

Smoke-inducing Fuel Use and Early Childhood Development: Accounting for Multilevel Heterogeneity in Malnutrition and Respiratory Illness among Bangladeshi Children

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Research Article

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Smoke-inducing Fuel Use and Early Childhood Development: Accounting for Multilevel Heterogeneity in Malnutrition and Respiratory Illness among Bangladeshi Children

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Abstract

Background

Household reliance on smoke-producing cooking fuel remains a leading environmental risk factor for child health in low- and middle-income countries. This study assesses the association between smoke-producing fuel adoption and child stunting, wasting, underweight, and Acute Respiratory Infection (ARI) in Bangladesh while accounting for socioeconomic and geographic confounding factors.

Methods

Using pooled data from five waves of the Bangladesh Demographic and Health Survey (2007–2018), this study examined the relationship between smoke-producing fuel adoption and child health outcomes using Propensity Score Matching (PSM) and multilevel mixed-effects logistic regression. PSM was applied to reduce selection bias in household fuel choice, while multilevel regression accounted for the hierarchical structure of the survey data.

Results

In the fully adjusted pooled multilevel model, fuel choice was not significantly associated with any outcome, indicating that the observed association between fuel type and child health was largely attributable to confounding by household wealth, maternal education, and geography. However, year-stratified PSM identified a significant positive association between smoke-producing fuel use and stunting and underweight in more recent survey waves (2014–2018), suggesting an independent association with chronic malnutrition beyond socioeconomic confounding. Formal interaction tests found no significant differences by child sex, residence, or wealth.

Conclusion

Clean fuel transition appears to be a relevant, though not singularly sufficient, component of strategies to reduce chronic child malnutrition in Bangladesh. Such interventions should be pursued alongside investments in household wealth, maternal education, and health infrastructure, incorporating feasible approaches such as subsidy programs and improved cookstoves.

Keywords: Solid Cooking Fuel, Early Childhood Health, Malnutrition, Acute Respiratory Infections, Propensity Score Matching, Instrumental Variables, Bangladesh

Background

Household air pollution (HAP) remains one of the most significant yet underrecognized public health challenges in low- and middle-income countries. Despite global progress toward cleaner energy transitions, approximately 2.4 billion people still rely on polluting cooking fuels such as firewood, agricultural residues, charcoal, and animal dung for daily household energy needs [1]. The combustion of these fuels in traditional and inefficient stoves produces substantial concentrations of harmful pollutants, including fine particulate matter (PM_{2.5}), carbon monoxide (CO), nitrogen oxides, and polycyclic aromatic hydrocarbons, often exceeding recommended indoor air quality standards [2].

Exposure to these pollutants has profound implications for child health, particularly through both respiratory and nutritional pathways. Biologically, chronic inhalation of particulate matter can trigger systemic inflammation, oxidative stress, and impaired immune function, which may compromise nutrient absorption and increase metabolic demands. Repeated respiratory infections caused by prolonged HAP exposure can further reduce appetite, impair digestion, and divert energy from growth toward immune responses. Moreover, carbon monoxide exposure reduces oxygen transport by binding to hemoglobin, limiting oxygen availability to growing tissues and potentially impairing physical and cognitive development. These mechanisms create a pathway through which HAP may contribute to childhood stunting, wasting, and underweight conditions. The burden of HAP disproportionately affects women and children, who spend longer periods near cooking environments and therefore experience higher cumulative exposure. The World Health Organization estimates that around seven million premature deaths occur annually due to air pollution, with household sources contributing substantially to this burden [1]. In Asia, HAP accounts for nearly 12% of total mortality and is strongly associated with acute lower respiratory infections (ALRIs), a leading cause of child mortality [3].

In Bangladesh, the health implications of HAP are especially severe. Although cleaner cooking alternatives have gradually expanded, biomass remains the dominant household energy source, particularly in rural and low-income settings [4]. This continued dependence contributes significantly to indoor pollution exposure and accounts for nearly 4% of the national disease burden [5]. Bangladesh also faces persistent child malnutrition, where approximately 45% of under-five deaths are linked to undernutrition. Stunting, wasting, and underweight remain critical public health concerns due to their long-term effects on physical growth, cognitive development, and survival [6, 7]. Acute Respiratory Infection (ARI) remains another major threat to child survival in Bangladesh and other developing countries. Existing evidence suggests that HAP increases the risk of respiratory morbidity through repeated exposure to toxic smoke and reduced lung function [8, 9]. However, findings remain inconsistent across contexts, with some studies reporting significant protective effects of clean fuel adoption, while others find weak or insignificant associations after adjusting for socioeconomic factors [10, 11].

Although prior studies have examined the relationship between household fuel use and child health, much of the literature has treated nutritional outcomes and respiratory illnesses as separate dimensions of health, limiting a comprehensive understanding of their interconnected effects. For instance, studies have linked household air pollution to childhood stunting and underweight through repeated infections and impaired nutrient absorption [12, 13], while others have primarily focused on respiratory outcomes such as acute respiratory infection (ARI) and pneumonia [14, 15]. Furthermore, many existing studies rely on conventional regression techniques, which may inadequately address the endogeneity of household fuel choice and the non-random selection of clean fuel adoption [16, 17]. Since clean fuel adoption is strongly correlated with household wealth, parental education, and housing conditions, estimates based on standard regression may suffer from omitted variable bias and selection effects, potentially overestimating or underestimating the true health consequences of smoke-producing fuel exposure [18, 19].

This study addresses these gaps by jointly examining child malnutrition (stunting, wasting, and underweight) and respiratory illness (ARI) within a unified analytical framework. Using pooled cross-sectional data from the Bangladesh Demographic and Health Survey (2007–2018), I apply Propensity Score Matching (PSM) estimation to reduce observable and unobservable biases in fuel

adoption. By this quasi-experimental approach, this study provides more robust estimates of the Average Treatment Effect on the Treated (ATT) and contributes new evidence on the multidimensional health consequences of smoke-producing fuel use in Bangladesh.

Materials and Method

Study Population and Data Overview

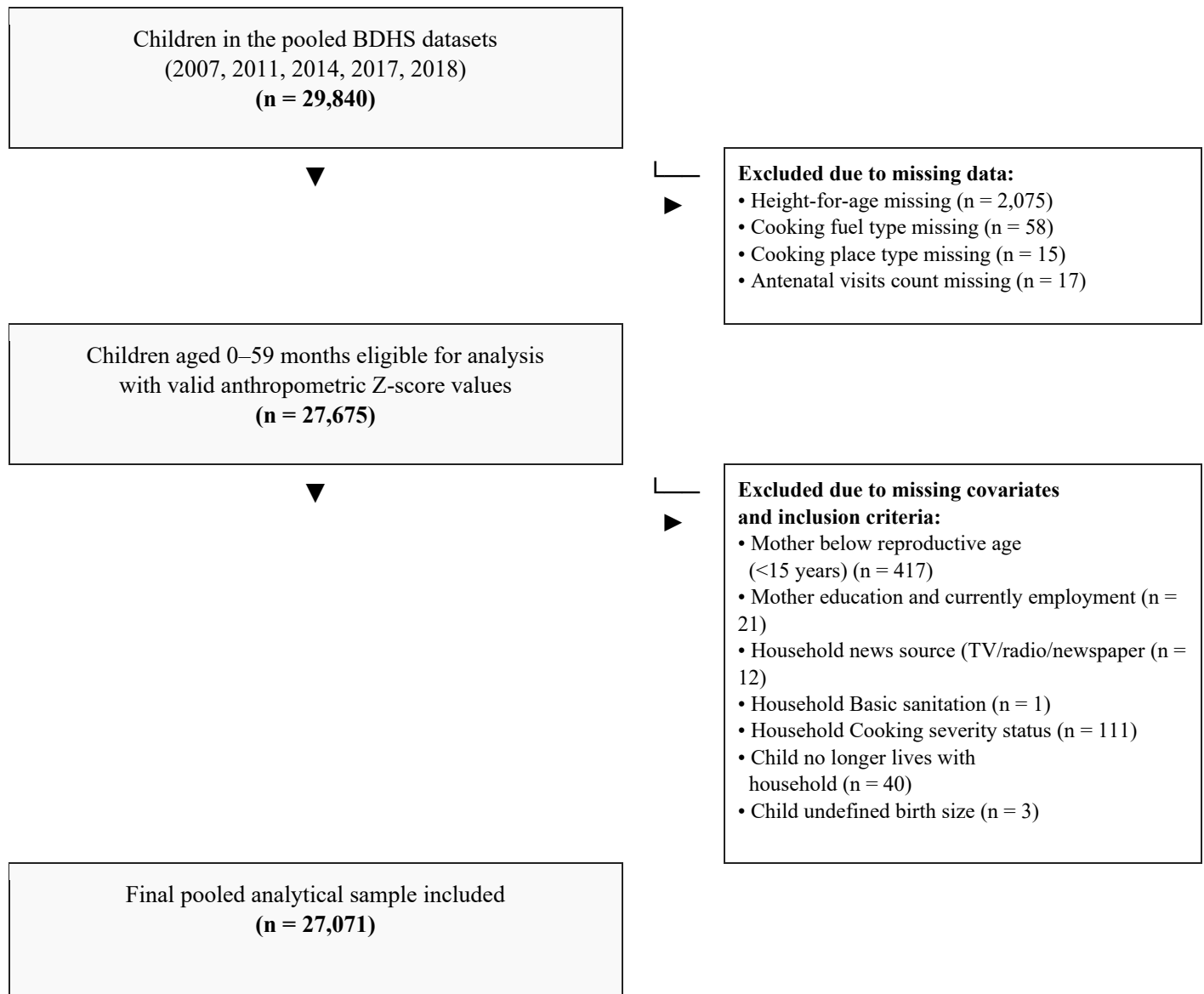
This study utilized a pooled, nationally representative sample from the Bangladesh Demographic and Health Surveys (BDHS) conducted in five waves: 2007, 2011, 2014, 2017, and 2018. The surveys utilized a two-stage stratified sampling method covering all administrative divisions of Bangladesh. In the first stage, 675 primary sampling units (PSUs) were selected with probability proportional to size; in the second stage, a systematic sample of households was selected from each PSU. For this study, I only considered consisted of ever-married women of reproductive age (15–49 years) and their children under the age of five. The surveys obtained ethical approval from the National Research Ethical Review Committees of the Bangladesh Medical Research Council and the Institutional Review Board of Macro International. Informed consent was obtained from all participants prior to data collection.

Data Collection

Data were collected by trained interviewers using specialized questionnaires including a household questionnaire, a woman's questionnaire, a men's questionnaire, a community questionnaire, and two verbal autopsy questionnaires. The household questionnaire was used to identify household characteristics and fuel types, and it also recorded height and weight measurements for children under five. The woman's questionnaire was used to collect information on socio-demographic characteristics, reproductive history, and maternal work, while also recording the birth history, immunization status, and illness history of the index child. Additionally, the place of cooking was ascertained via the household questionnaire to determine if cooking was performed in the living house, a separate building, or outdoors, where cooking within the living house was considered exposure to indoor cooking. The study excluded the incomplete information of height-to-weight

measurements and undetermined value labels like cooking fuel types, cooking places and antenatal care visits (figure 1). Also due to excluding criteria, 417 women were dropped due to being age below reproductive threshold (<15 years), 40 children excluded due to their migration from household and 3 were out due to undefined birth size.

Figure 1. Flowchart of sample selection and exclusion criteria for the pooled BDHS analysis.



Exposure

The primary exposure variable is smoke-producing fuel adoption, which was ascertained in the household questionnaire by asking what type of fuel the household mainly uses for cooking. Following WHO guidelines [20], primary cooking fuels are classified into binary "healthy" (electricity, LPG, natural gas, biogas) and "unhealthy" (biomass, charcoal, coal, kerosene) cohorts based on emission targets. Healthy options leverage efficient combustion to remain below health-damaging PM2.5 and CO thresholds, whereas unhealthy alternatives generate severe indoor exposure. Kerosene is classified as unhealthy because empirical evidence shows its emissions of fine particulates and carcinogenic compounds match or exceed the toxicological hazards of solid biomass. A comprehensive set of socio-demographic and environmental control variables was included to adjust for potential confounders, including child characteristics such as age (categorized in 12-month intervals), sex, and birth size. I also adjusted for maternal and healthcare factors like mother's age at birth and antenatal care utilization, as well as household characteristics including wealth quintile, geographical division, urban/rural residence, and household smoking status. Environmental factors such as sanitation facility type, water treatment practices, and the presence of handwashing facilities were also incorporated into the model.

2.4 Outcome (Nutritional Status and ARI)

The primary outcome variables for this study include child nutritional status and respiratory morbidity. Nutritional status is assessed using three anthropometric indicators: stunting (height-for-age), wasting (weight-for-height), and underweight (weight-for-age). These anthropometric Z-scores were obtained directly from the DHS datasets—where they are systematically computed using the WHO standard reference flags—rather than recalculated by the authors. Children whose respective Z-scores fall below minus two standard deviations (-2 SD) from the median of the WHO Child Growth Standards reference population [21] are classified as undernourished. This measure of stunting, or low height-for-age, is a critical indicator of chronic undernutrition, reflecting a failure to receive adequate nutrition over a prolonged period. To ensure measurement accuracy, weight was recorded using lightweight SECA digital scales, and height or length was measured using Shorr Productions boards. Recumbent length was recorded for children under age two or shorter than 85 cm, while standing height was measured for all other children. Crucially, these standardized anthropometric measurement protocols, equipment specifications, and field-worker training guidelines remained strictly uniform across all pooled survey waves, ensuring exceptional

data quality and rigorous temporal comparability across the entire study period [22, 23]. In addition to these anthropometric measures, the study includes Acute Respiratory Infection (ARI) symptoms as a clinical outcome, identified through maternal reports of a cough accompanied by short, rapid breathing within the two weeks preceding the survey. However, a key limitation of this metric is its reliance on subjective maternal recall, which is inherently susceptible to recall bias, potential over- or under-reporting based on maternal health literacy, and a lack of clinical diagnostic validation.

Statistical Analysis

To address potential selection bias arising from non-random adoption of cooking fuel type, I employed Propensity Score Matching (PSM) as my primary identification strategy. The systemic disparities between households adopting smoke-producing fuels and those using clean energy often result in sample selection error, as energy preferences vary based on household-specific needs [24]. PSM addresses this dimensionality issue by condensing observed covariates into a single propensity score, effectively pairing smoke-producing fuel adopters and non-adopters with similar observable profiles to approximate the conditions of a randomized comparison [25]. Following the approach of previous studies examining environmental health impacts [26, 27], I employed nearest-neighbor matching (5 neighbors, caliper = 0.001) to pair households with similar characteristics, with common support imposed to ensure comparability across matched groups. Balancing tests were conducted post-matching to confirm that covariate differences between treated and matched control groups were minimized [28], and sensitivity to unobserved confounding was assessed using Rosenbaum bounds [29]. Given the pooled, repeated cross-sectional nature of my data, propensity scores and matching were estimated separately by survey year to account for temporal shifts in fuel access and health trends [30]. To complement the PSM analysis and account for the hierarchical structure of the data — children nested within households, nested within primary sampling units/clusters — I additionally estimated a multivariable multilevel ordered logistic regression model. This approach allows outcome severity (e.g., normal, moderate, and severe malnutrition categories based on WHO anthropometric thresholds) to be modeled as an ordinal outcome while explicitly accounting for within-cluster correlation via random intercepts at the household and/or cluster level, offering an additional robustness check that is not dependent on the matching-based counterfactual construction used in PSM.

All analyses were conducted using STATA 17.0. The 'svy' command was used to adjust for the two-stage sampling design with appropriate sampling weights, and standard errors were clustered at the community level in all non-multilevel models to ensure robust statistical inference.

Propensity Score Matching (PSM) and ATT Estimation

To address selection bias arising from systematic disparities between households using solid versus non-smoke-producing fuels, I employ Propensity Score Matching (PSM). The propensity score—the conditional probability of a household adopting smoke-producing fuel—is derived from a selection function:

$$Fuel_i^* = X_i\alpha + \varepsilon_i \text{ where } Fuel_i = \begin{cases} 1 & \text{if } Fuel_i^* > 0 \\ 0 & \text{otherwise} \end{cases}$$

where $Fuel_i^*$ is a latent variable representing the utility derived from fuel choice. If the utility is positive, the household prefers smoke-producing fuels ($= 1$). The vector includes covariates hypothesized to jointly influence fuel choice and child health: child age and sex, maternal age, maternal education and employment status, household wealth, sanitation and drinking water treatment, antenatal care utilization, cooking location, media exposure, and residence/region. Propensity scores were estimated separately by survey wave (2007–2018) using a probit model to balance observed covariates between smoke-producing and clean-fuel households within each wave. I estimate these scores using a probit model to ensure the balancing of observed covariates between groups [24].

Assessing the Average Treatment Effect on the Treated (ATT)

To isolate the impact of fuel choice on child health outcomes Y , I define Y_{1i} as the health outcome for a child in a treated household (smoke-producing fuel) and Y_{0i} as the outcome for a child in a control household. The primary goal is to estimate the Average Treatment Effect on the Treated (ATT):

$$ATT(X) = E\{Y_{1i} - Y_{0i} | T_i = 1\}$$

Since the counterfactual for treated children is unobserved, I match each treated child with a control child whose propensity score falls within a specified range. This approach relies on the conditional mean independence assumption (i.e., $E(Y_0 | T = 1, p(X)) = E(Y | p(X))$), which implies that once I control for the propensity score $p(X)$ —the conditional probability of receiving

treatment given observed covariates—the assignment to the treatment group is effectively random among households with comparable observed characteristics. [25]. In this study, I utilize the Nearest Neighbor Matching method. To maximize the utility of my large sample size and reduce matching bias, I restrict matching to the region of common support and establish a strict caliper of 0.001 [31]. All estimations were performed using the `psmatch2` command in Stata.

Mixed-effect analysis

The BDHS uses a multi-stage cluster sampling technique, whereby data are hierarchical — children and mothers are nested within households, and households are nested within clusters (enumeration areas). Given this hierarchical structure, children and households within the same cluster are likely to be more similar to one another than to children and households in other clusters, violating the standard regression assumption of independent observations and equal variance across clusters. This creates a need to explicitly model between-cluster variability. Accordingly, a two-level multilevel mixed-effects logistic regression was used to estimate the effects of individual/household-level and community-level determinants on child stunting, wasting, underweight, and ARI, complementing the Propensity Score Matching analysis presented in this paper.

For a binary outcome O_{icp} (child i , cluster p), the linear predictor of Equation (3) reduces to the standard two-level mixed-effects logistic form:

$$\text{logit}[Pr(O_{icp} = 1)] = \beta_0 + \beta_1 S_{icp} + \beta_2 X_{icp} + u_p$$

where S_{icp} is smoke-producing fuel adoption, X_{icp} is the vector of individual/household- and community-level covariates depending on the model specification, and u_p is the cluster-level random intercept, $u_p \sim N(0, \sigma^2_u)$

Identification Strategy and Methodological Considerations

I initially considered an instrumental variable (IV) approach, using parental education as an instrument for smoke-producing fuel adoption, based on the premise that educational attainment predicts the transition to clean energy through increased health literacy and socioeconomic aspirations, while plausibly not directly affecting child growth except through household fuel choice. However, on closer empirical and theoretical examination, I found that this instrument did not satisfy the exclusion restriction with sufficient confidence to support a causal interpretation. Specifically, diagnostic checks indicated that the relationship between parental education and fuel adoption in my sample was not robustly consistent with the hypothesized mechanism, suggesting that parental education is likely correlated with regional, cultural, and infrastructural factors that influence fuel choice through pathways unrelated to household energy transition — for example, local fuel market availability, migration patterns, and community-level infrastructure, all of which could independently affect child health outcomes [24]. This is consistent with concerns raised in the broader literature that parental education may directly influence child health through health-seeking behavior, nutritional knowledge, and healthcare utilization, independent of any effect on fuel choice [32, 33], thereby violating the exclusion restriction required for valid instrumental variable estimation.

Given these concerns, I did not rely on an instrumental variable approach. As an alternative candidate, I also considered household media exposure — a composite index derived via Principal Component Analysis from newspaper, radio, and television exposure — as an instrument for fuel adoption. However, diagnostic testing indicated that this candidate instrument was similarly unsuitable: although correctly signed, the first-stage relationship was weak (implied F-statistic ≈ 5.6 , below conventional strong-instrument thresholds), and reduced-form estimates showed media exposure remained significantly associated with child stunting and underweight status even after controlling for the full set of household, individual, and community-level covariates, indicating a likely direct pathway to child health independent of fuel choice and a violation of the exclusion restriction. Having found no credible instrument within this dataset for this research question, I instead adopted Propensity Score Matching as my primary identification strategy, complemented by multilevel mixed-effects logistic regression to account for the hierarchical structure of the data. I acknowledge that, unlike a valid instrumental variable approach, neither PSM nor multilevel modeling can fully rule out bias from unobserved confounding, as both rely on adjustment for observable characteristics rather than a source of exogenous variation. I attempted to formally assess the sensitivity of my matching estimates to potential hidden bias; however, standard sensitivity bound approaches (e.g., Rosenbaum bounds) rely on rank-based tests designed for continuous outcomes and did not yield informative results for my binary health outcomes, for which Mantel-Haenszel-type bounds would be more appropriate but

were beyond the scope of the present analysis. I therefore discuss the possibility of residual unobserved confounding explicitly as a limitation of both identification strategies (see Limitations section), rather than relying on a formal sensitivity bound. I believe this combined approach nonetheless offers a more credible and transparent basis for my findings than an instrumental variable strategy whose core identifying assumptions I could not adequately support for either candidate instrument.

Results

Sample demographics

This study included a total of 27,071 children under the age of 5 years in the analysis (Table 1) with an almost equal proportion of boy (51.4%; $n = 13,917$) and girl (48.6%; $n = 13,154$). About 86% ($n = 23,282$) children were from households which prefer unhealthy fuel for cooking purpose and remaining (14%; $n = 3789$) were from households using non-smoke inducing fuel types for cooking. Characteristics of household are presented in Table 1. About 67.4% ($n = 18236$) of the child from the rural households, whereas about 32.6% ($n = 8835$) of the child were from the urban area. In the sample, almost 34.3% ($n=9277$) of mothers do not have at least primary level education.

Table 1 Characteristics of the children under 5 years by household fuel type

Characteristics	Total (N = 27,071)	Smoke-producing fuel (n = 23,282)	Clean fuel (n = 3789)
year of interview			
2007	5066 (18.7)	4570 (19.6)	496 (13.1)
2011	7477 (27.6)	6599 (28.3)	878 (23.2)
2014	6787 (25.1)	5868 (25.2)	919 (24.3)
2017	4069 (15.0)	3357 (14.4)	712 (18.8)
2018	3672 (13.6)	2888 (12.4)	784 (20.7)
Mother education			
No education	9277 (34.3)	8568 (36.8)	709 (18.7)
Have at least primary education or more	17794 (65.7)	14714 (63.2)	3080 (81.3)
Children Gender			
Male	13917 (51.4)	11932 (51.2)	1985 (52.4)

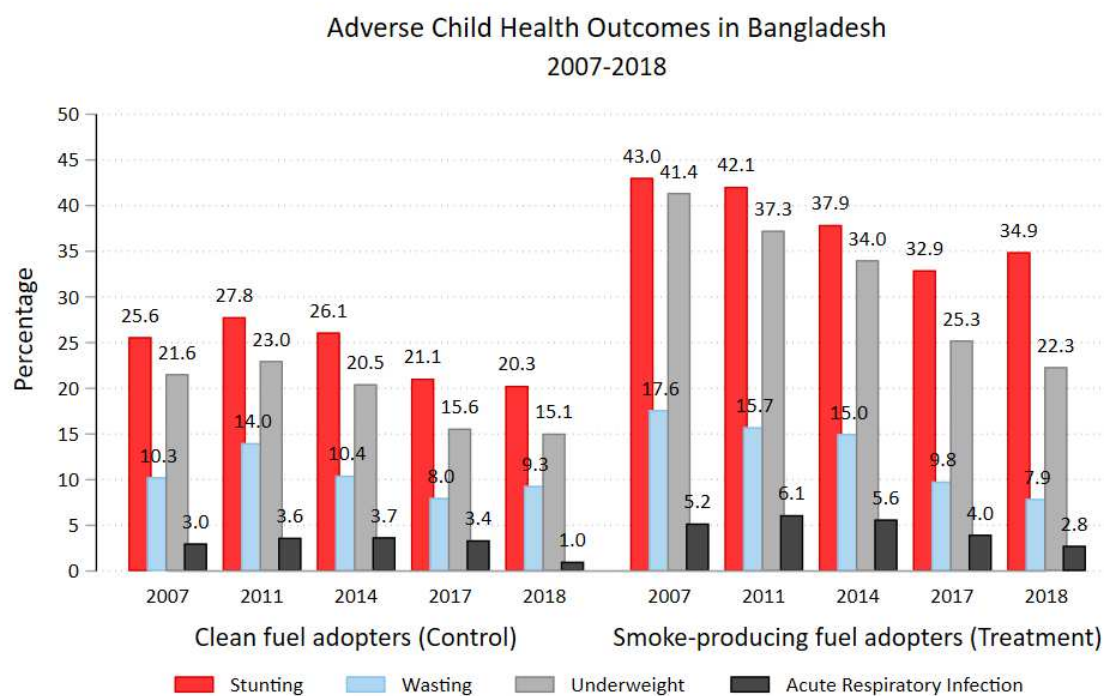
Female	13154 (48.6)	11350 (48.8)	1804 (47.6)
Child's age group in months			
0-12 months	5478 (20.2)	4729 (20.3)	749 (19.8)
13-24 months	5932 (21.9)	5103 (21.9)	829 (21.9)
25-36 months	5302 (19.6)	4577 (19.7)	725 (19.1)
37-48 months	5439 (20.1)	4644 (19.9)	795 (21.0)
49-59 months	4920 (18.2)	4229 (18.2)	691 (18.2)
Mother age during child birth			
below 20 years	7900 (29.2)	7026 (30.2)	874 (23.1)
20-29 years	15106 (55.8)	12836 (55.1)	2270 (59.9)
30-40 years	3827 (14.1)	3193 (13.7)	634 (16.7)
40-49 years	238 (0.9)	227 (1.0)	11 (0.3)
Mother current employment			
No	20306 (75.0)	17304 (74.3)	3002 (79.2)
Yes	6765 (25.0)	5978 (25.7)	787 (20.8)
Media exposure (TV/Radio/Newspaper)			
No exposure	23149 (85.5)	20630 (88.6)	2519 (66.5)
Any exposure	3922 (14.5)	2652 (11.4)	1270 (33.5)
Stunting Status			
no	17072 (63.1)	14203 (61.0)	2869 (75.7)
yes	9999 (36.9)	9079 (39.0)	920 (24.3)
Wasted: WHZ<-2			
no	17794 (86.4)	8568 (85.9)	3389 (89.4)
yes	9277 (13.6)	3283 (14.1)	400 (10.6)
Underweight: WAZ<-2			
no	18504 (68.4)	15441 (66.3)	3063 (80.8)
yes	8567 (31.6)	7841 (33.7)	726 (19.2)

ARI symptoms in the 2 weeks before the survey			
no	25771 (95.2)	22095 (94.9)	3676 (97.0)
yes	1300 (4.8)	1187 (5.1)	113 (3.0)
Household Cooking Place			
Outdoor Cooking	5021 (18.5)	4881 (21.0)	140 (3.7)
Indoor Cooking	22050 (81.5)	18401 (79.0)	3649 (96.3)
Household Smoke Exposure Risk Levels			
High SER	18401 (68.0)	18401 (79.0)	0 (0.0)
Medium SER	4881 (18.0)	4881 (21.0)	0 (0.0)
Low SER	3649 (13.5)	0 (0.0)	3649 (96.3)
Very Low SER	140 (0.5)	0 (0.0)	140 (3.7)
wealth index			
poorest	5813 (21.5)	5809 (25.0)	4 (0.1)
poorer	5312 (19.6)	5288 (22.7)	24 (0.6)
middle	5114 (18.9)	4996 (21.5)	118 (3.1)
richer	5337 (19.7)	4596 (19.7)	741 (19.6)
richest	5495 (20.3)	2593 (11.1)	2902 (76.6)
Basic or limited sanitation			
basic sanitation	10794 (39.9)	8646 (37.1)	2148 (56.7)
limited sanitation	5093 (18.8)	4068 (17.5)	1025 (27.1)
unimproved sanitation	10315 (38.1)	9699 (41.7)	616 (16.3)
open defecation	869 (3.2)	869 (3.7)	0 (0.0)
Treat water before drinking			
No treatment	24486 (90.5)	22198 (95.3)	2288 (60.4)
Some treatment	2585 (9.5)	1084 (4.7)	1501 (39.6)
Antenatal Care During Pregnancy			
no or less 8 visits	17785 (65.7)	15669 (67.3)	2116 (55.8)
8 or more visits	9286 (34.3)	7613 (32.7)	1673 (44.2)
currently breastfeeding			
no	9854 (36.4)	8092 (34.8)	1762 (46.5)

yes	17217 (63.6)	15190 (65.2)	2027 (53.5)
division			
barishal	3077 (11.4)	2945 (12.6)	132 (3.5)
chattogram	5091 (18.8)	4176 (17.9)	915 (24.1)
dhaka	4596 (17.0)	3055 (13.1)	1541 (40.7)
khulna	3055 (11.3)	2860 (12.3)	195 (5.1)
mymensingh	3464 (12.8)	3230 (13.9)	234 (6.2)
rajshahi	3448 (12.7)	3289 (14.1)	159 (4.2)
rangpur	3188 (11.7)	2788 (11.9)	400 (10.5)
sylhet	1158 (4.3)	945 (4.0)	213 (5.6)
type of place of residence			
urban	8835 (32.6)	5606 (24.1)	3229 (85.2)
rural	18236 (67.4)	17676 (75.9)	560 (14.8)

WAZ= weight-to-age Z-score, WHZ= weight-to-height Z-score. Numbers in the parenthesis are percentage (%)

Figure 2: % of children (<5 years) faced adverse health outcomes based on household fuel choice between 2007-2018



Source: Bangladesh Demographic and Health Survey (BDHS)

The study assesses the prevalence of adverse child health outcomes in Bangladesh based on household fuel adoption. Figure 2 presents the comparative trends of stunting, wasting, underweight, and acute respiratory infections between non-solid (control) and smoke-producing fuel (treatment) adopters from 2007 to 2018. The figure illustrates a consistently higher burden of malnutrition and respiratory illness among children in households utilizing smoke-producing fuels, with stunting levels peaking at over 43% in the early survey years compared to a maximum of 29% in the control group.

Multivariable multilevel logistic regression: factors associated with child health outcomes

To assess the extent to which the null association between smoke-producing fuel adoption and child health reported in my primary, fully adjusted model reflects confounding by socioeconomic status and geography, I re-estimated the multilevel logistic regression excluding household wealth index and administrative region — the two covariates most strongly associated with fuel choice throughout my analysis. Table 2 presents the results of this reduced specification.

In marked contrast to the fully adjusted model, smoke-producing fuel adoption was significantly associated with three of the four outcomes once wealth index and region were excluded: stunting (AOR = 1.43, 95% CI: 1.30–1.59), underweight (AOR = 1.46, 95% CI: 1.31–1.62), and ARI (AOR = 1.35, 95% CI: 1.07–1.70). Wasting remained non-significant (AOR = 1.11, 95% CI: 0.97–1.28). This pattern — a null association in the fully adjusted model that becomes significant and positive once wealth and region are dropped — provides direct evidence that household wealth and geographic factors jointly explain a substantial share of the raw, unadjusted relationship between cooking fuel type and child health observed descriptively (Figure 1). This is consistent with my propensity score matching results (Section 3.3) and my instrumental variable diagnostics (Section 2.6), both of which independently identified wealth as the single strongest predictor of fuel choice in this sample.

Several other covariates retained consistent associations with child health outcomes in this specification, broadly mirroring the fully adjusted model. Maternal literacy (at least primary

education) was strongly protective across stunting (AOR = 0.61, 95% CI: 0.58–0.65), underweight (AOR = 0.64, 95% CI: 0.60–0.68), wasting (AOR = 0.84, 95% CI: 0.78–0.91), and ARI (AOR = 0.88, 95% CI: 0.77–1.00). Antenatal care attendance (8+ visits) remained protective across wasting, stunting, and underweight. Sanitation showed a clear dose-response pattern, with unimproved sanitation and open defecation associated with progressively higher odds of stunting and underweight (open defecation: stunting AOR = 1.91, underweight AOR = 1.71) relative to basic sanitation facilities.

Child age continued to show the expected risk gradient for stunting and underweight, peaking at 37–48 months (stunting AOR = 3.90; underweight AOR = 3.17), and a protective pattern for ARI at older ages. Rural residence was associated with modestly higher odds of stunting (AOR = 1.14) and underweight (AOR = 1.15) relative to urban residence — notably, this association was not visible in the fully adjusted model, where region-level variation likely absorbed some of this relationship, consistent with rural residence and administrative region being closely correlated in this sample.

The substantial and consistent secular decline in adverse outcomes by 2017 and 2018 relative to 2007 persisted in this specification, as in the fully adjusted model, indicating this trend is robust to the inclusion or exclusion of wealth and region.

Overall, this sensitivity analysis indicates that the apparent protective effect of clean-fuel adoption on child stunting, underweight, and ARI is substantially, though not entirely, attributable to the socioeconomic and geographic confounding captured by household wealth and administrative region. I report both specifications transparently: the fully adjusted model (Section 3.2) as my primary estimate, given its more complete adjustment for known confounders of fuel choice, and this reduced specification as a sensitivity analysis illustrating the confounding structure underlying the raw association.

Table 2: Multilevel logistic regression analysis to determine associated factors of wasting, stunting, underweight and respiratory infections among Bangladeshi children below 5 years (BDHS 2007-2018)

	(1)		(2)		(3)		(4)	
	Wasting, AOR	(95% CI)	Stunting, AOR	(95% CI)	Underweight, AOR	(95% CI)	ARI, AOR	(95% CI)
Fuel choice								
Clean fuel adopters (Control)	1.00		1.00		1.00		1.00	
Smoke-producing fuel adopters (Exposed)	1.11	[0.97,1.28]	1.43***	[1.30,1.59]	1.46***	[1.31,1.62]	1.35*	[1.07,1.70]
Child gender								
Male	1.00		1.00		1.00		1.00	
Female	0.87***	[0.81,0.93]	0.97	[0.92,1.02]	1.07**	[1.02,1.13]	0.78***	[0.70,0.88]
Child age								
0-12 months	1.00		1.00		1.00		1.00	
13-24 months	1.04	[0.94,1.16]	3.02***	[2.76,3.29]	1.86***	[1.70,2.03]	0.96	[0.82,1.13]
25-36 months	0.93	[0.83,1.05]	3.70***	[3.37,4.07]	2.67***	[2.43,2.94]	0.74**	[0.61,0.89]
37-48 months	1.13	[0.98,1.30]	3.90***	[3.49,4.36]	3.17***	[2.83,3.55]	0.60***	[0.48,0.76]
49-59 months	1.23**	[1.06,1.43]	3.01***	[2.68,3.38]	3.37***	[3.00,3.79]	0.54***	[0.43,0.70]
Mother age at birth								
below 20 years	1.00		1.00		1.00		1.00	
20-29 years	0.93	[0.86,1.01]	0.89***	[0.83,0.94]	0.88***	[0.82,0.93]	0.86*	[0.76,0.98]
30-40 years	0.90	[0.80,1.02]	0.90*	[0.83,0.99]	0.97	[0.89,1.06]	0.90	[0.75,1.09]
40-49 years	0.99	[0.70,1.41]	1.26	[0.96,1.66]	1.08	[0.82,1.43]	0.86	[0.47,1.57]
Mother currently employed								
No	1.00		1.00		1.00		1.00	
Yes	1.06	[0.97,1.15]	1.04	[0.98,1.11]	1.03	[0.96,1.10]	1.13	[0.98,1.29]
Mother literacy								
No literacy	1.00		1.00		1.00		1.00	
At least primary education or more	0.84***	[0.78,0.91]	0.61***	[0.58,0.65]	0.64***	[0.60,0.68]	0.88*	[0.77,1.00]
Sanitation facilities								
basic sanitation	1.00		1.00		1.00		1.00	
limited sanitation	1.01	[0.91,1.12]	1.31***	[1.21,1.41]	1.29***	[1.19,1.40]	1.08	[0.92,1.27]

unimproved sanitation	1.14**	[1.04,1.24]	1.51***	[1.42,1.61]	1.49***	[1.39,1.59]	1.09	[0.95,1.25]
open defecation	1.20	[0.99,1.45]	1.91***	[1.64,2.22]	1.71***	[1.47,1.99]	1.35*	[1.01,1.81]
Water treatment								
No treatment	1.00		1.00		1.00		1.00	
Some treatment	1.14	[1.00,1.31]	0.88*	[0.79,0.98]	0.93	[0.84,1.04]	0.88	[0.69,1.12]
Antenatal care visits								
no or less 8 visits	1.00		1.00		1.00		1.00	
8 or more visits	0.75***	[0.68,0.83]	0.91**	[0.84,0.98]	0.81***	[0.76,0.88]	0.96	[0.82,1.13]
Breastfeeding status								
no	1.00		1.00		1.00		1.00	
Yes	1.18**	[1.07,1.30]	1.23***	[1.15,1.32]	1.37***	[1.27,1.47]	0.96	[0.82,1.13]
Cooking place								
Outdoor Cooking	1.00		1.00		1.00		1.00	
Indoor Cooking	0.94	[0.86,1.03]	0.97	[0.90,1.04]	0.95	[0.89,1.02]	0.96	[0.83,1.11]
Place of residence								
urban	1.00		1.00		1.00		1.00	
rural	1.10*	[1.01,1.21]	1.14***	[1.07,1.22]	1.15***	[1.07,1.23]	1.15	[1.00,1.33]
Survey period								
year2007	1.00		1.00		1.00		1.00	
year2011	0.93	[0.84,1.03]	1.06	[0.98,1.14]	0.91*	[0.84,0.99]	1.24**	[1.06,1.47]
year2014	0.96	[0.86,1.07]	1.00	[0.92,1.09]	0.94	[0.86,1.02]	1.14	[0.96,1.36]
year2017	0.60***	[0.52,0.68]	0.82***	[0.75,0.91]	0.63***	[0.57,0.70]	0.80*	[0.65,1.00]
year2018	0.50***	[0.43,0.59]	0.86**	[0.78,0.95]	0.52***	[0.47,0.58]	0.49***	[0.38,0.64]
Cluster-level variance (community random effect)	1.06***	[1.03,1.09]	1.07***	[1.05,1.09]	1.05***	[1.03,1.07]	1.09**	[1.02,1.16]
Observations	27071		27071		27071		27071	

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Quasi-experimental results

The year-stratified Propensity Score Matching analysis provides evidence of a positive association between smoke-producing fuel adoption and child stunting and underweight status, with the strength and significance of this association increasing over time. No significant ATT was observed for any outcome in 2007, when smoke-producing and clean-fuel households showed comparable rates of stunting (ATT = 0.003, ns), wasting (ATT = 0.027, ns), underweight (ATT = 0.026, ns), and ARI (ATT = 0.025, ns) after matching.

From 2011 onward, a pattern of higher stunting prevalence among smoke-producing fuel households relative to matched clean-fuel controls becomes apparent, reaching statistical significance in 2017 (ATT = 0.085, $p < 0.01$) and 2018 (ATT = 0.114, $p < 0.01$). This indicates that, holding child age and sex, maternal age and education, antenatal care utilization, and residence constant, children in smoke-producing fuel households in these waves had an 8.5 to 11.4 percentage point higher probability of stunting than their matched clean-fuel counterparts. A similar pattern emerged for underweight, with significant positive ATTs in 2014 (ATT = 0.141, $p < 0.001$) and 2017 (ATT = 0.100, $p < 0.001$), indicating a 10 to 14 percentage point higher probability of underweight status among smoke-producing fuel households in these waves.

Wasting and ARI showed no statistically significant ATT in any survey wave under this specification, suggesting that the detrimental association between smoke-producing fuel use and child health in this analysis is concentrated in chronic, cumulative growth indicators (stunting, underweight) rather than acute nutritional status (wasting) or respiratory morbidity (ARI). This distinction is consistent with the biological mechanism through which prolonged exposure to household air pollution and its associated socioeconomic correlates would be expected to affect long-term growth trajectories more than short-term or episodic health events.

Table 3: Estimates of the average treatment effects on the treated (ATT(X)), PSM adjusting for different sample years

Health Outcomes	2007	2011	2014	2017	2018
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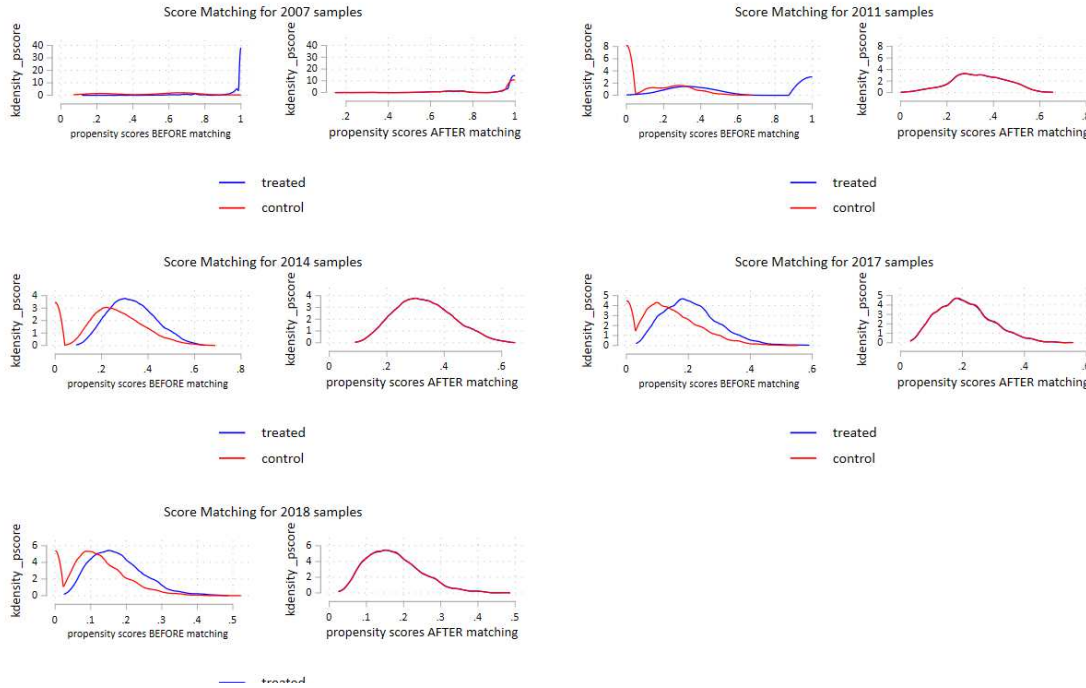
Stunting					
Treated	0.396	0.415	0.379	0.325	0.335
Controls (Matched)	0.393	0.327	0.307	0.24	0.221
Difference (ATT)	0.003	0.087	0.073	0.085**	0.114**
(SE)	(0.066)	(0.064)	(0.040)	(0.031)	(0.037)
Wasting					
Treated	0.167	0.156	0.15	0.098	0.077
Controls (Matched)	0.141	0.143	0.116	0.065	0.105
Difference (ATT)	0.027	0.013	0.034	0.033	-0.027
(SE)	(0.044)	(0.050)	(0.028)	(0.019)	(0.025)
Underweight					
Treated	0.384	0.364	0.341	0.251	0.21
Controls (Matched)	0.358	0.348	0.201	0.151	0.193
Difference (ATT)	0.026	0.016	0.141***	0.100***	0.017
(SE)	(0.063)	(0.061)	(0.037)	(0.028)	(0.033)
ARI (Acute Respiratory)					
Treated	0.047	0.061	0.056	0.041	0.027
Controls (Matched)	0.022	0.052	0.034	0.054	0.014
Difference (ATT)	0.025	0.009	0.022	-0.012	0.013
(SE)	(0.027)	(0.027)	(0.017)	(0.015)	(0.009)

Common neighbor (5), Caliper set at 0.001, ***p-value<0.01 (1% significance level), **p-value<0.05 (5% significance level), SE= standard error

3 presents the kernel density plots of the propensity scores for both treated and control groups across the five survey waves (2007–2018). The "Before Matching" panels illustrate the initial selection bias, showing a clear divergence in the probability of treatment based on observable covariates—most notably in 2007, where a significant portion of the treated group lacks a comparable control counterpart. Conversely, the "After Matching" panels demonstrate a nearly perfect overlap in the distributions. This visual alignment confirms that the matching procedure

successfully balanced the characteristics of the two groups, ensuring that the estimated ATTs

Figure 3: Kernel density of propensity scores by treatment status by different years: before matched (left panel) and after matched (right panel)



reflect the impact of cooking fuel rather than underlying socioeconomic differences.

Heterogeneity analysis

To assess whether the association between smoke-producing fuel adoption and child stunting varies across demographic and socioeconomic subgroups, I tested treatment $\times$ subgroup interactions within the multilevel logistic regression framework for child sex, residence, and household wealth. Subgroups were defined as: child sex (male, female); residence (urban, rural); and household wealth (below-mean: wealth index categories 1–3 [poorest, poorer, middle]; above-mean: categories 4–5 [richer, richest]). Interaction models included the same covariate set as the primary multilevel analysis, with a cluster-level random intercept to account for the hierarchical survey design. Statistical significance of subgroup differences was assessed via the Wald test on each treatment $\times$ subgroup interaction coefficient.

None of the three interaction terms reached statistical significance: treatment $\times$ female (AOR = 0.97, 95% CI: 0.82–1.14, $p = 0.71$), treatment $\times$ rural (AOR = 1.10, 95% CI: 0.87–1.38, $p = 0.43$), and treatment $\times$ below-mean wealth (AOR = 0.99, 95% CI: 0.68–1.42, $p = 0.94$). These results indicate that the association between smoke-producing fuel adoption and child stunting does not differ significantly by child sex, residence, or household wealth in this sample. I therefore do not find robust statistical evidence of differential vulnerability to smoke-producing fuel exposure across the demographic and socioeconomic subgroups examined. This finding suggests that the elevated stunting risk associated with smoke-producing fuel adoption (Table 4) is broadly consistent across population subgroups, rather than being concentrated among specific vulnerable populations such as girls, rural households, or lower-wealth households.

Table 4: Formal tests of effect heterogeneity in the association between smoke-producing fuel use and child malnourishment

Interaction term	AOR	95% CI	p-value
Treatment $\times$ Female (ref: Male)	0.97	[0.82, 1.14]	0.712
Treatment $\times$ Rural (ref: Urban)	1.10	[0.87, 1.38]	0.430
Treatment $\times$ Below-mean wealth (ref: Above-mean)	0.99	[0.68, 1.42]	0.941

Note: Each row reports the treatment $\times$ subgroup interaction coefficient from a separate multilevel logistic regression model, adjusting for child age, maternal age, maternal education, maternal current employment, sanitation, water treatment, antenatal care, breastfeeding status, and (as appropriate) residence, with a community-level random intercept. A non-significant interaction indicates no statistically significant difference in the fuel-stunting association between subgroup levels.

Discussion

This study examined the association between smoke-producing fuel adoption and child health outcomes in Bangladesh using two complementary identification strategies: Propensity Score Matching and multilevel mixed-effects logistic regression. I initially explored an instrumental variable approach but found, after rigorous diagnostic testing of two candidate instruments, that neither satisfied the exclusion restriction with sufficient confidence to support causal interpretation. I therefore do not present IV estimates as evidence in this study, and any causal claims below should be understood as resting on the identifying assumptions of PSM and multilevel adjustment for observable confounders — assumptions that are more limited than those of a valid instrumental variable design, as discussed further below and in my Limitations section.

My two identification strategies produced partially divergent findings, which I consider informative rather than contradictory. The fully adjusted multilevel logistic regression, which pools observations across all survey waves and controls for household wealth, maternal education, sanitation, water treatment, antenatal care, media exposure, and administrative region, found no statistically significant association between smoke-producing fuel adoption and any of the four child health outcomes examined. In contrast, year-stratified PSM analysis found a significant positive association between smoke-producing fuel adoption and both stunting and underweight status emerging in later survey waves (2014–2018), even after matching on the same set of observable characteristics. I interpret this divergence as reflecting a key methodological difference between the two approaches: the pooled multilevel model assumes a constant treatment effect across the entire 2007–2018 period, which may average out a genuine effect that intensified over time, whereas the year-stratified PSM analysis allows the effect to vary freely by wave and more directly captures this temporal pattern. This interpretation is further supported by a sensitivity analysis in which excluding household wealth and administrative region from the multilevel model — the two variables most strongly associated with fuel choice in my data — restored a significant positive association between fuel choice and stunting, underweight, and ARI. Together, these results suggest that household wealth and

geography are the dominant confounders of the raw fuel-health association, but do not fully explain it, particularly in more recent survey waves.

Contrary to my original hypothesis and previous literature emphasizing acute respiratory outcomes, I did not find robust evidence of an association between smoke-producing fuel use and ARI in my, year-stratified PSM analysis; earlier findings suggesting elevated or declining ARI risk across waves did not hold up under a corrected model specification and are not reported here. I note this explicitly because ARI is measured via a single, retrospectively reported two-week symptom recall in the DHS, which is subject to considerable measurement error (see Limitations); a null finding for this outcome should not be interpreted as evidence that respiratory effects of household air pollution are absent, but rather that my data and design are not well suited to detecting them reliably.

The pattern of significant associations concentrated in stunting and underweight, rather than wasting or ARI, is broadly consistent with the underlying biological pathway through which household air pollution is hypothesized to affect chronic child growth. This is consistent with a growing body of evidence: household air pollution exposure has been associated with increased odds of both moderate and severe stunting [34], and a national analysis of Indian survey data found that early-life PM_{2.5} exposure was associated with reduced height-for-age at later ages, supporting a dose-response relationship between household air pollution and linear growth [35]. Proposed biological mechanisms include prenatal oxidative stress and systemic inflammation, and postnatal impairment of immune development that increases the frequency of subclinical infection, plausibly operating cumulatively over early childhood in a manner more consistent with chronic growth faltering than acute wasting [34]. Similarly, well-documented environmental determinants of childhood nutrition and well-being, including household water, sanitation, and hygiene conditions, are recognized as compounding risk factors that may interact with air pollution exposure in shaping child health outcomes [36]. For ARI specifically, household air pollution exposure has been associated with substantially increased risk of acute lower respiratory infection and other respiratory morbidity in children [37, 38], underscoring that my null ARI finding departs from an otherwise fairly consistent literature and should be

interpreted in light of the measurement limitations discussed below, rather than as evidence against a respiratory pathway. The broader role of the primary healthcare system and household environmental conditions in shaping child health outcomes further underscores the multi-sectoral nature of this pathway [39]. Additionally, exposure to household air pollution may operate partly through maternal pathways, including reduced maternal respiratory and nutritional status during pregnancy, which is independently associated with lower birth weight and subsequent growth faltering. I am unable to test these specific biological mechanisms directly with my data, and I flag this as an important direction for future research combining survey data with direct exposure and biomarker measurement

The formal heterogeneity analysis found no statistically significant differences in the fuel-health association by child sex, residence, or household wealth. I had originally hypothesized greater vulnerability among girls, rural households, and lower-wealth households; while descriptive point estimates showed modest numerical differences in these directions, formal interaction tests did not support these as statistically robust patterns. I therefore do not find evidence for a gender-differentiated health burden in this sample, and I do not interpret my data as supporting claims of differential biological or exposure-based vulnerability by child sex.

Policy Implications

My findings suggest a more nuanced policy picture than a simple, uniform causal effect of fuel choice on child health. The persistence of a significant association between smoke-producing fuel use and chronic malnutrition indicators (stunting, underweight) in recent survey waves, even after adjusting for household wealth and geography, indicates that clean fuel transition remains a relevant lever for child nutrition policy in Bangladesh — but my finding that this association is substantially attenuated once wealth and region are accounted for also indicates that fuel-focused interventions alone are unlikely to be sufficient without complementary poverty-reduction and geographically targeted health investment.

Given the cost and infrastructure demands of large-scale LPG subsidy programs, policymakers should carefully weigh feasibility and long-term sustainability alongside

impact. LPG subsidies require sustained fiscal commitment, reliable distribution infrastructure reaching remote rural areas, and are vulnerable to international price volatility, which has previously constrained subsidy programs in South Asia [40]. Given these constraints, improved biomass cookstoves — which reduce, though do not eliminate, indoor air pollution at substantially lower cost and infrastructure burden than LPG transition — may serve as a valuable complementary or interim strategy, particularly in remote rural communities where LPG distribution networks remain limited [41, 42].

Successful implementation in rural and low-income communities will likely require addressing well-documented barriers beyond fuel cost alone, including entrenched cooking practices and taste preferences for biomass-cooked food, limited awareness of the health consequences of household air pollution, and the practical logistics of cylinder refilling and stove maintenance in areas with limited transport infrastructure [43]. Community-based behavior changes programs, delivered alongside subsidies rather than as a substitute for them, are likely to be more effective than subsidy provision alone in achieving sustained adoption.

Finally, given the strong, consistent, and statistically robust protective associations we found for maternal education, antenatal care utilization, and household wealth across nearly every outcome examined, we recommend that clean energy interventions be integrated within, rather than treated as separate from, broader maternal and child health and poverty-reduction programming, rather than positioned as a standalone solution to child malnutrition.

Limitations and Directions for Future Research

Several limitations should be considered when interpreting my findings. First, this study relies on cross-sectional DHS data, which limits my ability to capture the cumulative, life-course effects of sustained exposure to smoke-producing fuel, and precludes within-household or within-child tracking of exposure and growth over time.

Second, my measure of cooking fuel type is based on a household-level categorical classification (smoke-producing versus clean fuel) drawn from a single survey response,

which may be subject to measurement error — for example, households using multiple fuel types (fuel stacking) may be misclassified into a single category, potentially attenuating my estimated associations toward the null. Similarly, my ARI outcome is based on maternal report of respiratory symptoms in the two weeks preceding the survey, which is subject to recall bias and may not reliably capture the true incidence or severity of respiratory illness; this measurement limitation may partly explain why we did not detect a significant association between fuel choice and ARI in my analysis.

Third, while PSM and multilevel regression both adjust for a broad set of observable household, individual, and community characteristics, neither method can rule out bias from residual confounding by unobserved factors — such as dwelling ventilation quality, the specific duration and intensity of a child's exposure to cooking smoke, or unmeasured household health behaviors — that may jointly influence both fuel choice and child health. Unlike a valid instrumental variable design, which can in principle account for confounding by unobserved factors, my reliance on PSM and multilevel adjustment means that some portion of the association we observe may reflect confounding rather than a causal effect of fuel choice itself. As detailed in Section 2.6, we explored two candidate instruments — parental education and a media exposure composite — and found that both violated core instrumental variable assumptions: parental education's first-stage relationship with fuel choice was inconsistent with the hypothesized direction, and media exposure's reduced-form estimates showed evidence of a direct effect on child health outcomes independent of fuel choice, indicating a likely violation of the exclusion restriction in both cases. We were unable to identify a credible instrument within the available data, and this remains an important constraint on the causal strength of my conclusions.

Fourth, and relatedly, we lack direct measurement of household air pollution exposure — such as $PM_{2.5}$ or carbon monoxide concentrations — relying instead on a binary proxy of fuel type. This limits my ability to assess the actual intensity of pollutant exposure experienced by individual children, which likely varies substantially by cooking location, ventilation, and time spent near the cookstove, even within the same fuel-type category.

Finally, the absence of qualitative data limits my understanding of the cultural and intra-household decision-making processes underlying fuel choice, and we were unable to

account for certain economic variables, such as household income or out-of-pocket medical expenditure, not consistently available across BDHS waves.

Future research would benefit from longitudinal or panel data capturing within-household changes in fuel use and child growth trajectories over time; mixed-methods designs triangulating survey data with direct indoor air quality monitoring (e.g., PM_{2.5} or CO sensors) and biomarker data; and continued search for valid instruments — such as historical fuel price shocks or physical distance to gas distribution infrastructure — that may more credibly satisfy the exclusion restriction than the candidates explored here.

Conclusion

This study examined the association between smoke-producing fuel adoption and child stunting, wasting, underweight, and acute respiratory infection in Bangladesh using Propensity Score Matching and multilevel mixed-effects logistic regression. We do not find a statistically significant association between fuel choice and any of the four outcomes in My fully adjusted, pooled multilevel model, indicating that much of the raw, descriptive relationship between cooking fuel and child health is attributable to confounding by household wealth, maternal education, and geography. However, my year-stratified PSM analysis found a significant positive association between smoke-producing fuel use and chronic nutritional outcomes — stunting and underweight — emerging in more recent survey waves, even after adjusting for the same observable confounders, suggesting that fuel choice may retain an independent association with chronic child malnutrition that intensified over the study period. We did not find evidence of a valid instrumental variable to support a stronger causal claim, nor did we find statistically robust evidence of differential vulnerability by child sex, residence, or wealth. Taken together, my findings suggest that clean cooking fuel transition remains a relevant, if not singularly sufficient, component of strategies to address chronic child malnutrition in Bangladesh, and that such interventions are likely to be most effective when integrated with, rather than positioned as a substitute for, broader investments in household wealth, maternal education, and geographically targeted health infrastructure.

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Data availability

The data used in this article were available on <http://dhsprogram.com>.

Declarations

Ethical approval and consent to participate The DHS received government permission, used informed consent, and assured respondents of confidentiality. Since this study involved secondary data analysis, we did not require ethics approval.

Consent for publication

Not applicable.

Competing interests

The author declares no competing interests

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