Schooling Without Earnings: Education Reforms and Labour-Market Outcomes in Nigeria

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Abstract

What happens to the returns to schooling when a country expands education at scale? We study Nigeria's 1999 Universal Basic Education reform, exploiting cohort-by-state variation in reform exposure interacted with pre-reform education deficits to identify reform-induced schooling. Fully exposed cohorts in the most deficient states gained about 1.4 additional years of schooling. Our 2SLS estimates show that an additional year of reform-induced schooling raises formal-sector employment by 4.2 percentage points but leaves the wage return statistically indistinguishable from zero. This pattern is consistent with credential supply outpacing formal-sector labour demand. Descriptive general-equilibrium evidence supports this reading, as university graduates face higher unemployment yet better job placement when employed, consistent with queueing for rationed formal jobs, while the access return erodes across cohorts and turns negative for the youngest. Supply-side schooling investment cannot restore these returns without a corresponding expansion in formal-sector labour demand.

JEL codes: I25, I26, J24, J31, O15, O55.

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1 Introduction

Mass schooling reforms do not always deliver the labor-market payoff that the returns to schooling literature would seem to promise, a pattern documented in a small but growing body of general-equilibrium evidence (Duflo, 2004; Khanna, 2023). The global private return to an extra year of schooling averages about 9 percent and is highest in low-income countries (Psacharopoulos and Patrinos, 2018), an estimate obtained using the earnings framework that Mincer (1974) established. As a result, the canonical policy inference is that if a year of schooling raises earnings by 5–10 percent, then large-scale schooling expansions are among the highest-return investments available to developing-country governments. However, the Mincerian return is fundamentally a price, and prices move with the supply of schooling. When a reform expands schooling for an entire cohort, the return the marginal graduate earns depends on whether labor demand expands enough to absorb them (Acemoglu and Autor, 2011; Duflo, 2004; Khanna, 2023).

The empirical challenge is that the reforms large enough to move a country's stock of skilled labor are exactly the reforms too large to study in partial equilibrium. An instrument that shifts one child's schooling while holding the labor market fixed identifies only a private return, and is uninformative about what happens when an entire cohort, or an entire country, is treated at once. Credible general-equilibrium evidence therefore requires two features that rarely coincide in the same setting: identifying variation sharp enough to support instrumental-variables estimation, and a labor market old enough, after the reform, to have finished absorbing the cohorts it produced. Khanna (2023) exemplifies both features in the case of India. Our paper supplies them for Sub-Saharan Africa, where large-scale schooling expansions have been common over the past three decades, but rigorous general-equilibrium evidence on their labor-market consequences has been largely absent.

Nigeria, Africa's most populous country, offers a natural setting to investigate this question. In September 1999 its newly elected civilian government launched the Universal Basic Education (UBE) programme, mandating nine years of fee-free, compulsory basic schooling. The reform acquired its legal standing under the UBE Act of 2004 and was financed by a dedicated federal intervention fund, equal to two percent of the Consolidated Revenue Fund, administered through state-level boards with federal matching grants for classroom construction, teacher recruitment, and instructional materials.[1] The expansion that followed was large by any measure, as gross secondary enrollment rose from 24 to 47 percent between 1990 and 2024.[2] A quarter-century later, the reform's first fully exposed cohorts are in their late twenties and early thirties, old enough to observe in the labor

[1] UBEC official FAQ: ubec.gov.ng/?p=2018. Confirms the September 1999 launch and the UBE Act 2004's 2% CRF allocation.

[2] World Bank, World Development Indicators, series SE.SEC.ENRR (school enrollment, secondary, % gross), Nigeria: data.worldbank.org/indicator/SE.SEC.ENRR.

market and recent enough that the reform's staggered timing across cohorts and states still identifies its effects.

We exploit that staggered timing, which is fixed deterministically by birth year. A child born in 1984 or earlier had aged out of basic-school age before the reform began. A child born in 1993 or later, by contrast, spent all nine basic-education years under it. This design has precedent in Nigeria's own earlier 1976 Universal Primary Education (UPE) reform, previously used by Osili and Long (2008) to study fertility effects. We further argue that UBE exposure interacts with geography, because the reform's impact depended on how much room each state had to expand. In the 1990 Demographic Health Survey,[3] more than 95 percent of women in the Northeast and Northwest had never entered secondary school, against 56 and 77 percent in the Southwest and the Southeast, respectively. Hence, a federally financed, nationally uniform mandate of nine free years therefore relaxed a binding constraint in the north, while largely ratifying an already-high status quo in the south.

This study instruments completed schooling with the interaction of cohort exposure and each state's pre-reform deficit, controlling for state-of-childhood and birth-year fixed effects, so the identifying variation is the extra schooling gained by exposed cohorts in high-deficit states relative to low-deficit ones. Three features discipline the exclusion restriction this requires. First, state-of-childhood and birth-year fixed effects absorb the two most obvious confounds which are; time-invariant differences between high- and low-deficit states, and any nationwide cohort trend in schooling. A remaining confound would have to hit high-deficit states differentially, timed to the exposure gradient, and work through some channel other than schooling. Second, because schooling is instrumented with four baselines from unrelated data systems, a confound specific to one measure would show up as disagreement between instruments; our over-identification tests below show none. Third, assignment is by state of childhood, not state of residence, so the estimates are not contaminated by selective migration of educated workers toward stronger labor markets.

A distinctive feature of our estimation strategy is that the pre-reform deficit is measured four separate ways, from four mutually independent data systems: (i) the share of women with no secondary schooling as contained in the *1990 Demographic and Health Survey (DHS)*, an outcome-based household-survey measure; (ii) the 1995 basic-enrolment deficit from the *National Bureau of Statistics Statistical (NBS) Year Book*, a contemporaneous administrative count; (iii) the 1995 share of unqualified primary-school teachers, an input-quality measure from the *UNESCO Education-for-All assessment*; and (iv) the distance from each state to the nearest pre-1999 Federal College of Education, a Card (1995)-style geographic measure of teacher-training infrastructure. Each baseline

[3]DHS 1990: dhsprogram.com/pubs/pdf/FR27/FR27.pdf.

generates its own instrument; each identifies a distinct margin along which UBE could and did expand schooling. That these four baselines arise from unrelated data-generating processes disciplines the identification far more rigorously than any single instrument could; the over-identification tests reported below confirm their agreement on the central outcome.

Three central findings emerge from our estimates. First, we show that the UBE reform increased the amount of schooling children received. The first-stage coefficient implies that a fully exposed child in a state with the maximal pre-reform deficit completed roughly 1.4 more years of schooling than an unexposed child in a state with no deficit. Second, the reform raised the probability of holding a formal job, but not the wages associated with holding one. Our two-stage least squares estimates show that each reform-induced year of schooling significantly raised the probability of formal-sector employment (a job with a written contract, pension, or government employer) by 4.2 percentage points, a large effect against a sample mean of 7.4 percent. The same year of schooling, however, produced a wage return statistically indistinguishable from zero. This absence of a wage effect is not an artefact of a single weak instrument. Across the fourteen usable instrument combinations, every wage estimate lies between -4.8 and $+4.2$ percent with none statistically significant, while every formal-sector estimate lies between $+4.0$ and $+6.5$ percentage points, with 12 of 14 significant at the five percent level. The cleanest over-identified specification, instrumenting jointly with the 1990 DHS outcome baseline and the 1995 NBS administrative baseline, two measures with no common data-generating process, yields a 2SLS coefficient of 4.9 percentage points on formal-sector employment, with a Hansen J statistic whose p-value of 0.495 fails to reject the null of instrument exogeneity.

Third, graduates face a queue for scarce good jobs rather than a shortage of relevant skills. Descriptively, in the pooled labor force survey (NLFS 2022Q4–2025Q2, ages 20–64), the ILO[4] unemployment rate rises with education, from 1.7 percent among primary-educated workers to 9.7 percent among university graduates (Figure 3). Our probit estimates (Table A3) confirm this is not simply composition. Controlling for age, gender, urban residence, and geographic and time fixed effects, university graduates remain 2.7 percentage points more likely to be unemployed than workers with no formal education, yet, among those who are employed, 35.9 percentage points less likely to hold a low-skill job. This queue has also grown longer over time, as our estimates in Table 4 show. Comparing cohorts by exposure to the reform, each additional year of schooling raised the probability of formal employment by 2.4 percentage points among pre-reform cohorts but only 0.8 among fully exposed cohorts, a 68 percent decline, with a matching 66 percent decline in the wage-employment effect. For the fully exposed cohort, an additional year of schooling now lowers the probability

[4]International Labour Organization.

of employment by 1.1 percentage points, a reversal of the positive pre-reform effect. The decline is steepest among men and in southern Nigeria, where private employers have historically valued credentials most.

This paper contributes to three literatures. First, it adds to the quasi-experimental literature on instrumental-variables estimates of the returns to schooling (Card, 2001) and, specifically, to evidence from developing-country schooling reforms (Duflo, 2001), offering what is, to our knowledge, the first causal estimate of the labor-market returns to one of Africa's largest basic-education reforms. It further pairs a partial-equilibrium 2SLS estimate with direct general-equilibrium evidence from the same labor market, following the methodological approach of Duflo (2004) and Khanna (2023), who similarly combine individual-level causal estimates with market-level evidence of price adjustment. Second, it speaks to the literature on education expansion and labour demand in Africa (Fox et al., 2016), providing individual-level causal evidence for the claim that job quantity, not workforce quality, is the binding constraint. Third, it gives empirical content to the older sociological concept of credential inflation (Collins, 1979), via the access-without-earnings asymmetry.

The remainder of the paper proceeds as follows. Section 3 describes the reform and the pre-reform educational geography that generates identification. Section 4 describes the data. Section 5 lays out the empirical strategy and discusses threats to identification. Section 6 presents the first stage and the 2SLS estimates. Section 7 documents the falling cohort return and its heterogeneity. Section 8 presents the aggregate evidence on absorption. Section 9 reports robustness, and Section 10 concludes.

2 Literature Review

This paper draws on four distinct but complementary literatures, each of which shapes both the design of our empirical strategy and the interpretation of our results. The first is the instrumental-variables literature on the returns to schooling. The second is the smaller literature using large government schooling expansions to identify general-equilibrium labor-market effects. The third is the literature on structural transformation and labor demand in Sub-Saharan Africa. The fourth is the older sociological literature on credential inflation.

2.1 Returns to schooling and the identification problem

The starting point for any assessment of a schooling reform is the observed relationship between schooling and earnings, formalized by Mincer (1974) in the human-capital earnings function that remains the workhorse specification of the literature. Psacharopoulos and Patrinos (2018), synthe-

sizing 1,120 estimates drawn from 139 countries between 1950 and 2014, report a global average private return to an additional year of schooling of approximately 9 percent. Returns are highest in Sub-Saharan Africa, at 13 percent, with the five highest-return countries in the world, Botswana, Ethiopia, Rwanda, South Africa, and Tanzania, all African. Ogundari and Aromolaran (2017) extend this line of inquiry to Nigeria specifically, estimating that labor-market and household-welfare returns are concentrated at the tertiary level, with primary, secondary, and postgraduate schooling yielding comparatively limited returns.

Cross-sectional Mincerian returns of this kind confound the causal effect of schooling with the effect of unobserved ability, family background, and other determinants of earnings correlated with schooling itself, a concern that motivated a substantial instrumental-variables literature seeking exogenous sources of variation in completed schooling. Card (1995) proposed one influential solution, instrumenting schooling with geographic distance to the nearest college, on the logic that proximity to a college lowers its effective cost without directly affecting wages. Card (2001) surveys this literature and its persistent econometric problems, documenting that instrumental-variables estimates of the return to schooling frequently exceed ordinary least squares estimates, a pattern the literature has come to attribute to heterogeneity in returns across the population, combined with the fact that an instrument identifies a local average treatment effect for the subpopulation of compliers rather than the population-average return. Imbens and Angrist (1994) formalize this local average treatment effect result, showing that an instrument reveals the causal effect only for individuals whose treatment status the instrument itself moves, and that this effect need not coincide with the effect for the population as a whole.

2.2 Large-scale schooling expansions and general equilibrium

A parallel literature exploits large government schooling expansions as natural experiments, typically finding effects at the individual level consistent with the returns literature above. Duflo (2001) studies the Indonesian government's 1973–1978 program to construct more than 61,000 primary schools, combining regional variation in the intensity of school construction with cohort variation induced by the timing of the program. She finds that each primary school built per 1,000 children raised completed schooling by 0.12 to 0.19 years and wages by 1.5 to 2.7 percent; across her instrumental-variables specifications, she reports implied returns to schooling of 6.8 to 10.6 percent, in line with the OLS benchmark reported by Psacharopoulos and Patrinos (2018). In Nigeria, Osili and Long (2008) exploit the country's earlier 1976 Universal Primary Education reform, using variation in program exposure across regions and birth cohorts to estimate that an additional year of female schooling reduces early fertility by 0.26 births, evidence that basic-education reforms of this kind

can generate substantial non-labor-market returns.

Each of these studies estimates a partial-equilibrium treatment effect, that is, the causal effect of schooling on an outcome, holding the surrounding labor market fixed. Duflo (2004) demonstrates why this framing can be misleading. Policy recommendations based on partial-equilibrium analysis can fail if general-equilibrium effects undo the policy's direct effects (Heckman et al., 1998). Studying the same Indonesian school-construction program using the wages of an older, unexposed cohort, she finds that wages fell in the regions where the program expanded schooling fastest, evidence that the increase in the supply of educated labor was not matched by a corresponding increase in physical capital. Khanna (2023) operationalizes this concern directly for India's district-level primary schooling expansion, developing a general-equilibrium model and using a regression-discontinuity design to separately estimate its sufficient statistics. He finds that a year of schooling raises earnings by 13.4 percent in partial equilibrium, but that general-equilibrium labor-market adjustment depresses this return by 6.6 percentage points, with distributional consequences across skill groups: unskilled workers gain from the reform, while skilled workers lose. Our paper's four-baseline instrumental-variables design for Nigeria's Universal Basic Education reform, paired with cohort-panel evidence from the Nigeria Labour Force Survey, follows this same general-equilibrium logic, extending it to a Sub-Saharan African setting where such evidence has, to our knowledge, been largely absent.

2.3 Structural change, labor demand, and credential inflation in Africa

Whether a schooling expansion raises earnings in general equilibrium depends on whether labor demand expands to absorb the graduates it produces. Acemoglu and Autor (2011) formalize this dependence in a task-based framework in which wages are set by the allocation of skills across tasks and by how technology reassigns those tasks between labor and capital, so that an increase in a skill group's relative supply depresses its relative wage unless matched by a corresponding shift in the tasks demanded of that skill group. McMillan and Headey (2014) examine the accompanying process of structural transformation in Sub-Saharan Africa, the movement of labor out of agriculture into other sectors, and the extent to which it has proceeded unevenly across countries and periods, bearing on the region's capacity to generate the formal, higher-productivity jobs into which a rising stock of credentialed workers might be absorbed. Filmer and Fox (2014) and Fox et al. (2016) document that most African youth cycle through agriculture, self-employment, and unpaid family work rather than wage employment regardless of educational attainment, and that this reflects, alongside skill deficits, a persistent shortage of formal wage and salaried jobs; both studies caution against policy responses that address human capital alone without also expanding labor demand.

Where credential supply outruns growth in the number of jobs that reward credentials, the

older sociological literature on credential inflation anticipates precisely the asymmetry this paper documents between access and earnings. Collins (1979) argues that formal credentials function partly as a positional good, in that employers use credentials to ration access to a relatively fixed number of desirable jobs, so that when credential supply rises faster than the number of such jobs, a credential's power to sort applicants into employment can persist even as its power to command a wage premium erodes. Brown et al. (2011) extend this argument to a global setting, describing how the international expansion of higher education has intensified positional competition for a limited pool of high-skill jobs across both developed and developing economies. Teal (2011) develops the labor-demand side of this argument for African labor markets, showing that education explains only a small share of the wide variation in wages across and within countries and arguing that the scarcity of firms able to generate rising labor demand, rather than the skill level of the workforce, is central to understanding persistently low prices for labor in the region, a diagnosis consistent with Rodrik (2008)'s broader argument that industrial policy should be organized around identifying and relaxing the binding constraints on structural transformation, rather than assuming that supply-side investments in human capital will by themselves generate the demand needed to absorb them.

Taken together, this literature suggests that the labor-market payoff to a schooling reform is not a fixed parameter that can be read off pre-reform cross-sectional returns. It depends on the elasticity of labor demand, on whether structural transformation accompanies educational expansion, and on whether formal-sector job growth keeps pace with the credentialed labor supply a reform creates. Nigeria's Universal Basic Education reform offers a setting in which each of these mechanisms can be examined directly, using variation both across the reform's exposed cohorts and across the pre-reform educational geography its four instruments recover.

3 The Universal Basic Education Reform

3.1 The programme

Nigeria returned to civilian rule in May 1999. Among the new government's first acts was the launch, in September 1999, of the Universal Basic Education programme which entailed nine years of free, compulsory schooling (six primary and three junior-secondary) for every Nigerian child.[5] The programme absorbed and superseded the states' heterogeneous primary regimes, created a federal funding stream (consolidated in 2004 as the UBE Intervention Fund, financed by two percent of the

[5] Universal Basic Education Commission (UBEC), "About UBEC / What is Universal Basic Education Programme?" ubec.gov.ng/?p=2018.

Consolidated Revenue Fund),[6] and was given statutory backing by the Compulsory, Free Universal Basic Education Act of 2004.[6] Implementation ran through state Universal Basic Education Boards, established under Section 12 of the Act, with federal matching grants for classroom construction, teacher recruitment, and instructional materials.[6]

The Act's legal architecture reinforced the programme's compulsory character on both sides of the parent–state relationship. Section 2 established every Nigerian child's right to free, compulsory, and universal basic education, while Section 4 placed a reciprocal duty on parents to ensure their child's enrollment and completion, backed by penal sanctions under Section 3.[6] The same Act established the Universal Basic Education Commission (UBEC) as the federal agency responsible for coordinating the programme nationally and prescribing minimum standards for basic education, with a mandate explicitly framed around eradicating illiteracy and accelerating national development.[6] Local implementation was further devolved to Local Government Education Authorities, established under Section 13, giving the programme a three-tier federal, state, and local administrative structure from its inception.

Essentially, two features of the programme matter for our identification. First, its incidence across birth cohorts is conspicuous. Basic education in Nigeria spans ages 6–14, so a child born in 1984 or earlier was already past intake age when the programme began, a child born in 1985 could be exposed for one year, and exposure accumulates linearly until children born in 1993 or later, who could complete all nine basic years under the programme. We define $ube_years_b = \min\{9, \max\{0,\, b - 1984\}\}$ for birth year b, the deterministic count of basic-school years a child of cohort b could spend under UBE. Second, the programme's impact varied enormously across states, because pre-reform schooling did. Nigeria's educational geography is starkly polarised along a north–south gradient with deep historical roots. It is this differential exposure, with cohorts exposed to the reform gaining substantially more schooling precisely where the pre-reform deficit was largest, that identifies the estimates presented in the sections that follow.

3.2 Graphical depiction of the first stage

Figure 1 shows the identifying variation in raw form. It plots mean completed years of schooling by year of birth in the NLSS 2022/23, separately for states above and below the median pre-reform deficit. Essentially, we observe that the level gap between high- and low-deficit states is large and old, that is, four to five years of schooling for cohorts born in the 1960s. The graph also suggests that the gap was closing slowly across pre-reform cohorts. And for UBE-exposed cohorts (born 1985

[6] Federal Republic of Nigeria. *Compulsory, Free Universal Basic Education Act, 2004.* Enacted 26 May 2004. Full text: media.unesco.org/webform/r2e002/....pdf.

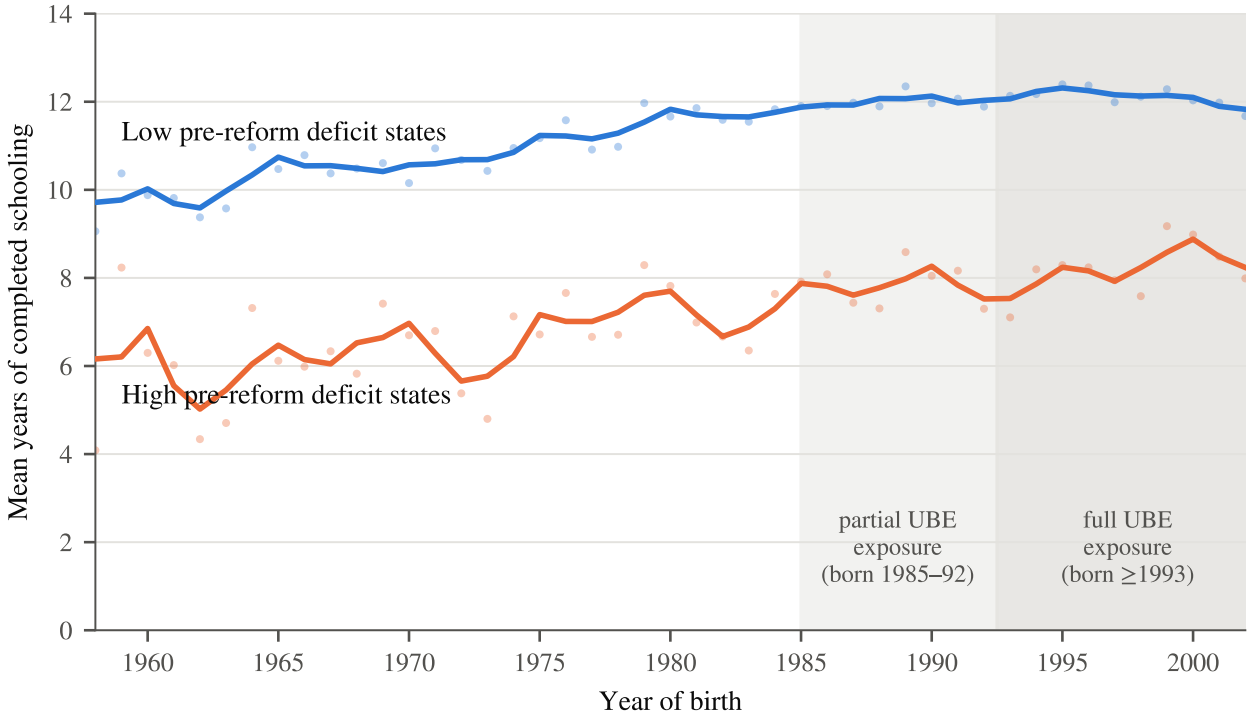


Figure 1. Schooling by birth cohort and pre-reform educational deficit

Notes. Mean years of completed schooling by year of birth, NLSS 2022/23 ($N = 94{,}848$ individuals born 1958–2002 with non-missing schooling; FCT excluded). States are split at the median of the pre-reform deficit (share of women born 1950–1975 with no secondary schooling, 1990 DHS), measured in the respondent's state of childhood. Dots are cohort means; lines are three-year centred moving averages. Shaded regions mark cohorts partially (born 1985–92) and fully (born ≥1993) exposed to the 1999 UBE reform.

onward, shaded), schooling in high-deficit states accelerates while the low-deficit series is flat at its ceiling - the low-deficit states had essentially completed the basic-schooling transition before the reform, leaving no room for further UBE-induced gains. The differential convergence in the shaded region, net of state and cohort fixed effects, is the first stage.

4 Data

4.1 Nigeria Living Standards Survey 2022/23

The main estimation sample is the individual file of the 2022/23 Nigeria Living Standards Survey (NLSS) (22,607 households; 108,461 individuals), fielded between August 2022 and March 2024. Three features make the NLSS uniquely suited to this design. First, a dedicated migration module records whether each individual has lived in their current state since birth, their years in state, and their prior state of residence, which together permit clean assignment of the state of childhood, the geography at which the instrument operates. Twelve percent of the working-age sample are cross-state migrants whose childhood state differs from their current state. Second, the survey records parental

education, enabling controls for family background. Third, it records main-job earnings, hours, occupation, and detailed formality indicators (written contract, pension contributions, employer type), supporting both the wage and the formal-sector outcome. The working-age (20–64) estimation sample, excluding the Federal Capital Territory, contains 36,672 individuals for the formal-sector outcome, of whom 22,761 report wage earnings; the four-instrument specifications use the 33,293 and 20,890 observations with all four baselines non-missing. We operationalize formal-sector employment as holding a job with a written contract, pension contribution, or a government or formal private employer.

4.2 Nigeria Labour Force Survey panel

Our general-equilibrium evidence uses the redesigned Nigeria Labour Force Survey (NLFS), which spans ten quarterly cross-sections from 2022Q4 through 2025Q2, stacked into a pooled sample of 445,427 person-quarter observations. Restricting to ages 20–64 with non-missing schooling and cohort yields our analytical sample of 172,219. The NLFS records International Standard Classification of Occupations (ISCO)-coded occupations, wage-employment and formality indicators, and employment status, but not wage amounts; wage returns are therefore estimated in the Nigeria Living Standards Survey (NLSS) only.

4.3 Cohort description and pre-reform baselines

We define three cohorts by UBE exposure: pre-reform (born before 1985; 38 percent of the sample), partially exposed (1985–92; 21 percent), and fully exposed (born 1993 or later; 41 percent). Our ube_years_b formula specified in section 3.1 assigns exposure by birth year alone, which implicitly assumes every child enters primary school at exactly age 6 and progresses one grade per year without repetition. Delayed entry, early entry, and grade repetition are all common in Nigeria, and any of them would cause a child's true exposure to the reform to diverge from what their birth year predicts. This measurement error is unlikely to bias our estimates if it is uncorrelated with the pre-reform deficit baselines. Noise in the exposure variable of this kind weakens the first stage, which we account for by reporting partial F statistics throughout, but does not bias the 2SLS coefficient. It would instead threaten identification if children in high-deficit states, disproportionately northern, poorer, and more rural, tend to enter school later than children in low-deficit states, for instance because of greater distances to the nearest school, since this would systematically miscalibrate the exposure variable in exactly the states that generate our identifying variation. We follow Duflo (2001), who confirms in the Indonesian setting that fewer than 3 percent of her boundary cohort were still in primary school when her program began, in treating this as a second-order concern given the size of

our estimated effects, though we cannot rule it out directly without individual-level data on age at school entry.

Our four state-level baselines are constructed as follow: the Demographic and Health Survey (DHS-1990) baseline is the share of women born 1950–1975 in each state with no secondary schooling, computed from the 1990 DHS individual recode and mapped from the then-21-state structure to the modern 36 states by a population-weighted crosswalk. The National Bureau of Statistics (NBS-1995) baseline is one minus the min–max-normalised ratio of 1995 primary-plus-junior-secondary enrolment to working-age population, from Tables 216 and 221 of the *Nigeria Statistical Year Book 1996* (NBS, 1997). The Federal Ministry of Education (FME-1995) baseline is the 1995 share of unqualified primary-school teachers by state, from the UNESCO Education-for-All country assessment (UNESCO, 2000). The Nigerian Certificate in Education (NCE-distance) baseline is the min–max-normalised haversine distance from each state centroid to the nearest of the 22 Federal Colleges of Education (and equivalent nce-awarding colleges) operating before 1999, whose locations were fixed by federal decisions made between 1962 and 1995.

The baselines are correlated (the DHS and NBS measures at $r = 0.68$, the DHS and FME measures at 0.83), as they should be if each measures the same underlying pre-reform educational deficit; but they derive from four disjoint data systems (household survey, administrative enrolment count, teacher register, geography), so their measurement errors and any source-specific confounds are independent. The 1990 and 2003 DHS baselines correlate at $r = 0.81$, confirming that the pre-reform geography is stable and well measured. The FCT is excluded throughout because its 1990 baseline rests on 24 observations.

4.4 Summary statistics

Table 1 reports summary statistics. Two important facts frame what follows. Working-age Nigerians average 8.8–9.3 years of schooling, so the median respondent has completed basic education but not senior secondary. And formal-sector employment is relatively small, with 7.4 percent of the working-age NLSS sample and 8.0 percent of the NLFS sample holding a formal job, against an employment-to-population ratio of 86 percent. Nearly all Nigerian work is informal; the formal-sector slot is the scarce prize over which the returns below are contested.

Table 1. Summary statistics

	NLSS 2022/23		NLFS 2022Q4–2025Q2
	Working age	Wage sample	Analytical sample
Years of schooling	9.33 (4.99)	9.91 (5.06)	8.84 (5.49)
Female	0.52	0.45	0.51
Urban	0.54	0.60	0.55
Age	33.2 (13.3)	38.8 (11.6)	36.1 (11.7)
Formal-sector employment	0.074	0.151	0.080
Wage employment	—	—	0.101
Employed	—	—	0.858
Over-educated	0.076	—	0.118
UBE exposure years	5.04 (4.17)	3.35 (3.93)	—
Father's years of schooling	3.56 (5.15)	3.88 (5.19)	—
Mother's years of schooling	2.35 (4.24)	2.65 (4.37)	—
Cohort shares: pre / partial / full	—	—	0.38 / 0.21 / 0.41
Observations	55,241	26,935	172,219

Notes. Means with standard deviations in parentheses for non-binary variables. NLSS columns: working-age (20–64) individuals excluding the FCT; the wage sample restricts to those reporting main-job earnings and hours. NLFS column: pooled analytical sample, ages 20–64 with non-missing schooling and cohort. Formal-sector employment: job with written contract, pension, or formal/government employer. Over-educated: completed schooling exceeds the modal requirement of the worker's ISCO occupation. Median hourly wage in the NLSS wage sample is ₦229 (August 2022–March 2024 naira).

5 Empirical Strategy

5.1 Specification

The structural equation is

$$y_i \;=\; \alpha + \beta\, s_i + X_i'\gamma + \mu_{s^c(i)} + \lambda_{b(i)} + \varepsilon_i, \tag{1}$$

where s_i is years of completed schooling, $\mu_{s^c(i)}$ is a fixed effect for the state of childhood, $\lambda_{b(i)}$ is a birth-year fixed effect, and X_i contains female, urban residence, and parental years of schooling (mean-filled with missingness indicators). The outcomes are log hourly wages and formal-sector employment. Schooling is instrumented with:

$$z_i^k \;=\; ube_years_{b(i)} \times baseline^k_{s^c(i)}, \qquad k \in \{\text{DHS}, \text{NBS}, \text{FME}, \text{NCE}\}, \tag{2}$$

the interaction of deterministic cohort exposure with each of the four pre-reform state deficits. With state-of-childhood and birth-year fixed effects absorbing both main effects, the identifying variation is the differential schooling gain of exposed cohorts in high-deficit states. Standard errors are clustered at the state of childhood (36 clusters) throughout.

5.2 Identification

The exclusion restriction requires that, conditional on the fixed effects and controls, cohort-by-state variation in UBE exposure intensity affects adult labour-market outcomes only through schooling, and we show that three observations supports this condition.

First, our interaction structure nets out the two first-order confounds. Any time-invariant difference between high- and low-deficit states, including the north–south differences in labour markets, religion, and income that generate the deficits themselves, is absorbed by the state fixed effects. Also, any nationwide cohort trend, including the secular schooling expansion visible in Figure 1 and any macroeconomic cohort effects, is absorbed by the birth-year fixed effects. A violation requires a shock that hit high-deficit states differentially, timed to the UBE exposure gradient across cohorts, and operating on adult outcomes outside the schooling channel. Second, having four baselines limits which confounding stories remain plausible. A confound operating through, say, unobserved determinants of women's 1990 schooling outcomes (the DHS baseline) has no mechanical reason to covary with distance to a teacher-training college whose location federal authorities determined in the 1960s–80s (the NCE baseline) or with the 1995 administrative enrolment count (the NBS baseline). If different instruments carried different confounds, they would identify different parameters and the over-identification tests would reject. Third, assignment is by state of *childhood*, recovered from the migration module, not state of residence, so the estimates are not contaminated by selective migration of the educated toward strong labour markets. Sorting into childhood state is determined by parents before the reform's announcement.

The parameter identified is a local average treatment effect (Imbens and Angrist, 1994) for compliers, that is, individuals whose schooling was changed by UBE. Formally, let $s_i(z)$ denote the schooling individual i would attain under instrument value z. The local average treatment effect identified by our instrument is

$$\text{LATE} \;=\; \frac{E[y_i \mid z_i = 1] - E[y_i \mid z_i = 0]}{E[s_i \mid z_i = 1] - E[s_i \mid z_i = 0]} \;=\; E\big[\beta_i \mid s_i(1) > s_i(0)\big], \tag{3}$$

the average of individual-level returns β_i taken only over compliers.

Table 2. First stage: completed schooling and UBE exposure intensity

	Dependent variable: years of schooling	
	Coefficient	s.e.
ube_years × deficit (DHS-1990)	0.157**	(0.069)
Female	−1.182***	(0.189)
Urban	2.414***	(0.290)
Father's years of schooling	0.216***	(0.022)
Mother's years of schooling	0.143***	(0.012)
State-of-childhood fixed effects	Yes	
Birth-year fixed effects	Yes	
Observations (wage / formal samples)	22,761 / 36,672	
Cluster F on instrument	5.2 – 14.4	
Partial F, four instruments	24.6 / 58.7	

Notes. Standard errors clustered at state of childhood (36 clusters) in parentheses. The implied effect of full exposure (nine years) in a maximal-deficit state is ≈1.4 years of schooling. FCT excluded. $^{**}p < 0.05$, $^{***}p < 0.01$.

6 The Returns to Reform-Induced Schooling

6.1 First stage

The specification underlying Table 2 is

$$s_i = \pi_0 + \pi_1 z_i^{\text{DHS}} + X_i'\pi_2 + \mu_{s^c(i)} + \lambda_{b(i)} + \nu_i, \tag{4}$$

where $z_i^{\text{DHS}} = ube_years_{b(i)} \times baseline_{s^c(i)}^{\text{DHS}}$ as defined in equation (2). Table 2 reports the single-instrument first stage. The coefficient on the DHS interaction is $\hat{\pi}_1 = +0.157$ (s.e. 0.069), that is, moving from no exposure and no deficit to full exposure (nine years) in a maximal-deficit state raises completed schooling by $9\,\hat{\pi}_1 \approx 1.4$ years. The control coefficients also appear to be consistent with *a priori* expectations as parental schooling is shown to strongly predict own schooling, urban residence adds 2.4 years, and the female deficit is 1.2 years. The single-instrument cluster F ranges from 5.2 (wage sample) to 14.4 (formal-sector sample); the four-instrument partial F statistics of 24.6 and 58.7 clear the Montiel Olea and Pflueger (2013) five-percent-bias threshold on both samples, and Appendix Table A1 shows each instrument's separate strength on the identical estimation sample.

6.2 OLS and 2SLS estimates

Table 3 presents our central results. Column 1 reports the OLS Mincerian benchmark: +3.9 percent per year of schooling on hourly wages, estimated and in line with the African returns literature (Psacharopoulos and Patrinos, 2018; Ogundari and Aromolaran, 2017). Columns 2-3 instrument schooling with the DHS instrument alone and with all four instruments. Both collapse the wage

Table 3. OLS and 2SLS estimates of the returns to schooling

	Log hourly wage			Formal-sector employment		
	(1) OLS	(2) 2SLS DHS only	(3) 2SLS Four IVs	(4) OLS	(5) 2SLS DHS only	(6) 2SLS Four IVs
Years of schooling	0.039***	−0.006	0.008	0.019***	0.035	0.042***
	(0.003)	(0.071)	(0.038)	(0.002)	(0.021)	(0.015)
First-stage partial F	—	5.3	24.6	—	14.4	58.7
Hansen J (p-value)	—	—	0.051	—	—	0.008
Observations	22,761	22,761	20,890	36,672	36,672	33,293

Notes. Each column is a separate regression of the indicated outcome on years of schooling, female, urban, parental schooling (mean-filled with missingness indicators), and state-of-childhood and birth-year fixed effects. The instruments are *ube_years* × baseline interactions using the DHS-1990 baseline (columns 2 and 5) and all four baselines (DHS-1990, NBS-1995, FME-1995, NCE distance; columns 3 and 6). The four-instrument columns use the subsample with all baselines non-missing. Standard errors clustered at state of childhood (36 clusters) in parentheses. FCT excluded. $^{***}p < 0.01$.

return to a statistical zero (−0.6 percent (s.e. 7.1) and +0.8 percent (s.e. 3.8)), respectively, the latter with a first stage well beyond weak-instrument concerns. The OLS–2SLS gap carries the usual interpretation of upward ability bias in OLS, compounded here by our general-equilibrium devaluation documented in Section 7.

Columns 4–6 repeat the exercise for formal-sector employment, and here the instrumented return is large and precise. Each reform-induced year of schooling raises the probability of holding a formal job by 4.2 percentage points ($p = 0.005$) in the four-instrument specification, a 57 percent effect relative to the 7.4 percent sample mean. That the 2SLS formal-employment return exceeds the OLS return (1.9 percentage points) is consistent with compliers, first-generation educated workers in high-deficit states, facing the steepest access gradient. For them the credential is the difference between eligibility and ineligibility for the formal queue.

The asymmetry between columns 3 and 6 is our paper's central fact. Essentially, we show that reform-induced schooling moves the marginal worker *into* the formal sector but does not raise the price their labour commands. A year of schooling that buys 4.2 percentage points of formal-sector access and zero wage growth functions as a rationing mechanism, not a productivity signal that employers price.

6.3 Robustness across instrument sets

Figure 2 shows that this asymmetry is a property of the reform variation itself, not of any particular instrument. It plots the 2SLS coefficient and 95 percent confidence interval for all fourteen usable combinations of the four instruments (each singly, all pairs, all triples, and the four-instrument speci-

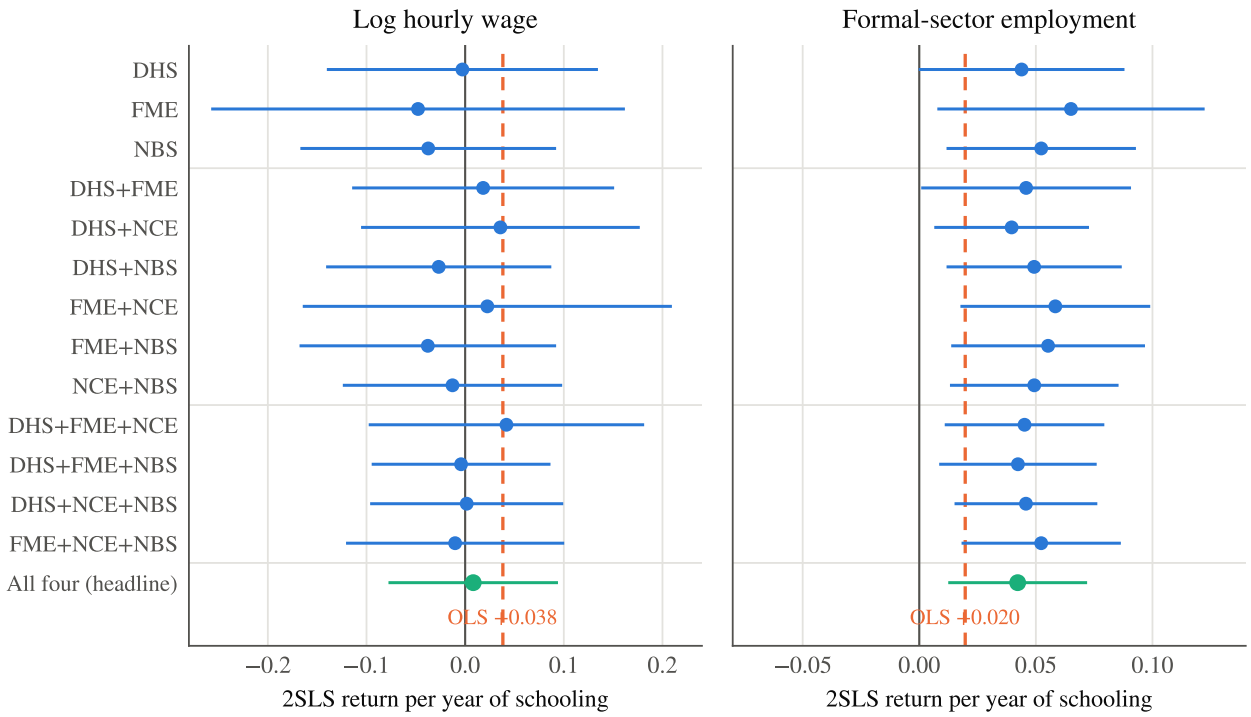


Figure 2. 2SLS estimates across all instrument sets

Notes. Each point is the 2SLS coefficient on years of schooling from a separate regression using the indicated instrument set, with 95 percent confidence intervals from standard errors clustered at state of childhood; all specifications use the common sample with all four baselines non-missing (N = 20,887 wage; 33,289 formal) and the controls and fixed effects of Table 3. The dashed line marks the OLS estimate. The NCE-distance instrument alone (first-stage $F \approx 0$) is omitted. DHS: 1990 women's no-secondary share; FME: 1995 unqualified-teacher share; NBS: 1995 enrolment deficit; NCE: distance to nearest pre-1999 Federal College of Education.

fication), all estimated on the common sample. Every wage estimate is statistically indistinguishable from zero; every formal-sector estimate is positive, between +4.0 and +6.5 percentage points, and 12 of 14 specifications are significant at the five percent level. Appendix Tables A1 and A2 report the underlying estimates, first stages, and over-identification tests.

We compute the Hansen J over-identification statistic as

$$J \;=\; N \times R^2_{\hat{u}}, \tag{5}$$

where $\hat{u}$ are the residuals from the second-stage 2SLS regression and $R^2_{\hat{u}}$ is the R^2 from regressing $\hat{u}$ on all exogenous regressors and instruments; under the null that all instruments are valid, $J \sim \chi^2(q)$ with q degrees of freedom equal to the number of over-identifying restrictions. Importantly, the over-identification results merit closer attention, because they distinguish a structural estimate from the idiosyncratic local average treatment effect of any one baseline. On the formal-sector outcome, the pair combining the two most independent data systems, 1990 household-survey schooling outcomes (DHS) and 1995 administrative enrolment (NBS), delivers $\hat{\beta} = +0.049$ ($p = 0.010$) with partial

$F = 30.1$ and a Hansen J p-value of 0.495. These two instruments with no common source, produce statistically indistinguishable returns. All three NBS-inclusive pairs have J p-values above 0.29. The four-instrument J rejects at $p = 0.008$ on the formal-sector margin, driven by pairs involving the weak NCE instrument and the FME+DHS pair (Appendix Table A2); since the strong, mutually independent pairs agree, we read the four-instrument estimate of +4.2 percentage points as a structural return rather than the idiosyncratic LATE of any single instrument. On the wage margin the J is borderline ($p = 0.051$), but the question of *which* zero the instruments identify is not economically consequential: every point estimate lies within one standard error of zero.

7 The Return in General Equilibrium

The 2SLS estimates answer a partial-equilibrium question regarding what the reform-induced schooling do for the complier, given the labour market she/he faces? But the reform also changed that labour market, roughly doubling the flow of credentialed entrants.[7] If demand for credentialed labour is inelastic, the same reform that educates the marginal worker should depress the return for everyone. This shows up in levels, as unemployment concentrates among the educated, and in changes, as the conditional return declines across successive cohorts.

7.1 Graduates are more likely to be unemployed

Figure 3 plots the ILO unemployment rate by education group in the pooled NLFS. The gradient is the reverse of what a human-capital-scarce economy should display as unemployment *rises* with education beyond primary schooling, from 1.7 percent for primary-educated workers to 5.0 percent for post-secondary graduates and 9.7 percent for university graduates, nearly six times the primary rate. The gradient survives conditioning as demonstrated in our probit estimates in Table A3 of unemployment on education-group indicators with age, sex, urban, zone, and quarter controls. We estimate the conditional marginal effects specified as:

$$\Pr(\mathit{Unemployed}_i = 1) \;=\; \Phi\big(\alpha + \sum_e \beta_e \, \mathit{Educ}_i^e + X_i'\gamma + \zeta_{z(i)} + \tau_{t(i)}\big), \tag{6}$$

where Educ_i^e are education-group indicators (reference: no formal education), Φ is the standard normal CDF, and ζ_z and τ_t are geopolitical-zone and survey-quarter fixed effects. We show that university graduates are 2.7 percentage points more likely to be unemployed than workers with no formal education ($p < 0.001$), and 64 percent of unemployed university graduates have been out of

[7] World Bank, World Development Indicators, series SE.SEC.ENRR (school enrollment, secondary, % gross), Nigeria: data.worldbank.org/indicator/SE.SEC.ENRR.

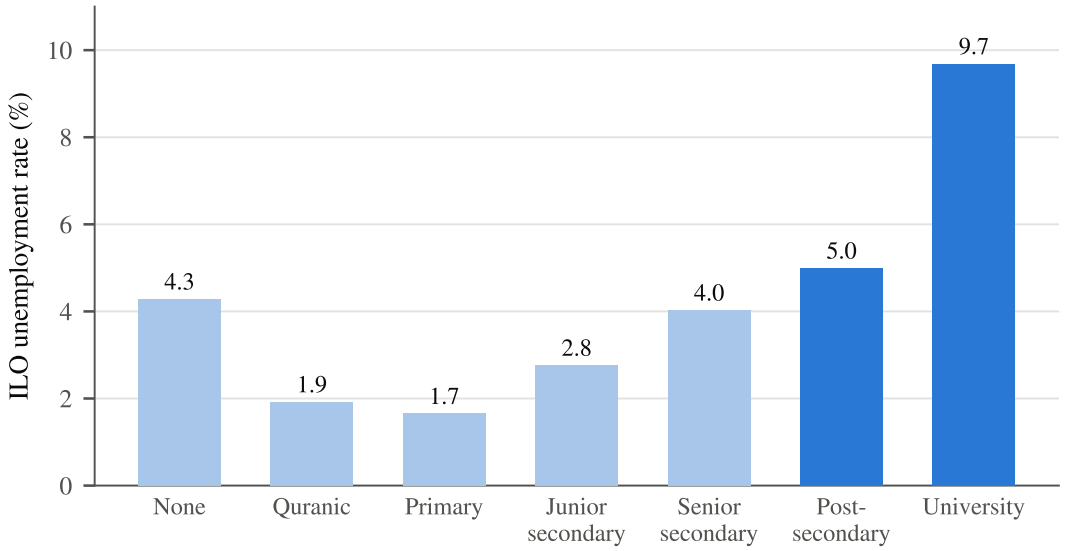


Figure 3. Unemployment rises with education

Notes. ILO unemployment rate (unemployed as a share of the labour force) by highest education level, pooled NLFS 2022Q4–2025Q2, ages 20–64 ($N = 153{,}462$ labour-force participants). The "None" group is small in this age band ($N = 817$ participants) and its rate is imprecisely estimated.

work for a year or more, the highest long-term-unemployment share of any group.

Importantly, two facts pin down the interpretation of this unemployment gradient, and together they rule out a straightforward skills-deficiency argument. First, graduate joblessness does not reflect withdrawal from the labor force. Discouragement falls with education, so educated workers remain active in the queue rather than exit the search. Second, graduates who do find work secure distinctly *better* jobs. Conditional on employment, a university graduate is 36 percentage points less likely than an uneducated worker to hold a low-skill occupation, the largest occupational effect in our data. A deficiency in graduate skills would confer little advantage in occupational placement among the employed, yet the advantage observed here is substantial. We argue that high unemployment alongside sharply better placement is the signature of queueing for rationed jobs (Harris and Todaro, 1970). What the queue is worth per year of schooling, and how fast that value is eroding, is what our cohort decomposition below measures.

7.2 Specification and results

On the pooled NLFS we estimate

$$y_{it} \;=\; \alpha + \beta_{\text{pre}}\, s_i + \beta_{\text{part}}\, s_i c_i^{\text{part}} + \beta_{\text{full}}\, s_i c_i^{\text{full}} + X_{it}'\gamma + \mu_s + \tau_t + \lambda_b + u_{it}, \tag{7}$$

Table 4. The conditional return to schooling by UBE cohort (NLFS)

	Return per year of schooling				
Outcome	Pre-UBE	Partial	Full	$\Delta_{\text{full}-\text{pre}}$	p-value
Formal-sector employment	0.0242	0.0167	0.0078	−0.0164	< 0.0001
Wage employment	0.0236	0.0168	0.0081	−0.0155	< 0.0001
Employed	0.0026	0.0009	−0.0114	−0.0140	< 0.0001
Over-educated	0.0328	0.0324	0.0226	−0.0102	< 0.0001
Log weekly hours	−0.0000	0.0031	0.0012	+0.0012	0.218
Observations	172,219 (LPM outcomes); 141,542 (log hours)				

Notes. Each row is a separate regression of the indicated outcome on years of schooling interacted with cohort indicators (equation 7), with state-of-residence, quarter, and birth-year fixed effects and female and urban controls, pooled NLFS 2022Q4–2025Q2. Cohorts: pre-UBE born before 1985; partial 1985–92; full born ≥1993. $\Delta_{\text{full}-\text{pre}}$ is the difference in the return between full-UBE and pre-UBE cohorts, with the p-value of its test. Standard errors clustered by state of residence (36 clusters).

where c^{part} and c^{full} indicate the partially (born 1985–92) and fully (born ≥1993) exposed cohorts, and the regression includes state-of-residence, quarter, and birth-year fixed effects plus female and urban controls, with standard errors clustered by state. The cohort-specific slopes β_{pre}, $\beta_{\text{pre}} + \beta_{\text{part}}$, and $\beta_{\text{pre}} + \beta_{\text{full}}$ measure the conditional return per year of schooling within each cohort, purged of aggregate cohort and period effects.

Table 4 and Figure 4 presents the results. The formal-sector return per year of schooling falls monotonically from 2.4 percentage points for pre-reform cohorts to 0.8 for fully exposed cohorts(a 68 percent decline), with Δ significant at $p < 10^{-15}$. The wage-employment return falls 66 percent in parallel. For the employment margin, the sign flips. Schooling raised the employment probability of pre-reform cohorts (weakly, +0.3 percentage points per year) but *lowers* it for the fully exposed cohort (−1.1 points per year). Young credentialed Nigerians are not only gaining less access per year of schooling than their predecessors, but at the margin, additional schooling now predicts joblessness. Harris and Todaro (1970) model rationing as operating through employment status rather than wage adjustment, and only weekly hours show no cohort gradient (those who do obtain jobs work normal hours), consistent with an extension of that logic in which rationing operates at the hiring margin rather than the hours margin.

Three alternative readings of the cohort gradient warrant explicit consideration. The first is an age-profile account, in which returns to schooling rise over the life cycle and the youngest cohort has simply not yet reached the point at which those returns materialize. Birth-year fixed effects partially absorb this possibility. Two further patterns argue against it directly. The gradient appears within narrow age bands, where a pure life-cycle story would predict little variation, and it remains stable across all ten survey quarters, which a career-stage explanation tied to age rather than calendar

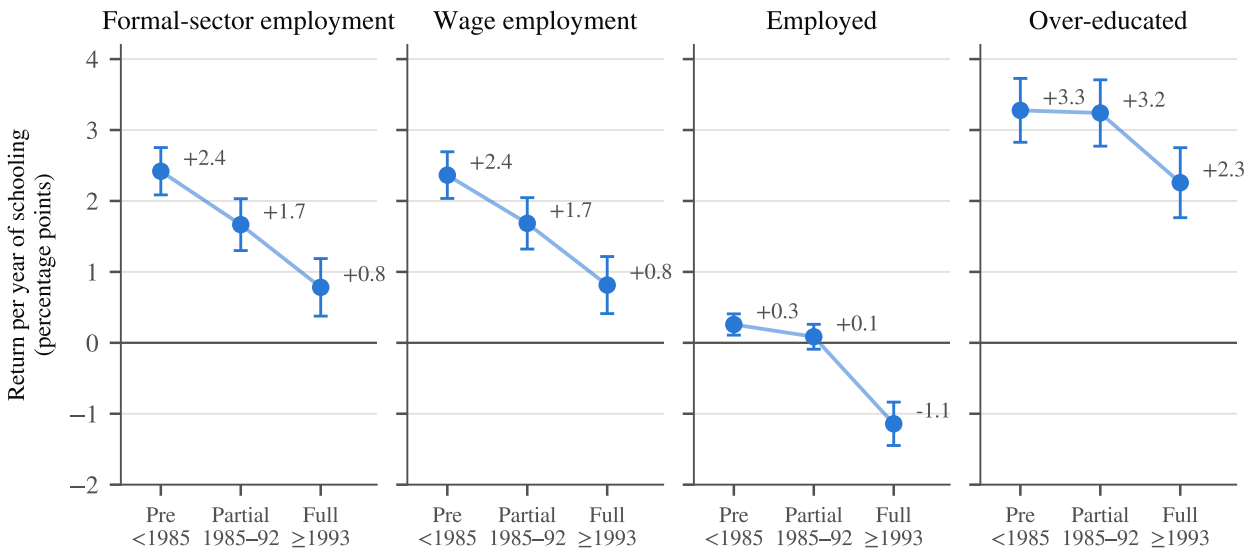


Figure 4. The conditional return to schooling, by UBE exposure cohort

Notes. Each panel plots the estimated return per year of schooling (in percentage points) within each UBE-exposure cohort, from equation (7) on the pooled NLFS 2022Q4–2025Q2 ($N = 172{,}219$). Whiskers are 95 percent confidence intervals from standard errors clustered by state of residence.

time would not produce. The second is a composition account, in which the marginal graduate induced into additional schooling by later cohorts is of lower ability than earlier compliers. This is not a genuine alternative to our mechanism so much as one channel through which it operates, since a decline in the average quality of the marginal credentialed worker is among the outcomes that credential inflation predicts, not a separate confound to be ruled out. The third is a demand-side account, in which the absorptive capacity of the formal sector has failed to expand alongside the supply of schooling. This is the interpretation we pursue directly, with supporting evidence presented in Section 8.

7.3 Who bears the decline?

In Table 5 we present heterogeneity of the cohort decline by gender and region. We document two patterns that stand out, and we consider both informative about mechanism. Formally, Table 5 is estimated by adding a group indicator (male/female or South/North) to equation (7) and interacting it with both cohort terms:

$$y_{it} \;=\; \alpha + \sum_{k\in\{\text{part,full}\}} \left(\beta_k + \delta_k\, G_i\right) s_i c_i^k \;+\; \gamma_G\, G_i \;+\; X_{it}'\gamma + \mu_s + \tau_t + \lambda_b + u_{it}, \tag{8}$$

where G_i is the group indicator. The reported p (diff.) column tests $\delta_{\text{full}} = 0$, whether the full-cohort decline differs significantly across groups. First, the decline is steeper for men. The full-cohort

Table 5. Heterogeneity in the cohort decline

	By gender			By region		
$\Delta_{\text{full}-\text{pre}}$ in:	Men	Women	p (diff.)	South	North	p (diff.)
Formal-sector employment	−0.0182	−0.0136	< 0.001	−0.0202	−0.0166	0.011
Wage employment	−0.0179	−0.0122	< 0.001	−0.0210	−0.0160	0.002
Employed	−0.0180	−0.0135	0.013	−0.0333	−0.0081	< 0.001
Observations	84,470	87,749		71,855	100,364	

Notes. Stratified estimates of $\Delta_{\text{full}-\text{pre}}$ from equation (7) within each subsample; p (diff.) is the p-value of the triple interaction $s \times c^{\text{full}} \times$ group in the pooled regression. South: South East, South South, South West zones; North: North Central, North East, North West. Standard errors clustered by state of residence.

employment penalty of schooling is −1.7 percentage points per year for men against −0.9 for women, and the formal-sector decline is a third larger (triple-interaction $p < 0.001$). That the decline is steeper for the historically advantaged group rather than the historically disadvantaged one is itself informative, since what determines the speed of erosion is the pre-existing size of the credentialed queue a complier joins, not their level of disadvantage before the reform. The female complier is more often a first-generation educated worker (pre-reform female schooling in the North was the lowest in the country), entering a segment of the labor market where credentialed competitors were still scarce, so the credential still moves access. The marginal male credential-holder, by contrast, joins a queue that was already long, since credentialed hiring among men was already the norm well before the reform, and each additional entrant therefore erodes the credential's marginal value faster.

Second, the decline is steeper in the South ($p \leq 0.011$ on every margin; on employment, −3.4 percentage points per year in the South against −0.7 in the North). One plausible explanation is that the southern labour market is more credential-responsive, given its historically larger private formal sector, which may have absorbed more of the post-reform supply surge and seen a correspondingly larger devaluation of credentials; northern formal employment, concentrated in government and parastatals, is less tightly organized around credentials and so had less price to erode. We do not observe region-specific data on private-sector size or historical credential pricing directly, and offer this as an interpretation of the pattern in Table 5 rather than as an independently verified mechanism.

8 Descriptive Mechanism: Supply Outran Absorption

Sections 6 and 7 establish two important microeconomic facts: reform-induced schooling buys formal-sector access without earnings, and that access return itself erodes across cohorts. Both facts are consistent with a specific mechanism, credential supply expanding faster than the stock

of jobs it competes for. This section descriptively diagnoses potential explanations that predict at the aggregate and cross-state level, this phenomenon, and checks those predictions against the data. Essentially, we argue that if credential supply has outrun absorption, three patterns should hold. First, at the national level, the growth in schooling should not be matched by growth in formal, wage-paying employment, the type of job a credential is meant absorb. Second, the sectors that would normally absorb a more educated workforce, manufacturing and other formal services, should not have expanded their share of output over the period. Third, at the state level, places that produce more credentials should show no corresponding advantage in formal-sector capacity, since the mechanism implies credentials are competing for a stock of jobs set by conditions other than local credential supply.

8.1 The national pattern

Figure 5A plots credential supply against formal-sector capacity in Nigeria from 1990 to 2024. Gross secondary enrolment rose from 24 to 47 percent over the period, roughly doubling. Over the same window, the wage-and-salaried share of employment, the population actually holding the kind of job a secondary credential is meant to absorb, was essentially flat (20.3 percent in 1991 against 19.7 percent in 2024). Hence, our first prediction holds – credential supply grew while formal wage employment did not. Our second prediction concerns the sectors that would need to expand to absorb this supply. Importantly, manufacturing's share of GDP fell by half over the same period, from 17.8 to 8.7 percent. We argue that this is the opposite of the structural transformation a credential-absorbing economy would need. We show that rather than schooling supply pulling workers into higher-productivity formal employment, both series moved independently, with the wage-employment share pinned near 20 percent regardless of how much schooling the population accumulated.

8.2 The state-level pattern

The national time series above cannot rule out a confound. Nigeria's formal sector could have stagnated for reasons unrelated to credential supply, oil price cycles, currency instability, or macroeconomic shocks that happen to coincide with the schooling expansion, rather than because credential supply itself outran demand. Panel B offers a cross-state check in the same spirit, but it is best read as suggestive rather than as an independent test that rules out these alternatives. It shares the national comparison's fixed-year national environment, but it introduces a confounding problem of its own. States differ in urbanization, historical infrastructure, and federal revenue allocation in ways that could independently drive both local credential supply and local formal-sector capacity, without

either causing the other. A raw cross-state correlation, with no fixed effects and no instrument, cannot separate these stories from the mechanism this paper proposes; it can only ask whether the pattern implied by that mechanism is present in the data at all.

Nonetheless, with those caveats in mind, across Nigeria's 36 states, credential-supply intensity, measured as UTME applications per 1,000 working-age adults, is uncorrelated with the mean formal-sector employment share over the NLFS panel ($\rho = 0.02$). Lagos States provides the most conspicuous illustration. It has by far the densest credential pipeline in the country, yet its formal-sector share, 7.5 percent, is statistically indistinguishable from the national mean. Essentially, if the supply of credential itself expanded the local absorptive capacity, states that produced disproportionately more graduates should show disproportionately larger formal sectors; they do not, in Lagos or elsewhere in the distribution. This is our third prediction confirmed, and it is the one hardest to explain away with a purely macroeconomic story, since it varies within the same national economy and the same time period as the credential expansion itself.

8.3 Connecting the aggregate pattern to the individual-level estimates

This configuration is what a fixed-slot queueing account, of the kind formalized by Harris and Todaro (1970) and applied to the unemployment gradient in Section 7, would predict. With the stock of formal jobs set independently of credential supply, an individual credential can still move its holder toward the front of the queue, which is consistent with the large, precisely estimated access return in Table 3, but it cannot expand the queue's capacity, which is consistent with that same table's null wage return: the marginal hire's credential no longer signals scarcity, so employers have no reason to pay a premium for it. It also explains why the access return itself erodes across cohorts in Table 4: each successive cohort of UBE graduates lengthens the queue against a stock of formal slots that Figure 5A shows was not expanding, mechanically diluting the access gain any single credential can still deliver. The same logic is visible directly in the unemployment data reported in Section 7: graduates are more likely to be unemployed, yet those who do find work are markedly more likely to hold high-skill occupations, exactly the signature of queueing for a rationed set of good jobs rather than a shortage of graduate skill.

We do not observe the queue directly, and an alternative account, in which some other regional or sectoral shock happens to be correlated with both credential supply and formal-sector stagnation, cannot be fully excluded by aggregate and cross-sectional evidence of this kind. What the state-level null correlation does rule out is the most direct alternative, that credential supply itself was expanding local labor demand. Taken together with the individual-level estimates, the evidence is consistent with an economy in which education reform succeeded as schooling policy and was overwhelmed as

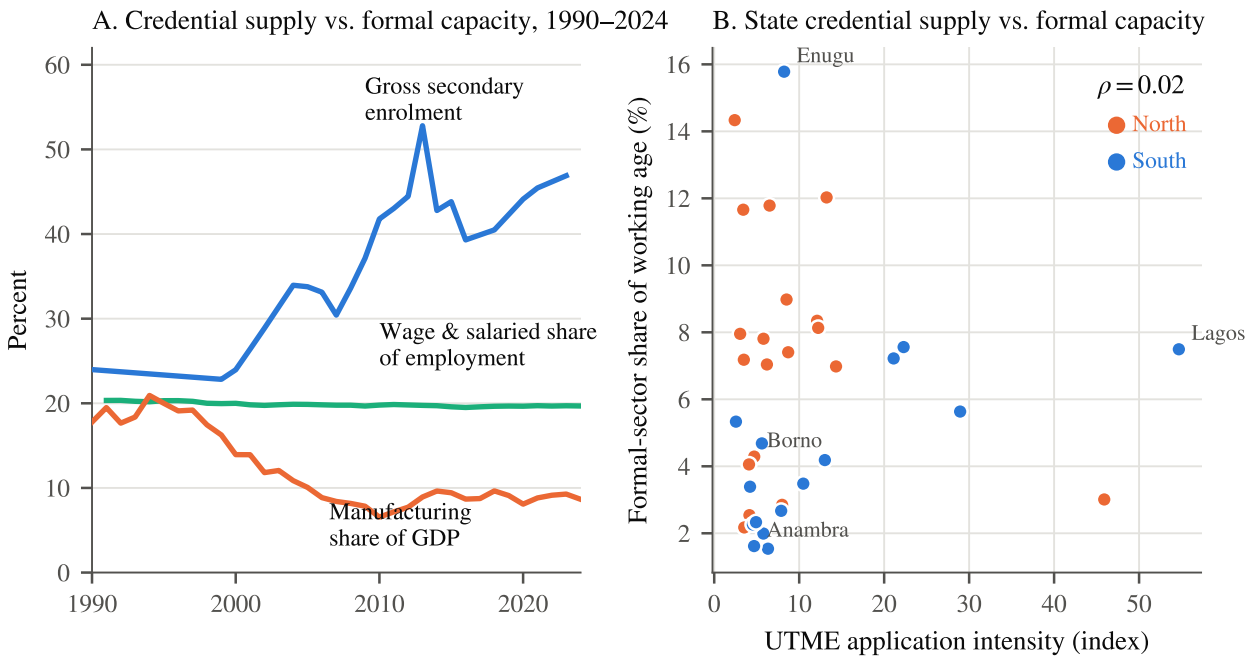


Figure 5. Credential supply and formal-sector capacity

Notes. Panel A: gross secondary enrolment (percent of relevant age group), wage-and-salaried employment (percent of total employment), and manufacturing value added (percent of GDP), Nigeria 1990–2024; World Development Indicators. Panel B: each dot is a state; the horizontal axis is 2026-cycle UTME applications per 1,000 sampled working-age adults (an intensity index), the vertical axis the mean formal-sector share of the working-age population over NLFS 2022Q4–2025Q2. The correlation is $\rho = 0.02$ ($N = 36$ states; FCT excluded).

labour-market policy.

9 Robustness

9.1 An administrative supply-side baseline

The sharpest concern about the headline DHS baseline is that it is outcome-based (women's realised schooling) and might proxy unobserved state-level factors that also shape adult labour markets. The NBS-1995 baseline addresses this directly as it is a contemporaneous administrative enrolment count, fielded independently of any household survey, measuring the supply margin UBE was designed to close. Table 6 shows that the credential-inflation asymmetry survives, and sharpens, under the administrative baseline. The primary-enrolment-deficit instrument attains first-stage F of 23.0 (formal sample), and its wage estimate turns *negative* and marginally significant (-0.124, $p = 0.089$), the only specification that can statistically distinguish the wage return from both zero and the OLS benchmark, and it lies below both. Formal-sector returns are positive and significant across all four NBS variants ($+0.052$ to $+0.059$), including under a Duflo (2001)–Osili and Long (2008) difference-in-differences template that replaces birth-year fixed effects with a binary exposure contrast

Table 6. Robustness: administrative NBS-1995 enrolment baseline

		Log hourly wage			Formal sector		
Instrument baseline	Template	F	$\hat{\beta}_{2\text{SLS}}$	p	F	$\hat{\beta}_{2\text{SLS}}$	p
NBS primary deficit	FE	13.2	−0.124	0.089	23.0	+0.059	0.031
NBS basic deficit	FE	9.3	−0.037	0.572	19.6	+0.052	0.012
NBS primary deficit	DiD	13.2	−0.138	0.080	22.8	+0.058	0.016
NBS basic deficit	DiD	13.6	−0.038	0.532	24.3	+0.052	0.002
DHS 1990 no-education	FE	5.1	−0.024	0.72	14.5	+0.038	0.04
DHS 1990 no-secondary	FE	5.3	−0.006	0.93	14.4	+0.035	0.10
FME 1995 unqualified teachers	FE	2.8	−0.066	0.53	12.7	+0.050	0.04

Notes. Each row is a pair of 2SLS regressions (wage; formal) with a single instrument built from the indicated baseline. FE: specification of equation (1) with birth-year fixed effects. DiD: Duflo–Osili-Long template with binary exposure (born ≥1993 vs. ≤1986; transition cohorts dropped), state fixed effects, and cohort main effect. F is the cluster-robust first-stage statistic. Samples: FE rows N = 19,205 wage / 30,668 formal (NBS rows) and 22,761 / 36,672 (reference rows); DiD rows N = 15,623 / 25,513. Standard errors clustered at state of childhood; FCT excluded.

and drops transition cohorts.

9.2 Alternative baseline definitions and stability of the cohort gradient

Four operationalisations of the DHS deficit (no schooling, primary non-completion, no secondary entry, continuous years-of-schooling deficit) deliver wage estimates between −0.029 and −0.010, all indistinguishable from zero. Replacing the 1990 baseline with the 2003 DHS or a pooled 1990–2003 measure leaves the conclusion unchanged (wage estimates +0.004 and −0.023).

On stability of the cohort gradient, the 68 percent formal-sector decline in Table 4 is invariant to dropping controls, restricting to individual quarters, conditioning on employment, and alternative cohort windows; estimates move within five percentage points of the headline in all variants. The gradient is present in every one of the ten survey quarters, so it is not an artefact of a single period's age composition.

10 Conclusion

Nigeria's Universal Basic Education reform did what education reforms are supposed to do. It put children in school. A fully exposed child in a high-deficit state completed roughly 1.4 additional years of schooling, and gross secondary enrolment nearly doubled nationally between 1990 and 2024. This paper asks what that schooling bought in the labour market, using the reform's staggered incidence across birth cohorts and states, validated against four independent measures of pre-reform educational deficit, as the source of identification.

This result complicates the canonical policy inference drawn from high cross-sectional Mincerian returns. Reform-induced schooling bought *access*. Each additional year raised the probability of formal-sector employment by 4.2 percentage points, a large, precisely estimated, and robustly identified effect that survives across fourteen instrument-set combinations and an administrative alternative baseline. However, it bought no measurable *earnings*. The 2SLS wage return is statistically indistinguishable from zero, with a confidence interval centered near zero and wide enough to encompass values well below the OLS benchmark. Descriptive evidence from the pooled labour force panel is consistent with this access return eroding in general equilibrium, falling by roughly two-thirds across birth cohorts, with the youngest, fully exposed cohort facing a negative marginal effect of schooling on employment itself. The decline is steeper for men than for women, and steeper in the South than the North, patterns consistent with an account in which the speed of erosion depends on how crowded the credentialed queue already was, not on how disadvantaged the entering complier had been. The aggregate and cross-state evidence in Section 8 is consistent with why. Between 1990 and 2024 the wage-and-salaried share of Nigerian employment did not move, manufacturing's share of GDP halved, and credential-supply intensity across states bore no relationship to local formal-sector capacity. Supply expanded into a demand side that, on the evidence available, did not expand in turn.

Three qualifications bound what this evidence can support. The general-equilibrium and absorption evidence is descriptive rather than causal. The cohort-panel returns are not instrumented and the cross-state absorption test lacks fixed effects so both are best read as consistent with the proposed mechanism rather than independent confirmation of it, and our paper's causal claims continue to rest on the individual-level 2SLS estimates. The four-instrument overidentification test also rejects on the formal-sector margin, a result we attribute to the weak NCE instrument rather than to the two strongest, most independent baselines, whose pairwise test does not reject, and the wage null should likewise be read as an inability to detect a return at conventional precision rather than tight evidence for an economically meaningful zero. However, none of these limitations bear on our paper's basic finding that reform-induced schooling raised formal-sector access without raising wages, an asymmetry confirmed across instrument sets, baseline definitions, and estimation templates. What they bound is the confidence with which the proposed mechanism, general-equilibrium credential inflation driven by absorption failure, can be distinguished from close alternatives.

These findings carry two implications for policymakers. First, expanding the supply of basic education is not, by itself, a sufficient labour-market strategy. The reform delivered schooling and formal-sector access at scale, but it delivered no wage gain, and the access return itself has been shrinking as each new cohort of graduates joins a stock of formal jobs that has not grown to meet

them. A further round of school construction or enrolment expansion, absent a parallel expansion in formal-sector demand, is likely to reproduce this same pattern rather than correct it. Second, the binding constraint this evidence points to is the capacity of the formal economy to create jobs, not the capacity of the schooling system to create graduates. Manufacturing's share of GDP fell by half over the same quarter-century that credential supply doubled, and the wage-and-salaried share of employment barely moved at all. Policies aimed at industrial capacity and the conditions for formal-firm growth address this constraint directly; policies aimed only at raising enrolment or credential attainment do not. Nigeria's quarter-century of education reform demonstrates that a country can win the schooling race and still lose the absorption race. The return to schooling, like any price, is jointly determined by the supply of credentialed labour and the demand for it, and this paper's evidence is that Nigeria expanded the supply curve for a quarter-century without a corresponding outward shift in demand.

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Appendix: Additional Instrument Diagnostics

Table A1. Single-instrument decomposition of the four-instrument 2SLS

	Log hourly wage				Formal sector			
Instrument	F	$\hat{\beta}_{2\text{SLS}}$	s.e.	p	F	$\hat{\beta}_{2\text{SLS}}$	s.e.	p
OLS (reference)	—	+0.038	0.003	0.000	—	+0.020	0.002	0.000
NBS-1995 enrolment deficit	9.3	−0.037	0.066	0.572	19.6	+0.052	0.021	0.012
DHS-1990 no-schooling share	4.4	−0.003	0.070	0.968	10.2	+0.044	0.023	0.051
FME-1995 unqualified teachers	2.7	−0.048	0.107	0.655	10.0	+0.065	0.029	0.026
NCE distance to FCE	0.0	+1.356	6.616	0.838	0.0	−0.145	0.980	0.883
All four (headline)	24.6	+0.008	0.044	0.852	58.7	+0.042	0.015	0.005
Hansen J p-value (4-IV)		0.051				0.008		
Observations		20,887				33,289		

Notes. Each single-instrument row runs 2SLS with one instrument on the same sample as the four-instrument specification (all four baselines non-missing), so first-stage strength is directly comparable. Rows ordered by formal-sector first-stage F. All specifications include the controls and fixed effects of Table 3; standard errors clustered at state of childhood (36 clusters); FCT excluded.

Table A2. All instrument combinations: first stage, 2SLS, and Hansen J

	Log hourly wage			Formal sector		
Instrument set	F	$\hat{\beta}_{2\text{SLS}}$	J (p)	F	$\hat{\beta}_{2\text{SLS}}$	J (p)
DHS+FME	4.6	+0.018	0.134	11.2	+0.046	0.001
DHS+NCE	4.6	+0.036	0.013	10.4	+0.040	0.295
DHS+NBS	11.1	−0.027	0.452	30.1	+0.049	0.495
FME+NCE	2.8	+0.022	0.012	13.0	+0.058	0.290
FME+NBS	9.3	−0.038	0.852	21.7	+0.055	0.297
NCE+NBS	11.3	−0.013	0.012	25.1	+0.049	0.290
DHS+FME+NCE	4.9	+0.042	0.036	13.5	+0.045	0.003
DHS+FME+NBS	19.2	−0.004	0.264	38.0	+0.042	0.003
DHS+NCE+NBS	14.8	+0.002	0.024	50.9	+0.046	0.415
FME+NCE+NBS	11.8	−0.010	0.037	33.0	+0.052	0.408
All four (headline)	24.6	+0.008	0.051	58.7	+0.042	0.008

Notes. All specifications estimated on the common sample (N = 20,887 wage; 33,289 formal) with the controls and fixed effects of Table 3. F is the partial first-stage statistic; J (p) the p-value of the Hansen over-identification test, computed from 2SLS residuals as $N \times R^2$ on the exogenous variables and instruments. Standard errors clustered at state of childhood; FCT excluded.

Table A3. Education and labour-market outcomes in the pooled NLFS: probit average marginal effects

	(1) ILO unemployed	(2) Discouraged	(3) Low-skill job (| employed)	(4) Unemployed (no controls)
Education (reference: no formal education)				
Quranic / Islamic	−0.004	−0.012***	0.065***	0.001
	(0.005)	(0.003)	(0.015)	(0.004)
Primary	−0.011*	−0.017***	−0.069***	−0.010**
	(0.005)	(0.003)	(0.014)	(0.004)
Junior secondary	−0.023***	−0.017***	−0.055***	−0.005
	(0.005)	(0.003)	(0.014)	(0.004)
Senior secondary	−0.002	−0.013***	−0.011	0.012***
	(0.005)	(0.002)	(0.013)	(0.004)
Post-secondary, non-univ.	0.015***	−0.009***	−0.241***	0.032***
	(0.005)	(0.003)	(0.014)	(0.004)
University / postgraduate	0.027***	−0.006*	−0.359***	0.062***
	(0.005)	(0.003)	(0.014)	(0.004)
Age	0.003***	−0.005***	0.003***	—
Age^2	−0.000***	0.000***	−0.000***	—
Female	0.022***	0.021***	0.032***	—
Urban	0.018***	0.009***	0.062***	—
Zone fixed effects	Yes	Yes	Yes	No
Quarter fixed effects	Yes	Yes	Yes	No
Observations	184,757	184,757	128,938	184,757
Pseudo R^2	0.067	0.057	0.106	0.022
Mean of dependent variable	0.031	0.017	0.597	0.031

Notes. Average marginal effects from probit models of the indicated outcome on education-group indicators, age, age squared, female, urban, and geopolitical-zone (six) and survey-quarter (ten) fixed effects, pooled NLFS 2022Q4–2025Q2, ages 15–64 with non-missing education. Heteroskedasticity-robust standard errors in parentheses. Column 1: ILO unemployment. Column 2: discouraged worker (not employed, did not search in the past four weeks, but reports wanting to work). Column 3: estimated on the subsample of employed workers with a non-missing ISCO-08 occupation code, excluding major group 6 (skilled agricultural workers); the outcome equals one for major groups 5, 7, 8, and 9 (services and sales, craft, plant and machine operators, elementary) and zero for groups 1–4 (managers, professionals, technicians, clerical). Column 4 reports the bivariate probit of unemployment on education with no controls as a benchmark. The age band (15–64) is wider than that of Figure 3 (20–64), so the unconditional rates differ slightly. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.