Curse of Anonymity or Tyranny of Distance? The Impacts of Job-Search Support in Urban Ethiopia

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ABSTRACT

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We conduct a randomised evaluation of two job-search support programmes for urban youth in Ethiopia. One group of treated respondents receives a subsidy to cover the transport costs of job search. Another group participates in a job application workshop where their skills are certified and they are given orientation on how to make effective job applications. The two interventions are designed to lower spatial and informational barriers to employment. We find that both treatments significantly improve the quality of jobs that young jobseekers obtain. Impacts are concentrated among women and the least educated. Using rich high-frequency data from a phone survey, we are able to explore the mechanisms underlying the results; we show that while the transport subsidy increases both the intensity and the efficacy of job search, the job application workshop mainly operates through an increase in search efficacy. Both interventions mitigate the adverse effects of spatial constraints on employment outcomes, and the job application workshop alleviates informational asymmetries by helping workers to signal their ability.

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1 Introduction

Over half of the world's population lives in a city and global urbanisation is proceeding at a rapid pace, especially in developing countries. 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). A key challenge for policy makers is to find ways to support a growing urban labour force, and in particular those workers who find it hardest to access the opportunities that cities have to offer. These individuals tend to be young and are predominantly female. They have left school after completing compulsory education, they have little work experience, and they live in poorly connected parts of the city. A growing literature argues that their labor market opportunities are reduced by the cost of job search and their inability to signal skills during the recruitment process (Card et al., 2007; Chetty, 2008; Crépon et al., 2013; Pallais, 2014). Limited labour market opportunities can reduce well-being, depreciate human capital, and change preferences (Clark and Oswald, 1994; Fisman et al., 2015). Search frictions also affect competition between firms and contribute to the misallocation of labour (Kaas and Kircher, 2015; Bryan and Morten, 2015). Yet, despite the importance of these effects, we have surprisingly little evidence documenting how job search constraints can be addressed in the growing cities of developing economies.

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 workshop. Participants are offered orientation on how to make effective job applications using CVs and cover letters and on how to approach job interviews. Further, during the event, participants take a mix of standardised personnel selection tests. We use information from the tests to certify participants' general skills. These two interventions are 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

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employment outcomes.

We evaluate these interventions using a random sample of about 4,000 young individuals in a rapidly expanding metropolis located in a fast growing country – namely, 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, unemployment rates above 20 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.[1]

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).

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 response to our interventions.[2] 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.

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[1] Individuals included in the study are between 18 and 29 years of age, have completed high school, are available to take up employment, and are not currently working in a permanent job.

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

We also present evidence consistent with the hypothesis that the interventions work by easing spatial and informational constraints. In particular, both interventions weaken the correlation between distance from the city centre and the type of occupation that participants undertake. Furthermore, using data from the personnel selection tests, we find that the job application workshop strengthens the correlation between skills and good employment outcomes for workers with less formal qualifications. The workshop does not seem to have worked simply by giving high scoring respondents very impressive looking certificates. With a regression discontinuity strategy we find no effect of being given a better looking CV (results were reported in discrete bands) when controlling for the raw test score.

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 can benefit from simple interventions that facilitate job search, at least in the context of least developed countries where the problem is particularly acute. Recent studies 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 assistance can also be effective for workers in urban labour markets.

There is little experimental evidence documenting the impacts of transportation subsidies and jobseekers' orientation interventions. Using a randomized field experiment in Washington DC, Phillips (2012) finds that a public transit subsidy has a large, short-run effect in reducing unemployment durations, with treatment causing the probability of finding employment within 40 days to increase by 9 percentage points, from 0.26 to 0.35. After 90 days, this difference narrows to a large but statistically insignificant 5 percentage points. For less developed countries, Franklin (2015) reports the results of an early trial of a trans-

port 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, our results contribute 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 the 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. Our results relate directly to the findings in Pallais (2014), who documents the positive employment effects of an interventions 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. We conclude with some thoughts about 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 Addis Ababa's population will more than double in the next 25 years (CSA, 2014; Davison, 2014).[3] Many of the city residents are unemployed. The overall unemployment rate is 23.5 percent.[4] 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). 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".[5] Working poverty is widespread: two workers out of five earn less than 2 USD per day.[6]

Job search is costly for unemployed youth. One of the most popular search methods

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[3] Other estimates suggest that the total population of the city is close to 4.5 million.

[4] 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.

[5] The median duration of these spells was 4 weeks for temporary workers and 8 weeks for the self-employed.

[6] 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.

used by the participants in our study is to visit the job vacancy boards.[7] The boards are located in the centre of the city, forcing participants who live in the periphery to travel frequently to the centre of the city, 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 example, the median jobseekers 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.[8] Job referrals are also frequent (Serneels, 2007; Caria, 2015). This puts young people at a disadvantage, as they have little work experience and less extensive networks. 55 percent of the participants in our study report having less than one year of work experience and only 16 percent have ever worked in a permanent job. 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 workers in their job-search. This paper focuses on the first two treatments. The third one, which is briefly described in Section 2.4, will be the focus of a separate paper.

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[7] 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.

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

3 The interventions

3.1 Treatment 1: The transport subsidy

Individuals in this treatment group are offered cash conditional on attending an office in the centre of Addis Ababa.[9] Recipients are required to attend in person, and to show photographic ID on each visit. Each recipient can collect cash once a day, up to three times a week. The amount that can be collected on any given day is calibrated to cover the cost of a return bus fare from the area of residence of the participant at baseline to the intervention office. [10]

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 is 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. We implement the intervention 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,

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

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

education, etc.). In the testing session we certify skills that are "difficult to observe" for employers, such as cognitive ability. The test certificates that we produce give jobseekers one more signal about their employability.

The intervention takes place over two days. On the first day, participants take the tests. On the second day, they attend the orientation session. The intervention is administered by the School of Commerce of Addis Ababa University, between September and October 2014. The School of Commerce has a reputation for reliable personnel selection services and many firms screen their applicants using tests developed, and sometimes administered by, the School of Commerce. In a 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 'work-sample' 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.[11] The certificates are officially issued by the School of Commerce and the Ethiopian Development Research Institute.[12]

We chose the tests on the basis of the results of several qualitative interviews with firm managers in the city.[13] 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

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[11] 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%.

[12] Participants collect the final certificates from the School of Commerce, after all testing sessions are completed.

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

to measure worker quality in several contexts (Dal Bó et al., 2013; Beaman et al., 2013). The tests of mathematical and linguistic ability are 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).

3.3 Job fair

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, free of charge a job fair. This is a distinct treatment, which we plan to evaluate in a separate paper.

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.[14] 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

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

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 for 4425 respondents. We then refined the sample in several ways. First, we dropped a small number of individuals who violated the inclusion criteria. This left 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.[15] We also dropped any individual who had found a permanent job and who retained the job for at least six weeks. Finally, we dropped individuals who had migrated away from Addis Ababa during the phone survey. Table 9 in the online appendix shows of 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 were are included in our experimental study.

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. We collect information about the sociodemographic characteristics of study participants, their education, work history, finances and their expectations and attitudes. We also include a module to study social networks.

We also construct a rich, high-frequency panel dataset through fortnightly phone interviews. We call all study participants through the duration of the study. In these interviews

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[15] We give details of the phone interviews in section 4.2 below

we administer a short questionnaire focused on job search and employment. Franklin (2015) shows that high-frequency phone surveys of this type do not generate Hawthorne effects, for example, they do not affect jobseekers' responses during the endline interview.

4.3 Randomisation

We randomly assigned geographic clusters to one of five treatment groups: (i) control, (ii) transport, (iii) job application workshop, (iv) job fair, (v) job fair and workshop. To ensure balance, we created blocks of clusters with similar baseline observables and randomly assigned clusters within each block to the five groups (Bruhn and McKenzie, 2009).[16]

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 those clusters.

In clusters assigned to the job application workshop we kept the level of saturation fixed. 80% of all individuals in these clusters were invited to attend the workshop. Having set cluster saturation levels, 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 final assignment to treatment is outlined in Table 1. Finally, individuals designated to receive the transport intervention were randomly assigned to a start and an end week. This is illustrated in table 10 in the online appendix.

< **Table 1 here.** >

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[16] We used the following variables to create the blocks: 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.

< **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 11 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 variables that we study.

Attrition in our sample 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 experiment groups. We cannot reject the null hypothesis that there are no differences in attrition rates between treatment 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, are correlated with treatment, we are not worried that this is affecting our results. Table 12 in the online appendix presents the analysis of attrition.

Attrition in the phone survey is also low, below 5% in the early months of the calls. We find that while attrition increases in the later week of the study, we are still able to contact more than 90% of respondents in the final month of the phone survey. Figure 1 shows the trajectory of monthly attrition rates over the course of the phone survey.[17]

< **Figure 1 here.** >

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[17] We do not report attrition rates at the very beginning of the phone survey since many respondents were only contacted for the first time in months 2 and 3 of the phone survey, either because they were surveyed towards the end of the baseline survey, and because of lags in setting up the phone survey.

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 collected the subsidies for at least two weeks tended to be dedicated users. 70% of those who took up the subsides did so on average 2 times a week over the course of the study. The average number of total collections among those who took it up was 16, corresponding to an average cash transfer of 320 ETB (15 USD) per active user, or 160 ETB (7.5 USD) per intended user.

63% of individuals in the job application workshop group attended the workshop. 80% of those attending later collected the certificates from the School of Commerce. Not all of these jobseekers chose to use the certificates to support their job applications. Among the study participants who collected the certificates and applied to at least one position, 42 percent report ever including the certificates in an application.[18] When they did so, participants report that the firms took the information into consideration.

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:

$$y_{ic} = \beta_0 + \beta_1 \cdot \texttt{transport}_i + \beta_2 \cdot \texttt{workshop}_i + \beta_3 \cdot \texttt{jobfair}_i$$
$$+ \gamma_1 \cdot \texttt{spillover1}_i + \gamma_2 \cdot \texttt{spillover2}_i + \alpha \cdot y_{ic,pre} + \boldsymbol{\delta} \cdot \boldsymbol{x}_{i0} + \mu_{ic} \quad (1)$$

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[18] This is unlikely to be caused by poor understanding, as participants report to have understood the information presented in the certificates well and certificates use is correlated with the score reported.

where y_{ic} is the endline outcome for individual i in cluster c and x_{i0} is the vector of baseline covariate values that were used for re-randomisation and blocking. transport$_i$, workshop$_i$, jobfair$_i$ are dummies that capture whether an individual has been *offered* a particular program.[19] Thus, our estimates measure the *intent-to-treat* impacts of the interventions. In this paper we focus on the effects of the transport and job application workshop. As discussed, the effects of the job fair intervention will be reported in a separate paper. spillover1$_i$ identifies control individuals residing in clusters assigned to the transport treatment, while spillover2$_i$ identifies control individuals in clusters assigned to the job application workshop. We correct standard errors to allow for correlation within geographical clusters. As we explained above, we sampled individuals who have attained high education levels with higher frequency. To obtain average treatment effects for the population of eligible individuals, we weight observations according to the inverse of the probability of being sampled.[20]

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 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. To deal with secondary outcomes, we will 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.

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[19] We separately evaluate two versions of the job fairs treatment. We hence run a regression that includes a dummy for each version of this intervention. To simply the exposition, in equation 1 we collapse these two dummies into one.

[20] We use the sampling weights reported in the pre-analysis plan.

6 Results

6.1 Primary employment outcomes

We present the impacts of each program on the primary employment outcomes. Table 3 summarises these results. Each row in the table corresponds to an outcome variable. We study impacts on overall employment rates, on the number of hours workers and on the probability of having a permanent job and a job with a written contract. In Ethiopia, the distinction between written and unwritten contracts is fundamental for determining workers' formal rights.[21] Thus, the written contract variable represents a salient measure of employment formality in the Ethiopian context. We also test for effects on the probability of being self employed, on earnings and work satisfaction. The first two columns of table 3 report the effects of the programs on treated individuals. Columns 3 and 4 show impacts on control individuals in treated clusters (the indirect effects of the programs). Below each coefficient we report a p-value in parentheses and a q-value in brackets. The mean outcome for the control group at endline is shown in column 5. In column 6 we report the results of an F-test for the equality of the effects of the two interventions.

< **Table 3 here.** >

We find that both interventions significantly improve the quality of the jobs workers get. 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

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[21] In theory, verbal agreements are legally binding. In practice, verbal agreements are very hard to uphold in court.

effects are also robust to the use of Lee bounds to correct for attrition.[22] We report these bounds in table 24 of the online appendix.

We also find that both interventions increases workers' chances to have a formal job (proxied by a written contract) 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 modest and insignificant effects on the overall employment rate of treated individuals. About 56 percent of controls are engaged in some form of work at endline. This goes up by an insignificant 4 percentage points for individuals in the transport treatment, and by an insignificant 2 percentage points for individuals who were invited to the job-application workshop. The types of occupations that workers are engaged in are also similar across treatment and control individuals. In figures 2 and 3 we give a breakdown of the ten most common occupations in the three experimental groups.

Earnings and work satisfaction are similar across experimental groups. This is in line with the findings of a number of recent experimental studies of labour market interventions in developing countries (Groh et al., 2012; Jensen, 2012; Franklin, 2015). Further, the coefficients we estimate are consistent with the magnitude of the effects on the quality of jobs outlined above. We cannot reject the hypothesis that the treatment impacts on these two variables do not differ from the naive prediction obtained from multiplying the treatment impacts on permanent and formal work by their marginal effects 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 the job search support group. The estimated treatment effect on work satisfaction for this experimental group is 2.1 percentage points, quantitatively close

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

and statistically indistinguishable from the naive prediction.

6.1.1 Which groups benefit from the interventions?

We investigate treatment effects for a number of groups defined 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, 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. When such characteristics are continuous, we create groups by separating individuals below and above the median level of the characteristic. For each outcome, we run the following specification:

$$y_{ic} = \sum_{v=0}^{m} \left[\beta_v + \sum_{f=1}^{4} \gamma_{vf} \cdot \texttt{treat}_{fi} \cdot I\left(x_{i,pre} = v\right) + \gamma_{v5} \cdot \texttt{spillover}_{1i} \cdot I\left(x_{i,pre} = v\right) \right.$$

$$\left. + \gamma_{v6} \cdot \texttt{spillover}_{2i} \cdot I\left(x_{i,pre} = v\right) \right] + \alpha \cdot y_{ic,pre} + \delta \cdot x_{i0} + \mu_{ic}, \tag{2}$$

where $x_{i,pre}$ is a categorical variable with values $\{0, \ldots, m\}$, and $I\left(x_{i,pre} = v\right)$ is a indicator variable that takes the value of 1 when $x_{i,pre}$ is equal to v. The coefficients γ_{vf} estimate the effect of treatment f for group v.

< **Figure 4 here.** >

< **Figure 5 here.** >

< **Figure 6 here.** >

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< **Figure 7 here.** >

We find that the effects on job quality (permanent and formal work) are concentrated among the least educated jobseekers and, to some extent, among women. To study heterogeneity by education, we separate individuals who have a high school degree and no further education, individuals who have a high school degree and further vocational training, and individuals who have a university degree or above. Figures 4 and 5 show that the effects of both interventions are mostly driven by the group that has only a high school degree. For this group, the job application workshop raises the probability of permanent employment by 10 percentage points, and the transport intervention raises it by 5 percentage points. On the other hand, treatment effects for individuals with higher educational attainment are close to zero. In the control group, about 5 percent of individuals with at most secondary education have a permanent job. The workshop, therefore, triples the probability of permanent employment for this group, and the transport subsidy doubles it. We find qualitatively similar results for the formality of employment. Impacts are concentrated among individuals with a high school degree, who experience an increase in the probability of formal employment of about one third.

Both the transport intervention and the workshop significantly improve the the quality of jobs for women. We report these results in figure 6, and table 6 in the appendix. The transport intervention significantly increases *both* women's probability of permanent work and probability of formal work by 35 percent, while women invited to the workshop are about 45 percent more likely to work in a permanent job, and 35 percent more likely to be in formal employment. Effects for men are less robust. The transport intervention does not increase the probability of permanent employment for men. We can reject the null hypothesis that the effect of the transport intervention on permanent work is equal for men and women at the 5 percent level. Further, the job application workshop has a small and insignificant impact on men's probability of being in formal employment, and effect that is statistically different from the effect we measure for women.

Second, the improvement in job quality is concentrated among individuals who do not search actively at baseline, individuals with no previous work experience, and individuals

with low savings (tables **??**, 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.

When we investigate how treatment effects vary with distance from the centre, we obtain a nuanced picture. The permanent employment effects are driven by individuals who live within 6km of the city centre. Treatment effects are smaller in magnitude and insignificant for individuals who live in the periphery. However, individuals living in the periphery experience a significant and marked decline in the probability of self employment thanks to the transport intervention. This effect is not observed for individuals living closer to the centre and for individuals invited to the job application workshop. In the final section of the paper we discuss this finding in greater deal.

6.2   Mechanisms

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:

$$y_{itc} = \eta_t + \sum_{g=1}^{4} \sum_{w=S_g}^{E_g} \beta_{gw} \cdot \mathtt{treat}_{gi} \cdot d_{wit}$$

$$+ \sum_{s=1}^{2} \sum_{w=S_s}^{E_s} \gamma_{sw} \cdot \mathtt{spillover}_{si} \cdot d_{wit} + \alpha_t \cdot y_{itc,pre} + \boldsymbol{\delta} \cdot \boldsymbol{x}_{i0} + \mu_{itc}. \qquad (3)$$

where η_t is a time-specific intercept term. We allow the effect of the baseline control term $y_{ic,pre}$ to vary over time by estimating α_t for each time period, while we estimate time-invariant effects of individual covariates x_{i0}. w is a variable indicating 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.[23] Individuals in the control group have all such dummy variables set to 0. Thus, β_{gw} is our estimate of the impact of intervention g, w weeks after the intervention started.

Second, we estimate the trajectory of treatment effects by pooling all post treatment ($w > 0$) time observations together and estimating quadratic trends over time of the treatment effect for each intervention.[24] To do this, we estimate equation 3, subject to quadratic constraints on β_{gw} and γ_{sw}, and to the assumption that no treatment has an effect before it commences.

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[23] 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_g denotes the earliest week for which we will be able to estimate a treatment effect for treatment or spillover group g. E_g denotes the final week. If, for example, a treatment began in week 15 of the study, then $S_g = -15$ and $E_g = 39$. For this treatment, we will use data from week 10 of the study to estimate the coefficient β_{g-5}.

[24] 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.

$$y_{itc} = \eta_t + \sum_{g=1}^{4} \sum_{w=S_g}^{E_g} \beta_{gw} \cdot \texttt{treat}_{gi} \cdot d_{wit}$$

$$+ \sum_{s=1}^{2} \sum_{w=S_s}^{E_s} \gamma_{sw} \cdot \texttt{spillover}_{si} \cdot d_{wit} + \alpha_t \cdot y_{itc,pre} + \boldsymbol{\delta} \cdot \boldsymbol{x}_{i0} + \mu_{itc} \tag{3}$$

subject to:

$$\beta_{gw} = \begin{cases} 0 & \text{if } w \leq 0; \\ \phi_{g0} + \phi_{g1} \cdot w + \phi_{g2} \cdot w^2 & \text{if } w > 0; \end{cases} \tag{4}$$

$$\text{and } \gamma_{sw} = \begin{cases} 0 & \text{if } w \leq 0; \\ \theta_{s0} + \theta_{s1} \cdot w + \theta_{s2} \cdot w^2 & \text{if } w > 0. \end{cases} \tag{5}$$

That is, instead of estimating parameters β_{gw} and γ_{sw}, we will estimate ϕ_{g0}, ϕ_{g1}, ϕ_{g2}, θ_{s0}, θ_{s1} and θ_{s2}.

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 5 percentage points more likely to be searching for a job than control individuals. This effects decreases linearly after the transport intervention ends. In the model where we impose quadratic constraints, we are able to estimate a significant effect at 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 8 and 9 illustrate these findings.

We find that study participants temporarily decrease the amount of work they take during weeks when the transport subsidy is available (figure 10). The effect is driven by a reduction of work in self-employment (figure 11). Franklin (2015) finds a similar effect of transport subsidies for a sample of active jobseekers.

More frequent search activity does not translate in a higher number of job applications for individuals in the transport group. On average, individuals in the control group

place 1.3 applications for temporary jobs and 2.3 applications for permanent jobs in the 12 months before the endline interview. As shown in table 4, these numbers are not significantly different for either the transport or the workshop interventions.

< **Figure 8 here.** >

< **Figure 9 here.** >

< **Figure 10 here.** >

6.2.2 Effects on search efficacy

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, described in table 4 are however not statistically significant. Focusing on high-school graduates, one of the high-impact groups of the intervention, reveals stronger and statistically significant effects. 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 we present more analysis that allows us to understand 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 study participants

We do not find evidence suggesting that treated individuals search for different jobs compared to individuals 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

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report being willing to work for the same wages as control individuals.[25] This holds on average, as well as within educational categories (see table 19 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 2 and 3 we compare the probability of working in a number of different occupations across the three groups. Occupation profiles look similar for the three groups, with a slight shift towards white collar jobs for individuals in the transport and workshop groups. 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.

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).[26] On the other hand, untreated youth living close to program recipients can 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

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[25] 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.

[26] 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.

participants to untreated individuals living in clusters where no jobseeker has been offered the program. 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 received the *transport* intervention. This allows us to run a regression of the form:

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

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

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 12 and 13 we present estimates from model 6. We document a *positive* indirect effect for control individuals in clusters with 25 (40) percent saturation for written agreement (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.[27]

6.4 Secondary outcomes

Up to now, we have focused on the effects of the interventions on key labour market outcomes, and the mechanisms through which these effects are generated. In this section, we study whether the interventions have affected a range of other outcomes. In the pre-analysis plan we have organised these secondary outcomes in seven families: (i) other outcomes about job quality, (ii) financial outcomes, (iii) expectations, reservation wages and aspirations, (iv) spatial mobility, (v) education and training, (vi) psychological outcomes and (vii) social and job networks. Table 5 reports the effects for indices that summarise each family. We report the results for individual families in tables 17 to 23 in the online appendix.

Overall we find little evidence that our interventions have changed outcomes in any of these seven dimensions. This is not surprising, given that we do not change either the overall employment rate, nor the incomes of treated study participants. 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 forecast to receive 20 percent more job offer in the next four months than individuals in the control group. They also expect five weeks less of unemployment before finding the next job. Only the first effect is significant, and does not survive the multiple comparison correction. We observe that jobseekers in both treatment groups aspire to earn wages that are 14 percent higher than control group

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

jobseekers, but this effect is imprecisely measured and not statistically significant.

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.[28]

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. If transport costs are indeed a barrier to job search, these individuals may be more likely to work in sectors of 'last resort' compared to those residing closer to the centre of the city. Some forms of self-employment are thought to be sectors of 'last resort' (Falco and Haywood, 2016). Further, if our interventions relax this spatial constraint to job search, we expect to observe a weaker spatial pattern in employment outcomes among treated individuals. We investigate these 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 city centre. The probability of having any employment, on the other hand, does not change with distance. This 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 eliminates the spatial patterns

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

of self-employment observed in the control group. Figure 17 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 of the city. Regression results with differential effects of distance by group (either linear or quadratic) confirm that the transport treatment significantly alters the effect of distance. 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 is statistically indistinguishable from zero for the treatment group.

< **Figure 17 here.** >

Figure 18 in the appendix shows that the job application workshop similarly mitigates the effects of space on the probability of self employment. Individuals in this experimental group do not search more intensely, but become more effective in job search. This suggests that effects of space 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 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 expect the workshop to be particularly useful for individuals with strong observable characteristics, who can benefit from being more able to convey their attractive employment characteristics.

Our results suggest the the ability to signal skills has improved for low-education jobseekers — the group which experienced the strongest effects from 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 26 and figure 15. We are unable to find similar effects when comparing low education jobseekers in the transport group to low education jobseekers in the control condition.[29] So this effect is unique to the job application workshop.

We also find that, among low-education individuals in the control group, high predicted skills are not associated with better labour market outcomes (see table 26). The labour market for workers with low levels of education does not seem to separate jobseekers on basis of the skills which we measure through our tests. This separation occurs, however, after we introduce the job application workshop program.

To measure predicted skills, we used characteristics that could be easily included in a CV, cover letter, or discussed in an interview. The orientation component of the job application workshop is aimed at making workers more effective in this part of the application process. The certification component, however, should enable workers to signal skills that are difficult to observe, and uncorrelated with basic jobseekers' 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.

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[29] This result is not reported, but is available upon request.

7.3 The role of certification

We look for evidence that the certification component of the workshop lead to better employment outcomes for individuals who scored well on the tests. If this were the case might expect only individuals who scored well on tests to benefit from having flattering signals, while low scoring individuals would lose out.

The certificates given to individuals in the job application workshop reported test scores in discrete bands. This allows us to study the impact of being placed in a higher band, controlling for the original test scores, in a regression discontinuity framework. Since raw marks were not reported to candidates, nor on their certificates, we would expect a discrete jump in employment outcomes across the reported bands.

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.[30]

We find no evidence that scoring in a higher band, across any of the test scores, leads to a significant jump 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 work with a written agreement.

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[30] 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.

Figure 16 illustrates this result, by showing the effect on having a permanent job of being above the median aggregate test score. The full set of results, using optimal and other bandwidth selections, are available on request.

< **Figure 16 here.** >

We interpret these results as suggestive evidence that the certificates did improve employment rates by improving the signals of the top candidates, while reducing the quality of signals of those candidates who scored badly. Rather the full set of activites during the workshop helped candidates to better signal their particular abilities in job applications. Two further pieces of evidence corroborate this hypothesis. First, self-reported data on the use of the certificate indicates that about 58 percent of the jobseekers who received the certificates and made at least one job application did not show the certificates to employers. Second, in the workshop group, we do not find a positive correlation between the residuals from the test score regressions and employment outcomes. This suggests that while workers invited to the workshop earn a premium for skills that are correlated with easy-to-observe characteristics, the labour market is still unable to separate workers on the basis of variation in skills that are idiosyncratic and harder to observe.

8   Conclusions

Cities in the developing world are growing at an unprecedented pace. A key challenge for policy makers is to identify effective interventions to support the large number of individuals who find it hard to access urban labour markets: the young, women and those without tertiary education. In this paper, we conduct a randomised evaluation of two interventions to improve the labour market prospects of young Ethiopian jobseekers. 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 the effects of 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. We also find that both interventions relax spatial constraints on occupational choices. 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. An interesting avenue for future research is to explore whether these programs can be successfully bundled with interventions that *increase* the skills of workers (Card et al., 2011; Attanasio et al., 2011; Hirshleifer et al., 2015). Experimental evaluation of these programs has so far revealed mixed results. However, these could be partly due to spatial and informational constraints that prevent trainees from reaping the full returns of their increased human capital.

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Figure 1: **Attrition (failure to complete one phone interview) from the Phone Survey by Month**
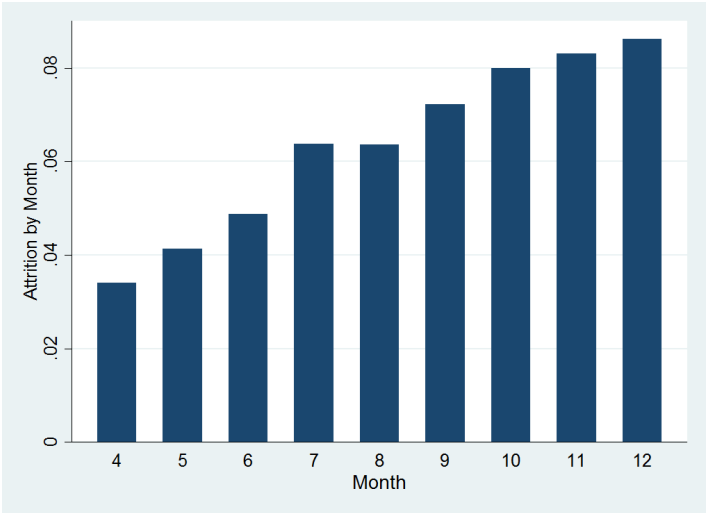
Figure 2: **Most Common Occupations: Control Group and Job Application Workshop Group**
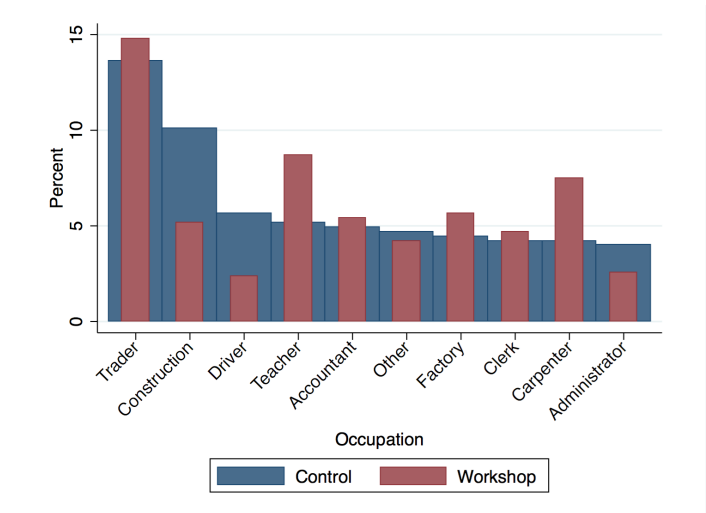
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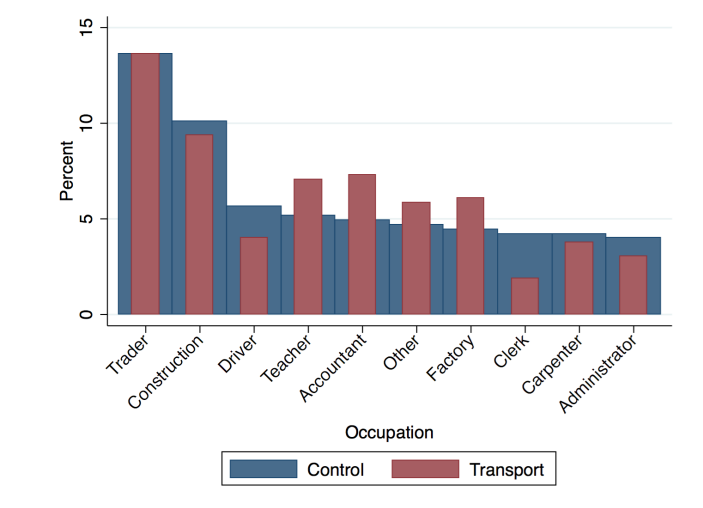
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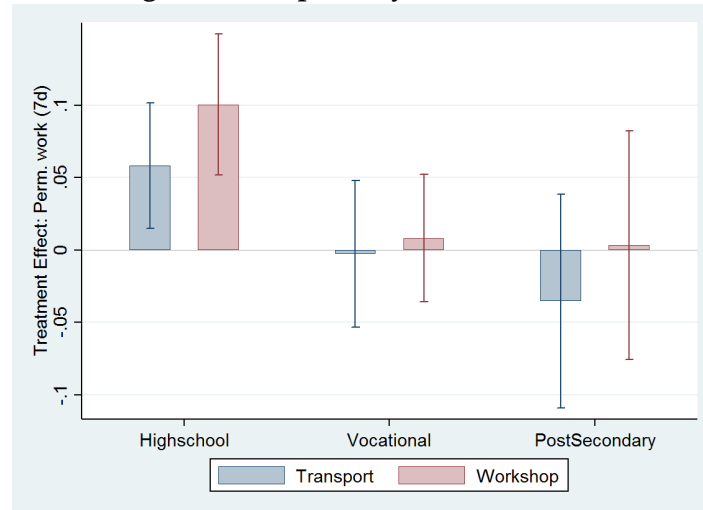
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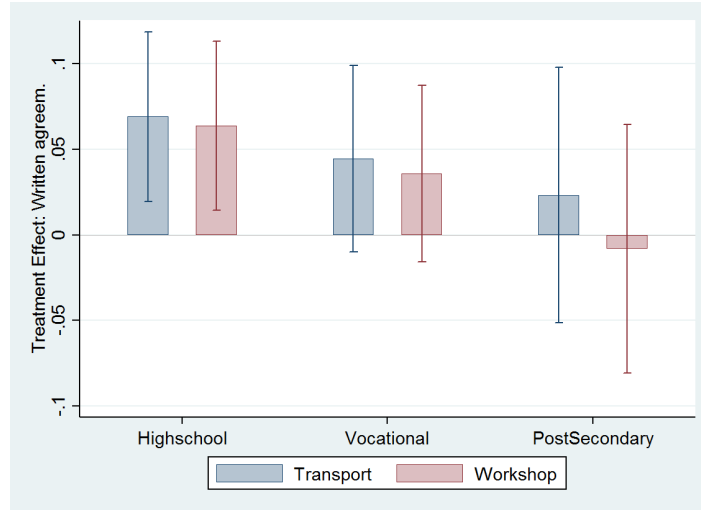
Figure 6: **Heterogeneous Impacts by Gender: Permanent Work**
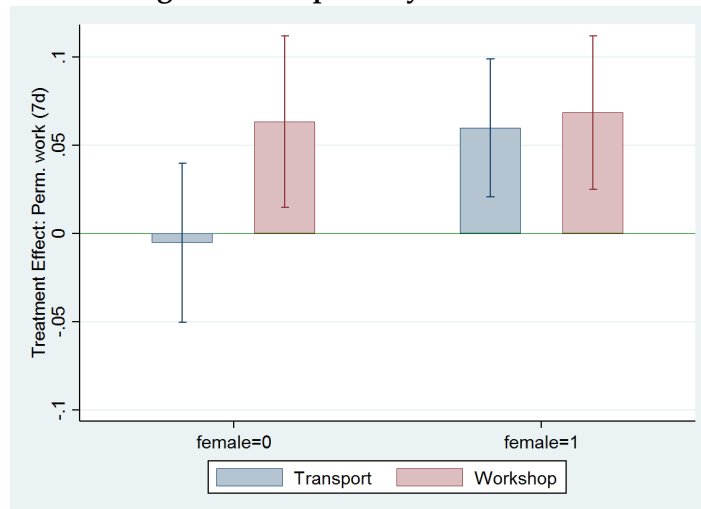
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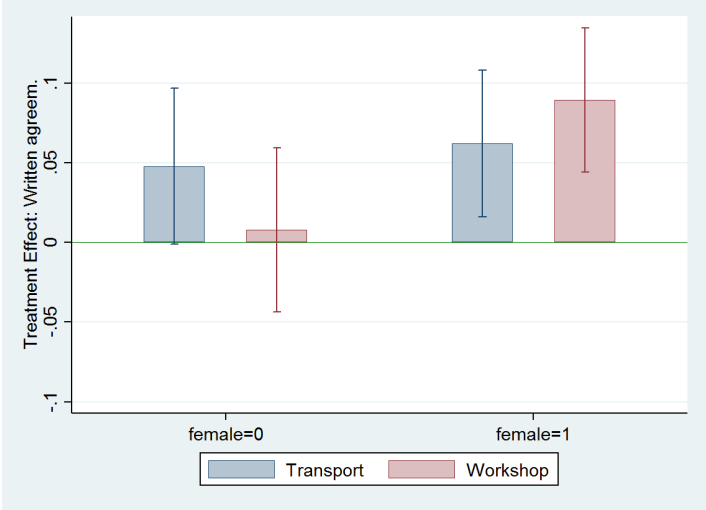
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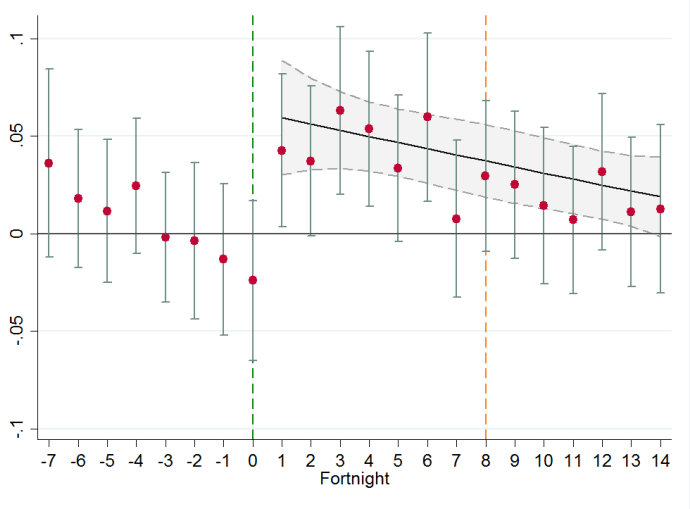
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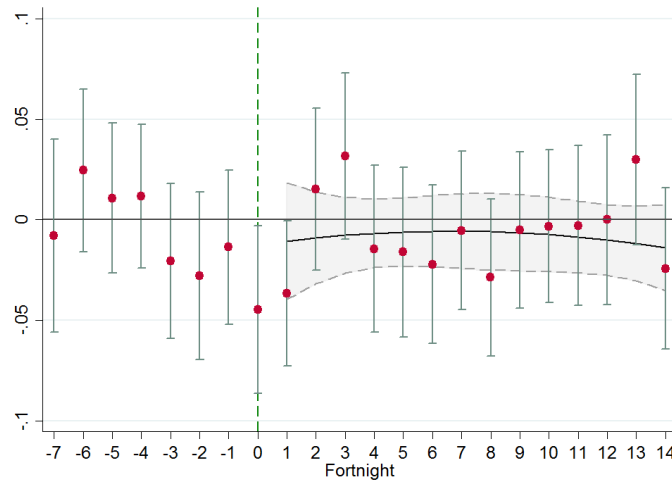
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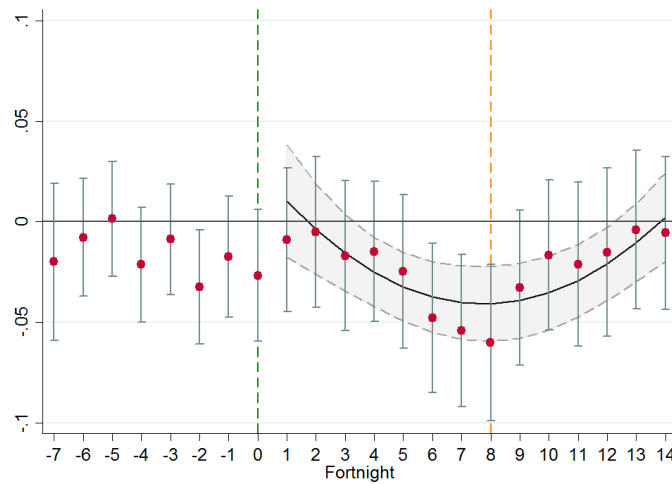
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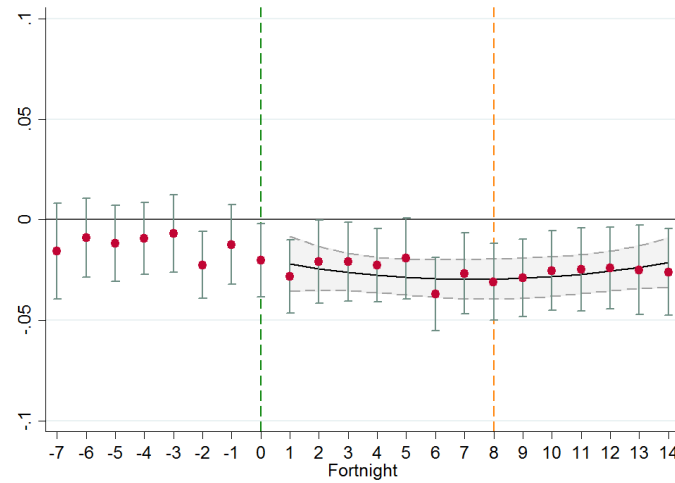
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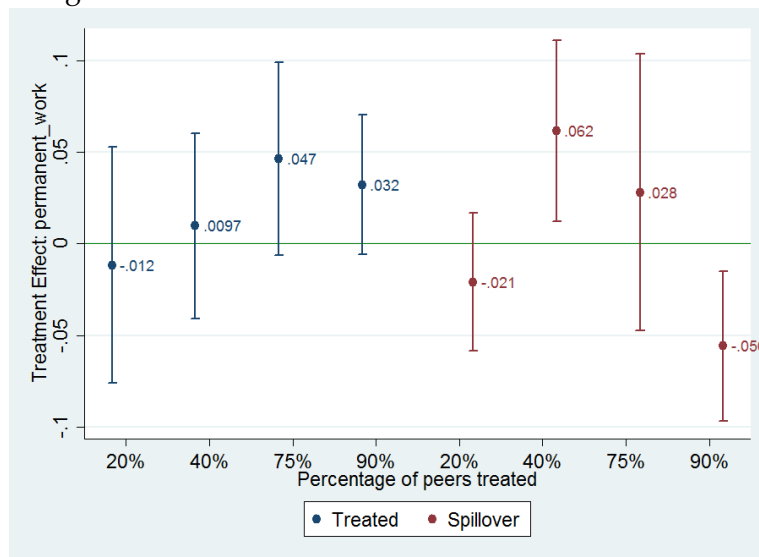
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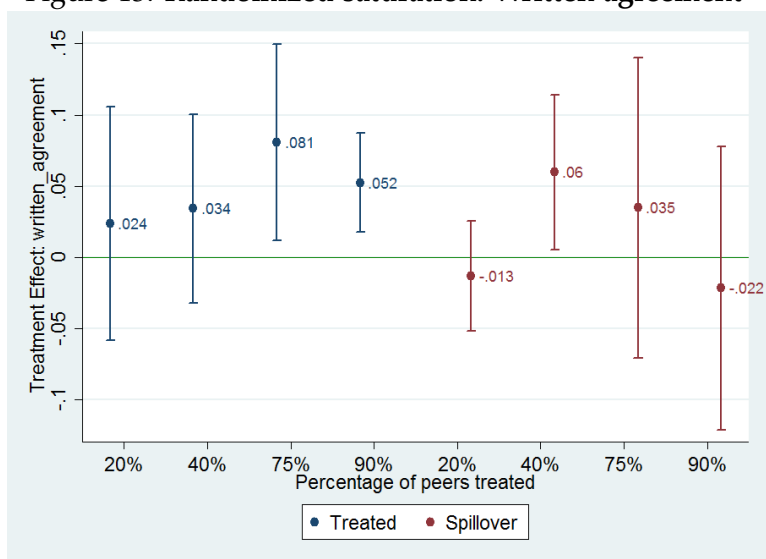
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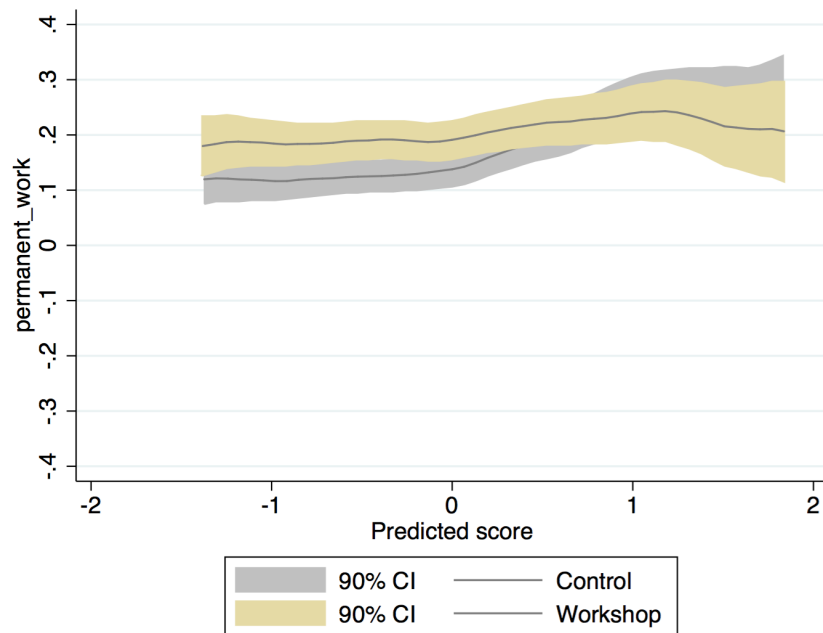
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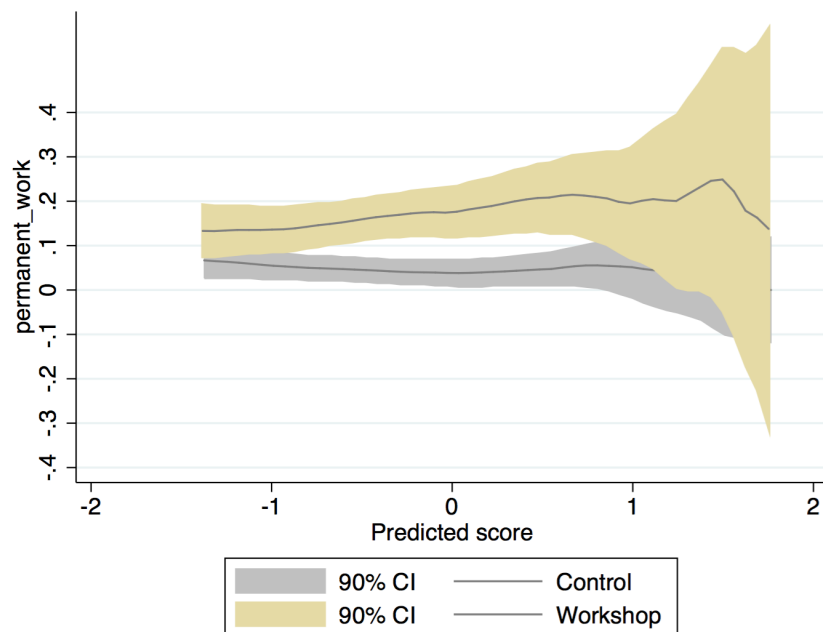
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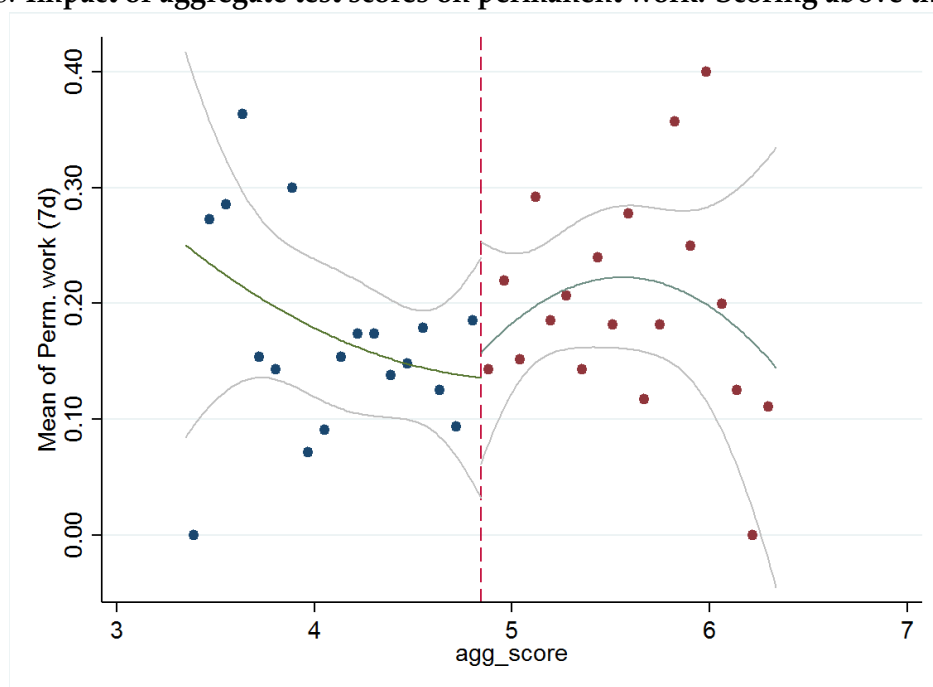
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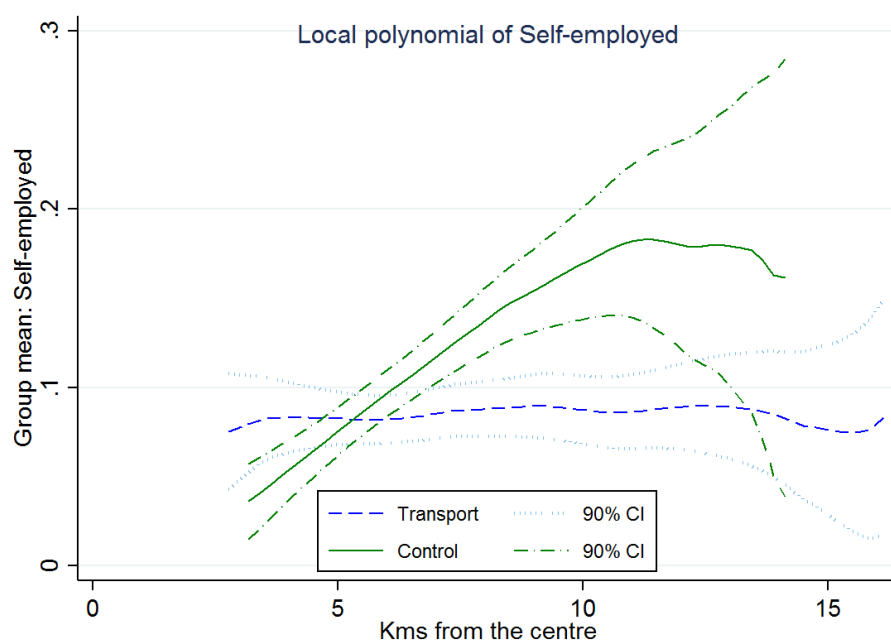
Figure 18: **Relationship between distance and self-employment: Impact of the job application workshop**
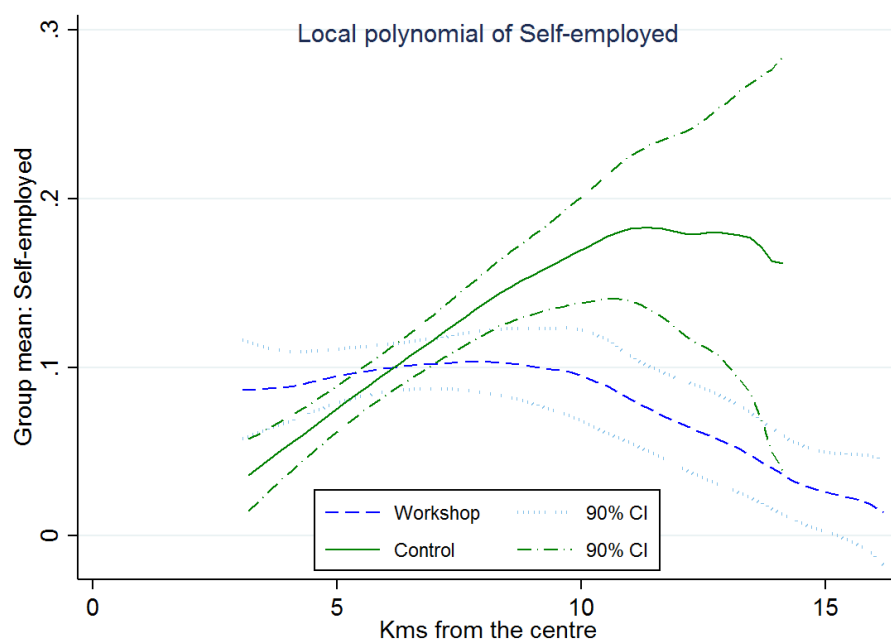
Table 1: **Final Assignment to Treatment for Individuals and Clusters**

	(1)	(2)	(3)	(4)
	Individuals			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 2: **Randomised Saturation Levels for the Transport Treatment**

	(1)	(2)	(3)	(4)
	Individuals		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 %

Table 3: Employment outcomes

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

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

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
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

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

Table 5: Family indices

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

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on the summary indices for different families of outcomes. These are obtained by least squares estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report a p-value in parenthesis and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. q-values are obtained using the sharpened procedure of Benjamini et al. (2006). 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 6: Effects on main outcomes by gender

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

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

Table 7: Effects on main outcomes by education

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

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.

Table 8: Effects on search outcomes by education

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

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

Table 9: **Sample selection before randomisation**

	Sample Size	No. Dropped	% dropped
Original baseline	4425		
Eligible at baseline	4388	37	0.84%
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		366	8.27%
Final Sample	4059		

Table 10: **Assignment to Start and End Weeks of the Transport Intervention**

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

Table 11: **Summary and Tests of Balance**

	N	Mean	S.Dev.	1st Q.	Median	3rd Q.	Min.	Max.	F-test P
degree	4055	0.18	0.39	0.00	0.00	0.00	0.0	1.0	0.654
vocational	4055	0.43	0.50	0.00	0.00	1.00	0.0	1.0	0.914
work	4055	0.30	0.46	0.00	0.00	1.00	0.0	1.0	0.710
search	4055	0.50	0.50	0.00	0.00	1.00	0.0	1.0	0.950
dipdeg	4055	0.25	0.43	0.00	0.00	1.00	0.0	1.0	0.933
female	4055	0.53	0.50	0.00	1.00	1.00	0.0	1.0	0.996
migrant_birth	4055	0.36	0.48	0.00	0.00	1.00	0.0	1.0	0.684
amhara	4055	0.44	0.50	0.00	0.00	1.00	0.0	1.0	0.366
oromo	4055	0.25	0.44	0.00	0.00	1.00	0.0	1.0	0.278
work_wage_6months	4055	0.45	0.50	0.00	0.00	1.00	0.0	1.0	0.452
married	4055	0.20	0.40	0.00	0.00	0.00	0.0	1.0	0.462
live_parents	4055	0.53	0.50	0.00	1.00	1.00	0.0	1.0	0.779
experience_perm	4055	0.13	0.33	0.00	0.00	0.00	0.0	1.0	0.430

search_6months	4055	0.75	0.43	0.00	1.00	1.00	0.0	1.0	0.565
respondent_age	4055	23.53	3.00	21.00	23.00	26.00	18.0	29.0	0.678
years_since_school	4050	38.35	259.95	1.00	3.00	5.00	0.0	1984.0	0.316
search_freq	4055	0.57	0.32	0.33	0.60	0.83	0.0	1.0	0.760
work_freq	4055	0.34	0.38	0.00	0.20	0.67	0.0	1.0	0.823
self_employed	4055	0.05	0.22	0.00	0.00	0.00	0.0	1.0	0.539
work_cas	4055	0.05	0.21	0.00	0.00	0.00	0.0	1.0	0.291
work_satisfaction	4055	0.09	0.28	0.00	0.00	0.00	0.0	1.0	0.496
total_savings	4055	2441.42	7232.39	0.00	0.00	2200.00	0.0	202998.0	0.320
res_wage	4014	1361.68	1072.74	800.00	1100.00	1700.00	0.0	20000.0	0.786
cent_dist	4055	5.91	2.57	3.76	5.31	7.57	2.5	12.7	0.269
travel	4050	1.90	2.10	0.00	1.00	3.00	0.0	18.0	0.391
written_agreement	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 13: **Variables Used for Re-Randomisation**

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

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search_freq	Proportion of weeks that individual searched for work (from the phone surveys)	Mean (over first 3 months of calls) of Dummy: $p1\_14 = 1$
work_freq	Proportion of weeks that the individuals worked (from the phone surveys)	Mean (over first 3 months of calls) of Dummy: $p1\_3 \neq 0$

Table 12: Predictors of Attrition

	(1) Attrition		(1) Attrition
Transport	-0.005	Respondent age	-0.000
	(0.017)		(0.0022)
Screening	-0.023	Born outside Addis	0.040***
	(0.017)		(0.014)
Spillover transport	-0.010	Amhara	-0.024
	(0.019)		(0.015)
Spillover screening	-0.014	Oromo	-0.026
	(0.026)		(0.017)
search freq	-0.064**	Wage empl (6m)	0.011
	(0.026)		(0.015)
work freq	-0.004	Married	-0.028
	(0.018)		(0.018)
Degree	-0.034***	Years since school	0.000
	(0.012)		(0.000029)
Worked (7d)	-0.044***	Lives with parents	-0.004
	(0.015)		(0.014)
Searched job (7d)	0.021	Ever had permanent job	0.003
	(0.016)		(0.018)
Female	0.022*	Searched job (6m)	-0.007
	(0.013)		(0.018)
Observations	3,045	R-squared	0.021
F-test (treatments)	0.560	F-test (covariates)	2.680
Prob > F	0.690	Prob > F	0.000

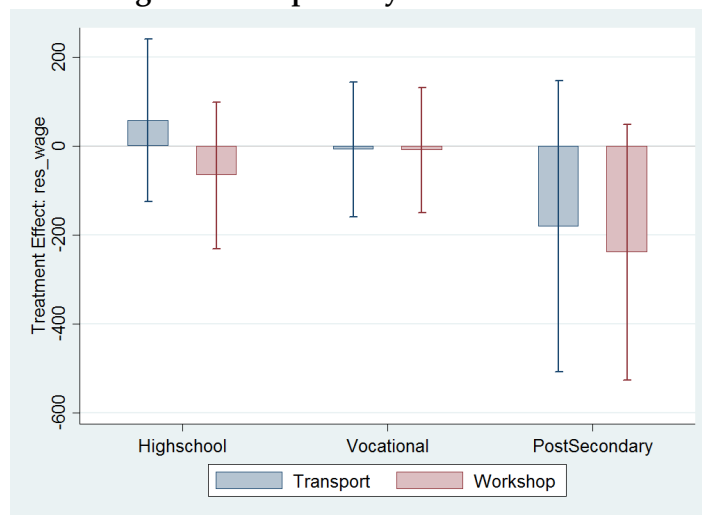Figure 19: **Heterogeneous Impacts by Education: Reservation wages**

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

Outcome	Transport			Job Application Workshop			Control Mean	
	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

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

Table 15: Effects on search outcomes by savings

Outcome	Transport			Job Application Workshop			Control Mean	
	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

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

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

Outcome	Transport			Job Application Workshop			Control Mean	
	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	0.0450	0.860	-0.0300	0.0740	0.0562	0.546	0.575
	(.42)	(.216)		(.48)	(.046)**			
	[1]	[.761]		[.725]	[.318]			
Hours worked	-0.0410	0.557	0.856	-1.882	2.245	0.201	26.10	26.20
	(.986)	(.788)		(.434)	(.249)			
	[1]	[1]		[.725]	[.47]			
Formal work	0.0690	0.0470	0.600	0.0450	0.0520	0.856	0.128	0.301
	(.014)**	(.109)		(.093)*	(.085)*			
	[.248]	[.547]		[.318]	[.318]			
Perm. work	0.0440	0.0240	0.554	0.0490	0.0770	0.462	0.116	0.215
	(.079)*	(.332)		(.068)*	(.005)***			
	[.547]	[1]		[.318]	[.073]*			
Self-employed	-0.0410	-0.00400	0.264	-0.0140	0.00700	0.519	0.125	0.0716
	(.086)*	(.852)		(.56)	(.743)			
	[.547]	[1]		[.725]	[.916]			
Monthly earnings	22.31	-7.386	0.845	92.92	69.42	0.889	1051	1221
	(.839)	(.942)		(.472)	(.526)			
	[1]	[1]		[.725]	[.725]			
Satis. with work	-0.0140	0.0170	0.555	0.0150	0.0390	0.632	0.255	0.222
	(.728)	(.633)		(.698)	(.247)			
	[1]	[1]		[.916]	[.47]			

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

Table 17: Other job quality measures

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
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

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.

Table 18: Financial outcomes

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
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

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.

Table 19: Expectations, aspirations, reservation wages

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
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

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.

Table 20: Mobility

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
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

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.

Table 21: Education and training

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
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

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.

Table 22: Psychological outcomes

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
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

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 23: Social networks

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	
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.
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.
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.
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.

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.

Table 24: Lee bounds

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

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

Table 25: Predicted skills and employment outcomes: all workers

	Work	Permanent work	Written agreement	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 corrected to allow for arbitrary correlation at the level of geographical clusters. The sample includes all individuals in the control group and in the job application group. *** p< 0.01, ** p <0.05, * p<0.1.

Table 26: Predicted skills and employment outcomes: workers with at most secondary education

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

Note. In each column we report the results of an ordinary least squares regression of the outcome in the column heading on predicted grades, a dummy for being invited to the job workshop and the interaction of these two variables. In parentheses, we report standard errors corrected to allow for arbitrary correlation at the level of geographical clusters. 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.