Employment Exposure: Employment and Wage Effects

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Abstract

This paper exploits an experiment that randomized probabilistic job offers and estimates the employment and wage effects of the short term jobs. I find the following key results. First, there is a 10.6 to 13.9 percentage point increase in average employment during the eight months following the job. Second, there is a sizeable increase in wages. Individuals earn approximately 60 to 67 percent more per day. There is suggestive evidence that individuals are switching into different occupations particularly clerical and related work away from agricultural based activities. Lastly, the estimated returns to the job are larger among those who perform worst on a high stakes numeracy and literacy test suggesting education is to some degree substitutable with the type of work experience offered here.

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1. Introduction

Understanding the key determinants of wage growth can inform policy interventions that reduce poverty. While the determinants of wages have been extensively studied in the United States and other developed country settings, much less evidence exists for developing countries. One exception to this is the extensive literature examining the returns to schooling. This literature shows that the returns to schooling are larger for females, and in countries with lower GDP per capita (Psacharopoulus, 1973, and 1994). An explanation for the higher returns is the scarcity of skilled labor (Mwabu and Schultz, 2000). Heterogeneous initial conditions in terms of the stock of skilled labor and other factors that affect the productivity of labor may imply that the determinants of wages differ across countries. Identifying the importance of factors such as experience, tenure and job mobility (or stability) is therefore important to understanding wage growth in different contexts. In this paper, I study the effect of experience on employment and wages in urban Malawi.

A key challenge in estimating the impact of work experience is that experience is endogenous and likely to be correlated with other factors that affect employment or wages. For example, individuals who acquire work experience may exhibit better non-cognitive skills not observable in the data. Several papers have shown that non-cognitive influence labor market outcomes (Bowles, Gintis, and Osborne 2001; Jacob 2002; Heckman, Stixrud and Urzua, 2006). Because so many characteristics are likely correlated both with past acquired work experience and future labor market outcomes, the assumptions for selection on observables are unlikely to be satisfied even when high quality survey or administrative data are available. Estimating the returns to work experience in developing countries is further constrained by the dearth of detailed labor force and panel data, particularly in Africa. Even though the prevalence of labor force panel studies is increasing they often lack detailed retrospective employment histories or sufficient detail on jobs to accurately measure acquired work experience. To circumvent this data limitation, most existing studies use a measure of "potential experience" that is the difference between an individual's age and his years of schooling in estimating the employment and wage effects of experience. However, the prevalence of interrupted or delayed schooling and periods of unemployment renders potential experience a poor proxy for actual experience in developing countries.

In this paper I overcome the identification challenge by exploiting an unusual source of random variation in short term employment. I also collect data that contain more detailed information about employment history than typically available, and measure actual rather than potential work experience. The exogenous variation I exploit derives from an experimental study conducted in Malawi and discussed in detail in Godlonton (2013). Specifically, job-trainees were randomly allocated a probabilistic chance of short term employment in a real job. There were six treatment groups. Individuals were assigned to receive a 0-, 1-, 5-, 50-, 75- or 100-percent chance of employment in research assistance activities at the completion of the training and recruitment process (even if they were not hired by the recruiter). These probabilistic chances of jobs can be used as an instrument for short term work experience. I have rich baseline data, including a baseline survey and resume for each of the 268 job trainees. Outcome data come from a follow-up survey that collects data on retrospective work histories for the eight month period following the experiment.

By instrumenting for an individual's work experience using his randomly-assigned chance of gaining experience from the short term job, I am able to estimate the effect of short term work experience on employment and job search strategies. First, the estimated impact on employment after eight months is positive, though imprecisely estimated. Individuals offered an alternative job were between 10.6 and 13.9 percentage points more likely to be employed on average during the post-intervention. The estimated impact of experience on the probability of job search and the likelihood of holding multiple concurrent jobs across the eight month period following the intervention is positive but not statistically significant.

Second, I do find a sizeable wage return to work experience. Individuals who were assigned to receive work experience earn on average approximately $3.80 to $4.19 more per day, as estimated in specifications that do not condition upon employment. This is a large return representing a 75 to 83 percent increase in daily wages. In specifications that exclude the unemployed and use logged wages, the estimated effect is only somewhat smaller, with experience increasing wages by between 60 and 67 percent. Some of the increase in the wage may be attributed to an increase in the number of hours worked as this increases by approximately four hours per week (although the effect is not statistically different from zero). Another mechanism for the increase in wages is changes in occupation. I find that the short term research assistance experience prompts a shift away from agriculture and related occupations and towards clerical and related occupations. I examine a number of potential mechanisms through which experience causes wage increases. The data do not support the hypotheses that expanded social networks, signaling of ability from letters of reference, or increased reservation wages are behind the increase in wages. Indirect evidence is most consistent with the idea that experience facilitates skill acquisition, and skill is rewarded in the external labor market.

Furthermore, there is interesting heterogeneity in the employment and wage effects. Specifically, individuals of lower ability (as assessed by a numeracy and literacy test) benefit the most from the work experience. For this subgroup, the effect of experience on the probability of employment is statistically significant. Although the small sample size limits statistical precision, there is suggestive evidence that the employment effects are more are in fact growing over time for low ability types.

Overall, the results in this paper suggest substantial wage returns to even very limited work experience. 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 (Heckman, Lalonde, Smith, 1999; Kluve, 2006). However, in this paper I study a very different context where the returns to experience may be significantly larger due to the scarcity of skills. Also, unlike most job training programs in developed countries, experience in this context is actually targeted to relatively skilled individuals, and individuals possibly gain general skills.

The paper is organized as follows. Section 2.2 provides background information both on related literature, relevant aspect of Malawian urban labor markets where this study is conducted and a description of the intervention. Section 2.3 describes the data used and Section 2.4 presents the empirical strategy. Section 2.5 presents and discusses the results. Section 2.6 concludes.

2 Context and data

2.1 Malawi: Education, experience and earnings in wage employment

Like much of Sub-Saharan Africa, the majority of Malawians depend primarily on subsistence agriculture. Internal migration to urban centers is high and rising (HDR, 2009), however. The trend towards urbanization means that understanding wage growth is particularly important in order to inform labor policies targeted to the growing urban labor force.

Previous studies of the return to education in Malawi estimate wage increases of between six and ten percent per additional year of schooling (Chirwa and Zgovu, 2001; and Chirwa and

Matita, 2009). These estimates of returns to each additional year of schooling are consistent with relatively high point estimates of the effects of completing primary, secondary and tertiary (Psacaharopoulos and Patrinos 2002; Castel, Phiri and Stampini, 2001). One study has estimated the Mincerian return to experience for Malawi, finding that every additional year of potential experience is associated with a wage increase of approximately five percent (Chirwa and Matita, 2009). A five percent return to each year of experience is high relative to the marginal value of education in other countries; King, Montenengro and Orazem (2012) review Mincerian estimates of the return to experience from 122 datasets across 86 developing countries and find estimates between -1 and 4.25 percent per additional year of experience.

However, using potential experience as a measure of accumulated experience has been widely criticized, particularly in labor markets where there is high job turnover and general employment instability. Light and Ureta (1995) use work history data from the United States to show that specifications using cumulative experience and potential experience produce misleading estimates of the returns to tenure and experience in the United States. Using potential experience to measure work experience is particularly flawed in low-income countries due to high rates of grade repetition in school; exit and re-enrollment in schooling; and long spells of unemployment (Lockheed, Verspoor, et al. 1991; Lam, Ardington and Leibbrandt 2011; and Pugatch, 2013).

In this paper, I exploit the experimental variation from a randomized controlled trial conducted in urban Malawi discussed in greater detail in Godlonton (2013). The exogenous variation in work experience generated by that experiment provides the opportunity to examine the causal impact of a short term work opportunity on later labor outcomes.

2.2 Experimental variation

This paper makes use of the exogenous variation in work experience generated by a randomized controlled trial that offered individuals undergoing a real recruitment process 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 competitive hiring process. Thus, the probabilistic job guarantee provides a lower bound on the probability that an individual had the opportunity for employment at the conclusion of the recruiting process. The randomization was stratified by ability and prior experience with the recruiter. The alternative employment opportunity offered the same duration and wage as the standard employment offer from the recruiter. Individuals were still able to earn a job through the recruitment process by performing well during the job training, and those who secured both jobs were required to take the recruiter's job or turn down both job offers. Given that the recruiter's job and the alternative jobs were of equal duration and paid the same wage, those who became employed through the project acquired the same amount of work experience at the same pay whether they ultimately worked for the recruiter or in the alternative job. Estimation of the effect of the probabilistic job guarantee must account for the fact that the probabilistic jobs increased the likelihood of both being selected for the recruiter's job and being eligible for the alternative job (see Godlonton, 2013 for details).

The work experience acquired is short term. The job provided individuals with five days of paid work experience. The recruiter's job was for employment as an interviewer. The alternative jobs were 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 a generic letter of reference.

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. For example, an individual assigned a 75-percent chance of 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 groups. Individuals assigned a 0-percent chance knew with certainty they were not eligible for alternative jobs and those assigned a 100-percent chance knew they were guaranteed alternative jobs, so no draws were conducted in those cases.

I use the treatment assignment (i.e. the probability of an alternative job) to instrument for acquired short term work experience. This unusual random determination of employment allows a unique opportunity to measure the causal effect of short term work experience on future labor market outcomes.

3 Data

Figure 2.1 outlines the timeline of the data used in this paper. The sample of respondents is drawn from a recruitment process hiring male interviewers, during which trainees also participated in an experiment that offered randomly determined probabilistic jobs. 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 hiring by the recruiter, and a follow-up survey that was conducted nine months after the completion of the work opportunities presented by the experiment.

Baseline data: Prior to the start of the recruitment process, respondents completed numeracy and literacy tests and submitted their resumes. Using the numeracy and literacy scores I construct an ability measure. In addition to this information a baseline survey was conducted.

The baseline survey collected information on basic demographics, general education and work experiences, as well as mental and physical health. The baseline survey was self-administered by respondents.

Probabilistic alternative job offers: I use both the assignment to treatment records, as well as the realization of the probabilistic draws (i.e. whether or not each participant was actually offered a job, conditional on the distribution he was randomly assigned to). Assignment to an employment probability was stratified by baseline ability quintile and prior experience with the recruiter. In Godlonton (2013) it is shown that the treatment assignment is balanced; in other words, there are no systematic differences in covariates between the different treatment groups.

Follow-up survey data: A follow-up survey was conducted nine months after the implementation of the experiment. While the reference period for the survey questions is the nine months following the completion of the work experience opportunities, some participants erroneously report work tied to the experiment. To deal with this survey recall error, I exclude the first month of recall data and rely only on the eight month period beginning one month after the completion of the work experience opportunities. The follow-up 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, current and past wages as well as a mental health module.

Table 2.1 shows that attrition was not statistically significantly associated with the treatment status. A total of 84.7 percent of the sample was successfully interviewed at follow-up. The attrition rate was lowest among participants who had received the 75-percent job guarantee (7.1 percent). Individuals assigned a 0-percent chance of an alternative job have the highest rate of attrition (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).

Table 2.2 shows that there is not differential attrition for other baseline characteristics including age, education, ability and previous work experience (Column 5). Respondents of the Ngoni tribe and those that 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. Moreover, there is no systematic differential attrition by treatment status (i.e. the probability of the alternative job) that is correlated with baseline characteristics. To test this, I 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 C.1).

The final analytical sample includes the 227 respondents found at follow-up. The average respondent in this sample is approximately 26 years old and 17.2 percent are married. Approximately 16.7 percent of the sample have at least one child, and of those that do have at least one child they have an average of 1.8 children. Respondents are relatively well educated for Malawi with an average of 13 years of education, but this is driven by the eligibility criteria of the recruiter which required individuals to have at minimum completed their secondary school education. Despite being relatively well-educated for Malawi all these men were actively seeking work at the time of the baseline sample and they reported earnings of only approximately $210 per month over three months prior to the experiment (Table 2.2, Column 2).

4 Empirical strategy

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, T_i is a dummy indicator for whether or not the individual received a job, and X_i is a set of individual characteristics. However, in this 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. I therefore implement an instrumental variables approach. The system of equations then estimated is:

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

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

where JO_i measures whether individual i was offered a short term job; $P1_i$, $P5_i$, $P50_i$, $P75_i$, $P100_i$ indicates the binary indicators for the treatment arms; and X_i represents a set of covariates. The set of covariates used is the same as those used in equation (2) and listed above. I also include stratification cell fixed effects to account for the fact that treatment assignment was stratified by ability and prior work experience with the recruiter. The key coefficient of interest is β_1. Y_i measures labor market outcomes of interest to examine both the intensive and extensive margins. To examine changes at the extensive margin I measure the impact the probability of being employed nine months after the experiment, and the fraction of months in which individuals are employed in the eight months following the intervention. To measure impacts at the intensive margin, I examine the average daily wage earned by individual i across that the eight month period. I allow for possible heteroskedasticity in the error terms by using heteroskedastic-robust standard errors.

For the probability of assignment to the alternative job to serve as a valid instrument for work experience, it needs to satisfy two conditions: i) the instrument must be correlated with the endogenous variable; ii) the probabilistic job offers must not affect later labor market outcomes except through the acquired work experience.

The first condition implies that assigned probability of alternative employment should predict whether or not the job-seeker acquired any job (recruiter or alternative job) through this intervention. Estimating the first stage relationship shows that the instrument is, indeed, relevant:

$$AnyJO_i = \pi_0 + \pi_1 P1_i + \pi_1 P5_i + \pi_1 P50_i + \pi_1 P75_i + \pi_1 P100_i + X_i'\varphi + \varepsilon_i \qquad (2)$$

In the equation above, $AnyJO_i$ is defined as a binary indicator equal to one if the respondent either received a randomly determined job or a recruiter's job. I use indicator variables for each of the treatment arms. $P1_i$ equals one if the individual received a one-percent probabilistic chance of a job, and $P5_i, P50_i, P75_i,$ and $P100_i$ are similar indicator variables for the 5-, 50-, 75- and 100-percent treatment arms. The omitted category is the group who received no chance of an outside job. X_i represents a set of covariates and 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.

Table 2.3 presents the first stage estimates. The first stage results show that the probabilistic jobs strongly predict the probability participants received any job (recruiter or alternative). This expected result derives mechanically from the assignment of alternative jobs, as well as through a behavioral response by participants to the job guarantees. As shown in Godlonton (2013) the probability of being hired by the recruiter was higher among those who received the 75- or 100- percent chance of an alternative job, likely because the improved outside option lowered stress and increased performance during the recruiting process. Both mechanisms work in favor of a higher probabilistic job guarantee causing a higher chance of subsequent employment. Table 2.3 Column 1 confirms this hypothesis. 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. The coefficients are positive as predicted, though the standard errors are large. Individuals assigned a 50-, 75- and 100- percent chance 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 of the alternative job. The first stage F-statistic is 101.11, 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 (recruiter or alternative). Monotonicity would have been violated if higher probabilistic job offers had reduced the likelihood of acquiring the recruiter's job. However, as shown in Godlonton (2013) this is not the case. In fact, individuals assigned a 75- or 100 –percent chance of an alternative job were about twice as likely to be hired by the recruiter as those who were not eligible at all for alternative jobs. A second concern is that the probabilistic job offers may have affected individuals' perceptions about their own ability to find employment. Results in Godlonton (2013) show that there is no effect of the probabilistic job offers on perception of ones' own likelihood of employment.

A third concern is that the probabilistic job offers affected skill acquisition during training, and that skill was subsequently rewarded by the labor market. The finding in Godlonton (2013) 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 this training. 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, interviewer positions for a health survey. Participants worked systematically through the questionnaire the recruiter planned to administer, in order to understand the terminology of and instructions for filling in each item. Participants were given systematic explanations about how to interpret questions, but the training was very specific to the survey in question. Skills related to this particular questionnaire are highly firm-specific and are unlikely to be marketable to the labor market. Moreover, for the training to have an impact in the labor market the differential performance of the participants needs to be observable to future employers. Individuals did not receive their grades on these assessment tests and letters of reference only described the nature of the job but not the employee'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. Given the nature of the recruiter's training, this is unlikely.[1] Generally, in this

[1] I 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).

context when individuals apply for a new interviewer position even within the same firm they still are required to undergo the same training for each new survey as the content of each training and skills taught are specific to that survey. In other words, experienced and novice interviewers undergo the same training for each survey they work on.

Conditional on instrument validity, β_1 captures the local average treatment effect (LATE) of the short term job on labor market outcomes – employment and wages.

5 Results

Work experience may affect employment at the extensive margin, by changing the probability of employment, and the intensive margin, changing wages conditional on employment. In this section, I use the variation generated by the experiment to study the return to experience at each of these margins.

5.1 Returns to experience

Table 2.4 presents the impact of the short term work experience on job search, employment, and the concurrent number of jobs held. This table uses data aggregated by individual across the eight month post-intervention time period. The employment variable used is the probability of employment during this timeframe. This is constructed by calculating the fraction of months that the individual is employed over the eight months following the intervention. Similarly, the job search variable is defined as the average probability an individual actively sought work (whether or not they were employed). Again, like the employment variable this is constructed as the fraction of months an individual actively sought work in the post-intervention period. The measure of concurrent number of jobs held is constructed as the average number of concurrent jobs held during the last eight months.

Work experience increases the probability of employment by all three measures. The short term work experience provided by the experiment increased the probability of subsequent by 10.6 to 13.9 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, representing a 25 to 33 percent increase in the probability of being employed. To explore the time dynamics behind the average effect estimated in Table 2.4, Figure 2.2 plots the estimated employment impacts of the job separately for each of the eight months following the intervention. Although the one-month estimates are imprecise, the effects are positive in each of the eight months and statistically different from one another.

Work experience also increases the probability of searching for a job (column 4) and the number of concurrent jobs held (column 7). These estimates are robust to including controls for stratification cell fixed effects and covariates.

Another margin along which employment may adjust is the number of days worked. Underemployment in Malawi is high, and there is plenty of 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, the relevant comparison group for the experimental sample, work only 23.4 hours per week conditional on being employed. The follow up survey uses the standard labor supply survey instrument (2010/2011 IHS), so it measures hours of work rather than days of work in the past week. While I cannot measure the change in days of work, I can examine the change in the number of hours worked, and compute the implied average wage per hour. These results are also presented in Table 2.5. I find that among the employed, individuals are working approximately 40 percent more hours per week. In the local context, however, individuals are more likely to be able to adjust their labor supply at the daily than hourly margin, and they are paid per day rather than per hour. It is probably more accurate to interpret differences in hours as indicative of differences in the responsibilities of the job. Therefore, the results for hourly wage should be interpreted with caution. These estimates and show no statistically significant impact on the hourly wage (Table 2.5 columns 4 through 6). The magnitude of the coefficient indicates an increase of $0.72 per hour which is large in magnitude but it is not statistically significant.

Before turning to the mechanisms behind the increase in employment, Table 2.5 explores the impact of work experience on wages. The outcome measure is the individual's average daily wage over the eight-month follow up period. This measure is not conditional on employment, so periods when the individual is unemployed are included (as zeros) in the average. Daily wages – rather than the hourly wages used in much of the related literature – are the relevant unit in this context. Institutionally, all Malawian labor policies pertain to daily employment; for example, the minimum wage law is with respect to daily wages, not hourly wages. 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. Therefore, while the literature about employment in developed countries uses hourly wages as the primary outcome of interest, daily wages are a more appropriate measure in this context.

Table 2.5 shows that individuals who gained work experience as a result of the experiment earn $3.80 to $4.19 more per day (Columns 7 through 9). This estimated effect is large relative to the average daily wage of approximately $5.08 among the control group. The estimated impact represents a 75 to 83 percent increase in daily wages. As we did with the extensive margin effects, we can also consider the effect on wages separately for each of the eight months in the follow up period. Month-by-month estimates are plotted in Figure 2.3. In all months, the effect on daily wages is positive; it ranges between approximately one and six dollars.

The estimated wage impacts are surprisingly large and deserve further discussion. First, these results are not conditional on being employed; the outcome measure incorporates periods of unemployment as wages of zero. Therefore, part of the increase in wages is attributable to the gains in employment as shown in Table 2.4. Logged wages drops the unemployed, these results are present in Table 2.5 columns 10 through 12. The positive wage results persist, but are as expected the estimated coefficients are smaller in magnitude. However it is still large - the impact on the daily wage is 60 to 67 percent. These large point estimates are not driven by outliers. Figure 2.4 documents the wage distributions for those who did and did not receive a job and shows that the wage distribution among those who received a job is shifted to the right.

5.2 Mechanisms

Understanding the mechanisms may be helpful in reconciling the effects in this experiment with the much smaller effects estimated from non-experimental Mincerian estimates in Malawi and other settings. I find that only five days of work experience results in a 57 to 63 percent increase in subsequent earnings. This is equivalent to approximately ten years of experience in the Malawi non-experimental estimates (Chirwa and Matita, 2009). There are many reasons why the non-experimental estimates may be substantially smaller. First, the non-experimental study also uses an inferior measure of work experience. Potential experience overstates the amount of accumulated experience (considerably) in this context. Second, the type of experience studied by the experiment may be of higher quality than experience otherwise available to even educated Malawian men. While the experience provided through the experiment was short term, it was with a private, international employer. It is unlikely that five daysworth of work in the civil service will yield impacts similar to that observed here. Finally, 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.

There are many theoretical reasons to expect that experience (even short term informal work experience) leads to increased employment and wages. In this section, I discuss a number of these possibilities and discuss which might be most relevant in the current context. The particular mechanisms that I consider include changes in job search strategies or occupational choice; changes in contract type, altered social networks; skills acquisition; altered wage expectations; and human capital accumulation. The experimental setting was not designed to test these mechanisms directly. However, I present suggestive evidence against the backdrop of these outlined mechanisms, before turning an exploration of heterogeneity in the return to experience.

Shifts in occupation

One possibility is that individuals change their occupation if they are induced to receive a job. Using the retrospective calendar job histories, I classify each job according to the standard two-digit ILO occupation classification codes (using the ISCO-08 classification system). I then analyze employment in each industry separately, using three measures of occupation-specific employment. The first is a binary indicator for whether each individual ever worked in a given occupation. The second is the total number of months the respondent worked in each occupation. The third indicator is a binary for the respondent's modal occupation over the eight month follow up period.

In Table 2.6, each row reports the effect of work experience on employment in a separate occupation from an IV regression. The left hand panel corresponds to the binary ever-worked outcome; the middle panel is the number of months in the occupation; and the right hand panel is an indicator for modal occupation, as described above. Increased work experience as a result of the experimental variation caused increases in employment in the following occupations: administrative and managerial; and clerical and related worked. The same pattern is observed for the modal occupation held. Individuals were also more likely to have recent experience as professional, technical or related occupations but this pattern does not hold for the modal occupation. For clerical and related occupations the effect is large large, with the 13.1 percentage point increase in the probability of working in clerical or related occupations representing a 62 percent increase in the probability of employment in that field. Individuals appear to be switching from agriculture related, service and production and related occupations, but stronger claims are limited by the lack of statistical precision.

Employment contract type

Another mechanism through which experience may have affected wages is by altering the type of wage contract individuals secured after the intervention. Jobs vary in their duration, and short term positions are common in Malawi. I do not directly observe the duration of contracts in the follow up survey, but I can use information from the unit in which individuals reported their current job as a proxy for contract duration. Individuals self-reported the unit of payment for their current (primary) job at the daily, weekly, fortnightly or monthly level. I 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 is reported. Table 2.7 reports effects of work experience on this proxy for job permanence. The estimated impact of work experience on payment frequency is -0.7. Individuals induced to receive work experience through the experiment appear to be working in less permanent positions. In this context, the change is consistent with higher wages, because wages for short term positions as research assistants or consultants on projects for international NGOs or donor agencies are often much higher than wages paid for the longer-term work offered by local employers or government agencies.

Social networks

Social networks have been touted as an important mechanism through which individuals acquire employment opportunities.[2] There are several theoretical reasons for why social connections are important in accessing employment. For the job-seeker, social connections can reduce search costs and lead to better quality matches (Calvo-Armengol, 2004; Mortensen and Vishwanath, 1994; Galeotti and Merlino, 2009).

Simply participating in jobs provided by this experiment may have facilitated new social connections between participants. These social connections may increase employment opportunities independently of the experience accrued. Unlike the experiments undertaken by Beaman and Magruder (2012) and Beaman et al. (2013) that are specifically 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 or to test specific manner in which social connections might matter. However, I do measure the prevalence of social interactions that may have facilitated employment, such as whether individuals heard about 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.

[2] See for example Beaman (2010) and Granovetter (1973).

Table 2.8 panel A shows that individuals who received work experience as a result of the experiment are 23.4 percentage points more likely to have heard about a work opportunity through someone they met during this intervention. However, while individuals claim to hear more about job opportunities, they are not more likely to secure employment through one of the new connections. Individuals are 12.6 percent less likely to report securing a job through someone they met during this intervention, but the estimate is not statistically significant at conventional levels.

In sum, while the broadened network does suggest a modest impact on information about job opportunities, this information does not translate into employment and therefore does not explain the effect of experience in this experiment.

Signaling

Another mechanism is signaling of worker quality to employers (Spence, 1973). In this case it is possible that employers do not infer any inherent value of the work experience on worker productivity, but merely interpret it as a signal of ability. Upon completion of the work experience all participants received a standard letter of reference, which 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 certification of skills acquired through experience.

Table 2.8 panel B shows that those who received work experience as a result of the experimental treatment were actually 7.4 percentage points *less* likely to use the reference letter than to those who did not receive a job.[3] Therefore, employers would not have received any signal about worker ability from the reference letters, and these letters are unlikely to have

[3] Individuals who received work in the alternative job and those who worked for the recruiter received reference letters as such it is possible that individuals who did not receive the randomly determined job used a reference letter. However, the large difference is not too surprising as a low fraction of those who received no alternative job offer worked for the recruiter, and therefore did not receive any reference letter that could be used for this purpose.

contributed later labor market outcomes. However, it may still be possible that individuals put the work experience on their resume and this acts as a signal of ability.

Wage expectations

The job may have altered individuals' wage expectations and reservation wages, with implications for job search strategies, duration of unemployment, and match quality. The 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.

I examine this mechanism by looking at self-reported reservation wages. Table 2.8 Panel C presents the results from this exercise. The impact of receiving a job on the monthly reservation wage is $121.25, but it 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 also high relative to wages reported in the follow up survey. Transforming reported wages into full-time equivalent salaries with the assumption that individuals worked 20 days per month, then the average monthly wage earned at follow up was approximately $240, higher than at baseline but considerably lower than the reported reservation wage. While measurement error in the reservation wage complicates the interpretation of these results, there is no evidence that an increase in reservation wages is an important mechanism.

Human capital accumulation

A final potential mechanism is that individuals acquired skills attributable to the work experience induced by the experiment. Individuals who secured a job either worked as an interviewer or were assigned to data entry; data transcription or translation; or archival research jobs.

The results discussed in section 2.5.2 and presented in Table 2.6 show a change in occupational type. Individuals who received work experience are less likely to be employed in agriculture and more likely to be employed in clerical activities. Furthermore, individuals are 18.1 percentage points more likely to report having worked as a research assistant, the specific occupation in which they acquired experience. This is suggestive evidence that the work experience provided through the experiment generated occupation-specific skills that were rewarded by future employers.

While the data do not permit a direct test of the mechanism through experience increases which wages and employment, the indirect evidence suggests individuals may have acquired skills that are rewarded by the external labor market.

Heterogeneity

Understanding heterogeneous returns to work experience can help us interpret the large average effects and design policies to use work experience to improve employment outcomes. I explore heterogeneous returns by ability, work experience and education. To do so, I 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. In this specification I instrument the endogenous regressors with the probability of an alternative job and this probability interacted with the baseline characteristic. Therefore, to examine the heterogeneity of the impacts I estimate the following set of equations:

$$Y_{it} = \alpha_0 + \beta_1 JO_i + \beta_2 Base_i + \beta_3 (JO_i * Base_i) + X_i' \delta + \varepsilon_{it} \qquad (5)$$

$$JO_i = \pi_0 + \pi_1 P_i + X_i' \varphi + \varepsilon_i \qquad (6)$$

$$(JO_i * Base_i) = \pi_2 + \pi_3 (P_i * Base_i) + X_i' \gamma + \varepsilon_i \qquad (7)$$

where: $Base_i$ is, in turn, the baseline ability score as determined by numeracy and literacy tests; a binary indicator for having completed college; and measures of current and cumulative labor market work experience.

Table 2.9 Panel A examines the heterogeneity of impacts by ability. To measure ability, I use test scores from a numeracy and literacy test administered to the respondents at baseline. I use a composite measure of ability that combines the numeracy and literacy test scores.[4] The estimated impacts are larger for individuals at the lower end of the ability distribution. To see this, consider an individual at the 25th percentile and the 75th percentile of the ability distribution. 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, and they 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, though they earn approximately \$2.20 more per day.

Figure 2.5 plots the average post-treatment employment rate by month for low and high ability types. Individuals are classified as low ability if they scored below the mean on the composite literacy and numeracy test; and as high ability otherwise. The small sample limits the precision of the estimates by ability level, but the pattern is informative. The estimated impact on employment for low ability types is increasing over time, while there is no consistent pattern for the high ability types. The pattern for wages is relatively constant across the time period (not shown). This pattern of results suggests that the low ability types not only gain the most from the job but also that the employment returns are increasing over time.

Education and experience can serve as substitutes or complements in a Mincerian model. To examine the relationship in this context I consider heterogeneity by whether or not the

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

respondent has a degree (Table 2.9 panel B). Due to sample restrictions imposed by the recruiter, the sample is composed entirely of individuals who have completed secondary schooling. Therefore, there is limited variation in educational attainment. The results show that the estimated impacts are largest for those without a university degree and are actually negative for those who have completed university.

Lastly, one possible reason that the estimated impacts are so large is that the experience provided in the experiment is the first job held by respondents. Table 2.9 Panels C and D explore the heterogeneity of the impacts with respect to work experience. Panel C uses recent job market attachment defined as whether the respondent was working a month prior to baseline; and Panel D uses an indicator for whether the individual has ever worked. Roughly 15 percent of the sample had no previous work experience. Perhaps surprisingly, the effects of work experience on subsequent employment do not differ by pre-experimental work experience.

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 in wages. The return to experience is large, with a 10.6 to 13.9 percentage point increase in post-intervention employment for those who received experience through the experiment relative to those who did not. Not only does experience increase the probability of being employed, but also, it has a sizeable effect on wages. Individuals who received work experience earn approximately 60 to 67 percent more per day than those who did not, with results concentrated among lower-ability individuals. This return to work experience is present in each of the eight months of the follow up period, and the average effects are larger than in previous estimates of the returns to experience in Malawi and other settings. Individuals shifted away from agricultural based occupations and into clerical and related work; they worked more hours per day and on contracts with shorter durations.

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. Proponents of work based programs believe that any job is a good job, and that getting a job will lead to job advancement and wage growth (Holcomb et al., 1998). However, the empirical evidence provides mixed results. In systematic reviews of the literature, the key take away is that the impact of job-training programs are modest at best (Heckman, Lalonde, Smith, 1999; Kluve, 2006). However, just like the returns to education, the impacts of such programs might be larger in low income countries. Betcherman, Olivas and Dar (2004) review the literature about impact evaluations of job training programs and find only 19 studies (none of which are in Africa) conducted in developing countries. In both this review and in another, by Nopo and Saavedra (2003) of the non-experimental literature in Latin America, the estimated impacts of job training programs appear to be larger in developing than developed countries.

The 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 cannot extrapolate from these results to the return to a longer period of experience. Lastly, the general equilibrium effects of such a program are not estimated. Given the small size of this intervention, it is not possible to determine if and the extent such a program if rolled-out would have on those individuals not participating. It is not clear if non-participants would be crowded out of the labor market or whether the returns are driven by increases in wages earned through entrepreneurship activities which would result in a net increase in employment.

While these caveats cannot be dismissed, the results presented here do provide the first experimental evidence about the effect of work experience on subsequent employment outcomes in a developing country. 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

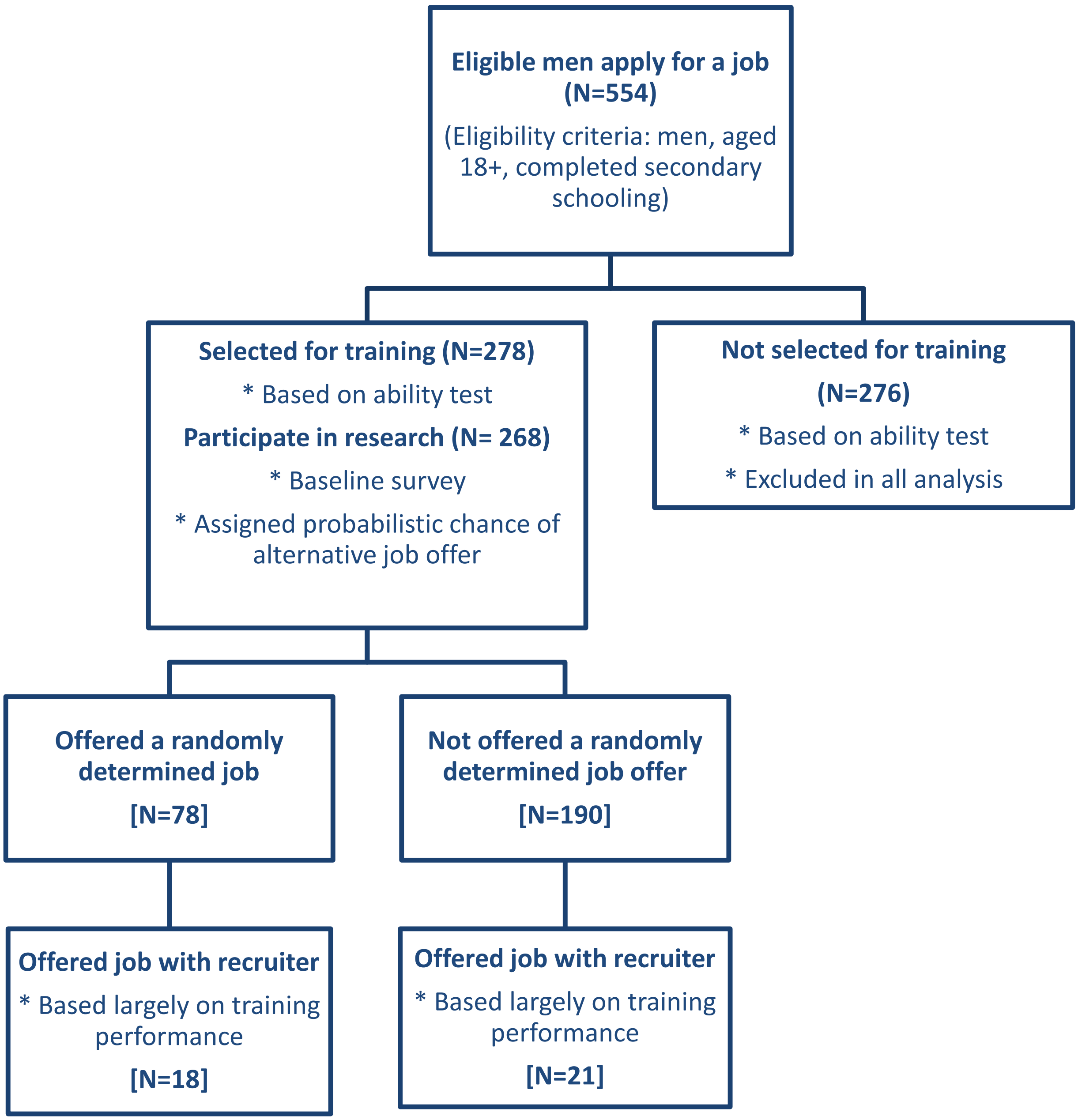

Figure 2: Estimated employment impact of job offer by month (IV estimates)

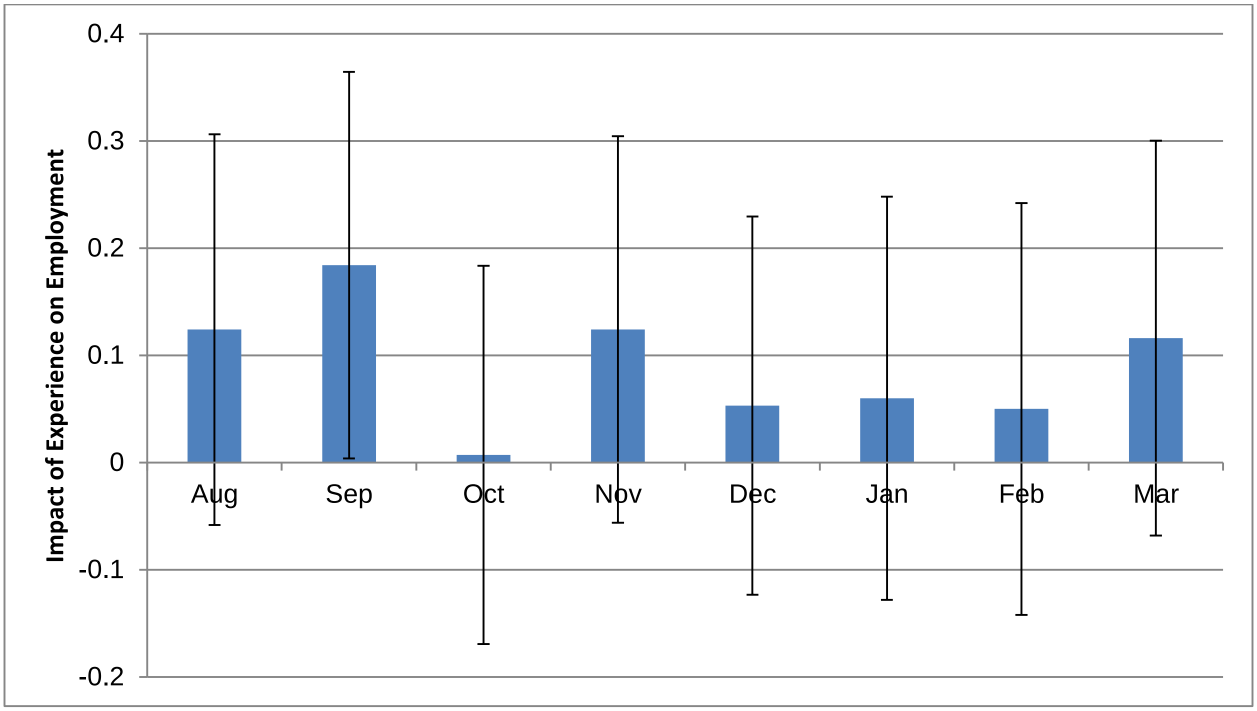

Figure 3: Estimated wage impact of job offer by month (IV estimates)

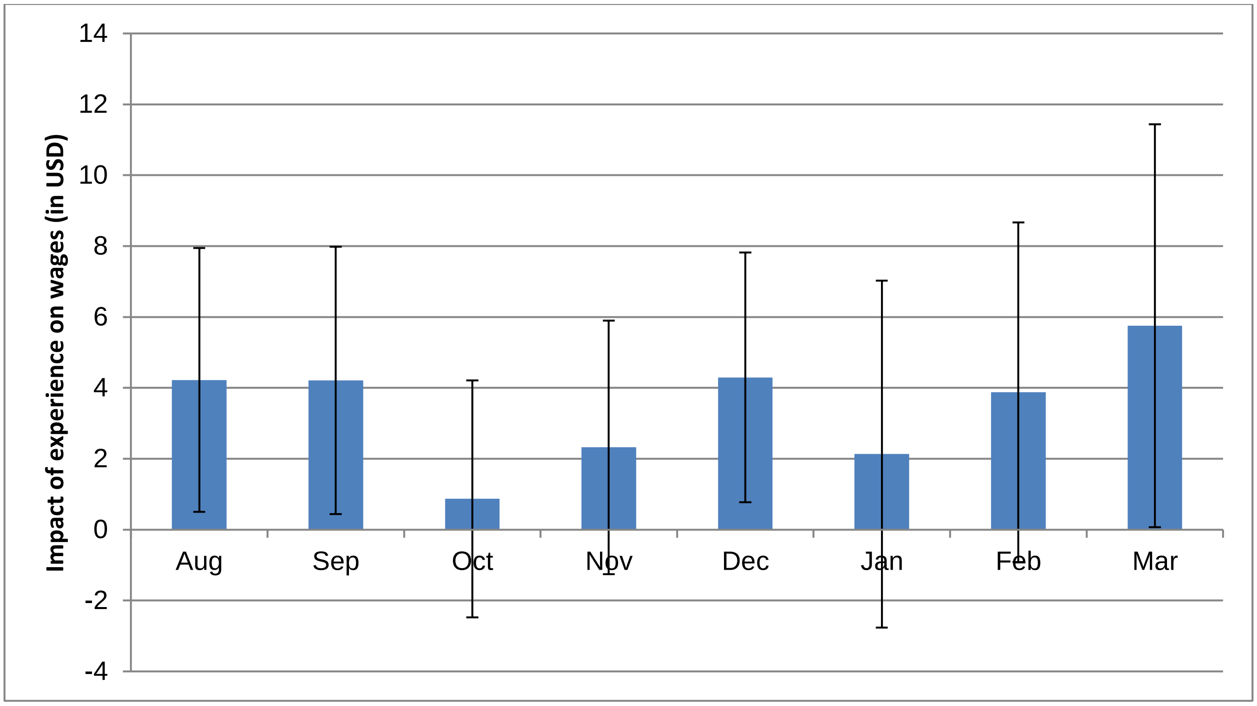

Figure 4: Distribution of wages

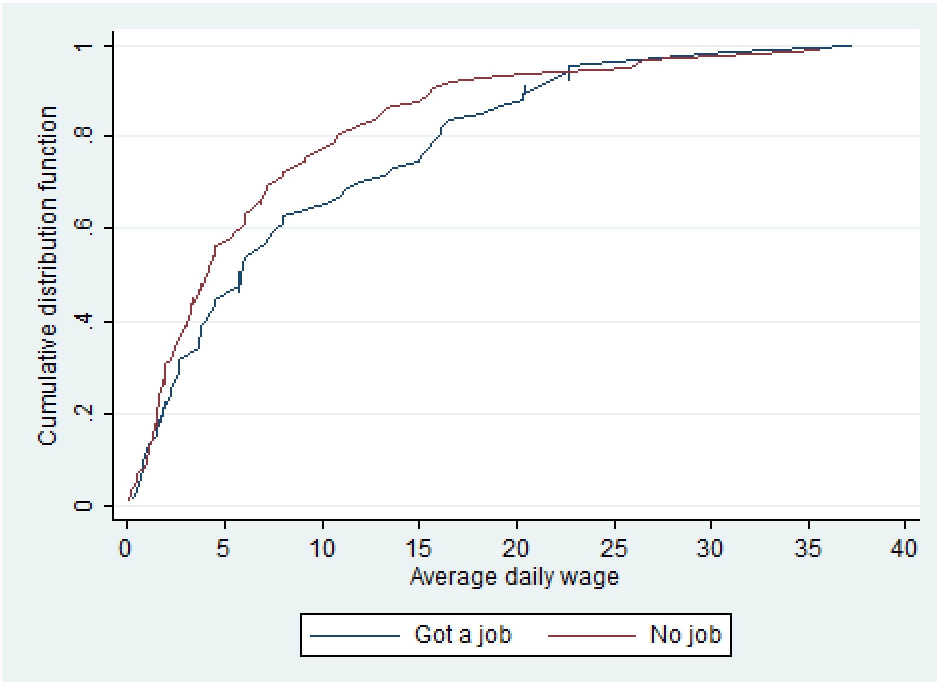

Figure 5: Estimated employment impact by ability of job offer by month (IV estimates)

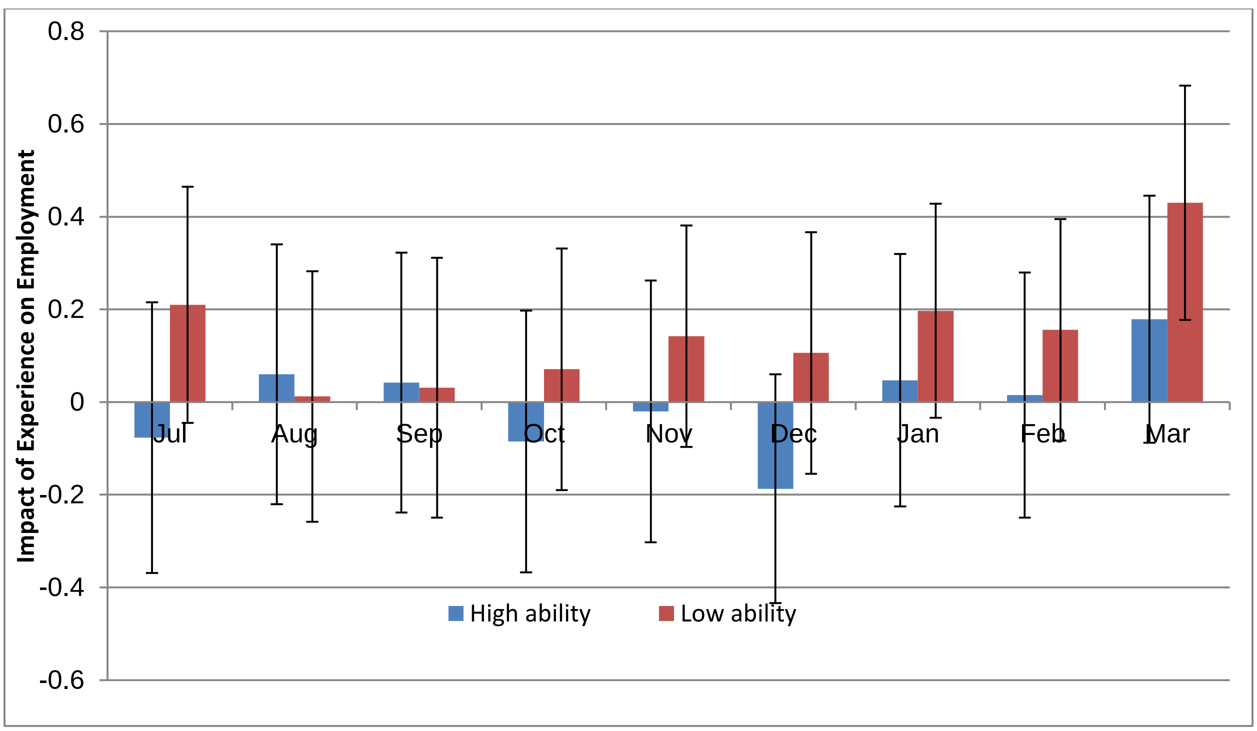

Table 1: Sample size and attrition

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:

	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. If they were assigned to the 1% probability group then they had 1 percent chance of receiving an alternative job. Similarly for the 5-, 50-, 75- and 100 percent probability groups. There were twice as many assigned to the high probability groups as compared to the lower groups due to budgetary considerations. The p-values denote the p-value associated with the F-test of whether the mean finding rate is the same in all treatment groups or in the case of the table the pair-wise t-test of differential attirion rates.

Table 2: Sample and Attrition

	Baseline N=268		Follow-Up N=227		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	
Number of fin dependents	7.959	9.355	8.264	9.406	-0.305	
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:						
Ever worked?	0.869	0.338	0.863	0.344	0.006	
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 and experiment discussed in Section 2. The follow-up sample (227 respondents) is the main sample used in this paper. The ability score is determined prior to the experiment. It consists of a numeracy and literacy component, and has been standardized.

Table 3: First Stage: Using dummy indicators for each treatment group to predict any job offer (recruiter or random job)			
Dependent Variable:	**Job offer or recruiter's job offer**		
	(1)	**(2)**	**(3)**
1% Job Guarantee	0.025	0.030	-0.004
	[0.081]	[0.078]	[0.083]
5% Job Guarantee	0.047	0.045	0.038
	[0.085]	[0.079]	[0.085]
50% Job Guarantee	0.402***	0.423***	0.439***
	[0.094]	[0.090]	[0.093]
75% Job Guarantee	0.568***	0.543***	0.565***
	[0.105]	[0.104]	[0.108]
100% Job Guarantee	0.837***	0.860***	0.866***
	[0.057]	[0.055]	[0.067]
Constant	0.163***	0.804***	0.544
	[0.057]	[0.153]	[0.370]
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	76.79
Average of dep variable		0.361	

Notes:

The sample used here is the sample of 227 men found at follow-up. 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. Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously. 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 4: Returns to Work Experience: Extensive Margin

Dependent Variable:	**Frac. months employed**			**Frac. months looked for work**			**Ave # concurrent jobs**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Got a job or recruiters job	0.106	0.128	0.139*	0.084	0.105	0.113	0.079	0.098	0.071
offer (IV)	[0.086]	[0.086]	[0.076]	[0.091]	[0.090]	[0.079]	[0.082]	[0.081]	[0.072]
Constant	0.376***	0.538***	-0.015	0.395***	0.520***	0.043	0.597***	0.754***	0.024
	[0.041]	[0.128]	[0.349]	[0.043]	[0.140]	[0.355]	[0.038]	[0.114]	[0.275]
Stratification cell FE's	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other covariates?	No	No	Yes	No	No	Yes	No	No	Yes
Observations	227	227	227	227	227	227	227	227	227
R-squared		0.087	0.282		0.085	0.279	0.019	0.047	0.249
Ave of dep variable (no job)		0.421			0.586			0.532	

Notes:

The regressions are IV estimates, where dummy indicators for the 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 fraction months employed variable is calculated as the number of months the individual was employed over the last 8 months, divided by 8. Similarly, the fraction months looked for work variable is computed using a retrospective calendar history, and is calculated as the number of months the individual actively sought work over the last 8 months, divided by 8.Lastly, the average number of concurrent jobs is the average of the total number of jobs held each month across the 8 month period.

Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.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: Intensive Margin

Dependent Variable:	Ave hrs worked per week			Hourly wage			Ave daily wage (incl. Unemployed)			Log (Ave daily wage)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Got a job or	-0.576	-0.567	-0.691*	5.463	5.836	7.854*	3.801*	4.191*	3.928**	0.668*	0.687*	0.605*
recruiters job offer	[0.381]	[0.376]	[0.387]	[4.361]	[4.404]	[4.173]	[2.149]	[2.218]	[1.885]	[0.373]	[0.387]	[0.354]
Constant	3.223***	4.033***	4.720***	21.559***	14.082***	18.413	4.133***	10.784	-1.611	1.206***	0.621	0.216
	[0.185]	[0.433]	[1.139]	[2.112]	[4.321]	[15.205]	[0.864]	[7.161]	[5.779]	[0.173]	[0.559]	[1.135]
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	166	166	166	167	167	167	227	227	227	164	164	164
R-squared	0.029	0.069	0.154		0.035	0.199		0.045	0.251		0.036	0.262
Ave of dep variable (no job)		23.265			0.489			5.079			1.361	

Notes:

The regressions are IV estimates, where dummy indicators for the 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 average daily wage is calculated using the restrospective job work history. The average daily wage is calculated as the average wage on the individual's main job in the last month. Columns 1 through 3, those who are unemployed are coded as 0's. Columns 4 through 6 uses the logged wage, therefore for individuals who earned $0 across all eight months are omitted.

Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.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: Shifts in occupations

Occupation:	Any job held in past 8 months: Avg dep var (no job) (1)	Coeff (2)	SE (3)	Num months in each occupation in past 8 months: Avg dep var (no job) (4)	Coeff (5)	SE (6)	Modal occupation in past 8 months Avg dep var (no job) (7)	Coeff (8)	SE (9)
Professional, technical, and related workers	0.368	0.158	[0.108]	1.515	0.661	[0.599]	0.475	-0.075	[0.142]
Administrative and managerial workers	0.007	0.052	[0.040]	0.007	0.279	[0.251]	0.000	0.083	[0.053]
Clerical and related workers	0.213	0.150	[0.106]	0.691	0.695	[0.485]	0.212	0.057	[0.125]
Sales workers	0.044	-0.015	[0.043]	0.096	0.117	[0.190]	0.030	0.009	[0.055]
Service workers	0.066	-0.053	[0.040]	0.419	-0.352	[0.259]	0.091	-0.056	[0.057]
Agriculture, animal husbandry, and forestry workers, fishermen, and hunters	0.066	-0.032	[0.038]	0.346	-0.130	[0.227]	0.081	-0.019	[0.052]
Production and related workers, transport equipment operators, and labourers	0.110	-0.029	[0.061]	0.471	-0.074	[0.284]	0.111	0.001	[0.079]

Notes:

The regressions are IV estimates, where dummy indicators for the 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 as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.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: Contract type		
Dependent Variable:	Unit of pay (1 = daily, 2 = weekly, 3 = fortnightly; 4 = monthly)		
	(1)	**(2)**	**(3)**
Got a job or recruiters job offer	-0.576	-0.567	-0.691*
(IV)	[0.381]	[0.376]	[0.387]
Constant	3.223***	4.033***	4.720***
	[0.185]	[0.433]	[1.139]
Stratification cell FE's	No	Yes	Yes
Other covariates?	No	No	Yes
Observations	166	166	166
R-squared	0.029	0.069	0.154
Ave of dep variable (no job)		3.169	

Notes:

The regressions are IV estimates, where dummy indicators for the treatment assignment (i.e. assignment to a 0-, 1-, 5-, 50-, 75-, or 100-percent chance of employment) are used to instrument for
The average daily wage is calculated using the restrospective job work history. The average daily wage is calculated as the average wage on the individual's main job in the last month. Columns 1 through 3, those who are unemployed are coded as 0's. Columns 4 through 6 uses the logged wage, therefore for individuals who earned $0 across all eight months are omitted. Ave hours worked per
Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.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

Table 8: Channels

Panel A: Social Networks:	Avg dep var (1)	Coeff (2)	SE (3)
Heard about a job opportunity	0.438	0.234**	[0.115]
# job opportunities	0.795	0.126	[0.268]
Secured a job opportunity	0.091	0.085	[0.056]
# job opportunities secured	0.080	0.077	[0.056]
Panel B: Signalling:			
Used any reference letter for a job in last 8 months	0.648	-0.074	[0.110]
Panel C: Wage Expectations:			
Self-reported monthly reservation wage	361.873	121.253	[84.563]

Notes:

The regressions are IV estimates, where dummy indicators for the 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 as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.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 9: Heterogeneity of wage and employment impacts			
Panel A: Ability Inteactions	**Frac. months worked**	**Avg daily wage (incl. unemployed)**	**Logged (Avg daily wage)**
	(1)	**(2)**	**(3)**
Got a job	0.119	4.443**	6.711**
	[0.074]	[1.820]	[3.137]
Ability score X Got job	-0.169**	-3.046*	-5.654*
	[0.079]	[1.773]	[3.038]
Ability score	0.099	-1.396	2.636
	[0.099]	[1.741]	[4.166]
Panel B: Degree interactions			
	(1)	**(2)**	**(3)**
Got a job	0.057	7.431*	1.545**
	[0.209]	[4.407]	[0.759]
Degree X Got a job	0.359	-21.501	-4.144
	[1.254]	[24.915]	[3.204]
Degree	-0.033	0.000	3.513**
	[0.000]	[12.706]	[1.645]
Panel C: Current labor attachment interactions:			
	(1)	**(2)**	**(3)**
Got a job	0.761	6.851	5.126
	[1.108]	[23.227]	[4.178]
Any work in last month X Got a job	-0.918	-4.075	-6.233
	[1.532]	[33.295]	[5.784]
Any work in last month	0.334	2.087	2.295
	[0.483]	[10.858]	[1.778]
Panel D: Any previous experience interactions:			
	(1)	**(2)**	**(3)**
Got a job	-0.233	9.528	8.844
	[0.478]	[9.609]	[9.874]
Ever worked X Got a job	0.530	-8.900	-11.214
	[0.742]	[15.287]	[13.496]
Ever worked	-0.189	4.898	4.136
	[0.277]	[5.617]	[4.853]

Notes:

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 $(Base_i * JO_i)$. The fraction months employed variable is calculated as the number of months the individual was employed over the last 8 months, divided by 8. The average daily wage is calculated using the restrospective job work history. Stratification cell fixed effects are included as the randomization was conducted by stratifying on baseline ability and whether the individual had ever worked with the recruiter previously.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.