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Anonymity or Distance? Experimental Evidence on the Obstacles That Young People Face in the Labour Market

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Anonymity or Distance?
Experimental evidence on the obstacles that young people face in the labour market[*]

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Abstract

Do search frictions constrain the labour market prospects of young workers? We conduct a randomised evaluation of two programmes designed to lower spatial and informational barriers to job search among 4,000 young Ethiopians. One group of subjects receives a transport subsidy. Another group participates in a workshop where their skills are certified and they receive training on how to make effective job applications. We find that both treatments significantly improve the quality of the jobs young workers obtain, and the effects are strongest for the most disadvantaged job-seekers. Upon investigating the underlying mechanisms, we show that both interventions mitigate the adverse effects of spatial constraints on labour allocation, and that the workshop helps job applicants to better signal their abilities.

JEL codes: O18, J22, J24 , J61, J64, M53.

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1 Introduction

Young workers throughout the world struggle to find good jobs. The problem is especially acute in the growing cities of developing countries, where scores of young people have low-quality jobs that are often precarious and largely informal. These jobs do not meet the aspirations for a stable livelihood of young urbanites, feeding social tension and political instability. As urbanisation accelerates – by 2050, the urban population of Africa is expected to triple, while that of Asia is expected to grow by 61 percent (United Nations, 2014) – these concerns will become ever more pressing. Yet, little is known about how policy makers can help young city dwellers to find good jobs.

A growing literature suggests that search frictions can be a key constraint for young people in the labour market (Card et al., 2007; Pallais, 2014; Crépon and den Berg, 2016). In particular, young job-seekers face two major obstacles. First, in large cities many of them are geographically distant from potential employers, without affordable options to travel to opportunity. Second, they are typically anonymous to potential employers, with little work experience and no effective way of signalling their ability. Reducing these search frictions may improve the employment prospects of young job-seekers, increasing their welfare and psychological well-being (Clark and Oswald, 1994; Paul and Moser, 2009; Krueger and Mueller, 2012). It can also generate aggregate gains to the economy, by enabling workers to find jobs that are a better match for their skills (Marimon and Zilibotti, 1999; Galenianos et al., 2011; Hsieh et al., 2013).

In this paper, we document the labour market effects of two interventions that reduce search frictions for young workers. These interventions are designed to separately ease the spatial and informational constraints to job search identified in the literature. The first intervention is a *transport subsidy*. Job search in our study area requires regular trips to the centre of town and we calibrate the subsidy amount to cover the cost of this journey. Participants can collect the subsidy from an office located in the centre of the city, up to three times a week. The second intervention is a *job application workshop*. Participants are offered orientation on how to make effective job applications using CVs and cover letters, and on how to approach job interviews. Further, their general skills are certified using a mix of standardised personnel selection tests. We evaluate these programs using a large sample of over 4,000 young individuals in Addis Ababa, Ethiopia.[1] This location is ideal for our purpose: a rapidly expanding metropolis with a large labour market where informality and precarious employment are widespread, especially among the youth. In Addis Ababa,

[1] Individuals included in the study are between 18 and 29 years of age, have completed high school, are available to take up employment, and are not currently working in a permanent job. Because of our interest in search costs related to transport, we focus on subjects who reside at least 2.5 km away from the centre of town.

young people below the age of 30 make up about half of the workforce, but take 70 percent of the temporary jobs.[2]

We find that both interventions help young job-seekers, and especially the most disadvantaged, get better jobs. Eight months after the end of the program, individuals invited to the job application workshop are nearly 40 percent more likely to have permanent employment and nearly 25 percent more likely to be in formal employment compared to individuals in the control group. Those who are offered the transport subsidy are 25 percent more likely to be in formal employment. The results are statistically significant, robust to a correction for multiple comparisons and economically meaningful. They are even more compelling in the context of a developing country, where many informal occupations are 'jobs of last resort', undertaken by unemployed people who have no other means to survive. The effects are stronger for women and less educated workers. These are the groups that typically find it hardest to obtain high quality employment, in many countries throughout the world (OECD, 2014, 2015). Using high-frequency data, we also show that permanent employment rates among less educated subjects in the control group are not on an upward trajectory. This suggests that permanent employment would not naturally increase in the absence of the intervention and that untreated individuals will not necessarily catch up as time goes by. In other words, the positive impact of the intervention is not only significant, but it is likely to be long-lasting.

To understand the mechanisms that drive these results, we conduct fortnightly phone interviews with all sampled job-seekers throughout the course of the study. This provides a rich, high-frequency dataset that allows us to observe how search behaviour evolves in response to our interventions.[3] We find that the transport subsidy allows job-seekers to search more intensely. Further, we find that both interventions improve job-seekers' search efficacy, particularly for the least educated. On average, control individuals with a high school degree receive an offer for a permanent job every 10.5 applications. The workshop and the transport programmme bring this down to about one offer every 5.2 applications.

Further, we show that the interventions work by easing spatial and informational constraints. First, we find that, in Addis Ababa, self-employment in the informal sector is more common on the outskirts of the city, as good employment opportunities are likely to be scarcer the further workers live from the city centre. Our interventions, particularly the transport subsidy, offset the hurdle of distance by enabling workers who live further away from the centre to access formal employment opportunities. Thanks to the interventions, self-employment rates among these workers drop to levels similar to people living in the

[2] Authors' calculations using data from CSA (2014).

[3] Recall bias would make it difficult to perform a similar analysis using retrospective questions asked in the endline survey.

centre of the city. Further, the job application workshop is effective in mitigating the ‘curse of anonymity’ that young inexperienced workers typically face when trying to demonstrate their skills to employers. Using data from the personnel selection tests, we show that participating in the job application workshop strengthens the correlation between skills and good employment outcomes for workers with less formal qualifications, who, in the absence of the intervention, find it hardest to signal their skills. These findings suggest that the two interventions lead to a more efficient allocation of labour.

Finally, we measure the indirect impacts of the interventions on the young individuals who reside close to programme participants (Crépon et al., 2013). Using a randomised saturation design, we find some positive indirect effects of the transport subsidy on the quality of employment when the proportion of treated job-seekers is low, and some negative effects when the proportion of treated job-seekers is high (90%). We do not find indirect effects of the job application workshop, despite a fairly high proportion of treated respondents in all clusters (80%). Of course, as most other interventions of this kind, we cannot rule out the existence of spillover effects outside the study areas and, therefore, we are unable to measure the overall efficiency and welfare gains of the treatment. Despite this common limitation, the results presented above suggest that the interventions deliver positive gains in terms of both overall equity and efficiency.

Our work expands the extremely limited evidence base on job-search assistance in developing countries (Kluve et al., 2016). The two studies that are most related to ours are Groh et al. (2015) and Franklin (2015). Groh et al. (2015) find that a matching intervention based on information about workers’ skills does not improve the employment outcomes of young educated Jordanian women. In line with their results, we are unable to find significant treatment effects for individuals with tertiary education, but we find that this type of intervention can benefit less educated workers significantly. Franklin (2015) reports the results of an early small-scale trial of the transport subsidy evaluated in this paper and documents positive effects on permanent employment for a sample of active jobseekers.

A related literature studies interventions that facilitate migration and connect rural workers to urban jobs (Jensen et al., 2012; Bryan et al., 2014; Beam, 2016). These interventions work partly by changing rural workers’ expectations about the employment opportunities available in nearby cities. Thus, they address constraints that are different from those faced by the urban job-seekers in our sample, who are likely to have greater awareness about existing opportunities.

In developed countries, several studies have evaluated job-search assistance programs that bundle different components: eg. counseling and information (Card et al., 2015). A small number of recent experimental evaluations have focused on specific interventions:

public transport subsidies (Phillips, 2014), information provision about vacancies and job search strategies (Altmann et al., 2015; Belot et al., 2015), reference letters that certify worker performance and skills (Pallais, 2014). We expand on this literature by designing an intervention that strengthens job-seekers' presentation skills and by comparing, for the first time, its effects with an alternative form of job-search assistance. We further use a regression discontinuity strategy to separate the effects of the *orientation* and *certification* components of the job application workshop, and we find evidence suggesting that the presentation skills strengthened by the orientation session are the binding constraint for the low educated workers in our sample.

More generally, the evidence we present points to the presence of significant search frictions in the labour market and suggest that policies that decrease these frictions have the potential to improve efficiency. This contributes to our understanding of how space affects the functioning of labour markets (Gollin and Rogerson, 2015; Bryan and Morten, 2015; Asher and Novosad, 2015): It also expands a growing literature that studies the economic importance of cognitive and non-cognitive skills (Bowles et al., 2001; Heckman et al., 2006; Borghans et al., 2008; Groh et al., 2012; Blattman et al., 2015; Heller et al., 2015; Hoffman et al., 2015).

The rest of the paper is organised as follows. Section 2 provides some information about the labour market in Addis Ababa. Section 3 describes the two interventions. In section 4, we introduce the experimental design, the data we use, and the core empirical specification. Section 5 presents the main results. In section 6, we discuss the nature of the constraints addressed by our interventions. We conclude in section 7.

2 Context

Addis Ababa is the largest city in Ethiopia. Official estimates suggest that the population of the city totalled 3.2 millions in 2014 and planners expect that it will more than double in the next 25 years. (CSA, 2014; Davison, 2014).[4] In this growing labour market, available jobs are often insecure, informal and poorly paid – a policy challenge faced by many low-income economies (AfDB, 2012). At the time of our endline survey, only 30 percent of the employed individuals in the control group have a permanent job. The others work in temporary, casual or self-employment. The stream of income from these occupations is unstable. For example, 25 percent of temporary workers report that they had to miss at least one week of work, since they started their current job, because "work was not available".[5]

[4] Other estimates suggest that the total population of the city is close to 4.5 million.
[5] The median duration of these spells was 4 weeks for temporary workers and 8 weeks for the self-employed.

Furthermore, working arrangement are often informal, which makes it difficult to enforce workers' rights and to collect taxes and social security contributions. Over half to the wage employees in the control group of our study do not have a written agreement with their employer.

Job search is costly for unemployed youth in the city. One of the most popular search methods used by the participants in our study is to visit job vacancy boards.[6] The boards are located in the centre of the city, forcing participants who live in the periphery to travel frequently to the centre, which is costly. In addition, job-seekers face the costs of gathering information through newspapers, printing CVs and cover letters, travelling to interviews, and so on. Among the active searchers in our sample, the median expenditure on job search at baseline amounts to about 16 percent of overall expenditure.[7]

Young job-seekers in Addis Ababa also find it hard to signal their skills to employers. To select a shortlist of candidates among a large number of applicants, firms in the city often use simple criteria such as whether the candidate has previous work experience.[8] Job referrals are also frequent (Serneels, 2007; Caria, 2015). This puts young people at a disadvantage, as they have little work experience and less extensive networks. 55 percent of the participants in our study report having less than one year of work experience and only 16 percent have ever worked in a permanent job. Further, many job-seekers do not seem to be familiar with the process and the standards of job applications. For example, while firms report valuing a well-written CV, 41 percent of the study participants who have applied for at least one job in the last six months have not prepared a CV to support their applications.

In light of these challenges, we devised two interventions to reduce the cost of job search and help workers signal their abilities to employers.

3 The interventions

3.1 Treatment 1: The transport subsidy

Individuals in this treatment group are offered a subsidy to cover the cost of traveling to the city centre. The subsidy takes the form of a cash transfer that is conditional on visiting

[6] At baseline, 36 percent of participants rank the job vacancy boards as their preferred method of search and 53 percent of active searchers have visited the boards at least once in the previous seven days.

[7] This goes up to 25 percent for job-seekers who report searching 6 days a week. These are large amounts, especially if we consider that the typical job-seeker spends a long time in unemployment before finding a job.

[8] 56 percent of firms report that for blue collar positions they only consider candidates with sufficient work experience, and 63 percent of firms use this selection method for white collar positions.

a disbursement point, located in an office in the centre of Addis Ababa.[9] Recipients are required to attend in person, and to show photographic ID on each visit. Each recipient can collect cash once a day, up to three times a week. The daily amount is sufficient to cover the cost of a return bus fare from the participant's area of residence at baseline to the disbursement point.[10] To access the subsidy, job-seekers need to have (or borrow) enough cash to make the first journey – which in our setting is almost always the case.[11]

Prior to the intervention, respondents in our sample do not travel frequently to the city centre.[12] By paying participants conditional upon their presence at our office, we directly incentivise travel to the centre. This allows us to focus on spatial constraints to job search. In addition, conditional transfers are a more realistic policy option in this context. Unconditional transfers have proved unpopular among voters in various countries in Sub-Saharan Africa (Ferguson, 2015; Sandefur et al., 2015) and the Ethiopian Government requests that the beneficiaries of social assistance programs are employed in public work schemes.[13]

The median subsidy available on a given day is equal to 20 Ethiopian Birr (1 USD at the exchange rate at the beginning of the intervention). This equals about two thirds of the median weekly expenditure on job search at baseline, and 10 percent of overall weekly expenditure. The minimum amount is 15 ETB (0.75 USD) and the maximum 30 ETB (1.5 USD). We stagger the start time and the end time of the subsidy, randomly. This generates variation across individuals in the number of weeks during which the treatment is available, and in the time of treatment. The number of weeks of treatment varied from 13 to 20, with a median of 16 weeks. The intervention was implemented between September 2014 and January 2015.

3.2 Treatment 2: The job application workshop

The job application workshop is designed to improve job-seekers' ability to accurately present their skills to potential employers, thus overcoming the challenge of anonymity

[9] This office is located close to the major job vacancy boards. The office was also near a central bus station, from which buses leave to destinations all around Addis Ababa.

[10] We calibrate the subsidy to allow participants to travel on minibuses. Study participants can in principle walk to the office or use less expensive large public buses – an inferior means of transport that is crowded and infrequent – and save a part of the transfer. Qualitative evidence suggests that this is not common. Further, we do not find that individuals in this treatment group increase their savings during the weeks of the intervention.

[11] While job-seekers have little cash on hand, our data shows that most of them have at least enough to pay for one journey, in the knowledge that this money will be reimbursed. About 95 percent of job-seekers in our sample have at least 15 ETB in savings, while 75 percent of job-seekers have at least 10 ETB available as cash-on-hand or at home. See Franklin (2015) for further discussion of this issue.

[12] In the week prior to the baseline interview, 70 percent of the sample travelled to the centre fewer than three times.

[13] For example, the flagship Productive Saftety Nets Program (PSNP) and the newly rolled out Urban PSNP.

that youths with limited work-experience typically face. The intervention has two components: an orientation session and a certification session. The orientation session helps participants make more effective use of their existing signals (job experience, education, etc.). In the certification session, we certify skills that are "hard to observe" for employers, such as cognitive ability, and we provide participants with an instrument (the certificates) to signal those skills. The design aims to mimic the orientation services available to job-seekers in several countries.[14]

The intervention takes place over two days. On the first day, participants take a series of personnel selection tests. On the second day, they attend the orientation session. The intervention is administered by the School of Commerce of Addis Ababa University, between September and October 2014. The School of Commerce has a reputation for reliable personnel selection services and many firms screen their applicants using tests developed, and sometimes administered by, the School of Commerce. In a separate survey of 500 medium to large enterprises in Addis Ababa, we find that about 40 percent of firms know about the personnel selection services offered by the School of Commerce. 80 percent of these firms report that they trust the services offered by the School of Commerce.

The orientation session covers three main topics: CV writing, application letters and job interviews. All the training materials were developed by the School of Commerce and later reviewed by our team. We administer four tests: (i) a Raven matrices test, (ii) a test of linguistic ability in Amharic, (iii) a test of mathematical ability and (iv) a 'work-sample' test. The results of the tests are presented in a certificate, which job-seekers can use in support of their job applications. The certificates explain the nature of the tests and report the relative grade of the individual for each test, and an aggregate measure of performance.[15] The certificates are officially issued by the School of Commerce and the Ethiopian Development Research Institute.[16]

We chose the tests on the basis of the results of several qualitative interviews with firm managers in the city.[17] The Raven test is a widely used measure of cognitive ability (Raven, 2000). It is believed to be one of the best predictors of worker productivity (Schmidt and Hunter, 1998; Chamorro-Premuzic and Furnham, 2010) and it has been used by economists

[14] Similar forms of support are often provided by Public Employment Services (PES). Differently from PES, however, we do not provide job-seekers with direct information about available vacancies, since we are interested in isolating and tackling constraints on workers' ability to signal their skills.

[15] We report relative performance using bands: a band for the bottom 50 percent of the distribution and then separate bands for individuals in the upper deciles of the distribution: 50-60%, 60-70%, 70-80%, 80-90%, 90-100%.

[16] Participants collect the final certificates from the School of Commerce, after all testing sessions are completed. To minimise threats to external validity, we explicitly avoided references to the University of Oxford in the certificates.

[17] These interviews highlight managers' information needs and the degree of familiarity that managers have with various tests.

to measure worker quality in several contexts (Dal Bó et al., 2013; Beaman et al., 2013). The tests of mathematical and linguistic ability were designed to capture general mathematical and linguistic skills, as in the OECD's PIAAC survey or the World Bank's STEP survey (OECD, 2013; Pierre et al., 2014). The 'work-sample' test captures participants' ability to carry out simple work tasks: taking minutes during a business meeting, carrying out a data entry task under time pressure, and meeting a deadline to complete a data entry task at home. The literature in organisational psychology suggests that 'work-sample' tests can be used alongside measures of cognitive ability to predict worker performance (Schmidt and Hunter, 1998).

4 Experimental design and estimation strategy

4.1 The sample

To obtain our experimental sample, we began by drawing a random selection of geographic clusters from the list of Ethiopian Central Agency (CSA) enumeration areas.[18] Given our interest in spatial constraints, we excluded all clusters within 2.5 km from the city centre and those outside the city boundaries. Directly adjacent clusters could not be selected to minimise potential spillovers.

In each selected cluster, we used door-to-door sampling to construct a list of all individuals who: (i) were 18 or older, but younger than 30; (ii) had completed high school; (iii) were available to start working in the next three months; and (iv) were not currently working in a permanent job or enrolled in full time education. We randomly sampled individuals from this list to be included in the study. Our lists included individuals with different levels of education. We sampled with higher frequency from the groups with higher education. This ensured that individuals with vocational training and university degrees are well represented in the study. In all, we interviewed 4059 individuals who are included in our experimental study.[19]

[18] CSA defines enumeration areas as small, non-overlapping geographical areas. In urban areas, these typically consist of 150 to 200 housing units.

[19] We initially completed baseline interviews with 4388 eligible respondents. Before assigning treatments, we attempted to contact all of them by phone and dropped individuals who could not be reached after three attempts over a period of one month (this helped us curtail problems of attrition, by exluding respondents who were likely to attrit.). We also dropped any individual who had found a permanent job by the time treatments were assigned (and had retained it for at least six weeks). Finally, we dropped individuals who had migrated away from Addis Ababa. Table 3 in the online appendix shows how many individuals were dropped from the sample at each point and the reasons for them being dropped.

4.2 Data collection: Face-to-face and the Phone survey

We collect data on study participants through both face-to-face and phone interviews. We complete baseline face-to-face interviews between May and July 2014 and endline interviews between June and August 2015. Face-to-face interviews record information about the sociodemographic characteristics of study participants, their education, work history, finances, expectations and attitudes.

We also construct a rich, high-frequency panel through fortnightly phone interviews. We call all study participants throughout the duration of the study. In these interviews we administer a short questionnaire focused on job search and employment. Franklin (2015) shows that high-frequency phone surveys of this type are reliable (i.e. do not generate Hawthorne effects).[20]

4.3 Randomisation

We randomly assigned geographic clusters to one of the treatment arms or the control group. To ensure balance, we created blocks of clusters with similar baseline observables and randomly assigned clusters within each block to the different treatment groups (Bruhn and McKenzie, 2009).[21]

Not all individuals in the clusters assigned to the transport intervention and job application workshop were offered treatment. Among those in the transport clusters, we implemented a randomised saturation design. We varied the proportion of sampled individuals who were offered treatment from 20% to 40%, 75% and 90% (see Table 1). In clusters assigned to the job application workshop we kept the level of saturation fixed at 80%. Having set cluster saturation levels, we assigned individuals within each cluster to a treatment or a control group. This was done by blocking individuals within clusters by their education level, and implementing a simple re-randomisation rule. The overall assignment to treatment is outlined in Table 1.[22]

< Table 1 here. >

[20] The study of Franklin (2015) is also focused on young urban job-seekers in Ethiopia.

[21] Following Bruhn and McKenzie (2009), to create the blocks we used variables that we expected to correlate with subjects' employment outcomes: distance of cluster centroid from city centre; total sample size surveyed in the cluster; total number of individuals with degrees; total number of individuals with vocational qualifications; total number of individuals who have worked in the last 7 days; total number of individuals who have searched for work in the last 7 days; total number of individuals of Oromo ethnicity; average age of individuals in the cluster.

[22] In addition, individuals designated to receive the transport intervention were randomly assigned to a start and an end week. This is illustrated in table 4 in the online appendix.

4.4 Balance and Attrition

We find that our sample is balanced across all treatment and control groups, and across a wide range of outcomes. This includes outcomes that were not used in the randomisation procedure. We present extensive balance tests in Table 5 in the online appendix. For each baseline outcome of interest, we report the p-values for a test of the null hypothesis that all experimental groups are balanced. We cannot reject this null for any of the variables analysed.

Attrition is low, especially compared to other studies of young adults in urban developing country contexts (Baird et al., 2011; Blattman et al., 2014). In the endline survey, we find 93.5% of all participants; and attrition is uncorrelated with treatment.[23] Table 7 in the online appendix presents the full analysis.[24]

Attrition in the phone survey is also low: below 5% in the early months of the calls. While it increases in later weeks, we are still able to contact more than 90% of respondents in the final month of the phone survey. Figure 14 in the online appendix shows the trajectory of monthly attrition rates over the course of the phone survey.

4.5 Take-up

Take-up was large albeit far from universal, for both treatments. 50% of individuals in the transport group collect the cash at least once. Of these, 81% return to collect the subsidy again. Those who collect the subsidies for at least two weeks tend to be dedicated users. Conditional on ever collecting the money, 74% of respondents take it at least once a week over the course of the entire study, with an average number of total collections is 16. This corresponds to an average cash transfer of 320 ETB (15 USD) per active user, or 160 ETB (7.5 USD) per intended user. Further, 63% of individuals who are invited to the job application workshop attend it. 80% of those attending later collect the certificates from the School of Commerce.

4.6 Estimation strategy

We follow a detailed pre-analysis plan registered at: www.socialscienceregistry.org/trials/911. The plan describes the empirical strategy, the outcome variables of interest, the

[23] We cannot reject the null hypothesis that there are no differences in attrition rates between treated and control individuals when we study each treatment individually, or when we run a joint test for all treatments.

[24] A number of covariates predict attrition. Since neither these variables, nor attrition itself, are correlated with treatment, we are not worried about the robustness of our results.

definition of these variables, the subgroup analysis, and our approach to multi-hypothesis testing and attrition.

Our primary objective is to estimate the effects of the programs on the labour market outcomes of study participants. For each outcome at endline, we will estimate the following equation:

$$y_{ic} = \beta_0 + \sum_{f} \left[\beta_f \cdot \texttt{treat}_{fic} + \gamma_f \cdot \texttt{spillover}_{fic}\right] + \alpha \cdot y_{ic,pre} + \boldsymbol{\delta} \cdot \boldsymbol{x}_{ic0} + \mu_{ic}, \tag{1}$$

where y_{ic} is the endline outcome for individual i in cluster c and $\boldsymbol{x}_{ic0}$ is the vector of baseline covariate values that were used for re-randomisation and blocking. $\texttt{treat}_{fic}$ is a dummy capturing whether an individual has been *offered* treatment f.[25] Thus, our estimates measure the *intent-to-treat* impacts of the interventions.$\texttt{spillover}_{fic}$ is a dummy that identifies control individuals residing in clusters assigned to treatment f. Thus, γ_f captures the indirect (spillover) effects of treatment f. We correct standard errors to allow for correlation within geographical clusters and we use sampling weights to obtain average treatment effects for the eligible population as a whole.[26]

In the pre-analysis plan, we specify a family of seven primary employment outcomes. For each one of them we we test the null hypothesis that each treatment had no impact. We report both a conventional p-value and a 'sharpened' q-value (Benjamini et al., 2006). The q-values control for the false discovery rate within the family of the seven hypotheses that we test for each program. We also specify two families of intermediate outcomes that help us elucidate what mechanisms drive the primary effects, and seven families of secondary outcomes.

5 Results

We find that both interventions significantly improve the quality of the jobs workers get. Table 2 reports the main impacts on our family of primary outcomes.[27] The application

[25] We follow the pre-analysis plan and include a dummy for each treatment. Alongside the two interventions studied in this paper, we also implemented a series of job fairs that a separate group of respondents was invited to attend, together with a random sample of firms from Addis Ababa. This is a distinct treatment and we evaluate it in a separate paper (Abebe et al., 2016). For the sake of consistency with the pre-analysis plan, job fair dummies are included in equation (1), but their effects are not reported in this article.

[26] As explained above, we sampled more educated individuals with higher frequency. In the regressions we thus weight observations by the inverse of the probability of being sampled. The sampling weights are reported in the pre-analysis plan.

[27] This outcomes were prespecified as our primary family in our pre-analysis plan.

workshop increases the probability of working in a permanent job by nearly 40 percent (raising the share of workers in permanent employment by 6.7 percentage points from a level of 17.1 percent in the control group). The effect is statistically significant at the 1 percent level and remains highly significant after correcting for multiple comparisons. The transport treatment boosts permanent employment by nearly 20 percent (a 3.4 percentage points increase from the control level). This effect is significant at the 10 percent level, but has a q-value of 0.23 once we account for multiple comparisons. Both effects are also robust to the use of Lee bounds to correct for attrition.[28] We report these bounds in Table 8 of the online appendix.

< Table 2 here. >

We also find that both interventions increase workers' chances to have a formal job (proxied by having a written contract throughout the paper) by nearly 25 percent.[29] Only 22 percent of the control group has a formal job at endline and both programmes increase that figure by 5 percentage points. The effects are robust to the multiple comparison correction and to the use of Lee bounds to correct for attrition.

The interventions have only modest effects on the overall employment rate of treated individuals. This goes up by 4 percentage points for individuals in the transport treatment, and by 2 percentage points for individuals who were invited to the job-application workshop. Both effects are statistically insignificant. Taken together, our results reveal that focusing exclusively on employment rates may deliver a very partial picture of treatment impacts, as it would miss the crucial dimension of job quality. This is particularly important in the context of a developing country, where workers are often employed in insecure, informal jobs of last resort. By facilitating job search, our interventions significantly reduce the likelihood of falling into such jobs.

Earnings and work satisfaction are not changed significantly by the interventions (in line with other recent studies, e.g. Groh et al. (2012); Jensen et al. (2012); Franklin (2015)). However, we cannot reject the hypothesis that the estimated treatment impacts on these two variables equal the naïve prediction obtained from multiplying the significant treatment impacts on permanent and formal work by the marginal effect of these two variables on

[28] This is not surprising, as attrition in this study is modest and is not significantly correlated with treatment.

[29] A written agreement is a salient proxy of formality. Having no contract makes it difficult for workers to uphold their formal rights and for the fiscal authorities to collect taxes and social security contributions (another indicator of informality often used in the literature, e.g. OECD (2015); Jütting and de Laiglesia (2009)). In our sample, workers with a written contract (only 40 percent of the total) are 60 percent less likely to report delays in wage payments and 22 percent more likely to have received information about health and safety regulations. They are also less likely to be working in small shops, construction sites and open markets (locations that are typically prone to informality), and more likely to be working in an office or a factory.

earnings and work-satisfaction. For example, among the controls permanent work is associated with a 25 percentage points increase in work satisfaction, and formal work with a 22 percentage point increase. Individuals invited to the workshop are 6.7 percentage points more likely to have permanent work, and 5.2 percentage points more likely to have formal work. Multiplying the two effects and adding up, we predict that work satisfaction should increase by 2.7 percentage points in the workshop group. The estimated treatment effect on work satisfaction for this treatment arm is 2.1 percentage points, quantitatively close and statistically indistinguishable from the naive prediction.

In addition to testing the effects of the interventions on the primary employment outcomes, we evaluate their impacts on a range of secondary outcomes, most notably other measures of job quality, worker expectations, reservation wages, aspirations and mobility (the full set of results is available in Tables 11 to 18 of the empirical appendix).[30] Overall we find little evidence that our interventions have changed outcomes in these areas. We have some limited evidence that the job-seekers who were invited to the job application workshop are more optimistic about their labour market prospects. They expect to receive 20 percent more job offers in the next four months than individuals in the control group, although this effect is not significant after correcting for multiple hypothesis testing.[31]

5.1 Which groups benefit the most from the interventions?

We further investigate how treatment effects vary across different categories of workers identified in the pre-analysis plan. We are interested in heterogeneity along two key dimensions. First, do the interventions help groups that typically have worse labour market outcomes — for example, women or the less educated? Second, do the interventions help those who face the strongest job search constraints? To identify constrained job-seekers, we first look at individuals who spent less than the median number of weeks searching for a job in the three months prior to the randomisation. We then investigate proxies for financial, spatial and informational constraints. In particular, we use baseline measures of savings, distance from the city centre, previous work experience, and the use of skills certificates or a CV in prior job applications. When such characteristics are continuous, we create groups by splitting respondents at the median. For each outcome and every dimension of heterogeneity (e.g. gender, education, and so on), we run the following specification:

[30] In addition to investigating each outcome in a family separately, we use a standard 'omnibus' approach: we construct an index for each family and test whether the index is affected by our treatments (see Table 11 in the appendix). For inference, we proceed as before: we report both p values and false discovery rate q-values by treating each index as a separate member of a 'super-family' of indices.

[31] They also expect five weeks less of unemployment before finding the next job, but this effect is not significant.

$$y_{ic} = \sum_{v=1}^{m} \left[\beta_{0v} + \sum_{f} \left(\beta_{vf} \cdot \texttt{treat}_{fic} + \gamma_{vf} \cdot \texttt{spillover}_{fic} \right) \right] \cdot I \left(x_{ic,pre} = v \right)$$
$$+ \alpha \cdot y_{ic,pre} + \boldsymbol{\delta} \cdot \boldsymbol{x}_{ic0} + \mu_{ic}, \qquad (2)$$

where $x_{ic,pre}$ is a categorical variable with values $1, ..., m$ corresponding to the m groups of interest (e.g. male and female in the case of gender), and $I\left(x_{ic,pre} = v\right)$ is a indicator variable that takes the value of 1 when $x_{ic,pre}$ is equal to v. The coefficients β_{vf} estimate the effect of treatment f for group v.

< **Figure 1 here.** >

< **Figure 2 here.** >

We find that the effects on job quality (permanent and formal work) are concentrated among the least educated job-seekers, as shown in Figures 1 and 2 (and Table 9 in the appendix). For this group, the job application workshop triples the probability of permanent employment (increasing it by 10 percentage points from a 5 percent baseline), while the transport intervention doubles it. On the other hand, treatment effects for more educated individuals are modest. The estimated impacts on having a formal job follow a similar pattern. Further, our high-frequency data shows that for low-educated individuals in the control group permanent employment rates are not on an upward trajectory when the study ends.[32] This suggests that permanent employment would not naturally increase in the absence of the intervention and that untreated individuals will not necessarily catch up as time goes by. In other words, the positive impact of the intervention is not only significant, but also long-lasting.

We also find that treatment effects are stronger among women, as shown in Figure 3 and 4 (and Table 10 in the appendix). The transport intervention significantly increases both women's probability of permanent work and probability of formal work by 6 percentage points (from a baseline of 15 and 20 percent respectively), while the workshop respectively increases them by 7 and 9 percentage points respectively. Effects for men are more mixed: the workshop increases their probability of permanent employment, while the transport subsidy has a positive effect on their probability formal employment.

[32] Among control subjects with only high school, rates of permanent employment rise gradually in the first half of the study because, we hypothesize, the more employable among them find work. However, there is no further increase over the last three months of the study, with the rate of permanent employment remaining at about 15%.

< Figure 3 here. >

< Figure 4 here. >

Further, treatment effects are concentrated among those who do not search actively at baseline, individuals with no previous work experience, and individuals with low savings (see Tables 19, 20 and 21 in the online appendix). For example, individuals who do not search actively at baseline experience a significant increase in the likelihood of permanent employment of about 9 percentage points when invited to the job application workshop. For active job-seekers on the other hand, we estimate an insignificant increase of 4 percentage points.

5.2 Mechanisms: How did treated individuals get better jobs?

How do treated individuals obtain jobs of higher quality? We study three possible mechanisms. First, individuals who are offered the interventions can search more intensely, for example they can dedicate more time to job search and they can make more applications. Second, treated individuals can change their search strategy. This can either involve searching for *different* jobs or using different search *methods*. Finally, treated job-seekers can become more effective at job search. We measure search effectiveness by computing the ratio of job interviews to applications and the ratio of job offers to applications in the 12 months prior to the endline interview.

In this section, we make extensive use of the high frequency data from the phone interviews. Pooling data from the phone calls across all weeks enables us to estimate the weekly impact of the interventions and the overall trajectory of treatment effects. We estimate two regression models. First, to obtain weekly impact estimates, we run:

$$y_{itc} = \sum_{f} \sum_{w=S_f}^{E_f} \left[\beta_{fw} \cdot \texttt{treat}_{fic} \cdot d_{wit} + \gamma_{fw} \cdot \texttt{spillover}_{fic} \cdot d_{wit} \right] + \alpha_t \cdot y_{itc,pre} + \boldsymbol{\delta} \cdot \boldsymbol{x}_{ic0} + \eta_t + \mu_{itc}, \tag{3}$$

where η_t is a time-specific intercept term. w indicates the number of weeks since each treated individual began receiving his/her treatment.[33] d_{wit} is a dummy variable equal to 1 in period t if an individual started receiving their treatment w periods ago. For example, for an individual assigned to receive the transport treatment from week 15 of the study

[33] $w = 0$ in the week when the treatment started, and is negative for weeks before that.

onwards, the dummy d_{0it} is equal to 1 in week 15 and to 0 in all other weeks.[34] Individuals in the control group have all such dummy variables set to 0. Thus, β_{fw} is our estimate of the impact of intervention f, w weeks after the intervention started. We allow the effect of the baseline control term $y_{ic,pre}$ to vary over time by estimating α_t for each time period, while we estimate time-invariant effects of individual covariates x_{ic0}.

Second, we estimate the trajectory of treatment effects by pooling all post treatment ($w > 0$) observations and estimating quadratic trends over time of the treatment effect for each intervention. To do this, we estimate equation 3, subject to the following quadratic constraints on β_{fw} and γ_{fw}.

$$\beta_{fw} = \begin{cases} 0 & \text{if } w \leq 0; \\ \phi_{f0} + \phi_{f1} \cdot w + \phi_{f2} \cdot w^2 & \text{if } w > 0; \end{cases} \tag{4}$$

$$\text{and } \gamma_{fw} = \begin{cases} 0 & \text{if } w \leq 0; \\ \theta_{f0} + \theta_{f1} \cdot w + \theta_{f2} \cdot w^2 & \text{if } w > 0. \end{cases} \tag{5}$$

That is, instead of estimating parameters β_{fw} and γ_{fw} directly, we will estimate ϕ_{f0}, ϕ_{f1}, ϕ_{f2}, θ_{f0}, θ_{f1} and θ_{f2}.

5.2.1 Effects on job search intensity

We find that the transport intervention increases the intensity of search. In the weeks when treatment is available, treated individuals are about 9 percentage points more likely to search for a vacancy at the job board– a form of job search that requires transport (see Figure 5). This is an increase of about 30 percent over a control mean of 28%. More generally, treated individuals are about 12.5 percent more likely to be doing any job search activity than control individuals (a 5 percentage point effect over a control mean of 40%, as shown in Figure 6. This effect decreases linearly after the end of the transport intervention. The job application workshop, on the other hand, does not affect the likelihood of searching for a job (Figure 7).

< Figure 5 here. >

[34] Similarly, for an individual who starts treatment in week 15, we set $d_{-1i14} = 1$, and $d_{5i20} = 1$, and so on. Note that because interventions ran for different lengths of time, the number of weeks for which we will be able to estimate the treatment effect relative to the start week of the treatment will differ by treatment. In the notation above S_f denotes the earliest week for which we will be able to estimate a treatment effect for treatment f. E_f denotes the final week. If, for example, a treatment began in week 15 of the study, then $S_f = -15$ and $E_f = 39$. For this treatment, we will use data from week 10 of the study to estimate the coefficient β_{f-5}.

< **Figure 6 here.** >

< **Figure 7 here.** >

We find that study participants temporarily decrease the amount of work they take during weeks when the transport subsidy is available (Figure 8). The effect is driven by a reduction of work in self-employment (Figure 9). Franklin (2015) finds a similar effect of transport subdisies for a sample of active job-seekers.

The average impacts of our treatments on a range of job search indicators are further documented in the online appendix (Table 22). One interesting result is that more frequent search activity among respondents in the transport group does not necessarily translate in a higher number of job applications. This is consistent with the hypothesis that increased search efforts allow employees to be more selective about the applications they make.

< **Figure 8 here.** >

< **Figure 9 here.** >

5.2.2 Effects on search efficacy

Both our treatments significantly increase the efficacy of job search for the high impact groups described above. Most notably, among low-educated job-seekers (high-school graduates), our two interventions reduce the number of applications necessary to obtain an offer of permanent employment from 10.5 TO 4.7, a very large impact (see Table 23 in the appendix).[35] In section 6, we further investigate what drives this effect. In the next section, we rule out the hypothesis that our interventions induce workers to search for easier-to-get jobs.

5.2.3 Effects on the types of jobs sought by participants

We do not find evidence suggesting that treated individuals search for different jobs compared to those in the control group. We test this hypothesis in three different ways. First, we use self-reported data on reservation wages and find that treated individuals report being willing to work for the same wages as control individuals.[36] This holds on average, as well

[35] In the full sample, the number is reduced from 6.1 to nearly 4 , but the effect is not statistically significant (see Table 22 in the online Appendix).

[36] This is consistent with the fact that average earnings are similar across experimental groups, as discussed above. Quantile regression analysis further confirms that the interventions did not affect earnings at the bottom of the distribution.

as within educational categories (see Figure 16 and Table 14 in the appendix). Second, in the endline survey we ask individuals whether they *stopped* searching for some occupation in the previous 12 months. This question was aimed at capturing job-seekers' discouragement. We find that individuals in the three experimental groups are equally likely to give up searching for at least one type of occupation and to stop searching for white collar jobs. Finally, in Figures 15a and 15b (online appendix) we compare the probability of working in a number of different occupations across the three groups. Occupation profiles look similar for the three experimental arms, with a slight shift towards white collar jobs for individuals in the transport and workshop group. Overall, this evidence suggests that treated individuals look for similar occupations and are willing to work for similar wages. The effects on job-search efficacy are thus unlikely to be driven by job-seekers' decision to search for 'easier-to-get' permanent and formal jobs.

5.3 Indirect effects on untreated job-seekers

In this section, we study the outcomes of untreated job-seekers who live close to program participants. The benefits of the interventions can extend to this group if the young job-seekers who are offered the programs share information, job referrals or resources with friends and acquaintances in the same neighbourhood. Information and risk sharing of this kind have been documented in several recent studies on developing countries' labour markets (Angelucci and De Giorgi, 2009; Magruder, 2010).[37] On the other hand, untreated youth living close to program recipients may experience negative effects if these groups compete for scarce jobs in the same neighbourhood.

As explained in the design section, some eligible respondents living in clusters assigned to treatment are not offered the program.[38] This is a 'partial population experiment' (Moffitt, 2001), which allows us to compare untreated individuals living close to program participants to untreated individuals living in clusters where no job-seeker has been offered the intervention. We report the results of this analysis in columns 4 and 5 of Table 2.

We do not find statistically significant differences between untreated individuals living in geographical clusters assigned to one of the two interventions and untreated individuals in pure control clusters. However, we are less powered to detect indirect effects compared to the direct effects we studied above. For example, we estimate that untreated individuals in clusters assigned to the job application workshop experience an increase in the probability of formal work of 5.6 percentage points. This effect is of the same magnitude as the

[37] The descriptive evidence from our surveys further confirms that social networks are an important source of information about work opportunities and are used extensively for job referrals.

[38] The proportion of untreated individuals was fixed at 20% in the clusters that received the job application worshop and randomly varied between 10% and 80% across those that received the transport subsidy.

treatment effect we estimate on individuals who are offered the job application workshop, but it is not statistically significant.

We also randomly vary the proportion of treated individuals in the clusters that receive the *transport* intervention. This allows us to run a regression of the form:

$$\begin{aligned} y_{ic} = &\kappa + \beta_{20} \cdot S_{20c} \cdot C_i + \beta_{40} \cdot S_{40c} \cdot C_i + \beta_{75} \cdot S_{75c} \cdot C_i + \beta_{90} \cdot S_{90c} \cdot C_i \\ &+ \gamma_{20} \cdot S_{20c} \cdot T_i + \gamma_{40} \cdot S_{40c} \cdot T_i + \gamma_{75} \cdot S_{75c} \cdot T_i + \gamma_{90} \cdot S_{90c} \cdot T_i \\ &+ \alpha \cdot y_{ic,pre} + \boldsymbol{\delta} \cdot \boldsymbol{x}_{i0} + \mu_{ic} \end{aligned} \tag{6}$$

where the sample is restricted to individuals in clusters assigned to pure control and clusters assigned to the transport intervention. T_i identifies individuals who have been assigned to the transport treatment, while C_i identifies individuals who have not been assigned to the transport treatment. S_{20c} is a dummy variable for individuals living in a cluster where 20% of individuals were offered the transport treatment. Thus, β_{20} captures the difference in outcomes between untreated individuals in these clusters and untreated individuals in clusters where nobody was treated. Further, γ_{20} measures the difference in outcomes between treated individuals in S_{20c} clusters and untreated individuals in untreated clusters. S_{40c}, S_{75c}, S_{90c}, and the remaining β and γ coefficients have a similar interpretation.

We find that the indirect effects of the transport treatment depend on the level of saturation. As the transport intervention has direct impacts on permanent and formal work, we concentrate on indirect impacts on these two dimensions (see Figures 10 and 11). We document a *positive* indirect effect on formal (permanent) work among control individuals in clusters with 25 (40) percent saturation. We also document that untreated individuals in clusters with 90 percent saturation are 5.6 percentage points *less* likely to be in permanent employment than individuals in pure control clusters.[39] They are not, however, less likely to be in formal employment.

< Figure 10 here. >

< Figure 11 here. >

[39] For the regression on permanent work we can reject the null hypothesis that all β coefficients are equal to 0.

6 What constraints did the interventions relax?

In this section, we present some additional analysis to explore the nature of the constraints faced by the job-seekers in our study and the extent to which the interventions relaxed these constraints.[40]

6.1 Spatial constraints

We have argued that high transport costs make job search difficult for young job-seekers, particularly for those who live far away from the centre of the city. High search costs may push young jobseekers to accept low-quality jobs or to choose self-employment, which can often be an occupation of 'last resort' in developing countries (Falco and Haywood, 2016). If our interventions relax spatial constraints to job search, we expect to observe a weakening of the correlation between distance from the city centre and the probability of having a lower quality job. We investigate this hypotheses by computing the smoothed local polynomial estimate of the relationship between distance from the city centre and various forms of employment.

First, we find that in the control group the probability of being self-employed increases almost linearly with distance from the centre (Figure 12). The probability of having any employment, on the other hand, does not change with distance.[41] These findings suggest that spatial barriers influence the occupational structure of the urban labour market and are consistent with a model where high transport costs distort job search. They also complement recent work on the effect of transportation costs on the occupational structure in villages (Asher and Novosad, 2015).

Most importantly, we find that the transport intervention mitigates the adverse effects of distance by unraveling the spatial patterns of self-employment observed in the control group. Figure 12 shows that, in the transport group, individuals who reside far from the city centre have the same probability of being in self-employment as individuals who live close to the centre. Regression results with differential effects of distance by group (either linear or quadratic) corroborate this conclusion: thanks to the treatment, the effect of distance on the probability of self-employment is statistically indistinguishable from zero.

< Figure 12 here. >

[40] The analysis in this section was not registered in the pre-analysis plan.

[41] Not shown for conciseness.

Figure 17 in the appendix shows that the job application workshop similarly mitigates the effect of space on the probability of self employment. As discussed in the previous section, individuals in this experimental group do not search more intensely, but become more effective at job search. This suggests that the effects of distance on job opportunities can also be mitigated by improving search efficacy.

6.2 Informational constraints

Did our interventions enable the labour market to separate more effectively high skilled workers from low skilled ones? To address this question, we exploit our detailed data from the personnel selection tests and analyse the correlation between workers' abilities and their labour market outcomes across treatment groups. If the job application workshop helps individuals to signal their skills more effectively, we would expect to find a larger positive correlation between predicted test scores and employment outcomes among treated individuals, compared to the control group. That is, we would expect the workshop to be particularly useful for individuals with better skills, who can benefit from being more able to convey their attractive characteristics to employers.

We begin by running a regression of individual test scores on a rich set of covariates (including demographic characteristics, educational attainment, and prior work history) and using the estimates to obtain a measure of predicted test scores for all the job-seekers in our sample. We then investigate how predicted skills correlate with the labour market outcomes of interest in different experimental groups. We focus on predicted scores for two reasons. First, the test scores regression includes only variables that are easily signalled to employers using a CV, a cover letter, or during the course of an interview. Hence, the predicted score is a good proxy of *observable* skills, which the orientation component of our intervention – CV writing, interview training, etc. – was designed to help job-seekers highlight (we turn to the effects of certification on 'hard-to-observe' skills in the next section). Second, since the personnel selection tests were part of the treatment and, as such, they were not administered to individuals in the control group, we rely on this method to obtain predicted test scores for both control and treated individuals.

Our results suggest that the ability to signal skills has significantly improved among low-educated job-seekers – the group that experienced the strongest treatment effects – thanks to the job application workshop (see Figure 13 below and Table 25 in the online appendix). In the absence of the intervention, control individuals in this category do not display a positive association between high skills and better labour market outcomes (i.e. having a permanent job). This highlights the difficulty of separating low-ability from high-ability individuals in the market for less educated young workers. When the intervention is introduced, the correlation becomes much stronger.[42] We also estimate that the correlation between predicted skills and earnings increases, although insignificantly so.

< Figure 13 here. >

In sum, the results in this section suggest a positive effect of the *orientation component* of the job application workshop, since we find an increased correlation between employment outcomes and observable characteristics that can be highlighted through the better CVs, improved cover letters and stronger interview skills delivered by our orientation. The *certification component*, on the other hand, should enable workers to signal skills that are 'hard to observe' (and not necessarily correlated with their observable characteristics). Does the additional information revealed in the certificates allow individuals to improve their employment outcomes? We turn to this question in the next section.

The role of certification

We finally test the hypothesis that the certification component of the workshop leads to better employment outcomes by helping job-seekers signal their hard-to-observe skills. To this end, we employ a regression discontinuity design. The certificates issued to respondents report test scores in discrete bands. The original test score is not reported on the certificates and it is not disclosed to study participants. This allows us to study the impact of being placed in a higher band, controlling for the original test score. The intuition behind this test is the following: if the information conveyed by the certificates is driving at least part of the treatment effect, we would expect a discrete improvement in outcomes when individuals are placed in a higher band.

We perform this analysis for the aggregate score (a summary measure of all test results) and the Raven test score, since we find that these measures have the strongest predictive power for endline employment outcomes. Scores below the median are lumped together into a bottom band, while individuals scoring above the median are divided into five bands (one for each of the higher deciles of the test score distribution). We implement a local linear regression to control for raw test scores on either side of the cut-offs, iteratively moving across all 5 relevant cut-offs. We use the optimal bandwidth selection rule suggested by Imbens and Kalyanaraman (2012), but results are consistent with different bandwidth se-

[42] This effect appears to be unique to the job application workshop, as we do not find similar impacts from the transport treatment.

lections.[43]

We find no evidence that being placed in a higher score-band leads to a significant improvement in labour market outcomes. Notably, this is the case for the two main outcomes our interventions have impacted (permanent work and formal work). Figure 18 in the online appendix illustrates this result by showing the effect of being above the median aggregate test score on having a permanent job.[44]

We interpret these results as suggestive evidence that the information about hard-to-observe skills carried by the certificates is not crucial in driving the effect of the job application workshop. Rather, the intervention helps workers improve the way they signal observable skills. Two further pieces of evidence corroborate these conclusions. First, self-reported data on the use of the certificates indicates that about 58 percent of the respondents who received the certificate and made at least one job application did not show the certificate to their prospective employer.[45] Second, the residuals from the above regression of test scores on observable characteristics (a rough proxy for unobservable abilities) do not correlate positively with employment outcomes.

7 Discussion and conclusions

In this paper, we show that two interventions designed to lower spatial and informational barriers to job search significantly help young job-seekers secure better jobs. Our results suggests that the interventions promote equal opportunity in a labour market where young people are often locked out of permanent, formal employment. We find that the job application workshop helps the least educated job-seekers to signal their skills, despite their limited employment experience. Thus, it creates a more level playing field between these young job applicants and more experienced individuals. We also find that, among control subjects, the likelihood of being wage employed falls with distance from the city centre and that the transport subsidy eliminates this difference. This suggest that the transport intervention equalises wage employment opportunities between job-seekers living near and far the centre of the city. We also document that, on average, untreated young jobseekers living in the proximity of treated ones are not negatively affected by the interventions.

[43] The optimal bandwidth selection is performed using the Stata command provided by Nichols (2007). The selected bandwidth differs across test outcomes and band cut-offs. For example, the optimal bandwidth for the aggregate test score at the 50th percentile is 0.62, where the median of the aggregate score is 4.843 with a standard deviation of 0.88. In cases where the selected bandwidth is larger than the reported test score band itself, we check that the results are robust to restricting the bandwidth to the range within the marks band.

[44] The full set of results, using optimal bandwidth and other selected bandwidths, is available on request.

[45] This is unlikely to be caused by poor understanding, as participants report having understood the information presented in the certificates well, and certificates use is correlated with test scores.

Reducing search frictions can lead to aggregate gains for the economy. Increased competition among workers can help employers either to find more suitable candidates, or to find them faster (Marimon and Zilibotti, 1999; Galenianos et al., 2011; Hsieh et al., 2013). Further, a better match between the skills that workers have and the skills that are required by their jobs may help reduce worker turnover – a key policy concern in Ethiopia and other developing countries (Blattman and Dercon, 2016). Our experiment is not directly aimed at estimating these aggregate welfare gains, partly because it is not possible to identify indirect treatment effects outside of well-defined study areas – a common challenge for all studies of this kind. However, we find evidence suggesting that overall efficiency in the labour market improves as a result of the interventions. In particular, we document that the job application workshop enables the labour market to better separate low and high skilled workers, removing information asymmetries that may lead firms to make the wrong hires. We also find that the transport subsidy improves the allocation of labour by giving workers who live out of town the opportunity to access salaried jobs, rather than having to fall back on self-employment. These findings suggest that job search assistance reduces labour market distortions and may produce aggregate efficiency gains.

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Figures and Tables

Figure 1: **Heterogeneous Impacts by Education: Permanent Work**

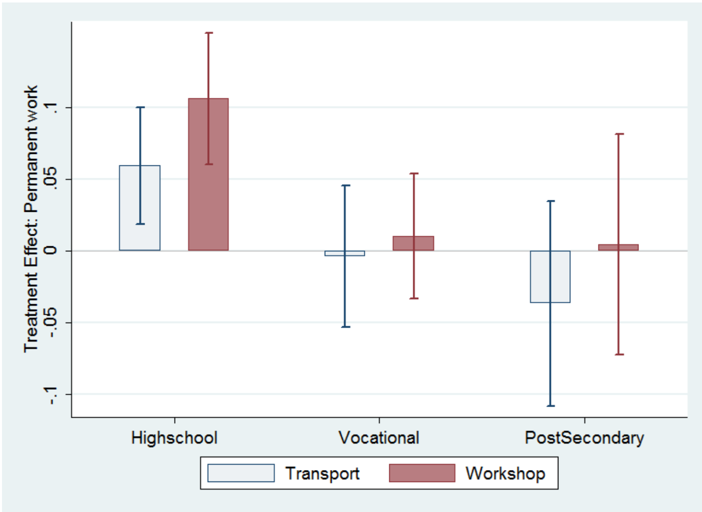


Figure 2: **Heterogeneous Impacts by Education: Formal Work**

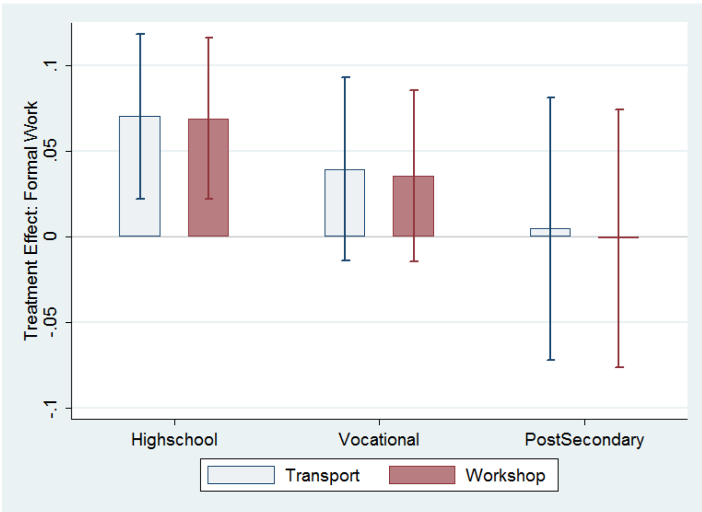


Figure 3: **Heterogeneous Impacts by Gender: Permanent Work**

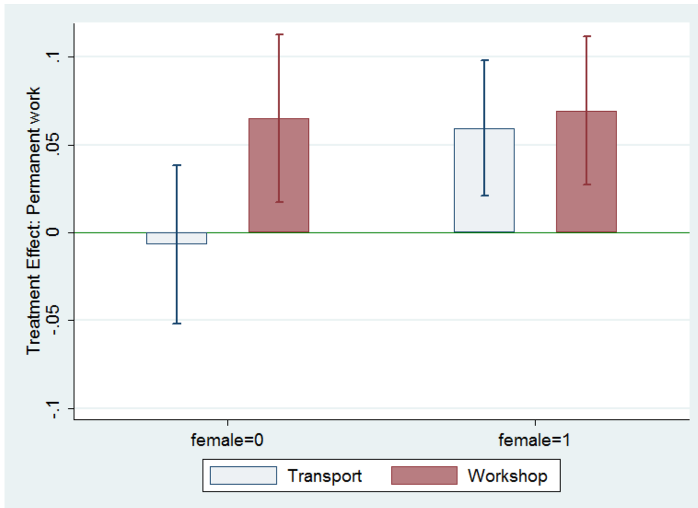


Figure 4: **Heterogeneous Impacts by Gender: Formal Work**

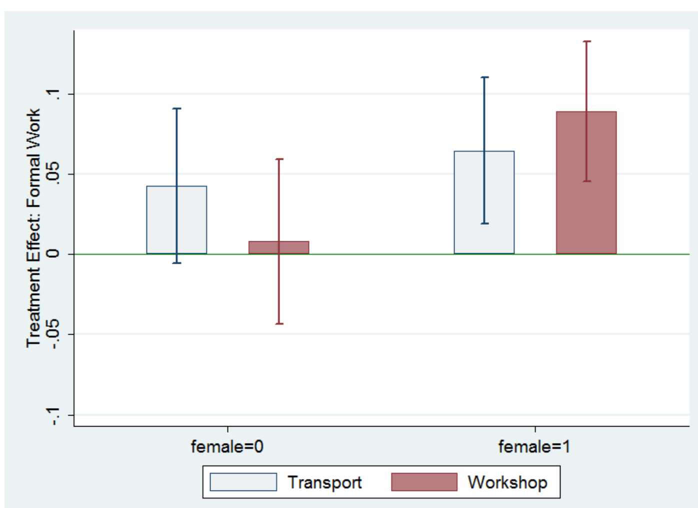
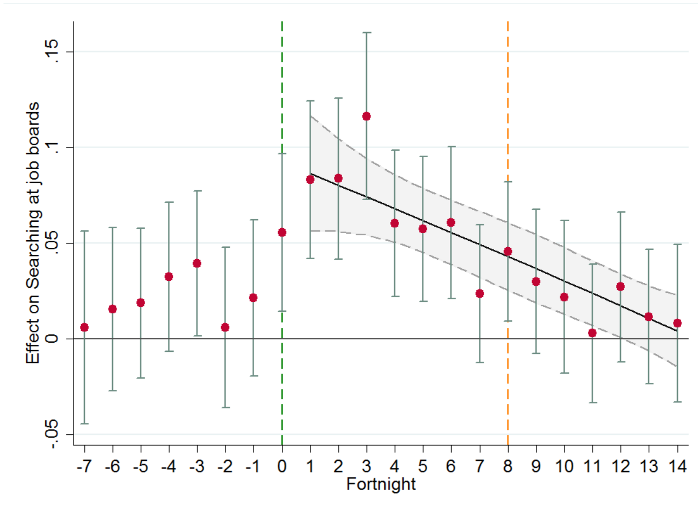
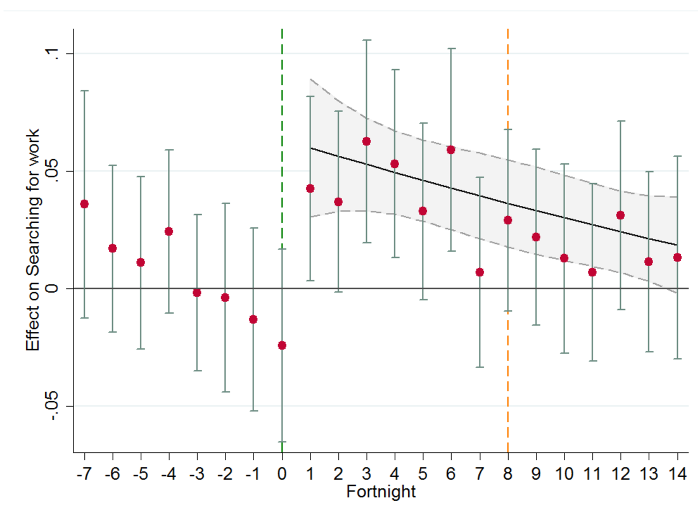


Figure 5: **Impact trajectory of the transport treatment: Searching at the job boards**



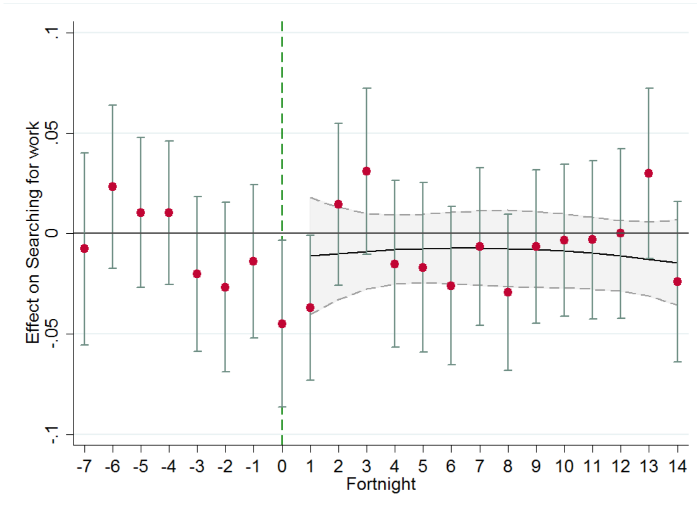
The green dotted line indicates the fortnight when the treatment begins.
The orange dotted line indicates the week when the week when the treatment ends.

Figure 6: **Impact trajectory of the transport treatment: Searching for work**



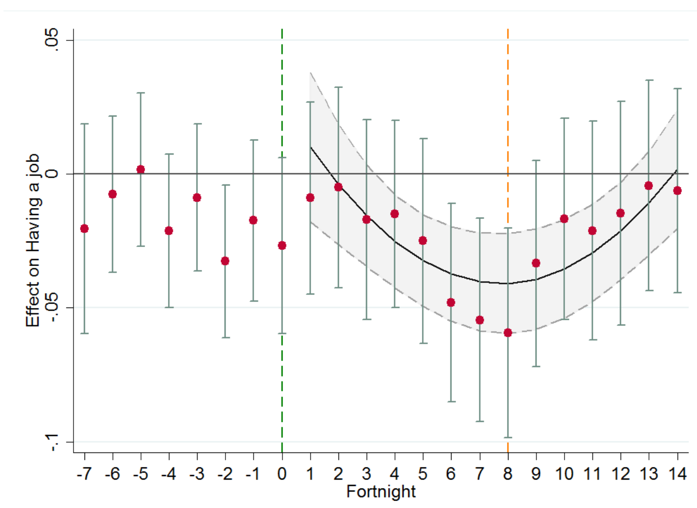
The green dotted line indicates the fortnight when the treatment begins.
The orange dotted line indicates the week when the week when the treatment ends.

Figure 7: **Impact trajectory of the application workshop: Searching for work**



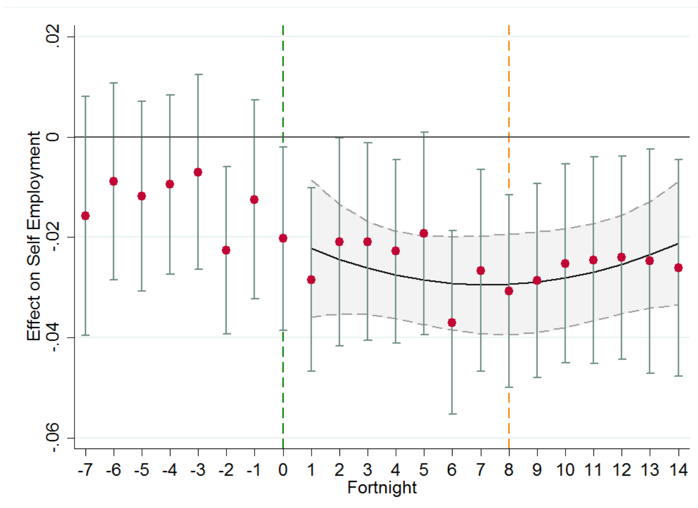
The green dotted line indicates the fortnight when the treatment begins.
The orange dotted line indicates the week when the week when the treatment ends.

Figure 8: **Impact trajectory of the transport treatment: Employment**



The green dotted line indicates the fortnight when the treatment begins.
The orange dotted line indicates the week when the week when the treatment ends.

Figure 9: **Impact trajectory of the transport treatment: Self-Employment**



The green dotted line indicates the fortnight when the treatment begins.
The orange dotted line indicates the week when the week when the treatment ends.

Figure 10: **Spillover effects of the transport treatment on Permanent Work (by randomized level of cluster saturation)**

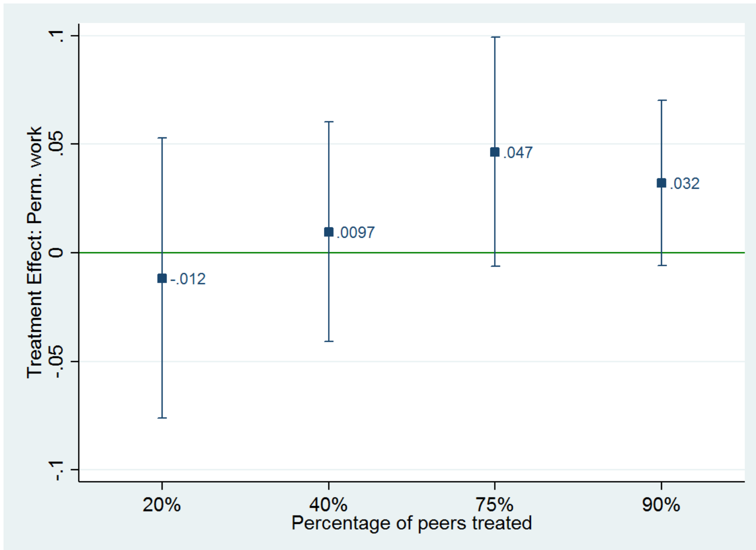


Figure 11: **Spillover effects of the transport treatment on Formal Work (by randomized level of cluster saturation)**

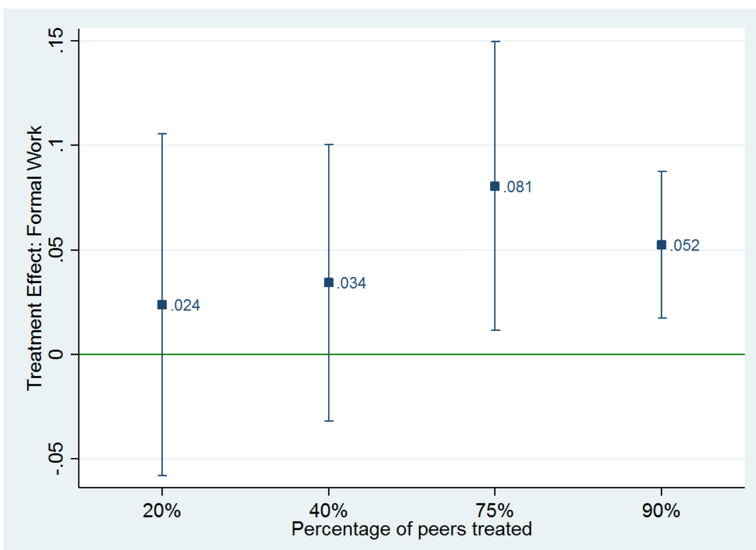


Figure 12: **Relationship between distance and self-employment: Impact of transport subsidies**

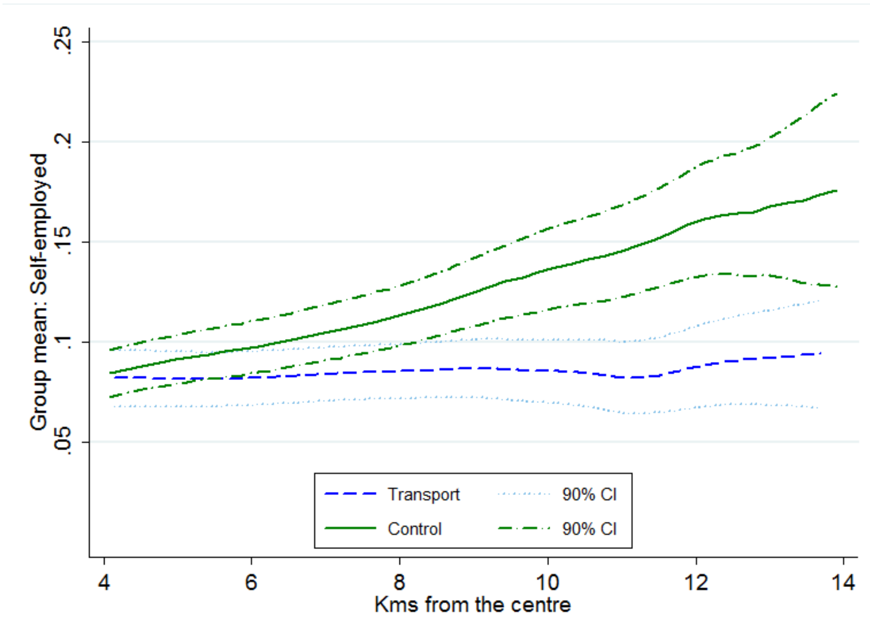


Figure 13: **Predicted grades and permanent work**
(job-seekers with at most a high school degree)

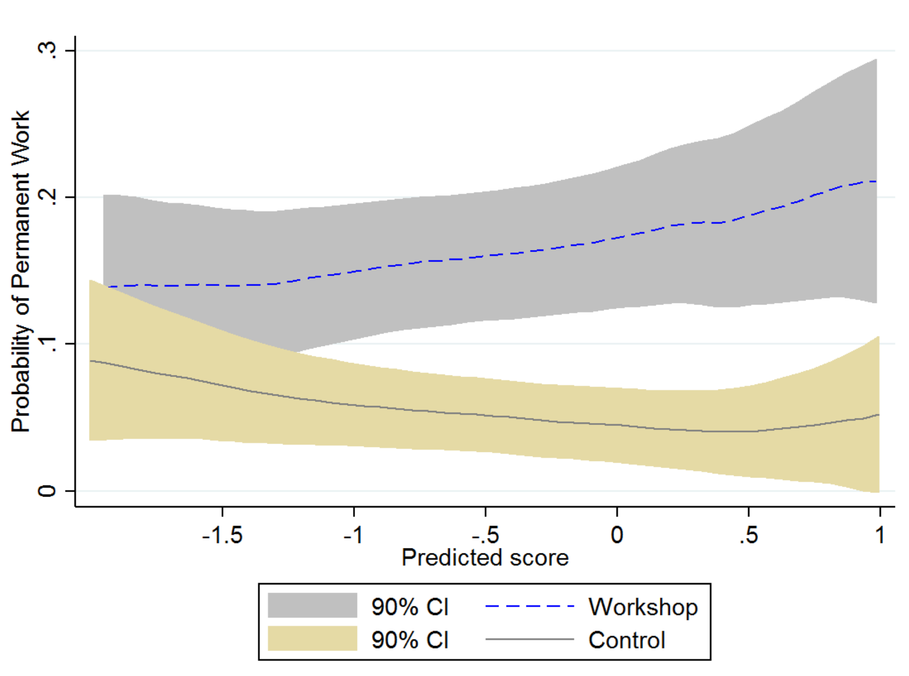


Table 1: **Treatment Assignment**

Transport clusters			
	Individuals		Clusters
Proportion Treated	Controls	Treated	
20%	256	65	18
40%	150	96	15
75%	56	191	15
90%	38	422	26
Total	**500**	**774**	**74**
Workshop clusters			
	Individuals		Clusters
Proportion Treated	Controls	Treated	
80%	187	768	56
Total	**187**	**768**	**56**
Control clusters			
	Individuals		Clusters
Proportion Treated	Controls	Treated	
0%	823	0	48
Total	**823**	**0**	**48**

1,007 additional individuals were assigned to a third treatment, which consisted of a series of job fairs (with a random sample of employers from Addis Ababa). This is a distinct intervention, which analyses both sides of the market, and constitutes the focus of a separate pape (Abebe et al., 2016).

Table 2: **Employment outcomes**

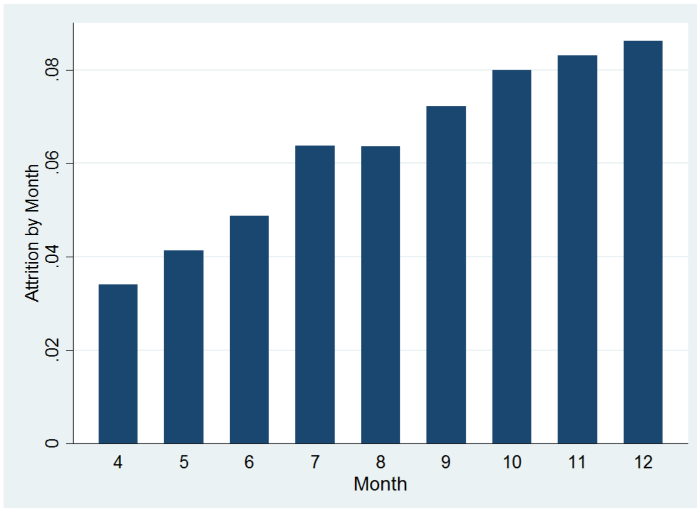
Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Worked	0.0400	0.0200	-0.0460	0.0320	0.562	0.515	3791
	(.171)	(.514)	(.178)	(.542)			
	[.331]	[1]	[1]	[1]			
Hours worked	0.159	-0.0780	-2.358	0.578	26.20	0.870	3784
	(.919)	(.96)	(.205)	(.821)			
	[.733]	[1]	[1]	[1]			
Formal work	0.0550	0.0530	0.0140	0.0600	0.224	0.932	3791
	(.004)***	(.007)***	(.49)	(.112)			
	[.029]**	[.022]**	[1]	[1]			
Perm. work	0.0340	0.0690	0.00700	0.0150	0.171	0.0877	3791
	(.063)*	(0)***	(.726)	(.566)			
	[.234]	[.003]***	[1]	[1]			
Self-employed	-0.0200	-0.00500	-0.0170	-0.0170	0.0950	0.351	3791
	(.199)	(.754)	(.381)	(.559)			
	[.331]	[1]	[1]	[1]			
Monthly earnings	1.251	61.52	-43.60	12.32	1145	0.425	3738
	(.987)	(.467)	(.628)	(.905)			
	[.733]	[1]	[1]	[1]			
Satis. with work	0.00100	0.0230	-0.0190	0.0500	0.237	0.488	3791
	(.967)	(.4)	(.44)	(.296)			
	[.733]	[1]	[1]	[1]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on primary employment outcomes. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). In the last three columns we report the mean outcome for the control group, the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***$p<$ 0.01, **p<0.05, *p<0.1.

Online Appendix

Additional Figures and Tables

Figure 14: **Attrition rate from the Phone Survey by Month**



Note. Attrition is defined as failure to complete one interview.

Figure 15: **Most Common Occupations**

(a) **Transport Subsidy** (b) **Job Application Workshop**

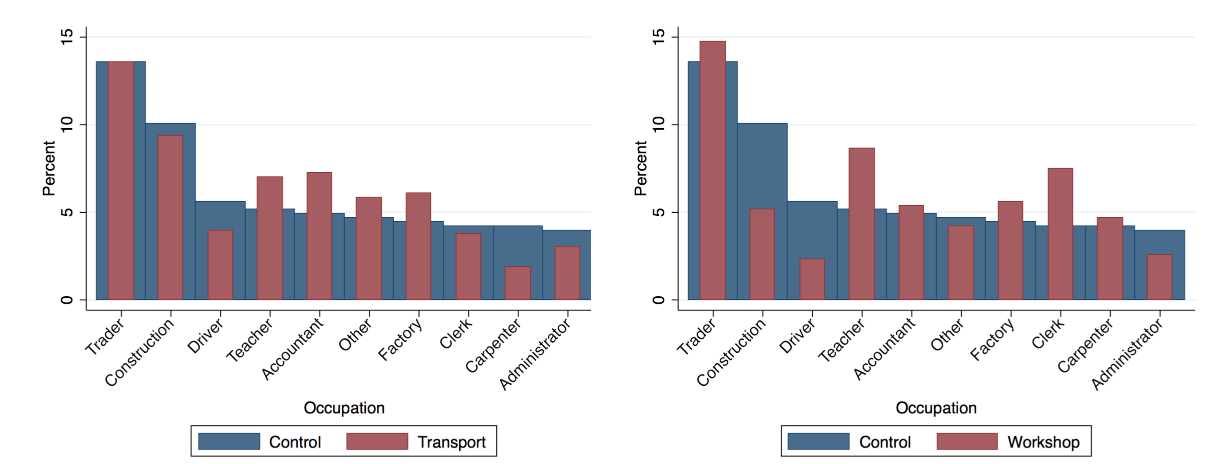


Figure 16: **Heterogeneous Impacts by Education: Reservation wages**

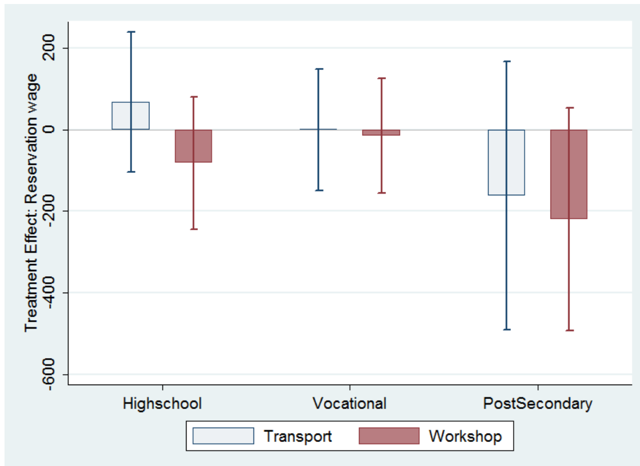


Figure 17: **Relationship between distance and self-employment: Impact of the job application workshop**

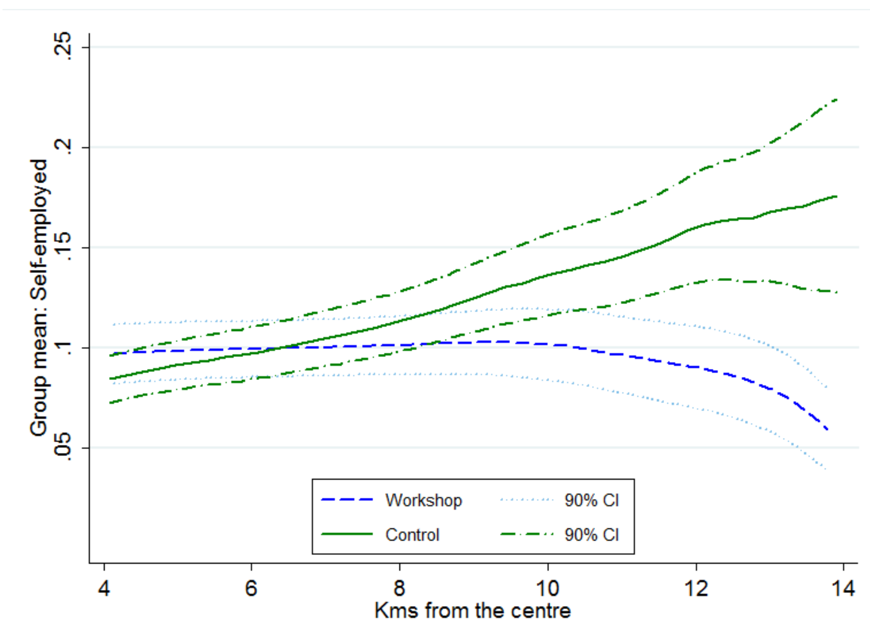


Figure 18: **Aggregate Test Scores and Permanent Work: Impact of scoring above the median**

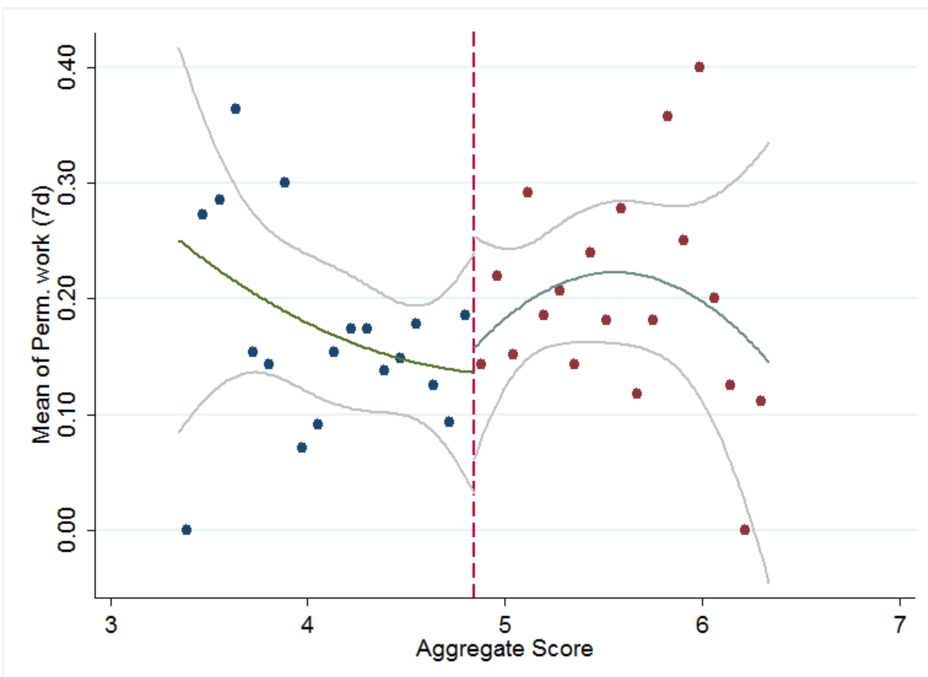


Table 3: **Sample selection before randomisation**

	Sample Size	No. Dropped	% dropped
Eligible at baseline	4388		
Found on phone	4314	74	1.69%
Stayed in phone survey	4254	60	1.39%
Without permanent work	4076	178	4.18%
Stayed in Addis	4059	17	0.42%
Total Dropped		329	7.58%
Final Sample	4059		

Table 4: **Assignment to Start and End Weeks of the Transport Intervention**

	End Week (2014-2015)						
Start Week (2014)	22-Dec	29-Dec	05-Jan	12-Jan	19-Jan	26-Jan	Total
01-Sep	12	11	14	13	0	0	50
08-Sep	12	21	38	29	0	0	100
15-Sep	6	10	12	22	0	0	50
22-Sep	10	15	27	24	0	0	76
29-Sep	16	23	29	78	25	29	200
06-Oct	0	0	0	53	51	46	150
13-Oct	0	0	0	59	44	45	148
Total	56	80	120	278	120	120	774

Table 5: **Summary and Tests of Balance**

	N	Mean	S.Dev.	1st Q.	Median	3rd Q.	Min.	Max.	F-test P
degree	4055	0.18	0.39	0.00	0.00	0.00	0.0	1.0	0.654
vocational	4055	0.43	0.50	0.00	0.00	1.00	0.0	1.0	0.914
work	4055	0.30	0.46	0.00	0.00	1.00	0.0	1.0	0.710
search	4055	0.50	0.50	0.00	0.00	1.00	0.0	1.0	0.950
dipdeg	4055	0.25	0.43	0.00	0.00	1.00	0.0	1.0	0.933
female	4055	0.53	0.50	0.00	1.00	1.00	0.0	1.0	0.996
migrant_birth	4055	0.36	0.48	0.00	0.00	1.00	0.0	1.0	0.684
amhara	4055	0.44	0.50	0.00	0.00	1.00	0.0	1.0	0.366
oromo	4055	0.25	0.44	0.00	0.00	1.00	0.0	1.0	0.278
work_wage_6months	4055	0.45	0.50	0.00	0.00	1.00	0.0	1.0	0.452
married	4055	0.20	0.40	0.00	0.00	0.00	0.0	1.0	0.462
live_parents	4055	0.53	0.50	0.00	1.00	1.00	0.0	1.0	0.779
experience_perm	4055	0.13	0.33	0.00	0.00	0.00	0.0	1.0	0.430
search_6months	4055	0.75	0.43	0.00	1.00	1.00	0.0	1.0	0.565
respondent_age	4055	23.53	3.00	21.00	23.00	26.00	18.0	29.0	0.678
years_since_school	4050	38.35	259.95	1.00	3.00	5.00	0.0	1984.0	0.316
search_freq	4055	0.57	0.32	0.33	0.60	0.83	0.0	1.0	0.760
work_freq	4055	0.34	0.38	0.00	0.20	0.67	0.0	1.0	0.823
self_employed	4055	0.05	0.22	0.00	0.00	0.00	0.0	1.0	0.539
work_cas	4055	0.05	0.21	0.00	0.00	0.00	0.0	1.0	0.291
work_satisfaction	4055	0.09	0.28	0.00	0.00	0.00	0.0	1.0	0.496
total_savings	4055	2441.42	7232.39	0.00	0.00	2200.00	0.0	202998.0	0.320
res_wage	4014	1361.68	1072.74	800.00	1100.00	1700.00	0.0	20000.0	0.786
cent_dist	4055	5.91	2.57	3.76	5.31	7.57	2.5	12.7	0.269
travel	4050	1.90	2.10	0.00	1.00	3.00	0.0	18.0	0.391
formal_job	4055	0.07	0.26	0.00	0.00	0.00	0.0	1.0	0.311
cv_application	4055	0.28	0.45	0.00	0.00	1.00	0.0	1.0	0.701
expect_offer	3812	1.44	2.13	0.00	1.00	2.00	0.0	60.0	0.084
aspiration	3803	5820.25	5778.53	3000.00	5000.00	6000.00	0.0	99999.0	0.793
network_size	4016	6.79	9.44	3.00	4.00	8.00	0.0	99.0	0.561
respondent_age	4055	23.53	3.00	21.00	23.00	26.00	18.0	29.0	0.678
present_bias	2768	0.13	0.34	0.00	0.00	0.00	0.0	1.0	0.563
future_bias	2768	0.07	0.25	0.00	0.00	0.00	0.0	1.0	0.186
life_satisfaction	4051	4.16	1.87	3.00	4.00	5.00	0.0	10.0	0.944

Table 6: **Variables Used for Re-Randomisation**

VARIABLE	DEFINITION	SOURCE (QUESTION NUMBER)
`degree`	Dummy: Individual has finished a degree (bachelors or above) at a recognised university	Dummy: b5=20 or b5=21
`vocational`	Dummy: Individual has finished a course or vocational training at an official vocational college or TVET	Dummy: b5 $\in \{9, \ldots, 16\}$
`work`	Individual has had any work for pay in the last 7 days	Dummy: j1_1 = 1
`search`	Individual has taken any active step to find work in the last 7 days	Dummy: s0_2 = 1
`post_secondary`	Individual has any kind of non-vocational post-secondary education (degree or diploma)	Dummy: b5 $\in \{17, \ldots, 21\}$.
`female`	Respondent is female	Dummy: respondent_gender = 2
`migrant_birth`	Respondent was born outside of Addis Ababa and migrated since birth	Dummy: b14!=10
`amhara`	Respondent is ethnically Amhara	Dummy: b21=1
`oromo`	Respondent is ethnically Oromo	Dummy: b21=2
`work_wage_6months`	Individual has worked for a wage at any point in the last 6 months	Dummy: j2_1 =1
`married`	Individual is married	Dummy: b1 = 1
`live_parents`	Respondents lives with his/her mother or father	Dummy: b22= 3 or b22= 4
`experience_perm`	Respondent has work experience at a permanent job	Dummy: b22= 3 or b22=4
`search_6months`	Respondent has searched for work any time in the last 6 months	Dummy: s0_1 = 1
`age`	Respondent age	respondent_age
`years_since_school`	Years since the respondent finished school (any school including university)	Constructed from j0_3 ($= 2006 - j0\_3$)
`search_freq`	Proportion of weeks that individual searched for work (from the phone surveys)	Mean (over first 3 months of calls) of Dummy: $p1\_14 = 1$
`work_freq`	Proportion of weeks that the individuals worked (from the phone surveys)	Mean (over first 3 months of calls) of Dummy: $p1\_3 \neq 0$

Table 7: **Predictors of Attrition**

Transport	-0.005	Respondent age	-0.000
	(0.017)		(0.0022)
Screening	-0.023	Born outside Addis	0.040***
	(0.017)		(0.014)
Spillover transport	-0.010	Amhara	-0.024
	(0.019)		(0.015)
Spillover screening	-0.014	Oromo	-0.026
	(0.026)		(0.017)
search freq	-0.064**	Wage empl (6m)	0.011
	(0.026)		(0.015)
work freq	-0.004	Married	-0.028
	(0.018)		(0.018)
Degree	-0.034***	Years since school	0.000
	(0.012)		(0.000029)
Worked (7d)	-0.044***	Lives with parents	-0.004
	(0.015)		(0.014)
Searched job (7d)	0.021	Ever had permanent job	0.003
	(0.016)		(0.018)
Female	0.022*	Searched job (6m)	-0.007
	(0.013)		(0.018)
Observations	3,045	R-squared	0.021
F-test (treatments)	0.560	F-test (covariates)	2.680
Prob > F	0.690	Prob > F	0.000

Table 8: **Lee bounds**

		Transport	Workshop
Permanent work	Upper bound	0.032	0.068
		(0.076)*	(0.000)***
	Lower bound	0.021	0.043
		(0.339)	(0.047)**
Formal Work	Upper bound	0.061	0.061
		(0.004)***	(0.004)***
	Lower bound	0.050	0.036
		(0.035)**	(0.130)

Note. In this table we report the Lee bounds for the estimates of effects of the transport intervention and the job application workshop on permanent work and formal work. ***p< 0.01, **p<0.05, *p<0.1.

Table 9: **Effects on main outcomes by education**

	Transport				Job Application Workshop				Control Mean			
Outcome	Hi Sch.	Voc.	Dip/Deg	*F(p)*	Hi Sch.	Voc.	Dip/Deg	*F(p)*	Hi Sch.	Voc.	Dip/Deg	N
Worked	0.0520	0.0400	-0.0220	0.505	-0.00700	0.0630	0.0340	0.397	0.508	0.564	0.624	3791
	(.232)	(.355)	(.636)		(.871)	(.079)*	(.467)					
	[1]	[1]	[1]		[1]	[.555]	[1]					
Hours worked	0.486	-0.506	-1.062	0.896	-2.204	2.128	3.331	0.211	24.80	27.10	26.30	3784
	(.84)	(.802)	(.641)		(.351)	(.247)	(.151)					
	[1]	[1]	[1]		[.989]	[.792]	[.792]					
Formal work	0.0700	0.0390	0.00500	0.525	0.0690	0.0360	-0.00100	0.407	0.108	0.216	0.376	3791
	(.017)**	(.228)	(.921)		(.016)**	(.243)	(.977)					
	[.218]	[1]	[1]		[.19]	[.792]	[1]					
Perm. work	0.0590	-0.00400	-0.0370	0.0770	0.106	0.0100	0.00400	0.0254	0.0583	0.169	0.310	3791
	(.016)**	(.899)	(.395)		(0)***	(.696)	(.927)					
	[.218]	[1]	[1]		[.003]***	[1]	[1]					
Self-employed	-0.0340	0.00600	-0.00600	0.520	-0.0320	0.0260	0.0640	0.0405	0.112	0.0878	0.0863	3791
	(.18)	(.777)	(.837)		(.187)	(.293)	(.048)**					
	[1]	[1]	[1]		[.792]	[.827]	[.432]					
Monthly earnings	91.70	-119.6	-166.4	0.254	70.65	66.32	-9.107	0.959	780	1057	1733	3738
	(.347)	(.243)	(.501)		(.531)	(.587)	(.972)					
	[1]	[1]	[1]		[1]	[1]	[1]					
Satis. with work	0.00800	-0.0240	-0.00700	0.774	0.00200	0.0430	0.0560	0.541	0.225	0.238	0.249	3791
	(.832)	(.509)	(.876)		(.954)	(.26)	(.214)					
	[1]	[1]	[1]		[1]	[.792]	[.792]					

Note. In this table we report, separately for each education category, the *intent-to-treat* estimates of the direct of the transport intervention and the job application workshop on primary employment outcomes. These are obtained by least squares estimation of equation (2). Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In columns 3 and 6 we report the p-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for individuals with different levels of education. In the last three columns we report the mean outcome in the control group for the different education categories.***p< 0.01, **p<0.05, *p<0.1.

Table 10: **Effects on main outcomes by gender**

	Transport			Job Application Workshop			Control Mean		
Outcome	Male	Female	*F(p)*	Male	Female	*F(p)*	Male	Female	N
Worked	0.0570	0.0250	0.579	-0.00200	0.0350	0.519	0.642	0.487	3791
	(.164)	(.552)		(.959)	(.404)				
	[.971]	[1]		[1]	[1]				
Hours worked	-0.284	0.251	0.871	-0.685	0.122	0.803	28.80	23.80	3784
	(.902)	(.912)		(.752)	(.958)				
	[1]	[1]		[1]	[1]				
Formal work	0.0420	0.0650	0.603	0.00800	0.0890	0.0535	0.246	0.203	3791
	(.147)	(.021)**		(.802)	(.001)***				
	[.971]	[.169]		[1]	[.013]**				
Perm. work	-0.00700	0.0590	0.0574	0.0650	0.0690	0.912	0.189	0.154	3791
	(.802)	(.012)**		(.025)**	(.007)***				
	[1]	[.169]		[.112]	[.049]**				
Self-employed	-0.0220	-0.0170	0.889	-0.00500	-0.00300	0.942	0.109	0.0821	3791
	(.449)	(.384)		(.862)	(.9)				
	[1]	[1]		[1]	[1]				
Monthly earnings	-22.80	18.41	0.794	74.26	52.17	0.901	1521	794	3738
	(.872)	(.793)		(.631)	(.544)				
	[1]	[1]		[1]	[1]				
Satis. with work	-0.0260	0.0140	0.435	0.0180	0.0230	0.936	0.287	0.190	3791
	(.519)	(.677)		(.679)	(.52)				
	[1]	[1]		[1]	[1]				

Note. In this table we report, separately for each gender, the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on primary employment outcomes. These are obtained by least squares estimation of equation (2), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In columns 3 and 6 we report the p-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for men and women. In the last two columns we report the mean outcome for men and women in the control group.***p< 0.01, **p<0.05, *p<0.1.

Table 11: **Family indices**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Job Quality	0.564	0.495	-0.174	0.811	-0.220	0.912	3791
	(.318)	(.433)	(.815)	(.456)			
	[1]	[1]	[1]	[1]			
Finan. Outcomes	0.177	0.145	0.0910	-0.0160	-0.188	0.889	3791
	(.459)	(.494)	(.725)	(.957)			
	[1]	[1]	[1]	[1]			
Expects and Asps	-0.0760	0.106	-1.051	-0.348	0.214	0.775	2814
	(.914)	(.857)	(.083)*	(.678)			
	[1]	[1]	[1]	[1]			
Mobility	0.0990	-0.0690	-0.434	-0.802	0.145	0.796	3785
	(.871)	(.92)	(.564)	(.322)			
	[1]	[1]	[1]	[1]			
Education/Skills	-0.800	-1.165	0.0640	-1.057	0.391	0.593	3791
	(.231)	(.126)	(.935)	(.296)			
	[1]	[1]	[1]	[1]			
Wellbeing	0.0570	0.193	0.0350	0.115	-0.0650	0.433	3787
	(.731)	(.216)	(.847)	(.612)			
	[1]	[1]	[1]	[1]			
Networks	-0.296	-0.353	-0.488	-0.221	0.111	0.872	3767
	(.38)	(.329)	(.2)	(.615)			
	[1]	[1]	[1]	[1]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on the summary indices for different families of outcomes. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In the last three columns we report the mean outcome for the control group, the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***p< 0.01, **p<0.05, *p<0.1.

Table 12: **Other job quality measures**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Received job by interview	0.0400 (.009)*** [.049]**	0.0440 (.016)** [.085]*	0.0250 (.236) [1]	0.0720 (.023)** [.133]	0.167	0.854	3791
Office work (7d)	0.0250 (.303) [.673]	0.00100 (.982) [1]	-0.0190 (.47) [1]	0.00500 (.889) [1]	0.201	0.289	3791
Skills match with tasks	0.00900 (.759) [1]	0.00500 (.873) [1]	0.0310 (.372) [1]	0 (.992) [1]	0.130	0.871	3791
Overqualified	0.0370 (.288) [.673]	0.0310 (.371) [1]	-0.0390 (.295) [1]	0.0610 (.228) [.839]	0.291	0.835	3791
Underqualified	-0.0160 (.402) [.673]	-0.0120 (.549) [1]	-0.0130 (.57) [1]	-0.0190 (.435) [1]	0.0820	0.786	3791

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on secondary employment outcomes. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In the last three columns we report the mean outcome for the control group, the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***p< 0.01, **p<0.05, *p<0.1.

Table 13: **Financial outcomes**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Expenditure (7d)	25.60	16.80	-8.186	-61.15	506.4	0.828	3791
	(.515)	(.664)	(.836)	(.139)			
	[1]	[1]	[1]	[.718]			
Savings (total)	513.2	-772.9	-466.8	110.4	6907	0.615	1694
	(.849)	(.564)	(.746)	(.947)			
	[1]	[1]	[1]	[1]			
	0.460	0.216	0.426	0.501	-0.404	0.645	3791
	(.402)	(.658)	(.506)	(.523)			
	[1]	[1]	[1]	[1]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on financial outcomes. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In the last three columns we report the mean outcome for the control group, the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***p< 0.01, **p<0.05, *p<0.1.

Table 14: **Expectations, aspirations, reservation wages**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Offers expected (next 4m)	-0.00600	0.267	-0.164	-0.195	1.421	0.0820	3527
	(.968)	(.079)*	(.268)	(.16)			
	[1]	[.346]	[.674]	[.326]			
Reservation wage	2.917	-95.30	-13.74	144.1	2023	0.275	3324
	(.972)	(.192)	(.877)	(.188)			
	[1]	[.346]	[1]	[.326]			
Aspiration wage (in 5y)	626.5	673.2	433.1	990.3	7097	0.958	3452
	(.367)	(.404)	(.523)	(.202)			
	[1]	[.346]	[1]	[.326]			
Weeks expected to be without permanent job	2.062	-4.513	-9.549	-5.421	30.23	0.0874	1812
	(.632)	(.179)	(.003)***	(.246)			
	[1]	[.346]	[.011]**	[.326]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on expectations, aspirations and reservation wages. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In the last three columns we report the mean outcome for the control group, the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***p< 0.01, **p<0.05, *p<0.1.

Table 15: **Mobility**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Trip to center (7d)	0.126	-0.0450	-0.147	-0.273	2.372	0.353	3352
	(.463)	(.806)	(.405)	(.235)			
	[1]	[.655]	[1]	[1]			
Works away from home	-0.0340	-0.0390	-0.0400	-0.0430	0.851	0.838	3791
	(.202)	(.14)	(.155)	(.261)			
	[1]	[.542]	[1]	[1]			
Location of main occupation/activity changed	0.0270	-0.0330	0.0220	-0.0320	0.269	0.101	3791
	(.498)	(.396)	(.628)	(.474)			
	[1]	[.542]	[1]	[1]			
Moved within Addis	-0.00200	0.0240	0.00700	0.00900	0.0820	0.196	3791
	(.932)	(.237)	(.777)	(.752)			
	[1]	[.542]	[1]	[1]			
Moved outside of Addis	0.00900	0.0120	0.00300	0.00300	0.00700	0.749	3791
	(.151)	(.078)*	(.67)	(.661)			
	[1]	[.542]	[1]	[1]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on outcomes related to mobility. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In the last three columns we report the mean outcome for the control group, the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***p< 0.01, **p<0.05, *p<0.1.

Table 16: **Education and training**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
In full-time education	-0.00700	0	0.00300	0.0330	0.0210	0.381	3791
	(.376)	(.962)	(.791)	(.128)			
	[.697]	[1]	[1]	[.207]			
In part-time education	-0.0470	-0.0330	-0.0130	-0.0200	0.142	0.462	3791
	(.019)**	(.141)	(.62)	(.516)			
	[.128]	[.603]	[1]	[.449]			
In informal training	-0.0100	-0.0100	-0.00700	-0.0410	0.0380	0.988	3791
	(.513)	(.494)	(.656)	(.001)***			
	[.697]	[.738]	[1]	[.009]***			
Graduated (in past 12m)	0.0120	-0.0120	0.0160	-0.0190	0.0850	0.136	3791
	(.488)	(.44)	(.436)	(.394)			
	[.697]	[.738]	[1]	[.42]			
Graduated from vocational degree (in past 12m)	0.0160	0.00600	0.00500	0.00200	0.0220	0.364	3791
	(.127)	(.522)	(.651)	(.892)			
	[.465]	[.738]	[1]	[.805]			
Graduated from training (in past 12m)	-0.00100	-0.0220	0.0190	-0.0280	0.0480	0.0954	3791
	(.932)	(.063)*	(.237)	(.024)**			
	[1]	[.603]	[1]	[.065]*			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on education and training. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In the last three columns we report the mean outcome for the control group, the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***p< 0.01, **p<0.05, *p<0.1.

Table 17: **Psychological outcomes**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Life satisfaction (0-10)	0.177	0.160	0.206	0.338	4.798	0.904	3356
	(.182)	(.235)	(.177)	(.134)			
	[1]	[1]	[1]	[1]			
Locus of control (0-10)	0.0120	-0.0320	-0.162	-0.00800	6.207	0.884	3358
	(.969)	(.912)	(.628)	(.981)			
	[1]	[1]	[1]	[1]			
Oneness with society	-0.0380	0.0400	-0.0260	0.113	4.738	0.550	3358
	(.787)	(.775)	(.856)	(.544)			
	[1]	[1]	[1]	[1]			
Trust in other people (1-4)	0.0790	0.0470	0.0260	-0.0250	2.027	0.717	3357
	(.333)	(.608)	(.766)	(.81)			
	[1]	[1]	[1]	[1]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on psychological outcomes. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In the last three columns we report the mean outcome for the control group, the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***$p< 0.01$, **$p<0.05$, *$p<0.1$.

Table 18: **Social networks**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
No. people with whom regularly shares info about job opport.	-0.372	-0.593	-0.637	-0.446	5.242	0.542	3754
	(.342)	(.112)	(.108)	(.355)			
	[1]	[.733]	[.758]	[.98]			
Number of people with permanent jobs in job network	0.123	0.120	-0.0600	0.390	2.440	0.989	3388
	(.561)	(.61)	(.806)	(.201)			
	[1]	[.733]	[1]	[.98]			
Can access guarantor for job (in next month)	-0.00500	-0.0670	-0.0260	-0.00400	1.220	0.228	3355
	(.92)	(.211)	(.66)	(.948)			
	[1]	[.733]	[1]	[.98]			
No. meetings of voluntary associations attended (past 30d)	0.00900	0.00700	-0.0330	-0.0550	0.0970	0.976	3791
	(.889)	(.91)	(.636)	(.371)			
	[1]	[.836]	[1]	[.98]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on social networks. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In the last three columns we report the mean outcome for the control group, the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***$p< 0.01$, **$p<0.05$, *$p<0.1$.

Table 19: **Effects on employment outcomes by job search at baseline**

	Transport			Job Application Workshop			Control Mean		
Outcome	Lo Search Intens.	Hi Search Intens.	*F(p)*	Lo Search Intens.	Hi Search Intens.	*F(p)*	Lo Search Intens.	Hi Search Intens.	N
Worked	0.0360 (.401) [1]	0.0400 (.353) [1]	0.945	0.0520 (.239) [.695]	-0.0130 (.77) [.916]	0.292	0.554	0.569	3791
Hours worked	-1.384 (.57) [1]	1.262 (.585) [1]	0.452	0.144 (.954) [.916]	-0.556 (.798) [.916]	0.839	26.60	25.90	3784
Formal work	0.0740 (.012)** [.201]	0.0340 (.23) [1]	0.353	0.0650 (.031)** [.257]	0.0390 (.206) [.695]	0.561	0.203	0.241	3791
Perm. work	0.0410 (.12) [1]	0.0190 (.469) [1]	0.572	0.0950 (.002)*** [.026]**	0.0420 (.118) [.642]	0.209	0.157	0.182	3791
Self-employed	-0.00600 (.809) [1]	-0.0300 (.155) [1]	0.455	-0.0100 (.707) [.916]	0.00200 (.92) [.916]	0.723	0.110	0.0827	3791
Monthly earnings	70.93 (.518) [1]	-62.63 (.522) [1]	0.352	183.7 (.163) [.642]	-47.86 (.679) [.916]	0.205	1103	1181	3738
Satis. with work	0.0240 (.524) [1]	-0.0290 (.425) [1]	0.310	0.0550 (.158) [.642]	-0.0110 (.756) [.916]	0.206	0.229	0.243	3791

Note. In this table we report heterogeneous treatment effects for individuals that were searching above and below the median number of weeks in the three months after being surveyed and before the start of the interventions. For each group, we report the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by least squares estimation of equation (2). Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). In columns 3 and 6 we report the p-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for individuals with different levels of education. In the last three columns we report the mean outcome in the control group for the different education categories. ***p< 0.01, **p<0.05, *p<0.1.

Table 20: **Effects on employment outcomes by experience in permanent employment**

	Transport			Job Application Workshop			Control Mean		
Outcome	Never Perm. Job	Had Perm. Job	*F(p)*	never Perm. Job	Had Perm. Job	*F(p)*	Never Perm. Job	Had Perm. Job	N
Worked	0.0410 (.194) [.91]	0.0290 (.693) [1]	0.889	0.0210 (.533) [1]	0 (1) [1]	0.803	0.544	0.688	3791
Hours worked	-0.377 (.823) [1]	3.571 (.395) [1]	0.372	-0.395 (.815) [1]	1.271 (.765) [1]	0.717	25.60	30.40	3784
Formal work	0.0590 (.004)*** [.059]*	0.00700 (.925) [1]	0.486	0.0540 (.009)*** [.061]*	0.0320 (.661) [1]	0.765	0.202	0.375	3791
Perm. work	0.0370 (.065)* [.732]	-0.0290 (.675) [1]	0.377	0.0720 (0)*** [.004]***	0.0320 (.674) [1]	0.611	0.150	0.313	3791
Self-employed	-0.0230 (.198) [.91]	0.0120 (.778) [1]	0.472	-0.0110 (.569) [1]	0.0560 (.272) [1]	0.231	0.0985	0.0729	3791
Monthly earnings	-43.41 (.565) [1]	389.3 (.133) [.91]	0.100	73.27 (.396) [1]	-40.07 (.86) [1]	0.628	1074	1639	3738
Satis. with work	-0.00400 (.895) [1]	-0.00100 (.994) [1]	0.969	0.0210 (.473) [1]	0.0210 (.792) [1]	0.999	0.224	0.323	3791

Note. In this table we report heterogenous treatment effects for individuals with and without experience in permanent employment. For each group, we report the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by least squares estimation of equation (2). Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). In columns 3 and 6 we report the p-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for individuals with different levels of education. In the last three columns we report the mean outcome in the control group for the different education categories. ***p< 0.01, **p<0.05, *p<0.1.

Table 21: **Effects on employment outcomes by savings**

	Transport			Job Application Workshop			Control Mean		
Outcome	Below Med.	Above Med.	*F(p)*	Below Med.	Above Med.	*F(p)*	Below Med.	Above Med.	N
Worked	0.113 (.054)* [.439]	0.0160 (.62) [.678]	0.119	0.100 (.107) [.24]	-0.0100 (.768) [.787]	0.109	0.554	0.565	3791
Hours worked	4.858 (.12) [.452]	-1.535 (.38) [.546]	0.0598	3.700 (.252) [.337]	-1.598 (.355) [.462]	0.138	24.70	26.70	3784
Formal work	0.0970 (.015)** [.277]	0.0400 (.084)* [.439]	0.245	0.0760 (.071)* [.229]	0.0430 (.055)* [.219]	0.488	0.251	0.214	3791
Perm. work	0.0500 (.18) [.489]	0.0240 (.225) [.489]	0.515	0.0490 (.199) [.333]	0.0730 (.001)*** [.013]**	0.572	0.195	0.162	3791
Self-employed	-0.0230 (.56) [.678]	-0.0180 (.323) [.546]	0.920	0.00300 (.944) [.787]	-0.00600 (.774) [.787]	0.856	0.0872	0.0980	3791
Monthly earnings	243.0 (.094)* [.439]	-76.91 (.392) [.546]	0.0663	296.9 (.041)** [.214]	-18.13 (.856) [.787]	0.0743	966	1207	3738
Satis. with work	0.0290 (.577) [.678]	-0.0140 (.629) [.678]	0.453	0.106 (.031)** [.214]	-0.00900 (.772) [.787]	0.0385	0.205	0.248	3791

Note. In this table we report heterogenous treatment effects for individual with baseline savings above and below the median. For each group, we report the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by least squares estimation of equation (2). Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). In columns 3 and 6 we report the p-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for individuals with different levels of education. In the last three columns we report the mean outcome in the control group for the different education categories. ***$p < 0.01$, **$p<0.05$, *$p<0.1$.

Table 22: **Job search**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Applied to temporary jobs	0.347	-0.0100	0.0160	-0.167	1.311	0.139	3775
	(.195)	(.96)	(.951)	(.483)			
	[.995]	[.967]	[1]	[1]			
Applied to permanent jobs	-0.0240	0.0430	0.0540	0.0270	2.279	0.727	3770
	(.925)	(.859)	(.853)	(.929)			
	[.995]	[.967]	[1]	[1]			
Interviews/Applications	-0.0360	-0.0370	0.0340	-0.0140	0.354	0.970	2143
	(.229)	(.172)	(.481)	(.791)			
	[.995]	[.755]	[1]	[1]			
Offers/Applications	0.00400	0.00100	-0.0150	0.0730	0.248	0.927	2144
	(.92)	(.983)	(.716)	(.278)			
	[.995]	[.967]	[1]	[1]			
Interviews/Applications (Perm)	0.00700	0.0110	0.00600	-0.0200	0.327	0.909	1662
	(.857)	(.763)	(.899)	(.717)			
	[.995]	[.967]	[1]	[1]			
Offers/Applications (Perm)	0.0460	0.0470	0.00800	0.0530	0.164	0.980	1661
	(.22)	(.168)	(.825)	(.301)			
	[.995]	[.755]	[1]	[1]			
Interviews/Applications (Temp)	-0.0740	-0.0630	0.0320	-0.0220	0.389	0.777	1318
	(.072)*	(.125)	(.682)	(.754)			
	[.995]	[.755]	[1]	[1]			
Offers/Applications (Temp)	-0.0510	-0.0420	-0.0260	0.110	0.332	0.824	1318
	(.249)	(.361)	(.646)	(.24)			
	[.995]	[.755]	[1]	[1]			
Uses CV for applications	0.0160	0.0440	0.0190	-0.00400	0.401	0.286	3791
	(.595)	(.135)	(.566)	(.915)			
	[.995]	[.755]	[1]	[1]			
Uses certificates	0.0340	0.0540	0.0240	0.0280	0.479	0.652	3791
	(.396)	(.241)	(.564)	(.614)			
	[.995]	[.755]	[1]	[1]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). In the last three columns we report the mean outcome for the control group, the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***$p< 0.01$, **$p<0.05$, *$p<0.1$.

Table 23: **Effects on search outcomes by education**

	Transport				Job Application Workshop				Control Mean			
Outcome	Hi Sch.	Voc.	Dip/Deg	*F(p)*	Hi Sch.	Voc.	Dip/Deg	*F(p)*	Hi Sch.	Voc.	Dip/Deg	N
Applied to temporary jobs	0.499 (.131) [1]	0.213 (.49) [1]	0.0160 (.982) [1]	0.669	-0.0250 (.909) [1]	0.0590 (.818) [1]	-0.205 (.755) [1]	0.906	0.887	1.380	1.720	3775
Applied to permanent jobs	-0.211 (.317) [1]	0.185 (.608) [1]	0.142 (.868) [1]	0.420	0.0770 (.733) [1]	-0.202 (.495) [1]	0.146 (.861) [1]	0.642	0.854	1.940	4.570	3770
Interviews/Applications	-0.0460 (.353) [1]	-0.0160 (.672) [1]	-0.0320 (.452) [1]	0.876	-0.0790 (.084)* [1]	0.0140 (.726) [1]	-0.0100 (.826) [1]	0.334	0.337	0.371	0.345	2143
Offers/Applications	0.0540 (.38) [1]	-0.0470 (.363) [1]	-0.0380 (.293) [1]	0.308	0.00900 (.878) [1]	-0.0240 (.666) [1]	0.0280 (.518) [1]	0.679	0.263	0.291	0.174	2144
Interviews/Applications (Perm)	0.0220 (.758) [1]	-0.00200 (.972) [1]	-0.0320 (.508) [1]	0.790	0.00600 (.922) [1]	0.0230 (.634) [1]	0.00400 (.937) [1]	0.965	0.287	0.351	0.321	1662
Offers/Applications (Perm)	0.121 (.045)** [.781]	-0.00600 (.908) [1]	-0.0420 (.319) [1]	0.0677	0.123 (.006)*** [.22]	-0.0280 (.606) [1]	0.00200 (.97) [1]	0.0411	0.0957	0.201	0.155	1661
Interviews/Applications (Temp)	-0.0900 (.15) [1]	-0.00600 (.916) [1]	-0.0780 (.185) [1]	0.459	-0.124 (.048)** [.876]	0.0160 (.774) [1]	0.00200 (.976) [1]	0.187	0.375	0.397	0.391	1318
Offers/Applications (Temp)	-0.0420 (.565) [1]	-0.0490 (.414) [1]	0.00100 (.982) [1]	0.775	-0.0800 (.322) [1]	-0.0190 (.761) [1]	0.0990 (.117) [1]	0.167	0.362	0.381	0.216	1318
Uses CV for applications	-0.0270 (.46) [1]	0.106 (.005)*** [.163]	0.0300 (.567) [1]	0.00777	0.0320 (.452) [1]	0.0730 (.046)** [.876]	0.0360 (.495) [1]	0.685	0.192	0.376	0.695	3791
Uses certificates	-0.00200 (.968) [1]	0.107 (.037)** [.781]	0.0770 (.193) [1]	0.154	0.0730 (.208) [1]	0.00100 (.984) [1]	0.0900 (.108) [1]	0.302	0.296	0.533	0.614	3791

Note. In this table we report, separately for each education category, the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by least squares estimation of equation (2). Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). In columns 3 and 6 we report the p-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for individuals with different levels of education. In the last three columns we report the mean outcome in the control group for the different education categories. ***p< 0.01, **p<0.05, *p<0.1.

Table 24: **Predicted skills and employment outcomes: all workers**

	Work	Permanent work	Formal work	Earnings
	(1)	(2)	(3)	(4)
Predicted score	.053	.047	.063	352.393
	(.022)**	(.017)***	(.019)***	(102.618)***
Workshop	.036	.034	.041	56.538
	(.029)	(.021)	(.022)*	(115.825)
Predicted score * workshop	-.022	-.018	-.044	-35.074
	(.029)	(.023)	(.028)	(141.522)
Const.	.560	.171	.222	1132.593
	(.022)***	(.016)***	(.014)***	(87.763)***
Obs.	1463	1463	1463	1448

Note. In each column we report the results of an ordinary least squares regression of the outcome in the column heading on predicted grades, a dummy for being invited to the job workshop and the interaction of these two variables. In parentheses, we report standard errors obtained through a bootstrapping procedure for generated regressors. The sample includes all individuals in the control group and in the job application group. ***$p< 0.01$, **$p<0.05$, *$p<0.1$.

Table 25: **Predicted skills and employment outcomes: workers with at most secondary education**

	Work	Permanent work	Formal Work	Earnings
	(1)	(2)	(3)	(4)
Predicted score	-.042	-.039	-.0004	-134.476
	(.057)	(.030)	(.036)	(202.789)
Workshop	.016	.155	.056	202.050
	(.062)	(.038)***	(.039)	(178.431)
Predicted score * workshop	.035	.091	-.021	253.776
	(.077)	(.053)*	(.057)	(334.006)
Const.	.482	.037	.109	687.601
	(.042)***	(.018)**	(.025)***	(114.227)***
Obs.	452	452	452	448

Note. In each column we report the results of an ordinary least squares regression of the outcome in the column heading on predicted grades, a dummy for being invited to the job workshop and the interaction of these two variables. In parentheses, we report standard errors obtained through a bootstrapping procedure for generated regressors. The sample includes all individuals with at most a high school degree in the control group and in the job application group. ***$p< 0.01$, **$p<0.05$, *$p<0.1$.