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Labour market effects of digital matching platforms

Experimental evidence from sub-Saharan Africa

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Abstract: Can digital labour market platforms reduce search frictions in either formal or informal labour markets? We study this question using a randomized experiment embedded in a tracer study of the work transitions of graduates from technical and vocational colleges in Mozambique. We implement an encouragement design, inviting graduates by SMS to join one of two local digital platforms: *Biscate*, a site to find freelancers for informal manual tasks; and *Emprego*, a conventional formal jobs website. In contrast to positive estimates of the contribution of both platforms to job outcomes from naïve (per-treatment) estimates, both intent-to-treat and complier average treatment effects are consistently zero in the full sample, while the impact on life satisfaction is negative. However, use of the informal jobs platform leads to better work outcomes for women, especially those with manual qualifications, for whom earnings rise by over 50 per cent.

Key words: digital labour platforms, search frictions, technical and vocational education, unemployment, Mozambique

JEL classification: J46, J64, J68, O15

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1 Introduction

Youth unemployment is a major policy concern in Africa. While one in every five people born on the continent is looking for a job (Bandiera et al. 2022), unemployment often co-exists with unmet labour demand (Banerjee and Sequeira 2020). A potential explanation for this is frictions that prevent employers linking with qualified job candidates (Chade et al. 2017). Such frictions encompass search costs, including basic transportation expenses (Franklin 2018), screening costs (Abebe et al. 2021a), inaccurate beliefs (Abebe et al. 2021b; Beam 2016), and other obstacles to labour market information flows.

In this paper we provide causal evidence on the extent to which digital platforms to match labour supply and demand improve the employment outcomes of recent college leavers in low-income Africa. Our focus is graduates of technical and vocational educational and training (TVET) institutes in Mozambique. We embed a randomized encouragement design within a tracer survey and study two matching platforms addressing different segments of the labour market. The first, called Biscate, attempts to match demand and supply of freelancers for specific tasks and services (e.g., plumbing, catering). The platform is focused on manual workers in the informal labour market and is designed such that clients search for workers with a suitable profile in their location. The second, called Emprego, is a more conventional platform allowing job-seekers to search and directly apply for formal jobs posted by employers.

We randomly allocate TVET graduates from our baseline survey into one of three experimental arms: (1) an SMS invitation to register on Biscate; (2) an SMS invitation to register on Emprego; and (3) a control group (no SMS). We estimate the impact of both platforms on a range of labour market outcomes, including rates of employment, hours worked, job quality, reservation wages, after-tax wage income, and metrics of jobs search. In line with recent literature concerned with non-material outcomes, such as job satisfaction (Abebe et al. 2021a; Suzuki et al. 2018), we also consider their effect on subjective well-being.

Naïve estimates of the relationship between platform usage and labour market outcomes are consistently positive. However, both intent-to-treat and complier average treatment effects are close to zero for all headline labour market outcomes in the full sample, suggesting strong self-selection onto the platforms. However, these findings contrast with evidence of positive labour market effects for female graduates, but only for users of the informal matching platform, and especially those women with manual (industrial/construction) qualifications. For this subgroup, platform usage increased paid employment rates by 11 percentage points against a 20 point counterfactual, while both hours worked and wage income rose by over half. Furthermore, in the full sample, we find a positive effect of platform usage on search intensity, but a negative effect on overall life satisfaction.

These findings contribute to several literatures. Broadly, they speak to studies of the effectiveness of programmes to enhance labour market outcomes among disadvantaged youth (see Card et al. 2018; McKenzie 2017). Papers in this field have examined both supply-side interventions aimed to enhance the employability of workers, such as through vocational training (e.g. Alfonsi et al. 2020), as well as demand-side interventions, such as wage subsidies (e.g. Groh et al. 2015). However, consistent positive effects from these kinds of interventions remain elusive, especially in low-income country contexts, as is their fiscal sustainability.

This paper specifically adds to the growing empirical literature on interventions to reduce matching frictions in developing countries. As noted, recent contributions have focused on addressing transport and screening costs, such as by providing reference letters, skill report cards, referrals, and information to placement officers about preferences of candidates (Abel et al. 2020; Banerjee and Chiplunkar 2018; Bassi and Nansamba 2022; Pallais and Sands 2016). A general consequence of these frictions is information asymmetries, whereby employees (employers) are not fully aware of suitable vacancies (candidates). Thus, often taking advantage of new technologies, informational interventions have been considered—for example, Dammert et al. (2015) provide information on vacancies to job-seekers in Peru via SMS, finding short-term employment gains as well as higher job search intensity among the treatment group (see also Belot et al. 2019; Kircher 2020; Kuhn and Mansour 2014).

We study a related low-cost mechanism that also may help to address information asymmetries, including screening costs: use of digital matching platforms. Due to improvements in access to technology, particularly smartphones (Bandiera et al. 2022), these platforms are increasingly used by both employers and job-seekers in a wide range of contexts, including lower-income Africa. However, despite the rapid growth of digital jobs platforms across the globe (ILO 2021), there is limited rigorous causal evidence regarding their ultimate benefits to workers, especially in developing country contexts or in (lower-skilled) informal labour markets.

Two previous studies have examined the impact of formal jobs platforms in developing Asia, finding complex and ambiguous effects. Kelley et al. (2021) examine the labour market outcomes of an online jobs website in India, whereby randomly selected vocational training graduates were registered by researchers on a portal and sent SMS information about job vacancies. They find the intervention temporarily increased voluntary unemployment, driven by an expectations channel—treated graduates increased their reservation wage and were 9 percentage points less likely to be employed for at least one year. More similar to our own study, Chakravorty et al. (2021) use a randomized encouragement design to nudge vocational training graduates in Bihar and Jharkhand (India) to use the government-run YuvaSampark jobs matching application. They find moderate uptake and no positive effects of the platform on a range of labour market outcomes.

Last, our finding of a negative effect of digital platform usage on subjective well-being speaks to literature on aspirations failure, particularly the possible negative incentive effects of unmet aspirations (Genicot and Ray 2017, 2020). In our context, we hypothesize that use of online platforms by recent TVET graduates may have augmented aspirations of finding employment or a good job. But, as our evidence suggests, in a weak jobs environment these aspirations were in large part not met. This may explain why individuals in our treatment groups who did use the platforms have lower subjective well-being than those in the control group—the experience of the platforms in the treated group did not generate systematically better labour market outcomes, despite higher search effort.

2 Context

Our experiment was embedded in a longitudinal (tracer) survey of the school-to-work transitions of graduates of TVET institutes in Mozambique. As elsewhere in the region, the country combines low average levels of human capital with limited formal or good-quality employment opportunities, particularly for the youth. As such, there has been long-standing interest in the potential of TVET to boost their employment outcomes (for general discussion see Alfonsi et al. 2020; Tripney et al. 2013). As Jones et al. (2021) summarize, Mozambique began to reform its technical and professional education system in the early 2000s: in 2001, the government approved a new ten-year TVET strategy; in 2006 the World Bank launched a 15-year project (*Reforma da Educação Profissional*) to improve the quality, relevance, and responsiveness of the TVET system to the labour market; a new framework Vocational Education Law was passed in 2014, establishing a new regulatory authority; and in 2017, the National Professional Education Fund (*Fundo Nacional de Educação Profissional*) was established.

At the end of almost two decades of reforms, the tracer survey sought to investigate how new TVET graduates fare in the labour market. The survey was undertaken in two phases. The first, which ran from

October to November 2019, was an in-person baseline survey of final-year students in TVET colleges selected to cover all regions of the country (specifically, institutions located in Maputo City, Maputo Province, Tete, Cabo Delgado, and Nampula provinces). This collected information on students' cognitive abilities, their family background, and expectations and aspirations for the future. The second phase, which started after completion of the preceding academic year and ran from January to November 2020, comprised a series of four follow-up telephone survey rounds. These collected data on the evolution of labour market outcomes of each participant over time. We sought to re-contact each participant in each follow-up round, yielding a panel of four quarterly observations per person (plus the baseline).[1]

3 Experiment

3.1 Target platforms

Within the framework of the tracer survey we partnered with the operator of two locally developed digital labour market platforms. Biscate, which means 'odd job' in Portuguese, is a platform to match demand and supply of manual freelancers for specific tasks or services. The platform allows individuals with practical/manual skills to advertise their availability and thereby expand their customer base. Typically, prospective clients browse the platform to find a contractor for a specific task, such as plumbing or manicure, for direct payment in cash. The platform is accessed mainly via mobile phone on the Vodacom network using Unstructured Supplementary Service Data, which is not reliant on smartphone technology; it also has a dedicated smartphone application and website (www.biscate.co.mz). The platform has around 50,000 registered workers, and since its launch in 2016 more than 30,000 customers have used the service and 120,000 worker contacts have been requested through it.

Emprego, which means 'job' or 'employment' in Portuguese, is a more conventional formal jobs website where employers post vacancies and can receive applications from registered users. The platform is only accessible via the internet (www.emprego.co.mz), including via a dedicated mobile application. Currently, the site is accessed by around 18,000 individuals daily and over 1,400 organizations in Mozambique use it, encompassing private firms and non-governmental agencies.

The two platforms target different segments of the labour market. Biscate relates to semi-skilled manual tasks demanded by private individuals outside the formal labour market (see the profile example in Appendix B1). Informal activities of this sort (or of a lower-skilled nature) dominate the Mozambican economy but nonetheless can be a means to gain experience and/or develop contacts, possibly leading to further work. In contrast, Emprego covers the smaller formal segment of the labour market, dominated by professional services roles. Vacancies posted on Emprego tend to demand a comparatively high level of education (e.g. tertiary), previous professional experience, and often English-language skills. Nonetheless, the site does include a smaller number of 'blue collar' vacancies, such as those with technical-vocational qualifications (see the vacancy example in Appendix B1).

Additionally, the two platforms differ in terms of the direction in which search takes place—under Biscate, clients will contact potential employees based on their location and profile, as well as any comments or ratings from previous *biscates*. Under Emprego, candidates can post their CVs and contact potential employers, based on their presumed suitability for a specific job. This difference may be relevant in the context of markets with excess supply.

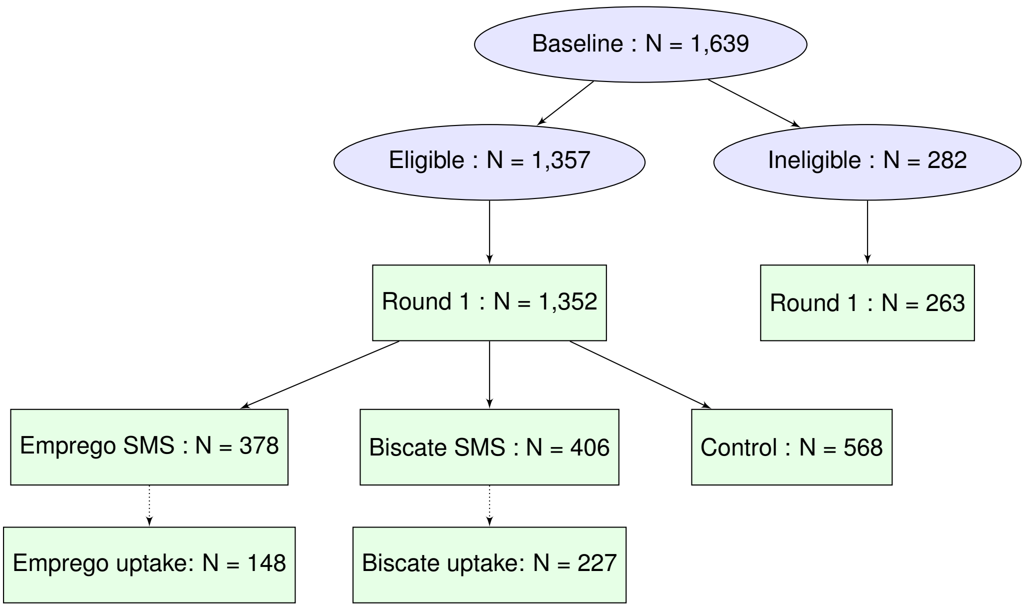
[1] Descriptive statistics from the survey, as well as further details on the sample structure, are found in Jones et al. (2021). Based on official information regarding the universe of TVET institutes, we construct post-stratification weights to correct for disproportions in the share of sampled observations versus the regional distribution of final-year students. These are applied throughout but only imply minor adjustments.

3.2 Encouragement intervention

To test the contribution of the two platforms to employment outcomes we adopted an encouragement design. Immediately before the start of the second round of the follow-up surveys, when participants had (in principle) completed their studies and begun looking for work, we sent SMS invitations to individuals randomly selected from the baseline sample. The experimental intervention (or nudge) thus comprised two separate treatment arms with no cross-over, namely: (1) an SMS invitation to register on Emprego; and (2) an SMS invitation to register on Biscate.

The general target population for the intervention was the full baseline TVET sample (N = 1,639). However, to minimize contamination, particularly from individuals with prior experience of either of the platforms, we placed a number of restrictions on the sample to identify an eligible subgroup. Concretely, we excluded from the full sample: (1) individuals who did not consent to participate in the follow-up telephone surveys; (2) individuals with shared or duplicated contact numbers in the baseline survey; (3) individuals without a Vodacom mobile phone contact number; and (4) individuals already registered on either the Biscate or Emprego platforms before the start of the first follow-up round. As summarized in Figure 1, this yielded a sub-sample of 1,357 eligible participants, equal to 8 per cent of the full baseline sample.

Figure 1: Count of participants classified by experimental status



Note: the flow chart summarizes and partitions the number of observations (N) in different survey rounds and groups; all lower nodes are subsets of higher nodes; ellipses refer to the baseline survey; boxes refer to follow-up telephone rounds.

Source: authors' compilation.

Following the pre-analysis plan (Jones and Santos 2020), we used a random number generator to assign individuals to one of three arms (Emprego, Biscate, control) assuring a ratio of $1 : 1 : \sqrt{2}$ (respectively), as per an optimal 'square root' allocation rule (Dunnett 1955; Liu 1997). We did this in two stages. First, we assigned eligible individuals into one of two groups—to receive an encouragement message or not. Second, considering the types of jobs offered on Emprego are generally better suited (but not exclusively so) to graduates of services-oriented courses, while tasks on Biscate are generally better oriented to graduates of manual (industrial/construction) courses, we employed a conditional or partial randomization rule to allocate those individuals selected to receive an intervention between the two nudge types. For individuals who studied manual-oriented courses, we randomly allocated 60 per cent of those selected to receive an SMS to the Biscate treatment arm, and the rest to the Emprego arm;

and for individuals who studied services-oriented courses we did the reverse (40 per cent to the Biscate arm).

Among the individuals re-contacted in round 1, 378 were sent the Emprego SMS nudge and 406 the Biscate SMS nudge (see Figure 1).[2] The SMS nudges were all sent on the same day (30 March 2020, between 11 a.m. and 12 p.m.), followed by a reminder ten days later (9 April). The messages were personalized, including the name of the course they had studied, and encouraged them to access and set-up a profile on the relevant platform. For individuals assigned to the Biscate nudge the SMS read as follows (for graduates of courses in accountancy):

> Mensagem para finalistas do curso Contabilidade: regista-te no Biscate para receberes oportunidades de trabalho. Liga gratuito para *777#

And the equivalent message for individuals assigned to the Emprego nudge read as:

> Mensagem para finalistas do curso Contabilidade: encontra vagas de emprego na internet acedendo a https://emprego.co.mz/

In this latter message we included an individual-specific link that took them to a bespoke landing page, where they were invited to sign-up to the platform.

3.3 Data and descriptive statistics

Table 1 summarizes the data from across the survey rounds. Panel (a) reports individual-level information collected at the baseline (only), where column (1) gives averages for different sub-samples, namely: the full baseline, the ineligible subgroup, and the subgroup deemed eligible for participation in the experiment. Column (2) reports averages for the same variables but now in the post-intervention period only (pooling follow-up rounds 2–4), again distinguishing between different samples (in order): all eligible observations, the control group, those assigned to the Emprego SMS nudge, and those assigned to the Biscate nudge. The final column reports results from a balance test, based on separate ordinary least squares (OLS) regressions, where the null hypothesis is that there is no difference between the means of the control group and the assigned group (jointly) in the post-intervention period.[3]

Two points merit note. First, the ineligible subgroup appears somewhat distinct from the eligible sample—for example, it is male-dominated and has more prior work experience. Second, with the exception of the type of course attended, which is associated with the type of encouragement message received by design, there is a reasonable balance of the baseline variables across the experimental groups. Nonetheless, both age and prior experience show some association, meriting their inclusion as controls in subsequent analysis.

[2] The difference here is primarily because after the initial assignment procedure was undertaken in early 2020, we found a small number of individuals who had already established a profile on Emprego before the start of round 2 and thus were ineligible (*ex post*).

[3] In running these regressions we also control for the stratifying variables deployed in the randomization process, as well as the type of course. These are individually excluded when they feature as the dependent variable.

Table 1: Descriptive statistics

		(1) Pre-intervention			(2) Post-intervention				
		All	Inelig.	Elig.	All	Ctrl	Emp	Bis	Pr.
(a)	Age	21.59	21.40	21.63	21.63	21.62	21.91	21.27	0.011
	Female	0.41	0.25	0.44	0.44	0.38	0.46	0.40	0.986
	Manual course	0.60	0.66	0.59	0.59	0.61	0.49	0.68	0.000
	Public school	0.66	0.72	0.65	0.65	0.65	0.64	0.70	0.237
	Work for self	0.79	0.75	0.80	0.80	0.77	0.81	0.81	0.234
	Work for others	0.83	0.85	0.82	0.82	0.84	0.81	0.84	0.312
	Prev. experience	0.46	0.52	0.45	0.45	0.48	0.44	0.43	0.092
	Phone/computer/internet	0.70	0.71	0.69	0.69	0.70	0.71	0.67	0.674
	Mother second. edu.	0.52	0.46	0.53	0.53	0.49	0.58	0.53	0.145
	Father second. edu.	0.62	0.54	0.63	0.63	0.60	0.62	0.65	0.556
(b)	Emprego profile (ext.)	0.03	0.15	0.00	0.05	0.06	0.16	0.08	0.000
	Emprego profile (self)	0.00	0.00	0.00	0.21	0.25	0.30	0.22	0.000
	Emprego user	0.04	0.13	0.02	0.11	0.14	0.19	0.13	0.000
	Biscate profile (ext.)	0.00	0.00	0.00	0.16	0.01	0.03	0.49	0.000
	Biscate profile (self)	0.00	0.00	0.00	0.17	0.07	0.18	0.33	0.000
	Biscate user	0.00	0.00	0.00	0.11	0.03	0.08	0.28	0.000
(c)	Worked (<7 days)	0.40	0.48	0.38	0.39	0.42	0.38	0.38	0.325
	Paid work (<7 days)	0.24	0.28	0.23	0.31	0.33	0.30	0.30	0.790
	Hours worked (week)	18.69	21.96	18.01	17.29	18.15	16.51	17.26	0.760
	Job quality index	0.25	0.31	0.24	0.24	0.25	0.23	0.24	0.757
	Reservation wage (month)	214.78	234.92	210.60	167.39	172.33	165.07	168.00	0.372
	Salary income (month)	39.21	44.14	38.18	41.79	47.68	39.87	40.10	0.212
	Looking for work	0.80	0.84	0.80	0.71	0.69	0.70	0.72	0.105
	Hours searching	9.18	9.37	9.14	4.51	4.48	4.43	4.96	0.222
	Satisfied with life	0.63	0.61	0.63	0.58	0.59	0.54	0.56	0.020
(d)	COVID -ve (self)	0.00	0.00	0.00	0.29	0.28	0.32	0.30	0.036
	COVID -ve (family)	0.00	0.00	0.00	0.21	0.20	0.19	0.23	0.043
	COVID -ve (comm.)	0.00	0.00	0.00	0.31	0.31	0.30	0.33	0.207
(e)	Obs.	1,639	282	1,357	3,975	2,376	1,188	1,299	

Note: the cells report means for different survey rounds and subgroups; column (1) refers to observations in the baseline survey (for panel (a)) or round 1 (other panels), showing results for the full sample, plus those eligible and ineligible for the experiment; column (2) pools follow-up rounds 2–4, separating between control, Emprego, and Biscate groups; panel (a) refers to fixed individual characteristics; panel (b) gives metrics of platform usage (external, self-reported, and mean); panel (c) summarizes headline outcomes; panel (d) are self-reported measures of negative COVID-19 impacts; the final column (Pr.) reports the probability that treatment group means jointly differ to those of the control in the post-intervention period.

Source: authors' estimates.

Panel (b) reports data collected on usage of the two platforms, which represent (endogenous) metrics of the 'treatments' of interest. For each platform we have three candidate variables: (1) information reported to us by the platform itself as to whether a given individual has registered, based on their phone number; (2) a self-reported measure of whether the individual has registered on the platform; and (3) a self-reported measure of whether the individual has used the platform to look for work (not shown). These indicators can differ for a variety of reasons—for example, if the individual uses a different number to register, if their profile is incomplete, if they confuse registration with more basic usage, or if they browse the platforms without registering. Indeed, although the variables are all positively associated, there are non-negligible differences—in particular, a much larger share of individuals report having a profile on Emprego (21 per cent in the post-intervention period) than is verified by the platform (5 per cent).

Without a strong *a priori* view as to which of these three metrics is most informative, for each individual we calculate their simple average and use this henceforth as our measure of platform usage. These are shown in the table (Emprego/Biscate user) and confirm that individuals exposed to either of the two nudges report a higher intensity of usage on the relevant platform than individuals in other arms. For example, treating the synthetic usage variable loosely as a probability (or intensity), individuals exposed to the Biscate nudge were 9.3 times more likely to use the platform compared to individuals in the control group; and for individuals exposed to the Emprego nudge they were 1.4 times more likely to use Emprego than the controls. This suggests both nudges were at least partially effective in prompting platform usage.

Panel (c) reports means for the core set of outcomes (following our pre-analysis plan). Estimates in column (1) refer to observations from the first follow-up round, before the nudge intervention; and those in column (2) refer to the post-intervention period (rounds 2–4). The first two variables are dummy variables capturing employment rates. Notably, these remained comparatively low *throughout* the survey period. For instance, in the first round just 38 per cent of the eligible group reported undertaking any work (paid or unpaid) in the seven days prior to being interviewed, increasing by just 1 percentage point in later rounds. Those reporting to have a formal job—defined as receiving payment in wages and also having a formal contract, fixed employment, or being enrolled in the contributory social security regime—was only 5 per cent in round 1, rising to an average of 9 per cent in later rounds.

The remainder of panel (c) covers other outcomes of interest. These include the number of hours worked (per week), an index of job quality, and self-reported measures of reservation wages and after-tax wage income (per month, in US dollars).[4] We also consider measures of job search, namely whether the individual reports to be actively seeking a(nother) job and the number of hours devoted to job search per week. Last we have a subjective measure of well-being—overall satisfaction with life, which takes a value of 0 if the individual is dissatisfied and 1 otherwise.

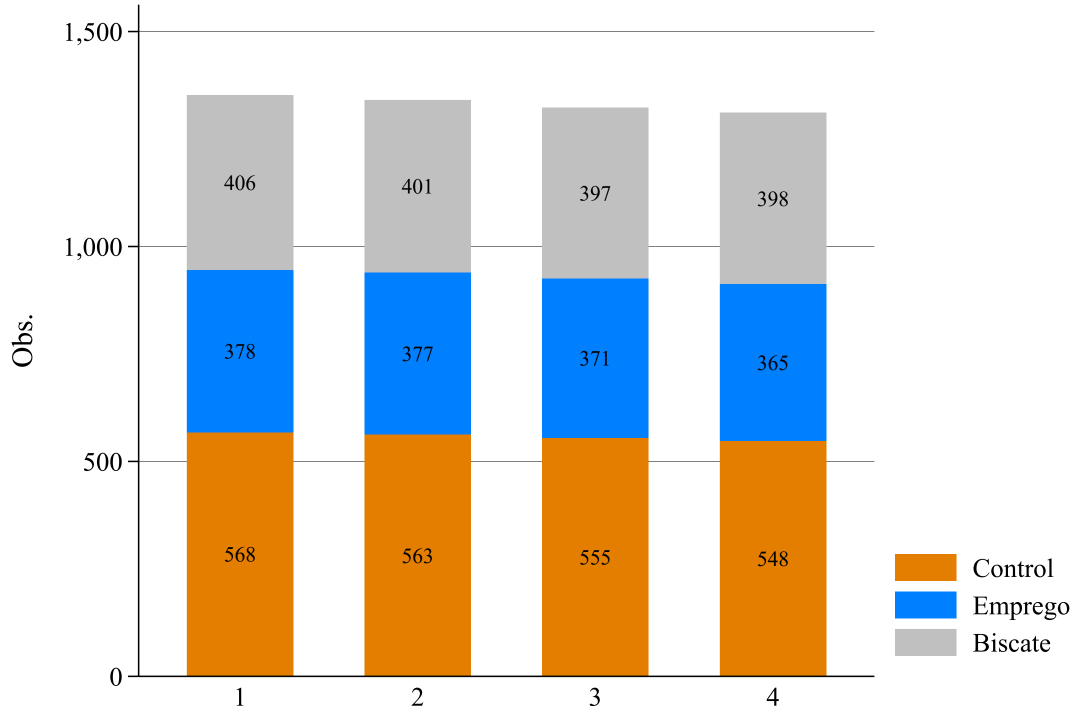
Results from balance tests applied to these outcomes, a preliminary form of 'intention-to-treat' analysis (see next section), indicate no strong systematic associations between assignment to the interventions and later outcomes. The main exception is life satisfaction, which appears moderately lower among both groups receiving a nudge and is significant at the 5 per cent level; and we note a positive effect on the propensity to be searching for work, which is borderline significant. Employment outcomes (such as being in paid work) are generally lower in the nudge groups versus the treatment, but these differences are generally substantially smaller than the minimal detectable effect (of 0.0755) we had estimated under simulations run prior to the experiment for our sample size.

Finally, panel (e) reports the number of observations in each group. As further clarified in Figure 1, just five of the eligible sample were lost in the first follow-up round; and even by the fourth round, more than a year after the baseline survey, we were able to contact 97 per cent of the eligible sample (1,311 individuals), implying an extremely low rate of attrition. This is supported by Figure 2, which reports the sum of observations in each follow-up round by eventual experimental group status, confirming low attrition across all experimental arms.[5]

[4] Outcomes are set to zero for individuals not working or without wage income. Reservation wages refer to the minimum salary individuals would accept to work in a full-time position. The job quality index is the simple average of eight dummy variables. These take a value of 1 (respectively) if the individual has a permanent or fixed position, has a written contract, is registered in the social security system (INSS), is working the desired number of hours (neither over- or under-employed), is actively seeking another job (while employed), works in the same area as one's studies, and works in a job in which technical qualifications are necessary to perform required tasks.

[5] Due to very low attrition, we do not consider this as a material source of bias.

Figure 2: Observations by follow-up round, by eventual experimental status



Note: the chart gives the number of observations in each follow-up survey round (1–4) by (*ex post*) treatment groups (treatment groups created and implemented only after round 1).

Source: authors' estimates.

4 Empirical strategy

Schematically, Figure A1 shows the set of relationships of interest. Our primary focus is the causal impact of the usage of digital labour market platforms (so-called 'treatments') on labour market outcomes. Intermediate outcomes pertain to measures of economic activity, which shed light on whether digital platforms help individuals move out of un- or under-employment. An individual already employed on a full-time basis also could use digital platforms to obtain more lucrative employment or negotiate better conditions. In this latter situation, while one may not observe an impact of platform usage on raw economic activity rates, final outcomes such as labour market earnings would be affected.

A necessary condition for our experiment to identify the contribution of digital platforms to these outcomes is that the SMS nudges significantly increased registration on and subsequent use of the platforms. This represents the first hypothesis to be tested, given by the following general model:

$$\text{Usage}_{ijt} = \alpha_j + \beta_j \text{Nudge}_{ijt} + X_{it}' \delta_j + \varepsilon_{ijt} \tag{1}$$

where j indexes the focus platforms ('Emprego' or Biscate'); i indexes individuals; and t is time (in follow-up rounds). The dependent variable captures platform usage; the main explanatory variable is the individuals' experimental status, which takes a value of 1 if they received an SMS nudge and 0 otherwise (being 0 in the first follow-up round for all); X is a vector of control variables, including: course type (to control for the conditional nature of treatment assignment), gender, college location, prior work experience, age at baseline, and a dummy variable for access to either a phone, the internet, or a computer (at baseline); we also include round-by-month fixed effects to capture general changes in economic conditions.

Equation (1) captures the direct effect of the encouragement on later uptake (usage) of the platforms. Since we expect actual platform usage to be endogenous, reflecting unobserved individual characteristics as well as time-varying labour market experiences and expectations, any assessment of the relationship between usage and labour outcomes may be biased. Two alternative effects are thus of interest. The first is the intent-to-treat effect (ITTE), which captures the direct net of the encouragement nudges on labour market outcomes, given by:

$$y_{it} = \alpha + \sum_j \delta_j \text{Nudge}_{ijt} + X_{it}'\gamma + \varphi_{it} \tag{2}$$

where y is the chosen outcome and all platforms are included simultaneously.

The second type of effect is the local average or complier average treatment effect (CATE), which captures the causal impact of the platforms (the treatments) on chosen outcomes for the specific subgroup of individuals induced to use the platforms on account of the SMS nudges.[6] This estimator employs the assignment to receive a nudge as an external instrumental variable for platform usage. Within this framework, equation (2) represents the reduced form relationship and equation (1) the first stage. The CATE can be derived in a variety of ways, one being to estimate the first stage and reduced forms simultaneously under the assumption $\forall j : (\varepsilon_{ijt}\varphi_{it}) \neq 0$, from which the CATE for a given platform is obtained as the estimate of: δ_j/β_j. Further details of the estimation methods are given in the presentation of results, to which we now turn.

5 Results

Table 2 begins with an analysis of the relationship between assignment to the SMS nudges and usage of the platforms. Following equation (1), we report results from separate OLS estimates of the alternative usage metrics for each platform, including assignment as covariates: the nudge, the core set of baseline controls (X, described above), and round-by-month fixed effects. As per Table 1, we note a consistent positive relationship running from the nudges to usage. The relationship is generally strongest for the Biscate nudge, where we find a marginal effect of 0.46 for the external metric of usage ('Ext.'). While the Biscate nudge has a much lower marginal effect on search on the same platform ('Srch'), this is unsurprising since it is clients rather than workers that typically search on the platform. Overall, we find exposure to the SMS nudges increased the synthetic measure of platform usage ('Mean') by 0.07 and 0.23 for the Emprego and Biscate messages, respectively. Thus, the nudges were successful in stimulating platform usage relative to the control group.

Table 3 summarizes different estimates for the series of outcomes described in Table 1, the only difference being that we apply the inverse hyperbolic sine (IHS) transform to the continuous variables (hours worked and wages) to allow for the presence of zeros (unemployed). For reference, panel (a) starts with a naïve analysis, which replicates the specification of equation (2), but replaces the nudge variables with observed synthetic (mean) platform usage. This is instructive since it indicates a clear positive association between most outcomes and usage, including an approximate 65 percent (0.5 log points) marginal increase in salary income, as well as more time spent on job search. However, a negative conditional association is found with regards to overall life satisfaction (subjective well-being) among users of the Biscate platform.

[6] This interpretation holds only under specific assumptions, including minimal 'defiance' and no direct effects of the nudge on outcomes (see De Chaisemartin 2017).

Table 2: Analysis of platform uptake

	(1) Emprego usage				(2) Biscate usage			
	Ext.	Self	Srch	Mean	Ext.	Self	Srch	Mean
Emprego SMS	0.09***	0.09***	0.03***	0.07***				
	(0.01)	(0.01)	(0.01)	(0.01)				
Biscate SMS					0.46***	0.22***	0.01**	0.23***
					(0.01)	(0.02)	(0.00)	(0.01)
Manual course	–0.01	–0.00	–0.01	–0.01	0.04***	0.01	–0.00	0.01**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Female	–0.03***	–0.09***	–0.05***	–0.05***	–0.02***	–0.04***	–0.00	–0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Prev. experience	0.01	0.02	0.01	0.01	0.02***	0.04***	0.00	0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Obs	5,327	5,327	5,327	5,327	5,327	5,327	5,327	5,327
R^2 adj.	0.06	0.13	0.02	0.11	0.39	0.16	0.01	0.32

Note: significance: * 10 per cent, ** 5 per cent, *** 1 per cent. The table summarizes results of estimates of equation (1) for different platforms (in columns) and measures of uptake (in sub-columns); 'ext.' takes a value of 1 if the individual has an externally verified profile on the platform; 'self' takes a value of 1 if the individual states they have a profile on the platform; 'srch' takes a value of 1 if the individual used the platform to search for jobs; 'mean' is the row-wise average of the three separate measures. Selected regression coefficients are shown. All follow-up rounds are pooled, with round-by-month fixed effects. Standard errors (in parentheses) clustered by unique baseline survey session and survey round.

Source: authors' estimates.

Panels (b)–(e) of Table 3 report effects that adjust for the possible endogeneity of observed platform usage. Panel (b) gives the ITTE estimates. Compared to those of panel (a), all estimates decline in magnitude toward zero, and the majority are no longer statistically significant at conventional levels. Effect estimates for outcomes pertaining to economic activity are very close to zero and fairly precise—for example, the 95 per cent confidence interval for the effect of the Biscate nudge on being in paid work is $[-0.04, 0.04]$. The only outcomes that remain marginally significant, but nonetheless much smaller than the naïve estimates, relate to job search (a small positive effect of the Biscate nudge) and satisfaction (negative for both nudges).

The remaining panels of Table 3 report different estimators for the CATE. In panel (c) we employ a structural equation model, estimated via maximum likelihood; panel (d) is a conventional 2SLS estimator; and panel (e) augments the latter with individual-specific fixed effects.[7] As expected given uptake was imperfect, the magnitudes of these estimates are consistently larger than those of the ITTE, but standard errors are also an order of magnitude larger, especially in panel (e). Thus, the point estimates generally remain indistinguishable from zero at conventional significance levels. Exceptions here include evidence of a positive impact of Biscate on job search and a negative impact of both platforms on life satisfaction; and, in the individual fixed-effects estimates, we also observe a small positive effect of the Biscate nudge on reservation wages. Adjustments to correct for multiple hypothesis testing naturally would further weaken these results. So, for the average TVET graduate we have little confidence that digital platform usage directly led to systematic differences in labour market outcomes.

[7] A key difference between estimates in panels (c) and (d) is that the first-stage regressions of the 2SLS estimator include *both* nudges, while in (c) the first stages only include the nudge specific to the given usage variable.

Table 3: Analysis of treatment effects (core specification)

Outcome →	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Naïve (as-treated) effect:									
Emprego user	0.08**	0.10***	0.37**	0.07***	0.00	0.53***	0.33***	0.72***	0.00
	(0.04)	(0.03)	(0.15)	(0.02)	(0.01)	(0.19)	(0.02)	(0.10)	(0.04)
Biscate user	0.02	0.07*	0.32**	0.02	—0.03***	0.52***	0.24***	0.70***	–0.11***
	(0.04)	(0.04)	(0.14)	(0.02)	(0.01)	(0.19)	(0.03)	(0.11)	(0.03)
R^2 adj.	0.10	0.10	0.12	0.12	0.15	0.13	0.07	0.08	0.04
(b) Intent-to-treat effect:									
Emprego SMS	–0.01	–0.01	0.01	0.00	0.00	0.00	0.03*	0.00	–0.05***
	(0.02)	(0.02)	(0.07)	(0.01)	(0.00)	(0.09)	(0.02)	(0.05)	(0.02)
Biscate SMS	–0.01	0.00	0.03	0.00	–0.00	0.05	0.04**	0.12**	–0.03**
	(0.02)	(0.02)	(0.08)	(0.01)	(0.00)	(0.09)	(0.02)	(0.05)	(0.02)
R^2 adj.	0.10	0.09	0.12	0.12	0.15	0.13	0.04	0.06	0.04
(c) Complier average treatment effect (SEM):									
Emprego user	–0.26	–0.18	–0.17	0.02	0.04	–0.56	0.20	–0.69	–0.72**
	(0.31)	(0.30)	(1.21)	(0.15)	(0.05)	(1.47)	(0.28)	(0.83)	(0.32)
Biscate user	–0.07	0.00	0.08	0.01	–0.01	0.17	0.16**	0.45*	–0.14**
	(0.08)	(0.07)	(0.35)	(0.04)	(0.01)	(0.40)	(0.07)	(0.24)	(0.07)
(d) Complier average treatment effect (2SLS):									
Emprego user	–0.13	–0.09	0.12	0.04	0.02	–0.10	0.30	–0.29	–0.60**
	(0.24)	(0.24)	(0.98)	(0.12)	(0.04)	(1.17)	(0.23)	(0.71)	(0.26)
Biscate user	–0.06	0.01	0.10	0.01	–0.01	0.20	0.17**	0.47**	–0.14**
	(0.08)	(0.07)	(0.33)	(0.04)	(0.01)	(0.37)	(0.07)	(0.22)	(0.07)
R^2 adj.	–0.00	0.01	0.01	0.01	0.06	0.01	0.05	0.04	–0.05
(e) Complier average treatment effect (IV-FE):									
Emprego user	–0.06	–0.17	–0.62	0.13	0.00	–0.77	0.19	–1.31	–0.40
	(0.53)	(0.46)	(2.05)	(0.23)	(0.07)	(2.24)	(0.37)	(1.26)	(0.42)
Biscate user	–0.02	–0.01	0.02	0.04	0.04*	0.35	0.04	–0.47	–0.32**
	(0.12)	(0.11)	(0.53)	(0.07)	(0.02)	(0.67)	(0.12)	(0.50)	(0.12)

Note: significance: * 10 per cent, ** 5 per cent, *** 1 per cent. N = 5,327. Dependent variables (in columns) follow Table 1(c): (1) being in work; (2) being in paid work; (3) hours worked (IHS transform); (4) job quality index; (5) reservation wage (IHS transform); (6) salary income (IHS transform); (7) looking for work; (8) hours searching; (9) satisfied with life. Panels refer to alternative models and estimators: (a) is a naïve (per-treatment) model; (b) follows equation (2); (c)–(e) are complier average treatment effects, based on simultaneous equation (maximum likelihood), 2SLS, and IV-FE estimators respectively. Baseline control variables and round-by-month fixed effects are included in all models (not shown). Standard errors (in parentheses) are clustered by unique baseline survey session and round.

Source: authors' estimates.

To verify the robustness of these results we extend our baseline analysis in four directions. First, rather than pooling all follow-up survey rounds, we separately compare each of the three post-treatment rounds to the first (pre-treatment) round. As reported in Appendix Tables A2–A4, these do not suggest radical differences, particularly for the headline labour market outcomes. Even so, both nudges stimulate stronger job search in the first post-treatment round (round 2), weakening over time. The negative effect of the platforms on life satisfaction also is stronger in the short-term, and there is an indication of a negative immediate effect of the Biscate nudge on reservation wages, shifting to a positive effect by the fourth round.

Second, returning to the pooled analysis, we augment the specification with additional fixed (baseline) and time-varying covariates. The former include scores on competency and intelligence tests, as well as employment expectations, and the latter are represented by the (self-reported) metrics of the severe COVID-19 impacts. Third, we employ an ANCOVA analysis, adding the observed value of the relevant dependent variable from the pre-intervention period (follow-up round 1) as an additional explanatory variable. These estimates are reported in Appendix Tables A5 and A6, respectively. Again they indicate no material deviations from the earlier estimates.

Finally, we pursue heterogeneity analysis, limiting the sample to specific subgroups, as defined by baseline characteristics; namely: (a) individuals with access to a phone, computer, or the internet (needed to access the platforms); (b) students of manual TVET courses; (c) female students; and (d) female students of manual TVET courses. Table 4 reports the ITTE estimates for each of these four subgroups, based on our core specification. The most distinctive results are for female students, for whom the Biscate nudge was associated with a significant increase in hours worked and job quality (panel (c)). For the smaller group of women with manual qualifications (fewer than one in five of our sample), we also find a positive effect of the Biscate nudge on their raw employment rates. Specifically, the intervention is associated with an 11 percentage point (or approximately 50 per cent) increase in the share reporting to have undertaken paid employment in the last seven days (column 2), and a 65 per cent increase in labour market earnings (column 6). These positive effects are confirmed in the CATE analysis, reported in Table A7.

Table 4: Intent-to-treat effects for subgroups (core specification)

Outcome →	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Participants with access to phone, internet, or computer:									
Emprego SMS	–0.01	–0.00	0.04	0.01	0.00	0.05	0.02	0.04	–0.04*
	(0.02)	(0.02)	(0.09)	(0.01)	(0.00)	(0.10)	(0.02)	(0.06)	(0.02)
Biscate SMS	–0.01	0.00	0.03	0.01	0.00	0.05	0.02	0.15**	–0.03
	(0.02)	(0.02)	(0.10)	(0.01)	(0.00)	(0.11)	(0.02)	(0.06)	(0.02)
R^2 adj.	0.09	0.09	0.11	0.11	0.15	0.13	0.03	0.06	0.04
(b) Students of manual courses:									
Emprego SMS	0.01	0.03	0.10	0.02	0.00	0.15	0.03	–0.03	–0.04
	(0.02)	(0.02)	(0.10)	(0.01)	(0.00)	(0.12)	(0.02)	(0.07)	(0.02)
Biscate SMS	–0.01	0.02	0.05	0.00	0.00	0.14	0.06***	0.14**	–0.06***
	(0.02)	(0.02)	(0.11)	(0.01)	(0.00)	(0.12)	(0.02)	(0.07)	(0.02)
R^2 adj.	0.10	0.09	0.12	0.12	0.11	0.12	0.03	0.05	0.04
(c) Female students:									
Emprego SMS	–0.04	–0.03	–0.02	–0.01	0.00	–0.12	0.02	0.06	–0.08***
	(0.03)	(0.02)	(0.11)	(0.01)	(0.00)	(0.12)	(0.03)	(0.07)	(0.03)
Biscate SMS	0.02	0.02	0.25**	0.03*	0.00	0.21	0.04	0.21***	–0.03
	(0.03)	(0.02)	(0.12)	(0.02)	(0.00)	(0.13)	(0.03)	(0.07)	(0.03)
R^2 adj.	0.03	0.03	0.03	0.03	0.07	0.04	0.03	0.05	0.05
(d) Female students of manual courses:									
Emprego SMS	–0.02	0.01	–0.08	–0.01	–0.01	–0.17	0.01	0.08	–0.12***
	(0.04)	(0.04)	(0.21)	(0.03)	(0.01)	(0.23)	(0.04)	(0.13)	(0.05)
Biscate SMS	0.10**	0.11***	0.49***	0.06**	0.00	0.51**	0.08*	0.33***	–0.08*
	(0.04)	(0.04)	(0.19)	(0.03)	(0.01)	(0.21)	(0.04)	(0.11)	(0.04)
R^2 adj.	0.05	0.05	0.04	0.04	0.06	0.06	0.04	0.05	0.04

Note: significance: * 10 per cent, ** 5 per cent, *** 1 per cent. All panels replicate ITTE estimates as in Table 3(b), but for distinct subgroups defined from baseline characteristics. Panel (a) excludes individuals without access to a phone, computer, or the internet, N = 3,745. Panel (b) refers to students of manual (industrial/construction/agricultural) courses, N = 2,917. Panel (c) excludes all men, N = 2,373. Panel (d) combines the exclusions of (b) and (c), N = 894.
Source: authors' estimates.

6 Conclusion

This paper engaged with a growing literature on how search frictions impede labour market outcomes for youth in developing countries. Complementing previous studies that have mostly considered transport and screening costs in formal labour markets, we studied the role of digital matching platforms for both informal and formal jobs. Focusing on recent graduates from TVET colleges in Mozambique, we embedded a randomized encouragement design within a tracer survey and invited participants to register on one of two platforms: Biscate, a portal to find informal manual freelancers; or Emprego, a conventional website posting formal employment opportunities.

In keeping with a handful of prior studies from Asia, our main finding is that there is no systematic causal effect of the platforms on labour market outcomes on average. That is, neither rates of employment, job quality, nor wage incomes altered as a result of platform usage. This contrasts with strong positive estimates of per-treatment effects, suggesting clear self-selection—individuals with better employment prospects are more likely to use these platforms. However, we find evidence of a reduction in life satisfaction associated with both platforms, possibly driven by higher job search and reservation wages.

Subgroup analysis revealed employment outcomes of female graduates did improve, but only from usage of the platform for informal work (Biscate) and particularly among those with manual TVET qualifications. The implication is that jobs platforms are unlikely to be a general panacea for un(der)employment in low-income Africa, at least where the supply of employment openings is limited. However, in specific market niches where search frictions are particularly high, matching platforms can play a positive role. At the same time, care must be taken to avoid unintended aspirations failures.

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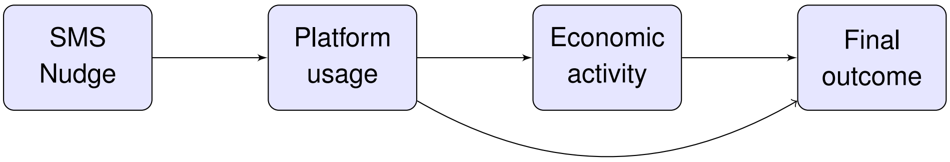
Appendix

A1 Additional figures and tables

Figure A1: Schematic representation of focus relationships



Source: authors' compilation.

Table A1: Eligibility for experiment

	Manual		Services		Total	
	Obs.	%	Obs.	%	Obs.	%
Cannot contact	5	0.5	12	1.7	17	1.0
Duplicate no.	8	0.9	2	0.3	10	0.6
Not on Vodacom	24	2.6	35	4.8	59	3.6
Registered user	88	9.6	32	4.4	120	7.3
Remaining (eligible)	791	86.4	642	88.8	1,433	87.4
Total	916	100.0	723	100.0	1,639	100.0

Note: the cells report partition of baseline sample as per the pre-analysis plan (Jones and Santos 2020).

Source: authors' calculations.

Table A2: Analysis of treatment effects (core specification), rounds 1 and 2 only

Outcome →	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Naïve (as-treated) effect:									
Emprego user	0.11	0.13*	0.62**	0.08**	0.00	0.58	0.40***	0.70***	0.00
	(0.08)	(0.07)	(0.31)	(0.04)	(0.01)	(0.35)	(0.05)	(0.20)	(0.08)
Biscate user	–0.05	0.02	0.17	0.00	–0.04***	0.62*	0.37***	0.83***	–0.07
	(0.06)	(0.05)	(0.28)	(0.03)	(0.01)	(0.32)	(0.05)	(0.19)	(0.07)
R^2 adj.	0.10	0.09	0.13	0.12	0.13	0.12	0.09	0.09	0.04
(b) Intent-to-treat effect:									
Emprego SMS	0.01	0.03	0.04	0.01	–0.00	0.05	0.11***	0.02	–0.05*
	(0.03)	(0.03)	(0.12)	(0.01)	(0.00)	(0.14)	(0.03)	(0.09)	(0.03)
Biscate SMS	–0.01	0.02	0.06	0.01	–0.01**	0.08	0.07**	0.07	–0.06*
	(0.03)	(0.03)	(0.15)	(0.02)	(0.00)	(0.15)	(0.04)	(0.09)	(0.03)
R^2 adj.	0.10	0.09	0.13	0.12	0.13	0.12	0.06	0.08	0.04
(c) Complier average treatment effect (SEM):									
Emprego user	0.12	0.42	0.37	0.06	0.01	0.25	1.18***	–0.38	–0.67
	(0.37)	(0.34)	(1.52)	(0.20)	(0.06)	(1.93)	(0.38)	(1.13)	(0.42)
Biscate user	–0.06	0.10	0.26	0.02	–0.05**	0.29	0.29*	0.21	–0.27*
	(0.15)	(0.13)	(0.71)	(0.09)	(0.02)	(0.70)	(0.17)	(0.41)	(0.15)
(d) Complier average treatment effect (2SLS):									
Emprego user	0.12	0.36	0.33	0.05	0.02	0.48	1.17***	0.07	–0.51
	(0.32)	(0.30)	(1.37)	(0.16)	(0.06)	(1.65)	(0.32)	(1.01)	(0.35)
Biscate user	–0.06	0.08	0.26	0.02	–0.05***	0.32	0.26*	0.30	–0.22*
	(0.13)	(0.12)	(0.66)	(0.08)	(0.02)	(0.63)	(0.15)	(0.38)	(0.13)
R^2 adj.	0.00	–0.01	0.00	–0.00	0.04	–0.00	–0.00	0.06	–0.02
(e) Complier average treatment effect (IV-FE):									
Emprego user	0.19	0.30	–0.52	0.12	0.02	–0.04	1.02***	–1.10	–0.30
	(0.47)	(0.44)	(2.03)	(0.21)	(0.06)	(2.40)	(0.39)	(1.40)	(0.41)
Biscate user	0.01	0.07	0.29	0.06	0.00	0.45	0.12	–0.72	–0.42**
	(0.17)	(0.14)	(0.82)	(0.10)	(0.02)	(0.94)	(0.19)	(0.65)	(0.17)

Note: significance: * 10 per cent, ** 5 per cent, *** 1 per cent. The table replicates Table 3, excluding data from follow-up rounds 3 and 4.
Source: authors' estimates.

Table A3: Analysis of treatment effects (core specification), rounds 1 and 3 only

Outcome →	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Naïve (as-treated) effect:									
Emprego user	0.07	0.10*	0.34	0.07**	–0.00	0.59*	0.32***	0.73***	–0.01
	(0.06)	(0.06)	(0.24)	(0.03)	(0.01)	(0.31)	(0.04)	(0.16)	(0.06)
Biscate user	0.09	0.15**	0.18	0.02	–0.03***	0.64**	0.23***	0.52**	–0.10
	(0.07)	(0.06)	(0.22)	(0.03)	(0.01)	(0.31)	(0.04)	(0.22)	(0.06)
R^2 adj.	0.10	0.10	0.11	0.12	0.18	0.13	0.05	0.06	0.04
(b) Intent-to-treat effect:									
Emprego SMS	–0.00	–0.00	0.05	0.01	0.00	0.03	–0.02	0.01	–0.06
	(0.03)	(0.03)	(0.12)	(0.02)	(0.00)	(0.14)	(0.03)	(0.09)	(0.04)
Biscate SMS	–0.01	0.01	–0.08	–0.00	0.00	0.02	0.04	0.18*	–0.04
	(0.03)	(0.03)	(0.15)	(0.02)	(0.01)	(0.18)	(0.03)	(0.10)	(0.03)
R^2 adj.	0.10	0.10	0.11	0.12	0.18	0.12	0.02	0.05	0.04
(c) Complier average treatment effect (SEM):									
Emprego user	–0.16	–0.23	0.49	0.12	0.06	–0.21	–0.50	–0.21	–0.87
	(0.55)	(0.57)	(2.02)	(0.23)	(0.08)	(2.22)	(0.45)	(1.47)	(0.61)
Biscate user	–0.03	0.04	–0.35	–0.01	0.02	0.05	0.15	0.75*	–0.17
	(0.14)	(0.13)	(0.62)	(0.07)	(0.02)	(0.73)	(0.10)	(0.43)	(0.11)
(d) Complier average treatment effect (2SLS):									
Emprego user	-0.02	-0.10	0.97	0.15	0.01	0.42	-0.34	-0.34	-0.72
	(0.44)	(0.47)	(1.63)	(0.19)	(0.06)	(1.81)	(0.40)	(1.36)	(0.50)
Biscate user	-0.03	0.05	-0.28	-0.01	0.02	0.11	0.14	0.70*	-0.19*
	(0.14)	(0.13)	(0.57)	(0.07)	(0.02)	(0.68)	(0.11)	(0.39)	(0.11)
R^2 adj.	-0.00	0.01	-0.01	-0.00	0.09	0.01	-0.04	0.03	-0.06
(e) Complier average treatment effect (IV-FE):									
Emprego user	0.07	–0.15	0.11	0.22	–0.01	–0.28	–0.37	–1.40	–0.42
	(0.66)	(0.64)	(2.51)	(0.29)	(0.09)	(2.79)	(0.47)	(1.53)	(0.50)
Biscate user	–0.01	0.02	–0.40	0.02	0.06**	0.21	–0.02	–0.30	–0.34**
	(0.14)	(0.13)	(0.60)	(0.07)	(0.02)	(0.75)	(0.13)	(0.58)	(0.14)

Note: significance: * 10 per cent, ** 5 per cent, *** 1 per cent. The table replicates Table 3, excluding data from follow-up rounds 2 and 4.
Source: authors' estimates.

Table A4: Analysis of treatment effects (core specification), rounds 1 and 4 only

Outcome →	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Naïve (as-treated) effect:									
Emprego user	0.05	0.06	0.19	0.05*	0.00	0.42	0.31***	0.99***	–0.02
	(0.05)	(0.05)	(0.20)	(0.03)	(0.01)	(0.27)	(0.03)	(0.14)	(0.05)
Biscate user	0.02	0.04	0.51**	0.04	–0.02**	0.38	0.16***	0.67***	–0.13**
	(0.07)	(0.07)	(0.25)	(0.04)	(0.01)	(0.33)	(0.05)	(0.17)	(0.05)
R^2 adj.	0.11	0.11	0.14	0.13	0.16	0.14	0.05	0.08	0.04
(b) Intent-to-treat effect:									
Emprego SMS	–0.04	–0.05*	–0.03	–0.00	–0.00	–0.08	–0.00	–0.02	–0.04
	(0.03)	(0.03)	(0.14)	(0.02)	(0.00)	(0.17)	(0.03)	(0.08)	(0.03)
Biscate SMS	–0.02	–0.03	0.10	0.01	0.00	0.08	0.02	0.11	–0.00
	(0.03)	(0.03)	(0.13)	(0.02)	(0.01)	(0.16)	(0.03)	(0.09)	(0.03)
R^2 adj.	0.11	0.10	0.13	0.12	0.16	0.13	0.03	0.05	0.04
(c) Complier average treatment effect (SEM):									
Emprego user	–0.93	–1.18	–1.42	–0.12	0.04	–2.49	–0.43	–1.74	–0.64
	(0.85)	(0.86)	(3.28)	(0.43)	(0.10)	(4.21)	(0.74)	(2.00)	(0.65)
Biscate user	–0.10	–0.11	0.34	0.02	0.01	0.29	0.06	0.41	0.00
	(0.13)	(0.12)	(0.51)	(0.07)	(0.02)	(0.63)	(0.11)	(0.38)	(0.11)
(d) Complier average treatment effect (2SLS):									
Emprego user	–0.55	–0.70	–0.82	–0.06	–0.01	–1.67	–0.12	–0.73	–0.66
	(0.55)	(0.55)	(2.23)	(0.29)	(0.07)	(2.91)	(0.49)	(1.37)	(0.52)
Biscate user	–0.10	–0.12	0.32	0.02	0.00	0.23	0.05	0.38	–0.03
	(0.14)	(0.15)	(0.51)	(0.07)	(0.02)	(0.64)	(0.12)	(0.36)	(0.12)
R^2 adj.	–0.06	–0.11	–0.01	–0.00	0.06	–0.02	–0.01	0.01	–0.06
(e) Complier average treatment effect (IV-FE):									
Emprego user	–0.49	–0.78	–1.70	0.01	0.01	–2.06	–0.21	–1.59	–0.45
	(0.78)	(0.63)	(2.99)	(0.34)	(0.10)	(3.00)	(0.58)	(1.58)	(0.63)
Biscate user	–0.08	–0.13	0.16	0.04	0.05**	0.37	–0.07	–0.53	–0.19
	(0.15)	(0.15)	(0.57)	(0.07)	(0.02)	(0.79)	(0.14)	(0.57)	(0.15)

Note: significance: * 10 per cent, ** 5 per cent, *** 1 per cent. The table replicates Table 3, excluding data from follow-up rounds 2 and 3.
Source: authors' estimates.

Table A5: Analysis of treatment effects (core specification with additional controls)

Outcome →	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Naïve (as-treated) effect:									
Emprego user	0.07*	0.09**	0.34**	0.07***	–0.00	0.47**	0.33***	0.70***	0.01
	(0.04)	(0.04)	(0.15)	(0.02)	(0.01)	(0.19)	(0.02)	(0.09)	(0.04)
Biscate user	0.02	0.05	0.27*	0.02	–0.03***	0.47**	0.23***	0.65***	–0.09***
	(0.04)	(0.04)	(0.14)	(0.02)	(0.01)	(0.19)	(0.03)	(0.12)	(0.03)
R^2 adj.	0.11	0.11	0.13	0.13	0.16	0.14	0.08	0.08	0.06
(b) Intent-to-treat effect:									
Emprego SMS	–0.01	–0.00	0.02	0.00	0.00	0.00	0.03	–0.02	–0.05**
	(0.02)	(0.02)	(0.07)	(0.01)	(0.00)	(0.09)	(0.02)	(0.05)	(0.02)
Biscate SMS	–0.02	–0.01	–0.00	0.00	–0.00	0.01	0.03*	0.08	–0.03*
	(0.02)	(0.02)	(0.08)	(0.01)	(0.00)	(0.09)	(0.02)	(0.06)	(0.02)
R^2 adj.	0.11	0.10	0.13	0.13	0.16	0.14	0.05	0.07	0.06
(c) Complier average treatment effect (SEM):									
Emprego user	–0.24	–0.16	–0.11	0.03	0.04	–0.56	0.16	–0.99	–0.67**
	(0.31)	(0.29)	(1.20)	(0.15)	(0.05)	(1.46)	(0.28)	(0.82)	(0.31)
Biscate user	–0.09	–0.03	–0.04	0.00	–0.01	0.00	0.11	0.31	–0.11
	(0.08)	(0.08)	(0.35)	(0.04)	(0.01)	(0.40)	(0.07)	(0.24)	(0.07)
(d) Complier average treatment effect (2SLS):									
Emprego user	–0.10	–0.05	0.24	0.05	0.01	0.00	0.28	–0.48	–0.57**
	(0.24)	(0.23)	(0.99)	(0.12)	(0.04)	(1.19)	(0.23)	(0.71)	(0.26)
Biscate user	–0.09	–0.02	–0.02	0.00	–0.01	0.04	0.12*	0.32	–0.12*
	(0.08)	(0.07)	(0.33)	(0.04)	(0.01)	(0.38)	(0.07)	(0.22)	(0.07)
R^2 adj.	0.01	0.02	0.02	0.02	0.07	0.02	0.06	0.04	–0.03
(e) Complier average treatment effect (IV-FE):									
Emprego user	–0.01	–0.15	–0.49	0.15	0.01	–0.65	0.19	–1.28	–0.38
	(0.53)	(0.46)	(2.07)	(0.23)	(0.07)	(2.27)	(0.37)	(1.27)	(0.42)
Biscate user	–0.02	–0.02	0.02	0.04	0.04**	0.33	0.04	–0.49	–0.31**
	(0.12)	(0.11)	(0.54)	(0.07)	(0.02)	(0.68)	(0.12)	(0.50)	(0.13)

Note: significance: * 10 per cent, ** 5 per cent, *** 1 per cent. The table replicates Table 3, adding additional baseline and time-varying control variables.

Source: authors' estimates.

Table A6: Analysis of treatment effects (core specification with lagged outcome)

Outcome →	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Naïve (as-treated) effect:									
Emprego user	0.04	0.06	0.18	0.05**	–0.01	0.39**	0.26***	0.36***	0.01
	(0.04)	(0.04)	(0.16)	(0.02)	(0.00)	(0.18)	(0.02)	(0.09)	(0.03)
Biscate user	0.04	0.06*	0.29**	0.03*	–0.02***	0.48***	0.20***	0.49***	–0.07**
	(0.04)	(0.03)	(0.13)	(0.02)	(0.01)	(0.18)	(0.03)	(0.11)	(0.04)
R^2 adj.	0.23	0.24	0.27	0.28	0.36	0.30	0.21	0.27	0.31
(b) Intent-to-treat effect:									
Emprego SMS	–0.01	–0.01	–0.01	0.01	0.00	–0.01	0.02	–0.04	–0.04**
	(0.02)	(0.02)	(0.07)	(0.01)	(0.00)	(0.09)	(0.02)	(0.05)	(0.02)
Biscate SMS	–0.01	–0.00	0.02	0.01	0.00	0.06	0.03	0.01	–0.06***
	(0.02)	(0.02)	(0.08)	(0.01)	(0.00)	(0.09)	(0.02)	(0.06)	(0.02)
R^2 adj.	0.23	0.24	0.27	0.28	0.36	0.30	0.19	0.26	0.31
(c) Complier average treatment effect (SEM):									
Emprego user	–0.22	–0.20	–0.57	0.07	0.06	–0.65	0.16	–1.30	–0.69**
	(0.32)	(0.30)	(1.22)	(0.15)	(0.05)	(1.44)	(0.28)	(0.79)	(0.31)
Biscate user	–0.05	–0.02	0.06	0.03	0.02	0.21	0.10	0.01	–0.25***
	(0.08)	(0.07)	(0.34)	(0.04)	(0.01)	(0.38)	(0.08)	(0.26)	(0.08)
(d) Complier average treatment effect (2SLS):									
Emprego user	–0.11	–0.10	–0.27	0.07	0.02	–0.27	0.26	–0.67	–0.43*
	(0.25)	(0.24)	(0.99)	(0.12)	(0.04)	(1.14)	(0.22)	(0.68)	(0.24)
Biscate user	–0.04	–0.01	0.08	0.03	0.02	0.24	0.11	0.04	–0.24***
	(0.07)	(0.07)	(0.31)	(0.04)	(0.01)	(0.35)	(0.07)	(0.24)	(0.07)
R^2 adj.	0.15	0.16	0.17	0.19	0.29	0.21	0.19	0.23	0.25
(e) Complier average treatment effect (IV-FE):									
Emprego user	–0.06	–0.17	–0.62	0.13	0.00	–0.77	0.19	–1.31	–0.40
	(0.53)	(0.46)	(2.05)	(0.23)	(0.07)	(2.24)	(0.37)	(1.26)	(0.42)
Biscate user	–0.02	–0.01	0.02	0.04	0.04*	0.35	0.04	–0.47	–0.32**
	(0.12)	(0.11)	(0.53)	(0.07)	(0.02)	(0.67)	(0.12)	(0.50)	(0.12)

Note: significance: * 10 per cent, ** 5 per cent, *** 1 per cent. The table replicates Table 3, adding outcome observed in the first follow-up round to the specification.
Source: authors' estimates.

Table A7: Analysis of complier-average treatments effects for subgroups (core specification)

Outcome →	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(a) Participants with access to phone, internet, or computer:									
Emprego user	–0.16	–0.09	0.20	0.10	0.08	0.20	–0.03	–0.49	–0.43
	(0.32)	(0.28)	(1.29)	(0.17)	(0.05)	(1.57)	(0.32)	(0.93)	(0.33)
Biscate user	–0.05	–0.00	0.08	0.05	0.01	0.20	0.03	0.55*	–0.10
	(0.10)	(0.09)	(0.44)	(0.05)	(0.02)	(0.50)	(0.10)	(0.29)	(0.09)
(b) Students of manual courses:									
Emprego user	0.04	0.30	0.98	0.19	0.07	1.48	0.13	–1.08	–0.44
	(0.33)	(0.32)	(1.32)	(0.16)	(0.05)	(1.65)	(0.30)	(1.03)	(0.33)
Biscate user	–0.05	0.08	0.14	0.01	0.00	0.48	0.21**	0.49*	–0.22***
	(0.09)	(0.09)	(0.42)	(0.05)	(0.02)	(0.47)	(0.09)	(0.28)	(0.08)
(c) Female students:									
Emprego user	–1.53	–1.08	–1.01	–0.24	0.16	–4.92	0.08	1.15	–2.97**
	(1.09)	(1.00)	(4.21)	(0.54)	(0.16)	(5.14)	(0.94)	(2.48)	(1.43)
Biscate user	0.11	0.12	1.31**	0.15**	0.01	1.11*	0.20	1.10***	–0.16
	(0.13)	(0.12)	(0.58)	(0.08)	(0.02)	(0.66)	(0.14)	(0.36)	(0.14)
(d) Female students of manual courses:									
Emprego user	–0.77	0.39	–3.24	–0.36	–0.21	–5.98	–0.35	1.46	–4.03
	(1.48)	(1.29)	(6.97)	(0.98)	(0.25)	(8.28)	(1.34)	(4.14)	(3.19)
Biscate user	0.46**	0.50***	2.21***	0.29***	0.01	2.29***	0.33*	1.49***	–0.38*
	(0.19)	(0.16)	(0.79)	(0.11)	(0.03)	(0.88)	(0.20)	(0.48)	(0.20)

Note: significance: * 10 per cent, ** 5 per cent, *** 1 per cent. The table replicates Table 4, showing results for the SEM complier average treatment effects estimator.
Source: authors' estimates.

B1 Digital platform examples

MZ Candidato Recrutador PT / EN

Pub Vaga Detalhes

voltar atrás

Technician (m/f)

Eazi Access Rental

Eazi Access Rental is recruiting a Technician (m/f), to be based in Matola, Mozambique.

Description

Job purpose: Carry out repairs, maintenance on EAZI Quip Africa Assets, the employee must also be prepared to carry out overtime duties. The artisan is requested to perform his/her duties in a professional and safe manner

Duties

Availability/Productivity: Delivering on time as per expected standards or requirements

Quality of work: Producing work correctly the 1st time

Cost Saving

Repair assets in cost effective manner to ensure in-house repairs stay cost effective and reduce turn-around time

Administrative accuracy and timing

Ensuring administrative duties are up to date and accurately completed

Health & Safety

Ensuring to comply with all health and safety requirements relevant to the department

Ability to stop a machine if unsafe and to prevent unsafe actions

Productivity

Repair and maintain assets as per the standard times set as benchmark in the department

Communicate and attain approval for all time spent in excess of benchmarks

Preventative maintenance is to be carried out to prevent breakdowns

Identify and log all snags. Should the snag be critical the unit must be reported and operation stopped by ensuring the status does not turn to green

Quality of work

Pre and post inspections on Eazi access equipment to be completed each time the unit is checked.

Quality of work must ensure no returns on work performed within 6

Candidata-te a esta vaga

Entidade	Eazi Access Rental
Local	Matola
Categoria	Técnico
Publicado	04.02.2020
Expira	29.02.2020

Partilhar vaga por email

Reportar erro

Traduzir para Português

Perguntas Frequentes

Como posso candidatar-me a vagas através do emprego.co.mz?

Ler artigo

Pesquisa Conta Ajuda

Canalização > Maputo > Marracuene

Orlando Mole

Canalização

Marracuene

Experiência
4-6 anos

Educação
-

33 Contactos

18 x Disponível
0 x Indisponível
7 x Não atendeu
4 x Desactivado

1 Avaliações

★★★★★ Preço
★★★★★ Qualidade
★★★★★ Tempo

6 Comentários

Anónimo 01.07.2017
O Orlando é um profissional incrível, estou muito satisfeito com o resultado do trabalho.

1 14.07.2018
ola

1 03.06.2018
SIM

Nelson Fernando 15.02.2019
gostei

Picoco Picoco 24.03.2019
Bom trabalho e excelente postura

Anónimo 08.01.2020
Bom trabalho!

Contactar

Acede ou cria uma conta no Biscate para obter os detalhes do trabalhador

exclusivo para clientes **Vodacom**

Acesso

Termos e Condições Política de Privacidade Sobre Perguntas Frequentes

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