## Anonymity or Distance?

## Removing Obstacles to Youth Employment in Urban Africa<sup>\*</sup>

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#### Abstract

Can job-search support improve the labour market prospects of young jobseekers? 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 orientation on how to make effective job applications. We find that both treatments significantly improve the quality of the jobs young people obtain. Upon investigating the underlying mechanisms, we show that both interventions mitigate the adverse effects of spatial constraints on employment outcomes, and that the workshop helps workers to better signal their abilities.

JEL codes: O12, O18, J22, J61, J64, M53.

## 1 Introduction

Youth unemployment is a major policy challenge throughout the world and it is especially pressing in urban areas, where high rates of joblessness feed social tension and political instability. As urbanisation accelerates, the problem will become ever more acute, especially in the developing world. Over half of the world's population already lives in a city. 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). Many of these new urbanites will struggle to find satisfying jobs.

How can policy makers help young workers in large cities find rewarding employment? A growing literature argues that alleviating search frictions plays a crucial role. In particular, reducing the cost of job search and helping jobseekers to signal their ability can improve their employment prospects (Card et al., 2007; Chetty, 2008; Crépon et al., 2013; Pallais, 2014). This, in turn, increases workers' well-being, prevents human capital depreciation, and may increase pro-social behaviour (Clark and Oswald, 1994; Paul and Moser, 2009; Krueger and Mueller, 2012; Fisman et al., 2015; Barr et al., 2016). In addition, reducing search frictions may have positive effects on the economy as a whole, by enhancing competition among firms and reducing distortions in labour allocation (Kaas and Kircher, 2015; Bryan and Morten, 2015). Yet, despite the importance of these mechanisms, we have surprisingly little evidence documenting how job search constraints can be alleviated in the growing cities of the emerging world.

In this paper, we experimentally evaluate two interventions that are designed to help young urban dwellers search for employment. 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. Individuals who are offered this program can collect the subsidy from an office located in the centre of the city, up to three times a week, for an average period of 16 weeks. The second intervention is a *job application work-shop*. Participants are offered orientation on how to make effective job applications using CVs and cover letters, and on how to approach job interviews. Further, during the event, they take a mix of standardised personnel selection tests. We use information from the tests

to measure a range of skills and provide participants with certificates documenting their results. The two interventions were designed to ease spatial and informational constraints to job search. We hypothesised that treated jobseekers would search more intensely and more effectively, leading to improved employment outcomes.

We evaluate these programs using a random sample of about 4,000 young individuals in a rapidly expanding metropolis located in a fast growing country – Addis Ababa, the capital of Ethiopia. This location is ideal for our purpose: a large city with a total population close to 4 million people, youth unemployment rates just below 30 percent, many insecure and poorly paid jobs, and rapid in-migration from outlying rural areas. 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.<sup>1</sup>

We find that both interventions help jobseekers 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. Individuals who are offered the transport subsidy are 25 percent more likely to be in formal employment. These effects are statistically significant; they are robust to a correction for multiple comparisons; and their magnitude is economically meaningful. The effects are stronger for women and for less educated workers (those who have at most secondary education). These are the groups that typically find it hardest to obtain high quality employment, in Ethiopia and in other developing countries (OECD, 2015). We show that for these marginalized individuals permanent employment rates are not on upward trajectory when the study ends, which suggests that the control group will not easily "catch-up" with the treatment group and that the effects we measure are likely to be persistent.

To understand the mechanisms that drive these results, we conduct fortnightly phone interviews with all sampled jobseekers throughout the course of the study. This provides a rich, high-frequency dataset that allows us to observe how search behaviour evolves in re-

<sup>&</sup>lt;sup>1</sup> 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.

sponse to our interventions.<sup>2</sup> We find that the transport subsidy allows jobseekers to search more intensely. Furthermore, we find that both interventions improve jobseekers' 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 programs bring this down to about one offer every 5.2 applications.

We also present evidence supporting the hypothesis that the interventions work by easing spatial and informational constraints. First, we find that in Addis Ababa selfemployment in the informal sector is more common on the outskirts of the city. Good employment opportunities may indeed become scarcer the further workers live from the city centre. Our interventions, particularly the transport subsidy, succeed in offsetting this tyranny of distance by increasing formal employment among workers who live further away from the centre. Self-employment rates among these workers drop to levels similar to people living in the centre of the city thanks to the interventions. Further, the job application workshop is effective in mitigating the curse of anonymity. 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. These workers, in the absence of the intervention, find it hardest to signal their abilities.

Finally, we measure the indirect impacts of the interventions on the young individuals who reside close to program participants. 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 jobseekers is low, and some negative effects when the proportion of treated jobseekers 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 %).

Taken together, our evidence shows that the urban unemployed in the least developed countries can benefit from simple interventions that facilitate job search. Recent studies in low income settings have documented that search subsidies and information can connect rural workers to urban jobs (Jensen, 2012; Bryan et al., 2014). We show that job search

<sup>&</sup>lt;sup>2</sup> Recall bias would make it difficult to perform a similar analysis using retrospective questions asked in the endline survey.

assistance can also be effective for workers in urban labour markets.

While recent evaluations in developed countries have focused on manipulating job search strategies (Altmann et al., 2015; Belot et al., 2015), there is limited experimental evidence documenting the impacts of interventions that target job-search constraints related to space and signalling of skills. Using a randomized field experiment in Washington DC, Phillips (2014) finds that a public transit subsidy has a large, short-run effect in reducing unemployment durations. For less developed countries, Franklin (2015) reports the results of an early trial of a transport subsidy intervention administered to a selected sample of motivated jobseekers in Addis Ababa. He also finds positive impacts on the probability of permanent work. Groh et al. (2015, 2016) find that a soft skills training program and a matching intervention do not improve the employment outcomes of young Jordanian women. These two interventions have a number of features in common with the job application workshop – chiefly, the focus on information about skills. However, they are offered to a sample of jobseekers with tertiary education, in a labour market with much higher unemployment rates. 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.

More generally, this paper contributes to our understanding of how space affects the functioning of labour markets (Gollin and Rogerson, 2010; Bryan and Morten, 2015; Asher and Novosad, 2015). We sample young individuals who reside in many different parts of Addis Ababa and we record detailed geo-coded information about their place of residence. This allows us to document the spatial pattern of occupational structure in the city and to calculate heterogeneous treatment effects by distance from the city centre. We find that both our interventions help jobseekers overcome spatial constraints on occupational choices.

We also contribute to a growing literature that studies the economic importance of cognitive and non-cognitive skills (Bowles et al., 2001; Heckman et al., 2006; Blattman et al., 2015). We draw from this literature to develop the personnel selections tests used in the study. Our evidence suggests that in some segments of the labour market employers may be unable to identify highly skilled workers. The job application workshop overcomes this obstacle. Our results relate directly to the findings in Pallais (2014), who documents the positive employment effects of an intervention that helps workers signal their skills in an online labour market.

Finally, we provide the first evidence on the local indirect effects of labour market interventions in a developing country. Using a randomised saturation design as in Crépon et al. (2013), we show that indirect effects on non-participants depend on the proportion of jobseekers that are offered the program in a given area.

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 main empirical specification. Section 4 presents the main results. In section 5, we discuss the nature of the constraints addressed by our interventions. Section 6 concludes and discusses ideas for future interventions.

## 2 Context

Addis Ababa is the largest city in Ethiopia (African Economist, 2012). 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).<sup>3</sup> Many of the city residents are unemployed. Young people between the age of 20 and 29 face an unemployment rate of 27.9 percent, and young women a rate of 33.6 percent (CSA, 2014).<sup>4</sup> Furthermore, available jobs are often insecure and poorly paid. At the time of the endline survey of our study, 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".<sup>5</sup> Working poverty is widespread: two workers out of

<sup>&</sup>lt;sup>3</sup> Other estimates suggest that the total population of the city is close to 4.5 million.

<sup>&</sup>lt;sup>4</sup> To calculate this rate, the Central Statistical Authority of Ethiopia counts as unemployed all individuals who do not have a job, but are available for work.

<sup>&</sup>lt;sup>5</sup> The median duration of these spells was 4 weeks for temporary workers and 8 weeks for the self-employed.

five earn less than 2 USD per day.<sup>6</sup>

Job search is costly for unemployed youth. One of the most popular search methods used by the participants in our study is to visit the job vacancy boards.<sup>7</sup> The boards are located in the centre of the city, forcing participants who live in the periphery to travel frequently to the centre, which can be costly. Job seekers also face the costs of gathering information through newspapers, printing CVs and cover letters, travelling to interviews, etc. Among the active searchers in our sample, the median expenditure on job search at baseline amounts to about 16 percent of overall expenditure. This goes up to 25 percent for jobseekers who report searching 6 days a week. These are large amounts especially if we consider that the typical jobseeker spends a long time in unemployment before finding a job. In our baseline sample, for instance, the median jobseeker has spent about 10 months looking for employment.

Young jobseekers in Addis Ababa find it hard to signal their skills and worth 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.<sup>8</sup> 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. Furthermore, many jobseekers 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 three interventions to support young unemployed

<sup>&</sup>lt;sup>6</sup> Median earnings in the control group at endline are about 70 USD per month. If we adjust this figure using the World Bank purchasing power parity conversion rates, this corresponds to about 205 USD. However, Addis Ababa is the most expensive city in Ethiopia. Thus when we use the nation-wide PPP adjustment factor we overestimate the real purchasing power of salaries in the capital city. Data from the Ethiopian Statistical Authority documents that in 2014 2 employed individuals out of 5 in Addis Ababa were available to work additional hours, further suggesting underemployment and low pay.

<sup>&</sup>lt;sup>7</sup> 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.

<sup>&</sup>lt;sup>8</sup> 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.

workers in their job-search. This paper focuses on the first two treatments. The third one, which is briefly described in Section **??**, is the focus of a separate paper (Abebe et al., 2016).

## **3** The interventions

## 3.1 Treatment 1: The transport subsidy

Individuals in this treatment group are offered cash conditional on visiting an office in the centre of Addis Ababa.<sup>9</sup> 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 aim of the subsidy is to reduce the cost of public transport. We thus calibrate the amount that can be collected on any given day to cover the cost of a return bus fare from the area of residence of the participant at baseline to the intervention office.<sup>10</sup> To access the subsidy jobseekers have to have enough cash to make the first journey, which in our setting is likely to be almost always the case.<sup>11</sup>

Making the transfer conditional ensures that the cash has to be spent on transport, at least for those people who in the absence of treatment would not travel to the centre of Addis Ababa regularly.<sup>12</sup> This allows us to focus on the 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.<sup>13</sup>

<sup>&</sup>lt;sup>9</sup> 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.

<sup>&</sup>lt;sup>10</sup> 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. Furthermore, we do not find that individuals in this treatment group increase their savings during the weeks of the intervention.

<sup>&</sup>lt;sup>11</sup> While jobseekers 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 jobseekers in our sample have at least 15 ETB in savings, while 75 percent of jobseekers have at least 10 ETB available as cash-on-hand or at home. See Franklin (2015) for further discussion of this issue.

<sup>&</sup>lt;sup>12</sup> This is likely to be a large majority of the sample. In the week prior to the baseline interview, 70 percent of the sample travelled to the centre of town less than three times.

<sup>&</sup>lt;sup>13</sup> For example, the flagship Productive Saftety Nets Program (PSNP) and the newly rolled out Urban PSNP.

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 was 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 help jobseekers signal their skills to employers. It has two components: an orientation session and a testing session. The orientation session helps jobseekers make more effective use of their existing signals (job experience, education, etc.). In the testing 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 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, be-tween 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 survey of 500 medium to large firms in Addis Ababa, we find that about 40 percent of firms know about the personnel selection services offered by the School of Commerce (Abebe et al., 2015). 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 'worksample' test. The results of the tests are presented in a certificate, which jobseekers 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.<sup>14</sup> The certificates are officially issued by the School of Commerce and the Ethiopian Development Research Institute.<sup>15</sup>

We chose the tests on the basis of the results of several qualitative interviews with firm managers in the city.<sup>16</sup> 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 to measure worker quality in several contexts (Dal B6 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).

<sup>&</sup>lt;sup>14</sup> 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%.

<sup>&</sup>lt;sup>15</sup> 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.

<sup>&</sup>lt;sup>16</sup> These interviews highlights managers' information needs and the degree of familiarity that managers have with various tests.

## 4 Experimental design and data

## 4.1 The sample

We randomise treatment assignment at two levels: geographic clusters and individuals. We defined geographic clusters using the Ethiopian Central Statistical Agency (CSA) enumeration areas.<sup>17</sup> Our sampling frame excluded clusters within 2.5 km of the centre of Addis Ababa, and clusters outside the city boundaries. Clusters were selected at random from our sampling frame, with the condition that directly adjacent clusters could not be selected, to minimise potential spill-over effects across clusters.

In each selected cluster, we used door-to-door sampling to construct a list of all individuals in the cluster 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. All selected individuals were contacted for an interview.

We completed baseline interviews with 4388 eligible respondents. We attempted to contact individuals by phone for at least a month (three months, on average); we dropped individuals who could not be reached after at least three attempted calls.<sup>18</sup> We also dropped any individual who had found a permanent job since being first contacted and who had retained the job for at least six weeks. Finally, we dropped individuals who had migrated away from Addis Ababa. Table 7 in the online appendix shows how many individuals were dropped from the sample at each point and the reasons for them being dropped. In all, we were left with 4059 individuals who are included in our experimental study.

<sup>&</sup>lt;sup>17</sup> CSA defines enumeration areas as small, non-overlapping geographical areas. In urban areas, these typically consist of 150 to 200 housing units.

 $<sup>^{18}</sup>$  We give details of the phone interviews in section 4.2 below

## 4.2 Face-to-face interviews 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).<sup>19</sup>

## 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).<sup>20</sup>

Not all individuals in the clusters assigned to the transport intervention and job application workshop were designated to receive 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%. Table 2 shows the number of clusters assigned to each saturation level and the number of individuals who were offered treatment. In clusters assigned to the job application workshop we kept the level of saturation fixed at 80%. Having set cluster saturation level

<sup>&</sup>lt;sup>19</sup> The study of Franklin (2015) is also focused on young urban jobseekers in Ethiopia.

<sup>&</sup>lt;sup>20</sup> 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.

els, we assigned individuals within clusters to treatment and controls. 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.<sup>21</sup>

< Table 1 here. >

< Table 2 here. >

## 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 9 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 Sub-Saharan Africa (Baird et al., 2011; Blattman et al., 2013). In the endline survey, we find 93.5% of all participants. The probability of tracking original study participants is very similar across treatment groups. 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. 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. Table 10 in the online appendix presents the full analysis of attrition.

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 16 in the online appendix shows the trajectory of monthly attrition rates over the course of the phone survey.

<sup>&</sup>lt;sup>21</sup> In addition, individuals designated to receive the transport intervention were randomly assigned to a start and an end week. This is illustrated in table 8 in the online appendix.

## 4.5 Take-up

50% of individuals in the transport group collect the cash at least once. Of these, 19% do not return to collect the subsidies 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. The average number of total collections for people in this group is 16, corresponding 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 in the job application workshop group attend the workshop. 80% of those attending later collect the certificates from the School of Commerce.

## 5 Empirical 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 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:

$$y_{ic} = \beta_0 + \sum_f \beta_f \cdot \text{treat}_{fic} + \sum_g \beta_g \cdot \text{spillover}_{gic} + \alpha \cdot y_{ic,pre} + \delta \cdot x_{ic0} + \mu_{ic}$$
(1)

where  $y_{ic}$  is the endline outcome for individual *i* in cluster *c* and  $x_{ic0}$  is the vector of baseline covariate values that were used for re-randomisation and blocking. treat<sub>fic</sub> is a dummy capturing whether an individual has been *offered* treatment f.<sup>22</sup> Thus, our estimates measure the *intent-to-treat* impacts of the interventions. The first spillover dummy,

<sup>&</sup>lt;sup>22</sup> We follow the pre-analysis plan and include separate dummies for the transport subsidy, and the job application workshop. Alongside the two treatments that we study in the paper, we also implemented a third intervention. Individuals assigned to this treatment were invited to attend a job fair. This is a distinct treatment, which we evaluate in a separate paper (Abebe et al., 2016). Dummy variables to estimate the impact of the job fairs are included in this estimation equation, but are not reported in this paper.

spillover<sub>1ic</sub> identifies control individuals residing in clusters assigned to the transport treatment, while the second spillover dummy, spillover<sub>2ic</sub> identifies control individuals in clusters assigned to the job application workshop. 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.<sup>23</sup>

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.

## 6 Results

We find that both interventions significantly improve the quality of the jobs workers get. Table 3 reports the main impacts on our family of primary outcomes.<sup>24</sup> The application 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.<sup>25</sup> We report these bounds in Table 25 of the online appendix.

<sup>&</sup>lt;sup>23</sup> 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.

<sup>&</sup>lt;sup>24</sup> This outcomes were prespecified as our primary family in our pre-analysis plan.

<sup>&</sup>lt;sup>25</sup> This is not surprising, as attrition in this study is modest and is not significantly correlated with treatment.

## < Table 3 here. >

We also find that both interventions increases workers' chances to have a formal job (proxied by having a written contract throughout the paper<sup>26</sup>) by nearly 25 percent. 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 interventions of these kinds can have strong positive effects on job quality without increasing overall employment. Hence, focusing exclusively on employment rates might miss the most important impacts of programmes designed to improve job-match quality.<sup>27</sup>

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

<sup>&</sup>lt;sup>26</sup> In Ethiopia, workers are often employed without a written contract, which makes it difficult for them to uphold their formal rights and for the fiscal authority to collect labour taxes. In our endline data, for example, workers with a written contract 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. Furthermore, they are less likely to be working in locations that are prone to informality such as small shops, construction sites and open markets, and more likely to be working in an office or a factory. Having a written contract is thus a salient measure of employment formality in this context.

<sup>&</sup>lt;sup>27</sup> The types of occupations that workers are engaged in are also similar across treatment and control individuals. In Figures 17 and 18 in the Appendix we give a breakdown of the ten most common occupations in the three experimental groups.

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 17 to 24 of the empirical appendix).<sup>28</sup> Overall we find little evidence that our interventions have changed outcomes in any of these areas. We have some limited evidence that the jobseekers 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.<sup>29</sup> Finally, we do not find evidence that the treatments have changed a number of outcomes related to mobility. Treated individuals have not made more trips to the centre of the city in the previous seven days and are not more likely to be working away from their neighbourhood. Combined with the modest effects on employment, these results suggest that treated jobseekers did not obtain the improvements in job quality discussed above by moving to distant jobs that have higher commuting costs and other distance-related disamenities.

## 6.1 Which groups benefit 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 jobseekers,

<sup>&</sup>lt;sup>28</sup> 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. 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.

<sup>&</sup>lt;sup>29</sup> They also expect five weeks less of unemployment before finding the next job, but this effect is not significant.

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, we run the following specification:

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

where  $x_{ic,pre}$  is a categorical variable with values 0, ..., *m* corresponding to the *m* groups of interest, and  $I(x_{ic,pre} = v)$  is a indicator variable that takes the value of 1 when  $x_{ic,pre}$  is equal to *v*. The coefficients  $\beta_{vf}$  estimate the effect of treatment *f* for group *v*.

We find that the effects on job quality (permanent and formal work) are concentrated among the least educated jobseekers, as shown in Figures 1 and 2. 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 results are similar for impacts on having a formal job. We also find that treatment effects are stronger among women, as shown in Figure 3. The transport intervention significantly increases both women's probability of permanent work and probability of formal work by 7 percentage points (from a baseline of 15 and 20 percent), 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. The effects are statistically significant from those on women, but they are less robust as they do not survive when we control for multiple-hypothesis testing.

Furthermore, 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 13, 14 and 15 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 jobseekers on the other hand, we estimate an insignificant increase of 4 percentage points.

It is significant that our interventions helped those with less education, who find it particularly hard to find good jobs. Using our high-frequency phone data we study job finding rates over time. For job seekers with only high-school in the control group, rates of permanent employment rise gradually in the first half of the study because, we hypothesize, the more employable job seekers find these jobs. However, for the last 3 months of the year there is no significant increase in the rate of permanent employment, which remains at about 15% throughout this period. In other words, it is not inevitable that these individuals will find permanent jobs eventually. By comparison, higher educated individuals were continuing to find jobs at the end of the study: 35% of untreated individuals with a degree have a permanent job by the end of the study.

## 6.2 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 jobseekers 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:

$$y_{itc} = \eta_t + \sum_f \sum_{w=S_f}^{E_f} \beta_{fw} \cdot \operatorname{treat}_{fic} \cdot d_{wit} + \sum_g \sum_{w=S_g}^{E_g} \gamma_{gw} \cdot \operatorname{spillover}_{gic} \cdot d_{wit} + \alpha_t \cdot y_{itc,pre} + \delta \cdot x_{ic0} + \mu_{itc}.$$
(3)

where  $\eta_t$  is a time-specific intercept term. w indicates the number of weeks since each treated individual began receiving his/her treatment. w = 0 in the week when the treatment started, and is negative for weeks before that.  $d_{wit}$  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 onwards, the dummy  $d_{0it}$  is equal to 1 in week 15 and to 0 in all other weeks.<sup>30</sup> Individuals

<sup>&</sup>lt;sup>30</sup> 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 or spillover group g.  $E_f$  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  $\beta_{g-5}$ .

in the control group have all such dummy variables set to 0. Thus,  $\beta_{fw}$  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  $\alpha_t$  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.<sup>31</sup> To do this, we estimate equation 3, subject to the following quadratic constraints on  $\beta_{fw}$  and  $\gamma_{fw}$ , and to the assumption that no treatment has an effect before it commences:

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

and 
$$\gamma_{gw} = \begin{cases} 0 & \text{if } w \le 0; \\ \theta_{g0} + \theta_{g1} \cdot w + \theta_{g2} \cdot w^2 & \text{if } w > 0. \end{cases}$$
 (5)

That is, instead of estimating parameters  $\beta_{fw}$  and  $\gamma_{fw}$ , we will estimate  $\phi_{f0}$ ,  $\phi_{f1}$ ,  $\phi_{f2}$ ,  $\theta_{g0}$ ,  $\theta_{g1}$  and  $\theta_{g2}$ .

#### 6.2.1 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%. In the same period, 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 0.4). This effect decreases linearly after the transport intervention ends. In the model where we impose quadratic constraints, we are able to estimate a significant effect on job search at

<sup>&</sup>lt;sup>31</sup> In the pre-analysis plan, we had originally intended to include w = 0 in the quadratic fit. On reflection, it is preferable to allow the treatment effect to begin the week after treatment begins.

least 2 months after the end of the intervention. The job application workshop, on the other hand, does not affect the likelihood of searching for a job. Figures 5, 6 and 7 illustrate these findings.

< Figure 5 here. >

#### < Figure 6 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 jobseekers.

The average impacts of our treatments on a range of job search indicators are further documented in the empirical appendix (Table 12). 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 7 here. >

< Figure 8 here. >

#### 6.2.2 Effects on search efficacy

Both our treatments significantly increase the efficacy of job search for the high impact groups described above. In the full sample, control respondents receive a job offer of permanent employment every 6.1 applications. Respondents in the two treatment groups receive an offer nearly every 4 applications. These effects, documented in Table 12 (online Appendix) are however not statistically significant. Focusing on high-school graduates, one of the high-impact groups of the intervention, reveals stronger and statistically significant impacts. Control individuals with a high school degree receive an offer for a permanent job every 10.6 applications. Both interventions reduce that Figure to 5.2 applications, a very large impact. In the discussion section 7 we further investigate what drives the increased search efficacy among workers with only secondary education. The next section rules out the possibility that this is due to a change in the types of jobs they search for after the intervention.

## 6.2.3 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.<sup>32</sup> This holds on average, as well as within educational categories (see Table 20 and Figure 19 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 jobseekers' discouragement. We find that individuals in the three experimental groups are equally likely to give up searching for at least one occupation and to stop searching for white collar jobs. Finally, in Figures 17 and 18 (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 jobseekers' decision to search for 'easier-to-get' permanent and formal jobs.

<sup>&</sup>lt;sup>32</sup> 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.

## 6.3 Indirect effects on untreated jobseekers

In this section, we study the outcomes of untreated jobseekers who live close to program participants. The benefits of the interventions can extend to this group if the young jobseekers 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).<sup>33</sup> 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. This is a 'partial population experiment' (Moffitt et al., 2001), which allows us to compare untreated individuals living close to program participants to untreated individuals living in clusters where no jobseeker has been offered the intervention. We report the results of this analysis in columns 4 and 5 of Table 3.

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

<sup>&</sup>lt;sup>33</sup> 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.

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

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,  $\beta_{20}$  captures the difference in outcomes between untreated individuals in these clusters and untreated individuals in clusters where nobody was treated. Further,  $\gamma_{20}$  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  $\beta$  and  $\gamma$  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 first study indirect impacts on these two dimensions. In Figures 10 and 11 we present estimates from model 6. We document a *positive* indirect effect for control individuals in clusters with 25 (40) percent saturation for formal (permanent) work. 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.<sup>34</sup>

#### < Figure 10 here. >

## < Figure 11 here. >

<sup>&</sup>lt;sup>34</sup> For the regression on permanent work we can reject the null hypothesis that all  $\beta$  coefficients are equal to 0.

## 7 Discussion: What constraints did the interventions relax?

In this section, we present some additional analysis to explore the nature of the constraints faced by the jobseekers in our study and the extent to which the interventions relaxed these constraints.<sup>35</sup>

## 7.1 Spatial constraints

We have argued that high transport costs make job search difficult for the young unemployed. These costs are particularly high for individuals that live far away from the centre of the city (for instance, self-employment is often thought to be an occupation of last resort in developing countries, as discussed by Falco and Haywood (2016)). If our interventions relax this spatial constraint 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 for of employment.

We find that in the control group the probability of being self-employed increases almost linearly with distance from the centre. The probability of having any employment, on the other hand, does not change with distance. This finding suggests that spatial barriers influence the occupational structure of the urban labour market and is consistent with a model where high transport costs distort job search. This finding complements recent work on the effect of transportat costs on the occupational structure in villages (Asher and Novosad, 2015).

Most importantly, we find that the transport intervention mitigates the tyranny of distance, by unraveling the spatial patterns of self-employment observed in the control group. Figure 14 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) confirm that the transport treatment significantly alters the effect of distance.

<sup>&</sup>lt;sup>35</sup> The analysis in this section was not registered in the pre-analysis plan.

These regressions confirm that lowering the monetary cost of transport to the city centre *removes* the spatial influence on the probability of self-employment: the effect of distance on the probability of self-employment is statistically indistinguishable from zero in the treatment group.

## < Figure 14 here. >

Figure 15 in the appendix shows that the job application workshop similarly mitigates the effect of space on the probability of self employment. 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.

## 7.2 Information constraints

Did our interventions enable the labour market to separate more effectively high skilled workers from low skilled workers? To address this question we exploit the detailed data from the personnel selection tests. For each test, we regress individual test scores on a rich set of covariates, including demographic characteristics, educational achievement, and the work history of study participants. We choose this set of variables because they can be readily signalled to an employer using a CV, a cover letter, or during the course of an interview. Using the estimated coefficients, we obtain predicted test scores for all individuals in our sample. Our objective is to study how predicted skills correlate with labour market outcomes in different experimental groups.

Suppose that the job application workshop has no effect on participants' ability to signal their skills to employers. In that case, we would expect the same correlation between predicted test scores and employment outcomes in the treatment and control groups. In contrast, suppose that the job application workshop allows individuals to signal their skills more effectively. In this scenario, we would expect to find a larger positive correlation between predicted test scores and employment outcomes among individuals invited to the workshop, compared to the control group. That is, we would expect the workshop to be particularly useful for individuals with strong observable characteristics, who can benefit from being more able to convey their attractive characteristics to employers.

Our results suggest the the ability to signal skills has improved for low-education jobseekers — the group that experienced the strongest treatments effects - thanks to the job application workshop. In this group, the correlation between predicted skills and having a permanent job is significantly stronger among individuals invited to the workshop compared to the control condition. We also estimate that the correlation between predicted skills and earnings increases, although insignificantly so. We report this analysis in Table 27 and Figure 12. We are unable to find similar effects when comparing low education jobseekers in the transport group to low education jobseekers in the control group.<sup>36</sup> Hence, this effect is unique to the job application workshop.

We also find that, among less-educated individuals in the control group, high predicted skills are not associated with better labour market outcomes (see Table 27). This indicates that the labour market for workers with low levels of education does not seem to separate jobseekers on the basis of the skills measured in our tests. This separation occurs, however, after we introduce the job application workshop program.

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 a measure of predicted skills based on observable worker characteristics (which can be easily included in a CV, cover letter or discussed in an interview). The second component of the job-application workshop - skill certification - 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.

<sup>&</sup>lt;sup>36</sup> This result is not reported, but is available upon request.

## 7.2.1 The role of certification

We look for evidence that the certification component of the workshop leads to better employment outcomes for individuals who scored well on the tests. 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 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 simple: if the information that we disclose 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 and the Raven test score since we find that these measures have the strongest predictive power for endline employment outcomes. Scores below the median were lumped together into a bottom band, while individuals scoring above the median were divided into five bands for each of the higher deciles of the test score distribution. We implement 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 all results are consistent with different bandwidth selections.<sup>37</sup>

We find no evidence that scoring in a higher band, across any of the test scores, leads to a significant improvement in employment outcomes, relatively to individuals scoring in a lower band, once we control for the test score itself. Notably this is true for the two main outcomes of job quality used in the paper so far: permanent work, and formal work. Figure 13 illustrates this result, by showing the effect of being above the median aggregate test score on having a permanent job. The full set of results, using optimal and other bandwidth selections, is available on request.

## < Figure 13 here. >

<sup>&</sup>lt;sup>37</sup> The optimal bandwidth selection is performed using the Stata command given 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 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.

We interpret these results as suggestive evidence that the information about 'hard-toobserve' 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. <sup>38</sup> Second, the residuals from a regression of test scores on observable characteristics (a rough proxy for unobservable workers' abilities) do not correlate positively with employment outcomes. In sum, while the job application workshop increased workers' capacity to signal their easier-to-observe skills, helping employers separate workers on the basis of idiosyncratic and hard-to-observe skills is more challenging.

## 8 Conclusions

Young people throughout the world find it hard to obtain, rewarding good jobs. The problem is pressing in rapidly growing cities, especially in the developing world, where discontent among youth is feeding social tension and instability. However, recent studies argue that many young workers are missing good opportunities due to disadvantages that policy makers can alleviate (Card et al., 2007; Chetty, 2008; Crépon et al., 2013; Pallais, 2014). In this paper, we conduct a randomised evaluation of two interventions designed to lower spatial and informational barriers to job search. The first intervention is a transport subsidy that covers the cost of travelling to the centre of the city to look for employment. The second intervention is a job application workshop with two components: certification of skills and orientation on how to make effective job applications through CVs, cover letters, and interviews. We hypothesised that the programs would enable jobseekers to search more intensely and more effectively, resulting in improved employment outcomes. A fortnightly panel of short phone interviews allows us to study these mechanisms in detail. We also randomised the proportion of treated jobseekers within geographical clusters to study

<sup>&</sup>lt;sup>38</sup> 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 the score reported.

indirect effects on non-participants residing in program localities.

We find that both interventions help jobseekers to secure better jobs. Individuals who are offered the job application workshop are 30 percent more likely to have a permanent job, and nearly 25 percent more likely to have a formal job than individuals in the control group. The transport subsidy leads to a 25 percent increase in formal employment. These effects are statistically significant and robust to a correction for multiple comparisons. Impacts are concentrated among the socio-demographic groups that have the worst employment outcomes at baseline: women and those with at most secondary education.

On average, we do not find indirect effects on untreated individuals living in the proximity of treated ones. Exploring variation in the intensity of the transport treatment across areas, we find some positive impacts on the job quality of untreated neighbours when the proportion of treated jobseekers is low and some negative effects when that proportion is very high (90%).

Detailed analysis based on a high-frequency phone survey reveals that the interventions operate through the hypothesised mechanisms. The transport intervention allows jobseekers to search more intensely, while search efficacy improves for jobseekers in both the transport and the workshop groups. Both interventions relax spatial constraints on occupational outcomes. The job application workshop also overcomes a crucial informational barrier by allowing low educated workers to better signal their skills to employers and earn higher returns on them. These results suggest that interventions of this kind can ease spatial constraints on job search and correct information asymmetries that may damage workers' prospects when ability cannot be easily signalled.

In conclusion, this paper shows that active labour market programs can improve the employment outcomes of urban jobseekers in developing countries, and especially the most disadvantaged: the young, women, and those without tertiary education. Exploring whether job-search support can be successfully bundled with interventions that increase workers' cognitive and non-cognitive skills (Card et al., 2011; Attanasio et al., 2011; Blattman et al., 2015; Ghosal et al., 2015; Heller et al., 2015; Hirshleifer et al., 2015) and how to best combine programs that assist job search with programs that foster entrepreneur-

ship (Blattman et al., 2013; Fafchamps et al., 2014; Fafchamps and Quinn, 2015; ??, mck) are interesting avenues for future research.

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## Tables and Figures for Insertion in Text



Figure 1: Heterogeneous Impacts by Education: Permanent Work







Figure 3: Heterogeneous Impacts by Gender: Permanent Work







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

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





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

Figure 8: Impact trajectory of the transport treatment: Employment





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

Figure 10: Spillover effects of the transport treatment on permanent work: By randomized level of cluster saturation







Figure 12: **Predicted grades and permanent work: Job seekers with at most a high school degree.** 





Figure 13: Aggregate Test Scores and Permanent Work: Impact of scoring above the median



Figure 14: Relationship between distance and self-employment: Impact of transport subsidies



Figure 15: Relationship between distance and self-employment: Impact of the job application workshop

	(1)	(2)	(3)	(4)
	In	Clusters		
Cluster Treatment	Control	Treated	Total	Total
Transport	500	774	1,274	74
Workshop	187	768	955	56
Job fairs	0	1007	1007	56
Control	823	0	823	48
Total	1,510	2,549	4,059	234

 Table 1: Final Assignment to Treatment for Individuals and Clusters

 Table 2: Randomised Saturation Levels for the Transport Treatment

	(1)	(2)	(3)	(4)	
	Indivi	duals	Clusters		
Proportion Treated	Controls	Treated	Count	Proportion	
20 %	256	65	18	24.32 %	
40 %	150	96	15	20.27 %	
75 %	56	191	15	20.27 %	
90 %	38	422	26	35.14 %	

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	Ν
Worked	0.0400 (.168) [.305]	0.0200 (.529) [1]	-0.0450 (.183) [1]	0.0320 (.538) [1]	0.562	0.494	3785
Hours worked	0.197 (.9) [.717]	-0.0920 (.953) [1]	-2.318 (.213) [1]	0.616 (.809) [1]	26.20	0.842	3778
Formal work	0.0550 (.004)*** [.031]**	0.0520 (.009)*** [.029]**	0.0140 (.491) [1]	0.0600 (.113) [1]	0.224	0.888	3785
Perm. work	0.0340 (.062)* [.23]	0.0670 (.001)*** [.005]***	0.00600 (.737) [1]	0.0150 (.566) [1]	0.171	0.114	3785
Self-employed	-0.0200 (.187) [.305]	-0.00500 (.739) [1]	-0.0170 (.363) [1]	-0.0180 (.542) [1]	0.0950	0.346	3785
Monthly earnings	2.401 (.974) [.717]	60.03 (.479) [1]	-43.01 (.634) [1]	12.12 (.907) [1]	1145	0.444	3732
Satis. with work	0.00300 (.919) [.717]	0.0250 (.356) [1]	-0.0170 (.483) [1]	0.0510 (.284) [1]	0.237	0.473	3785

Table 3: Employment outcomes

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.

		Transport		Job Application Workshop			Control Mean	
Outcome	Male	Female	F(p)	Male	Female	F(p)	Male	Female
Worked	0.0610 (.138) [.709]	0.0210 (.612) [1]	0.502	0.00100 (.986) [1]	0.0370 (.379) [1]	0.529	0.642	0.487
Hours worked	0.118 (.958) [1]	0.356 (.872) [1]	0.943	-0.283 (.891) [1]	0.486 (.836) [1]	0.815	28.80	23.80
Formal work	0.0490 (.099)* [.651]	0.0670 (.013)** [.094]*	0.660	0.00700 (.827) [1]	0.0880 (.001)*** [.016]**	0.0476	0.246	0.203
Perm. work	-0.00300 (.909) [1]	0.0660 (.003)*** [.039]**	0.0413	0.0620 (.032)** [.149]	0.0650 (.011)** [.079]*	0.938	0.189	0.154
Self-employed	-0.0290 (.305) [1]	-0.0190 (.326) [1]	0.786	-0.00200 (.942) [1]	-0.00600 (.765) [1]	0.910	0.109	0.0821
Monthly earnings	-3.308 (.981) [1]	17.48 (.807) [1]	0.894	102.3 (.495) [1]	63.11 (.47) [1]	0.818	1521	794
Satis. with work	-0.0230 (.569) [1]	0.0210 (.547) [1]	0.390	0.0220 (.598) [1]	0.0290 (.404) [1]	0.899	0.287	0.190

Table 4: Effects on main outcomes by gender

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). 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.

		Trans	port		Job	Applicatio	on Workshop		Co	ontrol Me	an
Outcome	High Sch.	Vocat.	Dip/Deg	F(p)	High Sch.	Vocat.	Dip/Deg	F(p)	High Sch.	Vocat.	Dip/Deg
Worked	0.0540 (.219) [1]	0.0270 (.458) [1]	-0.0220 (.648) [1]	0.534	-0.00400 (.929) [1]	0.0750 (.023)** [.177]	-0.0140 (.787) [1]	0.153	0.508	0.546	0.694
Hours worked	0.690 (.767) [1]	-0.343 (.848) [1]	-0.907 (.731) [1]	0.897	-1.744 (.446) [1]	2.935 (.081)* [.571]	2.402 (.379) [1]	0.233	24.80	26	29
Formal work	0.0750 (.01)*** [.121]	0.0250 (.429) [1]	0.0740 (.186) [1]	0.504	0.0680 (.019)** [.177]	0.0450 (.13) [.736]	-0.0430 (.389) [1]	0.150	0.108	0.228	0.403
Perm. work	0.0650 (.008)*** [.121]	-0.00700 (.805) [1]	-0.0210 (.72) [1]	0.0997	0.102 (0)*** [.007]***	0.0170 (.48) [1]	-0.0200 (.73) [1]	0.0414	0.0583	0.172	0.354
Self-employed	-0.0370 (.144) [1]	0 (.994) [1]	-0.0250 (.521) [1]	0.529	-0.0300 (.196) [.736]	0.0260 (.215) [.736]	0.0600 (.155) [.736]	0.0939	0.112	0.0780	0.111
Monthly earnings	99.64 (.32) [1]	-134.0 (.136) [1]	-121.1 (.724) [1]	0.197	101.0 (.377) [1]	121.3 (.285) [.941]	-202.2 (.58) [1]	0.699	780	1023	2072
Satis. with work	0.0120 (.73) [1]	-0.0210 (.56) [1]	0 (.998) [1]	0.746	0.0130 (.707) [1]	0.0470 (.196) [.736]	0.0340 (.567) [1]	0.760	0.225	0.234	0.264

Table 5: Effects on main outcomes by education

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). 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.

		Transp	oort		Job	Applicatio	on Workshop		Co	ntrol Mea	an
Outcome	High Sch.	Vocat.	Dip/Deg	F(p)	High Sch.	Vocat.	Dip/Deg	F(p)	High Sch.	Vocat.	Dip/Deg
Applied to temporary jobs	0.471 (.142) [1]	0.118 (.693) [1]	0.242 (.736) [1]	0.631	-0.00800 (.974) [1]	0.0230 (.926) [1]	-0.115 (.868) [1]	0.973	0.887	1.420	1.740
Applied to permanent jobs	-0.170 (.356) [1]	0.210 (.549) [1]	0.402 (.688) [1]	0.385	0.136 (.528) [1]	0.0890 (.767) [1]	-0.339 (.721) [1]	0.872	0.854	2	5.380
Interviews/Applications	-0.0540 (.283) [1]	-0.0190 (.586) [1]	-0.0470 (.236) [1]	0.765	-0.0890 (.046)** [.552]	0.0200 (.581) [1]	-0.0270 (.56) [1]	0.168	0.337	0.371	0.337
Offers/Applications	0.0580 (.328) [1]	-0.0510 (.236) [1]	-0.0250 (.57) [1]	0.259	0.00300 (.954) [1]	-0.0130 (.791) [1]	-0.00100 (.985) [1]	0.961	0.263	0.279	0.161
Interviews/Applications (Perm)	0.00200 (.978) [1]	-0.0190 (.662) [1]	-0.0380 (.388) [1]	0.878	-0.00300 (.966) [1]	0.0170 (.702) [1]	-0.0160 (.723) [1]	0.868	0.287	0.352	0.312
Offers/Applications (Perm)	0.108 (.085)* [1]	-0.0130 (.768) [1]	-0.0590 (.172) [1]	0.0878	0.110 (.014)** [.552]	-0.0180 (.715) [1]	-0.0340 (.414) [1]	0.0359	0.0957	0.202	0.143
Interviews/Applications (Temp)	-0.0880 (.181) [1]	-0.00600 (.905) [1]	-0.134 (.045)** [1]	0.244	-0.136 (.024)** [.552]	0.0170 (.726) [1]	-0.0230 (.775) [1]	0.120	0.375	0.394	0.398
Offers/Applications (Temp)	-0.0300 (.672) [1]	-0.0490 (.278) [1]	0 (.996) [1]	0.840	-0.0900 (.251) [1]	0.00100 (.991) [1]	0.0340 (.676) [1]	0.552	0.362	0.346	0.241
Uses CV for applications	-0.0270 (.481) [1]	0.0880 (.016)** [.97]	0.0520 (.32) [1]	0.0281	0.0290 (.488) [1]	0.0720 (.049)** [.552]	0.0380 (.471) [1]	0.673	0.192	0.406	0.736
Uses certificates	-0.0150 (.739) [1]	0.0900 (.059)* [1]	0.0720 (.243) [1]	0.163	0.0650 (.27) [1]	0.0100 (.845) [1]	0.0870 (.173) [1]	0.421	0.296	0.538	0.632

Table 6: Effects on search outcomes by education

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.

# **Online Appendix**

## **Appendix A: Further details**



Figure 16: Attrition (failure to complete one phone interview) from the Phone Survey by Month

Figure 17: Most Common Occupations: Control Group and Job Application Workshop Group



Table 9: Summary and Tests of Balance

	N	Mean	S.Dev.	lst Q.	Median	3rd Q.	Min.	Max.	F-test P
					1				
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 11: Variables Used for Re-Randomisation

VARIABLE	DEFINITION	SOURCE (QUESTION NUMBER)
degree	Dummy: Individual has finished a degree (bachelors or	Dummy: b5=20 or b5=21
	above) at a recognised university	
vocational	Dummy: Individual has finished a course or vocational	Dummy: $b5 \in \{9,, 16\}$
	training at an official vocational college or TVET	
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	Dummy: $s0_2 = 1$
	last 7 days	
post_secondary	Individual has any kind of non-vocational post-	Dummy: $b5 \in \{17,, 21\}$ .
	secondary education (degree or diploma)	
female	Respondent is female	Dummy: respon-
		dent_gender = 2
migrant_birth	Respondent was born outside of Addis Ababa and mi-	Dummy: b14!=10
	grated since birth	
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	Dummy: j2_1 =1
	6 months	
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	Dummy: $s0_1 = 1$
	months	
age	Respondent age	respondent_age
years_since_school	Years since the respondent finished school (any school	Constructed from i0.3 (=
		constructed from jo_o (=
	including university)	$2006 - j0_3$
	including university) Proportion of weeks that individual searched for work	$\frac{2006 - j0_3}{\text{Mean (over first 3 months of })}$
search_freq	including university) Proportion of weeks that individual searched for work (from the phone surveys)	$\frac{2006 - j0_3}{2006 - j0_3}$ Mean (over first 3 months of calls) of Dummy: $p1_14 = 1$
search_freq work_freq	including university) Proportion of weeks that individual searched for work (from the phone surveys) Proportion of weeks that the individuals worked (from	$2006 - j0_3)$ Mean (over first 3 months of calls) of Dummy: $p1_14 = 1$ Mean (over first 3 months of

Figure 18: Most Common Occupations: Control Group and Transport Subsidy Group



Table 7: 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 8: Assignment to	Start and End Weeks of	the Transport Intervention
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		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				

		(1)	
	A	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
C .	(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)	1 ,	(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 10: Predictors of Attrition



Figure 19: Heterogeneous Impacts by Education: Reservation wages

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	Ν
Applied to temporary jobs	0.344 (.199) [.963]	-0.0110 (.956) [1]	0.0160 (.949) [1]	-0.168 (.481) [1]	1.311	0.141	3769
Applied to permanent jobs	-0.0210 (.936) [.963]	0.0490 (.842) [1]	0.0600 (.838) [1]	0.0260 (.931) [1]	2.279	0.720	3764
Interviews/Applications	-0.0360 (.23) [.963]	-0.0370 (.168) [.732]	0.0340 (.472) [1]	-0.0140 (.789) [1]	0.354	0.957	2139
Offers/Applications	0.00400 (.924) [.963]	0.00300 (.95) [1]	-0.0150 (.72) [1]	0.0730 (.28) [1]	0.248	0.971	2140
Interviews/Applications (Perm)	0.00800 (.841) [.963]	0.0110 (.766) [1]	0.00600 (.885) [1]	-0.0210 (.71) [1]	0.327	0.929	1659
Offers/Applications (Perm)	0.0470 (.215) [.963]	0.0470 (.169) [.732]	0.00800 (.821) [1]	0.0520 (.305) [1]	0.164	0.995	1658
Interviews/Applications (Temp)	-0.0750 (.07)* [.963]	-0.0630 (.125) [.732]	0.0330 (.666) [1]	-0.0220 (.755) [1]	0.389	0.773	1314
Offers/Applications (Temp)	-0.0510 (.245) [.963]	-0.0380 (.401) [.732]	-0.0250 (.658) [1]	0.109 (.241) [1]	0.332	0.756	1314
Uses CV for applications	0.0150 (.604) [.963]	0.0420 (.155) [.732]	0.0190 (.576) [1]	-0.00500 (.914) [1]	0.401	0.313	3785
Uses certificates	0.0340 (.4) [.963]	0.0520 (.257) [.732]	0.0240 (.561) [1]	0.0280 (.619) [1]	0.479	0.677	3785

Table 12: Job search

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.

		Transport		Job App	lication Workshop		Control Mean		
Outcome	Low Search Intens.	High Search Intens.	F(p)	Low Search Intens.	High Search Intens.	F(p)	Low Search Intens.	High Search Inte	
Worked	0.0420 (.323) [.797]	0.0370 (.39) [.797]	0.932	0.0590 (.176) [.459]	-0.0120 (.775) [.868]	0.243	0.554	0.569	
Hours worked	-0.777 (.738) [1]	1.202 (.593) [.977]	0.565	0.610 (.804) [.868]	-0.185 (.929) [.868]	0.815	26.60	25.90	
Formal work	0.0770 (.008)*** [.13]	0.0420 (.127) [.701]	0.417	0.0640 (.028)** [.223]	0.0400 (.197) [.459]	0.605	0.203	0.241	
Perm. work	0.0440 (.089)* [.63]	0.0260 (.311) [.797]	0.647	0.0930 (.002)*** [.023]**	0.0390 (.159) [.459]	0.198	0.157	0.182	
Self-employed	-0.0100 (.679) [1]	-0.0360 (.081)* [.63]	0.397	-0.00800 (.747) [.868]	-0.00300 (.878) [.868]	0.889	0.110	0.0827	
Monthly earnings	95.94 (.375) [.797]	-73.46 (.447) [.797]	0.234	205.1 (.118) [.459]	-28.94 (.803) [.868]	0.190	1103	1181	
Satis. with work	0.0350 (.347) [.797]	-0.0300 (.416) [.797]	0.203	0.0670 (.067)* [.369]	-0.0110 (.756) [.868]	0.117	0.229	0.243	

## Table 13: Effects on employment outcomes by job search at baseline

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.

	Т	ransport		Job Appli	cation 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
Worked	0.0420 (.182) [.773]	0.0210 (.767) [1]	0.798	0.0230 (.473) [1]	-0.00300 (.972) [1]	0.753	0.544	0.688
Hours worked	-0.0700 (.966) [1]	2.879 (.476) [1]	0.499	0.0520 (.975) [1]	0.803 (.85) [1]	0.872	25.60	30.40
Formal work	0.0660 (.001)*** [.017]**	-0.00500 (.941) [1]	0.344	0.0540 (.008)*** [.056]*	0.0210 (.761) [1]	0.650	0.202	0.375
Perm. work	0.0440 (.025)** [.194]	-0.0440 (.514) [1]	0.231	0.0690 (0)*** [.005]***	0.0170 (.819) [1]	0.488	0.150	0.313
Self-employed	-0.0290 (.093)* [.597]	0.0230 (.608) [1]	0.299	-0.0120 (.496) [1]	0.0610 (.223) [1]	0.190	0.0985	0.0729
Monthly earnings	-38.04 (.607) [1]	391.5 (.13) [.637]	0.105	97.29 (.278) [1]	-55.85 (.809) [1]	0.526	1074	1639
Satis. with work	0.00100 (.967) [1]	-0.00200 (.975) [1]	0.964	0.0260 (.354) [1]	0.0270 (.734) [1]	0.987	0.224	0.323

## Table 14: Effects on employment outcomes by experience in permanent employment

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.

		Transport		Job App	lication Worksh	юр	Contro	ol Mean
Outcome	Below Med.	Above Med.	F(p)	Below Med.	Above Med.	F(p)	Below Med.	Above Med.
Worked	0.112 (.052)* [.314]	0.0160 (.599) [.476]	0.110	0.105 (.089)* [.194]	-0.00900 (.785) [.75]	0.0942	0.554	0.565
Hours worked	5.004 (.101) [.314]	-1.311 (.413) [.416]	0.0503	4.243 (.182) [.262]	-1.305 (.43) [.546]	0.115	24.70	26.70
Formal work	0.108 (.005)*** [.076]*	0.0430 (.056)* [.314]	0.154	0.0830 (.046)** [.15]	0.0390 (.075)* [.194]	0.353	0.251	0.214
Perm. work	0.0600 (.11) [.314]	0.0270 (.147) [.346]	0.406	0.0490 (.202) [.262]	0.0680 (.002)*** [.023]**	0.662	0.195	0.162
Self-employed	-0.0260 (.502) [.441]	-0.0230 (.201) [.346]	0.936	-0.00300 (.946) [.75]	-0.00500 (.802) [.75]	0.962	0.0872	0.0980
Monthly earnings	252.4 (.093)* [.314]	-71.21 (.406) [.416]	0.0645	318.3 (.028)** [.138]	-0.0310 (1) [.75]	0.0652	966	1207
Satis. with work	0.0330 (.524) [.441]	-0.0100 (.744) [.544]	0.456	0.112 (.024)** [.138]	-0.00400 (.885) [.75]	0.0370	0.205	0.248

Table 15: Effects on em	ployment outcomes l	by savings
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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.

	Tra	ansport		Job Application Workshop			Control Mean	
Outcome	Not used CV/Cert	Used CV/Cert	F(p)	Not used CV/Cert	Used CV/Cert	F(p)	Not used CV/Cert	Used CV/Cert
Worked	0.0350 (.42) [1]	0.0450 (.216) [.761]	0.860	-0.0300 (.48) [.725]	0.0740 (.046)** [.318]	0.0562	0.546	0.575
Hours worked	-0.0410 (.986) [1]	0.557 (.788) [1]	0.856	-1.882 (.434) [.725]	2.245 (.249) [.47]	0.201	26.10	26.20
Formal work	0.0690 (.014)** [.248]	0.0470 (.109) [.547]	0.600	0.0450 (.093)* [.318]	0.0520 (.085)* [.318]	0.856	0.128	0.301
Perm. work	0.0440 (.079)* [.547]	0.0240 (.332) [1]	0.554	0.0490 (.068)* [.318]	0.0770 (.005)*** [.073]*	0.462	0.116	0.215
Self-employed	-0.0410 (.086)* [.547]	-0.00400 (.852) [1]	0.264	-0.0140 (.56) [.725]	0.00700 (.743) [.916]	0.519	0.125	0.0716
Monthly earnings	22.31 (.839) [1]	-7.386 (.942) [1]	0.845	92.92 (.472) [.725]	69.42 (.526) [.725]	0.889	1051	1221
Satis. with work	-0.0140 (.728) [1]	0.0170 (.633) [1]	0.555	0.0150 (.698) [.916]	0.0390 (.247) [.47]	0.632	0.255	0.222

## Table 16: Effects on employment outcomes by use of certificates at baseline

Note. In this table we report heterogenous treatment effects for individuals who at baseline used certificates or CVs in job applications and individuals that did not use certificates or CVs. 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.

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	Ν
Job Quality	0.569 (.315) [1]	0.482 (.448) [1]	-0.168 (.821) [1]	0.812 (.456) [1]	-0.218	0.889	3785
Finan. Outcomes	0.188 (.436) [1]	0.153 (.477) [1]	0.104 (.688) [1]	-0.00600 (.983) [1]	-0.188	0.878	3785
Expects and Asps	-0.0910 (.897) [1]	0.104 (.86) [1]	-1.055 (.083)* [1]	-0.358 (.669) [1]	0.215	0.759	2809
Mobility	0.105 (.865) [1]	-0.0420 (.951) [1]	-0.419 (.578) [1]	-0.801 (.324) [1]	0.145	0.821	3779
Education/Skills	-0.828 (.215) [1]	-1.175 (.124) [1]	0.0630 (.936) [1]	-1.070 (.291) [1]	0.394	0.612	3785
Wellbeing	0.0480 (.772) [1]	0.185 (.234) [1]	0.0250 (.888) [1]	0.106 (.637) [1]	-0.0640	0.430	3781
Networks	-0.301 (.375) [1]	-0.353 (.332) [1]	-0.493 (.198) [1]	-0.228 (.604) [1]	0.111	0.883	3761

Table 17: Family indices

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). 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.

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	Ν
Received job by interview	0.0410 (.009)*** [.046]**	0.0440 (.015)** [.083]*	0.0250 (.235) [1]	0.0710 (.024)** [.137]	0.167	0.860	3785
Office work (7d)	0.0260 (.287) [.62]	0.00200 (.93) [1]	-0.0180 (.498) [1]	0.00600 (.867) [1]	0.201	0.305	3785
Skills match with tasks	0.00900 (.76) [.916]	0.00500 (.861) [1]	0.0310 (.372) [1]	0 (.998) [1]	0.130	0.886	3785
Overqualified	0.0380 (.279) [.62]	0.0300 (.384) [1]	-0.0380 (.304) [1]	0.0620 (.22) [.783]	0.291	0.797	3785
Underqualified	-0.0160 (.397) [.659]	-0.0120 (.547) [1]	-0.0130 (.565) [1]	-0.0200 (.429) [1]	0.0820	0.781	3785

## Table 18: Other job quality measures

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). 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.

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	Ν
Expenditure (7d)	26.52 (.502) [1]	18.00 (.643) [1]	-7.040 (.859) [1]	-60.53 (.145) [.768]	506.4	0.833	3785
Savings (total)	513.6 (.849) [1]	-772.0 (.566) [1]	-468.5 (.746) [1]	100.5 (.951) [1]	6907	0.616	1692
	0.490 (.376) [1]	0.240 (.627) [1]	0.467 (.466) [1]	0.527 (.503) [1]	-0.404	0.636	3785

Table 19: Financial outcomes

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). 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.

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	Ν
Offers expected (next 4m)	-0.00700 (.962) [1]	0.268 (.079)* [.366]	-0.165 (.263) [.652]	-0.194 (.16) [.333]	1.421	0.0804	3521
Reservation wage	3.910 (.962) [1]	-93.73 (.201) [.366]	-12.69 (.887) [1]	143.6 (.19) [.333]	2023	0.277	3319
Aspiration wage (in 5y)	624.7 (.37) [1]	673.2 (.404) [.366]	438.8 (.517) [1]	981.2 (.206) [.333]	7097	0.957	3446
Weeks expected to be without permanent job	2.044 (.636) [1]	-4.518 (.179) [.366]	-9.528 (.003)*** [.011]**	-5.373 (.249) [.333]	30.23	0.0883	1809

Table 20: Expectations, aspirations, reservation wages

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). 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.

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	Ν
Trip to center (7d)	0.134 (.435) [1]	-0.0410 (.821) [.677]	-0.136 (.444) [1]	-0.264 (.252) [1]	2.372	0.340	3347
Works away from home	-0.0330 (.211) [1]	-0.0370 (.167) [.638]	-0.0390 (.166) [1]	-0.0430 (.267) [1]	0.851	0.891	3785
Location of main occupation/activity changed	0.0270 (.507) [1]	-0.0330 (.392) [.645]	0.0220 (.636) [1]	-0.0330 (.461) [1]	0.269	0.102	3785
Moved within Addis	-0.00200 (.914) [1]	0.0240 (.242) [.638]	0.00600 (.792) [1]	0.00800 (.767) [1]	0.0820	0.192	3785
Moved outside of Addis	0.00900 (.151) [1]	0.0120 (.078)* [.638]	0.00300 (.668) [1]	0.00300 (.661) [1]	0.00700	0.748	3785

## Table 21: Mobility

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). 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.

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	Ν
In full-time education	-0.00700 (.363) [.694]	0 (.975) [1]	0.00300 (.803) [1]	0.0330 (.131) [.212]	0.0210	0.379	3785
In part-time education	-0.0480 (.016)** [.108]	-0.0330 (.139) [.61]	-0.0130 (.619) [1]	-0.0210 (.508) [.439]	0.142	0.431	3785
In informal training	-0.0100 (.512) [.694]	-0.0100 (.498) [.739]	-0.00700 (.655) [1]	-0.0410 (.001)*** [.009]***	0.0380	0.995	3785
Graduated (in past 12m)	0.0110 (.5) [.694]	-0.0130 (.436) [.739]	0.0160 (.434) [1]	-0.0190 (.392) [.416]	0.0850	0.138	3785
Graduated from vocational degree (in past 12m)	0.0160 (.134) [.504]	0.00600 (.531) [.739]	0.00500 (.644) [1]	0.00200 (.893) [.807]	0.0220	0.372	3785
Graduated from training (in past 12m)	-0.00100 (.931) [1]	-0.0220 (.063)* [.61]	0.0190 (.238) [1]	-0.0280 (.024)** [.064]*	0.0480	0.0952	3785

## Table 22: Education and training

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). 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.

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	Ν
Life satisfaction (0-10)	0.166 (.207) [1]	0.155 (.248) [1]	0.199 (.19) [1]	0.330 (.143) [1]	4.798	0.934	3351
Locus of control (0-10)	0.0200 (.946) [1]	-0.0280 (.922) [1]	-0.152 (.648) [1]	0.00200 (.994) [1]	6.207	0.871	3353
Oneness with society	-0.0310 (.827) [1]	0.0480 (.731) [1]	-0.0210 (.887) [1]	0.118 (.526) [1]	4.738	0.547	3353
Trust in other people (1-4)	0.0780 (.338) [1]	0.0470 (.608) [1]	0.0260 (.767) [1]	-0.0250 (.81) [1]	2.027	0.725	3352

#### Table 23: Psychological outcomes

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). 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 24: Social networks

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	Ν
Number of people with who regularly shares information about job opportunities	-0.378 (.335) [1]	-0.592 (.115) [.85]	-0.641 (.106) [.741]	-0.454 (.346) [.975]	5.242	0.558	3748
Number of people with permanent jobs in job network	0.120 (.57) [1]	0.118 (.618) [.859]	-0.0630 (.799) [1]	0.384 (.208) [.975]	2.440	0.991	3383
Can access guarantor for job (in next month)	-0.00300 (.955) [1]	-0.0640 (.231) [.85]	-0.0240 (.69) [1]	-0.00200 (.974) [.975]	1.220	0.232	3350
Number of meetings of voluntary associations attended (past 30d)	0.00800 (.892) [1]	0.00700 (.913) [.859]	-0.0330 (.635) [1]	-0.0550 (.37) [.975]	0.0970	0.977	3785

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). 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.

		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)

## Table 25: Lee bounds

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 26: 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.

	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

## Table 27: Predicted skills and employment outcomes: workers with at most secondary education

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.