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**ADVANCE PUBLICATION**

# The Effect of Schooling on Women's Overweight and Obesity: A Natural Experiment in Nigeria

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**ABSTRACT** An extensive social scientific literature has documented the importance of schooling in preventing overweight and obesity among women. However, prior quasi-experimental studies investigating the causal effect of schooling on women's overweight and obesity have focused almost exclusively on high-income countries (HICs). Schooling effects may differ in low- or middle-income countries (LMICs), where information about the harms of being overweight is often sparse and where larger body sizes can be socially valued. Here I evaluate the causal impact of schooling on women's probability of being overweight or obese in an LMIC, Nigeria, using data from the 2003, 2008, and 2013 Demographic Health Surveys. In 1976, the Nigerian government abolished primary school fees and increased funding for primary school construction, creating quasi-random variation in access to primary school according to an individual's age and the number of newly constructed schools in their state of residence. I exploit both sources of variation and use a two-stage instrumental variables approach to estimate the effect of increased schooling on the probability of being overweight or obese. Each additional year of schooling increased the probability of being overweight or obese by 6%, but this effect estimate was not statistically different from zero. This finding differs from the protective effect of schooling documented in several HICs, suggesting that contextual factors play an important role calibrating the influence of additional schooling on overweight or obesity. Furthermore, my findings contrast markedly with the positive correlation between schooling and overweight/obesity identified in previous studies in Nigeria, suggesting that studies failing to account for selection bias overestimate the causal effect of schooling. More robust causal research is needed to examine the effect of schooling on overweight and obesity in LMIC contexts.

**KEYWORDS** Overweight • Obesity • Education • Natural experiment • Lower- and middle-income countries

## Introduction

The proportion of adults who were overweight or obese grew by as much as 27.5% worldwide between 1980 and 2013, a phenomenon often described as the "obesity epidemic" (Hill and Peters 1998; Jaacks et al. 2019). Figure 1 plots overweight

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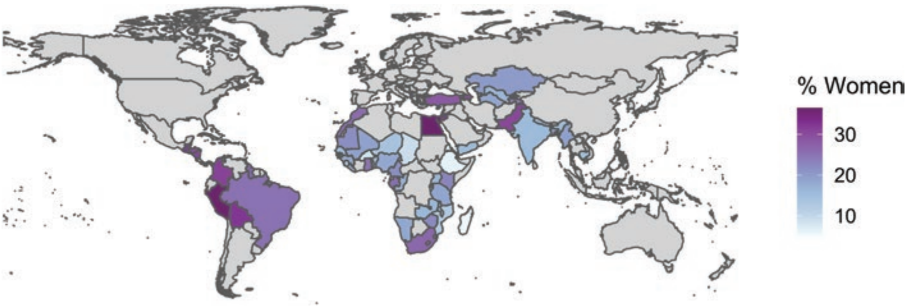


**Fig. 1** Proportion of women who were overweight or obese in the most recent DHS (various years) in each country, accessed via DHS STATcompiler. Gray indicates that no data are available.

prevalence across 70 low- and middle-income countries (LMICs) in the most recent year that comparative survey data were available. The figure reveals substantial variation. Approximately 5.1% women surveyed were overweight or obese in Madagascar (2008–2009), compared with 18.2% in Nigeria (2018) and 36.5% in Peru (2014). The proportion of individuals living in LMICs that is overweight or obese is, on average, smaller in LMICs relative to high-income countries (HIC). However, the rate of increase in overweight and obesity is currently fastest in many LMICs, many of which also have high levels of undernutrition (Ford et al. 2017).

Overweight and obesity are associated with several diseases, including type 2 diabetes, coronary heart disease, and some cancers (Wyatt et al. 2006). Women are often disproportionately exposed to these risks: a 2016 review of 130 LMICs found that women had a higher overweight/obesity prevalence relative to men in 119 countries (Ng et al. 2016). The obesity epidemic and its gendered inequities have diverse biological, socioeconomic, and environmental causes, and understanding the contribution of each of these factors can help to identify targets for intervention to prevent the harms from excess body weight and associated inequalities (Marmot 2007).

Education is often described as one of the most important social predictors of women's health behaviors and outcomes, including diet, physical activity, and body weight (Liu and Guo 2015; Sobal 2011). Establishing causality is difficult because of potential confounding with unobserved characteristics, such as family wealth. These challenges are compounded by difficulties in measuring education, which is a broad concept pertaining to the skills, abilities, knowledge, cultural understandings, and values required to participate in a particular society (Pritchett 2013:18–19). Robust evaluations of education's causal effects often exploit quasi-random differences in one educational input—years of schooling—resulting from changes in the cost of schooling or mandatory school-leaving ages (Cawley 2015). Previous quasi-experimental studies have found that additional years of schooling reduce the probability of being overweight or obese among women in several HICs (Hamad et al. 2018).

However, schooling effects may differ in LMICs, where information about the harms of excess body weight is sparse and where unhealthy diet, physical inactivity, and larger body size among women can be socially valued (Brown and Konner 1987; Dinsa et al. 2012). Observational analyses have identified a positive association between years of schooling and overweight or obesity status in several LMICs, including Nigeria (Neupane et al. 2015). Yet, to my knowledge, no study has investigated the relationship between schooling and overweight or obesity in an LMIC using methods for causal inference that address the endogeneity of schooling.

I address these research gaps using the implementation of a 1976 Universal Primary Education (UPE) in Nigeria as a quasi-experimental exogenous change to estimate the effect of schooling attainment. Exposure to the UPE reform varied according to an individual's year of birth and the number of new schools constructed between 1974 and 1978 in their state of residence. Using data from the Nigerian Demographic Health Surveys (DHS) in 2003, 2008, and 2013, I exploit both sources of variation to estimate two-stage least squares instrumental variable models and evaluate the causal effect of additional years of schooling on the probability that an individual was overweight or obese at the time of the survey.

## Background

### Theoretical Framework and Mechanisms Rooted in HIC Contexts

The theoretical literature has identified three broad pathways through which additional schooling is expected to have a protective effect on health behaviors and outcomes. First, schooling can improve health and lead to health-promoting behaviors by enhancing an individual's ability to use and respond to knowledge (including medical information), thereby producing good health (Grossman 1972, 2006). Second, schooling can increase individuals' *learned effectiveness*, defined as higher levels of personal control and self-efficacy, which are psychological predictors of health-promoting behaviors (Mirowsky 2017). Third, schooling may improve health indirectly by increasing access to higher-paying professions and hence increasing incomes. Increased income facilitates access to healthcare, health-promoting goods, and a healthier environment (Kemptner et al. 2011).

These mechanisms are particularly relevant for explaining schooling-related disparities in overweight and obesity in HICs (McLaren 2007). Additional years of schooling can enable individuals to maintain a healthy weight by facilitating the acquisition of information about the harms of excess body weight and how to prevent them (Nayga 2000). Additional schooling can also cultivate psychological traits, such as self-control, that help individuals to regulate their caloric intake and exercise sufficiently (Barlow et al. 2016; Wilson et al. 2015). Furthermore, schooling facilitates access to higher-paying jobs. These jobs provide the material resources that are often needed to purchase healthy meals and engage in recreational exercise, whereas obesogenic environments create barriers to healthy eating for lower-income groups because of the low cost and widespread availability of unhealthy, calorie-dense foods (McLaren 2007).

These mechanisms can be relevant for all genders but are often especially important for women because of a strong stigma surrounding larger women's body sizes in many HICs (Sobal 2011). Such stigma creates a stronger pressure for women to maintain a smaller body weight, but women with greater years of schooling have greater access to the resources and information necessary to attain this idealized body type.

### Contextual Contingencies in LMICs

The aforementioned mechanisms and impacts may have limited generalizability to LMICs. First, the symbolic value of thinness and fatness is contextually contingent. Anthropological studies have observed that bigger bodies represent status and power in many societies and that the association between thinness and status in Western industrialized countries is an exception to a global, historical preference for larger bodies (Brown and Konner 1987; Cassidy 1991). For example, Oe (2009:4) observed that in Nigeria, larger female body weight is seen as “alluring, and a sign of good health and prosperity” among some groups, such as the Efiks, Annangs, and Igbos. Osayomi and Orhiere (2017:104) similarly described the proverbial expression among the Yorubas, “Agba ti ko yo’kun, ahun looni,” meaning “It is only a miser that would refuse to grow fat.” One manifestation of this preference is a cultural tradition in which women gain weight before marrying by staying in “fattening rooms,” where they are fed large quantities of foods high in fat and sugar (Oe 2009).

Second, the extent to which food environments are obesogenic because of the widespread availability of low-cost, calorie-dense food also varies between countries. In some countries, high-calorie snacks and restaurant meals can be more expensive or available primarily in urban settings, where wealthier families are significantly more likely to reside (Popkin et al. 2012).

Third, the social value and stigma of the behaviors that contribute to obesity can vary among countries. For example, low physical activity levels and consumption of calorie-dense Western food products serve as status symbols in some LMICs (Swami 2015).

Taken together, these three considerations illustrate how overweight, obesity, and the behaviors that contribute to them might be socially desirable for women in LMICs with the aforementioned characteristics, such as Nigeria. However, overweight/obesity may be attainable only for those with more years of schooling and correspondingly higher incomes and resources necessary to purchase and access calorie-dense or Western foods and to maintain low physical activity levels.

There is also an additional, fourth reason why schooling effects may differ: the mechanisms through which schooling increases individuals’ ability to acquire information and knowledge about the harms of overweight and how to prevent it may not apply to all LMICs. Specifically, information about the harmful effects of overweight and obesity and about how to maintain a healthy body weight is often sparse in LMICs (Haase et al. 2004; Nyaruhucha et al. 2003). This general lack of information may limit the importance of additional schooling in the acquisition of knowledge about the harm of excess body weight and the behaviors that prevent it.

These contextual contingencies have important theoretical implications: if the impact of additional schooling on overweight and obesity in HICs does not extend to LMICs, such as Nigeria, then the protective effects of schooling observed in HICs may not be fully attributable to an impact of schooling alone. This point is explicitly recognized by fundamental cause theory (Phelan et al. 2010). A core principal of this theory is that disparities in health according to an individual's schooling may not exist in all historical or regional contexts. Instead, disparities are said to emerge in contexts where the material and intellectual capacities to avoid risk are available but accessible only to advantaged socioeconomic groups (Hayward et al. 2015).

## Empirical Literature and Methodological Challenges

Descriptive studies have found that women with higher levels of schooling attainment are more likely to be overweight or obese in some LMICs, including Nigeria (Dinsa et al. 2012). For example, Kandala and Stranges (2014) analyzed data from the 2008 Nigerian Demographic Health Survey (DHS) and found that women with primary, secondary, or higher education were 1.61 times more likely to be overweight or obese than women without any education. Neupane et al. (2015) used DHS data to assess educational disparities in overweight and obesity across 32 sub-Saharan African countries in 2005–2013. They found that women with secondary or postsecondary education were, on average, 1.81 times more likely to be overweight or obese than women with no or primary education.

These observational associations may be confounded by early-life anthropometrics and unobserved sociodemographic or biological characteristics, including parental preferences, genetic endowments, and family wealth, all of which can also predict individuals' overweight or obesity status independent of schooling (Cawley 2015; Currie and Vogl 2013). For example, the wealth, educational attainment, and preferences of a child's parents can exert a strong influence on an individual's years of schooling (Ermisch and Francesconi 2001). However, these characteristics can directly influence adult body weight and its determinants independent of years of schooling via their influence on individuals' preferences and resources for engaging in healthy dietary behaviors and physical activity (Benton 2004).

Robust evaluations of the causal effect of schooling have frequently attempted to address this by drawing on differences in individuals' years of schooling according to an individual's eligibility for reductions in the cost of schooling (e.g., school fee removal) or increases in mandatory school-leaving ages (Cawley 2015). Additional years of schooling have been found to reduce the probability of being overweight or obese in the HICs of Italy (Braga and Bratti 2013), the United States (Fletcher 2015), Germany (Kemptner et al. 2011), Australia (Li and Powdthavee 2015), and the United Kingdom (James 2015), as well as in the upper-middle income country Turkey (Dursun et al. 2018).

No study, to my knowledge, has investigated the relationship between schooling and overweight or obesity status in an LMIC using methods for causal inference that address the endogeneity of schooling. This may reflect limited data access or a tendency to associate LMICs with diseases of poverty and associated risk factors that historically corresponded to lower levels of development, such as child mortality, undernutrition, and infectious diseases such as HIV (McKeown 1988). Hence, many quasi-experimental studies of schooling effects in LMICs have examined these outcomes (Hamad et al. 2018). Examining schooling's effects on overweight and obesity is, however, increasingly pertinent to the changing nature of disease in LMICs: many of these countries now face a double burden of diseases traditionally associated with affluence, such as overweight and obesity, in addition to the diseases of poverty that have received much attention in the literature on schooling and health in LMICs (Ebrahim et al. 2013).

## The Universal Primary Education (UPE) Reform in Nigeria

Here I address the above research gaps by using the implementation of UPE in Nigeria as a quasi-experimental exogenous change to estimate the effect of schooling attainment. A nationwide program funded by the federal government, UPE was implemented in September 1976 (Aluede 2006). The reform was designed to increase educational attainment and schooling by providing tuition-free primary school and increasing the number of primary schools, classrooms, and qualified teachers (Bray 1981). According to Csapo (1983:91), the reform resulted in an "explosion" in primary school enrollment. The number of children attending primary school in Nigeria increased from 4.4 million in 1974 to 13.8 million by 1981 (Osili and Long 2008).

Studying the Nigerian reform brings several advantages. A central goal of UPE was to reduce regional disparities in access to primary school across Nigerian states by building schools throughout the country (Aluede 2006). The Federal government disbursed 700 million naira (approximately 2% GDP) for primary school and classroom construction, leading to a significant increase in the number of new schools that were founded, as illustrated in Figure 2.

This school-building effort created a natural policy experiment in which exposure to UPE varied along two dimensions: (1) exposure varied between birth cohorts of primary school age before and after UPE was implemented; and (2) the number of newly constructed schools in each state in 1974–1978 varied substantially (mean = 1,135.9, standard deviation = 866.8), leading to interstate variation in exposure. The greatest increase in the number of schools occurred in the northern state of Kano, where an additional 2,726 schools were built. In contrast, 166 schools were built over the same period in the western state of Ogun. Exploiting variation among those who were eligible for the reform according to state-level school construction levels helps address potential confounding due to unobserved differences between cohorts of primary school age before and after the implementation of UPE. The analytical advantages of this approach have been well-recognized in econometric studies examining the socioeconomic effects of schooling (Andriano and Monden 2019; Duflo 2001).

In addition, Nigeria's per capita GDP of $3,731, $4,597, and $5,495 in 2003, 2008, and 2013, respectively, make it an LMIC in those years according to the World Bank's classification scheme (World Bank 2015). Finally, women with secondary and postsecondary education are more likely to be overweight or obese than less-educated women in Nigeria (Kandala and Stranges 2014; Neupane et al. 2015). This difference has been attributed to cultural pressures that encourage large body size, greater consumption of high-calorie restaurant meals, and sedentary lifestyles among more-educated women (Oe 2009). Nigeria is therefore an LMIC in which greater years of schooling may increase women's overweight and obesity, providing theoretically relevant insights into the impact of schooling in a country where the sociocultural context appears to modify schooling's influence.

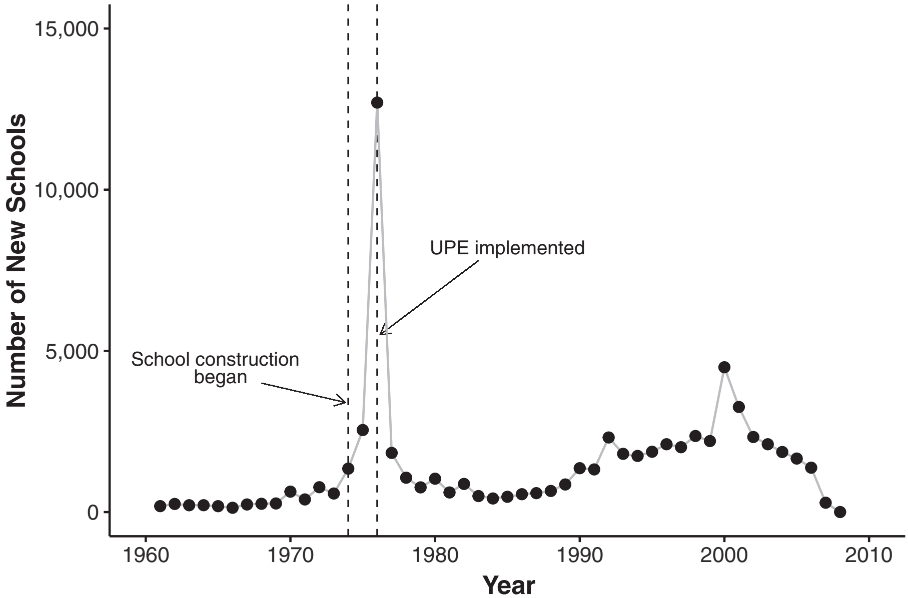


**Fig. 2** Number of new public schools in Nigeria, 1960–2010. *Source:* Nigerian Primary School Census 2008.

## Methods

### Data Sources and Measurement

To study the impact of schooling on women's risk of being overweight or obese in Nigeria I obtained individual-level anthropometric and sociodemographic data from the 2003, 2008, and 2013 DHS. These data are provided in a harmonized format in the IPUMS-DHS files developed by Boyle and colleagues (2016). The DHS have been described in extensive detail elsewhere (Corsi et al. 2012). The Nigerian component is a repeated cross-sectional survey containing responses from 7,000–35,000 women in each wave. Data on body weight and height in Nigeria were first collected in 1999 but were missing for a large proportion of respondents. I therefore used data sets from 2003, 2008, and 2013, when anthropometric data were missing for only a small percentage (2.4%) of respondents.

I matched individual-level data from the IPUMS-DHS with state-level school-founding data from the 2008 Nigerian Primary School Census, provided by Larreguy and Marshall (2017). The IPUMS-DHS and school-founding data are disaggregated according to the 37 administrative units (36 states plus the Federal Capital Territory, Abuja) that existed at the time of the school census and the DHS. These 37 units were all subdivided from the 19 states that existed when UPE was implemented. Section 1 of the online appendix describes how I harmonized state names and matched individuals to state data.

The key exposure variable in my analysis is an individual's expected years of schooling, and my outcome of interest is whether an individual was overweight or obese at the time of the survey. Expected years of schooling (DHS variable v133) are calculated by DHS staff using country-specific measures of the number of years of schooling an individual is expected to have completed to attain the highest educational level an individual reported having attended at the time of the survey and the number of years of actual attendance at that level. Grade repetition may introduce measurement error; although this caveat should be borne in mind when interpreting the results, there are no alternative indicators or terminologies that lack similar drawbacks.[1] Overweight and obesity status are based on weight and height data recorded by trained personnel using standardized procedures. I divided body weight by height squared to calculate each individual's body mass index (BMI) and transformed this into a dichotomous indicator of whether an individual had a BMI above pre-established overweight and obese thresholds set by the World Health Organization (WHO 2015) based on risks to cardiovascular health: 25 kg/m$^2$ (overweight or obese) and 30 kg/m$^2$ (obese).

### Procedures and Statistical Analysis

As a benchmark for my analysis of the impact of schooling on overweight or obesity, I estimate linear probability models to assess the association between years of schooling and the probability of being overweight/obese. These are presented for descriptive purposes and for comparison with subsequent models. Let $O_i$ denote whether an individual $i$ was overweight or obese at the time of the survey. The baseline descriptive model is

$$O_i = \beta_0 + \beta_1 Schooling_i + \beta_2 Age_i + Birthyear_i + Wave_t + State_s + \varepsilon_i, \tag{1}$$

where $i$ denotes the individual, $t$ is the survey year, and $s$ is the state. $Schooling_i$ is the number of years of schooling completed by an individual at the time of the survey, with coefficient $\beta_1$; $\beta_0$ is the intercept. I control for age measured in years ($Age_i$) as well as dummy variables for each survey wave ($Wave_t$) and birth year ($Birthyear_i$). For comparison with subsequent models using state-level variables, I incorporate state fixed effects to control for unobserved time-invariant state characteristics correlated with the dependent and independent variables. Standard errors are clustered by state to address the spatial clustering of UPE intensity. I also report wild bootstrap $p$ values given the small number of states (Cameron et al. 2008). All estimates are weighted using individual weights provided by the DHS.

As noted earlier, years of schooling may be correlated with one or more unmeasured factors that account for the association between schooling and overweight or obesity status. To address this possibility, I estimate a series of models exploiting quasi-

[1] Grade repetition may imply that the conversion to years of schooling from an individual's educational level and the country-specific measures of the number of years of schooling typically required to complete that level could underestimate an individual's actual number of years of attendance. One alternative strategy is to label this variable *grades of schooling*. However, this does not resolve issues associated with grade repetition because the conversion to an individual's school grade from information about the number of years at the highest level attended could overestimate an individual's highest grade if they repeated a grade. Furthermore, *grades* has an ambiguous meaning in a non-U.S. context, where it can also refer to a score on a test.

random variation in exposure to UPE and corresponding differences in schooling. I estimate multiple models addressing different sources of bias (e.g., noncompliance, unobserved confounding) and with different policy implications.

## Intent-to-Treat Effect

I estimate the intention-to-treat (ITT) effect (i.e., the effect of policy eligibility) because of its policy relevance: the offer of a program such as free education is a policy lever that can be controlled, whereas forcing participation is more challenging (Ten-Have et al. 2008). The ITT also provides insight into the overall health impacts of the reform. This is relevant for assessing the reform's welfare consequences, which have been extensively debated elsewhere (Oyelere 2010). I draw on quasi-random variation in UPE eligibility according to an individual's year of birth and estimate the ITT of exposure to UPE on the probability of being overweight or obese. I reestimate Eq. (1) but replace the schooling variable ($Schooling_i$) in Eq. (1) with a dummy indicator of eligibility for UPE ($Eligible_i$); all other features of the model remain unchanged.

$Eligible_i$ takes a value of 1 for individuals born in years that would make them eligible for UPE, and a value of 0 if the respondent was born in a year that would make them too old to be eligible for free primary school. Because Nigerian children enter primary school when they are approximately 6 years old, UPE might have affected all individuals aged 6 or younger when the reform was implemented in 1976, with all those aged 7 or older being ineligible at the age they started primary school. I modify these treatment and control group age cutoffs for four reasons. First, I restrict the treatment and control group cohorts to a six-year age range to reduce bias that might arise from comparing cohorts who were born many years apart. Second, new school construction increased from 1974 onward, two years before the implementation of UPE. As a result, individuals entering primary school in 1974 may have also benefitted from increased access to primary school. I therefore assigned those who were of primary-school age in 1974 (1–6 years old in 1974, born in 1968–1973), rather than 1976, as eligible for the UPE.

Third, Nigeria has a high prevalence of over-age enrollments in primary school. Historic data are not available, but data from 2008 show that 29% of children entering the first grade of primary school were 8 years old (National Population Commission 2016). In addition, because poor early-life anthropometrics can lead to delayed school entry, individuals with low BMIs born in earlier years may have entered primary school after UPE was implemented (Glewwe and Jacoby 1995; Glewwe et al. 2001). Some older children may have therefore received the "treatment." This would be particularly problematic if, for example, schooling reduces the probability of being overweight or obese but the ineligible (control) group contained individuals who also received additional schooling and were especially more likely to have poor adult anthropometrics because of poor child health (Black et al. 2013). The inclusion of treated individuals in the control group could then reduce differences between the treated and control groups because of the inclusion of individuals with lower body weight as a result of education in both groups, thereby exerting downward bias in my estimates. Furthermore, the additional resources granted to primary schools as part of UPE may have benefitted those already enrolled. I therefore use a slightly older group of women as a control group: those aged 15–20 in 1974 (born in 1954–1959). In robustness checks, I test the sensitivity of my results to my categorization decisions.

### Local Average Treatment Effect

The ITT does not capture the effect of the treatment actually received (years of schooling) (Angrist and Pischke 2009) because of potential noncompliance. I therefore use an instrumental variables (IV) model to estimate the effect of a unit increase in schooling on overweight/obesity among individuals who actually increased their years of schooling because of the UPE reform. I first estimate two-stage least squares (2SLS) regression models using the dichotomous indicator of UPE eligibility ($Eligible_i$) to instrument for total years of schooling in the first stage:

$$Schooling_i = \beta_0 + \beta_1 Eligible_i + \beta_2 Age_i + Birthyear_i + Wave_t + State_s + \varepsilon_i. \quad (2)$$

Let $\widehat{Schooling}_i$ be the predicted value of individual $i$'s years of schooling from the first-stage regression in Eq. (2). The second-stage regression is then as follows:

$$O_i = \beta_0 + \beta_1 \widehat{Schooling}_i + \beta_2 Age_i + Birthyear_i + Wave_t + State_s + \varepsilon_i. \quad (3)$$

I interpret these IV estimates as local to the subpopulation that complied with treatment assignment, the local average treatment effect (LATE). Following recommendations in the statistical literature for applying IVs to binary dependent variables, I estimate linear probability models (Angrist and Pischke 2009).

For IV estimates to have a causal interpretation, the instrument must be independent of confounders. Here, this implies that people born before and after the UPE reform were not dissimilar in ways that would bias the observed associations. However, an unobserved change to cohort-level determinants of overweight/obesity may have occurred simultaneously or just after the reform. To address this, I estimate a further model in which I exploit within-cohort variation in exposure to the reform because of state differences in the number of new schools per capita in 1974–1978. I interact *Eligible* with my measure of the number of new schools per capita in an individual's state of residence in 1974–1978 to create a new instrument, $Newschools \times Eligible$. I then reestimate Eqs. (2) and (3), with $Newschools \times Eligible$ replacing *Eligible* as the instrument for schooling in the first stage (Eq. (2)). All other features of the model specification remain the same.

Figure 3 plots the coefficients and confidence intervals of the interactions between the number of new schools per capita in an individual's state of residence and year of birth. The coefficients of this interaction are not statistically different from zero for the oldest cohorts (those *unexposed*). For those who were *partly exposed* (born in 1960–1967), only some coefficients are statistically significant. Among the *exposed* cohort, all coefficients are statistically different from zero, suggesting that average years of schooling increased among those who were actually eligible for UPE and were born in states where more new schools were built. Furthermore, the mixed results for those who were partly exposed supports my decision to exclude them from the treatment group.

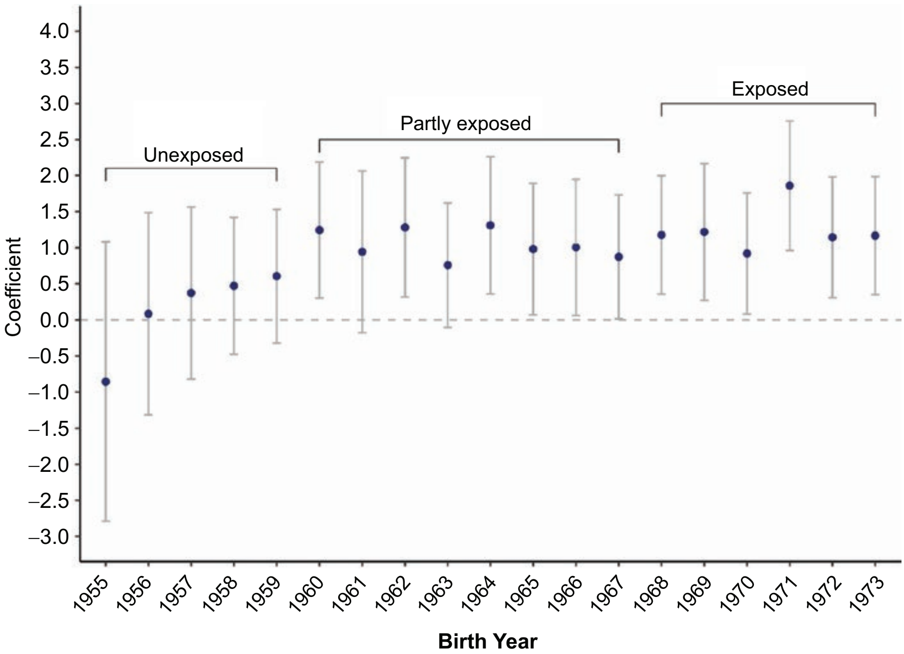


**Fig. 3** Years of schooling across birth cohorts. The figure displays coefficients of the interactions of year of birth × new schools per capita in an individual's state of residence.

## Instrument Validity

UPE eligibility and new school construction per capita capture changing schooling opportunities. To be valid instruments and satisfy the exclusion restriction, new school construction and UPE eligibility must affect overweight or obesity status only via changes in schooling. New school construction may be correlated with state-level factors that affect women's overweight or obesity risk independent of changes in schooling opportunities, including the determinants of new school construction, such as unmeasured economic conditions. To help address this, I include state fixed effects to control for unobserved or unmeasured time-invariant state-level factors that may predict overweight/obesity and correlate with differences in schooling opportunities across states. In robustness checks, I also substitute state fixed effects for a control for pre-UPE female primary school enrollment rate in the state in 1970. This adjusts for state-varying factors that may be correlated with new school construction, that may influence overweight/obesity directly, and that are also captured in the pre-UPE enrollment share; examples of such factors are the number of schools in an individual's state of residence before the reform, parental schooling levels, and local economic characteristics.

Two additional violations of the exclusion restriction are possible. Because the UPE reform included curriculum changes, it may have affected knowledge of the harms of overweight and obesity and how to prevent them independent of years of schooling. However, De Cao and La Mattina (2019) found no evidence that the new curriculum included health education. Another possible violation of the exclusion restriction is an impact of UPE on school quality. Although data limitations prevent me from definitively ruling out this possibility, Oyelere (2010) systematically assessed changes in potential proxies for school quality and found no evidence of deteriorating quality (Oyelere 2010). I must also assume monotonicity: that exposure to the reform caused individuals to obtain more schooling or to have no change in schooling but did not lead some individuals to obtain less schooling. Previous analyses of the reform noted that it was an unexpectedly popular program and led to widespread increases in schooling, suggesting that violations of this assumption are unlikely (Bray 1981; Csapo 1983).

A final requirement for valid inference from both of my IV estimates is that the instruments must be relevant in terms of having had a sufficiently strong effect on schooling. Figure 3 provides suggestive graphical evidence to support this for the UPE eligibility×new school construction instrument. Furthermore, this assumption is testable in a first-stage analysis of the association of years of schooling with the respective instruments. This test also provides an indication of whether overage enrollment was an issue for my analysis, which would weaken the association between coded UPE eligibility status and years of schooling. Each instrument can be considered relevant if the $F$ statistic for the instrument is greater than 10.

## Results

### Descriptive Analysis

Figure 4 outlines the selection of the 10,746 DHS respondents included in my analytic sample. Table 1 summarizes their demographic and anthropometric characteristics. On average, 33.6% of the sample was overweight or obese across all survey years. This number is slightly higher than the proportion reported in previous analyses of DHS data, which likely reflects the slightly older age of my sample (Kandala and Stranges 2014; Neupane et al. 2015).

Figure 5 plots schooling disparities in overweight/obesity; years of schooling are grouped according to the schooling levels associated with different years of schooling. The figure shows that the probability of being overweight/obese was lowest among those with the least years of schooling (0 years of schooling/no education) and highest among those with the most years of schooling (>12 years of schooling/post-secondary education). The proportion of respondents who were overweight or obese increased over time, from 1.9% in 2003 to 12.1% in 2013. The pattern in Figure 5 is similar when I examine schooling disparities in the proportion of obese respondents (Figure A1, online appendix). In both Figure 5 and Figure A1 (online appendix), schooling disparities are shown to be similar in all survey years.

The crude association between respondents' years of schooling and overweight/obesity in Figure 5 is also apparent in a multivariate regression model, shown in Table 2 (Model 1). Each additional year of schooling was associated with a 3.0% (95% CI: 2.0 to 3.0) increase in the probability of being overweight or obese after the model adjusts for covariates. Furthermore, individuals with secondary or post-secondary education were 1.48 times (95% CI: 1.01 to 1.96) more likely to be

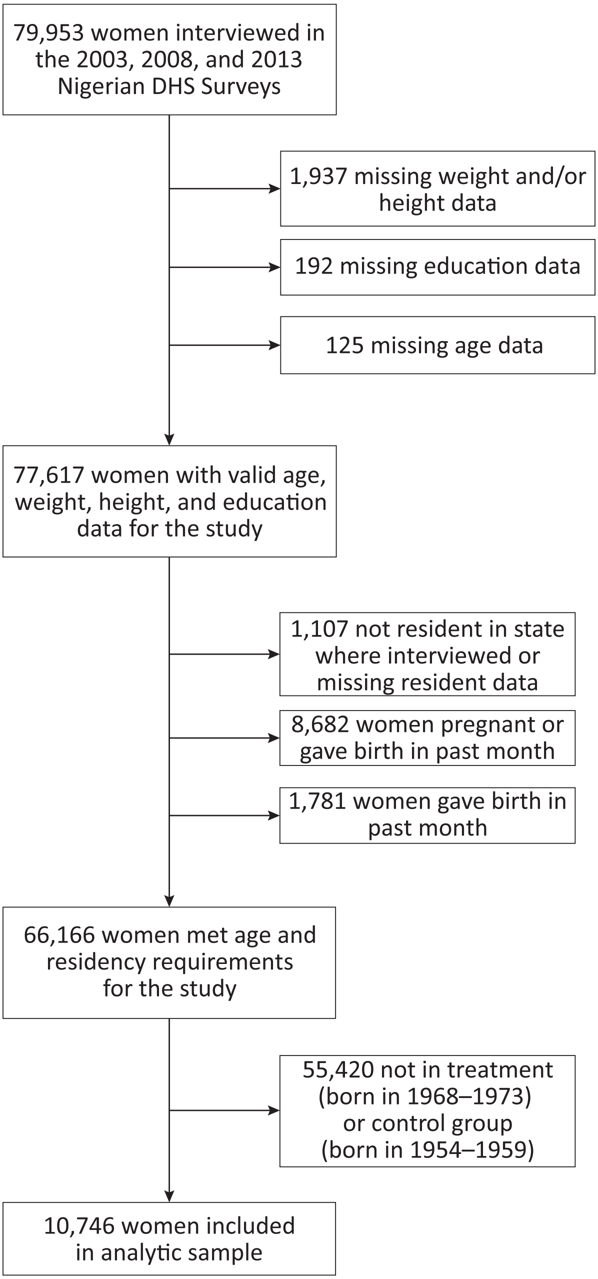


**Fig. 4** Selection of DHS respondents for analysis

overweight or obese compared with those with no education (see section 3, online appendix)—a finding comparable to a previous observational analysis of Nigeria (Neupane et al. 2015). Although suggestive, these associations might still be confounded by unobserved differences between those who attained more or fewer years of schooling.

**Table 1** Participant characteristics: 2003, 2008, and 2013 Nigerian DHS samples

| Variable | 2003 | 2008 | 2013 |
|---|---|---|---|
| Overweight or Obese (%) | 0.30 | 0.31 | 0.40 |
| | (0.46) | (0.46) | (0.49) |
| Age | 28.3 | 29.5 | 32.6 |
| | (7.27) | (4.08) | (1.88) |
| Years of Schooling | 4.9 | 6.1 | 6.2 |
| | (5.06) | (5.40) | (5.48) |
| Number of Observations | 1,568 | 5,098 | 4,080 |

*Notes:* Standard deviations are shown in parentheses. Statistics are calculated using DHS sample weights. Sample sizes are unweighted.

*Source:* Demographic Health Surveys Nigeria (2003, 2008, 2013).

### IV Estimates: First-Stage Results

Figure 6 provides suggestive graphical evidence that average years of schooling increased among cohorts eligible for UPE. Although the UPE reform abolished primary rather than secondary school fees, the proportion completing secondary school also increased (sections 4–5, online appendix). This finding likely reflects increased eligibility for secondary school (as a result of primary school completion), awareness of the benefits of schooling, the freeing of resources previously spent on primary education fees, or a need for greater education to remain competitive the labor market. The proportion completing postsecondary education did not increase.

My multivariate regression models testing formally for a first-stage association of being eligible for UPE and years of schooling are presented in Table 2 (Model 2). Average years of schooling among those eligible for the reform was 1.40 years higher (95% CI: 0.51 to 2.30) than among ineligible individuals. The instrument is considered relevant if the $F$ statistic on the instrument in the first stage is greater than 10. The first stage in my models is strong, yielding an $F$ statistic of 35.1. Model 3 in Table 2 confirms the relevance of the UPE eligibility×new schools per capita instrument, as illustrated in Figure 3. Each additional school per capita in an individual's state of residence was associated with an extra 1.05 years of schooling (95% CI: 0.12 to 1.93) among those eligible for UPE. As a formal test of relevance, I again compute the $F$ statistic. Here, the $F$ statistic was 36.2, again indicating that my instrument was not weakly correlated with schooling.

### ITT Estimates

Model 4 in Table 2 also shows the results from my estimate of the ITT—that is, the impact of being eligible for UPE on the probability of being overweight or obese. Women who were eligible for UPE were more likely to be overweight or obese than those not eligible, but the coefficient of being eligible for UPE was not statistically different from zero at the 1%, 5%, or 10% significance threshold (effect estimate: 0.02; 95% CI: –0.09 to 0.12).

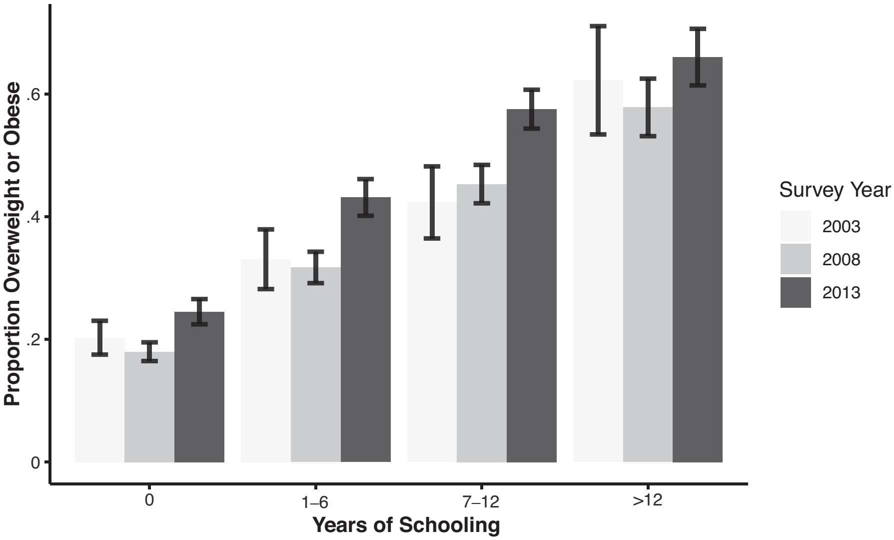


**Fig. 5** Overweight and obesity by years of schooling in Nigeria, grouped according to approximate levels of attainment. Error bars show 95% confidence intervals for mean proportions. The *x*-axis categories correspond to years of schooling among respondents: no education (0 years), primary education (1–6 years), secondary education (7–12), and postsecondary education (>12 years). The main models use a continuous schooling measure. Averages are calculated using DHS sample weights. Figure A1 in the online appendix shows obesity proportions only. *Source:* Demographic Health Surveys Nigeria (2003, 2008, and 2013).

## LATE Estimates

My ITT estimate of exposure to the UPE reform was consistent in my 2SLS IV models of the LATE using UPE eligibility to instrument for years of schooling (Table 2, Model 5). Each additional year of schooling induced by the reform was associated with a 4.0% increase in the probability of being overweight or obese, but this figure was not statistically different from zero (effect estimate: 0.04; 95% CI: –0.01 to 0.09). This result could be explained by unobserved cohort differences, which I address by exploiting within-cohort variation in exposure to the reform due to state-level differences in the number of newly constructed schools per capita in each state in 1974–1978 interacted with UPE eligibility. Model 6 in Table 2 shows that my 2SLS estimates exploiting state-level heterogeneity produced results that were consistent with those presented earlier: again, schooling had no statistically identifiable impact on the probability of being overweight or obese (effect estimate: 0.06; 95% CI: –0.04 to 0.13).

## Robustness Checks

Table 3 presents the results from a series of robustness checks. First, I reestimate my model using a linear (Model 1) and quadratic (Model 2) year-of-birth control to adjust for potential linear and nonlinear birth cohort trends. Results are consistent with my main models. Second, it is still plausible that my null finding is attributable to differences

**Table 2** Effect estimates

| Model | Exposure | Outcome | Effect Estimate (95% confidence interval) | Wild Bootstrap *p* Value |
|---|---|---|---|---|
| Model 1: Correlation | Years of schooling | Overweight or obese | 0.03 (0.02 to 0.03) | <.001 |
| Model 2: First Stage | UPE eligibility[a] | Years of schooling | 1.40 (0.51 to 2.30) | .01 |
| Model 3: First Stage | UPE × new schools[b] | Years of schooling | 1.05 (0.12 to 1.93) | .004 |
| Model 4: ITT | UPE eligibility | Overweight or obese | 0.02 (–0.09 to 0.12) | .76 |
| Model 5: 2SLS, UPE Eligibility | Years of schooling | Overweight or obese | 0.04 (–0.01 to 0.09) | .13 |
| Model 6: 2SLS, UPE × New Schools | Years of schooling | Overweight or obese | 0.06 (–0.04 to 0.13) | .60 |

*Notes:* All models are estimated using DHS sample weights. All models control for age, birth-year fixed effects, and state fixed effects. Models 1 is estimated per Eq. (1). Model 2 is estimated per Eq. (2). Model 3 is per Eq. (2) but substitutes UPE eligibility with UPE × new schools. Model 4 is per Eq. (1) but substitutes schooling with UPE eligibility status. Models 5 and 6 are per Eq. (3) but with different instruments in the first-stage Eq. (2). All models control for a continuous measure of age; the rate of female primary school enrollment in 1970; and survey, state, and birth-year fixed effects. Standard errors used to calculate 95% confidence intervals are clustered at the state level. Wild cluster bootstrap *p* values are calculated using procedures described in Cameron et al. (2008). Residuals are repeatedly resampled by cluster to form a pseudo-dependent variable, and the model is estimated for each resampled data set. The *p* values are the proportion of bootstrapped *t* statistics that are at least as large as the *t* value from the original model.

*Source:* Demographic Health Surveys Nigeria (2003, 2008, 2013).

[a] UPE eligibility is based on birth year.

[b] The coefficient of UPE eligibility is interacted with new schools per capita in the respondent's state.

in preexisting trends in overweight/obesity in each state. I therefore estimate my models with a birth year × state interaction to adjust for state-level birth-year trends (Model 3). Third, I substitute state fixed effects for the pre-UPE primary enrollment share (Model 4). This model helps adjust for unobserved factors that are correlated with pre-UPE schooling levels and new school construction and that might influence overweight and obesity independent of changing schooling opportunities, such as local economic conditions and parental economic circumstances (McLaren 2007).

I then conduct a placebo analysis in which I code individuals born in 1956–1961 as *treated* and those born in 1950–1955 as *untreated*. Because neither group should have been affected by UPE, observing a negative or positive UPE effect (which could offset the true schooling effect) when comparing these two groups would diminish confidence that my models can capture any schooling effect, rather than a time trend. I find no significant effect (Model 5, Table 3).

My analysis links individuals to states based on their state of residence at the time they were interviewed for the DHS, but individuals may have migrated since childhood. Data from the 2010 Internal Migration Survey conducted by the National

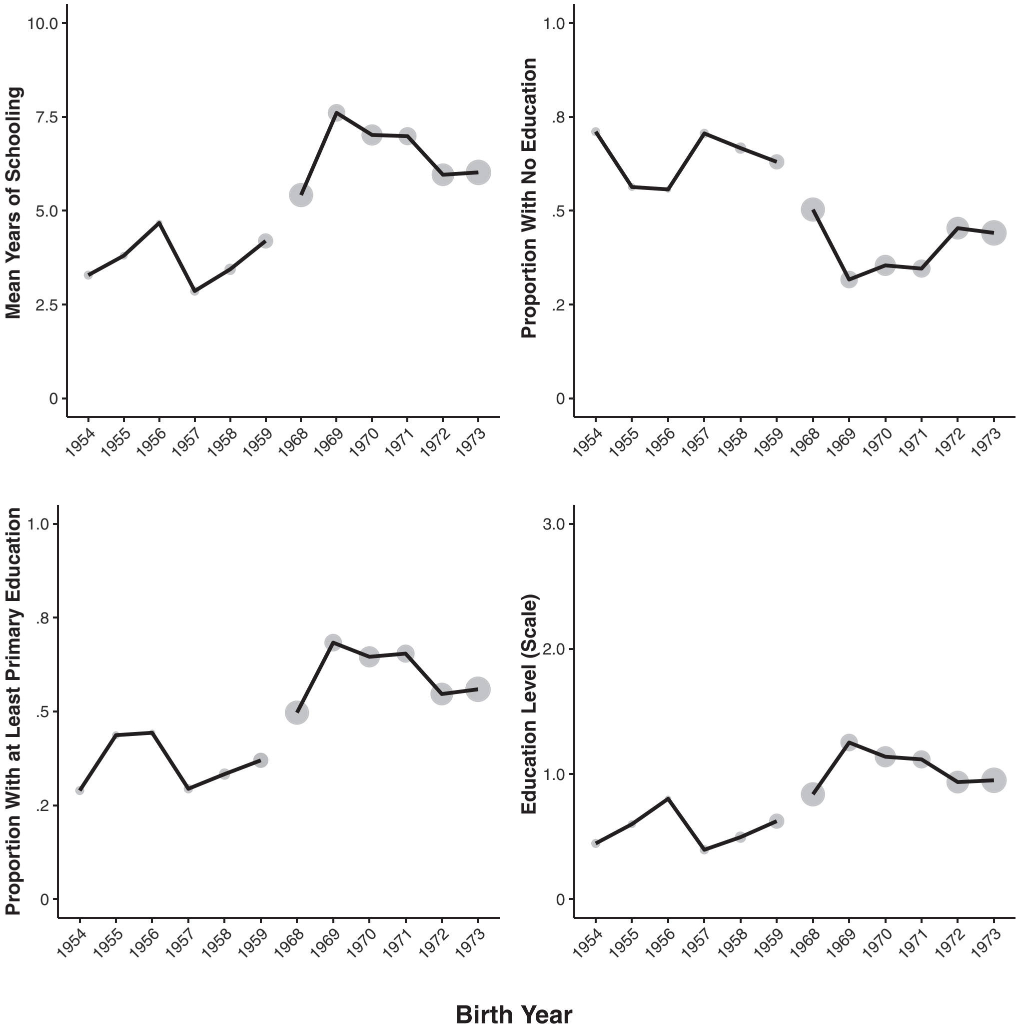


**Fig. 6** Trends in educational attainment by birth cohort and UPE exposure. The point size is proportional to the number of respondents. Averages are calculated using DHS sample weights. The education scale refers to highest education level achieved: 0 = no primary education, 1 = primary education, 2 = secondary education, and 3 = postsecondary education. See Figure A1 in the online appendix for an additional figure showing changes to secondary and postsecondary education. *Source:* Demographic Health Surveys Nigeria (2003, 2008, and 2013).

Population Commission show that 11.4% (standard deviation = 8.5) of Nigerians had migrated internally at least once by the date of the survey (International Organization for Migration 2016). Migrants tend to be more healthy than nonmigrants (Kennedy et al. 2006; Newbold 2005). Thus, a concern for my analysis is whether a healthy migrant effect offset the true effect of schooling. For example, if relatively healthy individuals who were less likely to be overweight or obese moved to states where more new schools were built but moved after they had completed primary school in another state, then my null finding could be attributable to a healthy migrant effect

**Table 3** Additional analyses and robustness checks: 2SLS results using UPE eligibility and new schools per capita to instrument for years of schooling

| Model | Effect Estimate | 95 % Confidence Interval | Wild Bootstrap *p* Value |
|---|---|---|---|
| Model 1: Linear Birth-Year Trend | 0.05 | (–0.03 to 0.13) | .51 |
| Model 2: Quadratic Birth-Year Trend | 0.05 | (–0.03 to 0.13) | .49 |
| Model 3: State Birth-Year Trends | 0.12 | (–0.17 to 0.41) | .58 |
| Model 4: Pre-UPE Primary Enrollment Rate Control | 0.05 | (–0.11 to 0.20) | .57 |
| Model 5: Placebo Treatment | 0.02 | (–0.43 to 0.47) | .83 |
| Model 6: Permanent Residents Only | 0.05 | (–0.04 to 0.13) | .55 |
| Model 7: Resident Since 4 Only | 0.05 | (–0.04 to 0.13) | .55 |
| Model 8: Exclude Lagos | 0.04 | (–0.04 to 0.13) | .64 |
| Model 9: Exclude High Migration States[a] | 0.09 | (–0.01 to 0.20) | .75 |
| Model 10: Exclude States With >10% Migrants | 0.09 | (–0.002 to 0.18) | .78 |
| Model 11: 1976 Cohort Cutoff | 0.02 | (–0.21 to 0.24) | .88 |
| Model 12: 1974 Cohort Cutoff, All Respondents | 0.06 | (–0.08 to 0.20) | .73 |
| Model 13: 1976 Cohort Cutoff, All Respondents | 0.004 | (–0.78 to 0.78) | .88 |
| Model 14: Federal Funds as Instrument[b] | –0.27 | (–6.90 to 6.40) | .75 |
| Model 15: Pre-UPE Enrollment as Instrument[c] | 0.01 | (–0.04 to 0.07) | .64 |
| Model 16: BMI Outcome | 0.34 | (–0.60 to 1.30) | .65 |

*Notes:* Similar exclusion restriction assumptions apply for these instruments and my original instrument. Standard errors to calculate 95% confidence intervals are clustered at the state level. Wild cluster bootstrap *p* values are calculated using procedures described in Cameron et al. (2008). Residuals are repeatedly resampled by cluster to form a pseudo-dependent variable, and the model is estimated for each resampled data set. The *p* values are the proportion of bootstrapped *t* statistics that are at least as large as the *t* value from the original model. All models are estimated using DHS sample weights. See the main text for a full description of each robustness check. Models 1–9 and 12–16 show an effect of an additional year of schooling on the probability of being overweight or obese from two-stage least squares regression models using UPE eligibility × new schools per capita as an IV; Models 10 and 11 interact eligibility with two alternative instruments listed in the left hand column.

[a] Model 9 excludes states with migrant share more than 1 standard deviation higher than the mean.

[b] $F$ statistic = 18.1.

[c] $F$ statistic = 23.5.

*Source:* Demographic Health Surveys Nigeria (2003, 2008, 2013).

counteracting the true, positive effect of schooling. Such concerns would be particularly important if internal migration was higher in states with more newly constructed schools. Although data availability is limited, the association between the internal migrant share in 2010 and the number of new schools per capita in 1974–1978 is weak and is not statistically significant (Pearson's $r = -.34$, $p = .15$).

Furthermore, I restrict the sample to individuals who said they had always lived where they were interviewed (Model 6, Table 3) and who had lived where they were interviewed since their 4th birthday (Model 7, Table 3). These restrictions are conservative because they exclude individuals who were educated where they currently live but moved elsewhere in between. As an additional test, I exclude Lagos from the sample (Model 8, Table 3) because it is a state with a large internal migrant share (National Population Commission 2016). I also exclude from my analysis all states with internal migrant shares more than 1 standard deviation larger than the mean (Model 9, Table 3) and larger than 10% in 2010 (Model 10, Table 3). I again find no significant effect of schooling.

Next, I evaluate the sensitivity of my results to my cohort categorization decisions. I reestimate the effect of schooling using the official UPE 1976 implementation date as a cohort cutoff, with those born in 1970–1975 in the treatment group and those born in 1956–1961 in the control group (Model 11, Table 3). I also expand the age range of the control and treatment groups: I include in the control group all individuals who were older than 15 when UPE was implemented in both 1974 (Model 12, Table 3) and 1976 (Model 13, Table 3) and include in the treatment group all individuals who were 6 or younger in those years. Results are comparable to my original estimates.

Two alternative instruments for years schooling have been used in demographic and political analyses of the Nigerian UPE: pre-UPE primary school enrollment and federal disbursements for new school construction (Larreguy and Marshall 2017; Osili and Long 2008). I use the number of new schools per capita as an instrument on the assumption that it is a better proxy for changing schooling opportunities. I nevertheless evaluate whether my results are sensitive to my choice of instruments. Models 14 and 15 in Table 2 show that my estimate of the effect of schooling is consistent when alternative instruments are used. However, the first stage is weaker compared with my original instrument, which supports my instrument choice.

I originally used a dichotomous indicator of an individual's overweight or obesity status rather than a continuous measure because increases in BMI can have very different implications depending on the initial value (Goryakin et al. 2015). I nevertheless reestimate my models using a BMI scale and find that this alternative operationalization produces consistent results (Model 16, Table 3). As a further robustness check, I estimate a generalized linear mixed model incorporating average years of schooling at the state level. This test assesses whether peer education and associated spillover effects influenced individual's overweight or obesity risk and attenuated the individual-level schooling coefficient (Kravdal 2012). In addition, I originally conducted all analyses using individual weights provided by the DHS. Incorporating sample weights can impact the efficiency of the estimators and statistical power (Solon et al. 2015). As shown in sections 6 and 7 of the online appendix, I find comparable results in both tests.

Finally, I assess whether the impact of schooling varies according to sociodemographic and contextual variables that can modify the association between schooling and body weight. I evaluate whether the impact of schooling varied by survey year (Dinsa et al. 2012), by urban (rather than rural) location (Kandala and Stranges 2014), and by religious affiliation (Kim and Sobal 2004). I further evaluate variation in schooling impacts according to an individual's age, marital status, and number of children (Gore et al. 2003; Lee et al. 2005). Additional schooling had no statistically significant impact in any subgroup (section 8, online appendix).

## Discussion

This paper presents what is, to my knowledge, the first quasi-experimental analysis of the causal link between women's schooling and overweight or obesity status in an LMIC: Nigeria. Using exogenous variation in women's years of schooling due to differences in primary school access across birth cohorts and states, I found that each additional year of schooling caused by the policy change was associated with a 6% increase in the probability of being overweight or obese. However, this effect estimate was not statistically different from zero. In subsequent analyses, I evaluated the sensitivity of my results to my model and sample specifications and examined whether the impact of schooling varied across subgroups. Additional schooling was correlated with a higher probability of being overweight or obese in all specifications; however, consistent with my main results, these associations were not statistically significant.

These findings have important implications for three interrelated bodies of scholarship. First, my results differ markedly from previous observational analyses of schooling-related disparities in overweight/obesity in Nigeria. Prior studies found a positive association between years of schooling and the probability of being overweight or obese among Nigerian women (Kandala and Stranges 2014; Neupane et al. 2015). Yet, my quasi-experimental analysis found no significant effect of additional schooling. This suggests that the positive association between schooling and overweight/obesity in Nigeria is attributable to unobserved characteristics that lead individuals to obtain more years of schooling rather than a causal effect of schooling itself. Correlational estimates therefore appear to overstate the impact of schooling on overweight/obesity status in Nigeria because they capture the influence of other factors. This has wider implications for research concerning women's schooling, overweight/obesity, and associated inequalities in LMICs. By showing that the association between schooling and overweight/obesity status in an LMIC is not robust to adjustments for unobserved confounders, my findings are relevant for research pertaining to many LMICs where a positive correlation between schooling and overweight/obesity was previously documented (Dinsa et al. 2012). I identified one case in which this cross-sectional association appears to be spurious, which calls into question the causal nature of such correlations elsewhere.

Second, my findings contrast with quasi-experimental studies finding that additional years of schooling reduced the probability of being overweight or obese among women in several HICs (Hamad et al. 2018). One possible explanation for this difference is that a range of contextual characteristics modify the scope or impact of mechanisms linking schooling to overweight/obesity in Nigeria. Contextual factors that may be relevant include idiosyncrasies in the education system or the broader Nigerian context that diminish the importance of schooling and/or the advantages that follow, such as an absence of health education in the curriculum or the sparse availability of relevant health information typically accessed by educated groups elsewhere. Schooling may also confer individuals with a range of advantages that reduce the risk of overweight or obesity, but these advantages may be offset by sociocultural pressures that encourage larger body size, high-calorie diets, and low physical activity (Brink 1989; Oe 2009).

Insofar as contextual factors do explain my results, the contrast between my findings and studies of HICs imply that schooling's protective effects in HICs could be fundamentally linked to societal characteristics that pertain to HICs and hence confer advantages to more-educated groups rather than any inherent, universal quality of additional schooling alone (Hayward et al. 2015; Phelan et al. 2010). Alternative interpretations, however, are possible: there may be unobserved confounders, and the

UPE reform may have corresponded to a reduction in the quality of education. Future research should examine multiple contexts to determine whether and how contextual factors in LMICs and HICs alter schooling's effects on overweight and obesity.

Third, scholars and policy professionals have regularly promoted schooling and education reforms as a means of improving population health, reducing health inequities, and achieving diverse health-related Sustainable Development Goals (Mechanic 2007; UN 2015; WHO 2017). Indeed, women's schooling opportunities have expanded considerably over the past 30 years (Roser and Ortiz-Ospina 2016). The results from this study, however, show a null effect of additional schooling on overweight/obesity in Nigeria. Although schooling reforms can be effective in improving diverse health behaviors and outcomes traditionally associated with LMICs, such as child nutrition, the results from this study suggest that such policies are likely to have limited effectiveness in reducing overweight/obesity in Nigeria and possibly in other LMICs. This implication is particularly relevant in the context of the changing disease burden in Nigeria and other LMICs, which is now shifting toward an increasing prevalence of overweight- and obesity-related illnesses that were once common only in HICs (Ebrahim et al. 2013; Lim et al. 2012). My findings suggest that although schooling may be a powerful driver of reductions in many so-called diseases of poverty traditionally associated with LMICs like Nigeria, these benefits may not necessarily apply to the contemporary challenge of overweight/obesity and associated diseases of affluence now facing Nigeria and, quite possibly, other LMICs.

## Limitations

This study has several limitations, and inferences from my IV estimates rest on some untestable assumptions. First, I assume that exposure to the UPE reform affected overweight/obesity risk only through changes in schooling and subsequent pathways (i.e., the exclusion restriction). Efforts to build new schools may have created employment opportunities in the construction sector. It could therefore be argued that the null effect could be attributable to an effect of improving family economic circumstances (increasing the ability to purchase calorie-dense foods, for example) counteracting the effect of schooling (if schooling reduces the risk of overweight/obesity). Prior research, however, has found that the direction of the association of family wealth and schooling on overweight/obesity status are comparable (rather than opposing), making it unlikely that schooling and parental economic circumstances had offsetting effects that led to a null result (Chukwuonye et al. 2013; Kandala and Stranges 2014; Neupane et al. 2015). A related point is that data were not available to assess whether new school construction was correlated with quality of education across states. Deteriorating schooling quality may have diminished or offset any impact of increased years of schooling, causing the schooling coefficient to underestimate the true effect. Although Oyelere's (2010) analysis suggests that this may not be the case, future studies should collect more detailed information concerning schooling quality during educational reforms in order to help overcome such issues.

Second, in order to interpret my results as the LATE, I must assume monotonicity. The popularity of the UPE reform suggests that violations of this assumption are unlikely but could be possible if, for example, persons with a strong preference for small school sizes dropped out as a result of increased school sizes after the reform.

Third, incomplete migration data in the DHS prevented me from identifying whether my results were attributable to migration patterns. However, my results were consistent in tests that evaluated the sensitivity of my results to selective migration with available data.

Fourth, an important limitation pertains to measurement issues. The DHS calculates expected years of schooling based on the number of years usually required to achieve the highest level of education an individual reported having completed, plus the number of years at higher levels. This calculation may capture measurement error due to grade repetition or grade skipping.

Fifth, the causal effects that I estimated are also local to the subpopulation of compliers and may not be generalizable to other subgroups. Furthermore, the coefficient of schooling was positive but not statistically significant in any of my model specifications, but the true effect may be positive for some individuals and negative for others, leading to no average effect. In my subgroup analyses, I investigated heterogeneity according to multiple sociodemographic characteristics. I identified no effect, but there may be unmeasured variation.

Furthermore, Nigerians may also value larger body sizes for men, and it remains uncertain whether the results from my study apply to men too. Nigeria is also a large country, and there may be within-country variation in the sociocultural and contextual variables that might explain my results. However, data were not available to assess sociocultural norms, values, and pressures that encourage larger body size, limiting my ability to assess the extent to which these factors explain my results. This points to a need to obtain such data and perform additional heterogeneity analyses in future studies.

Finally, my findings may be particular to the Nigerian context (Brink 1989; Oe 2009). Several studies have suggested that larger body sizes are culturally valued in other LMICs and have identified a positive association between schooling and overweight/obesity (Dinsa et al. 2012; Neupane et al. 2015). My findings may also apply to those contexts. More research is nevertheless necessary to assess the generalizability of my results.

## Conclusion

These limitations notwithstanding, this study identified that each additional year of schooling was not causally associated with a change in women's overweight or obesity risk in Nigeria. The contrast between this finding and previous results from HICs suggests that contextual factors may play an important role in calibrating the effect of schooling on women's overweight and obesity in different countries and contribute to a null effect in the case of Nigeria. Furthermore, these results suggest that correlational studies are confounded by unobserved variation and thus overestimate the causal effect of schooling on overweight and obesity in Nigeria. This calls into question the causal nature of positive associations identified in other contexts and demonstrates the need for more robust quasi-experimental research to examine the effect of schooling on overweight and obesity in LMICs. ■

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