LinkedIn(to) Job Opportunities: Experimental Evidence from Job Readiness Training*

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Abstract

Online professional networking platforms are widely used and offer the prospect of alleviating labor market frictions. We run the first randomized evaluation of training workseekers to join and use one of these platforms. Training increases the end-of-program employment rate by 10% (7 percentage points), and this effect persists for at least twelve months. The available employment, platform use, and job search data suggest that employment effects are explained by workseekers using the platform to acquire information about prospective employers, and perhaps by workseekers accessing referrals and conveying information to prospective employers on the platform.

JEL codes: J22, J23, J24, J64, M51, O15

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1 Introduction

Youths in many countries face substantially higher rates of unemployment, underemployment, and unstable employment than older cohorts (International Labour Organization, 2017). These patterns are consistent with many economic explanations, including growing evidence that labor market information frictions impede transitions into employment (Caria and Lessing, 2019). Information frictions may be particularly important for young workseekers, who may lack references from past employers, lack access to referral networks, or lack experience with job search. Even if these frictions only delay transitions into employment, temporary distortions can have long-term labor market consequences in both developed and developing countries (Kahn, 2010; Oreopolous et al., 2012; Kuchibhotla et al., 2017). And, while information frictions alone may explain a small share of youth unemployment, they may be easier and quicker to address than factors such as aggregate skills mismatches.

Online platforms for job search, networking, and hiring may reduce information frictions. They may increase supply-side access to information about labor markets and specific firms, increase demand-side access to information about workers through public profiles, facilitate demand- and supply-side network connections that can share information and referrals, and lower pecuniary costs of job posts and applications. They have become an increasingly important feature of many labor markets (Agrawal et al., 2015). However, there is little evidence about the causal effect of using these platforms on employment outcomes.

We run the first randomized evaluation of training workseekers to join and use LinkedIn, the world's largest online professional networking platform. We work with participants in existing job readiness programs in large South African cities. The programs help young, disadvantaged workseekers prepare and apply for jobs in growing sectors, primarily call centers. We randomly assign some participants to four hours of LinkedIn training during their program. LinkedIn is widely used in South Africa, with 264,000 active job postings and 7.1 million active profiles (roughly 40% of the workforce). We train participants to open accounts, build their profiles, advertize skills they acquired during job readiness training, get public recommendations from their training manager, make connections, and search and apply for jobs. We measure participants' employment with independent survey data and their platform use with LinkedIn administrative data at the end of the job readiness program and six and twelve months later.

We find two main results. First, treatment substantially and persistently increases employment. The end-of-program employment rates in the treatment and control groups are respectively 77 and 70%. Employment increases because treated participants are more likely to convert applications submitted as part of the job readiness program into job offers, not because treatment changes job search outside the program. The employment effect persists for at least twelve months after treatment. The mean earnings gain per treated participant during those twelve months is 8 to 23 times higher than the per-participant intervention cost.

Second, treatment substantially increases the probability of having a LinkedIn account and multiple measures of platform use. The experiment alone does not identify causal relationships between post-treatment employment and LinkedIn usage outcomes without additional assumptions. But several patterns suggest that the treatment-induced changes in LinkedIn usage explain the treatment-induced changes in employment.

Our experiment is not designed to identify the economic mechanisms through which LinkedIn use increases employment. But we evaluate six potential mechanisms and find partial evidence supporting three of them. Treated workseekers view more on-platform job advertisements and view more profiles of other users, suggesting that treatment addresses *supply-side information frictions* by allowing workseekers to learn more about prospective jobs and employers. Treated workseekers have more informative profiles that are viewed more often by other users, suggesting that treatment might address *demand-side information frictions* by allowing prospective employers to learn more about prospective workers. But, in informal interviews, several firms who hire workseekers in our sample do not report using LinkedIn profiles in hiring decisions. Treated workseekers are connected to more people on LinkedIn, particularly people with more education and more management-level jobs, consistent with a role for *on-platform referral networks*. But they do not form more connections at their new employer before starting work there, so the timeline of events does not strongly support a role for on-platform referrals. Our results are not consistent with three other mechanisms. Treated participants rarely use LinkedIn's on-platform job application function, suggesting that *lower job search cost* is not an important mechanism.[1] Treatment does not change workseekers' *engagement with the existing job readiness programs*, nor does it change their *self-beliefs*.

We draw on the extensive literature on active labor market programs (ALMPs) to help interpret the economic mechanisms through which LinkedIn use may increase employment. In the context of this literature, we emphasize three features of the intervention we study. First, this intervention targets young, disadvantaged participants: they are from low-income households, few have university education or substantial work experience, and they are searching for work in an economy with high unemployment. Several metastudies

[1]Consistent with this result, Banerjee and Sequiera (2020) find that transport subsidies for a similar population of workseekers in South Africa have no effect on employment. In contrast, several studies in urban Ethiopia find that job search or job application subsidies at least temporarily shift both job search and employment (Abebe et al., 2016, 2019; Franklin, 2017).

show that employment effects of ALMPs are on average larger for disadvantaged participants (Card et al., 2017; McCall et al., 2016). Other metastudies show that ALMPs aimed at young participants are relatively effective at raising participants' employment in developing countries, though less so in developed countries (Card et al., 2010; Escudero et al., 2019; Kluve et al., 2019; Stöterau, 2020). These patterns suggest that the large employment effect of LinkedIn training partly reflects the intervention's focus on young, disadvantaged workseekers. However, the metastudies provide limited evidence about why ALMPs are relatively effective for these populations. To better understand this, we turn to the second feature of the intervention.

Second, we study a job search assistance intervention designed to reduce search frictions. Job search assistance programs are a common form of ALMP and include interventions such as search advice and search subsidies. Although there is mixed evidence on the effectiveness of job search assistance programs in general (e.g., Card et al. 2010, 2017; Kluve et al. 2019), recent reviews show particularly promising effects for job search assistance programs targeted specifically at information frictions (Caria and Lessing, 2019; J-PAL, 2018). One set of studies shows that search behavior and employment can change as workseekers acquire more information about specific vacancies or aggregate labor market conditions (Altmann et al., 2018; Ahn et al., 2019; Beam, 2016; Belot et al., 2019; Jensen, 2012; Subramanian, 2020). This literature is consistent with the supply-side information mechanism we discuss above. Another set of studies shows search behavior and sometimes employment change when workseekers acquire more information about their skills or past job performance that they can share with firms (Abel et al., 2019; Abebe et al., 2016; Bassi and Nansamba, 2017; Carranza et al., 2020; Pallais, 2014). This may reflect worker- and/or firm-side learning, aligning with the supply- and demand-side information mechanisms we discuss above.

The intersection of the these two features – targeting young disadvantaged workseekers and addressing information frictions – may be particularly important. Abebe et al. (2016) and Carranza et al. (2020) show that information interventions have larger employment effects for more disadvantaged workseekers. This is consistent with evidence that firms infer negative signals about workseekers' skills from long periods of unemployment (Eriksson and Rooth, 2014; Kroft et al., 2013). Information frictions may be particularly important for the types of workseekers in our sample: young Black South Africans from disadvantaged backgrounds. Malindi (2017) and Pugatch (2019) show these types of workseekers leave school with limited information about their employment prospects and then face statistical discrimination in the labor market.

Third, both treated and control participants are already enrolled in a program that combines elements of job readiness training and vocational training. The existing training program is designed to help participants prepare for jobs in specific sectors that are growing rapidly in this context, particularly call centers. The additional LinkedIn training can help participants communicate their participation in the program and the skills they have acquired as well as help participants gather more information about work in those industries. Other research shows that combining interventions in this way may be important. Alfonsi et al. (2017) show that vocational training by external providers has larger long-term employment effects than on-the-job training because the former provides certification that workers can use in future job search. Kluve et al. (2019) show that ALMPs that target multiple frictions simultaneously are on average more effective than ALMPs that target one friction.[2]

In sum, existing research on ALMPs suggests that programs focused on addressing information-based search frictions facing young, disadvantaged workseekers may be particularly effective. We contribute to this literature by providing the first evidence that digital professional networking training aimed at this population can substantially increase participants' employment rates. We find that digital professional networking helps participants already enrolled in full-time job readiness training, suggesting that information-based search frictions can bind even for graduates of traditional ALMPs.

In Section 2, we describe the LinkedIn platform, intervention, economic context, sample, data collection, and research design. We present the main empirical findings in the next three sections, showing that treatment increases LinkedIn usage in Section 3, that treatment increases employment in Section 4, and that the former treatment effect may explain the latter in Section 5. In Section 6 we evaluate the economic mechanisms through which LinkedIn usage can increase employment. We conclude in Section 7 and discuss what our results might imply for the economic effects of digital professional networking in other settings. We study a small increase in the number of users, of a large and established network, all of whom also receive job readiness training. Results may be different for large increases in network usage, for a new network, or without the general job readiness training. Our experiment is not designed to answer these questions, so our discussion is necessarily speculative. We can, however, show that treated workseekers in our experiment are unlikely to displace control workseekers competing for the same jobs. Appendices A - E present sensitivity analyses for key treatment effects, treatment effects on additional outcomes, the relationship between our pre-analysis plan and final paper, benefit-cost calculations, and the training curriculum.

[2]The combination of Harambee's training program and the LinkedIn training course has some elements in common with US-based "sectoral programs." These programs provide intensive training and certification, typically through community colleges, aimed at fast-growing sectors for disadvantaged participants who pass screening tests. See Fein and Hamadryk (2018) and the citations therein for detailed discussion.

2 Economic Environment

2.1 The LinkedIn Platform and Training Course

The intervention trains participants in existing job readiness training programs to open and use LinkedIn accounts. LinkedIn is a social media site geared toward professional networking and development. Users can create public profiles on the site with information about their educational and employment history, skills, and certifications. Profiles may also contain public recommendations written by supervisors or colleagues. Users engage with the platform in four main ways. They can connect with other users and join groups (https://www.linkedin.com/people/search), search and apply for jobs (https://www.linkedin.com/jobs), read articles written or shared by other users, and complete online training (https://www.linkedin.com/learning). Employers can create accounts and use the platform to post vacancies (https://www.linkedin.com/talent/post-a-job), solicit applications, and screen applicants based on user profiles.

The existing job readiness programs are run by the Harambee Youth Employment Accelerator, a social enterprise that builds solutions to address a mismatch of demand and supply in the South African youth labor market by connecting employers with inexperienced workseekers. These are full-time programs lasting six to eight weeks and covering workplace simulations, team building, and non-cognitive skill development. Harambee helps candidates submit job applications during the programs, including to jobs at firms where Harambee has long-term, actively managed relationships. Harambee's role in hiring ends at helping with applications and, at some firms, setting up interviews. Many other active labor market programs offer similar job application support.

Each job readiness program is aimed at a specific sector. Of the 30 cohorts we study, 27 are aimed at call center jobs and three for face-to-face sales jobs in the insurance sector.[3] The 27 call center cohorts are trained to work both in business process outsourcing firms and in banks and insurers with in-house call centers, for both inbound customer service roles and outbound sales roles. Mean annual earnings for entry-level call center roles in South Africa in this period were roughly USD16,000 before benefits, similar to mean annual earnings for all employed workers.[4] Interviews with program staff at Harambee suggest that most participants who get jobs at the end of the programs do so in the target sectors.

We work with 30 cohorts trained by Harambee between May 2016 and January 2018 in four large cities

[3] All treatment effects are robust to dropping participants from the 3 face-to-face sales cohorts.

[4] The USD16,000 figure is calculated from the nationally representative Quarterly Labour Force Surveys for 2017-2019, using only call center workers in urban areas with less than three years work experience. Industry association reports based on firm surveys report slightly higher mean annual earnings. See Appendix E.2 for detailed calculations.

in South Africa (Cape Town, Durban/eThekwini, Johannesburg, and Pretoria/Tshwane). We split the sample into 15 control and 15 treated cohorts. Control cohorts received Harambee's standard job readiness program. Treated cohorts received a general 'introduction to LinkedIn' presentation in their first week and in subsequent weeks received in-person coaching, discussion sessions, and emails with advice and encouragement. The initial presentation and subsequent sessions explained how to open an account, construct a profile, join groups, make connections, view profiles of prospective employers, and ask for recommendations. Participants were encouraged to list the job readiness program on their profile, get a recommendation from their program manager, and connect with program alumni. The intervention curriculum was jointly developed by Harambee and the research team. Appendix E.3 shows the guide used to train program managers to deliver the curriculum. Program managers could choose what existing content to drop or adapt to accommodate the LinkedIn training. We do not observe these decisions. Treatment effects on employment are similar when we condition on program manager fixed effects.

Treatment displaced roughly four hours of Harambee's standard job readiness program over six to eight weeks and cost roughly USD48 PPP per candidate. Appendix E.2 contains detailed cost calculations.

2.2 Context and Sample

We work with a sample of young, disadvantaged workseekers in four large South African cities. Youth unemployment was 39-43% in these cities at the time.[5] South Africa's high unemployment has been attributed to factors such as slow economic growth, apartheid-era restrictions on informal firms and land seizures that constrained smallholder agriculture, a weak education system, labor market regulation, and spatial segregation that separates workers from jobs (Banerjee et al., 2008). In this context, transitions into employment are difficult for young workseekers. Weak education leads to a low correlation of measured skills with grade progression and hence years of education, limiting their signal value (Lam et al., 2011; Taylor et al., 2011). Hiring and firing are tightly regulated, firms report difficulty understanding regulations, and legal disputes over hiring are common (Bhorat and Cheadle, 2009; Rankin et al., 2012; Bertrand and Crépon, 2019). Faced with downside risks of bad hires and noisy signals of young workers' productivity, firms disproportionately hire experienced workers or hire through referrals (Magruder, 2010; Rankin and Roberts, 2011). These obstacles contribute to very low aggregate job entry rates (Donovan et al., 2012). Unemployment is particularly high for young Black people, who face statistical discrimination in the labour market (Malindi, 2017).

[5]Authors' calculations from data in Statistics South Africa (2017). Employment rates are for ages 18-29 in the provinces containing the four study cities, conditional on completing high school and classifying discouraged workseekers as unemployed.

Table 1: Sample Characteristics

Variable	N	Mean	Std Dev	10th ptile	90th ptile	p-value	Std Diff
Age	1636	23.7	3.0	19.9	27.7	0.11	-0.16
Numeracy score	1547	-0.03	1	-1.48	1.32	0.59	-0.06
Communications score	1610	0.08	0.96	-1.03	1.18	0.05	0.15
Cognitive score	1617	0.04	0.98	-1.32	1.66	0.52	0.07
Female	1633	0.61	0.49			0.49	-0.05
High school education	1500	0.99	0.08			0.39	0.06
Post-secondary education	1500	0.38	0.48			0.49	-0.07
University education	1500	0.06	0.24			0.17	-0.1
Previously employed	1571	0.38	0.49			0.47	-0.05
Size of cohort	30	55	25	31	99	0.32	0.37
Program completion rate	30	0.86	0.13	0.71	1	0.53	-0.23

Table shows summary statistics for the sample of 1,638 workseekers. Assessment scores are standardized to have mean zero and standard deviation one in the control group. The cognitive test administered by Harambee is similar to a Raven's test. p-values are based on regressions that include stratification block fixed effects and heteroskedasticity-robust standard errors clustered by cohort. Standardized mean differences reported in the final column are the differences between treatment and control group means divided by the sample standard deviation. The program completion rate refers to the share of participants completing the job readiness program, not the LinkedIn training.

We study 1638 workseekers from the 30 experimental cohorts, described in Table 1.[6] All candidates applied for Harambee's programs, so all were active workseekers. Harambee only accepts candidates from 'disadvantaged backgrounds.' Their definition is complex but, in practice, this excludes candidates from middle- and upper-income households. Only 6% of the sample have university education and 62% have no work experience. Candidates are negatively selected on employment prospects relative to the general population. However, candidates are only eligible for job readiness programs if they perform well on Harambee's cognitive, communication, and numeracy skill assessments, generating positive selection conditional on other correlates of employment prospects.

Online professional networking may be important in this setting and for this sample. Workseekers without work experience or successful job search experience can use online platforms to search for information about specific vacancies and the general labor market. Firms can use information on public profiles as a partial substitute for signals from work experience or university education. On-platform connections can provide referrals, which are commonly used in off-platform hiring. On-platform job applications will be cheaper than in-person applications given South Africa's spatial segregation and transport costs (Kerr, 2017).

[6]The randomization successfully balanced treatment and control candidates. The means of candidate-level characteristics differ by at most 0.16 standard deviations. The means of cohort-level characteristics have slightly larger standardized differences. No mean differences are statistically significantly different to zero.

LinkedIn is a relevant recruiting medium for workseekers with these types of backgrounds in South Africa. In November 2019, several months after the study period, 12 of the 20 largest employers of workseekers in our sample had active job vacancies posted on LinkedIn. All 12 had posted at least one job that required only high school education and multiple jobs that did not require university education. These vacancies covered insurance claim processors at insurance firms, call center agents at business process outsourcing firms, and customer service representatives at banks. LinkedIn may be more important for a higher-skilled part of the labor market, but it is also relevant for the types of workseekers we study.

These features of online professional networking are likely to be important in other settings as well. Distortions due to limited information and search costs have been documented in developed- and developing-country labor markets (see citations in the introduction). Education-skill relationships are noisy in many developing countries (Pritchett, 2013). Many labor markets face more regulations governing hiring, firing, and probation than in South Africa (Botero et al., 2004). While South Africa has the highest youth unemployment rate in the world, 23 other countries across Africa, Latin America, the Middle East and Southern Europe also have youth unemployment rates above 30% (World Bank, 2018).

2.3 Measurement

We combine four rounds of survey data with data on platform usage from LinkedIn and administrative data from Harambee. First, we conduct a baseline survey at the beginning of each job readiness program, before starting LinkedIn training. This measures participants' demographics, education, and prior work experience. We match these data to scores on Harambee's communication, numeracy, and general cognitive assessments.[7] Second, we conduct a second survey at the end of the job readiness training to measure participants' self-beliefs and engagement with the program. We match this to Harambee's administrative data on end-of-program employment, program completion, and program performance.

Third, we conduct phone surveys six and twelve months after the job readiness program.[8] These surveys measure participants' employment, job characteristics, and self-beliefs. There is some non-response, which is balanced across treatment and control cohorts and weakly related to baseline covariates. Our main findings are robust to accounting for non-response (Appendix A).

Fourth, we match participants to LinkedIn administrative data using email addresses and names. These

[7] The communication assessment covers verbal and written English comprehension. The numeracy assessment covers high school arithmetic. The general cognitive assessment is similar to a Raven's matrix test. More information is available at https://www.assessmentreport.info/.

[8] See Garlick et al. (2019) for an experimental validation of phone-based labor market surveys in this setting.

data were exftracted by LinkedIn at the end of the job readiness program and again six and twelve months later. For each participant with an account, the data show the account opening date, profile completeness, number of network connections, attributes of network connections, and frequency and type of site usage.

The data collection design limits the scope for strategic misreporting by data providers. LinkedIn collects outcome data but does not observe treatment assignments. Harambee observes treatment assignments but only provides outcome data at the end of the job readiness program. The phone surveys six and twelve months later are conducted by an independent survey firm, blinded to treatment assignment.

2.4 Research Design

We use a cohort-level randomized controlled trial. We split 30 cohorts into treatment and control groups using within-city, sequentially-paired randomization. Within each of the four cities, we randomly assign treatment/control status to each of cohorts $1, 3, 5, \ldots$ We then assign cohorts $2, 4, 6, \ldots$ to the opposite status. Harambee learned treatment assignments on the first day of each program, too late to change participants or program managers. Program managers obviously knew treatment status while running the programs and could in principle change their behavior to distort the outcome of the evaluation. Individual program managers did not, however, have any direct stake in the outcome of the evaluation. Our results are robust to dropping programs run by the one manager who co-developed the intervention.

We estimate treatment effects using

$$Y_{icr} = T_{cr} \cdot \beta + \mathbf{S_{cr}} + \epsilon_{icr}, \tag{1}$$

where i, c, and r index respectively individual participant, cohort, and city/region. Y, T, and $\mathbf{S}$ denote respectively outcomes, treatment assignment, and stratification block fixed effects. The blocks are based on cohort-pairs defined above and account for regional and temporal variation in outcomes. We estimate heteroskedasticity-robust standard errors, clustered by cohort. We also report wild bootstrap p-values for key outcomes, clustered by cohort. We winsorize right-skewed outcomes at the 95$^{\text{th}}$ percentile, though this does not change results. Five treated cohorts did not fully finish the LinkedIn curriculum. We report intention-to-treat effects throughout the paper and treatment-on-the-treated effects in Appendix B.

The study was preregistered during the baseline data collection and implementation period on the AEA's Trial Registry at https://doi.org/10.1257/rct.1624-9.1. The final intervention, sample definition, randomization, and data collection all follow the preregistration. The analysis in the paper differs from the pre-analysis plan in three small ways. First, we omit prespecified program manager fixed effects, because several program managers managed only one cohort and several cohorts were co-managed. Second, we omit some prespecified outcomes that were either dropped from the follow-up survey instrument to reduce survey length or had little variation due to ceiling effects. Third, we add some non-prespecified outcomes that we collected in response to reviewer feedback. These are marked with a * in the relevant tables. We compare our final and prespecified analysis in more detail in Appendix D.

3 Treatment Increases LinkedIn Use

We begin by showing how the LinkedIn training program changes participants' use of the platform. Treatment increases the share of participants with LinkedIn accounts by 32 percentage points, from a control group mean of 48%. This effect occurs during the job readiness program (Table 2, columns 1-2). This shows high compliance with the first part of the LinkedIn curriculum. Treatment also increases self-reported time spent on LinkedIn during the job readiness program from 0.6 to 1.7 hours per week. LinkedIn training involved only four contact hours over six to eight weeks, not all of which were spent using LinkedIn, so this demonstrates some use outside training.

Treatment also increases an index of LinkedIn usage by 0.94 standard deviations (Table 2, column 3). We construct this index as the first principal component of ten measures of LinkedIn use collected in the twelve months after training such as profile completeness, number of connections, and number of profiles viewed. We discuss these measures in more detail in Section 6 and Appendix C. This shows that treated candidates not only open LinkedIn accounts during training, but also use them after training.

Treatment increases LinkedIn usage both by increasing the share of candidates with accounts and increasing use conditional on having an account. To show this, we adapt a decomposition used by Attanasio et al. (2011) and Carranza et al. (2020). We define the *extensive margin treatment effect on LinkedIn usage* as the treatment effect on the probability of having an account multiplied by mean usage for control group candidates with accounts. The extensive margin is the treatment effect that would occur if treatment were to increase usage only by getting more candidates to open accounts. We define the *intensive margin treatment effect on LinkedIn usage* as the difference between this extensive margin effect and the average treatment effect on usage. The intensive margin is the treatment effect on usage that would occur if treatment were to change how candidates use their accounts but not induce any extra candidates to open accounts. The extensive and intensive margin effects on usage account for respectively 32 and 68% of the average effect

Table 2: Treatment Effects on LinkedIn Account Opening and Usage

	(1) LinkedIn account	(2) Account during training*	(3) LI usage index
Treated cohort	0.314	0.422	0.935
	(0.049)	(0.050)	(0.145)
Share of treatment effect due to			
... more accounts			0.319
... using accounts more			0.682
Control group mean	0.484	0.094	0.000
Control mean|account			0.950
# respondents	1638	1566	1300
# cohorts	30	30	30
Adjusted R2	0.140	0.282	0.187

Coefficients are from regressing a measure of LinkedIn usage on a treatment indicator and stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. 'LinkedIn account' indicates whether the participant had a LinkedIn account at any time from baseline to the 12 month-follow up. 'Account during training' indicates that the account was created during the training program. The LinkedIn usage index is the first principal component of ten measures of LinkedIn usage: having an account, opening an account during training, the number of profiles viewed, the number of jobs viewed, profile completeness, the number of times the profile is viewed, the total number of connections, the number of connections with bachelors degree, the number of connections with managerial jobs, and the number of job applications submitted on LinkedIn. The conditional control group mean is the average value for control respondents conditional on having a LinkedIn account. Starred outcomes are not prespecified.

on usage, showing that treatment increases both account opening and use conditional on having an account.

4 Treatment Increases Employment

Having shown that the LinkedIn training program changes participants' use of the platform, we next examine how the program changes participants' outcomes in the labor market. Treatment increases end-of-program employment by 7 percentage points (standard error 2.1), from a control group mean of 70% (Table 3, column 1). Treatment increases employment six and twelve months later by respectively 8.1 and 6.9 percentage points (standard errors respectively 3.9 and 2.4). The employment effects remain statistically significantly different to zero using a wild bootstrap clustered by cohort, except at six months where $p = 0.138$. Treatment also increases weekly hours worked at the six- and twelve-month points by respectively 4.2 and 2.9 hours (Table 4, column 1). Using the same decomposition introduced in the previous section, the hours effects are concentrated at the extensive margin: they are explained by the rise in employment, rather than longer hours conditional on employment (Table B.3). Consistent with the underlying program's focus on call center jobs, 85% of employed candidates work in business process outsourcing firms or banks/insurers with large in-house call centers. The treatment effect on employment occurs entirely in these two sectors, as we discuss in Section 6.

Table 3: Treatment Effects on Employment

	(1) End of program	(2) 6 months	(3) 12 months
Treated cohort	0.070	0.081	0.069
	(0.021)	(0.039)	(0.024)
	[0.000]	[0.138]	[0.000]
Control group mean	0.701	0.638	0.704
# respondents	1626	1119	988
# cohorts	30	30	30
Adjusted R2	0.050	0.073	0.041

Coefficients are from regressing an employment indicator in each of the three survey waves on a treatment indicator and stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. Wild cluster bootstrap p-values are shown in brackets. The wild bootstrap clusters by training cohort, uses 1000 replications, imposes the null hypothesis in each bootstrap replication, and uses Rademacher weights.

We do not observe earnings, but we can roughly estimate earnings gains in two scenarios. First, we assign employed participants the average earnings of entry-level call center workers in nationally representative labor force data. Under this assumption, in the first year after treatment, employed participants earn roughly USD16,000 on average, treatment increases the average participant's earnings by roughly USD1,100 (7% of 16,000), and the benefit:cost ratio of treatment is 23. Second, we make the much more conservative assumption that employed participants earn the statutory minimum wage. Under this assumption, in the first year after treatment, employed participants earn roughly USD6,050 on average, treatment increases the average participant's earnings by roughly USD420, and the benefit:cost ratio of treatment is 8.7. All figures are reported in 2017 USD using purchasing power parity conversion factors. See Appendix E.2 for details.

The persistent treatment effect on average employment reflects persistent treatment effects on individual-level employment. Treatment increases the probability of being employed at both end-of-program and six months later by 10.7 percentage points and the probability of being employed at both end-of-program and twelve months later by 12.6 percentage points (Table 4, column 2). Treatment has no effect on turnover during the first six months and slightly reduces turnover during the next six months (Table 4, column 3). These estimates imply that treated participants who find jobs at the end of the program are slightly more likely than control group participants to retain them for the next twelve months.[9] As a benchmark, the median job tenure for young South Africans at the time was eleven months (Zizzamia and Ranchhod, 2019).

[9] Our tenure analysis has one important caveat. We observe how many employers each participant has between baseline and each survey. This does not distinguish between multiple jobs held sequentially or simultaneously. Hence the 12% of participants reporting two or more employers might have held these jobs sequentially (implying turnover) or simultaneously.

Table 4: Treatment Effects on Employment Type

	(1) Hours	(2) Employed at end of program & current wave*	(3) Multiple employers	(4) Permanent contract	(5) Promoted
Panel A: Six Months After Program Completion					
Treated cohort	4.200	0.107	0.001	0.026	0.007
	(1.700)	(0.040)	(0.021)	(0.026)	(0.010)
Control group mean	25.523	0.585	0.123	0.129	0.038
Control mean|employment					
# respondents	1107	1117	1114	1113	1117
# cohorts	30	30	30	30	30
Adjusted R2	0.078	0.077	0.007	0.104	0.001
Panel B: Twelve Months After Program Completion					
Treated cohort	2.879	0.126	-0.044	0.034	-0.023
	(1.029)	(0.027)	(0.025)	(0.025)	(0.021)
Control group mean	29.233	0.602	0.144	0.189	0.118
Control mean|employment					
# respondents	985	987	988	983	986
# cohorts	30	30	30	30	30
Adjusted R2	0.046	0.059	0.020	0.059	-0.001

Coefficients are from regressing each employment characteristic on a treatment indicator and stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. Column 1 shows hours worked in the week preceding the survey. Column 2 shows the probability of being employed at both the end of program and 6 months later (Panel A) or the end of program and 12 months later (Panel B). Column 3 shows the probability that the workseeker had more than one employer between the end of the program and relevant survey. Column 4 shows the probability that the job is permanent, rather than temporary. Column 5 shows the probability that the workseeker was promoted between the end of the program and relevant survey, without changing employers. All outcomes are set equal to zero for non-employed workseekers. Starred outcomes are not prespecified.

Job security is an important dimension of match quality for South African workseekers, ranked ahead of earnings and promotion prospects in survey data (Mncwango, 2016).

We further explore the role of match quality by decomposing retention into two components: an extensive-margin effect due to changes in employment and an intensive-margin effect due to changes in retention conditional on employment. We use the same decomposition described in Section 3 and report detailed results in Table B.3. The extensive and intensive margin effects on retention account for respectively 70 and 30% of the average effect on retention at six months and 47 and 53% at twelve months. This is consistent with a role for higher match quality as well as higher employment, although the intensive margin effects are not statistically significantly different to zero.

Treatment does not increase other match quality proxies. Treatment effects on the probability of having a permanent contract and promotion are small and not statistically significant (Table 4, columns 4-5). Decomposing these into extensive and intensive margins shows positive and significant extensive-margin effects but no intensive-margin effects, suggesting that treatment does not shift match quality on these dimensions (Table B.3).

How large are the employment effects we find? A recent metastudy finds that the mean effects of active labor market programs (ALMPs) are 1.6 and 5.4 percentage points in respectively the first and second years after treatment (Card et al., 2017). Our employment effects are larger than these averages. As discussed in Section 1, within the universe of ALMPs, we study a job search assistance program targeted at disadvantaged participants. Our employment effects are closer to the averages of interventions in the metastudy targeting disadvantaged participants (4.2 and 5.3 percentage points in the first and second years) but substantially larger than the averages of job search assistance interventions in the metastudy (1.2 and 2.0 percentage points in the first and second years).

The employment effects are robust to adjustments for non-response and to conditioning on baseline covariates. Non-response is initially low but rises over time: it is under 1% for the end-of-program employment measures, 32% in the six-month surveys, and 40% in the twelve-month surveys. Non-response does not differ by treatment status and is weakly related to baseline covariates and their interactions with treatment (Tables A.1 and A.2). The employment effects are robust to reweighting the sample to account for the small differences between responders and nonresponders in baseline characteristics and to conditioning on baseline covariates using a Lasso estimator (Table A.3). The Lasso uses a data-driven rule to condition on covariates that predict either employment or treatment status in the sample of responders, which will include any covariates that differentially predict non-response by treatment status (Belloni et al., 2014). Lee bounds on the employment effects are less than 2 percentage points wide (Table A.3).

5 Relating Treatment Effects on Employment and LinkedIn Usage

Having shown that the LinkedIn training program increases participants' use of the platform and improves their outcomes in the labour market, we next assess if these two treatment effects are related. Our experiment alone does not identify causal relationships between post-treatment outcomes without additional assumptions. But in this section we describe four patterns in the data that suggest a link between treatment effects on LinkedIn usage and employment.

First, the treatment effect on having a LinkedIn account occurs entirely during the job readiness training program (Table 2, column 2). This occurs before participants start any post-program employment. This

Table 5: Employment Effects by Prior LinkedIn Account

	(1)	(2)	(3)	(4)
	Previously employed		Employed at end of program	
LI account at baseline	0.130	0.090	0.068	0.035
	(0.035)	(0.035)	(0.044)	(0.049)
Covariates included?		Y		Y
# respondents	1475	1475	699	699
Adjusted R2	0.009	0.116	0.002	0.024

Coefficients are from regressing an indicator for pre-baseline work experience (columns 1-2) or post-training employment (columns 3-4) on an indicator for having a LinkedIn account at baseline. Results in columns 2 and 4 are conditional on age, gender, education, past employment (column 4 only), psychometric assessment scores, and city fixed effects. Results in columns 3-4 use only control group candidates. Heteroskedasticity-robust standard errors are shown in parentheses. None of the analysis in this table is prespecified.

timing is more consistent with LinkedIn usage increasing employment than employment leading to higher LinkedIn usage.

Second, employment and LinkedIn use are positively associated using non-experimental variation. At baseline, candidates with LinkedIn accounts are 13 percentage points (standard error 3.5) more likely to have ever worked than candidates without accounts (Table 5, column 1). In the control group, candidates with LinkedIn accounts are 6.8 percentage points (standard error 2.5) more likely to be employed immediately after the program than candidates without accounts (Table 5, column 3). Both associations shrink slightly but remain positive when we condition on education, psychometric assessment scores, age, gender, and city. These associations need not reflect causal relationships, but they are consistent with positive labor market returns to LinkedIn use.

Third, treatment is more effective for candidates with less prior LinkedIn exposure. Treatment effects on employment are 3-10 percentage points smaller for candidates who have LinkedIn accounts at baseline than those who do have accounts, although these differences are not statistically significantly different to zero (Table 6). However, this treatment effect heterogeneity may partly reflect baseline correlates of having a LinkedIn account and we do not have enough data to precisely estimate heterogeneous treatment effects by multiple dimensions simultaneously. Having an account at baseline is also a noisy measure of LinkedIn usage, as it does not capture variation in how candidates with accounts use them.

Fourth, we run two formal mediation analyses in Appendix C that estimate the relationship between treatment-induced changes in LinkedIn usage and treatment-induced changes in employment. Both analyses estimate that treatment-induced changes in LinkedIn usage explain 67 to 73% of the treatment-induced changes in employment. These are likely to be underestimates, as we do not observe every way in which

Table 6: Employment Effects by Prior LinkedIn Account

	(1) End of program	(2) 6 months	(3) 12 months
Treated cohort	0.083	0.099	0.087
	(0.023)	(0.041)	(0.026)
.. × baseline LinkedIn account	-0.031	-0.106	-0.067
	(0.030)	(0.067)	(0.051)
Control group mean | Account	0.777	0.740	0.833
Control group mean | No account	0.709	0.630	0.690
# respondents	1518	1073	949
# cohorts	30	30	30
Adjusted R2	0.053	0.080	0.062

Coefficients are from regressing an employment indicator in each of the three survey waves on a treatment indicator and stratification block fixed effects, with the treatment indicator and block fixed effects interacted with an indicator for having a LinkedIn account at baseline. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. Control group means are shown for candidates with and without LinkedIn accounts at baseline.

participants use LinkedIn. Each approach relies on additional assumptions: one approach assumes that treatment increases LinkedIn use but does not affect the relationship between LinkedIn use and employment, while the other approach assumes that there are no omitted variables correlated with both LinkedIn use and employment conditional on treatment (Robins and Greenland, 1992; Imai et al., 2010; Heckman and Pinto, 2015). These assumptions are strong, so we interpret the finding with caution. But the four results in this section jointly provide at least suggestive evidence that treatment increases employment by increasing LinkedIn usage.

6 Economic Mechanisms Linking Employment and LinkedIn Usage

Having shown that treatment increases LinkedIn use and employment, and that these treatment effects are plausibly related, we next explore the economic mechanisms that might link LinkedIn use to employment. LinkedIn use might increase employment through multiple economic mechanisms. Our experiment is not designed to generate independent exogenous variation in each of these mechanisms. But in this section we present suggestive evidence on the relative importance of six mechanisms: supply-side information acquisition, demand-side information acquisition, referrals, lower job search costs, higher engagement with the job readiness program, and higher self-beliefs. We find some evidence consistent with the supply-side information mechanism and mixed evidence on the role of demand-side information and referrals. We do not find evidence to support the other three mechanisms.

First, we find evidence consistent with a *supply-side information* mechanism. In principle, LinkedIn might allow workseekers to learn about general labor market conditions and specific employers. Altmann et al. (2018), Belot et al. (2019), Ahn et al. (2019), and Carranza et al. (2020) have all documented systematic inaccuracies in workers' beliefs about the labor market and shown these can be shifted by information-provision interventions. Survey evidence from the US shows that workseekers use LinkedIn to learn about prospective employers and about the individual staff members interviewing them (Sharone, 2017). Consistent with this mechanism, treatment increases an index of LinkedIn usage measures related to worker learning by 0.32 standard deviations (Table 7, column 1).[10] Treatment increases one component of this index, the average number of times workseekers' view other profiles on LinkedIn, from 0.5 to 2.1 in the month of program completion (Figure B.1). This occurs at the same time as the treatment-induced rise in employment and may reflect workseekers researching interviewers on LinkedIn. Can the treatment effect on profile views quantitatively explain the treatment effect on employment? Answering this is difficult, because neither our paper nor any existing paper provides estimates of the causal effect of researching interviewers on the probability of job offers. But, as a back-of-the-envelope illustration, we note that this mechanism can fully explain the 7 percentage point rise in enrollment if each of the 1.6 additional profiles viewed increases the probability of converting an interview into an offer by 4.4 percentage points.

What types of information does LinkedIn help workseekers to acquire? We see two patterns in the employment data that suggest treated candidates mainly acquire information about a wider range of jobs, firms, or interviewers in their target sector. Treatment increases the share of candidates employed in the program's target sectors – business process outsourcing and finance – by 15.5 percentage points (Table 7, column 2). This is roughly twice as large as the treatment effect on overall employment, showing that treatment shifts candidates into the target sectors from both non-employment and from employment in other sectors. This pattern is not consistent with candidates using LinkedIn to acquire information about a wider range of sectors and searching in those sectors. Treatment also decreases the share of candidates employed in Harambee's partner firms by a statistically insignificant 2.4 percentage points (Table 7, column 3). Harambee's long-term partnerships with selected private sector firms account for 34 percentage points of the 70% post-program employment rate in the control group. But treated candidates are not more likely to secure a job with a long-term partner. This pattern is consistent with candidates using LinkedIn to acquire information about new jobs or firms in the target sectors, rather than performing better in applications to firms that they know

[10] This index is the first principal component of the number of jobs and number of profiles that each participant views on LinkedIn, with zeroes assigned to participants without LinkedIn accounts.

Table 7: Treatment Effects on Potential Mechanisms

	(1) LI: Supply-side info.	(2) Employed in target sector*	(3) Employed at partner firm*	(4) LI: Demand-side info.	(5) LI: # connections
Treated cohort	0.316	0.155	-0.024	0.842	0.559
	(0.073)	(0.034)	(0.059)	(0.144)	(0.093)
Control group mean	0.000	0.539	0.342	0.000	0.000
Control mean | account	0.393			0.888	0.416
# respondents	1493	1626	1626	1348	1629
# cohorts	30	30	30	30	30
Adjusted R2	0.052	0.096	0.207	0.170	0.121
	(6) LI: Job apps on platform	(7) Self-placed*	(8) Completed program	(9) Engagement index	(10) Aspirations index
Treated cohort	0.009	-0.013	-0.023	0.151	0.065
	(0.004)	(0.009)	(0.030)	(0.122)	(0.054)
Control group mean	0.014	0.056	0.882	0.000	-0.000
Control mean | account	0.030				
# respondents	1493	1626	1612	1246	1231
# cohorts	30	30	30	29	29
Adjusted R2	0.018	0.012	0.024	0.176	0.035

Coefficients are from regressing each outcome measure on a treatment indicator and stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. Columns 1, 4, 5, 9, and 10 show the first principal components of a set of measures corresponding to each mechanism, rescaled to have standard deviation one in the control group. The supply-side information index in column 1 is based on the number of profiles and the number of jobs a workseeker views on LinkedIn. The demand-side information index in column 4 is based on completeness of a workseeker's profile and the number of times another user views the workseeker's LinkedIn profile in the final month of the training program. The connections index in column 5 is based on the number of network connections on the platform, the number of network connections with a bachelors or higher degree, and the number of network connections in managerial positions. The engagement index in column 9 is based on workseekers' engagement with the job readiness program using both self-reports and reports from the program manager. The aspirations index in column 10 is based on workseekers' self-reported reservation wage, aspirational wage, locus of control, and excitement about the future at the end of the training program. All LinkedIn usage measures are set to zero for workseekers without LinkedIn accounts before constructing the principal components. Number of connections, job applications, jobs viewed, profiles viewed, and profile views are winsorized at the 95th percentile. All LinkedIn usage measures except the number of views of a workseeker's profile are averages across the three waves of LinkedIn data: at the end of the training program and roughly six and 12 months later. Missing values are due to workseekers with LinkedIn accounts whose usage data could not be compiled by LinkedIn (columns 1, 4, and 5) and survey non-response, partly because one training manager did not administer and end-of-program survey (columns 9 and 10). The conditional control group mean in columns 1, 4, 5, and 6 is the average value for control workseekers with LinkedIn accounts. Starred outcomes are not prespecified.

well from their training at Harambee.

Second, we find mixed evidence for a *demand-side information* mechanism. In principle, LinkedIn might allow firms to learn about workseekers through their LinkedIn profiles. The profiles might be more credible than information on resumes, as the former is public, making it riskier to report false or inaccurate information than on a resume. The profiles might also act as signals of proactivity or technological engagement. Farber and Gibbons (1996), Altonji and Pierret (2001), Lange (2007), and Pallais (2014) have all documented that employers have limited information about prospective workers at the time of hiring. Survey evidence, mainly from the US, shows that firms collect information about job applicants using online media, including LinkedIn profiles (Stamper, 2010; Shepherd, 2013; Guilfoyle et al., 2016; Kluemper et al., 2016). Roulin and Levashina (2019) find that assessments of job applicants' LinkedIn profiles are predictive of job performance. Employers who rely on LinkedIn in hiring reportedly focus on the completeness of an applicant's profile; the number of network connections; information about skills, training, and work experience listed in the profile; and the existence of a profile photo (Caers and Castelyns, 2011; Roulin and Levashina, 2019).

Consistent with this mechanism, treatment increases an index of LinkedIn usage measures related to firm learning by 0.84 standard deviations (Table 7, column 4).[11] The treatment effect is driven mainly by an increase from 0.65 to 1.85 in the number of times participants' profiles are viewed by other users in the month of program completion, when participants are searching for jobs. Furthermore, the treatment effects on employment are substantially larger for candidates with low measured communication skills (Table B.7). This is consistent with LinkedIn profiles providing an alternative source of information that offsets weak written or verbal communication in applications or interviews.[12] However, our limited data from firms does not support this mechanism. We interview human resource staff at three large firms (one business process outsourcing, two insurance) that jointly employ 20% of the candidates in our sample. None of the hiring managers report that they view job applicants' LinkedIn profiles during hiring. We thus view the demand-side information mechanism as less plausible than the supply-side information mechanism.

Third, we see mixed evidence for a *referrals* mechanism. In principle, LinkedIn allows workseekers to communicate more easily with prior connections or form new connections. This is consistent with the large role for referrals in hiring in many contexts (Topa, 2011). This is also consistent with the 0.56 standard deviation treatment effect on an index of LinkedIn network strength (Table 7, column 5).[13] However, most initial placements persist for at least a year. So if candidates use referral networks, they use them only to

[11] This index is the first principal component of a profile completeness score and the number of times other participants view the profile, with zeroes assigned to participants without LinkedIn accounts. Profile completeness is a four-point score assigned by a non-public LinkedIn algorithm that takes into account whether a profile includes a photograph, profile summary, location, skills, education history, and work history.

[12] In contrast, we see no quantitatively important heterogeneity in the employment effects over candidates' cognitive skill, numeracy skill, education, previous employment, age, or gender. The heterogeneity by communication scores remains statistically significant when we adjust for testing across all these dimensions of heterogeneity, except in the six-month survey. Treatment effects on the other mechanisms discussed in this section do not vary by communication skill.

[13] This index is the first principal component of the total number of connections the participant has, the number of connections to people with university degrees, and the number of connections to people with managerial jobs, with zeroes assigned to participants without LinkedIn accounts. Participants' numbers of connections are relatively small by LinkedIn standards, with an average of 6 connections for control group participants and 15 for treatment group participants. However, even this is larger than offline job search networks in urban Ethiopia and South Africa (Abebe et al., 2017; Caria et al., 2018; Carranza et al., 2020).

transition into employment and not for subsequent on-the-job search. We do not observe candidates' self-reported use of connections in job applications. But we construct a proxy using data from LinkedIn. We observe the number of connections candidates have with workers at the firm where they are hired, before they complete the job readiness training program. Treatment has no effect on this measure. However, this measure is missing for candidates who do not list an employer with a LinkedIn page and cannot distinguish connections formed just before or just after job interviews. It is only constructed 20 to 40 months after the programs end, so it misses connections for candidates who themselves switch employers or whose connections switch employers. Given these caveats, we do not view the evidence for or against the referrals mechanism as conclusive.

Fourth, we find no evidence consistent with a *search cost* mechanism. In principle, LinkedIn could change job search by allowing users to search cheaply and quickly for vacancies and submit applications. Carranza et al. (2020) show that pecuniary search costs are relatively high in this setting. Treatment does increase the number of job applications submitted through LinkedIn, but from a low mean of 0.014 applications to a still-low 0.025 applications (Table 7, column 6). Even if every marginal application induced by treatment leads to employment, this can explain only 2.7 of the 7 percentage point effect of treatment on employment. Treatment has no effect on the probability of "self-placement:" getting a job before the program ends from an application submitted outside the job training program (Table 7, column 7). Instead, the increase in employment is explained entirely by applications submitted as part of the job training program.[14] These results suggest a smaller role for LinkedIn's capacity to reduce pecuniary search costs than for LinkedIn's capacity to reduce information frictions, as discussed in the preceding mechanisms.

Fifth, we find no evidence consistent with a *program engagement* mechanism. In principle, treatment might increase or decrease candidates' effort in the job readiness training program and/or probability of completing the program, depending on their beliefs about the production function relating effort, program completion, and LinkedIn use to subsequent outcomes in the labor market. However, we see no shifts in measures of completion or engagement. Treated and control candidates have similar probabilities of completing the job readiness training program, respectively 86 and 88% (Table 7, column 8). Treated candidates score 0.15 standard deviations higher on an index of program engagement, based on self-reports and training manager reports, but this estimate has a standard error of 0.12 (Table 7, column 9). Treatment

[14]The job training program includes time dedicated to applying for jobs, including vacancies at firms where Harambee has long-term partnerships. By design, this process is identical for treated and control cohorts.

effects on the individual components of this index are even smaller (Table B.9).

Sixth, we find no evidence consistent with a *self-beliefs* mechanism. In principle, LinkedIn might change participants' self-beliefs through some mechanism other than standard labor market information acquisition, such as exposure to role models through the platform (Beaman et al., 2012). We measure candidates' locus of control, external trust, hope, reservation wages, and the wages they aspire to earn, following Lippman et al. (2014) and Orkin et al. (2020). The treatment effect on an index combining these self-belief measures is only 0.07 standard deviations and has a standard error of 0.05 (Table 7, column 10). Turning to the individual components, treatment has no effect on locus of control, external trust, or hope and only small positive effects on reservation and aspirational wages (Table B.10). The latter effects occur after the rise in employment and may be an outcome of employment, rather than a cause of employment. While we measure only part of the universe of potential self-beliefs, these results do not suggest a central role for changes in self-beliefs.

7 Conclusion

We report the first experimental evidence that training participants in job readiness programs to join and use an online professional networking platform improves their outcomes in the labor market. Treatment increases their employment rate by approximately 10% (7 percentage points) for at least one year. Treatment does not substantially change the probability of retention, promotion, or obtaining a permanent contract conditional on employment. This suggests that treatment is not increasing employment by putting marginal candidates in lower-quality matches than the inframarginal matches that candidates obtain without treatment. Treatment also increases LinkedIn usage, both by increasing the share of participants with LinkedIn accounts and increasing account usage conditional on having an account. We show suggestive evidence that the treatment effects on LinkedIn usage explain part of the treatment effects on employment.

These findings suggest several directions for future research. First, what aspects of online professional networking drive the employment effects? Our results suggest an important role for information provision to workseekers about the labor market or to firms about workseekers. Our results are also consistent with some use of referrals. Future work could identify referral mechanisms using better data on the identities and dates of workseekers' on-platform connections, as well as integrating administrative platform data with survey data on job search behavior.

Second, what might large increases in online professional networking achieve in general equilibrium?

Some but not all studies of large-scale active labor market programs find smaller effects at larger scale (Blundell et al., 2004; Lise et al., 2004; Crépon et al., 2013). Our experiment generates a tiny market-level increase in LinkedIn use: 285 extra users on a base of roughly 7.1 million. Our experiment is not designed to identify general equilibrium effects, but we speculatively discuss three possible scenarios. First, LinkedIn use at scale may deliver smaller gains if our results are driven by treated workseekers displacing control workseekers when they compete for the same jobs. We can test this idea by comparing outcomes across control cohorts more and less likely to compete against treated cohorts (using variation in city and timing of treatment). We find no evidence of spillovers on control cohorts' employment or LinkedIn use, as we discuss in Appendix B. Second, LinkedIn use may deliver smaller gains at scale if profiles simply serve as a proxy for proactivity or digital proficiency and lose that value when use is widespread. We cannot directly test this idea. But, anecdotally, Harambee helps multiple workseekers from the same cohort apply to the same firms. This means that treated candidates might often be competing against other treated candidates even in the small scale of our experiment. Third, LinkedIn use may still deliver meaningful welfare gains at scale if it provides information that allows higher firm-worker match quality or reduces pecuniary and time costs of job search and posting. We cannot directly test this idea, and our match quality proxies do not provide strong evidence for or against it.

The effects of an increase in online professional networking might depend on the level of networking use, as well as the size of the increase. We study a setting where LinkedIn use is relatively common: the 7.1 million users represent roughly 40% of the national workforce. In a new network with few users, effects might be smaller because few employers use it to screen candidates or post vacancies, or possibly larger because the signal value of early use is even higher.

Third, how might workseekers use online professional networking outside the context of job readiness programs? Both treatment and control workseekers received six to eight weeks of programming and job search assistance. These might complement online professional networking by giving workseekers content for profiles, connections to program alumni, and advice for on-platform search and job applications. On the other hand, online professional networking might have higher returns without job readiness training and job search assistance because they operate through overlapping mechanisms.

These findings have important implications for policy design even if they apply only to job readiness program participants. Given the prevalence and cost of these programs, faster post-program transitions into employment are valuable. Our findings show that substantial gains are possible from small, low-cost design changes that use new technology and are guided by research on labor market frictions.

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A Robustness Checks for Employment Effects

In this appendix we show that our employment results are robust to accounting for non-response and to conditioning on baseline covariates. We also provide more information on survey non-response.

Non-response is unrelated to treatment and weakly related to baseline covariates. Tables A.1 and A.2 demonstrate this by showing the relationship between non-response, treatment, and baseline covariates in the surveys respectively six and twelve months after treatment. Non-response is balanced across treatment and control candidates in both survey rounds (column 1). Non-response is decreasing in education in the six-month survey and is lower in Johannesburg/Pretoria than in Cape Town and Durban (the omitted region) in both surveys (column 2). The interaction between treatment and baseline work experience predicts lower non-response in both survey rounds (column 3). Both higher education and baseline work experience predict subsequent employment. So it is possible that non-response skews our survey data toward candidates with strong employment prospects, particularly in the treatment group. However, we show below that our results are robust to accounting for differential response rates by treatment assignment and baseline covariates.

The treatment effects on employment are robust to reweighting the sample of responders to resemble the full sample on baseline covariates. Table A.3 Panel A demonstrates this by reporting inverse-probability-weighted treatment effect regressions. The weights account for any differences between responders and non-responders in the observed baseline covariates listed in Tables A.1 and A.2. The sign and magnitude of effects are robust across unweighted and weighted estimates. We omit the end-of-program employment effects from this table because the response rate is above 99% and the weighting model does not converge in some bootstrap resamples.

The treatment effects on employment are also robust to conditioning on baseline covariates. To implement this check, we run a post-double selection lasso on the observed baseline covariates listed in Tables A.1 and A.2. The post-double-selection lasso selects any covariates that predict either treatment or employment in the sample of nonresponders (Belloni et al., 2014). Hence the lasso automatically selects and conditions on any covariates that differentially predict non-response by treatment status. The conditional employment effects are slightly smaller than the unconditional effects but the sign and rough magnitude of effects are the same (Table A.3 Panel B).

The treatment effects on employment are robust to accounting for differential non-response by treatment arm. Table A.3 Panel C demonstrates this. The panel reports bounds on employment effects assuming that

Table A.1: Predictors of Non-Response in 6-Month Follow-up Survey

	(1)	(2)	(3)
Outcome		Non-response	
Treatment	-0.012		-0.428
	(0.049)		(0.200)
Age		0.004	-0.004
		(0.004)	(0.006)
Gender		-0.028	-0.048
		(0.026)	(0.035)
Previously employed		0.007	0.064
		(0.025)	(0.044)
Numeracy score		-0.019	-0.001
		(0.015)	(0.022)
Communications score		-0.006	-0.010
		(0.013)	(0.011)
Cognitive score		-0.021	-0.018
		(0.012)	(0.018)
Post-secondary education		-0.062	-0.036
		(0.022)	(0.034)
University education		-0.100	-0.035
		(0.055)	(0.077)
Cape Town		0.023	-0.051
		(0.074)	(0.046)
Johannesburg and Pretoria		-0.149	-0.249
		(0.062)	(0.027)
Age X Treatment			0.012
			(0.009)
Gender X Treatment			0.030
			(0.050)
Previously employed X Treatment			-0.102
			(0.050)
Numeracy score X Treatment			-0.031
			(0.029)
Communications score X Treatment			0.011
			(0.024)
Cognitive score X Treatment			-0.010
			(0.024)
Post-secondary education X Treatment			-0.047
			(0.045)
University education X Treatment			-0.145
			(0.103)
Cape Town X Treatment			0.155
			(0.121)
Johannesburg and Pretoria X Treatment			0.199
			(0.094)
# respondents	1638	1492	1492
# cohorts	30	30	30
Non-response mean	0.317		
p-value joint significance	0.804	0.000	0.000
F-stat joint significance	0.063	4.934	44.666

Coefficients are from regressing a non-response indicator on a treatment indicator, baseline covariates, and treatment interacted with covariates. Sample excludes respondents with missing values for any baseline covariate. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. The cognitive assessment is a test similar to Raven's.

Table A.2: Predictors of Non-Response in 12-Month Follow-up Survey

	(1)	(2)	(3)
Outcome		Non-response	
Treatment	0.002		-0.573
	(0.051)		(0.196)
Age		-0.007	-0.018
		(0.004)	(0.008)
Gender		-0.044	-0.104
		(0.036)	(0.025)
Previously employed		0.047	0.117
		(0.025)	(0.038)
Numeracy score		-0.010	-0.004
		(0.014)	(0.018)
Communications score		0.015	0.017
		(0.012)	(0.017)
Cognitive score		-0.008	-0.004
		(0.010)	(0.014)
Post-secondary education		-0.049	-0.057
		(0.027)	(0.030)
University education		-0.056	0.016
		(0.051)	(0.072)
Cape Town		0.054	0.021
		(0.052)	(0.058)
Johannesburg and Pretoria		-0.175	-0.225
		(0.045)	(0.050)
Age X Treatment			0.021
			(0.008)
Gender X Treatment			0.104
			(0.062)
Previously employed X Treatment			-0.122
			(0.047)
Numeracy score X Treatment			-0.010
			(0.027)
Communications score X Treatment			-0.005
			(0.025)
Cognitive score X Treatment			-0.011
			(0.022)
Post-secondary education X Treatment			0.014
			(0.050)
University education X Treatment			-0.148
			(0.099)
Cape Town X Treatment			0.073
			(0.099)
Johannesburg and Pretoria X Treatment			0.088
			(0.081)
# respondents	1638	1492	1492
# cohorts	30	30	30
Non-response mean	0.397		
p-value joint significance	0.968	0.000	0.000
F-stat joint significance	0.002	6.239	12.732

Coefficients are from regressing a non-response indicator on a treatment indicator, baseline covariates, and treatment interacted with covariates. Sample excludes respondents with missing values for any baseline covariate. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. The cognitive assessment is a test similar to Raven's.

Table A.3: Sensitivity Analysis for Treatment Effects on Employment

	(1) End of program	(2) 6 months	(3) 12 months
Panel A: Weighted by Inverse Probability of Nonresponse			
Treated cohort		0.074	0.070
		(0.032)	(0.031)
Panel B: Conditional on Lasso-selected Baseline Covariates			
Treated cohort	0.064	0.071	0.065
	(0.020)	(0.038)	(0.023)
Panel C: Lee bounds			
Treated cohort: lower bound	0.070	0.081	0.057
Treated cohort: upper bound	0.084	0.099	0.061

Panel A and B coefficients are from regressing an employment indicator in each of the three waves on a treatment indicator and stratification block fixed effects. Panel A regressions are weighted by the inverse probability of nonresponse in each wave, estimated from a logit regression of nonresponse on the list of covariates in column 2 of Tables A.1 and A.2. Standard errors in parentheses are from 1000 iterations of a bootstrap that resamples cohorts and estimates both the weights and employment regressions in each iteration. End-of-program employment is omitted from this sensitivity analysis because the high response rate means the weighting model cannot be estimated in many bootstrap samples. Panel B regressions also condition on a vector of baseline covariates selected by the post double selection lasso estimator. The lasso estimator selects from the same list of covariates. In each regression it chooses only some of the skill and education measures. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. Panel C shows Lee bounds, tightened using region fixed effects. Lee bounds trim the sample to equalize the nonresponse rates across treatment arms. Standard errors are omitted in Panel C because the analytical variance estimator for Lee bounds does not account for clustering.

the small number of extra responders in the treatment group are all unemployed (row 1) or all employed (row 2), following Lee (2009). The bounds are never wider than 1.8 percentage points. This result is unsurprising, as the response rates in both rounds differ by at most 1.2 percentage points between treatment and control groups.

B Additional Results Discussed in Paper

This appendix reports additional results discussed in the main paper text. Table B.1 shows treatment effects on the ten LinkedIn usage measures used to construct the indices discussed in Sections 3 and 6. Treatment significantly increases each of these measures, though the effect sizes range substantially.

Table B.2 reports the decomposition of each of these effects into extensive- and intensive-margin effects, using the same decomposition introduced in Section 3. Intuitively, the extensive margin effects on LinkedIn usage are the effects on the probability of having a LinkedIn account, multiplied by mean level of LinkedIn usage for control group candidates with accounts. This is the treatment effect on LinkedIn usage that would

Table B.1: Treatment Effects on LinkedIn Use

	(1) Has LinkedIn account	(2) Opened LI account during training*	(3) Profile completeness	(4) Profiles viewed	(5) Jobs viewed
Treated cohort	0.314	0.422	0.243	0.584	0.058
	(0.049)	(0.050)	(0.036)	(0.129)	(0.023)
Control group mean	0.484	0.094	0.301	0.378	0.178
Control mean|account			0.631	0.810	0.381
# respondents	1638	1566	1599	1493	1493
# cohorts	30	30	30	30	30
Adjusted R2	0.140	0.282	0.116	0.086	0.029
	(6) # connections	(7) # bachelors connections	(8) # manager connections	(9) # job applications	(10) # views of profile
Treated cohort	8.609	0.754	0.543	0.009	1.198
	(1.513)	(0.130)	(0.095)	(0.004)	(0.276)
Control group mean	6.145	0.503	0.365	0.014	0.654
Control mean|account	12.807	1.048	0.761	0.030	1.664
# respondents	1629	1629	1629	1493	1362
# cohorts	30	30	30	30	30
Adjusted R2	0.111	0.124	0.118	0.018	0.108

Coefficients are from regressing a measure of LinkedIn usage on a treatment indicator and stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. All variables except those in columns 1, 2, and 10 are averages across the three waves of LinkedIn data: at the end of the training program and roughly six and 12 months later. Individuals without LinkedIn accounts are included as zeros in usage variables. Missing values therefore indicate that the individual has a LinkedIn account but is missing a value for the usage statistic. Number of connections, jobs viewed, profiles viewed, and profile views are winsorized at the 95th percentile. Account during training indicates that the account was created during the training program; profile completion is a binary indicator of whether an individual scores above the median in terms of profile completion; # connections is the number of network connections on the platform; # bachelors connections is the number of network connections with a bachelors or higher degree; # manager connections is the number of network connections in managerial positions; and # job applications is the number of applications submitted through the LinkedIn platform only. # views of profile is the number of times another user views the workseeker's LinkedIn profile and is measured only in the final month of the training program. The conditional control group mean is the average value for control respondents conditional on having a LinkedIn account. Starred outcomes are not prespecified.

Table B.2: Decomposition of LinkedIn Usage into Extensive and Intensive Margins

	(1) Profile completeness	(2) Profiles viewed	(3) Jobs viewed	(4) # connections
Total treatment effect	0.243	0.584	0.058	8.609
	(0.036)	(0.128)	(0.023)	(1.506)
Extensive margin	0.198	0.254	0.119	4.015
	(0.031)	(0.039)	(0.018)	(0.619)
Intensive margin	0.046	0.330	-0.061	4.593
	(0.025)	(0.107)	(0.017)	(1.309)
Conditional treatment effect	0.058	0.418	-0.078	5.822
	(0.032)	(0.136)	(0.022)	(1.659)
Control mean	0.631	0.810	0.381	12.807

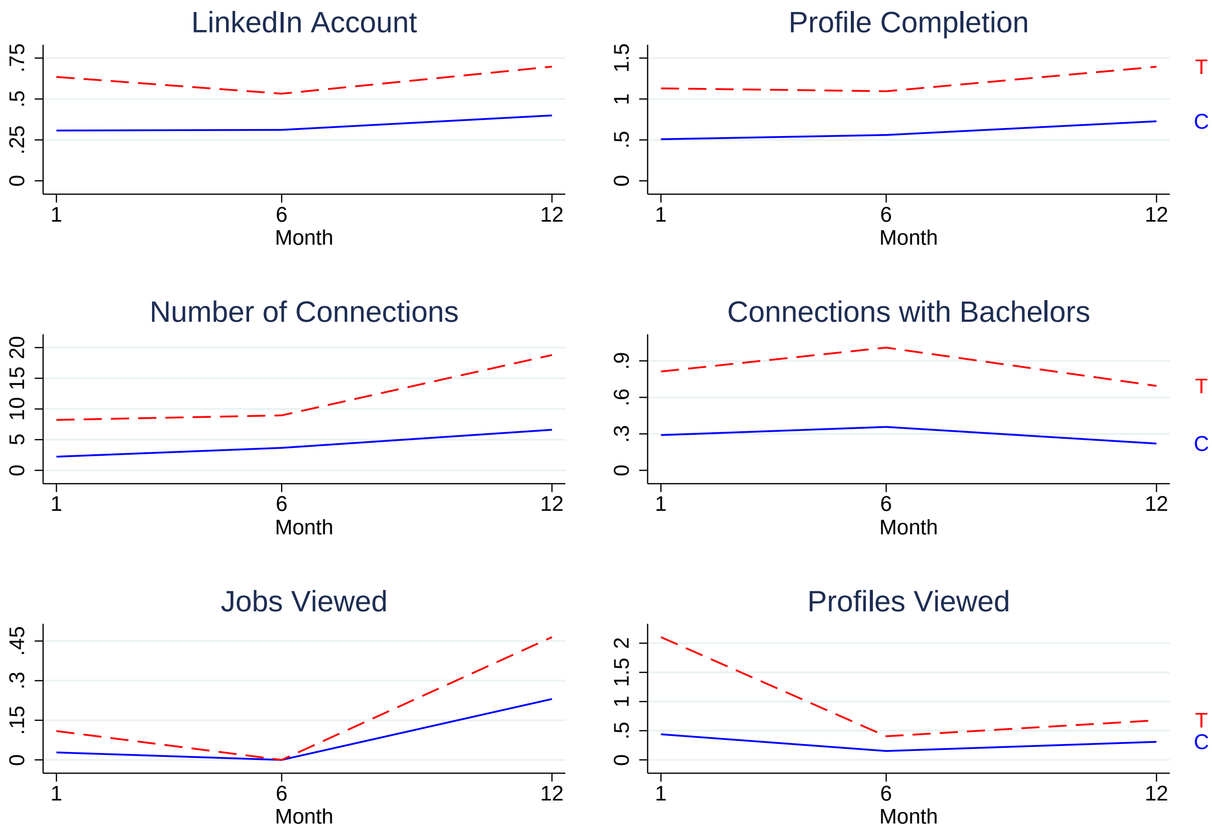
	(5) # bachelors connections	(6) # manager connections	(7) # job applications	(8) # views of profile
Total treatment effect	0.754	0.543	0.009	1.198
	(0.129)	(0.095)	(0.004)	(0.275)
Extensive margin	0.329	0.239	0.009	0.522
	(0.051)	(0.037)	(0.001)	(0.080)
Intensive margin	0.425	0.304	-0.000	0.676
	(0.121)	(0.096)	(0.003)	(0.232)
Conditional treatment effect	0.539	0.386	-0.000	0.857
	(0.153)	(0.121)	(0.004)	(0.294)
Control mean	1.048	0.761	0.030	1.664

This table reports decompositions of treatment effects on LinkedIn use into extensive and intensive margins. The extensive margins are the treatment effects on LinkedIn use due to the treatment effect on having a LinkedIn account, evaluated at mean LinkedIn usage for control group candidates with LinkedIn accounts. The intensive margins are the residual treatment effects on LinkedIn usage, which must be due to treatment effects on engagement with the LinkedIn platform for candidates with accounts. The conditional effect is the implied mean change in LinkedIn usage per treatment group candidate with a LinkedIn account. The control group means are conditional on having an account. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort and constructed using the Delta method.

occur if treatment shifted the share of candidates with accounts but had no effect on how those accounts are used. The difference between each average treatment effect and average extensive margin treatment effect is the average intensive margin treatment effect, which captures changes in engagement with the platform conditional on having an account. The relative importance of the intensive and extensive margins varies across LinkedIn usage measures. Treatment shifts both margins for most of the usage measures. The only exceptions are profile completeness, which changes mainly at the extensive margin, jobs viewed, where treatment increases extensive-margin use and decreases intensive-margin use, and job applications, which changes only at the extensive margin.

Figure B.1 reports selected measures of LinkedIn usage through time for the control and treatment

Figure B.1: LinkedIn Usage by Treatment Status



This figure displays measures of LinkedIn usage by treatment status over time: at the end of the job readiness program, 6 months after, and 12 months after. The red dashed line labeled 'T' reports averages for participants assigned to the treatment group; the blue solid line labeled 'C' reports averages for participants assigned to the control group. The number of connections and connections with bachelors figures represent total connections at that point in time, not new connections since the previous point.

groups. The probability of having an account and multiple usage measures rise immediately after treatment. In particular, the treatment effect on the number of profiles viewed is particularly large at the end of the job readiness program, consistent with candidates using LinkedIn to prepare for applications or interviews. But for most measures there is not a general upward or downward trend in the 12 months after treatment.

Table B.3 reports the decomposition of the treatment effects on employment characteristics reported in Table 4 into extensive and intensive margin effects. The treatment effect on hours worked reflects mainly an extensive-margin effect at six months and only an extensive-margin effect at twelve months. The treatment effects on retention at six and twelve months reflect both extensive- and intensive-margin changes. Decomposing the near-zero average treatment effects on promotion and contract status shows positive and

Table B.3: Decomposition of Employment Type into Extensive and Intensive Margins

	(1) Hours	(2) Employed at end of program & current wave*	(3) Multiple employers	(4) Permanent contract	(5) Promoted
Panel A: Six Months After Program Completion					
Total treatment effect	4.200	0.107	0.001	0.026	0.007
	(1.689)	(0.040)	(0.021)	(0.025)	(0.010)
Extensive margin	3.273	0.075	0.011	0.017	0.004
	(1.568)	(0.036)	(0.005)	(0.008)	(0.002)
Intensive margin	0.927	0.032	-0.010	0.010	0.003
	(0.323)	(0.011)	(0.025)	(0.026)	(0.010)
Conditional treatment effect	1.281	0.044	-0.014	0.014	0.004
	(0.447)	(0.016)	(0.035)	(0.036)	(0.014)
Control mean	40.211	0.916	0.140	0.204	0.053
Panel B: Twelve Months After Program Completion					
Total treatment effect	2.879	0.126	-0.044	0.034	-0.023
	(1.021)	(0.026)	(0.025)	(0.024)	(0.021)
Extensive margin	2.881	0.059	0.010	0.019	0.011
	(1.009)	(0.021)	(0.004)	(0.007)	(0.004)
Intensive margin	-0.002	0.067	-0.054	0.015	-0.033
	(0.321)	(0.014)	(0.027)	(0.025)	(0.020)
Conditional treatment effect	-0.002	0.088	-0.071	0.020	-0.044
	(0.421)	(0.018)	(0.035)	(0.032)	(0.026)
Control mean	41.590	0.855	0.148	0.269	0.155

This table reports decompositions of treatment effects on employment characteristics into extensive and intensive margins. The extensive margins are the treatment effects on employment type due to the treatment effect on employment, evaluated at the mean level of the employment characteristic for employed control group candidates. Employment is defined contemporaneously, i.e. either at six months or twelve months post-training program. The intensive margins are the residual treatment effects on employment characteristics, which must be due to treatment effects on employment characteristics for candidates employed immediately. The conditional effect is the implied mean change in employment characteristic per treatment group candidate that found employment at the end of the training program. The control group means are conditional on employment. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort and constructed using the Delta method. Starred outcomes are not prespecified.

statistically significant extensive-margin effects but smaller and imprecisely estimated intensive-margin effects, suggesting that treatment does not shift match quality on these dimensions.

Table B.4 reports average treatment-on-the-treated effects that account for partial compliance. The treatment was partly implemented for 14 of the 15 cohorts assigned to treatment and fully implemented for 10 cohorts. Incomplete implementation typically occurred because the program managers ran out of time for some scheduled LinkedIn discussion sections or missed sending some advice/encouragement emails. We estimate these effects by regressing employment outcomes on a treatment implementation indicator, instrumented by treatment assignment, and stratification block fixed effects. The first-stage coefficient is 0.62,

Table B.4: Average Treatment on the Treated (ATET) Effects on Employment

	(1) End of program	(2) 6 months	(3) 12 months
Treatment compliance	0.113	0.135	0.118
	(0.040)	(0.074)	(0.055)
Kleibergen-Paap F-statistic	35.59	28.22	25.82
# respondents	1626	1119	988
# cohorts	30	30	30

Coefficients are from regressing an employment indicator in each of the three waves on treatment compliance, instrumented by treatment assignment, and stratification block fixed effects. Compliance is defined as complete treatment programming implemented for the cohorts assigned to treatment. The first stage coefficient in the full sample is 0.62 with standard error 0.10. The F-statistics shown in the table measure first stage instrument strength, following Kleibergen and Paap (2006). Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort.

with standard error 0.10, so all employment effects on the treated candidates are roughly 60% larger than the corresponding intention-to-treat effects.

We also estimate treatment effects of LinkedIn use on employment, instrumenting LinkedIn use by assignment to treatment. As in Section 3, we define LinkedIn use as the standardized first principal component of ten measures: having an account, opening an account during training, the number of profiles viewed, the number of jobs viewed, profile completeness, the number of times the profile is viewed, the total number of connections, the number of connections with bachelors degree, the number of connections with managerial jobs, and the number of job applications submitted on LinkedIn. The first principal component explains 48% of the joint variation in these ten measures. This approach identifies local average treatment effects of LinkedIn use if treatment affects employment only via LinkedIn use (i.e. treatment is excludable from the outcome equation), the single index captures all relevant dimensions of LinkedIn use (i.e. there is no measurement error on the index that would violate the exclusion restriction), and treatment weakly increases LinkedIn use for all candidates (i.e. the instrument has a monotonic effect). These are strong assumptions that are difficult to test, so we interpret this as only suggestive evidence about the magnitude of the LinkedIn-employment relationship.

Using this approach, a one standard deviation increase in LinkedIn use increases employment by 8-12 percentage points (Table B.5). LinkedIn use also increases hours worked six and twelve months after the program (Table B.6). There is some evidence of a positive effect on job quality at twelve months, with LinkedIn use raising the probability of having a permanent contract by 4 percentage points and lowering the probability of turnover by 5 percentage points. LinkedIn use effects on job quality measures at six months are smaller and never significantly different to zero.

Table B.5: Local Average Treatment Effects of LinkedIn Use on Employment

	(1) End of program	(2) 6 months	(3) 12 months
LI usage index	0.087	0.120	0.080
	(0.022)	(0.048)	(0.027)
Kleibergen-Paap F-statistic	42.64	31.25	33.00
Control mean	0.701	0.638	0.704
# respondents	1288	883	776
# cohorts	30	30	30
Adjusted R2	0.066	0.007	-0.002

Coefficients are from regressing an employment indicator in each of the three waves on LinkedIn usage, instrumented by treatment assignment, and stratification block fixed effects. LinkedIn usage is the same index reported in Table 2: the first principal component of having an account, opening an account during training, the number of profiles viewed, the number of jobs viewed, profile completeness, the number of times the profile is viewed, the total number of connections, the number of connections with bachelors degree, the number of connections with managerial jobs, and the number of job applications submitted on LinkedIn. This is standardized to have mean zero and standard deviation one in the control group. The first stage coefficient in the full sample is 0.94 with standard error 0.14. The F-statistics shown in the table measure first stage instrument strength, following Kleibergen and Paap (2006). Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort.

Table B.6: Local Average Treatment Effects of LinkedIn Use on Employment

	(1) Hours	(2) Employed at end of program & current wave*	(3) Multiple employers	(4) Permanent contract	(5) Promoted
Panel A: Six Months After Program Completion					
LI usage index	5.272	0.137	0.001	0.035	0.014
	(2.036)	(0.054)	(0.027)	(0.030)	(0.013)
Kleibergen-Paap F-statistic	32.18	31.32	31.15	31.13	31.15
Control mean	25.523	0.585	0.123	0.129	0.038
# respondents	872	881	879	879	881
# cohorts	30	30	30	30	30
Panel B: Twelve Months After Program Completion					
LI usage index	3.271	0.139	-0.051	0.038	-0.012
	(1.074)	(0.036)	(0.031)	(0.024)	(0.023)
Kleibergen-Paap F-statistic	33.75	33.02	33.00	32.59	33.36
Control mean	29.233	0.602	0.144	0.189	0.118
# respondents	773	775	776	771	775
# cohorts	30	30	30	30	30

Coefficients are from regressing each employment-related outcome on LinkedIn use, instrumented by treatment assignment, and stratification block fixed effects. LinkedIn usage is the same index reported in Table 2: the first principal component of having an account, opening an account during training, the number of profiles viewed, the number of jobs viewed, profile completeness, the number of times the profile is viewed, the total number of connections, the number of connections with bachelors degree, the number of connections with managerial jobs, and the number of job applications submitted on LinkedIn. This is standardized to have mean zero and standard deviation one in the control group. The first stage coefficient in the full sample is 0.94 with standard error 0.14. The F-statistics shown in the table measure first stage instrument strength, following Kleibergen and Paap (2006). Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. Starred outcomes are not prespecified.

Table B.7: Heterogeneous Treatment Effects on Employment by Communication Skill

	(1) End of program	(2) 6 months	(3) 12 months
Treated cohort	0.068	0.078	0.068
	(0.021)	(0.038)	(0.022)
Treated X communication score	-0.054	-0.055	-0.096
	(0.020)	(0.026)	(0.028)
Communications score	0.068	0.084	0.094
	(0.016)	(0.018)	(0.022)
Control mean	0.701	0.638	0.704
# respondents	1626	1119	988
# cohorts	30	30	30
Adjusted R2	0.060	0.088	0.059
p: interaction = 0	0.010	0.047	0.002
q: interaction = 0	0.072	0.198	0.015

Coefficients are from regressing an employment indicator in each of the three survey waves on a treatment indicator, communication assessment score, their interaction, and stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. The communication skill score is standardized to have mean zero and standard deviation one in the control group. The q-values adjust for multiple testing across treatment interactions with baseline communication skill, cognitive skill, numeracy skill, education, previous employment, age, and gender.

Table B.7 reports treatment effects on employment outcomes for candidates with different levels of communication skill. These are estimated by regressing employment outcomes on a treatment assignment indicator, standardized communication score, the interaction between these two terms, and stratification block fixed effects. The results show that treatment effects are decreasing in communication scores. For example, candidates with one standard deviation higher communication scores are 6.8 percentage points more likely to be employed after the program, but treatment reduces this gap to 1.4 percentage points. The heterogeneous effects at the end of the program and 12 months later remain statistically significant when we estimate q-values that control the false discovery rate across tests based on all baseline heterogeneity measures, following Benjamini et al. (2006). The other baseline heterogeneity measures we consider are age, gender, education, previous employment, numeracy skill, and cognitive skill. None of the other interactions is large and few are statistically significant after adjusting for multiple testing.

Table B.8 shows treatment effects on the probability of working in selected sectors at the end of the job readiness training. Sectors are constructed from firm names. The three largest sectors – finance, hospitality & retail, and call centers – are shown separately. The largest sectors in the 'other' category are construction, logistics, and the 3.7% of candidates whose firms we cannot classify. All sector indicators are coded as zero for candidates who are not employed at the end of the job readiness program.

Table B.8: Treatment Effects on Sector of Employment

	(1) Finance	(2) Hospitality & retail	(3) Call center	(4) Other	(5) No immediate employment
Treated cohort	0.085	-0.012	0.070	-0.073	-0.070
	(0.040)	(0.012)	(0.029)	(0.019)	(0.021)
Control mean	0.501	0.043	0.037	0.119	0.299
# respondents	1626	1626	1626	1626	1626
# cohorts	30	30	30	30	30
Adjusted R2	0.212	0.047	0.218	0.048	0.050

Coefficients are from regressing an indicator for each employment sector on a treatment indicator and stratification block fixed effects. Sector indicator variables classify the types of jobs participants entered into following the job readiness program. All sector indicators are coded as zeros for candidates who are not employed immediately after the job readiness training program. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. None of the analysis in this table is prespecified.

Table B.9: Treatment Effects on Engagement

	(1) Engagement	(2) Curiosity	(3) Enthusiasm	(4) Energy
Treated cohort	-0.003	0.105	0.038	0.061
	(0.029)	(0.096)	(0.093)	(0.093)
Control mean	4.829	0.062	0.066	0.075
# respondents	1250	1602	1602	1602
# cohorts	29	30	30	30
Adjusted R2	0.009	0.096	0.049	0.063

Coefficients are from regressing an indicator for each engagement measure on a treatment indicator and stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. The engagement variable in column 1 is a self-report collected in an end-of-training survey about how useful the candidate found the job readiness training program, on a scale from one to five. Columns 2-4 report treatment effects on training managers' evaluations of candidates, averaging standardized scores for the last three weeks of the training program.

Treatment effects on LinkedIn use appear to explain most of the treatment effects on employment, but other mechanisms may also be relevant. First, LinkedIn training may change the nature of the job readiness program in ways that are unrelated to LinkedIn usage. For instance, treatment may increase candidates' enthusiasm for the program and hence increase the effort they exert, or it may lead to complacency and hence decrease the effort they exert. We estimate treatment effects on self-reported measures of engagement in the program as well as trainer reports of candidates' energy and intellectual curiosity. Treatment has no statistically significant effect on any of these measures, although some effects are not trivial relative to the control group means (Table B.9). The drop-out rate from the program is roughly 13% in both treatment and control cohorts (p-value for test of equal means = 0.62). These results suggest that our intervention was a small curriculum change rather than a fundamental reorganization of the job readiness program.

Second, LinkedIn training may change candidates' beliefs about their labor market prospects through some mechanism other than information acquisition. For example, using LinkedIn might expose candidates to role models that change their ideas about what jobs are available to them and hence change their job search behavior or job performance (Beaman et al., 2012; Bernard et al., 2014; Dee, 2005; Fairlie et al., 2014; Greene et al., 1982; Stout et al., 2011). This mechanism may be particularly important for this sample in this context, where there are large gaps in labor market outcomes by race and gender and most candidates are from disadvantaged backgrounds. This mechanism still attributes employment effects to LinkedIn use and training, but not to changes in conventional job search or hiring processes. We measure indices of candidates' sense of control over their lives (locus of control), excitement, and trust in others following Lippman et al. (2014). We also measure the wage candidates aspire to earn as a measure of their economic aspirations, following Orkin et al. (2020). Finally, we measure candidates' reservation wages. The only treatment effects are small increases in reservation wages and the wages candidates aspire to earn (Table B.10, columns 1-2). These increases only appear 6 to 12 months after the program, not during the program. So these may be driven by the employment effects, rather than vice versa.

Third, there may be spillover effects of training on candidates in control cohorts. Five of the 15 control cohorts received at least one day of training while a treated cohort was being trained in the same location, so interaction is possible. Spillover effects might attenuate the treatment effects on employment – if control candidates learn to use LinkedIn from treated cohorts – or overstate the effects – if control candidates compete against treated candidates for the same jobs. The latter mechanism is particularly plausible in this setting. Harambee helps multiple candidates from the same cohort to apply for the same jobs at the same firms. They may also help candidates from adjacent cohorts to apply for different jobs at the same firms. We test for spillover effects by adding an indicator for overlapping cohorts to equation (1). Including this indicator does not substantially change the estimated treatment effects on employment or opening a LinkedIn account. The coefficient on the indicator is small and not statistically significant for all outcomes. This is not consistent with quantitatively important net spillover effects. However, we cannot rule out the possibility that control candidates learn something about using LinkedIn from treated candidates but that their gains from doing so are offset by competing against treated candidates with more comprehensive LinkedIn training.

Table B.10: Treatment Effects on Aspirations

	(1) Aspiration wage	(2) Reservation wage	(3) Excitement about future	(4) Trust in future	(5) Locus of control
Panel A: End of Program					
Treated cohort	0.047	0.043	0.036	-0.023	0.026
	(0.037)	(0.039)	(0.021)	(0.015)	(0.024)
Control mean	10.518	9.249	0.646	0.680	0.535
# respondents	1247	1233	1252	1252	1252
# cohorts	29	29	29	29	29
Adjusted R2	0.097	0.149	0.001	0.020	0.008
Panel B: Six Months After Program Completion					
Treated cohort	0.090	0.023	-0.002	0.037	-0.023
	(0.043)	(0.025)	(0.031)	(0.020)	(0.023)
Control mean	10.469	9.289	0.706	0.680	0.723
# respondents	1119	1119	1119	1119	1119
# cohorts	30	30	30	30	30
Adjusted R2	0.101	0.081	-0.006	0.004	0.003
Panel C: Twelve Months After Program Completion					
Treated cohort	0.052	0.061	0.005	-0.007	0.022
	(0.034)	(0.032)	(0.026)	(0.025)	(0.027)
Control mean	10.565	9.435	0.708	0.715	0.695
# respondents	988	988	988	988	988
# cohorts	30	30	30	30	30
Adjusted R2	0.070	0.082	0.014	0.004	0.001

Coefficients are from regressing an indicator for each aspirations measure on a treatment indicator and stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. All measures are self-reports collected in an end-of-training survey (panel A) and follow-up phone surveys six and twelve months later (panels B and C). Reservation and aspiration wage have been transformed using the inverse hyperbolic sine function. Excitement about the future, trust in the future, and locus of control are indicators for above-median values of the underlying continuous scores.

C Alternative Approach to Explaining Treatment Effects

Treatment increases LinkedIn use on every observed margin, but can this quantitatively explain the increase in employment? We answer this question using a reduced-form framework that decomposes the treatment effect on employment into two components, one explained by LinkedIn use and one not (Robins and Greenland, 1992; Imai et al., 2010; Heckman and Pinto, 2015). We estimate the system

$$\text{Employ}_{icr} = T_{cr} \cdot \beta + \mathbf{S_{cr}} + \epsilon_{icr} \tag{2}$$

$$LI_{icr} = T_{cr} \cdot \gamma + \mathbf{S_{cr}} + \nu_{icr} \tag{3}$$

$$\text{Employ}_{icr} = T_{cr} \cdot \tilde{\beta} + LI_{icr} \cdot \alpha + \mathbf{S_{cr}} + \varepsilon_{icr}. \tag{4}$$

β is the average effect of treatment on employment and γ is the average effect of treatment on LinkedIn use. $\alpha \cdot \gamma$ is defined as the 'indirect effect' of treatment on employment via LinkedIn use and $\tilde{\beta}$ is defined the 'direct effect' of treatment on employment not explained by LinkedIn use (Robins and Greenland, 1992; Heckman and Pinto, 2015). By construction, $\alpha \cdot \gamma + \tilde{\beta} = \beta$, so $S_1 = \frac{\alpha \cdot \gamma}{\beta}$ is the share of the total treatment effect attributable to the indirect path through LinkedIn use. Given the persistence of the employment effect, we focus on explaining treatment effects on end-of-program employment rather than later employment.

Using this approach, LinkedIn use explains at least two thirds of the treatment effect on end-of-program employment. Treatment increases employment by 7 percentage points and the probability of having a LinkedIn account by 32 percentage points (Table C.1, panel A, column 1). The indirect effect accounts for 73% of the treatment effect on initial employment with standard error 31 percentage points (panel B, column 1). The direct effect of treatment on employment, not explained by LinkedIn use, is only 1.9 percentage points and is not statistically significantly different to zero. Having a LinkedIn account is not a perfect measure of LinkedIn use. We therefore repeat the exercise replacing this indicator with the LinkedIn usage index introduced in Section 3: the first principal component of having an account, opening an account during training, the number of profiles viewed, the number of jobs viewed, profile completeness, the number of times the profile is viewed, the total number of connections, the number of connections with bachelors degree, the number of connections with managerial jobs, and the number of job applications submitted on LinkedIn.[15] This shifts $\hat{S}_1$ to 0.67 with standard error 0.25 (panel B, column 2).

The indirect effect is identified under the assumption that there are no omitted variables correlated with

[15]The first principal component accounts for 48% of the variation in these ten measures. The index is missing for 21% of the sample due to missing values in the administrative data from LinkedIn.

Table C.1: Relationship between Treatment, Initial Employment, and LinkedIn Use

LinkedIn use measure	(1) LinkedIn account	(2) Summary index
Panel A: Parameter estimates		
Treatment effect on employment (β)	0.070	0.083
	(0.020)	(0.020)
Treatment effect on LinkedIn use (γ)	0.321	0.954
	(0.049)	(0.145)
Treatment effect on employment | LinkedIn use ($\tilde{\beta}$)	0.019	0.028
	(0.026)	(0.025)
Association between employment & LinkedIn use | treatment (α)	0.158	0.058
	(0.027)	(0.014)
Association between employment & LinkedIn use in control group (δ)	0.146	0.059
	(0.026)	(0.017)
Panel B: Share of treatment effect explained by LinkedIn use		
$S_1 = \alpha \cdot \gamma / \beta$	0.729	0.668
	(0.306)	(0.253)
$S_2 = \delta \cdot \gamma / \beta$	0.672	0.678
	(0.266)	(0.259)
Sample size	1626	1288

Panel A shows estimates of the parameters of equation systems (2) - (4) and (5) - (7). Panel B row 1 shows the share of the treatment effect on employment explained by the treatment effect on LinkedIn use in the system (2) - (4): $S_1 = \frac{\alpha \cdot \gamma}{\beta}$. Panel B row 2 shows the share of the treatment effect on employment explained by the treatment effect on LinkedIn use, scaled by the relationship between employment and LinkedIn use in the control group in the system (5) - (7): $S_2 = \frac{\delta \cdot \gamma}{\beta}$. The equations are estimated as systems using using only observations with non-missing values for both employment and the relevant LinkedIn use measure. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. The standard errors on S_1 and S_2 are estimated using the Delta method. All models include stratification block fixed effects. None of the analysis in this table is prespecified.

both LinkedIn use and employment.[16] This is a strong assumption and we present three sensitivity analyses related to this assumption. First, we estimate the system (2)-(4) conditional on age, gender, education, past employment, and psychometric assessment scores. This increases the share of the employment effects explained by LinkedIn use by three percentage points.

Second, we repeat the analysis using an indicator for opening a LinkedIn account during the job readiness training program. Relative to the indicator for having a LinkedIn account used above, this measure is less likely to be correlated with unobserved pre-treatment characteristics such as experience working in an environment where LinkedIn is widely used. This measure explains 50% (standard error 24 percentage points) of the treatment effect on employment. Even this measure may be correlated with unobserved characteristics such as candidates' openness to new technology. But the scope for bias from correlated

[16] In the potential outcomes framework, this assumption is called 'sequential ignorability.' Vansteelandt (2009) and Acharya et al. (2016) propose a modified approach called 'sequential g-estimation' that is identified under a slightly weaker assumption. We obtain almost identical results using their approach.

unobserved characteristics is smaller than for other measures of LinkedIn use.

Third, we repeat the analysis with a multidimensional measure of LinkedIn use to account for possible measurement error from collapsing use to a single measure. This addresses the possibility of measurement error violating the identifying assumption (Heckman and Pinto, 2015; VanderWeele, 2012). We replace the scalar LI_{icr} with the four measures of LinkedIn use presented in Table 7: standardized indices for measures corresponding to each of supply-side information, demand-side information, and connections, as well as the number of job applications submitted on LinkedIn. The four components jointly explain 82% of the employment effect (standard error 28 percentage points).

We also implement an alternative method to relate the treatment effects on employment and LinkedIn usage, similar to the method proposed by Gelbach (2016). This approach is based on the system

$$\text{Employ}_{icr} = T_{cr} \cdot \beta + \mathbf{S_{cr}} + \epsilon_{icr} \tag{5}$$

$$LI_{icr} = T_{cr} \cdot \gamma + \mathbf{S_{cr}} + \nu_{icr} \tag{6}$$

$$\text{Employ}_{icr} = LI_{icr} \cdot \delta + \mathbf{S_{cr}} + \eta_{icr}. \tag{7}$$

β is the average effect of assignment to treatment on employment and γ is the average effect of assignment to treatment on LinkedIn use. δ is the non-experimental relationship between employment and LinkedIn use, estimated using only control group data. We define $S_2 = \frac{\delta \cdot \gamma}{\beta}$ as the share of the treatment effect on employment explained by LinkedIn use. This measures 'how much' of the employment effect β can be explained by the LinkedIn use effect γ via the non-experimental relationship δ.

Using this approach, LinkedIn use explains roughly two thirds treatment effect on initial employment. Defining LinkedIn use as having an account generates $\hat{S}_2 = 67\%$, with standard error 27 percentage points (Table C.1, panel B, column 1). Measuring LinkedIn use with the summary index generates $\hat{S}_2 = 68\%$ with standard error 26 percentage points (panel B, column 2).

This approach assumes that an estimate of δ based on non-experimental variation captures the effect of an experimentally-induced shift in LinkedIn on employment. This assumption may be violated if marginal candidates induced to use LinkedIn by treatment use it differently for job search to inframarginal candidates who would use it anyway. This assumption may also be violated if there are omitted characteristics associated with both LinkedIn use and employment or if LinkedIn use is measured with error. The direction of the bias from omitted variables and measurement error is theoretically ambiguous.[17] Given these concerns, we

[17]Classical measurement error in LinkedIn use will lead to a downward-biased estimate of δ, though measurement error in

interpret this exercise as suggestive but not conclusive evidence that treatment effects on LinkedIn use can explain treatment effects on initial employment.

Across all of these approaches, treatment effects on observed LinkedIn use explain 50-82% of the treatment effect on employment. The remaining 18-50% may be explained by unobserved components of LinkedIn use (e.g. time spent on LinkedIn after the program finishes or specific information workseekers acquire from LinkedIn use) or entirely different mechanisms. As we do not observe all possible components of LinkedIn use, we interpret these results as evidence for a quantitatively important channel from LinkedIn to employment, rather than a precise description of this relationship.

this context is not necessarily classical. Omitted variables might be positively linked with both employment and LinkedIn (e.g. proactivity, digital proficiency) or negatively linked to one of them (e.g. selection into LinkedIn use due to unemployment).

D Deviations from the Pre-Analysis Plan

We pre-registered our research design on the AEA's RCT Trial Registry at the start of the intervention at https://doi.org/10.1257/rct.1624-9.1. In this appendix we describe some differences between the pre-analysis plan and final analysis reported in the paper. The differences are relatively small and follow the spirit of (Duflo et al., 2020).

The design and implementation of the intervention follow the preregistration. We had no scope to alter the sample selection process. As described in Appendix E.1, we drew our study participants from the pool of candidates enrolled in Harambee's job readiness training programs. Harambee's eligibility criteria and screening processes did not change at any point during the intervention. As prespecified, we conducted pair-wise randomization of 30 training cohorts, 15 of which would receive the LinkedIn training and 15 of which would not. We announced treatment assignments to training managers at the start of each training program. We co-developed the LinkedIn training curriculum with a senior Harambee staff member before writing the pre-analysis plan. The version of the curriculum included in the pre-analysis plan and in Appendix E.3 is the same version that we disseminated to the training managers responsible for implementation. As we discuss in Appendix B, the LinkedIn training program was not fully implemented in five of the cohorts assigned to receive treatment. In Table B.4, we report estimates of the treatment-on-the-treated effects that account for partial compliance.

Data collection largely adhered to the pre-analysis plan. We administered web-based baseline and end-line surveys at the respective beginning and end of each job readiness training program. As prespecified, we also administered follow-up surveys six and twelve months post-training. We planned to administer follow-up surveys via web or SMS. But we instead used phone surveys after a companion study found low rates of response to web- and SMS-based surveys in the same setting (Lau et al., 2018). As anticipated, Harambee provided us with administrative data on the characteristics of candidates at baseline and performance data on the performance of candidates during training. LinkedIn provided us with the site usage measures we anticipated but did not provide us with the data in the time frame we anticipated. Due to organizational changes within LinkedIn and the introduction of the European Union's General Data Protection Regulation (GDPR), we experienced delays in receiving the six- and twelve-month LinkedIn data. These delays do not systematically vary with treatment status.

Our analysis deviates from the pre-analysis plan in three small ways. First, we omit the prespecified training manager fixed effects because several program managers managed only one cohort and several cohorts were co-managed. Including these fixed effects in the employment regressions does not substantively change our conclusions, yielding only slightly larger treatment effects and standard errors. Our pre-analysis plan specified that we would control for baseline covariates that were not balanced across control and treatment cohorts. None of the baseline covariates we observe are unbalanced, so we do not control for any covariates.[18]

Second, we do not report treatment effects on twelve prespecified outcomes due to data quality or availability. We prespecified four measures of post-training job search and employment that we ultimately dropped from the survey instrument due to time constraints (job search strategy, additional training/education, difficulty obtaining employment, and part- or full-time status). In addition, we prespecified three outcomes related to labor market knowledge (knowledge of relevant skills, degrees, and companies) and three outcomes related to match quality (job satisfaction, perceived fit, promotion schedule) that we do not report due to ceiling effects. Finally, we prespecified two aggregate measures of LinkedIn usage that we do not report because they were constructed by LinkedIn using a proprietary algorithm that we could not independently verify (activity level and network power).

Third, we add some non-prespecified outcomes that we collected in response to reviewer feedback. We did not prespecify treatment effects on program completion and post-training job placements (Table 7, columns 2, 3, and 7; Table B.8), on opening a LinkedIn account during training (Table 2, column 2), or on the probability of being employed at both the end of the training program and the current wave (Table 4, column 2). The LinkedIn summary indices in Tables 2 and 7 were added in response to reviewer feedback; they are constructed from prespecified outcomes but are not themselves prespecified. The non-experimental associations between employment and LinkedIn use and the mediation analysis reported in Section 5 were not prespecified. All other analysis, including subgroup analysis, was prespecified in the pre-analysis plan.

[18]The administrative data we received from Harambee did not contain three baseline measures we expected to receive: information about disability status, mode of transportation, and airtime. We were unable to test for balance on those dimensions.

E Intervention Details

E.1 The Default Job Readiness Training

The job readiness training programs are run by the Harambee Youth Employment Accelerator, a social enterprise that builds solutions to address a mismatch of demand and supply in the youth labor market by connecting employers with first-time workseekers.

Candidates enter these job readiness training programs after a three-stage recruitment and selection process. First, candidates learn about Harambee from word-of-mouth, social media, or conventional advertising. They complete an application, typically online using a mobile device, that determines their eligibility. Candidates are eligible to proceed if they are age 18-29, have completed secondary school, have legal permission to work in South Africa, have no criminal record, have fewer than 12 months of formal work experience, and come from a 'disadvantaged' background. The definition of disadvantaged varied during the recruitment period but the goal is to exclude candidates from upper-income households with existing access to employment opportunities through referrals. The sample of eligibles is likely to be negatively selected on employment prospects relative to the general population.

Eligible candidates complete psychometric assessments in communication, numeracy, 'concept formation' (similar to a Raven's matrix test), and a career matching assessment designed to assess how well their habits match to different job types. Candidates who perform well in the first three assessments, match to white-collar jobs, and live near an area where Harambee anticipates demand for jobs are invited to job readiness training. The sample of training participants is likely to be positively selected on employment prospects relative to the sample of eligibles. We cannot characterize the employment prospects of the training participants relative to the general population.

The job readiness programs last 6 to 8 weeks and require full-time attendance. They cover simulations of workplace environments, team building, and non-cognitive skill development. The programs are explicitly designed for people with limited or no work experience, rather than designed to retrain displaced workers. Their goal is to help candidates find and retain jobs in sectors such as financial services, logistics, operations, manufacturing, or construction.

Harambee helps candidates apply to jobs at the end of training programs, including some jobs at firms where Harambee has long-term, actively managed relationships. Harambee has no role in firms' hiring processes after helping to set up initial interviews. Many active labor market programs offer this type of end-of-program application support, including many employment services funded by US federal and state governments.

E.2 Intervention Cost and Benefit-Cost Calculations

The intervention costs USD48 per candidate at the purchasing power parity exchange rate, or USD21 at the nominal exchange rate.[19] We estimate this figure by multiplying Harambee's average per-candidate cost of an 8-week job readiness program, USD3,833, by the share of the program time allocated to the intervention, 1.25%. Harambee allocated approximately 4 hours of each job readiness program to LinkedIn training: 1.5 hours in the first week, and five 30-minute sessions later in the program. The job readiness program cost covers staff time for training, administration, and liaising with employers about interviews; facility rental; IT costs; and a stipend of USD6 per participant.

The intervention increases employment by 7 percentage points in the sample of 890 treated candidates (using the estimate in column 1 of Table 3). This implies 62 more employed candidates and hence a cost of USD685 per additional candidate employed. This cost-per-placement is lower than almost any developing country program reviewed by McKenzie (2017). This cost reflects the way the intervention built on an existing program and may not generalize to a stand-alone LinkedIn training program.

We also calculate a pecuniary benefit-cost ratio by valuing the extra employment for two scenarios. First, we assign employed participants "typical" earnings for their sector. We assign USD16,000: the mean earnings for call center workers in urban locations with at most 3 years tenure in that job from South Africa's Quarterly Labour Force Surveys (QLFS) for 2017-2019. Under this assumption, treatment increases the average participant's annual earnings by roughly USD1,100 (= USD16,000 times the 7 percentage point employment effect). This implies a benefit:cost ratio of 23. Second, we make the much more conservative assumption that employed participants earn the statutory minimum wage of USD3 per hour and work full time, implying annual earnings of roughly USD6,050. Under this assumption, treatment increases the average participant's annual earnings by roughly USD420, implying a benefit:cost ratio of 8.7.

The benefit side of the benefit-cost calculation comes with several caveats. We do not directly observe participants' earnings, so both scenarios we consider require extra assumptions. The minimum wage scenario is extremely conservative, as the minimum wage is close to the 5$^{\text{th}}$ percentile of the national distri-

[19] We report all figures in 2017 USD with purchasing power parity conversion factors from http://wdi.worldbank.org/table/4.16, averaged over the study period.

bution of earnings for the employed (Finn, 2015).[20] The call center scenario assumes participants all work in call centers. This is plausible for most participants given the names of their employers and interviews with program staff, but we do not directly observe participants' job titles or descriptions. The QLFS data on call center workers' earnings have relatively small samples, as they account for only 0.7% of all workers surveyed for the QLFS. But the mean is not too far from the mean annual salary of roughly USD19,600 reported by the industry association (Business Process Enabling South Africa, 2018). The industry association values the mean non-salary benefits package at an extra USD4,700. We exclude non-salary benefits from the benefit-cost calculations using QLFS data, as the QLFS does not report the financial value of non-salary benefits.

The cost side of the benefit-cost calculation also comes with several caveats. We calculate the average per-candidate cost of implementing the intervention at Harambee's existing scale. This is likely to be higher than the marginal cost of training additional candidates, but we do not have data that allow an accurate split between fixed and variable costs. Running a stand-alone intervention outside of an existing active labor market program might entail substantially different costs. Similarly, running a stand-alone intervention might generate different benefits.

Despite these caveats, the benefit-cost ratios are so high that this program warrants policy attention. The LinkedIn training program is relatively short, uses an open-source curriculum, was not delivered by very highly paid specialists, and hence could plausibly be incorporated in existing active labor market programs operating in comparable economic settings.

E.3 LinkedIn Training Curriculum

The remainder of the appendix shows the curriculum given to Harambee job readiness training managers to help them train candidates to use LinkedIn. The training managers were trained by a senior Harambee staff member who co-developed the curriculum. The intervention curriculum was jointly developed by Harambee, LinkedIn, and the research team.

The intervention started with a one-hour presentation on LinkedIn in the first week of the job readiness program. Participants received additional in-person coaching, discussion sessions, and email tips in later weeks of the program. The initial presentation and subsequent sessions covered:

[20] We use the national minimum wage purely as an illustrative benchmark. This was only introduced in January 2019, toward the end of our survey period. Minimum wages before this varied by sector and geographic location. Given the national earnings distribution reported above, it is extremely unlikely that participants in our study earned on average lower than the national minimum wage.

- how to construct a profile;
- what information to include in a profile (e.g. work experience, education, volunteering);
- how to describe the job readiness training on a profile;
- how to join groups, including a group created for the members of each training cohort;
- how to identify groups for people working in a target occupation;
- how to make connections and what types of connections can be useful;
- how to view profiles of companies that have previously hired graduates of the job readiness program; and
- how to ask for recommendations on LinkedIn and get a recommendation from the manager of the job readiness program.

Introducing LinkedIn to Workforce Training Participants

A Curriculum

Developed in partnership by
Harambee Youth Employment Accelerator and RTI International

A Global Center for Youth Employment Initiative

Global Center for Youth Employment

harambee
WORK FOR WORK

RTI INTERNATIONAL

INTRODUCTION: This curriculum presents an approach for introducing young people to LinkedIn and other digital professional networks, to help them understand the multiple functions of the sites (signaling, networking, labor market information) and develop the habit of using such tools throughout their careers. This curriculum was developed by RTI International and Harambee Youth Employment Accelerator in South Africa and is calibrated for a short training course, such as Harambee's 8-week training programs, though it could be easily adapted for short or longer training experiences.

The curriculum developers intentionally took a "light touch" approach, with a recommended one-hour introduction to LinkedIn in week 1, followed by seven weekly "nudge" emails that contain short instruction or motivation and related article links or videos. The material spans topics ranging from setting up an account, building a profile, making connections, exploring job openings, and joining industry groups, to reading articles and opinions from one's future professional field. Trainers also use three 30-minute in-person check-ins, one in each of weeks 2, 5, and 7, to answer questions, provide guidance, and test participants' knowledge. When the training is complete, the trainers connect with their participants on the site, write them a boiler plate recommendation, and invite them to join a LinkedIn alumni group.

The Global Center for Youth Employment (GCYE) offers this curriculum now as an open source resource that can be used to introduce LinkedIn to program participants. LinkedIn maintains a micro-site of high quality, professionally produced training materials, to be used in concert with this resource that can be included as presentations or handouts within this structure. An example of a LinkedIn-produced profile "checklist" is provided in Annex A of this document. More information on the LinkedIn materials is available on this LinkedIn google drive. LinkedIn plans to develop materials tailored for job seeking populations throughout the developing world in the future.

BACKGROUND: This curriculum was developed and piloted as a part of an impact evaluation conducted by RTI International, Duke University, and Harambee. The evaluation is a GCYE initiative and seeks to understand the education- and work-related impacts among marginalized work seekers who used LinkedIn vs. those among control group populations who did not. LinkedIn supported the study by providing data on (consenting) user profiles, networks, and site usage. Results were measured at training baseline, end-line, and 6 and 12 months post-graduation. More information on the study can be found on the GCYE website: www.employyouth.org

USAGE: This curriculum is intended to be used as an integrated part of larger training programs, likely short-course programs. However, it could easily be condensed and delivered in a concentrated half day, or expanded and used across a semester or year. The emphasis here falls on developing the demand and interest among young people to use professional networking sites, over time—not through force feeding or required usage. If you use, adapt, or improve the curriculum, please do let us know.

Thanks!

The Global Center for Youth Employment— gcye@rti.org

Week	Instruction to Training Manager	Details
Week 1: Getting Started	• Present "Introducing LinkedIn" to candidates • Elicit discussion with candidates • Candidates spend dedicated time to join LinkedIn and start exploring it for at least 30 minutes	Refer to Introducing LinkedIn presentation
	• Confirm email addresses before sending LinkedIn invitation • Email invitation from Training Manager	**EMAIL #1** Hello everyone! You are about to embark on your journey to securing a job and building your career. Are you interested in becoming a true professional and building your professional network? If you are nodding away, click on the link below to join the best online professional network: https://www.linkedin.com/ It's easy to sign up. All you need is: • An email address, a picture of yourself, and some thought about your work experience and educational background. • Follow the steps on LinkedIn to help you build your profile. If you want to know more about LinkedIn before signing up, check out this video from the link below: https://www.youtube.com/watch?v=ZVlUwwgOfKw Looking forward to inviting you to join our cohort group once you have signed up!
	Conducts face-to-face check-in after Email #1 • After checking to see who has signed up, have a conversation to find out why those who have not, haven't • Team pop quiz on LinkedIn #1 • Discuss why LinkedIn may be useful for candidates	

Week	Instruction to Training Manager	Details
	Send out Email #2 before the end of the week with tips for building a great profile	**EMAIL #2** Hello everyone! Now that you have signed up, you may want to know more about how to use LinkedIn to develop your profile and help you build your professional network. I strongly encourage you to check out the links below: **THE POWER OF A GOOD PROFILE** https://blog.linkedin.com/2015/05/13/how-linkedin-connects-me-to-future-opportunities https://www.linkedin.com/pulse/how-create-killer-linkedin-profile-get-you-noticed-bernard-marr As you build your profile and create a great network here are some things to think about… • What would you want your first manager/employer to see about you? • What would you want your colleagues to know about you if you connect with them, when starting your first job? • What should you include in your profile summary? • Once you have your profile, try to connect with other people you know to build your network. • Please don't worry if your profile is not perfect, or very long – you can fill it in over time, but you have to start somewhere! Now that you have a profile, connect with others in your training group and alumni by joining your training cohort group and the training program alumni groups on LinkedIn. Leave a comment/inspirational quote to motivate others in the group. **TOP TIP:** When describing your Harambee work experience you should paste the following: **JOB TITLE**: Work Readiness Program candidate

Week	Instruction to Training Manager	Details
		COMPANY: Harambee Youth Employment Accelerator **TIME FRAME:** (Year of your program) **DESCRIPTION**: The Harambee Youth Employment Accelerator Bridging Program is an intensive 8-week, unpaid work simulation experience that accelerates youth into first time job success and career progression by instilling behaviors and foundation skills needed for succeeding in the world of work. These include attendance, punctuality, positive attitude, energy, and curiosity in combination with skills development in business communications, call center theory and simulation, computer skills, sales, and customer service experience. Looking forward to sharing information with you on our group! Regards, Your Training Manager
Week 2 Creating Your Profile & Building Your Network	**Face-to-Face check-in after Email #2** • Discuss what makes a great profile – what parts of your profile can help you now before you start work; link to interview preparation: – What experience have you had volunteering, working in your community that could add value to your profile in the absence of work experience? • What is a professional network, and how can you start to build a good network? • Find out who has joined the group/Why/Why not	

Week	Instruction to Training Manager	Details
	Hand out LinkedIn print out to each team for further investigation – *Profile Checklist* and *Profile Quick Tips and Personal Brand* from the LinkedIn micro-site	
	NUDGE: • Email a series of links that share useful information about LinkedIn and interesting articles/info/groups you can access on LinkedIn • Utilize this LinkedIn presentation on building your network. • Where possible, upload the link to the cohort group on LinkedIn • Encourage sharing of new information with one another both online and through the face-to-face sessions	The training manager should send out suggestions and links around building a network and sharing information. The material should be relevant and engaging for candidates – something that captures their interest. **EMAIL #3** Hello everyone! Now that you're on your way to building a great profile, you can really get started on building your network! Connecting with the right people, group, and companies can help you to build a great professional network. **TOP TIP**: A great place to start is by connecting with everyone you already know – old friends, family connections, or old school connections and work colleagues. You never know what opportunities you may find one day through your personal network. BUT, when you plan to connect with people you don't know or haven't worked with before, you should first ask yourself: will this person or group add value to my career and can I offer them value in return? Do some research on LinkedIn to find people you know, companies and groups that you think may be useful or interesting to follow or join considering the type of entry-level job opportunities you think you may interview for at the end of your program. If you want to know more about why building your network is important for your career and how to grow your network, I suggest you check out some of these links below!

Week	Instruction to Training Manager	Details
		https://www.youtube.com/watch?v=JmvumZbpaNI&feature=youtu.be http://www.careerealism.com/linkedin-invitation-tips/ Regards, Your Training Manager
Week 3: Complete Your Profile	**NUDGE** Email a message suggesting why completing a profile as far as they can while in training is worthwhile, and then provide links for employers and pulse channel to follow	The training manager should send out an email suggesting that candidates revise their profile and providing some useful groups to think about joining and companies to follow. **EMAIL #4** Hello everyone! Now that you have started connecting with others, and you may have seen what other people's profiles look like, I suggest you visit your own profile and add some stuff to make it more interesting or more professional. Write down what you have put down as your profile summary to unpack in the next check in session so we can share and help everyone to improve. I also highly recommend that you check out the following research done on what completing your profile can do for you: https://www.linkedinsights.com/why-you-should-complete-your-linkedin-profile/ Search on LinkedIn for professional groups and join them as you continue to build your network. Here are some examples: • *Contact Centre and Call Centre community* • *Customer Service Champions.* If you find anything interesting that you think is worth sharing, post it to our group.

Week	Instruction to Training Manager	Details
Week 4: Using LinkedIn for Job Prep	**Face-to-face check-in after Emails #4 and #5:** • Connect the interview prep process (at this stage in the Harambee training) to the development of the candidates' profiles and their insights from networking (joining groups/following companies). What can they share that will add value to their profile and how they can use their LinkedIn profile to help sell themselves in an interview? • Connect to volunteering, achievements, how one's profile can add value to one's CV • Have candidates share info or articles/groups/companies they have joined or have found interesting • Hand out LinkedIn print out of writing, reading, sharing on LinkedIn • Team pop quiz on LinkedIn #2	
Week 5: Labor Market and Industry Info on LinkedIn	**NUDGE** Email a message suggesting why completing a profile as far as they can while in training is worthwhile, and then provide links for employers and pulse channels to follow	The training manager should send out links to relevant employers/companies/articles that candidates can follow and suggestions to follow the LinkedIn Pulse Career Channel (see links in email – the training manager may add one or two extra links for relevant companies) **EMAIL #5:** Hello everyone! Here are a few links to follow some of our employers on LinkedIn as you start to think about new employer networks and what employers expect from you. Also check and see if you have any connections at these companies!

Week	Instruction to Training Manager	Details
		https://www.linkedin.com/company/standard-bank-south-africa?trk=affco https://www.linkedin.com/company/4731?trk=vsrp_companies_hero_name&trkInfo=VSRPsearchId%3A4425198414465428567 26%2CVSRPtargetId%3A4731%2CVSRPcmpt%3Ahero https://www.linkedin.com/company/614583?trk=vsrp_companies_res_name&trkInfo=VSRPsearchId%3A4425198414465442430 80%2CVSRPtargetId%3A614583%2CVSRPcmpt%3Aprimary https://www.linkedin.com/company/17634?trk=vsrp_companies_cluster_name&trkInfo=VSRPsearchId%3A442519841447136489971%2CVSRPtargetId%3A17634%2CVSRPcmpt%3Acompanies_cluster https://www.linkedin.com/company/12696?trk=vsrp_companies_res_name&trkInfo=VSRPsearchId%3A442519841447136666271%2CVSRPtargetId%3A12696%2CVSRPcmpt%3Aprimary
Weeks 6 and 7: Become a Strong Life-Long Learner on LinkedIn	**NUDGE** Suggest that candidate read articles for insight into how to be a great performer at work and invitation to join the Harambee Alumni Group. • Use this LinkedIn presentation on updating one's profile over time.	The training manager should send out an email with links relevant to attitude, performance, and work. There is also a link that goes out here to join Harambee alumni group. **EMAIL #6** Hello everyone! You now have a profile; perhaps you've joined a group or two, and you are following some great companies. Well done! You are starting to build your network so keep at it! But remember a great profile and a powerful network is only the first step. You also have to perform at work to build and maintain your professional reputation so people trust what they see on your LinkedIn profile. Check out these articles about how to be a great performer at work:

Week	Instruction to Training Manager	Details
		https://www.linkedin.com/pulse/eight-tips-being-great-employee-curtis-rogers https://www.linkedin.com/pulse/why-attitude-more-important-than-iq-dr-travis-bradberry I also strongly encourage you to join the training Alumni Group – this group will be a powerful professional support network to help you stay focused and progress in your career. Regards, Your Training Manager
Week 6	**Face-to-face check-in after Email #6:** • Have a follow up conversation about what candidates have found regarding performance in the work place – why is it important to match what you do with your online brand? • Discuss why being part of the Harambee alumni group can help build a career • Team pop quiz on LinkedIn #3	
Week 7	**Final check-in week 7:** • Who will use LinkedIn? Why/Why not? • How can you use it to benefit your career when you get to work? • What have you enjoyed/found challenging about using this social media platform?	
Post-Training	**NUDGE** Send out final Email #7 with a link about posting and publishing on LinkedIn and then some information about asking for recommendations – the ins and outs of asking for recommendations	**Email #7 (week after end of training)** Hello everyone! Now that you have completed your bridging program and some of you may have started work already, you will continue to build a powerful profile as you gain experience and grow your network. When you have settled in

Week	Instruction to Training Manager	Details
		to your new work environment, you might consider publishing a post on LinkedIn to share your experience and advice for other people who might be on a similar journey to you. Remember: Anything you post says something about your personal brand, so post wisely! Check out these links to learn how to publish a post and what's worth writing about: https://students.linkedin.com/student-publishing (cut and paste this link) Look at monthly topics on the home page to give you an idea of what's worth writing about at different times of the year! http://blog.linkedin.com/2015/04/15/why-i-publish-on-linkedin-the-power-of-storytelling/ Also, once you have been working for a while, you may want to ask for recommendations from your colleagues to enhance your profile. BUT first check out this link with tips on asking for recommendations: http://www.likeable.com/blog/2014/10/how-and-when-to-ask-for-a-linkedin-recommendation Wishing you the best of luck on your career! Regards, Your Training Manager

Annex: Proposed Descriptions That Can Be Adapted per Training Managers' Needs

Generic recommendation comment that can be edited as per training manager's needs:

I am pleased to say that ___________ completed the XYZ training program successfully and has met the necessary criteria to succeed as a first-time employee. This candidate has shown the ability to deliver work under pressure, work with and contribute to a team, and to manage his/her performance at work.

Proposed Summary for Harambee Alumni group

This group is an alumni group for all people who have completed a bridging program. It is a professional support group to help Harambee alumni stay focused and progress in their careers.

Description for cohort group purpose:

This group is your first professional network. It is for sharing professional tips, interesting articles, and information that you find or learn about. The group may also be used as a forum for feedback on projects, presentations, and any work you may want to share that you feel will contribute to other people's learning.

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