below or above the median per-capita number of primary health facilities providing pediatric services. We expect to see a more pronounced decline in under-five mortality in the states where children have more access to pediatric services health facilities. Children born after the FMCHP roll out and residing in states with more pediatric services are more likely to survive beyond their fifth birthday relative to their pre-treatment counterparts. We can also reject the null hypothesis that these child health outcomes are the same across the two population subgroups (p-value: 0.01).

Estimates in the last panel of Figure 3 provide evidence for the role of having more pediatric services in influencing child health outcomes. More access to pediatric services leads to better survival chances for newborns. Building on these estimates in the last panel, the first three panels highlight the role of more access to health facilities and health personnel on under-five mortality.

Overall, results in this subsection paint a consistent picture of FMCHP closing existing socioeconomic disparities in child survival in Nigeria. We establish the role of FMCHP in improving child health outcomes for socioeconomically disadvantaged subgroups of the population. We also demonstrate the importance of having access to health facilities and personnel in improving child mortality. Our results show that having wider access to pediatric services improves child health outcomes. In the next subsection, we highlight mechanisms that might be leading to the improvement in under-five mortality in the states where FMCHP is rolled out.

5.4 Mechanisms

In this subsection, we examine potential mechanisms that might lead to a reduction in the under-five mortality that we document in Section 5.1. We highlight the role of the increase in demand for institutional care and preventive care in facilitating the improvement in child mortality.

We begin by examining whether FMCHP leads to an increase in demand for institutional care. The event-study estimates from the specification in Equation 1 are presented in Figure 4.[28] We observe that the demand for institutional care by births potentially exposed to FMCHP increased. Not only are the children potentially exposed to FMCHP more likely to be delivered at a health institution, but the delivery is also more likely to be assisted by a health personnel (doctor, nurse, or midwife). Furthermore, we are unable to reject the null hypothesis that there is a one-to-one substitution between health institution and home delivery (p-value: 0.78).

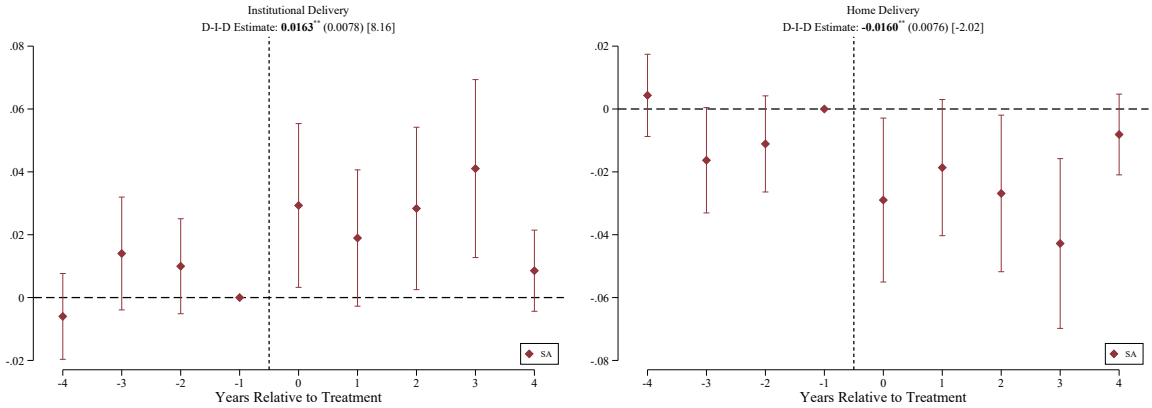
Additional evidence for the increase in demand for institutional care due to potential exposure to FM-

[28] The corresponding estimates are reported in Table A8.

Figure 4: Event-study Estimates: Demand for Institutional Care

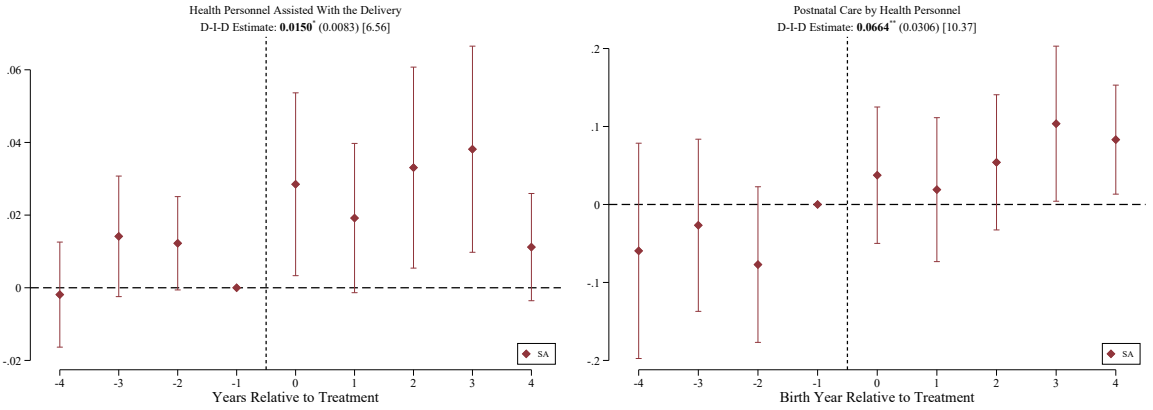
(a) Health Institution Delivery

(b) Home Delivery



(c) Health Personnel Assisted Delivery

(d) Postnatal Care by Health Personnel



Note: The figure presents event-study estimates and 95% confidence intervals from a specification with mother and year-month-of-birth fixed-effects. This specification is presented in Equation 3. This specification also includes an indicator for multiple births, birth order, and an indicator for the sex of the child. The last panel reports estimates from a specification where the mother fixed-effects are replaced by residence state fixed-effects and additional controls for indicator variables for mothers' year-of-birth, indicator variables for households' religion, and number of household members are included. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

CHP is provided by the increase in postnatal care. Existing work highlights the role of postnatal care in reducing maternal and child mortality (Ahinkorah et al., 2022). In the last panel of Figure 4, we establish that children potentially exposed to FMCHP are more likely to have obtained postnatal care delivered by health personnel.[29] We note that there is no increase in postnatal care overall. Conditional on receiving postnatal care, however, the children are more likely to have received it from health personnel. Not only is the increase in demand for institutional care highly statistically significant, but the increase is also quantitatively large at approximately 7% for the smallest observed increase across the outcomes that we examine.

Existing work demonstrates the role of continuous interaction between healthcare providers and patients in the use of intravenous vaccines and supplements (Akinajo et al., 2024; Okafor et al., 2014; Sanjo-Odutayo, 2023). The role of such interaction is even more acute in our context where the levels of health facility use are very low partly due to various negative perceptions about the medical providers (see Figure A2). Furthermore, FMCHP outreach initiatives include extensive use of community health workers who may have played a role in educating and convincing pregnant mothers about the benefits of institutional healthcare during pregnancy (Ezenwaka et al., 2022; Oguntunde et al., 2018; Onwujekwe et al., 2018, 2019).[30] Thus, by examining if the mothers are more likely to report diagnostic use and receipt of injections or supplements in addition to antenatal care during pregnancy, we provide evidence for pre-delivery interaction with medical providers as the pathway to an increase in demand for institutional care.

Figure 5 reports event-study estimates from the standardized weighted index constructed using various pre-delivery interaction indicator variables.[31] The estimates in this figure show a large and instantaneous increase in the pre-delivery interaction with medical providers by pregnant mothers exposed to FMCHP. The dynamics of the FMCHP exposure on pre-delivery interaction with the medical providers

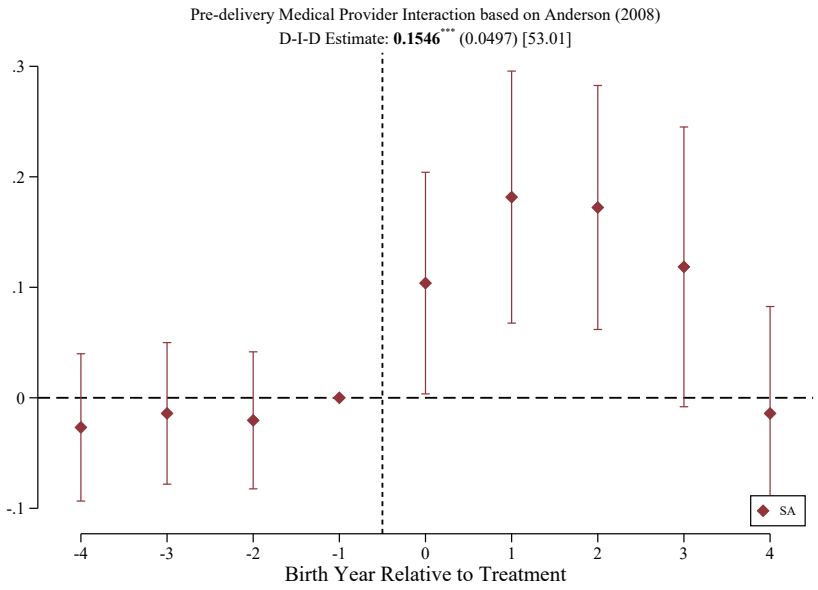
[29] The corresponding estimates are reported in Table A9.

[30] Estimates from specification in Equation 2 with an indicator variable for whether the antenatal care is provided by a community health personnel suggest a large increase of approximately 51% over the pre-treatment mean for the treated group. The point estimate from this estimation is 0.01995 with a standard error of 0.01041. The corresponding event-study estimates are presented in Figure A14.

[31] In Figure A15, we report event-study estimates for each of the indicator variables. Table A10 reports corresponding estimates from the specification in Equation 2.

follow dynamics for under-five mortality with a loss of statistical significance in the final two periods when the FMCHP began to face funding difficulties. The increased pre-delivery interaction due to FMCHP exposure gains added significance when we situate it in the context of arguably large potential mistrust between medical providers and potential beneficiaries before the FMCHP roll out.[32]

Figure 5: Event-study Estimates: Pre-delivery Interaction with the Medical Providers



Note: The figure presents event-study estimates and 95% confidence intervals from a specification with state and year-month-of-birth fixed-effects. This specification is presented in Equation 2. This specification also includes indicators for the mother's year-of-birth, birth order, an indicator for multiple births, indicators for religion, number of household members, and an indicator for the sex of the child. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. The dependent variable is a standardized weighted index of multiple indicator variables using a GLS weighting procedure as described in Anderson (2008). The indicator variables used to construct this index are reported in Table A10. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

To provide further evidence that community health personnel outreach which is emphasized by FMCHP succeeded, we discuss two additional pieces of evidence. First, we show that there is suggestive evidence that the health information of the respondents potentially exposed to FMCHP improved. Additionally, we also show that the increase in community health personnel provided antenatal care is concentrated only in ethnic groups who are non-matrilineal in their kinship structure. The estimates

[32] In recent work Archibong and Annan (2021) show that in the wake of failed antibiotic human trials in Nigeria, medical mistrust developed in the affected Muslim community. This mistrust in healthcare providers led not only to a decline in child vaccination but also to reduced prenatal care by the mothers. As the post-FMCHP period is within a decade of the disclosure of the failed trial, community health personnel provided antenatal care may have helped in rebuilding trust with medical providers underscoring the emphasis put on community outreach by FMCHP.

from the event-study specification for the potential improvement in health information of FMCHP exposed respondents are reported in Figure A16.[33] The estimates in this figure provide suggestive evidence that pregnant mothers who are potential beneficiaries of FMCHP are more informed. We observe statistically significant improvement in respondents' knowledge of the ovulatory cycle, awareness about acquired immunodeficiency syndrome (AIDS), and contraception use. We note that the outcomes in Figure A16 and Table A11 reflect knowledge or information that may not directly influence child health.

In Figure A17 and Table A12, we show that the increase in the likelihood of antenatal care provided by community health personnel is observed only for ethnic groups who do not follow matrilineal kinship structure. Ethnic groups who follow matrilineal kinship structure are shown to have greater autonomy for women along with experiencing less domestic violence (Lowes, 2022). Furthermore, women in this kinship structure have children who are healthier and better educated (Lowes, 2020). If the community health personnel educated and encouraged pregnant women to demand institutional care, we would expect to see a statistically significant effect on community health personnel provided antenatal care only for those women who are not part of the matrilineal ethnic groups.[34] Reassuringly, this is exactly what we find.[35,36] We can reject the null hypothesis that FMCHP exposure effect on the likelihood of community health personnel provided antenatal care for both kinship structures is the same (p-value: 0.09).

Overall, we conclude that FMCHP through its community outreach informed pregnant women about the benefits of health institution care which led to these women interacting more with the medical providers. These interactions eventually matured into the FMCHP exposed women demanding institutional care during delivery and for postnatal care.

Increased health institution utilization by pregnant women may incentivize relatively wealthy women to increase the use of private health institutions for delivery. This effect is likely to be more pronounced

[33] The corresponding estimates from the specification in Equation 2 are reported in Table A11.

[34] We plot the boundaries of matrilineal ethnic groups in Figure A11.

[35] To classify women as belonging to an ethnic group that follows matrilineal kinship structure, we use the reported ethnicity in the DHS. Ethnic groups following kinship structure are derived from Murdock (1967).

[36] The estimates are reported in Table A12.

in states that have more primary health facilities per-capita. Existing evidence points to higher quality of services rendered in private health facilities in Nigeria (Hirose et al., 2018; Odusanya et al., 2018). In Figure A18 and Table A13, we examine if this is indeed the case. We do not observe an increase in the likelihood of relatively more affluent women using private health institutions for delivery after the roll out of FMCHP in states with a potential for substitution away from public health facilities towards those that are privately owned. This suggests that a congestion-driven shift away from public health facilities due to FMCHP is not likely to widen the existing socioeconomic disparities in healthcare utilization and health outcomes. Moreover, the lack of substitution between facilities of different ownership types by wealthier households suggests that FMCHP exposure has led to increased utilization of slack capacity in the existing health facilities.

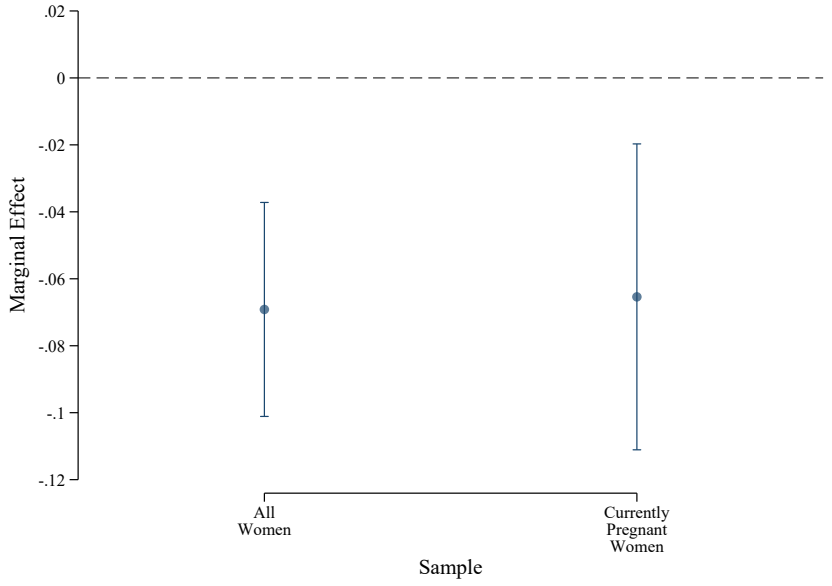
How large is the slack capacity in the treated states? To measure slack capacity in the treated states in the post-treatment period, we make use of the HFR data. We focus on primary health facilities that are operating in the post-treatment period in the states that had an FMCHP roll-out. We, then, create a measure of available beds at each primary health facility for each year in the post-treatment period that they are operating in. Using the existing estimates of the average length of stay for an institutional birth in Nigeria (Adeoye et al., 2024), we generate a measure of serviceable births by assuming that each institutional birth will lead to an occupancy duration of 2.6 days. Using this approach, we arrive at 36.71 million births that can be delivered at primary health facilities. Using the measure of institutional births during the same period in the states that ever rolled out FMCHP, we measure only 4.2 million institutional births. Thus, despite an increase in institutional delivery after the FMCHP roll out, considerable capacity is available for institutional deliveries.

Our measure of slack capacity admittedly has some limitations. We do not account for the spatial distribution of health facilities within a state. An efficient spatial distribution that maximizes the health institution use for delivery will site health facilities where the demand is relatively more elastic. We also assume that each institutional delivery has homogeneous bed occupancy for the inpatient care. We hope future work can address some of these shortcomings.

As further evidence for the increased utilization of slack capacity in the existing health facilities, we

examine if the respondent reports financial barriers in access to healthcare when they are sick. As FMCHP eliminated out-of-pocket healthcare expenditures sought by pregnant women, we may expect them to experience a lower incidence of financial hardship when seeking treatment or medical advice on account of their being sick. In Figure 6, we study if this is the case for pregnant women exposed to FMCHP. We find that women who are exposed to FMCHP are less likely to report money being a big problem when seeking medical advice or treatment during their sickness in the post-FMCHP period. This conclusion is unaltered when we restrict the estimation sample to those women who are pregnant at the time of the survey. Taken together, the reduction in financial hurdles in accessing medical care and increased utilization of slack capacity in existing health facilities suggest that initial low levels of demand are due to prohibitively expensive institutional care.

Figure 6: Money as the Reason for Not Seeking Healthcare When Sick



Note: The figure presents point estimates and 95% confidence intervals of a specification with mothers' residence state and year-month-of-birth fixed-effects. This specification also includes indicator variables for religion and controls for family size. The variable on the vertical axis is the estimate of the interaction of an indicator variable for mothers' residence state ever having Free Maternal and Child Health Program (FMCHP) roll out and an indicator for the last birth or the survey date to be after FMCHP is rolled out. "All Women" estimate is for all surveyed women. The "Currently Pregnant Women" estimate drops all women who were not pregnant at the time of the survey from the analytical sample. The dependent variable is an indicator variable for whether the respondent reports getting money needed for treatment as a big problem when she is sick and wants to get medical advice or treatment. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth for the estimation sample labeled "All Women". Exposure to the FMCHP is determined based on the survey year and month for the estimation sample labeled 'Currently Pregnant Women". The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

We now turn to discuss the second of the two mechanisms that may have led to a reduction in the under-five mortality of the children potentially exposed to FMCHP. We examine if these children are

more likely to be immunized. By ruling out the increase in acute care takeup, we provide suggestive evidence for preventive care being the likely channel through which children are more likely to survive beyond their fifth birthday.

In the pre-treatment period, approximately 23% of mothers respond that the child is not vaccinated for fear of side-effects or contracting disease.[37] We report these statistics in Figure A12 and Figure A13, with the latter being only for vaccinations related to polio. Lack of information, however, is a big reason why women are potentially not vaccinating their children.[38] Exposure to community health workers in addition to health institutional visits may have reduced the information asymmetries for such mothers. Our data is not well suited to examine if this is indeed the case after the FMCHP is rolled out.[39] Regardless, as we have demonstrated earlier, improved health information of potentially exposed mothers due to community health personnel provided antenatal care and pre-delivery interaction with the medical providers is a potential pathway through which child mortality improves.

Figure A19 study the likelihood of routine immunization of the children.[40] In particular, we look at Bacillus Calmette-Guérin (BCG), measles, Diphtheria-Pertussis-Tetanus (DPT), and polio vaccinations. Except for BCG, we find a quantitatively large and highly statistically significant increase in the likelihood of being vaccinated for children who are born after their residence state rolls out FMCHP.[41] This finding concurs with the existing work documenting the beneficial effects of childhood vaccination on mortality (McGovern and Canning, 2015). The lack of parallel pre-trends suggests caution in interpreting these estimates as causal. This conclusion is also applicable to the last panel of Figure A19 where we use a standardized weighted index constructed from the individual indicator variables in the first four panels. As one of the tenets of FMCHP is community outreach, informing households about the benefits of child vaccination might have led to these households focusing on them during infancy and early childhood (Nghargbu and Olaniyan, 2017). Overall, the estimates in Figure A19 and Table

[37] Together, fear of side-effects and fear that the child may get the disease are the reasons for 22.897% of children not being vaccinated in the pre-treatment period of our sample in the states where FMCHP is rolled out.

[38] Approximately 18.5% of the mothers report that they did not vaccinate their child due to a lack of information.

[39] Data on reasons for not vaccinating the child is available only for the 2008 round of DHS.

[40] The corresponding estimates from the specification in Equation 1 are reported in Table A14.

[41] The increase in vaccination for children exposed to FMCHP is between 4% to 17% of the pre-treatment mean for the states where FMCHP is rolled out.

A14 suggest preventive care as a likely mechanism for the reduction in under-five mortality of children potentially exposed to FMCHP.

Seeking healthcare at a health institution for acute ailments can be beneficial in managing the symptoms and preventing triggering life-threatening complications. We conclude this subsection by studying if this is the case in our context. In Figure A20 and Table A15, we report no statistically significant change post the FMCHP roll out in the treatment of diarrhea, fever/cough, or either of these two ailments at a health institution.[42] Furthermore, we also fail to detect any statistically significant change in the likelihood of taking any drug as part of the treatment regimen for fever or cough. This indicates that acute care changes are not driving the better survival odds of the children exposed to FMCHP.

5.5 Quantifying the Costs and Benefits

In this subsection, we provide back of the envelope estimate of the welfare benefits and costs of the Free Maternal and Child Health Program (FMCHP). To estimate these benefits and costs, we rely on multiple data sources each of which we discuss in great detail below.

We first obtain a count of births for the years the treated states are exposed to FMCHP. These data are provided by the National Population Commission (NPC) of Nigeria. For states that roll out FMCHP in the first phase, we derive birth counts for the years 2009 to 2015. For the remaining states that are treated in the second phase of the policy, birth counts from 2010 to 2015 are used. We note that the lack of systematic birth registration precludes us from using birth certificate data. The NPC data are population projections that take into account the information from the 2006 Population and Housing Census along with the age and fertility distribution of women. Admittedly, these estimates may be an undercount of the actual births which would lead us to underestimate the welfare benefits and overestimate the costs. On the other hand, changes in fertility trends might lead to an overestimate of the total births. These shortcomings notwithstanding, NPC data are the best available administrative

[42] There is a statistically significant increase in the likelihood of the child having diarrhea in the two weeks preceding the survey date. The statistically significant change in this outcome variable, however, could be driven by the false discovery resulting from multiple hypothesis testing.

source of birth counts during our sample period.

NPC estimates total births in the treated states during the FMCHP period to be 15,112,636. Recall that our estimate of the reduction in the under-five mortality is 0.02683 (see column (2) of Table A3 and under-five mortality row). Thus, the number of child deaths before the age of five averted in the states where FMCHP is rolled out in the post-roll out period is 405,425. We obtain the total expenditure incurred on FMCHP from Onwujekwe et al. (2016b). FMCHP disbursed 13,237,031,694.03 *Naira* to the states where FMCHP was rolled out between November 2008 and September 2014. To translate this expense to U.S. dollar value, we use the Central Bank of Nigeria's exchange rate for the period 2009 to 2015. The estimated expenditure on FMCHP in U.S. dollars is 82,590,232 or \$0.08259023 billion.

Combining the number of child deaths averted before age five with the total expenditure on FMCHP, we estimate that it costs approximately \$203.71 to save a child's life before their fifth birthday. We compare these estimates to the cost of averting the death of a young nonwhite child due to Medicaid's introduction in the U.S. in the 1960s and 1970s. Goodman-Bacon (2018) estimate that it costs \$2.1 million to save a young (age one to four) nonwhite child. Our estimate of the cost per death averted of a young child is a paltry proportion of this cost estimate.

Another way we can think of the welfare cost of averting the death of a child under the age of five is to estimate what fraction of annual household expenditure is the amount incurred to save this life. To make progress in this direction, we use Nigeria General Household Survey Wave One (2010-2011). Specifically, we use post-harvest household data on expenditures incurred on meals away from home, food expenditures, and non-food expenditures. Our estimates suggest that an average household expends \$4777 annually. Therefore, we estimate that FMCHP saves the life of a child under the age of five for 4.26% of annual expenditure for an average household.

Evaluating multiple interventions related to health and nutrition in Niger, Amouzou et al. (2012) find that these programs reduced child deaths annually by 58,795. Our estimates suggest a very close but slightly higher annual child lives saved estimate of 67,570. Nonetheless, Amouzou et al. (2012) eval-

uates a confluence of policies not all of which are intended to provide free healthcare for the mothers and the child. Furthermore, the absence of program costs precludes these authors from estimating the cost per life saved of a young child.

6 Discussion and Conclusion

In this paper, we study the health effects of a free maternal and child health program in Nigeria. We leverage variation in program implementation timing along with variation across states in program exposure. We show that children potentially exposed to FMCHP are more likely to survive beyond their fifth birthday and weigh more at birth. The improvements in the health outcomes are more pronounced for relatively more disadvantaged subpopulations. We shed light on the crucial role of health facility access in moderating the changes in health outcomes due to FMCHP exposure. Increased demand for institutional care and an increase in child immunizations are the mechanisms we highlight as the driving forces behind the observed improvement in health outcomes. Our back-of-the-envelope estimates suggest that FMCHP may have saved over $400,000$ lives for children under the age of five over a period of six years.

This work has several limitations. The health outcomes we observe are short- to medium-run in nature. Failure to account for life-cycle changes in health outcomes might lead us to underestimate the cost-to-benefit ratio of the program. We also do not observe other outcomes that may have been impacted by the program exposure, such as educational attainment. Not having information on the specific community engagement practices precludes us from delving into the role of such outreach campaigns on healthcare utilization. As the program puts great emphasis on these activities, having evidence about the precise components that help facilitate the change in health outcomes might be informative for policymakers. We hope future work can address some of these shortcomings.

Our work has multiple takeaways for policymakers working on expanding healthcare access for mothers and their children in a low-middle-income country. Reducing financial barriers to access healthcare

services not only improves the survival likelihood of children, it also induces more intense utilization of institutional facilities by women. These changes in demand for healthcare, however, depend crucially on access to healthcare facilities. This suggests that without supply-side changes, programs that aim to stimulate healthcare demand may not achieve their stated objectives. As programs that reduce the financial costs of healthcare are inherently costly, policymakers can prioritize those subpopulations that have low baseline rates of healthcare use or are socially and economically disadvantaged. Finally, we also note the important role played by community engagement in facilitating a change in preferences and perceptions about healthcare obtained through health institutions. As of this writing, Mali and Zimbabwe are deliberating a similar roll out. The policymakers have much to learn from our work to improve the effectiveness of their program.

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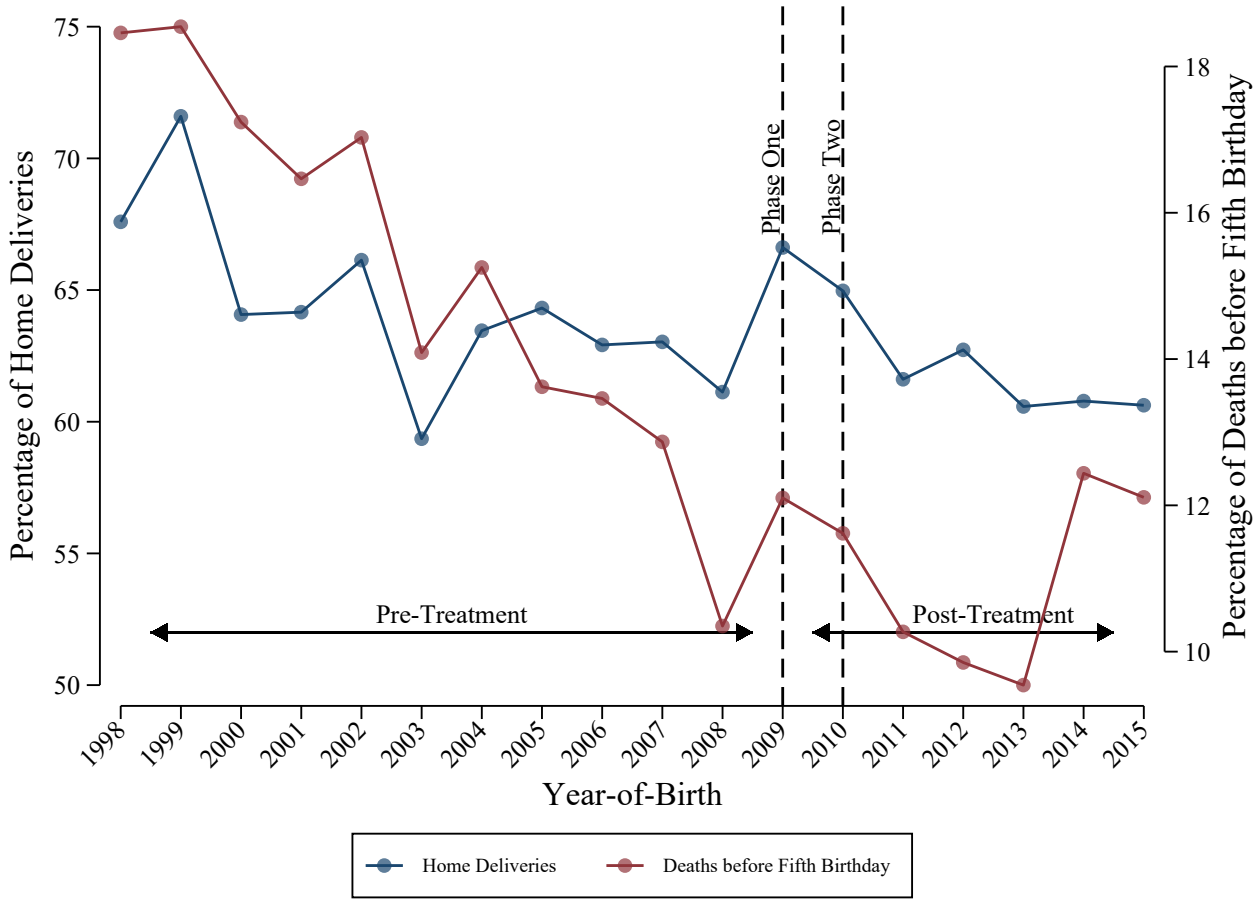
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Appendices

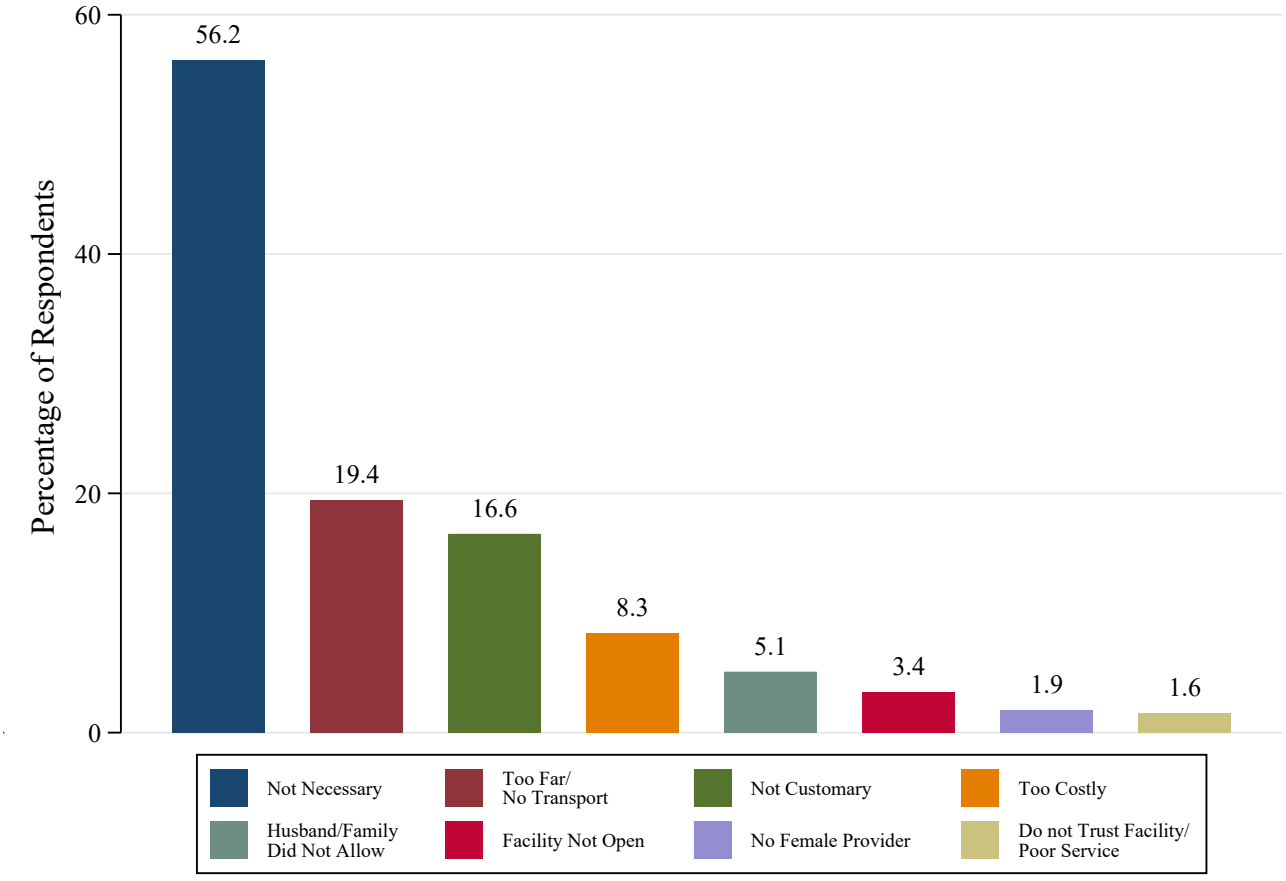
Appendix A Figures and Tables

Figure A1: Temporal Variation in Home Deliveries and Deaths before Fifth Birthday



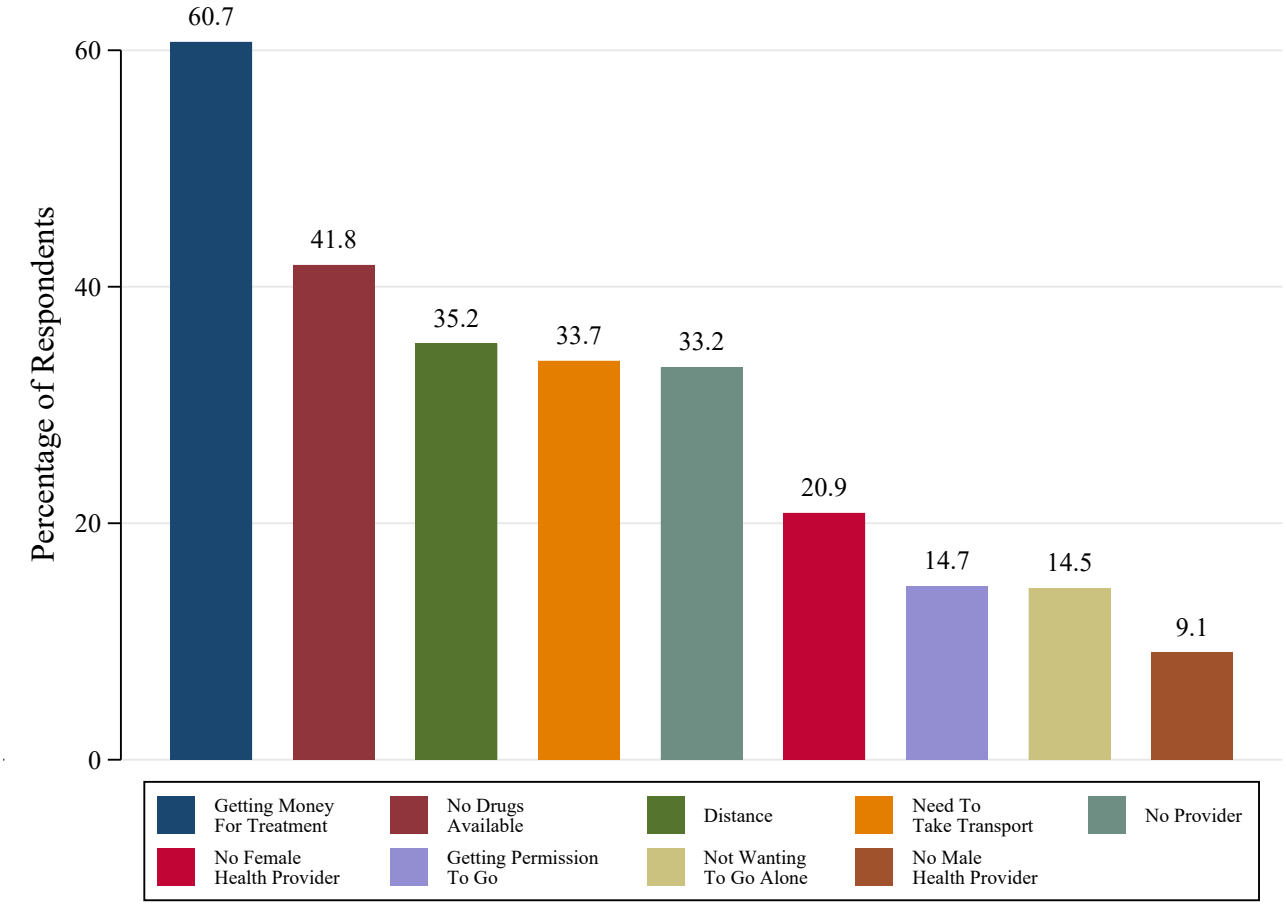
Note: Data from Demographic Health Survey 2008, 2013, and 2018 rounds. Survey weights are used to aggregate individual births to the year-level. These weights account for complex survey design. Two vertical dashed rounds indicate the beginning of two phases of the Free Maternal and Child Health Program (FMCHP).

Figure A2: Reasons for Not Delivering at a Health Facility



Note: Data from Demographic Health Survey 2008 and 2013 rounds. Pre-treatment data only. The reasons for not delivering at a health facility are not mutually exclusive. Survey weights are used to account for complex survey design.

Figure A3: Reasons for Not Seeking Healthcare When Sick



Note: Data from Demographic Health Survey 2008 round. Pre-treatment data for states where FMCHP is rolled out only. The reasons for not seeking healthcare when sick are not mutually exclusive. A reason is coded in affirmation if the respondent reports it as a big problem when she is sick and wants to get medical advice or treatment. Survey weights are used to account for complex survey design.

Figure A4: Timeline of FMCHP

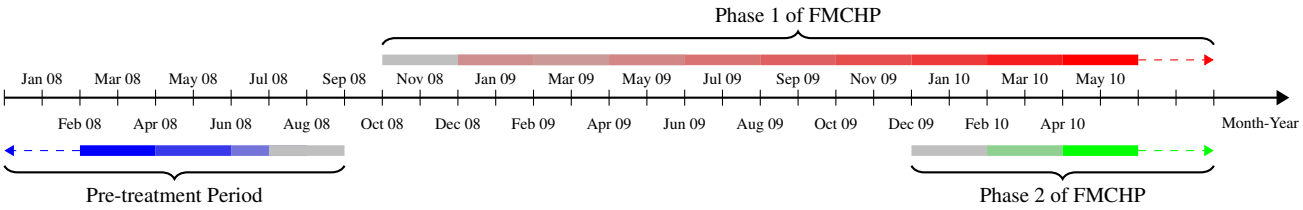
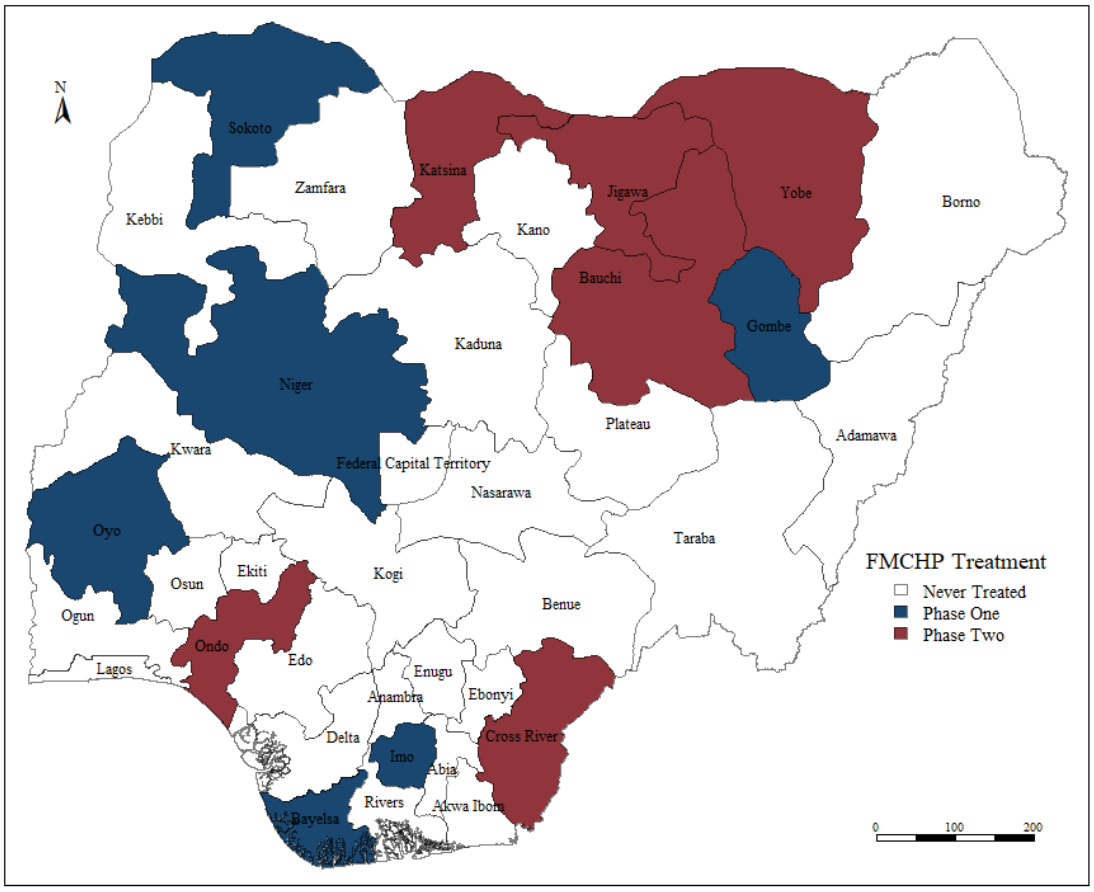
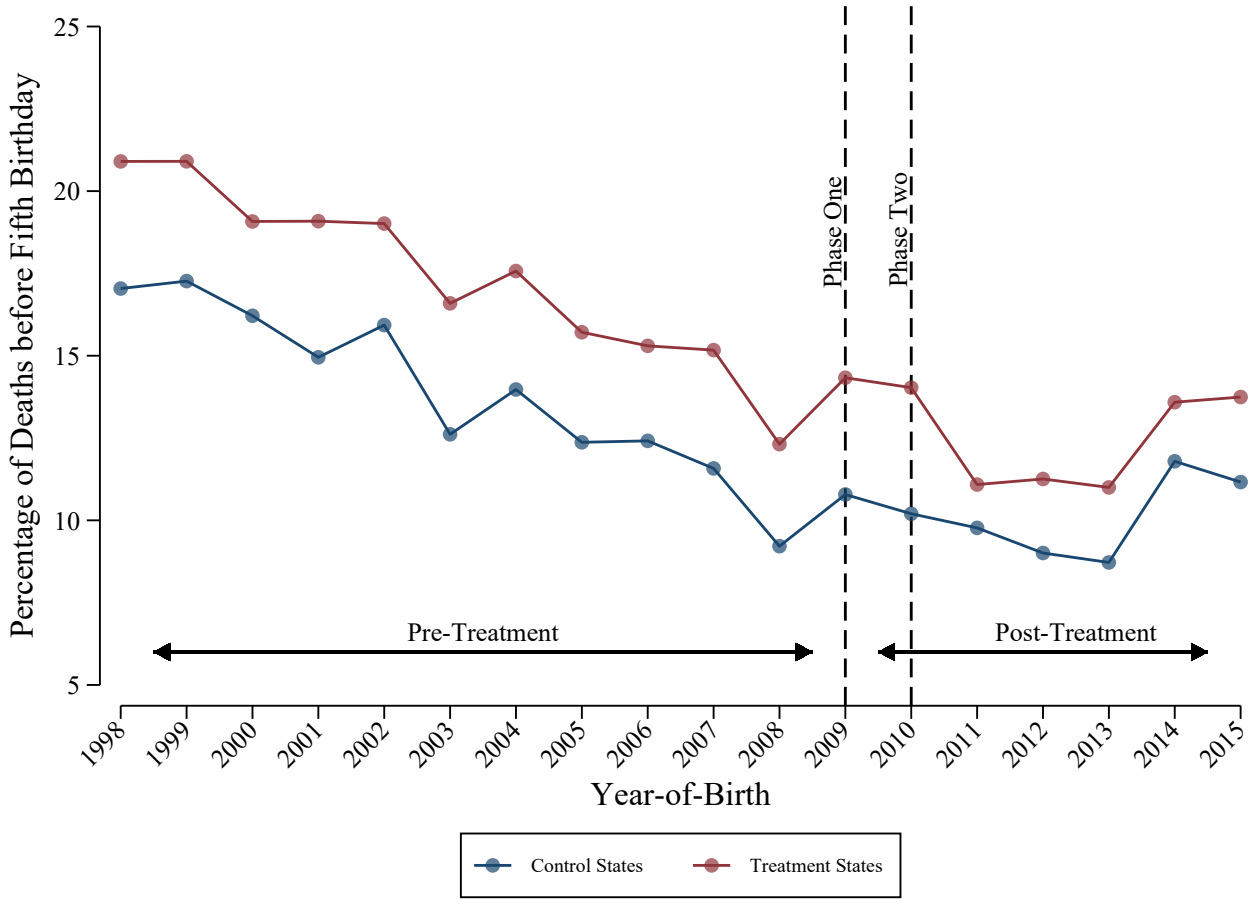


Figure A5: Spatial Variation in Free Maternal and Health Care Program (FMCHP) Roll Out



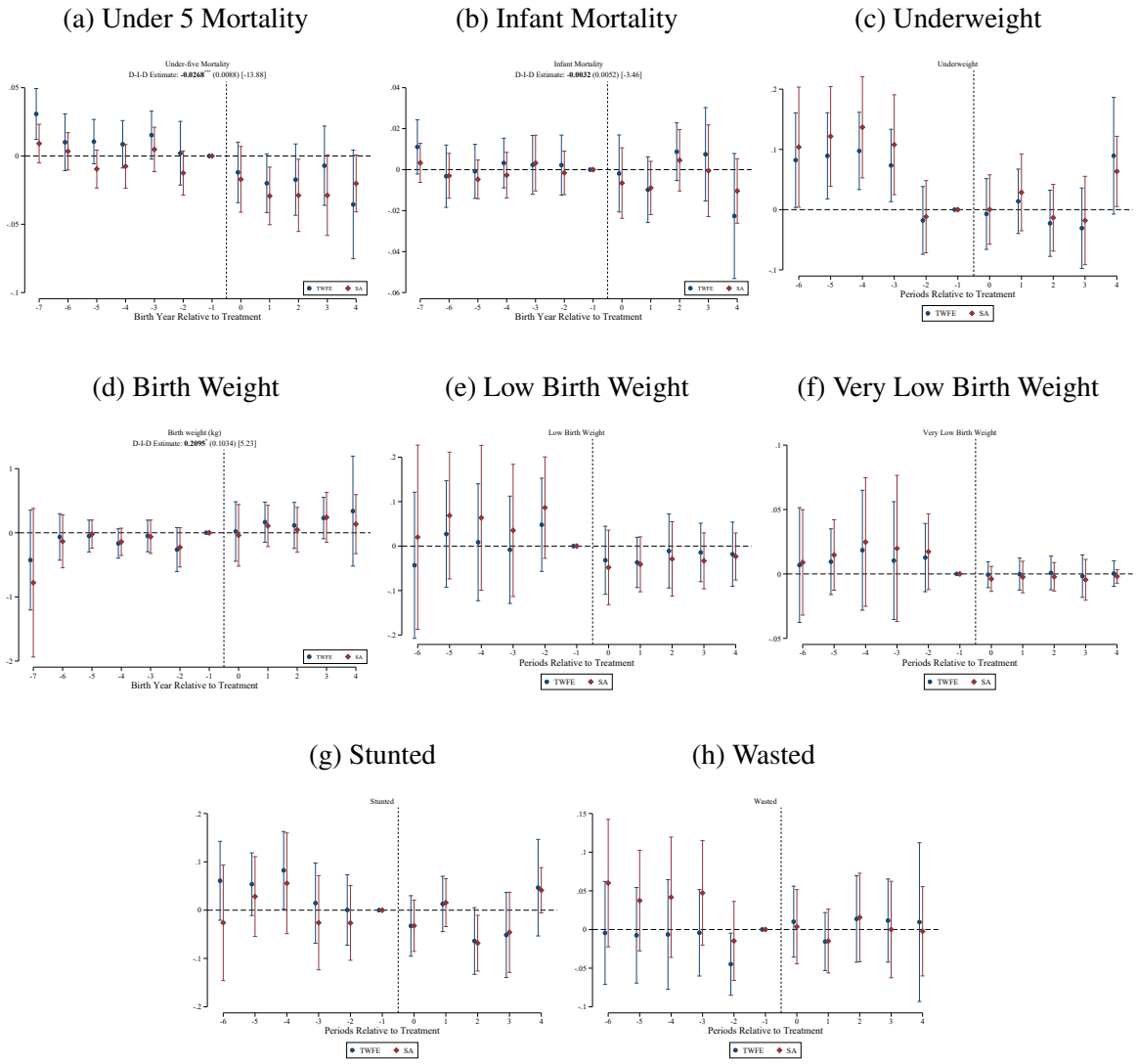
Note: Shapefiles data comes from Global Administrative Areas (GADM).

Figure A6: Temporal Variation in Deaths before Fifth Birthday



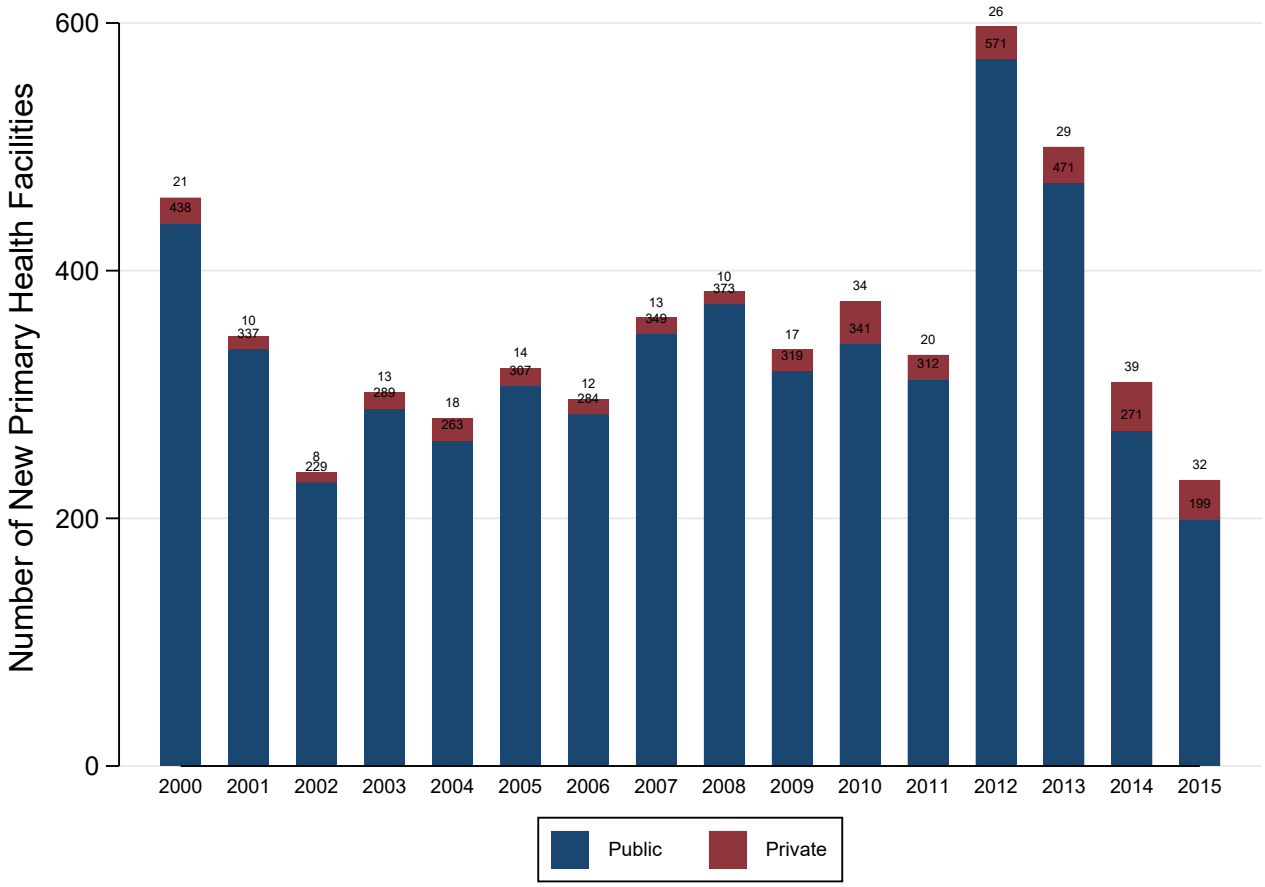
Note: Data from Demographic Health Survey 2008, 2013, and 2018 rounds. Survey weights are used to aggregate individual births to the year-level. These weights account for complex survey design. Two vertical dashed rounds indicate the beginning of two phases of the Free Maternal and Child Health Program (FMCHP).

Figure A7: Effect of Free Maternal and Child Health Program (FMCHP) on Child Outcomes



Note: The figure presents event-study estimates and 95% confidence intervals from a specification with mother and year-month-of-birth fixed-effects. This specification is presented in Equation 3. This specification also includes an indicator for multiple births, birth order, and an indicator for sex of the child. All estimates are relative to the period immediately before the first year of treatment. Estimates labeled "SA" are from Sun and Abraham (2021) while those labeled "TWFE" are from two-way-fixed-effects specifications. The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Figure A8: New Primary Health Facilities in Treated States 2000-2015



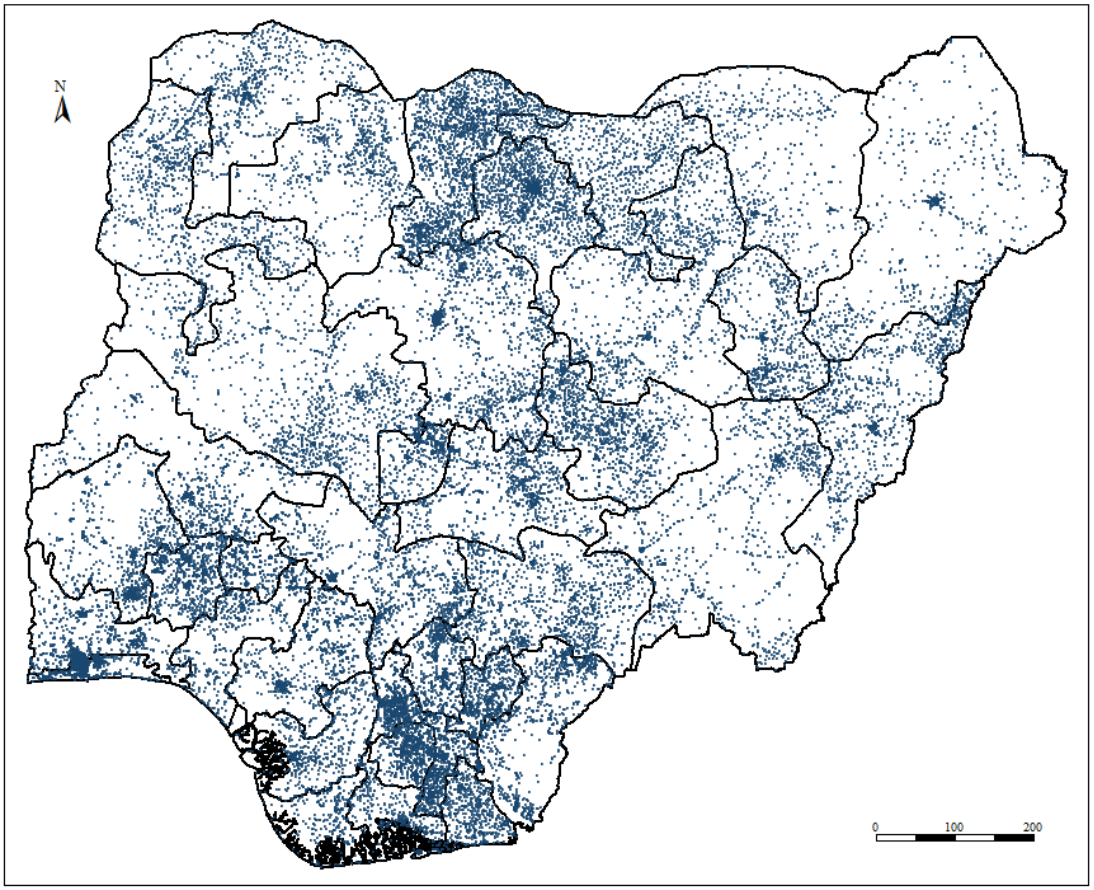
Note: Data on health facilities are derived from the Nigeria Health Facility Registry. These data are scraped in February 2024. Only those health facilities whose level is reported as "primary" in the data are used. Health facilities with missing start dates are dropped (approximately 11.74% of all health facilities). Health facilities that are reported to be located in states where the Free Maternal and Child Health Program (FMCHP) is ever rolled out are used. The estimate at the top of each bar represents the count of new health facilities in the corresponding year. The bottom estimate is for new public primary health facilities while the top estimate is for new private primary health facilities.

Figure A9: Dropping Births at Different Birth Order



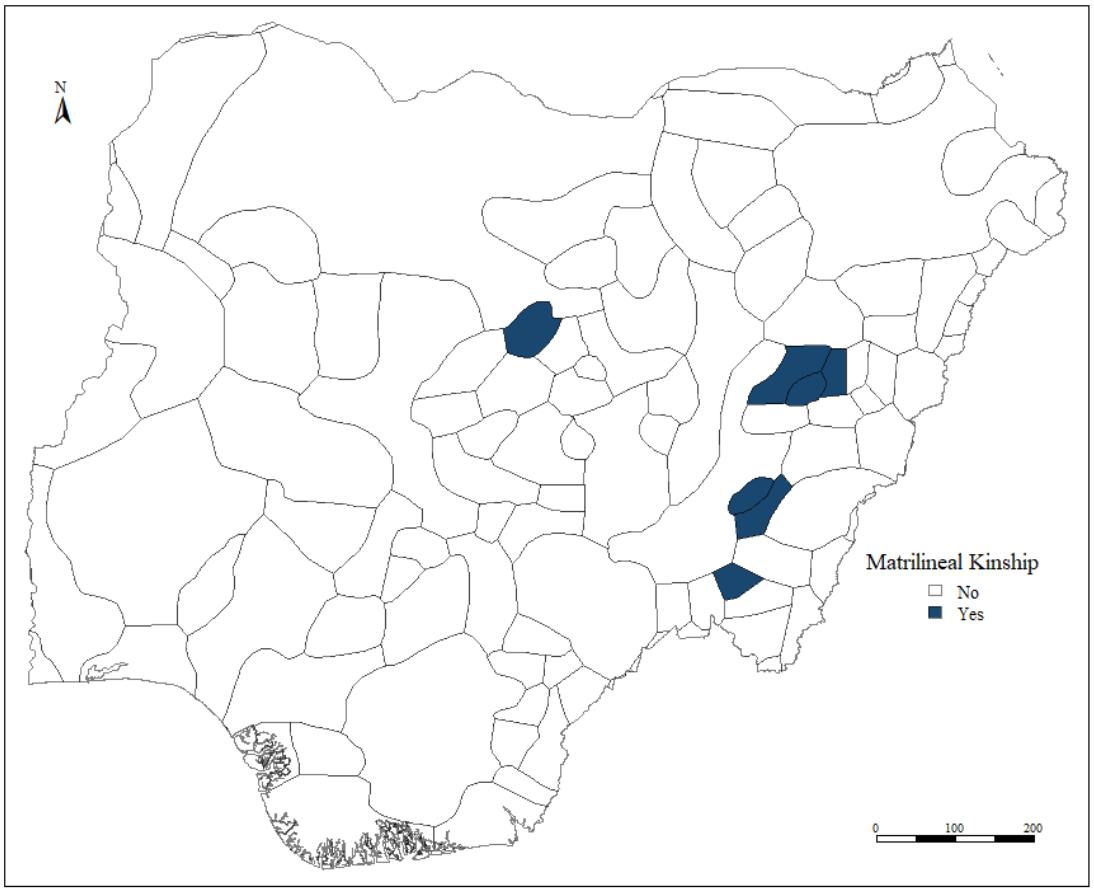
Note: The figure presents estimates and 95% confidence intervals of Equation 1. The variable on the vertical axis is the estimate of the interaction of an indicator variable for mothers' residence state ever having Free Maternal and Child Health Program (FMCHP) roll out and an indicator for the birth to be after FMCHP is rolled out. Each specification includes an indicator for multiple births, birth order, an indicator for sex of the child, mother fixed-effects, and child's month- and year-of-birth fixed-effects. "Baseline" estimate is for births at all birth orders. "Drop BO 1" estimate drops all first order births from the analytical sample. "Drop BO 2" estimate drops all second order births from the analytical sample. "Drop BO 3" estimate drops all third order births from the analytical sample. "Drop BO 4" estimate drops all fourth order births from the analytical sample. The dependent variable is the under-five mortality. Refer to Section 3.1 for the definition of the dependent variable. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Figure A10: Spatial Distribution of Primary Health Facilities in Nigeria



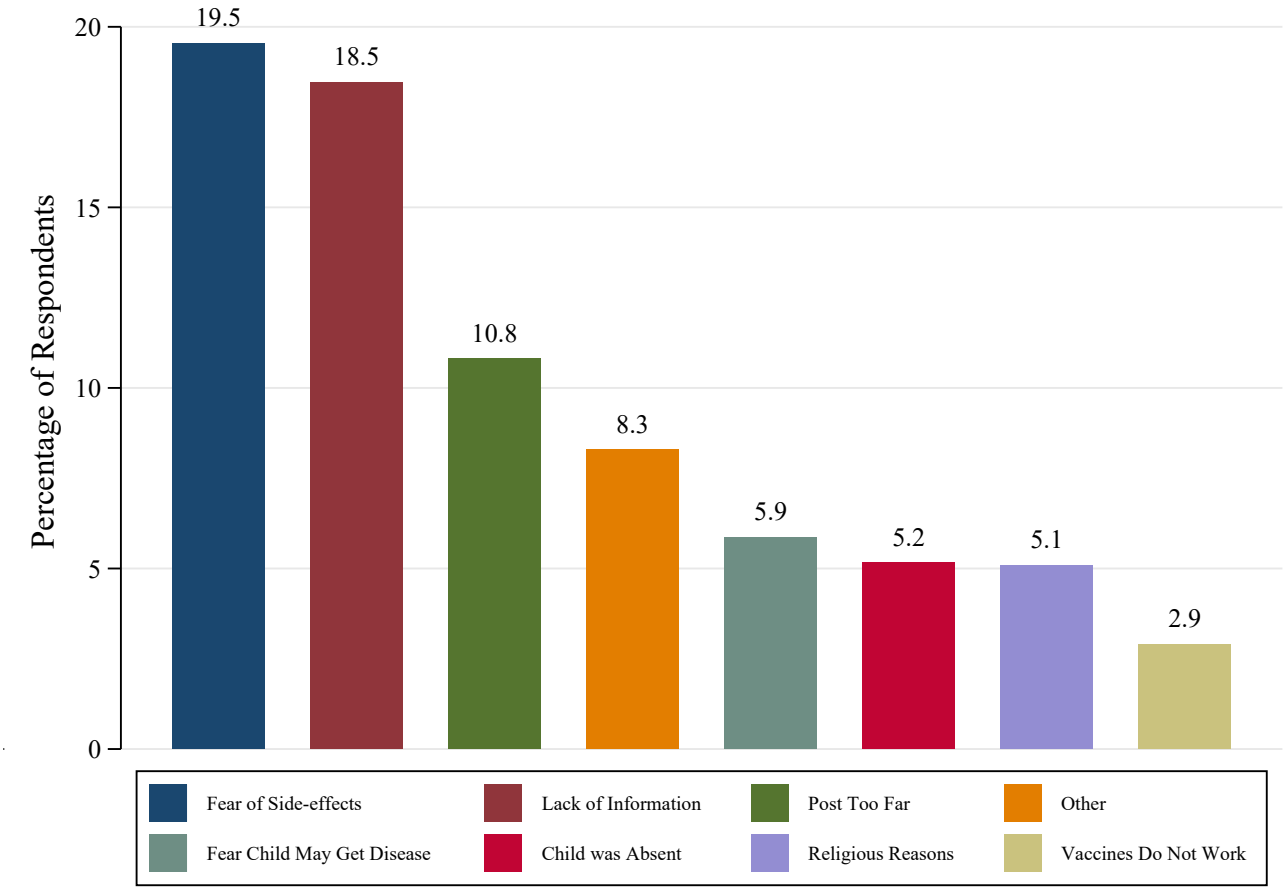
Note: Shapefiles data comes from Global Administrative Areas (GADM). Data on health facilities are derived from the Nigeria Health Facility Registry. These data are scraped in February 2024. Only those health facilities whose level is reported as "primary" in the data are used.

Figure A11: Spatial Distribution of Ethnic Groups Following Matrilineal Kinship Structure



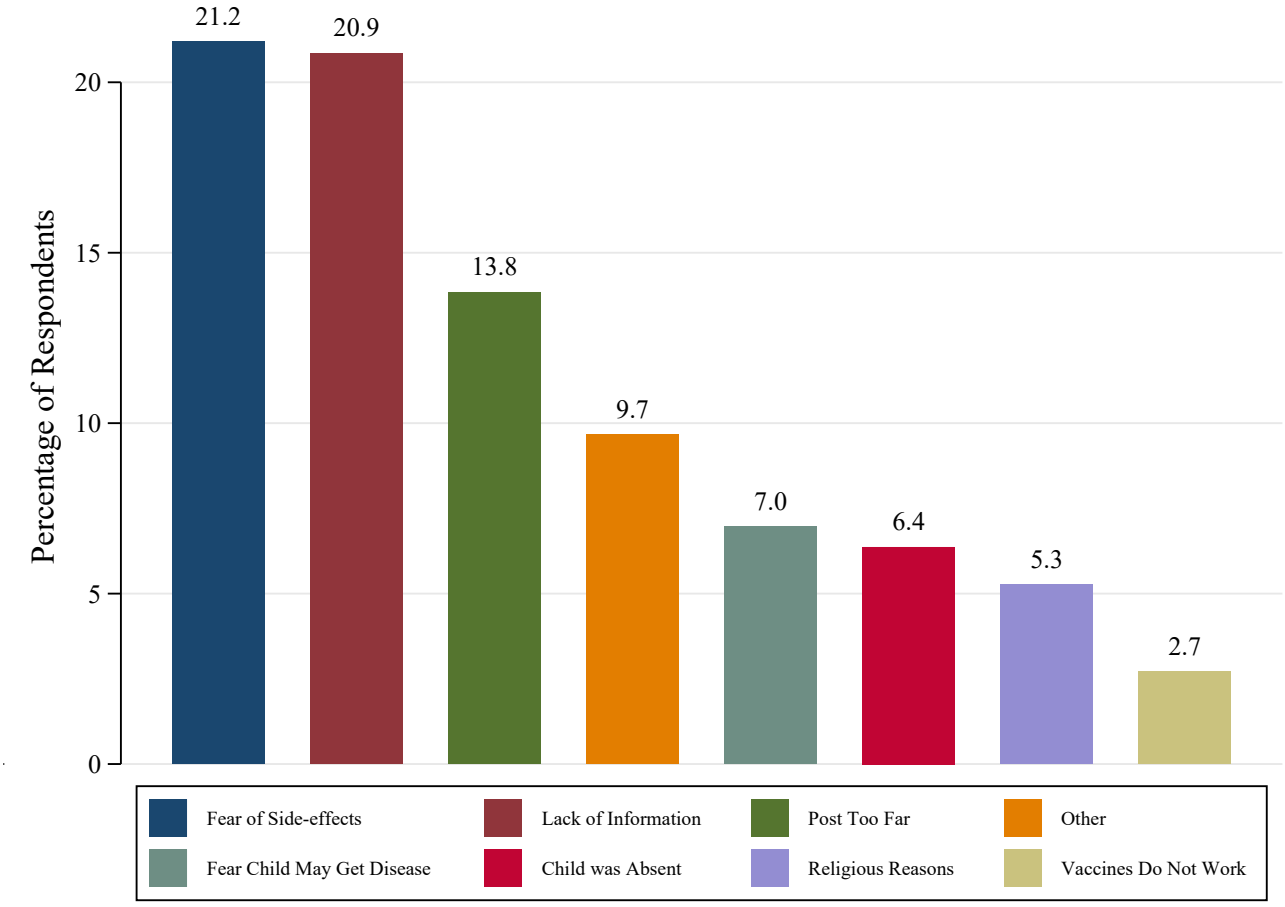
Note: Shapefiles data on ethnic group boundaries are derived from Nunn and Wantchekon (2011). The information on kinship structure is derived from Murdock (1967).

Figure A12: Reasons for Not Vaccinating the Child



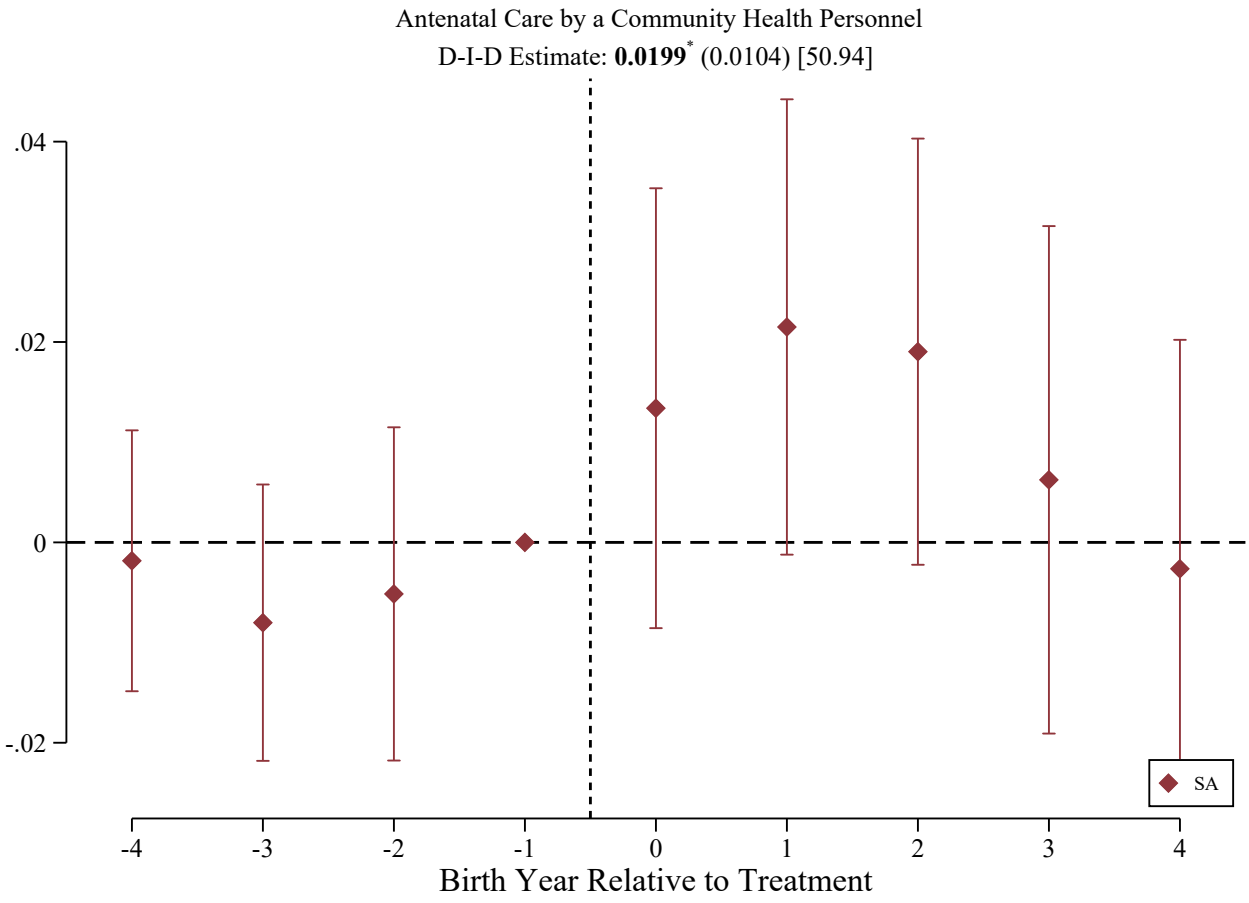
Note: Data from Demographic Health Survey 2008 round. Pre-treatment data for states where FMCHP is rolled out only. The reasons for not vaccinating the child are not mutually exclusive. Survey weights are used to account for complex survey design.

Figure A13: Reasons for Not Vaccinating the Child Against Polio



Note: Data from Demographic Health Survey 2008 round. Pre-treatment data for states where FMCHP is rolled out only. The reasons for not vaccinating the child are not mutually exclusive. Survey weights are used to account for complex survey design.

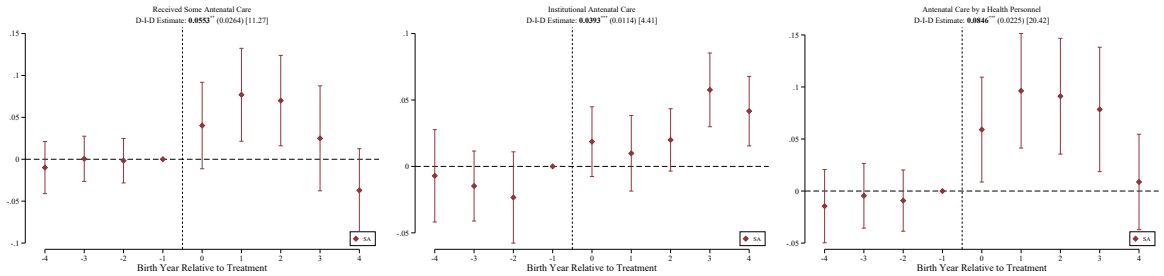
Figure A14: Event-study Estimates: Antenatal Care by a Community Health Personnel



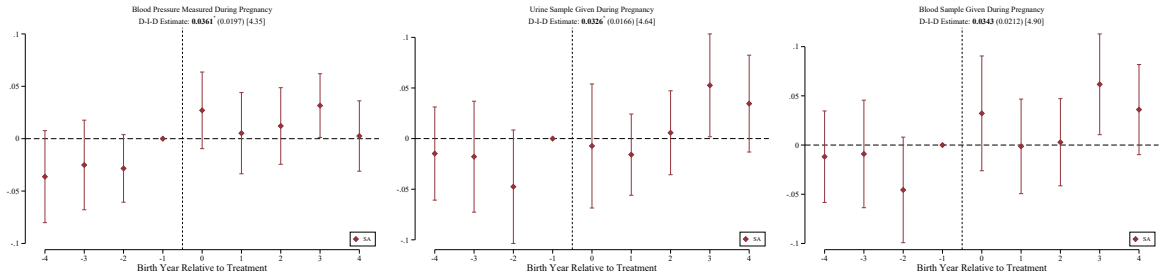
Note: The figure presents event-study estimates and 95% confidence intervals from a specification with state and year-month-of-birth fixed-effects. This specification is presented in Equation 2. This specification also includes indicators for the mother's year-of-birth, birth order, an indicator for multiple births, indicators for religion, number of household members, an indicator for the sex of child. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. The dependent variable is an indicator variable for whether the antenatal care is provided by a community health personnel. This variable is defined only for those respondents who report receiving some antenatal care. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Figure A15: Event-study Estimates: Pre-delivery Interaction with the Medical Providers

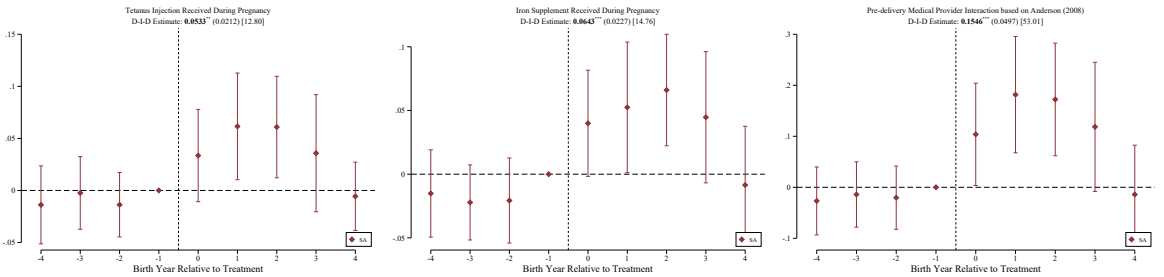
(a) Received Some Antenatal Care (b) Institutional Antenatal Care (c) Antenatal Care by a Health Personnel



(d) Blood Pressure Measured During Pregnancy (e) Urine Sample given During Pregnancy (f) Blood Sample given During Pregnancy



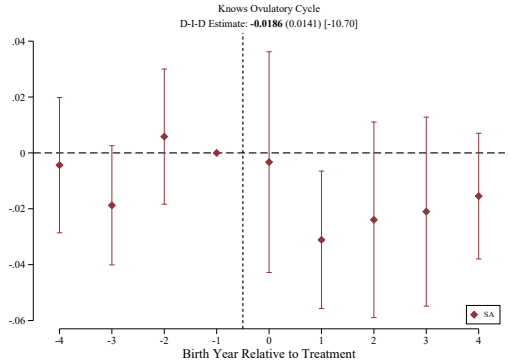
(g) Tetanus Injection received During Pregnancy (h) Iron Supplement received During Pregnancy (i) Pre-delivery Interaction with Medical Providers Index



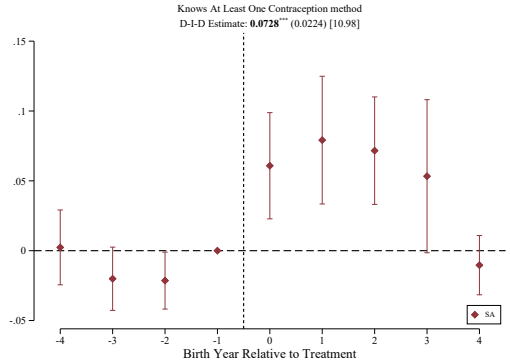
Note: The figure presents event-study estimates and 95% confidence intervals from a specification with state and year-month-of-birth fixed-effects. This specification is presented in Equation 2. This specification also includes indicators for the mother's year-of-birth, birth order, an indicator for multiple births, indicators for religion, number of household members, an indicator for the sex of child. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. The dependent variable in the last panel is a standardized weighted index of multiple indicator variables using a GLS weighting procedure as described in Anderson (2008). The indicator variables used to construct this index are reported in the first five panels. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Figure A16: Event-study Estimates: Information

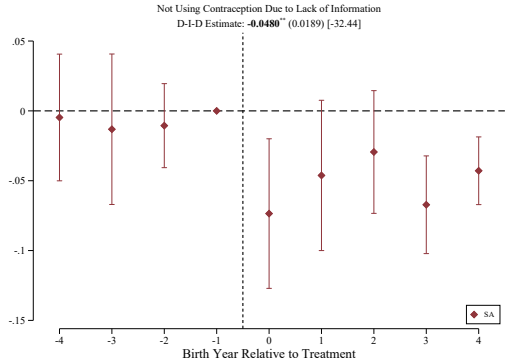
(a) Knows Ovulatory Cycle



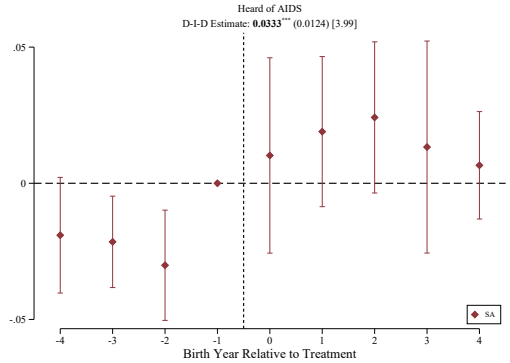
(b) Knows At Least One Contraception Method



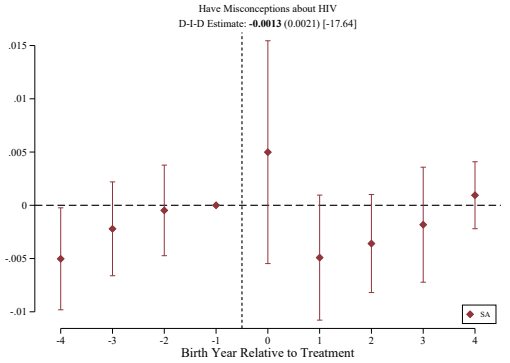
(c) Not using Contraception due to Lack of Information



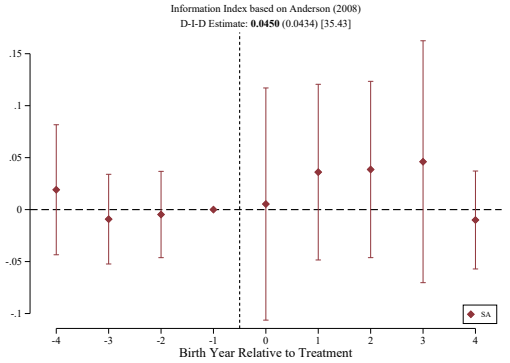
(d) Heard of AIDS



(e) Have Misconceptions about AIDS

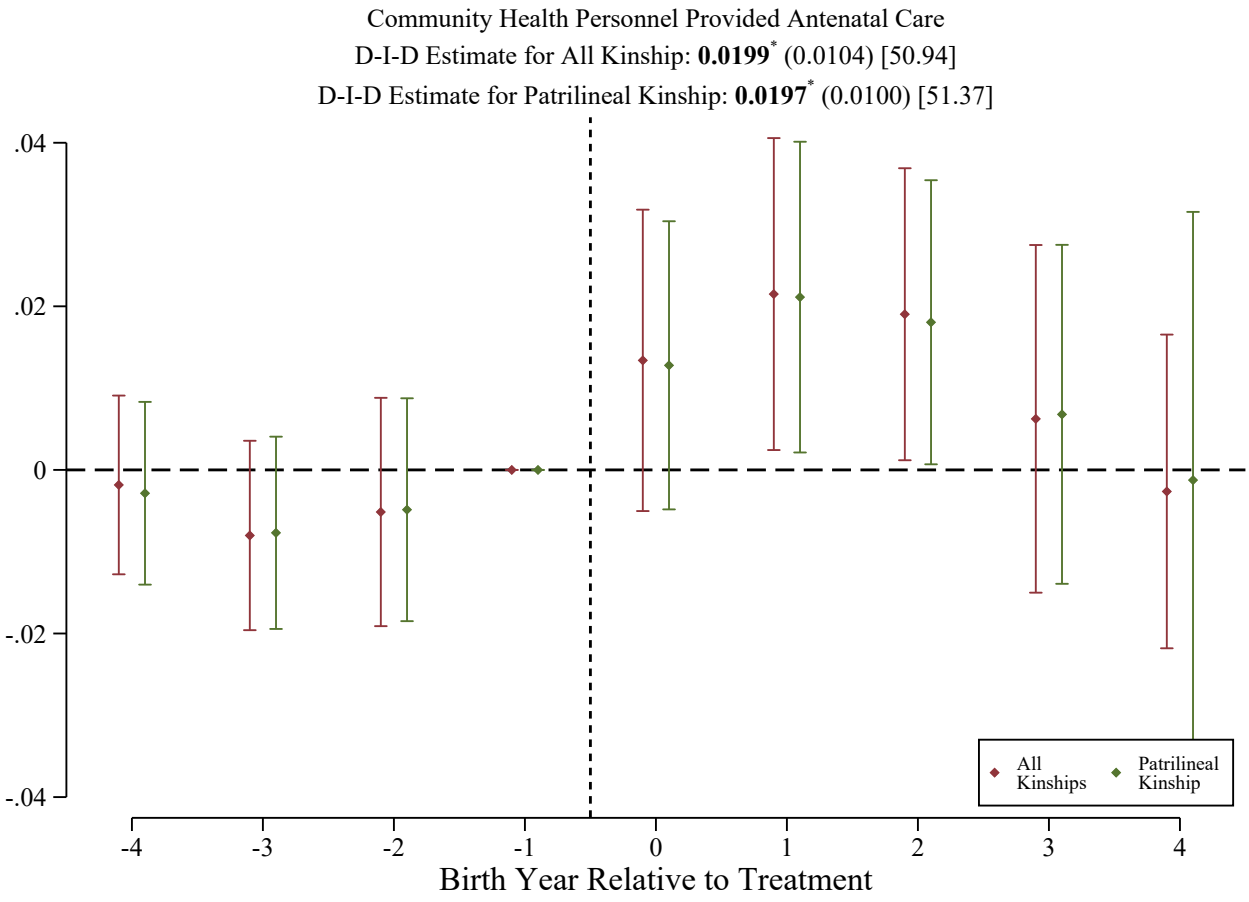


(f) Information Index



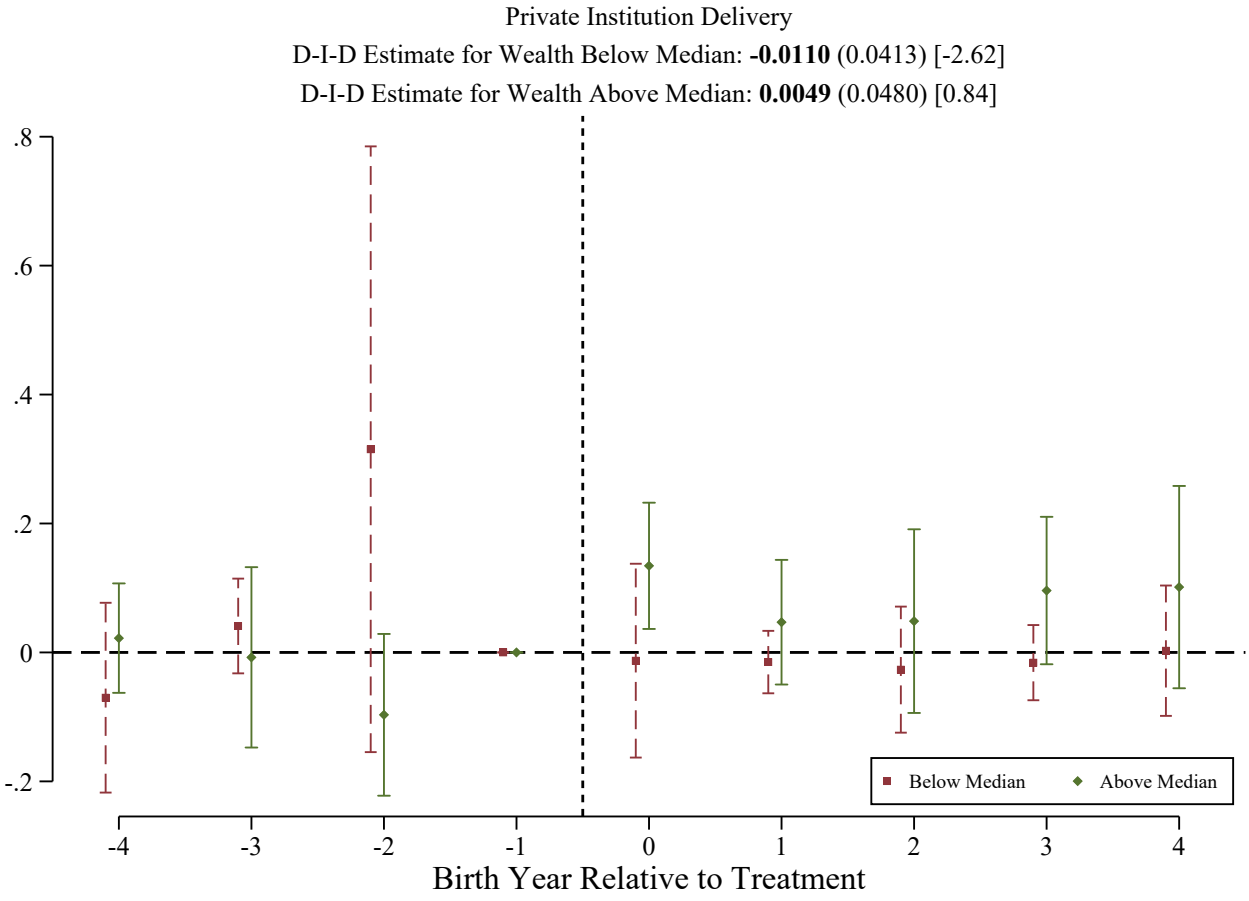
Note: The figure presents event-study estimates and 95% confidence intervals from a specification with state and year-month-of-birth fixed-effects. This specification is presented in Equation 2. This specification also includes indicators for the mother's year-of-birth, indicators for religion, and number of household members. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. The dependent variable in the last panel is a standardized weighted index of multiple indicator variables using a GLS weighting procedure as described in Anderson (2008). The indicator variables used to construct this index are reported in the first five panels. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Figure A17: Event-study Estimates: Community Health Personnel Provided Antenatal Care by Kinship Structure of Ethnic Group



Note: The figure presents event-study estimates and 95% confidence intervals from a specification with state and year-month-of-birth fixed-effects. This specification is presented in Equation 2. This specification also includes indicators for the mother's year-of-birth, birth order, an indicator for multiple births, indicators for religion, number of household members, an indicator for the sex of child. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. The dependent variable is an indicator variable for whether the antenatal care is provided by a community health personnel. This variable is defined only for those respondents who report receiving some antenatal care. To classify women as belonging to an ethnic group that follows matrilineal kinship structure, we use the reported ethnicity in the Demographic and Health Survey (DHS). Ethnic groups following kinship structure are derived from Murdock (1967). All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

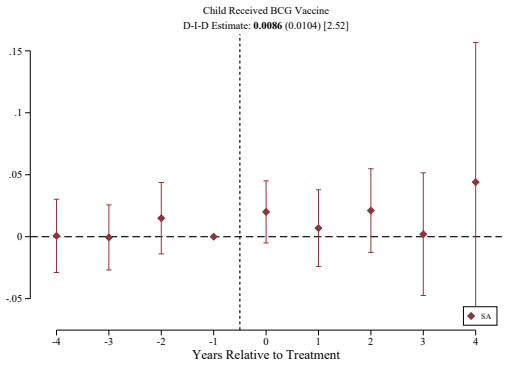
Figure A18: Event-study Estimates: Woodwork Effect



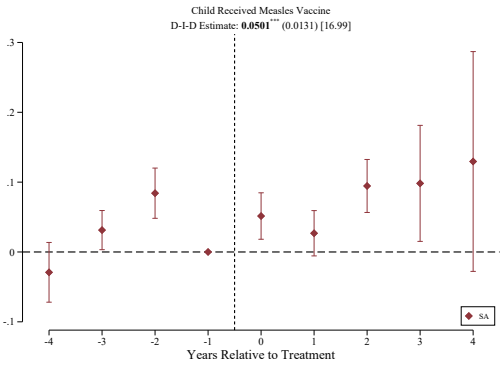
Note: The figure presents event-study estimates and 95% confidence intervals from a specification with mother and year-month-of-birth fixed-effects. This specification is presented in Equation 3. This specification also includes an indicator for multiple births, birth order, and an indicator for the sex of the child. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. All subpopulations are constructed using the reported data in the DHS. Only primary facilities whose reported ownership type is for profit are used to restrict the analytical sample to those states that are above the median of health facilities per-capita distribution. Population data at the state-level are derived from the population and housing census conducted in 2006. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Figure A19: Event-study Estimates: Preventive Care

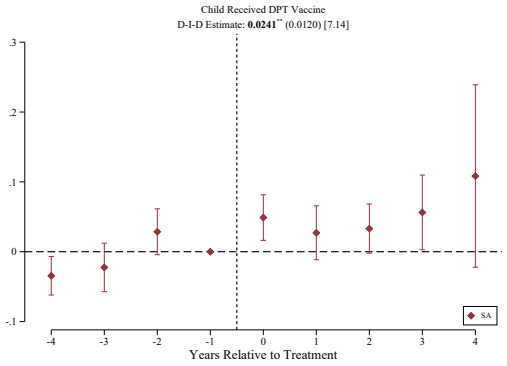
(a) BCG Vaccine



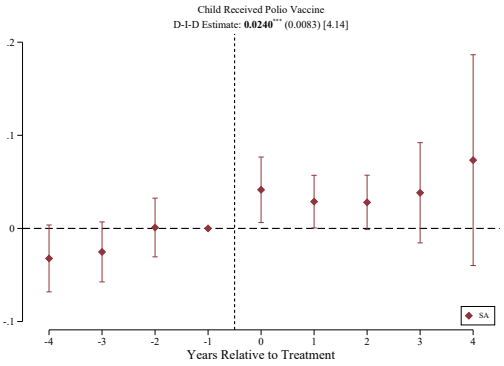
(b) Measles Vaccine



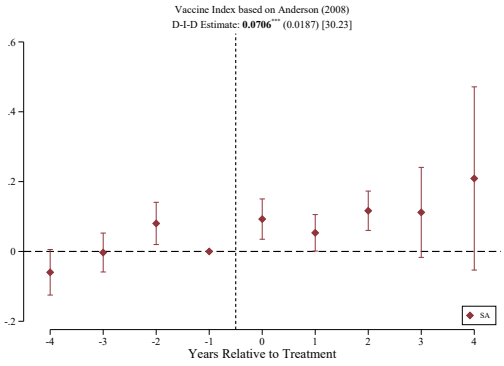
(c) DPT Vaccine



(d) Polio Vaccine



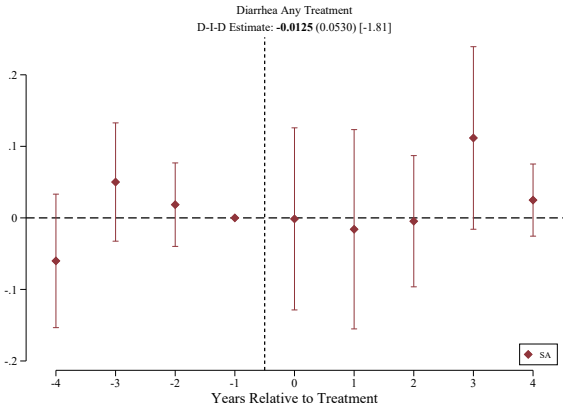
(e) Vaccination Index



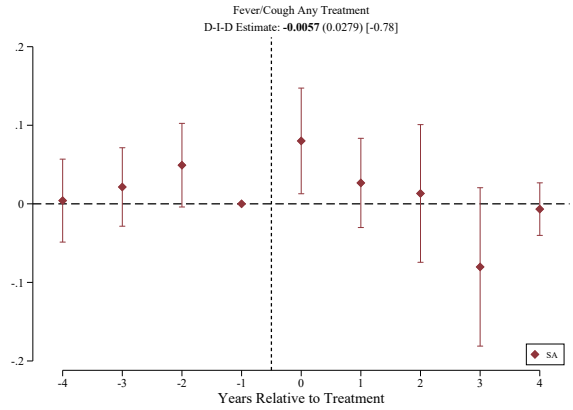
Note: The figure presents event-study estimates and 99% confidence intervals from a specification with mother and year-month-of-birth fixed-effects. This specification is presented in Equation 3. This specification also includes an indicator for multiple births, birth order, and an indicator for the sex of the child. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. The dependent variable in the last panel is a standardized weighted index of multiple indicator variables using a GLS weighting procedure as described in Anderson (2008). The indicator variables used to construct this index are reported in the first four panels. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Figure A20: Event-study Estimates: Acute Care

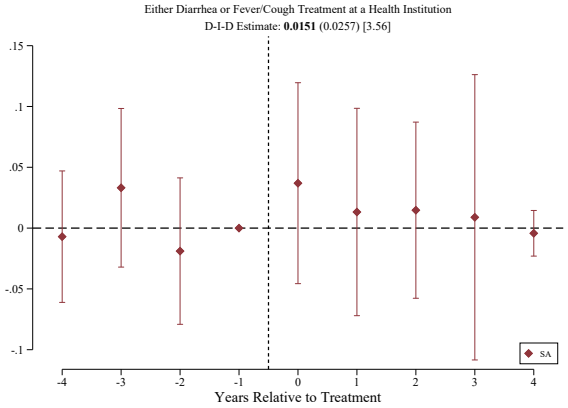
(a) Any Treatment for Diarrhea



(b) Any Treatment for Fever or Cough



(c) Treatment at a Health Institution



Note: The figure presents event-study estimates and 95% confidence intervals from a specification with mother and year-month-of-birth fixed-effects. This specification is presented in Equation 3. This specification also includes an indicator for multiple births, birth order, and an indicator for the sex of the child. All estimates are relative to the period immediately before the first year of treatment. Estimates are from the interaction weighted (IW) estimator in Sun and Abraham (2021). The estimates are for indicator variables that take a value of one if the difference between the year-of-birth and the first year of treatment is as indicated. The first year of treatment is denoted by zero. The dependent variable in all three panels is defined only for those children who have had either diarrhea, fever, or cough in the two weeks preceding the survey date. The child is designated to have received treatment for diarrhea and fever or cough in the last two weeks if they sought advice or treatment for the illness from any source. The child is designated to have received treatment for diarrhea and fever or cough in the last two weeks at a health institution if they receive treatment at a government hospital, government health center, government health post, private hospital or clinic, private pharmacy, or private doctor. In the last panel, the dependent variable takes a value of one if the child has had either diarrhea, fever, or cough in the two weeks preceding the survey date and received treatment at a health institution. All births after 2015 are dropped from the analytical sample. Confidence intervals are constructed using heteroskedasticity robust standard errors clustered by the local government area. Exposure to the Free Maternal and Child Health Program (FMCHP) is determined based on the date-of-birth of the child. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A1: Balance of Covariates: Child Health Outcomes

	Pre-Treatment		Post-Treatment		T-Test (Pre-Treatment)	D-I-D
	Control	Treatment	Control	Treatment	Treatment - Control	
Under-five Mortality	0.150	0.191	0.098	0.119	0.041	-0.021
	(0.357)	(0.393)	(0.298)	(0.323)	(0.001)	
Infant Mortality	0.084	0.091	0.061	0.068	0.007	-0.000
	(0.278)	(0.288)	(0.239)	(0.251)	(0.001)	
Underweight	0.203	0.321	0.224	0.313	0.117	-0.029
	(0.402)	(0.467)	(0.417)	(0.464)	(0.005)	
Birth Weight	3.573	4.006	3.476	3.848	0.433	-0.061
	(1.426)	(2.288)	(1.202)	(1.917)	(0.052)	
Low Birth Weight	0.071	0.085	0.067	0.070	0.014	-0.011
	(0.257)	(0.278)	(0.250)	(0.255)	(0.008)	
Very Low Birth Weight	0.004	0.007	0.006	0.004	0.003	-0.005
	(0.062)	(0.084)	(0.076)	(0.062)	(0.002)	
Stunted	0.385	0.465	0.313	0.429	0.081	0.035
	(0.487)	(0.499)	(0.464)	(0.495)	(0.006)	
Wasted	0.110	0.175	0.150	0.167	0.065	-0.047
	(0.313)	(0.380)	(0.357)	(0.373)	(0.004)	
N	126,463	74,993	13,617	8,438		

Notes: This table presents the descriptive statistics and balance of covariates in the pre-treatment and post-treatment period of the Free Maternal and Child Health Care Program (FMCHP). The mean of the variables and standard deviations are presented. Standard deviations are in parenthesis. The variable is displayed in the left column. Refer to Section 3.1 for the definition of each variable. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the date-of-birth of the child. The treatment group consists of all the births that happened in the states where FMCHP is rolled out. All births in states where FMCHP is not rolled out constitute the control group. The pre-treatment period is all the years before the rollout of the FMCHP policy, while the post-treatment period is all the years after the rollout of the FMCHP policy. The T-test column provides test statistics for the two-group mean-comparison test in the pre-treatment period between the treatment and control group. D-I-D estimate in the last column is the difference in unconditional means of treatment and control group between post- and pre-treatment periods. The data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A2: Composition of Mothers

	Mother Illiterate	Mother No Education	Household Top Two Wealth Quintiles	Mother Married	Spouse No Education	Covered by Health Insurance	Urban Residence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat × Post	0.01951	0.01369	0.01098	-0.00872	0.00113	-0.00353	-0.04929
	(0.01666)	(0.01610)	(0.01795)	(0.00843)	(0.01876)	(0.00429)	(0.03026)
	[2.82]	[2.30]	[4.14]	[-0.97]	[0.22]	[-28.60]	[-20.23]
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	50,831	51,065	51,065	51,064	49,547	50,868	51,065

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. The treated group comprises all states where the Free Maternal and Child Health Care Program (FMCHP) is rolled out. Each column is a separate estimation of Equation (2). Each specification also includes indicators for the mother's year-of-birth, indicators for religion, number of household members, mother's residence state fixed-effects, child's month- and year-of-birth fixed-effects. The dependent variable is displayed in the column header. Refer to Section 4 for the definition of each dependent variable. The Independent variable in each column is the interaction of an indicator variable for mothers' residence state ever having FMCHP rollout and an indicator for the birth to be after FMCHP is rolled out. The interaction is displayed in the left column. All births after 2015 are dropped from the analytical sample. Exposure to the Free Maternal and Child Health Care Program (FMCHP) is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A3: Main Effects: Child Health Outcomes

	State Fixed-effects		Mother Fixed-effects	
	(1)	(2)	(3)	(4)
Under-five Mortality	-0.03794***	-0.04129***	-0.02904***	-0.02683***
	(0.00571)	(0.00585)	(0.00905)	(0.00879)
	[-19.83]	[-21.58]	[-15.18]	[-14.02]
Infant Mortality	-0.01061***	-0.01328***	-0.00467	-0.00315
	(0.00393)	(0.00378)	(0.00524)	(0.00517)
	[-11.66]	[-14.60]	[-5.14]	[-3.46]
Underweight	-0.03827***	-0.03952***	-0.00144	0.00218
	(0.01474)	(0.01409)	(0.02039)	(0.02036)
	[-11.94]	[-12.33]	[-0.45]	[0.68]
Birth Weight	0.17744	0.18531	0.19225*	0.20955**
	(0.14312)	(0.13915)	(0.10372)	(0.10345)
	[4.43]	[4.63]	[4.80]	[5.23]
Low Birth Weight	-0.00577	-0.00313	0.00047	0.00078
	(0.01552)	(0.01521)	(0.01907)	(0.01946)
	[-6.83]	[-3.70]	[0.56]	[0.92]
Very Low Birth Weight	-0.00372	-0.00320	-0.00050	-0.00098
	(0.00414)	(0.00405)	(0.00505)	(0.00531)
	[-52.82]	[-45.40]	[-7.14]	[-13.95]
Stunted	0.02687*	0.02437*	-0.01678	-0.01290
	(0.01465)	(0.01426)	(0.01965)	(0.01961)
	[5.77]	[5.24]	[-3.61]	[-2.77]
Wasted	-0.05233***	-0.05177***	0.00732	0.00913
	(0.01353)	(0.01322)	(0.01580)	(0.01598)
	[-29.89]	[-29.57]	[4.18]	[5.21]
State FEs	Yes	Yes	No	No
Mother FEs	No	No	Yes	Yes
Year × Month FEs	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. The treated group comprises all states where the Free Maternal and Child Health Care Program (FMCHP) is rolled out. Each cell in the first two columns is a separate estimation of Equation (2). Each specification in the first two columns includes the mother's residence state fixed-effects and the child's month- and year-of-birth fixed-effects. To the specification in column (1), the specification in column (2) adds indicators for the mother's year-of-birth, birth order, an indicator for multiple births, indicators for religion, number of household members, and an indicator for the sex of the child. Each cell in the last two columns is a separate estimation of Equation (1). Each specification in the last two columns includes mother fixed-effects and child's month- and year-of-birth fixed-effects. To the specification in column (3), column (4) adds indicators for multiple births, birth order, and sex of the child. The dependent variable is displayed in the left column. Refer to Section 3.1 for the definition of each dependent variable. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having FMCHP rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the Free Maternal and Child Health Care Program (FMCHP) is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A4: Robustness Checks: Child Health Outcomes

	Baseline	Drop Birth Controls	Drop Lagos and Abuja	Add Post 2015 Observations	Add LGA Time Trends	Date of Conception	Exposure Months	Drop Above 40 Mothers	Drop Phase One States	Drop Existing FMCHP States	Drop First Order Births	First Order Singleton Births
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\mathbb{1}\{Treat_{s(b)}\} \times$	-0.02683***	-0.02967***	-0.02578***	-0.02683***	-0.02814***	-0.02979***		-0.02780***	-0.04558***	-0.03234***	-0.02880***	-0.03762***
$\mathbb{1}\{Post_{MoB(b),YoB(b)}\}$	(0.00879)	(0.00901)	(0.00906)	(0.00879)	(0.00880)	(0.01042)		(0.00878)	(0.01098)	(0.00935)	(0.01033)	(0.01155)
	[-14.02]	[-15.51]	[-13.47]	[-14.02]	[-14.71]	[-15.75]		[-14.53]	[-21.93]	[-16.58]	[-14.89]	[-20.57]
Exposure in Months							-0.00028**					
							(0.00011)					
							[-0.15]					
Mother FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Year × Month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LGA Time Trends	No	No	No	No	Yes	No	No	No	No	No	No	No
State FEs	No	No	No	No	Yes	No	No	No	No	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.126	0.114	0.127	0.126	0.126	0.126	0.126	0.128	0.126	0.127	0.124	0.049
N	224,194	224,194	215,201	224,194	224,194	224,194	224,194	220,824	187,398	200,931	172,992	50,506

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell is a separate estimation of Equation 1 except for column (12) which reports estimates from Equation 2. Column (2) drops birth specific control variables. In column (3), all respondents whose residence state is either Lagos or Abuja are dropped from the analytical sample. Column (4) adds births that occur post-2015 to the analytical sample. In column (5), Local Government Area (LGA) linear time trends are added. Column (6) determines treatment based on the date-of-conception. Date-of-conception is nine months preceding the date-of-birth. In column (7), treatment is determined based on the months of exposure to FMCHP. In column (8), all women who are over 40 years of age at the time of birth are dropped from the analytical sample. In column (9), all states where FMCHP is rolled out in the first phase are dropped from the analytical sample. In column (10), all states where FMCHP may have existed before 2008 are dropped from the analytical sample. These states are derived from Okonofua et al. (2011). Column (11) drops all first order births. In column (12), the analytical sample is restricted to first order and singleton births only. The dependent variable is displayed in the left column. Refer to the main text for the definition of each dependent variable. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having FMCHP rollout and an indicator for the birth to be after FMCHP is rolled out except for column (7) where the independent variable is the number of months the child is exposed to Free Maternal and Child Health Care Program (FMCHP). All births after 2015 are dropped from the analytical sample except in column (4). Each specification also includes indicators for multiple births, birth order, sex of the child, and mother fixed-effects except for column (2). In column (12), additional control variables on indicators for mother's month-year-of-birth, indicator variables for religion, and number of household members are included. Exposure to FMCHP is determined based on the date-of-birth of the child except in column (6) and column (7). The date-of-conception in column (6) is defined as nine months before the reported date-of-birth. Months of exposure are constructed by capping the policy exposure to ten months for mothers exposed to FMCHP during the entire pregnancy and 69 months for children exposed from conception till they are five years of age. For mothers and children exposed to FMCHP for months smaller than the cap, exposure is defined by the months the FMCHP policy is under operation in their residence state. These data come from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A5: Heterogeneity: Socioeconomic Characteristics

	Education		Age		Religion		Wealth		Health Insurance		Multigenerational Household		Region		Previous Pregnancy Complication		Mass Media Consumption		Urban Residence	
	No Education	Primary or Higher	Below 30	Above 30	Islam	Christianity	Below Median	Above Median	Yes	No	Yes	No	North	South	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
$\mathbb{1}\{Treat_{s(b)}\} \times$	-0.0178*	-0.0090	-0.0182*	-0.0298**	-0.0245**	0.0055	-0.0263***	-0.0016	-0.0181	-0.0262***	-0.0442	-0.0262***	-0.0302***	-0.0008	-0.0232	-0.0273***	-0.0215**	-0.0277**	-0.0235*	-0.0217**
$\mathbb{1}\{Post_{MoB(b),YoB(b)}\}$	(0.0097)	(0.0094)	(0.0101)	(0.0131)	(0.0107)	(0.0113)	(0.0094)	(0.0082)	(0.0355)	(0.0087)	(0.0325)	(0.0092)	(0.0108)	(0.0100)	(0.0168)	(0.0099)	(0.0091)	(0.0113)	(0.0117)	(0.0106)
	[-8.06]	[-6.85]	[-9.31]	[-17.19]	[-11.34]	[4.68]	[-12.05]	[-1.29]	[-13.47]	[-13.66]	[-21.65]	[-13.72]	[-13.83]	[-0.70]	[-10.64]	[-14.71]	[-12.84]	[-13.38]	[-16.83]	[-10.56]
Adj. R^2	0.141	0.082	0.144	0.135	0.141	0.077	0.136	0.084	0.031	0.126	0.141	0.125	0.137	0.075	0.121	0.127	0.114	0.132	0.088	0.133
N	119,328	104,866	171,037	53,157	127,098	91,783	134,784	89,410	3,357	219,918	11,229	212,809	151,044	73,150	32,509	191,404	102,158	121,791	65,360	158,834

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell in all columns is a separate estimation of Equation (1). Each specification includes an indicator for multiple births, birth order, an indicator for sex of the child, mother fixed-effects, and child's month- and year-of-birth fixed-effects. The dependent variable in all columns is under-five mortality. Refer to Section 3.1 for the definition of the dependent variable. The subpopulation that constitute the estimation sample is denoted in the column header. All subpopulations are constructed using the reported data in the DHS. Wealth distribution in the respective survey round is used to designate a household to be either below or above median of the wealth distribution. Households are designated to be residing in the southern region if they reside in Oyo, Osun, Ekiti, Ondo, Edo, Anambra, Enugu, Ebonyi, Cross River, Akwa Ibom, Abia, Imo, Rivers, Bayelsa, Delta, Lagos, or Ogun. Households are designated to be residing in the urban area if their place of residence is reported to be urban. Women are designated to have had pregnancy complication if they report ever having a pregnancy that miscarried, was aborted, or ended in a stillbirth. Women are designated to consume mass media if they report any mass media (newspaper, magazine, radio, or TV) use at least once a week. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having Free Maternal and Child Health Care Program (FMCHP) rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A6: Heterogeneous Effects: Marital Status

	Currently Married		Ever Divorced		Polygamous Household					
							Islam		Wealth	
	Yes	No	Yes	No	Yes	No	Yes	No	Below Median	Above Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\mathbb{1}\{Treat_{s(b)}\} \times$	-0.02762***	-0.00137	-0.04734**	-0.02185**	-0.02274*	-0.02576***	-0.02324	0.01199	-0.02663*	0.01295
$\mathbb{1}\{Post_{MoB(b),YoB(b)}\}$	(0.00881)	(0.02910)	(0.01974)	(0.00929)	(0.01346)	(0.00890)	(0.01568)	(0.02624)	(0.01464)	(0.01540)
	[-14.21]	[-0.92]	[-19.71]	[-11.84]	[-10.59]	[-14.73]	[-10.33]	[8.01]	[-11.33]	[8.65]
Adj. R^2	0.127	0.120	0.125	0.124	0.138	0.114	0.144	0.109	0.143	0.103
N	205,551	18,642	27,157	195,905	82,801	125,226	61,816	20,602	57,193	25,608

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell in all columns is a separate estimation of Equation (1). Each specification includes an indicator for multiple births, birth order, and indicator for sex of the child, mother fixed-effects, and child's month- and year-of-birth fixed-effects. The dependent variable in all columns is under-five mortality. The subpopulation that constitutes the estimation sample is denoted in the column header. All subpopulations are constructed using the reported data in the DHS. Wealth distribution in the respective survey round is used to designate a household to be either below or above median of the wealth distribution. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having Free Maternal and Child Health Care Program (FMCHP) rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A7: Heterogeneous Effects: Health Facilities

	Health Facilities Per-capita		Health Personnel Per-capita		Beds Per-capita		Pediatric Facilities Per-capita	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}\{Treat_{s(b)}\} \times$	-0.00788	-0.04302***	-0.00728	-0.04052***	-0.00128	-0.05214***	-0.00757	-0.05063***
$\mathbb{1}\{Post_{MoB(b),YoB(b)}\}$	(0.01160)	(0.01056)	(0.01296)	(0.01092)	(0.01133)	(0.01227)	(0.01338)	(0.01411)
	[-4.22]	[-22.11]	[-3.59]	[-28.21]	[-0.72]	[-25.66]	[-3.96]	[-26.28]
Adj. R^2	0.129	0.125	0.133	0.094	0.131	0.122	0.133	0.111
N	106,334	117,860	138,096	86,098	110,475	113,719	127,002	97,192

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell in all columns is a separate estimation of Equation (1). Each specification includes an indicator for multiple births, birth order, and indicator for sex of the child, mother fixed-effects, and child's month- and year-of-birth fixed-effects. The dependent variable in each column is under-five mortality. The subpopulation that constitutes the estimation sample is denoted in the column header. All subpopulations are constructed using the Nigeria Health Facility Registry (HFR) data. Only primary facilities are used to construct subpopulations. Health personnel consist of doctors, nurses, and midwives. A facility is designated to provide pediatric services if they report providing any pediatric care service. Population data at the state-level is derived from the population and housing census conducted in 2006. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having Free Maternal and Child Health Care Program (FMCHP) rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A8: Mechanisms: Demand for Institutional Care

	Health Institution Delivery	Home Delivery	Public Health Institution Delivery	Private Health Institution Delivery	C-section	Health Personnel Assisted Delivery
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{Treat_{s(b)}\} \times$	0.01628**	-0.01604**	0.01169	0.00458	0.00011	0.01499*
$\mathbb{1}\{Post_{MoB(b),YoB(b)}\}$	(0.00783)	(0.00763)	(0.00843)	(0.00487)	(0.00294)	(0.00828)
	[8.16]	[-2.02]	[8.03]	[8.49]	[1.22]	[6.56]
Adj. R^2	0.788	0.793	0.712	0.804	0.565	0.810
N	59,534	59,534	59,534	59,534	59,784	59,292

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell in all columns is a separate estimation of Equation (1). Each specification includes an indicator for multiple births, birth order, and indicator for sex of the child, mother fixed-effects, and child's month- and year-of-birth fixed-effects. The dependent variable is displayed in the column header. A delivery is designated to have occurred at a health institution if it didn't occur either at the respondents' or other persons' home in addition to the reported place of delivery not being . A delivery is designated to have occurred at home if it occurred either at respondents' or other persons' home. A public health institution is either a government hospital, a government health center, a government health post, or other public health institutions. A private health institution is either a private hospital, private clinic, or other private medical institution. Health personnel consists of a doctor, nurse, midwife, and auxiliary midwife. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having Free Maternal and Child Health Care Program (FMCHP) rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A9: Mechanisms: Demand for Postnatal Care

	Any Postnatal Care	Postnatal Care at Institution	Postnatal Care by Health Personnel
	(1)	(2)	(3)
$\mathbb{1}\{Treat_{s(b)}\} \times$	-0.00758	0.03918	0.06640**
$\mathbb{1}\{Post_{MoB(b),YoB(b)}\}$	(0.01419)	(0.03275)	(0.03062)
	[-6.45]	[6.85]	[10.37]
Adj. R^2	0.187	0.215	0.213
N	31,852	7,192	7,182

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell in all columns is a separate estimation of Equation (2). Each specification includes indicators for the mother's year-of-birth, birth order, an indicator for multiple births, indicators for religion, number of household members, an indicator for the sex of child, the mother's residence state fixed-effects, and the child's month- and year-of-birth fixed-effects. The dependent variable is displayed in the column header. The child is designated to have received postnatal care if they were checked on within two months of their birth. The child is designated to have received postnatal care at an institution if they receive postnatal care at any public or private medical facility. The child is designated to have received postnatal care by health personnel if they are checked on by a doctor, nurse, midwife, or auxiliary midwife. The variables in the last two columns are defined only for those children who receive any postnatal care within two months of their birth. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having FMCHP rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the Free Maternal and Child Health Care Program (FMCHP) is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A10: Mechanisms: Pre-delivery Interaction with Medical Providers

	Received Some Antenatal Care	Institutional Antenatal Care	Antenatal Care by a Health Personnel	Blood Pressure Measured During Pregnancy	Urine Sample Given During Pregnancy	Blood Sample Given During Pregnancy	Tetanus Injection Received During Pregnancy	Iron Supplement Received During Pregnancy	Pre-delivery Interaction Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\mathbb{1}\{Treat_{s(b)}\} \times$	0.05527**	0.03934***	0.08459***	0.03608*	0.03255*	0.03430	0.05331**	0.06431***	0.15461***
$\mathbb{1}\{Post_{MoB(b),YoB(b)}\}$	(0.02642)	(0.01137)	(0.02253)	(0.01969)	(0.01656)	(0.02118)	(0.02121)	(0.02272)	(0.04970)
	[11.27]	[4.41]	[20.42]	[4.35]	[4.64]	[4.90]	[12.80]	[14.76]	[53.01]
Adj. R^2	0.288	0.076	0.269	0.060	0.087	0.077	0.281	0.229	0.289
N	38,052	23,995	37,880	24,303	24,283	24,280	37,877	37,649	37,998

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell in all columns is a separate estimation of Equation (2). Each specification includes indicators for the mother's year-of-birth, birth order, an indicator for multiple births, indicators for religion, number of household members, an indicator for the sex of child, the mother's residence state fixed-effects, and the child's month- and year-of-birth fixed-effects. The dependent variable is displayed in the column header. The dependent variable in column (2) and column (3) is defined only for those respondents who report receiving some antenatal care. Respondent is designated as having blood pressure checked during pregnancy if she reports blood pressure being measured at least once during her antenatal care visit. Respondent is designated as having her urine sample drawn during pregnancy if she reports giving a urine sample at least once during her antenatal care visit. Respondent is designated as having her blood sample drawn if she reports giving a blood sample at least once during her antenatal care visit. The variables in column (5) to column (7) are defined only for those respondents who received any antenatal care. Respondent is designated as having received a tetanus injection during the pregnancy if she reports having received a tetanus injection during pregnancy. Respondent is designated as having received an iron supplement if she reports buying or receiving an iron tablet or syrup during pregnancy. The dependent variable in the last column is a standardized weighted index of multiple indicator variables using a GLS weighting procedure as described in Anderson (2008). The indicator variables used for the construction of this index are reported in the first five columns. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having Free Maternal and Child Health Care Program (FMCHP) rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A11: Mechanisms: Information

	Knows Ovulatory Cycle	Knows at least One Contraception Method	Not Using Contraception Due to Lack of Information	Heard of AIDS	Have Misconceptions About AIDS	Information Index
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{Treat_{s(b)}\} \times$	0.07282***	-0.01856	-0.04805**	0.03325***	-0.00127	0.04499
$\mathbb{1}\{Post_{MoB(b),YoB(b)}\}$	(0.02244)	(0.01409)	(0.01895)	(0.01239)	(0.00215)	(0.04336)
	[11.03]	[-10.70]	[-32.11]	[4.00]	[-17.43]	[35.12]
Adj. R^2	0.216	0.105	0.074	0.118	0.008	0.152
N	51,065	50,743	17,592	51,024	44,659	51,065

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell in all columns is a separate estimation of Equation (2). Each specification includes indicators for the mother's year-of-birth, indicators for religion, number of household members, the mother's residence state fixed-effects, and the child's month- and year-of-birth fixed-effects. The dependent variable is displayed in the column header. Respondent is designated to know the contraception methods if they report that they know at least one contraception method. Respondent is designated to know the ovulatory cycle if they respond that a woman is more likely to become pregnant if the sexual relations are halfway between two menstrual periods. The dependent variable in the third column is defined only for those respondents who report an intention to not have another child or not have another child soon but do not use a contraception method. The respondent is designated to have a lack of information as the reason for not using contraception if they report not knowing any contraception method or not knowing a source for contraception. Respondent is designated to have misconceptions about AIDS if their response is wrong on either of the following questions: reduce risk of getting AIDS by not having sex at all, reduce chances of AIDS by always using condoms during sex, reduce chance of AIDS by having one sex partner with no other partner, can get AIDS from mosquito bites, can get AIDS by sharing food with person who has AIDS, can a healthy person have AIDS, AIDS can be transmitted during pregnancy, AIDS can be transmitted during delivery, AIDS can br transmitted by breastfeeding, can get AIDS by witchcraft or supernatural means. The dependent variable in the last column is a standardized weighted index of multiple indicator variables using a GLS weighting procedure as described in Anderson (2008). The indicator variables used for the construction of this index are reported in the first five columns. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having Free Maternal and Child Health Care Program (FMCHP) rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth. The data comes from the DHS. Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A12: Community Health Personnel Provided Antenatal Care by Kinship Structure of Ethnic Group

	Baseline	Non-matrilineal Kinship Ethnic Group	Matrilineal Kinship Ethnic Group
	(1)	(2)	(3)
$\mathbb{1}\left\{Treat_{s(b)}\right\} \times$	0.01995*	0.01970*	-0.12320
$\mathbb{1}\left\{Post_{MoB(b),YoB(b)}\right\}$	(0.01041)	(0.00999)	(0.11832)
	[50.94]	[51.37]	[-92.40]
Adj. R^2	0.078	0.074	0.132
N	37,880	37,400	362

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. The treated group comprises all states where the Free Maternal and Child Health Care Program (FMCHP) is rolled out. Each cell in all columns is a separate estimation of Equation (2). Each specification in all columns includes indicators for mother's year-of-birth, birth order, an indicator for multiple birth, indicators for religion, number of household members, an indicator for sex of the child, mother's residence state fixed-effects and the child's month- and year-of-birth fixed-effects. The dependent variable in all columns is an indicator variable for whether the antenatal care is provided by the community health worker. This variable is defined only for those respondents who report receiving some antenatal care during the pregnancy. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having FMCHP rollout and an indicator for the birth to be after FMCHP is rolled out. To classify women as belonging to an ethnic group that follows matrilineal kinship structure, we use the reported ethnicity in the Demographic and Health Survey (DHS). Ethnic groups following kinship structure are derived from Murdock (1967). All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth. The data comes from the DHS. Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A13: Heterogeneity by Wealth for States with Above Median Private Health Facilities Per-capita: Private Health Institution Delivery

	Wealth	
	Below Median	Above Median
	(1)	(2)
$\mathbb{1}\left\{Treat_{s(b)}\right\} \times$	-0.01105	0.00494
$\mathbb{1}\left\{Post_{MoB(b),YoB(b)}\right\}$	(0.04127)	(0.04800)
	[-2.62]	[0.84]
Adj. R^2	0.733	0.764
N	10,771	12,291

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell in all columns is a separate estimation of Equation (1). Each specification includes an indicator for multiple births, birth order, an indicator for sex of the child, mother fixed-effects, and child's month- and year-of-birth fixed-effects. The dependent variable in each column is whether the place of delivery is a private health institution. The subpopulation that constitute the estimation sample is denoted in the column header. All subpopulations are constructed using the reported data in the DHS. The estimation sample is restricted to states that have above median number of private primary health facilities per-capita. The count of private primary health facilities is for those facilities that begin operation before 2008. This measure is constructed at the state-level. A primary health facility is designated as privately owned if it's reported ownership type is for profit in the health facility registry. Wealth distribution in the respective survey round is used to designate a household to be either below or above median of the wealth distribution. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having Free Maternal and Child Health Care Program (FMCHP) rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A14: Mechanisms: Vaccination

	BCG	Measles	DPT	Polio	Vaccination Index
	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}\{Treat_{s(b)}\} \times$	0.00860	0.05007***	0.02411**	0.02398***	0.07062***
$\mathbb{1}\{Post_{MoB(b),YoB(b)}\}$	(0.01042)	(0.01312)	(0.01196)	(0.00828)	(0.01870)
	[2.52]	[16.99]	[7.14]	[4.14]	[30.23]
Adj. R^2	0.771	0.627	0.745	0.709	0.761
N	53,745	53,455	53,640	53,743	53,989

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell in all columns is a separate estimation of Equation (1). Each specification includes an indicator for multiple births, birth order, and indicator for sex of the child, mother fixed-effects, and child's month- and year-of-birth fixed-effects. The dependent variable is displayed in the column header. The child is designated to have received the BCG vaccine if they receive the first BCG vaccine. Similarly, the child is designated to have received DPT and Polio vaccines if they receive the first dose of these vaccines. The child is designated to have received the measles vaccine if they report receiving the measles vaccine. The dependent variable in the last column is a standardized weighted index of multiple indicator variables using a GLS weighting procedure as described in Anderson (2008). The indicator variables used for the construction of this index are reported in the first four columns. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having FMCHP rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the Free Maternal and Child Health Care Program (FMCHP) is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.

Table A15: Mechanisms: Acute Care

	Diarrhea Last Two Weeks	Fever Last Two Weeks	Cough Last Two Weeks	Diarrhea Any Treament	Fever/Cough Any Treatment	Diarrhea Treatment at a Health Institution	Fever/Cough Treatment at a Health Institution	Either Treatment at a Health Institution	Any Drug Taken For Diarrhea	Any Drug Taken For Fever/Cough	Oral Rehydration For Diarrhea
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$\mathbb{1}\{Treat_{s(b)}\} \times$	0.02394*	0.01189	0.01482	-0.01245	-0.00574	-0.03018	0.01979	0.01513	-0.01095	0.02607	0.03311
$\mathbb{1}\{Post_{MoB(b),YoB(b)}\}$	(0.01431)	(0.01130)	(0.01100)	(0.05301)	(0.02791)	(0.04490)	(0.02990)	(0.02574)	(0.06850)	(0.02158)	(0.02098)
	[19.05]	[7.54]	[15.00]	[-1.81]	[-0.78]	[-7.44]	[4.69]	[3.56]	[-2.44]	[3.05]	[11.93]
Adj. R^2	0.289	0.378	0.433	0.882	0.828	0.896	0.851	0.789	0.839	0.783	0.851
N	53,338	53,286	53,211	5,570	9,302	5,570	9,302	12,366	5,551	9,234	5,545

Notes: Heteroskedasticity robust standard errors clustered by the local government area in parentheses. (* p<.10 ** p<.05 *** p<.01). The ratio of point estimate in each cell and pre-treatment mean for the treated group is in square brackets. Each cell in all columns is a separate estimation of Equation (1). Each specification includes an indicator for multiple births, birth order, and indicator for sex of the child, mother fixed-effects, and child's month- and year-of-birth fixed-effects. The dependent variable is displayed in the column header. In the first three columns, the child is designated to have the ailment if they have that ailment in the two weeks preceding the survey date. The child is designated to have received treatment for diarrhea and fever or cough in the last two weeks if they sought advice or treatment for the illness from any source. The child is designated to have received treatment for diarrhea and fever or cough in the last two weeks at a health institution if they receive treatment at a government hospital, government health center, government health post, private hospital or clinic, private pharmacy, or private doctor. In column (8), the indicator variable takes a value of one if the indicator variable in either column (6) or column (7) takes a value of one. In column (9) and column (10), the indicator variables take a value of one if the respondent reports that the youngest child born in the last five years took any drug as part of treatment for diarrhea and fever or cough in the last two weeks, respectively. This variable is only defined for those children who had the illness in the last two weeks. In the last column, the child is designated to have received oral rehydration as part of diarrhea treatment if they report receiving it for diarrhea in the last two weeks from the survey date. This variable is defined only for those children who have had diarrhea in the last two weeks from the survey date. The Independent variable in each cell is the interaction of an indicator variable for mothers' residence state ever having Free Maternal and Child Health Care Program (FMCHP) rollout and an indicator for the birth to be after FMCHP is rolled out. All births after 2015 are dropped from the analytical sample. Exposure to the FMCHP is determined based on the child's month- and year-of-birth. The data comes from the Demographic and Health Survey (DHS). Waves three to five of DHS are used. The respondents in these waves were interviewed between 2003 and 2018.