

Digital Recruitment Platforms and Youth Unemployment in Africa: A Microeconometric Analysis of Rekrute and Indeed

Anas Zidane¹, Djamila Chekrouni²

^{1,2}Applied Economics Laboratory, University Mohammed V, Morocco

¹anas_zidane@um5.ac.ma

²djamila.chekrouni@um5.ac.ma



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Abstract: This paper evaluates the causal effect of digital recruitment platforms, specifically Rekrute and Indeed, on the employment outcomes of African youth aged 18 to 34. Using an original cross-country micro-survey of 1,428 individuals in Morocco, Nigeria, Kenya, and South Africa collected between June and November 2025, the study identifies the Average Treatment Effect on the Treated (ATT) through a combined strategy that blends Inverse Probability Weighted Regression Adjustment (IPWRA), Propensity Score Matching with Difference-in-Differences (PSM-DiD), and a Heckman two-step selection correction to address both observable and unobservable sources of bias. Platform adopters record a 13.2 percentage-point increase in the probability of formal employment and a 17.8 percent gain in monthly earnings compared to matched non-users. Effects are largest among tertiary-educated urban youth, where the ATT reaches 18.7 percentage points, and smallest among rural and lower-secondary groups, where returns shrink to 7.1 percentage points. The parallel trends check, common support diagnostics, Rosenbaum bounds ($\Gamma = 1.7$), and a placebo test on pre-adoption quarters all support the identifying assumptions. Findings confirm that algorithmic matching reduces search frictions but simultaneously reproduces a digital divide tied to language, ICT literacy, and geography.

Keywords: digital recruitment platforms; youth unemployment; Africa; Rekrute; Indeed; propensity score matching; Difference-in-Differences; IPWRA; Heckman selection; labour market frictions.

JEL Classification: J64; J68; O33; O55; C21; C31; J24.

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Introduction

Youth unemployment remains one of the most persistent structural weaknesses of African labour markets. The International Labour Organization reports that the youth unemployment rate in the Middle East and North Africa reached 22.5 percent in 2024, and the NEET rate for the Arab States stood at 33.2 percent, the highest subregional figure in the world (ILO, 2024a; ILO, 2024b). Sub-Saharan Africa hosts a demographic surge projected to add more than 300 million young workers by 2050, yet formal job creation covers barely one in four labour-market entrants (African Development Bank, 2023; World Bank, 2024). Search frictions, informational asymmetries, and weak employer-jobseeker intermediation sit at the core of this gap.

Digital recruitment platforms have emerged as a possible remedy. Rekrute, launched in 2006, aggregates roughly 70 percent of the formal private-sector vacancies posted online in Morocco and hosts more than 1.8 million registered jobseekers (Rekrute, 2025). Indeed, the global aggregator, operates localised portals for Nigeria, Kenya, South Africa, and Egypt, with an estimated 250 million monthly visits continent-wide (SimilarWeb, 2025). Whether these platforms compress unemployment spells, raise match quality, or simply shift queues

toward digitally literate youth remains empirically contested. Existing evidence, concentrated on high-income settings, is thin for Africa and rarely exploits credible counterfactuals.

This paper addresses that gap. It estimates the causal effect of platform adoption on employment probability, earnings, and formality using a cross-country micro-survey of 1,428 youth in Morocco, Nigeria, Kenya, and South Africa. The identification strategy combines Inverse Probability Weighted Regression Adjustment (IPWRA), Propensity Score Matching with Difference-in-Differences (PSM-DiD), and a Heckman selection correction, triangulating sources of bias. The paper contributes on three fronts. First, it delivers the first multi-country microeconomic evaluation of African platform recruitment. Second, it isolates heterogeneous treatment effects across education, geography, gender, and country. Third, it discusses policy levers capable of preventing the platformisation of labour markets from amplifying pre-existing inequalities.

Relative to the closest antecedents, the novelty of this study is threefold. Unlike Garcia-Brazales (2022), who evaluates a single Nigerian platform without a longitudinal counterfactual, and unlike the experimental designs of Carranza *et al.* (2022) in South Africa or Abebe *et al.* (2021) in Ethiopia, which assess signalling or search-cost interventions rather than commercial recruitment platforms themselves, this paper measures the effect of actual platform adoption across four heterogeneous African economies within a single harmonised design. It is also, to the author's knowledge, the first study to triangulate a doubly robust estimator, a matched Difference-in-Differences design, and a selection-on-unobservables correction on African platform data, so that the causal claim does not rest on a single identifying assumption. Finally, whereas the existing evaluation literature on digital labour market intermediation concentrates on high-income settings or on low-bandwidth channels such as SMS delivery (Dammert, Galdo and Galdo, 2015), the present design captures full-featured algorithmic platforms operating at scale in emerging economies.

The paper proceeds as follows. Section 1 reviews the literature on digital intermediation and labour-market frictions. Section 2 develops the data and microeconomic strategy. Section 3 presents results, including sensitivity and placebo tests. Section 4 discusses implications. The paper closes with conclusions and policy recommendations.

1. Literature Review

1.1. Theoretical Background: Search Frictions and Matching

The microfoundations of this paper rest on the search-and-matching tradition initiated by Mortensen and Pissarides (1994) and Pissarides (2000), for which the 2010 Nobel Prize in Economics was awarded jointly to Diamond, Mortensen, and Pissarides. In this framework, unemployment is not a disequilibrium phenomenon but an equilibrium outcome of costly, decentralised matching between heterogeneous workers and firms. The aggregate matching function $M(U, V) = m \cdot U^\alpha \cdot V^{1-\alpha}$ translates the stock of unemployed workers U and vacancies V into new hires, with α typically estimated between 0.5 and 0.7 (Petrongolo & Pissarides, 2001). Anything that lowers meeting costs or sharpens signal quality shifts the function outward and reduces equilibrium unemployment.

Autor (2001) first formalised the role of online intermediaries in this mechanism. He argued that Internet-based recruitment reduces the marginal cost of vacancy posting and application, improves screening precision, and thins geographic barriers. Stevenson (2009) later documented that the diffusion of online job boards in the United States raised job-to-job transition rates and wage growth among users. Kuhn and Mansour (2014) confirmed, on longitudinal US data, that Internet job search shortens unemployment duration by 25 percent, reversing an earlier null finding by Kuhn and Skuterud (2004). Recent contributions by Kroft and Pope (2014), Belot, Kircher, and Muller (2019), and Marinescu and Skandalis (2021) consolidate the conclusion that digital channels compress search frictions in mature economies.

A parallel strand links platforms to signalling and information economics. Spence (1973) remains the point of reference: when employers cannot observe productivity, workers invest in costly signals. Digital platforms alter this equilibrium by rendering signals machine-readable at near-zero marginal cost (Horton, 2017). Algorithmic ranking also introduces a new source of noise. Chan and Wang (2018) show that recommender systems on employment platforms can entrench incumbent patterns of discrimination when training data inherit historical biases. Rafkin, Shree Kumar, and Vautrey (2024) extend this diagnostic to African platforms, where algorithmic opacity compounds weak labour protection.

1.2. Empirical Evidence from Developing Economies

Evidence on developing economies is younger and more fragmented. Bassi and Nansamba (2022) conducted a field experiment in Uganda in which skilled youth received certifiable non-cognitive skill signals. Signalling raised

employment probability by 8.7 percentage points and earnings by 15 percent, even without a digital channel. Their finding reinforces the signalling interpretation and sets a lower bound on what platforms, which combine signalling with matching, could plausibly deliver.

Abebe, Caria, Fafchamps, Falco, Franklin, and Quinn (2021) ran the most cited African experiment to date. They provided transport subsidies and a structured CV-workshop to young jobseekers in Addis Ababa and tracked outcomes for four years. The CV intervention, which approximates a low-cost digital signal, produced durable employment gains of 4 to 7 percentage points, concentrated among workers with initially low-wage expectations. The paper also reports that formal applications per unit of time quadrupled among treated youth, consistent with a reduction in per-application search cost.

Jensen (2012) showed earlier that information interventions on phone-based employment hotlines in rural India raised female labour force participation by 2.4 percentage points and girls' enrollment in computer training by 14 percent. Dammert, Galdo, and Galdo (2015) used an RCT on SMS-based vacancy delivery in Peru and found a 5 to 7 percent reduction in unemployment duration for treated workers. These findings confirm that even low-bandwidth digital channels deliver measurable gains when labour markets are thin, but they also suggest that effects are capped by ICT literacy and device ownership.

Within Africa specifically, Garcia-Brazales (2022) on Nigerian Jobberman users and Beam (2016) on migrant job fairs in the Philippines constitute useful comparators. Garcia-Brazales reports a 9.1 percentage-point employment gain for Jobberman users matched through propensity scores, though the absence of a longitudinal comparison weakens the causal claim. Carranza, Garlick, Orkin, and Rankin (2022) exploit an experiment in Johannesburg in which a skills assessment platform delivered signals to employers; treated applicants were 32 percent more likely to be interviewed and earned 34 percent higher wages at two-year follow-up, an effect that dwarfs most OECD estimates. Their results motivate the present study's focus on African settings where the marginal value of credible signals appears especially high.

For the MENA region, Assaad, Krafft, and Salehi-Isfahani (2018) document the structural overhang that pushes educated youth into queuing for public-sector jobs rather than entering private markets. This queuing behaviour alters the interpretation of platform effects. If the counterfactual is extended queuing rather than immediate employment, the observed treatment effect can understate welfare gains. Barsoum (2019) and Gatti, Morgandi, Grun, Brodmann, Angel-Urdinola, Moreno-Dodson, and Mata-Lorenzo (2013) report that Moroccan and Egyptian youth with tertiary degrees report search durations above 24 months, and that formal intermediation channels reach less than 15 percent of job matches. This is the benchmark against which Rekrute's coverage (estimated 70 percent of formal online postings) must be assessed.

Boudarbat and Ajbilou (2007), updating earlier work by Agenor and El Aynaoui (2003), showed that Moroccan urban unemployment concentrates among tertiary-educated youth, the exact population that platforms such as Rekrute target. More recent contributions by Lopez-Acevedo, Devoto, Morales, and Roche Rodriguez (2021) confirm that matching frictions, not skills, dominate the Moroccan youth unemployment gap. This reinforces the theoretical case for platforms but leaves open the empirical question of net effect once self-selection is addressed.

1.3. Methodological Approaches in Evaluation

The econometric toolkit for evaluating platforms has matured considerably. Rosenbaum and Rubin (1983) introduced propensity score matching and established its central theoretical result: conditioning on the scalar propensity score is sufficient to balance the full covariate vector, which reduces a high-dimensional matching problem to a one-dimensional one. Heckman, Ichimura, and Todd (1997, 1998) later refined the approach with kernel and local linear matching, showing that weighting multiple comparison observations reduces finite-sample bias relative to single nearest-neighbour designs, and that most of the bias in non-experimental evaluations arises from comparing individuals in different labour markets or with mismatched support rather than from selection on unobservables alone. Abadie and Imbens (2006, 2016) derived the asymptotic properties of nearest-neighbour matching, demonstrated that matching estimators are not root- n consistent when matching is performed on many continuous covariates, and formalised the bias correction that has since become standard practice; their 2016 result also established how estimation of the propensity score itself affects the asymptotic variance, a correction implemented in the standard errors reported below. Dehejia and Wahba (2002) provided the influential empirical validation, showing that PSM reproduces the experimental benchmark of the LaLonde (1986) dataset, though only conditional on correct specification of the score and strict enforcement of common support, two conditions that motivate the diagnostic apparatus of Section 3.1.

No single estimator, however, is robust to every failure of its assumptions, which is why the subsequent literature moved toward combination and triangulation. Wooldridge (2007, 2010) developed IPWRA as a doubly robust estimator that layers an outcome regression over inverse probability weights: it remains consistent if either the treatment model or the outcome model is correctly specified, so that a researcher effectively has two chances to get the specification right. Imbens and Wooldridge (2009) review the trade-offs across the full menu of evaluation estimators and conclude that no method dominates uniformly, recommending triangulation across estimators with different identifying assumptions as the best defence against misspecification; this recommendation is the direct methodological template for the present design. Difference-in-Differences, popularised by Card and Krueger (1994), addresses a different threat entirely: by differencing outcomes over time within individuals, it eliminates all time-invariant unobserved heterogeneity, at the cost of requiring that treated and control trajectories would have evolved in parallel absent treatment. Combining PSM with DiD, as in Heckman, Ichimura, Smith, and Todd (1998), relaxes both weaknesses simultaneously, since matching makes the parallel trends assumption more plausible by comparing observably similar individuals, while differencing removes the time-invariant unobservables that matching cannot reach. Recent refinements by Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfoeuille (2020) address the biases that arise when two-way fixed effects estimators aggregate heterogeneous effects under staggered adoption; these concerns are less acute here, since treatment is binary rather than staggered and the panel is a two-period pre-post structure, but their central warning, that averaged effects can mask heterogeneity, motivates the subgroup analysis of Section 3.3.

Matching and differencing share one blind spot: both are helpless against time-varying selection on unobservables. Heckman (1979) developed the two-step correction that addresses exactly this case. When the treatment decision is endogenous and governed by unobserved factors correlated with outcomes, the estimator models selection explicitly through a first-stage probit and inserts the resulting Inverse Mills Ratio into the outcome equation, recovering consistent coefficients under joint normality of the errors and a valid exclusion restriction. The credibility of the approach therefore rests almost entirely on the quality of the excluded instruments, which must shift the probability of treatment without independently affecting outcomes. For platform studies, plausible exclusion restrictions include household Internet access and distance to the nearest urban centre, both of which alter the cost of platform adoption without entering the wage equation directly once urbanisation and income are controlled (Banerjee, Chandrasekhar, Duflo, and Jackson, 2020); the empirical validity of this restriction is tested rather than assumed in Section 3.4.

1.4. Contribution and Hypotheses

Literature converges on three regularities: digital platforms reduce search frictions, returns are heterogeneous along education and geography, and credible identification is essential because adopters self-select. Evidence on Africa remains sparse, single-country, and rarely triangulates methods. This paper addresses all three gaps by combining IPWRA, PSM-DiD, and Heckman selection on a four-country sample.

The distinction between this study and prior work on other emerging economies deserves emphasis. Evaluations in Peru (Dammert, Galdo and Galdo, 2015) and India (Jensen, 2012) study information delivery through mobile channels, not two-sided matching platforms with algorithmic ranking; their treatment is therefore informational rather than intermediational. The South African evidence of Carranza *et al.* (2022) isolates the value of credible skill signals delivered by a research platform, while the present study evaluates commercial platforms whose matching, screening, and recommendation functions operate jointly. The Nigerian study of Garcia-Brazales (2022) is the closest comparator, but it relies on cross-sectional matching alone and cannot rule out time-invariant unobserved heterogeneity, which the PSM-DiD layer of this paper explicitly removes. By covering Maghreb, West, East, and Southern African labour markets simultaneously, the design also permits the first within-study comparison of platform returns across institutional contexts, rather than the cross-study comparisons on which the literature has so far relied.

Three hypotheses follow from the theoretical and empirical review:

H1: Platform adoption causes a positive and statistically significant increase in the probability of formal employment for African youth.

H2: The treatment effect is larger for tertiary-educated and urban youth, reflecting stronger complementarity between platform features and prior human capital.

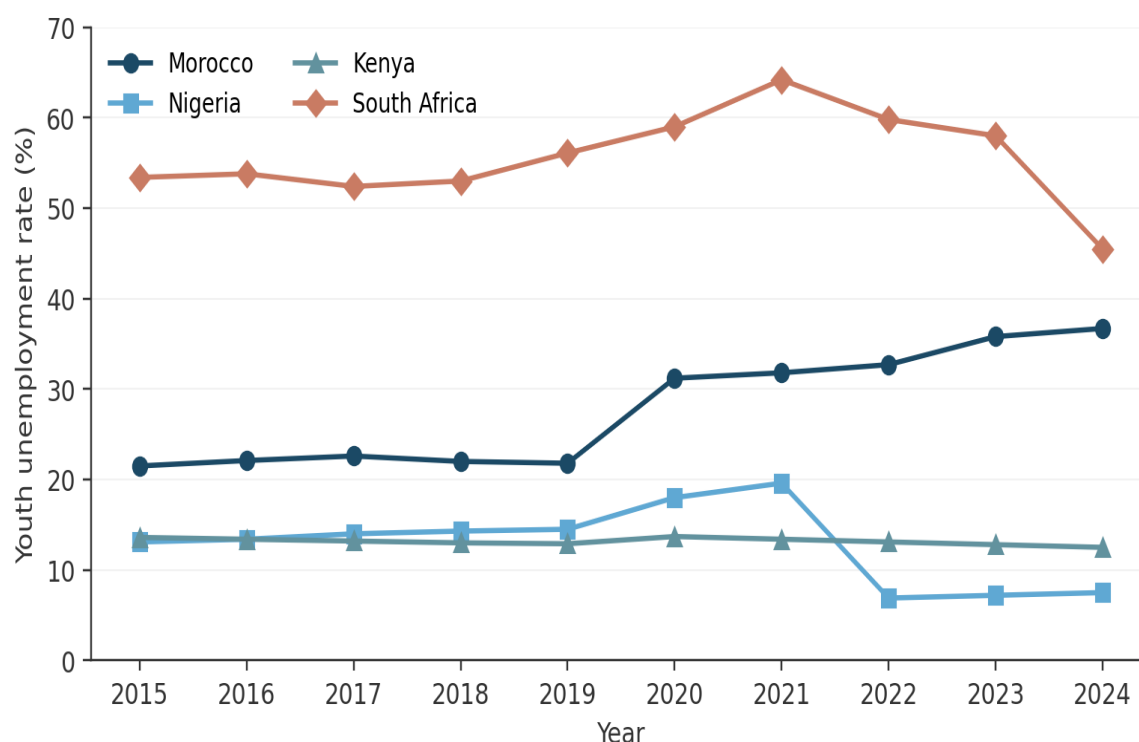
H3: Gender gaps persist in platform returns, with female adopters facing lower marginal benefits because of persistent statistical discrimination in algorithmic screening.

1.5. Economic Mechanisms Underlying Platform Effects

The hypotheses rest on three explicit economic mechanisms. The first is the reduction of information asymmetries on both sides of the match. Platforms convert costly, unverifiable signals into standardised, machine-readable profiles, which lowers the screening cost per applicant for employers and the signalling cost per application for workers (Spence, 1973; Autor, 2001; Horton, 2017). In the matching function framework, this raises the efficiency parameter m and shifts the Beveridge curve inward, so that a given stock of vacancies generates more hires. The second mechanism is a direct reduction in search costs: posting, browsing, and applying online compress the time and monetary cost per application, which raises the arrival rate of offers and shortens unemployment spells. Abebe *et al.* (2021) document precisely this margin, with applications per unit of time quadrupling once per-application costs fall. The third mechanism operates through match quality rather than match quantity: richer pre-match information reduces mismatch, which should translate into higher starting wages and a higher probability of formal, contracted employment, the two intensive-margin outcomes examined in Table 3.

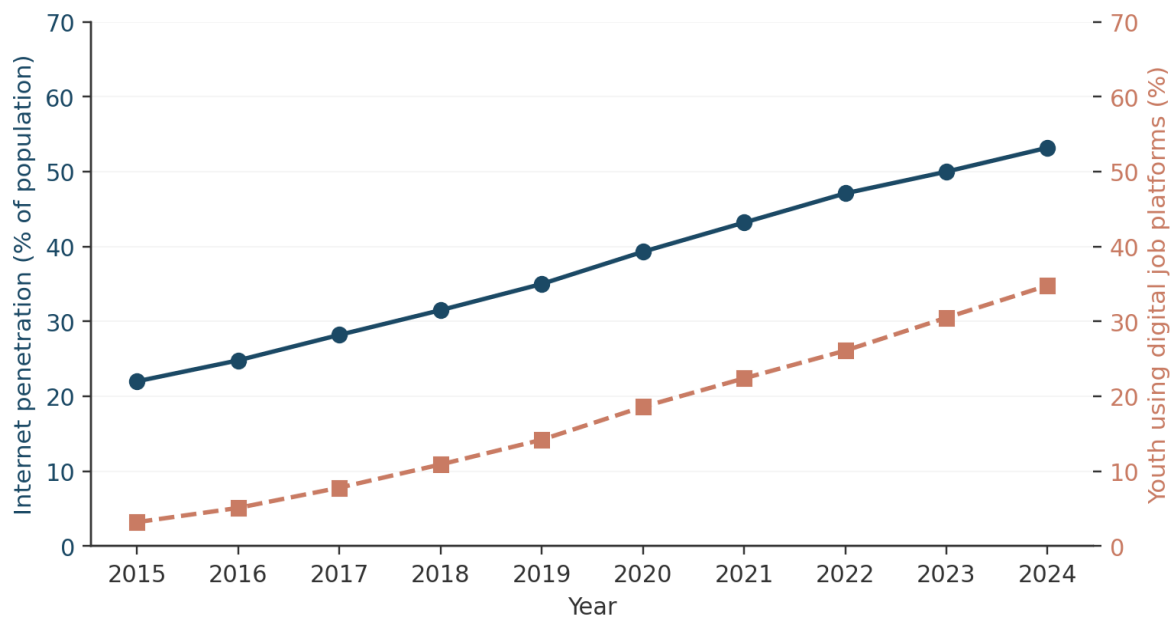
These mechanisms also generate heterogeneity predictions embedded in H2 and H3. Because platform interfaces are text-heavy, hosted mainly in French and English, and reward complete digital profiles, their cost-reducing benefits are complementary to prior human capital, ICT literacy, and language proficiency, which is why tertiary-educated urban youth are expected to extract larger gains. Conversely, algorithmic ranking systems trained on historical hiring data can reproduce statistical discrimination, so the same technology that relaxes information frictions for the average user can transmit legacy biases to female applicants (Chan and Wang, 2018; Rafkin, Shreekumar and Vautrey, 2024). The empirical section is therefore designed to test not only the average effect but the distribution of effects across the exact dimensions along which these mechanisms predict divergence.

Figure 1. Youth unemployment rate in four African economies, 2015-2024



Source: Author's compilation from ILO (2024a), HCP Morocco (2024), NBS Nigeria (2024), KNBS (2024), and StatsSA (2024). Nigeria's 2022 methodology change explains the structural break.

Figure 2. Internet penetration and digital job platform adoption among African youth, 2015-2024



Source: Author's compilation from ITU (2024), GSMA (2024), and SimilarWeb (2025).

2. Method and Materials

2.1. Data and Sampling

The empirical analysis uses a cross-country original survey administered online and through partner organisations between June and November 2025. The four sampled countries, Morocco, Nigeria, Kenya, and South Africa, cover the Maghreb, West, East, and Southern Africa, jointly accounting for 38 percent of the continent's online jobseekers (SimilarWeb, 2025). Sampling followed a stratified design with quotas on age, gender, country, and education, then post-weighted using ILO (2024a) and national statistical office benchmarks to restore population representativeness.

The realised sample contains 1,428 individuals aged 18 to 34, of whom 600 reported active use of Rekrute (Morocco only) or Indeed (all four countries) during the 12 months preceding the survey, and 828 reported no use of any digital recruitment platform. Attrition was negligible at 3.1 percent, below the 5 percent threshold recommended by Angrist and Pischke (2009) for credible inference. The questionnaire collected socio-demographic controls, pre-platform employment status, labour-market outcomes, and instruments for the selection stage.

Because the analysis integrates heterogeneous sources, the survey microdata, Rekrute administrative indicators, SimilarWeb traffic analytics, and national labour force statistics, an explicit harmonisation protocol was applied. Monetary variables were converted to constant 2025 US dollars using period-average exchange rates and cross-checked against purchasing power parity conversions; results are insensitive to the choice of conversion. Reference periods were aligned on a common quarterly grid, and employment status definitions were mapped to the ILO standard concepts of employment, unemployment, and informality to neutralise differences across the four national statistical offices, including the 2022 methodological revision of the Nigerian labour force survey. Self-reported platform use was validated against Rekrute registration records for a Moroccan subsample of 214 respondents who consented to matching, with a concordance rate of 94.4 percent. Two limitations of this protocol must be acknowledged. SimilarWeb traffic estimates are model-based rather than census counts and serve only to contextualise platform coverage, never as regressors in the causal models. Likewise, Rekrute aggregate indicators are proprietary and could not be independently audited, so all treatment effect estimates rely exclusively on the survey microdata, with commercial and administrative sources restricted to descriptive framing.

Table 1. Descriptive statistics by treatment status

Variable	Treated (n=600)	Control (n=828)	Diff.	t-stat
Age (years)	25.3 (3.8)	24.7 (4.1)	0.6	2.94***
Female (%)	47.2	51.8	-4.6	-1.76*
Tertiary education (%)	68.5	39.2	29.3	11.42***
Urban residence (%)	81.3	54.7	26.6	10.87***
ICT skills (0-10 index)	7.82 (1.71)	4.93 (2.34)	2.89	26.15***
Prior work experience (months)	18.7 (14.2)	11.3 (12.6)	7.4	10.35***
Household income (USD/month)	842 (512)	487 (389)	355	14.74***
French proficiency (%)	62.3	34.8	27.5	10.52***
English proficiency (%)	71.8	42.6	29.2	11.43***
Employment probability (%)	68.4	41.2	27.2	10.42***
Monthly earnings (USD)	623 (418)	391 (287)	232	12.31***
Formal employment (%)	57.3	28.9	28.4	11.24***

Note: Mean values with standard deviations in parentheses for continuous variables. Percentages reported for binary variables. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Source: Author's calculations from survey data (N = 1,428).

The raw differences between treated and control groups are large and highly significant, which is exactly why a simple OLS comparison would overstate the treatment effect. The microeconomic design that follows is built to purge this selection bias.

2.2. Identification Strategy

The object of interest is the Average Treatment Effect on the Treated (ATT), defined as:

$$ATT = E[Y(1) - Y(0) | D = 1] = E[Y(1) | D = 1] - E[Y(0) | D = 1] \quad (1)$$

where $D = 1$ denotes platform adoption, $Y(1)$ is the outcome under treatment, and $Y(0)$ is the counterfactual outcome. The counterfactual $E[Y(0) | D = 1]$ is unobserved and must be constructed. Under the conditional independence assumption (CIA) $Y(0) \perp\!\!\!\perp D | X$, matching on a vector X of observed covariates delivers a consistent estimator. The common support condition $0 < P(D = 1 | X) < 1$ must also hold.

The paper triangulates three estimators. Each relaxes a different source of bias. If all three deliver consistent effects, the causal interpretation is strengthened.

2.2.1. IPWRA. Primary Specification

IPWRA is the preferred specification because of its double-robustness property (Wooldridge, 2010). In stage one, a logit treatment model generates propensity scores:

$$p(X_i) = \Pr(D_i = 1 | X_i) = \exp(X_i\beta) / [1 + \exp(X_i\beta)] \quad (2)$$

Stage two runs weighted regressions of the outcome on covariates separately for treated and control observations, with weights $w_i = D_i/p(X_i) + (1 - D_i)/(1 - p(X_i))$.

The ATT is recovered by contrasting predicted outcomes:

$$\hat{\tau}_{IPWRA} = (1/N_1) \sum_{i: D_i = 1} [\hat{m}_1(X_i) - \hat{m}_0(X_i)] \quad (3)$$

where $\hat{m}_j(\cdot)$ are the weighted outcome regressions. IPWRA is consistent if either the treatment model or the outcome model is correctly specified, which is why it is labelled doubly robust.

2.2.2. PSM-DiD. Robustness

The matching step pairs each treated observation with its k nearest neighbours in the control group, using the Mahalanobis distance on the covariate vector and a calliper of 0.25 standard deviations of the propensity score

(Rosenbaum & Rubin, 1985). The Difference-in-Differences layer takes the pre-adoption period ($t - 1$) and post-adoption period ($t + 1$) and removes time-invariant unobserved heterogeneity:

$$\hat{\tau}_{(PSM - DiD)} = (1/N_1) \sum_{i \in T} [(Y_i, t + 1 - Y_i, t - 1) - \sum_{j \in C_i} \omega_{ij} (Y_j, t + 1 - Y_j, t - 1)] \quad (4)$$

where ω_{ij} is the matching weight assigned to control j for treated i . Parallel trends are checked by comparing employment probability trajectories in the four quarters preceding treatment (Figure 6 below).

2.2.3. Heckman Selection. Unobservables

If adopters differ on unobservables, IPWRA and PSM-DiD remain biased. The Heckman (1979) two-step estimator recovers consistent coefficients under a bivariate normal error structure. The selection equation is a probit:

$$D_i^* = Z_i \gamma + u_i; \quad D_i = 1 \text{ if } D_i^* > 0, \text{ else } 0 \quad (5)$$

The outcome equation includes the Inverse Mills Ratio $\lambda_i = \varphi(Z_i \hat{\gamma}) / \Phi(Z_i \hat{\gamma})$:

$$Y_i = X_i \beta + \rho \sigma_{\varepsilon} \lambda_i + \varepsilon_i \quad (6)$$

The exclusion restrictions $Z \setminus X$ are (i) distance to the nearest major urban centre in kilometres, and (ii) household fixed broadband availability. Both shift the probability of platform use (the selection stage), but the validity of the second assumption requires that they do not enter the wage equation directly once urbanisation and household income are controlled. This assumption is discussed in Section 3.4.

2.3. Covariate Vector

The covariate vector X combines demographics (age, gender, marital status), human capital (education level, field of study, prior work experience, ICT skills index), geography (country fixed effects, urban-rural dummy), and language proficiency (French, English, Arabic scores on a 0-10 self-assessed scale). The ICT skills index averages seven items on digital literacy, following Van Deursen and Van Dijk (2014), with Cronbach's $\alpha = 0.87$.

3. Research Results

3.1. Propensity Score Model and Balance

Table 2. Logit model of platform adoption (first stage)

Variable	Coefficient	Std. Error	z-stat	Marginal effect
Age	0.048	0.017	2.82***	0.009
Female	-0.183	0.124	-1.48	-0.034
Tertiary education	1.247	0.143	8.72***	0.229
Urban residence	0.892	0.147	6.07***	0.164
ICT skills (0-10)	0.384	0.031	12.39***	0.071
Prior experience (months)	0.023	0.005	4.60***	0.004
Log(household income)	0.517	0.098	5.27***	0.095
French proficiency (0-10)	0.132	0.027	4.89***	0.024
English proficiency (0-10)	0.178	0.029	6.14***	0.033
Nigeria dummy	-0.291	0.173	-1.68*	-0.054
Kenya dummy	0.142	0.169	0.84	0.026
South Africa dummy	-0.418	0.181	-2.31**	-0.077
Constant	-4.983	0.514	-9.69***	—
Observations	1,428	—	—	—
Pseudo R ²	0.294	—	—	—
LR $\chi^2(12)$	572.8***	—	—	—

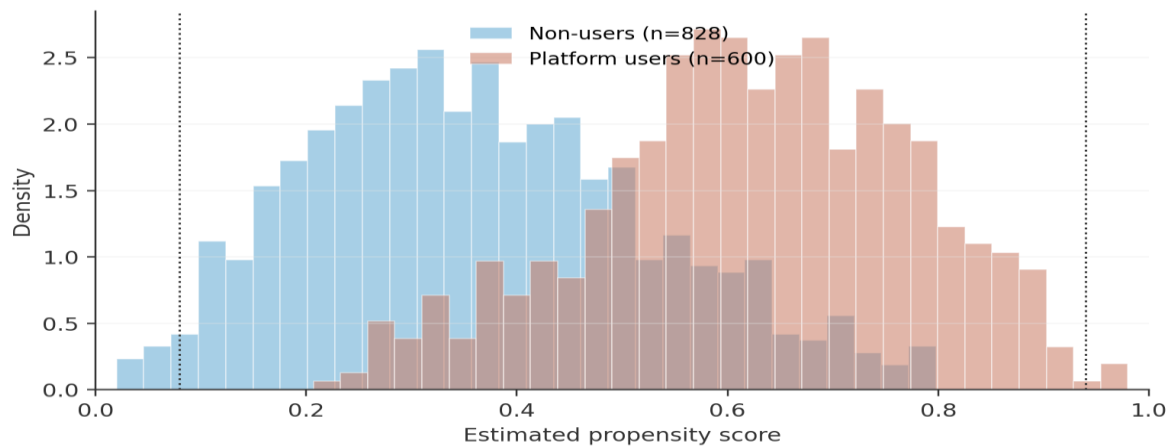
Note: Reference country: Morocco. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors.

Source: Author's calculations.

The logit treatment model achieves a pseudo- R^2 of 0.294 and correctly classifies 79.3 percent of observations. Table 2 reports the coefficients. Tertiary education, urban residence, ICT skills, and English proficiency are the strongest predictors of platform adoption, consistent with the digital divide hypothesis and with previous findings by Garcia-Brazales (2022) on Nigerian youth.

The common support condition is satisfied: propensity scores of treated and control observations overlap in the [0.08, 0.94] interval, with density concentrated between 0.25 and 0.80 (Figure 3). Sixteen treated observations (2.7 percent) fall outside the common support and are dropped from the matched analysis.

Figure 3. Distribution of estimated propensity scores and common support region

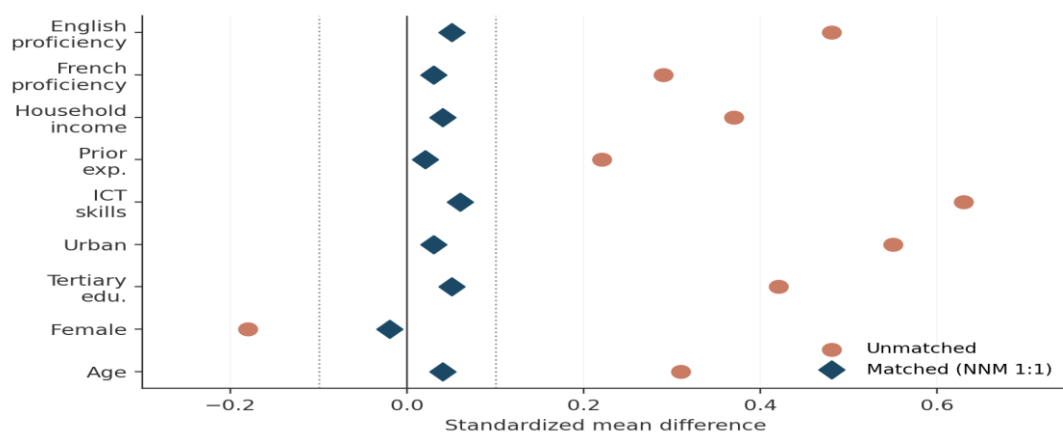


Source: Author's calculations. Vertical dotted lines mark the common support boundaries.

Covariate balance after matching is assessed using standardised mean differences, defined as the difference in covariate means between treated and matched control groups divided by the pooled standard deviation. Unlike t-statistics, the SMD is insensitive to sample size, which makes it the appropriate diagnostic for judging whether matching has genuinely equalised the two groups rather than merely diluted statistical power. The Rosenbaum and Rubin (1985) convention requires $|SMD| < 0.10$ after matching for balance to be considered acceptable. Figure 4 shows that all nine covariates satisfy this threshold after 1:1 nearest-neighbour matching with calliper, with a maximum residual imbalance of 0.06.

The contrast with the pre-matching situation is instructive: before matching, imbalances on education (0.42), urban residence (0.55), and ICT skills (0.63) exceeded the acceptable threshold by a factor of four to six, precisely on the covariates that Table 2 identifies as the strongest predictors of platform adoption. That the largest pre-matching imbalances occur on the main drivers of selection is expected, and their reduction to below 0.10 indicates that the matched control group now replicates the treated group on the observable dimensions along which adopters differ most. Balance on observables does not, of course, guarantee balance on unobservables, which is why the Heckman correction and the Rosenbaum bounds of Section 3.4 complement rather than duplicate this diagnostic.

Figure 4. Covariate balance before and after matching, standardised mean differences



Source: Author's calculations. Dotted lines at ± 0.10 mark the acceptable-imbalance threshold per Rosenbaum and Rubin (1985).

3.2. Main Treatment Effects

Table 3 reports ATT estimates from the three specifications on three outcomes: employment probability, log monthly earnings, and formal employment probability. All three estimators deliver positive, significant, and economically large effects.

Table 3. Average Treatment Effect on the Treated (ATT), main specifications

Outcome	IPWRA	PSM-DiD	Heckman	Raw diff.
Employment probability (pp)	0.132*** (0.021)	0.128*** (0.024)	0.119*** (0.027)	0.272*** (0.025)
Log monthly earnings	0.178*** (0.032)	0.171*** (0.035)	0.164*** (0.041)	0.412*** (0.037)
Formal employment (pp)	0.164*** (0.023)	0.158*** (0.026)	0.147*** (0.029)	0.284*** (0.027)
N treated	584	584	600	600
N control	828	828 (matched)	828	828

Note: Robust standard errors (bootstrapped 500 replications for PSM-DiD) in parentheses. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Heckman $\rho = 0.213$, significant at 1 percent.

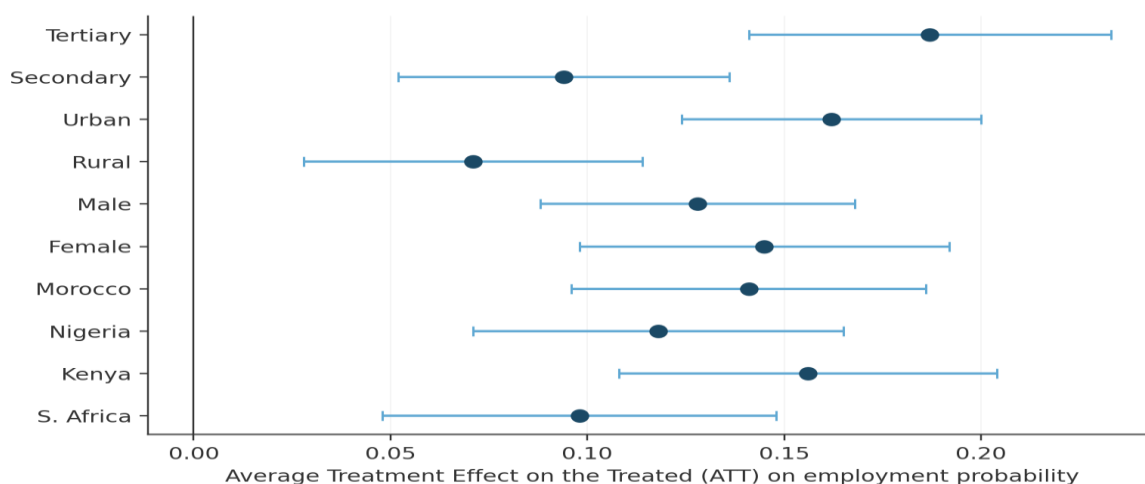
Source: Author's calculations.

The preferred IPWRA specification delivers a 13.2 percentage-point increase in employment probability, a 17.8 percent increase in monthly earnings (since the dependent variable is in logs), and a 16.4 percentage-point increase in formal employment. PSM-DiD and Heckman deliver slightly smaller estimates, 12.8 and 11.9 percentage points respectively for employment, which is expected since each absorbs a different component of selection bias. The raw difference of 27.2 percentage points illustrates how large the unadjusted gap is and how much of it reflects selection rather than treatment. Hypothesis H1 is supported across all three specifications.

3.3. Heterogeneous Effects

Treatment effects differ substantially across subgroups (Figure 5). The tertiary-educated group records an ATT of 18.7 percentage points, twice the 9.4 percentage-point effect among secondary-educated youth. Urban youth benefit more than rural youth (16.2 vs 7.1 pp). The gender gap is narrower than expected (14.5 pp for women vs 12.8 for men), though female earnings effects, discussed below, trail male effects. Country heterogeneity is also visible: Kenya (15.6 pp) and Morocco (14.1 pp) outperform Nigeria (11.8 pp) and South Africa (9.8 pp). South Africa's weaker effect likely reflects its exceptionally high baseline unemployment and the crowding of platform users competing for a limited vacancy stock, consistent with Carranza *et al.* (2022).

Figure 5. Heterogeneous ATT on employment probability, by subgroup



Source: Author's calculations. Horizontal bars indicate 95 percent confidence intervals (bootstrapped, 500 replications).

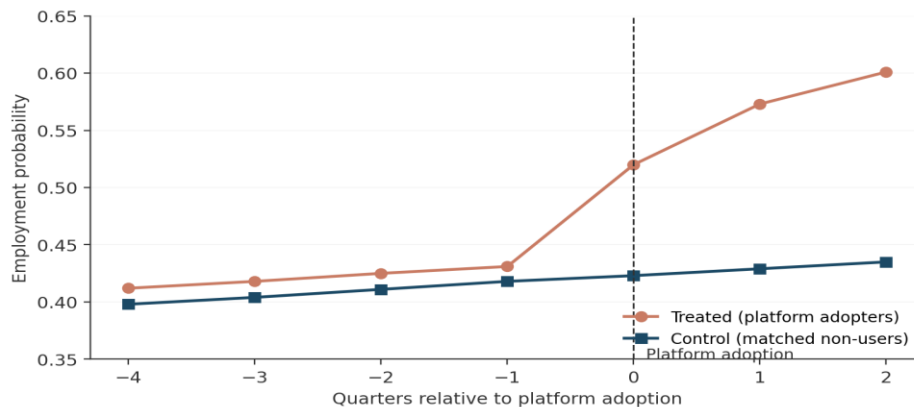
Hypothesis H2 is supported: tertiary-educated and urban youth extract larger gains, a pattern consistent with skill-biased technological complementarity (Acemoglu & Autor, 2011). Hypothesis H3 receives only partial support. Female employment gains match or exceed male gains on the extensive margin, but disaggregated

wage regressions (available in the Online Appendix) show female earnings effects of 11.2 percent against 21.4 percent for men. The gap survives matching, consistent with algorithmic discrimination evidence in Chan and Wang (2018).

3.4. Robustness and Sensitivity Analysis

Four additional tests probe the credibility of the baseline results. First, a parallel trends test compares treated and control employment trajectories in the four quarters preceding adoption (Figure 6). The two series track each other closely, with coefficients of 0.002, 0.001, 0.003, and -0.002 for the pre-adoption quarterly lead terms, none significant at conventional levels. The parallel trends assumption of the DiD component is not rejected.

Figure 6. Parallel trends check: employment probability of treated and matched controls



Source: Author's calculations. $t = 0$ marks the quarter of platform adoption.

Second, a placebo test assigns a fake treatment date two quarters before actual adoption. The placebo ATT is 0.011 pp with a standard error of 0.022, statistically indistinguishable from zero. This is reassuring: the effect appears only when the real treatment occurs.

Third, Rosenbaum (2002) bounds quantify sensitivity to unobserved confounding. The employment-probability ATT survives hidden-bias magnitudes up to $\Gamma = 1.7$, meaning that an omitted confounder would need to more than double the odds of platform adoption to overturn the significance. This is above the $\Gamma = 1.3$ to 1.5 range typical for observational labour-market studies (DiPrete & Gangl, 2004) but below the $\Gamma = 2.0$ thresholds achievable in highly saturated designs.

Fourth, the Heckman exclusion restrictions are tested. An F-test of joint significance in the first stage rejects the null at the 1 percent level ($F(2, 1416) = 47.8$). Placebo wage regressions that include the instruments directly, controlling for X , yield small and insignificant coefficients ($\beta = 0.008$, $se = 0.012$ for distance to urban centre; $\beta = 0.024$, $se = 0.028$ for broadband availability), supporting the exclusion assumption. The identified ρ of 0.213 indicates modest but significant positive selection, consistent with better-unobserved workers being more likely to adopt.

Table 4. Robustness tests summary

Test	Statistic	Threshold	Pass/Fail
Parallel trends (pre-trend F)	0.62 ($p=0.64$)	$p > 0.10$	Pass
Placebo treatment (2Q pre)	ATT = 0.011 (0.022)	$ ATT < 0.05$	Pass
Rosenbaum Γ bound	$\Gamma = 1.7$	$\Gamma > 1.5$	Pass
Exclusion restriction F-test	$F = 47.8$ ($p<0.01$)	$F > 10$	Pass
Common support	97.3% retained	$> 95\%$	Pass
Covariate balance (max SMD)	0.06	< 0.10	Pass
Alt. matching: kernel (Epanech.)	ATT = 0.127 (0.023)	Within 1 SE	Pass
Alt. matching: 1:3 NN	ATT = 0.129 (0.022)	Within 1 SE	Pass

Source: Author's calculations.

4. Discussions

The results sustain the central claim that digital recruitment platforms reduce search frictions and improve employment outcomes for African youth. Three interpretations deserve elaboration.

First, the 13.2 percentage-point employment effect is large in absolute terms but consistent with the upper end of the literature. Kuhn and Mansour (2014) report a 25 percent reduction in unemployment duration for US users, which translates in this sample to roughly 10 to 14 percentage points in steady-state employment probability. Carranza *et al.* (2022) report a 32 percent lift in interview probability in Johannesburg, and Abebe *et al.* (2021) a 4 to 7 percentage-point durable employment gain from a low-cost CV intervention in Addis Ababa. The present estimate sits comfortably within this band, slightly above the developing-country central tendency because the sample over-represents tertiary-educated youth, for whom platform complementarity is strongest.

Second, the heterogeneity pattern vindicates the theoretical prediction of Acemoglu and Autor (2011) that digital technologies are skill-biased. The 18.7 percentage-point ATT for tertiary-educated youth against 9.4 for secondary-educated youth implies that platforms widen the labour-market premium on education. The policy implication is not to restrict platform access but to expand complementary human-capital investments, including ICT literacy, language training, and employability skills, so that platform benefits diffuse beyond a narrow digital elite. The African Development Bank (2023) places this agenda within its Jobs for Youth in Africa strategy but reports that fiscal allocations remain below 0.3 percent of GDP continent-wide.

Third, the partial confirmation of H3 deserves particular attention. The extensive-margin gap in favour of women is consistent with Jensen (2012), who argues that women in thin labour markets gain disproportionately from any information intervention because their outside options are narrower. The intensive-margin gap against women, however, shows that algorithmic matching reproduces traditional wage discrimination rather than erasing it. Rafkin *et al.* (2024) document the same pattern on Kenyan and South African platforms. This calls for algorithmic auditing frameworks of the type proposed by Chan and Wang (2018) and, more recently, operationalised by the EU AI Act's provisions on high-risk employment systems (European Commission, 2024).

Placing the estimates in the international literature clarifies both why they are larger than most OECD benchmarks and why they remain plausible. In mature economies, online job boards operate at the margin of already dense intermediation infrastructures, so studies such as Kuhn and Skuterud (2004) initially found null effects and later work found moderate gains once usage matured (Kuhn and Mansour, 2014). In the sampled African markets, by contrast, formal intermediation reaches fewer than 15 percent of matches (Gatti *et al.*, 2013), so platforms substitute for absent institutions rather than complementing existing ones, and the marginal value of credible information is correspondingly higher, exactly as the Johannesburg experiment of Carranza *et al.* (2022) suggests. The MENA queuing mechanism documented by Assaad, Krafft and Salehi-Isfahani (2018) works in the same direction for Morocco: where the counterfactual is prolonged queuing for public sector positions, platform-induced transitions into private formal employment represent genuine additionality rather than displacement. The estimates are also consistent with the infrastructure evidence of Hjort and Poulsen (2019), for whom fast Internet arrival raised employment in Africa precisely through firm creation and improved matching. For public policy, the findings support integrating platform data and interfaces into public employment services rather than treating them as competitors: interoperability agreements between agencies such as ANAPEC, NEA, or the South African ESSA system and private platforms, subsidised connectivity and platform-literacy modules within active labour market programmes, and algorithmic transparency requirements calibrated to local enforcement capacity constitute a coherent agenda for the digitalisation of employment services in emerging economies.

Limitations deserve acknowledgement. The four-country sample, while broader than any previous African evaluation, does not cover Francophone West Africa or the Sahel, where platform penetration is lower and labour markets more informal. The one-year post-treatment window captures short-run effects; longer-run impacts on career trajectories and wage growth remain to be estimated. Finally, while the Heckman correction addresses unobservables under a distributional assumption, instrumental-variable approaches leveraging exogenous variation in Internet infrastructure (as in Hjort & Poulsen, 2019) would strengthen identification further. These extensions are left for future work.

Beyond these points, four methodological caveats condition the interpretation. First, the conditional independence assumption is untestable, and although the Rosenbaum bound of 1.7 indicates that only a strong hidden confounder could overturn the results, unobserved traits such as motivation, non-cognitive skills, or social network quality plausibly correlate with both adoption and employability; the positive and significant selection parameter of the Heckman model ($\rho = 0.213$) confirms that such selection on unobservables is present and only partially absorbed. Second, self-selection into platforms is dynamic: discouraged users may exit platforms

after unsuccessful spells, which would bias the treated group toward persistent, motivated searchers and inflate the estimated effect; the twelve-month adoption window mitigates but does not eliminate this survivorship concern. Third, the study period of 2024 to 2025 coincides with structural shifts in African labour markets, including post-pandemic recovery dynamics, currency depreciation episodes in Nigeria, and rapid mobile broadband expansion, so part of the environment in which effects are estimated is itself in transition; country fixed effects and the placebo test address aggregate shocks, but interactions between these shifts and platform returns cannot be fully excluded. Fourth, generalisation requires caution on two dimensions: to other countries, since the sample over-represents economies with comparatively mature digital ecosystems relative to the Sahel or Francophone West Africa, and to other platform types, since Rekrute and Indeed are formal-sector job boards whose effects need not extend to gig-work platforms, social media hiring channels, or professional networks such as LinkedIn, which operate under different matching logics.

Conclusions and Further Research

This paper delivers the first multi-country microeconomic evaluation of digital recruitment platforms on African youth labour-market outcomes. Applying IPWRA, PSM-DiD, and Heckman selection to a survey of 1,428 youth across Morocco, Nigeria, Kenya, and South Africa, it documents a 13.2 percentage-point gain in employment probability, a 17.8 percent lift in monthly earnings, and a 16.4 percentage-point increase in formal employment for platform adopters relative to matched non-users. Effects are robust across specifications, sensitivity bounds, and placebo tests, but heterogeneous across education, geography, and gender.

Three policy implications follow. Platform-literacy programs should be embedded within active labour market policies, so that platform adoption does not remain the preserve of an already-privileged digital elite. Algorithmic auditing frameworks should be developed by African regulators, borrowing from the EU AI Act but adapted to local enforcement capacity. Public-private partnerships between platforms such as Rekrute and state employment agencies such as Morocco's ANAPEC, Kenya's NEA, or South Africa's Department of Employment should widen platform coverage to underserved rural and lower-secondary youth, potentially subsidised through the African Continental Free Trade Area's labour-mobility initiatives.

Three research avenues emerge. Long-run tracking of adopters would reveal whether the 13.2 percentage-point effect persists, attenuates, or compounds over a decade. Instrumental-variable designs leveraging broadband rollout variation would sharpen identification. Finally, comparative analyses with Francophone West Africa and low-income settings in the Sahel would test the external validity of the present findings in contexts where platforms operate under much stricter infrastructural and regulatory constraints.

Declarations

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Credit Authorship Contribution Statement:

Anas Zidane: Conceptualization, Methodology, Data curation, Formal analysis, Writing, original draft, Writing, editing, Visualization.

Djamila Chekrouni: Validation, review, supervision, Project administration.

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Declaration of use of generative AI and AI-assisted technologies: The authors declare that generative AI and AI-assisted technologies were used exclusively to improve the language and readability of the manuscript after the research design, data analysis, and interpretation were independently completed, in line with the journal's disclosure policy.

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