CENTRE FOR THE STUDY OF AFRICAN ECONOMIES

CSAE Working Paper WPS/2016-10-2

Anonymity or Distance?

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

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August 19, 2018

Abstract

We show that helping young job-seekers to signal their skills to employers can generate large and persistent improvements in 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. We find that in the short-run both interventions have large positive effects on the probability of finding formal jobs. 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, while the effects of the transport subsidy have dissipated. These gains are concentrated among groups 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.

*We are grateful to Gharad Bryan, Erica Field, Markus Goldstein, Douglas Gollin, Supreet Kaur, Julien Labonne, Jeremy Magruder, Marco Manacorda, Muhammad Meki, David McKenzie, Mushfiq Mobarak, Amanda Pallais, Barbara Petrongolo, Pieter Serneels, Alemayehu Seyoum Taffesse, Francis Teal and Christopher Woodruff for helpful comments and to Jali Bekele, Giulio Schinaia, Vaclav Tehle, Biruk Tekle, Marc Witte, Alemayehu Woldu and Ibrahim Worku for outstanding research assistance. Data collection and experimental implementation were funded by GLM | LIC ('Assisting Job Search in Low-Employment Communities: The Effect of Information Provision and Transport Vouchers in Addis Ababa') and by the International Growth Centre ('Assisting Job Search in Low-Employment Communities: The Effect of a Screening Intervention in Addis Ababa'). The project would not have been possible without the constant support of Rose Page and the Centre for the Study of African Economies (University of Oxford), nor without the support of the Ethiopian Development Research Institute in Addis Ababa. This RCT was registered in the American Economic Association Registry for randomized control trials under Trial number AEARCTR-0000911. It was reviewed by the Research Ethics Committee of the Department of Economics of the University of Oxford and received official approval (Econ DREC Ref.No. 1314/0023).

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1 An experiment to help youth access the labour market

Finding employment for young job-seekers is one of the major policy challenges facing the world today. Young adults commonly work less, earn less, and face more job insecurity than older workers. This is true in developed economies, but even more so in parts of the world where the population is expanding rapidly. In Africa, for instance, the labour force includes almost 200 million people between the ages of 15 and 24. The exclusion of young adults from economic opportunity is a major source of inefficiency and inequity in labour markets.

What policy interventions can help young people find good jobs? The existing evidence is largely inconclusive, especially in developing countries (Kluve et al., 2016; McKenzie, 2017). One common view is that *reducing the cost of job search* is crucial, as this allows job-seekers to gather more information about existing opportunities and apply for the ones 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. An alternative view is that the main difficulty faced by young job-seekers is to convey accurate information 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 need not help them secure the well-paid, stable employment that they crave – although it may result in a temporary increase in less desirable forms of employment. Under this view, improving young people's *ability to signal their skills* would be more effective.

To investigate which of these two competing views hold more truth, 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.[1] 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

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

increases job-search intensity and increases the probability of having a formal job eight months after treatment — but, 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 outcome. The job application workshop, in contrast, shows long-lasting effects. In the short run it increases the probability of permanent and formal work *without* increasing the intensity of job search. Four years after treatment, the workshop shows a large positive impact on wages, amounting to a 20% increase over the control group mean. This suggests that improving a young job seeker's ability to signal skills changes their long-term prospects in the labour market.

Additional findings suggest that the young job-seekers in our study have productive skills that employers fail to detect. Raising the quality of their signals improves the quality of job-matches and increases productivity. Several pieces of evidence support this conclusion. First, the intervention generates earnings growth by increasing wages, rather than by increasing hours worked or employment (which are only changed modestly and insignificantly by the intervention). We argue that these earnings increases reflect productivity, and therefore match quality, since they are sustained four years after the intervention and, on average, 20 months after workers got their current jobs. Further, 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. Taken together, this evidence is consistent with the idea that, on average, the workshop treatment makes young workers appear more productive to prospective employers and this perception is subsequently confirmed on the job (i.e. the intervention does not operate by simply making job-seekers 'look better' in the eyes of employers). The earning gain of treated workers is 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 controls, wages only grow at roughly the rate of inflation. To the extent that enabling employers to observe, price, and employ under-utilised skills generates net gains for the economy, our intervention could make labour markets more efficient.

We also show that our workshop intervention has the potential to reduce inequality in labour market outcomes. Groups who usually have worse labour market outcomes benefit most. The short-run gains from both interventions are concentrated among the most disadvantaged socio-demographic groups. The long-run earning impact of the workshop is similarly concentrated among workers with the worst labour market prospects, i.e., those with the least education and experience. As a result, our interventions lead to a reduction in income inequality of a relevant magnitude. 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.

Last but not least, we are able to show that helping young people signal their skills is a remarkably cost-effective policy 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 compares favourably to other available labour market interventions: the benefit-to-cost ratio that we estimate for the workshop intervention comfortably exceeds that of other interventions documented in the literature recently reviewed by McKenzie (2017). 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). In addition, the job application workshop is easy to implement and to scale up, which is not the case for all labour market interventions.

This paper makes a contribution to the literature on labour markets in developing countries by providing empirical evidence that information asymmetries hinder 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. (2016) have shown 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, we show that employment prospects can be improved simply by providing workers with tools to verify their abilities that they can use independently. Our workshop can be implemented with any individual, regardless of their previous work experience, educational background, and the labour market in which they are searching. The intervention works without any additional skills training, and with no collaboration with firms: workers independently choose whether and how to use their improved signals. 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 expanded 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 information constraints different from those at play in a population already exposed to an urban labour market.[2]

Our study also overcomes some of the shortcomings in the recent experimental literature on active labour market interventions in developing economies (as reviewed, for example, by 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 selected along a particular economic dimension (e.g., whether they are actively searching for work, or 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. Third, we follow a pre-analysis plan that specifies all of our main outcomes of interest.[3] 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.

Finally, our findings provide original evidence on the key role played by job mobility in the urban labour market of a large developing country. Workers in our study have short average job tenures and, by the time of the second endline, most have changed job at least once. Through mediation analysis, we are able to document two important channels by which our interventions raise long-run earnings. These mediators relate to employment quality at the first follow-up, namely: earnings; and whether the job is permanent. This

[2] 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. This intervention relaxes constraints for both firms and workers and uses randomisation at both the firm and the worker level. Our analysis of that job fair intervention reveals that job-fairs have no significant impacts on employment and earnings but can help correcting biased beliefs on the part of firms and workers regarding the composition of the labour force and the available opportunities in the market.

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

suggests some form of path-dependency, suggesting that precarious jobs may not be a stepping stone to better employment later in a worker's career. A similar path dependency has been documented in developed countries using both quasi-experimental (Oreopoulos et al., 2012; Kahn, 2010) and experimental (Kroft et al., 2013) methods. Other work has similarly shown that taking a temporary job has a detrimental long-run effect for OECD workers (Perez et al., 2016). Our results indicate that similar dynamics are at play in developing countries. In fact, given that labour flows tend to be larger in poorer economies (Donovan et al., 2018), employment history may have an even stronger effect on worker trajectories. Overall, our results emphasize the need to intervene early in workers' career to limit the scarring effects of a bad start. An intervention like the job application workshop that we have tested in this study represents a viable and effective policy option to serve this objective.

2 The interventions

2.1 The challenge of youth employment in developing countries

Finding satisfactory employment is a common challenge for young people around the world. Recent empirical evidence suggests that, in developing countries, this challenge is particularly related to the quality of employment. In particular, three key stylised facts emerge from cross-country labour market comparisons. First, open unemployment is less common in developing countries (Feng et al., 2017). Second, a particularly high share of employment in developing countries is either informal (without a written contract) or is temporary work with a fixed duration (AfDB, 2012). This exposes workers to high levels of job insecurity and forces them to change job frequently (Donovan et al., 2018). Third, the wages of workers in developing countries grow less over time (Lagakos et al., 2018). Weak wage growth may actually be the result of a patchy work experience composed of many short spells in different temporary jobs, which discourage investment in skills and may be (mis)interpreted as a negative signal by recruiters (Donovan et al., 2018)

The labour market in Addis Ababa, the growing capital city of Ethiopia where this study is conducted, exemplifies these broad trends.[4] First, informal and temporary work is very common: only 30% of control individuals in our study have secured a formal job with an open-ended contract by the time of the second endline. As a result, average employment spells are short (in our one year phone panel, we estimate that 72% of jobs are terminated

[4] Addis Ababa's population totalled 3.2 million in 2014; city planners expect this number to double within the next 25 years (CSA, 2014; Davison, 2014). Other estimates suggest that the total population of the city is close to 4.5 million.

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 (when employment ends, the worker is able to find another job immediately in only 18% of cases). Further, the frequent lack of a written agreement makes it difficult to enforce workers' rights and provide social security. Second, employment growth is weak. Average wages in the control group grew at roughly the rate of inflation, during a period when the economy was growing at 8% per year. This lack of wage growth appears to be related to the inability to find stable work. When we regress control group earnings after four years on employment outcomes after one year (in the control group) and a full set of baseline controls, we find that having a permanent job or formal job after one year is correlated with significantly higher earnings. Most strikingly, having any job after one year is not correlated with earnings at all after controlling for having a permanent or formal job. That is, individuals who had temporary or informal jobs after year do not have higher earnings that those that were unemployed.

For these reasons, permanent jobs, and jobs with written contracts, are highly sought after by young Ethiopians. Workers define their jobs as being permanent if their tenure is guaranteed — or without a specified end date — either according to a written or verbal contract.[5] Our data shows that young people search primarily for permanent work: when asked what kind of work they were looking for, 64% said they were looking specifically for a permanent job, whereas only 25% reported they were applying to jobs without consideration for the contract type. Only 11% of respondents said that they were specifically seeking temporary or casual work.[6] 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 it is permanent. When our respondents were asked for the most desirable characteristic of a job, the second most common answer (20.4% of responses) was "work stability", while only 6.7% of respondents chose "working hours".

Finally, our data shows that access to permanent and formal jobs is particularly difficult for workers belonging to the most disadvantaged backgrounds, such as the less educated, women, and young people without any kind of permanent work experience. For instance, a worker with tertiary education is seven times more likely to have a permanent job and four times more likely to have a formal job than a worker who has only completed high school. So what obstacles prevent young workers, and especially those from the most disadvantaged backgrounds, from searching more effectively and achieving better labour

[5] We asked a number of questions to investigate respondents' understanding of the definition of permanent work. 83% of respondents with permanent jobs say that they are sure it will be available until they retire, compared to 32% of workers in other kinds of jobs. 92% of permanent jobs have no fixed end date to their contracts, for 79% of permanent jobs that is agreed in writing.

[6] Similarly, we find little evidence that young people in our sample are seeking to be self-employed. Only 5.4% of people said they were trying to start their own business as a reason for not searching work.

market outcomes?

2.2 Costs vs. Quality of Signals

Job search is costly. One of the most popular search methods used by the participants in our study is to visit job vacancy boards.[7] The boards are located in the centre of the city, forcing participants who live in the periphery to travel frequently to the centre, which is costly: among individuals in the control group, 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). The majority of job-seekers who travel to the job boards come to look for permanent and formal jobs. Temporary work, in lower skilled professions, tends to be more readily available throughout the city, and is more often found through social networks. In addition, those seeking formal jobs face the costs of gathering information through newspapers, printing CVs and cover letters, travelling to interviews, and so on. Among the active searchers in our sample, the median expenditure on job search at baseline amounts to about 16 percent of overall expenditure.[8]

Young job-seekers in Addis Ababa also find it hard to signal their skills to employers. To select a shortlist of candidates among a large number of applicants, firms in the city often use simple criteria such as whether the candidate has previous work experience.[9] Job referrals are also frequent (Serneels, 2007; Caria, 2015). This puts young people at a disadvantage, as they have little work experience and less extensive networks. 55 percent of the participants in our study report having less than one year of work experience and only 16 percent have ever worked in a permanent job. Further, many job-seekers do not seem to be familiar with the process and the standards of job applications. For example, while firms report valuing a well-written CV, 41 percent of the study participants who have applied for at least one job in the last six months have not prepared a CV to support their applications. Anecdotally, firms often mention that recent changes to the education system have made it more challenging to distinguish between candidates with very similar grades. On the other hand, career advice or job search assistance is almost completely lacking from high-school and university curricula. Many formal firms complain about the poor quality of presentation of job applications, and express a demand for such training to

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

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

[9] 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.

be implemented as part of the education system.

In light of the above 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, that build on the existing literature, and, crucially, that provide a direct comparison between very different forms of active labour market policies (ALMPs) — one targeting search costs, and one targeting informational costs.

2.3 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 youths 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.[10]

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.[11]

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 explain the nature of the tests and report the relative grade of the individual for each test, and an aggregate measure of

[10] 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.

[11] In a separate survey of 500 medium to large enterprises in Addis Ababa, we find that about 40 percent of firms know about the personnel selection services offered by the School of Commerce. 80 percent of these firms report that they trust the services offered by the School of Commerce.

performance.[12] The certificates are officially issued by the School of Commerce and the Ethiopian Development Research Institute.[13]

We chose the tests on the basis of the results of several qualitative interviews with firm managers in the city.[14] 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.[15] 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).

We hypothesise that both components of this intervention (the training and the certificate) change employment outcomes by improving the precision of signals that workers can send to firms about their ability. In a theoretical appendix (Section A.4) we present a simple signal-processing model, studying the interaction between a representative worker and a firm. The key insight from this model is that tightening signals over workers' ability increases match quality and therefore wages, as long as the firm is risk neutral or moderately risk averse. Further, we illustrate how, if workers observe their match quality before meeting the firm and can choose the precision of their signals, only a worker with high match quality will want to send a more precise signal to the firm. The distribution of the gains of the intervention will thus depend on the extent to which each worker in our

[12] 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%.

[13] 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.

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

[15] 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.

sample is a good match for at least some jobs. Finally, we consider a case where the firm has access to a second signal of ability (e.g. work experience). The intervention will enable the firm to put less weight on this signal, thus generating earning gains that are strongest for those workers who would otherwise have the worst outcomes in the labour market. We return to this framework in Section 5 to discuss the efficiency implications of our results.

2.4 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, job-seekers need to have (or borrow) enough cash to make the first journey — which in our setting is almost always the case.[16]

Prior to the intervention, respondents in our sample do not travel frequently to the city centre.[17] By paying participants conditional upon their presence at our office, we directly subsidize travel to the centre. This allows us to focus on spatial constraints to job search.[18] We hypothesize that the intervention works to reduce the costs of travelling 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

[16] 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.

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

[18] 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.

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.[19] The intervention was implemented between September 2014 and January 2015.

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.[20] Given our interest in spatial constraints, we excluded all clusters within 2.5 km from the city centre 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

[19] 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.12).

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

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.[21]

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).[22] 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. Throughout the paper, we report wages reported using a one-month recall period.[23] We

[21] 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.

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

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

also collected an incentivised measure of present bias.[24] 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.[25]

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).[26] 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.[27] 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

[24] We follow the method proposed by Giné et al. (2017), which identifies present bias from the revision of a former decision. During the baseline interview, participants have to allocate an endowment of seven tokens between two future payment dates (30 and 60 days after the interview). Each token allocated to the earlier date activates a transfer of 5 Ethiopian Birr on that date, while tokens allocated to the later date activate a transfer of 7 ETB. Further, we assign one extra token (on top of the seven tokens allocated by the respondent) to the early date and one extra token to the later date. This ensure that a payment will be made for sure on both dates. The allocation decision will thus only reflect time preferences, and not a preference for lumpy payments (Afzal et al., 2017). We use mobile phone transfers to make these payments. In the phone call that participants receive just before the first payment date (typically three days before payment was due), participants are given the option to revise their allocation. Individuals who allocate more money to the first payment date are considered present biased. Finally, to measure sophistication, in the baseline questionnaire we ask individuals whether they anticipate that they would revise their allocation decision if they were given the option to do so. Participants who anticipate correctly their revision decision are considered sophisticated.

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

[26] 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.

[27] 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.

results, in an online appendix, Section A.3.

< Table 1 here. >

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.[28] Table A.8 in the online appendix presents the full analysis.[29] 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

[28] 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.

[29] 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.

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. 80% of those attending later collect the certificates from the School of Commerce. Take-up rates do not vary substantially with observable covariates.[30]

3.6 Estimation strategy

We follow a detailed pre-analysis plan, registered at `www.socialscienceregistry.org/trials/911`. The plan describes the empirical strategy, the outcome variables of interest, the definition of these variables, the subgroup analysis, and our approach to multi-hypothesis testing and attrition.

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

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

where y_{ic} is the endline outcome for individual i in cluster c and $\boldsymbol{x}_{ic0}$ is the vector of baseline covariate values that were used for re-randomisation and blocking. $\texttt{treat}_{fic}$ is a dummy capturing whether an individual has been offered treatment f. 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 indirect (spillover) effects of treatment f. We correct standard errors to allow for correlation within geographical clusters and we use sampling weights to obtain average treatment effects for the eligible population as a whole.[31]

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

[30] 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.

[31] 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.

q-values control the false discovery rate within the family of six hypotheses that we test for each program.[32] We also specify two families of intermediate outcomes that help us elucidate what mechanisms drive the primary effects, and seven families of secondary outcomes.

To 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.[33] d_{wit} is a dummy variable equal to 1 in period t if an individual started receiving their treatment w periods ago.[34] Individuals in the control group have all such dummy variables set to 0. Thus, β_{fw} is our estimate of the impact of intervention f, w fortnights after the intervention started.[35]

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}:

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

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

[32] 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.

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

[34] 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.

[35] 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.

4 Treatment Impacts

4.1 Short-run impacts

Table 2 reports the main impacts on our pre-specified family of six primary outcomes.[36] We find no significant average treatment effects on the probability of having a job, on hours worked, on earnings or on job satisfaction. Existing meta-analyses show that, in the short run, active labour market policies on average increase employment rates by about 1.6-2 percentage points and earnings by about 7 percent (Card et al., 2015; McKenzie, 2017). The effect sizes that we document are in line with these figures. Employment rates increase by 3.8 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). Further, the workshop is associated with an (insignificant) 6.2 percent increase in earnings, while the effect of the transport intervention on earnings is very close to zero.

< Table 2 here. >

Table 2 also reveals a striking result on job quality — measured both in terms of whether work is formal (in the sense of having a written contract), and whether work is permanent (in the sense of not having a specified end date). As we foreshadowed earlier, both characteristics are highly sought among job-seekers — 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, raises permanent employment by an insignificant 2.9 percentage points. 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 the use of Lee bounds to correct for attrition. Finally, the effects are larger among the most disadvantaged workers (e.g. less educated job-seekers), with important implications for equity. This will be discussed in Section 5.

In addition to testing the effects of the interventions on the primary employment outcomes, we evaluate their impacts on a range of secondary outcomes, most notably other

[36] These outcomes were pre-specified as our primary family in our pre-analysis plan.

measures of job quality, worker expectations, reservation wages, aspirations and mobility (the full set of results is available in Tables A.10 to A.17 of the empirical appendix).[37] Overall, we find little evidence that our interventions have changed outcomes in these areas. We have some limited evidence that the job-seekers who were invited to the job application workshop are more optimistic about their labour market prospects. They expect to receive 19 percent more job offers in the next four months than individuals in the control group, although this effect is not significant after correcting for multiple hypothesis testing.[38]

4.2 Long-run impacts

The results from our long-term follow-up show stark differences between the two treatments and the large potential gains from improving young job-seekers' ability to signal their skills. 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 twenty 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 wage growth between the two endline surveys. The effect is statistically significant at the 5 percent level, and is robust to the correction for multiple comparison. Quantile regressions show that the effects are large and significant across the distribution of earnings (see Table A.22 in the appendix: earnings take on positive values from 40th percentile and up, we find significant effects from the 60th to 90th percentiles).[39] We also document a 7 percentage point increase in job satisfaction (a 12 percent gain over the control mean). We measure this effect somewhat less precisely: the effect is significant at the 10 percent level and has a q-value of 0.219. Both effects are significantly larger than the impacts of the transport intervention.

We also document that the workshop does not have long-term impacts on employment

[37] 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.10 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.

[38] They also expect five weeks fewer of unemployment before finding the next job, though this effect is not significant.

[39] These results are for 2018 wage earnings. The results are robust to adding self-employment earnings to wage earnings (see Table A.21, and Table A.23 for the quantile regressions for 2018 earnings, which show significant effects from the 45th percentile and up). If we separately consider profits from self-employment, we find no effects — something that is not surprising, given both 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.

rates, nor on the type of employment contract. Four years after the intervention, employment rates are an insignificant 2.7 percentage points higher than for the control group. Using recall data from the second endline survey, we find that the employment impacts of the workshop grew from 2 percentage points 8 months after treatment, to a significant 5 percentage points in the second year after the intervention, and then decreased again to 2.7 points (Figure A.3 in the Appendix). Further, we document that the effects on permanent employment gradually decreased over time (Figure A.4). Four years after the intervention, permanent employment rates among treated individuals are very similar to those of the control group. There are also no long-term impacts on formal employment.[40]

We also find that the gains from the transport subsidy have dissipated 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 recall data suggests that the initial (insignificant) 2.9 percentage points effect on permanent employed was eroded quickly (Figure A.4). There are also no significant long-run impacts on earnings or job satisfaction. In particular, the impact on earning 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 6.3 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.

4.3 How did treated individuals get better jobs?

In this section, we investigate the mechanisms through which the two interventions change labour market outcomes. We designed the treatments to affect different margins of the job search process and we are able to find direct evidence for the intended channels of impact. First, we document that the transport intervention has large and significant effects on job search *intensity*. This helps young people get formal jobs faster. However, increased search effort does not lead to sustained earning gains, likely because young people fail to convince employers that they have the skills required to perform better-paid jobs. Second, we show that the workshop enables young people to search more *effectively*. The job-seekers in this treatment group send the same number of job applications as those in the control group, but are more likely to be offered jobs that are well paid and that have open-ended contracts. Third, we present direct evidence of increased match quality after the workshop by showing that treated workers stay in their jobs for longer periods and make better use of their skills. Finally, we use mediation analysis and data on job tenure to understand the

[40] We do not have recall data for formal employment.

growth of the earnings effect between the two endline surveys.

4.3.1 Job 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%.[41] Finally, treated respondents are more likely to travel to the centre of the city for a number of months while the subsidies are in place (see Figure A.7).[42] 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.18). This is notable and consistent with the hypothesis that financial constraints prevent job-seekers from increasing search effort: if the workshop motivates job-seekers to search harder, many of them lack the resources to do so.[43]

< Figure 1 here. >

< Figure 2 here. >

[41] We also document a contemporaneous, temporary reduction in the probability of working (Figure A.5). 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.

[42] 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.

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

4.3.2 Match quality

Several results indicate that the job application workshop improves job match quality. In Table 3, we offer three pieces of evidence in support of this interpretation. First, treated young people stay in the same job for a longer period of time. Employment duration is often considered a key indicator of match quality as it is evidence that both the firm and the employee value the match. To show this, 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. The effect of the workshop on job spell duration is also significantly larger than the the effect of the transport intervention. Second, we find that treated workers are 8 percentage points more likely to work in jobs where they employ their skills (that is, in their current jobs treated respondents are more likely to make regular use of abilities they have acquired in previous jobs or at school). Third, we show that the workshop significantly raises earnings conditional on employment by 370 ETB, or 15 percent. The bounds for selection of this effect are 113 ETB (5 percent) and 673 ETB (30 percent), showing that selection into employment is unlikely to be driving this result (Attanasio et al., 2011). This large and robust effect confirms that the skills the workshop has enabled young people to signal have a high value in the eyes of employers.[44]

4.4 The value of information about skills

Having demonstrated that the workshop increases search effectiveness and earnings, we now dig deeper into a key aspect of the intervention — namely, disclosing information about worker skills through test certificates — and show that it plays an important role in improving labour market outcomes. We employ a regression discontinuity design which exploits the fact that the certificates issued as part of the job application workshop report test scores in discrete bands and make no mention of the original score.[45] This allows us to study the impact of being placed in a higher band, while controlling for the original test score. If our workshop treatment operated primarily through a certification mechanism, we would expect large discrete improvements in employment prospects at band cut-offs. We

[44] The workshop obtains these increases in employment outcomes without requiring additional search effort. In this sense, *job search effectiveness* increases. Indeed, in Table A.18 we show that the workshop improves the conversion rates of job applications to job offers (in the time period between the baseline the first endline survey). 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.

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

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.[46] We find that being placed in a higher band generates a large, but noisily estimated increase in wages in 2018. When we use the optimal bandwith (Imbens and Kalyanaraman, 2012), we find that being just above the cut-off leads to large increase in earnings of 0.33 standard deviations, which is marginally insignificant ($p = 0.13$). We then explore robustness to the use of bandwidths that are respectively half and twice the optimal values. We find that the effect is consistently between 0.2 and 0.3 standard deviations and is significant at the 10 percent level when we use the larger bandwidth.

4.5 Linking short-run effects to long-run effects: Evidence from a mediation analysis

Finally, we use mediation analysis and data on job tenure to understand the relationship between short and long-run impacts of the job application workshop. In particular, we are interested to investigate why the effect of the workshop on earnings is modest and insignificant at the time of the first endline, but grows into a large and significant effect three years later.

Following Acharya et al. (2016), we compute the Average Controlled Direct Effect (ACDE) of the workshop on long-run earnings, fixing selected short-run outcomes. The ACDE captures the impact of an intervention when a particular mediator is not allowed to respond to the treatment. We can thus assess the importance of a given mediator by comparing the original treatment effect to the ACDE. We show this comparison in Figure 3. We find that a large share of the long-run earning impacts (56%) can be explained by the short-run earning effect of the intervention. Further, the short-run impacts on permanent work can explain about 23% of the long-run effect on earnings. If we fix both short-term earnings and permanent work, we can account for 62% of the original treatment effect. Regressions of 2018 earnings on 2015 employment outcomes in the control group underscore the importance of finding secure work: controlling for individual characteristics and a range of 2015 employment outcomes, we find that having a permanent job in 2015 is correlated with significantly higher earnings three years later. By contrast, having any employment at all is not correlated with 2018 earnings once we control for permanent work. This suggests that staying in temporary worker does not offer wage growth in this context, which is consistent with the results of Donovan et al. (2018).

[46] 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 ways. We subtract the bin-specific cut-off score and divide by the bin-specific standard deviation.

< **Figure 3 here.** >

This analysis shows that the young workers who look more attractive to employers in the short-run drive the long-run earning effect. These workers are likely to be those who are able to signal new skills thanks to the workshop – a further piece of evidence consistent with our interpretation. A second important observation is that treated workers increase their initial earning advantage by changing job. Only about 13 percent of workers hold the same job that they had at the first endline, three years before. Further, treated workers have not been employed in their current job for longer than control workers (Table 3). These findings underscore the importance of job mobility for wage growth – a point that the literature has documented for both developed and developing economies (Topel and Ward, 1992; Menzel and Woodruff, 2017). They also suggests that job security can have positive dynamic effects: the workshop's early impacts on permanent contracts may shield treated workers from the need to accept poorly paid jobs to avoid unemployment.

< **Table 4 here.** >

5 Discussion

In this section we discuss three important questions that emerge from our results. First, we compare the treatment effects on earnings of the job application workshop to those found by the experimental evaluations of ALMPs in developing countries reviewed by McKenzie (2017). We find that our results are among the largest impacts in this literature. When accounting for the fact that the workshop is much cheaper than most of the other active labour market policies evaluated in recent years, this intervention stands out as being substantially more cost-effective than others that have been reported in the literature. Second, we discuss the implications of our results for match quality and welfare. Finally, we show that, regardless of whether the job application workshop improves labour market efficiency or not, this policy has a strong equity rationale — with its benefits generally being concentrated among job-seekers who, on average, would be less successful in the labour market.

5.1 How does the workshop compare to other active labour market policies?

We show that the job application workshop is a highly cost-effective policy option. To make this point, we use the data reported by McKenzie (2017) on the costs and the earning impacts of active labour market policies in developing countries. In Figure 4, we plot the distribution of earning impacts (in percentage terms) and of the ratio of impacts to costs (in USD).[47] Two key messages emerge. First, the earning impacts of the job application workshop are close to the top of the distribution of documented impacts. Second, this intervention is unusually cheap (high-impact interventions tend to be training programs that cost several hundred dollars per participant). As a result, the ratio of the monthly earning gains to the marginal one-off cost is unusually high for this intervention. Further, a similar picture emerges if we compare the job application workshop to recent evaluations of cash transfer programs, which entail large costs to generate large gains (e.g. Blattman et al. (2014) document that a grant worth 382 USD increases earnings by 38 percent).

< Figure 4 >

5.2 Match quality and efficiency

What, then, are the welfare implications of our results? To answer this question, we need a framework for thinking about how our workshop is likely to affect the quality of matches between firms and job-seekers. In the online appendix (Section A.4), we present a stylised signal-processing model for understanding how more accurate information about job-seekers' ability increases match quality, and therefore, wages, in a single interaction between a job-seeker and a risk-averse firm.

In this model, the firm receives a noisy signal of the job-seeker's firm-specific productivity, and uses this signal to construct the posterior distribution of productivity. The model has several implications. First, it implies that a treatment reducing the noise of an applicant's signal will enable the firm to make a better assessment of the applicant's suitability for the job compared to other candidates. This will increase the expected value of a match for the firm, and increase the expected wage. Second, we then extend the model, to allow the job-seeker to choose a signal technology with higher variance or lower variance (for example, a job-seeker may choose to use the workshop certificate, or to highlight a key skill in an interview, to lower the variance on signal). We find that job-seekers with a higher productivity (specifically, a productivity that the firm would find attractive to hire) would prefer the smallest noise possible; job-seekers below this threshold trade off the desire for some noise (to allow the possibility that they will receive a positive idiosyncratic signal) against the danger that the increased variance will discourage the risk-averse firm from hiring. Finally, we introduce an observable covariate, which correlates with the job-seeker's productivity (we have in mind, for example, gender, or previous work history). Under this

[47] 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 (however, most studies included have a shorter time frame than ours) and it does not take into account any variation in context.

extension, we find that the reduction in signal noise should be particularly valued by job-seekers who (i) are a strong match for the firm (in the sense of having a high firm-specific productivity), and/or (ii) job-seekers who have a less attractive observable (and, therefore, must overcome a greater stereotype disadvantage from the firm).

We can add nuance these predictions in several ways. First, note that, for simplicity, our framework assumes complete displacement in hiring. That is, if our representative firm does not hire our representative worker, that firm hires the next best candidate, who is assumed to be known to have average ability. Hiring in our model leads to efficiency gains if and only if the firm hires a worker above average ability, but it does not increase employment directly; in this framework, efficiency is improved through increased match quality, which may have indirect effects on the overall number of jobs only if it leads to firms opening more vacancies.[48]

Further, note that our baseline model looks at a case where a single worker has a single draw of firm-specific match quality. If a worker meets several firms, that worker would receive multiple draws of match quality, one for each firm. If those draws are perfectly correlated, such that each worker has the same match quality for each firm, then workers with lower abilities will always choose the higher-variance signal technology. Therefore, these workers would be harmed in an equilibrium where the workshop was scaled up, and firms expect to see the lower variance signal. However, if workers have some idiosyncratic firm match quality, they might like to use the lower variance signal technology for those job applications for which they have better match quality. This implies that were the intervention to be scaled up, it would be less likely that any workers would be harmed by the intervention: even those with low average ability would more likely to get the jobs for which they are better suited.

Although we cannot conclusively claim that few workers would be harmed by the policy in equilibrium, our empirical results offer suggest evidence in favour of the idiosyncratic match quality story. In particular, the distribution of skills in the population and the broad-based nature of the earning gains constitute evidence for this story. The certificates produced at the workshop are designed to allow workers to signal their ability along multiple dimensions. Indeed, 44% of workers score in the top band for at least one skill and only 10% of workers are in the lower half of the distribution for all skills. Further, we find that earnings in the treatment group strictly stochastically dominate earnings in the control

[48] Of course, this assumption could be relaxed — for example, by assuming instead that the firm does not hire any worker at all unless the firm finds a candidate with ability above a certain (strictly positive) threshold. This would generate the result, in our framework, that improving the precision of signals increases the total number of jobs in the economy, by reducing the probability that each vacancy goes unfilled. However, we do not have empirical evidence that this happens in the matches we observe, so we maintain the simplifying assumption of full displacement.

group (Figure A.8 in the Appendix).[49] As noted above, in partial equilibrium, low-quality types will not use the more precise signal offered by the intervention and thus, in the experiment, no worker should harmed by the intervention. However, observing gains all along the earning distribution suggests that the share of low-quality workers who have nothing to gain from improved signals is likely to be relatively small.

We consider two other possible mechanisms through which the workshop could lower match quality. First, the intervention may lead to congestion in the labour market when offered at scale (Gautier et al., 2018). In a richer framework, more precise signals may motivate high-quality workers to increase search intensity, generating an increase in application rates that will ultimately make it harder for firms to screen talented workers. We are confident that this is not at play in our setting. Crucially, both the workshop and the transport subsidies lead to improved employment outcomes without increasing the number of applications made by job-seekers. This suggests that scaled up versions of our interventions would not have congestion effects.

Second, we consider the possibility that the workshop allows job seekers to 'fool the market'.[50] Our framework assumes that the job application workshop improves the precision of signals. However, it is conceivable that some workers may actually learn to oversell their quality and skills. This would reduce the accuracy of their signals by biasing them upwards. In this case, the workshop would make it harder for employers to screen candidates, leading to lower match quality and to a loss in overall welfare. The impacts of the workshop on match quality and job mobility are inconsistent with this mechanism. If the treated workers misrepresented their skills at the time of hiring, they would presumably be fired by the firm during the probation period. Instead, we find that treated workers work in the same job for longer periods of time — a key indicator of match quality — and that, when they find new employment, they are again offered better conditions. In our view, it is highly unlikely that an inflated presentation of one's skills could survive these multiple screening rounds.

Overall, the available evidence is consistent with a model where — as in our stylised framework in Section A.4 — match quality is significantly improved, where workers with the lowest observable skills benefit the most, and where relatively few workers would lose out from having more accurate signals.

[49] Quantile regressions also show large and significant effects across the positive range of the earnings distribution (Table A.23). Earnings are zero for more than 35% of the distribution in both treatment and control groups.

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

5.3 Who benefits the most from the workshop?

As noted, our model framework predicts that the workshop treatment should have a stronger effect among those job-seekers whose observable characteristics generally correlate with lower labour market success — and who, therefore, must overcome a greater stereotype disadvantage from prospective employers. With this prediction in hand, we turn finally to test for heterogeneity in effects on 2018 wage earnings by baseline job-seeker characteristics.

Specifically, we conduct a sub-group analysis using a list of covariates specified in our pre-analysis plan. We report results in Table 5, where we show differential treatment effects side-by-side; in each case, the covariate is coded such that 'Covariate = 0' refers to the group who, in general, one might expect to face greater labour market disadvantage. Across a wide range of covariates, we find that the effect size is substantial for the more disadvantaged category.[51] For example, job-seekers without tertiary education have an effect of about 40 percent of the control mean.

As a way of synthesising across our multiple different pre-specified dimensions, we then run an 'endogenous stratification' exercise, following Abadie et al. (2017); this is reported in the final row of Table 5. This analysis was not included in the pre-analysis plan and should thus be seen as an exercise in aggregation — in the sense that it is prompted by results from our pre-specified hypotheses, and seeks to generalise the insights generated from those regressions. To run this estimation, we stratify by predicted earnings at endline. In a first stage, we use 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., 2017). 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: $p = 0.0696$). The estimated effect size for the low-predicted-earnings group is about 50% of the control mean. This causes a large reduction in earning inequality: the earning gap between the low and the high earnings group drops from 142 percent to 54 percent and, strikingly, the gap between experienced and inexperienced workers is fully erased. Overall, these results illustrate the large equity gains that can be generated by helping young workers to access the labour market through improved signalling.

[51] In Table 5, we report a selection of the covariates we specified. We report the full set of covariates in Table A.20 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.

Short-run results on job quality are consistent with this pattern of results. In Tables A.24 and A.25 we show sub-group analyses for impacts on job quality at the time of the first endline; again, we find that effects are generally larger for more disadvantaged groups. Table A.26 shows the results when we split our sample by predicted employment. We find starkly different patterns between (predicted) high-employment and low-employment groups. We find large and significant effects on total employment for the low-employment group (an increase in the employment rate of 10 percentage points from each treatment, on a control-group mean of 38%); this is significantly different to the high-employment group — where, as in the earlier estimations, we find no treatment effect. We find significant effects on both formal work and permanent work for the low-employment group; in the case of the workshop treatment, we formally reject a null of equal effects for formal work ($p = 0.048$) and are close to rejecting the same null for permanent work ($p = 0.145$). We also find a significant increase in total earnings from the workshop treatment on the low-employment group: a harbinger of the earnings effects that would become more pronounced at the four-year mark. Note that the short-run effects of the transport treatment were also concentrated among the low-employment group — however, as in the sample as a whole, these effects have dissipated by the time of our long-term follow-up.

< Table 5 >

6 Conclusion

What policy interventions can help young people to find good jobs? We show that improving the quality of information about workers' skills can play a crucial role. In particular, we demonstrate that while reducing the cost of job search (through a transport subsidy) has only transitory effects on labour market outcomes, improving workers' ability to signal their competences to employers (through a job application workshop and skill certificates) has long-term effects on earnings, which far outweigh the costs of the intervention. In addition, by improving match quality, the workshop has positive effects on overall efficiency. In other words, this is the first paper to show that young people in a developing country have valuable unobserved skills that, once certified, generate welfare improvements. Further, since the impacts of the intervention are strongest among the more disadvantaged socio-demographic groups, the treatment reduces inequality.

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., 2016; 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.

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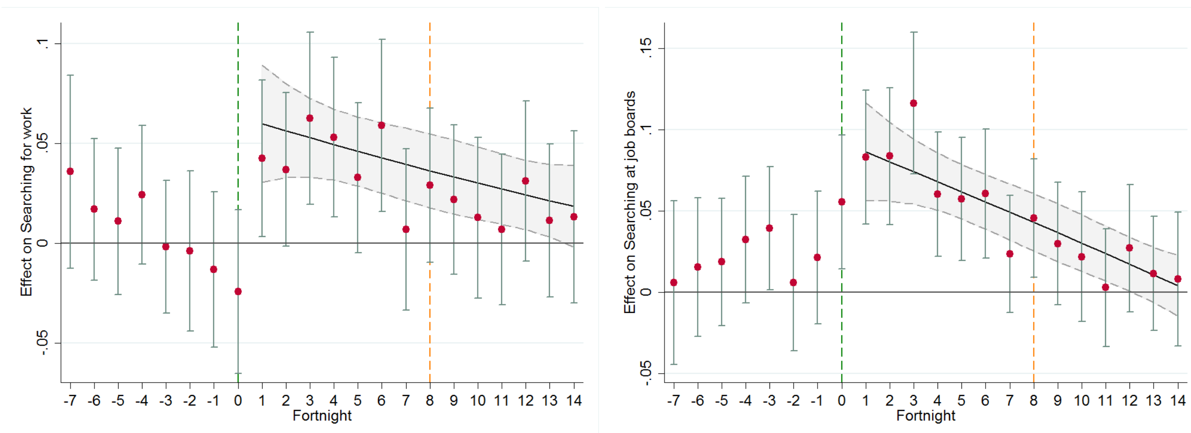
Figures and Tables

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

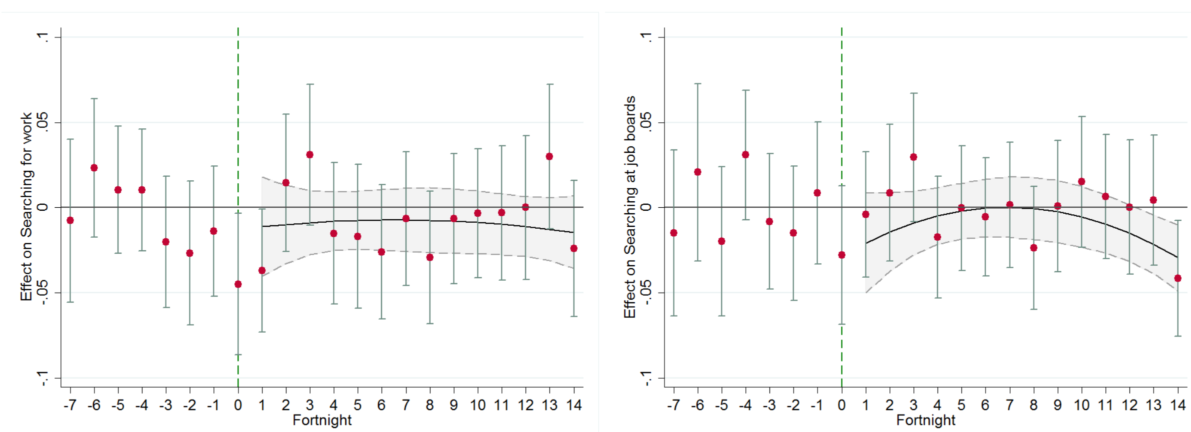
(a) Impact on search (any active step) (b) Impact on searching at the job boards



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

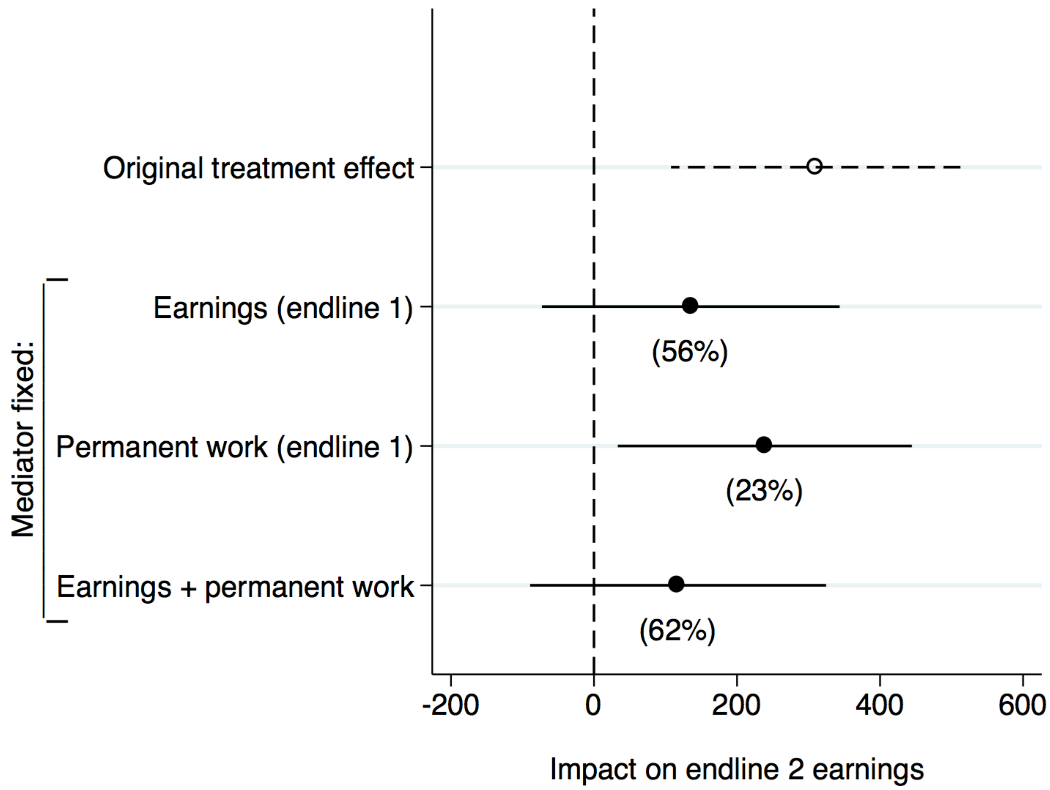
Figure 2: **Fortnightly impacts of the job application workshop on job search**

(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.

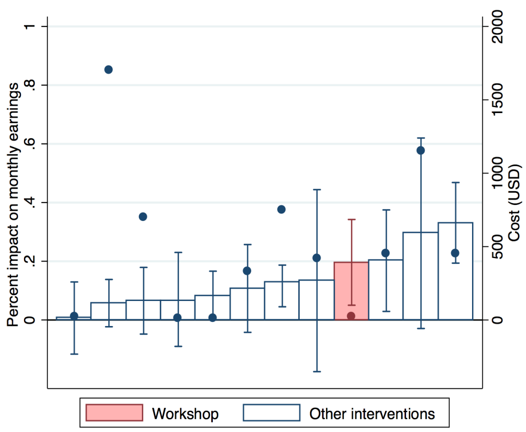
Figure 3: **Mediation analysis: Job Application Workshop**



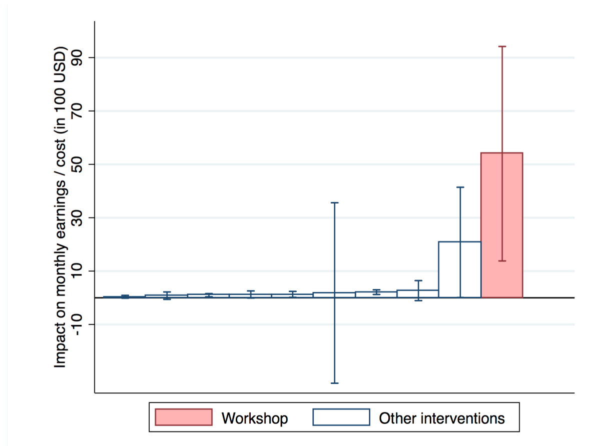
Note. This figures reports coefficient estimates and 90% confidence intervals of the impact of the job application workshop on endline 2 earnings. The first row reports the original treatment effect. The following rows report the Average Controlled Direct Effect (ACDE) of the intervention, obtained by 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 comparison, we report below each coefficient the share of the original treatment effect that is accounted for by the mediator.

Figure 4: **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 that report earning effects which are included in the review by McKenzie (2017). For some studies, we obtain additional information from the papers (e.g. for Maitra and Mani (2017)).

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**

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)
Work	0.562	0.037 (0.029) [0.366]	0.021 (0.031) [1.000]	0.57	0.693	-0.058* (0.035) [0.411]	0.029 (0.032) [0.958]	0.00
Hours worked	26.176	0.183 (1.543) [0.837]	-0.214 (1.533) [1.000]	0.79	28.250	-2.499* (1.486) [0.411]	0.218 (1.426) [1.000]	0.04
Monthly wages	857.882	65.879 (63.864) [0.437]	3.363 (65.667) [1.000]	0.30	1,531.488	30.916 (102.352) [0.753]	299.469** (121.383) [0.096]	0.02
Permanent job	0.171	0.033* (0.018) [0.215]	0.069*** (0.019) [0.004]	0.09	0.307	-0.034 (0.025) [0.411]	-0.010 (0.028) [1.000]	0.30
Formal job	0.224	0.054*** (0.019) [0.032]	0.053*** (0.020) [0.021]	0.95	0.318	-0.005 (0.030) [0.753]	-0.007 (0.030) [1.000]	0.96
Job satisfaction	0.237	-0.001 (0.027) [0.837]	0.022 (0.027) [1.000]	0.45	0.575	-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. 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 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 3: Impacts on Job Tenure and Conditional Earnings

Outcome	Control mean (1)	N (2)	ITT Estimates Transport Coeff (3)	ITT Estimates Workshop Coeff (4)	Equality pval (5)
Longest tenure (months)	11.845	1,739	0.294	1.197*	0.103
			(0.561)	(0.619)	
Current job tenure (months)	21.326	1,383	0.199	-0.539	0.536
			(1.165)	(0.977)	
Promoted in current job	0.190	1,383	0.022	0.006	0.525
			(0.025)	(0.023)	
Uses skills in current job	0.323	2,016	0.032	0.082**	0.211
			(0.040)	(0.040)	
Earnings conditional on working	2,209.3	1,383	195.0	370.4**	0.283
			(143.1)	(157.6)	

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. 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. ***: $p < 0.01$, **: $p<0.05$, *: $p<0.1$.

Table 4: Regression Discontinuity Estimates

	Impact on standardised earnings (endline 2)		
	(1)	(2)	(3)
Above cut-off	0.332 (0.219)	0.227 (0.281)	0.322 (0.169)*
Bandwidth	Optimal	0.5*Optimal	2*Optimal
Obs.	246	204	304

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 5: **Heterogeneous effects on 2018 wage earnings by baseline characteristics**

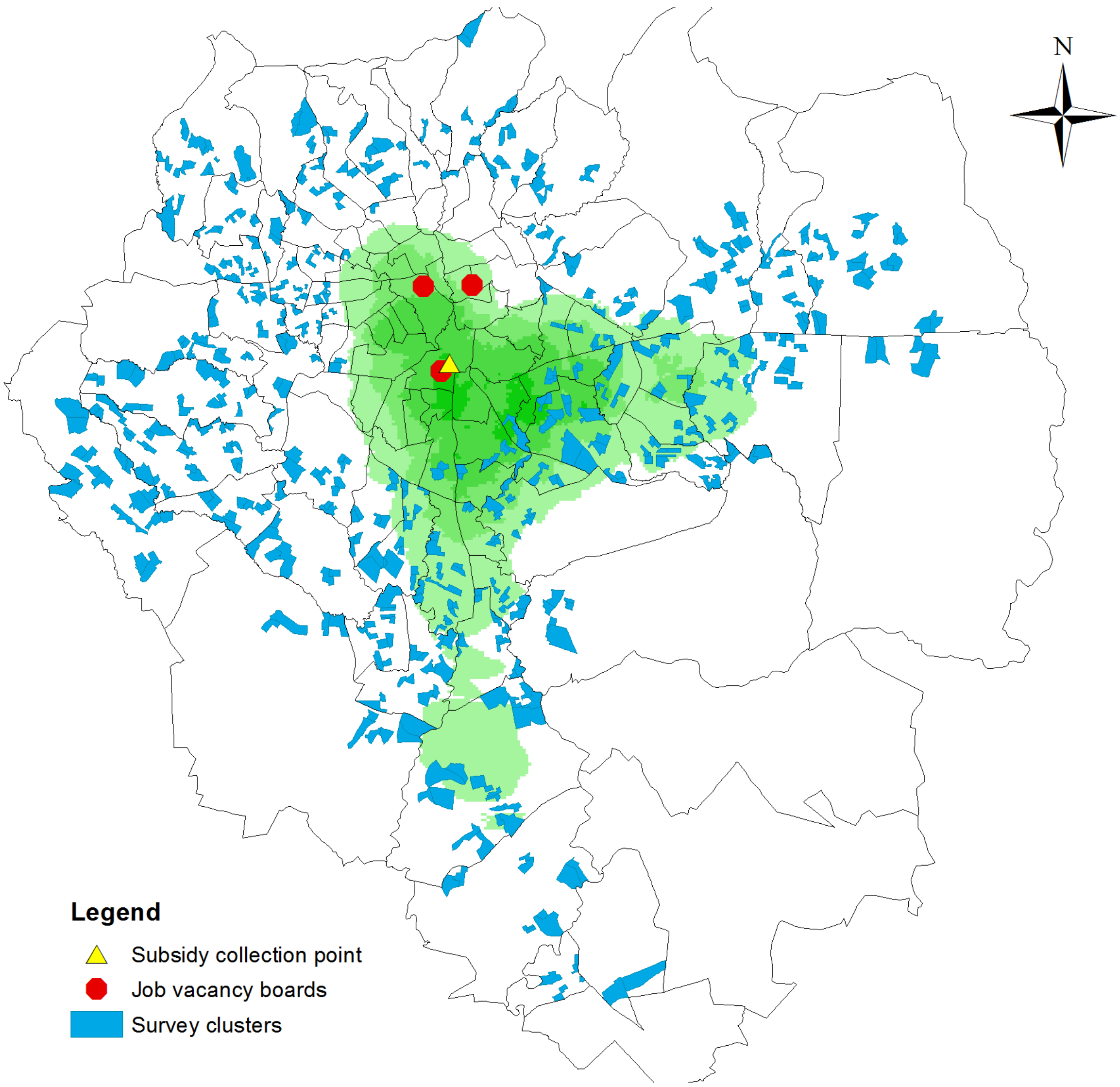
	Covariate = 0			Covariate = 1			Transport	Workshop
Baseline covariate	Control mean (1)	Transport (2)	Workshop (3)	Control mean (4)	Transport (5)	Workshop (6)	Equality (pval) (7)	Equality (pval) (8)
Tertiary Education	826.4	15.1	470.9**	1,835.1	54.2	37.3	0.83	0.07
		(124.4)	(188.1)		(159.9)	(149.8)		
		[1.000]	[0.034]		[1.000]	[0.993]		
Male	1,181.9	-40.0	132.1	1,892.4	104.7	475.5*	0.47	0.21
		(110.0)	(116.4)		(179.3)	(245.1)		
		[1.000]	[0.087]		[1.000]	[0.363]		
Active searcher	1,442.2	3.1	351.9*	1,625.8	62.5	235.5	0.77	0.67
		(132.7)	(188.9)		(160.0)	(183.1)		
		[1.000]	[0.050]		[1.000]	[0.663]		
Ever had permanent job	1,465.8	40.2	356.5***	1,975.7	-42.3	-288.7	0.82	0.09
		(104.7)	(136.7)		(367.8)	(350.3)		
		[1.000]	[0.034]		[1.000]	[0.696]		
Lives close to the centre	1,468.8	41.8	406.2**	1,606.3	52.2	141.9	0.96	0.29
		(151.0)	(196.9)		(143.0)	(150.3)		
		[1.000]	[0.042]		[1.000]	[0.696]		
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 in 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 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 the row (1) column (3) shows the treatment effect of the workshop for this group (470.9). 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

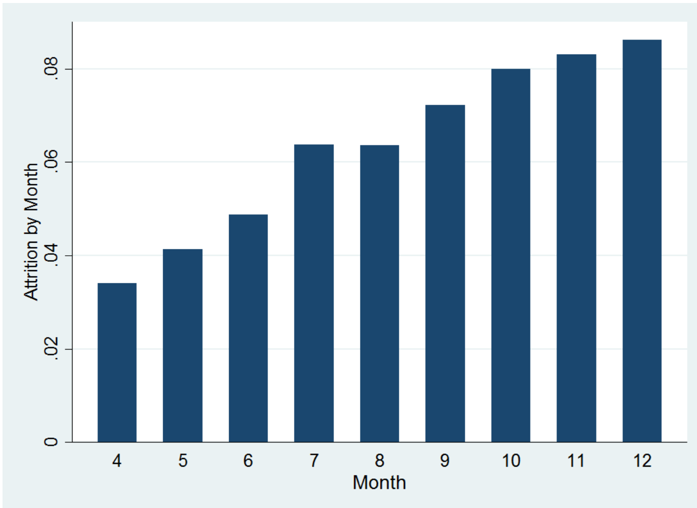
A.1 Additional Figures and Tables

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: **Impact trajectories: Employment**

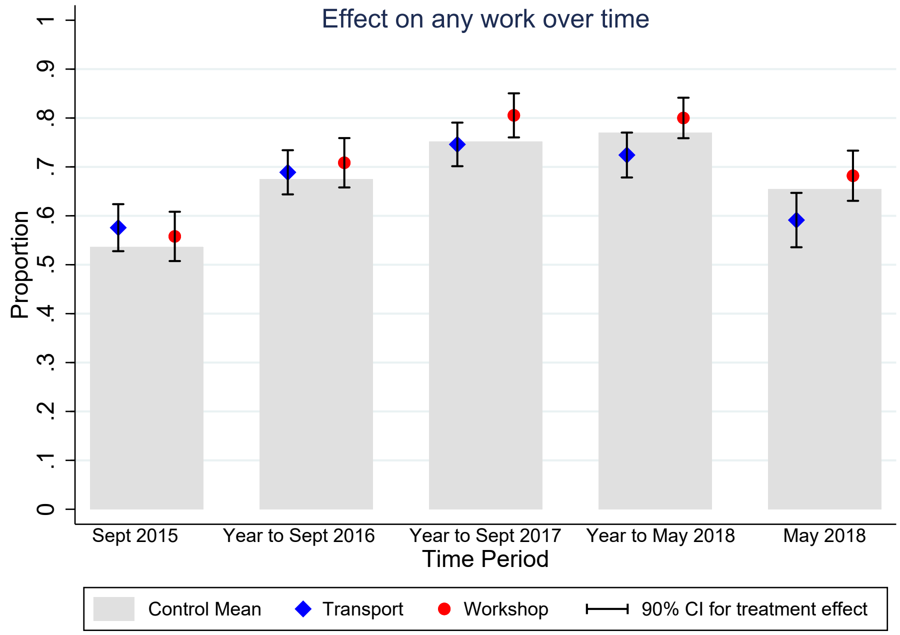


Figure A.4: **Impact trajectories: Permanent employment**

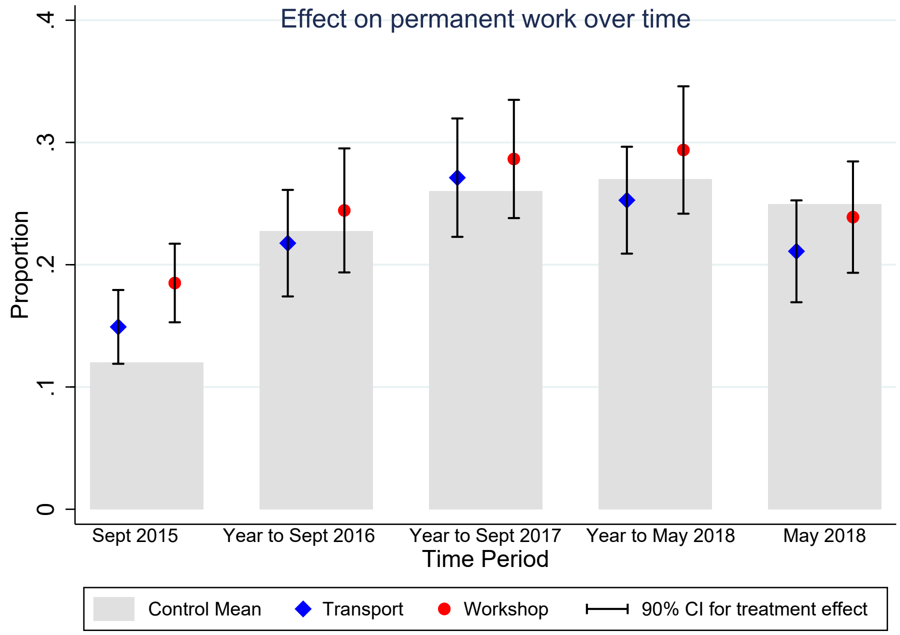


Figure A.5: **Impact trajectories: Employment in year 1**

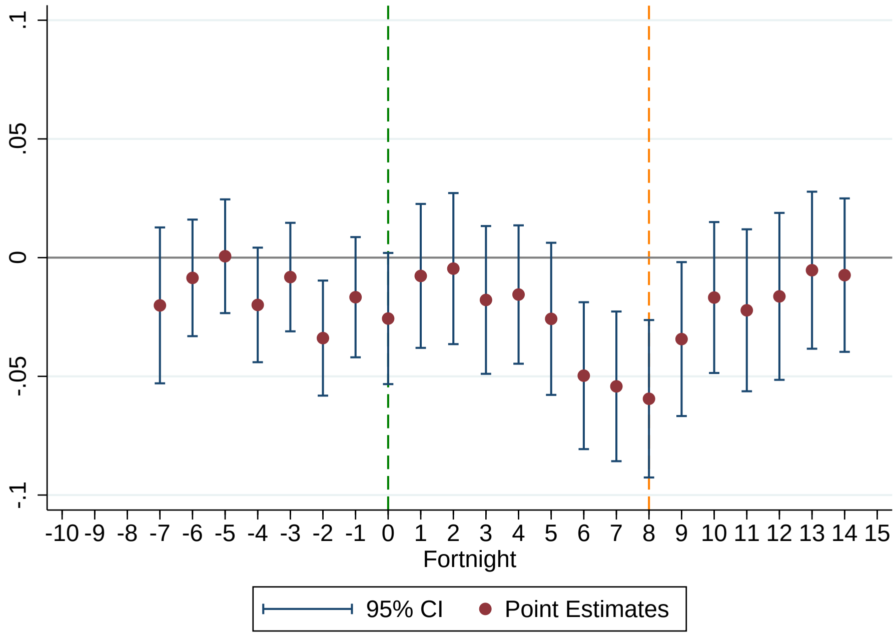
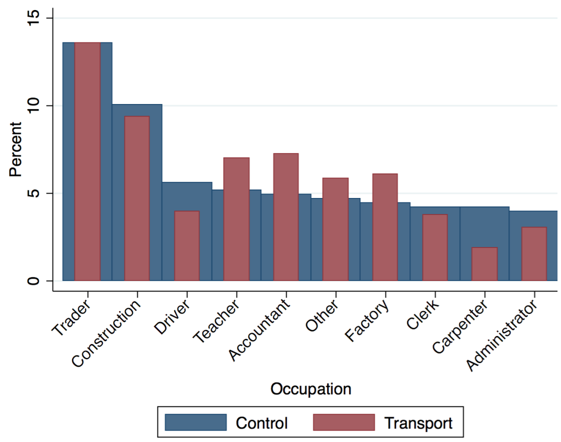


Figure A.6: **Most common occupations**

(a) **Transport Subsidy**



(b) **Job Application Workshop**

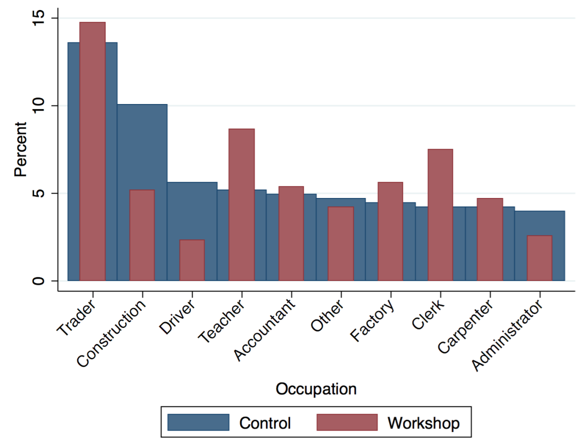
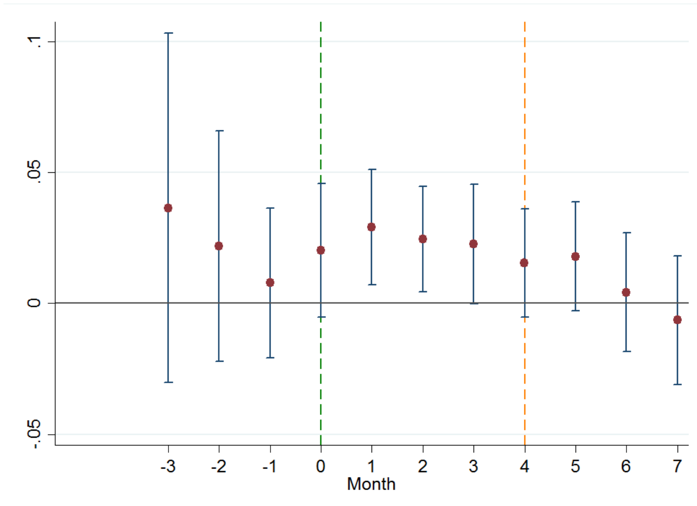


Figure A.7: **Impact trajectory of the transport treatment: Travelled to city centre**



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

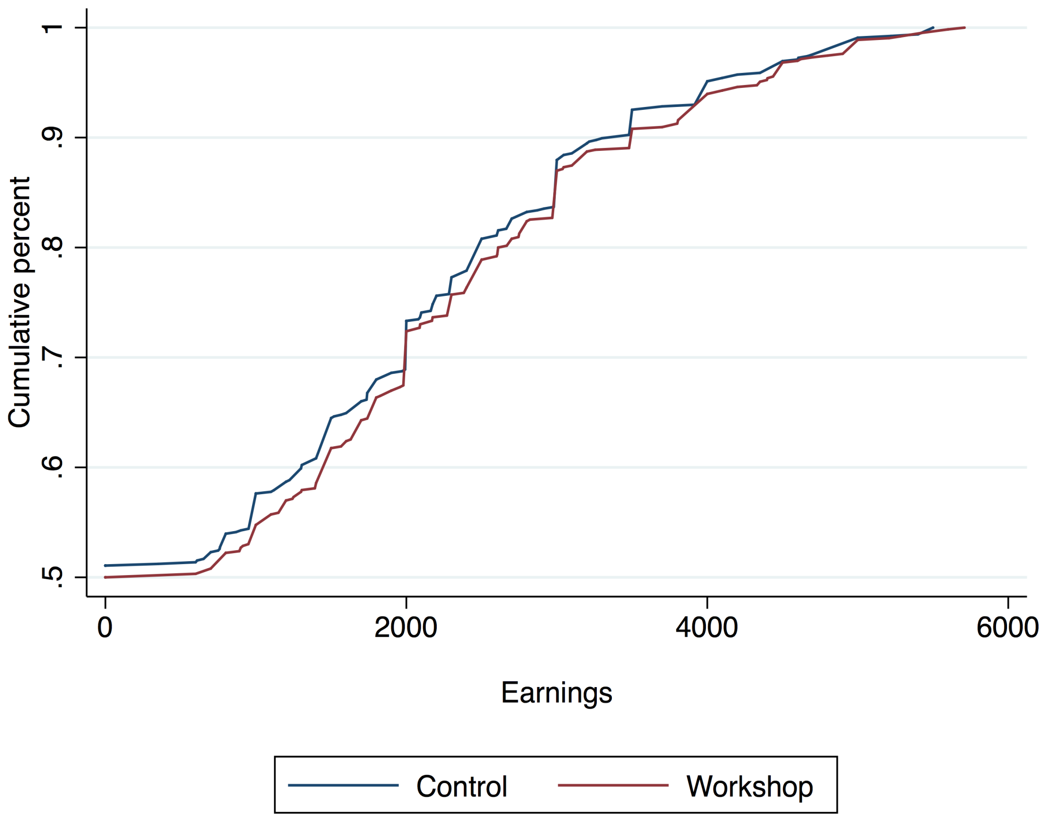


Table A.1: **Summary statistics of the tests administered in the job application workshop**

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 test of balance. Variable defitions are provided in Table A.7 below. We show the mean and standard deviation in the control mean (columns 1 and 2), then the difference in the mean of the outcome for the Transport and Workshop treatment groups in columns 3 and 4, respectively. Column 5 shows the p-value on the F-test that the difference between all three groups (control, workshop and transport) is jointly significant.

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: **Family indices**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Job Quality	0.534	0.493	-0.177	0.709	-0.859	0.947	2841
	(.57)	(.629)	(.743)	(1.097)			
	[1]	[1]	[1]	[1]			
Finan. Outcomes	0.190	0.142	0.0980	-0.0280	-0.559	0.831	2841
	(.238)	(.212)	(.259)	(.299)			
	[1]	[1]	[1]	[1]			
Expects and Asps	-0.166	-0.00300	-1.006	-0.491	-0.0390	0.795	2134
	(.698)	(.585)	(.597)*	(.827)			
	[1]	[1]	[1]	[1]			
Mobility	0.456	0.324	-0.479	-0.299	-0.740	0.787	2836
	(.471)	(.535)	(.636)	(.638)			
	[1]	[1]	[1]	[1]			
Education/Skills	-0.763	-1.160	0.0410	-1.040	0.578	0.565	2841
	(.67)	(.763)	(.785)	(1.01)			
	[1]	[1]	[1]	[1]			
Wellbeing	0.0540	0.186	0.0360	0.0910	-0.153	0.444	2837
	(.166)	(.156)	(.18)	(.225)			
	[1]	[1]	[1]	[1]			
Networks	-0.301	-0.357	-0.487	-0.229	0.0890	0.873	2823
	(.34)	(.359)	(.375)	(.438)			
	[1]	[1]	[1]	[1]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on the summary indices for different families of outcomes. These are obtained by 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.11: **Other job quality measures**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Received job by interview	0.0400	0.0430	0.0240	0.0670	0.115	0.879	2841
	(.016)***	(.018)**	(.021)	(.032)**			
	[.053]*	[.11]	[1]	[.219]			
Office work (7d)	0.0270	0.00300	-0.0190	0.00700	0.181	0.307	2841
	(.024)	(.023)	(.026)	(.037)			
	[.6]	[1]	[1]	[1]			
Skills match with tasks	0.00800	0.00500	0.0300	0	0.120	0.915	2841
	(.029)	(.029)	(.035)	(.038)			
	[.882]	[1]	[1]	[1]			
Overqualified	0.0380	0.0310	-0.0380	0.0580	0.280	0.841	2841
	(.035)	(.034)	(.037)	(.051)			
	[.6]	[1]	[1]	[.984]			
Underqualified	-0.0170	-0.0130	-0.0130	-0.0210	0.0790	0.791	2841
	(.019)	(.019)	(.022)	(.025)			
	[.607]	[1]	[1]	[1]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on 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.12: **Financial outcomes**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Expenditure (7d)	28.54	18.18	-7.868	-59.19	474.4	0.797	2841
	(39.377)	(38.661)	(39.758)	(41.197)			
	[1]	[1]	[1]	[.826]			
Savings (total)	352.4	-969.6	-486.9	63.68	5803	0.603	1259
	(2726.672)	(1350.114)	(1432.001)	(1619.663)			
	[1]	[1]	[1]	[1]			
	0.467	0.195	0.432	0.483	-1.055	0.605	2841
	(.549)	(.488)	(.633)	(.784)			
	[1]	[1]	[1]	[1]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on financial outcomes. These are obtained by 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.13: **Expectations, aspirations, reservation wages**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Offers expected (next 4m)	-0.00600	0.270	-0.151	-0.205	1.383	0.0757	2641
	(.143)	(.154)*	(.149)	(.141)			
	[1]	[.367]	[.872]	[.265]			
Reservation wage	8.790	-86.57	-8.547	151.8	1799	0.286	2480
	(82.503)	(73.081)	(90.346)	(110.807)			
	[1]	[.367]	[1]	[.265]			
Aspiration wage (in 5y)	689.8	706.5	447.8	1031	6237	0.985	2607
	(700.322)	(817.629)	(683.274)	(786.078)			
	[1]	[.367]	[1]	[.265]			
Weeks expected to be without permanent job	1.468	-5.010	-9.276	-5.820	32.20	0.0923	1347
	(4.323)	(3.345)	(3.126)***	(4.633)			
	[1]	[.367]	[.013]**	[.265]			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on expectations, aspirations and reservation wages. These are obtained by 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.14: **Mobility**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Trip to center (7d)	0.129 (.172) [1]	-0.0330 (.183) [1]	-0.133 (.176) [1]	-0.272 (.231) [1]	2.171	0.379	2500
Works away from home	0.00300 (.034) [1]	-0.0190 (.035) [1]	-0.0860 (.043)** [.299]	-0.0130 (.047) [1]	0.378	0.501	2841
Location of main occupation/activity changed	0.0290 (.04) [1]	-0.0320 (.039) [1]	0.0230 (.046) [1]	-0.0310 (.045) [1]	0.250	0.0957	2841
Moved within Addis	-0.00200 (.019) [1]	0.0240 (.02) [.925]	0.00600 (.023) [1]	0.00900 (.027) [1]	0.0770	0.186	2841
Moved outside of Addis	0.0100 (.007) [1]	0.0120 (.007)* [.702]	0.00300 (.006) [1]	0.00200 (.006) [1]	0.00500	0.789	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 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.15: **Education and training**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
In full-time education	-0.00700	0.00100	0.00300	0.0330	0.0210	0.387	2841
	(.008)	(.01)	(.011)	(.022)			
	[.777]	[1]	[1]	[.203]			
In part-time education	-0.0480	-0.0330	-0.0140	-0.0200	0.138	0.453	2841
	(.02)**	(.023)	(.026)	(.031)			
	[.11]	[.52]	[1]	[.466]			
In informal training	-0.00900	-0.0100	-0.00700	-0.0430	0.0470	0.951	2841
	(.016)	(.015)	(.016)	(.013)***			
	[.777]	[.696]	[1]	[.008]***			
Graduated (in past 12m)	0.0120	-0.0130	0.0150	-0.0180	0.0770	0.121	2841
	(.017)	(.016)	(.02)	(.023)			
	[.777]	[.696]	[1]	[.453]			
Graduated from vocational degree (in past 12m)	0.0160	0.00700	0.00500	0.00300	0.0240	0.380	2841
	(.011)	(.01)	(.012)	(.016)			
	[.45]	[.696]	[1]	[.729]			
Graduated from training (in past 12m)	0	-0.0230	0.0190	-0.0280	0.0440	0.0730	2841
	(.014)	(.012)*	(.016)	(.012)**			
	[1]	[.475]	[1]	[.061]*			

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on education and training. These are obtained by 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.16: **Psychological outcomes**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Life satisfaction (0-10)	0.164 (.132) [1]	0.147 (.134) [1]	0.202 (.151) [1]	0.320 (.224) [1]	4.676	0.901	2503
Locus of control (0-10)	0.0150 (.299) [1]	-0.0400 (.285) [1]	-0.160 (.337) [1]	-0.0280 (.331) [1]	6.114	0.853	2505
Oneness with society	-0.0260 (.14) [1]	0.0530 (.14) [1]	-0.0200 (.144) [1]	0.123 (.186) [1]	4.694	0.554	2505
Trust in other people (1-4)	0.0790 (.081) [1]	0.0400 (.092) [1]	0.0250 (.086) [1]	-0.0360 (.106) [1]	2.048	0.655	2504

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on psychological outcomes. These are obtained by 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: **Social networks**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
No. people with whom regularly shares info about job opport.	-0.347 (.372) [1]	-0.601 (.348)* [.504]	-0.612 (.377) [.724]	-0.425 (.468) [1]	5.182	0.464	2807
Number of people with permanent jobs in job network	0.118 (.212) [1]	0.121 (.233) [.778]	-0.0680 (.246) [1]	0.394 (.306) [1]	2.178	0.987	2528
Can access guarantor for job (in next month)	-0.00500 (.054) [1]	-0.0660 (.054) [.504]	-0.0240 (.06) [1]	-0.00400 (.068) [1]	1.244	0.235	2504
No. meetings of voluntary associations attended (past 30d)	0.0100 (.061) [1]	0.00900 (.063) [.802]	-0.0330 (.069) [1]	-0.0540 (.062) [1]	0.119	0.984	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 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.18: **Job search**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Applied to temporary jobs	0.337 (.267) [.905]	-0.0210 (.205) [.985]	0.0190 (.255) [1]	-0.163 (.241) [1]	1.129	0.140	2832
Applied to permanent jobs	-0.0400 (.251) [.905]	0.0210 (.24) [.985]	0.0550 (.289) [1]	0.00600 (.297) [1]	1.616	0.752	2827
Interviews/Applications	-0.0360 (.03) [.905]	-0.0370 (.027) [.703]	0.0320 (.048) [1]	-0.0140 (.052) [1]	0.349	0.948	1584
Offers/Applications	0.00300 (.039) [.905]	0 (.039) [.985]	-0.0170 (.042) [1]	0.0730 (.067) [1]	0.256	0.940	1586
Interviews/Applications (Perm)	0.00300 (.038) [.905]	0.00900 (.035) [.985]	0.00100 (.044) [1]	-0.0250 (.056) [1]	0.316	0.854	1240
Offers/Applications (Perm)	0.0500 (.036) [.905]	0.0530 (.031)* [.703]	0.0110 (.034) [1]	0.0580 (.049) [1]	0.138	0.924	1238
Interviews/Applications (Temp)	-0.0770 (.042)* [.905]	-0.0650 (.042) [.703]	0.0290 (.078) [1]	-0.0270 (.072) [1]	0.384	0.759	986
Offers/Applications (Temp)	-0.0560 (.044) [.905]	-0.0490 (.046) [.703]	-0.0280 (.057) [1]	0.104 (.094) [1]	0.346	0.875	986
Uses CV for applications	0.0120 (.03) [.905]	0.0410 (.029) [.703]	0.0170 (.033) [1]	-0.00600 (.041) [1]	0.307	0.291	2841
Uses certificates	0.0280 (.04) [.905]	0.0480 (.046) [.703]	0.0220 (.042) [1]	0.0230 (.057) [1]	0.401	0.650	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 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.19: **Predicted skills**

Female	-.153
	(.107)
Age	-.006
	(.017)
Married	-.033
	(.139)
Amhara	.098
	(.106)
Oromo	-.086
	(.115)
Born outside of Addis Ababa	-.293
	(.099)***
Vocational education	.625
	(.372)*
Degree	1.045
	(.431)**
Wage work (last 6 months)	.120
	(.170)
Weeks of wage employment (last 6 months)	-.005
	(.009)
Number of jobs (last 6 months)	.063
	(.041)
Every employed in permanent job	.029
	(.249)
Self employment (last 6 months)	-.091
	(.195)
Weeks of self-employment (last 6 months)	.340
	(.185)*
Const.	4.089
	(.995)***
Obs.	465
Prob $> F$	0.000

Note. The dependent variable is the aggregate score on all tests. All covariates are measured at baseline. We also include dummies for: the occupation, contract type and wage band of the current job; the occupation, contract type and wage band of the highest-paying previous job; the highest educational qualification achieved and the institution where this was achieved; self-assessed computer literacy skills.

Table A.20: **Heterogeneous effects on 2018 wages by baseline characteristics**

	Covariate = 0			Covariate = 1			Transport	Workshop
Baseline covariate	Control mean (1)	Transport (2)	Workshop (3)	Control mean (4)	Transport (5)	Workshop (6)	Equality (pval) (7)	Equality (pval) (8)
Tertiary Education	826.4	28.4	493.4**	1,835.1	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	1,181.9	-39.1	141.3	1,892.4	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	1,442.2	-0.5	361.8*	1,625.8	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	1,465.8	36.2	369.1***	1,975.7	-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	1,468.8	15.5	428.6**	1,606.3	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	1,524.5	-207.5	147.7	1,535.4	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	1,266.2	-7.3	314.9**	2,231.1	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	1,548.9	93.6	468.0***	1,656.5	-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	1,347.3	105.8	279.8*	1,705.8	-38.2	343.6	0.51	0.81
		(128.9)	(144.9)		(164.1)	(209.5)		
		[1.000]	[0.043]		[1.000]	[0.316]		

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 in 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 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.21: **Effects on 2015 and 2018 earnings with alternative earnings measures**

	2015				2018			
Outcome	Control mean (1)	Transport (2)	Workshop (3)	Equality (pval) (4)	Control mean (5)	Transport (6)	Workshop (7)	Equality (pval) (8)
Monthly wages	857.8	65.879 (63.864) [1.000]	3.363 (65.667) [1.000]	0.30	1,531.4	30.916 (102.352) [1.000]	299.469** (121.383) [0.023]	0.02
Total earnings (with profits)	1,145.0	10.994 (74.959) [1.000]	76.754 (85.239) [1.000]	0.39	2,184.2	-101.236 (135.372) [1.000]	405.842** (160.515) [0.023]	0.00
Total earnings (winsorised profits)	1,098.9	43.324 (72.544) [1.000]	99.640 (84.851) [1.000]	0.48	2,118.8	-125.423 (126.926) [1.000]	341.783** (148.911) [0.023]	0.00

This table shows the effects on earnings at the second endline (2018) with different measures of earnings using Equation (1). In row 1 we show our main measure of monthly wagaes. In row we show the results where we estimate total earnings including monthly profits from self-employment, including raw responses which include large outliers. In row 3 we show total earnings including monthly profits but where we winsorise profits at the 99th percentile to remove outliers.

Table A.22: **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 on 2018 wage earnings only, exluding self-employment earings, with controls for baseline outcomes.

Table A.23: **Quantile regression results: Impact on 2018 wage plus self-employment 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 on 2018 earnings, with controls for baseline outcomes.

Table A.24: Heterogeneous effects on probability of permanent employment by baseline characteristics

Interaction	Transport		Equality (p)	Workshop		Equality (p)	Control means		Obs.
Saving	Below med.	Above med.		Below med.	Above med.		Below med.	Above med.	
	0.0490	0.0240	0.532	0.0480	0.0720	0.565	0.149	0.110	2841
	(.037)	(.020)		(.038)	(.022)***				
Ever had a permanent job?	No	Yes		No	Yes		No	Yes	
	0.0360	-0.0290	0.379	0.0710	0.0300	0.598	0.103	0.269	2841
	(.020)*	(.069)		(.020)***	(.075)				
Search intensity	Low	High		Low	High		Low	High	
	0.0410	0.0190	0.563	0.0950	0.0410	0.191	0.106	0.134	2841
	(.026)	(.027)		(.03)***	(.027)				
Gender	Female	Male		Female	Male		Female	Male	
	0.0590	-0.00700	0.0558*	0.0700	0.0620	0.834	0.104	0.138	2841
	(.024)**	(.027)		(.026)***	(.029)**				
Born in Addis?	No	Yes		No	Yes		No	Yes	
	0.0140	0.0400	0.500	0.0500	0.0760	0.521	0.127	0.116	2841
	(.030)	(.023)*		(.033)	(.024)***				
Distance	> 5.8 Km	$\leq$ 5.8 Km		> 5.8 Km	$\leq$ 5.8 Km		> 5.8 Km	$\leq$ 5.8 Km	
	0.00700	0.0540	0.184	0.0390	0.105	0.0776*	0.117	0.124	2841
	(.023)	(.028)**		(.026)	(.027)***				
Used CVs or certificates?	No	Yes		No	Yes		No	Yes	
	0.0350	0.0220	0.727	0.0500	0.0800	0.451	0.0827	0.162	2841
	(.026)	(.025)		(.027)*	(.028)***				
Job search network	Small	Large		Small	Large		Small	Large	
	0.0510	-0.00100	0.201	0.0910	0.0380	0.207	0.122	0.118	2817
	(.023)**	(.032)		(.024)***	(.034)				
Present bias?	Yes	No		Yes	No		Yes	No	
	0.0270	0.0380	0.879	0.0680	-0.0850	0.0138**	0.115	0.180	1956
	(.022)	(.071)		(.024)***	(.056)				
Present bias & not anticipating revision?	Yes	No		Yes	No		Yes	No	
	0.0310	0.122	0.811	0.0480	0.00400	0.871	0.120	0.340	1956
	(.022)	(.384)		(.022)**	(.269)				

Heterogeneity by education:

Transport			Equality (p)	Workshop			Equality (p)	Control means			Obs.
High	Vocat.	Dip/deg.		High	Vocat.	Dip/deg.		High	Vocat.	Dip/deg.	
0.0590	-0.00600	-0.0340	0.0795*	0.106	0.00800	0.00600	0.0225**	0.0583	0.169	0.300	2841
(.025)**	(.030)	(.043)		(.028)***	(.026)	(.047)					

Table A.25: Heterogeneous effects on probability of formal employment by baseline characteristics

Interaction	Transport		Equality (p)	Workshop		Equality (p)	Control means		Obs.
Saving	Below med.	Above med.		Below med.	Above med.		Below med.	Above med.	
	0.0980	0.0390	0.218	0.0780	0.0420	0.451	0.190	0.166	2841
	(.039)**	(.023)*		(.042)*	(.022)*				
Ever had a permanent job?	No	Yes		No	Yes		No	Yes	
	0.0590	0.00700	0.495	0.0540	0.0330	0.782	0.158	0.293	2841
	(.020)***	(.071)		(.021)***	(.072)				
Search intensity	Low	High		Low	High		Low	High	
	0.0740	0.0330	0.338	0.0650	0.0380	0.556	0.149	0.194	2841
	(.029)**	(.028)		(.030)**	(.03)				
Gender	Female	Male		Female	Male		Female	Male	
	0.0630	0.0430	0.638	0.0900	0.00500	0.0427**	0.152	0.195	2841
	(.028)**	(.029)		(.027)***	(.031)				
Born in Addis?	No	Yes		No	Yes		No	Yes	
	0.0710	0.0430	0.530	0.0870	0.0290	0.186	0.151	0.185	2841
	(.035)**	(.024)*		(.032)***	(.027)				
Distance	> 5.8 Km	$\leq$ 5.8 Km		> 5.8 Km	$\leq$ 5.8 Km		> 5.8 Km	$\leq$ 5.8 Km	
	0.0580	0.0450	0.717	0.0490	0.0560	0.873	0.155	0.194	2841
	(.026)**	(.028)		(.027)*	(.028)**				
Used CVs or certificates?	No	Yes		No	Yes		No	Yes	
	0.0690	0.0270	0.378	0.0420	0.0640	0.660	0.135	0.236	2841
	(.026)***	(.034)		(.025)*	(.039)*				
Job search network	Small	Large		Small	Large		Small	Large	
	0.0650	0.0440	0.591	0.0370	0.0620	0.552	0.171	0.173	2817
	(.031)**	(.024)*		(.031)	(.027)**				
Present bias?	Yes	No		Yes	No		Yes	No	
	0.0570	0.0140	0.577	0.0280	-0.0510	0.291	0.180	0.228	1956
	(.027)**	(.069)		(.025)	(.068)				
Present bias & not anticipating revision?	Yes	No		Yes	No		Yes	No	
	0.0530	0.240	0.549	0.0150	0.150	0.574	0.183	0.340	1956
	(.024)**	(.311)		(.023)	(.237)				

Heterogeneity by education:

Transport			Equality (p)	Workshop			Equality (p)	Control means			Obs.
High	Vocat.	Dip/deg.		High	Vocat.	Dip/deg.		High	Vocat.	Dip/deg.	
0.0710	0.0370	0.00700	0.536	0.0690	0.0350	-0.00200	0.401	0.108	0.216	0.370	2841
(.029)**	(.033)	(.046)		(.029)**	(.030)	(.045)					

Table A.26: Heterogeneous effects on 2015 labour market outcomes by predicted probability of endline employment

	Worked	Hours worked	Formal work	Permanent work	Temporary work	Self-employment	Earnings	Employed earnings	Satisfaction with work
High employment group:									
Transport	-0.019	-2.324	0.064	0.013	0.001	-0.049	-131.779	-86.794	-0.043
	(0.034)	(1.995)	(0.033)*	(0.029)	(0.042)	(0.027)*	(143.085)	(177.579)	(0.040)
Workshop	-0.053	-3.082	0.013	0.038	-0.071	-0.024	-174.617	-62.390	-0.017
	(0.035)	(2.059)	(0.030)	(0.030)	(0.044)	(0.027)	(150.041)	(185.254)	(0.036)
Low employment group:									
Transport	0.095	2.155	0.051	0.045	0.046	0.003	113.641	-22.707	0.032
	(0.040)**	(2.076)	(0.024)**	(0.022)**	(0.038)	(0.019)	(83.427)	(150.791)	(0.031)
Workshop	0.103	3.207	0.088	0.088	0.002	0.012	276.794	245.293	0.057
	(0.042)**	(2.276)	(0.027)***	(0.024)***	(0.036)	(0.021)	(106.232)***	(184.871)	(0.035)
Intercepts:									
High employment	0.218	10.375	0.086	0.100	0.129	0.051	597.055	251.870	0.122
	(0.036)***	(2.077)***	(0.024)***	(0.021)***	(0.039)***	(0.024)**	(144.132)***	(198.226)	(0.030)***
Const.	0.378	19.314	0.114	0.075	0.252	0.066	555.123	1447.526	0.160
	(0.027)***	(1.564)***	(0.013)***	(0.011)***	(0.025)***	(0.013)***	(58.273)***	(114.341)***	(0.018)***
Obs.	2201	2201	2201	2201	2201	2201	2201	2201	2201
Tests of common effects:									
Transport (p)	0.022**	0.104	0.741	0.334	0.389	0.107	0.123	0.755	0.089*
Workshop (p)	0.003***	0.034**	0.048**	0.145	0.129	0.251	0.011**	0.186	0.119
Tests among 'low employment':									
Zero effects (p)	0.018**	0.343	0.003***	0.000***	0.408	0.848	0.031**	0.319	0.226
Common effects (p)	0.845	0.632	0.194	0.119	0.249	0.684	0.129	0.153	0.522

A.2 Sensitivity analysis

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.

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.[52] 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.[53] 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.[54] 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.

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

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

[54] 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: **Sensitivity analysis: effect of attrition**

Imputation method	Control mean (1)	ITT Estimate Coeff (2)	ITT Estimate Std. Err. (3)
Predicted earnings	1,535.6	248.1**	109.3
Predicted earnings +/- 0.25 SDs	1,548.5	223.4**	109.3
Predicted earnings +/- 0.5 SDs	1,561.5	198.7*	109.4
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

A.3 Indirect effects on the untreated

In this section, we study the outcomes of untreated job-seekers who live close to program participants. Not all individuals in the clusters assigned to the transport intervention and job application workshop were offered treatment. Some eligible respondents living in clusters assigned to treatment are not offered the program. The proportion of untreated individuals was fixed at 20% in the clusters that received the job application worshop and randomly varied between 10% and 80% across those that received the transport subsidy. This allows us to compare untreated individuals living close to program participants to untreated individuals living in clusters where no job-seeker has been offered the intervention. Among those in the transport clusters, we implemented a randomised saturation design. We varied the proportion of sampled individuals who were offered the transport subsidies from 20% to 40%, 75% and 90%.

This allows us to run a regression of the form:

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

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.[55] S_{20c} is a dummy variable for individuals living in a cluster where 20% of individuals were offered the transport treatment. Thus, β_{20} captures the difference in outcomes between untreated individuals in these clusters and untreated individuals in clusters where nobody was treated. Further, γ_{20} measures the difference in outcomes between treated individuals in S_{20c} clusters and untreated individuals in untreated clusters.

The benefits of the interventions can extend to untreated individuals in treated areas if the young job-seekers who are offered the programs share information, job referrals or resources with friends and acquaintances in the same neighbourhood.[56] We do not think that this research design is likely to detect displacement effects, due to the reallocation of jobs from untreated to treated individuals. Most workers in our sample commute to work. We estimate that roughly 30% of young people are able to walk to work (they do not use public transport). Among those who use public transport, median commuting time (one-way) is 35 minutes, and more than 90% commute further than 15 minutes each. This makes it unlikely that any displacement effects will be heavily concentrated in the small geographic clusters from which we drew the spillover groups. These clusters were typically never more than 300m in diameter, while the average distance from the CBD in our sample is roughly 6km. Workers who get formal, higher paid work, or especially likely to commute to jobs out of their local area. Therefore, we do not expect to see local

[55] The sample is restricted to individuals in clusters assigned to pure control and clusters assigned to the transport intervention.

[56] Information and risk sharing of this kind have been documented in several recent studies on developing countries' labour markets (Angelucci and De Giorgi, 2009; Magruder, 2010). The descriptive evidence from our surveys further confirms that social networks are an important source of information about work opportunities and are used extensively for job referrals.

labour market effects concentrated in the areas where treatment was saturated. To the extent that Addis Ababa constitutes one large labour market, or several tightly integrated labour markets, any reallocation of jobs would take place at the level of the entire city, and our intervention is too small (relative to the size of the city) to have significant spillover effects. Instead, we interpret these results as being driven through social networks rather than labour market displacement.

In Table A.28 we show the spillover effects from the two intervention on employment outcomes in the first endline survey (2015). We find no difference between untreated individuals living in geographical clusters assigned to one of the two interventions and untreated individuals in pure control clusters.[57]

We find some evidence that the indirect effects of the transport treatment depend on the level of saturation, shown in Table A.29, that were masked by the average spillover effects in Table A.28. We document a positive indirect effect on formal and permanent work among control individuals in clusters with 40 percent saturation. We also document that untreated individuals in clusters with 90 percent saturation are 5.6 percentage points less likely to be in permanent employment than individuals in pure control clusters.[58] They are not, however, less likely to be in formal employment. We take these results to be suggestive evidence of local spillovers. They should be interpreted with caution, given the small sample sizes, and the number of tests run in Tables A.29 and A.30.

[57] We are less powered to detect indirect effects compared to the direct effects we studied above. For example, we estimate that untreated individuals in clusters assigned to the job application workshop experience an increase in the probability of formal work of 6 percentage points. This effect is of the same magnitude as the treatment effect we estimate on individuals who are offered the job application workshop, but it is not statistically significant.

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

Table A.28: **Spillover effects of the transport and workshop intervention on employment outcomes (2015)**

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

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.29: **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.30: **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 Theoretical appendix

A.4.1 A simple signal-processing model

In this appendix, we present a stylised model, to guide intuition on how a reduction in noise might increase an individual's chance of formal employment. For simplicity, we focus on the signal-inference problem, to guide intuition for thinking about match quality.

Suppose that individual i applies to firm f. The true productivity of individual i, were (s)he to be hired by firm f, is given by x_{if}. However, this is observed by the firm with noise; denote this noise as ε_{if}; specifically, the observed signal is given by $y_{if} = x_{if} + \varepsilon_{if}$. For tractability, we assume a 'Normal-Normal' structure; namely:[59]

$$x_{if} \sim \mathcal{N}(0, 1); \tag{6}$$

$$\varepsilon_{if} \sim \mathcal{N}(0, \sigma^2). \tag{7}$$

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

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

$$x_{if} \,|\, y_{if} \sim \mathcal{N}\left(\frac{y_{if}}{1+\sigma^2}, \frac{\sigma^2}{1+\sigma^2}\right). \tag{8}$$

Suppose that, in addition to the application from worker i, the firm receives a larger number of applications from other applicants, such that the preferred applicant of that set is fixed at zero.[60] Therefore, if the firm chooses to hire worker i, it will displace some other applicant in doing so; if the firm hires applicant i and applicant i has $x_{if} > 0$, then we say that the match quality has improved relative to the status quo.

Therefore, the firm will hire if and only if:

$$y_{if} - 0.5r \cdot \sigma^2 \geq 0. \tag{9}$$

Note that, in comparative statics, a firm will be more demanding — i.e. less likely to hire

[59] Note that the assumption that $\text{Var}(x_{if}) = 1$ is without loss of generality; we can think of this as a normalisation, as with the assumption that x_{if} and ε_{if} are centred at zero.

[60] We could choose any value other than zero here, of course; we normalise to zero for ease of exposition. Similarly, we could also think of the quality of the preferred applicant as a stochastic variable, but we fix this for tractability.

— as (i) the firm is more risk averse, and (ii) the noise is greater. Thus we immediately have a result that a reduction in applicant i's signal noise will increase the unconditional probability that the firm will hire. Two implications follow from this. . .

Probability of hiring: Note that, in unconditional distribution, $y_{if} \sim \mathcal{N}(0, 1+\sigma^2)$. It therefore follows that the unconditional probability of worker i being hired is:

$$\Phi\left(\frac{-0.5r\sigma^2}{\sqrt{1+\sigma^2}}\right), \tag{10}$$

where Φ denotes the *cdf* of the Normal distribution. Straightforwardly, this unconditional probability is decreasing in the signal noise, σ^2; that is, a reduction in σ^2 will increase the probability that worker i is hired.

Match quality: For the firm, the expected value of a match is therefore given by:

$$\mathbb{E}(x_i \,|\, y_i > 0.5r\sigma^2) \cdot \Pr(y_i > 0.5r\sigma^2) + 0 \cdot \Pr(y_i \leq 0.5r\sigma^2) = \frac{\phi\left(\frac{0.5r\sigma^2}{\sqrt{1+\sigma^2}}\right)}{\sqrt{1+\sigma^2}}. \tag{11}$$

This function is decreasing in σ^2: if the job-seeker can reduce σ^2, (i) the job-seeker increases the probability of getting a job, and (ii) the expected match quality for the firm increases.

What about the expected value of a match conditional on employment? Following the same reasoning, this is given by:

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

This is a useful expression for guiding our intuition about the possible wage effects of our workshop intervention. In particular, suppose that, in the long run, the firm learns x_{if} perfectly — and that the firm pays the worker according to this value.[61] It can be shown numerically that equation 12 is decreasing in σ^2 for any reasonable risk aversion parameters.[62] That is, a reduction in σ^2 will lead both to an increase in the value of the match, and to an increase in the hired worker's remuneration.

[61] We do not model the forces that might lead the firm to do this; in a more complex setup, we could consider competition from other firms, or bargaining between firms and workers, *etc*. For our purposes, this would complicate the exposition substantially, without assisting our key intuition.

[62] 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, note that (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.

A.4.2 Allowing the worker to choose a higher or lower σ^2

There are several ways in which our experiment might allow a worker to choose her or his signal variance — for example, through improved interview/presentation skills, and through using the formal certificate provided. What implications does this have for the signal-processing framework?

To answer this question, suppose now that the worker observes x_{if} (but not ε_{if}) before choosing σ^2. This captures the intuition that a worker will know whether (s)he has special skills that are well directed to a particular job, but does not know the idiosyncratic perception of those skills that the firm will form (only the uncertainty attached to that perception). Assume that the firm observes σ^2; that is, the firm is aware of how precise or imprecise is the signal technology being used.[63]

Assume that the worker seeks to maximise the probability of being hired. How does this probability depend upon σ^2 for a worker with a given value x_{if}? Note that, conditional upon x_{if}, we have:

$$y_{if} \mid x_{if} \sim \mathcal{N}\left(x_{if}, \sigma^2\right). \tag{13}$$

Therefore, the probability of being hired is:

$$\Pr\left(y_{if} \geq 0.5r \cdot \sigma^2 \mid x_{if}\right) = \Phi\left(\frac{x_{if}}{\sigma} - 0.5r\sigma\right). \tag{14}$$

Note that this is monotonic in σ. Suppose that our experiment allows a given worker to reduce the variance from σ^2 to $\tilde{\sigma}^2 < \sigma^2$. Then this will be preferred for any case in which:

$$\begin{aligned} 0.5r + \frac{x_{if}}{\sigma^2} &> 0 \\ \Leftrightarrow x_{if} &> -0.5r\sigma^2. \end{aligned} \tag{15}$$

Several insights follow from this result. First, note that, a worker with $x_{if} > 0$ will always prefer to have the smallest variance possible. This makes intuitive sense: the firm would prefer to hire such a worker, so the worker prefers to have her or his quality known as precisely as possible. Second, for workers with $x_{if} < 0$, the optimal choice about variance depends upon trading off two competing considerations: (i) the worker welcomes some noise, in order to allow the possibility that the firm will (wrongly) infer that $x_{if} > 0$; however, (ii) the worker does not want so much noise that the risk-averse firm becomes too demanding in the quality of observed signal. In sum, *ceteris paribus*, workers will prefer a lower noise if they are a better fit for the position.

[63] For simplicity, assume that the firm draws no further inference about the particular type of worker who might choose a particular value of σ^2; we seek to capture a context where σ^2 is decided by the treatment, so we model individual's preference for σ^2 without allowing firms to infer anything from the particular choice of σ^2.

A.4.3 Introducing an observable covariate

The previous results help to guide our intuition about the likely general effects of our workshop intervention. However, we are also interested in effect heterogeneity: how should we expect the signal value to differ between groups who are more or less disadvantaged in the labour market?

To answer this question, we introduce an additional variable: an observable covariate that correlates with match quality. To this point, we have considered heterogeneity only in the *unobservable* match quality (x_{if}) and noise (ε_{if}). We now consider what happens if firms have some *observable* proxy for suitability. (We have in mind, for example, labour market experience or gender. Note that education is unlikely to be a suitable candidate for this variable, given that the labour market in our study is heavily segmented by education; we thank an anonymous referee for this observation.) 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 assume that z has the same variance as x (*i.e.* normalised to 1), and that z and x have a bivariate normal distribution, with correlation ρ:

$$\begin{pmatrix} x_{if} \\ z_i \end{pmatrix} \sim \mathcal{N}\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix}\right). \tag{16}$$

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

$$x_{if} \,|\, z_i \sim \mathcal{N}\left(\rho \cdot z, (1-\rho^2)\right). \tag{17}$$

We can then revisit the earlier theoretical results, thinking about heterogeneous effects.[64] First, note that, by Bayes' Rule, the firm now infers:

$$x_{if} \,|\, y_{if}, z_i \sim \mathcal{N}\left(\frac{y_{if} \cdot \sigma^{-2} + \rho \cdot z_i \cdot \left(1-\rho^2\right)^{-1}}{\sigma^{-2} + (1-\rho^2)^{-1}}, \frac{1}{\sigma^{-2} + (1-\rho^2)^{-1}}\right). \tag{18}$$

Therefore, the firm will now hire if and only if $y_{if} \geq 0.5r\sigma^2 - \dfrac{\rho\sigma^2}{1-\rho^2} \cdot z_i$. This has several implications for our earlier results.

Probability of hiring: Note that $y_{if} \,|\, z_i \sim \mathcal{N}\left(\rho \cdot z_i, 1-\rho^2+\sigma^2\right)$. Therefore, the probability of the worker being hired is:

$$\Phi\left(\frac{-0.5r\sigma^2}{\sqrt{1-\rho^2+\sigma^2}} + z_i \cdot \frac{\rho\sqrt{1-\rho^2+\sigma^2}}{1-\rho^2}\right). \tag{19}$$

[64] Note, of course, that these results will nest the earlier results for the special case $\rho = 0$.

Therefore, the effect of additional advantage (namely, a higher value of z_i) depends upon the sign and magnitude of:

$$\frac{\rho\sqrt{1-\rho^2+\sigma^2}}{1-\rho^2}.$$

Note that this is increasing in both ρ and σ^2. This increases in σ^2 for reasons of statistical discrimination: in a noisier environment, the firm places relatively more weight on z_i because of its value as a proxy for x_{if}. However, note that the term does not go to zero in the limit as $\sigma^2 \to 0$. This shows that z_i is relevant not merely for inference purposes; so long as $\rho > 0$, z_i correlates with x_{if} and therefore proxies for productivity, rather than merely providing a basis for signalling.

Signal improvement: Who benefits, in this extended model, from reducing σ^2? We know that $y_{if} \mid x_{if}, z_i \sim \mathcal{N}\left(x_{if}, \sigma^2\right)$. Therefore, the probability of being hired is:

$$\Pr\left(y_{if} \geq 0.5r \cdot \sigma^2 - \frac{\rho\sigma^2}{1-\rho^2} \cdot z \,\middle|\, x_{if}, z_i \right) = \Phi\left(-0.5r \cdot \sigma + \frac{x_{if}}{\sigma} + \frac{\rho\sigma}{1-\rho^2} \cdot z \right). \tag{20}$$

Straightforwardly, this probability is decreasing in σ if and only if:

$$-0.5r - \frac{x_{if}}{\sigma^2} + \frac{\rho}{1-\rho^2} \cdot z < 0. \tag{21}$$

This maps a linear indifference curve in (x_{if}, z_i) space; the condition shows that a reduction in noise is valued by applicants who (i) are a strong match (that is, higher x_{if}), and (ii) who have a worse observable (that is, lower z_i).