Employment Exposure: Employment and Wage Effects in Urban Malawi

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I. Introduction

Extensive research shows that positive labor-related events are critical to exiting poverty, whereas job losses or limited job opportunities prevent such mobility (Fields et al. 2003; Baulch 2011; Inchauste 2012). Furthermore, the 2013 World Development Report (World Bank 2013) documents that simply being employed is not enough to lift people out of poverty; rather, increased labor earnings are also necessary. To better inform pro-poverty-reduction policies, it is thus important to understand the key determinants of wage growth, specifically among young people. One important driver of wage growth is acquired work experience, either firm specific or general experience.[1] Young people who have acquired the least experience are also the most at risk of unemployment and stagnant future-wage growth.

This paper contributes to this literature in the context of a low-income urban area in a developing country. It examines the effect of short-term work experience with a private employer on employment and wages in Malawi. The sample of relatively inexperienced male youths provides a novel opportunity to analyze work experience as a driver of wage growth. Empirically, this is challenging, as work experience is correlated with other, unobservable factors affecting employment or wages. For example, individuals who acquire work experience may exhibit better noncognitive skills that are not observable in the

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[1] There is debate regarding how large the effects of job tenure are, but there is consensus on the sign of the effect (Altonji and Shakotko 1987; Topel 1991; Altonji and Williams 2005; Buchinsky et al. 2010). There is also debate regarding the impact of in-school labor market experience on wages in the United States. Most studies find a sizeable labor market payoff to this type of experience (Meyer and Wise 1982; Coleman 1984; Ruhm 1995, 1997; Light 1999, 2001).

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data.[2] To overcome this identification challenge, we exploit an unusual source of random variation in short-term employment taken from another experimental study, which is discussed in detail by Godlonton (2020). The experimental study randomly allocated a probabilistic chance of short-term employment in a real job during a real recruitment process. The randomly determined employment options provide a suitable instrument for acquired short-term work experience. By accounting for an individual's work experience using his randomly assigned chance of gaining experience from the short-term job, we estimate the effect of short-term work experience on employment and wages.

This approach also helps us overcome an additional common problem inherent in measuring the returns-to-work experience in developing countries—the dearth of detailed work experience data that would allow for more accurate measurements than simply relying on an experience proxy (such as age − years of schooling − 6). We use employment history data for the 8-month period following the experiment and, importantly, measure actual experience rather than potential experience. Potential experience is considered a poor proxy in general, and the prevalence of interrupted or delayed schooling and periods of unemployment in the developing-country context renders this an even poorer proxy for actual experience in these areas (Lockheed and Verspoor 1991; Lam, Ardington, and Leibbrandt 2011; Pugatch 2018).

We find that acquired short-term work experience has a positive, albeit imprecisely estimated, impact on employment. We also find a sizeable (and statistically significant) wage return to work experience. The work experience opportunity provided in the experiment increases average wages by slightly less than $4 per day during the postintervention period. These wage impacts do not appear to be concentrated among a few individuals; rather, we see a distributional shift among those acquiring the short-term work experience opportunity. Month-to-month estimates are noisy but document a relatively consistent pattern across the 8-month postintervention period. The results are robust to minimum-maximum bounds and weighting methods that adjust for attrition. Notably, we also establish that the wage returns are not driven by continued employment with the recruiter.

To further understand the driving forces behind these large estimated wage returns, we explore how the impacts vary with employees' ability and experience (two characteristics on which the randomization was stratified). We observe important heterogeneity. Individuals of lower ability (as assessed by a numeracy and literacy test) benefit the most from the work experience. This is

[2] Several papers have shown that noncognitive skills influence labor market outcomes (Bowles, Gintis, and Osborne 2001; Jacob 2002; Heckman, Stixrud, and Urzua 2006).

consistent with potential inefficiencies in the low-skilled sector of the urban labor market, induced by employers hiring based primarily on test scores, be they results of Malawi's national secondary school examination or other recruitment tests. It is also consistent with such individuals having the most to benefit from the resulting broadened social networks, whereas higher-ability individuals can overcome the dearth of social connectedness. The value of connections is arguably more valuable to lower-ability types.

Using ancillary data, we consider several competing theories that may underpin these results. We find suggestive support using quantitative and qualitative follow-up data that the broadened employment network achieved through the employment opportunity may be a key contributing factor. Other potential mechanisms through which experience leads to wage increases are explored. The data do not support the hypotheses that using reference letters or increased reservation wages drives the observed wage increases.

These results add to the policy debate about active labor market programs, which are designed to improve employment outcomes by providing participants with work experience. The empirical evidence on such programs provides mixed results. In systematic reviews of the literature, the key takeaway has been that the impact of job-training programs are modest at best (Heckman, Lalonde, and Smith 1999; Kluve 2006), although Card, Kluve, and Weber (2010) show that certain types of programs, such as job-search assistance programs, exhibit more favorable impacts, particularly in the medium run. Also, more recently, Pallais (2014) finds large employment effects in the context of short-term experience through oDesk. Furthermore, just like the returns to education, the impacts of such programs may be larger in low-income countries; however, these programs are less extensively studied in a developing-country context. Betcherman, Olivas, and Dar (2004) review the impact-evaluation literature regarding job-training programs and find only 19 studies conducted in developing countries (and none in Africa). In both this and Ibarrarán and Shady's (2009) review of job-training programs in Latin America, the estimated impacts of job-training programs appear to be larger in developing than developed countries. Finally, in a recent review, Blattman and Ralston (2015) focus on low-income and fragile states and include several studies in Africa. They find little support that skills training has been effective and instead push for policy makers to place more emphasis on programs that include capital injections, as the evidence base increasingly suggests that such policies are more effective (e.g., Blattman and Dercon 2018).

This paper is organized as follows: Section II presents the experimental variation and data used; Section III presents the empirical strategy and the main results; Section IV examines and discusses potential mechanisms; and Section V concludes.

II. Experiment and Data

A. Experimental Variation

The experiment was a collaborative effort between a local independent recruiter and the research team. The sample of respondents was drawn from a recruitment process that hired male interviewers, during which trainees also participated in an experiment that offered randomly determined probabilistic jobs. The recruiter posted advertisements to recruit individuals for short-term interviewer positions. Interested applicants who met the eligibility criteria (male, aged 18 and older, who completed secondary schooling and arrived punctually for the initial screening assessment test) were required to write an initial assessment test and were encouraged to submit their résumé. The top-performing applicants, totaling 278 individuals, were offered an opportunity to participate in the extended training and recruitment process. Figure 1 outlines the timeline of the data used in this paper.

Consenting individuals ($N = 268$) participating in the recruitment and training process were offered a probabilistic chance of an alternative employment opportunity. Individuals were assigned a 0%, 1%, 5%, 50%, 75%, or 100% chance of alternative employment in the event that they failed to secure employment through the recruiter's normal competitive hiring process. The recruiter's job (earned job) and the alternative job (lottery job) were of equal duration and paid the same wage.[3] Thus, those who became employed through the project acquired the same amount of work experience at the same pay, regardless of whether they ultimately worked for the recruiter or in the alternative job.

Estimation of the effect of the probabilistic job needs to account for the fact that the experiment increased the likelihood of being both selected for the recruiter's job and eligible for the alternative job. As shown in Godlonton (2020), the probability of being hired by the recruiter was higher among those who received the 75% or 100% chance of an alternative job. A core criterion for the recruiter's job was good performance on tests administered during the initial training. Anecdotally, this is true, and it is further supported by an empirical analysis of the determinants of the recruiter's hiring decision.[4] The R^2 of a univariate regression of employment with the recruiter on participants' standardized average test score during training is 0.357. Controlling for a host of other covariates, the importance of the test score remains sizeable. An increase of

[3] Individuals were still able to earn a job through the recruitment process by performing well during the job training; those who secured both jobs were required to either take the recruiter's job or turn down both job offers.

[4] Furthermore, the costs of the alternative jobs were not a burden to the recruiter, as they were funded by external research funds; i.e., there is no reason for the recruiter to disproportionately hire those assigned higher outside options to minimize wage costs.

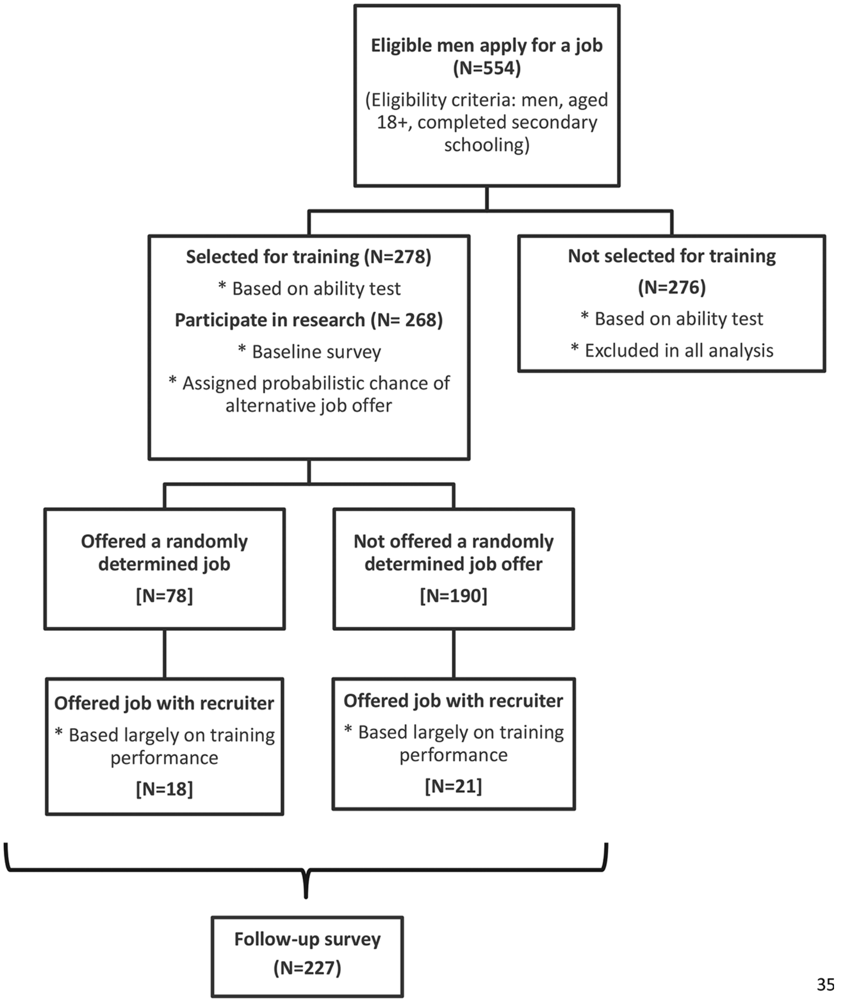

Figure 1. Timeline of experiment and data collection activities.

1 standard deviation in the composite test score results in a 9.7 percentage point increase in the likelihood that the individual is hired. Individuals who received higher outside probabilistic offers performed better during the training on these tests as well as on other performance indicators measured during the training. Godlonton (2020) shows that a possible pathway for this behavior is a stress response: individuals with more secure outside options were able to perform better due to reduced stress related to job uncertainty.

The recruiting firms' employment decision was primarily driven by performance on tests administered during training. Participants receiving the guarantee of employment performed significantly better on these tests. We refer to this phenomenon as the behavioral response to probabilistic job offers. We will return to this issue and its implications for the empirical strategy in Section III.

Once the recruitment process was completed, the probabilistic chances of employment were realized. For individuals who were assigned a 1%, 5%, 50%, or 75% chance of an alternative job, random draws were conducted.[5] Here, we use the treatment assignment (i.e., the probability of an alternative job) to instrument for acquired short-term work experience (either employment with the recruiter or in the randomly determined alternate jobs). This unusual determination of employment provides a novel opportunity to measure the causal effect of short-term work experience on future labor market outcomes in a low-income urban context.

The work experience opportunity provided was short term: 5 days of paid work experience. The job offered by the recruiter was for standard employment as an interviewer. The alternative jobs included different research assistant (RA) tasks, including archival research, data entry, and translation and transcription of qualitative interviews. Many of these tasks may embody some real acquisition of new and transferable skills for the participants. On completion of the job, participants received a generic letter of reference.

B. Data

Data come from a baseline survey collected prior to the start of the recruitment process, administrative records about treatment assignment and employment realizations for both probabilistic alternative jobs and standard recruiter jobs, and a follow-up survey conducted 9 months after the completion of the experiment's work opportunities.

1. Baseline Data

Prior to the start of the recruitment process, respondents completed numeracy and literacy tests and submitted their résumés. These tests are used to construct an ability measure. A baseline survey complements these data, providing information on basic demographics and general education and work experience. The baseline survey was self-administered by respondents.

[5] For example, an individual assigned a 75% chance of getting an alternative job drew a token from a bag that contained 75 red tokens and 25 green tokens. If the individual drew a red token, then he was offered the alternative job; if he drew a green token, he was not. Similar draws were conducted by each individual, with token distributions adjusted for his randomly assigned probabilistic treatment group. Individuals assigned a 0% chance knew with certainty that they were not eligible for alternative jobs, whereas those assigned a 100% chance knew that they were guaranteed alternative jobs, so no draws were conducted in those cases.

2. Probabilistic Alternative Job Offers

The analysis uses both the assignment to treatment records and the realization of the probabilistic draws (i.e., whether each participant was actually offered a job). Assignment to an employment probability was stratified by baseline-ability quintile and prior experience with the recruiter.

Table 1 shows results from balance tests across all treatment groups for the full sample. Columns 1–6 show the means of selected relevant baseline variables for all six treatment groups. Column 7 shows that the *p*-values for the test that averages among all six groups are equal to one another. The groups appear to be well balanced, with only two *p*-values less than .10. Individuals assigned to the employment guarantee program and the 1% outside job offer are less likely to be from the Chewa community. Further, those in the 0% and 1% outside-job-offer group report being more likely to have conducted a job search in the past month.

3. Follow-Up Survey Data

A follow-up survey was conducted 9 months after the implementation of the experiment. The survey was conducted by phone and included an extensive module on job search, labor market perceptions (current and future likelihood of finding employment), current employment, and employment experiences over the past 8 months, and current and past wages. Although the reference period for the survey is the 9-month period following the completion of the work experience opportunity, some participants erroneously report work tied to the experiment a month after it was completed. To deal with this survey recall error, we exclude the first month of recall data and rely only on the 8-month period beginning 1 month after the completion of the work related to the experiment.[6] To determine whether the results are driven by employment with the recruiting firm itself, we also construct average employment and wage outcomes that exclude employment with the recruitment firm. We revisit this issue in Section IV.

Table 2 shows that attrition is not statistically significantly associated with treatment status; 84.7% of the sample were successfully interviewed at follow-up. The attrition rate is lowest among participants who had received the 75% job guarantee (7.1%) and highest among those receiving a 0% chance of an alternative job (18.9%). The difference in attrition between these two groups, although large, is not statistically significant (p-value $= .168$). Moreover, the probability of receiving an alternative job does not predict the probability of

[6] All results are qualitatively similar when including the first month following the employment opportunity.

TABLE 1
SUMMARY STATISTICS AND BALANCING TESTS

	Treatment Assignment						
	0% (1)	1% (2)	5% (3)	50% (4)	75% (5)	100% (6)	*F*-Statistic[a] (7)
	A. Demographics						
Age	25.887	25.893	24.865	25.463	26.464	25.240	.757
	(.711)	(.633)	(.601)	(.475)	(1.116)	(.922)	
Married	.189	.250	.135	.093	.250	.120	.207
	(.054)	(.058)	(.048)	(.040)	(.083)	(.066)	
Any child	.189	.214	.154	.074	.250	.120	.169
	(.054)	(.055)	(.051)	(.036)	(.083)	(.066)	
Number of children	.358	.393	.250	.130	.500	.200	.225
	(.121)	(.119)	(.099)	(.070)	(.196)	(.115)	
Years of education	13.264	13.071	13.115	13.130	13.071	13.600	.255
	(.118)	(.124)	(.144)	(.130)	(.154)	(.200)	
Income (USD, 3 months)	187.550	274.048	167.350	200.539	279.472	319.250	.241
	(36.134)	(51.676)	(29.336)	(30.126)	(63.410)	(87.822)	
Ability score	−.076	−.007	−.020	.033	.116	.009	.978
	(.132)	(.137)	(.138)	(.145)	(.188)	(.203)	
	B. Tribe						
Chewa	.396	.179	.365	.333	.429	.120	.005
	(.068)	(.052)	(.067)	(.065)	(.095)	(.066)	
Lomwe	.075	.125	.096	.093	.107	.240	.636
	(.037)	(.045)	(.041)	(.040)	(.060)	(.087)	
Ngoni	.132	.143	.173	.222	.107	.200	.738
	(.047)	(.047)	(.053)	(.057)	(.060)	(.082)	
Tumbuka	.170	.250	.115	.204	.143	.280	.390
	(.052)	(.058)	(.045)	(.055)	(.067)	(.092)	
Other	.208	.250	.192	.148	.214	.160	.831
	(.056)	(.058)	(.055)	(.049)	(.079)	(.075)	
	C. Work Experience						
Work experience on CV	.434	.339	.288	.537	.429	.480	.270
	(.069)	(.064)	(.063)	(.068)	(.095)	(.102)	
Ever worked with recruiter	.113	.107	.115	.093	.143	.040	.715
	(.044)	(.042)	(.045)	(.040)	(.067)	(.040)	
Any work in past month	.623	.679	.673	.593	.571	.800	.385
	(.067)	(.063)	(.066)	(.067)	(.095)	(.082)	
Any work in past 6 months	.792	.911	.904	.815	.893	.960	.137
	(.056)	(.038)	(.041)	(.053)	(.060)	(.040)	
Fraction of 6 months worked	.462	.473	.404	.420	.393	.507	.739
	(.053)	(.048)	(.047)	(.052)	(.067)	(.073)	
Any job search in past month	.132	.232	.096	.037	.071	.080	.051
	(.047)	(.057)	(.041)	(.026)	(.050)	(.055)	

Note. Shown are group means or proportions (where applicable, e.g., married), with standard errors reported in parentheses. The main sample of 268 participants is used. Income is measured in US dollars (USD) and includes all self-reported income from the past 3 months in the following categories: farming, *ganyu* (piecework), formal employment, own business, remittances, pension, and other. The ability scores are a composite measure of literacy and numeracy scores and are presented in standardized units. CV = curriculum vitae.
[a] These *p*-values correspond to the joint *F*-test of the means/proportions being equal across all treatment groups.

TABLE 2
SAMPLE SIZE AND ATTRITION

	N (1)	Mean (2)	Standard Deviation (3)		
Treatment conditions:					
0% probability	53	.811	.395		
1% probability	56	.857	.353		
5% probability	52	.827	.382		
50% probability	54	.852	.359		
75% probability	28	.929	.262		
100% probability	25	.840	.374		
Full sample	268	.847	.361		
p-value of *F*-test of joint significance[a]		.827			
	1%	5%	50%	75%	100%
p-values of *t*-tests of pairwise differences in finding rate means:					
0%	.510	.826	.564	.168	.745
1%		.666	.939	.396	.844
5%			.724	.233	.882
50%				.364	.893
75%					.376

Note. Individuals were assigned to one of six treatment groups. If they received a 0% chance of an alternative (i.e., if they were in the 0% probability treatment group), then they had no chance of receiving the alternative job, and similarly for the 1%, 5%, 50%, 75%, and 100% probability groups. Due to budgetary considerations, there were twice as many assigned to the lower-probability groups as compared with the lower groups.
[a] 0% = 1% = 5% = 50% = 75% = 100%.

being interviewed at follow-up (coefficient = 0.049, p-value = .433). Given that the level of attrition is nontrivial, we will examine the robustness of the results to minimum-maximum bounds and weighting.

Table 3 does not show differential attrition for many other baseline characteristics, including age, education, ability, and previous work experience (col. 5). Respondents from the Ngoni tribe, as well as those who had worked in the 6 months prior to baseline, are slightly less likely to attrit (significant at the 5% and 10% levels, respectively). However, these differences are not large in magnitude. There is limited systematic differential attrition by treatment status (i.e., the probability of the alternative job) that is correlated with baseline characteristics.[7]

The final analytical sample includes the 227 respondents found at follow-up (table 3). The average respondent in this sample is approximately 26 years old;

[7] To test this, we regress an indicator for being in the follow-up sample on the probability of being assigned an alternative job, the baseline characteristic of interest, and that probability interacted with the baseline characteristic (table A1). Of all the baseline covariates considered, we find only one characteristic that matters differentially with treatment status in predicting attrition: job-search activity in the previous month.

TABLE 3
SAMPLE AND ATTRITION

	Baseline (N = 268)		Follow-Up (N = 228)		Difference (Baseline Mean – Follow-Up Mean)
	Mean (1)	SD (2)	Mean (3)	SD (4)	(5)
	A. Demographics				
Age	25.604	4.638	25.718	4.662	−.114
Married	.172	.378	.172	.378	.000
Any child	.164	.371	.167	.374	−.003
Number of children	.299	.784	.313	.811	−.014
Years of education	13.183	.940	13.220	.938	−.037
Income (USD, 3 months)	206.123	228.803	210.617	237.777	−4.494
Ability score	−.001	1.003	.030	1.017	−.031
	B. Tribe				
Chewa	.310	.463	.300	.459	.010
Lomwe	.108	.311	.110	.314	−.002
Ngoni	.164	.371	.181	.386	−.016**
Tumbuka	.190	.393	.189	.393	.001
Other	.201	.402	.198	.400	.003
	C. Education and Work				
Work experience on CV	.649	.478	.648	.479	−.009
Ever worked with recruiter	.104	.306	.097	.296	.008
Any work in past month	.646	.479	.665	.473	−.020
Any work in past 6 months	.869	.338	.890	.314	−.020*
Fraction of 6 months worked	2.657	2.176	2.727	2.175	−.070
Any job search in past month	.116	.320	.110	.314	.006

Note. The baseline sample consists of 268 individuals who participated in the recruitment process. The follow-up sample (227 respondents) is the main analytical sample used. USD = US dollars; CV = curriculum vitae.
* Statistical significance at the 10% level.
** Statistical significance at the 5% level.

17.2% are married. Approximately 16.7% of the sample have at least one child, and those who do have at least one child have 1.8 children on average. Respondents are relatively well educated for Malawi, with an average 13 years of education. This result is driven by the eligibility criteria of the recruiter, which required candidates to have completed secondary school. Despite being relatively well educated, however, all men in the sample were actively seeking work at the time of the baseline survey. They report earnings of approximately $210 per month spanning the 3-month period prior to the experiment.

III. Empirical Strategy

A. Empirical Approach

If experience were randomly assigned across individuals, then we could estimate the average treatment effect of experience on employment and wages using ordinary least squares (OLS). In that case, one would estimate the following regression equation:

$$y_i = \alpha + \beta_1 JO_i + X_i^{\prime}\delta + \varepsilon_i, \tag{1}$$

where $y_i =$ employment (or wages) for individual i, JO_i is a dummy indicator for whether the individual was offered a job, and X_i represents a set of covariates.

However, in our setting, work experience was not itself randomly assigned. Instead, individuals were randomly assigned different probabilities of obtaining work experience. These probabilistic job guarantees affected their likelihood of obtaining experience from one of two different types of jobs: the recruiter's job and the alternative job. We therefore present two sets of estimates. First, we show the intention-to-treat (ITT) estimates:

$$Y_i = \alpha_0 + \beta_1 \mathrm{JobOfferProbability}_i + X_i^{\prime}\delta + \varepsilon_i, \tag{2}$$

where $\mathrm{JobOfferProbability}_i$ captures the probability assigned to the individual i of receiving an alternative job. To complement this approach, we also adopt an instrumental variable (IV) approach to measure the impacts among those induced into receiving work experience through the probabilistic job offers. To do so, we estimate the following set of regressions:

$$Y_i = \alpha_0 + \beta_1 \mathrm{AnyJO}_i + X_i^{\prime}\delta + \varepsilon_i, \tag{3}$$

$$\begin{aligned} \mathrm{AnyJO}_i = {} & \pi_0 + \pi_1 P1_i + \pi_1 P5_i + \pi_1 P50_i \\ & + \pi_1 P75_i + \pi_1 P100_i + X_i^{\prime}\varphi + \varepsilon_i, \end{aligned} \tag{4}$$

where AnyJO_i measures whether individual i was offered a short-term job and $P1_i$, $P5_i$, $P50_i$, $P75_i$, and $P100_i$ are binary indicators for the different treatment arms. We use the full set of job probability treatment indicators due to the dual effect of these probabilities on the increased realization of the lottery jobs and the impacts on the recruiter-attained jobs. For the latter, there is a nonlinear relationship, implying that the full set of treatment indicators is the more appropriate specification for the first stage. The variable X_i is a set of individual-specific covariates that includes age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator for whether the respondent has any work experience or reports any work or job search in the past month, and the number of months in the past 6 months (at baseline) in which the respondent has worked.

We include stratification-cell fixed effects to account for the stratification of treatment assignment by ability and prior work experience with the recruiter. The key coefficient of interest is β_1. Conditional on instrument validity, β_1 captures the local average treatment effect of the short-term employment on labor market outcomes: employment and wages. We allow for possible heteroskedasticity in the error terms by using heteroskedastic-robust standard errors.

The labor market outcomes of interest are measured with Y_i. We examine employment impacts using the share of months employed in the subsequent 8-month period. To examine the effects at the intensive margin, we focus on (i) the average number of days worked and (ii) the average daily wage earned by individual i across the 8-month period. We present the wage effects both in levels and using the inverse hyperbolic sine transformation of wages. "Day" is used as the reference unit as this is most appropriate in the local context. Institutionally, Malawian labor policies pertain to daily employment; for example, the minimum wage law is with respect to daily wages and not hourly wages.[8]

Regression tables (tables 5–7, 9) follow a similar structure; ITT estimates are presented in panel A (eq. [2]), and results from the IV approach are presented in panel B (eq. [4]). All monetary values are expressed in dollars.

B. *Identification Assumptions*

For the randomized outside option probabilities to serve as a valid instrument for work experience, they need to satisfy two conditions: (1) the instrument must be correlated with the endogenous variable, and (2) the probabilistic job offers must not affect later labor market outcomes except through the acquired work experience.

The first condition implies that the assigned probability of alternative employment should predict whether the job seeker acquired any job (either the earned job or the lottery job) through this intervention. Estimating the first-stage relationship shows that the instrument is, indeed, relevant (table 4). The probabilistic outside options strongly predict the probability with which participants received any job (earned or lottery job). This expected result derives mechanically from the assignment of alternative jobs, as well as through participants' behavioral response to the job guarantees mentioned earlier. Both mechanisms (mechanical and behavioral) work in favor of a higher probabilistic job guarantee resulting in a higher chance of subsequent employment. Table 4, column 1, confirms this pattern. A total of 16.3% of individuals assigned a zero chance of an alternative job got a job.

[8] Daily or even more highly aggregated wages are also salient to respondents. The follow-up survey allowed individuals to choose the time unit for reporting their wages, with 75.8% of respondents reporting monthly wages and 18.5% reporting daily wages.

TABLE 4
FIRST-STAGE RESULTS

	Job Offer or Recruiter's Job Offer		
	(1)	(2)	(3)
Job guarantee:			
1%	.025	.030	−.005
	[.081]	[.078]	[.082]
5%	.047	.045	.038
	[.085]	[.079]	[.086]
50%	.402***	.423***	.443***
	[.094]	[.090]	[.093]
75%	.568***	.543***	.568***
	[.105]	[.104]	[.107]
100%	.837***	.860***	.864***
	[.057]	[.055]	[.067]
Constant	.163***	.804***	.648
	[.057]	[.153]	[.481]
Observations	227	227	227
R^2	.327	.382	.431
Stratification-cell fixed effects	No	Yes	Yes
F-statistic (of instruments)	101.11	87.47	76.36
Average of dependent variable		.361	

Note. The treatment group with 0% chance of alternative employment is omitted in these regressions. The dependent variable is whether the individual received an alternative job offer or one of the recruiter's job offers. The set of covariates includes age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator for whether the respondent has any work experience or reports any work or job search in the past month, and the number of months in the past 6 months (at baseline) in which the respondent has worked. Robust standard errors are reported in brackets.
*** Statistical significance at the 1% level.

Individuals assigned a 1% or 5% chance of an alternative job are not more likely than those who were assigned a 0% chance to get any job through this recruitment process. The coefficients are positive as predicted, although the standard errors are large. Individuals assigned a 50%, 75%, or 100% chance of receiving of an alternative job are respectively 40.2, 56.8, or 83.7 percentage points more likely to get any job than those with no chance of receiving an alternative job. The first-stage *F*-statistic is 76.36 for the preferred specification (table 4, col. 3), far above the rule-of-thumb threshold for weak instrument concerns. These results are robust to the inclusion of stratification-cell fixed effects (col. 2) and additional covariates (col. 3).

The exogeneity condition for the IV strategy requires that, conditional on baseline characteristics, the probabilistic job offers do not affect later employment outcomes independently of acquiring a job through the experiment (earned or lottery job). Monotonicity would be violated if higher probabilistic job offers had reduced the likelihood of acquiring the recruiter's job. However, as discussed previously, this is not the case.

Another concern is that the probabilistic job offers affected skill acquisition during training and that those skills were subsequently rewarded by the labor market. The finding in Godlonton (2020) that individuals perform differentially on recruiter-administered training tests during the recruitment process may initially heighten that concern. However, it is unlikely that there were general benefits to the training given by this experiment. The training conducted by the recruiter and evaluated in the performance tests was tailored to the specific needs of that particular recruiter's temporary job, which was interviewer for a health survey. Participants worked systematically through the questionnaire that the recruiter planned to administer to understand the terminology of and instructions for filling in each item. Skills related to this particular questionnaire are highly firm- and project-specific and are unlikely to be valuable in the general labor market. Moreover, for the training to have an impact on the labor market, the differential performance of the participants needed to be observable to future employers prior to employment. Individuals did not receive their grades on these assessment tests, and letters of reference only described the nature of the job, not the trainee's specific performance. As such, the only way for the differential performance during training to affect subsequent employment and earnings in the outside labor market after the intervention was for outside employers to value the specific content of the training conducted by the recruiter during the experiment. As stated previously, this was unlikely.[9] Generally, in this context, even when individuals apply for a new interviewer position within the same firm, they still are required to undergo training. In other words, experienced and novice interviewers undergo the same training for each survey on which they work. Nonetheless, repeated exposure to survey training may be valued.

IV. Results

Table 5 presents the main results, which is the impact of the short-term work experience on employment and wages. Outcomes are aggregated by individual across the 8-month postintervention period. Columns 1, 4, 7, and 10 present the simple OLS specification without any controls; columns 2, 5, 8, and 11 include stratification-cell fixed effects; and columns 3, 6, 9, and 12 add the full set of covariates.

[9] We restrict the analysis by excluding those assigned the 100% treatment group and those assigned the 0% treatment group. These subgroups show that the results are slightly smaller and, in some cases, lose statistical significance, which is not surprising given the small sample sizes. These estimates also show that the results are not eliminated by dropping either of these groups, which suggests that the results are not driven by differential learning (results not shown).

TABLE 5

RETURNS TO WORK EXPERIENCE: EMPLOYMENT AND WAGE RESULTS

	Proportion of 8-Month Postintervention Period Employed			8-Month Postintervention Period Average: Number of Days Worked per Week			Daily Wage			IHS Daily Wage[a]		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. ITT Estimates												
Probability of outside job offer	.050	.050	.059	.478	.533	.664*	2.918*	3.182*	3.192**	.301	.340	.334
	[.072]	[.072]	[.071]	[.389]	[.384]	[.351]	[1.678]	[1.727]	[1.570]	[.275]	[.275]	[.245]
B. IV Estimates[b]												
Got a job or recruiter's job offer (IV)	.068	.068	.088	.622	.745	.858**	3.801*	4.191*	3.829**	.398	.468	.415
	[.090]	[.090]	[.090]	[.486]	[.479]	[.419]	[2.149]	[2.218]	[1.904]	[.346]	[.347]	[.291]
Stratification-cell fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other covariates	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	227	227	227	227	227	227	227	227	227	227	227	227
Average of dependent variable (no job)		.415			2.272			5.036			1.535	

Note. The set of covariates include age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator for whether the respondent has any work experience, reports any work or job search in the past month, and the number of months in the past 6 months the respondent has worked. IHS = inverse hyperbolic sine; ITT = intention to treat; IV = instrumental variable.

[a] The IHS log transformation has been used.

[b] Dummy indicators for treatment assignment (i.e., assignment to a 0%, 1%, 5%, 50%, 75%, or 100% chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

* Statistical significance at the 10% level.

** Statistical significance at the 5% level.

A. Extensive Margin: Employment Impacts

The key employment variable is the proportion of months employed during the postintervention period.[10] These results are presented in table 5, columns 1–3. As the probability of the outside offer increases, so, too, does the probability of subsequent employment (table 5, panel A). Specifically, for every 10 percentage point increase in the probability of the outside offer, subsequent employment increases by approximately 0.5 percentage points. However, these results are not statistically significant.

Turning to the IV results (table 5, panel B), we find them to be consistent. Short-term work experience increases the probability of subsequent employment by 6.8–8.8 percentage points. The estimated coefficients increase in magnitude and precision when we include stratification-cell fixed effects (col. 2) and covariates (col. 3). The estimated effect is large, approximately a 20% increase in the probability of being employed, but it continues to be statistically insignificant. Figure 2 plots the estimated employment impacts of the job separately for each of the 8 months following the intervention. Notably, estimated coefficients are relatively consistent across the observed time period.

B. Intensive Margin: Days Worked and Wage Impacts

Although the employment effects are suggestive of a net positive impact, they are imprecise. Next, we turn to document the impacts on the intensive margin. Given the high rate of underemployment in the Malawian context, there is considerable scope to increase labor supply along the intensive margin. Data from a nationally representative household survey shows that urban men who have completed secondary school work only 23.4 hours per week, conditional on being employed. In this labor market, individuals are more likely to adjust their labor supply at the daily rather than the hourly margin; they are also paid per day rather than per hour. Therefore, our preferred specifications, presented in table 5, pertain to the number of days worked per week (cols. 4–6), daily wages measured in US dollars (cols. 7–9), and the inverse hyperbolic sine transformation of daily wages (cols. 10–12).

Individuals assigned higher outside job probabilities work more days per week and earn higher wages (table 6, panel A). Results are similar, albeit unsurprisingly slightly larger, for the IV results in panel B. Individuals induced into work experience from the experimental job probabilities work one additional day per week on average and earn \$3.83 more per day. This implies an almost 80% increase in daily wages. The logged wage results also exhibit a large wage

[10] This is constructed by calculating the fraction of months in which the individual is employed over the 8-month period following the intervention.

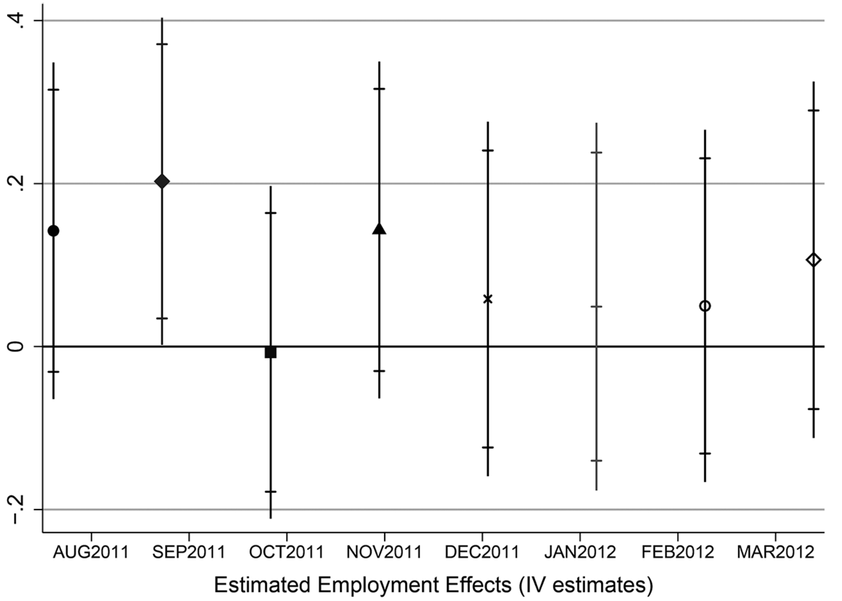

Figure 2. Estimated employment impact of job offer by month (instrumental variable [IV] estimates).

return. However, these results are smaller in magnitude and not statistically significant at conventional levels. Still, the results are broadly consistent.

To address concerns related to the nontrivial level of attrition, we conduct two bounding exercises, presented in table A2. We present both weighted results, in which weights are constructed using (the inverse of) predicted probabilities of noncompletion by treatment status (Fitzgerald, Gottschalk, and Moffitt 1998), and conservative minimum-maximum bounds (Horowitz and Manski 1998). In both cases, we find broadly consistent results.

To further unpack the wage impacts, we consider month-by-month impacts and examine plots of the cumulative distribution function (CDF) of average daily wages measured across the 8-month period. Month-by-month estimates are plotted in figure 3. In all months, the effect on daily wages is positive, ranging from approximately \$1–\$6. Due to the imprecision of the estimates, despite the large range of effect sizes across months, the individual monthly estimates are not statistically different from one another. Figure 4 shows the wage CDFs for those who received no chance of the lottery job, some chance of the lottery job, and a guarantee of the lottery job. We see that the wage CDFs for those with some chance of a lottery job is shifted to the right of those with no chance, whereas those with a guarantee exhibit a wage distribution shifted even farther to the right. Figure 5 takes an alternative approach and plots the wage CDFs for three groups: earned job, lottery job, and no job. This classification is not free of selectivity bias but still provides suggestive evidence of the underlying shifts in

TABLE 6
ARE RETURNS DRIVEN BY RECRUITER EMPLOYMENT?

	Proportion of 8-Month Postintervention Period		8-Month Postintervention Period Average			
	Employed		Daily Wage		IHS Daily Wage[a]	
	(1)	(2)	(3)	(4)	(5)	(6)
	A. ITT Estimates					
Probability of outside job offer	.013	.107	2.914*	4.596***	.239	.580**
	[.074]	[.070]	[1.598]	[1.689]	[.258]	[.270]
	B. IV Estimates[b]					
Got a job or recruiter's job offer (IV)	.028	.125*	3.533*	4.988***	.308	.640**
	[.089]	[.074]	[1.932]	[1.837]	[.305]	[.284]
Stratification-cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes
Wage/sample adjustment[c]	1	2	1	2	1	2
Observations	227	189	227	189	227	189
Average of dependent variable (no job)	.329	.409	5.528	5.218	1.173	1.152

Note. The set of covariates include age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator for whether the respondent has any work experience, reports any work or job search in the past month, and the number of months in the past 6 months the respondent has worked. Robust standard errors are reported in brackets. IHS = inverse hyperbolic sine; ITT = intention to treat; IV = instrumental variable.

[a] The IHS log transformation has been used.

[b] Dummy indicators for treatment assignment (i.e., assignment to a 0%, 1%, 5%, 50%, 75%, or 100% chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

[c] Wage/sample adjustment type 1 means that all wages and employment with the recruiter during this time period are treated as missing in the construction of the average. Wage/sample adjustment type 2 means that the sample has been restricted to all individuals who did not work for the recruiter in the post-intervention period.

* Statistical significance at the 10% level.

** Statistical significance at the 5% level.

*** Statistical significance at the 1% level.

the wage distribution. We observe a clear rightward shift in the distribution of those receiving the lottery jobs, with the wage distribution of those receiving earned jobs shifted even farther to the right.[11]

C. *Does Recruiter Employment or Sector Employment Drive These Effects?*

We observe large wage impacts, so understanding how these were achieved is important. One potential pathway could be through additional employment with the recruiter after the intervention. We use two approaches to try to tease apart whether this is the case. First, we construct an alternative version of the labor outcomes. We exclude all months in which the individual worked for the recruiter in

[11] Figure A1 shows similar results using the average wage outcome that excludes any wages while employed with the recruiter.

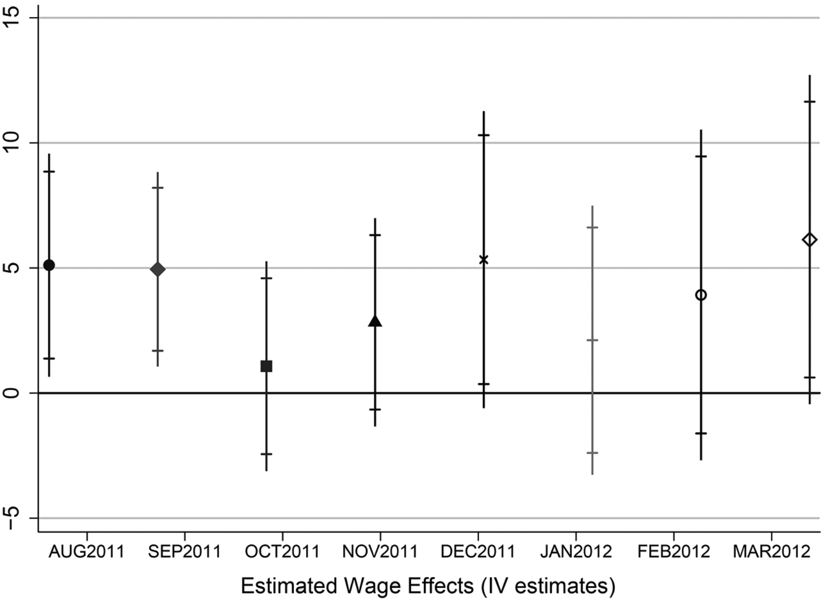

Figure 3. Estimated wage impact of job offer by month (instrumental variable [IV] estimates).

the construction of the average employment and wage outcomes over the 8-month postintervention period. In doing so, we maintain the full sample, as no participant worked for the recruiter for all 8 months following the intervention. Our second approach excludes all participants who worked for the recruiter at any point during the 8-month postintervention period from the analytical sample.

These results are presented in table 6. The odd columns show results for the first and preferred approach; even-numbered columns show results for the second approach. In both cases, we measure the impact on employment and wages measured with firms other than the recruiter. Overall, we find results remarkably similar to our earlier findings. Although the point estimates are fairly similar for the first (and preferred approach), the point estimates for the second approach indicate larger and more precise impacts. Employment with the recruiter does not appear to be the pathway for sustained wage impacts.

Another plausible pathway for the observed effects is sector-specific recruitment.[12] To examine this possibility, we first estimate the treatment impacts of

[12] We also attempted to examine occupational shifts using retrospective calendar job histories and categorizing jobs using the standard two-digit International Labor Organisation classification codes (International Standard Classification of Occupations 2008; https://www.ilo.org/global/publications/ilo-bookstore/order-online/books/WCMS_172572/lang–en/index.htm). Limited statistical power inhibits the ability to make strong claims for the observed occupational shifts. However, the pattern of results

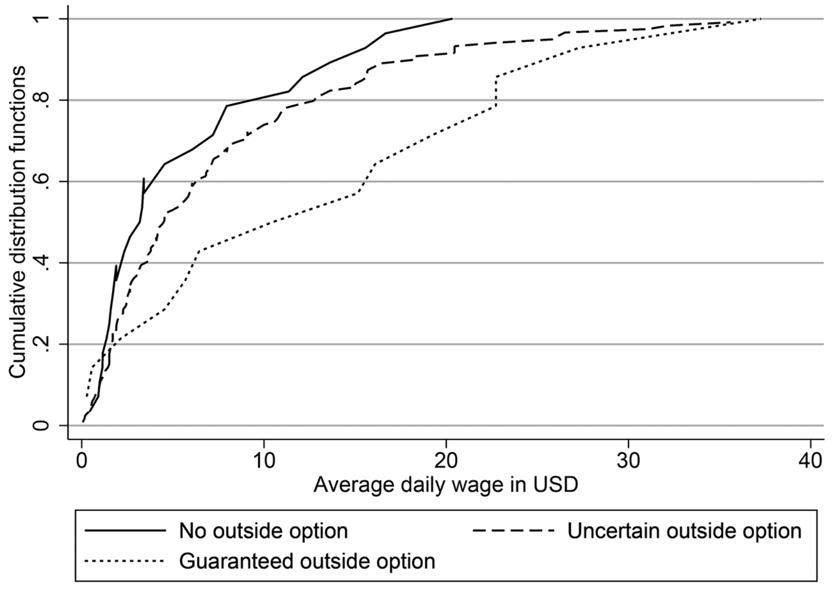

Figure 4. Distribution of average postintervention wages. USD = US dollars.

the employment experience on whether the individual worked any job in the past 8 months that can be classified as an RA position and how many months they held an RA position. Although both coefficients are positive, suggesting that there may have been an increase in such employment, the point estimates are noisy (table 7, cols. 1, 2).[13] To further unpack these findings, we consider whether the wage gains are driven through other RA or non-RA positions. These results are presented in table 7. The employment results mirror the earlier findings: we estimate (noisy) positive point estimates of the impact of the experience on employment in RA positions (col. 3) and non-RA positions (col. 4). We observe a very interesting pattern for wages. Although the point estimates are positive for both RA positions (cols. 5, 7) and non-RA positions (cols. 6, 8), the large wage returns seem to be driven by employment in non-RA positions. The difference between the estimates in levels relative to the logged form suggest that for a few individuals, these wage gains are considerable, pointing to the

suggests that work experience increases employment in both administrative and managerial roles, as well as in clerical and related work, whereas it reduces employment in agriculture and related occupations. However, none of the results are statistically significant.

[13] A previously circulated version of the paper showed significant point estimates, the difference being that the earlier version included the first month after the intervention. As such, the point estimate was driven by continued employment with the recruiter in the RA positions.

Figure 5. Distribution of average postintervention wages. USD = US dollars.

importance of impact heterogeneity, an issue we return to in the next section to the extent possible, given our limited sample.

As a further test, we consider treatment impacts on job permanence. Entry-level RA positions in Malawi are typically short term and higher paying, particularly for projects for international nongovernmental organizations or donor agencies, relative to wages paid for permanent jobs offered by local employers or government agencies. To proxy for job permanence, we use information from the unit in which individuals reported their current pay unit. Individuals self-reported the unit of payment for their current (primary) job at the daily, weekly, fortnightly, or monthly level. We infer that lower-frequency reporting levels correspond to longer duration contracts and construct a frequency of payment variable equal to 1 if the individual reports daily remuneration, 2 if weekly, 3 if fortnightly, and 4 if monthly remuneration. Table 7, column 9, reports the effects of work experience on this proxy for job permanence. The negative coefficient suggests that individuals induced to receive work experience through the experiment work in less permanent positions.

In sum, we find sizeable but noisy employment effects and significant wage impacts in response to the short-term work opportunity. The wage increases appear to be driven by increased employment by firms external to the recruiter. Further, the wage gains are highest among those in non-RA positions and are also associated with less permanent job contracts.

TABLE 7

ARE RETURNS DRIVEN BY EMPLOYMENT WITHIN THE SECTOR?

	Proportion of 8-Month Period Employed as an RA	Number of Months Employed as an RA	Proportion of 8-Month Postintervention Period		8-Month Postintervention Period Average				Pay (1 = Daily, 2 = Weekly, 3 = Fortnightly, 4 = Monthly)
			Employed		Daily Wage		IHS Daily Wage[a]		
			RA Post	Non-RA Post	RA Post	Non-RA Post	RA Post	Non-RA Post	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. ITT Estimates									
Probability of outside job offer	.040	.420	.042	.025	.462	2.730*	.224	.249	−.318
	[.065]	[.317]	[.039]	[.068]	[.781]	[1.563]	[.191]	[.268]	[.327]
B. IV Estimates[b]									
Got a job or recruiter's job offer (IV)	.042	.487	.049	.044	.541	3.288*	.249	.317	−.386
	[.078]	[.381]	[.047]	[.082]	[.921]	[1.908]	[.226]	[.321]	[.402]
Stratification-cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	227	227	227	227	227	227	227	227	166
F-statistic (first stage)									3.169
Average of dependent variable (no job)	.128	.515	.062	.353	1.233	3.803	.370	1.238	3.199

Note. The set of covariates include age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator for whether the respondent has any work experience, reports any work or job search in the past month, and the number of months in the past 6 months the respondent has worked. Robust standard errors are reported in brackets. RA = research assistant; IHS = inverse hyperbolic sine; ITT = intention to treat; IV = instrumental variable.

[a] The IHS log transformation has been used.

[b] Dummy indicators for treatment assignment (i.e., assignment to a 0%, 1%, 5%, 50%, 75%, or 100% chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

* Statistical significance at the 10% level.

V. Discussion

The average effects found are much larger than those obtained from nonexperimental Mincerian estimates in Malawi and other similar settings.[14] However, they are comparable to a recent experimental study (Pallais 2014) in the context of low-skill online work (oDesk). There are several reasons why the nonexperimental estimates may be substantially smaller. First, nonexperimental estimates typically use an inferior (but readily used and available) measure of work experience. Potential experience (considerably) overstates the amount of accumulated experience in this context. Recent evidence from an audit study in the Philippines highlights the importance of experience relative to education: callback rates were increased by 11% among those with any experience, but there were was no return to technical or vocational training (Beam, Hyman, and Theoharides 2017). Second, the type of experience studied by the experiment may be of higher quality than the average experience obtained in the labor market. Experience provided through the experiment was short term with a private, international employer. It is unlikely that 5 days of work in civil service would yield impacts similar to those observed here. In fact, Card, Kluve, and Weber (2010) do find less promising impacts for public sector programs in their review of active labor market programs. Also, the nonexperimental estimates represent average returns to experience for a population that is less educated than the highly skilled men included in the experiment. Although the experimental subjects still experience frequent periods of unemployment, they may experience substantively different returns than a less educated counterpart.

In addition, while the wage point estimates are large, they also exhibit large standard errors. Thus, it seems wise to put more weight on the direction of the effects and less weight on the precise magnitude of the effects. The noisy estimates also suggest highly heterogeneous returns, which we turn to next.

By examining heterogeneous returns by ability and several dimensions of prior experience, we hope to better understand what mechanisms are driving the observed wage impacts. Random assignment was stratified on respondents' ability scores and their prior experience with the recruiter. Both of these baseline characteristics are likely to interact with additional experience in important ways.

To proxy for ability, we use the composite measure of numeracy and literacy tests administered at baseline.[15] The numeracy and literacy tests were conducted in a timed environment in a classroom. Although intended to measure non-subject-specific knowledge, the composite measure is likely correlated with

[14] The estimated wage returns in this paper are equivalent to approximately 10 years of experience in the Malawi nonexperimental estimates obtained by Chirwa and Matita (2009).

[15] The results are similar when using the numeracy and literacy scores separately.

school grades.[16] School transcripts are also typically submitted to firms in Malawi, and as many as one in four individuals in Lilongwe report writing a test to be part of the job recruitment process.[17] In this labor market, experience may act as a complement to good grades in school and performance on recruitment tests and may thus disproportionately boost the employment prospects of top-performing individuals. Alternatively, individuals performing worse in school and on written tests may benefit the most from accumulated work experience, if, for example, top-performing individuals are hired independent of their work experience.

We also consider multiple dimensions of previously acquired work experience. Although the randomization was stratified on experience with the recruiter, only 10% of the sample had prior experience working with the recruiter. Given the sample size, this prohibits any rigorous heterogeneity analysis along this particular dimension. Instead, we focus on other definitions of prior experience that exhibit attributes similar to that of the experience acquired in this particular setting. These include any experience, experience with an international employer, and experience as an RA. Existing experience may act as a substitute for or complement to experimentally induced experience. If individuals already have experience, this particularly short-term opportunity may not add much value to a résumé. Alternatively, exposure to the world of work may have taught individuals how to network more effectively and introduced them to the importance of investing in their social (job) network for future employment. Such individuals may strategically use the opportunity as a means to broaden their network to leverage it for future job opportunities. Characterized in this way, the experiment may be less about the experience and skills gained and more about the social network provided.

To conduct the heterogeneity analysis, we interact an indicator variable for having received an alternative job (JO_i) with the baseline characteristic of interest ($\text{Base}_i \times JO_i$), using the set of treatment dummies as instruments for work experience. We instrument the endogenous regressors with the probability of an alternative job, and this probability interacted with the baseline characteristic. We focus only on two key outcomes: (1) the proportion of the 8-month post-intervention period in which the respondent is employed and (2) the inverse hyperbolic sine transformation of average wages across the 8-month period.

Table 8, column 1, shows the heterogeneity of impacts by ability. Estimated impacts are larger for individuals at the lower end of the ability distribution. For example, consider individuals at the 25th and 75th percentiles of the ability

[16] Unfortunately, school grades were not collected.

[17] This is calculated using unpublished data collected by Chinkhumba, Godlonton, and Thornton (2012) that sampled approximately 1,200 men aged 18–40 in Lilongwe.

TABLE 8
HETEROGENEITY OF WAGE AND EMPLOYMENT IMPACTS

	Ability (1)	Any Experience (2)	International Employer Experience (3)	Research Experience (4)
	A. Proportion of 8-Month Postintervention Period Employed			
Got a job	.108	.343	.058	.022
	[.073]	[.887]	[.083]	[.089]
Got job × baseline variable	−.171**	−.420	.224	.186
	[.078]	[1.396]	[.216]	[.171]
Variable	.106	.226	−.101	−.031
	[.099]	[.537]	[.117]	[.093]
Observations	227	226	227	227
p-value on interaction	.031	.966	.191	.204
Bonferroni-adjusted *p*-value	.118	1.000	.572	.599
Bonferroni-adjusted (for correlation) *p*-value	.069	1.000	.467	.553
	B. 8-Month Postintervention Period Average IHS Daily Wage[a]			
Got a job	.525*	3.413	.026	−.009
	[.279]	[4.622]	[.280]	[.334]
Got job × baseline variable	−.568**	−4.834	1.731**	1.079*
	[.269]	[7.251]	[.763]	[.618]
Variable	.010	2.023	−.337	−.309
	[.280]	[2.787]	[.399]	[.324]
Observations	227	226	227	227
p-value on interaction	.037	.561	.014	.06
Bonferroni-adjusted *p*-value	.140	.963	.055	.219
Bonferroni-adjusted (for correlation) *p*-value	.082	.939	.041	.196

Note. The probability of alternative employment (P_i) and the interaction of the baseline characteristic and the probability of alternative employment assigned ($\text{Base}_i \times P_i$) are used to instrument for the binary indicator JO_i and the interaction of the baseline characteristic and the job offer ($\text{Base}_i \times JO_i$). Stratification-cell fixed effects are included. The set of covariates include age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator for whether the respondent has any work experience, reports any work or job search in the past month, and the number of months in the past 6 months the respondent has worked. IHS = inverse hyperbolic sine.
[a] The IHS log transformation has been used.
* Statistical significance at the 10% level.
** Statistical significance at the 5% level.

distribution, respectively. Individuals at the 25th percentile were 25 percentage points more likely to be employed if they were induced to receive job experience through the experiment; they also earn approximately $11.01 more per day. In contrast, individuals at the 75th percentile were 1.5 percentage points less likely to be employed, although they earn approximately $2.20 more per day.[18] This

[18] Multiple hypothesis testing is of concern when examining multiple dimensions of heterogeneity (Fink, McConnell, and Vollmer 2014). We present the Bonferroni-adjusted *p*-values as well as Bonferroni adjustments that correct for correlation (Sankoh, Huque, and Dubey 1997). The ability and research experience interaction are not statistically significant at the 10% level when accounting for the Bonferroni correction, but the international experience interaction remains significant.

shows significantly larger wage returns among those scoring poorly on a written test. Thus, the experience seems to provide a foot in the door for candidates for whom other observable performance indicators are weaker and may otherwise have been screened out.

Table 8, columns 2–4, examine the extent to which effects vary by different prior experience. Because the sample consists of young men, the average estimated wage returns may be large because the job is one of the first they have held. Roughly 15% of the sample had no previous work experience based on the self-reported measure of the baseline survey. This number rises to 35% when measured using work experience reported on the participants' résumés. This gap suggests that individuals may not regularly update their résumés.[19] Or it may suggest that they define which jobs are suitable for inclusion on a résumé differently from the survey definition, which was intended to be broad and inclusive.[20] The information firms would receive is that provided on the résumé rather than that based on the survey data; thus, this is the measure used in this study. Somewhat surprisingly, the effects of work experience on subsequent employment and wages do not differ by work experience prior to the experiment. Yet, the wage returns are magnified for those with either international employer or existing research experience.

This set of heterogeneity results is consistent with the idea that the training opportunity provides participants with a broader social network and that those with previous experience in labor markets relying more heavily on short-term contracts—and, therefore, referrals—have learned the value of leveraging it. Social networks have been touted as an important mechanism through which individuals acquire employment opportunities (e.g., Granovetter 1973; Burns, Godlonton, and Keswell 2010; Beaman and Magruder 2012). For the job seeker, social connections can reduce search costs and lead to better-quality matches (Mortensen and Vishwanath 1994; Calvó-Armengol 2004; Galeotti and Merlino 2014). This could, in turn, lead to higher-paying wage opportunities. Simply participating in the jobs provided by this experiment may have facilitated new social connections between participants. Viewed in this way, the pattern of heterogeneity results across experience measures may suggest that this opportunity was complementary to certain types of previous experience. Qualitative interviews

[19] Balakasi and Godlonton (2014) find some evidence for this when comparing résumé and survey responses. Measures of internal consistency are better for opportunities held further in the past, suggesting that participants often fail to update their résumés in a timely manner.

[20] The specific question asked is as follows: "Have you ever worked? (Remember to think about jobs very broadly; that is, think about both part-time work and full-time work experiences. Any job for which you signed a formal contract or had an explicit conversation with an employer that lasted a minimum of 5 days.)"

conducted with human resources personnel suggest that short-term contracts typically rely more heavily on recruitment strategies that use personal networks. Thus, those with previous exposure to such jobs may have learned the value of networking more so than those who have gained experience in other sectors, such as public employment (e.g., those in teaching and other low-paying, entry-level civil servant jobs where one's social network may affect location of work but is less important for employment itself).

To further explore the role of the broadened network in facilitating the improved outcomes, we examine whether social network referrals and reference letters were differentially used. Unlike the experiments undertaken by Beaman and Magruder (2012) and Beaman, Keleher, and Magruder (2018), which are set up to test various aspects regarding the role of social connections in job referrals, this experiment was not designed to induce variation in social connections. However, we do measure the prevalence of social interactions that may have facilitated employment, such as whether individuals heard about other job opportunities through individuals met during the job opportunity and whether the jobs they held during the 8-month period following this job opportunity were a direct result of a referral.

Table 9, column 1, shows that individuals who received work experience as a result of the experiment are 22.2 percentage points more likely to have heard about a work opportunity through someone they met during the intervention. Individuals are also more likely to secure employment through one of these new connections, although the coefficient is not statistically significant (table 9, col. 3). In many cases, individuals may not be aware of the role that their network played in securing the job; individuals may also be reluctant to report others as being responsible for their employment status. Both of these factors could result in measurement error that would limit us from finding a result. The pattern of results for job referrals is broadly consistent across measures. We construct a social network index following Kling, Liebman, and Katz (2007) of the standardized job referral measures available and present those results in column 4. Here we see further suggestive evidence that social networks may have played a mediating role. Thus, the short time span of the experience was likely sufficient to forge new ties that are later leveraged for future employment opportunities.

Beaman, Keleher, and Magruder (2018) study referrals in this context and find that men are more likely to recommend other men for positions in general. In addition, when offered performance incentives for referral, men recommend higher-quality candidates, suggesting that under normal operating considerations, they do not necessarily recommend the highest-quality candidates they know. This is further supported by qualitative interviews we conducted with human resources personnel across a broad spectrum of employer types. These

TABLE 9
MECHANISMS

	Any Job Referral (1)	Number of Job Referrals (2)	Secured a Job through Referral (3)	Social Network Index (4)	Submitted Any Reference Letter (5)	Number of Times Used Any Reference Letter (6)	Self-Reported Month Reservation Wage (7)	Minimum Accepted Wage (8)	Proportion of Postperiod Engaged in Job Search (9)
	A. ITT Estimates								
Probability of outside job offer	.167*	.045	.067	.236*	−.067	−.329	99.606	3.040	.073
	[.095]	[.227]	[.047]	[.136]	[.092]	[.458]	[67.420]	[3.369]	[.062]
	B. IV Estimates[a]								
Got a job or recruiter's job offer (IV)	.222**	.100	.074	.306*	−.079	−.381	124.397	3.764	.089
	[.112]	[.266]	[.056]	[.159]	[.110]	[.546]	[88.030]	[4.156]	[.074]
Stratification-cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	215	214	214	215	227	227	221	165	227

Note. Stratification-cell fixed effects are included. The set of covariates include age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator for whether the respondent has any work experience, reports any work or job search in the past month, and the number of months in the past 6 months the respondent has worked. Robust standard errors are reported in brackets. ITT = intention to treat; IV = instrumental variable.

[a] Dummy indicators for treatment assignment (i.e., assignment to a 0%, 1%, 5%, 50%, 75%, or 100% chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

* Statistical significance at the 10% level.

** Statistical significance at the 5% level.

interviews highlight the importance of relying on personal networks as central to recruitment, with the following among responses: "We do post adverts. In addition, we mostly utilize personal networks to get additional CVs"; "If you know someone, then you would extend the advert to those people"; "After people have been referred through a contact, we do not follow up on references." Further they highlight the distinction of relying more heavily on broader social networks when they are advertising for short-term positions as opposed to long-term positions.

In sum, the broadened social (job) network acquired through this process seems to be a contributing factor in the large realized wage returns. Although the changes to the employment network seem to have played a role, the evidence presented is only suggestive. In what follows, we consider several other possibilities.

A. Reference Letters and Signaling

Information constraints on the employer's side may generate large wage effects. To test for the role of such informational constraints, we examine the use of reference letters. Employers may not infer any inherent impact of the work experience on worker productivity but may merely interpret it as a signal of an employee's ability (Spence 1973). Empirical evidence from a recent audit study in South Africa (Abel, Burger, and Piraino 2017) finds that callbacks increase dramatically when a reference letter is included.

On completion of the work experience, all participants received a standard letter of reference; this letter described the job in general terms but did not provide information about individual-specific performance. Given that these letters came from an international employer, however, employers may value the letter as a signal of underlying ability rather than a certification of skills acquired through experience.

Column 5 of table 9 shows that those who received work experience as a result of the experimental treatment were actually 7.9 percentage points less likely to use a reference letter when applying to a job. Consistent with this result, the average number of times that a reference letter was used to support a job application was lower for those who received experience. In both cases, the estimated coefficients are large. This may be a rational response. The Abel, Burger, and Piraino (2017) study finds that while women are more likely to find employment when randomly encouraged to submit a reference letter, men do not benefit from the use of such a letter. It is also consistent with a social network story in which individuals apply through someone they know and this personal recommendation invalidates the need for a reference letter, as highlighted by the qualitative interviews. Although we certainly cannot rule out the role of signaling, the explicit role of reference letters seems minimal.

B. Wage Expectations

Work experience may have increased subsequent labor market outcomes through altered wage expectations and reservation wages, with implications for job-search strategies, duration of unemployment, and match quality. Wages paid during this experiment may have been higher than reservation wages at baseline. If individuals updated their expectations by increasing their reservation wage, then the estimated impact on the employment effect might be muted, as individuals may be searching longer and differently for better-paying jobs.

We examine self-reported reservation wages and engagement in job searches, the results of which are presented in table 9, columns 7–9.[21] The impact of receiving a job on the monthly reservation wage is $124.40 but not statistically significant at conventional levels. More generally, the reported reservation wages are high, approximately 1.5 times higher than the average monthly income earned at baseline. Self-reported reservation wages are also high relative to wages reported in the follow-up survey. These results suggest that an increase in reservation wages is not likely an important pathway.

Another way to look at this pathway is to examine participants' job-search behavior. Table 9, column 9, examines the impact of the experience on the proportion of the 8-month period in which individuals actively sought work. If individuals changed their wage expectations, we would expect to observe more active job searches. We find limited support for this. Although the estimated coefficients are positive, they are not statistically significant.

VI. Conclusion

This paper uses a novel experiment that generated exogenous variation in short-term work experience to estimate the effect of such experience on employment and wages. The return to experience is large. Although we find imprecise but sizeable postintervention employment impacts, we document a large wage return. Individuals who received work experience earn approximately $4 more per day than those who did not, with results concentrated among job candidates with lower ability and those with prior experience with an international employer. The return to work experience persists throughout the 8-month period following the intervention.

The results are large when compared with nonexperimental estimates that rely on variation in potential experience. However, making direct comparisons to the nonexperimental estimates is difficult given the lack of variation in the amount of experience acquired for those induced to work by the experiment. The impacts are also large relative to experimental estimates of job-training programs, which typically find modest effects at best. However, the magnitude of the results is

[21] Unfortunately, individuals were not asked about whether they turned down any jobs during the postintervention period.

comparable to Pallais (2014). Additional analyses and qualitative reports suggest that the most likely explanation for the large observed wage returns is the broadened social network that individuals acquired through this process.

These results may not be generalizable to a less skilled population within Malawi or to a country whose underlying skill distribution and labor market conditions are different from Malawi. Even within Malawi, the treatment provided in the experiment is not available through any current public or private sector job-training initiatives. Because the job opportunities provided within the experiment were of uniform duration, we also cannot extrapolate the return to a longer period of work experience. Finally, the general equilibrium effects of such a program are not estimated. Given the small size of this intervention, it is not possible to determine the impact that such a program may have on nonparticipants if more widely rolled out.

However, while these caveats cannot be dismissed, the results presented here do provide rigorous evidence of the effect of work experience on subsequent employment outcomes in a low-income urban setting. The effects are substantial, suggesting that short-term training or employment programs that include work experience have transformative potential and providing justification for further research on the topic.

Appendix

Figure A1. Distribution of wages (excluding recruiter wages). USD = US dollars.

TABLE A1

SAMPLE AND ATTRITION

	Baseline (N = 268)			Covariate × Probability of Job Offer
	Mean	SD	Covariate	
	(1)	(2)	(3)	(4)
			A. Demographics	
Age	25.604	4.638	.004	.001
Married	.172	.378	−.031	.136
Any child	.164	.371	.000	.087
Number of children	.299	.784	.028	−.029
Years of education	13.183	.940	.064**	−.104
Income (USD, 3 months)	206.123	228.803	.00004	.00001
Ability score	−.001	1.003	.035	−.035
			B. Tribe	
Chewa	.310	.463	−.064	.093
Lomwe	.108	.311	.125*	−.304
Ngoni	.164	.371	.057	.138
Tumbuka	.190	.393	−.041	.112
Other	.201	.402	.029	−.188
			C. Education and Work	
Ever worked	.869	.338	−.014	−.152
Ever worked with recruiter	.104	.306	−.093	.107
Any work in past month	.646	.479	.039	.131
Any work in past 6 months	.869	.338	.109	.167
Fraction of 6 months worked	2.657	2.176	.008	.015
Any job search past month	.116	.320	−.085	.270**

Note. Columns 3 and 4 are from the same regression predicting where the dependent variable is whether or not the individual was found at follow-up. Columns 3 and 4 present the coefficient on the baseline characteristic and the interaction of the baseline coefficient and the assigned probability of a job offer, respectively. USD = US dollars.
* Statistical significance at the 10% level.
** Statistical significance at the 5% level.

TABLE A2

RETURNS TO WORK EXPERIENCE: EMPLOYMENT AND WAGE RESULTS

	Proportion of 8-Month Postintervention Period Employed			8-Month Postintervention Period Average: Number of Days Worked per Week			Daily Wage			IHS Daily Wage[a]		
	Weights (1)	Min. (2)	Max. (3)	Weights (4)	Min. (5)	Max. (6)	Weights (7)	Min. (8)	Max. (9)	Weights (10)	Min. (11)	Max. (12)
A. ITT Estimates												
Probability of outside job offer	.091	−.030	.165***	.607*	−.057	1.169***	2.623*	.652	5.520***	.277	−.134	.629**
	[.063]	[.067]	[.061]	[.359]	[.387]	[.380]	[1.530]	[1.996]	[1.796]	[.246]	[.266]	[.244]
B. IV Estimates[a]												
Got a job or recruiter's job offer (IV)	.112	.028	.101	.812*	.709*	1.198**	3.206*	4.203**	6.572***	.353	.298	.605**
	[.076]	[.081]	[.072]	[.431]	[.429]	[.472]	[1.861]	[2.022]	[2.305]	[.293]	[.297]	[.305]
Stratification-cell fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other covariates	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	227	227	227	227	227	227	227	227	227	227	227	227
Average of dependent variable (no job)		.415			2.272			5.036			1.535	

Note. Stratification-cell fixed effects are included. The set of covariates include age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator for whether the respondent has any work experience, reports any work or job search in the past month, and the number of months in the past 6 months the respondent has worked. IHS = inverse hyperbolic sine; Min. = minimum; Max. = maximum; ITT = intention to treat; IV = instrumental variable.

[a] Dummy indicators for treatment assignment (i.e., assignment to a 0%, 1%, 5%, 50%, 75%, or 100% chance of employment) are used to instrument for the binary indicator got a job offer from recruiter or through random determination.

* Statistical significance at the 10% level.

** Statistical significance at the 5% level.

*** Statistical significance at the 1% level.

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