Anonymity or Distance?

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

Girum Abebe,[†] Stefano Caria,[‡] Marcel Fafchamps,[§] Paolo Falco,[¶]
Simon Franklin[||] and Simon Quinn[**]

July 31, 2018

Abstract

We show that an easy-to-scale intervention that helps young job-seekers to signal their ability to employers can generate large improvements in labor market outcomes. We are also uniquely able to compare this intervention (the 'job application workshop') to a transport subsidy treatment designed to reduce search costs. We find that in the short-run both interventions have large positive effects on the probability of finding formal jobs. The workshop also helps young people access stable jobs with an open-ended contract. Four years later, the workshop has a large and significant impact on earnings, while the effects of the subsidy have dissipated. The results are driven largely by groups that are at the greatest disadvantage in the labor market, leading to strong equity gains. Our results show the crucial role that effective signalling of skills can play in supporting the inclusion of youth in the labor market.

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

*We are grateful to Gharad Bryan, Erica Field, Markus Goldstein, Douglas Gollin, Supreet Kaur, Jeremy Magruder, Marco Manacorda, 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).

[†]Ethiopian Development Research Institute: girumabe@gmail.com

[‡]Oxford Department of International Development, University of Oxford: stefano.caria@qeh.ox.ac.uk

[§]Freeman Spogli Institute, Stanford University: fafchamp@stanford.edu

[¶]OECD: paolo.falco@oecd.org

[||]Centre for Economic Performance, London School of Economics: s.franklin1@lse.ox.ac.uk

[**]Department of Economics, University of Oxford: simon.quinn@economics.ox.ac.uk

1 An experiment to help youth access the labour market

Throughout the world, young people work less, earn less and face more job insecurity compared to older workers. This is a major policy challenge, especially in Africa, the youngest continent in the world, with a population of almost 200 million people aged between 15 and 24. If excluded from economic opportunity, young people can represent a major source of instability for the continent. If employed productively, they can turn into a key asset for growth.

How can labour markets be improved to 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 is the case, policies that reduce search costs, such as subsidised or improved transport systems and online job posting, hold great promise for improving labour markets. However, if the problem is primarily related to the perceived employability of youth, merely increasing job search will have only weak or short-lived effects. Young people will obtain low-quality jobs faster, but they will largely be unable to secure well-paid, stable employment. Under this alternative view, improving young people's *ability to signal their skills* will be a more effective policy. With little formal work experience and limited credentials, it may be particularly hard for young people to demonstrate their employability. Interventions that generate credible signals about skills may thus be able to unlock better employment opportunities and generate long-run economic gains for youth.

We investigate these competing views on the inclusion of young people in the labour market by running two parallel field experiments with a representative sample of over 3,000 young people in Addis Ababa, Ethiopia.[1] The first intervention – aimed at reducing the cost of job search – is a *transport subsidy*. Participants are reimbursed 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, up to three times a week. 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. We evaluate these programs through two endline surveys, eight months and then approximately four years after the end of the interventions.

[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.

We find starkly different results from the two interventions. In the short run (eight months after treatment), the transport intervention increases job-search intensity and has significant impacts on the probability of having a formal job. However, four years later, we find that these effects have dissipated completely. Lowering search costs can help workers to obtain a formal job more quickly, but it does not change long-term employment outcomes. The job application workshop, in contrast, shows long-lasting effects. In the short run, it increases the probability of both permanent and formal work *without* increasing the intensity of job-search. Four years after treatment, the workshop also shows a large positive impact on earnings, which amounts to a 20% increase over the control group mean. Improving the ability to signal skills thus changes young people's long-term prospects in the labour market.

Our findings show that the young job-seekers in our context have productive skills that employers are unable to detect. Raising the quality of their signals improves job-matches and increases productivity. Several pieces of evidence support this unique conclusion. First, the intervention generates earnings growth by increasing wages (which track productivity), rather than by increasing hours worked or employment (which are only changed modestly and insignificantly by the intervention). 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. This indicates that employers perceive young workers as more productive thanks to the workshop, and that this perception is 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 gains are also particularly impressive when contrasted with the experience of people in the control (and transport) group. While it is relatively easy for control individuals to find work (the control employment rate reaches 70 percent by the time of second endline), higher salaries seem to be persistently out of reach for this group (control wages grow by only 8 percent over a period of three years, well below the rate of inflation). In light of all this evidence, enabling the labour market to observe, price, and employ under-utilised skills is likely to generate net gains for the economy. This points to an *efficiency* rationale for improving young people's skill signals.

Our results also highlight that job search assistance can be used to promote equity in the labour market. In the short-run, the job-quality gains from both interventions are concentrated among the most disadvantaged socio-demographic groups. In the long-run, the earning impacts of the workshop are also driven by the workers with the worst prospect in the labour market: the least educated and the least experienced. This reduces income inequality in our sample in a meaningful way. For example, at the time of the second endline, we observe a 34 percent earnings gap between control individuals who had some permanent work experience at the beginning of the study and those who did not have that experience. This gap is eliminated among young people in the job application workshop group. This points to a second, *equity*-based rationale for this intervention.

Finally, we show that helping young people signal their skills is a remarkably cost-effective policy option. The job application workshop generates earning gains of US$ 10 per month for a one-off, marginal cost of US$18.20 per individual. This compares favourably with other available labour market interventions, which are typically much more expensive: the ratio of the earning gains to the cost of the intervention that we estimate in our study is the largest documented in the recent literature (we focus on the studies included in the review of McKenzie (2017)). The long-term positive effects of the workshop are also in stark contrast with recent results from the cash transfer literature showing that the earning impacts from increased entrepreneurial activity can be relatively short lived (Haushofer and Shapiro, 2018). In addition, the job application workshop is easy to implement and scale up (the intervention is delivered in two days and follows standardised protocols), which is not the case for all labour market interventions.

Our main contribution is to show that information asymmetries can be a major barrier for youth in the labour market. More specifically, this is the first paper to show that young people in developing countries have valuable uncertified skills — which, if certified, can generate substantial long-term earnings gains. This is also the only study, to the best of our knowledge, that shows the effectiveness of a cheap scalable intervention that improves the ability of job-seekers to signal their skills. Pallais (2014) and Abel et al. (2016) emphasize the informational content of reference letters from past employers. However, reference letters are only useful to workers who have had previous (presumably positive) work experience. We independently verify the skills of unemployed workers, many of whom have never had 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 simply providing workers with an improved signal of their abilities, for them to use independently, can improve their employment prospects.[2]

Furthermore, this is the first study that directly compares the impacts of two different types of active labour market policies and, in doing so, highlights the relative importance of distinct frictions. In line with Franklin (2017) and Phillips (2014), who study the short-term impacts of transport subsidies on non-representative samples, we find evidence that search costs are a significant barrier to job search. However, we also find that these effects are weaker in a representative sample, and ultimately short lived. A recent literature has shown that transport subsidies can have more persistent effects when they connect rural

[2] A related literature studies the role of information provision in developed economies. For example, Altmann et al. (2015) find positive effects of a brochure designed to encourage job search among disadvantaged communities, and Belot et al. (2015) improve search efficacy through expanded job suggestions in an online market.

workers to urban jobs (Bryan et al., 2014). However, these interventions relax a set of information constraints that is different to those that are at play in a population already exposed to the urban labour market.

Our study also overcomes some notable shortcomings of the recent experimental literature on active labour market programmes in developing economies (McKenzie, 2017). First, as mentioned above, we work with a large representative sample and complete a four-year follow up. Many other studies rely on groups that are selected along important economic dimensions (whether they are actively searching for work, whether they are part of a particular government program, etc..) and only document short term impacts. Second, we have very low attrition, even in the four-year follow-up survey. Third, we follow a pre-analysis plan, which specifies all of our main outcomes of interest.[3] This enables us to formally control for the multiple hypotheses tested – all of our main results are robust to this correction – and eliminates concerns about selective reporting. Fourth, we conduct a high-frequency phone survey that allows us to investigate the mechanisms through which job-seekers find better jobs. In particular, we are able to analyse the immediate responses to the 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 developing countries' labour markets. Workers in our study have short average tenures and, by the time of the second endline, most workers have changed job at least once. However, through mediation analysis we show that two important mediators of the long-run earning effects are related to the quality of employment at the time of the first follow-up (in particular, earnings and whether the job is a permanent job). This shows an important path-dependency and suggests that precarious jobs may not be a stepping stone to better employment later in workers' careers. Such path dependency has been identified in developed countries, using both quasi-experimental (Oreopoulos et al., 2012; Kahn, 2010) and experimental (Kroft et al., 2013) methods. Similarly, recent work has shown detrimental long run effects of temporary jobs for OECD workers (Perez et al., 2016). Our results suggest that similar dynamics may be at play in developing countries as well. As labour flows tend to be much larger in poorer economies (Donovan et al., 2018), the importance of job mobility in workers trajectories may be even greater in these contexts. Overall, these results point to the need of intervening early in workers' lives to avoid the scarring effects of a bad start in the labour market. Interventions like the job application workshop we have tested in this study represent a viable and effective option for policy makers.

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

2 The interventions

2.1 The challenge of finding good work in Addis Ababa

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).[4] In this growing labour market, finding satisfactory job opportunities is a major challenge for many young workers. Access to *some* form of employment is relatively easy: only 19% of the young job-seekers in our sample remain unemployed throughout the study period of 13 months. However, available jobs are often insecure, informal and poorly paid — a policy challenge faced by many low-income economies (AfDB, 2012). In such a context, a policy evaluation of active labour market policies should crucially assess their impacts on job quality.

One of the key characteristics of a good job in Addis Ababa is having a permanent contract, and only a minority of young workers enjoy that kind of stability. 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] At the time of our endline survey, only 17% of individuals in the control group have a permanent jobs (approximately 30% of all jobs are permanent). This is consistent with labour force data for Ethiopia: in 2012, 18.4% of urban youth had permanent work, compared to 30% of adults over the age of 30. The others work in temporary, casual or self-employment. Such precarious work has many downsides. In our sample, we find that a job is five times more likely to end because a temporary contract came to an end than because the worker was laid off. When workers leave jobs, we find that only 18% of them do so with another job lined up to start in the week after. This leads to enormous volatility of incomes and disruption to regular employment. In addition, non-permanent jobs typically provide irregular work streams, even when they are not terminated: at endline workers at temporary jobs say that they did not work on average 12% of the weeks since they got the job, compared to only 2% in permanent jobs.[6] Further, much temporary employment lacks a written agreement (in this paper, we refer to jobs with written contracts as 'formal jobs'). Lack of written contracts makes it difficult to enforce workers' rights, collect taxes and provide social security. Over half of the wage employees in the control group of our study do not have such a formal job.

For these reasons, permanent jobs are highly sought after by young Ethiopians. Our data

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

[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] The median duration of these spells without work was 4 weeks for temporary workers and 8 weeks for the self-employed.

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.[7] 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 named chose working hours.[8] We also observe that young people with irregular work keep on searching for better jobs (in our sample, people in temporary jobs search for work 35% of the weeks they are working). This is despite the fact that the wage premium for permanent jobs is relatively small for entry level jobs. Indeed, some temporary jobs, such as casual labour in the construction sector, pay high wages for physically tiring and often dangerous work.

Our data also shows that access to good jobs is particularly difficult for workers belonging to the most disadvantaged backgrounds, such as the less educated, women, people living in poor areas and in the outskirts of the city. 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 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.[9] 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, job-seekers for

[7] 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.

[8] "Social life with colleagues" was the most popular answer (21.5%), only slightly above work stability. The choice set here excluded "Pay" and "Work Satisfaction" (broadly defined).

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

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.[10]

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.[11] 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 expressed a demand for such training to 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 signal their abilities to employers, in the formal sector. 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 that provide an interesting comparison between contrasting forms of ALMPs.

2.3 Treatment 1: The transport subsidy

Individuals in this treatment group are offered a subsidy to cover the cost of traveling to the city centre. The subsidy takes the form of a cash transfer that is conditional on visiting a disbursement point, located in an office in the centre of Addis Ababa.[12] Recipients are required to attend in person, and to show photographic ID on each visit. Each recipient

[10] 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.

[11] 56 percent of firms report that for blue collar positions they only consider candidates with sufficient work experience, and 63 percent of firms use this selection method for white collar positions.

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

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.[13] 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.[14]

Prior to the intervention, respondents in our sample do not travel frequently to the city centre.[15] By paying participants conditional upon their presence at our office, we directly incentivise travel to the centre. This allows us to focus on spatial constraints to job search.[16] In addition, conditional transfers are a more realistic policy option in this context. Unconditional transfers have proved unpopular among voters in various countries in Sub-Saharan Africa (Ferguson, 2015; Sandefur et al., 2015) and the Ethiopian Government requests that the beneficiaries of social assistance programs are employed in public work schemes.[17]

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

[13] 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.

[14] 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.

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

[16] 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.

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

[18] 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 pct 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, the proportion of disbursements that were used to subsidise commuting is likely to be smaller than 6 percent.

2.4 Treatment 2: 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.[19]

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.[20]

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 performance.[21] The certificates are officially issued by the School of Commerce and the Ethiopian Development Research Institute.[22]

We chose the tests on the basis of the results of several qualitative interviews with firm

[19] 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.

[20] 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.

[21] 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%.

[22] 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.

managers in the city.[23] 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.[24]

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).

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.[25] 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, 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

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

[24] 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.

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

individuals from this list to be included in the study. Our lists included individuals with different levels of education. We sampled with higher frequency from the groups with higher education, to ensure that individuals with vocational training and university degrees are well represented in the study; we estimate using appropriate sampling weights. In all, we interviewed 3,052 individuals who are included in our experimental study.[26]

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).[27] 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 interviews between June and August 2015. Face-to-face interviews recorded information about the socio-demographic characteristics of study participants, their education, work history, finances, expectations and attitudes. We also collected an incentivised measure of

[26] 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.

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

present bias.[28] 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. We called all study participants throughout the duration of the study. In these interviews we administered a short questionnaire focused on job search and employment.[29]

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).[30]

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

[28] 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.

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

[30] 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.

outlined in Table 1.[31]

< 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 endline survey, we find 93.5% of all participants; and attrition is uncorrelated with treatment.[32] Table A.8 in the online appendix presents the full analysis.[33] 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.1 in the online appendix shows the trajectory of monthly attrition rates over the course of the phone survey. In the long-run follow-up survey attrition has increased, but we are still able to track a high proportion of respondents. We are able to find more than 85% of respondents, a relatively high number over such a long period. Table A.9 shows the correlates of attrition in this sample. We find that individuals in the workshop sample were slightly more likely to respond.

3.5 Take-up

Take-up is substantial for both treatments. 50% of individuals in the transport group collect the cash at least once. Of these, 81% return to collect the subsidy again. Those who collect the subsidies for at least two weeks tend to be dedicated users. Conditional on ever collecting the money, 74% of respondents take it at least once a week over the course of the entire study, with an average of 16 collections in total. Further, 61% of individuals who are invited to the job application workshop attend it. 80% of those attending later collect

[31] 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 in the online appendix.

[32] 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.

[33] 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.

the certificates from the School of Commerce. Take-up rates do not vary substantially with observable covariates.[34]

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.[35]

In the pre-analysis plan, we specify a family of six primary employment outcomes. For each one of them we test the null hypothesis that each treatment had no impact. We use 'sharpened' q-values to deal with multiple comparisons (Benjamini et al., 2006). The q-values control the false discovery rate within the family of six hypotheses that we test

[34] In Table A.10 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.

[35] 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.

for each program.[36] 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.[37] d_{wit} is a dummy variable equal to 1 in period t if an individual started receiving their treatment w periods ago.[38] 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.[39]

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}:

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

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

[36] 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.

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

[38] 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.

[39] 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.[40] 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 point. 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.

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

A back-of-envelope calculation suggests that, on average, helping one extra worker obtain a *formal* job costs about 365 USD in the transport treatment and 344 USD in the workshop. These figures are equivalent to 3.7 and 3.4 months of earnings, at mean wage. Further, for the workshop intervention we estimate that helping one extra worker obtain a *permanent* job costs about 264 USD, or 2.7 months of mean earnings.[41]

In addition to testing the effects of the interventions on the primary employment outcomes, we evaluate their impacts on a range of secondary outcomes, most notably other measures of job quality, worker expectations, reservation wages, aspirations and mobility (the full set of results is available in Tables A.11 to A.18 of the empirical appendix).[42] 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.[43]

4.2 Long-run impacts

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 20 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 education in our data and to 60 percent of the control group nominal earning growth between the two endline surveys.[44] The effect is statistically significant at the 5 percent level, and is robust to the correction for multiple comparisons. 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 .156. Both effects are significantly larger than the impacts of the transport intervention,

[41] It is important to note that these benefits are not offset by higher commuting costs, as there is no evidence that our treatments lead workers to take jobs further away from home (Table A.15).

[42] 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.11 in the appendix). For inference, we proceed as before: we report both p values and false discovery rate q-values by treating each index as a separate member of a 'super-family' of indices.

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

[44] Control group individuals experienced a 34 percent increase in nominal earnings between the two endline surveys. According to the general price index for Addis Ababa published by the Central Statistical Agency of Ethiopia, prices rose by about 23 percent during the same period.

which we discuss below.

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.3). 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. Further, they stay in the same job for a longer period of time. Finally, we use mediation analysis and data on job tenure to understand the 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%. 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.5).[45] 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.19). 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.[46]

< Figure 1 here. >

< Figure 2 here. >

4.3.2 Job search effectiveness and match quality

We find that the workshop improves job search effectiveness. In a labour market where worker quality is difficult to observe, a higher ability to signal skills should make workers more competitive for positions that are better paid and have higher job security. Consistently with this, we have shown above that young people in the job application workshop group are more likely to secure permanent jobs (in the short run) and high-paying jobs (insignificantly in the short run, and significantly in the long run) than the controls, while sending the same number of job applications.[47] In other other words, the workshop makes young people more *effective* when searching for work.

In Table 3 we present two additional pieces of evidence that reinforce this interpretation. First, treated young people stay in the same job for a longer period of time, a strong indicator that the quality of the job matches has improved. To show this, in the second endline

[45] 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.

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

[47] Indeed, in Table A.19 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.

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 show that the workshop significantly raises earnings conditional on employment by 386 ETB, or 17 percent. The productivity bounds of this effect are between 113 ETB (5 percent) and 673 ETB (30 percent), implying that selection effects are unlikely to be driving this result (Attanasio et al., 2011). This large and robust productivity effect confirms that the skills that the workshop has enabled young people to signal have a high value in the eyes of employers.

4.3.3 Why does the earning effect grow over time?

We use mediation analysis and data on job tenure to understand why the earning impacts of the job application workshop grow with time. 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 percent) can be explained by the short-run earning effect of the intervention. Further, the short-run impacts on permanent work can explain about 23 percent of the long run effect on earnings. If we fix both short-term earnings and permanent work, we can account for 62 percent of the original treatment effect.

< **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 85 percent of workers hold the same job in both endline surveys. 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.

4.4 The value of information about skills

We conclude this section by showing evidence that the information about worker skills that we disclose directly influences labor 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.[48] 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 perform this analysis for the aggregate score (a summary measure of all test results) and, to maximise power, we normalise this score and pool the data for all discontinuities together.[49] We find that being placed in a higher band generates a large, but noisily estimated increase in long-term earnings. 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 .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 .2 and .3 standard deviations and is significant at the 10 percent level when we use the larger bandwidth.

< **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 active labor market policies in developing countries included in (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 labor market policies evaluated in recent years, this intervention stands our as uniquely cost effective. Second, we discuss the equilibrium implications of our findings. Finally, we show that, regardless of whether the job application workshop

[48] There is not other way for study participants to access information about their original score.

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

improves labor market efficiency or not, this policy has a strong equity rationale as its benefits are concentrated among jobseekers who would otherwise be at the bottom of the earnings distribution.

5.1 How does the workshop compare to other active labor 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).[50] 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 Who benefits the most from the workshop?

We conclude by showing evidence of the strong equity-enhancing effects of the job application workshop. We do this by conducting a series of sub-group analyses using a list of covariates specified in our pre-analysis plan. Further, to identify a common pattern across multiple dimensions, we study treatment impacts by predicted earnings. This allows us to identify the most disadvantaged workers as those with relatively low predicted earnings based on a large vector of baseline covariates (the first-stage coefficients of the model used for the predictions are reported in Table **??**). We then use a 'split sample' method to estimate treatment heterogeneity between high predicted earnings and low predicted earnings individuals Abadie et al. (2017).

We find that the effects of the workshop are significantly larger for the most disadvantaged. In particular, we show in Table 5 that the least educated, the least experienced, and those with the lowest expected earnings benefit the most from the interventions. For

[50] 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.

other dimensions, we are unable to find significant differences in response to treatment.[51] The size of the effects for the worst-off workers is substantial. For example, young people without tertiary education increase the earnings by almost 60 percent, while the low predicted earnings group experiences a 50 percent increase. 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.

< Table 5 >

[51] In Table 5, we report a selection of the covariates we specified. We report the remaining covariates in Table A.23 in the Online Appendix. In Tables A.24 and A.25 we show the heterogeneity of the short-term impacts on job quality of the two interventions. Both interventions have larger impacts for the most disadvantaged also in the short run.

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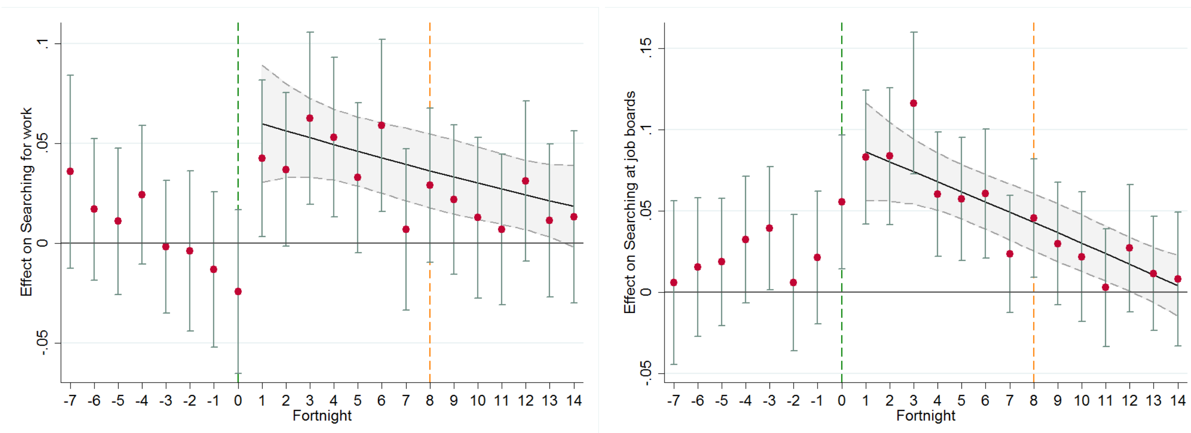
Figures and Tables

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

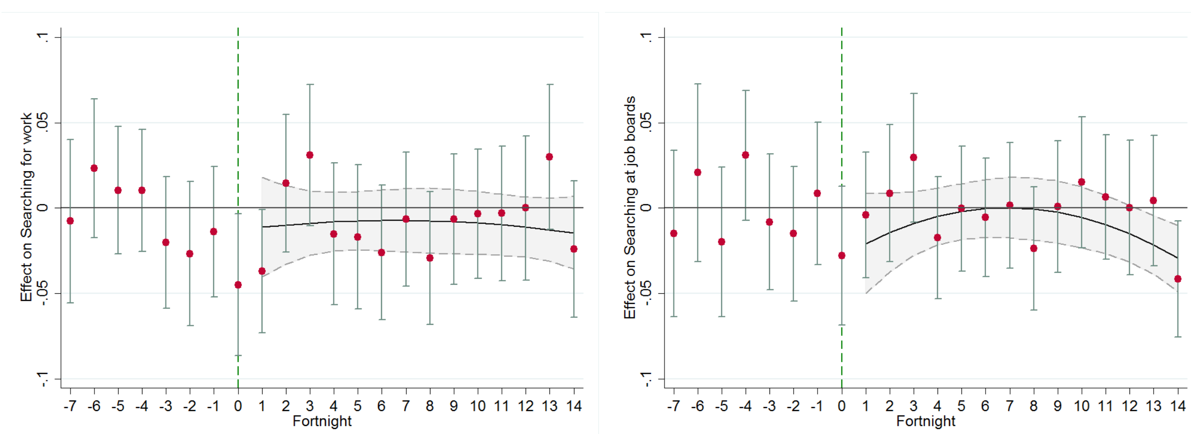
(a) Impact on search (any active step) (b) Impact on searching at the job boards



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.

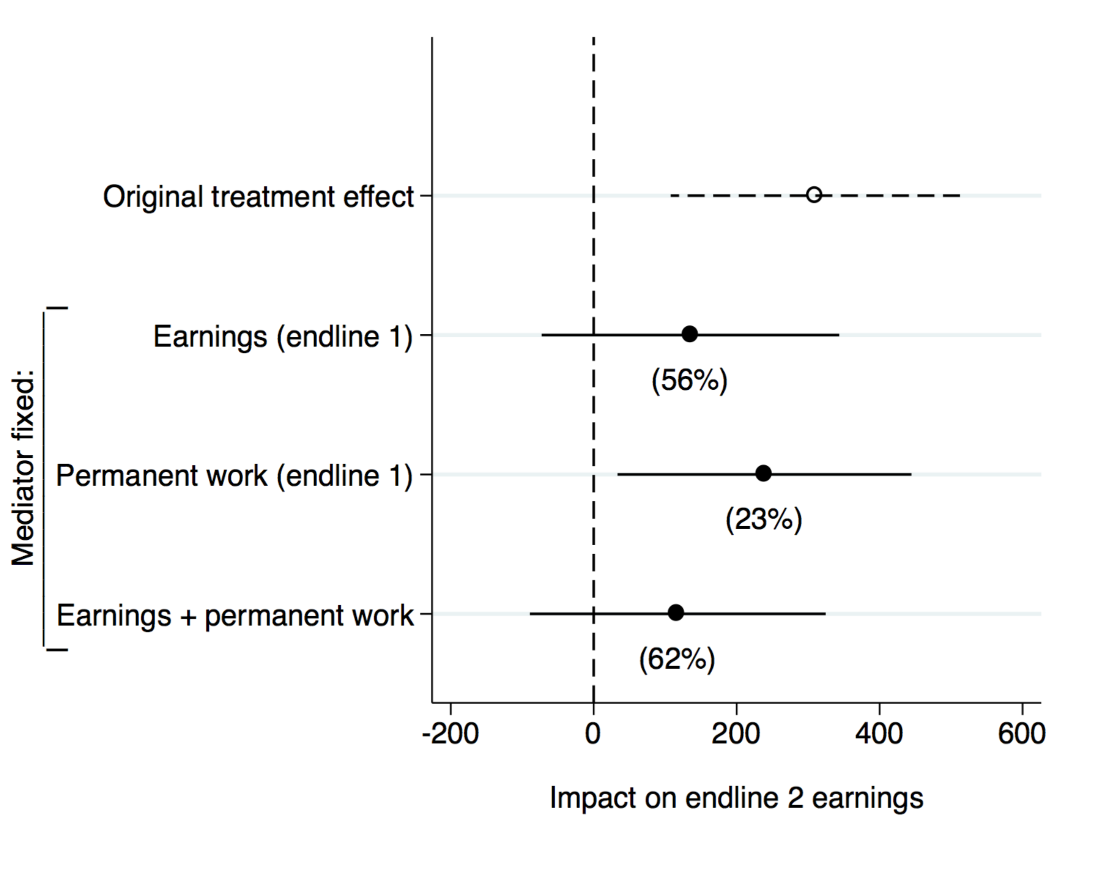
Figure 2: **Fortnightly impacts of the job application workshop on job search**

(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

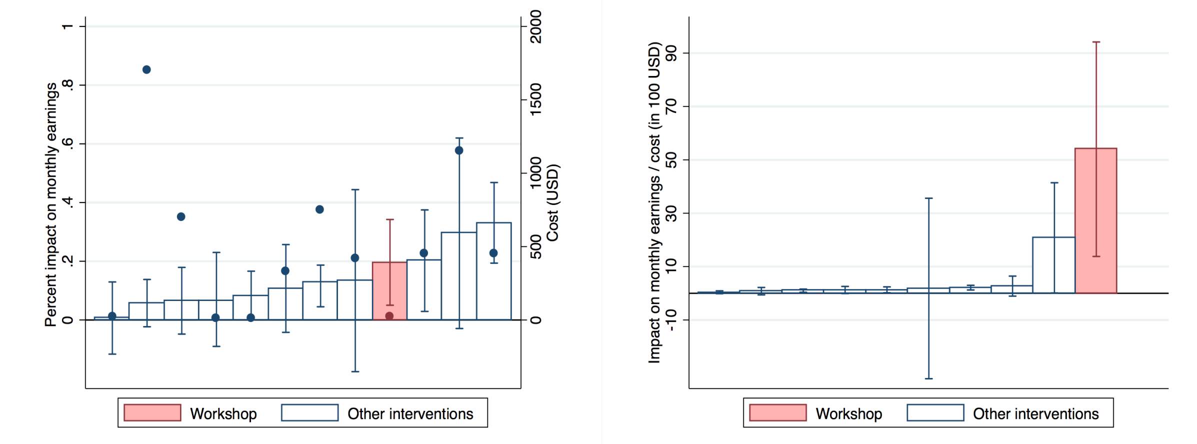


Table 1: **Treatment Assignment**

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

Table 2: **Impacts on Employment outcomes**

	2015				2018			
Outcome	Control mean (1)	Transport (2)	Workshop (3)	Equality (pval) (4)	Control mean (5)	Transport (6)	Workshop (7)	Equality (pval) (8)
Work	0.562	0.037	0.021	0.57	0.693	-0.058*	0.029	0.00
		(0.029)	(0.031)			(0.035)	(0.032)	
		[0.366]	[0.674]			[0.411]	[0.958]	
Hours worked	26.18	0.183	-0.214	0.79	28.25	-2.499*	0.218	0.04
		(1.543)	(1.533)			(1.486)	(1.426)	
		[0.917]	[1.000]			[0.411]	[1.000]	
Monthly earnings	1,145.0	11.0	76.8	0.39	1,531.5	30.9	299.5**	0.02
		(75.0)	(85.2)			(102.4)	(121.4)	
		[0.917]	[0.674]			[0.753]	[0.096]	
Permanent job	0.171	0.033*	0.069***	0.09	0.307	-0.034	-0.010	0.30
		(0.018)	(0.019)			(0.025)	(0.028)	
		[0.215]	[0.004]			[0.411]	[1.000]	
Formal job	0.224	0.054***	0.053***	0.95	0.318	-0.005	-0.007	0.96
		(0.019)	(0.020)			(0.030)	(0.030)	
		[0.032]	[0.021]			[0.753]	[1.000]	
Job satisfaction	0.237	-0.001	0.022	0.45	0.575	-0.025	0.066*	0.01
		(0.027)	(0.027)			(0.037)	(0.036)	
		[0.917]	[0.674]			[0.593]	[0.219]	

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on primary employment outcomes. These are obtained by 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). 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 3: Impacts on Job Tenure and Conditional Earnings

Outcome	Control mean	N	ITT Estimates: Transport Coeff	Transport Std. Err.	Workshop Coeff	Workshop Std. Err.	Equality pval
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Longest tenure (months)	11.845	1,739	0.070	0.579	1.043	0.632	0.086
Current job tenure (months)	1.232	2,016	-0.076	0.079	0.018	0.080	0.246
Promoted in current job	0.132	2,016	0.011	0.016	0.012	0.017	0.972
Earnings conditional on working	2,209.307	1,383	205.285	141.476	386.291**	162.443	0.288

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 earnings by baseline characteristics**

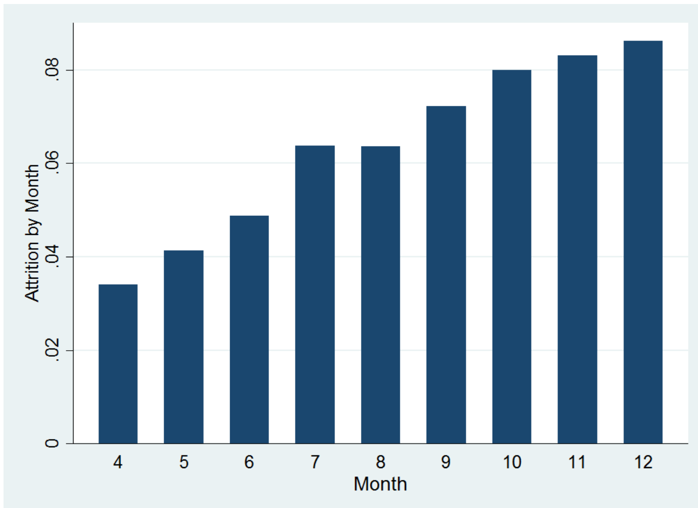
	Covariate = 0			Covariate = 1			Transport	Workshop
Baseline covariate	Control mean (1)	Transport (2)	Workshop (3)	Control mean (4)	Transport (5)	Workshop (6)	Equality (pval) (7)	Equality (pval) (8)
Above high school	826.4	15.1	470.9**	1,835.1	54.2	37.3	0.83	0.07
		(124.4)	(188.1)		(159.9)	(149.8)		
		[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.

For Online Publication

A.1 Additional Figures and Tables

Figure A.1: **Attrition rate from the phone survey by month**



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

Figure A.2: **Impact trajectories: Employment**

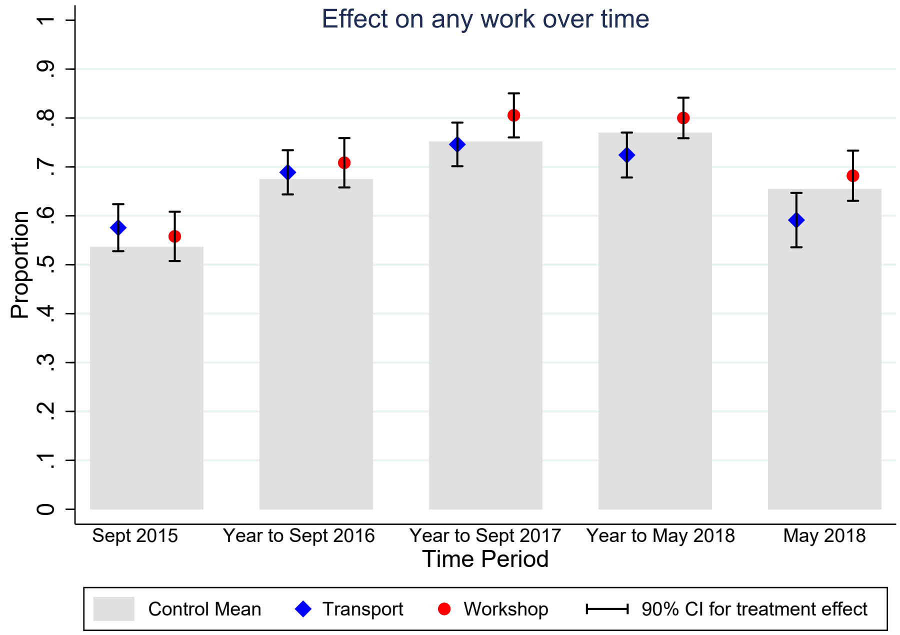


Figure A.3: **Impact trajectories: Permanent employment**

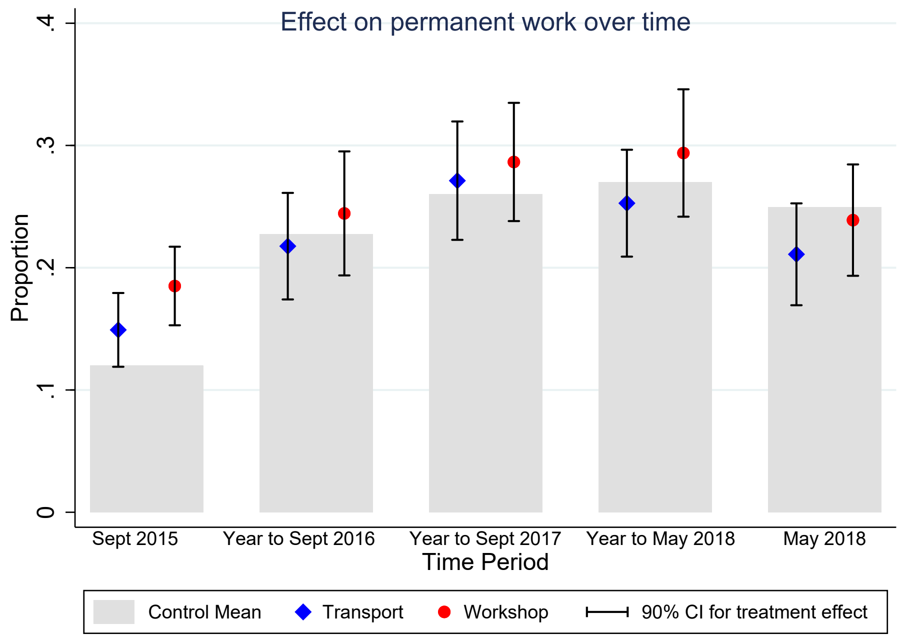
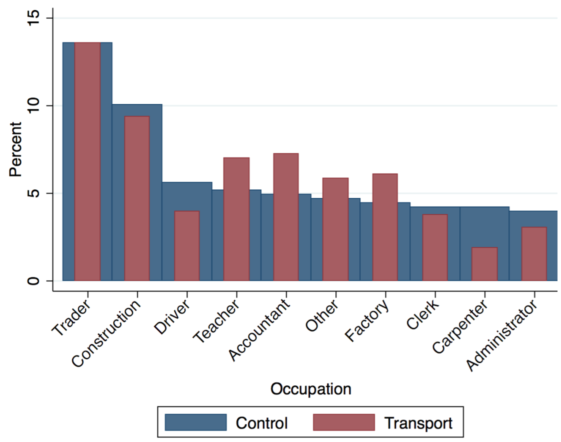


Figure A.4: **Most common occupations**

(a) **Transport Subsidy**



(b) **Job Application Workshop**

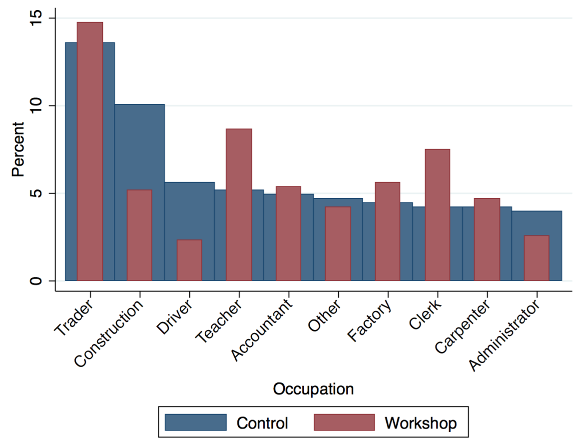
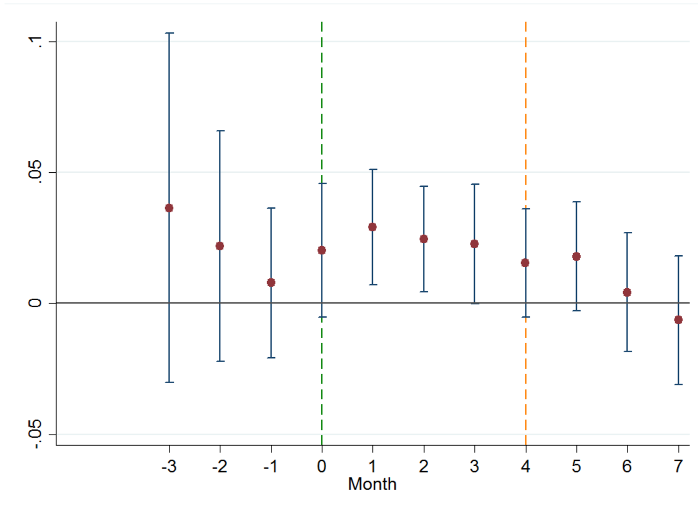


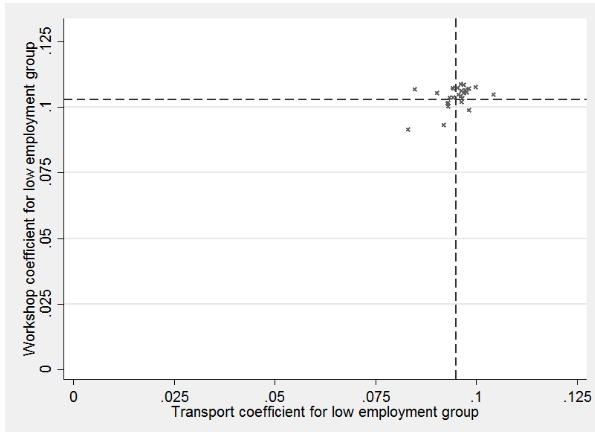
Figure A.5: **Impact trajectory of the transport treatment: Travelled to city centre**



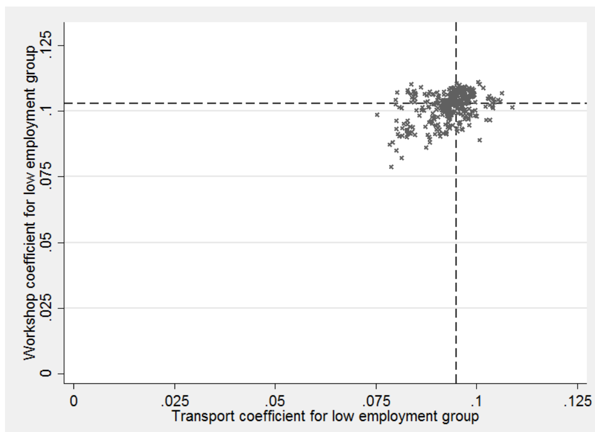
The green dotted line indicates the month when the treatment begins.
The orange dotted line indicates the month when the treatment ends.

Figure A.6: **Robustness of stratification to predictor covariates**

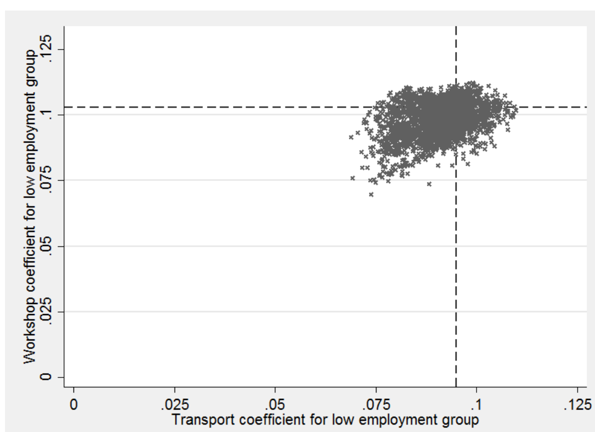
(a) Effects of dropping each of the 26 predictor covariates



(b) Effects of dropping each pair of the 26 predictor covariates



(c) Effects of dropping each triple of the 26 predictor covariates



Each figure repeats the estimation in column 1 of Table **??**; in each case, we show the estimated effects for individuals with low predicted probability of employment. In sub-figure (a), we drop one of the 26 predictors in each estimation; we therefore have 26 points. Sub-figure (b) shows coefficients when we drop each of the ${}^{26}\mathcal{C}_2 = 325$ pairs of predictors. Sub-figure (c) shows coefficients when we drop each of the ${}^{26}\mathcal{C}_3 = 2600$ triples of predictors. In each figure, the dotted lines show the original coefficient pair: $(0.095, 0.103)$.

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**

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)		

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: 2015 survey**

Transport	-0.005	Born outside Addis	0.040***
	(0.017)		(0.014)
Screening	-0.023	Degree	-0.034***
	(0.017)		(0.012)
Spillover transport	-0.010	Years since school	0.000
	(0.019)		(0.000)
Spillover screening	-0.014	Worked (last 7 days)	-0.044***
	(0.026)		(0.015)
Female	0.022*	Searched for work (last 7 days)	0.021
	(0.013)		(0.016)
Age	-0.000	Work frequency (before treatment)	-0.004
	(0.002)		(0.018)
Married	-0.028	Search frequency (before treatment)	-0.064**
	(0.018)		(0.026)
Lives with parents	-0.004	Wage work (last 6 months)	0.011
	(0.014)		(0.015)
Amhara	-0.024	Searched for work (last 6 months)	-0.007
	(0.015)		(0.018)
Oromo	-0.026	Ever had permanent job	0.003
	(0.017)		(0.018)
Observations	3,045	R-squared	0.021
F statistic (treatments)	0.560	F statistic (covariates)	2.680
Prob $> F$	0.690	Prob $> F$	0.000

Table A.9: **Predictors of attrition: 2018 survey**

Dependent Variable: No-response or refused	Only Treatment		All Covariates	
	Coeff	Std. error	Coeff	Std. error
	(1)	(2)	(3)	(4)
Transport	-0.007	0.021	-0.008	0.021
Workshop	-0.035	0.020*	-0.037	0.020*
Search intensity (baseline)			-0.010	0.023
Degree			0.001	0.019
Worked (7d)			-0.002	0.020
Searched job (7d)			-0.002	0.019
Female			0.038	0.016**
Respondent age			-0.003	0.003
Born outside Addis			0.027	0.018
Amhara			-0.012	0.020
Oromo			-0.032	0.020
Wage empl (6m)			-0.008	0.017
Married			-0.043	0.024*
Years since school			-0.000	0.000
Lives with parents			-0.018	0.020
Ever had permanent job			0.037	0.025
Searched job (6m)			0.026	0.020
P-value of F-test	0.1567		0.0066	
N	2,365		2,365	

Table A.10: **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.11: **Family indices**

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

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.12: **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.13: **Financial outcomes**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Expenditure (7d)	28.54 (39.377) [1]	18.18 (38.661) [1]	-7.868 (39.758) [1]	-59.19 (41.197) [.826]	474.4	0.797	2841
Savings (total)	352.4 (2726.672) [1]	-969.6 (1350.114) [1]	-486.9 (1432.001) [1]	63.68 (1619.663) [1]	5803	0.603	1259
	0.467 (.549) [1]	0.195 (.488) [1]	0.432 (.633) [1]	0.483 (.784) [1]	-1.055	0.605	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 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.14: **Expectations, aspirations, reservation wages**

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

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.15: **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.16: **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.17: **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.18: **Social networks**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
No. people with whom regularly shares info about job opport.	-0.347 (.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.19: **Job search**

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

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by 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.20: **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.21: **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. ***$p< 0.01$, **$p<0.05$, *$p<0.1$.

Table A.22: **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.23: **Heterogeneous effects on 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)
Born in Addis Ababa	1,524.5	-206.8	136.0	1,535.4	175.7	395.9**	0.08	0.31
		(154.8)	(183.7)		(141.2)	(168.8)		
		[1.000]	[0.114]		[1.000]	[0.160]		
Uses CV/Certificates	1,266.2	-4.0	307.8**	2,231.1	178.3	252.8	0.48	0.86
		(110.3)	(136.6)		(238.5)	(284.8)		
		[1.000]	[0.036]		[1.000]	[0.527]		
Present bias	1,548.9	87.8	456.8***	1,656.5	-83.1	-141.2	0.65	0.07
		(115.5)	(147.8)		(358.4)	(289.3)		
		[1.000]	[0.022]		[1.000]	[0.643]		
Job Search Network	1,347.3	102.3	266.8*	1,705.8	-25.7	347.0*	0.56	0.76
		(132.0)	(143.2)		(166.4)	(209.2)		
		[1.000]	[0.049]		[1.000]	[0.301]		

Note.

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

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 (.029)**	0.0370 (.033)	0.00700 (.046)	0.536	0.0690 (.029)**	0.0350 (.030)	-0.00200 (.045)	0.401	0.108	0.216	0.370	2841