UNIVERSITY OF ALBERTA
FACULTY OF ARTS
Department of Economics

Working Paper No. 2019-14

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

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September 2019

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September 11, 2019

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 one of these platforms. Training increases employment at the end of the program from 70 to 77% and this effect persists for at least twelve months. Treatment effects on platform use explain most of the treatment effect on employment. Administrative data suggest that platform use increases employment by providing information to prospective employers and to workseekers. It may also facilitate referrals but does not reduce job search costs or change self-beliefs.

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

Keywords: employment, information frictions, online platforms, social networks, field experiment

*This paper would not be possible without exceptional work on curriculum development and intervention delivery by staff at the Harambee Youth Employment Accelerator (particularly Edwin Lehoahoa, Jenny Mamdoo, Gina Stoltz, and Rob Urquhart) and valuable assistance from staff at LinkedIn (particularly Meg Garlinghouse). We acknowledge funding from RTI's Global Center for Youth Employment. We are grateful for helpful discussions with Luis Crouch, Rebecca Dizon-Ross, Andrea Kiss, Rachel Kranton, Kate Orkin, Seth Sanders, Anna Wetterberg, and particularly Peter Arcidiacono and Jeffrey Smith. This project was pre-registered on the AEA's RCT Trial Registry at https://doi.org/10.1257/rct.1624-9.1 and approved by Institutional Review Boards at Duke University (#D0365) and RTI (#13900).

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1 Introduction

In many countries, youths face substantially higher rates of unemployment, underemployment, and unstable employment than older labor market participants (Filmer and Fox, 2014; International Labour Organization, 2017). These patterns are consistent with many economic factors and a growing body of research demonstrating that labor market information frictions impede transitions into employment, particularly into formal, stable employment (Caria and Lessing, 2019). Information frictions may be especially important for young workseekers, who may lack references from past work experience that would help them signal their productivity to prospective employers, lack access to referral networks, or make poor job search decisions due to lack of search experience. Even if these frictions only temporarily distort transitions into employment, temporary distortions can have long-term consequences in both developed and developing country labor markets (Kahn, 2010; Oreopolous et al., 2012; Ismail and Kollamparambil, 2015; Kuchibhotla et al., 2017). And while information frictions alone may not explain a large share of youth unemployment, they may be easier and quicker to address than factors such as aggregate skills mismatches.

Online job search, networking, and hiring platforms offer the prospect of reducing information frictions. They may increase supply-side access to information about specific employers and labor markets, increase demand-side access to information about prospective workers through public profiles, and facilitate both demand- and supply-side network connections that can share information and referrals. 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.[1]

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 and randomly assign some participants to a short LinkedIn training course. LinkedIn is a widely used platform in South Africa, with 264,000 jobs posted in early 2019 and 7.1 million active profiles (roughly 40% of the workforce). The course trains participants to open LinkedIn accounts, build their profiles, create networks, and search and apply for jobs. We measure their employment status, job attributes, and platform use with independent survey data and LinkedIn administrative data at the end of the job readiness program, six months later, and twelve months later.

We find a substantial and persistent treatment effect on employment. Treatment increases the probability

[1] Several studies show that there are quantitatively important information frictions even on these platforms (Pallais, 2014; Agrawal et al., 2016; Stanton and Thomas, 2016).

of post-program employment from 70 to 77%. 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 of the program. The post-program employment effect persists for at least one year after treatment. Under conservative assumptions, this implies a benefit-cost ratio of 10 over the first post-program year. There is some survey attrition but this does not differ by treatment status and all employment effects are robust across multiple methods that account for the missing data.

Treatment also increases the probability of having a LinkedIn account and multiple measures of platform use and on-platform networks. All platform use measures rise quickly after treatment and do not show a consistent upward or downward trend over the following year. Our measures of LinkedIn use explain at least half of the treatment effect on employment. We demonstrate this using a reduced-form approach that decomposes the treatment effect on employment into a component explained by treatment effects on LinkedIn use and a residual, following Imai et al. (2010) and Heckman and Pinto (2015).

These results show that LinkedIn training increases LinkedIn use, which helps workseekers convert job applications to job offers and retain these jobs. LinkedIn use might increase job offers through at least six economic mechanisms. Our experiment is not designed to separately identify the role of each mechanism, but we observe partial measures of each mechanism, which we use to evaluate their importance. Our results are consistent with LinkedIn use alleviating either or both of two information frictions. LinkedIn use may provide *demand-side information*, which helps firms screen workseekers, and *supply-side information*, which helps workseekers target job search and perform well in interviews. This reinforces recent work showing that information frictions distort job search and hiring in South Africa (Abel et al., 2019; Carranza et al., 2019; Pugatch, 2019). Some but not all of our results are consistent with a role for on-platform *referral networks*. Our results are not consistent with three other mechanisms. We see very low levels of on-platform *job search*, although LinkedIn allows users to quickly and cheaply search for job postings and submit applications. LinkedIn use does not change workseekers' *program engagement* with the existing job readiness programs or their *self-beliefs*.

Our findings contribute to three literatures. First, we contribute to the literature on information technology in job search and hiring. IT interventions have been proposed for building workseekers' skills, helping firms screen prospective workseekers, lowering job posting costs, lowering search costs, and motivating workseekers to minimize risks of discouragement. However, few interventions in this space have been rigorously evaluated. We provide the first experimental evidence that training workseekers to use an existing job search and networking technology can improve their employment outcomes. This complements recent work showing that Facebook access can increase employment and earnings, potentially by facilitating referrals (Armona, 2019). Gee et al. (2017) also document patterns of job-switching consistent with referrals from Facebook friends. Related work shows that algorithmic hiring recommendations can lower turnover, while algorithmic job search recommendations can generate more interviews (Hoffman et al., 2018; Horton, 2017; Belot et al., 2019). In contrast, Kroft and Pope (2014) find that the advent of Craigslist lowered job posting costs without changing employment, potentially because this occurred in an environment with high baseline employment.

Second, we contribute to a growing literature on information frictions in the labor market. On the demand side, employers may lack information about prospective workers' skills and productivity, distorting hiring decisions and wage offers (Spence, 1973; Farber and Gibbons, 1996; Altonji and Pierret, 2001; Lange, 2007). On the supply side, workseekers may lack information about job attributes, application processes, or skills demanded by employers. Recent work has shown that improving firms' information about workseekers' skills or past performance can change search and employment outcomes (Pallais, 2014; Abebe et al., 2016; Bassi and Nansamba, 2017; Carranza et al., 2019). Similarly, improving workseekers' information about job postings can change job search behavior (Belot et al., 2019; Altmann et al., 2018; Ahn et al., 2019).[2] We study a population where information frictions are likely to matter. Our sample of workseekers are from disadvantaged backgrounds, have little formal work experience, and have limited post-secondary education. We show that a light-touch intervention using a decentralized, existing platform can alleviate information frictions for this disadvantaged population, without the need for heavier-touch interventions like centralized matching, personalized job search counseling, or productivity assessments.[3] However, both control and treated workseekers have performed well on psychometric screening tests and receive some job readiness programming. Our findings may not generalize to unscreened workseekers without any support.

Third, our work relates to the large literature on active labor market programs (ALMPs). ALMPs are widespread in both developed and developing countries, though systematic reviews show evidence on their effectiveness is mixed (Heckman et al., 1999; Card et al., 2017; McKenzie, 2017; Kluve et al., 2019). We

[2]A related literature documents how firms and workseekers use referrals to overcome information frictions (Topa, 2001; Ioannides and Loury, 2004). Hiring through referrals can increase job performance but referrals may be driven by social ties, limiting performance gains and contributing to inequality (Beaman and Magruder, 2012; Pallais and Sands, 2016; Beaman et al., 2018; Heath, 2018; Witte, 2019).

[3]Our intervention may facilitate firm-worker matching by increasing platform use. But any matching that takes place is decentralized, not managed centrally as part of the treatment. The literature on centralized matching has yielded mixed results, with few studies finding large positive effects on employment (Groh et al., 2015; Beam, 2016; Abebe et al., 2017).

show that a quick (4 hour) and relatively cheap (US$48 at purchasing power parity exchange rate) addition to an existing ALMP can substantially change employment outcomes. Given the popularity and persistence of ALMPs, there may be high social value to rigorously testing specific design tweaks like the one we consider. In a similar spirit, other work has shown that labor market effects of ALMPs can be sensitive to design changes: delivering job counseling via public versus private providers, delivering information to workseekers using different media, adding job counseling to financial incentives for search, and varying flexibility in training financing (Friedlander et al., 1997; Perez-Johnson et al., 2011; Dammert et al., 2013; Behaghel et al., 2014). McCall et al. (2016) review related work on heterogeneous ALMP impacts across different providers, organizational features, and rules for selecting participants and assigning them to providers.

In Section 2 of the paper, we describe the LinkedIn platform, the training course, and an informal conceptual framework showing the economic mechanisms through which LinkedIn use might change employment. We then describe the economic context and sample, our data, and our research design. In Section 3, we report treatment effects on transitions into employment, employment persistence, and job attributes. In Section 4, we report treatment effects on LinkedIn use and show that these explain a large share of the treatment effect on transitions into employment. Throughout sections 3 and 4 we link the patterns of treatment effects on employment and LinkedIn use to our conceptual framework. We conclude in Section 5 and briefly discuss what our results imply for online professional networking training outside of job readiness programs and general equilibrium effects of large-scale increases in networking. As a preview on the general equilibrium question, the experiment generates a very small market-level increase in LinkedIn use from a large base. But our treated candidates often compete against each other for the same jobs and are still employed at higher rates, providing suggestive evidence against complete crowd-out.

In Appendix A, we report a series of robustness checks on the employment results, including methods to account for survey non-response. In Appendix B, we display secondary results mentioned in the main paper. In Appendix C, we describe the intervention in more detail and report cost and benefit-cost calculations.

2 Setup

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, search and apply for jobs, learn about the labor market by reading articles, and complete online training courses. Employers can create accounts and use the platform to post vacancies, solicit applications, and screen applicants based on user profiles.

The existing 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 South African youth labor market by connecting employers with first-time workseekers. The programs last 6-8 weeks and cover simulations of workplace environments, team building, and non-cognitive skill development. They are designed to help candidates find and retain jobs in sectors such as financial services, sales, 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. Even at these long-term partners, Harambee has no role in firms' hiring processes after helping to set up initial interviews. Many active labor market programs offer similar job application support, including many employment services funded by US federal and state governments.

We work with 30 cohorts trained by Harambee between May 2016 and January 2018 in four large cities in South Africa (Cape Town, Durban/eThekwini, Johannesburg, and Pretoria/Tshwane). We split the sample into 15 control and 15 treated cohorts. There was no scope for selection of either participants or training managers, as Harambee only learned the cohort-level treatment assignments on the first day of training. The control cohorts received Harambee's standard job readiness program.

In treated cohorts, roughly 4 hours of standard programming was replaced by training on how to use LinkedIn to learn about the labor market, search for jobs, and apply for jobs. The intervention started with a one-hour presentation on LinkedIn in the first week of the job readiness program. In the later weeks of the program, participants received additional in-person coaching, discussion sessions, and emails with tips and encouraging messages. 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 references. Participants are encouraged to list the job readiness program on their profile, get a reference from their program manager, and connect with alumni of the program. The intervention curriculum was jointly developed by Harambee, LinkedIn, and the research team. Appendix C.3 shows the reference guide given to program managers when they were trained to deliver the curriculum, including a list of all content covered.

The treatment displaced roughly 4 hours of Harambee's standard job readiness program over 6-8 weeks and cost approximately US$48 to deliver per candidate (US$20 at the nominal, rather than purchasing power parity, exchange rate). Appendix C.2 contains detailed cost calculations.

2.2 Conceptual Framework

In this section we describe six ways LinkedIn training and use can change labor market outcomes. Our discussion is deliberately informal and intended to guide interpretation of the results. The experiment is not designed to separately identify each of these mechanisms. But the mechanisms generate slightly different predictions about treatment effects on employment and LinkedIn use. We argue in Sections 3 and 4 that our results are most consistent with the first two mechanisms. All of these are partial equilibrium mechanisms, describing how LinkedIn use can change labor market outcomes for individual users. The experiment is not designed to test general equilibrium effects of large-scale changes in LinkedIn use. We return to this issue briefly in Section 5.

First, LinkedIn may provide *demand-side information* that helps employers screen prospective workers. Business success depends on workers' productivity, but firms imperfectly observe prospective employees' skills at the time of hiring (Altonji and Pierret, 2001; Arcidiacono et al., 2010; Kahn, 2013). Thoroughly screening applicants to identify those with low or noisy skill signals but high true skill may be costly, particularly for smaller firms without large, specialized human resource departments. Employers may use LinkedIn profiles as a signal of proactivity or technological engagement or may view information on LinkedIn as more credible than information on a resume, as the former is public and cannot be tailored to individual employers.[4] Better information can increase firm-level hiring if there are uninsured downside risks from bad hires such as damaged capital equipment, offended customers, severance pay, or legally complex firing procedures. See Carranza et al. (2019) and Pallais (2014) for a more formal model and direct empirical evidence that demand-side information frictions can change hiring decisions.

Under this mechanism, employment effects should (1) be associated with having a complete LinkedIn profile and (2) occur relatively soon after workseekers open accounts. Employment effects need not be associated with workseekers' networks and how they use LinkedIn, conditional on having an account.

Second, LinkedIn may provide *supply-side information* by giving workseekers access to more informa-

[4] We do not directly observe if firms view applicants' LinkedIn profiles during hiring. Research from the US finds mixed evidence on firms' use of LinkedIn profiles during recruitment (Landers and Schmidt, 2016; McDonald and Damarin, 2015).

tion about the labor market, potentially at lower cost. Belot et al. (2019) and Altmann et al. (2018) both show that workseekers have limited information about labor market conditions and new information can improve their labor market outcomes. LinkedIn allows users to view firm profiles, worker profiles, and job ads and informs users about new and updated information on profiles. This may substitute for alternative information sources such as job advertisements, search engines, or word-of-mouth.

Under this mechanism, employment effects should be (1) associated with using LinkedIn to view relevant information and (2) might occur either quickly or slowly after opening accounts, depending on the speed of supply-side learning. Employment effects should be less closely associated with workseekers' networks and not associated with inactive accounts.

Third, LinkedIn may help workseekers access larger or more valuable *referral networks*. Faced with limited information, firms may advertize or hire through network referrals and workseekers may learn about job opportunities through network referrals. There is substantial evidence that networks are important for job search and employment outcomes (Topa, 2001; Ioannides and Loury, 2004). There are multiple explanations for why networks might matter in the labor market: workseekers with more connections might learn about more job opportunities, employers might use network hiring to incentivize existing workers, and/or referees might use networks to help their friends and relatives. (Caria et al., 2018) show that networks used for job search can change rapidly during changes in economic conditions, so workseekers might use LinkedIn to form new network connections as well as reinforce existing offline connections. Referral networks are related to the first two mechanisms, as referral networks may provide both demand- and supply-side information. For conceptual clarity, we interpret the first two mechanisms as information acquisition through means other than referrals.

Under this mechanism, employment effects should be (1) associated with the size and characteristics of workseekers' networks on LinkedIn and (2) might occur either quickly or slowly after opening accounts, depending on the speed of forming and using networks. Employment effects should be less closely associated with workseekers' viewing of information and not associated with inactive accounts.

Fourth, LinkedIn may lower pecuniary *job search costs*. Conventional search strategies may require costly travel to business centers to apply for jobs in person and to stationary or internet shops to scan, print, and fax application documents. Subsidies for transport or application costs can change search decisions and employment outcomes, consistent with a quantitatively important role for these frictions (Franklin, 2017; Abebe et al., 2019). LinkedIn can lower per-application costs by allowing on-platform job applications.

LinkedIn can also lower per-candidate job search costs if any of the first three mechanisms moves candidates into employment faster. We interpret this as part of the first three mechanisms, limiting the job search cost mechanism to per-application cost reduction.

Under this mechanism, employment effects (1) should be associated with the number of on-platform job applications and (2) might occur either quickly or slowly after opening accounts. Employment effects should not be associated with having an inactive account but might be associated with the number of on-platform job views if these precede off-platform applications.

Fifth, LinkedIn might change workseekers' job readiness *program engagement* by shifting engagement from the standard program activities to search on LinkedIn or increasing the perceived value of program activities. Sixth, LinkedIn might change workseekers' *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). These mechanisms do not generate clear predictions about the time path of employment effects without more specific assumptions. We evaluate these mechanisms by measuring both program engagement and self-beliefs and reporting treatment effects on these measures in Section 4.3.

2.3 Context and Sample

We work with a sample of young, disadvantaged workseekers in four large South African cities. Unemployment in these cities during the implementation period ranged from 22 to 30% overall, ranging from to 39 to 43% for young people.[5] The high unemployment rate has been attributed to a variety of factors, including 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 may be particularly difficult for young workseekers. The weak education system means that measured skills are weakly correlated with grade progression and hence with years of education (Lam et al., 2011; Taylor et al., 2011). Hiring and firing are tightly regulated, firms report difficulty understanding these regulations, and lengthy and costly 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 referral networks (Magruder,

[5] Authors' own calculations from the Quarterly Labour Force Surveys in quarters 1 and 2 of 2017 (Statistics South Africa, 2017). Employment rates are for the provinces containing the four study cities, conditional on completing high school and classifying discouraged workseekers as unemployed. Calculations for 'young people' use the age range 18-29.

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 1 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.

2010; Rankin and Roberts, 2011). Consistent with these obstacles to youth employment, aggregate job entry rates are very low in South Africa (Donovan et al., 2012).

We study a sample of 1,638 workseekers who start the 30 experimental cohorts, described in Table 1.[6] All candidates in our sample applied for Harambee's programming, so all candidates are active workseekers. Harambee only accepts candidates who are from 'disadvantaged backgrounds.' The definition of disadvantage shifted during our implementation period but, in practice, this excludes candidates from middle- and upper-income households who are likely to have good access to referral networks. This is consistent with only 6% of the sample having university education and 62% having no previous work experience. Candidates are negatively selected on employment prospects relative to the general population. However, all candidates have performed well on Harambee's skill assessments and receive 6-8 weeks of job readiness programming from Harambee.

All mechanisms described in Section 2.2 may be relevant in this setting and for this sample. Demand-side information frictions are plausible when firms are evaluating young candidates with limited work experience and without university education. Demand-side information frictions may also be quantitatively important barriers to employment when hiring and firing are costly. Supply-side information frictions are

[6]Table 1 also shows that the randomization successfully balanced treatment and control candidates. The means of all candidate-level characteristics differ by less than one sixth of a standard deviation and these differences not statistically significantly different from zero. The means of cohort-level characteristics have slightly larger standardized differences but these are also not statistically significant.

plausible when candidates have limited work experience. Referral networks are commonly used in hiring and may be an important source of information to both sides of the market. The disadvantaged workseekers in our sample may know few older adults in formal employment, limiting their access to referral networks and role models. Job application costs are high given South Africa's spatial segregation (Kerr, 2017).

However, none of these features is unique to the South African labor market. Education-skill relationships are noisy in many developing countries (Pritchett, 2013). Distortions due to limited information and search costs have been documented in both developed- and developing-country labor markets (Altonji and Pierret, 2001; Pallais, 2014; Abebe et al., 2016; Bassi and Nansamba, 2017; Franklin, 2017; Altmann et al., 2018; Belot et al., 2019). And, while South Africa arguably has the world's highest rate of youth unemployment, youth unemployment is very high in parts of the Middle East, North Africa, Southern Europe and elsewhere in Southern Africa.

2.4 Measurement

We combine four rounds of survey data with administrative data on platform usage from LinkedIn and administrative data from Harambee.

We conduct a baseline survey at the beginning of the job readiness training, before starting any LinkedIn training. This measures participants' demographics, education, and prior work experience using a computerized, self-administered questionnaire. We match this to Harambee's administrative data on results in communication, numeracy, and general cognitive assessments.[7]

We conduct a second survey at the end of the job readiness training. This measures participants' self-beliefs and engagement with training using a computerized, self-administered questionnaire. We match this with Harambee's administrative data on end-of-program employment, program completion, and program performance.

We conduct phone surveys six and twelve months after the job readiness training.[8] These surveys measure participants' employment status, job characteristics, and self-beliefs. There is some non-response to the follow-up surveys but we show in Appendix A that non-response is balanced across treatment and control cohorts and weakly related to baseline covariates, and that our main findings are robust to accounting for non-response in several ways.

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

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

We use email addresses and names to match participants to administrative data from LinkedIn. These data were extracted by LinkedIn roughly at the same time as the survey rounds (at the end of job readiness training and roughly six and twelve months later). The data show if each participant has an account, the account opening date, and conditional on having an account: profile completeness, number of network links, attributes of network links, and frequency and type of site usage. These data allow us to measure how treatment changed participants' online professional networking activities.

LinkedIn collects outcome data for participants but does not observe their treatment assignments. Harambee observes treatment assignments but only provides outcome data at the end of immediate end of the job readiness training. The phone surveys six and twelve months later are conducted by an independent survey firm, blinded to treatment assignment. This limits the scope for strategic measurement error from data providers.

2.5 Research Design

We use a cohort-level randomized controlled trial to study how LinkedIn training changes labor market engagement and outcomes. We split 30 cohorts into treatment and control groups using within-city, sequentially-paired randomization. We work in four South African cities: Cape Town, Durban/eThekwini, Johannesburg, and Pretoria/Tshwane. Within each city, 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.

Given this design, we estimate treatment effects using equations of the form

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

where i, c, and r index respectively individual participant, cohort, and region. Y, T, and $\mathbf{S}$ denote respectively outcomes, treatment assignment, and a vector of cohort-pair fixed effects. The cohort-pair fixed effects account for regional and temporal variation in outcomes. We estimate heteroskedasticity-robust standard errors, clustered by cohort. We winsorize left-skewed outcomes at the 95$^{\text{th}}$ percentile, though this makes little difference to the results.

We report intention-to-treat effects throughout the paper. The intervention was not implemented in one of the 15 cohorts assigned to treatment and some parts of the intervention were skipped in 4 more cohorts. The partial non-compliance reflected time constraints during training or communication challenges with the training managers. We report compliance-adjusted treatment effects on key outcomes in Appendix B.

Table 2: Treatment Effects on Employment

	(1) 3 waves pooled	(2) End of program	(3) 6 months	(4) 12 months
Treated cohort	0.073	0.070	0.081	0.069
	(0.022)	(0.021)	(0.039)	(0.024)
Control group mean	0.683	0.701	0.638	0.704
# respondents	3733	1626	1119	988
# cohorts	30	30	30	30
Adjusted R2	0.044	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. Column 1 reports estimates from pooling all three survey waves into a single dataset.

3 Treatment Effects on Employment and Labor Market Outcomes

Treatment generates a large increase in employment immediately after the job readiness programs, which persists for at least 12 months. Post-program employment increases by 7 percentage points (Table 2, column 2). The employment effects at six and twelve months are similar: respectively 8.1 and 6.9 percentage points (Table 2, columns 3-4). These effects are at least 10% increases relative to the control group mean at each time point. Treatment also increases weekly hours worked by 4.2 and 2.9 hours at the six and twelve month points respectively (Table 3, panels A and B, column 1). We do not directly observe earnings. But pricing the additional hours at the national minimum wage implies that treatment raises earnings per participant by at least US$480 over twelve months. This is ten times higher than the cost of treatment of per participant. See Appendix C.2 for details.

The higher employment in the treatment group is persistent at the individual level. Treatment increases the probability of being employed at both the post-program and 6 month points by 10.7 percentage points and the probability of being employed at both the post-program and 12 month points by 12.6 percentage points (Table 3, column 2). Treatment has no effect on turnover in the first six months after training and slightly reduces turnover in the twelve months after training (Table 3, column 3). These estimates imply that almost all treated participants who find jobs at the end of the program retain those for at least twelve months.[9] As a benchmark, the median job tenure in South Africa at the time was eleven months for youths, with very high rates of churn (Zizzamia and Ranchhod, 2019). To the extent that tenure is a proxy for match quality, these results show that treatment-induced matches are of reasonably higher quality. Job security

[9] Our tenure analysis has one important caveat. Our measure of 'multiple employers since program completion' does not distinguish between multiple jobs held sequentially or simultaneously. Hence the 12% of participants reporting 2 or more employers might have held these jobs sequentially (implying turnover) or simultaneously.

Table 3: 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
	(1.701)	(0.040)	(0.021)	(0.026)	(0.010)
Control group mean	25.523	0.585	0.123	0.129	0.038
Control mean|employment	40.211	0.916	0.140	0.204	0.053
# respondents	1107	1117	1114	1113	1117
# cohorts	30	30	30	30	30
Adjusted R2	0.078	0.076	0.006	0.104	-0.000
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	41.590	0.855	0.148	0.269	0.155
# respondents	985	987	988	983	986
# cohorts	30	30	30	30	30
Adjusted R2	0.045	0.058	0.019	0.059	-0.002

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 2 indicates the probability of being employed at both the end of program and 6 month (Panel A) or end of program and 12 month (Panel B) points. ‘Multiple employers’ indicates that the workseeker had more than one employer between end of the program and relevant survey. ‘Permanent’ indicates that the job is permanent, rather than temporary. ‘Promoted’ indicates 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.

is an important dimension of match quality for South African workseekers, ranked ahead of earnings and promotion prospects in a recent nationally representative survey (Mncwango, 2016).

Treatment does not increase other proxies for match quality. Treatment effects are small and not statistically significant for the probability of being in a permanent job (Table 3, column 4) or the probability of being promoted (column 5). We cannot reject that the mean value of each of these match quality proxies, conditional on employment, is equal across the treatment and control groups. This suggests that the marginal matches added by treatment have similar match quality to the inframarginal matches that candidates would obtain in the absence of treatment.

The timing of employment effects shows that the underlying mechanism(s) must operate during or immediately after the program. The treatment effect on post-program employment is driven entirely by higher placement rates in job applications initiated by Harambee, rather than job applications independently initiated by candidates.[10] Hence this must be driven by better performance in job applications and interviews around the end of the programs, not better long-term job search. We find one pattern of treatment heterogeneity that is directly consistent with treatment improving application and interview performance. The probability of employment is increasing in participants' measured communication skill but treatment eliminates most of the communication skill gap in employment.[11] This suggests that treatment use provides an alternative way for candidates with low oral or written communication skills to market themselves to employers or acquire information, consistent with the information friction mechanisms.

Returning to our conceptual framework, the employment results are more consistent with alleviating demand- or supply-side information frictions than with referrals or lower search costs. The quick rise in employment, negative heterogeneity by communication scores, and importance of facilitated applications and interviews, is not consistent with an important role for lower search or application costs. The zero effect on employment from independent applications casts doubt on an important role for referrals. The persistence of initial placements also shows that candidates are not using LinkedIn to obtain referrals that facilitate subsequent job switching. But we cannot reject the possibility that LinkedIn connections to workers at targeted firms help candidates convert Harambee-facilitated applications into offers. The quick, persistent employment effects are consistent with LinkedIn helping firms learn about applicants or candidates learn about firms or the labor market in general. In Section 4.1 we examine treatment effects of specific measures of LinkedIn use to further narrow the space of potential mechanisms.

The employment effects are larger than the mean effects of active labor market programs in a recent metastudy (Card et al., 2017). However, our results are comparable to the mean effects of ALMPs for long-term unemployed participants. Our sample of youths with limited prior work experience is perhaps more similar to the long-term unemployed than recently displaced workers.[12]

[10]Toward the end of each program, Harambee sends candidates' applications to firms and arranges interviews for short-listed candidates. By design, there are no differences in this process between treatment and control cohorts. There is no treatment-control difference in share of candidates who complete programs and are hence eligible for application support (mean = 87%, p-value for equal mean = 0.45). There is also no treatment-control difference in the share of candidates who obtain jobs through independent applications (mean = 4.7%, p-value for equal mean = 0.17). Facilitating applications and interviews is not an unusual feature of active labor market programs. Harambee is arguably unusual in actively managing long-term relationships with selected private sector firms. These firms account for 34 percentage points of the 70% post-program employment rate in the control group. But treatment has no effect on the probability of securing a job with a long-term partner (p-value = 0.69).

[11]See Table A.7 for point estimates. 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 these seven dimensions of heterogeneity.

[12]To compare our results to the metastudy we divide our employment effects by the sample standard deviations to obtain standardized effect sizes. Our standardized effect sizes are 0.15 - 0.17. Card et al. (2017) find that the mean standardized effect sizes of active labor market programs are 0.04 over the first year and 0.12 over the second year. For the long-term unemployed, these effects are respectively 0.17 and 0.30. For job search assistance programs, which are arguably most similar to our intervention, the

We show in Appendix A that the employment effect results are robust to adjustments for non-response and to conditioning on baseline covariates. Non-response is under 1% in the administrative data on post-program employment but rises to 32% in the six-month survey and 40% in the twelve-month survey. Survey non-response does not differ by treatment status and is only weakly related to baseline covariates and to the interactions between treatment status and baseline covariates (Tables A.1 and A.2). The employment effects are largely unchanged when we re-weight the sample to account for the small differences between responders and non-responders in baseline characteristics (Table A.3). Lee bounds on the employment effects are less than 2 percentage points wide (A.4). The employment effects are also robust to conditioning on baseline covariates using a Lasso estimator (Table A.5). The Lasso uses a data-driven rule to condition on covariates that predict either employment or treatment status in the sample of responders, which effectively includes any covariates that differentially predict non-response by treatment status (Belloni et al., 2014). Given these results, we argue that non-response is unlikely to explain the employment effects we observe.

4 Mechanisms Generating Employment Effects

In this section we argue that the treatment effects on employment are mediated by LinkedIn use, which appears to address information frictions in the hiring process. First, we show that treatment substantially increases multiple measures of LinkedIn use. We argue that the patterns of treatment effects on employment and LinkedIn use are more consistent with LinkedIn providing information to the demand and potentially supply sides of the labor market than LinkedIn facilitating referrals or lowering job search costs. Second, we show that, under the assumptions required for formal mediation analysis, LinkedIn use mediates the employment effects. Third, we show there are limited treatment effects on survey measures of two alternative mechanisms: changes in program engagement and self-beliefs.

4.1 Treatment Effects on LinkedIn Use

Treatment increases the share of participants with LinkedIn accounts from 48 to 80% (Table 4, column 1). Furthermore, 42% of treated participants opened accounts during their Harambee job readiness programming period, compared to 9% of control participants. This shows high compliance with the first part of the LinkedIn curriculum.[13] We also ask participants to report their weekly time spent on LinkedIn during the

mean standardized effect sizes are 0.04 over the first and second years.

[13]We observe snapshots of LinkedIn administrative data at roughly one, six, and twelve months after training. In this section we report treatment effects on the average of these three measures. We show in Figure A.1 that there is not a consistent upward or downward trend across all the usage metrics. The results in this section are robust to replacing averages with only the first

Table 4: Treatment Effects on LinkedIn Usage

	(1) LinkedIn account	(2) Account during training	(3) Profile completion	(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 mean	0.484	0.094	0.301	0.378	0.178
Control mean|account	1.000	0.201	0.631	0.810	0.381
# respondents	1638	1566	1599	1493	1493
#cohorts	30	30	30	30	30
Adjusted R2	0.140	0.281	0.115	0.085	0.028
	(6) # connections	(7) # bachelors connections	(8) # manager connections	(9) Average power	(10) # job apps
Treated cohort	8.609	0.754	0.543	0.537	0.009
	(1.513)	(0.130)	(0.095)	(0.092)	(0.004)
Control mean	6.145	0.503	0.365	0.844	0.014
Control mean|account	12.807	1.048	0.761	1.829	0.030
# respondents	1629	1629	1629	1579	1493
#cohorts	30	30	30	30	30
Adjusted R2	0.111	0.124	0.118	0.062	0.017

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 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, and profiles viewed 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; average power is a measure of the quality of the network connections; # job applications is the number of applications submitted through the LinkedIn platform only. The conditional control group mean is the average value for control respondents conditional on having a LinkedIn account.

job readiness training program. Treatment increases this from 0.6 to 1.7 hours per week, for a total increase of 8 hours over the duration of the training (not shown in table). The LinkedIn training involved only 4 contact hours, not all of which were spent using LinkedIn, so this demonstrates some use outside training.

We observe eight other measures of LinkedIn use, which we use to evaluate the four mechanisms introduced in the conceptual framework. Treatment increases LinkedIn's measure of account completeness (Table 4, column 3).[14] Treatment substantially increases the number of profiles viewed and number of jobs viewed in the 30 days before each data snapshot (columns 4-5). However, the levels of profile and job views

post-treatment measure. When candidates do not have LinkedIn profiles, we code all their other use metrics as zeros.

[14] Profile completion is a scalar value calculated by LinkedIn using a non-public formula, which takes into account whether a profile includes a photograph, profile summary, location, skills, education history, and work history.

are low. Treated participants view an average of only 0.2 and 0.3 jobs in the preceding month. Treatment more than doubles participants' number of connections, number of connections with bachelors or higher degrees, and number of connections with managerial jobs (columns 6-8).[15] However, even treated candidates have on average only 1.2 connections with bachelors or higher degrees and 0.9 connections with managerial jobs. Treated participants have networks with higher average 'power,' a LinkedIn-constructed average across a participant's network connections based on the connections' profile completeness, job title, education, and network size (column 9). Treatment marginally increases the number of job applications submitted through LinkedIn in the 30 days before each data snapshot, but the total is very low in both treatment and control groups (column 10).

These patterns provide some information about the relative importance of the four mechanisms in our conceptual framework. First, the results are consistent with demand-side information acquisition. Treatment has large effects on opening a LinkedIn account and profile completeness. If firms use these as signals of potential work-readiness or productivity, these could drive employment effects. The quick treatment effects on employment are also consistent with demand-side information acquisition, which could occur immediately after candidates create accounts.

Second, the results may be consistent with supply-side information acquisition. Treatment increases time spent on LinkedIn, profiles viewed, and jobs viewed, all of which might give candidates information. But the treatment effects on profile and job views are small and off low bases. This mechanism is likely to be quantitatively important only if candidates get substantial information without viewing jobs or profiles (e.g. from the news feed) or if the few jobs and profiles they view are well-targeted (e.g. profiles of prospective interviewers). Consistent with the latter idea, the treatment effect on profile views is high at the end of the program and then falls to zero in later data snapshots (Figure A.1) The quick treatment effects on employment are also consistent with supply-side information acquisition, which could occur quickly after candidates create accounts.

Third, the results may be consistent with network referrals. Treatment increases the size of candidates' networks and several measures of network quality or power. Participants do make some network connections soon after opening accounts, which could generate quick referrals and drive the quick treatment effects on

[15] These are small networks relative to the average LinkedIn user. But they are larger than offline job search networks in similar settings. Young workseekers in Addis Ababa and Johannesburg regularly discuss their job search with a median of 2-6 people (Abebe et al., 2017; Caria et al., 2018; Carranza et al., 2019). However, we do not observe if workseekers in our sample regularly interact with their LinkedIn connections on or off the platform.

employment (Figure A.1). But turnover in the following twelve months is low and is not higher for treated candidates. Hence referrals can explain the time path of employment only if candidates use referral networks for initial search but not subsequent on-the-job search.

Fourth, the results are not consistent with lower search costs. Treatment effects on job applications and job views are positive but very small and off low bases. It is possible that treatment gives candidates better information, allowing them to better target off-platform job applications, hence lowering off-platform search costs. However, even this possibility is not consistent with the large treatment effect on employment from Harambee-facilitated applications and zero treatment effect on employment from independently-initiated applications.

Taken together, these results show that treatment substantially increases LinkedIn use in ways that are more consistent with information acquisition and certain types of referrals, rather than lower search costs.

4.2 LinkedIn Use as A Mediator for Employment Effects

In this section we take the treatment effects on employment and LinkedIn use as given and ask if treatment changes employment by changing LinkedIn use. Given the persistence of individual-level employment, we focus on explaining the treatment effect on initial post-program employment. We use two approaches to mediation analysis. Both approaches evaluate 'how much' of the treatment effect on employment can be explained by treatment effects on LinkedIn. Both approaches require assumptions about the model linking employment, LinkedIn use, and treatment that are not necessarily satisfied even under random treatment assignment.

First, we assess if LinkedIn use can explain the treatment effect on initial employment. We estimate the system

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

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

$$\text{Employ}_{icr} = LI_{icr} \cdot \delta + \mathbf{S_{cr}} + \eta_{icr}. \tag{4}$$

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 $S_1 = \frac{\delta \cdot \gamma}{\beta}$ as the share of the treatment effect on employment explained by LinkedIn use. This measures 'how much' of the employment effect β can be

Table 5: Relationship between Treatment, Initial Employment, and LinkedIn Use

LinkedIn use measure	(1) Has account	(2) Use index
Panel A: Parameter Estimates		
Treatment effect on employment	0.070	0.086
(β in equation 5)	(0.020)	(0.019)
Treatment effect on LinkedIn use	0.326	0.694
(γ in equation 6)	(0.050)	(0.109)
Treatment effect on employment conditional on LinkedIn use	0.021	0.032
($\tilde{\beta}$ in equation 7)	(0.026)	(0.025)
Relationship between employment & LinkedIn use conditional on treatment	0.151	0.078
($\tilde{\delta}$ in equation 7)	(0.028)	(0.013)
Sample size	1626	1445
Panel B: Share of Treatment Effect Explained by LinkedIn Use		
$S = \tilde{\delta} \cdot \gamma / \beta$	0.705	0.632
	(0.304)	(0.227)

Panel A shows estimates of the parameters of equation system (5) - (7). Panel B shows the share of the treatment effect on employment explained by the treatment effect on LinkedIn use: $S = \frac{\tilde{\delta} \cdot \gamma}{\beta}$. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. The equations are estimated as a system and the standard errors on S are estimated using the Delta method. All models include stratification block fixed effects. The treatment effect on having a LinkedIn account is slightly different to that reported in Table 4 because 12 observations with missing employment data are excluded from this analysis.

explained by the LinkedIn use effect γ via the non-experimental relationship δ.

Using this approach, LinkedIn use explain at least half of the treatment effect on initial employment. Table 5 shows that treatment increases employment by 7 percentage points (row 1, column 1) and the probability of having a LinkedIn account by 33 percentage points (row 2, column 1). Candidates with LinkedIn accounts are 14 percentage points more likely to be employed (row 3, column 1). Hence $\hat{S}_1 = 0.65$, with standard error 0.22 (panel B, column 1). The indicator for having a LinkedIn account might be a poor measure of general LinkedIn use. We therefore replace this indicator with an index of six LinkedIn use measures. This shifts $\hat{S}_1$ to 0.56 with standard error 0.19 (panel B, column 2).[16]

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 associ-

[16]The LinkedIn index is the first principal component of six measures: an indicator for having an account, the number of connections, average power, profile completion, profiles viewed, and jobs viewed. The first principal component accounts for 60% of the variation in these six measures. The index is missing for 12% of the sample due to missing values in the administrative data from LinkedIn.

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

Second, we assess whether LinkedIn use mediates the treatment effects on employment, following Heckman and Pinto (2015), Imai et al. (2010), and Robins and Greenland (1992) amongst others. In this approach, a 'mediator' is a variable that is influenced by treatment and in turn influences employment. We estimate the system

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

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

$$\text{Employ}_{icr} = T_{cr} \cdot \tilde{\beta} + LI_{icr} \cdot \tilde{\delta} + \mathbf{S_{cr}} + \varepsilon_{icr} \tag{7}$$

using the same notation as above. If there are no omitted variables correlated with both LinkedIn use and employment, then $\tilde{\delta} \cdot \gamma$ captures the effect of treatment on employment via LinkedIn use. Heckman and Pinto (2015) and Robins and Greenland (1992) call this the 'indirect effect' of treatment and Imai et al. (2010) call this the 'average causal mediation effect.' $S_2 = \frac{\tilde{\delta} \cdot \gamma}{\beta}$ is the share of the total treatment effect attributable to the indirect path through LinkedIn use. $\tilde{\beta}$ is the 'direct effect' of treatment on employment not mediated by LinkedIn use.

Using this approach, LinkedIn use mediates at least half of the treatment effect on initial employment. Table 5 column 4 shows that the treatment effect on having a LinkedIn account explains 0.71 of the treatment effect on initial employment with standard error 0.30 (panel C, column 1). Replacing the indicator for LinkedIn use with the LinkedIn index defined above reduces this share to 0.63 with standard error 0.23 (panel C, column 2). The direct effect of treatment on employment, not mediated by LinkedIn use, is only 2.1 - 3.2 percentage points and not statistically significantly different to zero (panel A, row 4, columns 1 and 2).

This approach assumes that there are no omitted variables correlated with both LinkedIn use and employment.[18] Heckman and Pinto (2015) and VanderWeele (2012) note that this assumption would be violated

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

[18] In the potential outcomes framework, this assumption is known as 'sequential ignorability,' and is typically presented as statistical independence of LinkedIn use from potential employment outcomes. Vansteelandt (2009) and Acharya et al. (2016)

by certain types of measurement error in the mediating variable(s). Both our LinkedIn use measures, the indicator for having an account and the index, may suffer from measurement error because they ignore the multidimensional nature of LinkedIn use. Given this concern, we replicate the analysis replacing the scalar LI_{icr} with a vector of all six components used to construct the LinkedIn index. The six components jointly mediate 61% of the employment effect (standard error 31%). This suggests that the greater precision from aggregating the six measures into a single index more than offsets any conceptual measurement error from the aggregation. Using the six measures separately also identifies the share of the employment effect mediated by each measure. The two most important mediators are the indicator for having a LinkedIn profile and the number of profiles viewed. The number of profiles viewed is small for most candidates but this might generate important supply-side information if they view profiles of their interviewers ahead of interviews.

Throughout this section we condition only on stratification block fixed effects, not baseline covariates. Conditioning on covariates (listed in Table A.1) shifts the share of the employment effects mediated by LinkedIn by at most four percentage points. This increases the plausibility of the assumption made in the mediation analysis - that there are no omitted variables correlated with both LinkedIn use and employment. We also show in Section 4.3 that there are at most small treatment effects on other variables that might violate this condition. However, we cannot rule out the possibility that treatment might shift unobserved variables such as off-platform job search that are correlated with both employment and LinkedIn use.

As a final sensitivity check, 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 64% (standard error 43%) and mediates 65% (standard error 35%) 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 in the mediation analysis from correlated unobserved characteristics is likely to be smaller for this than our other measures of LinkedIn use.

Under assumptions similar to those imposed by the mediation analysis, the local average treatment effect of LinkedIn on initial employment is identified. Specifically, if LinkedIn use is the only mediator from treatment to employment and treatment weakly increases LinkedIn use for all candidates, then we can estimate the local average treatment effect by regressing employment on LinkedIn use, instrumented

propose a modification to this procedure called 'sequential g-estimation' that makes a very slightly weaker assumption. We obtain almost identical results using sequential g-estimation.

by treatment. We report detailed results from this analysis in Tables A.8 and A.9. In brief, assignment to treatment increases LinkedIn use by 0.7 standard deviations, and a one standard deviation unit increase in LinkedIn use increases employment by 12 to 16 percentage points. These results suggest a quantitatively important role for online networking platforms in the transition from job training to employment.

4.3 Alternative Mediators for Employment Effects

Treatment effects on LinkedIn use appear to explain most of the treatment effects on employment, but other mediators may also be relevant. In this section we discuss three possible mediators: program engagement, self-beliefs, and job search. We also test for spillovers between treated and control participants.

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 interest in the program as well as trainer reports of candidates' energy and intellectual curiosity. Treatment has no effect on any of these measures (Table A.10). 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; Tanguy 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. (2019). Finally, we measure candidates' reservation wages. Treatment slightly increases aspirational wages by 5-9% and has no large or statistically significant effect on any other measure (Table A.10).

Third, LinkedIn may shift off-platform search behavior. On-platform search may be lower cost and hence crowd out off-platform search. LinkedIn use may also increase candidates' knowledge about the labor market, allowing them to direct search better and changing the optimal level of search effort. We do not observe measures of off-platform search. But the pattern of employment effects – immediate rise after training, via Harambee-facilitated job applications, sustained over the next 12 months – does not seem consistent with a large role for changes in off-platform search.

Finally, there may be spillover effects of training on candidates in control cohorts. Five of the fifteen 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 – if control candidates learn to use LinkedIn from treated cohorts – or overstate the treatment effect – 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.

5 Conclusion

We present the first experimental evidence that training participants in job readiness programs to join and use an online professional networking platform improves their labor market outcomes. Treatment increases the employment rate from 70 to 77% at the end of the program and this effect persists for at least a year. Jobs in the treatment and control groups have roughly equal probabilities of retention, promotion, and obtaining a permanent contract. This suggests that the marginal matches added by treatment have similar match quality to the inframarginal matches that candidates would obtain in the absence of treatment. Treatment effects on LinkedIn use explain or mediate more than half of the treatment effect on initial employment.

These findings point to several directions for future research. First, what aspects of online professional networking are particularly important for improving employment outcomes? Our results suggests an important role for information provision to firms about workseekers or to workseekers about the labor market. The results are also consistent with a role for referrals, although any referrals that occur do not influence subsequent job transitions. Future work could identify referral mechanisms using the identities of workseekers' on-platform connections and the exact dates of link formations, job applications, and job starts.

Second, what might be the general equilibrium effects of large increases in online professional networking?[19] Our experiment is not designed to answer this question, but we offer some speculative evidence. Our experiment generates a tiny market-level increase in LinkedIn use: 285 extra users from a base of roughly 7.1 million. But our experiment may generate a substantial vacancy-level increase in LinkedIn use, as Harambee helps multiple workseekers from the same job readiness training cohort to apply for the same jobs. Our employment results are thus consistent with LinkedIn use increasing firm-level hiring, potentially by alleviating information frictions and allowing higher firm-worker match quality. Even if treatment effects on employment are zero at scale, welfare gains are still possible through lower search costs or search time or higher match quality. Even if treatment effects on employment are attenuated at scale, the 10-1 benefit-cost ratio in our experiment suggests that substantial increases in scale may still pass benefit-cost tests. Even if treatment effects on employment are zero at scale, welfare gains are still possible through lower search costs or search time or higher match quality.

Third, how might workseekers use online professional networking outside the context of a job readiness training program? Both treatment and control workseekers in our sample received 6-8 weeks of programming and job search assistance. The programming and job search assistance might complement online professional networking by giving workseekers profiles that showcase skills learned in programming, connections to former program participants, and advice on targeting on-platform search and job applications. On the other hand, online professional networking might have higher returns when not combined with job readiness training and job search assistance because they operate through overlapping mechanisms.

These findings have important implications for policy design even if they only apply to participants in job readiness programs. Given the prevalence and cost of these programs in many countries, increases in post-program employment transition rates would be valuable. Our findings show that substantial increases are possible from small, low-cost design changes that use new technology and are informed by growing research on information frictions.

[19]There is some evidence that active labor market programs have spillover effects on non-participants (Lise et al., 2004; Crépon et al., 2013; Gautier et al., 2018). In contrast, Blundell et al. (2004) find no quantitatively important spillovers from a large-scale search assistance and wage subsidy program.

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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 relationship between non-response, treatment, and baseline covariates in respectively the six-month and twelve-month surveys. 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 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 standard errors on the reweighted employment effects are slightly larger than the unweighted effects, reflecting the additional uncertainty from the estimated weights. But the sign and magnitude of effects is robust across unweighted and weighted estimates. We omit the 1-month employment effects from this table because the response rate is very high.

The treatment effects on employment are robust to accounting for differential non-response by treatment arm. Table A.4 demonstrates this. The table reports bounds on employment effects assuming that 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 are less than 1 percentage point higher in the treatment than control group.

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

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

	(1)	(2)	(3)
Outcome		Non-response	
Treatment	-0.012		-0.395
	(0.049)		(0.190)
Age		0.002	-0.004
		(0.004)	(0.006)
Gender		-0.023	-0.039
		(0.028)	(0.035)
Post-secondary education		-0.041	-0.035
		(0.020)	(0.032)
University education		-0.081	-0.039
		(0.052)	(0.073)
Previously employed		-0.002	0.057
		(0.026)	(0.047)
Cape Town		-0.014	-0.095
		(0.080)	(0.052)
Johannesburg and Pretoria		-0.167	-0.276
		(0.065)	(0.029)
Numeracy score		-0.014	-0.004
		(0.015)	(0.025)
Communications score		-0.009	-0.009
		(0.014)	(0.013)
Cognitive score		-0.014	-0.015
		(0.013)	(0.019)
Age X Treatment			0.009
			(0.008)
Gender X Treatment			0.022
			(0.053)
Post-secondary education X Treatment			-0.012
			(0.042)
University education X Treatment			-0.100
			(0.097)
Previously employed X Treatment			-0.107
			(0.053)
Cape Town X Treatment			0.164
			(0.116)
Johannesburg and Pretoria X Treatment			0.219
			(0.075)
Numeracy score X Treatment			-0.021
			(0.031)
Communications score X Treatment			0.007
			(0.027)
Cognitive score X Treatment			-0.001
			(0.026)
# respondents	1638	1388	1388
# cohorts	30	30	30
Non-response mean	0.317		
p-value joint significance	0.804	0.005	0.000
F-stat joint significance	0.063	3.372	46.866

Coefficients are from regressing a non-response indicator on a treatment indicator, baseline covariates, treatment interacted with covariates, and stratification block fixed effects. 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		-0.520
	(0.051)		(0.181)
Age		-0.008	-0.018
		(0.004)	(0.007)
Gender		-0.038	-0.094
		(0.037)	(0.030)
Post-secondary education		-0.035	-0.054
		(0.028)	(0.029)
University education		-0.043	0.016
		(0.048)	(0.067)
Previously employed		0.046	0.126
		(0.028)	(0.040)
Cape Town		0.035	-0.008
		(0.056)	(0.065)
Johannesburg and Pretoria		-0.189	-0.250
		(0.046)	(0.058)
Numeracy score		-0.003	-0.003
		(0.014)	(0.020)
Communications score		0.011	0.015
		(0.013)	(0.018)
Cognitive score		-0.005	-0.001
		(0.011)	(0.014)
Age X Treatment			0.018
			(0.008)
Gender X Treatment			0.095
			(0.065)
Post-secondary education X Treatment			0.034
			(0.050)
University education X Treatment			-0.123
			(0.093)
Previously employed X Treatment			-0.141
			(0.053)
Cape Town X Treatment			0.095
			(0.101)
Johannesburg and Pretoria X Treatment			0.113
			(0.077)
Numeracy score X Treatment			-0.001
			(0.029)
Communications score X Treatment			-0.005
			(0.028)
Cognitive score X Treatment			-0.009
			(0.024)
# respondents	1638	1388	1388
# 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	5.053	13.032

Coefficients are from regressing a non-response indicator on a treatment indicator, baseline covariates, treatment interacted with covariates, and stratification block fixed effects. 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: Treatment Effects on Employment Weighting by Inverse Probability of Nonresponse

	(1) 3 waves pooled	(2) End of program	(3) 6 months	(4) 12 months
Treated cohort	0.073	-	0.077	0.066
	(0.052)	-	(0.066)	(0.042)
# respondents	3731	1624	1119	988
# cohorts	30	30	30	30

Coefficients are from regressing an employment indicator in each of the three waves on a treatment indicator and stratification block fixed effects. 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 500 iterations of a bootstrap that resamples cohorts and estimates both the weights and employment regressions in each iteration.

Table A.4: Upper and Lower Bounds for Employment Effects: Lee Bounds

	(1) 3 waves pooled	(2) End of program	(3) 6 months	(4) 12 months
lower	0.075	0.070	0.081	0.057
upper	0.076	0.084	0.099	0.061
# respondents	4914	1638	1638	1638

Lee bounds are tightened using region fixed effects. Lee bounds trim the sample such that the number of observations is equal across treatment and control. Coefficients are from regressing an employment indicator in each of the three survey waves on a treatment indicator and stratification block fixed effects. Standard errors are omitted because the analytical variance estimator for Lee bounds does not account for clustering. Column 1 reports estimates from pooling all three survey waves into a single dataset.

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.5).

Table A.5: Treatment Effects on Employment Conditional on Baseline Covariates

	(1) 3 waves pooled	(2) End of program	(3) 6 months	(4) 12 months
Treated cohort	0.059	0.063	0.073	0.065
	(0.022)	(0.020)	(0.038)	(0.023)
# respondents	3731	1624	1119	988
# cohort	30	30	30	30

Coefficients are from regressing an employment indicator in each of the three waves on a treatment indicator, stratification block fixed effects, and a vector of baseline covariates selected by the post double selection lasso estimator. The lasso estimator is allowed to select from the list of covariates in Table 1, missing data indicators, and pairwise interactions. In each regression it chooses only some of the skill and education measures. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort.

B Additional Results Discussed in Paper

This appendix reports additional results discussed in the main paper text. Table A.6 reports average treatment effects on employment outcomes for treated participants. The treatment was fully implemented for 10 of the 15 cohorts assigned to treatment and partly implemented for another 4 cohorts. These are estimated 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, with standard error 0.10, so all employment effects on the treated candidates are roughly 60% larger than the corresponding intention-to-treat effects.

Table A.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 significantly larger than zero when we estimate q-values that control for 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 these interactions is large and few are statistically significant after adjusting for multiple testing.

Figure A.1 reports control group levels of and treatment effects on selected measures of LinkedIn usage through time. This figure shows that the probability of having an account and multiple usage measures rise immediately after treatment. But there is not a general upward or downward trend over the following 12 months.

Tables A.8 and A.9 report treatment effects of a standardized single index of multiple LinkedIn use measures on employment outcomes, using treatment assignment as an instrument for LinkedIn use.[20] This approach identifies local average causal 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

[20]The LinkedIn index is the first principal component of six measures: an indicator for having an account, the number of connections, average power, profile completion, profiles viewed, and jobs viewed. The first principal component accounts for 60% of the variation in these six measures. Results are qualitatively similar using other combinations of the LinkedIn use measures.

Table A.6: Employment Effects based on Instrumenting Compliance with Treatment

	(1) 3 waves pooled	(2) End of program	3) 6 months	(4) 12 months
Treatment compliance	0.121	0.113	0.135	0.118
	(0.048)	(0.041)	(0.076)	(0.056)
# respondents	3733	1626	1119	988
# cohorts	30	30	30	30

Treatment assignment instruments for an indicator of perfect compliance to treatment. Compliance is defined as complete treatment programming implemented for the cohorts assigned to treatment. Coefficients are from regressions that include stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. Column 2 reports estimates from pooling all three survey waves into a single dataset. The first stage coefficient is 0.62 with standard error 0.10 and F-statistic 35.2.

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 12-16 percentage points. Hours also increase and 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 6 percentage points and lowering the probability of turnover by 7 percentage points. LinkedIn use effects on job quality measures at six months are smaller and never significantly different to zero.

Table A.10 reports treatment effects on alternative mechanisms that we measure but are largely unaffected by treatment. Treatment has at most small effects on locus of control, excitement about the future, and trust in others, which are not statistically significantly different to zero. Treatment does not shift candidates' self-reported engagement with the job readiness training or their trainers' assessments of their curiosity, enthusiasm, or energy. We conclude that treatment does not shift candidates' aspirations or engagement with the general job readiness training, so these are unlikely to drive the employment effects. Treatment does slightly increase the wages candidates aspire to earn and reservation wages. But 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.

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

	(1) Employed end of program	(2) Employed 6 months	(3) Employed 12 months
Treated cohort	0.068	0.078	0.068
	(0.021)	(0.038)	(0.022)
Treated×communication score	-0.054	-0.055	-0.096
	(0.020)	(0.026)	(0.028)
Communication score	0.068	0.084	0.094
	(0.016)	(0.018)	(0.022)
Control group 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)	0.076	0.197	0.015

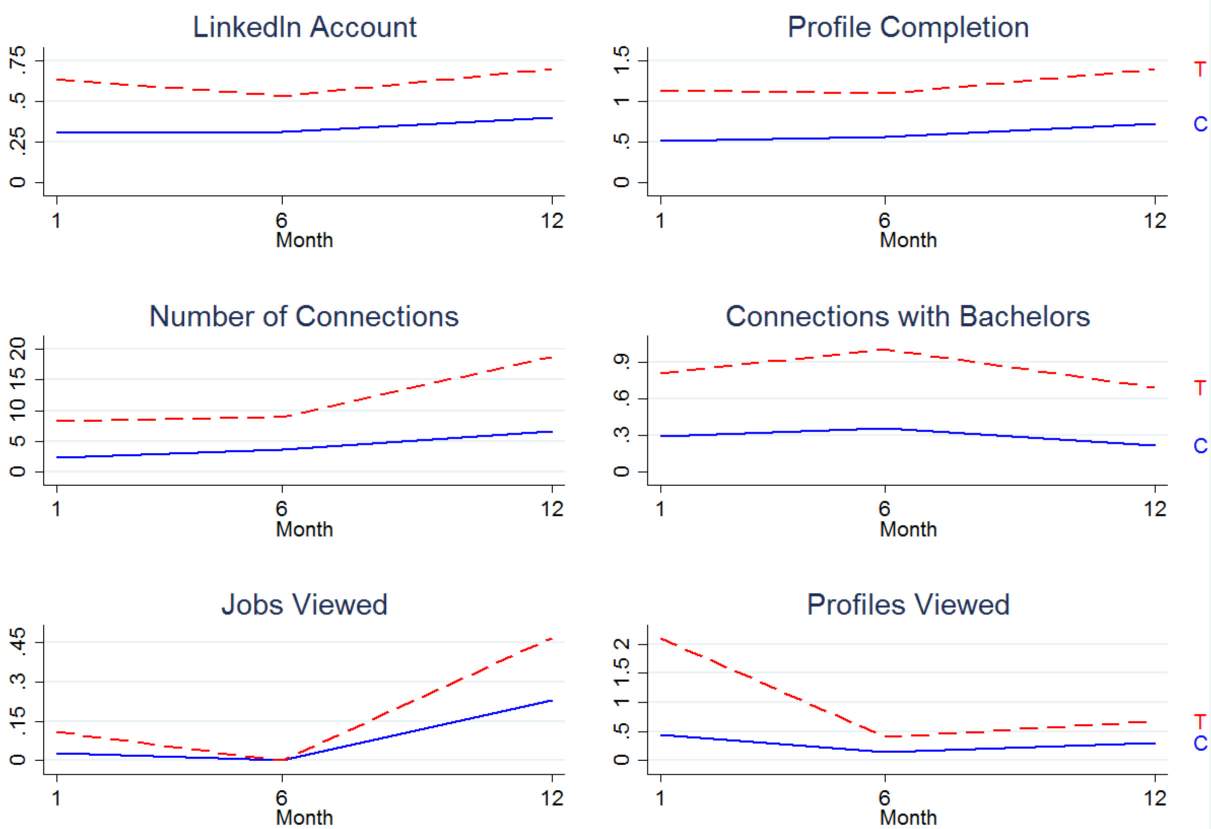
Coefficients are from regressing an employment indicator in each of the three survey waves on a treatment indicator, communication assessment score, their interaction, and stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. The 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 the seven dimensions of baseline heterogeneity discussed in the text.

Table A.8: Local Average Treatment Effects of LinkedIn Use on Employment

	(1) End of program	(2) 6 months	(3) 12 months
LinkedIn use	0.123	0.159	0.119
	(0.032)	(0.062)	(0.035)
Control mean	0.701	0.638	0.704
# respondents	1445	1008	897
#cohorts	30	30	30
Adjusted R2	0.074	0.011	0.010

Coefficients are from regressing an employment indicator in each of the three waves on LinkedIn use, instrumented by treatment assignment, and stratification block fixed effects. LinkedIn use is defined as the first principal component of an indicator for having an account, the number of connections, average power, profile completion, profiles viewed, and jobs viewed. This is standardized to have mean zero and standard deviation one in the control group. The first stage coefficient is 0.68 with standard error 0.11 and F-statistic 39.8. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort.

Figure A.1: LinkedIn Usage by Treatment Status



Note: This figure displays extensive- and intensive-margin 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 line labeled 'T' reports averages for participants assigned to the treatment group; the 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.

Table A.9: Local Average Treatment Effects of LinkedIn Use on Job Attributes

	(1) Hours	(2) Permanent	(3) Promoted	(4) >1 Employer
Panel A: Six Months After Program Completion				
LinkedIn use	7.191	0.050	0.015	0.004
	(2.627)	(0.040)	(0.017)	(0.032)
Control mean	25.523	0.129	0.038	0.123
# respondents	996	1002	1006	1003
#cohorts	30	30	30	30
Adjusted R2	-0.004	0.104	0.001	0.007
Panel B: Twelve Months After Program Completion				
LinkedIn use	4.889	0.056	-0.023	-0.069
	(1.352)	(0.030)	(0.029)	(0.037)
Control mean	29.233	0.189	0.118	0.144
# respondents	894	892	895	897
#cohorts	30	30	30	30
Adjusted R2	0.021	0.044	-0.008	-0.021

Coefficients are from regressing each employment-related outcome on LinkedIn use, instrumented by treatment assignment, and stratification block fixed effects. LinkedIn use is defined as the first principal component of an indicator for having an account, the number of connections, average power, profile completion, profiles viewed, and jobs viewed. This is standardized to have mean zero and standard deviation one in the control group. The first stage coefficient is 0.68 with standard error 0.11 and F-statistic 39.8. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort.

Table A.10: Treatment Effects on Alternative Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)	
	Aspiration wage			Reservation wage			
	Program end	6 month	12 month	Program end	6 month	12 month	
Treated cohort	0.047	0.090	0.052	0.043	0.023	0.061	
	(0.037)	(0.043)	(0.034)	(0.039)	(0.025)	(0.032)	
Control mean	10.518	10.469	10.565	9.249	9.289	9.435	
# respondents	1247	1119	988	1233	1119	988	
# cohorts	29	30	30	29	30	30	
Adjusted R2	0.096	0.100	0.069	0.148	0.080	0.081	
	(7)	(8)	(9)	(10)	(11)	(12)	
	Excitement about future			Locus of control			
	Program end	6 month	12 month	Program end	6 month	12 month	
Treated cohort	0.036	-0.002	0.005	0.026	-0.023	0.022	
	(0.021)	(0.031)	(0.026)	(0.024)	(0.023)	(0.027)	
Control mean	0.646	0.706	0.708	0.535	0.723	0.695	
# respondents	1252	1119	988	1252	1119	988	
# cohorts	29	30	30	30	30	30	
Adjusted R2	-0.000	-0.007	0.013	0.007	0.002	-0.000	
	(13)	(14)	(15)	(16)	(17)	(18)	(19)
	Trust in future			Engagement	Curiosity	Enthusiasm	Energy
	Program end	6 month	12 month				
Treated cohort	-0.023	0.037	-0.007	-0.003	0.105	0.038	0.061
	(0.016)	(0.020)	(0.025)	(0.029)	(0.096)	(0.093)	(0.093)
Control mean	0.680	0.680	0.715	4.829	0.062	0.066	0.075
# respondents	1252	1119	988	1250	1602	1602	1602
# cohorts	29	30	30	29	30	30	30
Adjusted R2	0.019	0.003	0.003	0.008	0.095	0.048	0.062

Coefficients are from regressing the variable in each column on a treatment indicator and stratification block fixed effects. Heteroskedasticity-robust standard errors are shown in parentheses, clustered by cohort. Variables in columns 1-15 are self-reports collected in an end-of-training survey and follow-up phone surveys six and twelve months later. Reservation and aspiration wage have been transformed by the inverse hyperbolic sine function. Excitement about future is a binary indicator of whether a participant's self-reported level of excitement about the future is greater than the median level of excitement. Locus of control and trust in future are also binary measures constructed in the same way. The engagement variable in column 16 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 17 through 19 report treatment effects on subjective measures provided by the managers responsible for training the cohorts. The variables are the average of the standardized scores for the last three weeks of the training program.

C Intervention Details

C.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 less 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, and '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 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 retraining 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.

C.2 Intervention Cost and Benefit-Cost Calculations

The intervention costs US$48 per candidate at the purchasing power parity exchange rate, or US$20 at the nominal exchange rate.[21] We estimate this figure by multiplying Harambee's average per-candidate cost of an 8-week job readiness program, US$3,833 PPP, by the share of the program time allocated to the intervention, 1.25%. Harambee allocated approximately 4 hours to the LinkedIn training per job readiness program: 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 participant stipends (US$6 PPP).

The intervention increases employment by 7.3 percentage points in the sample of 890 treated candidates (using the estimate in column 1 of Table 2). This implies 65 more employed candidates and hence a cost of US$656 PPP 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 hours worked at the national minimum wage. Treatment increases average weekly hours by 4.2 and 2.9 at respectively six and twelve months after the program. This mostly reflects the extensive margin increase in employment. If treated participants work an extra 2.9 hours in each of 50 weeks in the year after treatment and are paid the national minimum hourly wage of US$3.33 PPP, then treatment increases earnings by US$480 PPP over one year. This implies a benefit-cost ratio of 10-1 over a one-year horizon. This is likely to be a lower bound on the true benefit-cost ratio per participant if participants retain their jobs for more than one year or earn more than the minimum wage. Assuming participants earn the national minimum wage is very conservative, as the minimum wage is close to the 5$^{\text{th}}$ percentile of the national distribution of earnings for the employed (Finn, 2015).[22] The benefit-cost ratio of a larger-scale increase in online professional networking training may obviously be lower.

[21] We use purchasing power parity conversion factors from http://wdi.worldbank.org/table/4.16, averaged over the intervention period.

[22] 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.

C.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:

- 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	

<table>
<tr><th>Week</th><th>Instruction to Training Manager</th><th>Details</th></tr>
<tr><td></td><td>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</td><td></td></tr>
<tr><td></td><td>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</td><td>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!</td></tr>
</table>

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%3A442519841446542856726%2CVSRPtargetId%3A4731%2CVSRPcmpt%3Ahero https://www.linkedin.com/company/614583?trk=vsrp_companies_res_name&trkInfo=VSRPsearchId%3A442519841446544243080%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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