IMPACT OF CONDITIONAL CASH TRANSFERS ON ROUTINE CHILDHOOD IMMUNIZATIONS IN NORTH WEST NIGERIA

FINAL REPORT

November 18, 2020

IDinsight
DATA. DECISIONS. DEVELOPMENT.

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Acronyms

BCG	*Bacillus Calmette–Guérin* (a tuberculosis vaccine)
CCT	Conditional cash transfer
CEA	Cost-effectiveness analysis
CHC	Child health card
CIR	Clinic immunization record
CSS	Compact segment sampling
DHS	Demographic and Health Survey
fIPV	Fractional dose of the inactivated polio vaccine
FSDR	Free step-down resampling
HH	Household
HIB	Haemophilus Influenza Type B
HIV	Human immunodeficiency virus
ICC	Intracluster correlation coefficient
IPV	Inactivated polio vaccine
LGA	Local government area
MenA	Meningitis A vaccine
NGO	Non-governmental organization
NI-ABAE	New Incentives - All Babies Are Equal Initiative
OLS	Ordinary least squares
PCV	Pneumococcal conjugate vaccine
Penta	Pentavalent vaccine (diptheria, tetanus, pertussis, hepatitis B, HIB)
RCT	Randomized controlled trial
RI	Routine immunization

Definition of Terms

Any Measles vaccine – Child received at least one dose of the Measles vaccine

Any PCV vaccine – Child received at least one dose of PCV vaccine

Any Penta vaccine – Child received at least one dose of Penta vaccine

BCG vaccine – Child received the BCG vaccine

BCG scar – Child had a scar on her/his arm from receiving the BCG vaccine

Ever vaccinated – Child received at least one injectable vaccine

Fully immunized (loose) – Child received BCG, Penta 1, and Measles 1 vaccines

Fully immunized (strict) – Child received BCG, three doses of Penta, and Measles 1 vaccines

Total (no PCV) - Total number of vaccines received by child (count includes BCG, Penta 1-3 vaccines, and Measles 1 vaccine; ranges from 0 to 5)

Total (with PCV) - Total number of vaccines received by child (count includes BCG, Penta 1-3 vaccines, PCV 1-3 vaccines, and Measles 1 vaccine; ranges from 0 to 8)

Executive Summary

Nigeria has among the lowest vaccination coverage levels in the world (UNICEF 2017), contributing to high child mortality – 40% of under-five deaths in Nigeria are from diseases that are preventable through vaccination (NRISP 2013). North West Nigeria has the lowest vaccination coverage of any region in Nigeria.

New Incentives,[1] an international non-governmental organization, aims to boost immunization by offering cash incentives to caregivers who have their child vaccinated at a program clinic. This effort is called the New Incentives - All Babies Are Equal Initiative's (NI-ABAE) Conditional Cash Transfers (CCTs) for Routine Immunizations (RI) Program.

This study is an impact evaluation of the NI-ABAE CCTs for RI Program in Katsina, Zamfara, and Jigawa States in North West Nigeria funded by Open Philanthropy[2] at the recommendation of GiveWell. GiveWell will use the evaluation results to inform their decision of whether to designate New Incentives a 2020 Top Charity and recommend funding for NI-ABAE to scale up their program.[3]

Evaluation Questions and Methods

This study sought to estimate the effect of NI-ABAE's CCTs for RI Program on the proportion of 12 to 16-month old children who received the BCG vaccine, the Penta 1 vaccine, and the Measles vaccine. Secondary outcomes included other doses of the Penta vaccine, full immunization, receipt of any injectable vaccine, presence of a BCG vaccine scar, vaccine timeliness, and vaccination counts in clinics' administrative records. We measured most outcomes using caregiver reports from a household survey, during which enumerators also checked for BCG vaccine scars and recorded data from vaccination records kept in the home. In a separate survey, we also collected vaccination counts from administrative records kept at health facilities.

The evaluation consisted of a two-arm clustered randomized controlled trial. We worked with NI-ABAE to identify clinic catchments in the three evaluation states that met its operational criteria. We then randomly selected our sample of 167 clinics from among these clinics.[4] Stratifying on the proportion of children (12-16 months) who had ever received an injectable vaccine (as measured by the baseline survey), remoteness, number of routine immunization staff, state, and security, we randomly assigned 84 clinics to the treatment arm and 83 clinics to the control arm. NI-ABAE began ramping up CCT program operations in assigned treatment clinics in January 2018. The program was fully operational by July 2018. The RCT window ran from July 2018 to October 2019.

[1] Background on New Incentives: https://www.newincentives.org/

[2] https://www.openphilanthropy.org/

[3] Background on Open Philanthropy: https://www.openphilanthropy.org/. Background on GiveWell: https://www.givewell.org/. Background on Top Charities: https://www.givewell.org/charities/top-charities

[4] We also ensured a minimum distance of 17km between pairs of study clinics to mitigate the risk of spillover and contamination in which the program's presence in treatment areas affects outcomes in control areas. In a handful of cases, it was not possible to maintain this buffer, but we still found no substantial evidence of contamination or spillovers.

The endline survey took place from November 2019 to February 2020. We used compact segment sampling to select approximately 25% of all households in every settlement in the catchment areas of treatment and control clinics.[5] We listed all households in the selected segments to identify children between 12 and 16 months of age. We then invited all caregivers of these children to complete a survey on immunization outcomes. We also collected data about immunization services at evaluation clinics and verified individual immunization status in clinic records. We compared outcomes (from various data sources) in these areas using both differences in means and multivariate regressions.

Results and Recommendations – Impact

NI-ABAE's CCTs for RI Program had a large, consistent positive impact on vaccination coverage. According to both primary and secondary outcomes, NI-ABAE's CCTs for RI Program had a substantial positive impact on vaccination coverage. Among children in treatment clinics compared to those in control clinics, self-reported[6] vaccination coverage was 16 percentage points higher for the BCG vaccine [95% CI: 12, 21], 21 percentage points higher for the Penta 1 vaccine [95% CI: 16, 26], and 14 percentage points higher for the Measles vaccine [95% CI: 10, 18]. All of these increases were statistically significant. Children in the catchment areas of treatment clinics were also statistically significantly more likely to have received Penta 2 and 3 vaccines, to be fully immunized, and to have a BCG vaccine scar compared to children in the catchment areas of control clinics. A higher percentage of children in treatment areas had received any injectable vaccine compared to children in control areas, though this increase was not statistically significant using our main definition. NI-ABAE improved timeliness of some vaccinations, with a large positive effect on timeliness of Measles vaccination.[7]

Impact estimates were positive across data sources. BCG vaccine scars and all administrative data sources showed large, positive impact – similar to self-reports. This suggests that there was a true and meaningful impact on vaccination not explainable by measurement error.

Plausible assumptions about data quality usually implied upward adjustments. Therefore, based on self-report error, we recommend GiveWell adjust self-report impact estimates upward for all vaccines. Making plausible assumptions about self-report measurement error and using them to adjust coverage and impact always generated positive impact. Most plausible adjustments produced impact estimates greater than unadjusted self-reports impact estimates, especially if we assumed measurement error was similar in treatment and control. Using BCG vaccine scars – also adjusted for

[5] We divided all settlements in all catchments into segments and randomly selected one quarter of the segments in each settlement to ensure a representative sample of land area. In some exceptional settlements (less than 10%), we surveyed 100% of land area because they were too small to divide. In these cases, we used weights to ensure they were properly represented in our analysis.

[6] Self-reported vaccination coverage is based on caregivers' reports of their child's vaccination status.

[7] The program caused a decrease in the timeliness of BCG vaccinations – these differences were not significant at the 5% level but were notable, nonetheless. However, we suspect they are driven largely by the measure we used, which included only children who received the vaccination. It is likely that many of the infants who receive the BCG vaccination late are those who do not receive the vaccination at all in the absence of the program, perhaps because they are not delivered at health facilities. There was no positive impact on timeliness of the first dose of Penta vaccine when defined as receipt within one month of the recommended age. There was a positive impact, however, when defined as receipt within two weeks of the recommended age.

measurement quality – yielded a range for impact on BCG vaccination that was entirely and meaningfully higher than unadjusted self-report impact. While it is difficult to extrapolate measurement error from BCG vaccine to other vaccines, we find it likely that impact is similar across vaccines.

We recommend that GiveWell apply a somewhat larger upward adjustment to Measles vaccine than to the other vaccines. For Measles vaccine, our impact estimate was higher when restricted to vaccinations delivered via RI activities (excluding those delivered via periodic campaigns). Given that large Measles vaccine campaigns appear to occur infrequently (but one did occur just before endline), it is probable that the program would have a higher impact on Measles vaccine in normal, non-post-campaign periods.

Results and Recommendations – Control Coverage

Based on self-report error, we recommend GiveWell adjust control coverage estimates downward for all vaccines.[8] Self-reported control coverage was 63% (95% CI: 57%, 69%) for BCG vaccine, 54% (95% CI: 48%, 61%) for Penta 1 vaccine, and 59% (95% CI: 54%, 64%) for Measles vaccine. Adjusting control coverage for self-report sensitivity and specificity produced ranges on either side of these unadjusted self-report estimates. Control coverage according to BCG vaccine scars (adjusted for scarring probability and scar-recording probability), however, fell well below the self-reports level.[9] This suggests that true BCG vaccine coverage in control was likely below that measured by self-reports. We recommend GiveWell adjust control coverage down for BCG vaccine based on self-report error. It is difficult to extrapolate this finding to other vaccines, but we would recommend that GiveWell also assume true coverage for Penta 1 vaccine and Measles vaccine in control is lower than that found via unadjusted self-reports.

We recommend that GiveWell assume RI coverage for Measles vaccine is roughly 11 percentage points lower than unadjusted Measles vaccine coverage.

Coverage in control areas at endline was higher than at baseline even after considering the effect of including Jigawa State and of changes in measurement methods between the two rounds. Campaigns reported during the study window targeted Measles vaccine but not BCG or Penta vaccines. For Measles vaccine, we found that control coverage included a non-trivial proportion of campaign-vaccinators to whom the program might not pay incentives. If GiveWell assumes that NI-ABAE pays incentives for less than 100% of vaccinations delivered to eligible children via campaigns, then they should adjust Measles vaccine coverage in control downward before using it to estimate the number of incentives paid.

[8] Our recommendation focuses on control coverage (rather than treatment coverage) for the following reason: For GiveWell's analysis, the most important parameters are control coverage and the impact estimate. To calculate treatment coverage, GiveWell adds the impact estimate to control coverage. Consequently, if GiveWell decides to revise control coverage downward, then treatment coverage will also be revised downward automatically.

[9] We calculated BCG vaccine coverage consistent with a range of scarring and scar-finding probabilities that we believed were plausible based on the literature and our observations in the field. These ranges never produced BCG vaccine coverage in control higher than 50%.

Introduction

North West Nigeria has some of the lowest vaccination rates in the world. According to UNICEF's immunization coverage estimates for 2019, Nigeria had the tenth lowest estimated coverage (57%) of the third dose of Diphtheria, Pertussis, and Tetanus vaccine (DTP3), a common indicator of how effectively countries are providing routine childhood immunization services (UNICEF 2019). These, low immunization rates are a significant contributor to Nigeria's high under-five mortality ratio (104 deaths per 1,000 live births, UNICEF 2017)[10] – 40% of under-five deaths in Nigeria are from diseases that are preventable through vaccination (NRISP 2013). Due to its even lower immunization rates, the region of North West Nigeria is vulnerable to frequent Measles outbreaks (NCDC 2016).

In recent years, the donor community has invested substantially in improving supply-side infrastructure for routine immunization (NRISP 2013), but coverage rates remain low – the global immunization coverage target is 90% (WHO 2017).

There are several studies that find that incentives can have a meaningful impact on immunization coverage, especially in low baseline coverage settings.[11] New Incentives,[12] an international non-governmental organization (NGO), aims to boost demand for immunization by offering cash incentives to caregivers who vaccinate their child at a program clinic.[13] This effort is called the New Incentives - All Babies Are Equal Initiative's (NI-ABAE) Conditional Cash Transfers (CCTs) for Routine Immunizations (RI) Program.

NI-ABAE staff accompany local health workers conducting routine immunization activities at clinics and in communities. After vaccinating a child, health workers direct caregivers eligible for the incentive to the NI-ABAE staff for enrollment and receipt of the cash payment.[14] Figure 1 outlines New Incentives' theory of change.

[10] Nigeria has the 8th highest rate of under-five mortality in the world (UNICEF 2017).

[11] In addition to the research discussed below, see Loevinsohn 1986, Chandir 2010, and Gibson 2017. There is also a broader literature base on conditional cash transfers to encourage health intervention uptake summarized by Lagarde 2007.

[12] New Incentives is an international NGO that uses conditional cash transfers to achieve development goals. Since 2014, New Incentives has provided hundreds of thousands of conditional cash transfers to Nigerian mothers. At first, these incentivized giving birth in health facilities with the goal of limiting mother-to-child transmission of human immunodeficiency virus (HIV). After re-evaluating which clinic healthcare service would be most cost-effective to incentivize, New Incentives shifted focus to routine childhood immunization in 2016. More details on New Incentives can be found on New Incentive's website (https://www.newincentives.org/).

[13] More details on New Incentives – All Babies Are Equal Initiative can be found in IDinsight's Baseline Report (https://files.givewell.org/files/DWDA%202009/NewIncentives/New_Incentives_Baseline_Report_Final_2019_06_03.pdf). A history of GiveWell's support to New Incentives is posted on GiveWell's website (https://www.givewell.org/charities/new-incentives/all-content). More details on the Nigerian routine immunization system, especially the structure of an average immunization visit day, can be found in Annex 1 of IDinsight's Baseline Report and in IDinsight's February 2017 site visit report (https://www.givewell.org/research/site-visits/february-2017).

[14] To be eligible for first-time enrollment, infants must be able to receive BCG vaccine and be from a program (treatment) clinic's catchment area.

Figure 1. New Incentives' Theory of Change for Evaluation Design

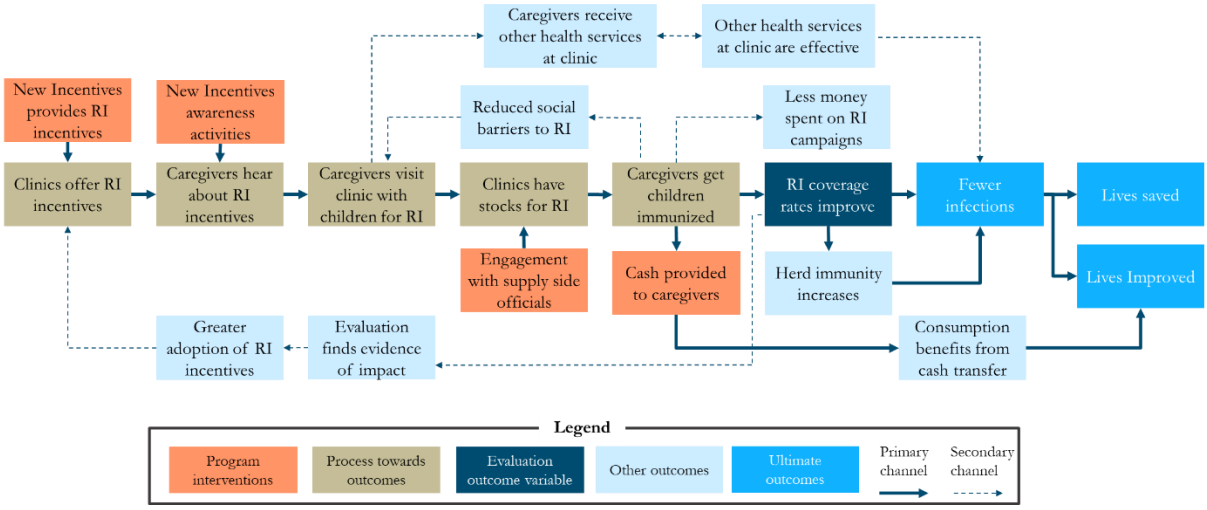

Table 1 lists the incentives offered to caregivers by vaccine. The incentive amount for the Measles vaccine is higher for two reasons. First, NI-ABAE believes that caregivers will need more inducement to return for the Measles vaccine after the longer time interval following the third Penta vaccination. Second, evidence suggests that there is greater potential health impact from the Measles vaccine.[15] Children do not need to have received the previous vaccine in the schedule to be eligible.[16]

Table 1. Description & Timing of Vaccinations

Immunization	Description	Doses	Timing (age) for doses	Incentive amount[1]
BCG vaccine	Vaccine against tuberculosis	1	At birth	₦[2]500
Penta vaccine	Five vaccines against: diphtheria, tetanus, pertussis, Hepatitis B and Haemophilus Influenza Type B (HIB)	3	At 6 weeks, 10 weeks, and 14 weeks	₦500 for each dose, when PCV is also received
PCV vaccine	Vaccine against pneumococcal bacteria	3	Same as for Penta vaccine	Same as for Penta vaccine
Measles vaccine[3]	Vaccine against Measles, mumps, rubella, and varicella	1	9 months	₦2000

[1] If an infant is eligible for multiple vaccines on a visit, the caregiver will only receive the incentive for the latest vaccine.

[2] Nigerian Naira

[3] At the time of the study, the Measles 2 vaccine had not yet been introduced in the Nigerian Routine Immunization schedule. Therefore, the program incentivized only one dose of the Measles vaccine.

[15] See IHME data for by-cause mortality: http://ghdx.healthdata.org/gbd-results-tool?params=gbd-api-2017-permalink/d1a61ecb86c01b9ac1499bd2b718d189

[16] Infants on a catch-up schedule - e.g., an unvaccinated infant comes to a clinic at twelve-months and receives BCG vaccine, PCV 1 vaccine, and Penta 1 vaccine - receive one incentive associated with the most recent vaccination visit. In this case, a caregiver would receive ₦500.

At the time of endline data collection (December 2019), NI-ABAE had operated in 98 health facilities across Zamfara, Katsina, and Jigawa States. They had dispersed 708,000 cash transfers to 194,000 enrolled children that totaled to 542,000,000 Nigerian Naira.[17]

GiveWell, a nonprofit dedicated to finding outstanding giving opportunities, identified NI-ABAE as a potential top charity.[18] They identify evidence-backed, underfunded "top charities" by assessing them according to four criteria: 1) evidence of effectiveness, 2) cost-effectiveness, 3) transparency, and 4) room for more funding. GiveWell engaged IDinsight[19] to conduct an independent impact evaluation to estimate the impact of the NI-ABAE CCTs for RI Program on coverage of vaccines included in Nigeria's routine immunization schedule[20] in three states[21] in North West Nigeria. GiveWell plans to translate the coverage increases for each vaccination into estimates of lives saved and illness prevented to compare the cost-effectiveness of the NI-ABAE CCTs for RI Program to other programs GiveWell recommends. This evaluation will therefore directly influence GiveWell's decision to recommend New Incentives as a top charity.[22] This report presents the results from this cluster randomized evaluation.

Evaluation Questions

The goal of the evaluation was to measure the causal effect of NI-ABAE's CCTs for RI Program on coverage for routine childhood vaccines.[23]

Primary research questions were:[24]

1. What is the effect of NI-ABAE's CCTs for RI Program on the proportion of 12 to 16-month olds in communities served by a study clinic who received BCG vaccine?

[17] 1,393,987 USD (using xe.com 2 July 2020 exchange rate)
[18] GiveWell is closely associated with Open Philanthropy which funds much of GiveWell's experimental and research work. GiveWell and Open Philanthropy identify new potential top charities and invest in their development and in further evaluation of their effectiveness. IDinsight's evaluation of the NI-ABAE CCTs for RI Program is one such effort and falls under a broader learning partnership between GiveWell and IDinsight.
[19] IDinsight is a client-service organization that helps social sector actors generate and use evidence to inform decisions. Our team has coordinated over 80 impact evaluations in Africa and Asia using experimental and quasi-experimental methodologies and works with a wide range of government, not-for-profit, and for-profit organizations.
[20] At the beginning of the study period, this included Bacillus Calmette–Guérin (BCG) vaccine for Tuberculosis; Hepatitis B 0; Oral Polio Vaccine (OPV) 0-3; Pentavalent Vaccine (Penta) 1-3; Pneumococcal Conjugate Vaccine (PCV) 1-3; Measles 1 vaccine; Yellow Fever vaccine; Vitamin A; and Injectable Polio Vaccine (IPV). The Pentavalent Vaccine protects against diphtheria, tetanus, pertussis, Hepatitis B and Haemophilus Influenza Type B (HIB). Meningitis A Conjugate Vaccine was added to the schedule part-way through the study period. Diptheria, Pertusis, Tetanus (DPT) vaccine 1-3 were previously part of the schedule but have not been administered in Nigeria since 2017.
[21] This evaluation was conducted in Katsina, Zamfara, and Jigawa States.
[22] Top charity status could lead to funding for NI-ABAE in excess of $20 million.
[23] Care was taken throughout the evaluation design to ensure that NI-ABAE's implementation during the RCT reflected their anticipated implementation at scale. Thus, we expect findings from this study to predict the impact of the NI-ABAE CCTs for RI Program at scale with reasonable accuracy.
[24] The registered pre-analysis plan framed the research questions as the effect of NI-ABAE's CCTs for RI Program on the probability that a 12 to 16-month old. The wording has been updated here to more accurately reflect that the outcome measure was a proportion, not a probability.

2. What is the effect of NI-ABAE's CCTs for RI Program on the proportion of 12 to 16-month olds in communities served by a study clinic who received Penta 1[25] vaccine?
3. What is the effect of NI-ABAE's CCTs for RI Program on the proportion of 12 to 16-month olds in communities served by a study clinic who received Measles 1 vaccine?

Secondary research questions were:

4. What is the effect of NI-ABAE's CCTs for RI Program on the proportion of 12 to 16-month olds in communities served by a study clinic who are fully immunized (loose and strict)?[26]
5. What is the effect of NI-ABAE's CCTs for RI Program on the timeliness of vaccination among 12 to 16-month olds in communities served by a study clinic?
6. What is the effect of NI-ABAE's CCTs for RI Program on the average number of vaccines received per 12 to 16-month old child in communities served by a study clinic?
7. What is the effect of NI-ABAE's CCTs for RI Program on the proportion of 12 to 16-month olds in communities served by a study clinic that received at least one injectable vaccine?
8. What is the effect of NI-ABAE's CCTs for RI Program on the proportion of 12 to 16-month olds in communities served by a study clinic who received at least one dose of PCV[27]?
9. What is the effect of NI-ABAE's CCTs for RI Program on the change over time in the volume of BCG, Penta 1, Penta 2, Penta 3, and Measles 1 vaccinations recorded in clinic administrative records between treatment and control? [28]
10. What is the effect of NI-ABAE's CCTs for RI Program on the proportion of 12 to 16-month olds in communities served by a study clinic who received non-incentivized immunizations (OPV1-3, Yellow Fever, and IPV) and vitamin A?
11. What is the effect of NI-ABAE's CCTs for RI Program on vaccine knowledge and attitudes of caregivers in communities served by a study clinic?
12. What is the effect of NI-ABAE's CCTs for RI Program on the frequency of vaccine stockouts in study clinics? [29]

[25] The wording from the pre-analysis plan was "at least one dose of Penta". This has been updated for clarity.
[26] Loose: Child received BCG, Penta 1, and Measles 1 vaccines; Strict: Child received BCG, Penta 1-3, and Measles 1 vaccines. While the "strict" definition is closest to the typical definition of full immunization in Nigeria (the Nigerian EPI's definition includes OPV), we included the "loose" definition of full immunization because we were concerned about the accuracy of self-reported Penta 2 and 3.
[27] PCV, while not a primary outcome, is part of NI-ABAE's incentives conditions and is generally given at the same visits as Penta vaccine. It is not as prominent in the evaluation in large part because we expected it to be both highly correlated with and easily confused with Penta vaccine, making self-reported coverage data for it somewhat more difficult to interpret.
[28] IDinsight collected these records as part of midline data collection in March/April 2019. We found a robust positive impact across vaccinations but these results were not for the entire evaluation window, so we also included this at endline. These administrative records are imperfect, and the program itself may cause them to improve, leading to differential data quality in treatment and control. Therefore, they should not be used to provide a quantitative impact estimate. However, they remain an important alternative source that we expect to qualitatively triangulate with self-reported vaccination data.
[29] Research questions 10, 11 and 12 were not included in our pre-analysis plan but were added because they were of value to immunization and government stakeholders in Nigeria.

Methods

IDinsight conducted a cluster randomized controlled trial (RCT) to assess the impact of NI-ABAE's CCTs for RI Program. In 2017, we randomized 167 clinics into a control group (83 clinics) and a treatment group (84 clinics) after conducting a baseline survey.[30] NI-ABAE started its intervention in early 2018, gradually ramping up operations in all treatment clinics. In June 2018, GiveWell, NI-ABAE, and IDinsight all assessed that NI-ABAE had reached steady-state operations in all clinics and the "RCT window" began in July 2018. In December 2019, after NI-ABAE had operated at every clinic for at least 17 months,[31] IDinsight conducted an endline household survey to assess the vaccination status of children in the catchments of study clinics.

This section summarizes the study's methodology to provide context to the results reported in later sections. Further details on methodology are in the pre-registered pre-analysis plan[32] and the Baseline Report, while information on the rationale for key design decisions is in various evaluation design documents published on GiveWell's website for the evaluation.[33]

Setting and Selection of Study Clinics

The evaluation took place across three states in North West Nigeria: Katsina State, Zamfara State, and Jigawa State. NI-ABAE initially chose Katsina and Zamfara States to implement its intervention after an extensive state selection process that looked at factors such as the presence of other incentive programs, state responsiveness to research, and vaccination coverage. Jigawa State – a state that was similar to Katsina State and Zamfara State along these dimensions – was added after the evaluation baseline was complete to increase the evaluation sample size and mitigate against the risk of any state-wide effects that might have altered our ability to estimate NI-ABAE's CCTs for RI Program at-scale impact at endline (e.g., state-wide strike of healthcare workers, security challenges, widespread vaccine stock-outs, etc.).

NI-ABAE screened clinics throughout the three states to identify a potential sample of clinics that met their operational requirements. From this set of clinics, IDinsight drew a sample of clinics for inclusion into the study.

This sampling process was relatively complex, as it had two goals: 1) to select a sample of clinics that were representative of NI-ABAE's expected operations at scale and 2) to ensure that a minimum of 17 kilometers separated any two study clinics, in order to minimize the risk of caregivers from control

[30] Each health clinic in Nigeria has a 'catchment' area, which contains the population that the clinic is officially designated to serve. Estimated catchment population sizes in the study area vary from fewer than 2,200 people to over 40,000 people. These clinic catchments constituted the clusters in our evaluation.

[31] Since we planned to survey 12 to 16-month olds at endline, we needed at least 16 months between the time the last program clinic was fully operational and the first day of surveying.

[32] The Pre-Analysis Plan is registered with Registry for Development Impact Evaluations (https://ridie.3ieimpact.org/index.php?r=search/detailView&id=767); ISRCTN (https://www.isrctn.com/ISRCTN10808433); and clinicaltrials.gov (https://clinicaltrials.gov/ct2/show/NCT03870061?term=conditional+cash&cond=immunization&draw=2&rank=3)

[33] GiveWell's website for the evaluation: https://www.givewell.org/charities/IDinsight/partnership-with-idinsight/new-incentives-rct

clinics visiting treatment clinics (contamination or spillovers).[34] IDinsight then ran several automated and manual iterations to select combinations of clinics that were at least 17 kilometers apart. This process continued until we identified the maximum number of safe, operable, and well-spaced clinics.[35]

The selected clinics covered nearly the entire geography of the three states. In Katsina State, there were evaluation clinics in 31 out of 34 local government areas (LGAs).[36] In Zamfara State, the evaluation covered all 14 LGAs. In Jigawa State, the evaluation clinics covered 25 out of 27 LGAs.

Randomization

Following the baseline survey,[37] we randomly allocated evaluation clinics to a treatment and a control group. Figure 2 shows the location of those clinics across the three evaluation states. To increase balance between the treatment and control group, we stratified randomization on five characteristics of each clinic: the proportion of children (12-16 months) who had ever received an injectable vaccine (as measured by the baseline survey); variables related to operability (remoteness and number of routine immunization staff); state; and security.

We conducted randomization in three waves to balance the need to start operations[38] while allowing time for NI-ABAE to reassess the security situation in some clinics. The randomization process is described in greater detail in IDinsight's pre-analysis plan.

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[34] IDinsight, NI-ABAE, and GiveWell collectively decided on a minimum distance of 17 kilometers between clinics. This distance was chosen based on the local cost of travel and the size of the financial incentive paid by NI-ABAE in treatment clinics. We deemed that the size of the incentive was unlikely to justify the cost of transport to cover this distance. Meanwhile, larger minimum distances would rule out too many clinics, producing a clinic sample smaller than we needed for our desired statistical precision (power). There were five pairs of clinics that ended up closer than our target (i.e., 10-14km rather than 17km apart). Further, these pairs had a limited number of non-study clinics between them to prevent direct contamination between treatment and control. Therefore, these pairs were randomized together. While these clinic pairs were analyzed as a cluster, we expected the intracluster correlation coefficient (ICC) to be low as the clinics were far enough apart to have distinct catchments. This appears to have been the case: actual ICC for BCG vaccine coverage in the full sample was 0.13, while ICC in the 10 paired clinics was 0.03.

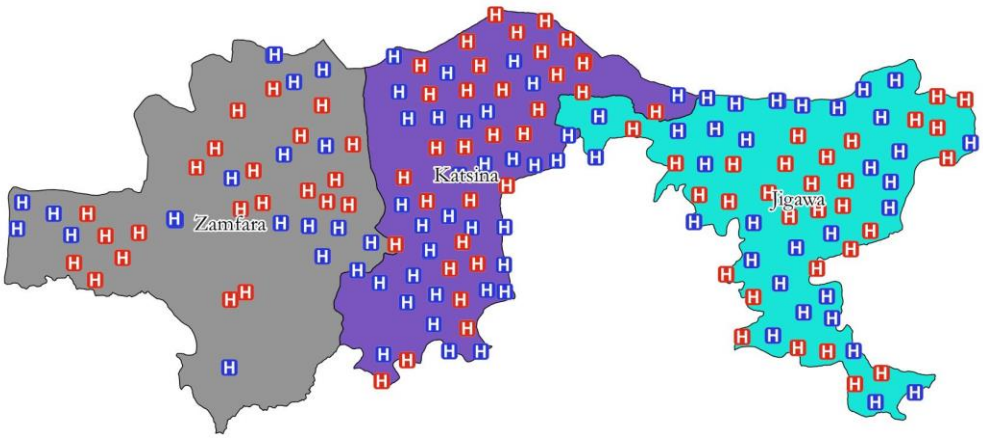
[35] There were some evaluation clinics which shared a catchment area with a non-evaluation clinic or had a poorly defined catchment border with a non-evaluation clinic within a town. We did not want to exclude these clinics, as this would exclude clinics that were within a town. Therefore, we suggested that NI-ABAE operates at these "complementary" clinics to ensure the catchment areas we surveyed were fully treated. For example, if people in a particular neighborhood customarily went to two clinics, we wanted to ensure both were served by NI-ABAE, even though only one was included in the evaluation. For clinic-record data collection and clinic-staff surveys, we visited both the study clinic and the complementary clinic. Appendix J lists all combinations of study and complementary clinics.

[36] The selection excluded one LGA because it included NI-ABAE's CCTs for RI Program pilot sites. The other three LGAs were excluded by chance given the small size of LGAs in Katsina State. Figure 1 shows roughly even distribution of study clinics across the state.

[37] The baseline survey took place between August 14 and October 17, 2017 in Katsina and Zamfara States. Jigawa State was added after the baseline was complete to increase the sample size for endline. Piloting activities to assess IDinsight's ability to conduct survey operations in Jigawa State took place in February-March 2018. Methods and findings of the baseline study are written up in IDinsight's Baseline Report.

[38] NI-ABAE gradually ramped up operations in all evaluation clinics between the end of baseline and June 2018. This allowed them to work out their operational model across all clinics and to get through the initial peak of "catch-up" vaccinations for children who started their vaccination schedule as a result of the NI-ABAE CCTs for RI Program. This ensured that the endline

Figure 2. Map of Evaluation Clinics Across Zamfara, Katsina, and Jigawa States, Nigeria



Note: Clinics in red are treatment clinics and clinics in blue are control clinics The area in southern Zamfara State with no clinics is an area of high insecurity where many clinics were dropped due to security concerns.

Baseline data showed balance between clinics in both arms (see Appendix A).

Sampling

As it was not financially feasible to create a full population register, we used compact segment sampling (CSS) to select our evaluation population. CSS avoids the need for a population register by using geographic area as a primary sampling unit: if we intend to randomly sample 25% of a catchment's population, we randomly sample 25% of its geographic area.

Three to four months prior to endline, we confirmed which settlements[39] comprised the catchment area of an evaluation clinic according to that clinic's routine immunization microplan. We called this process "map verification." It involved in-person visits to all study clinics to record the microplan settlement list, compare it to previous settlement lists collected at baseline or during other data collection activities, and interview clinic officials to clarify any discrepancies. IDinsight therefore had settlement lists both from prior to program implementation and from map verification. Generally, the map verification lists were longer than those collected previously, including some additional settlements and some settlements that had been split into multiple settlements while maintaining similar boundaries. After discussions with NI-ABAE, we decided to err on the side of inclusion,

cohort (born in July 2018 or later) received their vaccinations when the NI-ABAE CCTs for RI Program was running as it would at scale.

[39] Within each catchment area, there are one or more 'settlements.' In many cases, settlements are defined through the local political process, typically corresponding to a natural community or geographical boundary. Baseline field work revealed that these boundaries are sometimes difficult to determine on the ground. IDinsight conducted field work prior to the endline survey to verify settlement boundaries.

surveying at endline those settlements included on the map verification list, even if they were not included on older lists.[40]

Each settlement was divided into several equally sized geographic segments (eight for most settlements, sixteen for larger settlements, four for smaller settlements, and one for settlements too small to divide).[41] We then randomly selected one quarter of these segments in each settlement to approximate 25% of study area and, on average, 25% of the population.[42] We then surveyed all households that were in that segment to identify eligible 12 to 16-month olds[43] ('household listing').[44] Every eligible child identified through this process was invited to participate in the endline routine immunization survey. Additional details on the CSS approach are described in IDinsight's pre-analysis plan.[45]

Data Collection

Instruments. Endline data collection used the following survey instruments:

- **Household Listing**: Collected basic economic and demographic information for the household and generated a list of its members. It then identified 12 to 16-month old children eligible for

[40] NI-ABAE analyzed the lists and determined that they had been operating in the majority of the "new" settlements for a substantial portion of the study period. IDinsight used GPS survey points from baseline to determine that a large proportion of the "new" settlements' land area had been included in the baseline survey area, albeit under different names. Accordingly, it seemed likely that using the later lists would not add a substantial number of households that had not been served by a study clinic for most of the study period. As a robustness check, we re-ran our analyses on only settlements listed at baseline and results were substantively unchanged.

[41] When dividing settlement area, we excluded areas that did not appear to have structures according to satellite imagery.

[42] For small settlements not divided, we surveyed the entire area and used weights in our analyses to avoid over-representing them.

[43] Only children aged 12 to 16 months were included in the study. In order to accurately measure the program's impact, it was important to only include children in the study that were born after the program was fully implemented (i.e. children born in July 2018 onwards). The endline survey started in December 2019 - the month during which children born in July 2018 would turn 17 months. Hence, by only including children in our study that were aged 16 months in December 2019, we ensured that all eligible children had been born in or after July 2018. Since we relied on self-reported coverage, we also wanted to minimize the time between receiving the vaccine and our survey. Furthermore, the Measles 1 vaccine, which is the last vaccine that is incentivized by the program, is supposed to be received at age 9 months. As many children often receive the vaccine with a delay, we decided to include a buffer of 3 months for catch-up vaccines. Hence, we decided that a child had to be at least 12 months old to be included in the study.

[44] To ensure the correct identification of children aged 12 to 16 months, enumerators conducted a detailed age verification process for all children reported to be under the age of two during the household listing. Enumerators first asked the child's caregiver about the age of the child in months. They then proceeded to ask the caregiver about the child's date of birth (caregivers could choose between the English and the Islamic calendar). In many cases, caregivers did not know the exact date of birth of their child. Enumerators used a calendar tool (which contained information on important events, such as religious or national holidays) to help the caregiver remember their child's date of birth. At the end of the age verification process, the electronic survey automatically compared the information on the age in months and the date of birth provided by the caregiver. If the discrepancy between these two sources was more than 30 days, then the estimate reported as being more accurate by the caregiver was used to determine the child's eligibility.

[45] In 25 settlements, IDinsight accidentally surveyed too many or too few segments in a settlement. Where this occurred, we used weights to correct for the over- or under-representation of these settlements in our analyses. In 95 settlements, insecurity prevented surveying at endline. This occurred for 52 settlements in treatment and 43 in control. In control, 27 of these settlements were in four catchments that could not be surveyed at all due to insecurity. In treatment, the missing settlements all come from partially surveyed catchment areas. In total, we surveyed 1,877 settlements so these issues affected a relatively small proportion of our sample.

the study by first identifying children under two years of age followed by a detailed age verification process for those children.

- **Routine Immunization (RI) Survey**: Collected vaccination information (as reported by the child's caregiver and recorded on any child-health records kept at home) and other health-related outcomes for all children aged 12 to 16 months identified in the household listing.[46]
- **Clinic Staff Survey**: Collected information on clinic operations (e.g., number of routine immunization days per month, monitoring data protocol, number of staff employed, etc.) from clinic staff.
- **Clinic Tally Sheets Survey**: Collected monthly vaccination counts kept at clinics in "tally sheets."[47]
- **Clinic Record Verification**: Checked for vaccination records for all children encountered during the RI survey by name in clinic immunization registers (CIR), and recorded which vaccinations children received according to these registers.[48] This occurred at study clinics and at select non-study clinics to which many caregivers reported taking their children for vaccinations.

Appendix B defines the indicators and covariates collected via these instruments.

Informed consent process. Informed consent was obtained at the community and individual level for the household surveys. When first arriving in a community, enumerators asked the community leader for permission to survey in the community. In addition, written consent was obtained separately from the household listing respondent and from the Routine Immunization survey respondent (usually the child's caregiver) at the start of each respective survey. If the respondent was illiterate, a thumbprint was taken instead. If the respondent refused, the enumerator went to the next household.

Consent was obtained at the health facility level from the facility manager to collect administrative data from clinic records. For the clinic staff survey, written consent was obtained from the respondent (usually either the Officer in Charge or the Routine Immunization Focal Person) before the interview.

Field work. Endline field data collection took place from December 2019 to February 2020. The local research partner, Hanovia Limited,[49] provided a team of roughly 175 enumerators, supervisors, back-

[46] Whenever possible, the household listing and RI survey occurred on the same visit. In cases where the child's primary caregiver was not available, enumerators returned later to re-attempt the RI survey.
[47] At midline, tally-sheet data was collected for the period March 2017-February 2019. At endline, tally-sheet data was collected for the period March 2019-December 2019 (December data is excluded from this analysis since the survey took place in many clinics during December). Data was collected in 175 clinics at midline and 160 clinics at endline. This includes all clinics that were reachable and maintained records. The total 175 clinics includes 167 evaluation clinics and eight complementary clinics. Greater security challenges at endline prevented data collection in some clinics that had been reachable at midline.
[48] This is a change from baseline. At baseline, enumerators only searched for records of children whose caregivers reported a vaccination or who had a BCG vaccine scar. We did this in order to use CIRs to also cross-check cases where caregivers did not report any vaccinations.
[49] Hanovia also conducted the baseline data collection.

checkers, and support/supervisory personnel to administer all of the above survey instruments across the study area.

Prior to data collection, IDinsight and Hanovia conducted two weeks of training and several days of field practice. During data collection, Hanovia and IDinsight conducted backchecks, spotchecks, audio audits, high-frequency checks, and GPS reviews to monitor and ensure data quality. IDinsight engaged a team of seven Nigerian field managers, whom we independently managed to provide additional supervision and data quality assurance (backchecks, spotchecks, audio audits, etc.). Additional details on our data quality measures are in Appendix C.

Statistical Analysis

For our primary analyses, we used ordinary least squares (OLS) to estimate the probability of an individual child having a positive self (caregiver) report for a given vaccination. This method provides easily interpretable results: the coefficient on a variable indicating whether a given child lived in a treatment catchment or not is the impact estimate; it tells us the change in the probability of positive vaccination attributable to being in the treatment group. This is equivalent to the increase in coverage caused by being in treatment (since the average probability of vaccination across the sample is the same as coverage in the sample). If we only include the treatment variable and an error term in the OLS regression, this coefficient is also equal to the difference between coverage in treatment areas and coverage in control areas (difference-in-proportions). For our primary specifications, however, we included a series of covariates to increase precision. These are defined in detail in Appendix B.

For the primary (child-level) analyses we estimated the below regression specification:[50, 51]

$$\boldsymbol{Y_{ij}} = \beta_0 + \beta_1 * T_j + \beta_2 * B_j + \beta' * P_{ij} + \beta' * \alpha_j + \beta' * S_j + \varepsilon_{ij}$$

- Y_{ij} is the endline vaccination status of eligible infant i in clinic cluster j. We estimated this regression for each child-level vaccination outcome.
- T_j is the treatment status of clinic cluster j which includes infant i.
- B_j is the baseline coverage rate for the outcome among 12 to 16-month olds for clinic cluster j. For Jigawa State, this variable takes the value of 0 with the variation taken by the state dummy.
- P_{ij} is a vector of individual and clinic level covariates. See Appendix B for details on the variables included.
- α_j is a vector of randomization strata dummies
- S_j is a vector of state dummies
- ε_{ij} is the error term for infant i in catchment j clustered at the clinic cluster-level[52]

[50] See page 13 of the pre-analysis plan.

[51] As a result of our sampling strategy – which used maps to randomly select 25% of the land area of each settlement – the sample is largely self-weighted. Observations from a few settlements (N=210) had to be reweighted as either more than or less than 25% of the land area was selected for surveying (in most cases these were very small settlements for which IDinsight could only draw 1 segment, which was surveyed completely).

[52] Out of 167 clinics included in the study, ten clinics were randomized as pairs. Standard errors are clustered at the unit of randomization, i.e. the clinic pair level.

There were a few missing values for most of the individual and clinic level covariates (see Appendix B for more details). In order to avoid excluding these observations from the main specification, we used the following approach: we coded the covariate as 0 when missing and included a dummy variable for each covariate taking the value of 1 if the covariate is missing for an observation. We report a regression excluding missing values as a robustness check (see the Section "Robustness to alternative specifications").

We also conducted various analyses using clinic-level administrative data – primarily tally sheets. Appendix D reports the specifications used for this analysis.

We defined statistical significance as a p-value < 0.05. All statistical analyses were done using Stata 13 SE (College Station, TX, USA).

Ethical Review

This study received ethical approval from Nigeria's National Health Research Ethics Committee, Zamfara State Health Research Ethics Committee, Katsina State Health Research Ethics Committee, and Jigawa State Primary Health Care Development Agency. The study's pre-analysis plan is registered at the Registry for International Development Impact Evaluations (RIDIE),[53] ISRCTN,[54] and clinicaltrials.gov.[55]

Results

Endline data was collected between 1 December, 2019 and 19 February, 2020.

Evaluation Sample

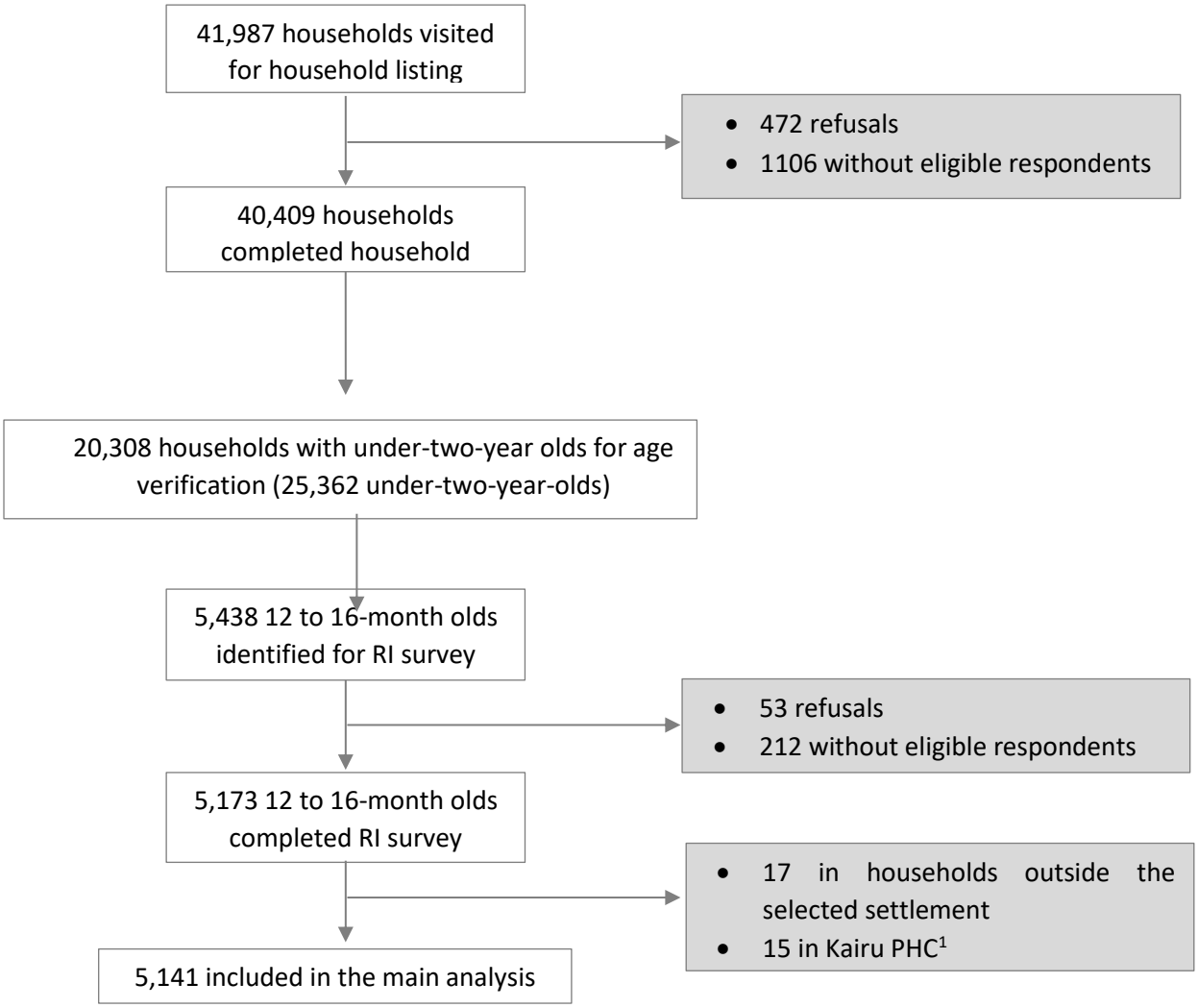
We visited 41,987 households across 163 clinic catchment areas in Katsina, Zamfara, and Jigawa States.[56] Of these 41,987 households visited, 1,106 (2.6%) did not have an eligible respondent[57] at the time of the survey and 472 (1.1%) households refused to participate, leaving us with 40,409 completed household listing surveys.

[53] https://ridie.3ieimpact.org/index.php?r=search/detailView&id=767
[54] https://www.isrctn.com/ISRCTN10808433
[55] https://clinicaltrials.gov/ct2/show/NCT03870061?term=conditional+cash&cond=immunization&draw=2&rank=3
[56] Note that we ultimately could not survey four catchment areas at endline due to insecurity.
[57] To be an eligible respondent for the household listing, a person had to meet the following two criteria:
1.) Be a household member (i.e. residing permanently in the household).
2.) Be knowledgeable about the people in the household (e.g. births, deaths, movement).
Enumerators were required to make up to three visits to a household to find an eligible respondent.

The household listing identified 5,438 12 to 16-month old children. Of these, 5,173 (95.1%) completed the RI survey, while 53 (0.1%) refused, and for 212 (3.9%), we were unable to identify an eligible respondent after three visits.[58]

Figure 3 shows the flow of households into the household listing sample and eligible children into the RI survey sample.

Figure 3. Flowchart for Participant Inclusion



[1]Kairu PHC was excluded from the main analysis because it was not included in the baseline survey, and, therefore, was missing data for this covariate.

[58] To be an eligible respondent for the RI survey, a person had to be the child's primary caregiver. If the child's primary caregiver was not available during the enumerator's first visit to a household, then the enumerator made up to two follow-up visits to the household. During these follow-up visits, the primary caregiver or the person taking the child to the clinic were eligible respondents. Regardless of the number of the visit, for the self-report vaccination section, enumerators asked for the person taking the child to the clinic to be present to support the primary caregiver in responding to the vaccination questions. In most cases, the primary caregiver and the person taking the child to the clinic were the same person.

Table 2 provides an overview of the evaluation sample.

Table 2. Evaluation Sample by State

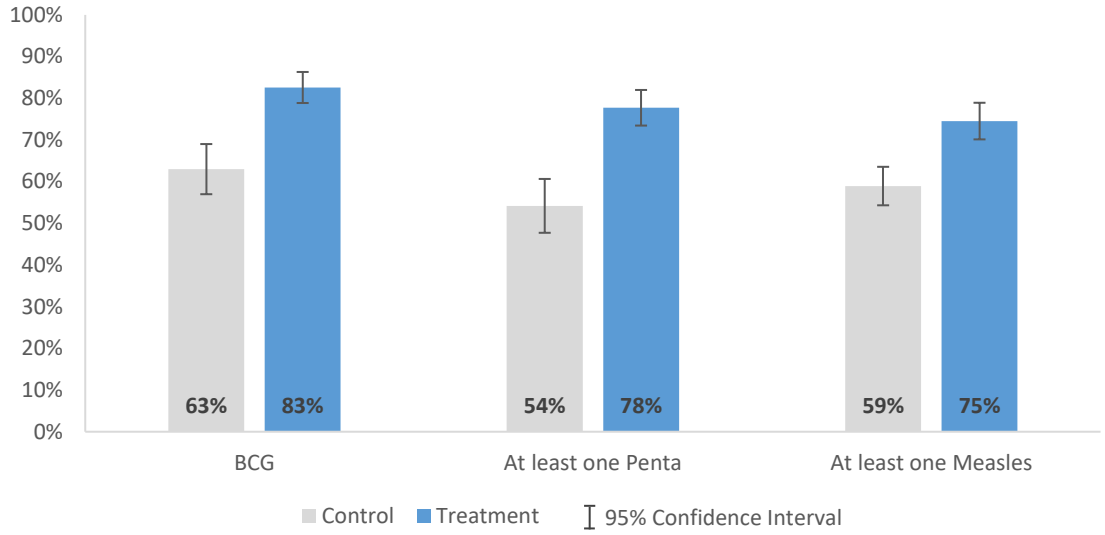
	Jigawa State	Katsina State	Zamfara State	Full Sample
Total number of household listings completed	16,078	13,755	10,576	40,409
Total number of RI surveys completed and included in analysis	2,214	1,671	1,271	5,156
Total number of clinics surveyed	61	66	36	163
Total number of settlements surveyed	870	494	381	1,745
Household Listing Sample				
Average number of surveys per clinic	263.6	208.4	293.8	247.9
Median number of surveys per clinic	226.0	165.5	277.5	206.0
Minimum number of surveys per clinic	52	55	90	52
Maximum number of surveys per clinic	804	985	577	985
Routine Immunization Sample				
Average number of surveys per clinic	36.3	25.3	35.3	31.6
Median number of surveys per clinic	31.0	20.5	32.0	28.0
Minimum number of surveys per clinic	6	1	9	1
Maximum number of surveys per clinic	86	138	94	138

Impact on Vaccination

Primary Outcomes

12 to 16-month olds in the catchment areas of NI-ABAE CCTs for RI Program clinics were statistically significantly more likely to have received BCG vaccine, Penta 1 vaccine, and Measles 1 vaccine compared to children in the catchment areas of control clinics. Figure 4 shows self-reported coverage for the control and treatment groups by vaccine. Coverage of each of the primary outcome vaccines among children in catchment areas of control clinics ranged from 54% [95% confidence interval, or CI: 48%, 61%] for Penta 1 vaccine to 63% [95% CI: 57%, 69%] for BCG vaccine. Coverage of each of these vaccines among children in NI-ABAE CCTs for RI Program treatment clinics ranged from 75% [95% CI: 70%, 79%] for Measles 1 vaccine to 83% [95% CI: 79%, 86%] for BCG vaccine.

Figure 4. Coverage by Vaccine (Self-Reports, No Covariate Adjustments)



After adjusting for covariates, the treatment effect was largest for Penta 1 vaccine, with the proportion of children having received Penta 1 vaccine in the treatment group being 21 percentage points (95% CI: 16, 26, p-value < 0.001) higher than the proportion in the control group, 16 percentage points (95% CI: 12, 21, p-value < 0.001) higher for BCG vaccine, and 14 percentage points (95% CI: 10, 18, p-value < 0.001) higher for Measles 1 vaccine.[59] Table 3 reports these results in detail, showing both unadjusted coverage in treatment and control and covariate-adjusted treatment effects from the main regression specification.

Table 3. Primary Outcomes – Means and Multivariate Regression Results

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Reported BCG vaccine	0.63 (0.57, 0.69)	0.83 (0.79, 0.86)	0.16 (0.12, 0.21)	**< 0.001**
Reported Penta 1 vaccine	0.54 (0.48, 0.61)	0.78 (0.73, 0.82)	0.21 (0.16, 0.26)	**< 0.001**
Reported Measles 1 vaccine	0.59 (0.54, 0.64)	0.75 (0.70, 0.79)	0.14 (0.10, 0.18)	**< 0.001**

Note 1: The treatment and control means are weighted for sampling probabilities but not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for the covariates listed in the methods section. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient, though both results show substantial, positive impact.

Note 2: The p-values shown have been adjusted for multiple hypothesis testing using the Bonferroni adjustment. All primary and secondary outcomes were included in the adjustment.

[59] The treatment effect for Measles vaccine was likely smaller than the treatment effect for BCG and Penta 1 because of Measles vaccine campaigns that occurred in most evaluation clinics just before endline. This is explored further in the Supplementary Analysis section.

Primary Outcomes by State

Children living in treatment catchment areas had statistically significantly higher coverage of Penta 1 vaccine and Measles 1 vaccine compared to children living in the control area in each of the three study states. The treatment effect was also statistically significant for BCG vaccine in Zamfara and Katsina States. In each of the study states, the treatment effect was larger for Penta 1 vaccine than for BCG vaccine. As shown in Table 4, after adjusting for covariates, 12- to 16-month olds in treatment areas in Zamfara State and Katsina State were both about 25 percentage points more likely to have received the BCG vaccine than children in control areas. In Jigawa State, the treatment effect was substantially lower (6 percentage points; 95% CI: 0, 12; p-value = 0.061). Treatment effects in Zamfara and Katsina States were even larger for any Penta vaccine. In Zamfara and Katsina States, the treatment effect for Measles 1 vaccine was the smallest of the three primary vaccines, while it was the largest in Jigawa State.

The treatment effect for BCG vaccine, Penta 1 vaccine, and Measles 1 vaccine was similar in Zamfara and Katsina States. The treatment effect for BCG vaccine and Penta 1 vaccine was statistically significantly smaller in Jigawa State than in Zamfara and Katsina States, while the treatment effect for Measles 1 vaccine was similar across the three states. The difference in the treatment effects between Zamfara State and Katsina State was never statistically significant. Zamfara State and Katsina State's treatment effects for BCG vaccine were 19 (p-value = 0.005) and 20 (p-value < 0.001) percentage points higher than the treatment effect in Jigawa State. Both of these differences were statistically significant. Similarly, the difference in the treatment effect on Penta 1 vaccine was 24 (p-value < 0.001) between Zamfara State and Jigawa State and 18 (p-value < 0.001) between Katsina State and Jigawa State. These differences were also statistically significant. Neither Zamfara State nor Katsina State had a statistically significantly higher treatment effect for Measles 1 vaccine than Jigawa State.

Figure 5. BCG Vaccine Coverage by Treatment Arm and State (Self-Reports, No Covariate Adjustments)

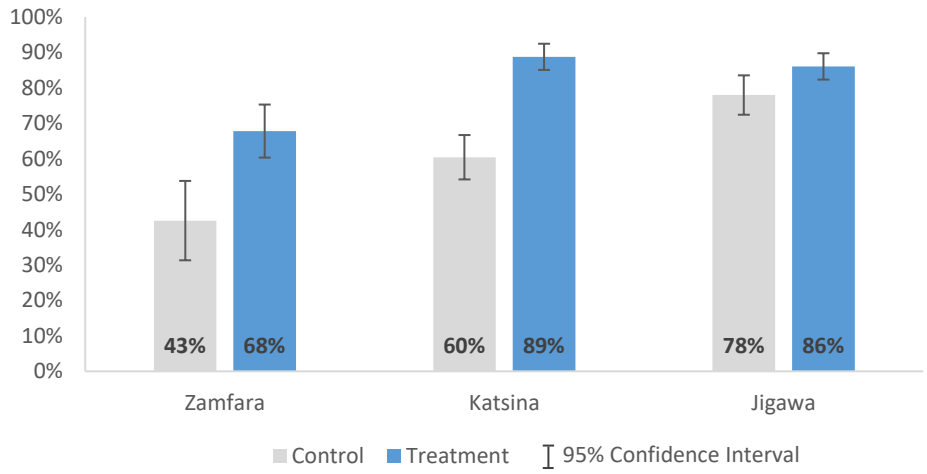


Figure 6. Penta 1 Vaccine Coverage by Treatment Arm and State (Self-Reports, No Covariate Adjustments)

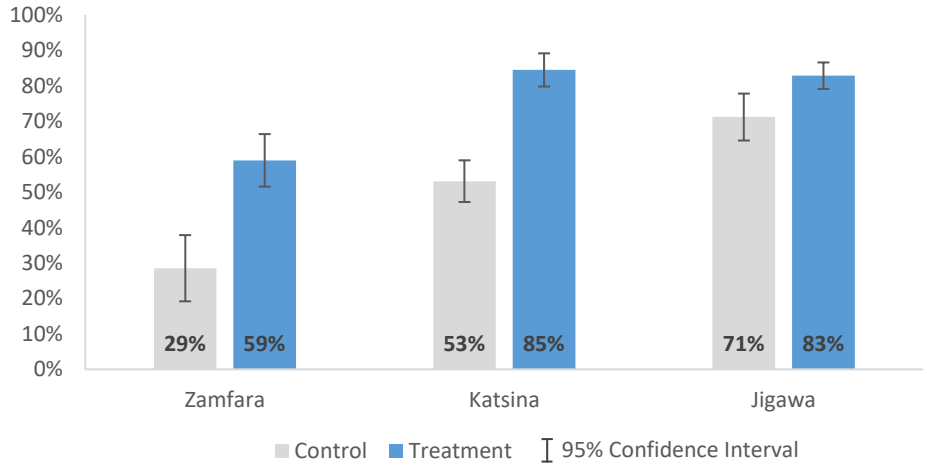
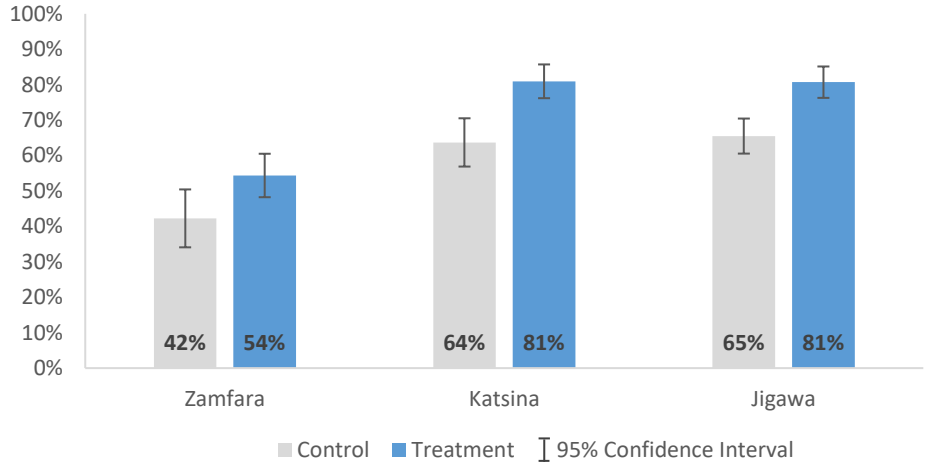


Figure 7. Measles 1 Vaccine Coverage by Treatment Arm and State (Self-Reports, No Covariate Adjustments)



Coverage in the control group was highest in Jigawa State and lowest in Zamfara State for all primary outcomes (BCG vaccine, Penta 1 vaccine, and Measles 1 vaccine), but coverage in the treatment group was similar in Katsina State and Jigawa State. While treatment effects were similar between Zamfara and Katsina States, control coverage was lower in Zamfara State than in Katsina State for each of the primary outcomes. Zamfara State's control coverage was 17 percentage points lower than Katsina State's for BCG vaccine, 24 percentage points lower for Penta 1 vaccine, and 22 percentage points lower for Measles 1 vaccine. While the treatment effects for BCG vaccine and Penta 1 vaccine were larger in Katsina State than Jigawa State, coverage in the treatment group was similar between these two states: BCG vaccine coverage for the treatment group was 89% and 86% in Katsina

and Jigawa States, respectively, and coverage for Penta 1 vaccine was 85% and 83%, respectively. Coverage of Measles 1 vaccine in both the treatment and the control groups was similar in Katsina State and in Jigawa State. Tables 4 and 5 report impact estimates and differences by state. Complete results are in Appendix E.

Table 4. Impact Estimates (Adjusted OLS) for Primary Outcomes by State

Outcome	Zamfara State	P-value	Katsina State	P-value	Jigawa State	P-value
Reported BCG vaccine	0.25 (0.14, 0.37)	**< 0.001**	0.26 (0.20, 0.32)	**< 0.001**	0.06 (-0.00, 0.12)	0.061
Reported Penta 1 vaccine	0.33 (0.23, 0.44)	**< 0.001**	0.28 (0.23, 0.33)	**< 0.001**	0.10 (0.03, 0.17)	**0.005**
Reported Measles 1 vaccine	0.16 (0.08, 0.24)	**< 0.001**	0.15 (0.08, 0.21)	**< 0.001**	0.13 (0.08, 0.19)	**< 0.001**

Table 5. Differences in Impact on Primary Outcomes by State

	Differences in impact estimate between states					
Outcome	Dif ZM-KT	P-value	Dif ZM-JG	P-value	Dif KT-JG	P-value
Reported BCG vaccine	-0.01	0.897	0.19	**0.005**	0.20	**< 0.001**
Reported Penta 1 vaccine	0.05	0.374	0.23	**< 0.001**	0.18	**< 0.001**
Reported Measles 1 vaccine	0.01	0.812	0.02	0.636	0.01	0.786

Note 1: Wald tests were performed to test whether the impact estimates for each individual state are significantly different from one another. All p-values shown are from the Wald test.

Overall, NI-ABAE's CCTs for RI Program's impact on vaccination coverage was higher in study states with lower control coverage. The similar treatment coverage in Katsina and Jigawa States suggests that there might be a ceiling to coverage. The self-reported estimates suggest this threshold lies around 85% to 90%. While it is possible that this result stemmed from imperfect recall,[60] it is more likely that some communities or households face greater barriers to vaccination that a modest cash incentive cannot fully overcome. These barriers could include the inaccessibility of vaccines as a result

[60] If some caregivers of vaccinated children do not recall vaccines correctly, then this implies that self-reported coverage will be less than 100% even in a fully vaccinated population. See the section "Supplementary Analysis for GiveWell's Cost-Effectiveness Model" for a more detailed discussion on how measurement error might affect coverage and impact estimates.

of remoteness or stock outs, a general mistrust of the safety or effectiveness of vaccines, or a lack of belief in vaccination's benefits.

Secondary Outcomes

Household-Level Coverage Outcomes

Children in treatment catchment areas were statistically significantly more likely to have received PCV (Table 6)**.** The average child in treatment catchment areas was 22 percentage points (95% CI: 18, 27, p-value < 0.001) more likely to have received at least one PCV than the average child in control catchment areas. This effect was similar to that on Penta 1 vaccine (21 percentage point increase), which we expect since these vaccines are administered at the same visits. As was the case at baseline, the percentage of caregivers reporting at least one PCV was lower in both groups than the percentage of caregivers reporting Penta 1 vaccine. PCV is a relatively new vaccine and seemed less salient to mothers, based on field observations. Additionally, it is given in the right leg along with other vaccines and so may be more easily confused, while Penta vaccine is the only vaccine administered in the left leg. Accordingly, it is reasonable to suspect that better recall accounts for much of this difference in self-reported coverage between Penta vaccine and PCV.

Table 6. Secondary Outcomes – Means and Multivariate Regression Results

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Reported at least one PCV	0.50 (0.43, 0.56)	0.75 (0.70, 0.79)	0.22 (0.18, 0.27)	**< 0.001**
Fully immunized (loose definition)	0.40 (0.35, 0.46)	0.68 (0.63, 0.73)	0.25 (0.21, 0.30)	**< 0.001**
Fully immunized (strict definition)	0.25 (0.21, 0.30)	0.54 (0.49, 0.60)	0.27 (0.23, 0.31)	**< 0.001**
Total number of vaccines received (no PCV)	2.46 (2.19, 2.72)	3.63 (3.41, 3.85)	1.06 (0.86, 1.25)	**< 0.001**
Total number of vaccines received (with PCV)	3.57 (3.14, 4.01)	5.55 (5.20, 5.91)	1.80 (1.48, 2.12)	**< 0.001**
Ever vaccinated	0.86 (0.82, 0.89)	0.89 (0.86, 0.92)	0.02 (-0.01, 0.06)	0.114
Has a BCG vaccine scar	0.41 (0.37, 0.46)	0.66 (0.61, 0.71)	0.22 (0.17, 0.26)	**< 0.001**

Note 1: The treatment and control means are weighted for sampling probabilities but not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for the covariates listed in the methods section. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient, though both results show substantial, positive impact.

Note 2: The p-values shown have been adjusted for multiple hypothesis testing using the Bonferroni adjustment. All primary and secondary outcomes were included in the adjustment.

Children in treatment catchment areas were statistically significantly more likely to be fully immunized. When using a loose definition of full vaccination (BCG vaccine, Penta 1 vaccine, Measles 1 vaccine), 12 to 16-month olds in treatment areas were 25 percentage points (95% CI: 21, 30, p-value < 0.001) more likely to be fully immunized than 12 to 16-month olds in control areas. When using the strict definition of full immunization (BCG vaccine, Penta vaccine 1-3, Measles 1 vaccine), the likelihood of being fully immunized was 27 percentage points (95% CI: 23, 31, p-value < 0.001) higher among children in the treatment areas.

Children in treatment catchment areas received statistically significantly more vaccine doses than those in control areas.[61] When considering BCG vaccine, all Penta vaccinations, and all Measles vaccinations, 12 to 16-month olds in treatment areas reported receiving 1.06 more doses (95% CI: 0.86, 1.25, p-value < 0.001) compared to children in the control areas. When adding all PCV vaccinations, this difference was 1.8 doses (95% CI: 1.48, 2.12, p-value < 0.001).

Children in treatment catchment areas were more likely to report having received at least one injectable vaccination. The statistical significance of this outcome varies by the type of metric used. During the RI survey, caregivers were explicitly asked whether their child had ever received an injectable vaccination. When using this variable to determine whether a child had ever been vaccinated (see Table 6), we found only a small difference between treatment and control. Caregivers of 12 to 16-month olds in treatment were 2 percentage points more likely to report that their child received an injectable vaccine [95% CI: -1, 6] and this difference was not significantly different from 0 (p-value = 0.114).

However, the magnitude and significance of these results changed if we used other indicators to measure a child's injectable vaccination status. Table 7 includes three additional variables which are based on the caregiver's answers to the individual vaccination questions. The first variable counts a child as having received an injectable vaccination if the caregiver responded with "Yes" to at least one of the injectable vaccination questions included in the RI survey.[62] Impact increased to 6 percentage points and became statistically significant (95% CI: 3, 10, p-value < 0.001). This suggests that some caregivers might have confused the non-injectable OPV vaccine, or possibly medical treatments involving an injection (e.g. malaria treatment), with injectable vaccinations when answering our survey question.[63] The impact estimate increased to up to 13 percentage points (95% CI: 9, 18, p-value < 0.001) if we excluded vaccines that are often received via campaigns (Measles vaccine, Yellow Fever vaccine, inactivated Polio vaccine or IPV).[64] Children that only received one or more of these three

[61] The vaccine dose count included BCG vaccine, Penta 1-3 vaccines, and Measles 1 vaccine. Hence, children could receive a maximum of 5 vaccine doses according to this indicator. The second indicator also included PCV 1-3 allowing for a maximum of 8 vaccine doses.

[62] In the RI survey, caregivers were asked if their child had received the following injectable vaccinations: BCG vaccine, Hepatitis B vaccine, Penta vaccine 1-3, PCV vaccine 1-3, IPV, At least one Measles-2 vaccine, and Yellow Fever vaccine.

[63] It is also possible that some children had received injectable vaccinations about which we did not ask caregivers explicitly in the RI survey. Two injectable vaccinations which were not included in the RI survey but might have been received by some children are: (1) the fractional IPV (fIPV) vaccination, and (2) the Meningitis vaccination (MenA). Based on the reports of our field teams, and the vaccination card data, we know that fIPV and MenA campaigns were carried out in large parts of our study area. As these vaccines should usually have been given out during campaigns, this would still imply that these children did not receive any injectable vaccinations as part of regular RI activities.

[64] The majority of study clinics had campaigns shortly before endline. More details are included in the Supplementary Analysis section.

vaccines have likely never interacted with the Nigerian RI system, and would not have received any injectable vaccinations in the absence of campaigns. This allows us to better estimate the additional share of children whose caregivers decided to take their children for RI vaccinations as a result of the NI-ABAE CCTs for RI Program.

Overall, these results suggest that the NI-ABAE CCTs for RI Program does motivate caregivers to start the RI schedule who otherwise would not have. However, a larger part of the program's impact seems to be driven by motivating caregivers who start the RI schedule to continue and complete the full schedule.

Table 7. Alternate Ever-Vaccinated Definitions

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Child received at least one injectable vaccine	0.79 (0.74, 0.83)	0.87 (0.83, 0.90)	0.06 (0.03, 0.10)	**< 0.001**
Child received at least one injectable vaccine (excluding Measles vaccine)	0.73 (0.68, 0.78)	0.85 (0.82, 0.89)	0.10 (0.06, 0.14)	**< 0.001**
Child received at least one injectable vaccine (excluding IPV, Yellow Fever vaccine, and Measles vaccine)	0.68 (0.62, 0.74)	0.84 (0.81, 0.88)	0.13 (0.09, 0.18)	**< 0.001**

Note 1: The treatment and control means are weighted for sampling probabilities but not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for the covariates listed in the methods section. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient, though both results show substantial, positive impact.

Note 2: The p-values shown have not been adjusted for multiple hypothesis testing.

Household-Level Timeliness Outcomes

Children in the treatment group were slightly less likely to receive BCG vaccine within one month of birth. This difference was not statistically significant.[65] Children in the treatment group were six percentage points (95% CI: -13, 1; p-value = 0.08) less likely to have received BCG vaccine within one month of birth compared to those in the control group. This is likely because those who receive BCG vaccine in the absence of the incentive are those who deliver in a health facility or otherwise interact

[65] As outlined in the pre-analysis plan, this analysis was restricted to those children with the vaccination recorded on a vaccination card (CHC or other card, such as a campaign card). We used the date of vaccination and the date of birth from the card. We also ran a robustness check using caregivers' reports of their child's age at vaccination (which were available for nearly all children with a positive self-report for a given vaccine) instead of card data. The results confirmed the findings from the card data based analysis presented in this section. We decided to use the card data based analysis as our main specification as we had two major concerns about the quality of the self-reported data on age at vaccination. Firstly, the recommended age of vaccination was included in our questions as a recall aid when asking for caregivers to self-report whether the child had received a given vaccine or not. Secondly, we were worried about differential recall of the age at vaccination in treatment and control as the program might have improved treatment caregivers' recall.

with the healthcare system close to birth. Those who are induced by the incentive to receive BCG vaccine are likely those who do not interact with the healthcare system at birth.

Children in the treatment group were statistically significantly more likely to have received the first dose of Penta vaccine within two weeks of the recommended age, but not statistically significantly more likely to have received it within one month of the recommended age. Timely vaccination using a two-week cutoff was 9 percentage points (95% CI: 3, 16; p-value = 0.007) higher in the treatment group than in the control group. Timely vaccination using a one-month cutoff was 4 percentage points (95% CI: -2, 9; p-value = 0.24) higher in the treatment group than the control group.

The incentive had the largest impact on the timeliness of the Measles 1 vaccine. Children in the treatment group who received the Measles 1 vaccine were 33 percentage points (95% CI: 28, 38; p-value < 0.001) more likely to have received the vaccination within one month of the recommended age (9 months) compared to those who received the Measles 1 vaccine in the control group. Measles 1 vaccinations were also more likely to be timely in the treatment group than in the control group when using a cutoff of within 2 weeks of the recommended age.

Table 8. Vaccination Timeliness Outcomes – Means and Multivariate Regression Results

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Timely BCG vaccine (within 1 month after birth)	0.66 (0.59, 0.72)	0.62 (0.56, 0.67)	-0.06 (-0.13, 0.01)	0.079
Timely BCG vaccine (within 2 weeks after birth)	0.22 (0.17, 0.28)	0.17 (0.13, 0.21)	-0.05 (-0.11, 0.01)	0.091
Timely Penta 1 vaccine (+/- 1 month)	0.68 (0.63, 0.73)	0.72 (0.67, 0.78)	0.04 (-0.02, 0.09)	0.239
Timely Penta 1 vaccine (+/- 2 weeks)	0.49 (0.43, 0.55)	0.58 (0.53, 0.63)	0.09 (0.03, 0.16)	**0.007**
Timely Measles 1 vaccine (+/- 1 month)	0.53 (0.48, 0.58)	0.86 (0.84, 0.87)	0.33 (0.28, 0.38)	**< 0.001**
Timely Measles 1 vaccine (+/- 2 weeks)	0.29 (0.25, 0.33)	0.69 (0.67, 0.72)	0.39 (0.35, 0.44)	**< 0.001**

Note 1: The treatment and control means are weighted for sampling probabilities but not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for the covariates listed in the methods section. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient.

Note 2: The p-values shown have not been adjusted for multiple hypothesis testing.

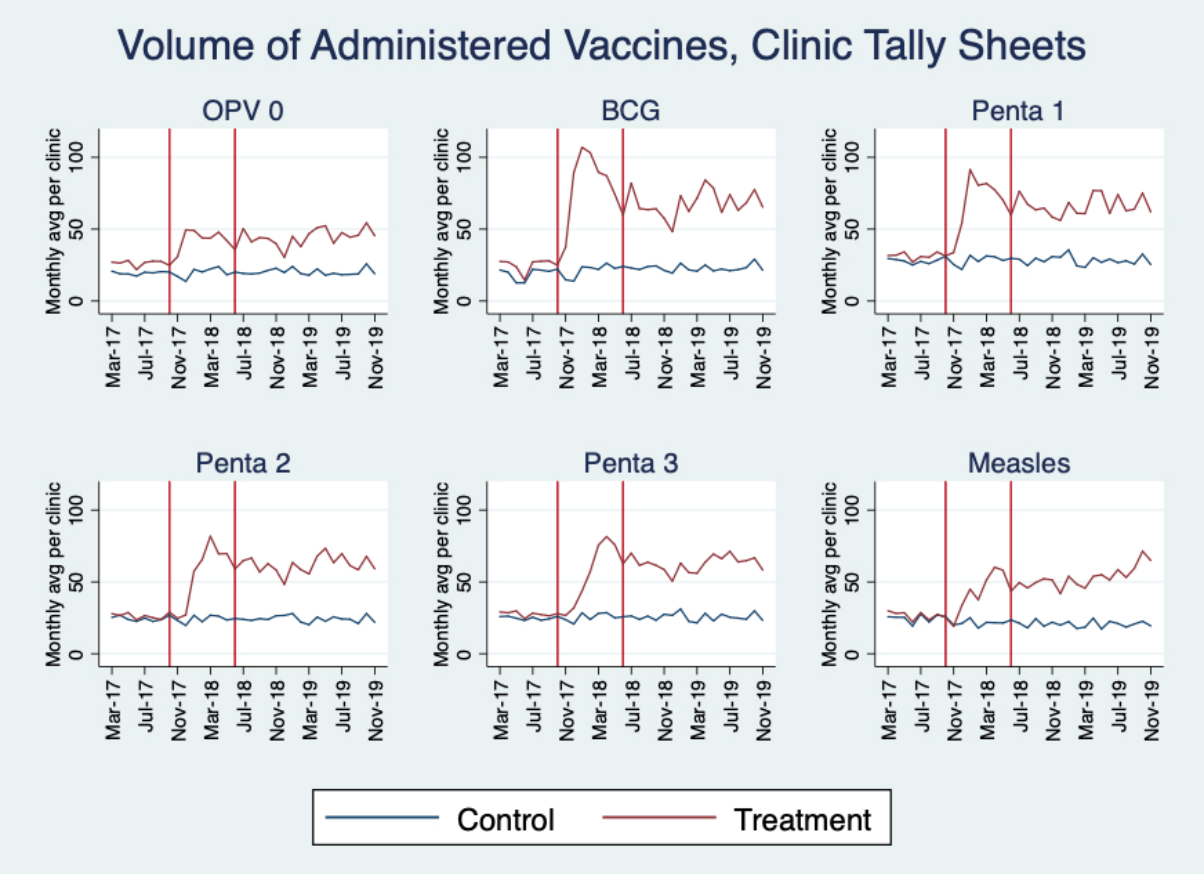
Clinic-level Vaccination Volumes

Vaccination volumes recorded at clinics have experienced statistically significantly larger increases since baseline in treatment clinics than in control clinics. Using clinic tally sheets (which count vaccination doses given at each clinic by vaccine and by month), we found that the average increase in monthly BCG vaccine doses administered between baseline and endline was 17.6 (95% CI: 9.1, 26.2,

p-value < 0.001) doses greater in treatment clinics than control clinics. For Penta 1, the difference was 34.4 (95% CI: 23.9, 44.9, p-value < 0.001), and for Measles vaccine, it was 32.3 (95% CI: 23.2, 41.3, p-value < 0.001). Figure 8 illustrates this these differences while Appendix F reports numeric results.

NI-ABAE was not operating in the study clinics in months prior to Oct 2017 and was fully operational in all study clinics by July 2018, the beginning of the RCT window.

Figure 8. Clinic Tally Sheets Results[66]



Note: For each of the graphs, the x-axis represents individual months and the y-axis represents the average number of distributed vaccines (as recorded) per clinic in each month. The blue line represents the control group, while the red line represents the treatment group. The vertical red lines represent the start and end of the program ramp-up phase.

The large differences observed in Figure 8 likely reflect a combination of the NI-ABAE CCTs for RI Program's influence on both actual immunization volumes and the quality of clinics' record-keeping.[67] However, the data shows changes in volume so large that we are confident they include meaningful

[66] Midline analyzed OPV0. This figure maintains OPV0 for consistency with midline, though endline does not analyze OPV0 in any detail. It is important to note that we would not expect OPV0 and BCG vaccine to have similar volumes as OPV0 is subject to stricter age restrictions – infants receiving catch-up BCG vaccine at older ages will not receive OPV0. At Midline, we found that treatment clinics had similar volumes for other coinciding vaccines as for vaccines for which NI-ABAE pays incentives directly, which is consistent with other results in this report (see Yellow Fever vaccine, for example).

[67] It is likely that the presence of NI-ABAE staff in treatment clinics led to better record keeping by clinic staff since clinic staff would administer vaccines and then refer the caregiver to the NI-ABAE staff member for the correlating incentive.

impact on actual vaccination volumes, consistent with the positive impact estimates from the household survey.[68, 69]

Secondary Outcomes by State

For secondary outcomes, we also found that the treatment effect of the NI-ABAE CCTs for RI Program was usually higher in Zamfara State and Katsina State than in Jigawa State, and that these differences were largely statistically significant. Tables 9 and 10 below show the results for secondary outcomes by state.

Table 9. Impact Estimates (Adjusted OLS) for Secondary Outcomes by State

Outcome	Zamfara State	P-value	Katsina State	P-value	Jigawa State	P-value
Reported at least one PCV	0.35 (0.26, 0.44)	**< 0.001**	0.29 (0.24, 0.35)	**< 0.001**	0.11 (0.04, 0.19)	**0.002**
Fully immunized (loose definition)	0.30 (0.22, 0.38)	**< 0.001**	0.31 (0.25, 0.37)	**< 0.001**	0.19 (0.12, 0.27)	**< 0.001**
Fully immunized (strict definition)	0.30 (0.23, 0.36)	**< 0.001**	0.31 (0.24, 0.38)	**< 0.001**	0.24 (0.16, 0.31)	**< 0.001**
Total number of vaccines received (no PCV)	1.41 (1.00, 1.81)	**< 0.001**	1.37 (1.10, 1.63)	**< 0.001**	0.68 (0.36, 1.00)	**< 0.001**
Total number of vaccines received (with PCV)	2.38 (1.78, 2.97)	**< 0.001**	2.32 (1.89, 2.75)	**< 0.001**	1.17 (0.62, 1.71)	**< 0.001**
Ever vaccinated	0.07 (-0.03, 0.16)	0.189	0.04 (0.00, 0.08)	**0.028**	-0.01 (-0.04, 0.03)	0.724
Has a BCG vaccine scar	0.20 (0.10, 0.31)	**< 0.001**	0.26 (0.20, 0.32)	**< 0.001**	0.20 (0.12, 0.27)	**< 0.001**

[68] Note that there is a clear spike in treatment vaccination counts for most vaccines during the ramp-up period. Leading explanations relate to catch-up vaccinations given to children who were not vaccinated at the recommended age prior to program ramp-up and came in for vaccination at older ages once the program began.

[69] Tally sheets are not directly comparable to the household-level coverage estimates, even if these estimates are converted from coverage into volumes. Tally sheets include a larger population than our coverage survey since they also reflect vaccinations given to infants from outside a clinic's catchment area and children of all ages eligible for each vaccine. By contrast, the household survey included children between 12 and 16 months of age who lived in official catchment areas. Nevertheless, like household data, tally sheets record a substantial positive impact of the program, corroborating the general, directional results.

Table 10. Differences in Impact on Secondary Outcomes by State

Outcome	Dif ZM-KT	P-value	Dif ZM-JG	P-value	Dif KT-JG	P-value
Reported at least one PCV	0.06	0.262	0.23	**< 0.001**	0.18	**< 0.001**
Fully immunized (loose definition)	-0.01	0.855	0.11	0.057	0.12	**0.017**
Fully immunized (strict definition)	-0.01	0.879	0.06	0.227	0.07	0.188
Total number of vaccines received (no PCV)	0.04	0.865	0.73	**0.007**	0.69	**0.002**
Total number of vaccines received (with PCV)	0.06	0.871	1.21	**0.004**	1.15	**0.002**
Ever vaccinated	0.03	0.628	0.07	0.184	0.05	0.082
Has a BCG vaccine scar	-0.05	0.384	0.01	0.894	0.06	0.213

Note 1: ZM = Zamfara State; JG = Jigawa State; KT = Katsina State
Note 2: Wald tests were performed to test whether the impact estimates for each individual state are significantly different from one another. All p-values shown are from the Wald test.

The program's treatment effect on the percentage of children that received at least one injectable vaccination was larger in Zamfara State and Katsina State than in Jigawa State. The magnitude and statistical significance of this difference varied based on the metric that was used. The effect of the program on our main "Ever vaccinated" outcome measure was modest across the three states, and state differences were not statistically significant. The effect of the program on the percentage of caregivers who answered "yes" to at least one of the individual vaccine questions was statistically significantly higher in Zamfara and Katsina States than in Jigawa State. In Zamfara State, children in treatment areas were 16 percentage points (95% CI: 5, 26; p-value = 0.003) more likely to have received at least one injectable vaccine than children in control areas. Impact estimates in Zamfara State and Katsina State are even higher for the other two alternative "Ever vaccinated" outcome measures. These findings suggest that the program successfully motivated caregivers to start the RI schedule in contexts where there is still a large number of children not receiving any injectable vaccine during regular RI activities, and hence substantial room for improvement.

Table 11. Impact Estimates (Adjusted OLS) for Ever-Vaccinated by State

Outcome	Zamfara State	P-value	Katsina State	P-value	Jigawa State	P-value
Child received at least one injectable vaccine	0.16 (0.05, 0.26)	**0.003**	0.08 (0.03, 0.13)	**0.002**	0.01 (-0.03, 0.05)	0.592
Child received at least one injectable vaccine (excluding Measles vaccine)	0.21 (0.10, 0.33)	**< 0.001**	0.14 (0.08, 0.19)	**< 0.001**	0.02 (-0.03, 0.07)	0.344
Child received at least one injectable vaccine (excluding IPV, Yellow Fever vaccine, and Measles vaccine)	0.26 (0.14, 0.38)	**< 0.001**	0.22 (0.16, 0.28)	**< 0.001**	0.02 (-0.03, 0.07)	0.459

Table 12. Differences in Impact on Ever-Vaccinated by State

Outcome	Dif ZM-KT	P-value	Dif ZM-JG	P-value	Dif KT-JG	P-value
Child received at least one injectable vaccine	0.08	0.164	0.15	**0.011**	0.07	**0.039**
Child received at least one injectable vaccine (excluding Measles vaccine)	0.08	0.228	0.19	**0.003**	0.11	**0.003**
Child received at least one injectable vaccine (excluding IPV, Yellow Fever vaccine, and Measles vaccine)	0.04	0.584	0.24	**< 0.001**	0.20	**< 0.001**

Note 1: Wald tests were performed to test whether the impact estimates for each individual state are significantly different from one another. All p-values shown are from the Wald test.

The results for BCG vaccine scars contradicted the findings for BCG vaccine self-reports. **Where self-reported BCG vaccine results suggested a low treatment effect in Jigawa State, BCG vaccine scar data produced a high (around 20 percentage points) and statistically significant treatment effect in all three states.** The difference resulted primarily from the fact that BCG vaccine scar coverage in control catchment areas in Jigawa State was as in Katsina State while self-reported BCG vaccine coverage in control catchment areas was much higher in Jigawa State. This is unlikely to have resulted from differential enumerator error, given that enumerators were trained together and several worked in each state. A more likely explanation is that caregivers in Jigawa State were more likely to over-report BCG vaccination. We heard from our field teams that they frequently encountered caregivers who used the vaccine name "BCG" as a synonym for any injectable vaccine. While we cannot be sure, it is possible that this was more prevalent in Jigawa State than the other two study states, and led to relatively more Jigawa State caregivers incorrectly reporting a BCG vaccine that had in fact not occurred.

Impact on Additional Outcomes

Additional Vaccine and Other Health-Service Outcomes

In addition to the primary and secondary outcomes, we also looked at the second and third doses of Penta vaccine as well as the second and third doses of PCV.[70] We also looked at vaccines that are not directly incentivized by the NI-ABAE CCTs for RI Program (birth dose of Hepatitis B vaccine or HepB, IPV, and Yellow Fever vaccine). Finally, since the NI-ABAE CCTs for RI Program may generally increase health service utilization or increase uptake of additional interventions during immunization visits, we also looked at the non-injectable vaccine outcomes receipt of oral polio vaccine (OPV), at least one dose of vitamin A, and first-time clinics visits.

The NI-ABAE CCTs for RI Program had a significant positive effect on vaccination coverage for all major injectable vaccines included in the Nigerian Routine Immunization schedule – including those vaccines which are not directly incentivized by the program.[71] Children in treatment areas were 27 percentage points (95% CI: 23, 32; p-value < 0.001) more likely to have received three Penta vaccinations, and 25 percentage points (95% CI: 20, 29; p-value < 0.001) more likely to have received three PCV vaccinations than children in control areas. These impact estimates are higher than for Penta 1 vaccine. It is likely that this is the result of two effects. First, the program seems to be particularly effective at ensuring that caregivers complete the full immunization schedule. Second, it is also likely that caregivers in treatment are better at recalling their second/third Penta/PCV vaccinations than caregivers in control as the incentive has made the receipt of the vaccine more memorable.[72]

Children in treatment areas were statistically significantly more likely than those in control areas to have received the birth dose of HepB vaccine, IPV, and the Yellow Fever vaccine – three vaccines that are not directly incentivized by the program but are usually given at the same time as incentivized vaccines. Children in treatment areas were 16 percentage points (95% CI: 11, 21) more likely to have received the first Hep B vaccine than children in control areas. This is similar to the impact found for BCG vaccine, which is also given at birth. IPV is typically given at the same time as Penta vaccine and PCV 3, but the impact estimate for IPV was lower than for Penta 3 vaccine / PCV3, the co-occurring incentivized vaccines. This difference could result from poorer recall of IPV; it is administered in the same leg as PCV so these may be difficult to differentiate. The impact estimate for the Yellow Fever vaccine was 18 percentage points, and thereby a few percentage points higher than the impact estimate for Measles 1 vaccine. According to the Routine Immunization schedule, both vaccines should be administered at the age of 9 months. As we explain in more detail in the section "Supplementary Analysis for GiveWell's Cost-Effectiveness Model", the lower impact estimate for Measles vaccine than for the other directly incentivized vaccines is likely the result of a large-scale

[70] These vaccines were not included as separate primary or secondary outcomes because of concerns that they would be more subject to recall error than the other vaccines. Penta 2 and Penta 3 vaccines were included in the strict definition of "full vaccination". Both Penta vaccine and PCV 2 and 3 were included in the number of vaccines given.

[71] NI-ABAE provides incentives during the routine immunization visits where HepB0, IPV, OPV, and Yellow Fever vaccine are administered. However, these vaccinations are not part of the minimum required vaccines that are required for the incentives to be disbursed.

[72] Accordingly, we should not assume that the full difference in effect size between any Penta vaccine and subsequent doses of Penta vaccine is due to real impact on coverage, though the precise size of true impact versus differential measurement error is unclear.

Measles vaccine campaign which was conducted by Nigerian authorities and international partners during November and December 2019.

Table 13. Additional Outcomes, Means and Multivariate Regression Results

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Reported HepB vaccine	0.49 (0.43, 0.56)	0.69 (0.64, 0.74)	0.16 (0.11, 0.21)	**< 0.001**
Reported Penta2 vaccine	0.40 (0.34, 0.46)	0.69 (0.64, 0.74)	0.27 (0.22, 0.31)	**< 0.001**
Reported PVC2 vaccine	0.36 (0.30, 0.41)	0.65 (0.60, 0.70)	0.27 (0.23, 0.32)	**< 0.001**
Reported Penta3 vaccine	0.30 (0.24, 0.35)	0.59 (0.54, 0.65)	0.27 (0.23, 0.32)	**< 0.001**
Reported PCV3 vaccine	0.27 (0.22, 0.32)	0.53 (0.48, 0.58)	0.25 (0.20, 0.29)	**< 0.001**
Reported IPV	0.23 (0.19, 0.27)	0.42 (0.37, 0.46)	0.18 (0.14, 0.22)	**< 0.001**
Reported Yellow Fever vaccine	0.40 (0.36, 0.44)	0.58 (0.53, 0.63)	0.18 (0.13, 0.22)	**< 0.001**
Reported at least one OPV	0.96 (0.95, 0.98)	0.97 (0.96, 0.98)	0.01 (-0.01, 0.02)	0.498
Reported at least four OPV	0.56 (0.52, 0.61)	0.65 (0.61, 0.69)	0.08 (0.04, 0.13)	**< 0.001**
Received at least one VitaminA dose	0.56 (0.50, 0.63)	0.60 (0.53, 0.66)	0.00 (-0.04, 0.04)	0.833
Ever visited clinic	0.87 (0.84, 0.91)	0.93 (0.91, 0.94)	0.05 (0.03, 0.08)	**< 0.001**

Note 1: The treatment and control means are weighted for sampling probabilities but not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for the covariates listed in the methods section. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient.

Note 2: The p-values shown have not been adjusted for multiple hypothesis testing.

Note 3: The outcome 'Ever visited clinic' was measured by asking the child's caregiver whether the child had ever been taken to a clinic (for any reason; including vaccinations).

Children in treatment areas were statistically significantly more likely to have received the full schedule of four OPV vaccines than children in control areas but not more likely to have received at least one dose of OPV. Nearly all children in our sample (96% in control, and 97% in treatment) were reported to have received at least one OPV vaccine, so it is unsurprising that the treatment

effect was close to zero.[73] Coverage of the full course of OPV (four doses) was lower, so there was more room for improvement in the treatment group. Children in treatment areas were 8 percentage points (95% CI: 4, 13; p-value < 0.001) more likely to have received at least four doses of OPV.

The program did not have a statistically significant effect on whether the caregiver reported that their child had received at least one Vitamin A dose. While the share of children with at least one reported Vitamin A dose is higher in the treatment group, the treatment effect is equal to 0 after adjusting for covariates.[74]

The program did have a modest but statistically significant effect on first time clinic visits. Children in treatment areas were 5 percentage points (95% CI: 3, 8; p-value < 0.001) more likely to have ever been taken to a clinic than children in control areas.

Knowledge and Attitudes Toward Vaccines

The NI-ABAE CCTs for RI Program had a statistically significant positive effect on respondents' knowledge of vaccines and the vaccination schedule.[75] Respondents in treatment areas were 7 percentage points (95% CI: 2, 12; p-value = 0.005) more likely to report that they knew where to get their child vaccinated than respondents in control areas. Furthermore, respondents were 6 percentage points (95% CI: 1, 12; p-value = 0.022) more likely to report that they knew at what ages children should receive vaccines. This was subsequently confirmed by asking caregivers questions to check their actual knowledge.[76] Respondents in treatment were 7 percentage points more likely to correctly identify the age at which a child should receive the first vaccine, and 5 percentage points more likely to correctly answer with what age the child should receive the Measles vaccine. Furthermore, respondents in treatment were 15 percentage points (95% CI: 11, 19; p-value < 0.001) more likely to know how many times a child should be taken to the clinic for vaccinations before the child reached the age of one year. This seems logical given that NI-ABAE's advertisement focuses heavily on reminding caregivers of the five visits they should make to the clinic. While caregivers in the treatment group had better knowledge of vaccines and the vaccine schedule, these differences

[73] The high coverage of OPV is likely because of oral polio campaign efforts in the study states.
[74] One possible explanation for this finding could be that Vitamin A supplements should usually be given at ages 6 months and 12 months, which do not coincide with the receipt of any of the vaccines that are conditions for incentive payments.
[75] The data shown in Table 14 was collected as part of the survey's household listing module. We decided prior to data collection that it would be potentially harmful to the quality of the main results of our study if these questions were included in the Routine Immunization survey which we conducted with caregivers of eligible children. Instead, we asked these knowledge questions to household listing respondents in households without eligible children but at least one child aged 5 to 11 months. In treatment communities, these households should also have been exposed to the NI-ABAE CCTs for RI Program as the program had been continuously operating in treatment communities. In total, responses from 7186 respondents were included.
[76] In order to protect the comfort and dignity of the respondents, only respondents who said that they knew at what ages children are supposed to get vaccines were asked the more detailed knowledge questions on first time vaccines and the Measles vaccines. The same approach was used for the questions on the number of times a child should receive vaccines. Responses from respondents who had already said "No" to the initial question that they did not know were coded as "No" for the more detailed knowledge questions. For example, 32% of respondents in control areas said that they knew at what ages children were supposed to get vaccines. These 32% of respondents were then subsequently asked about the age at which a child should receive their first injectable vaccine. 53% of these respondents provided the correct answer (at birth / as soon as possible after birth). Responses from the 68% of respondents who said in the first place that they did not know were coded as "No" leading to a total of 17% (32% * 0.53) of respondents to have responded with the correct answer.

were more modest than the differences in actual coverage. This suggests that the NI-ABAE CCTs for RI Program does more than improving caregiver knowledge about the timing of vaccines; more importantly, it results in greater action to actually take one's child for vaccination.

Table 14. Knowledge Outcomes - Means and Multivariate Regression Results

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Reportedly knows where to get vaccines for the child	0.71 (0.67, 0.75)	0.77 (0.73, 0.81)	0.07 (0.02, 0.12)	**0.005**
Reportedly knows at what ages children are supposed to get vaccines	0.32 (0.28, 0.37)	0.37 (0.31, 0.42)	0.06 (0.01, 0.12)	**0.022**
Correctly knew at what age a child should receive the first injectable vaccine	0.17 (0.14, 0.21)	0.23 (0.19, 0.27)	0.07 (0.03, 0.12)	**0.001**
Correctly knew at what age a child should receive the Measles vaccine	0.09 (0.07, 0.12)	0.14 (0.11, 0.18)	0.05 (0.02, 0.09)	**0.003**
Reportedly knows how many times children should get vaccines	0.25 (0.21, 0.29)	0.38 (0.32, 0.43)	0.14 (0.09, 0.19)	**< 0.001**
Correctly knew how many times a child should be taken to the clinic for vaccinations before the age of one	0.11 (0.09, 0.14)	0.25 (0.21, 0.30)	0.15 (0.11, 0.19)	**< 0.001**
Knew the answers to all questions listed above	0.03 (0.02, 0.04)	0.07 (0.05, 0.09)	0.05 (0.03, 0.07)	**< 0.001**
Did not know any of the answers to the questions listed above	0.27 (0.23, 0.31)	0.22 (0.18, 0.25)	-0.07 (-0.11, -0.02)	**0.003**

Note 1: The treatment and control means are weighted for sampling probabilities but not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for the covariates listed in the methods section. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient.

Note 2: For the knowledge questions, the following answers by caregivers were counted as correct (based on the Nigerian Routine Immunization schedule): (1) Age at first vaccination: 'At birth, or as close as possible', (2) Age at Measles vaccine: '9 months after birth', (3) Number of times child should be taken to clinic for vaccinations: '5 times'.

The NI-ABAE CCTs for RI Program had some modest effects on caregivers' attitudes towards vaccines. Caregivers in treatment were slightly more likely to say that they thought that vaccines were more beneficial than harmful for children. There were no statistically significant differences for a question that asked caregivers whether they thought that it was a parent's responsibility to ensure that their child got vaccinated.

Caregivers in the treatment group were 3 percentage points less likely than caregivers in the control group to report that it was difficult for their community to vaccinate their children. Caregivers in treatment were also more likely to report that they had heard positive messages from local leaders. Nearly no caregiver in both treatment and control areas reported that they had heard negative messages about vaccines from local leaders.

These effects on attitudes likely reflect a lower bound of the effects that the NI-ABAE CCTs for RI Program would have at scale. Firstly, NI-ABAE did less sensitization than they would at scale as they were worried about attracting children from outside of treatment catchment areas to the program. Secondly, all of the questions shown in table 15 likely suffer from substantial measurement error as a result of respondents wanting to provide socially desirable answers. This measurement error likely leads to a downward bias in our impact estimates.[77]

Table 15. Attitudes Outcomes – Means and Multivariate Regression Results

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Thinks that vaccines are more beneficial than harmful for children	0.88 (0.86, 0.89)	0.90 (0.88, 0.92)	0.02 (0.00, 0.04)	**0.027**
Thinks that it is a parent's responsibility to ensure that their child gets vaccinated	0.79 (0.76, 0.83)	0.81 (0.78, 0.85)	0.03 (-0.01, 0.07)	0.185
Thinks that it is difficult for their community to vaccinate their children	0.08 (0.07, 0.09)	0.05 (0.04, 0.06)	-0.03 (-0.05, -0.01)	**< 0.001**
Has heard positive messages about vaccines from local leaders	0.68 (0.64, 0.72)	0.72 (0.69, 0.74)	0.05 (0.01, 0.09)	**0.008**
Has heard negative messages about vaccines from local leaders	0.01 (0.00, 0.01)	0.01 (0.00, 0.01)	0.00 (-0.00, 0.00)	0.786

Note 1: The treatment and control means are weighted for sampling probabilities but not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for the covariates listed in the methods section. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient.

Vaccine Stockouts

The NI-ABAE CCTs for RI Program had a statistically significant impact on reducing the frequency of vaccine stockouts in clinics. Treatment clinics were 18 percentage points (95% CI: 6, 30; p-value = 0.004) more likely than control clinics to report that they had experienced no vaccine stockouts during the 12 months preceding the endline survey. Treatment clinics were also 16 percentage points (95% CI: 1, 30; p-value = 0.033) more likely to report that vaccine stockouts occurred rarely (twice per year or less). Only a very small number of clinics in both the treatment and the control group reported that they had constantly experienced vaccine stockouts during the preceding 12 months. The difference between treatment and control was not statistically significant for this final indicator.

[77] Please see our discussion in the section "Supplementary Analysis for GiveWell's Cost—Effectiveness Model" for more details on how measurement error might be effecting our results.

Table 16. Vaccine Stockout Outcomes – Means and Multivariate Regression Results

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Clinic reported that no vaccine stockouts occur	0.10 (0.03, 0.17)	0.27 (0.18, 0.37)	0.18 (0.06, 0.30)	**0.004**
Clinic reported that vaccine stockouts occur rarely (twice per year or less)	0.59 (0.48, 0.70)	0.74 (0.64, 0.83)	0.16 (0.01, 0.30)	**0.033**
Clinic reported that vaccine stockouts occur constantly (almost every week)	0.05 (0.00, 0.10)	0.05 (0.00, 0.09)	-0.02 (-0.08, 0.05)	0.586

Note 1: The treatment and control means are not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for randomization strata and state dummies. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient.

Our data shows that this significant difference between treatment and control clinics in the frequency of vaccination stockouts did not exist prior to the implementation of the NI-ABAE CCTs for RI Program. This effect is likely observed as a result of NI-ABAE's staff checking the quality and stock of vaccines on RI day. If vaccine stock is low, NI-ABAE staff encourage clinic staff to procure more vaccines. This finding suggests that the program's impact goes beyond strengthening the demand for vaccines and includes the reduction of supply side constraints.

Robustness to Alternative Analyses

To test the influence of various analysis choices on our regression results for the primary outcomes, we carried out several robustness checks:

- **Covariates:** A first group of checks ("Drop missing", "No covariates", "Additional covariates") varied the covariates in the regression. The first drops observations with missing covariates, the second conducts the regression without any covariates, (i.e. estimates simple difference in means), the third adds additional covariates.[78]
- **Coding of "don't know" responses:** The next check, ("Drop dk") estimated the primary specification when coding "Don't know" outcome responses as missing – and dropping the respective observations – as opposed to coding "Don't Know" as 0 ("No"), as we do in the main analysis.

[78] The following additional covariates were included: (1) a binary variable indicating whether the caregiver ever received non-cash incentives for vaccinating; (2) a binary variable indicating whether the caregiver had heard positive messages about vaccinations from local leaders. These covariates were not included in the main specification as they could have been affected by treatment.

- **Sample inclusion:** Prior to endline, we visited all study clinics to verify which settlements were included in their catchment areas. While the majority of these settlements overlapped with the baseline list of settlements, some settlements had been added since baseline and others were on the settlement lists collected by IDinsight but not those NI-ABAE understood to define the catchment. This third group of robustness checks ("NI settlements", "BL settlements", and "No Damaga") explored whether conclusions changed when using different criteria to define the settlement lists. "NI settlements" includes only observations from settlements which NI-ABAE recognized as part of treatment catchments (N=5,106). "BL settlements" includes only observations from settlements on baseline settlement lists (N=3,390). "No Damaga" drops one Zamfara State clinic in which NI-ABAE was not able to operate for several months due to security (N=5121).

- **Card-based outcomes**[79]: Finally, we calculated the impact estimate for each outcome using vaccination card data instead of self-reported data. While card data may be more accurate (since it is not subject to recall error), we did not include this as our primary data source because of concerns about card retention.[80] At endline, we found at least one vaccination card for 74% of all children in our sample (80% in treatment and 66% in control).[81] For those children without any cards, though, 45% of caregivers in treatment areas and 55% of caregivers in control areas still reported at least one injectable vaccination. Accordingly, we would expect card-based coverage to miss a meaningful number of children who were vaccinated but never received a card, lost their card, or were unable to find it during the survey for the enumerator to record. We estimated the main regression specification and a specification without any covariates using only data from CHCs ("CHC main", and "CHC none"), as well as using data from all vaccination cards (including CHCs, campaign cards, etc.) ("Card main", and "Card none").[82]

Overall Trends – A Large, Positive, Stable Treatment Effect

Detailed results for each robustness check are provided in Appendix G.[83] **Robustness checks confirm the overall finding that the NI-ABAE CCTs for RI Program had a substantial positive impact on vaccination coverage**. Across all primary vaccination outcomes, the coefficients on the treatment variable were large and highly statistically significant regardless of specification. These large positive effects were also relatively stable, ranging from about 15 to 20 percentage points for BCG vaccine, from about 19 to 23 percentage points for Penta vaccine, and from about 14 to 16 percentage points

[79] We also coded outcomes based on BCG vaccine scars, Child Immunization Registers, and various combinations of measures. We report and analyze these outcomes measures in a separate section on self-report data quality.

[80] At endline, child health cards (and other vaccination cards) were available for a much larger share of children than at baseline: For 61% of all eligible children surveyed at endline at least one child health card was found while at baseline only 12.5% had a child health card.

[81] A child health card (CHC) was found for 60.9% of all children (73% in treatment and 44.6% in control). However, caregivers often also had a campaign or other card that also recorded vaccination information.

[82] The card data based impact estimates shown here code the response for a child without a child health card / any vaccination card as not having received a vaccine. IDinsight also ran card based robustness checks which drop children without a card. The tables showing the outputs of these robustness checks can be found in Appendix G.

[83] In addition to these specification-choice robustness checks, Appendix H reports results for outcomes calculated for the average clinic (i.e. giving all clinics average weight, regardless of population).

for Measles 1 vaccine. Figures 9a-9c below show specification curves for the three primary outcomes. For each outcome, we estimate 112 possible regression specifications; these specifications are all potential permutations arising from the following choices:

- Which variables to include as covariates:
  - child and HH-level covariates
  - catchment/clinic-level covariates
  - state dummies
  - baseline coverage
  - randomization strata dummies
- Whether to include an observation when a covariate is missing:
  - include the observation (with the covariate set to zero, and a corresponding dummy variable set to 1)
  - drop the observation
- How to treat "don't know" responses in the outcome
  - treat as "no"
  - exclude from the analysis

Each graph shows the estimated impact of treatment for all specifications, sorted by magnitude (for each specification, the blue dot indicates the point estimate of the treatment coefficient, while the violet area above and below indicates the 95%-confidence interval).

Figure 9a. Robustness Check Specification Curves (BCG Vaccine)

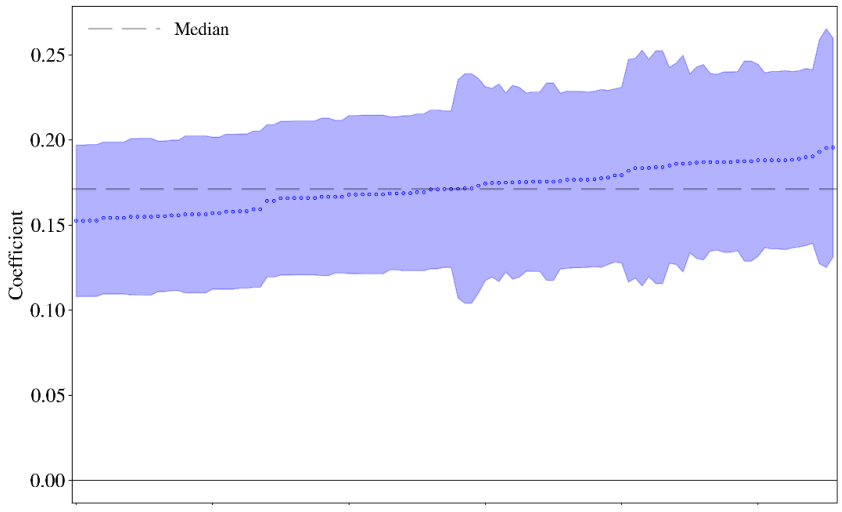


Figure 9b. Robustness Check Specification Curves (Penta 1 Vaccine)

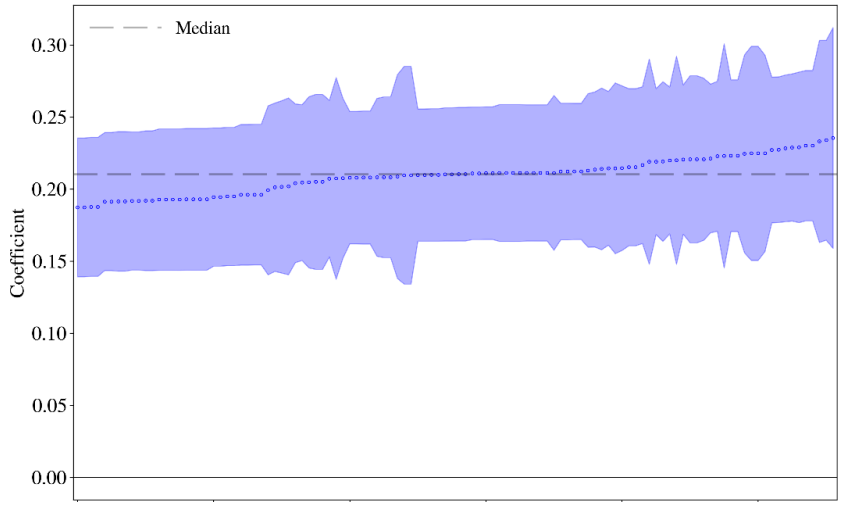
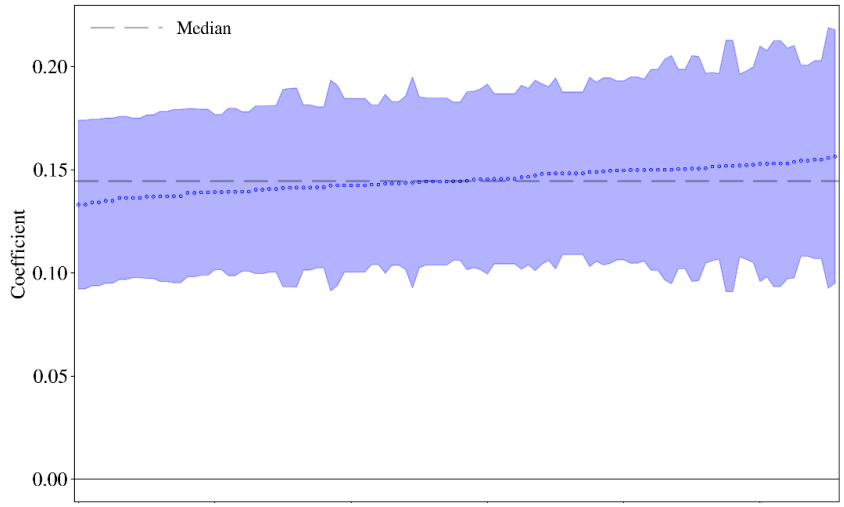


Figure 9c. Robustness Check Specification Curves (Measles 1 Vaccine)



Limitations

The results presented in this section leave us confident of overall trends – the NI-ABAE CCTs for RI Program clearly has a large, positive impact on most outcomes. However, these results are subject to several limitations. Subsequent sections explore three broad categories of limitations – contamination/spillovers, drivers of control coverage, and self-report data quality – in detail. In addition to these, there are several limitations whose implications are smaller but still worth noting here.

Missing Data due to Insecurity

Insecurity at endline prevented enumerators from surveying four catchment areas and a total of 95 settlements (including those in the four missing catchments). Our population estimates suggested

that around 267 eligible children lived in the missing areas (146 in treatment and 121 in control). We would expect coverage to differ in these missing areas relative to the rest of the sample since we found insecurity to be predictive of coverage during our baseline survey. We cannot be sure, however, how much it will differ and how it interacts with the treatment effect. However, given the relatively small number of missing values (267 relative to our overall sample-size of 5,141), we would not expect this bias to meaningfully alter our results.[84]

Uncertainty driven by Geographic Sampling

We are confident the segments we selected at endline are a random sample and have no reason to expect that they departed very far from the 25% of total land area we intended to select (in most cases). However, we cannot be certain. It is also possible that enumerators systematically failed to find inhabited structures or eligible children within surveyed segments, though our data-quality measures suggest this was unlikely.[85]

While we cannot verify the validity of our sample with 100% confidence (since there is no updated population register for the study area), we did compare key statistics from our sample to other sources to check for plausibility. First, we compared the total survey area population implied by our household survey (1,286,343) to that estimated by the Vaccine Tracking System's (VTS) population-prediction algorithm (1,266,369).[86] The relatively small difference between these numbers is easily within a range explainable by random sampling error of our survey and prediction error of the VTS algorithm. We then compared the birth rate implied by our survey (39 crude births per 1,000 people per year) to that estimated by the World Bank for Nigeria (38 per 1,000 per year) and Niger (46 per 1,000 per year).[87] Again, these numbers are well within a plausible range of each other. This left us confident that the numbers of people and children identified by our survey are reasonably accurate and not likely to lead to misleading conclusions.

External Validity

This evaluation estimated the impact of the NI-ABAE CCTs for RI Program on immunization coverage among 12 to 16 month olds following a ramp-up period of six months. It is possible that the NI-ABAE program was still refining operations when the study cohort would have been receiving their first

[84] Even making extreme assumptions like assigning 0 for all missing outcomes in treatment and 1 for all missing outcomes in control can only shift our impact estimate by a few percentage points given a relatively small number of missing outcomes. We conducted a back-of-the-envelope estimation to confirm this impression. It resulted in bounds of +/- 5 or 6 percentage points on the BCG vaccine impact estimate. These bounds should not be taken literally as they are rough estimates generated by implausible extreme assumptions. Rather, they should reassure GiveWell that bias from insecurity-driven missing data is unlikely to lead them to draw inaccurate conclusions.

[85] In the final weeks of endline, for example, field teams found a gradually decreasing number of eligible infants per clinic. This could be due to a number of factors since the order of surveying was non-random (i.e. the survey firm planned when to visit each catchment based on logistical, security, and other constraints). We are confident, however, that our in-field data quality measures (described in Appendix C) would have identified any systematic failure to find eligible infants who actually lived in these catchments.

[86] See http://vts.eocng.org/population/LGA?s=&l=&gender=MF&from=0&to=100 for output by administrative division. In practice, we used an associated layer in the open source Geographic Information System program QGIS to generate estimates for the land area we mapped for endline surveying.

[87] World Bank Crude Birth Rate Data: https://data.worldbank.org/indicator/SP.DYN.CBRT.IN

immunizations. This may underestimate NI-ABAE's effectiveness (or cost-effectiveness) over time. On the other hand, caregivers may initially be excited by the novelty of the incentives program and may, therefore, be more likely to respond to it. Over time, however, caregivers may be less swayed by the presence of incentives.

Relatedly, other external factors may change over time which can affect the effectiveness of incentives. These factors include increases in immunization coverage due to other programs, changes in social norms, or other factors –which may reduce the cost-effectiveness of incentives by reducing the unvaccinated population available to be incentivized. Similarly shifts in the economic status of households living in the program areas may change the intrinsic value of the incentive. At the time that this evaluation report is being written, the world is responding to the COVID-19 pandemic. This may have dramatic impacts on the health system in Nigeria as well as the population's trust in it. It is difficult to anticipate how this will affect NI-ABAE's impact: it may increase it as the incentive may be enough to keep caregivers coming for immunization or it may reduce it if the incentive is insufficient to overcome supply-side challenges or larger demand-side barriers such as fear of contracting COVID-19. Further, we do not know how long these challenges will last.

Need for Additional Research

This evaluation found that NI-ABAE's CCTs for RI Program successfully increased immunization coverage for routine childhood immunizations. Further research is needed to measure the long-term impacts of CCT programs for immunizations in Nigeria on immunization coverage as well as any effects (positive or negative) on health service delivery and health service utilization by community members. CCT programs often invoke questions related to sustainability. More research is needed on the optimal timing of and approach to phasing out CCT programs once desired coverage levels (or social norms in favor of immunization) are achieved.

Conclusion

Self-reported and clinic-recorded data show positive, consistent, and significant program impact. We have seen that the NI-ABAE CCTs for RI Program had a substantial positive impact on both primary and secondary vaccination outcomes according to both household and clinic data sources. This impact holds across a range of specifications and is consistently statistically significant. While impact is smaller for ever-vaccinated outcomes, it is still positive, suggesting that the NI-ABAE CCTs for RI Program motivates caregivers to start the RI schedule who otherwise would not have but that a larger part of the program's impact comes from motivating caregivers who start the RI schedule to continue and complete it.

To make best use of these results, GiveWell requires additional information to determine whether to adjust the results presented so far before using them in its cost-effectiveness model. The following section reports on those analyses and their results.

Supplementary Analysis for GiveWell's Cost-Effectiveness Model

GiveWell will use the results of this impact evaluation to update their cost-effectiveness analysis (CEA) for the NI-ABAE CCTs for RI Program, which is a key input to the decision of whether to recommend NI-ABAE as a 'top charity'. The self-reported impact and coverage estimates that have been reported thus far would be the default inputs into GiveWell's CEA. This section explores potential adjustments to these estimates that GiveWell might consider making to ensure the CEA's output is as close to the NI-ABAE CCTs for RI Program's true cost-effectiveness at scale as possible. Adjustments are warranted if there is reason to believe that the default inputs do not reflect what actually happened in the treatment or control group, that what happened in the control group does not reflect what would have happened in the absence of the program, or a combination of the two.

Below, we explore three possible reasons why an adjustment might be warranted: 1) contamination and spillovers, 2) drivers of control coverage, and 3) self-report data quality.

Contamination and Spillovers

Contamination and spillovers both refer to vaccinations that occur in control areas as a result of the program implemented in treatment areas. Contamination vaccinations happen as a result of children in control areas accessing the treatment. Spillover vaccinations result from indirect effects of the program, such as improvements in the state-wide vaccine supply, caregivers valuing vaccinations more because they heard that some clinics were paying for them, etc.

Both contamination and spillovers bias impact estimates by changing coverage in the control areas as a result of the program – decreasing its validity as a counterfactual. In our setting, we would generally expect contamination and spillovers to inflate control coverage (since we conclude that the program has a positive effect in treatment areas, we would expect it to affect control areas in the same direction, if it affects them at all). In the context of the CEA, evidence of contamination or spillovers would also mean the control group overestimates the proportion of the treatment group that are "always-vaccinators".[88]

Contamination

Children from control catchment areas may come to treatment clinics, deceive program staff about whether they are from the catchment area,[89] enroll in the program, and receive vaccinations and

[88] "Always-vaccinators" refers to children who would have been vaccinated with or without the existence of the program. Always vaccinators are important for cost effectiveness since they contribute to program costs but the program cannot have an impact on their vaccination status (by definition). Since the program pays incentives to all caregivers of eligible children who are vaccinated in treatment areas, it necessarily pays incentives for always vaccinators as well as for children who are vaccinated because of the program.

[89] Clinic staff ask caregivers where they live and are instructed not to refer anyone who lives outside of the catchment area to NI-ABAE. It is possible, however, that caregivers from outside the catchment area know about this rule and, therefore, claim to be from a location within the catchment area.

incentives. NI-ABAE has implemented and continues to refine several anti-fraud measures designed to prevent this from happening and clinic staff support these efforts. However, NI-ABAE admits that no anti-fraud system is perfect and expects that some out-of-catchment infants still receive vaccinations at their clinics.[90] We looked for evidence that these out-of-catchment infants include infants from control areas by checking CHCs for NI-ABAE stamps and by checking if caregivers had NI-ABAE cards (caregivers receive both stamps and cards whenever their child is enrolled in the program). We also asked caregivers if they had received cash incentives for vaccinations.

There is no substantial evidence of children in control areas receiving the incentive. We found a negligible number of control households that had either a CHC with a NI-ABAE stamp (n = 2, 0.1%) or where the caregiver reported receiving cash incentives for vaccination (n = 9, 0.4%). We did not find any control caregivers with a NI-ABAE card. Accordingly, by these three measures, there is no evidence that a meaningful number of infants from control areas were enrolled in the program.

Table 16. Indicators of NI-ABAE Enrollment in Treatment and Control

	Has a child health card with NI-ABAE stamp N (%)	Has an NI-ABAE card with at least one vaccine recorded N (%)	Caregiver reports receiving cash incentives for vaccination N (%)
Control (N = 2201)	2 (0.09%)	0 (0.00%)	9 (0.42%)
Treatment (N = 2955)	1968 (66.59%)	1839 (62.22%)	2273 (76.91%)

Spillovers

Spillovers also could have improved coverage indirectly in the control group. A potential source of spillovers was health officials directing campaigns to control clinics to compensate for the lack of the program. If this occurred, we would expect campaign frequency to have been higher in control than in treatment clinics. Alternatively, if NI-ABAE CCTs for RI Program staff were particularly effective advocates for vaccination within the health system, we might expect greater campaign frequency in treatment than control.

Neither hypothesis is borne out by data from our clinic staff survey. Treatment and control clinics reported virtually equal numbers of campaigns – each reporting an average of 2.8 campaigns between June 2018 and December 2019.

Spillovers may also have occurred if caregivers in the control group were under the impression that they would receive a vaccine at their catchment clinic or if they attempted and failed to enroll at a

[90] Out-of-catchment children vaccinated in treatment clinics could be from control clinics or non-study clinics. This section refers only to control clinics, since we did not collect household survey data from non-study clinics. NI-ABAE's cost and monitoring data would likely be affected by out-of-catchment children from non-study areas. We also expect a much larger proportion of any out-of-catchment vaccinations in treatment clinics to be from non-study clinics as opposed to control clinics. This is because of the 17km buffer we applied to selecting study clinics; there are often several non-study clinics that are meaningfully closer to a treatment clinic than the closest control clinic.

treatment clinic but received a vaccination in the process.[91] Unfortunately, we cannot test for this directly. We can, however, examine whether study conditions made various spillover mechanisms likely or whether evidence from other sources suggests that they occurred.

The evaluation's primary defense against control caregivers traveling to treatment clinics was distance. Control surveys occurred an average of 20 kilometers (SD = 6km) from the nearest treatment clinic - as we would expect given the 17km buffer built into clinic selection. By contrast, the average control survey took place 2.4km away (SD = 3.4) from the nearest control clinic. This is compared to the 5km average distance traveled to the clinic reported by caregivers prior to baseline.[92]

In addition, NI-ABAE limited its community sensitization about the incentive within treatment areas. To test whether distance worked to limit knowledge, we checked for awareness of incentives given at other clinics among control caregivers at endline. We found that 13% [95% CI: 11.6%, 14.4%] of control caregivers reported knowledge of cash incentives for vaccination at other clinics (besides their own). For spillovers to occur, these caregivers who were aware of incentives at other clinics would have had to expect that they would also be able to receive them – either because they were also available at their own catchment clinic or because they could travel to the treatment clinic and obtain them. This expectation would then have had to induce them to obtain vaccinations they would not have obtained otherwise. It is unlikely that all 13% of control caregivers who were aware of incentives also met the other conditions necessary to induce spillovers. Accordingly, while we cannot say definitively how often this occurred, any spillover effect via this channel was likely small.

One additional potential spillover effect is from the mere presence of the NI-ABAE CCTs for RI Program in the region. This could have increased overall interest in vaccination both in treatment and control catchments or led to improvements in vaccine supply at the LGA level or higher, also affecting both treatment and control. Such general spillovers would lead overall coverage in both treatment and control to be higher than it would be absent the program. This would bias the estimate of the program's impact in a scenario where the program is new (i.e., if the NI-ABAE CCTs for RI Program scales to a new state). However, NI-ABAE's scale-up plans include expanding to additional facilities within the study states – therefore, these new facilities will also exist in areas affected by the current presence of the NI-ABAE CCTs for RI Program. Accordingly, while we cannot rule out (or in) any of these generalized spillovers, it is unlikely that they substantially altered the validity of the impact estimate for GiveWell's purposes.

Conclusion

We found no substantial evidence of contamination or spillovers that would require an adjustment before using study results in the CEA. There is no evidence that control caregivers accessed the program in meaningful numbers nor does it appear likely that control coverage estimates are

[91] Children who were turned away for the incentive due to out-of-catchment residence were still offered vaccinations for which they were eligible at that visit. If large numbers of control caregivers attempted to obtain incentives and failed once they reached the clinic, then the program could still have increased vaccination coverage in control areas despite low control-caregiver enrollment. Importantly, this only induces bias in the impact estimate if these caregivers would not have had their children vaccinated otherwise.

[92] See Pre-Analysis Plan, footnote 69

meaningfully affected by proximity to treatment clinics or knowledge of the program being implemented there. While we cannot rule out a set of more generalized spillovers at the regional level, these are less likely to be of decision-relevant concern to GiveWell, especially if NI-ABAE scales up in the same states.

Drivers of Control Group Coverage

Control coverage has increased meaningfully since baseline, and the difference is not entirely attributable to the inclusion of Jigawa State or changes in measurement methods. Campaigns appeared to contribute a portion (but not all) of the increase for the Measles vaccine; this was not the case for BCG or Penta vaccines. In addition, it appears that program impact on Measles vaccine coverage was likely driven downward by campaigns. If GiveWell expects campaigns to remain relatively irregular in the scale-up context, they should adjust the impact estimate upward for Measles vaccine.

Our analysis suggests that control coverage for Measles 1 vaccine excluding campaigns is likely lower than the unadjusted estimate. Our best guess is that that the self-reported RI-only coverage for Measles 1 vaccine in control is 48% (11 percentage points lower than the actual self-reported rate we found). If GiveWell expects that NI-ABAE does not pay incentives to a large proportion of children vaccinated only through campaigns, then they should adjust control coverage downward before using it as an estimate of the proportion of children who enroll in the program but would have been vaccinated anyway in its absence.[93] Our analysis also suggests that program impact on Measles vaccine would have been roughly 22 percentage points in the absence of a campaign. Moreover, our data suggests that large-scale campaigns that meaningfully affect coverage are relatively infrequent. Accordingly, the scale-up context for the NI-ABAE CCTs for RI Program is likely to be primarily characterized by time periods without large-scale campaigns. Accordingly, in the interest of making our impact estimate more generalizable to the scale-up context, we recommend GiveWell use 22 percentage points as the self-reported impact estimate for Measles vaccine.[94]

[93] NI-ABAE staff are uncertain of the proportion of campaign-delivered vaccinations that ultimately lead to caregivers receiving incentives. In most cases, children who would have been eligible for the incentive had they received the vaccination via RI activities but received it through a campaign are eligible to receive the incentive if they provide proof of eligibility and vaccination. However, since campaigns often vaccinate in communities, caregivers would usually have to make an extra trip to the clinic or to the clinic outreach site to receive the incentive. We used vaccination card data to investigate how commonly this seemed to happen. In cases where caregivers did receive the incentive for campaign vaccinations, the child should have an entry for the same Measles vaccine on two different cards: (1) a campaign card, and (2) the CHC (as the CHC would have been filled in retrospectively when the caregiver visited the clinic to obtain the incentive for the campaign vaccine). As we describe in more detail below, we found that only around 12% of children in treatment had a Measles vaccine recorded on both a campaign card and a CHC. Furthermore, we compared the dates on these two cards, and found that in treatment the month/year information on the two cards was only the same for 6% of children with two cards (less than 1% of all children in the treatment group). This leads us to believe that it was not common for caregivers of children in the treatment group who received their first Measles vaccine during a campaign to subsequently claim the incentive.

[94] Strictly speaking, even rare campaigns will vaccinate some children who would not be vaccinated in the absence of the program, lowering the program's potential impact, even at scale. However, not adjusting the impact estimate at all implies assuming that the scale-up context will be like endline – in other words, it will follow a major Measles vaccine campaign. Given that only one campaign of this scale occurred in the 19 months covered by the clinic staff survey, assuming that the average cohort of eligible children is not affected by a major campaign is a better approximation of the truth than is assuming that the average cohort is fully covered by a major campaign. Moreover, RI Measles vaccinations are likely much more timely

Change in Vaccination Coverage Since Baseline

Across the study area, **coverage in the control group was substantially higher at endline than at baseline.**[95] This was true for each of the primary study vaccines, with the largest difference for the Measles vaccine: 17.8% of children at baseline versus 57.2% of children at endline had reportedly received Measles 1 vaccine (Table 16). We did not expect a change of this magnitude. Because particular drivers of control coverage (especially the role of periodic vaccination campaigns) may have implications for GiveWell's cost-effectiveness analysis, we conducted a careful examination of control coverage.

Table 16. Control Group Vaccination Coverage from Baseline to Endline (%)

Outcome	Baseline Coverage	Endline Coverage	Endline Coverage (Katsina and Zamfara States only)
Reported BCG vaccine	0.27 (0.22, 0.32)	0.63 (0.57, 0.69)	0.53 (0.46, 0.60)
Reported Penta 1 vaccine	0.22 (0.17, 0.28)	0.54 (0.48, 0.61)	0.42 (0.36, 0.49)
Reported Measles 1 vaccine	0.18 (0.14, 0.21)	0.59 (0.54, 0.64)	0.54 (0.48, 0.61)
Has a BCG vaccine scar	0.19 (0.14, 0.23)	0.41 (0.37, 0.46)	0.37 (0.31, 0.44)

Note 1: Coverage estimates are reported as percentages (%) of all respondents. 95% CIs are reported in parentheses. These coverage estimates are not adjusted for any covariates.

Note 2: Baseline coverage rates are calculated by reweighting the baseline sample using endline data. Hence, baseline coverage rates shown are slightly different from the rates included in the Baseline Report.

Evaluation Differences Between Baseline and Endline

Changes between baseline and endline likely reflect actual differences in coverage. Changes in the evaluation design and survey methodology explain only part of the observed increase in coverage.

The inclusion of Jigawa State – the third study state that was added after baseline – explains some of the increase between the two survey waves but not all. The third column of Table 16 reports the coverage estimate in control areas after removing Jigawa State from the endline sample. The difference between the two surveys is smaller after removing Jigawa State but still substantial; coverage for each vaccine at endline is still approximately twice what it was at baseline.

than campaign Measles vaccinations - again because campaigns are infrequent. There is - therefore - reason to favor vaccinations delivered via RI, as we do implicitly by using RI impact. This case is stronger when we recall that we found the NI-ABAE CCTs for RI Program to substantially increase the timeliness of Measles vaccinations.

[95] Baseline used a sampling approach that resulted in an approximately equal number of children across catchment areas, whereas endline used a sampling approach that resulted in an approximately population proportionate sample of children across catchment areas. For this section, we reweighted the baseline sample using the endline data to ensure that children from a given catchment area were comparably weighted at baseline and endline.

Another possible explanation for the increase in coverage is differential measurement error between the baseline and endline surveys. Changes in the questionnaire or enumerator technique could have led to increased recording of vaccinations via the survey even if true vaccination coverage had not changed.[96] This could result from either improved recall or increased social desirability, or both. This hypothesis could explain why we do not see similar increases in clinic tally sheets, which record relatively stable control-group vaccination volumes between baseline and endline (see Appendix I). However, we also saw a substantial increase in the percentage of children with a BCG vaccine scar in Katsina State and Zamfara State from 19% at baseline to 37% at endline. This change is roughly proportionate to the change in self-reported BCG vaccine coverage in those states (27% to 53%), making it unlikely that changes in questionnaire wording or enumerator technique explain a large part of the baseline-to-endline increase in coverage.[97]

If not explained by changes in methodology, then the change in measured control coverage between baseline and endline likely reflects a change in true vaccination coverage, which warrants further investigation.

Vaccine Delivery – Location

More Measles vaccinations occurred outside of clinics at endline than baseline.

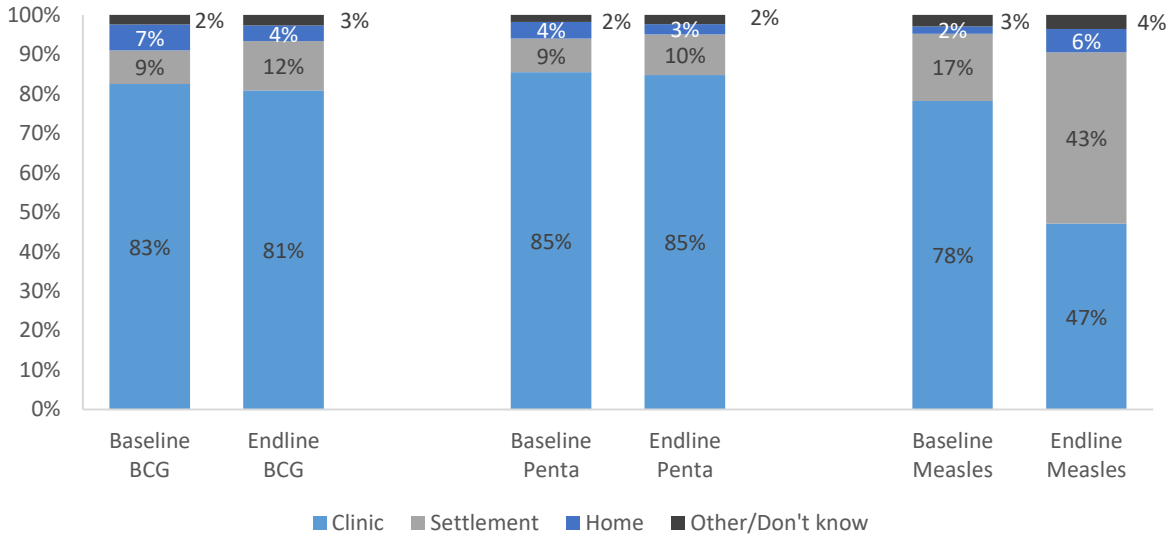
We asked every caregiver who reported a vaccination to identify the location at which it occurred.

Figure 10 below decomposes control coverage at baseline and endline by location. For both baseline and endline, we found that vaccinations were most often given at the clinic, followed by the settlement. Only a small share of vaccinations was received at home. At both baseline and endline, the Measles vaccine was more likely to have been received in the settlement than the BCG or the Penta vaccines.

[96] At endline, in addition to using body part and age, we also structured the interview according to vaccination occasions (i.e. clinic visits or in-settlement vaccination activities), with the aim of improving caregiver recall.

[97] The share of self-reported Measles vaccinations corroborated by CHC entries was higher at endline. This can be explained by a variety of factors, including changes in card-retention, and is, therefore, not definitive. However, it does not provide evidence of increased false reporting in control at endline relative to baseline.

Figure 10. Control Group Vaccination by Self-Reported Location at Baseline and Endline



Note 1: The figure excludes Jigawa State since we do not have baseline numbers for that state.

It is clear from Figure 10 that there was an increase from baseline to endline in the percentage of vaccines reported as received in the settlement. This increase was modest for BCG and Penta vaccines with a 3 percentage point and 1 percentage point increase, respectively. The increase for the Measles vaccine was substantially larger: 43% of Measles vaccinations were reportedly received in the settlement at endline compared to 17% at baseline (a 26 percentage-point increase). There was also an increase in the percentage of Measles vaccines received at home.

Overall, there was little baseline-to-endline change in the percentage of BCG and Penta vaccinations reported as received outside of the clinic, while there was a large increase in the percentage of Measles vaccinations reported as received outside of the clinic.

Importantly, vaccines delivered outside clinics may be delivered through either RI outreach or vaccination campaigns. RI outreach refers to clinic staff visits to settlements to provide vaccinations – either at a central location or door-to-door. Campaigns refer to intensive, short duration vaccination efforts applying additional staff and resources beyond those engaged in routine healthcare and often targeting a small number of priority vaccines. Similar to RI outreach, campaigns often deliver vaccinations directly in communities – also either door-to-door or at central, public locations.[98] Campaigns generally involve support from international organizations such as the World Health Organization (WHO) and the United Nations Children's Fund (UNICEF) as well as from the Nigerian federal health system.

[98] Some campaigns do occur at the health facilities themselves.

Vaccine Delivery – Activity

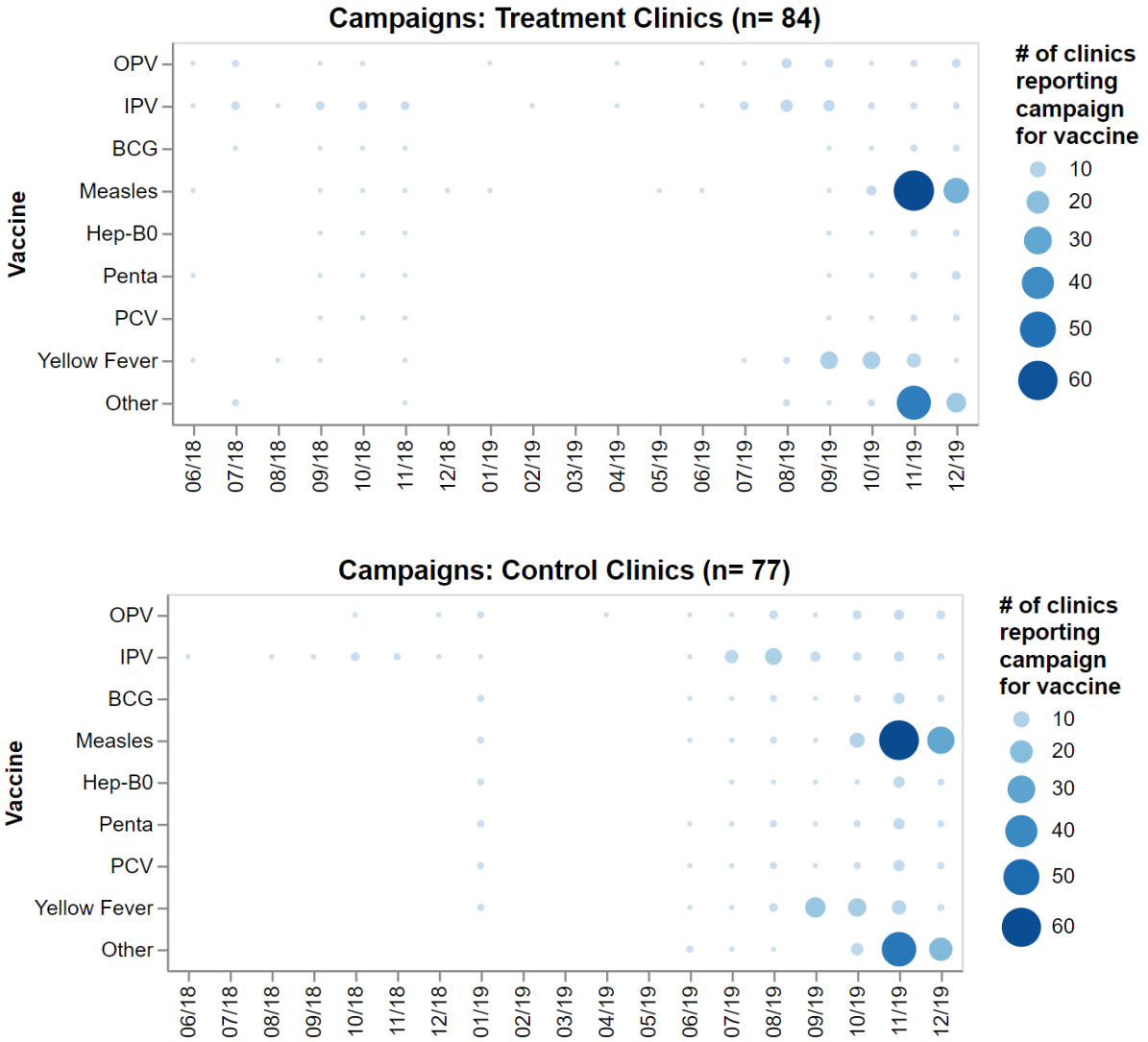
The source of vaccination is relevant for GiveWell's cost-effectiveness estimate for two reasons. Firstly, NI-ABAE intends to (and generally does) pay incentives for vaccinations distributed through RI outreach but likely pays incentives for a smaller share of vaccines delivered via campaigns.[99] This means that vaccinations that occur in the control group only because of campaigns are not always vaccinations for which New Incentives would pay incentives. Therefore, GiveWell may wish to account for the portion of vaccinations of this type when using control-group coverage in the CEA as an estimate of incentives paid to caregivers whose children would be vaccinated in the absence of the program. Secondly, campaigns may reduce the impact of the program by reaching children in control areas who would not be vaccinated in the absence of a campaign but would be induced to be vaccinated by the NI-ABAE CCTs for RI Program. Since large campaigns are relatively irregular, a control group coverage that excludes those vaccinations that only occur via campaigns is a better counterfactual for estimating program impact.

We found evidence that a moderate proportion of Measles vaccinations reported in control were delivered via campaigns. Campaigns did not appear to account for a meaningful proportion of BCG and Penta vaccinations. We also found evidence that program impact on Measles vaccine was somewhat higher when excluding campaigns.

There was a large Measles vaccine campaign across the study area immediately prior to endline data collection. To explore the extent of campaign activity in the evaluation area, we asked clinic staff about the frequency of campaigns in the period leading up to the endline survey in the clinic staff survey. Both treatment and control clinics reported that an average of 2.8 vaccination campaigns [95% CI: 2.6, 3.0] occurred in each catchment area during the 19 months up to and including the first month of the endline survey. Figure 11 illustrates the incidence of specific vaccines' inclusion in campaigns as reported by clinic staff. For most of the study period, clinic staff reported sporadic, small-scale campaigns targeting a few clinics and vaccines at a time. Just prior to endline, however, nearly all clinics reported a major campaign targeting Measles vaccine and (usually) Meningitis vaccine (captured under "other"). Many clinics also reported Yellow Fever vaccine and Polio vaccine (including fractional IPV, fIPV) campaigns.

[99] NI-ABAE staff work hand-in-hand with RI staff and activities and NI-ABAE CCTs for RI Program incentive eligibility is structured to be similar to eligibility for RI vaccinations. Because campaigns are less frequent and have different eligibility criteria, NI-ABAE staff are not always able to integrate enrollment activities with campaign vaccination activities. For example, for the large Measles vaccine campaign in November 2019, NI-ABAE staff – in discussions with campaign officials – decided not to accompany campaign staff to communities as the campaign hoped to vaccinate children whose ages would not make them eligible for NI-ABAE CCTs for RI Program enrollment and who, therefore, might resent the program for enrolling others at the same time.

Figure 11. Campaign Incidence by Vaccine (June 2018-December 2019)



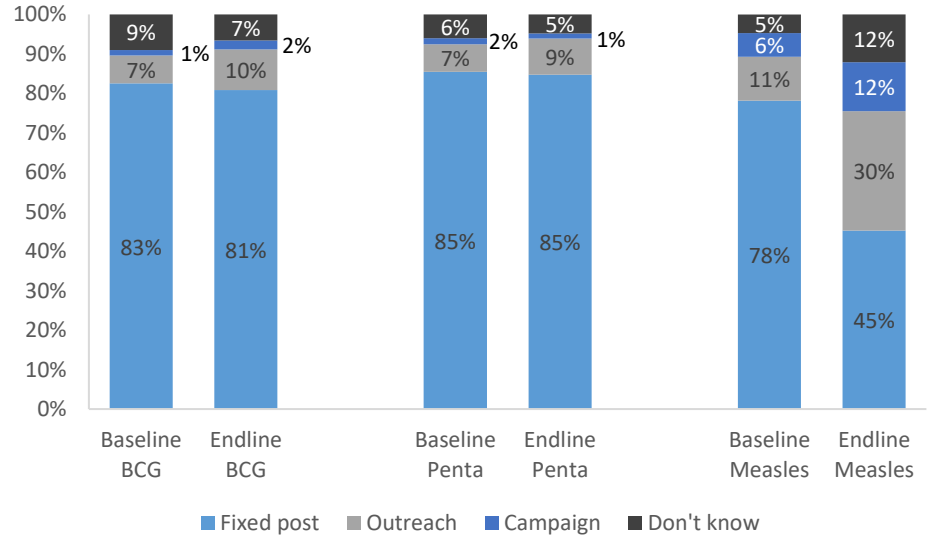
In addition, we asked clinic staff to report the incidence of outreach in their clinics, along with the vaccines distributed in outreach activities. **There was an increase in outreach days between baseline and endline.** At endline, staff in control clinics reported an average of 3.5 outreach days in their clinics per month [95% CI: 3.2, 3.8], an additional 1.5 outreach days per month compared to baseline (95% CI: 1.2, 1.9, p-value < 0.01). All but one clinic reported distributing Measles vaccines through outreach, while all but four clinics reported distributing BCG and Penta vaccines during outreach.

We also analyzed household survey data on the activity through which children received vaccinations (fixed post, outreach, campaign, or other/don't know). When caregivers reported that their child received a given vaccine outside a clinic, they were asked whether the vaccination was distributed as part of a campaign or an RI outreach activity.[100]

[100] Examples of the questions asked to elicit this report can be found in Appendix K.

Despite the large Measles vaccine campaign shortly before endline, the majority of caregivers of children in the control areas who received Measles vaccinations outside of the clinic reported having received Measles vaccine as part of clinic outreach, as seen in Figure 12. There were meaningful increases in both reported outreach and campaign vaccinations for Measles between baseline and endline. Based on caregivers' self-report at baseline, 11% of Measles vaccines were administered during outreach (the highest share of all vaccines), and 6% of Measles vaccines were administered during campaigns. At endline, 30% of Measles vaccines in control were received during outreach and 12% during campaign activities.

Figure 12. Control-Group Vaccinations by Self-Reported Activity



Note 1: The figure excludes Jigawa State since we do not have baseline numbers for that state.

Note 2: "Don't know" responses include the following scenarios: (1) The caregiver did not know at which location the vaccine was received, or reported that the vaccine was received at another location (not at a clinic, at home, or in the settlement). In this case, the questionnaire did not ask the caregiver whether the vaccine received was a campaign vaccine or not. (2) The caregiver knew at which location the vaccine was received but said that they "did not know" whether the vaccine received was a campaign vaccine or not.

Importantly, respondents may have confused RI outreach and campaigns since both take place outside of clinics[101] and clinic staff often participate in both.[102] The share of outreach vaccinations at endline had also increased substantially more for Measles vaccine than for BCG vaccine or Penta vaccine, a surprising result that could have been driven by caregiver misreports. To explore this possibility further, we analyzed what share of self-reported Measles vaccinations were recorded on CHCs (which are completed as part of RI activities), and on campaign cards. In total, 53% of all self-reported Measles vaccines in the control group were corroborated by an entry on a vaccination card

[101] Some clinics hold campaigns at the health facility, but this is less common than those that occur in settlements.
[102] See Appendix L for additional exploration of the reliability of self-reported vaccination location and activity data.

(CHC or campaign card).[103] If we only consider CHCs, the share was 40%. Hence, for 13% of self-reported Measles vaccinations the child only had a campaign card record (making it likely that the vaccination occurred during a campaign). As we discuss in more detail in the next section, this suggests a non-negligible role for campaigns in driving Measles vaccine coverage. However, the fact that a large majority of children with a card record of the Measles vaccine had a record on their CHC suggests that increased RI vaccines (likely via outreach as shown in Figure 12) were also important for driving Measles vaccine coverage increases.

Estimating Coverage and Impact for RI Vaccinations

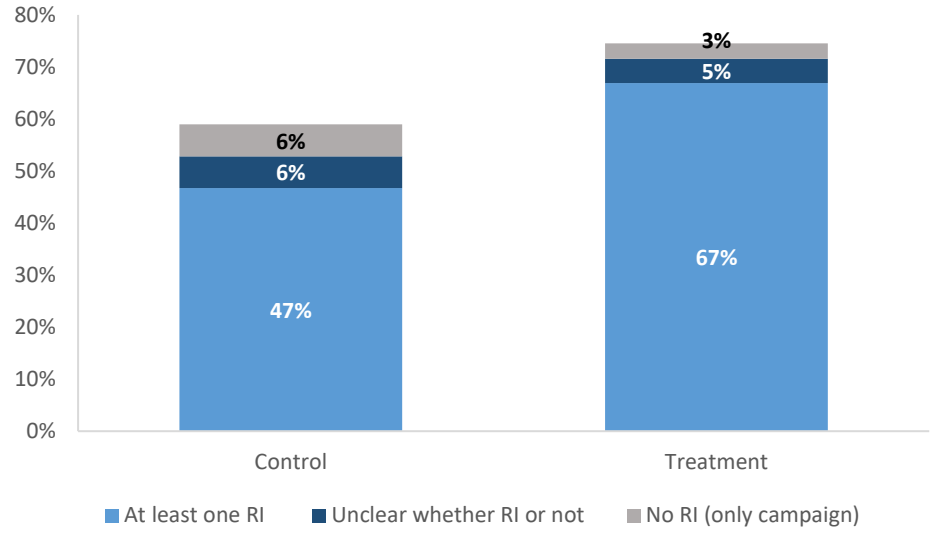
We explored several approaches to estimating the percentage of vaccinations that were delivered through RI (fixed post at clinics or outreach) versus campaigns. While uncertainty remains, these approaches provide a clearer idea of what Measles vaccine coverage would have been in treatment and control if there had not been a large-scale Measles vaccination campaign in November and December 2019.

As discussed in the previous sections, caregivers were asked to report whether they thought that a given vaccination had been delivered as part of RI or campaign activities. Figure 13 breaks Measles vaccine coverage in treatment and control into the following categories based on this self-reported data:[104]

- **At least one RI**: The caregiver reported that the child received Measles 1 vaccination at a clinic (fixed post) as part of RI activities or during RI outreach.
- **No RI (only campaign)**: The caregiver reported that the child only received a Measles vaccination during a campaign. No Measles vaccinations were reportedly received through RI activities.[105]
- **Unclear**: The caregiver reported receipt of Measles 1 vaccination, however, based on the self-reported information, we cannot tell it was delivered during RI or campaign activities.

[103] As a comparison: for the BCG vaccine, 59% of all self-reported vaccines in the control group were corroborated by an entry on a vaccination card (CHC or campaign card).
[104] Detailed RI coverage rates and resulting impact estimates for each of the primary vaccines (BCG vaccine, Penta vaccine, Measles vaccine) are shown in Tables 17-19 below.
[105] During fieldwork, we received reports that - in some cases - the recent Measles vaccine campaign had delivered vaccinations at clinics. At that point, we updated the questionnaire section on Measles vaccine. Originally, it had only asked caregivers who received the Measles vaccine outside of the clinic (e.g. in the settlement or at home) whether they had received it through RI outreach or campaign activities. After the questionnaire change, caregivers were also asked this question if they reported that their child had received a Measles vaccination at the clinic. Only 3.5% of all caregivers in the sample who reported Measles 1 vaccination at the clinic and where subsequently asked whether it had been a campaign or RI vaccination responded that it had been a campaign vaccination.

Figure 13. Self-Reported RI-Only Coverage for Having Received Measles 1 Vaccine



Note 1: "Unclear" responses include the following scenarios: (1) The caregiver did not know at which location the vaccine was received, or reported that the vaccine was received at another location (not at a clinic, at home, or in the settlement). In this case, the questionnaire did not ask the caregiver whether the vaccine received was an RI or a campaign vaccine. (2) The caregiver knew at which location the vaccine was received but said that they "did not know" whether the vaccine received was a campaign vaccine or not.

If we define RI Measles vaccines as those that were reported as RI (and exclude vaccines categorized as “unclear”), then RI Measles vaccine coverage was 47% in the control group and 67% in the treatment group. If we include the “unclear” vaccines as RI vaccinations, RI Measles vaccine coverage was 53% in the control group and 72% in the treatment group.

Due to the likelihood that caregivers misreported vaccination activities, we also used vaccination card data as an additional data source. Enumerators asked caregivers for all cards with health information on them and recorded the vaccinations shown on these cards. We used the card data to refine our estimates of RI-only coverage by recoding cases in which cards suggested caregivers had misreported vaccination activity. We started with self-reported activity and defaulted to the self-report in the absence of contradictory card data. However, if the only available card data suggested a different activity than that reported by the caregiver, we recoded the vaccination accordingly. Figure 14 illustrates this approach.

Figure 14. Re-Classifying Vaccinations as RI and Campaign Based on Card Data

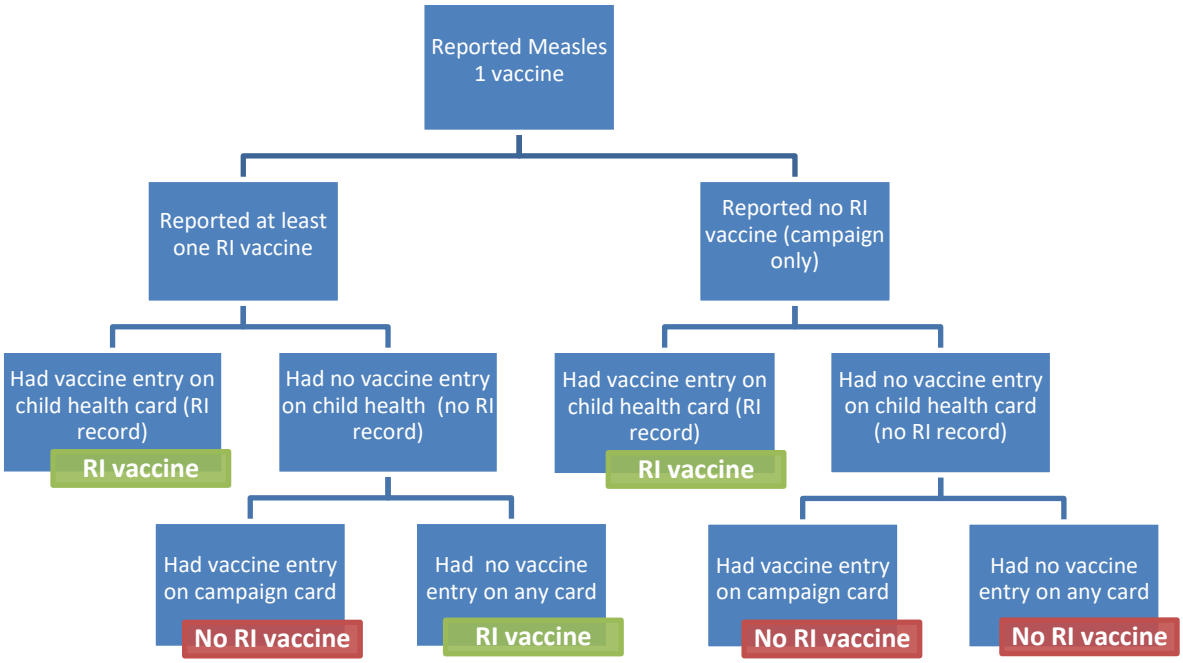
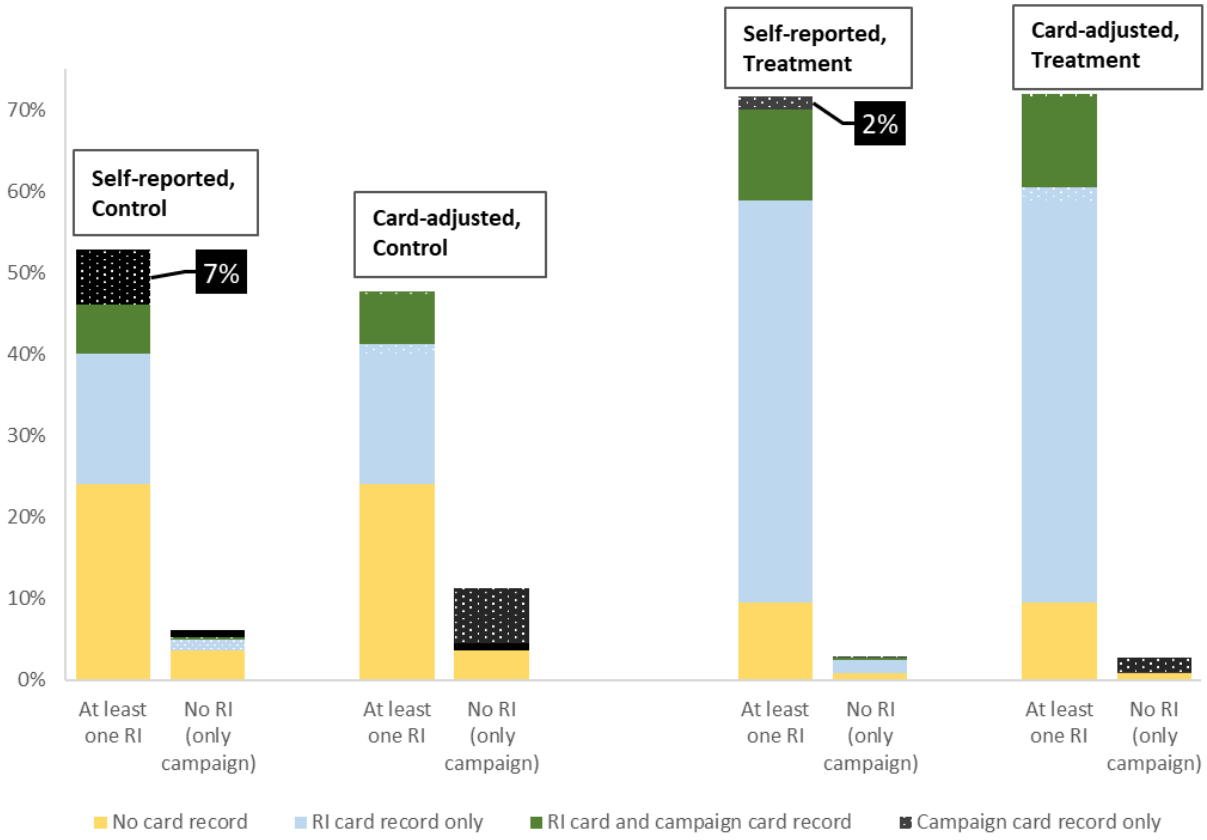


Figure 15 shows results of implementing the previously described approach in terms of coverage. The left-most bar for control and treatment shows the self-reported RI-only coverage (including "unclear" responses) broken down based on the card information available on these vaccinations. We see that in control roughly half of all self-reported RI vaccines are not backed up by any card information (i.e. enumerators did not find a card at all or the cards they did find did not have any Measles vaccinations recorded) (yellow bars).

Figure 15. Using Self-Reported and Card-Adjusted Data to Calculate RI-Only Coverage



In treatment, the vast majority of RI Measles vaccines are confirmed by a card record. In most cases, this record is an RI vaccination card (i.e. a CHC).[106] The black bars at the top of each graph illustrate those children that only had a campaign card despite their caregiver reporting that they had received the Measles vaccine during RI activities. This share was 7% in control, and 2% in treatment. The card-adjusted measures reclassify these vaccinations as campaign based on the card.

The second bar to the right of each graph shows the card breakdown for those children for whom caregivers reported only a non-RI Measles vaccine (i.e. a campaign vaccine). In control, most of these vaccines are not backed up by any card record. In both treatment and control, we see that a small share of these children did actually have an RI vaccination card. The card-adjusted measures reclassify these vaccinations as RI.[107]

[106] 6% of children in control, and 11% of children in treatment, had both an RI vaccination card (i.e. CHC) and a campaign card for Measles vaccine (green bar), suggesting they received two Measles vaccinations (one via RI and one via campaign). We checked self-reported data and found these numbers to be reasonable. 7% of caregivers in control, and 12% of caregivers in treatment, reported that their child had received two Measles vaccines.

[107] We investigated one possible limitation to our re-classification method. We were initially concerned that a large number of Measles campaign vaccinations might have been recorded on CHCs instead of (or in addition to) campaign cards. If this had been the case, then it would have been inaccurate to use CHC information to recode whether a Measles vaccines was received during RI activities. We investigated this potential limitation by analyzing the distribution of Measles vaccines on CHCs over time using vaccination dates recorded on CHCs. Our analysis showed that there was no spike in Measles vaccinations recorded on CHCs in November or December 2019 (the time period of the large-scale Measles

After applying the card adjustments, coverage in control drops to 48%, and coverage in treatment remains largely constant at 72%.

Table 17 shows treatment and control coverage as well as the impact estimate derived from running our main specification for the different RI-only variables discussed above. The loose RI definitions count "unclear" vaccines as RI vaccines. The strict definitions count "unclear" vaccines as non-RI vaccines. The impact estimate for RI-only Measles vaccines increases to up to 25 percentage points depending on the definition applied.

Table 17. RI Coverage and Impact Estimates for Measles Vaccinations (Using Various Measures)

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Received Measles 1 vaccine (main)	0.59 (0.54, 0.64)	0.75 (0.70, 0.79)	0.14 (0.10, 0.18)	**< 0.001**
Received at least one RI Measles vaccine loose (self-report)	0.53 (0.48, 0.57)	0.72 (0.67, 0.76)	0.17 (0.14, 0.21)	**< 0.001**
Received at least one RI Measles vaccine strict (self-report)	0.47 (0.42, 0.51)	0.67 (0.62, 0.72)	0.19 (0.15, 0.23)	**< 0.001**
Received at least one RI Measles vaccine loose (self-report and card correction)	0.48 (0.43, 0.52)	0.72 (0.68, 0.76)	0.22 (0.18, 0.26)	**< 0.001**
Received at least one RI Measles vaccine strict (self-report and card correction)	0.44 (0.40, 0.49)	0.71 (0.66, 0.75)	0.25 (0.20, 0.29)	**< 0.001**

Note 1: The 'loose definition' counts all vaccines for which it was unclear whether they were an RI (or a campaign vaccine) as RI vaccines. The 'strict definition' only includes vaccines that were reported to have been RI vaccines (all unclear vaccines are assumed to have been campaign vaccines).

Note 2: The treatment and control means are weighted for sampling probabilities but not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for the covariates listed in the methods section. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient, though both results show substantial, positive impact.

Tables 18 and 19 show the RI-coverage and impact results for the BCG and Penta vaccines. BCG vaccine and Penta vaccine coverage remain basically unchanged when applying the different adjustments. As a result, the impact estimate also remains largely unchanged.

vaccine campaign). In treatment, only around 12% of Measles vaccinations recorded on CHCs were received in November/December 2019. In control, it was even less (only around 10%). This makes us confident that the vast majority of Measles vaccinations recorded on CHCs are RI vaccinations and not campaign vaccinations.

Table 18. RI Coverage and Impact Estimates for BCG Vaccinations (Using Various Measures)

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Received BCG vaccine (main)	0.63 (0.57, 0.69)	0.83 (0.79, 0.86)	0.16 (0.12, 0.21)	**< 0.001**
Received RI BCG vaccine loose (self-report)	0.62 (0.56, 0.68)	0.81 (0.78, 0.85)	0.16 (0.12, 0.21)	**< 0.001**
Received RI BCG vaccine strict (self-report)	0.58 (0.52, 0.65)	0.79 (0.75, 0.83)	0.18 (0.13, 0.22)	**< 0.001**
Received RI BCG vaccine loose (self-report and card correction)	0.62 (0.56, 0.68)	0.82 (0.78, 0.86)	0.17 (0.12, 0.21)	**< 0.001**
Received RI BCG vaccine strict (self-report and card correction)	0.61 (0.55, 0.67)	0.81 (0.78, 0.85)	0.17 (0.13, 0.22)	**< 0.001**

Note 1: The 'loose definition' counts all vaccines for which it was unclear whether they were an RI (or a campaign vaccine) as RI vaccines. The 'strict definition' only includes vaccines that were reported to have been RI vaccines (all unclear vaccines are assumed to have been campaign vaccines).

Note 2: The treatment and control means are weighted for sampling probabilities but not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for the covariates listed in the methods section. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient, though both results show substantial, positive impact.

Table 19. RI Coverage and Impact Estimates for Penta Vaccinations (Using Various Measures)

Outcome	Mean (95% CI)		Difference (95% CI)	
	Control	Treatment	Adjusted OLS Results	P-value
Received Penta 1 vaccine (main)	0.54 (0.48, 0.61)	0.78 (0.73, 0.82)	0.21 (0.16, 0.26)	**< 0.001**
Received at least one RI Penta vaccine loose (self-report)	0.53 (0.47, 0.60)	0.77 (0.73, 0.81)	0.21 (0.16, 0.26)	**< 0.001**
Received at least one RI Penta vaccine strict (self-report)	0.52 (0.45, 0.58)	0.76 (0.71, 0.80)	0.21 (0.17, 0.26)	**< 0.001**
Received at least one RI Penta vaccine loose (self-report and card correction)	0.54 (0.47, 0.60)	0.77 (0.73, 0.81)	0.21 (0.16, 0.25)	**< 0.001**
Received at least one RI Penta vaccine strict (self-report and card correction)	0.53 (0.47, 0.60)	0.77 (0.72, 0.81)	0.21 (0.16, 0.26)	**< 0.001**

Note 1: The 'loose definition' counts all vaccines for which it was unclear whether they were an RI (or a campaign vaccine) as RI vaccines. The 'strict definition' only includes vaccines that were reported to have been RI vaccines (all unclear vaccines are assumed to have been campaign vaccines).

Note 2: The treatment and control means are weighted for sampling probabilities but not adjusted for covariates. Adjusted OLS Results are the treatment coefficient from a multivariate regression controlling for the covariates listed in the methods section. Accordingly, the difference between treatment and control means is not equal to the treatment coefficient, though both results show substantial, positive impact.

Given that the reported campaigns did not usually target BCG or Penta vaccines, these results are expected. The RI Measles vaccine impact estimate is higher than the RI BCG vaccine impact estimate, and comparable to the Penta vaccine impact estimate. This is an important result as it suggests that campaigns explain why the unadjusted impact estimate is lower for Measles vaccine than for BCG vaccine and Penta vaccine. RI control group coverage for Measles vaccine is also comparable to control coverage for Penta vaccine, if not a bit lower depending on the definition used.

There remains some uncertainty about true RI Measles vaccine coverage and impact. Caregivers' recall of the activity via which their child received the vaccination is imperfect. Furthermore, vaccinations cards were missing for many children (especially in control) implying that the card-based corrections are imperfect, as well.

Conclusion: Adjusting for the CEA

The increase in coverage in control areas since baseline is not fully explained by methodological changes or by campaigns. Coverage for all three primary outcomes appears to have increased via routine immunization activities. For BCG and Penta vaccines, campaigns do not account for any meaningful proportion of vaccinations in control areas. Accordingly, we would assume that nearly all of the vaccinations recorded in the control group for BCG vaccine and Penta vaccine represent always-vaccinators who receive vaccinations through RI activities. If we assume that the NI-ABAE CCTs for RI Program pays incentives for all RI-delivered vaccinations, then control coverage is a reasonable proxy for the number of incentives paid to always-vaccinators for BCG and Penta vaccines.

For the Measles vaccine, we expect that control coverage included a non-trivial proportion of campaign-vaccinators to whom the program may not always pay incentives. There remains uncertainty about the precise proportion, and our data only allows us to generate approximations. However, our best-guess is that RI Measles vaccine coverage in control is about 48% (or 11 percentage points lower than unadjusted self-reports coverage). This value falls in the middle of our various RI-only coverage estimates, which range from 44% to 53% depending on the adjustments applied.[108] If GiveWell assumes that a large proportion of campaign-delivered vaccinations do not lead to program enrollment, then GiveWell should make a downward adjustment to control Measles vaccine coverage before using it as an estimate of enrolled always-vaccinators.[109]

Similarly, there is evidence that the impact estimate for the Measles vaccine is higher when restricted to vaccinations delivered via RI activities. Given that campaigns appear to occur infrequently (but one did occur just before endline), it is likely that the program would have a higher impact on Measles vaccine in non-post-campaign periods. We recommend that GiveWell considers an upward adjustment to the Measles vaccine impact estimate. Again, our best-guess would constitute the

[108] In addition to its falling in the middle of this range, this value is also that produced by assuming that unclear reports are RI vaccinations and applying card-based adjustments. Most vaccinations in the full sample are RI, so it is likely that most unclear reports are as well. We also expect that cards, when present, are an accurate source for vaccination activity (i.e. a child with Measles vaccine on an RI card probably got Measles vaccine via RI). These factors lead us to choose this estimate as our best-guess.

[109] Card data suggests that relatively few campaign vaccinations result in caregivers' receiving the incentive. However, we recommend GiveWell also discuss this further with NI-ABAE as card data is not perfect and there may be further insights to be gained from process and field observations of program staff.

impact estimate of 22 percentage points obtained when using the loose card adjusted RI-only coverage rates.

In the next section, we will discuss self-reported data quality, which may imply further adjustments to both control coverage and the impact estimate. We recommend GiveWell to make any such additional adjustments to the already adjusted Measles vaccine coverage rate and impact estimate described in this section. This section has tried to quantify the campaign effect on self-reported Measles vaccine coverage and impact. The next section deals with the wider issue of measurement error in self-reports which is of substantial importance for the self-reported coverage rates of all vaccines.

Quality of Self-Reports

There are no perfect measures of vaccination status; even gold-standard tests for biological immunity include error. Our primary data source for this evaluation was self-reported coverage. However, given that self-reported data is prone to recall error and social desirability bias, we collected various sources with the intent of using them to supplement, cross-check, and assess the quality of self-reports.[110] Namely, endline provided the following additional imperfect sources of vaccination information: 1) CHCs and other health cards available at the household; 2) CIRs stored at clinics; and 3) BCG vaccine scars (for BCG vaccine only). Like self-reported coverage, each of these sources was subject to measurement error, which can lead to misleading conclusions if not accounted for:

1. Measurement error that is not affected by/correlated with the treatment induces downward bias in the impact estimate as measured by binary ("yes/no") variables.[111]
2. When measurement error is correlated with treatment, it can lead to an upward or downward bias in the impact estimate.[112]
3. Measurement error can bias *coverage* estimates in either direction.

Absent information on whether and the extent to which these effects occurred, we cannot be certain if our coverage and impact estimates were higher or lower than the truth.[113] We can, however, explore the consistency of results across multiple sources that each measures the same outcome to

[110] Importantly, we explored the use of oral-fluid biomarker testing, which ultimately proved infeasible in our context. Even biomarkers, however, are imperfect. Researchers seeking to estimate true immunity are always left with some uncertainty derived from imperfection in the available data sources.

[111] In other words, if our measures are imperfect (which we know they are) but are equally imperfect in treatment and control, then they cause the impact estimate to be smaller than the true impact.

[112] For example, if NI-ABAE makes vaccinations more memorable, then more vaccinations will be recorded in treatment than in control for any true impact, making the impact estimate higher than it would be if recall were equal in the two groups. Similarly, if the program increases the social desirability of reporting vaccination, then more vaccinations may be falsely reported in treatment than control, making the impact estimate higher than it would be if social desirability were equal in the two groups.

[113] See Appendix M for additional examples and explanation of the implications of measurement error. See Appendix C for detailed description of in-field data quality measures employed at endline to maximize data quality.

shed light on the extent to which measurement error could make our observed results differ from true coverage and impact.[114]

Coverage and Impact According to Various Measures

Impact is positive and large regardless of the coverage source analyzed.

We looked at coverage according to each data source we had: self-reports, cards, CIRs, and BCG vaccine scar (for BCG vaccine only). We should not treat these measures equally as their limitations differ. Self-reports were subject to recall and social desirability bias, while administrative records (cards and CIR) were not. Cards and CIR, however, were limited by record keeping quality – clinic staff may forget to make a record, run out of blank records, lose a record, or invent a record. In the case of cards, caregivers might lose them or forget to bring them to a vaccination visit. In the case of CIRs, clinic staff may not update them for vaccinations given outside the clinic or on particularly busy days. The quality of these measures may also be affected by the program itself, via its awareness activities and emphasis on record-keeping. BCG vaccine scars, meanwhile, have the advantage of being a biological marker that is highly unlikely to be affected by treatment status. They are still imperfect, however; not every child scars, and enumerators may not correctly find and identify every scar.

We might expect cards to be more accurate than self-reports based on the assumption that caregivers have difficulty recalling all vaccinations, while written records should not be subject to this problem. This is only true, however, when a card is present for the enumerator to find and record. Card possession is relatively high in our sample but not perfect and appears to be affected by treatment status: in the treatment group, 89% [95% CI: 87%, 91%] of children with BCG vaccine scars also had a vaccination card of any kind while in the control group, 69% of children with BCG vaccine scars had cards [95% CI: 65%, 74%].[115]

Figures 16-18 present coverage in treatment and control based on several sources. In each graph, the bars illustrate coverage for one vaccine as measured by each of self-reports, immunization cards (CHC and others), and CIRs. For BCG vaccine, the figure also includes BCG vaccine scars and scars plus self-reports. For each measure, there is a bar showing coverage in treatment and a bar showing coverage in control. The difference between the two is an impact estimate for the NI-ABAE CCTs for RI Program according to that data source or combination of data sources.

Impact was positive (and almost always statistically significant) regardless of the source used. However, precise coverage and impact varied widely depending on the source, largely because of each source's limitations. Coverage was generally highest according to self-reports and cards and lowest according to CIRs. Differences between treatment and control coverage (impact estimates

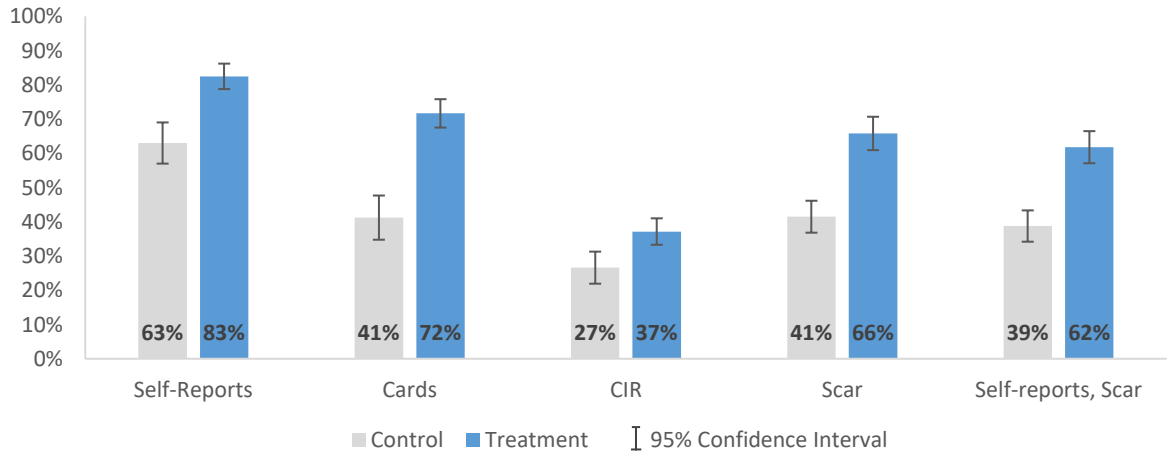
[114] The analysis in this section includes the 15 observations from Kairu PHC, which were excluded from the main analysis due to missing baseline data. As no baseline data is used in this section, we decided to include these additional observations.

[115] We limited this analysis to children with BCG vaccine scars as we expected that card possession was affected by vaccination status. (In the extreme, there is little reason to expect a child who has never been vaccinated to have a card). Accordingly, any analysis of card possession in treatment versus control must attempt to account for true vaccination – comparing with BCG vaccine scars was one simple way to do that and had the advantage that we did not expect treatment to influence.

without covariates) according to self-reports were about 20 percentage points for BCG vaccine, 24 percentage points for Penta vaccine, and 16 percentage points for Measles vaccine. According to cards, impact estimates were much larger: 30 percentage points for BCG vaccine, 30 percentage points for Penta vaccine, and 34 percentage points for Measles vaccine. This difference is largely explained by differences in card retention between treatment and control, as explained above.

For CIRs, we had far fewer records in both treatment and control, leading to lower coverage for all three vaccines. The largest drop-off from card-based coverage to CIR coverage was for Measles vaccine where treatment coverage was 72% [95% CI: 67%, 76%] according to cards and 28% [95% CI: 24%, 32%] according to CIRs, and control coverage was 37% [95% CI: 32%, 42%] according to cards and about 15% [95% CI: 12% to 18%] according to CIRs. This may be partially due to campaigns and outreach, since vaccines given during these activities were less likely to be recorded in CIRs but were often recorded on cards. It may also be due to the difficulty of both maintaining CIRs (for clinic staff) and recording them (for enumerators).[116] We expected CIR-only coverage to be low for data quality reasons and, so, expect that true coverage in both the treatment and the control groups is substantially higher than the CIR-only estimates.

Figure 16. BCG Vaccine Coverage According to Various Data Sources (95% CI)



[116] CIRs are difficult to maintain: they require writing in several pieces of information for a child initially and then finding that child's record on every subsequent visit to add new vaccinations. CIR booklets are large, so clinic staff may not bring them to outreach immunization sessions, forcing them to record outreach vaccinations in a separate CIR or update the CIR upon their return. CIRs are also tied to particular clinics – children who receive vaccinations at a clinic we did not survey will not show up in CIRs. We visited both study clinics and alternate clinics that a large proportion of caregivers reported visiting for immunizations – however, there may be alternate clinics we did not visit, causing us to miss records kept there. CIR data was also challenging for enumerators to record: they had to search through dozens of pages of records and try to match demographic information from the household survey to that recorded in the CIR. Alternate names, misspellings, typos in birthdates, and enumerator error or lack of thoroughness could have led to missing a record during data collection.

Figure 17. Penta Vaccine Coverage According to Various Measures (95% CI)

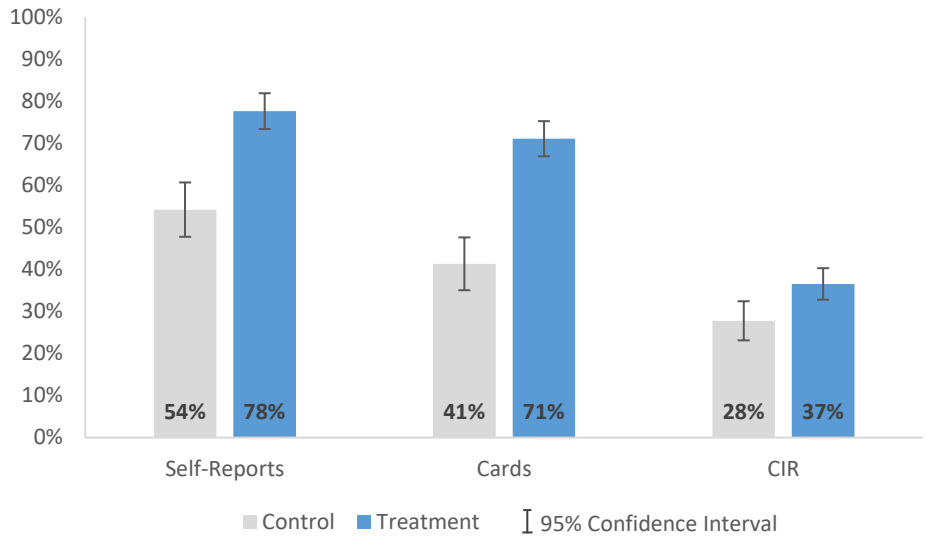
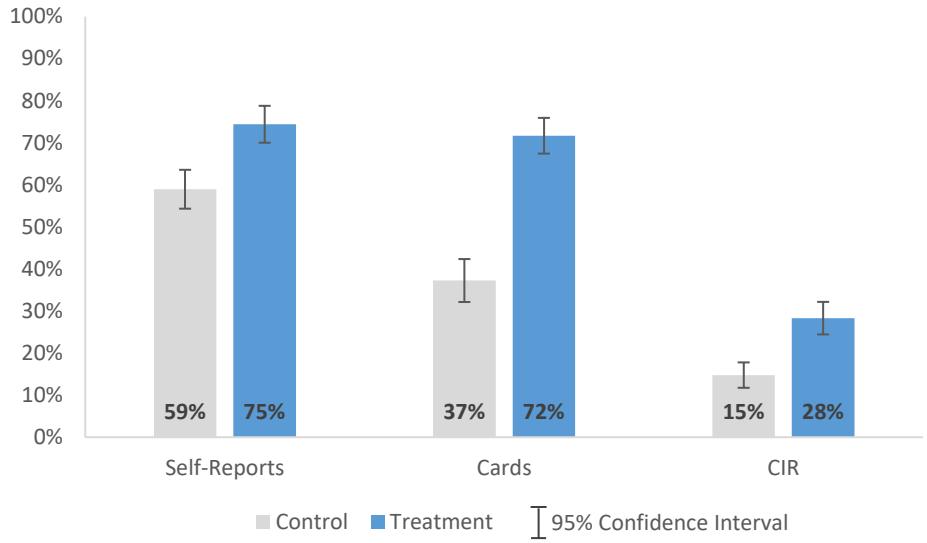


Figure 18. Measles Vaccine Coverage According to Various Measures (95% CI)



Coverage remained high when considering vaccinations reported/recorded by both self-reports and cards, suggesting a high degree of agreement between these two sources.

For BCG vaccine, coverage according to scars was comparable to coverage according to cards and lower than that according to self-reports, while impact was higher according to both scars (25 percentage points) and cards (30 percentage points) than according to self-reports (20 percentage points).

That all these data sources show large positive impact makes it highly likely that true impact is also positive.[117] This directional agreement is indicative but leaves considerable uncertainty as to specific coverage and impact. The next section looks more closely at the correspondence among coverage sources and explores what this reveals about their quality and therefore what they tell us about the truth.

Correspondence Among Sources and Sensitivity and Specificity of Self-Reports

Given that our primary outcomes were measured using self-reports, this section focuses on assessing self-report quality using information from other measures.

Measurement error for binary outcomes can be summarized by two statistics: sensitivity and specificity. Sensitivity is the probability that the measure shows a positive value given that the true outcome is positive (i.e. the probability of a positive self-report for a truly vaccinated child). Specificity is the probability that the measure shows a negative value given that the true outcome is negative (i.e. the probability of a negative self-report given for a truly non-vaccinated child).

Sensitivity of Self-Reports

We investigated the sensitivity of self-reports using the information from card and CIR data, which was available for all vaccines (BCG vaccine, Any Penta vaccine, and Measles vaccine), and BCG vaccine scars for BCG vaccine. This data only allows us to draw conclusions if we are willing to make certain assumptions about the accuracy of vaccine cards, CIRs, and BCG vaccine scars, themselves.

To approximate the sensitivity of self-reports, we first assumed that every child with a positive entry for the respective vaccine on a CHC (or any other vaccination card), in a CIR, or based on a BCG vaccine scar was truly vaccinated.[118] We then approximated the sensitivity of self-reports as:

Self-report sensitivity = Positive self-report / Positive entry in other measure[119]

Sensitivity was relatively high for BCG vaccine, Penta 1 vaccine, and Measles 1 vaccine and was higher in treatment than in control. Sensitivity was highest for BCG vaccine and lowest for any Penta vaccine (Table 20). BCG vaccine sensitivity was higher when using scars than when using either cards or CIRs. Overall, these relationships among the estimated sensitivities for the different vaccines were of reasonable magnitude and in line with expectations. The BCG vaccine commonly leaves a scar and is also amongst the first vaccines children receive. This should make it relatively easy for caregivers to remember. We would also have expected that it is easier for a caregiver to remember that their child

[117] This is especially true for data sources whose error was less-likely to be correlated, such as CIRs and self-reports or BCG vaccine scars and CIRs. It is difficult to think of reasons why both a record made at a clinic at the time of vaccination and the development of a scar several days/weeks after that vaccination would have similar results except if they were both influenced by a true vaccination. They have little in common other than the truth.

[118] Cards + CIR sensitivity calculates the share of positive self-reports out of all children with a positive card and CIR record. We can be very confident that these children were truly vaccinated.

[119] Note that we used division to simplify the intuition. The cases in the numerator were restricted to those in which the denominator took a positive value (i.e. a self-reported vaccination for a child who does not have a positive record on their card is excluded from the measure altogether). In other words, these measures were bounded by 0 and 1.

received Penta 1 vaccination but that it can be difficult to recall the exact number of Penta vaccinations received. Lastly, that scar-based sensitivity was highest was also unsurprising given that this measure restricted the sample to those BCG vaccinations that left a physical reminder (the scar) on the child and tests whether caregivers report them.

Table 20. Self-Report Sensitivity for the Primary Vaccines Based on Other Data Sources

	Using cards			Using CIR			Using cards + CIR		
	Estimate (%)	95% CI		Estimate (%)	95% CI		Estimate (%)	95% CI	
BCG				**BCG**			**BCG**		
Control (n = 908)	90	88	93	Control (n = 579)	89	86	93	Control (n = 396)	93	90	96
Treatment (n = 2116)	97	96	98	Treatment (n = 1094)	93	92	95	Treatment (n = 948)	97	96	98
Total (n = 3024)	95	94	96	Total (n = 1673)	92	90	94	Total (n = 1344)	96	94	97
Penta1				**Penta1**				**Penta1**			
Control (n = 892)	85	80	89	Control (n = 588)	80	75	86	Control (n = 391)	87	83	92
Treatment (n = 2079)	95	93	96	Treatment (n = 1053)	91	88	94	Treatment (n = 914)	95	93	97
Total (n = 2971)	92	90	94	Total (n = 1641)	87	84	90	Total (n = 1305)	93	90	95
Penta2				**Penta2**				**Penta2**			
Control (n = 834)	69	64	75	Control (n = 540)	65	59	71	Control (n = 356)	72	67	78
Treatment (n = 2059)	86	84	89	Treatment (n = 1022)	84	80	87	Treatment (n = 893)	88	85	91
Total (n = 2893)	82	79	85	Total (n = 1562)	77	73	81	Total (n = 1249)	83	80	86
Penta3				**Penta3**				**Penta3**			
Control (n = 777)	57	51	63	Control (n = 482)	55	49	61	Control (n = 328)	62	55	68
Treatment (n = 2039)	76	72	80	Treatment (n = 966)	72	67	76	Treatment (n = 844)	76	72	80
Total (n = 2816)	71	67	74	Total (n = 1448)	66	62	70	Total (n = 1172)	72	68	76
Measles				**Measles**				**Measles**			
Control (n = 828)	84	80	88	Control (n = 323)	81	76	87	Control (n = 242)	87	82	92
Treatment (n = 2116)	90	87	92	Treatment (n = 832)	88	85	91	Treatment (n = 739)	91	88	93
Total (n = 2944)	88	86	90	Total (n = 1155)	86	83	89	Total (n = 981)	90	88	92

	Using scars		
	Estimate (%)	95% CI	
BCG			
Control (n = 866)	98	97	99
Treatment (n = 1857)	98	98	99
Total (n = 2723)	98	98	99

This exercise calculated sensitivity of self-reports according to other available data sources. However, the measure that we ultimately care about is the sensitivity of self-reports according to a child's true vaccination status. While we cannot know the true sensitivity of self-reports, based on what we do know, we believe that sensitivity is likely lower than that calculated according to these measures. This is based on the assumptions that underlie the measures and the direction and extent to which we expect reality to depart from those assumptions. Specifically, the calculated sensitivities assume that **the lack of a record means that the child was not vaccinated.** This will leave out vaccinations that occurred but were not recorded from the denominator. For cards, we expect that some caregivers of vaccinated children never received a card, lost the card, or did not show the card to the enumerator. For CIRs, we expect that clinic staff failed to record some vaccinations that occurred, that some children were vaccinated at clinics whose records we did not check, and that enumerators failed to find some records. For scars, we expect that some portion of children who received BCG vaccine did not scar and that enumerators failed to find some proportion of scars.

In all of these cases, therefore, our calculated measure will fail to include a true vaccination (since there is no record of it) in the denominator. The effect this has on the calculated sensitivity (relative to the true sensitivity) depends on what happens in the numerator. If self-report sensitivity is lower in cases in which there is no record of the vaccine than in cases where there is a record, then the calculated measure is higher than the truth. For cards and scars, we would expect the record itself to aid caregiver recall (since these records are present in the home), meaning that self-report sensitivity should be lower (fewer true positives to add to the numerators) in this sub-group. For CIRs, our expectation is less clear but the sample size with positive CIRs is also relatively small, so we hesitate to draw too many conclusions from it. We expect, then, that our calculated sensitivities are overestimates of the actual sensitivity of self-report.[120]

The fact that sensitivity was higher in treatment than in control may suggest that the program's awareness-building activities and the act of receiving cash make vaccination more salient and memorable in caregivers' minds. If true, this means that the downward bias in the impact estimate induced by measurement error that occurs in both groups is partially counteracted by an upward bias induced by improved reporting in treatment relative to control. We explore this further below while also accounting for specificity.

Specificity of Self-Reports

In addition to sensitivity, we investigated the specificity of self-reports using the same approach. Determining a reasonable approximate estimate for specificity using card/CIR data is more difficult than for sensitivity. We have at least some degree of confidence (the exception being falsified cards)

[120] Strictly, this is only true if self-report sensitivity is lower in cases in which there is no record of the vaccine than in cases where there is a record. If we knew true vaccination status, we could add the unrecorded vaccinations back in to the denominator of our sensitivity measure. To calculate true sensitivity, we would then also have to add the corresponding self-report to the numerator. For cards and scars, we would expect the record itself to aid caregiver recall (since these records are present in the home), meaning that self-report sensitivity should be lower (fewer true positives to add to the numerators) in this sub-group. For CIRs, our expectation is less clear but the sample size with positive CIRs is also relatively small, so we hesitate to draw too many conclusions from it.

that a child that has a vaccine recorded on a vaccination card actually did receive the vaccine. However, we have less confidence in the related assumption for specificity that a child who did not have a vaccine recorded (or had no vaccine record at all) did not actually receive the vaccine. A child may have been missing a record for a variety of reasons that were unrelated to vaccination status.

In this section, we used the percentage of children whose caregiver reported that the child did not receive the vaccine among children who did not have a given vaccine recorded on a card or CIR (or did not have a scar in the case of BCG vaccine) as an approximation of the specificity of self-reports:

Self-report specificity = Negative self-report / No positive entry from other source

This treated both children with records that did not have the vaccine recorded and children who did not have a record at all as "non-vaccinated."[121] In other words, the lack of a positive record of a vaccination was interpreted as the lack of a vaccination.

Across measures, specificity was much lower than sensitivity by these approximations.[122]

For BCG vaccine, we found that among the control group, specificity based on scars was higher than that based on cards or CIRs, while among the treatment group, scar-based specificity fell between the two records-based measures. As in the case of card/CIR data, we know that at least some children counted as non-vaccinated by this measure were actually vaccinated but failed to scar. Hence, the approximate specificity based on scars, like that based on cards and CIRs, is almost certainly lower than the true specificity.

[121] The measure cards + CIR counts a child as unvaccinated only if there was *neither* a card nor a CIR record of that vaccination.

[122] As discussed earlier, we can be confident that a meaningful proportion of these children were actually vaccinated and simply lost their card or did not get it filled out when the vaccination occurred. The current measure assumes that none of these cases are truly vaccinated. In cases where one of these children is truly vaccinated and the caregiver reports as much, the measure counts this as a false positive, reducing specificity. The more cases like this that exist in the data, the higher would be true specificity relative to this measure.

Table 21. Self-Reports Specificity Based on Other Sources

	Using cards			Using CIR			Using cards + CIR		
	Estimate (%)	95% CI		Estimate (%)	95% CI		Estimate (%)	95% CI	
BCG				**BCG**			**BCG**		
Control (n = 1293)	56	51	62	Control (n = 1622)	47	40 53	Control (n = 1110)	62	57 68
Treatment (n = 839)	55	49	60	Treatment (n = 1861)	24	19 29	Treatment (n = 693)	60	54 66
Total (n = 2132)	56	51	60	Total (n = 3483)	34	30 39	Total (n = 1803)	62	58 66
Penta1				**Penta1**			**Penta1**		
Control (n = 1309)	67	61	72	Control (n = 1613)	55	49 62	Control (n = 1112)	73	68 78
Treatment (n = 876)	63	58	68	Treatment (n = 1902)	30	25 35	Treatment (n = 737)	68	63 73
Total (n = 2185)	65	61	69	Total (n = 3515)	42	36 47	Total (n = 1849)	71	67 75
Penta2				**Penta2**			**Penta2**		
Control (n = 1367)	79	74	83	Control (n = 1661)	69	63 75	Control (n = 1183)	83	79 87
Treatment (n = 896)	73	67	78	Treatment (n = 1933)	39	34 45	Treatment (n = 767)	77	73 81
Total (n = 2263)	76	73	80	Total (n = 3594)	53	48 58	Total (n = 1950)	81	78 84
Penta3				**Penta3**			**Penta3**		
Control (n = 1424)	85	81	89	Control (n = 1719)	77	72 83	Control (n = 1270)	88	85 91
Treatment (n = 916)	78	73	83	Treatment (n = 1989)	47	41 53	Treatment (n = 794)	81	77 85
Total (n = 2340)	82	79	85	Total (n = 3708)	61	56 66	Total (n = 2064)	85	83 88
Measles				**Measles**			**Measles**		
Control (n = 1373)	56	52	60	Control (n = 1878)	45	40 50	Control (n = 1292)	57	53 62
Treatment (n = 839)	64	59	69	Treatment (n = 2123)	31	26 36	Treatment (n = 746)	68	63 72
Total (n = 2212)	59	56	62	Total (n = 4001)	38	34 41	Total (n = 2038)	61	58 64

	Using scars		
	Estimate (%)	95% CI	
BCG			
Control (n = 1234)	61	55	67
Treatment (n = 970)	48	42	54
Total (n = 2204)	55	50	60

We expect these calculated specificities to be substantially lower than true specificity. This is because it was very likely that some truly vaccinated children did not have records. When this occurred, the measure treated them as unvaccinated, incorrectly adding them to the denominator of the calculated

specificity. This then also added their self-reports to the numerator. It is reasonable to assume that caregivers of truly vaccinated children who are missing records are more likely to report a vaccination than caregivers of truly unvaccinated children (because true vaccination status does affect self-reports). Therefore, our calculated measures – by including these cases and treating them as false positive reports – are likely lower than true specificity.[123]

While it is also notable that approximate specificity was usually higher for control than for treatment, these results should be interpreted with substantial caution. There exist several possible interpretations, not all of which imply that true specificity is lower in treatment. As long as there is true program impact, we would expect the approximate specificity measure to be higher in control than in treatment even if card ownership rates and the true underlying specificity are the same in treatment and control.[124]

Implications for Impact Estimates and Coverage

Based on other sources, we expect that the true sensitivity of self-reports is less than the roughly 95% approximation using vaccination cards, though we are unsure by how much. Similarly, we expect that true self-reports specificity is higher than the roughly 60% approximations using vaccination cards and CIRs, though we are unsure by how much. With this starting point, we explored the effect of a wide variety of different assumed sensitivity and specificity on coverage and impact estimates. We found that adjusted impact was almost always higher than that implied by unadjusted self-reports.[125]

This result is consistent with that implied by BCG vaccine scars, which – unadjusted – show an impact of 23 percentage points. We know, however, that BCG vaccine scars are also an imperfect measure, though their error is highly likely to be similar in treatment and control. Literature suggests that around 90% of children vaccinated with BCG develop a scar (Dhanawade 2015). We assume, however, that enumerators failed to find scars in some number of cases so that scar sensitivity is somewhat less than 90%. Scar specificity should be virtually 100% since a BCG vaccine scar cannot develop without a BCG vaccination. However, some enumerators may have misidentified birth marks or non-vaccination scars as BCG vaccine scars. Therefore, we might expect that true scar specificity is slightly less than 100%. Importantly, since we are confident this error is similar in treatment and control, we know it will bias the impact estimate toward zero. Therefore, we can be even more confident that true impact is higher than the 25 percentage points found using unadjusted scars.

[123] This effect is likely at play for all measures but particularly for CIRs given how difficult it was to match CIR records. Unsurprisingly, specificity as estimated using CIRs was noticeably lower than that estimated using cards or scars.

[124] For example, imagine that there are 100 children each in treatment and control. 60 children in control, and 80 children in treatment are truly vaccinated. Assume that the sensitivity and specificity of self-reports is 100% - self-reports are completely accurate. Assume that - in both treatment and control - all non-vaccinated children have no vaccine recorded on a vaccination card (so 40 children in control, and 20 in treatment). In both treatment and control, 50% of vaccinated children have a vaccination card with the vaccine recorded on it (so 30 in control, and 40 in treatment). We can now calculate our estimated specificity measure: Control group: 40 / (40 + 30) = 40/70 = 57%; Treatment group: 20 / (20 + 40) = 20/60 = 33%.

[125] For example, assuming BCG vaccine self-report sensitivities between 80% and 97% in treatment and 80% and 90% in control, specificities between 70% and 90% in both groups, and minimal difference between treatment and control in these measures, we found that impact cannot be lower than about 19 percentage points and can be substantially higher. Relaxing the assumption that sensitivity and specificity are similar in treatment and control produced wider bounds, but still *usually* implied impact estimates greater than the 16.4 percentage points according to unadjusted self-reports.

For control coverage, measurement error can theoretically produce an upward or downward bias. However, when analyzing a large range for scar sensitivity and specificity, we always found control coverage to be below the 63% measured by self-reports.[126] This suggests that control coverage for BCG vaccine is very likely to be lower than that estimated using self-reports.

Other Vaccines

We do not expect self-report sensitivity and specificity for BCG vaccine to accurately represent other vaccines. In particular, we expect self-report sensitivity to be higher for BCG vaccine than for other vaccines (as we see using our approximate measures). However, we would expect that program impact was similar across vaccines, especially since this was also a trend across sources. We also expect bias in control coverage as measured by self-reports was *broadly* similar across vaccines. Given that true BCG vaccine coverage was almost certainly lower than the self-reports estimate (perhaps substantially so), this is also likely the case for other vaccines. **We therefore recommend that GiveWell use similar (directional) adjustments for impact estimates and control coverage for BCG vaccine, Penta vaccine, and Measles vaccines.**

[126] We allowed scar sensitivity as low as about 79% and as high as about 95% and scar specificity of as low as about 85% and as high as about 98%. Combinations in these ranges never produced control coverage higher than about 50%

Conclusion and Recommendations for GiveWell

Impact of New Incentives – All Babies Are Equal Initiative CCTs for RI Program

The NI-ABAE CCTs for RI Program had a large, consistent positive impact on vaccination coverage.

According to both primary and secondary outcomes, the NI-ABAE CCTs for RI Program had a substantial positive impact on vaccination. This impact held across a wide range of specifications and was consistently statistically significant, usually at better than the 0.1% level.

Based on self-report error, we recommend GiveWell adjust impact estimates upward for all vaccines.

- **Impact estimates were positive across data sources.** This suggests that there is a true and meaningful impact on vaccination not explainable by measurement error alone.

- **Plausible assumptions about data quality usually implied upward adjustments.** Making plausible assumptions about self-report measurement error and using them to adjust coverage and impact always generates positive impact, though with a great deal of remaining uncertainty about its size. Most of this uncertainty in the upward direction: most plausible adjustments produce impact estimates greater than unadjusted self-reports impact estimates, especially if we assume measurement error is similar in treatment and control. Using BCG vaccine scars – also corrected for measurement quality – yields a range for impact on BCG vaccination that is entirely and meaningfully higher than unadjusted self-reports. While it is difficult to extrapolate measurement error from BCG vaccine to other vaccines, we find it likely that impact is similar across vaccines.

We recommend that GiveWell apply a somewhat larger upward adjustment to Measles vaccine than to the other vaccines.

For Measles vaccine, our impact estimate was higher when restricted to vaccinations delivered via RI activities. Given that large Measles vaccine campaigns appear to occur infrequently (but one did occur just before endline), it is possible that the program would have a higher impact on Measles vaccine in normal, non-post-campaign periods.

The Counterfactual – Areas without NI-ABAE

Based on self-report error, we recommend GiveWell adjust control coverage estimates downward for all vaccines.

Adjusting control coverage for self-report sensitivity and specificity produced ranges on either side of the coverage estimate according to unadjusted self-reports. Control coverage according to BCG vaccine scars (adjusted for scarring probability and scar-recording probability), however, falls well below the self-reports level. This suggests that true BCG vaccine coverage in control is likely to be below that measured by self-reports. We recommend GiveWell adjust control coverage down for BCG vaccine based on self-report error. It is difficult to extrapolate this finding to other vaccines, but we

would recommend that GiveWell also assume true coverage for Penta 1 vaccine and Measles 1 vaccine in control is lower than that found via unadjusted self-reports.

We recommend that GiveWell assume RI-only coverage for Measles 1 vaccine is roughly 11 percentage points lower than unadjusted coverage.

Coverage in control areas at endline was higher than at baseline even after considering the effect of including Jigawa State and changes in measurement methods between the two rounds. Campaigns reported during the study window targeted Measles vaccine but not BCG or Penta vaccines. For Measles vaccine, we found that control coverage included a non-trivial proportion of campaign-vaccinators to whom the program might not pay incentives.

Summarized Recommendations for the CEA

- Adjust self-report impact estimates upward for all vaccines (due to measurement error). Make a larger upward adjustment for Measles vaccine (due to campaigns)
- Adjust self-report control coverage downward for all vaccines (due to measurement error), with the largest downward adjustment for Measles vaccine (due to campaigns)

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