Anonymity or Distance?

Job Search and Labour Market Exclusion in a Growing African City*

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Abstract

We show that helping young job-seekers signal their skills to employers generates large and persistent improvements in their labour market outcomes. We do this by comparing an intervention that improves the ability to signal skills (the 'job application workshop') to a transport subsidy treatment designed to reduce the cost of job search. In the short-run, both interventions have large positive effects on the probability of finding a formal job. The workshop also increases the probability of having a stable job with an open-ended contract. Four years later, the workshop significantly increases earnings, job satisfaction, and employment duration, but the effects of the transport subsidy have dissipated. Gains are concentrated on individuals who generally have worse labour market outcomes. Overall, our findings highlight that young people possess valuable skills that are unobservable to employers. Making these skills observable generates earning gains that are far greater than the cost of the intervention.

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

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1 An experiment to help youth find better jobs

Helping young workers to find good jobs is one of the major policy challenges facing the world today. Young adults generally work less, earn less, and face more job insecurity than older workers. Why do young people suffer these poor labour market outcomes? The constraints they face are not fully understood, especially in developing countries (Kluve et al., 2019; McKenzie, 2017). In particular, the role of frictions in the search and matching process remains under-researched.

In this paper, we provide experimental evidence on two key matching frictions: job search costs and the inability to signal skills. These frictions are at the heart of two distinct and widely-held views on urban labour markets in developing countries. The first view is that the cost of job search is a crucial constraint in large, sprawling cities — as it prevents job-seekers from effectively gathering information about existing opportunities and applying for those that match them best. If this view is correct, policies that reduce search costs — such as subsidised or improved transport systems and online job posting — hold great promise. A second view is that the main difficulty faced by young job-seekers is to convey accurate information about their talents to employers. With little formal work experience and limited credentials, it may be particularly hard for young people to demonstrate their employability. If so, encouraging young job-seekers to increase their search effort may result in little or no improvement in their chances of attaining good jobs in the long run. Under this view, improving young people's ability to signal their skills would be more effective.

To investigate which of these two competing views is more accurate, we run an experiment with two parallel treatment arms. The first intervention — aimed at reducing the cost of job search — is a *transport subsidy*. Participants are reimbursed, up to three times a week, for the cost of a bus fare from their place of residence to the centre of the city, where they can find information about jobs and visit firms. The second intervention — aimed at improving the ability to signal skills — is a *job application workshop*. We certify young people's general skills using a mix of standardised personnel selection tests. Further, we offer orientation on how to signal skills in job applications and job interviews. The experiment is conducted with a representative sample of over 3,000 young people in Addis Ababa, Ethiopia. We evaluate these interventions with two endline surveys taking place eight months and four years after the end of treatment, respectively.

We find starkly different results from the two interventions. The transport intervention increases job-search intensity and increases the probability of having a formal job eight months after treatment. However, four years after treatment, these effects have dissipated completely. In other words, lowering the search cost gets young workers a formal job faster, but it does not change their long-term employment outcomes. The job application workshop, in contrast, shows long-lasting effects. In the short run it increases the probability of permanent as well as formal work *without* increasing the intensity of job search. Four years after treatment, the workshop shows a large positive impact on earnings, amounting to a 25% increase over the control group mean. These earning gains are particularly impressive when contrasted with the trajectory of individuals in the control group. While it is relatively easy for control individuals to find work — they reach a 70 percent employment rate by the second endline — higher salaries remain out of their reach: among respondents in the control group, wages only grow at roughly the rate of inflation.

These findings show that while both the inability to signal skills and the cost of job search are significant temporary barriers to formal employment, only the inability to signal skills impacts labour market outcomes in the long-run. To explain why this may be the case, we use a simple theoretical framework and several supporting empirical results. In the framework, firms and workers meet each other at a frequency determined by the intensity of workers' job search. When they meet, the firm observes a noisy signal of match quality (i.e. the suitability of the worker's skills for the position) and then decides whether to offer a formal employment contract. Policies that subsidise job search — such as the transport intervention — increase the rate at which firms and workers meet each other. As a result, workers get formal jobs faster. However, when job-search support is withdrawn, the control group progressively catches up and thus the treatment effect dissipates over time. Further, these policies do not change firms' ability to identify suitable workers and thus leave match quality unchanged. On the other hand, policies that improve the information jobseekers convey about themselves — such as the job application workshop — help workers to find formal jobs, but also enable firms to target their offers more effectively, and can thus improve match quality. Higher match quality will in turn translate into higher earnings, possibly with a delay due to wage-setting frictions. Unlike the employment effects, the match quality and earning effects will persist over time: the control group has access to a noisier matching technology and is thus not able to close the gap in employment quality.

We provide several pieces of evidence in support of the mechanisms proposed by our framework. In particular, we confirm the key prediction that the workshop will have persistent impacts on match quality, while the transport intervention will not. First, we show that the workshop affects two proxies of match quality: workers in the treatment group stay in the same job for significantly longer periods of time and their skills are better matched to their jobs. We do not find similar impacts on these proxies of match quality among the transport subsidies group.[1] Second, we show that the workshop generates long-run earnings growth by increasing wages, rather than by increasing hours worked or employment. These effects are robust to a standard correction for selection (Attanasio et al., 2011) and are sustained 20 months after workers got their current jobs: they are thus likely to reflect higher match quality and increased productivity on the job. Third, we show, using formal mediation analysis, that the bulk of the long-term increase in earnings can be accounted for by the initial change in match quality (measured by the proxies discussed above). Taken together, this evidence supports the conclusion that the job application workshop makes valuable skills more easily observable. This enables employers to better price and employ these skills and is thus likely to improve the allocation of young people's talent, generating net gains for the economy even if the total number of jobs remains constant.[2] We also investigate the specific role played by the certificates by using the fact that marks are reported in discrete bands. In a regression discontinuity framework, we find suggestive, albeit noisy, evidence that being placed in a higher band is associated with higher earnings. This suggests that the certification component and the information it produces drive at least part of the overall effect of this intervention.

Finally, we show that improving the ability of workers to signal skills has the potential to reduce inequality in labour market outcomes. Our theoretical framework predicts that workers who belong to groups that traditionally fare poorly in the labour market (e.g. inexperienced workers and those without strong formal qualifications) stand to gain the most from the job application workshop. When a credible signaling technology is not available, employers will make negative inferences about the skills of these workers based on their observable group membership. Providing them with credible signals of skills will make these inferences unnecessary. Our results strongly confirm this prediction, since both the short-run and long-run gains from the workshop are concentrated among the socio-demographic groups that have the worst labour markets outcomes in the control group. This heterogeneity in treatment effects is large and, as a result, the job application workshop leads to a sizable reduction in earnings inequality. For example, at the time of the second endline, we observe a 34 percent earnings gap between control individuals who had permanent work experience at the beginning of the study and those who did not; this gap is eliminated for young people in the workshop group.

This paper makes a contribution to the literature on labour markets in developing coun-

[1] The permanent employment impacts of the workshop — which are significantly higher than those of the transport intervention — also confirm that match quality has increased, as they show that employers feel more confident about the skills of treated applicants and are thus more likely to enter into long-term commitments with them.

[2] In section 6, we also discuss how, in an equilibrium where firms have unfilled vacancies, better signals about workers' skills could help firms fill more vacancies and thus increase overall employment in the economy. Further, we explore a number of issues that may emerge when the intervention is offered at scale.

tries by providing empirical evidence that information asymmetries hinder the quality of youth employment. To our knowledge, this is the first paper to show that young people in a developing country have valuable unobserved skills that, once certified, generate substantial long-term earnings gains. In addition, this is also, to the best of our knowledge, the only study demonstrating the effectiveness of a cost-effective, scalable intervention to enable young job-seekers with no job experience to signal their skills. Pallais (2014) and Abel et al. (2020) demonstrate the informational content of reference letters from past employers, but these are only available to workers with previous work experience. In contrast, we independently verify the skills of unemployed workers, many of whom have never been in permanent employment before. In contrast to Bassi and Nansamba (2017), who reveal information about workers' skills in a controlled setting of arranged meetings between workers and firms, our intervention does not require a collaboration with firms: workers independently choose whether and how to use their improved signals. Our workshop can be implemented with any individuals, regardless of their previous work experience, educational background, and the labour market in which they are searching. This allows us to make general statements about the role of information in the workings of this labour market, and makes our intervention easy to scale up. Our findings also complement a related literature studying the role of information provision in developed economies — notably Altmann et al. (2015), who find positive effects of a brochure designed to encourage job search among disadvantaged communities, and Belot et al. (2015), who improve search efficacy through job suggestions in an online market.

Further, this is the first study that directly compares the impacts of two different active labour market interventions and, in doing so, is able to quantify the relative importance of two types of labour market frictions. In line with Franklin (2017) and Phillips (2014), who study the short-term impacts of transport subsidies on non-representative samples, we find confirmation that search costs are a significant barrier to job search. However, in our representative sample these effects are weak and ultimately short lived. These findings also complement a recent literature showing that transport subsidies have persistent effects when they connect rural workers to urban jobs (Bryan et al., 2014): such interventions relax constraints that are likely to be different from those at play in a population already exposed to an urban labour market like ours [3]

Our study overcomes some of the shortcomings in the recent experimental literature on active labour market interventions in developing economies (as reviewed, for example, by

[3] A final strand of this literature tries to match job-seekers to firms by recommending candidates for specific vacancies (Groh et al., 2015), or by organising job fairs that lower search barriers for both workers and firms (Beam, 2016; Abebe et al., 2017). These interventions have not produced detectable effect on employment or earnings. Abebe et al. (2017) is a companion field experiment to this paper, which uses an additional sample of job-seekers drawn from the same population.

McKenzie (2017)). First, as mentioned above, we work with a large representative sample that we follow up to four years after the intervention. In comparison, other studies often rely on populations of youth selected along a particular economic dimension (e.g., whether they have been searching for work, or are part of a specific government program), and they typically document short-term impacts only. Second, we have low attrition, even in the four-year follow-up survey.[4] Third, we follow a pre-analysis plan that specifies all of our main outcomes of interest.[5] This enables us to formally control for multiple hypotheses testing — all of our main results are robust to this correction — and it eliminates concerns about selective reporting. Fourth, we combine face-to-face survey data with a high-frequency phone questionnaire. This enables us to document the mechanisms through which job-seekers find better jobs and to analyse their immediate response to each intervention in a way that recall data would not permit. Lastly, we are able to study the key issue of match quality by using data on long-run earnings and employment duration, which few studies in this literature have been able to do.

From a policy perspective, our results emphasise the value of intervening early in workers' careers to limit the scarring effects of a bad start. An intervention like our job application workshop represents a viable and effective policy instrument to serve this objective. Indeed, we show that helping young people to signal their skills is a remarkably cost-effective option. The job application workshop generates an average wage gain of USD 10 per month per worker, for a one-off cost of USD 18.20 per individual. This benefit-to-cost ratio comfortably exceeds that of other interventions documented in the literature recently reviewed by McKenzie (2017).[6]

2 Interventions and conceptual framework

2.1 The challenge of matching young workers with good jobs in developing countries

In many developing countries, where informal employment is readily available, employment rates are often high by international standards and workers typically work longer hours compared to developed countries (Feng et al., 2017; Bick et al., 2018). However,

[4] We were able to find more than 85% of respondents in the four-year follow-up survey. We are 3.5 percentage points more likely to find respondents in the workshop sample than in the control sample (a statistically significant difference, with $p = 0.08$); in section 3, we show that our results are robust to allowing for differential attrition.

[5] This plan was registered at `www.socialscienceregistry.org/trials/911`.

[6] The long-term benefit from the workshop also stands in contrast with recent results from the cash transfer literature, which suggest that the earning impact from increased entrepreneurial activity is relatively short-lived (Haushofer and Shapiro, 2018).

available jobs are often of poor quality: they offer limited formal protections, low tenure security, and slow wage growth (Banerjee and Duflo, 2007; AfDB, 2012; Lagakos et al., 2018), and job separations are more frequent than in developed countries (Donovan et al., 2018). These challenges are particularly severe for young workers.

The labour market in Addis Ababa, the growing capital city of Ethiopia where this study is conducted, exemplifies these broad trends. First, informal work is very common and very often temporary and unstable. While 65% of the young individuals in the control group of our study are employed by the time of the second endline survey, only 25% have a formal job with an open-ended contract (throughout the rest of the paper, we define a job that has a written contract with the employer as a *formal job*, and a job that has an open-ended contract with no fixed duration as a *permanent job*). This is representative of the outcomes of young people under the age of 30 in the city; older workers, on the other hand, have rates of formal permanent employment that are 40 percent higher than those of the under 30s (see Table A.3 in the Online Appendix). Second, real wage growth is weak, particularly for informal and short-term jobs. In our sample, the earnings of control group workers do not grow in real terms in the three years between the two endline surveys. Individuals who are employed at both endlines experience some real wage growth over the same period, but this is about twice as high for people in formal and permanent employment compared to workers in informal or temporary jobs. Stable jobs with formal contracts are thus highly sought by young Ethiopians.[7] Third, worker turnover rates are high, pointing specifically to poor match quality. For example, in a sample of 500 local employers, we find that churning (defined as hires and separations over and beyond those required by adjustments in firm size) accounts for about 65 percent of all worker flows — a higher figure than what Kerr (2018) recently reports for South Africa. Worker turnover is also the most commonly reported HR problem among firms in the same survey.[8]

It is unclear what precisely prevents high-quality matches from forming. While causal evidence on this question is scarce, several pieces of descriptive evidence suggest that search frictions can be a major driver of low match quality in developing countries' labour markets. The cost of job search constitutes a first likely source of friction. In Addis Ababa, for example, a significant amount of information about jobs is disseminated through job

[7] When asked what kind of work they were looking for, 64% said they were looking specifically for a permanent contract. Further, we find that young people are almost twice as likely to say that they would like to stay in their current job in the very long run if they have an open-ended contract.

[8] The firm survey is described in detail in Abebe et al. (2017). We observe similar patterns of turnover and churning using the one-year phone panel survey that we collected for this study (and which we discuss in detail in Section 3 below). Average employment spells among jobseekers in this panel are short (72% of jobs are terminated within the first three months) and irregular (temporary workers did not work on average 12% of the weeks since they got the job, compared to only 2% for permanent workers), and job insecurity is high (in 82% of job terminations, the worker is unable to find another job right away).

vacancy boards located in the centre of the city.[9] Effective job search thus requires frequent trips to the centre of town to consult this information. In addition, job-seekers need to spend money to buy newspapers, print CVs and cover letters, and travel to employers for job applications and interviews. As a result, the median baseline job-search expenditure for an active job-seeker amounts to about 16 percent of his or her overall expenditure.[10] These costs are larger for people who live farther away from the city centre. In our baseline, we document that living 10 km closer to the centre of the city is associated with visiting the job boards 6.7 more times in a year (0.4 of a standard deviation) and making 1.9 more applications to permanent jobs (0.5 of a standard deviation).

A second potential source of friction relates to information about skills. In Addis Ababa, firms often mention that the recent expansion of the higher education system has made it more challenging to identify high-ability candidates. Further, career advice or job search assistance is almost completely lacking from high-school and university curricula. Many young job-seekers are thus not 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 the absence of good signals about skills and ability, firms often resort to selecting workers on the basis of previous work experience or job referrals (Serneels, 2007; Caria, 2015).[11] This puts young people at a disadvantage, as they have little work experience and less extensive networks,[12] but is also inefficient for firms. Anecdotally, managers often complain about the poor quality of job applications and express a demand for job-search training to be implemented as part of the education system.

In light of these challenges, we devised two interventions to reduce the cost of job search and help workers to signal their abilities to employers. Among the available options, we chose two relatively low-cost interventions that could be easily implemented in other contexts. In the rest of this section we describe these interventions and we then use a simple theoretical framework to motivate a number of testable predictions about the effects of these interventions in a labour market where job search is costly and firms are uncertain about worker skills.

[9] 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.

[10] This goes up to 25 percent for job-seekers who report searching 6 days a week.

[11] 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.

[12] 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.

2.2 Treatment 1: The job application workshop

The job application workshop is designed to improve job-seekers' ability to present their skills accurately to potential employers, thus overcoming the challenge of anonymity that youth with limited work experience typically face. The intervention has two components: an orientation session and a certification session. The orientation session helps participants to make more effective use of their existing signals (job experience, education, etc). In the certification session, we certify skills that are 'hard to observe' for employers, such as cognitive ability, and we provide participants with an instrument (the certificates) to signal those skills. The design aims to mimic the orientation services available to job-seekers in several countries.[13]

The intervention takes place over two days. On the first day, participants take a series of personnel selection tests. On the second day, they attend the orientation session. The intervention was 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; many firms screen applicants using tests developed, and sometimes administered, by the School of Commerce.[14]

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. The certification session includes four tests: (i) a Raven matrices test, (ii) a test of linguistic ability in Amharic, (iii) a test of mathematical ability and (iv) a 'work-sample' test. The results of the tests are presented in a certificate, which job-seekers can use in support of their job applications. The certificates are officially issued by the School of Commerce and the Ethiopian Development Research Institute.[15] 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. 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%. This enables us to investigate the effects of the information disclosed in the certificates in a regression discontinuity framework.

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

[14] In the firm survey we introduced above, we find that about 40 percent of firms know about the personnel selection services offered by the School of Commerce. 80 percent of these firms report that they trust the services offered by the School of Commerce.

[15] Participants collect the final certificates from the School of Commerce, after all testing sessions are completed. To minimise threats to external validity, we made no references to the University of Oxford in the certificates. Employers wishing to receive additional information could contact the School of Commerce.

We chose the tests on the basis of the results of several qualitative interviews with firm managers in the city.[16] The Raven test is a widely used measure of cognitive ability (Raven, 2000). It is believed to be one of the best predictors of worker productivity (Schmidt and Hunter, 1998; Chamorro-Premuzic and Furnham, 2010) and it has been used by economists to measure worker quality in several contexts (Dal Bó et al., 2013; Beaman et al., 2013). The tests of mathematical and linguistic ability were designed to capture general mathematical and linguistic skills, as in the OECD's PIAAC survey or the World Bank's STEP survey (OECD, 2013; Pierre et al., 2014). The 'work-sample' test captures participants' ability to carry out simple work tasks: taking minutes during a business meeting, carrying out a data entry task under time pressure, and meeting a deadline to complete a data entry task at home. The literature in organisational psychology suggests that 'work-sample' tests can be used alongside measures of cognitive ability to predict worker performance (Schmidt and Hunter, 1998). We report some summary statistics of the tests in Table A.1 of the Appendix.[17] Per person, the intervention cost about 35 USD, including fixed costs related to developing the tests. Excluding these fixed costs, the sum is 18.2 USD — a figure in line with other recent information interventions (Dammert et al., 2015; Bassi and Nansamba, 2017).

2.3 Treatment 2: The transport subsidy

Individuals in this treatment group are offered a subsidy to cover the cost of traveling to the city centre. The subsidy takes the form of a cash transfer that is conditional on visiting a disbursement point, located in an office in the centre of Addis Ababa. The centre of the city is where most employers are located (Figure A.1). Further, the office is located close to the major job vacancy boards and to a central bus station, from which buses leave to destinations all around Addis Ababa. Recipients are required to attend in person, and to show photographic ID on each visit. Each recipient can collect cash once a day, up to three times a week. The daily amount is sufficient to cover the cost of a return bus fare from the participant's area of residence at baseline to the disbursement point. We calibrate the subsidy to allow participants to travel on minibuses. Study participants can in principle walk to the office or use less expensive large public buses — an inferior means of transport that is crowded and infrequent — and save a part of the transfer. Qualitative evidence suggests that this is not common. Further, we do not find that individuals in this treatment group increase their savings during the weeks of the intervention. To access the subsidy,

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

[17] We document substantial variation in performance for all the tests we administered. For example, the distribution of Raven test scores has a maximum of 56 correctly answered questions (out of 60), a minimum of 0, a mean of 30.5, and a standard deviation of 13.

job-seekers need to have (or borrow) enough cash to make the first journey — which in our setting is almost always the case.[18]

Prior to the intervention, respondents in our sample do not travel frequently to the city centre.[19] By paying participants conditional upon their presence at our office, we directly subsidise travel to the centre. This allows us to focus on spatial constraints to job search.[20] We hypothesize that the intervention works to reduce the costs of traveling to the centre to gather information about jobs and to visit firms located near the city centre. This could lead unemployed youth to gather information about more vacancies, and therefore increase the probability of finding an opportunity for which they are well suited, or to make more job applications (which require in person trips to the firms' locations), or both.

The median subsidy available on a given day is equal to 20 Ethiopian Birr (1 USD at the exchange rate at the beginning of the intervention). This equals about two thirds of the median weekly expenditure on job search at baseline, and 10 percent of overall weekly expenditure. The minimum amount is 15 ETB (0.75 USD) and the maximum 30 ETB (1.5 USD). On average, each person in this treatment group receives a transfer of about 191 ETB (9.3 USD). The full cost of the intervention, which comprises both direct transfers and other variable costs, is 19.8 USD per person. For logistical reasons, we stagger the start time and the end time of the subsidy, randomly. This generates variation across individuals in the number of weeks during which the treatment is available, and in the time of treatment. The number of weeks of treatment varied from 13 to 20, with a median of 16 weeks.[21] The intervention was implemented between September 2014 and January 2015.

2.4 Conceptual framework

To guide intuition about the likely effects of these two treatments — and the mechanisms by which such effects might operate — we now discuss a simple conceptual framework (which

[18] While job-seekers have little cash on hand, our data shows that most of them have at least enough to pay for one journey, in the knowledge that this money will be reimbursed. About 95 percent of job-seekers in our sample have at least 15 ETB in savings, while 75 percent of job-seekers have at least 10 ETB available as cash-on-hand or at home. See Franklin (2017) for further discussion of this issue.

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

[20] We tried to minimise priming and experimenter demand effects as much as possible. When we contacted respondents to offer the subsidy, we explained that the program was designed to help them travel to the city centre. We gave no further instruction on how to use the money.

[21] In principle, a job-seeker who finds a job in the centre of Addis Ababa before the end of treatment can use the transfer to subsidise his or her commute to work. In practice, this is very rare. We calculate that only 6 percent of the disbursements were given to individuals who had found permanent employment. As some of these jobs would be based outside of the centre of town, 6 percent should be considered as an upper bound of the proportion of disbursements that subsidised commuting. This is consistent with the fact that, as we discuss in the Results section below, the intervention does not significantly affect savings or expenditure (Table A.18).

the Online Appendix outlines in detail). We are particularly interested to explore how our interventions can affect (i) the probability of formal employment,[22] and (ii) the quality of a match between employer and employee. In order to focus on the direct impacts on these outcomes, we deliberately do not allow behavioural responses through reservation wages, and we abstract away from general equilibrium considerations. The framework is thus stylised. In Section 5 we present some evidence showing that reservation wages are indeed not affected by our interventions. Further, we come back to the issue of general equilibrium effects in Section 6.

The framework is built around two key labour market frictions: (i) it takes time for a worker to find a vacancy (Rogerson et al., 2005), and (ii) firms make offers on the basis of match quality, but observe match quality with noise (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Kahn and Lange, 2014; Pallais, 2014). To capture the latter friction, our framework assumes that when an unemployed worker is matched to a job vacancy, the firm observes a signal about match quality. This signal comprises both (i) true match quality (specific to a worker-firm pair) and (ii) idiosyncratic noise. The firm thus faces a signal-processing problem and will use Bayes' rule to form a posterior belief about the quality of a prospective match. Uncertainty about match quality is likely to be costly in this context: poor hiring decisions may decrease productivity or lead the firm to fire the worker (which requires the firm to pay severance pay) and screen other candidates. We thus allow firms to be moderately risk averse in their hiring preferences: ceteris paribus, firms prefer applicants with tighter signals and will hire the applicant only if the firm's expected utility exceeds some threshold.

To capture the first friction, we assume that job search take place over multiple periods of time and that, in every period, jobseekers find a vacancy with a probability that is less than one. The frequency at which jobseekers find vacancies is a reduced form parameter that reflects the intensity of job search and that can be changed by external interventions. After a worker has found a vacancy, the expected outcome of the signal-processing decision then determines the probability that the worker is hired. For simplicity, we do not allow offers to be rejected or jobs to be destroyed (so that employment rates grow constantly with time as we observe in our empirical data).

[22] As discussed in section 2.1, the formality of employment is a key dimension of job quality in Ethiopia. A second dimension that we emphasize in the paper is whether the contract is permanent or temporary. Empirically, formal employment and permanent employment are correlated (partly because informal work rarely comes with any guarantees of stable employment and partly because both of these are desirable job attributes in our context). However, one key distinction between these two concepts comes from the fact that permanent formal jobs impose additional firing costs compared to temporary formal jobs (which have no firing costs once the end of the contract is reached). A permanent contract may thus also be a signal of match quality: the firm would be reluctant to offer such a contract unless it was sufficiently convinced of the quality of the match with the applicant. To reflect this distinction, we focus the model on the search for formal work, and interpret permanent work as one of our proxies of match quality.

This setup immediately suggests two stylised ways in which active labour market interventions might seek to improve employment prospects. First, an intervention might encourage a job-seeker to increase the rate at which vacancies are viewed. Our transport intervention clearly falls into this class of policy. In our conceptual framework, we can represent this by having the individual match with firms at higher frequency. Second, an intervention might decrease the asymmetry of information about workers' skills. Our workshop intervention matches this description; in our signal-processing framework, the effect of the workshop can be captured by an increase in the precision of the signal observed by the firm. Intuitively, we can think of the former class of policy as improving the *intensity* of search ('searching harder'), and the latter class of policy as improving the *efficacy* of search ('searching better', in the sense of having a higher probability of converting a contact with a vacancy into a match).

We make two observations about these different strategies to improve employment outcomes. First, there is no reason in theory why either strategy should be more effective, and hence why an intervention should outperform the other. This will depend on the strength of each friction, which we investigate empirically using our experiment. Second, the reduction in search costs provided by our transport subsidy is only temporary and the search-efficacy effect of the workshop is also likely to weaken over time (as the skills acquired by jobseekers depreciate and the results of the tests become progressively outdated). When treatment ends, unemployed people in the treatment group have the same probability of finding a job as unemployed people in the control group. However, there are more unemployed people in the control group at that point. Thus, more control workers find jobs in each period compared to treated workers and the treatment effect on formal employment will dissipate gradually.[23] In light of this, we obtain the first two predictions of our framework:

Prediction 1 (effect on formal employment): *Both the transport intervention and the workshop intervention will increase the rate of employment in formal jobs. These effects progressively dissipate after job-search support is withdrawn.*

Prediction 2 (search intensity vs. search efficacy): *The interventions generate these effects through different mechanisms. The transport intervention increases the number of job vacancies that are viewed during the treatment period; the workshop intervention does not. Instead, the workshop increases the probability that a worker is offered a job after viewing a vacancy.*

[23] The prediction that the treatment effect on employment declines with time does not hinge on the assumption that job search support is temporary. It also does not depend on whether the impacts on search intensity and efficacy stop as soon as treatment ends or taper off gradually. When control employment rates are on an upward trajectory, as is the case in our framework and empirical setting, permanent shocks to search intensity or signal accuracy would also generate treatment effects on employment that start to decline after a given period of time.

Our framework also predicts that the workshop will raise match quality — as it will improve the firm's ability to select high-productivity workers — and the effect should be sustained in the long run as the control group has access to a worse signalling technology and thus cannot close the gap in match quality. The transport subsidy, on the other hand, will not change match quality: workers will be screened by more firms, but the efficiency of this screening process (and the expected quality of the matches it produces) will not change.

Prediction 3 (effect on match quality): *The workshop intervention leads to a persistent increase in the quality of the match; the transport intervention does not.*

In our empirical analysis, we will look at a number of proxies for match quality, but our main outcome of interest will be wage earnings. It is widely accepted that wage earnings at least partly reflect labour productivity, and thus match quality. However, in practice, it may take some time for the earning effects to materialize as firms may be constrained by compressed salary scales (Breza et al., 2017) or may use career incentives and thus delay workers' match-quality compensation to raise effort (Lazear, 1979, 2018). For these reasons, earnings might only rise in line with increased productivity in the long run.

Finally, our framework makes a prediction about the heterogeneity of impacts. To think about heterogeneity, we introduce an additional variable in our signal processing model: an observable covariate that correlates with match quality. To this point, we have considered heterogeneity only in unobservable match quality and noise. We now consider what happens if firms have some observable proxy for suitability and use it for statistical discrimination, to compensate for signal noise. As that noise is reduced by the workshop, such discrimination becomes less severe. The final prediction follows directly from this result:

Prediction 4 (heterogeneity of impacts): *The effect of the workshop intervention is higher for individuals with worse observable characteristics.*

3 Experimental design and estimation strategy

3.1 The sample

To obtain our experimental sample, we began by drawing a random selection of geographic clusters from the list of Ethiopian Central Agency (CSA) enumeration areas.[24] Given our interest in spatial constraints, we excluded all clusters within 2.5 km from the city centre

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

and those outside the city boundaries. To minimise potential spillovers between clusters, our sampling method ensured that we did not select any directly adjacent clusters.

Within our selected clusters, we sought respondents of direct interest to active labour market policies. Specifically, we used door-to-door sampling to construct a list of all individuals who: (i) were between 18 and 29 years of age; (ii) had completed high school; (iii) were available to start working in the next three months; and (iv) were not currently working in a permanent job or enrolled in full time education. We randomly sampled individuals from this list to be included in the study. Our lists included individuals with different levels of education. We sampled with higher frequency from the groups with higher education, to ensure that individuals with vocational training and university degrees are well represented in the study; we estimate using appropriate sampling weights. In all, we interviewed 3,052 individuals who are included in our experimental study in 179 clusters.[25]

How does our sample compare to the youth population of Addis Ababa? The Online Appendix shows that individuals in our experiment are on average more educated than the overall youth population (Table A.2).[26] This is due to the fact that we exclude from our study all job-seekers who have not completed high-school. On the other hand, since we only focus on individuals who do not have a permanent job at baseline, workers in our sample have significantly worse labour market outcomes than the general population, including among those with comparable education levels (Table A.3). Overall, we estimate that about 20% of all youth in Addis Ababa would be eligible for our study.

3.2 Data collection: Face-to-face and the phone survey

We collected data on study participants through both face-to-face and phone interviews. We completed baseline face-to-face interviews between May and July 2014 and endline face-to-face interviews between June and August 2015; we then completed long-term follow-up interviews, by phone, in May 2018. These interviews recorded information about the socio-demographic characteristics of study participants, their education, work history, finances, expectations and attitudes. The bulk of survey focussed on labour market outcomes.

[25] We initially completed baseline interviews with 4,388 eligible respondents. Before assigning treatments, we attempted to contact all of them by phone and dropped individuals who could not be reached after three attempts over a period of one month (this helped us curtail problems of attrition, by excluding respondents who were likely to attrite.). We also dropped any individual who had found a permanent job by the time treatments were assigned (and had retained it for at least six weeks). Finally, we dropped individuals who had migrated away from Addis Ababa. This left us with 4,059 individuals. 1,007 of them were assigned to a separate unrelated treatment, which is the subject of a different study (Abebe et al., 2017). Table A.4 in the Online Appendix shows how many individuals were dropped from the sample at each point and the reasons for them being dropped.

[26] We obtain representative data on the population of Addis Ababa from the 2013 Labour Force Survey.

Throughout the paper, we measure wages using a one-month recall period as reported by respondents.[27] We also collected an incentivised measure of present bias. We did not inform study participants at baseline that some of them would be offered job search assistance.

Between the baseline and the first endline, we also constructed a rich, high-frequency panel through fortnightly phone interviews. In these interviews we administered a short questionnaire focused on job search and employment. These questions were asked in exactly the same way (e.g. using as much as possible the same wording) as the questions in face-to-face surveys.[28]

3.3 Randomisation

We randomly assigned geographic clusters to one of the treatment arms or the control group. To ensure balance, we created blocks of clusters with similar baseline observables and randomly assigned clusters within each block to the different treatment groups (Bruhn and McKenzie, 2009).[29] In addition, we implemented a randomised saturation design, whereby we varied the proportion of sampled individuals in treated clusters who were offered treatment. We randomly assigned individuals within each treated cluster to a treatment or a control group.[30] This was done by blocking individuals within clusters by their education level, and implementing a simple re-randomisation rule. The overall assignment to treatment is outlined in Table 1. The randomized saturation rule is used to look at the spillover effects of the intervention through social networks. We do not focus on the results from this design in the paper. Instead we discuss this design, and the main results, in an Online Appendix, Section A.3.

< Table 1 here. >

[27] By 2018, 88% of salaries are paid monthly.

[28] Franklin (2017) shows that high-frequency phone surveys of this type are reliable, in the sense of not generating Hawthorne effects.

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

[30] In addition, individuals designated to receive the transport intervention were randomly assigned to a start and an end week. This is illustrated in Table A.5.

3.4 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 A.6 in the Online Appendix. For each baseline outcome of interest, we report the p-values for a test of the null hypothesis that all experimental groups are balanced. We cannot reject this null for any of the variables analysed.

Attrition is low, especially compared to other studies of young adults in urban developing country contexts (Baird et al., 2011; Blattman et al., 2014). In the first endline survey, we find 93.5% of all participants, and attrition is uncorrelated with treatment.[31] Table A.8 in the Online Appendix presents the full analysis.[32] Attrition in the phone survey is also low: below 5% in the early months of the calls. While it increases in later weeks, we are still able to contact more than 90% of respondents in the final month of the phone survey. Figure A.2 in the Online Appendix shows the trajectory of monthly attrition rates over the course of the phone survey. In the long-term follow-up survey attrition has increased, but we are still able to find more than 85% of respondents, a very high number over such a long period of time. Columns (3) and (4) of Table A.8 shows the correlates of attrition in this sample. We do find that individuals in the workshop sample were slightly less likely to attrite in the second endline. The difference in response rates between workshop and control is 3.5 percentage points ($p = 0.08$), which is not unusually large for this literature (Blattman et al., 2014). We conduct detailed sensitivity tests, using methods suggested by Karlan and Valdivia (2011), which allow us to conclude that our main result from the long-term follow-up (the earnings impact of the workshop) is not driven by differential attrition. We present this analysis in Online Appendix A.2.

3.5 Take-up

Take-up is substantial for both treatments. 50% of individuals in the transport group collect the cash at least once. Of these, 81% return to collect the subsidy again. Those who collect the subsidies for at least two weeks tend to be dedicated users. Conditional on ever collecting the money, 74% of respondents take it at least once a week over the course of the entire study, with an average of 16 collections in total.

Further, 61% of individuals who are invited to the job application workshop attend it.

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

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

80% of those attending later collect the certificates from the School of Commerce. Take-up rates do not vary substantially with observable covariates.[33]

3.6 Estimation strategy

Our primary objective is to estimate the effects of the programs on the labour market outcomes of study participants. For each outcome at endline (both the 8-month and the 4-year endline), we estimate the following equation:

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

where y_{ic} is the endline outcome for individual i in cluster c and $\boldsymbol{x}_{ic0}$ is the vector of baseline covariate values that were used for re-randomisation and blocking. $\texttt{treat}_{fic}$ is a dummy capturing whether an individual has been offered treatment f. Thus, our estimates measure the *intent-to-treat* impacts of the interventions. The variable $\texttt{spillover}_{fic}$ is a dummy that identifies control individuals residing in clusters assigned to treatment f. Thus, γ_f captures the spillover effects of treatment f. We report the estimates of these spillover effects in Appendix A.3. We correct standard errors to allow for correlation within geographical clusters and we use sampling weights to obtain average treatment effects for the eligible population as a whole.[34]

In the pre-analysis plan, we specify a family of six primary employment outcomes. For each one of them we test the null hypothesis that each treatment had no impact. We use 'sharpened' q-values to deal with multiple comparisons (Benjamini et al., 2006). The q-values control the false discovery rate within the family of six hypotheses that we test for each program.[35] We also specify two families of intermediate outcomes that help us elucidate what mechanisms drive the primary effects, and seven families of secondary outcomes.

[33] In Table A.9 in the appendix we report the correlates of take-up. We find that individuals who search frequently before the roll-out of the interventions are significantly more likely to use the transport subsidy and to attend the workshop. Further, individuals born outside of Addis Ababa are 7 percentage points more likely to use the transport subsidy. We find no evidence that the individuals who attend the workshop are positively selected. For example, individuals who have completed higher levels of education or have more work experience are not more likely to attend the workshop.

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

[35] The 'sharpened' q-value procedure is designed for the case of independent or positively dependent test statistics (Benjamini and Yekutieli, 2001; Benjamini et al., 2006). This is likely to apply in this study, as all main outcomes have positive covariance and treatment is likely to affect these outcomes in the same direction.

To measure treatment effects on the outcomes obtained from the high-frequency phone interviews conducted prior to the first endline, we estimate the following model:

$$y_{itc} = \sum_{f} \sum_{w=S_f}^{E_f} \left[\beta_{fw} \cdot \texttt{treat}_{fic} \cdot d_{wit} + \gamma_{fw} \cdot \texttt{spillover}_{fic} \cdot d_{wit} \right] + \alpha_t \cdot y_{itc,pre} + \boldsymbol{\delta} \cdot \boldsymbol{x}_{ic0} + \eta_t + \mu_{itc}, \tag{2}$$

where w indicates the number of fortnights since each treated individual began receiving his/her treatment.[36] d_{wit} is a dummy variable equal to 1 in period t if an individual started receiving their treatment w periods ago.[37] Individuals in the control group have all such dummy variables set to 0. Thus, β_{fw} is our estimate of the impact of intervention f, w fortnights after the intervention started.[38]

We then estimate the trajectory of treatment effects by pooling all post treatment ($w > 0$) observations and estimating quadratic trends of the treatment effects over time. To do this, we estimate equation 2, subject to the following quadratic constraints on β_{fw} and γ_{fw}:

$$\beta_{fw} = \begin{cases} 0 & \text{if } w \leq 0; \\ \phi_{f0} + \phi_{f1} \cdot w + \phi_{f2} \cdot w^2 & \text{if } w > 0; \end{cases} \tag{3}$$

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

4 Treatment Impacts

In this section, we discuss the main impacts of our interventions (and, in doing so, we test Prediction 1 from our model). 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. We focus the discussion on six outcomes that we pre-specified as primary. These outcomes – which describe employment,

[36] $w = 0$ in the fortnight when the treatment started, and is negative for fortnights before that.

[37] 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. Similarly, for an individual who starts treatment in fortnight 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 fortnights for which we will be able to estimate the treatment effect relative to the start fortnight of the treatment will differ by treatment. In the notation above S_f denotes the earliest fortnight for which we will be able to estimate a treatment effect for treatment f. E_f denotes the final fortnight.

[38] We allow the effect of the baseline control term $y_{ic,pre}$ to vary over time by estimating α_t for each time period, while we estimate time-invariant effects of individual covariates x_{ic0}. η_t is a time-specific intercept term.

employment quality, and earnings – are the typical targets of active labour market policies. In the remainder of this section, we focus entirely on primary pre-specified outcomes, measured at both one-year and four-year follow-ups.[39]

4.1 Short-run impacts

Table 2 reports the short and long-run impacts of the interventions on our primary outcomes. In the short run, we find no significant average treatment effects on the probability of having a job, on hours worked, on earnings or on job satisfaction. This is consistent with existing meta-analyses, which show that active labour market policies lead to negligible changes in employment and earnings over short horizons.[40]

< Table 2 here. >

However, our interventions are successful in increasing jobseekers' chances of attaining *good jobs* in the short run, as indicated by the impacts on two key indicators of job quality: whether work is formal (in the sense of having a written contract), and permanent (in the sense of not having a specified end date). As we foreshadowed, both characteristics are highly sought by jobseekers — for whom temporary work is often relatively easy to obtain. Specifically, the application workshop increases the probability of working in a permanent job by nearly 60 percent (raising the share of workers in permanent employment by 6.9 percentage points from a level of 12 percent in the control group). As a result of the job application workshop, the gap in permanent employment between youth and older workers is reduced by about 20 percent. The effect is statistically significant at the 1 percent level and remains highly significant after correcting for multiple comparisons. The transport treatment, on the other hand, had a modest impact on permanent employment, which is significantly smaller than the effect of the workshop. We also find that both interventions increase workers' chances to have a formal job by about 30 percent. Only 17 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 bounding exercises making various assumptions about the distribution of outcomes for missing observations, including the use of Lee Bounds (see Section A.2 in the Online Appendix).

[39] We wrote our pre-analysis plan in preparation for the analysis of the short-run impacts of the interventions. We reproduce the same pre-specified analysis for the long-run impacts.

[40] Over similar short time horizons, existing meta-analyses show that active labour market policies increase employment rates by about 1.6-2 percentage points and earnings by about 7 percent, on average (Card et al., 2015; McKenzie, 2017). The effect sizes that we document are in line with these figures. Employment rates increase by 3.7 percentage points for individuals in the transport treatment, and by 2 percentage points for individuals who were invited to the job-application workshop (both statistically insignificant).

4.2 Long-run impacts

The results from our long-term follow-up show significant gains from improved signalling from the workshop, and almost no long-run effects of the transport subsidies. Specifically, we find that the job application workshop has large and significant positive long-run impacts on earnings and job satisfaction. We report these impacts in the last four columns of Table 2. Four years after the intervention, individuals in this treatment group earn 25 percent more than the individuals in the control group. This is a substantial increase, which corresponds to about half of the earnings premium associated with vocational (tertiary) education in our data and to 60 percent of the control group nominal earning growth between the two endline surveys. The effect is statistically significant at the 5 percent level, and is robust to the correction for multiple comparisons. We also detect a 7 percentage point increase in job satisfaction (a 12 percent gain over the control mean), though this effect is measured less precisely.

Additional checks (available in the Online Appendix) show that the earnings effect of the workshop is robust to estimating the effect on log wages and to winsorizing at the top of the distribution to eliminate outliers (Table A.10). Further, quantile regressions show that the effects are large and significant across the distribution of earnings (Table A.12).[41] Figure 3 corroborates this conclusion by showing a clear rightward shift of the earnings distribution for workshop participants compared to the control group. A Kolmogorov-Smirnov test of the equality of the distributions produces a D-statistic of 0.0647 (with a p-value of 0.115 for the two-sided test, and 0.058 for the one-sided test where the alternative hypothesis is that earnings are higher in the treatment group compared to the control).[42] In Section A.2 of the Online Appendix we show that the effect of the workshop on earnings is not driven

[41] We find significant effects from the 60th to 90th percentiles (note that earnings take on positive values from 40th percentile and up).

[42] These results are for 2018 wage earnings — our main variable of interest. This variable does not include profits from self-employment and assigns a value of 0 to all individuals that do not have a wage-paying job. Throughout the rest of the paper, we will refer to this variable as 'wage earnings' or simply as 'earnings'. Our results are robust to using several alternative definitions of this outcome variable — in particular, (i) a broader measure of earnings which we obtain by summing wage earnings and profits from self-employment and (ii) a conditional measure of wage earnings that assigns a missing value to all individuals that do not have a wage-paying job. Throughout the rest of the paper, we will refer to the first variable as 'total earnings' and to the second variable as 'wages'. Table A.11 shows that impacts on total earnings are, if anything, larger than the effects on wage earnings alone. Similarly, Table A.13 shows quantile regressions for total earnings in 2018 which confirm the results for wage earnings reported in Table A.12. For total earnings, we document significant effects from the 45th percentile and up. Finally, we can reject the equality of the distribution of total earnings in the workshop and control groups using a Kolmogorov-Smirnov test (p-value of 0.066 for the two-sided test). If we separately consider only profits from self-employment, we find no effects — something that is not surprising, given the substantial noise in self-employment profits, and given that our intervention was directed solely at improving access to wage employment. Similarly, we also find no extensive-margin effect on the probability of self-employment either. We report results on wages in the following section.

by differential rates of attrition: the result is robust to a number of assumptions about the distribution of outcomes for missing observations.

< **Figure 3 here.** >

Four years after the intervention, formal employment rates in the workshop group are very similar to those of the control group. Between 2015 and 2018, the treatment effect on this variable has dropped by a significant 5 percentage points (p=0.068). This finding is consistent with our stylised framework, which predicts that control individuals eventually catch up in terms of the probability of having a formal job. Further, the workshop does not have long-term impacts on overall employment rates or on permanent employment. Second-endline employment rates among treated individuals are an insignificant 2.7 percentage points higher than in the control group. Permanent employment rates have also equalised, which is not surprising, given that by the second endline the correlation between formal and permanent work is high.[43]

The gains from the transport subsidy dissipate after the first endline survey. Four years after the interventions, permanent and formal employment rates in the transport subsidy group are not statistically different from those in the control group. The treatment effects on both of these variables are also significantly different compared to the treatment effects at the first endline (p=0.023 for permanent work and p=0.071 for formal work, respectively). The distribution of earnings in the transport and control group look remarkably similar (see Figure A.3) and we cannot reject equality of the two distributions using a Kolmogorov-Smirnov test (p = 0.996). Recall data suggests that the initial 3.3 percentage points effect on permanent employment was eroded quickly after the 1-year follow up (Figure A.5). There are also no significant long-run impacts on earnings or job satisfaction. In particular, the impact on earnings of the transport subsidy is about ten times smaller than that of the workshop, a difference which is significant at the 5 percent level. Finally, we document that individuals in the transport intervention group are about 5.8 percentage points less likely to be in employment. This effect is significant at the 10 percent level, but is not robust to the correction for multiple comparisons and we thus do not interpret it further. Overall, these findings are in line with our stylised framework, which predicts no long run effects

[43] As we have already discussed, improved rates of finding formal jobs will not automatically translate into higher employment rates in a setting where informal work is readily available. However, in the first endline we also document an increase in permanent work. The stability offered by permanent employment could enable treated individuals to work for more weeks each year, compared to control individuals, and thus to be more likely to work on any given week. We have some suggestive empirical evidence of this in the recall data plotted in Figure A.4: the employment impacts of the workshop grew from 2 percentage points eight months after treatment, to a significant 5 percentage points in the second year after the intervention, and then decreased again to 2.7 points. Further, we show in Figure A.5 that the effect on permanent employment gradually decreased over time. We do not have recall data on formal employment.

of the transport subsidy, since it does not lead to an increase in match quality, as discussed in the next section.

5 Mechanisms

In this section, we present evidence on the mechanisms that generate our results. We do this by investigating predictions 2 to 4 of our stylised framework (we confirmed prediction 1 in the previous section).[44] Our analysis shows that the interventions operate through the hypothesised mechanisms, that match quality improves on several dimensions, and that the pattern of heterogeneity is consistent with our predictions.

5.1 Prediction 2: How did treated individuals get better jobs?

Our framework predicts that we should observe an increase in search intensity in response to the transport intervention, and an improvement in search efficacy in response to the workshop. We look at each of these predictions in what follows.

5.1.1 Search intensity

We find that the transport intervention causes people to search for work more frequently, while the workshop does not lead to any change in search effort. We show this by estimating the fortnightly impact of each intervention on the probability of searching for work using equation 2. When the transport subsidy is available, treated individuals are about 12.5 percent more likely to look for work than control individuals (a 5 percentage point effect over a control mean of 40%, as shown in Panel (a) of Figure 1). This effect decreases linearly after the end of the transport intervention. We also find that when the transport subsidy is available, treated individuals are about 9 percentage points more likely to visit the job vacancy boards, where formal jobs are typically advertised (see Panel (b) of Figure 1). This is an increase of nearly 30 percent over a control mean of 28%.[45] Finally, treated respondents are more likely to travel to the centre of the city while the subsidies are in

[44] The analysis presented in section 5.1 is pre-registered, with the exception of the regression discontinuity design. The regressions reported in section 5.2 are not pre-registered and should be treated as exploratory. Finally, in section 5.3 we study a number of pre-specified dimensions of heterogeneity and then summarise them with a measure of expected earnings that was not pre-specified.

[45] We also document a contemporaneous, temporary reduction in the probability of working (Figure A.6). This is in line with the results reported in Franklin (2017) and is consistent with a model where individuals are unable to search optimally due to credit constraints (Herkenhoff et al., 2016; Abebe et al., 2018). When resources for job search are exhausted, credit constrained job-seekers are forced to accept poorly-matched jobs.

place (see Figure A.7).[46] These findings help to explain why the increase in search intensity translates into the effects on formal work discussed above: most formal jobs, regardless of firm location, are advertised at the central job boards, while informal jobs are generally not.

The job application workshop, on the other hand, does not affect the likelihood of searching for a job (Figure 2) or the number of job applications sent (Table A.15). This is notable and consistent with the hypothesis that financial constraints prevent job-seekers from increasing search effort: if the workshop did motivate job-seekers to search harder, they would appear to lack the resources to do so.[47]

< Figure 1 here. >

< Figure 2 here. >

5.1.2 Search efficacy

We also find evidence that the workshop increases search efficacy. First, the results above show that individuals in the workshop treatment are significantly more likely to obtain formal and permanent jobs while doing the same amount of job search as individuals in the control group. This is consistent with the prediction of our framework: the workshop does not relax the constraints that prevent individuals from intensifying job search, but it makes firms more likely to extend a job offer to treated individuals. To quantify this effect, we compute the conversion rate of applications to offers for permanent jobs. In Table A.15 we show that the workshop improves this conversion rate in the time period between the baseline and the first endline survey (eight months after treatment). People in the control group receive an average of one offer for a permanent job every 7.2 applications. The workshop brings this down to one offer every 5.2 applications. The magnitude of the effect is meaningful, but our estimates are noisy: the effect is significant at the 10 percent level and has a q-value above standard levels of significance.

Second, we leverage the fact that our certificates report test scores in discrete bands and make no mention of the candidate's precise test score.[48] This allows us to study the impact of being placed in a higher band, while controlling for the precise test score, in a regression discontinuity framework. If our workshop treatment operated primarily through

[46] By the time of the endline interview, we cannot find significant effects on the number of trips to the centre of the city made in the previous seven days. Consistently with this, we do not find significant effects on whether individuals work outside of their *woreda* (a broadly defined administrative unit within the city). This is likely to be because workers choose jobs that do not require long commutes.

[47] We find no impacts on other measures and methods of job search.

[48] There is no other way for study participants to access information about their original score.

a certification mechanism, we would expect large discrete improvements in employment prospects at band cut-offs. We perform this analysis for the aggregate score (a summary measure of all test results) and, to maximise power, we normalise this score and pool the data for all discontinuities together.[49] We find that being placed in a higher band generates a large, but noisily estimated increase in earnings in 2018 (see Table A.14). When we use the optimal bandwidth (Imbens and Kalyanaraman, 2012), we find that being just above the cut-off leads to a large increase in earnings of 0.36 standard deviations, which is marginally insignificant ($p = 0.146$). We then explore robustness to the use of bandwidths that are respectively half and twice the optimal values. We find that the effect is as large as 0.46 standard deviations and is significant at the 10 percent level when we use the larger bandwidth. Overall, while somewhat noisy, this evidence suggests that the certification element of the intervention contributes to the overall treatment effect. Further, it is consistent with our prediction that the workshop increases search effectiveness by providing information about skills.[50]

5.1.3 Other channels

In addition to testing the effects of the interventions on the primary employment and job search outcomes, we evaluate their impacts on a range of pre-specified secondary outcomes, including worker expectations and aspirations, mobility, and social networks (the full set of results is available in Tables A.16 to A.23 of the empirical appendix). Overall, we find little evidence that our interventions have changed outcomes in these areas: we are unable to find significant changes in any of the family indices and none of the individual tests is robust to our correction for multiple comparisons.[51] Importantly, we do not find significant changes in beliefs or aspirations, which may have plausibly been affected by the certification component of the job application workshop. In sum, these results suggest that the interventions work directly through the hypothesised channels of job search intensity and skills signalling.

[49] To do this, we first divide the score data in bins around each cut-off point (using the midpoints of the intervals between cut-offs). We then normalise the score in two steps. We subtract the bin-specific cut-off score and divide by the bin-specific standard deviation.

[50] On the other hand, we are unable to find evidence of impacts on a dimension of match quality that we further discuss below: employment duration. The estimates of this model are noisier than those of the earnings model, perhaps because we are working with a recall variable. Further, the weaker effects may be due to the fact that the skills of the small group of individuals close to the discontinuity are not well represented by the average skill level in either of the adjacent bands. Thus, for this group, being placed in a higher band does not necessarily make it easier to find the right job.

[51] In addition to investigating each outcome in a family separately, we use a standard 'omnibus' approach: we construct an index for each family and test whether the index is affected by our treatments (see Table A.16 in the appendix). For inference, we proceed as before: we report both p-values and false discovery rate q-values by treating each index as a separate member of a 'super-family' of indices.

5.2 Prediction 3: Did match quality increase?

5.2.1 Direct evidence on match quality

Several results indicate that the job application workshop improves job match quality, as predicted by our framework, but that the transport subsidies do not. In Table 3, we offer two pieces of evidence in support of this interpretation. First, we show that the earning effects reported in Section 4 are driven by higher wages (which can be reasonably expected to track productivity and the quality of matches in the long run, as discussed above) and not by selection into employment. In particular, earnings conditional on employment increase by 563 ETB, or 22 percent. We follow Attanasio et al. (2011) and compute bounds for these effects that account for potential selection of high and low earning individuals into employment. The lower bound of the effect on earnings conditional on employment is 405 ETB (a 16 percent increase) and the upper bound is 720 ETB (a 28 percent increase). This shows that our impacts on earnings are driven by higher wages, consistently with our predictions.

< Table 3 here. >

Second, we show positive impacts on two proxies of match quality: employment duration and skills use. Employment duration is often considered the most effective indicator of match quality as a longer tenure shows that both the firm and the employee value the match. To measure employment duration, in the second endline survey we collect information on the longest spell of work with a single employer that study participants have completed. We find that the duration of this work spell significantly increases by about 10 percent when young people are offered the job application workshop. Second, we collect information about whether individuals work in jobs where they make regular use of skills they have acquired in previous jobs or at school. This captures a different dimension of match quality — the effective sorting of skills and tasks. We find that individuals who receive the workshop are eight percentage points more likely to work in jobs where they employ their existing skills (conditional on having a job). We find no such evidence of improved match quality for the group receiving the transport subsidy.

Further, the short-term effects on formal and permanent work discussed earlier confirm that only the job application workshop improves match quality. In Table 2 we show that while both treatments have similar effects on formal work, the job application workshop increases permanent employment by about twice as much as the transport subsidies – a statistically significant difference. Open-ended work contracts impose higher firing costs on firms compared to fixed-term contracts. Recruiters are unlikely to offer permanent positions unless they are confident of the worker's ability to perform on the job. Thus, the differential impact on permanent work is consistent with the hypothesis that match quality is higher as a result of the workshop. In our theoretical framework, this unique match quality effect of the workshop is what causes the divergence in earnings between the two treatments. We explore this point in more detail in the next subsection.

Finally, we find some evidence that the workshop helps young workers sort out of occupations with worse career prospects (Figure A.8). In particular, in 2015, individuals in the workshop group are significantly less likely to be working in construction ($p = 0.025$), an occupation associated with very high rates of turnover, low earnings (at the time of the second endline), and poor working conditions.

5.2.2 The timing of the effects: how higher match quality translates into higher earnings

Our framework predicts that the workshop will first impact match quality and then affect earnings. As we discuss in Section 2, earnings may reflect match quality with a significant delay caused by wage-setting frictions. This could explain why we find immediate evidence of match quality improvements, but we only observe impacts on earnings in the second endline. If this is true, we should find that the initial improvements in match quality — as measured by having a permanent work contract and by the longest employment spell — drive the long-run earning effects of the intervention. To show this, we proceed in two steps. First, we show that our proxies of match quality are correlated with earnings in the second endline. Second, we carry out a formal mediation analysis using the techniques discussed in Acharya et al. (2016).

In the first step (presented in Table A.24), we show that our proxies for match quality predict 2018 earnings among control group individuals. In particular, controlling for individual characteristics and for employment and hours worked in 2015, we find that having a permanent job in 2015 is significantly and positively correlated with 2018 earnings. Similarly, we find that the duration of the longest employment spell is a significant predictor of 2018 earnings. By contrast, having any employment in 2015 is not correlated with 2018 earnings once we control for employment quality. These regressions support the hypothesis that improved match quality drives the treatment effect on earnings, but do not quantify the precise contribution that it makes.

In the second step, we use mediation analysis to quantify the share of the treatment effect on 2018 earnings that can be accounted for by the initial change in match quality. Following the recommendations by Acharya et al. (2016), we compute the Average Controlled Direct Effect (ACDE) of the workshop on long-run earnings, fixing the potential mediators of interest (the three proxies of match quality). The ACDE captures the impact of an intervention when a particular mediator is not allowed to respond to treatment and thus, by construction, cannot drive the treatment effect on the outcome of interest.[52] We can thus assess the importance of a given mediator by comparing the original treatment effect to the ACDE: if the mediator accounts for a large share of the impact of the intervention, the ACDE will be much smaller than the original treatment effect. We show these comparisons in Figure 4. We find that a large share of the earning impacts (63%) can be explained by the changes in our proxies for match quality. When looking at each proxy individually, we find that the length of the longest employment spell alone mediates 46 percent of the earnings effect. Further, permanent work at endline 1 can explain 22 percent of the long-run effect on earnings. Overall, this analysis is consistent with the prediction of our model: the earning effects are driven by the improvements in match quality.

< Figure 4 here. >

5.3 Prediction 4: Who benefits the most from the workshop?

Our stylised framework predicts that the workshop treatment should have a stronger effect among job-seekers whose observable characteristics correlate with lower labour market success — and who, therefore, are at a greater disadvantage when approaching prospective employers. We confirm this prediction by examining the heterogeneity in effects by baseline jobseeker characteristics, and by showing that patterns of heterogeneity are similar across 2015 and 2018 outcomes.

Specifically, we conduct a sub-group analysis using the list of covariates specified in our pre-analysis plan. In Table 4 we show different treatment effects on 2018 wage earnings for different values of several baseline covariates; in each case, the covariate is coded such that 'Covariate = 0' refers to the group that, in general, might be expected to face greater labour market disadvantage. Across a wide range of covariates, we find that the effect on

[52] Acharya et al. (2016) propose to estimate the ACDE in two steps. First, one runs a regression of the outcome variable on the mediators of interest, the treatment dummies, a set of controls, and the interaction between the mediators and all other variables. One then computes the predicted value of the outcome when all mediators are fixed to have value zero. This predicted value captures the variation of the outcome that cannot be explained by the variation in the mediators. Second, one regresses the predicted value on the treatment dummies. The treatment effect estimated by this regression corresponds to the ACDE. In an experimental setting, the key identification assumption required by this procedure rules out omitted variables that, conditional on all controls, are correlated with the mediator and the outcome of interest.

earnings is substantial for the more disadvantaged category.[53] For example, job-seekers without tertiary education experience an effect of about 60 percent of the control mean, and the effect of the workshop is significantly larger for this lower educated group than it is for individuals who have some tertiary education.

To summarise across multiple pre-specified dimensions, we report in the final row of Table 4 an 'endogenous stratification' exercise suggested by Abadie et al. (2018). Since this analysis was not included in the pre-analysis plan, it should be seen as an aggregation exercise: it is prompted by the results from pre-specified hypotheses, and seeks to generalise the insights from these regressions. To implement this approach, we stratify by predicted earnings at endline. In a first stage, we use a linear regression to predict endline (2018) earnings using our pre-specified baseline covariates. We then use a 'split sample' method to estimate treatment heterogeneity between high predicted earnings and low predicted earnings individuals (Abadie et al., 2018). The results show that the effect for the low-predicted-earnings group is large, and substantially larger than for those with high predicted earnings (indeed, we can reject the null hypothesis that the effects are equal between groups: $p = 0.070$). The estimated effect size for the low-predicted-earnings group is about 50% of the control mean.

We also conduct sub-group analysis on outcomes at the first endline and we show that the groups experiencing the largest short-run gains in job quality from the workshop intervention are the same as those who experience the most significant long-run gains in earnings. The consistency between the two sets of results lends further support to the mechanism we have outlined: the workshop improves workers' signals about unobservable skills and is particularly valuable for job-seekers with poor observable characteristics. The results, reported in Tables 5 and 6, follow the same structure as Table 4, but focus on the two short-term outcomes changed by the workshop: permanent work and formal work. We find that the effects on permanent work are significantly larger for individuals who have no tertiary education and larger in magnitude (although not significantly) for individuals with no job experience in a permanent employment contract. When we repeat our Abadie et al. (2018) stratification by predicted earnings in 2018, we find significant effects on both formal work and permanent work for the low-predicted-earnings group. In the case of the workshop treatment, we reject the null of equal effects between groups for both formal work ($p = 0.035$) and for permanent work ($p = 0.074$): the workshop has significantly larger effects on individuals with worse observable traits.

[53] In Table 4, we report a selection of the covariates we specified. We report the full set of covariates in Table A.25 in the Online Appendix, including with q-values adjusted for the full set of coefficients. One dimension that deserves further discussion is whether the respondent used to include a CV or a certificate in job applications at baseline. We do not find significant heterogeneity with respect to this dimension. This suggests that existing signals tend to be of low quality even among those individuals that have access to some form of certification.

Overall, these results imply that the workshop reduces the earning premium of observable correlates of ability such as education or experience. This reduction in earning premium is evidence that better signals reduce statistical discrimination, as predicted by our framework. It is also a natural measure of the value of the information provided. Specifically, we find that the earning premium of vocational education is reduced by a significant 83 percent thanks to the workshop, while the premium of having a degree decreases by 33 percent (not significant). Further, the premium of previous permanent work experience is fully erased — but the effect is imprecisely estimated ($p = 0.110$). These changes cause a 54 to 142 percent drop in the earning gap between those predicted to have low and high earnings on the basis of their baseline covariates (last row of Table 4). These results illustrate the large equity gains generated by helping young workers access the labour market through improved signalling. The full set of results is in Table A.26 of the Online Appendix.

6 Discussion

In this section we first show that the impact of the workshop on earnings is attained at lower cost than any other intervention discussed in the literature (McKenzie, 2017). We then discuss the possible effect of scaling up the intervention.

6.1 Is the workshop cost-effective compared to other active labour market policies?

The job application workshop is highly cost-effective. To make this point, we compare our findings to those summarised by McKenzie (2017) on the cost and earning impacts of active labour market policies in developing countries. In Figure 5, we plot the relative gain in earnings and the ratio of impact to cost for each intervention discussed in McKenzie (2017) and for our workshop and transport interventions.[54] Two key messages emerge. First, the impact of the job application workshop on earnings is close to the top of the distribution. Second, the workshop is cheap relative to other high-impact interventions, which tend to be training programs that cost hundreds of dollars per person. As a result, its earnings to cost ratio is unusually high. A similar picture emerges when we compare it to cash transfer programs, which generate large gains but have high costs (e.g. Blattman et al. (2014) document that a 382 USD grant increases earnings by 38 percent).

< Figure 5 here. >

[54] It is important to note that, while useful, this exercise comes with a number of caveats. In particular, it does not consider the trajectory of impacts and it does not take into account variation in context. Most studies included have a shorter time frame than ours, however, so that criticism affects them even more.

6.2 General equilibrium, congestion, and signal inflation

What, then, are the welfare implications of our results? Our framework and empirical results suggest that the job application workshop improves match quality between workers and firms. This should improve efficiency and raise the overall welfare of workers through higher pay.

Our framework predicts that the workshop increases efficiency even though it does not increase overall labour demand. Indeed, our framework assumes complete displacement in hiring: each firm has a suitable reserve candidate who does not get the job if a treated worker is hired. However, under several plausible alternative assumptions, the job application workshop can raise labour demand. On possibility is that the firm does not have a suitable reserve candidate, in which case some vacancies remain unfilled. In this scenario, improving the precision of signals would reduce the share of unfilled vacancies. Alternatively, firms' labour demand may depend on the match quality of their current workers. By improving match-quality, the workshop can foster future hiring. Such indirect effects are plausible but our experimental design does not enable us to test whether they are at work in our setting, and so we maintain the conservative assumption of full displacement. As our framework makes clear, even under this assumption, the workshop is able to generate efficiency gains by improving match quality.

An important question is whether these effects would remain if the workshop were scaled up to all jobseekers. We consider three potential mechanisms that could adversely affect scale-up. First, low-ability workers may find it harder to secure employment when employers expect a skills certificate. Second, the intervention may reduce matching efficiency by causing congestion. Third, the workshop may enable workers to send inflated signals of their skills.

Regarding the first issue, our framework shows that the effect of a scaled-up intervention on workers of different abilities depends on the distribution of match quality. Consider first the case where the quality of each worker-firm pair results from an independent draw from an identical distribution. This captures the notion that each worker is a good match for some jobs. If this is true, scaling up means all workers have a higher chance of finding a job with high match quality and better pay. Further, workers all prefer to use the more informative signal than the old signal, at least for the positions for which they are well suited. It follows that no worker is harmed by scale-up. Alternatively, suppose that the match quality of 'low ability' workers is drawn from a lower distribution. These workers could be harmed by scale-up if firms expect to see the certificate and, if it is not shown, they interpret it as evidence of low ability. In equilibrium, firms would identify low-ability workers more easily and thus would either not offer them jobs or decrease their pay.

The empirical results from our study population offer some evidence that all workers have good match quality for at least some jobs. First, workers have different strengths: 44% of the workers in the job application workshop score high for at least one skill; only 10% of them are in the lower half of the distribution for all tested skills. By reporting results for each test, the certificates thus enable workers to demonstrate their particular skills. Second, the earning gains generated by the workshop are broad based: as shown in Section 4.2 and Figure 3, the earnings of the treated stochastically dominate those of the controls, a finding confirmed by quantile regressions shown in Table A.13. While we cannot establish that no workers would be harmed by scale-up, the evidence suggests that the share of low-ability workers with nothing to gain from improved signals is small.

A second concern is that scale-up may lead to an excessive number of applications per job posting (Gautier et al., 2018). More precise signals may induce high-quality workers to apply to many more jobs, thereby crowding out firms' ability to screen applicants. This is not what we find: the workshop improves employment outcomes without changing search intensity or increasing the number of applications.

Finally, the workshop may allow job-seekers to oversell their skills, making it harder for employers to screen candidates and thereby reducing match quality.[55] This is not what we find. If treated workers had misrepresented their skills at hiring, they should be laid off during the 45 day probation period required by Ethiopian law for all formal jobs. Instead, we find that treated workers work in the same job for longer. Furthermore, they are offered better conditions when they move to another job. Even if job-seekers could have fooled one employer about their skills, they cannot fool all of them.

7 Conclusion

Do labour market frictions prevent young educated people from finding good jobs? In this paper, we show that the inability to convey information about skills can be a crucial barrier for young job seekers in Ethiopia. In particular, we demonstrate that improving the ability to convey this information through a job application workshop and a skill certificate has long-term effects on earnings that far outweigh the costs of the intervention. In addition, by improving match quality, the workshop has positive effects on overall efficiency. To the best of our knowledge, this paper is the first to show that young people in a developing country have valuable unobserved skills that, once certified, generate welfare improvements. Further, since the impact of the workshop is strongest among disadvantaged socio-demographic groups, the intervention reduces inequality.

[55] We thank an anonymous referee for this observation.

The financial cost of job search, on the other hand, only constitutes a short-term impediment to job search, but does not reduce long-run job quality. We reach this conclusion by testing the impacts of a second intervention that provides workers with a transport subsidy to search for work. While the subsidy leads to a short-term improvement in job quality through an increase in search intensity, the effect dissipates over time. Although we cannot rule out that reducing search costs for a longer time period or for a more targeted sample could have more persistent effects, this initial evidence suggests that, for the average worker in our study, the financial cost of job search is not the main constraint on job quality in the long run.

Our results also highlight that active labour market policies like the ones we test are unlikely to impact the extensive margin of employment in a developing country. This is in line with a growing consensus that is consolidating in the literature (Kluve et al., 2019; McKenzie, 2017), and it is probably to be expected in a context where informal employment is widespread and casual jobs of 'last resort' can be accessed relatively easily. By contrast, our intervention has significant impacts along key dimensions of job quality. Treated workers obtain more permanent and more formal jobs in the short-run, and higher earnings in the long run. These results have important implications for our understanding of labour market frictions in developing countries, and suggest a novel basis for labour market policy. Researchers looking for ways of helping young job seekers in growing urban markets of the developing world may want to build on our results by integrating into standard models of job search the frictions that we have identified here.

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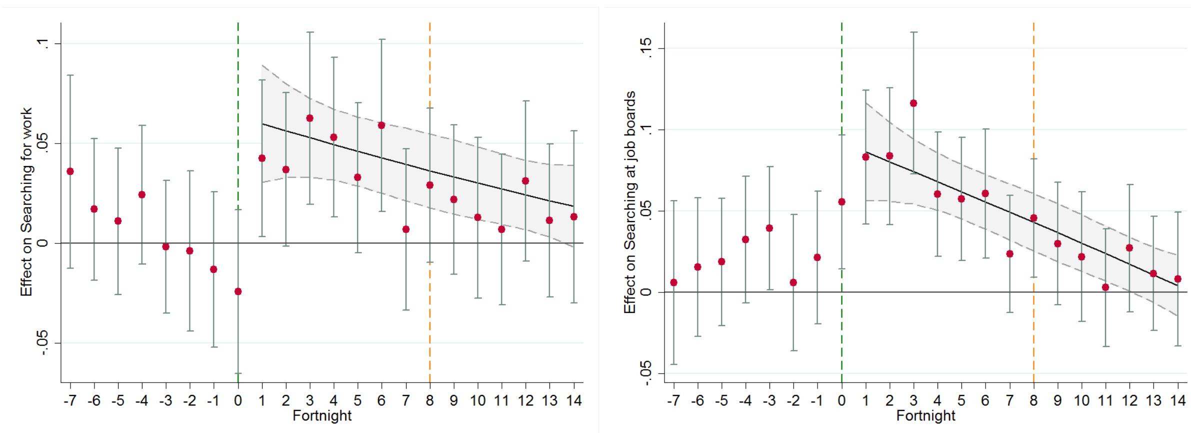
Figures and Tables

Figure 1: **Fortnightly impacts of the transport treatment on job search**

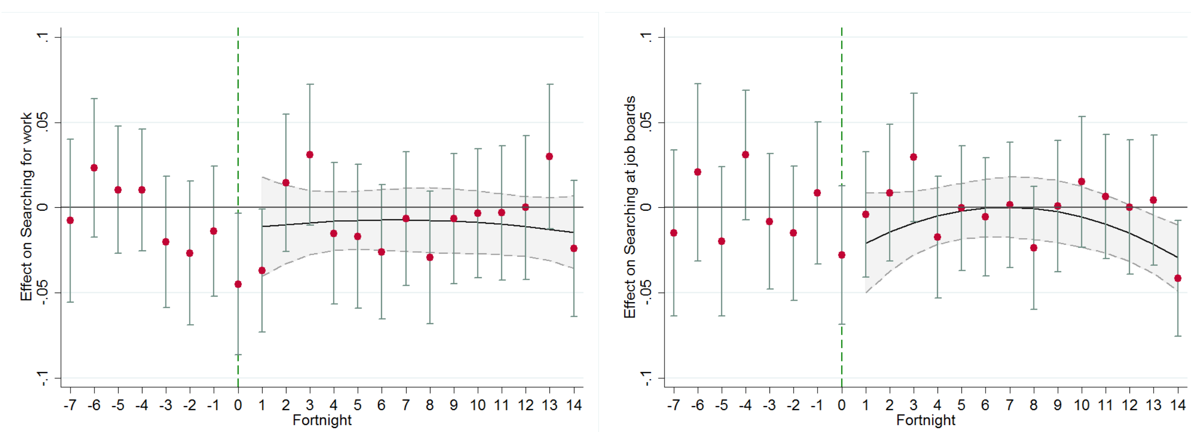
(a) Impact on search (any active step) (b) Impact on searching at the job boards



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

Figure 2: **Fortnightly impacts of the job application workshop on job search**

(a) Impact on search (any active step) (b) Impact on searching at the job boards



The green dotted line indicates the fortnight when the treatment begins.

Figure 3: **The distribution of endline 2 earnings in the workshop and control group**

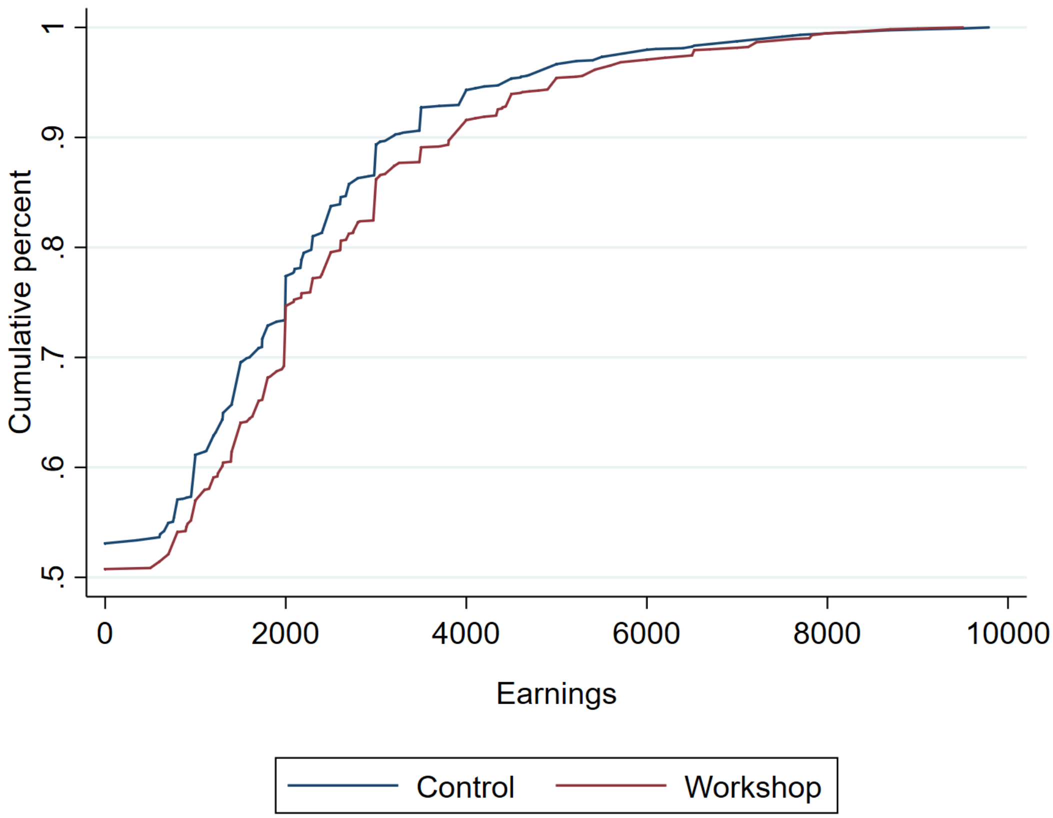
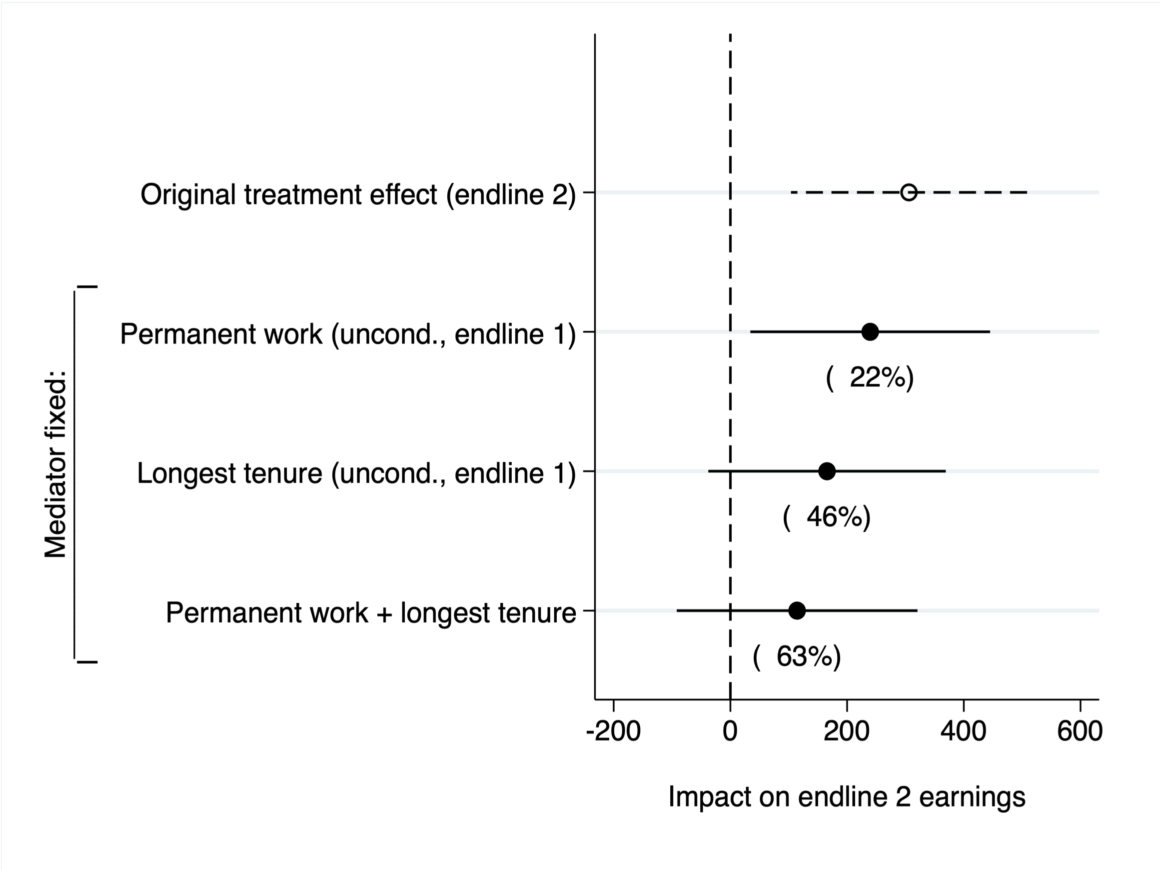


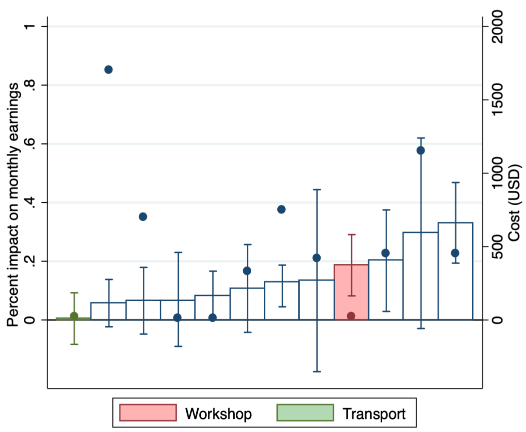
Figure 4: **Mediation analysis: Job application workshop**



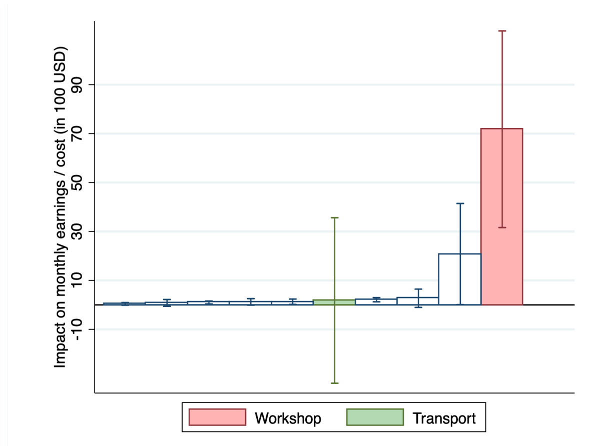
Note. This figures reports coefficient estimates and 90% confidence intervals of the impact of the job application workshop on endline 2 earnings. The first row reports the original treatment effect on endline 2 earnings. The following rows report the Average Controlled Direct Effect (ACDE) of the intervention, obtained by fixing the mediator indicated in the row's name (Acharya et al., 2016). We can assess the importance of a given mediator by comparing the original treatment effect to the ACDE. To facilitate the comparison, we report below each coefficient the share of the original treatment effect that is accounted for by the mediator. We consider the following mediators: permanent work at endline 1 and the lenght of the longest employment spell (which captures employment spells that started just after endline 1, and was measured with a retrospective question in endline 2). In the last row, we also report an estimate of the ACDE obtained by including both mediators. Finally, if we use the same procedure with our third proxy of match quality — whether a worker uses their skills in their current job — we can account for 77 percent of the original treatment effect. However, this mediator refers to the job that respondents hold at endline 2 and is thus more likely to violate the unconfoundedness assumption required by the mediation procedure. We thus do not report this result in the figure above.

Figure 5: **Comparison with other ALMPs in developing countries**

(a) Impacts on earnings and cost



(b) Impact/cost ratio



Note. We report the estimates of the studies included in the review by McKenzie (2017) which report both positive earnings effects and costs (only three studies report negative earnings effects). For some studies, we obtain additional information from the papers (e.g. for Maitra and Mani (2017)). We also include the recent estimates of Alfonsi et al. (2017) and the second-endline estimates from our paper (the original review only included the estimates from the first endline).

Table 1: **Treatment assignment**

Proportion Treated	**No. Individuals**		**No. Clusters**
	Controls	Treated	
Transport clusters			
20%	256	65	18
40%	150	96	15
75%	56	191	15
90%	38	422	26
Total	*500*	*774*	*74*
Workshop clusters			
80%	*187*	*768*	*56*
Control clusters			
0%	*823*	*0*	*48*
Total	**1,510**	**1,542**	**178**

Table 2: **Impacts on employment outcomes**

	2015				2018			
Outcome	Control mean (1)	Transport (2)	Workshop (3)	Equality (pval) (4)	Control mean (5)	Transport (6)	Workshop (7)	Equality (pval) (8)
Work	0.537	0.037 (0.029) [0.366]	0.021 (0.031) [1.000]	0.57	0.657	-0.058* (0.035) [0.411]	0.029 (0.032) [0.958]	0.00
Hours worked	25.558	0.183 (1.543) [0.837]	-0.214 (1.533) [1.000]	0.79	26.497	-2.499* (1.486) [0.411]	0.218 (1.426) [1.000]	0.04
Wage earnings	739.230	65.879 (63.864) [0.437]	3.363 (65.667) [1.000]	0.30	1,216.811	30.916 (102.352) [0.753]	299.469** (121.383) [0.096]	0.02
Permanent job	0.120	0.033* (0.018) [0.215]	0.069*** (0.019) [0.004]	0.09	0.248	-0.034 (0.025) [0.411]	-0.010 (0.028) [1.000]	0.30
Formal job	0.172	0.054*** (0.019) [0.032]	0.053*** (0.020) [0.021]	0.95	0.259	-0.005 (0.030) [0.753]	-0.007 (0.030) [1.000]	0.96
Job satisfaction	0.231	-0.001 (0.027) [0.837]	0.022 (0.027) [1.000]	0.45	0.538	-0.025 (0.037) [0.593]	0.066* (0.036) [0.219]	0.01

Note. In this table we report 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 OLS estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. N=2,201 for 2015 results, and N=2,018 for 2018 results. Because we did not follow up with the spillover groups in 2018, we are unable to include the individuals in the spillover groups in the 2018 regressions. For consistency, we drop the spillover observations from the 2015 regressions as well. Results for 2015 are qualitatively unchanged when those observations are included. Below each coefficient estimate, we report the *s.e.* in parentheses 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). We do this for the data from the first endline in 2015 (Columns 1-4) and then for second endline in 2018 (Columns 5-8). For each endline and each outcome, we report the mean outcome for the control group and the p-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect. ***: $p < 0.01$, **: $p<0.05$, *: $p<0.1$.

Table 3: **Impacts on match quality**

Outcome	Control mean (1)	N (2)	ITT Estimates Transport Coeff (3)	Workshop Coeff (4)	Equality pval (5)
Wages (conditional on a wage job)	2,580.479	1,041	77.783	563.545***	0.011
			(161.592)	(188.878)	
Longest tenure in months (conditional on any jobs)	12.276	1,361	0.557	1.129*	0.382
			(0.664)	(0.679)	
Longest tenure (unconditional)	10.132	1,751	0.200	1.255**	0.065
			(0.559)	(0.626)	
Uses skills in current job (unconditional)	0.282	2,016	0.030	0.082**	0.196
			(0.040)	(0.040)	
Promoted in current job (unconditional)	0.092	2,016	0.005	0.007	0.916
			(0.015)	(0.015)	

Note. In this table we report the *intent-to-treat* estimates of the impacts of the transport intervention and the job application workshop on several outcomes related to match-quality. These are obtained by OLS estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. All outcomes have been measured in the 2018 endline. Because we did not follow up with the spillover groups in 2018, we are unable to obtain coefficient estimates for these treatment groups and they are thus absent from the sample used for estimation by default. Below each coefficient estimate, we report the *s.e.* in parentheses. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. ***: $p < 0.01$, **: $p<0.05$, *: $p<0.1$. In Row 1 we report wages conditional on having a wage job at the time of the second in 2018. The number of observations reflects the number of individuals reporting positive values for wage earnings at the time of the second endline. In Row 2 we report effects on the longest job tenure the respondent has had in the last two years (using recall data), conditional on having had *at least one* wage job in the last two years. In Row 3 we report the effect on the longest tenure in any job in the last two years. Individuals who have not had a job in the last two years are coded has having a tenure of 0 months. In Rows 4 and 5 we report unconditional job characteristics (i.e. observations associated with individuals without jobs take the value of zero). In Rows 2 and 3 the number of observations is smaller than for other outcomes because of item non-response: some individuals reported working in at least one job in the last two years but could not recall the length of their longest work spell.

Table 4: **Impacts on 2018 wage earnings by baseline characteristics**

Baseline covariate	Covariate = 0			Covariate = 1			Transport	Workshop
	Control mean	Transport	Workshop	Control mean	Transport	Workshop	Equality (pval)	Equality (pval)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Above high school	826.4	28.4	493.4**	1,755.5	43.4	47.4	0.93	0.06
		(123.5)	(190.9)		(150.3)	(134.8)		
		[1.000]	[0.028]		[1.000]	[0.772]		
Male	905.5	-39.1	141.3	1,564.3	102.3	499.9**	0.45	0.16
		(110.0)	(104.7)		(162.0)	(234.6)		
		[1.000]	[0.069]		[1.000]	[0.209]		
Active searcher	1,096.7	-0.5	361.8*	1,363.7	60.7	244.3	0.73	0.65
		(124.9)	(184.1)		(143.8)	(174.8)		
		[1.000]	[0.032]		[1.000]	[0.489]		
Ever had permanent job	1,160.5	36.2	369.1***	1,687.4	-22.0	-259.5	0.87	0.10
		(99.7)	(132.9)		(359.5)	(347.6)		
		[1.000]	[0.028]		[1.000]	[0.695]		
Lives close to the centre	1,171.5	15.5	428.6**	1,278.1	43.6	122.5	0.89	0.19
		(134.9)	(179.8)		(146.4)	(142.6)		
		[1.000]	[0.028]		[1.000]	[0.695]		
Predicted endline earnings (above the median)	930.8	123.1	467.1***	2250.4	-226.4	-99.0	0.475	0.0696
		(115.5)	(170.3)		(227.8)	(224.1)		

Note. This table shows differential treatment effects by individual baseline characteristics on earnings at the second endline (2018) of the workshop and transport treatments. We estimate heterogenous treatment effects in a saturated model where we interact the treatment with dummies for baseline covariate =0, and for baseline covariate =1. Otherwise, the model is the same as the model presented in Equation (1). We weight each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report the *s.e.* in parentheses and a *q*-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. Columns (1)-(3) shows the results for the sub-sample with the baseline covariate =0, while columns (4)-(6) show the results for sub-sample where the covariate =1. For example, row (1), column (1) shows the control mean for individuals who did *not* study at a tertiary level (826.4 Birr) and row (1), column (3) shows the treatment effect of the workshop for this group (493.4). We do this for five main baseline characteristics. In the last row we show the results where we split the sample by predicted earnings using a range of baseline covariates. For this row, standard errors are derived using bootstrap methods. See Section 5.3 for discussion. Finally, in columns (7) and (8) we test for the equality of the treatment effects between the "covariate=0" and "covariate=1" group, for the transport and workshop treatment, respectively.

Table 5: **Impacts on 2015 permanent employment by baseline characteristics**

Baseline covariate	Covariate = 0 Control mean (1)	Covariate = 0 Transport (2)	Covariate = 0 Workshop (3)	Covariate = 1 Control mean (4)	Covariate = 1 Transport (5)	Covariate = 1 Workshop (6)	Transport Equality (pval) (7)	Workshop Equality (pval) (8)
Above high school	0.058	0.063**	0.109***	0.213	-0.011	0.010	0.04	0.01
		(0.025)	(0.028)		(0.025)	(0.024)		
		[0.027]	[0.001]		[1.000]	[0.371]		
Male	0.104	0.064***	0.074***	0.138	-0.005	0.063**	0.05	0.77
		(0.023)	(0.026)		(0.027)	(0.029)		
		[0.027]	[0.004]		[1.000]	[0.068]		
Active searcher	0.109	0.039	0.083***	0.135	0.026	0.053*	0.72	0.46
		(0.024)	(0.027)		(0.028)	(0.030)		
		[0.069]	[0.003]		[1.000]	[0.087]		
Ever had permanent job	0.103	0.042**	0.073***	0.269	-0.040	0.032	0.28	0.60
		(0.020)	(0.019)		(0.069)	(0.076)		
		[0.041]	[0.001]		[1.000]	[0.371]		
Lives close to the centre	0.117	0.001	0.031	0.124	0.053*	0.110***	0.14	0.03
		(0.022)	(0.025)		(0.028)	(0.027)		
		[0.240]	[0.046]		[0.435]	[0.001]		
Predicted endline earnings (above the median)	0.076	0.053**	0.087***	0.203	-0.019	0.018	0.022	0.074
		(0.022)	(0.025)		(0.030)	(0.031)		

Note. This table shows differential treatment effects by individual baseline characteristics on permanent employment at the first endline (2015) of the workshop and transport treatments. We estimate heterogenous treatment effects in a saturated model where we interact the treatment with dummies for baseline covariate =0, and for baseline covariate =1. Otherwise, the model is the same as the model presented in Equation (1). We weight each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report the *s.e.* in parentheses and a *q*-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. Columns (1)-(3) shows the results for the sub-sample with the baseline covariate =0, while columns (4)-(6) show the results for sub-sample where the covariate =1. For example, row (1), column (1) shows the control mean for individuals who did *not* study at a tertiary level (0.058) and row (1), column (3) shows the treatment effect of the workshop for this group (0.109). We do this for five main baseline characteristics. In the last row we show the results where we split the sample by predicted earnings using a range of baseline covariates. For this row, standard errors are derived using bootstrap methods. See Section 5.3 for discussion. Finally, in columns (7) and (8) we test for the equality of the treatment effects between the "covariate=0" and "covariate=1" group, for the transport and workshop treatment, respectively.

Table 6: **Impacts on 2015 formal employment by baseline characteristics**

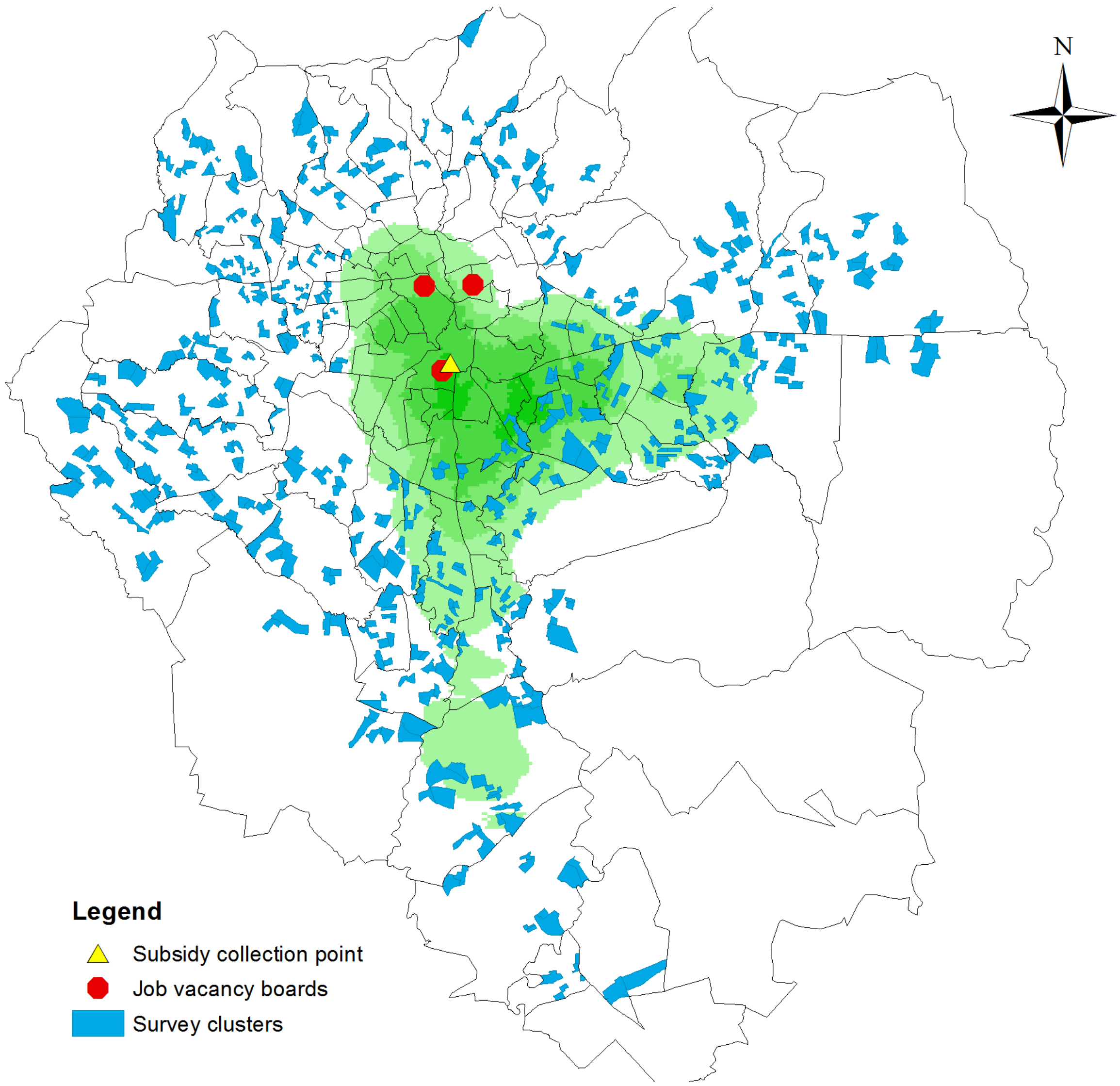
Baseline covariate	Covariate = 0 Control mean (1)	Covariate = 0 Transport (2)	Covariate = 0 Workshop (3)	Covariate = 1 Control mean (4)	Covariate = 1 Transport (5)	Covariate = 1 Workshop (6)	Transport Equality (pval) (7)	Workshop Equality (pval) (8)
Above high school	0.108	0.071**	0.071**	0.268	0.031	0.026	0.36	0.26
		(0.029)	(0.029)		(0.028)	(0.026)		
		[0.021]	[0.020]		[0.461]	[0.471]		
Male	0.152	0.065**	0.093***	0.195	0.044	0.006	0.63	0.04
		(0.028)	(0.027)		(0.030)	(0.031)		
		[0.021]	[0.004]		[0.461]	[1.000]		
Active searcher	0.154	0.067**	0.058**	0.195	0.040	0.049	0.53	0.83
		(0.027)	(0.027)		(0.030)	(0.031)		
		[0.021]	[0.025]		[0.461]	[0.330]		
Ever had permanent job	0.158	0.062***	0.055***	0.293	-0.012	0.033	0.32	0.76
		(0.021)	(0.021)		(0.069)	(0.070)		
		[0.017]	[0.016]		[0.505]	[0.927]		
Lives close to the centre	0.155	0.052**	0.039	0.194	0.040	0.063**	0.76	0.53
		(0.026)	(0.026)		(0.028)	(0.028)		
		[0.027]	[0.042]		[0.461]	[0.136]		
Predicted endline earnings (above the median)	0.100	0.071***	0.081***	0.248	0.023	-0.001	0.236	0.035
		(0.025)	(0.026)		(0.032)	(0.032)		

Note. This table shows differential treatment effects by individual baseline characteristics on formal employment at the first endline (2015) of the workshop and transport treatments. We estimate heterogenous treatment effects in a saturated model where we interact the treatment with dummies for baseline covariate =0, and for baseline covariate =1. Otherwise, the model is the same as the model presented in Equation (1). We weight each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report the *s.e.* in parentheses and a *q*-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. Columns (1)-(3) shows the results for the sub-sample with the baseline covariate =0, while columns (4)-(6) show the results for sub-sample where the covariate =1. For example, row (1), column (1) shows the control mean for individuals who did *not* study at a tertiary level (.108) and row, (1) column (3) shows the treatment effect of the workshop for this group (.071). We do this for five main baseline characteristics. In the last row we show the results where we split the sample by predicted earnings using a range of baseline covariates. For this row, standard errors are derived using bootstrap methods. See Section 5.3 for discussion. Finally, in columns (7) and (8) we test for the equality of the treatment effects between the "covariate=0" and "covariate=1" group, for the transport and workshop treatment, respectively.

For Online Publication

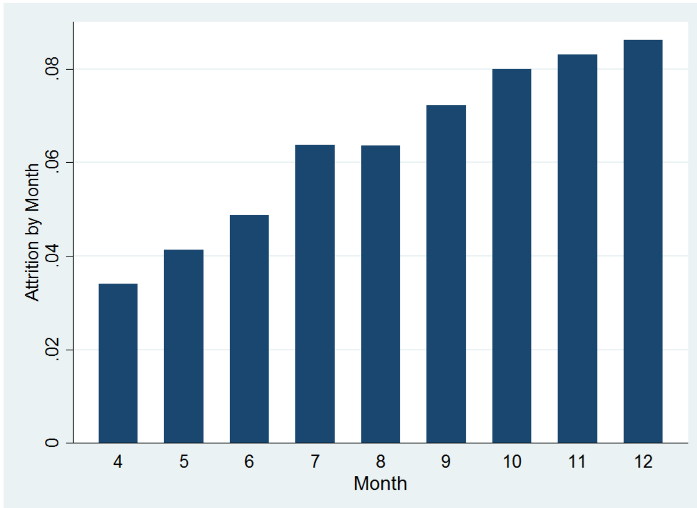
A.1 Additional Figures and Tables

Figure A.1: **Where are jobs located in Addis Ababa?**



Note. This map was created using data from a representative survey of 500 firms (Abebe et al., 2015). The survey was restricted to firms with more than 10 employees. Darker shades of green indicate a higher density of jobs. The areas randomly selected for this study are shaded in light blue. The map also shows the location of the main job boards and the disbursement centre of the transport subsidy.

Figure A.2: **Attrition rate from the phone survey by month**



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

Figure A.3: **The distribution of endline 2 earnings in the transport and control group**

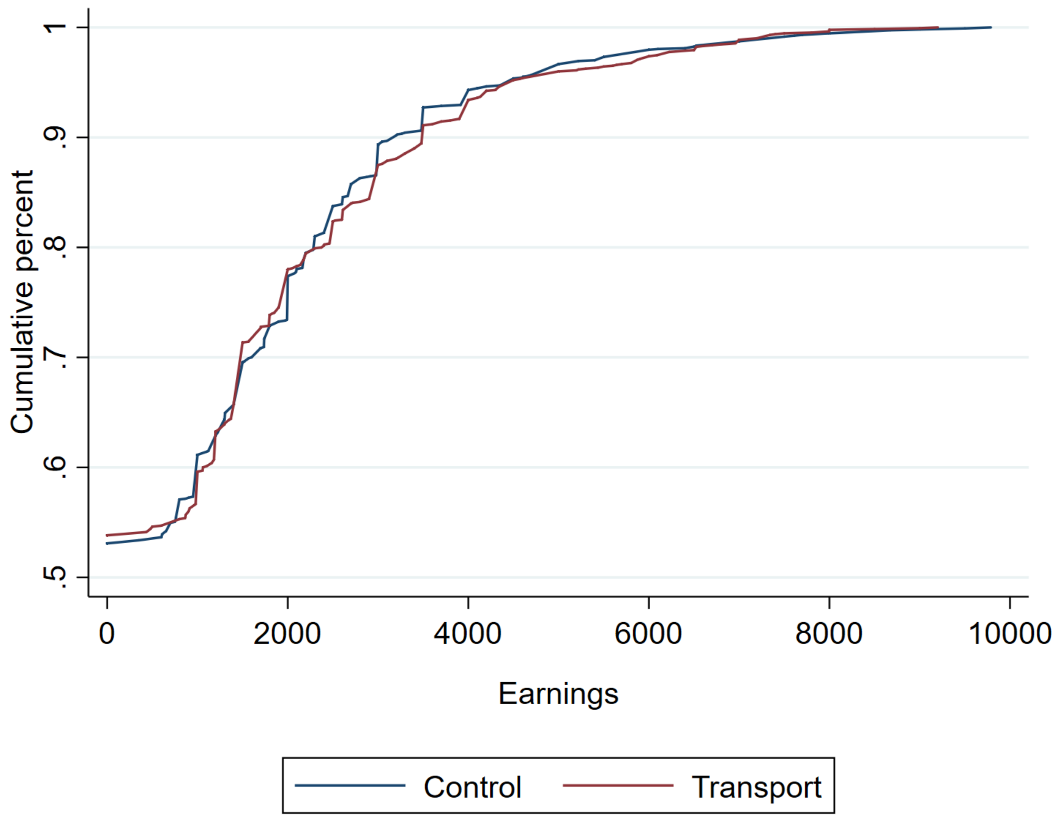


Figure A.4: **Impact trajectories by year: Employment**

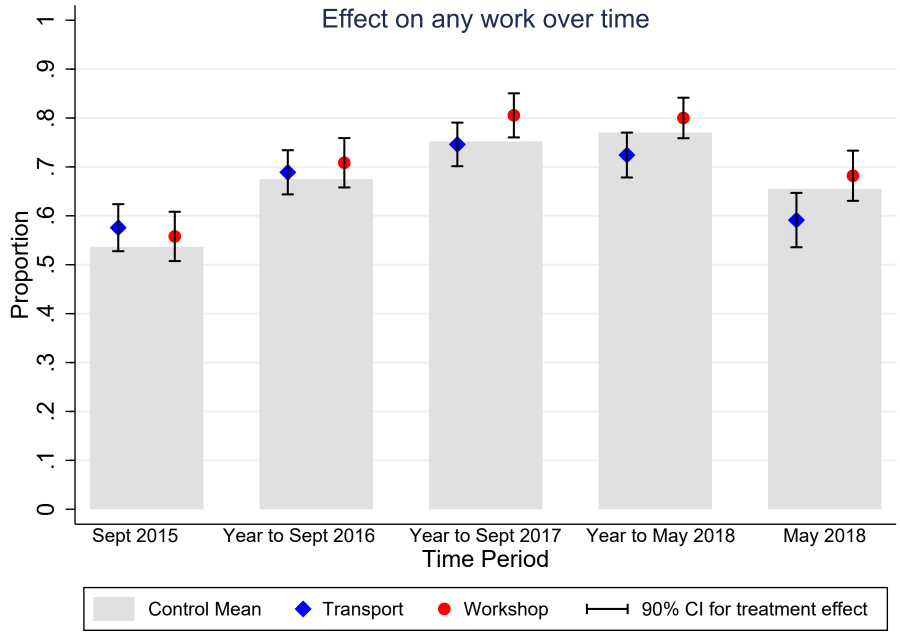


Figure A.5: **Impact trajectories by year: Permanent employment**

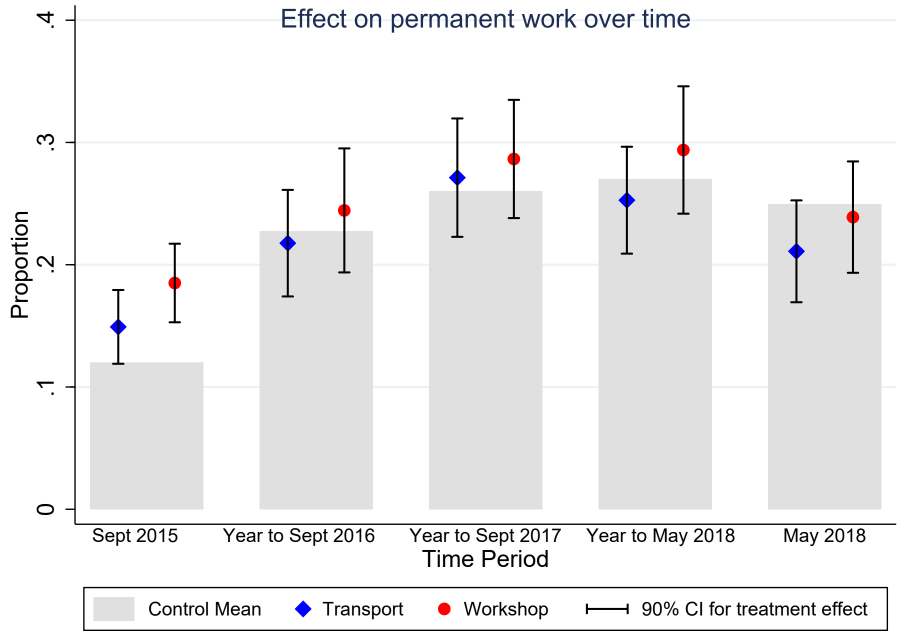
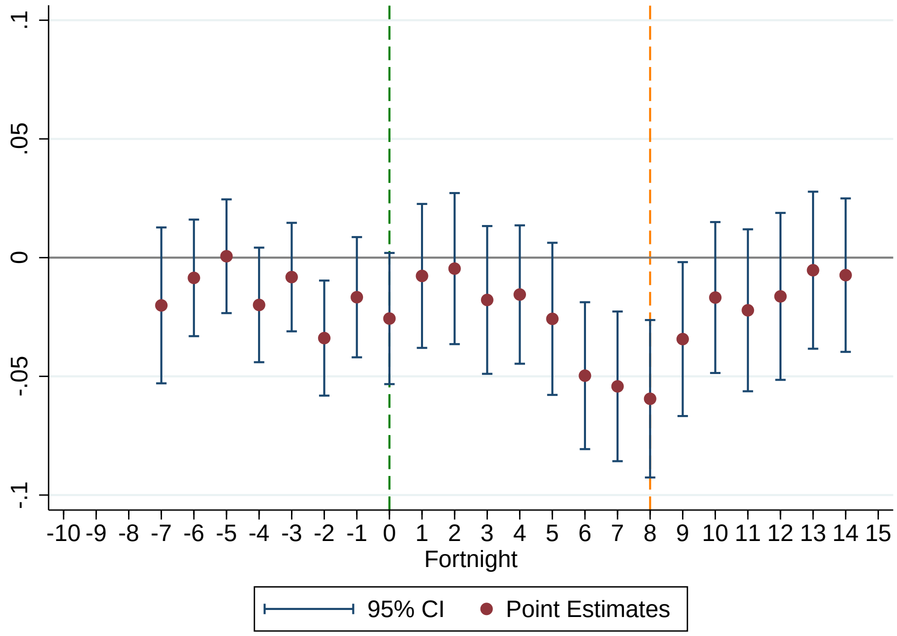
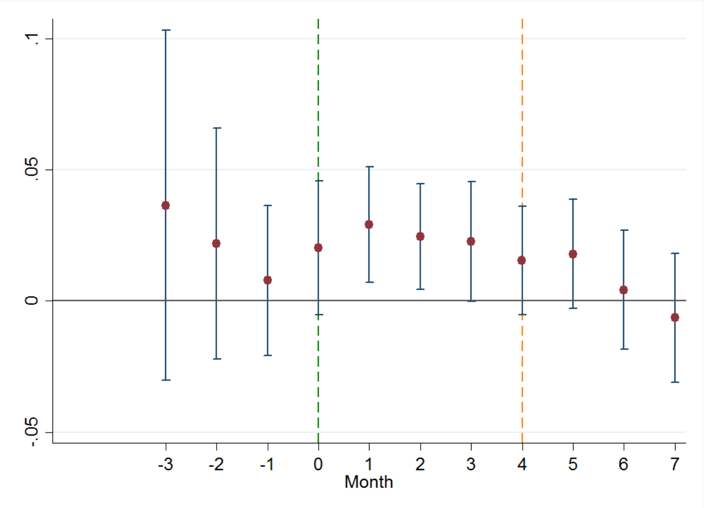


Figure A.6: **Impact trajectories (fortnightly): Employment in year 1**



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

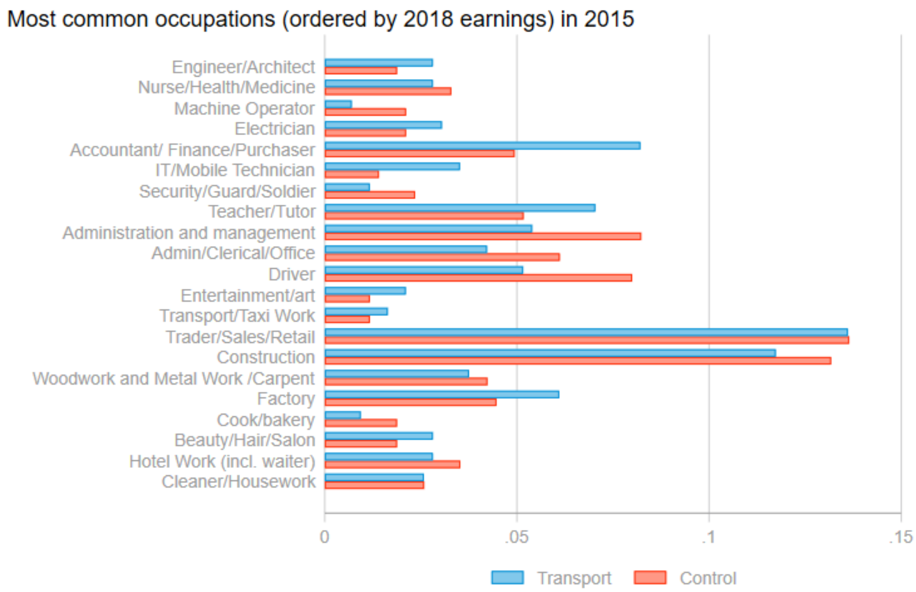
Figure A.7: **Impact trajectory (fortnightly) of the transport treatment: Travelled to city centre**



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

Figure A.8: **Most common 2015 occupations ordered (descending) by 2018 earnings**

(a) **Transport Subsidy**



(b) **Job Application Workshop**

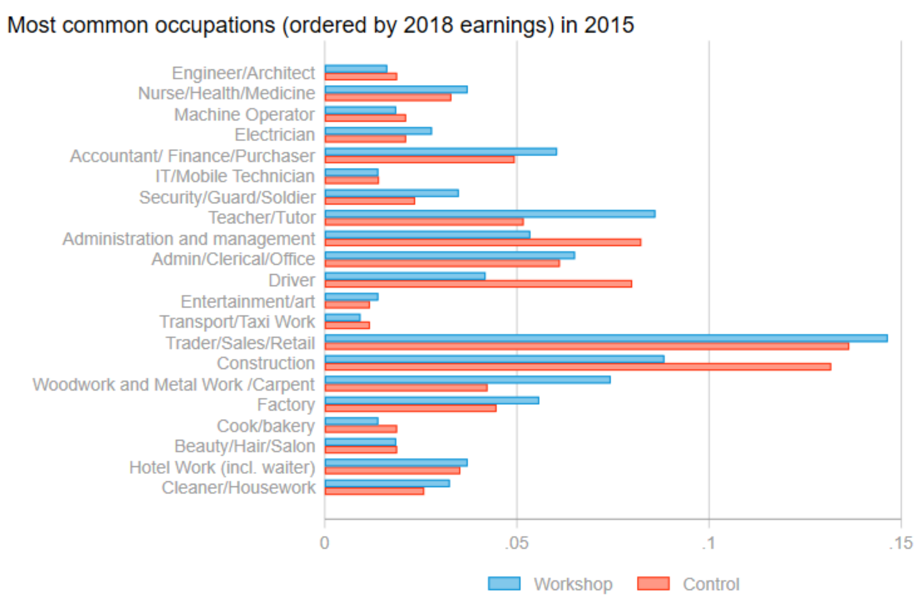


Table A.1: **Summary statistics of the tests administered in the job application workshop**

Variable	Mean	Std. Dev.	Min.	Max.
Raven test	30.5	13.2	0	56
Mathematical ability test	6.6	2.6	0	19
Linguistic ability test	11.4	3.3	0	17
Work sample 1: Minutes of business meeting	7.4	7.2	0	32
Work sample 2: Data entry under time pressure	20	10.7	0	40
Work sample 3: Meet a deadline	27.9	19.2	0	45
N		469		

Note. For each test we report the number of items that the subject has completed correctly. The Raven test has 60 items. The tests of mathematical and linguistic ability have 20 items each. The three work sample tests have 40 items each. In the third work sample test, we add five units to the overall score if the subject has taken her or his work sample back to the testing centre. Thus, subjects who fail to bring back the work sample to the testing centre have a score of 0 in this test. Subjects who bring back a work sample where no item is correctly completed have a score of 5. Subjects who bring back a work sample with all items correctly completed get a score of 45.

Table A.2: **Comparison of study sample characteristics at baseline to representative data**

	Representative LFS Data Youth not in full time education			Study Sample (Weighted)
	(1)	(2)	(3)	(4)
	All	No Perm Work	Sample Screen	Baseline
Female	44%	47%	51%	55%
Age	24.18	24.07	24.25	23.22
Employed	61%	62%	34%	30%
Migrant	47%	49%	29%	39%
Married	26%	26%	17%	22%
Work Experience	3%	6%	8%	10%
Live with parents	39%	38%	56%	50%
Education:				
None	10%	11%	0%	0%
Primary	34%	39%	0%	0%
Secondary	32%	34%	68%	60%
Vocational	13%	10%	20%	27%
Diploma	2%	2%	3%	4%
Degree	9%	4%	9%	9%
N	7,305	4,513	1,423	3,049

Table A.3: **Comparison of study sample (control group) employment outcomes at endline to representative data with similar education levels**

	Representative LFS Data (Addis Ababa 2013)			Study Sample
	All adults	Over 30	Youth	Control group
Permanent Job	38.4%	43.6%	31.7%	12.0%
Unemployed (strict definition)	10.4%	6.4%	15.2%	22.3%
Work	68.2%	71.2%	64.0%	53.7%
Wage per worker (2013 Birr)	2015.0	2374.4	1486.6	1564.5
Hourly Wage (2013 Birr)	11.2	13.0	8.2	9.3
Average Hours	47.0	46.24	48.0	47.9

Table A.4: **Sample selection before randomisation**

	Sample Size	No. Dropped	% dropped
Eligible at baseline	4388		
Found on phone	4314	74	1.69%
Stayed in phone survey	4254	60	1.39%
Without permanent work	4076	178	4.18%
Stayed in Addis	4059	17	0.42%
Total Dropped		329	7.58%
Total Sample	4059		
Assigned to a separate treatment*		1,007	
Final Sample	3,052		

* 1,007 individuals were assigned to a separate treatment, which consisted of a series of job fairs (with a random sample of employers from Addis Ababa). This is a distinct intervention, which analyses both sides of the market, and constitutes the focus of a separate paper (Abebe et al., 2017).

Table A.5: **Assignment to start and end weeks of the transport Intervention**

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

Table A.6: **Summary and tests of balance**

Outcome	Control Mean	SD	Transport	Workshop	N	F-test P
	(1)	(2)	(3)	(4)	(5)	(6)
degree	0.18	0.39	0.01	-0.01	3049	0.347
			(0.63)	(0.74)		
vocational	0.43	0.49	0.01	0.01	3049	0.717
			(0.82)	(0.59)		
work	0.31	0.46	-0.01	-0.02	3049	0.881
			(0.61)	(0.56)		
search	0.50	0.50	-0.01	0.00	3049	0.804
			(0.83)	(0.96)		
dipdeg	0.25	0.43	0.00	-0.01	3049	0.557
			(0.94)	(0.68)		
female	0.52	0.50	0.00	0.00	3049	0.968
			(0.98)	(0.96)		
migrant_birth	0.37	0.48	0.01	-0.01	3049	0.530
			(0.72)	(0.84)		
amhara	0.46	0.50	-0.01	-0.06	3049	0.078
			(0.87)	(0.11)		
oromo	0.26	0.44	-0.00	0.02	3049	0.489
			(0.88)	(0.59)		
work_wage_6months	0.46	0.50	-0.00	-0.01	3049	0.659
			(0.99)	(0.67)		
married	0.20	0.40	0.01	-0.03	3049	0.131
			(0.81)	(0.26)		
live_parents	0.52	0.50	-0.01	0.01	3049	0.451
			(0.79)	(0.66)		
experience_perm	0.13	0.34	0.00	-0.01	3049	0.370
			(0.84)	(0.56)		
search_6months	0.75	0.43	-0.01	0.00	3049	0.606
			(0.67)	(0.89)		
respondent_age	23.44	3.00	0.06	0.05	3049	0.934
			(0.70)	(0.78)		
years_since_school	42.30	273.93	6.40	-13.78	3045	0.128
			(0.71)	(0.37)		
search_freq	0.57	0.31	-0.01	0.00	3049	0.782
			(0.75)	(1.00)		
work_freq	0.34	0.38	-0.00	0.00	3049	0.846
			(0.94)	(0.90)		
self_employed	0.05	0.22	-0.00	-0.00	3049	0.636
			(0.97)	(0.66)		
work_cas	0.06	0.23	-0.01	-0.01	3049	0.880
			(0.39)	(0.53)		
work_satisfaction	0.09	0.28	0.00	0.00	3049	0.881
			(0.79)	(0.91)		
total_savings	2279.23	6203.56	407.17	-160.84	3049	0.094
			(0.23)	(0.59)		
res_wage	1327.22	1235.30	72.65	13.61	3021	0.306
			(0.28)	(0.83)		
cent_dist	5.92	2.24	0.22	0.30	3049	0.887
			(0.65)	(0.58)		
travel	1.83	2.03	0.03	0.03	3045	0.991
			(0.84)	(0.86)		
written_agreement	0.06	0.23	0.02	0.02	3049	0.789
			(0.17)	(0.15)		

cv_application	0.28	0.45	0.01	0.02	3049	0.659
			(0.61)	(0.41)		
expect_offer	1.46	2.09	0.15	-0.04	2864	0.292
			(0.43)	(0.86)		
aspiration	5583.33	5830.85	300.29	402.24	2883	0.743
			(0.37)	(0.29)		
network_size	6.74	9.63	-0.67	0.20	3014	0.384
			(0.51)	(0.87)		
respondent_age	23.44	3.00	0.06	0.05	3049	0.934
			(0.70)	(0.78)		
present_bias	0.12	0.33	0.02	0.02	2067	0.814
			(0.42)	(0.35)		
future_bias	0.08	0.27	-0.03	0.00	2067	0.063
			(0.17)	(0.92)		
life_satisfaction	4.20	1.85	-0.03	-0.05	3045	0.892
			(0.87)	(0.78)		

Note. This table shows summary statistics for baseline covariates and a battery of balance tests. Variable definitions are provided in Table A.7 below. For each variable, we first show the mean and standard deviation for the control group (columns 1 and 2). We then show the difference between the mean of the variable in the Transport and Workshop groups, respectively, and the mean in the control group (columns 3 and 4). Column 5 shows the p-value for a test of the joint null hypothesis that that the covariates are balanced across the three groups (control, workshop and transport). We conduct a joint F-test for balance against control (where we omit the four variables having fewer than 3000 observations); for testing the transport intervention against control, we obtain $p = 0.997$, and for testing the workshop against control, we obtain $p = 0.270$.

Table A.7: **Variables used for re-randomisation**

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

Table A.8: **Predictors of attrition: Both endline surveys**

	Dep Var: No-response or refused			
	2015 Endline		2018 Endline	
	(1)	(2)	(3)	(4)
Transport	-0.002	-0.004	-0.007	-0.008
	(0.017)	(0.017)	(0.021)	(0.021)
Workshop	-0.019	-0.022	-0.035	-0.037
	(0.019)	(0.019)	(0.020)*	(0.020)*
Search intensity (baseline)		0.002		-0.010
		(0.019)		(0.023)
Degree		-0.020		0.001
		(0.014)		(0.019)
Worked (7d)		-0.037		-0.002
		(0.018)**		(0.020)
Searched job (7d)		0.008		-0.002
		(0.018)		(0.019)
Female		0.030		0.038
		(0.013)**		(0.016)**
Respondent age		-0.005		-0.003
		(0.003)*		(0.003)
Born outside Addis		0.034		0.027
		(0.016)**		(0.018)
Amhara		-0.024		-0.012
		(0.018)		(0.020)
Oromo		-0.030		-0.032
		(0.019)		(0.020)
Wage empl (6m)		0.018		-0.008
		(0.015)		(0.017)
Married		-0.033		-0.043
		(0.021)		(0.024)*
Years since school		0.007		-0.000
		(0.003)**		(0.000)
Lives with parents		-0.005		-0.018
		(0.015)		(0.020)
Ever had permanent job		0.024		0.037
		(0.020)		(0.025)
Searched job (6m)		-0.016		0.026
		(0.018)		(0.020)
P-value of F-test	0.5699	0.0026	0.1567	0.0066
N	2,365	2,365	2,365	2,365
Control Mean	0.081		0.160	

Table A.9: **Predictors of take-up**

	Transport	Workshop
Female	-.004	-.044
	(.042)	(.042)
Age	-.002	.004
	(.008)	(.006)
Married	.041	.035
	(.056)	(.045)
Lives with parents	-.033	.051
	(.054)	(.047)
Amhara	.054	-.006
	(.047)	(.041)
Oromo	.006	-.005
	(.051)	(.044)
Born outside of Addis Ababa	.062	.071
	(.046)	(.046)
Degree	.038	-.035
	(.063)	(.052)
Years since school	-.00009	-.0001
	(.000)	(.000)*
Worked (last 7 days)	.105	.043
	(.048)**	(.048)
Searched for work (last 7 days)	-.057	-.066
	(.060)	(.039)*
Work frequency (before treatment)	-.039	-.011
	(.081)	(.054)
Search frequency (before treatment)	.254	.212
	(.072)***	(.065)***
Wage work (last 6 months)	-.019	-.072
	(.055)	(.048)
Searched for work (last 6 months)	-.036	-.010
	(.065)	(.056)
Ever had permanent job	-.072	-.090
	(.058)	(.059)
Const.	.407	.532
	(.211)*	(.178)***
Obs.	600	653
F statistic	2.513	3.005
Prob $> F$	0.004	0.001

For the transport intervention, take-up is defined as collecting the subsidy at least once during the course of the study. For the job-application workshop, take-up is defined as attending the workshop.

Table A.10: **Impacts on 2015 and 2018 wage earnings with alternative specifications**

Outcome	2015 Control mean (1)	2015 Transport (2)	2015 Workshop (3)	2015 Equality (pval) (4)	2018 Control mean (5)	2018 Transport (6)	2018 Workshop (7)	2018 Equality (pval) (8)
Wage earnings	739.230	65.879 (63.864) [0.295]	3.363 (65.667) [1.000]	0.30	1,216.811	30.916 (102.352) [1.000]	299.469** (121.383) [0.025]	0.02
Log of wage earnings	7.203	0.011 (0.060) [0.542]	0.008 (0.058) [1.000]	0.96	7.643	0.028 (0.051) [1.000]	0.163*** (0.049) [0.006]	0.01
Wage earnings winsorized at 99th percentile	716.522	70.993 (56.527) [0.268]	6.101 (59.313) [1.000]	0.26	1,197.949	12.452 (93.336) [1.000]	250.128** (104.700) [0.025]	0.02
Wage earnings winsorized at 95th percentile	657.741	83.462* (46.230) [0.226]	29.099 (50.902) [1.000]	0.30	1,145.810	16.161 (83.298) [1.000]	194.265** (89.690) [0.031]	0.05
Wage earnings winsorized at 90th percentile	619.538	74.568* (41.425) [0.226]	32.176 (45.888) [1.000]	0.37	1,070.490	9.553 (74.224) [1.000]	151.441* (77.242) [0.031]	0.07

Note. This table shows impacts on both endline 1 (2015) and endline 2 (2015) wage earnings under different definitions of the outcome variable. We estimates impacts using Equation (1), weighting each observation by the inverse of the probability of being sampled. In row 1, we reproduce the impacts on our main measure of wage earnings. In row 2, we show the impact on the log of earnings. In rows 3, 4 and 5, we show results for wage earnings winsorized at the 99th, 95th and 90th percentiles of the distribution.

Table A.11: **Effects on 2015 and 2018 earnings including profits**

Outcome	2015 Control mean (1)	2015 Transport (2)	2015 Workshop (3)	2015 Equality (pval) (4)	2018 Control mean (5)	2018 Transport (6)	2018 Workshop (7)	2018 Equality (pval) (8)
Wage earnings	739.230	65.879 (63.864) [1.000]	3.363 (65.667) [1.000]	0.30	1,216.811	30.916 (102.352) [1.000]	299.469** (121.383) [0.023]	0.02
Total earnings (with profits)	971.395	10.994 (74.959) [1.000]	76.754 (85.239) [1.000]	0.39	1,811.911	-101.236 (135.372) [1.000]	405.842** (160.515) [0.023]	0.00
Total Earnings (winsorised profits)	953.008	43.324 (72.544) [1.000]	99.640 (84.851) [1.000]	0.48	1,774.559	-125.423 (126.926) [1.000]	341.783** (148.911) [0.023]	0.00

Note. This table shows impacts on both endline 1 (2015) and endline 2 (2015) wage earnings under different definitions of the outcome variable. We estimates impacts using Equation (1), weighting each observation by the inverse of the probability of being sampled. In row 1, we reproduce the impacts on our main measure of wage earnings. In row 2 we show impacts on total earnings, which are defined as wage earnings plus monthly profits from self-employment. This measures includes some values of profits that are large outliers. In row 3, we show impacts on total earnings (wage earnings plus profits from self employment), winsorising profits at the 99th percentile to remove outliers.

Table A.12: **Quantile regression results: Impact on 2018 wage earnings**

Quantile	Transport	Workshop
0.4	0.0	0.0
	(0.9)	(1.1)
0.45	0.2	0.4
	(17.3)	(17.1)
0.5	1.5	2.2
	(34.8)	(78.6)
0.55	16.1	65.7
	(47.3)	(129.1)
0.6	37.1	263.2*
	(83.8)	(139.4)
0.65	32.6	338.1**
	(97.0)	(143.1)
0.7	-102.3	214.0
	(135.7)	(162.2)
0.75	-87.9	370.0***
	(138.1)	(142.8)
0.8	-67.9	304.6**
	(168.9)	(144.1)
0.85	-85.1	281.0*
	(136.0)	(168.0)
0.9	26.7	591.7**
	(176.5)	(233.9)

Note. We show quantile effects for both the workshop and the transport intervention on 2018 wage earnings, including controls for baseline covariates.

Table A.13: **Quantile regression results: Impact on 2018 total earnings**

Quantile	Transport	Workshop
0.4	-6.96	147.05
	(44.4)	(107.8)
0.45	-36.31	256.68**
	(64.2)	(117.2)
0.5	-130.9*	231.71**
	(74.1)	(102.2)
0.55	-105.71	255.22**
	(96.6)	(104.2)
0.6	-152.07	295.3**
	(109.1)	(115.3)
0.65	-133.41	270.94**
	(124)	(117.1)
0.7	-154.7	281.74*
	(128.2)	(148.2)
0.75	-162.55	342.78**
	(134.6)	(164.5)
0.8	-221.67	386.44**
	(160)	(184.3)
0.85	-202.54	449.68*
	(185.7)	(252.7)
0.9	-172.72	626.05 **
	(209.7)	(289.3)

Note. We show quantile effects for both the workshop and the transport intervention on 2018 total earnings, including controls for baseline covariates. Total earnings include both wage earnings and profits from self employment.

Table A.14: Regression Discontinuity Estimates

	Impact on standardised earnings (endline 2)		
	(1)	(2)	(3)
Above cut-off	0.366 (0.248)	0.263 (0.307)	0.464 (0.187)**
Bandwidth	Optimal	0.5*Optimal	2*Optimal
Obs.	248	206	308
	Impact on standardised longest tenure		
	(1)	(2)	(3)
Above cut-off	0.024 (0.214)	0.051 (0.319)	-0.273 (0.210)
Bandwidth	Optimal	0.5*Optimal	2*Optimal
Obs.	206	173	258

Note. In this table we report RDD estimates of the earning effects of being placed in a higher band in the job application workshop certificate. These are calculated using the Stata command provided by Nichols (2007). Following Imbens and Lemieux (2008), we report results obtained using a rectangular kernel and then check robustness to the use of different kernels. Results for a triangular kernel are qualitatively unchanged.

Table A.15: **Job search**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
Applied for temporary jobs	0.337 (.267) [.905]	-0.0210 (.205) [.985]	1.129	0.140	2832
Applied for permanent jobs jobs	-0.0400 (.251) [.905]	0.0210 (.24) [.985]	1.616	0.752	2827
No. Interviews / No. Applications	-0.0360 (.03) [.905]	-0.0370 (.027) [.703]	0.349	0.948	1584
No. Offers / No. Applications	0.00300 (.039) [.905]	0 (.039) [.985]	0.256	0.940	1586
No. Interviews / No. Applications (Perm. Jobs)	0.00300 (.038) [.905]	0.00900 (.035) [.985]	0.316	0.854	1240
No. Offers / No. Applications (Perm. Jobs)	0.0500 (.036) [.905]	0.0530 (.031)* [.703]	0.138	0.924	1238
No. Interviews / No. Applications (Temp. Jobs)	-0.0770 (.042)* [.905]	-0.0650 (.042) [.703]	0.384	0.759	986
No. Offers / No. Applications (Temp. Jobs)	-0.0560 (.044) [.905]	-0.0490 (.046) [.703]	0.346	0.875	986
Uses CV for job applications	0.0120 (.03) [.905]	0.0410 (.029) [.703]	0.307	0.291	2841
Uses certificates of training/qualifications for job applicaitons	0.0280 (.04) [.905]	0.0480 (.046) [.703]	0.401	0.650	2841

Note. In this table we report the *intent-to-treat* estimates of the effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by OLS estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report the *s.e.* in parentheses and the 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 A.16: **Family indices**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
Job Quality	0.534	0.493	-0.859	0.947	2841
	(.57)	(.629)			
	[1]	[1]			
Financial Outcomes	0.190	0.142	-0.559	0.831	2841
	(.238)	(.212)			
	[1]	[1]			
Expectations and Aspirations	-0.166	-0.00300	-0.0390	0.795	2134
	(.698)	(.585)			
	[1]	[1]			
Mobility	0.456	0.324	-0.740	0.787	2836
	(.471)	(.535)			
	[1]	[1]			
Education and Skills	-0.763	-1.160	0.578	0.565	2841
	(.67)	(.763)			
	[1]	[1]			
Wellbeing	0.0540	0.186	-0.153	0.444	2837
	(.166)	(.156)			
	[1]	[1]			
Networks	-0.301	-0.357	0.0890	0.873	2823
	(.34)	(.359)			
	[1]	[1]			

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

Table A.17: **Other job quality measures**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
Obtained job through an interview	0.0400 (.016)*** [.053]*	0.0430 (.018)** [.11]	0.115	0.879	2841
Did office work over past 7 days	0.0270 (.024) [.6]	0.00300 (.023) [1]	0.181	0.307	2841
Skills match current tasks	0.00800 (.029) [.882]	0.00500 (.029) [1]	0.120	0.915	2841
Overquilified for current job	0.0380 (.035) [.6]	0.0310 (.034) [1]	0.280	0.841	2841
Underqualified for current job	-0.0170 (.019) [.607]	-0.0130 (.019) [1]	0.0790	0.791	2841

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

Table A.18: **Financial outcomes**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
Total expenditure over past 7 days (birr)	28.54 (39.377) [1]	18.18 (38.661) [1]	474.4	0.797	2841
Total savings (bank, cash, etc.) - (birr)	352.4 (2726.672) [1]	-969.6 (1350.114) [1]	5803	0.603	1259
Total value of tangible assets (birr)	0.467 (.549) [1]	0.195 (.488) [1]	-1.055	0.605	2841

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

Table A.19: **Expectations, aspirations, reservation wages**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
No. job offers expected in next 4 months	-0.00600 (.143) [1]	0.270 (.154)* [.367]	1.383	0.0757	2641
Monthly reservation wage	8.790 (82.503) [1]	-86.57 (73.081) [.367]	1799	0.286	2480
Wage aspiration (monthly) in 5 years	689.8 (700.322) [1]	706.5 (817.629) [.367]	6237	0.985	2607
Expected no. weeks before obtaining perm. job	1.468 (4.323) [1]	-5.010 (3.345) [.367]	32.20	0.0923	1347

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

Table A.20: **Mobility**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
No. trips to city centre over past 7 days	0.129 (.172) [1]	-0.0330 (.183) [1]	2.171	0.379	2500
Works away from home	0.00300 (.034) [1]	-0.0190 (.035) [1]	0.378	0.501	2841
Location of main work changed over past year	0.0290 (.04) [1]	-0.0320 (.039) [1]	0.250	0.0957	2841
Moved (house) within Addis Ababa over past 12 months	-0.00200 (.019) [1]	0.0240 (.02) [.925]	0.0770	0.186	2841
Moved (house) outside Addis Ababa over past 12 months	0.0100 (.007) [1]	0.0120 (.007)* [.702]	0.00500	0.789	2841

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

Table A.21: **Education and training**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
In full-time education	-0.00700 (.008) [.777]	0.00100 (.01) [1]	0.0210	0.387	2841
In part-time education	-0.0480 (.02)** [.11]	-0.0330 (.023) [.52]	0.138	0.453	2841
In informal training	-0.00900 (.016) [.777]	-0.0100 (.015) [.696]	0.0470	0.951	2841
Graduated from any school over past 12 months	0.0120 (.017) [.777]	-0.0130 (.016) [.696]	0.0770	0.121	2841
Graduated (vocat. school) over past 12 months	0.0160 (.011) [.45]	0.00700 (.01) [.696]	0.0240	0.380	2841
Graduated (training course) over past 12 months	0 (.014) [1]	-0.0230 (.012)* [.475]	0.0440	0.0730	2841

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

Table A.22: **Psychological outcomes**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
Life satisfcation (0 - 10)	0.164 (.132) [1]	0.147 (.134) [1]	4.676	0.901	2503
How much freedom & control do you feel you have over your life (0-10)?	0.0150 (.299) [1]	-0.0400 (.285) [1]	6.114	0.853	2505
Onenness with society (1-7)[a]	-0.0260 (.14) [1]	0.0530 (.14) [1]	4.694	0.554	2505
How much do you trust others in this country? (1-4)	0.0790 (.081) [1]	0.0400 (.092) [1]	2.048	0.655	2504

Note. In this table we report the *intent-to-treat* estimates of the effects of the transport intervention and the job application workshop on psychological outcomes. These are obtained by OLS estimation of equation (1), weighting each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report the *s.e.* in parentheses and the *q*-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. *q*-values are obtained using the sharpened procedure of Benjamini et al. (2006). Changing number of observations due to missing values in the dependent variable. In the last three columns we report the mean outcome for the control group, the *p*-value from a F-test of the null hypothesis that transport subsidies and the job application workshop have the same effect, and the number of observations. ***p< 0.01, **p<0.05, *p<0.1.
[a]To measure "oneness with society", respondents were shown a sequence of 7 figures and asked: "Which one of the following figures best represents your relationship with society?" Each figure depicts two circles, one representing society, the other one representing the respondent. From figures 1 to 7 the circles change from being completely disjoint to entirely overlapping.

Table A.23: **Social networks**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
No. people with whom you regularly share info about job opportunities	-0.347 (.372) [1]	-0.601 (.348)* [.504]	5.182	0.464	2807
No. people with permanent jobs among those with whom you share job info	0.118 (.212) [1]	0.121 (.233) [.778]	2.178	0.987	2528
Can access a guarantor if needed for a job over the next month	-0.00500 (.054) [1]	-0.0660 (.054) [.504]	1.244	0.235	2504
No. of meetings of voluntary associations attended over past 30 months	0.0100 (.061) [1]	0.00900 (.063) [.802]	0.119	0.984	2841

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

Table A.24: **Correlates of 2018 wage earnings from the 2015 endline in the control group**

	(1)	(2)	(3)	(4)
	Dependent variable: 2018 wages			
Employment (2015)	454.2	116.7	222.7	16.38
	(366.2)	(336.0)	(335.1)	(323.7)
Hours worked (2015)	-6.453	-5.168	-0.596	-1.066
	(5.817)	(5.602)	(5.231)	(5.269)
Earnings (2015)	0.0944	0.0884	0.0208	0.0283
	(0.0898)	(0.0929)	(0.0901)	(0.0943)
Permanent work (2015)	1,423***	1,408***	1,130***	1,163***
	(234.9)	(222.2)	(232.9)	(231.4)
Longest work spell (2015-2018)		358.6***		301.1***
		(72.04)		(67.55)
Intercept	838.952	598.501		
	(102.719)	(103.635)		
Baseline controls	NO	NO	YES	YES
Observations	647	647	646	646
R squared (partial)	0.089	0.151	0.051	0.098

Note. This table shows the results of restricting the sample to the control group and then regressing wage earnings measured in 2018 on employment outcomes in 2015. Columns (1) and (2) show the results without baseline (2014 data) controls, while Columns (3) and (4) introduce our standard set of baseline controls defined in Table A.7. We do not report the intercept in Columns (3) and (4) as the many controls used in these regressions make the intercept impossible to interpret.

Table A.25: **Impacts on 2018 wage earnings by baseline characteristics**

Baseline covariate	Covariate = 0 Control mean (1)	Covariate = 0 Transport (2)	Covariate = 0 Workshop (3)	Covariate = 1 Control mean (4)	Covariate = 1 Transport (5)	Covariate = 1 Workshop (6)	Transport Equality (pval) (7)	Workshop Equality (pval) (8)
Above high school	826.4	28.4	493.4**	1,755.5	43.4	47.4	0.93	0.06
		(123.5)	(190.9)		(150.3)	(134.8)		
		[1.000]	[0.029]		[1.000]	[0.676]		
Male	905.5	-39.1	141.3	1,564.3	102.3	499.9**	0.45	0.16
		(110.0)	(104.7)		(162.0)	(234.6)		
		[1.000]	[0.077]		[1.000]	[0.184]		
Active searcher	1,096.7	-0.5	361.8*	1,363.7	60.7	244.3	0.73	0.65
		(124.9)	(184.1)		(143.8)	(174.8)		
		[1.000]	[0.043]		[1.000]	[0.403]		
Ever had permanent job	1,160.5	36.2	369.1***	1,687.4	-22.0	-259.5	0.87	0.10
		(99.7)	(132.9)		(359.5)	(347.6)		
		[1.000]	[0.025]		[1.000]	[0.585]		
Lives close to the centre	1,171.5	15.5	428.6**	1,278.1	43.6	122.5	0.89	0.19
		(134.9)	(179.8)		(146.4)	(142.6)		
		[1.000]	[0.033]		[1.000]	[0.585]		
Born in Addis Ababa	1,217.3	-207.5	147.7	1,216.5	171.7	398.8**	0.08	0.34
		(155.0)	(186.8)		(139.2)	(170.3)		
		[1.000]	[0.106]		[1.000]	[0.184]		
Uses CV/Certificates	1,053.7	-7.3	314.9**	1,912.1	180.2	250.8	0.47	0.84
		(108.0)	(137.5)		(235.5)	(288.1)		
		[1.000]	[0.033]		[1.000]	[0.585]		
Present bias	1,234.1	93.6	468.0***	1,358.3	-53.2	-121.8	0.69	0.07
		(112.7)	(150.1)		(351.7)	(292.1)		
		[1.000]	[0.020]		[1.000]	[0.676]		
Job Search Network	1,031.4	105.8	279.8*	1,402.3	-38.2	343.6	0.51	0.81
		(128.9)	(144.9)		(164.1)	(209.5)		
		[1.000]	[0.043]		[1.000]	[0.316]		

Note. This table shows differential treatment effects by individual baseline characteristics on earnings at the second endline (2018) of the workshop and transport treatments. We estimate heterogenous treatment effects in a saturated model where we interact the treatment with dummies for baseline covariate =0, and for baseline covariate =1. Otherwise the model is the same as the model presented in Equation (1). We weight each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report the *s.e.* in parentheses and a q-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. Columns (1)-(3) shows the results for the sub-sample with the baseline covariate =0, while columns (4)-(6) show the results for sub-sample where the covariate =1. In the last row we show the results where we split the sample by predicted earnings using a range of baseline covariates. For this row, standard errors are derived using bootstrap methods. See Section 5.3 for discussion. Finally, in columns (7) and (8) we test for the equality of the treatment effects between the "covariate=0" and "covariate=1" group, for the transport and workshop treatment, respectively.

Table A.26: **Declining premia for observables**

	Dep var. wage earnings (endline 2)	
	(1)	(2)
Workshop	486.447 (188.123)**	359.829 (129.746)***
Vocational	506.645 (130.007)***	
Degree	1695.112 (749.836)**	
Workshop * Vocational	-419.046 (246.215)*	
Workshop * Degree	-554.849 (437.646)	
Experience		340.324 (252.413)
Workshop * Experience		-596.542 (373.660)
Obs.	2013	2013

Note. This table shows how education premia are affected by the job application workshop intervention. To do this, we estimate an augmented version of model (1) that includes dummies for vocational education and university education, and the interactions between the two treatment dummies and these two education dummies. In the table, for conciseness, we only report the interaction with the workshop dummy as this is our coefficient of interest. As before, we weight each observation by the inverse of the probability of being sampled.Below each coefficient estimate, we report the *s.e.* in parentheses. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

A.2 Sensitivity to attrition

A.2.1 Impacts on earnings at the second endline

We run a series of robustness checks, to ensure that our main result — the effect of the workshop on earnings at the second endline— is not driven by differential rates of attrition by treatment status.

First, we do not find any evidence suggesting that high earning individuals are more likely to attrite from the control group compared to the job application workshop group. Endline 2 attrition is generally uncorrelated with previous earnings – endline 1 earnings or predicted earnings using baseline outcomes.[56] Further, and most importantly, when we repeat these tests but interact earnings and predicted earnings with a dummy for the workshop treatment, we find no evidence that the pattern of attrition is significantly different between the workshop and control groups. If anything we find that in the workshop group individuals with higher earnings in endline 1 are more likely to attrite, relative to individuals with high earnings in the control group (p=0.378). A similar pattern emerges when we perform this analysis with permanent work at endline 1.

Second, we show that our result is robust to several plausible assumptions about the earnings of missing individuals. We follow Karlan and Valdivia (2011) and Blattman et al. (2014) and construct different missing data scenarios. First, we simply impute earnings for *all* missing observations by using predicted earnings.[57] This assumes no differences in the pattern of attrition between the workshop and control groups. We then turn to scenarios with differential attrition between groups. For the control group, we impute missing earnings by using predicted earnings *plus* 0.25 or 0.5 standard deviations of the predicted outcome. For the workshop group, we impute predicted earnings *minus* 0.25 or 0.5 standard deviations of the predicted outcome. Third, we impute missing values by simply imputing the mean plus or minus 0.25 or 0.5 standard deviation of the outcome in the control group. This is a conservative assumption: it is equivalent to imputing, respectively, the 72nd and 80th percentile of the control group distribution – a very strong assumption about the pattern of missing data which is hard to reconcile with the results on attrition reported above. Thus we tighten our bounds by using mean earnings for a given education level and gender.[58] Table A.27 shows the results. As we impose increasingly conservative assumptions, the point estimate of the effect of the workshop naturally decreases. However, we are able to estimate economically large and statistically significant effects of the workshop in the large majority of cases. For instance, the size of the effect is above 10 percent of the control group mean in all simulations but one. Even when we impute a full 0.5 standard deviations of the control standard deviation – the most conservative test – the point estimate of the effect is still positive.

[56] We do not use actual baseline earnings as these are zero for a large number of jobseekers.

[57] We predict earnings using our main set of baseline covariates, estimated on the non-attrited control group.

[58] Given the large earnings differentials between these groups, we believe this is the most sensible approach. High earners are typically university graduates and male. It would be implausible to assume that missing individuals without tertiary education earn as much as the top university graduates, or that missing women earn as much as top male earners.

Table A.27: **Effect of attrition on 2018 earnings results – lower bounds (workshop)**

Outcome	Control mean (1)	ITT Estimate Coeff (2)	Std. Err. (3)
Predicted earnings	1,531.1	250.3**	109.2
Predicted earnings +/- 0.25 SDs	1,545.6	222.0**	109.2
Predicted earnings +/- 0.5 SDs	1,560.1	193.6*	109.3
Mean control earnings +/- 0.25 SDs	1,574.6	187.0*	109.6
Mean control earnings +/- 0.5 SDs	1,649.0	45.6	110.9
Extreme cases			
95th / 5th percentile	2,222.7	-717.7***	139.1
Max/min	5,295.6	-4,265.6***	460.5

For completeness, we perform a bounding exercise with extreme assumptions about the missing data. We impute the 95th percentile to missing values in the control group and the 5th percentile to missing observations the treatment (workshop) group. In practice, this means imputing zeroes to missing observations in the treatment group, and imputing four times the mean to the control group (earnings is a notoriously skewed variable). Not surprisingly, therefore, we estimate a large negative coefficient with this extreme assumption. This is in line with the findings of other recent RCTs that have calculated bounds on treatment effects on earnings using extreme attrition assumptions (e.g. Blattman et al. (2014)). Similarly, when we impute the control group maximum to the control group (fifteen times the mean) and the minimum to the treatment group (zeros) we estimate a very large and negative coefficient (the Manski (1990) bounds). These two results are shown in the final two rows of Table A.27.

Table A.28: **Effect of attrition on 2018 earnings results – upper bounds (workshop)**

Outcome	Control mean (1)	ITT Estimate Coeff (2)	Std. Err. (3)
Predicted earnings	1,531.1	250.3**	109.2
Predicted earnings +/- 0.25 SDs	1,516.7	278.6**	109.2
Predicted earnings +/- 0.5 SDs	1,502.2	307.0***	109.2
Mean control earnings +/- 0.25 SDs	1,425.7	469.9***	110.4
Mean control earnings +/- 0.5 SDs	1,351.3	611.3***	112.4
Extreme cases			
95th / 5th percentile	1,285.9	1,194.1***	136.5
Max/min	1,285.9	3,132.2***	316.2

Finally, we show sensitivity in the other direction. We perform the same excercise as the one above but this time imputing the higher values to missing observations in the treatment group, and lower values in the control group. See Table A.28.

A.2.2 Impacts on employment outcomes at the first endline

We repeat the bounding excercise used above to check our main results at the first endline (2015), namely the effects on permanent work and formal work. The results are presented in Table A.29 to Table A.32, for permanent work and formal work, respectively. We show that our finding of significant impacts of the workshop on job quality is robust to a number of assumptions about the pattern of attrition. Furthermore, we can show that our results are robust even to the more stringent method of bounding by Lee (2009).

Table A.29: **Effect of attrition on 2015 permanent work results (workshop)**

Outcome	Control mean (1)	Workshop Coeff (2)	Workshop Std. Err. (3)
Imputed permanent work	0.170	0.063***	0.018
Imputed permanent work +/- 0.25 SDs	0.171	0.061***	0.018
Imputed permanent work +/- 0.5 SDs	0.172	0.058***	0.018
Mean control permanent work +/- 0.25 SDs	0.177	0.056***	0.018
Mean control permanent work +/- 0.5 SDs	0.184	0.043**	0.019

Table A.30: **Effect of attrition on 2015 formal work results (workshop)**

Outcome	Control mean (1)	Workshop Coeff (2)	Workshop Std. Err. (3)
Imputed written agreement	0.223	0.054***	0.018
Imputed written agreement +/- 0.25 SDs	0.224	0.051***	0.018
Imputed written agreement +/- 0.5 SDs	0.225	0.048***	0.018
Mean control written agreement +/- 0.25 SDs	0.230	0.044**	0.018
Mean control written agreement +/- 0.5 SDs	0.238	0.029	0.019

Table A.31: **Effect of attrition on 2015 permanent work results (transport)**

Outcome	Control mean (1)	Transport Coeff (2)	Transport Std. Err. (3)
Imputed permanent work	0.170	0.034**	0.016
Imputed permanent work +/- 0.25 SDs	0.171	0.031*	0.016
Imputed permanent work +/- 0.5 SDs	0.172	0.029*	0.016
Mean control permanent work +/- 0.25 SDs	0.177	0.024	0.017
Mean control permanent work +/- 0.5 SDs	0.184	0.010	0.017

Table A.32: **Effect of attrition on 2015 formal work results (transport)**

Outcome	Control mean (1)	Transport Coeff (2)	Transport Std. Err. (3)
Imputed written agreement	0.223	0.057***	0.018
Imputed written agreement +/- 0.25 SDs	0.224	0.055***	0.018
Imputed written agreement +/- 0.5 SDs	0.225	0.052***	0.018
Mean control written agreement +/- 0.25 SDs	0.230	0.046**	0.018
Mean control written agreement +/- 0.5 SDs	0.238	0.029	0.018

Table A.33: **Lee Bounds for 2015 permanent and formal work**

		Transport		Workshop	
		Coeff (1)	Std. Err. (2)	Coeff (3)	Std. Err. (4)
Formal Work	lower	0.057**	0.024	0.041*	0.024
	upper	0.059***	0.021	0.059**	0.021
Permanent work	lower	0.028	0.022	0.049**	0.022
	upper	0.031*	0.018	0.067***	0.019

A.3 Indirect effects on the untreated

In this section, we study the outcomes of untreated job-seekers who live close to program participants. To do this, we leverage the fact that we did not offer the treatments to all eligible individuals that we interviewed in the clusters assigned to a given treatment (as explained in the design section, we only treated a random subsample of individuals in each cluster). We introduced this design feature to capture a number of potential spillover effects of the interventions. In particular, we were interested in spillover effects related to the sharing of information, job referrals, or financial support among friends and acquaintances in the same neighbourhood. These types of social interaction have been documented in several recent studies on developing countries' labour markets (Angelucci and De Giorgi, 2009; Magruder, 2010), and are consistent with qualitative and descriptive evidence on our setting.

This research design is however not well-suited to detect displacement effects due to the reallocation of jobs from untreated to treated individuals. The geographical clusters that we use for randomization rarely exceed 300m in diameter. This distance enables us to capture an area with dense social interactions, but is inadequate to circumscribe the relevant labour market or commuting zone in which displacement may occur. This is evident form our baseline data, where only 30% of employed young people walk to work. Among those who use public transport, median commuting time (one-way) is 35 minutes, and more than 90% commute further than 15 minutes. Further, workers who get formal, higher paid work, are especially likely to hold jobs that are not in their immediate vicinity. This makes it unlikely that the displacement effects of our interventions, if they exist, will be observable in the clusters that we use for the spillover design. The results reported in this section should thus not be interpreted as a test of displacement effects.

The spillover design was implemented in a slightly different way for the two interventions. For the job application workshop, the proportion of treated individuals in treated clusters was fixed at 80%. For the transport intervention, we randomly varied the proportion of treated individuals from 20% to 40%, 75% and 90% of all eligible individuals. Both designs enable us to compare untreated individuals living close to program participants to untreated individuals living in clusters where no job-seeker has been offered the intervention. Additionally, we can study the effect of different levels of saturation of the transport intervention by estimating a regression model of the following form:

$$\begin{aligned} y_{ic} = {} & \kappa + \beta_{20} \cdot S_{20c} \cdot C_i + \beta_{40} \cdot S_{40c} \cdot C_i + \beta_{75} \cdot S_{75c} \cdot C_i + \beta_{90} \cdot S_{90c} \cdot C_i \\ & + \gamma_{20} \cdot S_{20c} \cdot T_i + \gamma_{40} \cdot S_{40c} \cdot T_i + \gamma_{75} \cdot S_{75c} \cdot T_i + \gamma_{90} \cdot S_{90c} \cdot T_i \\ & + \alpha \cdot y_{ic,pre} + \boldsymbol{\delta} \cdot \boldsymbol{x}_{i0} + \mu_{ic} \end{aligned} \tag{5}$$

where 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.[59] 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

[59] The sample for this analysis is restricted to individuals in clusters assigned to pure control and clusters assigned to the transport intervention.

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 (the other βs and γs follow the same definition for different levels of saturation).

For both interventions, we find no significant difference, on average, between untreated individuals living in treated clusters and untreated individuals in pure control clusters (Table A.34).[60] Behind this average result, however, we find some evidence that the indirect effects of the transport treatment depend on the level of saturation (Table A.35). In clusters with 40 percent saturation, we document a positive indirect effect of the transport treatment on formal and permanent work. On the other hand, 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.[61] They are not, however, less likely to be in formal employment. Due to the limitations outlined above, we can only interpret these results as tentative evidence of local spillovers. Given the small sample sizes and the number of tests run in Tables A.35 and A.36, this evidence should be interpreted with caution.

[60] One should keep in mind, however, that we are less powered to detect spillover effects than we are to investigate core treatment impacts. Some of these indirect effects, therefore, may have been detected as significant with greater power.

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

Table A.34: **Spillover effects of the transport and workshop intervention on employment outcomes (2015)**

Outcome	Spillover Transport	Spillover Workshop	Control Mean	F	N
Worked	-0.0460	0.0280	0.537	0.541	2841
	(.034)	(.053)			
	[1]	[1]			
Hours worked	-2.382	0.409	25.57	0.925	2835
	(1.855)	(2.573)			
	[1]	[1]			
Formal work	0.0140	0.0570	0.172	0.929	2841
	(.02)	(.038)			
	[1]	[1]			
Perm. work	0.00600	0.0120	0.120	0.0927	2841
	(.019)	(.027)			
	[1]	[1]			
Self-employed	-0.0150	-0.0160	0.102	0.301	2841
	(.019)	(.029)			
	[1]	[1]			
Monthly earnings	-41.10	13.46	971.4	0.417	2802
	(89.847)	(103.597)			
	[1]	[1]			
Satis. with work	-0.0170	0.0440	0.231	0.482	2841
	(.024)	(.048)			
	[1]	[1]			

Note. In this table we report the *intent-to-treat* estimates of the 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. In the far right column, we report N for the full saturated model of equation (1), although the we only report the coefficients for the spillover groups. Below each coefficient estimate, we report the *s.e.* in parentheses 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 A.35: **Spillover effects of the transport treatment on the untreated (by randomised level of cluster saturation)**

	20%	40%	75%	90%	*F(p)*
Worked	-0.0900	-0.0150	-0.00200	0.0170	0.457
	(0.048)*	(0.040)	(0.078)	(0.081)	
Hours worked	-4.664	-1.003	-1.262	3.055	0.418
	(2.585)*	(2.433)	(3.635)	(4.836)	
Formal work	-0.0110	0.0620	0.0270	-0.0400	0.204
	(0.023)	(0.033)*	(0.066)	(0.062)	
Perm. work	-0.0170	0.0640	0.0220	-0.0680	0.003***
	(0.023)	(0.030)**	(0.045)	(0.026)***	
Self-employed	-0.0250	0.00300	-0.00200	-0.00500	0.841
	(0.024)	(0.028)	(0.054)	(0.045)	
Monthly earnings	-111.6	53.27	-49.21	73.69	0.627
	(109.497)	(131.878)	(249.147)	(172.380)	
Satis. with work	-0.0320	0.00700	-0.0240	-0.00700	0.868
	(0.031)	(0.041)	(0.058)	(0.071)	

In the last column we report the p-value from an F-test of the null hypothesis that spillover effects are the same at all saturation levels. ***$p< 0.01$, **$p<0.05$, *$p<0.1$.

Table A.36: **Spillover effects of the transport treatment on the treated (by randomised level of cluster saturation)**

	20%	40%	75%	90%	*F(p)*
Worked	0.0250	0.0670	0.0220	0.0420	0.905
	(0.083)	(0.051)	(0.046)	(0.035)	
Hours worked	-1.234	0.0560	-1.039	0.631	0.909
	(4.233)	(2.924)	(2.337)	(1.891)	
Formal work	0.0240	0.0320	0.0880	0.0530	0.696
	(0.051)	(0.043)	(0.041)**	(0.021)**	
Perm. work	-0.0120	0.0100	0.0510	0.0330	0.543
	(0.040)	(0.031)	(0.032)	(0.023)	
Self-employed	0.0520	-0.0390	-0.0110	-0.0280	0.334
	(0.051)	(0.033)	(0.017)	(0.019)	
Monthly earnings	-11.68	-66.47	-6.404	25.39	0.906
	(198.954)	(122.883)	(130.482)	(80.677)	
Satis. with work	0.0560	-0.0170	0.00800	-0.0100	0.800
	(0.067)	(0.054)	(0.037)	(0.036)	

In the last column we report the p-value from an F-test of the null hypothesis that spillover effects are the same at all saturation levels. ***$p< 0.01$, **$p<0.05$, *$p<0.1$.

A.4 Changing search intensity and changing search efficacy: A simple theoretical framework

In this appendix, we present a simple framework to guide intuition on how a respondent's labour market experience might be affected either by a search subsidy (through the transport intervention) or by an improvement in his/her ability to signal skills (through the workshop intervention). The framework is built around two key labour market frictions: (i) it takes time for a worker to find a vacancy, and (ii) firms want to hire the worker that is best suited for the job but observe match quality with noise. Both frictions are the subject of an extensive literature (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Rogerson et al., 2005; Kahn and Lange, 2014; Pallais, 2014). Further, as discussed in Section 2, the descriptive evidence suggests that both frictions are likely to play an important role in the Addis Ababa labour market.

We focus the discussion on the direct effects that relaxing these frictions has on (i) the probability of employment with a formal contract, and (ii) the quality of a match between employer and employee. Our stylised framework is not intended to be a comprehensive and quantifiable model of the labour market we study. We deliberately abstract away from general equilibrium effects or behavioural responses through reservation wages. We restrict heterogeneity to the minimum needed to develop our main predictions.

A.4.1 A hazard model framework

As discussed in the body of the paper, it is relatively straightforward for any worker in Ethiopia to find some kind of job — including, if necessary, in the informal sector. The focus of this model, therefore, is on the challenge of finding employment with a formal contract.

We begin by assuming that workers have idiosyncratic heterogeneity in the quality of their match to particular firms, and in the signals that such firms receive. We imagine a large labour market, and consider the experiences of a worker entering at some time $t = 0$. Denote E_t as the probability that the individual is, at time t, employed with a formal contract (for shorthand, we describe this simply as 'employment'). We focus on a worker who is initially unemployed: $E_0 = 0$.

Formally, we frame the model in discrete time, where $t > 0$ is the number of periods (*e.g.* weeks) that have passed since job search begins. We assume that, in any given period, there is a probability $p \in (0, 1)$ that an unemployed worker is matched with a firm (this event is independent across periods). p is a reduced-form parameter that captures the intensity of job search. If a firm-worker match is made in a given period, the firm observes a signal about the worker and decides whether to hire. For simplicity, we assume a homogeneous first-order Markov process converting job matches into hires. Let s represent the probability of being hired (*i.e.* of transitioning from non-employment to employment), conditional on having been matched to a firm. We assume formal employment to be an absorbing state, so the probability of employment after time $t > 0$ for an individual in the control group is:[62]

$$E_t = 1 - (1 - ps)^t \,. \tag{6}$$

This setup immediately suggests two stylised ways in which an active labour market intervention might seek to improve employment prospects. First, it may reduce the cost of viewing available job vacancies — and, therefore, encourage job-seekers to increase their job application rate. Our transport intervention falls into this class of policy. It can be represented by having the individual matched with a firm with probability $\tilde{p} > p$ in any given period. Second, an intervention may improve the technology of search — such that, for each job application, a job-seeker has an increased probability of being hired. This can be represented by scaling up the probability of success to $\tilde{s} > s$. We next present a signal-processing framework to illustrate how the workshop treatment may increase s.

A.4.2 Search success in a signal-processing framework

To analyse what determines the probability of a hire s, let us consider the case of an individual i matched with firm f. The true match quality of individual i with firm f is given by x_{if}. However, x_{if} is observed by the firm with noise, which we denote as ε_{if}; specifically, the observed signal is given by $y_{if} = x_{if} + \varepsilon_{if}$. For tractability, we assume a 'Normal-Normal' structure, with the variance of match quality normalised to 1; namely:

$$x_{if} \sim \mathcal{N}(0, 1); \tag{7}$$

$$\varepsilon_{if} \sim \mathcal{N}(0, \sigma^2). \tag{8}$$

We assume that, each time an individual applies for a job, a firm f is drawn randomly from the population of firms in the economy, and that x_{if} and ε_{if} are independent of each other.

We allow firms to be risk averse in their hiring preferences; this reflects the substantial costs that firms incur in screening workers. For tractability, we assume that firms have CARA utility in worker quality, with coefficient of absolute risk aversion r: for each firm, $u(x) = -\exp(-rx)$; this implies that the certainty equivalent of a hire is given by $\mathbb{E}(x) - 0.5r \cdot \mathrm{Var}(x)$.

How, then, does a firm react to receiving a signal y_{if}? By Bayes' Rule, the firm infers:

$$x_{if} \,|\, y_{if} \sim \mathcal{N}\left(\frac{y_{if}}{1+\sigma^2}, \frac{\sigma^2}{1+\sigma^2}\right). \tag{9}$$

We set the firm's outside option to an expected utility of zero. We can think of this as the

[62] The assumption that employment is an absorbing state enables us to capture in a stylised way a labour market where the employment rate is growing over time, in line what we we observe in the data. This assumption could be relaxed straightforwardly, at the expense of tractability and model intuition. Similarly, we could provide explicit micro-foundations for the decision to make a job application or to accept an offer — but this, too, would unnecessarily complicate the exposition; further, it is a stylised fact of this labour market that job-seekers do not decline offers for formal jobs.

expected utility of hiring the best alternative applicant. Our framework can thus be used to describe a labour market where there are no unfilled vacancies: if the firm chooses to hire worker i, it displaces another applicant with lower expected match quality.

The firm hires if and only if:

$$y_{if} - 0.5r \cdot \sigma^2 \geq 0. \tag{10}$$

It follows that a firm is less likely to hire when (i) the firm is more risk averse, and (ii) the signal is noisier. This implies that a reduction in applicant i's signal noise increases the unconditional probability that the firm will hire.[63]

Two implications follow from this:

Probability of hiring: Given the unconditional distribution, $y_{if} \sim \mathcal{N}(0, 1+\sigma^2)$, it follows that the probability of worker i being hired, conditional on a match, is:

$$s = \Phi\left(\frac{-0.5r\sigma^2}{\sqrt{1+\sigma^2}}\right), \tag{11}$$

where Φ denotes the *cdf* of the Normal distribution. Since this probability is decreasing in the signal noise, σ^2, an intervention that improves the quality of a jobseeker's signal, like the job application workshop, increases the probability s that a treated worker is hired.

Match quality: The expected quality of a match is therefore given by:

$$\mathbb{E}(x_i \,|\, y_i > 0.5r\sigma^2) \cdot \Pr(y_i > 0.5r\sigma^2) + 0 \cdot \Pr(y_i \leq 0.5r\sigma^2) = \frac{\phi\left(\frac{0.5r\sigma^2}{\sqrt{1+\sigma^2}}\right)}{\sqrt{1+\sigma^2}}. \tag{12}$$

Since this function is decreasing in σ^2, if the job-seeker reduces σ^2, the expected match quality increases.

What about the expected quality of a match conditional on being hired? Following the same reasoning, this is given by:

$$\mathbb{E}(x_i \,|\, y_i > 0.5r\sigma^2) = \frac{\phi\left(\frac{0.5r\sigma^2}{\sqrt{1+\sigma^2}}\right)}{\sqrt{1+\sigma^2} \cdot \left[1 - \Phi\left(\frac{0.5r\sigma^2}{\sqrt{1+\sigma^2}}\right)\right]}. \tag{13}$$

As we discuss shortly, this expression is useful for guiding our intuition about the possible wage effects of the workshop intervention. It can be shown numerically that equation 13 is

[63] We assume throughout that the expected value of the firm's outside option is zero. We could generalise this by assuming that the expected value of the firm's outside option is given by some x_a. In that case, the negative relationship between applicant signal noise and the probability of hiring will hold if $x_a < r \cdot (1 + 0.5\sigma^2)$.

decreasing in σ^2 for any reasonable risk aversion parameters.[64] That is, an intervention that improves jobseekers' signals, like the job application workshop, will lead to higher-quality matches between workers and firms, both overall and conditional on hiring. This is an intuitive result: the intervention improves the information available to the firm and thus enables the firm to make a more accurate assessment of which candidate is the best match for the position.

A.4.3 Impacts on formal employment and match quality of the two interventions

The simple framework we have just presented enables us to characterise the impacts of the two interventions on formal employment and match quality and how they evolve with time. To study impact dynamics, we assume that the direct effects of the transport treatment (namely, increasing p to $\tilde{p}$) and of the workshop treatment (increasing s to $\tilde{s}$) last for a fixed number of periods T, after which the match rate and the success rate revert respectively to p and to s.[65] Our framework implies that, under these assumptions, both interventions will have a positive impact on formal employment that declines with time. Consider an arbitrary period $t > T$. At time t, the employment rate in the control group is $1 - (1 - ps)^t$; the employment rate in the workshop group is $1 - (1 - p\tilde{s})^T \cdot (1 - ps)^{t-T}$; and the employment rate in the transport group is $1 - (1 - \tilde{p}s)^T \cdot (1 - ps)^{t-T}$. It follows that the increase in the rate of employment in the workshop group relative to the control group is:

$$ATE(x)_{\text{workshop, employment}} = (1 - ps)^{t-T} \cdot \left[(1 - ps)^T - (1 - p\tilde{s})^T\right], \tag{14}$$

whereas the increase in the rate of employment in the transport subsidy group relative to the control group is:

$$ATE(x)_{\text{transport, employment}} = (1 - ps)^{t-T} \cdot \left[(1 - ps)^T - (1 - \tilde{p}s)^T\right]. \tag{15}$$

The effects of both interventions are largest immediately after the treatment ends ($t = T$). However, in the limit as $t \to \infty$, both average treatment effects on the employment rate go to zero. The reason is that, when treatment ends, there are fewer jobless individuals in the treatment group. Thus, while jobless individuals in both groups now have the same probability of finding employment in a given period, *the number* of people who do so is

[64] Specifically, the expression is decreasing in σ^2 for any $\sigma^2 > 0$ so long as $r < 1.2533$. This critical value is at least two orders of magnitude larger than most estimates of reasonable values for the coefficient of risk aversion (see, for example, Cohen and Einav (2007)). To put the absurdity of $r > 1.2533$ in perspective, using an interpretative device from Cohen and Einav (2007), a firm having $r = 1.2533$ would be indifferent between accepting and refusing a lottery having a 50% chance of winning \$100 and a 50% chance of losing just 55 cents.

[65] Of course, there is no reason that the workshop intervention should only affect search for this limited period; it is entirely possible, for example, that the certificate remains credible after this period, or that the job-seeker remembers the interview skills after this time. But this simplifying assumption ensures that the differences in model predictions follow directly from the different mechanisms of the search interventions, rather than following from an arbitrary assumption about search skill depreciation.

larger in the control group.[66]

What about match quality? Denote the average match quality in the control group by m, and the average match quality in the workshop group by αm, with $\alpha > 1$ (where m and αm are obtained by evaluating equation 13, for a specific value of r and σ^2.) We normalise the average match quality among the non-employed to zero. At time $t > T$, the average match quality in the control group is $m \cdot \left[1-(1-ps)^t\right]$. The average match quality in the workshop treatment group is $\alpha m \cdot \left[1-(1-p\tilde{s})^T\right] + m \cdot \left[(1-p\tilde{s})^T \cdot \left(1-(1-ps)^{t-T}\right)\right]$. The treatment effect on match quality at time $t > T$ among the workshop group, relative to the control group, is thus:

$$m \cdot \left\{ \underbrace{\alpha \cdot \left[1-(1-p\tilde{s})^T\right] + (1-p\tilde{s})^T \cdot \left[1-(1-ps)^{t-T}\right]}_{\text{average match quality in the workshop group}} - \underbrace{\left[1-(1-ps)^t\right]}_{\text{average match quality in the control group}} \right\} \tag{16}$$

$$= m \cdot \left\{ \underbrace{\alpha \cdot \left[1-(1-p\tilde{s})^T\right] - \left[1-(1-ps)^T\right]}_{\text{effect during first } T \text{ periods}} - \underbrace{\left[(1-ps)^T-(1-p\tilde{s})^T\right] \cdot \left(1-(1-ps)^{t-T}\right)}_{\text{convergence during periods } t>T} \right\}. \tag{17}$$

As with the effect on employment rates, this match quality effect is maximised for $t = T$ — that is, before $\tilde{s}$ reverts to s. In the limit as $t \to \infty$, the average workshop treatment effect on match quality does not go to zero; rather, it goes to $m \cdot (\alpha - 1) \cdot \left[1-(1-p\tilde{s})^T\right]$. The intuition for this is straightforward: a share $1-(1-p\tilde{s})^T$ of individuals in the workshop group found formal jobs during the first T periods, and these individuals enjoy a permanent increase of $m \cdot (\alpha - 1)$ in match quality.

Finally, note that the average match quality in the transport group is also given by m. There is a temporary effect of this intervention on unconditional match quality entirely driven by the higher rate of employment in the formal sector compared to the control group. This effect disappears as the main effect on formal employment dissipates.

A.4.4 Introducing an observable covariate

The previous results help to guide our intuition about the likely general effects of the workshop intervention. However, we are also interested in effect heterogeneity: how should we expect the signal value to differ between groups that, absent the intervention, would secure different outcomes in the labour market?

To answer this question, we introduce an additional variable: an observable covariate that correlates with match quality. Until now, we have considered heterogeneity only in *un-*

[66] Formally, the rate of decay of the treatment effect is $\dfrac{ATE(t)-ATE(t+1)}{ATE(t)} = 1 - \dfrac{ATE(t+1)}{ATE(t)} = ps$.

observable match quality (x_{if}) and noise (ε_{if}). We now consider what happens if firms have some *observable* proxy for job suitability, such as previous work experience or place of birth. Formally, we introduce a variable z_i, which is fixed at the individual level and known both to the worker and to the firm. We normalise z to have the same variance as x (*i.e.* normalised to 1), and we assume that z and x have a bivariate Normal distribution, with correlation $\rho \geq 0$:

$$\begin{pmatrix} x_{if} \\ z_i \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix}\right). \tag{18}$$

Using standard results from the bivariate normal, we know that the distribution of x_{if}, conditional on the observed value of z_i, is:

$$x_{if} \mid z_i \sim \mathcal{N}\left(\rho \cdot z_i, (1-\rho^2)\right). \tag{19}$$

We can then extend the earlier results to think about heterogeneous effects.[67] First, note that, by Bayes' Rule, the firm now infers:

$$x_{if} \mid y_{if}, z_i \sim \mathcal{N}\left(\frac{y_{if} \cdot \sigma^{-2} + \rho \cdot z_i \cdot \left(1-\rho^2\right)^{-1}}{\sigma^{-2} + (1-\rho^2)^{-1}}, \frac{1}{\sigma^{-2} + (1-\rho^2)^{-1}}\right). \tag{20}$$

Therefore, the firm hires if and only if $y_{if} \geq 0.5r\sigma^2 - \frac{\rho\sigma^2}{1-\rho^2} \cdot z_i$. Since $y_{if} \mid z_i \sim \mathcal{N}\left(\rho \cdot z_i, 1-\rho^2+\sigma^2\right)$, the probability of a worker being hired now is:

$$S_i = \Phi\left(\frac{-0.5r\sigma^2}{\sqrt{1-\rho^2+\sigma^2}} + z_i \cdot \frac{\rho\sqrt{1-\rho^2+\sigma^2}}{1-\rho^2}\right). \tag{21}$$

The main object of interest for our purpose is the sign of the cross-partial derivative of equation 21 with respect to z_i and σ. It is positive given that, by construction, $\rho > 0$. Thus, a *reduction* in signal noise is more valuable for job-seekers with a *worse* observable proxy, z_i. The intuition for this result is already captured in equation 20: because z correlates with x, the firm uses z for statistical discrimination to compensate for signal noise. A reduction in noise thus makes this disadvantage less severe.

A.4.5 Predictions: Applying the model framework

We summarise the insights from this stylised framework with the following four predictions:

Prediction 1 (effect on formal employment): *Both the transport intervention and the workshop intervention increase the rate of employment in formal jobs. These effects progressively dissipate*

[67] Note, of course, that these results will nest the earlier results for the special case $\rho = 0$. For $\rho > 0$, the earlier results go through *conditional* on a realised value of z; *i.e.* we could rewrite the earlier results in terms of S_z.

after job-search support is withdrawn.

Prediction 2 (search intensity vs. search efficacy): *T*he interventions generate these effects through different mechanisms. The transport intervention increases the number of job vacancies that are viewed during the treatment period; the workshop intervention does not. Instead, the workshop increases the probability that a worker is offered a job after viewing a vacancy.

Prediction 3 (effect on match quality): *The workshop intervention leads to a persistent increase in the quality of the match; the transport intervention does not.*

Prediction 4 (heterogeneity of impacts): *The effect of the workshop intervention is higher for individuals with worse observable characteristics.*

Although we have not included a model of wage formation in our framework, it is widely accepted that wage earnings at least partly reflect labour productivity, and thus match quality: the better the match, the higher we expect the wage to be, conditional on hiring. It follows that predictions 3 and 4 also apply to wage earnings. In practice, it may take some time for the earning effects to materialise, as firms may be constrained by compressed salary scales (Breza et al., 2017), may be legally bound by the wages listed in the job vacancy, or may use career incentives and thus delay young workers' match-quality compensation to raise effort (Lazear, 1979, 2018).