Online Appendix

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

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A Robustness Checks for Employment Effects

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

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

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

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

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

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

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

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

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

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

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

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

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

Table A.4: Treatment Effects on Employment Using Stable Sample

	(1) End of program	(2) 6 months	(3) 12 months
Treated cohort	0.133	0.115	0.088
	(0.025)	(0.041)	(0.030)
Control group mean	0.747	0.633	0.689
# respondents	873	873	873
# cohorts	30	30	30

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. Sample includes only respondents with employment data from all three waves

same (Table A.3 Panel B).

The treatment effects on employment are robust to accounting for differential non-response by treatment arm. Table A.3 Panel C demonstrates this. The panel reports bounds on employment effects assuming that the small number of extra responders in the treatment group are all unemployed (row 1) or all employed (row 2), following Lee (2009). The bounds are never wider than 1.8 percentage points. This result is unsurprising, as the response rates in both rounds differ by at most 1.2 percentage points between treatment and control groups.

We also estimate treatment effects on employment for the 873 candidates (53% of the sample) whose employment status is observed in all three waves and report these results in Table A.4. The estimated effects on employment at the end of the program and 6 months later are slightly larger in this sample than in the full sample, showing that treated participants who do not get jobs at the end of the program are slightly more likely to attrit from future survey waves.

B Additional Results Discussed in Paper

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

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

Table B.1: Treatment Effects on LinkedIn Use

	(1) Has LinkedIn account	(2) Opened LI account during training*	(3) Profile completeness	(4) Profiles viewed	(5) Jobs viewed
Treated cohort	0.314	0.422	0.243	0.584	0.058
	(0.049)	(0.050)	(0.036)	(0.129)	(0.023)
Control group mean	0.484	0.094	0.301	0.378	0.178
Control mean|account			0.631	0.810	0.381
# respondents	1638	1566	1599	1493	1493
# cohorts	30	30	30	30	30
Adjusted R2	0.140	0.282	0.116	0.086	0.029

	(6) # connections	(7) # bachelors connections	(8) # manager connections	(9) # job applications	(10) # views of profile
Treated cohort	8.609	0.754	0.543	0.009	1.198
	(1.513)	(0.130)	(0.095)	(0.004)	(0.276)
Control group mean	6.145	0.503	0.365	0.014	0.654
Control mean|account	12.807	1.048	0.761	0.030	1.664
# respondents	1629	1629	1629	1493	1362
# cohorts	30	30	30	30	30
Adjusted R2	0.111	0.124	0.118	0.018	0.108

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

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

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

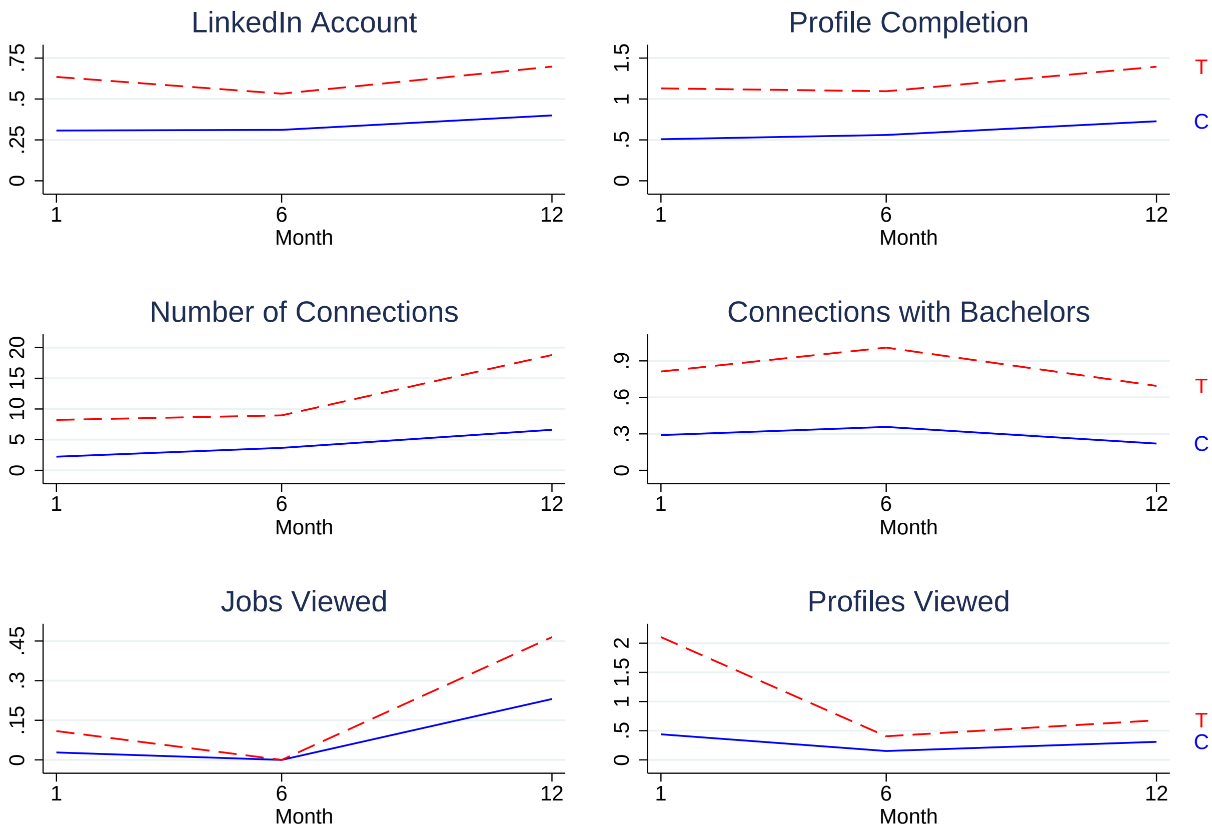
	(5) # bachelors connections	(6) # manager connections	(7) # job applications	(8) # views of profile
Total treatment effect	0.754	0.543	0.009	1.198
	(0.129)	(0.095)	(0.004)	(0.275)
Extensive margin	0.329	0.239	0.009	0.522
	(0.051)	(0.037)	(0.001)	(0.080)
Intensive margin	0.425	0.304	-0.000	0.676
	(0.121)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

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

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

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

Figure B.1: LinkedIn Usage by Treatment Status



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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Table B.8: Treatment Effects on Sector of Employment

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

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

Table B.9: Treatment Effects on Engagement

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

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

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

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

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

Table B.10: Treatment Effects on Aspirations

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

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

C Alternative Approach to Explaining Treatment Effects

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

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

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

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

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

Using this approach, LinkedIn use explains at least two thirds of the treatment effect on end-of-program employment. Treatment increases employment by 7 percentage points and the probability of having a LinkedIn account by 32 percentage points (Table C.1, panel A, column 1). The indirect effect accounts for 73% of the treatment effect on initial employment with standard error 31 percentage points (panel B, column 1). The direct effect of treatment on employment, not explained by LinkedIn use, is only 1.9 percentage points and is not statistically significantly different to zero. Having a LinkedIn account is not a perfect measure of LinkedIn use. We therefore repeat the exercise replacing this indicator with the LinkedIn usage index introduced in Section 3: the first principal component of having an account, opening an account during training, the number of profiles viewed, the number of jobs viewed, profile completeness, the number of times the profile is viewed, the total number of connections, the number of connections with bachelors degree, the number of connections with managerial jobs, and the number of job applications submitted on LinkedIn.[14] This shifts $\hat{S}_1$ to 0.67 with standard error 0.25 (panel B, column 2).

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

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

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

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

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

both LinkedIn use and employment.[15] This is a strong assumption and we present three sensitivity analyses related to this assumption. First, we estimate the system (2)-(4) conditional on age, gender, education, past employment, and psychometric assessment scores. This increases the share of the employment effects explained by LinkedIn use by three percentage points.

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

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

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

Third, we repeat the analysis with a multidimensional measure of LinkedIn use to account for possible measurement error from collapsing use to a single measure. This addresses the possibility of measurement error violating the identifying assumption (Heckman and Pinto, 2015; VanderWeele, 2012). We replace the scalar LI_{icr} with the four measures of LinkedIn use presented in Table 6: standardized indices for measures corresponding to each of supply-side information, demand-side information, and connections, as well as the number of job applications submitted on LinkedIn. The four components jointly explain 82% of the employment effect (standard error 28 percentage points).

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

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

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

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

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

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

This approach assumes that an estimate of δ based on non-experimental variation captures the effect of an experimentally-induced shift in LinkedIn on employment. This assumption may be violated if marginal candidates induced to use LinkedIn by treatment use it differently for job search to inframarginal candidates who would use it anyway. This assumption may also be violated if there are omitted characteristics associated with both LinkedIn use and employment or if LinkedIn use is measured with error. The direction of the bias from omitted variables and measurement error is theoretically ambiguous.[16] Given these concerns, we

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

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

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

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

D Deviations from the Pre-Analysis Plan

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

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

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

Our analysis deviates from the pre-analysis plan in three small ways. First, we omit the prespecified training manager fixed effects because several program managers managed only one cohort and several cohorts were co-managed. Including these fixed effects in the employment regressions does not substantively change our conclusions, yielding only slightly larger treatment effects and standard errors. Our pre-analysis plan specified that we would control for baseline covariates that were not balanced across control and treatment cohorts. None of the baseline covariates we observe are unbalanced, so we do not control for any covariates.[17]

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

Third, we add some non-prespecified outcomes that we collected in response to reviewer feedback. We did not prespecify treatment effects on program completion and post-training job placements (Table 6, columns 2, 3, and 7; Table B.8), on opening a LinkedIn account during training (Table 2, column 2), or on the probability of being employed at both the end of the training program and the current wave (Table 3, column 2). The LinkedIn summary indices in Tables 2 and 6 were added in response to reviewer feedback; they are constructed from prespecified outcomes but are not themselves prespecified. The non-experimental associations between employment and LinkedIn use and the mediation analysis reported in Section 5 were not prespecified. All other analysis, including subgroup analysis, was prespecified in the pre-analysis plan.

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

E Intervention Details

E.1 The Default Job Readiness Training

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

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

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

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

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

E.2 Intervention Cost and Benefit-Cost Calculations

The intervention costs USD48 per candidate at the purchasing power parity exchange rate, or USD21 at the nominal exchange rate.[18] We estimate this figure by multiplying Harambee's average per-candidate cost of an 8-week job readiness program, USD3,833, by the share of the program time allocated to the intervention, 1.25%. Harambee allocated approximately 4 hours of each job readiness program to LinkedIn training: 1.5 hours in the first week, and five 30-minute sessions later in the program. The job readiness program cost covers staff time for training, administration, and liaising with employers about interviews; facility rental; IT costs; and a stipend of USD6 per participant.

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

We also calculate a pecuniary benefit-cost ratio by valuing the extra employment for two scenarios. First, we assign employed participants "typical" earnings for their sector. We assign USD16,000: the mean earnings for call center workers in urban locations with at most 3 years tenure in that job from South Africa's Quarterly Labour Force Surveys (QLFS) for 2017-2019 (Statistics South Africa, 2016, 2017a, 2018). Under this assumption, treatment increases the average participant's annual earnings by roughly USD1,100 (= USD16,000 times the 7 percentage point employment effect). This implies a benefit:cost ratio of 23. Second, we make the much more conservative assumption that employed participants earn the statutory minimum wage of USD3 per hour and work full time, implying annual earnings of roughly USD6,050. Under this assumption, treatment increases the average participant's annual earnings by roughly USD420, implying a benefit:cost ratio of 8.7.

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

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

bution of earnings for the employed (Finn, 2015).[19] The call center scenario assumes participants all work in call centers. This is plausible for most participants given the names of their employers and interviews with program staff, but we do not directly observe participants' job titles or descriptions. The QLFS data on call center workers' earnings have relatively small samples, as they account for only 0.7% of all workers surveyed for the QLFS. But the mean is not too far from the mean annual salary of roughly USD19,600 reported by the industry association (Business Process Enabling South Africa, 2018). The industry association values the mean non-salary benefits package at an extra USD4,700. We exclude non-salary benefits from the benefit-cost calculations using QLFS data, as the QLFS does not report the financial value of non-salary benefits.

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

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

E.3 LinkedIn Training Curriculum

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

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

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

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

Introducing LinkedIn to Workforce Training Participants

A Curriculum

Developed in partnership by
Harambee Youth Employment Accelerator and RTI International

A Global Center for Youth Employment Initiative

Global Center for Youth Employment

harambee
WORK FOR WORK

RTI INTERNATIONAL

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

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

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

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

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

Thanks!

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

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

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

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

Global Center for Youth Employment

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

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

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

Week	Instruction to Training Manager	Details
		https://www.linkedin.com/company/standard-bank-south-africa?trk=affco https://www.linkedin.com/company/4731?trk=vsrp_companies_hero_name&trkInfo=VSRPsearchId%3A4425198414465428567 26%2CVSRPtargetId%3A4731%2CVSRPcmpt%3Ahero https://www.linkedin.com/company/614583?trk=vsrp_companies_res_name&trkInfo=VSRPsearchId%3A4425198414465442430 80%2CVSRPtargetId%3A614583%2CVSRPcmpt%3Aprimary https://www.linkedin.com/company/17634?trk=vsrp_companies_cluster_name&trkInfo=VSRPsearchId%3A4425198414471364899 71%2CVSRPtargetId%3A17634%2CVSRPcmpt%3Acompanies_cluster https://www.linkedin.com/company/12696?trk=vsrp_companies_res_name&trkInfo=VSRPsearchId%3A4425198414471366662 71%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.

Appendix References

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