Causal Inference Methods for Evaluating Vaccine Hesitancy Interventions: An Application of Instrumental Variable and Regression Discontinuity Designs to Community-Based Educational Campaigns

Deborah B Okunola [1*], **Oladimeji Adewuyi** [2]
[1-2] Faculty of Science, Federal University of Technology, Akure, Nigeria

* Corresponding Author: **Deborah B Okunola**

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Abstract

Background: Vaccine hesitancy—defined by the World Health Organization as the delay in acceptance or refusal of vaccination despite availability—has emerged as one of the top ten threats to global health. Community-based educational campaigns are widely deployed to address hesitancy, but rigorous causal evidence of their effectiveness remains sparse. Observational evaluations are compromised by confounding from self-selection, access to care, income, and political ideology.

Objective: This study demonstrates how two quasi-experimental methods—instrumental variable (IV) analysis and regression discontinuity (RD) design—can be applied to estimate the causal effect of a statewide community health worker (CHW) vaccine education program on childhood vaccination uptake in a setting where randomization was not feasible.

Methods: We analyzed administrative vaccination records for 68,714 children aged 12–35 months across 412 communities in a Midwestern state (2016–2021). The CHW program was allocated using a composite community risk score; communities scoring above a threshold received the intervention. We exploited this assignment mechanism using a fuzzy RD design at the eligibility threshold and, independently, used distance to the nearest CHW training center as an instrument for program participation. Outcomes included completion of the combined 7-vaccine series by 24 months and the measles-mumps-rubella (MMR) first-dose rate by 15 months.

Results: The RD estimate of the local average treatment effect (LATE) on 7-vaccine series completion was 8.6 percentage points (95% CI: 3.4–13.8, p = 0.001). The IV estimate was 7.1 percentage points (95% CI: 2.8–11.4, p = 0.001). For MMR first-dose by 15 months, the RD estimate was 6.2 pp (95% CI: 2.1–10.3) and the IV estimate was 5.1 pp (95% CI: 1.4–8.8). Both methods produced estimates that were concordant and substantially larger than naïve OLS estimates, consistent with attenuation from confounding bias.

Conclusions: Community-based CHW vaccine education programs produce meaningful increases in childhood vaccination rates. The concordance of IV and RD estimates strengthens confidence in a causal interpretation. These methods should be more widely adopted in public health program evaluation to overcome the limitations of observational comparisons.

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1. Introduction

The success of vaccination programs depends not only on vaccine availability but on the willingness of individuals and communities to accept vaccination. Over the past two decades, vaccine hesitancy has increased in many high-income countries, driven by a complex interplay of factors including misinformation, erosion of trust in public health institutions, ideological alignment, and the paradox of success—as vaccine-preventable diseases become rarer, the perceived necessity of vaccination declines while attention to potential adverse effects intensifies. The COVID-19 pandemic has amplified these dynamics, with

politicization of public health measures, the rapid deployment of novel vaccine platforms, and the proliferation of misinformation on social media converging to produce historically high levels of vaccine skepticism that have spilled over into attitudes toward routine childhood immunizations.

Even before the pandemic, the consequences of hesitancy were not abstract: outbreaks of measles in previously well-controlled settings, declining influenza vaccination rates, and widening geographic pockets of undervaccination had all been documented. The 2019 measles resurgence in the United States, with over 1,200 confirmed cases concentrated in communities with high nonmedical exemption rates, starkly illustrated the public health costs of declining vaccine confidence.

In response, public health agencies have invested heavily in community-based educational interventions, often deploying community health workers (CHWs) to conduct outreach, provide culturally tailored information, address concerns through motivational interviewing, and facilitate access to vaccination services. The rationale is intuitive and grounded in health behavior theory: trusted community members delivering personalized, evidence-based information should be more persuasive than impersonal mass communication campaigns. But does this approach actually work? And if so, by how much?

Answering these questions rigorously is challenging. Randomized controlled trials (RCTs) of community-level educational interventions are logistically complex, expensive, and ethically constrained—communities with high hesitancy and low vaccination rates may be harmed by random assignment to a control condition that withholds potentially beneficial outreach. Observational comparisons, meanwhile, are plagued by confounding: communities that receive CHW programs may differ systematically from those that do not in ways that independently predict vaccination uptake. Self-selection—whereby more health-conscious communities are more likely to participate in and respond to educational outreach—would bias naïve estimates upward. Conversely, if programs are preferentially targeted to the most hesitant communities (as is often the case), confounding by indication would bias estimates downward.

Quasi-experimental methods from econometrics and the causal inference literature offer a principled solution. Instrumental variable (IV) analysis and regression discontinuity (RD) design are two approaches that, under clearly articulated assumptions, can recover causal effects from observational data by exploiting features of the treatment assignment mechanism that are plausibly unrelated to the outcome except through their effect on treatment. Despite their theoretical appeal and widespread use in economics, education, and political science, these methods remain underutilized in public health program evaluation.

This paper makes two contributions. First, we apply IV and RD designs to evaluate a real statewide CHW vaccine education program, demonstrating the practical implementation of these methods in a public health context. Second, by presenting estimates from two independent identification strategies, we provide a triangulation framework that strengthens causal claims beyond what either method alone could achieve. When two distinct sources of variation in treatment assignment yield concordant causal estimates, the probability that both are biased in the same direction and by similar magnitudes is substantially lower than for either alone.

2. Background and Literature Review

2.1. The Landscape of Vaccine Hesitancy

Vaccine hesitancy is not a monolithic phenomenon. The WHO's Strategic Advisory Group of Experts (SAGE) on Immunization has proposed a framework organized around three dimensions: confidence (trust in vaccine safety, efficacy, and the healthcare system), complacency (perceived low risk of vaccine-preventable diseases), and convenience (access to and affordability of vaccination services). Subsequent work has added additional dimensions, including calculation (the extent to which individuals engage in cost-benefit reasoning) and collective responsibility (the sense of duty to protect community health through individual vaccination).

In the United States, vaccine hesitancy is geographically clustered and associated with a heterogeneous mix of demographic, political, and cultural factors. Research has documented higher rates of hesitancy in communities with lower educational attainment, greater political conservatism, stronger religious objection traditions, and higher rates of nonmedical exemption from school vaccination requirements. However, hesitancy is not confined to any single demographic stratum: clusters of under vaccination have been identified in affluent, highly educated communities driven by concerns about vaccine ingredients and "natural" health philosophies, as well as in underserved communities where structural barriers compound attitudinal resistance. The COVID-19 pandemic has further complicated this landscape, with pandemic-era skepticism influencing parental attitudes toward routine childhood vaccines in ways that are only beginning to be understood.

2.2. Community Health Worker Interventions

Community health workers occupy a unique position in the public health workforce. Typically drawn from and trusted by the communities they serve, CHWs bridge the gap between formal healthcare systems and populations that may be distrustful of or disconnected from medical institutions. In the vaccination context, CHW-delivered interventions generally combine informational, motivational, and logistical components: providing evidence-based information about vaccine safety and efficacy, using motivational interviewing to explore and address concerns, and connecting families with vaccination services by helping to schedule appointments, arrange transportation, or co-locate vaccination with other community services.

The evidence base for CHW vaccination interventions is suggestive but methodologically limited. Systematic reviews have identified numerous studies reporting positive associations between CHW outreach and vaccination uptake, but the majority rely on pre-post comparisons, uncontrolled cross-sectional designs, or observational comparisons with nonequivalent control groups. The few RCTs that have been conducted tend to be small, limited to specific populations (e.g., a single immigrant community), and may have limited external validity. The gap between the widespread deployment of CHW programs and the rigor of the evidence supporting them motivates the present study.

2.3. Causal Inference Methods in Public Health

2.3.1. Instrumental Variable Analysis

Instrumental variable analysis addresses confounding by identifying a variable (the instrument) that satisfies three conditions: relevance (the instrument predicts treatment assignment), independence (the instrument is not associated with unmeasured confounders), and the exclusion restriction (the instrument affects the outcome only through its effect on treatment). When these conditions hold, two-stage least squares (2SLS) estimation recovers a consistent estimate of the causal effect of treatment even in the presence of unmeasured confounding.

In the context of geographic access to health services, distance-based instruments have a rich history. The seminal work of McClellan, McNeil, and Newhouse (1994) [8] used differential distance to cardiac catheterization facilities as an instrument for the receipt of intensive cardiac procedures, establishing a template that has since been applied to study the effects of neonatal intensive care, surgical procedures, and pharmaceutical interventions. The core argument is that geographic distance affects the probability of receiving treatment but is plausibly unrelated to unmeasured health characteristics conditional on observable covariates.

2.3.2. Regression Discontinuity Design

Regression discontinuity exploits the fact that many policies and programs assign treatment based on whether a continuous running variable exceeds a predetermined threshold. Near the threshold, units that barely qualify for treatment are expected to be similar to those that barely miss the cutoff—the assignment is "as good as random" in a local neighborhood around the threshold. The RD estimand is a local average treatment effect (LATE) at the cutoff, which can be estimated nonparametrically using local polynomial regression or parametrically with flexible functional forms for the running variable.

The distinction between sharp and fuzzy RD designs is important. In a sharp RD, treatment status is a deterministic function of the running variable: all units above the threshold receive treatment, and none below do. In a fuzzy RD, crossing the threshold increases the probability of treatment but does not determine it. The fuzzy RD is analogous to an IV analysis in which the indicator for being above the threshold serves as an instrument for treatment receipt. Our application is a fuzzy RD: communities above the risk score threshold were eligible for the CHW program, but participation was voluntary.

3. Methods

3.1. Setting and Intervention

The study setting is a Midwestern state that implemented a statewide CHW vaccine education program beginning in 2018. The program was designed to improve childhood vaccination rates in communities with high estimated hesitancy and low uptake. Community health workers were recruited from target communities, trained in motivational interviewing and vaccine science communication, and deployed to conduct home visits, community presentations, and one-on-one counseling with parents of young children. The program continued through the COVID-19 pandemic, with CHWs adapting to virtual and socially distanced outreach modalities beginning in March 2020.

Program eligibility was determined by a composite Community Vaccination Risk Score (CVRS) calculated for each of the state's 412 incorporated communities.

The CVRS integrated five components: historical vaccination rates (based on school-entry immunization records), nonmedical exemption rates, Medicaid enrollment density, health professional shortage area status, and a social vulnerability index. Communities with a CVRS above the 60th percentile threshold were designated as eligible for CHW deployment. Among eligible communities, participation required local health department agreement and identification of suitable CHW candidates; approximately 71% of eligible communities ultimately participated.

3.2. Data Sources

Vaccination records were obtained from the state's immunization information system (IIS), which captures provider-reported vaccinations for all children regardless of insurance status. The analytic dataset included 68,714 children born between January 1, 2016, and December 31, 2019, who were residents of the 412 study communities. For each child, we observed the complete vaccination history through age 35 months, ensuring that all children in the cohort had the opportunity to complete the recommended series by the end of the observation period (December 2021).

Community-level covariates were obtained from the American Community Survey (2015–2019 5-year estimates), the CDC's Social Vulnerability Index, the HRSA Area Health Resource File, and state administrative records. Individual-level covariates included child age, sex, birth order, insurance type, and maternal age at delivery. The CVRS and its components were obtained from the state health department's program records.

3.3. Outcome Measures

The primary outcome was completion of the combined 7-vaccine series by 24 months of age. This composite measure, recommended by the CDC, includes 4 or more doses of diphtheria-tetanus-acellular pertussis (DTaP), 3 or more doses of inactivated poliovirus (IPV), 1 or more doses of MMR, the full hepatitis B series, 1 or more doses of varicella, 4 or more doses of pneumococcal conjugate (PCV), and 2 or more doses of hepatitis A vaccine. The secondary outcome was receipt of the first dose of MMR vaccine by 15 months, a key indicator of timely vaccination for a disease with documented outbreaks linked to hesitancy.

3.4. Identification Strategies

3.4.1. Fuzzy Regression Discontinuity Design

The CVRS-based assignment rule creates a natural regression discontinuity. Let c_i denote the CVRS for community i, and let c_0 = 60th percentile denote the eligibility threshold. Define $T_i = 1$ if community i participated in the CHW program and $Z_i = 1\{c_i \geq c_0\}$ as the indicator for being above the threshold. In the fuzzy RD framework, Z_i serves as an instrument for T_i:

First stage: $T_i = \alpha_0 + \alpha_1 Z_i + f(c_i - c_0) + W_i^T\gamma + v_i$

Second stage: $Y_i = \beta_0 + \beta_1 \hat{T}_i + g(c_i - c_0) + W_i^T\delta + \varepsilon_i$

where $f(\cdot)$ and $g(\cdot)$ are flexible functions of the centered running variable (estimated using local linear regression with a triangular kernel), W_i is a vector of pre-determined covariates, and β_1 is the LATE of the CHW program at the eligibility threshold. The bandwidth was selected using the Imbens-Kalyanaraman (IK) optimal bandwidth selector and the Calonico-Cattaneo-Titiunik (CCT) robust bias-corrected procedure; results are reported for both.

3.4.2. Instrumental Variable Analysis

Independent of the RD design, we exploited geographic variation in access to the CHW training infrastructure as an instrument for program participation. The state established six regional CHW training centers, and their locations were determined by the availability of existing community college campuses with health education programs—a factor plausibly unrelated to community-level vaccination behavior. We defined the instrument as the log-transformed driving distance from each community's population centroid to the nearest training center.

The identifying assumptions are: (1) relevance—communities closer to training centers were more likely to participate in the CHW program, because recruitment and supervision of CHWs was facilitated by proximity to training resources; (2) independence—conditional on observed covariates, distance to training centers is unrelated to unmeasured determinants of vaccination uptake; (3) exclusion—distance to training centers affects vaccination only through its effect on CHW program participation, not through direct effects on vaccination behavior. We assess these assumptions empirically where possible and discuss threats to validity in the limitations section.

The 2SLS estimation proceeded as:

First stage: $T_i = \pi_0 + \pi_1 \cdot \log(Distance_i) + X_i^T\theta + \eta_i$

Second stage: $Y_i = \beta_0 + \beta_1\hat{T}_i + X_i^T\lambda + \varepsilon_i$

Standard errors were clustered at the community level to account for within-community correlation. The first-stage F-statistic was used to assess instrument strength, with the conventional threshold of $F > 10$ and the more conservative Montiel Olea-Pflueger effective F-statistic reported.

3.5. Robustness Checks and Sensitivity Analyses

We conducted a battery of robustness checks for both identification strategies. For the RD design: (1) McCrary density test for manipulation of the running variable, (2) balance tests on pre-determined covariates at the threshold, (3) sensitivity to bandwidth choice across a range from 0.5× to 2× the optimal bandwidth, (4) placebo tests using false thresholds at the 40th and 80th percentiles, and (5) donut-hole specifications that exclude observations within narrow bands of the threshold. For the IV analysis: (1) over-identification tests using alternative instruments (log-distance to second-nearest center, state legislative district fixed effects interacted with distance), (2) reduced-form estimates, (3) Anderson-Rubin confidence intervals robust to weak instruments, and (4) sensitivity analysis to violations of the exclusion restriction using the methodology of Conley, Hansen, and Rossi (2012).

4. Results

4.1. Descriptive Statistics

The analytic sample comprised 68,714 children across 412 communities. The overall 7-vaccine series completion rate by 24 months was 67.2%. The MMR first-dose rate by 15 months was 81.4%. Communities that participated in the CHW program (n = 117) had baseline vaccination rates approximately 7.6 percentage points lower than non-participating communities (61.8% vs. 69.4%), reflecting the program's targeting of high-risk areas. Participating communities also had higher poverty rates, greater proportions of racial and ethnic minority residents, and lower densities of healthcare providers.

Table 1: Community Characteristics by CHW Program Participation Status

Characteristic	Participants (n=117)	Non-Participants (n=295)	p-value
7-series completion (%)	61.8	69.4	< 0.001
MMR-1 by 15 mo (%)	76.3	83.4	< 0.001
Median household income ($)	46,870	55,410	< 0.001
% below poverty line	19.2	13.1	< 0.001
HPSA designation (%)	66.7	39.3	< 0.001
Mean CVRS (percentile)	75.1	41.9	< 0.001

HPSA = Health Professional Shortage Area; CVRS = Community Vaccination Risk Score. p-values from two-sample t-tests or chi-square tests as appropriate.

4.2. Naïve OLS Estimates

Before presenting the causal estimates, we report naïve OLS regression results for comparison. After adjusting for the full set of individual- and community-level covariates, the OLS-estimated association between CHW program participation and 7-vaccine series completion was 1.9 percentage points (95% CI: 0.3–3.5, p = 0.021). For MMR first-dose by 15 months, the OLS estimate was 1.2 percentage points (95% CI: –0.2–2.6, p = 0.089). As anticipated, these estimates are substantially smaller than the quasi-experimental estimates presented below, consistent with residual negative confounding from the program's preferential targeting of high-risk communities.

4.3. Regression Discontinuity Estimates

The McCrary density test revealed no evidence of manipulation of the CVRS at the eligibility threshold (p = 0.38), supporting the integrity of the assignment mechanism. Balance tests confirmed that 11 of 12 pre-determined covariates were statistically balanced at the threshold (the single imbalanced covariate—proportion of Hispanic residents—was included as a control in sensitivity analyses with no meaningful change in results).

The first-stage relationship was strong: crossing the CVRS threshold was associated with a 36.7 percentage point increase in the probability of CHW program participation (F = 44.1, p < 0.001), confirming the fuzzy RD as a valid design. The estimated LATE for the 7-vaccine series completion was 8.6 percentage points (CCT robust 95% CI: 3.4–13.8, p = 0.001) using the IK-optimal bandwidth of 11.8 percentile points. For MMR first-dose by 15 months, the LATE was 6.2 percentage points (95% CI: 2.1–10.3, p = 0.003).

Table 2: Causal Effect Estimates: CHW Program on Vaccination Outcomes

Method	Outcome	Estimate (pp)	95% CI	p	First-Stage F
OLS	7-series	1.9	0.3, 3.5	0.021	—
OLS	MMR-1	1.2	–0.2, 2.6	0.089	—
Fuzzy RD	7-series	8.6	3.4, 13.8	0.001	44.1
Fuzzy RD	MMR-1	6.2	2.1, 10.3	0.003	44.1
IV (distance)	7-series	7.1	2.8, 11.4	0.001	29.4
IV (distance)	MMR-1	5.1	1.4, 8.8	0.007	29.4

pp = percentage points; CI = confidence interval; RD = regression discontinuity; IV = instrumental variable. RD estimates use CCT robust bias-corrected inference. IV standard errors clustered at the community level.

4.4. Instrumental Variable Estimates

The first-stage relationship between log-distance to the nearest CHW training center and program participation was strong and negative: each unit increase in log-distance was associated with a 13.1 percentage point decrease in participation probability (F = 29.4, well above conventional thresholds). The Montiel Olea-Pflueger effective F-statistic was 26.2, exceeding the critical value for 10% maximal IV size.

The IV estimate of the causal effect of CHW program participation on 7-vaccine series completion was 7.1 percentage points (95% CI: 2.8–11.4, p = 0.001). For MMR first-dose by 15 months, the IV estimate was 5.1 percentage points (95% CI: 1.4–8.8, p = 0.007). Anderson-Rubin confidence intervals, which are robust to weak instrument concerns, yielded similar inference: the 95% AR interval for the 7-series effect was [2.2, 12.6]. The over-identification test using distance to the second-nearest center as an additional instrument failed to reject the null of instrument validity (Hansen J p = 0.31).

4.5. Robustness and Sensitivity

The RD estimates were stable across bandwidth choices ranging from 8 to 17 percentile points (the optimal was 11.8), with point estimates for the 7-series outcome ranging from 7.1 to 10.2 percentage points. Placebo tests at false thresholds (40th and 80th percentiles) yielded statistically insignificant estimates, as expected. The donut-hole specification excluding communities within 2 percentile points of the threshold produced a point estimate of 8.2 (95% CI: 2.4–14.0), consistent with the main result.

For the IV analysis, the Conley-Hansen-Rossi sensitivity analysis indicated that the 7-series estimate would remain statistically significant at the 5% level under violations of the exclusion restriction up to approximately 38% of the magnitude of the reduced-form effect, providing substantial robustness to moderate violations. Specifications including state legislative district fixed effects, rural-urban continuum controls, and interactions between distance and community size produced estimates ranging from 5.8 to 8.4 percentage points, all statistically significant. A specification including birth-year fixed effects to account for potential pandemic-related disruptions in the 2020–2021 observation window produced estimates virtually identical to the main results.

5. Discussion

This study demonstrates that community-based CHW vaccine education programs produce meaningful causal increases in childhood vaccination rates, with estimated effects of 7.1–8.6 percentage points for the combined 7-vaccine series and 5.1–6.2 percentage points for timely MMR vaccination. The concordance of estimates from two independent quasi-experimental designs—fuzzy RD and IV analysis—substantially strengthens the causal interpretation and addresses a critical evidence gap in the vaccine hesitancy intervention literature.

5.1. Interpretation of Effect Magnitudes

The estimated effects are clinically and policy-relevant. An 8-percentage-point increase in 7-series completion, if generalized to the approximately 3.6 million children born annually in the United States, would translate to roughly 288,000 additional fully vaccinated children per year. From a herd immunity perspective, even the lower-bound estimate of 3.4 percentage points would meaningfully narrow the gap between observed and target vaccination rates in high-risk communities. These effects are substantially larger than the naïve OLS estimates (1.9 pp for 7-series completion), confirming that residual confounding from targeting attenuates observational associations and highlighting the importance of using appropriate causal inference methods.

It is important to note that both the RD and IV estimates identify local average treatment effects for specific subpopulations: compliers at the eligibility threshold (RD) and compliers whose participation is influenced by distance to training centers (IV). These may differ from the average treatment effect across all communities. Nevertheless, the concordance of the two estimates across different complier populations provides reassurance that the effects are not limited to a narrow subgroup.

5.2. Implications for Program Design and Evaluation

Our findings have several implications for the design and evaluation of vaccine hesitancy interventions. First, the strong first-stage relationship between distance to CHW training centers and program participation suggests that infrastructure investment in training capacity is a rate-limiting step in CHW program deployment. Expanding the number and geographic distribution of training centers could increase program reach, particularly in remote rural communities that are currently underserved.

Second, the disparity between the OLS and quasi-experimental estimates illustrates a fundamental challenge in public health program evaluation: programs that are targeted at the most in-need populations will appear less effective in naïve comparisons precisely because they are well-targeted. This confounding by indication is pervasive in public health but insufficiently recognized. We advocate for the routine application of quasi-experimental methods in program evaluation whenever randomization is not feasible, and for the pre-specification of identification strategies in program implementation protocols.

Third, the triangulation of results from two independent identification strategies provides a model for strengthening causal claims in observational research. When multiple sources of plausibly exogenous variation in treatment assignment yield concordant estimates, the joint probability of bias is multiplicatively lower than for any single design. We encourage researchers to seek such triangulation opportunities when evaluating public health interventions.

5.3. COVID-19 Pandemic Considerations

Our study period partially overlaps with the COVID-19 pandemic, which disrupted both vaccination delivery and CHW outreach activities. Several considerations are relevant. First, childhood vaccination rates declined nationally during 2020, particularly during the early months of the pandemic when stay-at-home orders reduced well-child visits. This disruption affected both treated and control communities, but CHW programs may have partially buffered the decline by maintaining contact with families through virtual outreach. Second, the pandemic may have altered the composition of the complier population, as communities with stronger pre-existing public health infrastructure may have been better positioned to sustain CHW activities through the disruption. Our birth-year fixed effects specification addresses secular trends but cannot fully account for these compositional shifts. Future work should examine whether CHW programs exhibited differential resilience during the pandemic.

5.4. Limitations

Several limitations should be considered. First, the RD design identifies a LATE at the eligibility threshold, which may not generalize to communities far from the threshold. Communities at the margin of eligibility may differ from those with very high or very low risk scores in ways that moderate the treatment effect. Second, the exclusion restriction for the distance-based IV—that proximity to training centers affects vaccination only through CHW program participation—could be violated if training center locations are correlated with other health infrastructure investments. We note that training centers were located at pre-existing community colleges rather than purpose-built facilities, mitigating this concern, and the sensitivity analysis suggests that moderate violations would not alter our conclusions.

Third, we cannot observe the specific mechanisms through which the CHW program increased vaccination: whether through information provision, trust-building, logistical assistance, or some combination. Mechanistic decomposition would require different designs, such as factorial experiments or mediation analysis with valid instruments for the mediator. Fourth, the analysis is limited to a single Midwestern state, and the results may not generalize to settings with different demographic compositions, healthcare infrastructures, or political landscapes. Fifth, while the IIS provides comprehensive vaccination records, it may undercount doses administered out of state or by nontraditional providers.

6. Conclusion

Community health worker-delivered vaccine education programs produce meaningful causal increases in childhood vaccination rates, with effects on the order of 5–9 percentage points for key vaccination outcomes.

The concordance of instrumental variable and regression discontinuity estimates, each exploiting distinct sources of variation in program assignment, provides robust causal evidence in a domain where randomized trials have been scarce. Naïve observational comparisons substantially underestimate the true effect due to confounding by program targeting, underscoring the importance of applying rigorous causal inference methods in public health program evaluation.

As vaccine hesitancy continues to challenge global immunization goals—a challenge intensified by the politicization of vaccination during the COVID-19 pandemic—the need for evidence-based, scalable interventions is urgent. Our results support continued investment in CHW programs as an effective strategy for improving vaccination uptake in high-risk communities. Equally important, our methodological demonstration highlights the availability and feasibility of quasi-experimental tools that can generate credible causal evidence from routinely collected administrative data, lowering the barrier to rigorous program evaluation in public health practice.

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