Employment Exposure: Employment and Wage Effects in Urban Malawi

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Abstract

Labor earnings are critical to helping people escape poverty; thus, understanding the returns to the determinants of wage growth is important. This paper examines the role of one important driver of wage growth: acquired work experience. We utilize an experiment that randomized probabilistic job offers to estimate the employment and wage effects of short-term jobs among young men in a low-income urban setting. The results suggest large returns even among relatively well-educated, yet still under-employed individuals. Returns are largest among those scoring poorly on a literacy and numeracy test and among those with prior experience with an international employer. Suggestive evidence points to exposure to a broader social job network as a likely driver for the observed returns.

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1. Introduction

Extensive research shows that positive labor-related events are critical to exiting poverty, while 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 necessary. To better inform pro-poverty reduction policies, it is thus important to understand the key determinants of wage growth, specifically among the youth. One important driver of wage growth is acquired work experience, either firm-specific or general experience.[1] Youth 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 youth 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 non-cognitive skills that are not observable in the data.[2] To overcome this identification challenge, we exploit an unusual source of random variation in short-term employment taken from another experimental study, discussed in detail in Godlonton (2014). 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 rather than simply relying on an experience proxy (such as age-years of schooling - 6). We utilize employment history data for the eight-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, Verspoor, et al. 1991; Lam, Ardington and Leibbrandt 2011; and 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 post-intervention 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 eight-month post-intervention period. The results are robust to min-max 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 the employee's 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 consistent with potential inefficiencies in the low-skilled sector of the urban labor market, induced by employers hiring based primarily on test scores, be it the results of Malawi's national secondary school examination (MCSE) or other recruitment tests. It is also consistent with such individuals having the most to benefit from 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 references letters or increased reservation wages drive 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 take-away has been that the impact of job-training programs are modest at best (Heckman, Lalonde, 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 (none of which are in Africa) conducted in developing countries. In both this review and a review by Ibarrarán and Shady (2009) of job training programs in Latin America, the estimated impacts of job training programs appear to be larger in developing than developed countries. Finally, a recent review, Blattman and Ralston (2015) focus on low-income and fragile states, including several studies in Africa. They find little support that skills training has been effective and instead push for policymakers to place more emphasis on programs that include capital injections, as the evidence base increasingly suggests that such policies are more effective (for example: Blattman and Dercon 2018).

This paper is organized as follows. Section 2 presents the experimental variation and data used. Section 3 presents the empirical strategy and the main results. Section 4 examines and discusses potential mechanisms, while Section 5 concludes.

2 Experiment and data

2.1 Experimental variation

The experiment was a collaborative effort between a local independent recruiter and the research team. The sample of respondents is 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, completed secondary schooling, and arrived punctually for initial screening assessment test) were required to write an initial assessment test and were encouraged to submit their resume. 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-percent 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 both being selected for the recruiter's job and being eligible for the alternative job. As shown in Godlonton (2014), the probability of being hired by the recruiter was higher among those who received the 75- or 100- percent 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 is further supported by empirical analysis that examines the determinants of the recruiter's hiring decision.[4] The R-squared of a univariate regression of employment with the recruiter on the participants' standardized average test score during training is 0.357. Controlling for a host of other covariates, the importance of the test scores remains sizeable: a one-standard-deviation increase 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 (2014) 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 the probabilistic job offers. We will return to this issue and its implications for the empirical strategy in Section 3.

Once the recruitment process was completed, the probabilistic chances of employment were realized. For individuals assigned a 1-, 5-, 50-, or 75 percent 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 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: five days of paid work experience. The recruiter's job was for standard employment as an interviewer. The alternative jobs included different research assistant 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. Upon completion of the job, participants received a generic letter of reference.

2.2 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 nine months after the completion of the experiment's work opportunities.

Baseline data

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

Probabilistic alternative job offers

The analysis uses both the assignment to treatment records and the realization of the probabilistic draws (i.e. whether or not 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 through 6 show the means of selected relevant baseline variables for all six treatment groups. Column 7 shows the p-value 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 0.10. Individuals assigned to the employment guarantee and the 1 percent outside job offer are less like to be from the Chewa community. Further, those in the 0 and 1 percent outside job offer group report being more likely to have conducted a job search in the last month.

Follow-up survey data

A follow-up survey was conducted nine 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 last eight months, and current and past wages. While the reference period for the survey is the nine-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 eight-month period beginning one 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 4.

Table 2 shows that attrition is not statistically significantly associated with treatment status. A total of 84.7 percent of the sample was successfully interviewed at follow-up. The attrition rate is lowest among participants who had received the 75-percent job guarantee (7.1 percent) and highest among those receiving a 0-percent chance of an alternative job (18.9 percent). The difference in attrition between these two groups, although large, is not statistically significant (p=0.168). Moreover, the probability of receiving an alternative job does not predict the probability of being interviewed at follow-up (coeff. = 0.049, p-value = 0.433). Given that the level of attrition is non-trivial, we will examine the robustness of the results to min-max bounds and weighting.

In Table 3, we do not observe differential attrition for many other baseline characteristics, including age, education, ability, and previous work experience (Column 5). Respondents from the Ngoni tribe, as well as those who had worked in the six months prior to baseline, are slightly less likely to attrit (significant at the 5 percent level and 10 percent level, 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; 17.2 percent are married. Approximately 16.7 percent of the sample have at least one child, and those that do have at least one child have 1.8 children on average. Respondents are relatively well educated for Malawi, with an average of 13 years of education. This result is driven by the eligibility criteria of the recruiter, which required candidates to have completed secondary school education. 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 three-month period prior to the experiment.

3 Empirical strategy

3.1. Empirical approach

If experience was 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:

$$y_i = \alpha + \beta_1 JO_i + X_i'\delta + \varepsilon_i \quad (1)$$

where y_i = employment (or wages) for individual i and JO_i is a dummy indicator for whether or not the individual was offered a job. 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 estimates:

$$Y_i = \alpha_0 + \beta_1 JobOfferProbability_i + X_i'\delta + \varepsilon_i \qquad (2)$$

where $JobOfferProbability_i$ captures the probability assigned to the individual i of receiving an alternative job. To complement this approach, we also adopt an instrumental variables 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:

$$Y_i = \alpha_0 + \beta_1 AnyJO_i + X_i'\delta + \varepsilon_i \qquad (3)$$

$$AnyJO_i = \pi_0 + \pi_1 P1_i + \pi_1 P5_i + \pi_1 P50_i + \pi_1 P75_i + \pi_1 P100_i + X_i'\varphi + \varepsilon_i \qquad (4)$$

where $AnyJO_i$ measures whether individual i was offered a short term job; $P1_i$, $P5_i$, $P50_i$, $P75_i$, $P100_i$ 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, as well as the impacts on the recruiter attained jobs. For the latter, there is a non-linear relationship, implying that the full set of treatment indicators is the more appropriate specification for the first stage. X_i is a set of individual-specific covariates which 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 in the past month and any job search in the past month, and the number of months in the last six 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 (LATE) 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.

Y_i measures the labor market outcomes of interest. We examine employment impacts using the share of months employed in the subsequent eight-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 that the eight-month period. We present the wage effects both in levels and using the inverse sine hyperbolic 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, not hourly wages.[8]

Regression tables follow a similar structure; intention-to-treat estimates are presented in Panel A (equation (2)) and results from the instrumental variables approach are presented in Panel B (equation (4)). All monetary values are expressed in dollars.

3.2. Identification assumptions

For the randomized outside option probabilities to serve as a valid instrument for work experience, they need to satisfy two conditions: the instrument must be correlated with the endogenous variable and 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 or not 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 job" 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 percent of individuals assigned a zero chance of an alternative job got a job. Individuals assigned a 1- or 5- percent chance of an alternative job are not more likely than those who were assigned a 0-percent 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-, and 100- percent chance receiving of an alternative job are respectively 40.2, 56.8, and 83.7 percentage points more likely to get any job than those with no chance receiving of the alternative job. The first-stage F-statistic is 76.36 for the preferred specification (Table 4 Column 3), far above the rule of thumb threshold for weak instrument concerns. These results are robust to the inclusion of stratification cell fixed effects (Column 2) and additional covariates (Column 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 job" 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 (2014) 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 an interviewer for a health survey. Participants worked systematically through the questionnaire that the recruiter planned to administer in order 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 needs 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 is for outside employers to value the specific content of the training conducted by the recruiter during the experiment. As stated previously, this is 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.

4 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 eight-month post-intervention 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, while Columns (3), (6), (9) and (12) add the full set of covariates.

Extensive margin: employment impacts

The key employment variable is the proportion of months employed during the post-intervention period.[10] These results are presented in Table 5 Columns (1) through (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 instrumental variable results (Table 5, Panel B), we find consistent results. Short-term work experience increases the probability of subsequent employment by 6.8 to 8.8 percentage points. The estimated coefficients increase in magnitude and precision when we include stratification cell fixed effects (Column 2) and covariates (Column 3). The estimated effect is large, approximately a 20-percent increase in the probability of being employed, but continues to be statistically insignificant. Figure 2 plots the estimated employment impacts of the job separately for each of the eight months following the intervention. Notably, estimated coefficients are relatively consistent across the observed time period.

Intensive margin: days worked and wage impacts

While 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 (Columns 4 through 6), daily wages measured in USD (Columns 7 through 9), and the inverse sine log transformation of daily wages (Columns 10 through 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-percent increase in daily wages. The logged wage results also exhibit a large wage 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 non-trivial level of attrition, we conduct two bounding exercises presented in Appendix Table 2. We present both weighted results in which weights are constructed using (the inverse of) predicted probabilities of non-completion by treatment status (Fitzgerald, Gottschalk, and Moffitt 1998) and conservative min-max 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 eight-month period. Month-by-month estimates are plotted in Figure 3. In all months, the effect on daily wages is positive, ranging from approximately one to six dollars. 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, while those with a guarantee exhibit a wage distribution shifted even further 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 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 even further shifted to the right.[11]

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 the construction of the average employment and wage outcomes over the eight-month post-intervention period. In doing so, we maintain the full sample, as no participant worked for the recruiter for all eight months following the intervention. Our second approach excludes all participants who worked for the recruiter at any point during the eight-month post-intervention 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. While 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 the employment experience on whether the individual worked in any job in the last eight months that can be classified as a research assistant position, as well as how many months they held a research assistant position. While both coefficients are positive, suggesting that there may have been an increase in such employment, the point estimates are noisy (Table 7, Columns 1 and 2).[13] To further unpack these findings, we consider whether the wage gains are driven through other "research assistant (RA)" positions or "non-research assistant positions (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 (Column 3) and in non-RA positions (Column 4). We observe a very interesting pattern for wages. While the point estimates are positive for both RA positions (Columns 5 and 7) and non-RA positions (Columns 6 and 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 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 research assistant positions in Malawi are typically short-term and higher paying, particularly for projects for international NGOs or donor agencies, relative to wages paid for permanent jobs offered by local employers or government agencies. To proxy for job permanence, we utilize 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 one if the individual reports daily remuneration, two if weekly, three if fortnightly, and four 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.

5 Discussion

The average effects found are much larger than those obtained from non-experimental 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 non-experimental estimates may be substantially smaller. Non-experimental 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: call back rates increased by 11 percent among those with any experience; while they find no return to technical or vocational training (Beam, Hyman and Theohardies, 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 five 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 non-experimental estimates represent average returns to experience for a population that is less educated than the highly skilled men included in the experiment. While 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, as well as 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 test was conducted in a timed environment in a classroom. While intended to measure non-subject-specific knowledge, it is likely correlated with 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 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. While the randomization was stratified on experience with the recruiter, only 10 percent 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 a research assistant. Existing experience may act as a substitute for or a complement to experimentally induced experience. If individuals already have experience, this particularly short-term opportunity may not add much value to a resume. Alternatively, exposure to the world of work may have taught individuals how to network more effectively, as well as 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 in order 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 ($Base_i*JO_i$), 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: the proportion of the eight-month post intervention period in which the respondent is employed and the inverse sine hyperbolic transformation of average wages across the eight-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 percentile and the 75th percentile of the ability 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. On the other hand, 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 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 screened them out.

Table 8, Columns 2 through 4 examine the extent to which effects vary by different prior experience. Since the sample consists of young men, the average estimated wage returns may be large because the job is among one of the first they have held. Roughly 15 percent of the sample had no previous work experience based on the self-reported measure of the baseline survey. This number rises to 35 percent when measured using work experience reported on the participants' resumes. This gap suggests that individuals either do not regularly update their resumes[19] or define which jobs are suitable for inclusion on a resume differently to the survey definition, which was intended to be broad and inclusive.[20] The information that firms would receive is that provided on the resume 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 pre-experimental work experience. Yet, the wage returns are magnified for those with either international employer or existing research experience.

This set of heterogeneity results is consistent with idea that the training opportunity provides participants with a broader social network and 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.[21] 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 conducted with human resource personnel suggest that short term contracts typically rely more heavily on recruitment strategies relying on personal networks. Thus, those with previous exposure to such jobs may have learned the value of networking more so than those who have experience but in other sectors, such as public employment (e.g. teachers and other low paying entry level civil servant jobs where ones' 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 they met during the job opportunity and whether the jobs they held during the eight-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, Column 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 then leveraged later on for future employment opportunities.

The 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 resource personnel across a broad spectrum of employer types. These interviews highlight the importance of relying on personal networks as central to recruitment: "… 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 more heavily relying on broader social networks when they are advertising for short term positions as opposed to longer 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. While 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.

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 call-backs increase dramatically when a reference letter is included.

Upon 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.

Table 9, Column 5 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 where individuals apply through someone they know, and as a result do not submit a reference letter as the personal recommendation invalidates the need for such a letter as highlighted by the qualitative interviews. While we certainly cannot rule out the role of signaling, the explicit role of reference letters seems minimal.

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 through 9.[22] The impact of receiving a job on the monthly reservation wage is \$124.40 but is 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 eight-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. While the estimated coefficients are positive, they are not statistically significant.

6 Conclusion

This paper uses a novel experiment that generated exogenous variation in short-term work experience in order to estimate the effect of such experience on employment and wages. The return to experience is large. While we find imprecise but sizeable post-intervention 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 eight-month period following the intervention.

The results are large when compared to non-experimental estimates that rely on variation in potential experience. However, making direct comparisons to the non-experimental 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 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 opportunity provided within the experiment was 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 non-participants 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.

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Figure 1: Timeline of experiment and data collection activities

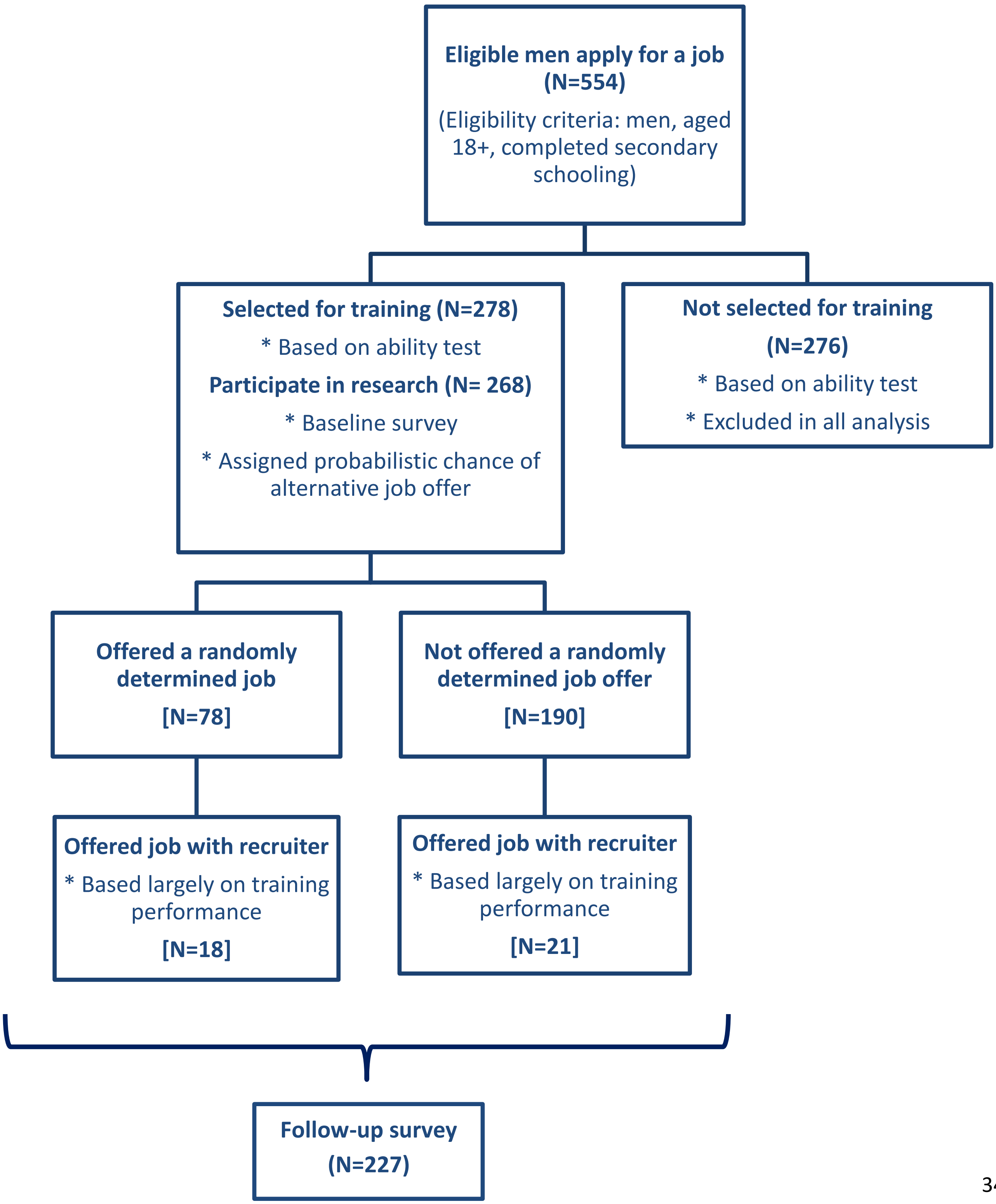


Figure 2: Estimated employment impact of job offer by month (IV estimates)

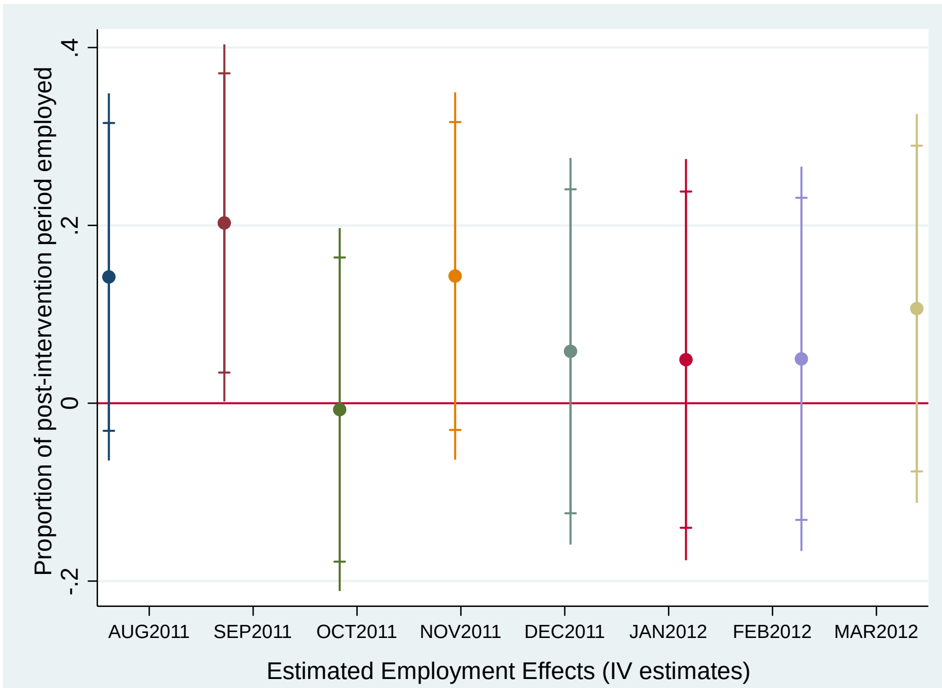


Figure 3: Estimated wage impact of job offer by month (IV estimates)

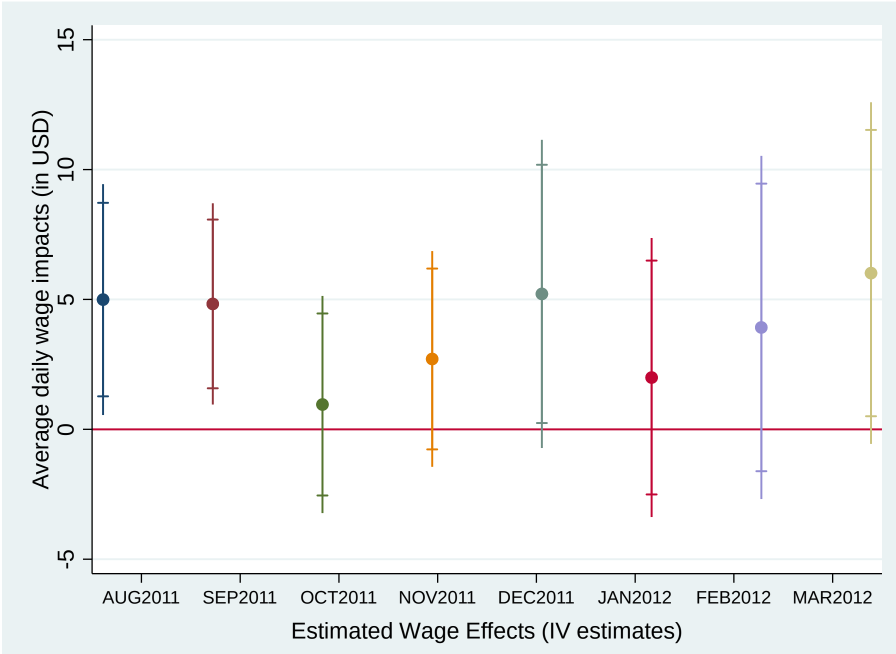


Figure 4: Distribution of average post-intervention wages

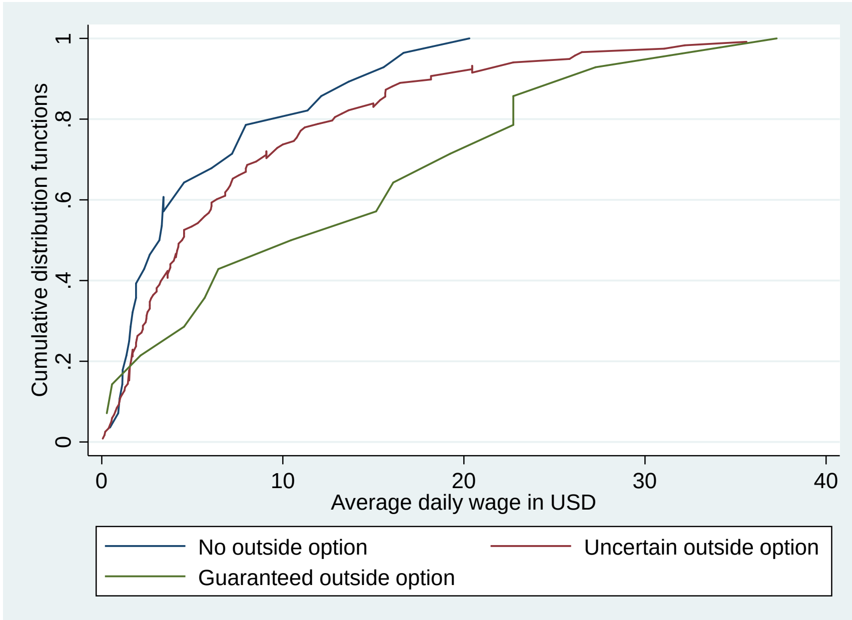


Figure 5: Distribution of average post-intervention wages

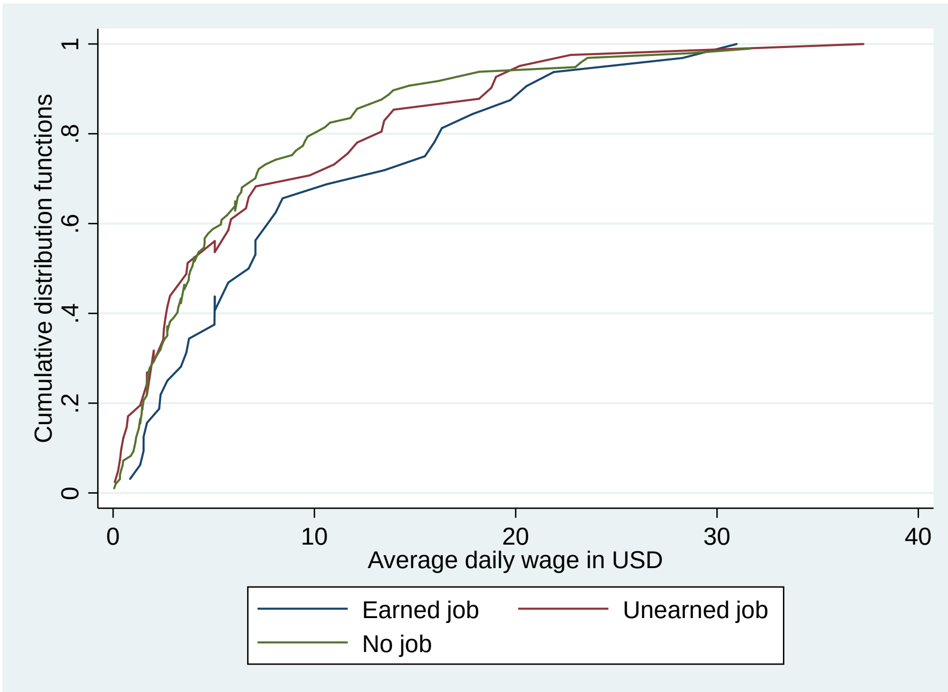


Table 1: Summary statistics and balancing tests

	Treatment Assignment						
	0%	1%	5%	50%	75%	100%	F-stat[1]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Demographics:							
Age	25.887	25.893	24.865	25.463	26.464	25.240	0.757
	(0.711)	(0.633)	(0.601)	(0.475)	(1.116)	(0.922)	
Married	0.189	0.250	0.135	0.093	0.250	0.120	0.207
	(0.054)	(0.058)	(0.048)	(0.040)	(0.083)	(0.066)	
Any child?	0.189	0.214	0.154	0.074	0.250	0.120	0.169
	(0.054)	(0.055)	(0.051)	(0.036)	(0.083)	(0.066)	
Number of children	0.358	0.393	0.250	0.130	0.500	0.200	0.225
	(0.121)	(0.119)	(0.099)	(0.070)	(0.196)	(0.115)	
Years of education	13.264	13.071	13.115	13.130	13.071	13.600	0.255
	(0.118)	(0.124)	(0.144)	(0.130)	(0.154)	(0.200)	
Income (USD, 3 months)	187.550	274.048	167.350	200.539	279.472	319.250	0.241
	(36.134)	(51.676)	(29.336)	(30.126)	(63.410)	(87.822)	
Ability score	-0.076	-0.007	-0.020	0.033	0.116	0.009	0.978
	(0.132)	(0.137)	(0.138)	(0.145)	(0.188)	(0.203)	
Tribe:							
Chewa	0.396	0.179	0.365	0.333	0.429	0.120	0.005
	(0.068)	(0.052)	(0.067)	(0.065)	(0.095)	(0.066)	
Lomwe	0.075	0.125	0.096	0.093	0.107	0.240	0.636
	(0.037)	(0.045)	(0.041)	(0.040)	(0.060)	(0.087)	
Ngoni	0.132	0.143	0.173	0.222	0.107	0.200	0.738
	(0.047)	(0.047)	(0.053)	(0.057)	(0.060)	(0.082)	
Tumbuka	0.170	0.250	0.115	0.204	0.143	0.280	0.390
	(0.052)	(0.058)	(0.045)	(0.055)	(0.067)	(0.092)	
Other	0.208	0.250	0.192	0.148	0.214	0.160	0.831
	(0.056)	(0.058)	(0.055)	(0.049)	(0.079)	(0.075)	
Work experience							
Work experience on cv	0.434	0.339	0.288	0.537	0.429	0.480	0.270
	(0.069)	(0.064)	(0.063)	(0.068)	(0.095)	(0.102)	
Ever worked with recruiter	0.113	0.107	0.115	0.093	0.143	0.040	0.715
	(0.044)	(0.042)	(0.045)	(0.040)	(0.067)	(0.040)	
Any work in last month	0.623	0.679	0.673	0.593	0.571	0.800	0.385
	(0.067)	(0.063)	(0.066)	(0.067)	(0.095)	(0.082)	
Any work in last 6 months	0.792	0.911	0.904	0.815	0.893	0.960	0.137
	(0.056)	(0.038)	(0.041)	(0.053)	(0.060)	(0.040)	
Frac of 6 mths worked	0.462	0.473	0.404	0.420	0.393	0.507	0.739

	(0.053)	(0.048)	(0.047)	(0.052)	(0.067)	(0.073)	
Any job search last month	0.132	0.232	0.096	0.037	0.071	0.080	0.051
	(0.047)	(0.057)	(0.041)	(0.026)	(0.050)	(0.055)	

Notes:

The table reports group means or proportions (where applicable, e.g. married). Standard errors are reported in parentheses. The main sample of 268 participants is used. Income is measured in USD and includes all self-reported income from the last three months including the following explicit categories: Farming; Ganyu (piece-work); 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.

[1] 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

Treatment conditions:	N (1)	Mean (2)	SD (3)
0% Probability	53	0.811	0.395
1% Probability	56	0.857	0.353
5% Probability	52	0.827	0.382
50% Probability	54	0.852	0.359
75% Probability	28	0.929	0.262
100% Probability	25	0.840	0.374
Full sample:	268	0.847	0.361
p-value of F-test of joint significance:			
0% = 1% = 5% = 50% = 75% = 100%		0.827	

p-values of t-tests of pair-wise differences in finding rate means:

	1%	5%	50%	75%	100%
0%	0.510	0.826	0.564	0.168	0.745
1%		0.666	0.939	0.396	0.844
5%			0.724	0.233	0.882
50%				0.364	0.893
75%					0.376

Notes:

Individuals were assigned to one of the six treatment groups. If they received a 0-percent chance of an alternative (i.e. in 0% probability treatment group) then they had no chance of receiving the alternative job. Similarly for the 1-, 5-, 50-, 75- and 100 percent probability groups. There were twice as many assigned to the lower probability groups as compared to the lower groups due to budgetary considerations.

Table 3: Sample and Attrition

	Baseline N=268		Follow-Up N=228		Difference	
	Mean	SD	Mean	SD	(3) - (1)	
	(1)	(2)	(3)	(4)	(5)	
Demographics:						
Age	25.604	4.638	25.718	4.662	-0.114	
Married	0.172	0.378	0.172	0.378	0.000	
Any child?	0.164	0.371	0.167	0.374	-0.003	
Number of children	0.299	0.784	0.313	0.811	-0.014	
Years of education	13.183	0.940	13.220	0.938	-0.037	
Income (USD, 3 months)	206.123	228.803	210.617	237.777	-4.494	
Ability score	-0.001	1.003	0.030	1.017	-0.031	
Tribe:						
Chewa	0.310	0.463	0.300	0.459	0.010	
Lomwe	0.108	0.311	0.110	0.314	-0.002	
Ngoni	0.164	0.371	0.181	0.386	-0.016	**
Tumbuka	0.190	0.393	0.189	0.393	0.001	
Other	0.201	0.402	0.198	0.400	0.003	
Education and Work:						
Work experience on cv	0.649	0.478	0.648	0.479	-0.009	
Ever worked with recruiter?	0.104	0.306	0.097	0.296	0.008	
Any work in last month	0.646	0.479	0.665	0.473	-0.020	
Any work in last 6 months	0.869	0.338	0.890	0.314	-0.020	*
Frac of 6 mths worked	2.657	2.176	2.727	2.175	-0.070	
Any job search last month	0.116	0.320	0.110	0.314	0.006	

Notes:

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.

Table 4: First Stage results

Dependent Variable:	Job offer or recruiter's job offer		
	(1)	(2)	(3)
1% Job Guarantee	0.025	0.030	-0.005
	[0.081]	[0.078]	[0.082]
5% Job Guarantee	0.047	0.045	0.038
	[0.085]	[0.079]	[0.086]
50% Job Guarantee	0.402***	0.423***	0.443***
	[0.094]	[0.090]	[0.093]
75% Job Guarantee	0.568***	0.543***	0.568***
	[0.105]	[0.104]	[0.107]
100% Job Guarantee	0.837***	0.860***	0.864***
	[0.057]	[0.055]	[0.067]
Constant	0.163***	0.804***	0.648
	[0.057]	[0.153]	[0.481]
Observations	227	227	227
R-squared	0.327	0.382	0.431
Stratification cell FE's	No	Yes	Yes
F-stat (of instruments)	101.11	87.47	76.36
Average of dep variable		0.361	

Notes:

The zero percent chance of alternative employment treatment group is the omitted category in these regressions.

The dependent variable "Got a job" is whether or not 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 that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.

*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 5: Returns to Work Experience: Employment and Wage Results

Dependent Variable:	Proportion of 8 month post-intervention period employed			8 month post-intervention period average … … number of days worked per week			… daily wage			… IHS daily wage[1]		
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Probability of outside job offer	0.050	0.050	0.059	0.478	0.533	0.664*	2.918*	3.182*	3.192**	0.301	0.340	0.334
	[0.072]	[0.072]	[0.071]	[0.389]	[0.384]	[0.351]	[1.678]	[1.727]	[1.570]	[0.275]	[0.275]	[0.245]
Panel B: IV estimates[2]												
Got a job or recruiters job offer (IV)	0.068	0.068	0.088	0.622	0.745	0.858**	3.801*	4.191*	3.829**	0.398	0.468	0.415
	[0.090]	[0.090]	[0.090]	[0.486]	[0.479]	[0.419]	[2.149]	[2.218]	[1.904]	[0.346]	[0.347]	[0.291]
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
Ave of dep variable (no job)		0.415			2.272			5.036			1.535	

<u>*Notes:*</u>

[1] The inverse sine hyperbolic log transformation has been used.

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

The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.

*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 6: Are returns driven by recruiter employment?

	Proportion of 8 month post-intervention period…		8 month post-intervention period average …			
	… employed		… daily wage		… IHS daily wage[1]	
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)
Probability of outside job offer	0.013	0.107	2.914*	4.596***	0.239	0.580**
	[0.074]	[0.070]	[1.598]	[1.689]	[0.258]	[0.270]
Panel B: IV estimates[2]						
Got a job or recruiters job offer (IV)	0.028	0.125*	3.533*	4.988***	0.308	0.640**
	[0.089]	[0.074]	[1.932]	[1.837]	[0.305]	[0.284]
Stratification cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates?	Yes	Yes	Yes	Yes	Yes	Yes
Wage/Sample adjustment [3]	1	2	1	2	1	2
Observations	227	189	227	189	227	189
Ave of dep variable (no job)	0.329	0.409	5.528	5.218	1.173	1.152

Notes:

[1] The inverse sine hyperbolic log transformation has been used.

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

[3] 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.

The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.

*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 7: Are returns driven by employment within the sector?

	Proportion of eight month period employed as an RA	Number of months employed as an RA	Proportion of 8 month post-intervention period… … employed: RA post	Proportion of 8 month post-intervention period… … employed: non-RA post	8 month post-intervention period average … … daily wage: RA post	… daily wage: non-RA post	… IHS daily wage[1]: RA post	… IHS daily wage[1]: non-RA post	Unit of pay (1 = daily, 2 = weekly, 3 = fortnightly, 4 = monthly)
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Probability of outside job offer	0.040	0.420	0.042	0.025	0.462	2.730*	0.224	0.249	-0.318
	[0.065]	[0.317]	[0.039]	[0.068]	[0.781]	[1.563]	[0.191]	[0.268]	[0.327]
Panel B: IV estimates[2]									
Got a job or recruiters job offer (IV)	0.042	0.487	0.049	0.044	0.541	3.288*	0.249	0.317	-0.386
	[0.078]	[0.381]	[0.047]	[0.082]	[0.921]	[1.908]	[0.226]	[0.321]	[0.402]
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
Ave of dep variable (no job)	0.128	0.515	0.062	0.353	1.233	3.803	0.370	1.238	3.199

Notes:

[1] The inverse sine hyperbolic log transformation has been used.

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

[3]Dummy indicators for treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for proportion of eight month post-intervention period worked.

The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.

*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Table 8: Heterogeneity of wage and employment impacts

Panel A: Dependent variable: Proportion of 8 month post-intervention period employed

Baseline Variable:	Ability (1)	Any experience (2)	International employer experience (3)	Research experience (4)
Got a job	0.108	0.343	0.058	0.022
	[0.073]	[0.887]	[0.083]	[0.089]
Got job X `*Baseline Variable*'	-0.171**	-0.420	0.224	0.186
	[0.078]	[1.396]	[0.216]	[0.171]
`Variable'	0.106	0.226	-0.101	-0.031
	[0.099]	[0.537]	[0.117]	[0.093]
Observations	227	226	227	227
p-value on interaction	0.031	0.966	0.191	0.204
Bonferroni adjusted p-value	0.118	1.000	0.572	0.599
Bonferroni (adjusting for correlation) adjusted p-value	0.069	1.000	0.467	0.553

Panel B: Dependent variable: 8 month post-intervention period average IHS daily wage[1]

Baseline Variable:	Ability (1)	Any experience (2)	International employer experience (3)	Research experience (4)
Got a job	0.525*	3.413	0.026	-0.009
	[0.279]	[4.622]	[0.280]	[0.334]
Got job X `*Baseline Variable*'	-0.568**	-4.834	1.731**	1.079*
	[0.269]	[7.251]	[0.763]	[0.618]
`Variable'	0.010	2.023	-0.337	-0.309
	[0.280]	[2.787]	[0.399]	[0.324]
Observations	227	226	227	227
p-value on interaction	0.037	0.561	0.014	0.06
Bonferroni adjusted p-value	0.140	0.963	0.055	0.219
Bonferroni (adjusting for correlation) adjusted p-value	0.082	0.939	0.041	0.196

Notes:

[1] The inverse sine hyperbolic log transformation has been used.

The probability of alternative employment (P_i) and the interaction of the baseline characteristic and the probability of alternative employment assigned ($Base_i * P_i$) are used to instrument for the binary indicator JO_i and the interaction of the baseline characteristic and the job offer ($Base_i * JO_i$).

Stratification cell fixed effects are included.

The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.

*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 10 percent level. Robust standard errors are reported.

Table 9: Mechanisms

Dependent Variable:	Any job referral	# job referrals	Secured a job through referral	Social network index	Submitted any reference letter	# times used any reference letter	Self-reported month reservation wage	Minimum accepted wage	Proportion of post-period engaged in job search
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Probability of outside job offer	0.167*	0.045	0.067	0.236*	-0.067	-0.329	99.606	3.040	0.073
	[0.095]	[0.227]	[0.047]	[0.136]	[0.092]	[0.458]	[67.420]	[3.369]	[0.062]
Panel B: IV estimates[1]									
Got a job or recruiters job offer (IV)	0.222**	0.100	0.074	0.306*	-0.079	-0.381	124.397	3.764	0.089
	[0.112]	[0.266]	[0.056]	[0.159]	[0.110]	[0.546]	[88.030]	[4.156]	[0.074]
Stratification cell FE's	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

Notes:

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

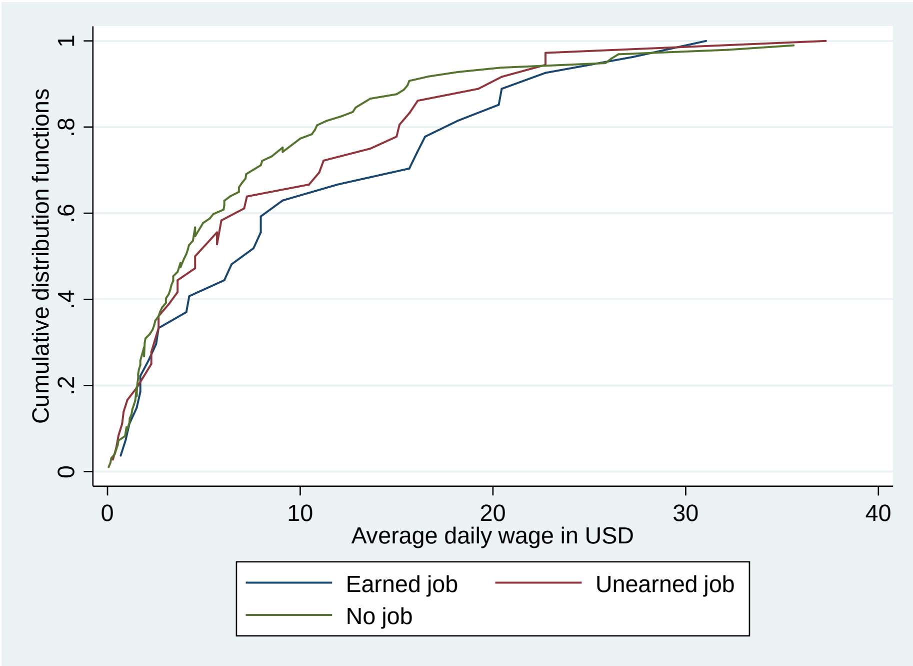
Stratification cell fixed effects are included.

The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.

*** denotes statistical significance at the 1 percent level, ** 5 percent level, and * 1 percent level. Robust standard errors are reported.

Appendix Figures

Appendix Figure 1: Distribution of wages (excluding recruiter wages)



Appendix Table 1: Sample and Attrition

	Baseline N=268 Mean (1)	SD (2)	Covariate (3)	Covariate * Probability of Job offer (4)
Demographics:				
Age	25.604	4.638	0.004	0.001
Married	0.172	0.378	-0.031	0.136
Any child?	0.164	0.371	0.000	0.087
Number of children	0.299	0.784	0.028	-0.029
Years of education	13.183	0.940	0.064**	-0.104
Income (USD, 3 months)	206.123	228.803	0.00004	0.00001
Ability score	-0.001	1.003	0.035	-0.035
Tribe:				
Chewa	0.310	0.463	-0.064	0.093
Lomwe	0.108	0.311	0.125*	-0.304
Ngoni	0.164	0.371	0.057	0.138
Tumbuka	0.190	0.393	-0.041	0.112
Other	0.201	0.402	0.029	-0.188
Education and Work:				
Ever worked?	0.869	0.338	-0.014	-0.152
Ever worked with recruiter?	0.104	0.306	-0.093	0.107
Any work in last month	0.646	0.479	0.039	0.131
Any work in last 6 months	0.869	0.338	0.109	0.167
Frac of 6 mths worked	2.657	2.176	0.008	0.015
Any job search last month	0.116	0.320	-0.085	0.270**

Notes:

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.

Appendix Table 2: Returns to Work Experience: Employment and Wage Results

Dependent Variable:	8 month post-intervention period average …											
	Proprotion of 8 month post-intervention period employed			… number of days worked per week			… daily wage			… IHS daily wage[1]		
	Weights	Min	Max	Weights	Min	Max	Weights	Min	Max	Weights	Min	Max
Panel A: ITT Estimates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Probability of outside job offer	0.091	-0.030	0.165***	0.607*	-0.057	1.169***	2.623*	0.652	5.520***	0.277	-0.134	0.629**
	[0.063]	[0.067]	[0.061]	[0.359]	[0.387]	[0.380]	[1.530]	[1.996]	[1.796]	[0.246]	[0.266]	[0.244]
Panel B: IV estimates[1]												
Got a job or recruiters job offer (IV)	0.112	0.028	0.101	0.812*	0.709*	1.198**	3.206*	4.203**	6.572***	0.353	0.298	0.605**
	[0.076]	[0.081]	[0.072]	[0.431]	[0.429]	[0.472]	[1.861]	[2.022]	[2.305]	[0.293]	[0.297]	[0.305]
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
Ave of dep variable (no job)		0.415			2.272			5.036			1.535	

<u>*Notes:*</u>

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

Stratification cell fixed effects are included.

The set of covariates includes: age, marital status, education dummies, a dummy indicator for whether the respondent has any children, the number of children that the respondent has, ability score (a composite measure of numeracy and literacy scores), dummy indicators for tribe, a dummy indicator if the respondent has any work experience, reports any work in the past month and any job search in the past month, and the number of months in the last six months he has worked.

Endnotes

[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; and Buchinsky et al. 2010). There is also a 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 and 1997; and Light 1999 and 2001).

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

[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 to 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. In other words, there is no reason for the recruiter to disproportionately hire those assigned higher outside options to minimize the wage bill.

[5] For example, an individual assigned a 75-percent 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-percent chance knew with certainty that

they were not eligible for alternative jobs, while those assigned a 100-percent chance knew that they were guaranteed alternative jobs, so no draws were conducted in those cases.

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

[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 (Appendix Table 1). 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.

[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 percent of respondents reporting monthly wages and 18.5 percent reporting daily wages.

[9] We restrict the analysis by excluding those assigned the 100-percent treatment group and those assigned the 0-percent treatment group. These sub-groups show that the results are slightly smaller and in some cases lose statistical significance, which is not surprising, as the sample sizes are small. 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).

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

[11] Appendix Figures 1 and 2 provide similar figures using the average wage outcome that excludes any wages while employed with the recruiter; these show similar results.

[12] We also attempted to examine occupational shifts using retrospective calendar job histories and categorizing jobs using the standard two-digit ILO occupation classification codes (ISCO-08 classification system). Limited statistical power inhibits the ability to make strong claims for the observed occupational shifts. However, the pattern of results suggests that work experience increases employment in both administrative and managerial roles, as well as in clerical and related work, while 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.

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

[15] A composite measure of ability (numeracy and literacy test scores) is used. The results are similar when using the numeracy and literacy scores separately.

[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.

[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 percent level when accounting for the Bonferroni correction, but the international experience interaction remains significant.

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

[20] The specific question used is: "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 last a minimum of 5 days.)"

[21] See, for example, Granovetter (1973), Burns, Godlonton and Keswell (2010), Beaman and Magruder (2012).

[22] Unfortunately, individuals were not asked about whether they turned down any jobs during the post-intervention period.