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Abstract

Do obstacles to job search contribute to labour market exclusion in developing countries? To answer this question, we contrast two very different interventions, designed to alleviate spatial and informational constraints for unemployed youth in a congested African city: a transport subsidy and a job-application workshop. Both treatments have large positive effects on the probability of finding stable and formal jobs. Neither treatment has a significant average effect on the overall probability of employment, but we detect a sizeable increase in earnings and employment rates among the most disadvantaged job-seekers. Our results highlight the importance of job-search constraints as mechanisms for exclusion of the most disadvantaged. They also show that, if targeted well, low-cost interventions can have large impacts, improving equity in the labour market.

JEL Classifications: O18; J22; J24; J61; J64; M53

1 An experiment in labour market inclusion

The labour market is the primary mechanism to distribute the gains from economic growth. This is especially true in developing countries, where weak social insurance and limited access to capital leave many workers entirely dependent on their own labour. Those who struggle to find good jobs therefore also struggle to share in the gains from growth. This problem is particularly acute for young people in congested cities, who can easily find themselves trapped in low-quality jobs that are precarious and largely informal. In large cities, many job-seekers are geographically distant from potential employers and lack affordable options to travel to opportunity. As urbanisation accelerates, these concerns will become ever more pressing.[1] Job-seekers are typically also anonymous to potential employers, with little formal work experience and no effective way of signalling their ability. Reducing these search frictions can improve workers' employment prospects, increase their welfare and psychological well-being (Krueger and Mueller, 2012), and help them share more equitably in the gains from growth. Yet, little is known about the precise mechanisms that drive labour market exclusion in developing countries, and little is known about how policy can help excluded youth to find good jobs (Kluve et al., 2016; McKenzie, 2017).

In this paper, we document the labour market effects of two interventions that reduce search frictions for young workers. We find large and highly significant impacts of both treatments on job quality (that is, job stability and job formality), and we find significant impacts on employment and earnings among workers who are most disadvantaged. Both interventions are designed to ease separately the spatial and informational constraints to job search identified in the literature. The first intervention is a *transport subsidy*. Job search in our study area requires regular trips to the centre of town and we calibrate the subsidy amount to cover the cost of this journey. Participants can collect the subsidy from an office located in the centre of the city, up to three times a week. The second intervention is a *job application workshop*. Participants are offered orientation on how to make effective job applications using CVs and cover letters, and on how to approach job interviews. Further, we certify their general skills using a mix of standardised personnel selection tests. In this way, both interventions are specifically geared to help job seekers to find formal and stable work.[2] We evaluate these programs using a large sample of over 3,000 young people (with

[1] By 2050, the urban population of Africa is expected to triple, while that of Asia is expected to grow by 61 percent (United Nations, 2014).

[2] Temporary and informal work is usually found through more informal methods, like social connections. Visits to job vacancy boards, and the use of certificates and CVs, are less likely to be useful in the search for these informal jobs.

very low attrition throught the study) in Addis Ababa, Ethiopia.[3] This location is ideal for our purpose: a rapidly expanding metropolis with a large labour market where informality and precarious employment are widespread, especially among the youth.[4] Indeed, we have relatively high take-up of both treatments: 50% of those assigned to the transport subsidy collect their payment at least once, while workshop attendance was 61%. Our interventions are relatively inexpensive. This is in line with existing evidence showing that job search programmes are typically cheaper than training, wage subsidy and unconditional transfer programmes (McKenzie, 2017). The marginal cost of offering the treatment to one individual is about US$19.80 for the transport intervention and US$18.20 for the workshop.

Despite finding large positive effects on job quality, we find no average treatment effects on the overall probability of having a job. This is consistent with a body of recent evidence showing relatively small impacts of active labour market programmes on overall employment (Crépon and van den Berg, 2016; McKenzie, 2017). McKenzie (2017) reviews nine recent papers on search and matching interventions in developing countries (including the two interventions in this paper, and two from other studies in the same context), and concludes that "traditional active labor market programs have had at most modest impacts on employment in most cases". This suggests that, for the *average* job-seeker, labour markets in urban developing countries work relatively well — at least for allocating casual and temporary work. Our results support this conclusion: our respondents generally find it relatively easy to obtain casual and temporary work — indeed, in our sample, 80% of respondents without permanent employment do at least one week of temporary work over the course of a year. However, the ready availability of casual and temporary work does not imply that labour markets work well for every type of job, nor for every type of job-seeker.

Our results extend the current literature on active labour market programmes by highlighting the central importance of two dimensions of heterogeneity: heterogeneity in job quality and heterogeneity in job-seekers. First, while precarious work may be relatively easy to find, stable formal work is a highly desired but largely unattainable goal for many workers, especially in countries with deeply segmented labour markets like Ethiopia.[5] In

[3] 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.

[4] In Addis Ababa, young people below the age of 30 are 40 percent less likely to be in a stable, permanent job compared to older workers (authors' calculations using data from CSA (2014)).

[5] A long literature emphasises the importance of labour market segmentation in developing countries (for example, see Fields (1975))) — showing that the market for casual employment (often in the informal sector) can be relatively frictionless, while obtaining more secure employment can be very difficult. A dualism with similar features is also often observed in the labour markets of developed economies. For example, many European labour markets are regulated by two-tier systems of employment protection (Boeri, 2010; Blanchard and Landier, 2002). In these markets, stable jobs are scarce and hard to secure for youth and disadvantaged groups.

this paper, we place substantial focus on such segmentation — in the sense of whether jobs are formal (*i.e.* provided with a written contract) and permanent (*i.e.* not having a specified end date). Lack of job formality and lack of job tenure are fundamentally important for driving employment volatility in Ethiopia — so that employment security is listed very high among Ethiopian job-seekers' desired job attributes. For this reason, we listed work permanence and work formality among the six primary outcomes of interest in our pre-analysis plan.[6] Eight months after the end of the programme, individuals invited to our job application workshop are nearly 60 percent more likely to have permanent employment and 31 percent more likely to be in formal employment (compared to individuals in the control group). This reduces by more than 20 percent the gap in permanent employment between youth and older workers. Those who are offered the transport subsidy are 32 percent more likely to be in formal employment. The results are statistically significant, robust to a correction for multiple comparisons, and economically meaningful.

Second, we consider how the effects labour market interventions differ by pre-specified characteristics of the job-seeker. We find that — using an omnibus test across a range of covariate splits — positive effects on job quality are concentrated among groups who typically find it harder to obtain high-quality employment. To explore this heterogeneity, we conduct an 'endogenous stratification' exercise (Abadie et al., 2017), in which we split our sample by the predicted probability of endline employment. Among those with a low predicted probability of endline employment — that is, a subsample facing greate facing the greatest risk of exclusion from the labour market — we find large and significant treatment effects on the probability of employment, the probability of formal and of permanent employment, and on total earnings. We argue that active labour market programmes can help to address problems of labour market segmentation, particularly for groups who might otherwise be at greater risk of labour market exclusion. This conclusion also explains why we do not find a significant average effect on employment across our sample: the ready availability of temporary work serves partially to offset the impacts of increased permanent work on overall employment.

To study the mechanisms underlying our treatment impacts, we run high-frequency phone interviews. High-frequency data allows us to disentangle the effects of the two interventions on three steps in the job search process: gathering information about vacancies, making applications to positions, and receiving formal job offers. We find that, despite having similar average treatment effects, our two different interventions operate through quite different mechanisms. Specifically, the transport subsidy allows job-seekers to search more intensely, *i.e.* they take more active steps to seek information about job vacancies, particularly through formal methods that require trips to the centre of town. Both interventions,

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

on the other hand, improve job-seekers' search efficacy (which we define as the rate at which applicants convert job applications into offers for permanent jobs) — a result driven by the least educated. Neither intervention significantly increases the number of applications submitted. We interpret these results as indicating that the transport subsidy allows job-seekers to draw on a larger pool of information and therefore improves their ability to select vacancies for which they are better suited, whereas the workshop improves the quality of applicants' signals. This interpretation is corroborated by the evidence we draw from the skill tests administered to respondents, which show that our workshop strengthens the correlation between skills and the probability of permanent work for job-seekers with less formal qualifications. In other words, our treatment helps the market sort talent in the absence of clearer signals.

Finally, we measure the indirect impacts of the interventions on the young individuals who reside close to programme participants. To do this, we use a saturation design (Crépon et al., 2013), in which we randomly vary the proportion of transport recipients in treated clusters. We also interview control respondents in clusters receiving the workshop intervention. We do not find local spill-over effects from either intervention.

What, then, does this paper add to the recent literature on active labour market programmes in developing economies? We join the growing consensus that, on average, such policies are unlikely to have large effects on whether or not respondents have a job (McKenzie, 2017). However, we nuance that conclusion by emphasising large potential gains from programmes that are carefully targeted — particularly for helping job-seekers to access permanent employment in a segmented labour market, and particularly for assisting job-seekers with low labour market attachment. Ours is the first study to contrast two very different types of active labour market policies and to provide an in-depth analysis of the different constraints that such policies relax.

In this way, our work builds on the results of Franklin (2017) — who, in an exploratory trial of the transport subsidy intervention, finds positive short-run treatment effects on young job-seekers in Addis Ababa. Similarly, we build on the experimental work of Phillips (2014) in Washington, DC, who finds short-run effects of transport subsidies on search intensity. We extend their work in two fundamental ways. First, and most importantly, both Franklin (2017) and Phillips (2014) have relatively small samples of very active job-seekers (that is, respondents chosen because of their active search behaviour). By drawing a large representative sample of unemployed youth — and, in particular, by including respondents who are not currently searching for work — we are able to speak more generally to the relationship between active labour market programmes and labour market exclusion. As our earlier discussion shows, this is fundamentally important to our emphasis on heterogeneity among job-seekers; indeed, in contrast to earlier work, our large sample size allows us to conduct detailed sub-group analysis. Second, by running our transport intervention in parallel with our job-application workshops, we are uniquely positioned to compare directly the labour market consequences of different constraints — of distance and of anonymity — using the same sample population, at the same point in time. We provide a rich discussion of the mechanisms through which such constraints operate different stages in the matching process — namely, the distribution of information about vacancies and the process of signalling and screening for jobs — and show how particular workers can be disadvantaged in two steps of this process.

Our study is also related to recent work on matching of job-seekers and vacancies in developing countries. Recent work by Groh et al. (2015) finds that a matching intervention based on information about workers' skills does not improve the employment outcomes of young educated Jordanian women. Similarly, Beam (2016) finds that job fairs in the rural Philippines do not improve employment with matched firms.[7] In line with these results, we do not find aggregate employment effects through improved market access. In a companion field experiment, we run a job fair with an additional sample drawn from the same population (Abebe et al., 2017); consistent with the results in this paper, and with results in Groh et al. (2015) and Beam (2016), that job fair generates very few successful matches, and has no detectable effect on total employment.[8] The current paper helps to explain why such matching interventions are unlikely to have large employment effects. Results from our transport intervention suggest a key role for job-seekers' discretion in targeting appropriate job vacancies; workers may have private information about their suitability for jobs that allows them to quickly eliminate thousands of irrelevant vacancies. Therefore, interventions such as job fairs and direct matching may be ineffective, because they expose job-seekers to a broad set of employers without leveraging this private information. Results from our workshop intervention highlight the central importance of high quality signals, for which face-to-face meetings are unlikely to be a sufficient substitute.[9]

A related literature studies interventions that facilitate migration and connect rural workers to urban jobs (Jensen et al., 2012; Bryan et al., 2014). These interventions work partly by

[7] Beam (2016) finds significant effects through a separate margin: namely, through formal sector employment and through job search outside the region.

[8] We designed the job fair intervention at the same time as the transport subsidy and the job-application workshop. As specified in our pre-analysis plan, however, we anticipated that we would report the results in different papers. We separate the job fairs analysis into a companion paper in order to focus the current analysis upon job-seeker behaviour, and to analyse more carefully the role of firm hiring in the second paper. The analysis in the current paper simply drops respondents who were assigned to the job fairs. For the sake of consistency with the pre-analysis plan, however, we repeated the estimation over the full sample and found no difference in our conclusions.

[9] This also relates to a recent literature on 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, Belot et al. (2015) improve search efficacy through expanded job suggestions in an online market, and Pallais (2014) uses reference letters to certify worker performance and skills.

changing rural workers' expectations about low-skilled employment opportunities available in nearby cities. Our work has a novel focus since the urban job-seekers in our sample face a different set of constraints than those investigated in this literature. On the one hand, they have greater awareness about existing job opportunities within the city. On the other hand, high competition for good urban jobs makes it particularly important to be able to signal one's skills, and to search intensively and widely.

More generally, the evidence we present points to the presence of significant search frictions in segments of the labour market, and suggests that policies that decrease these frictions have the potential to improve efficiency. Our results suggest that spatial frictions affect the allocation of labour within cities, extending a recent literature that studies how similar frictions affect the structural transformation of the rural economy (Gollin and Rogerson, 2015; Bryan and Morten, 2015; Asher and Novosad, 2015). We also contribute to a growing literature that studies the economic importance of cognitive and non-cognitive skills (Bowles et al., 2001; Heckman et al., 2006; Groh et al., 2012; Hoffman et al., 2015; Abel et al., 2016; Bassi and Nansamba, 2017). We show that these skills are not always accurately perceived by firms, suggesting that informational frictions can dampen the incentives for the optimal allocation of human capital.

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).[10] 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).

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.[11] 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

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

[11] 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.

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.[12] 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 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.[13] 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.[14] 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

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

[13] 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.

[14] "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).

have a formal job than a worker who has only completed high school. So what obstacles prevent such disadvantaged workers from achieving better labour market outcomes?

2.2 Constraints on job search

Job search is costly. One of the most popular search methods used by the participants in our study is to visit job vacancy boards.[15] 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 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.[16]

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.[17] 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

[15] 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.

[16] 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.

[17] 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.

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 contrast 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.[18] Recipients are required to attend in person, and to show photographic ID on each visit. Each recipient can collect cash once a day, up to three times a week. The daily amount is sufficient to cover the cost of a return bus fare from the participant's area of residence at baseline to the disbursement point.[19] 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.[20]

Prior to the intervention, respondents in our sample do not travel frequently to the city centre.[21] 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.[22] 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.[23]

[18] 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.

[19] 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.

[20] 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.

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

[22] 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.

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

The median subsidy available on a given day is equal to 20 Ethiopian Birr (1 USD at the exchange rate at the beginning of the intervention). This equals about two thirds of the median weekly expenditure on job search at baseline, and 10 percent of overall weekly expenditure. The minimum amount is 15 ETB (0.75 USD) and the maximum 30 ETB (1.5 USD). 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.[24] The intervention was implemented between September 2014 and January 2015.

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.[25]

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

[24] 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.

[25] 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.

sometimes administered, by the School of Commerce.[26]

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.[27] The certificates are officially issued by the School of Commerce and the Ethiopian Development Research Institute.[28]

We chose the tests on the basis of the results of several qualitative interviews with firm managers in the city.[29] 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.[30]

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).

[26] 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.

[27] 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%.

[28] 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.

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

[30] 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.

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.[31] 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 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.[32]

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).[33] 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.

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

[32] 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.

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

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 present bias.[34] We did not inform study participants at baseline that some of them would be offered job search assistance.

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.[35]

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).[36]

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

[34] 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.

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

[36] 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.

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 outlined in Table 1.[37]

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.[38] Table A.8 in the online appendix presents the full analysis.[39] 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.

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

[37] 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.
[38] 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.
[39] 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.

are invited to the job application workshop attend it. 80% of those attending later collect the certificates from the School of Commerce. Take-up rates do not vary substantially with observable covariates.[40]

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, we estimate the following equation:

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

where y_{ic} is the endline outcome for individual i in cluster c and $\boldsymbol{x}_{ic0}$ is the vector of baseline covariate values that were used for re-randomisation and blocking. $\texttt{treat}_{fic}$ is a dummy capturing whether an individual has been offered treatment f. Thus, our estimates measure the *intent-to-treat* impacts of the interventions. The variable $\texttt{spillover}_{fic}$ is a dummy that identifies control individuals residing in clusters assigned to treatment f. Thus, γ_f captures the 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.[41]

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

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

[41] 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.[42] 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, 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.[43] d_{wit} is a dummy variable equal to 1 in period t if an individual started receiving their treatment w periods ago.[44] 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.[45]

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}:

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

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

[42] 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.

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

[44] 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.

[45] 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 Average impacts

Table 2 reports the main impacts on our pre-specified family of six primary outcomes.[46] We find no average treatment effects on the probability of having a job, on hours worked, on earnings or on job satisfaction. Specifically, we estimate that the interventions have only modest average effects on the overall employment rate of treated individuals. This increases 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). As noted earlier, this is highly consistent with recent evidence on active labour market programmes in developing countries, showing that "traditional active labor market programs have had at most modest impacts on employment in most cases" (McKenzie (2017); see also Crépon and van den Berg (2016), Groh et al. (2012) and Jensen et al. (2012)).

However, Table 2 also reveals a striking and novel 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 boosts permanent employment by nearly 30 percent (a 3.3 percentage point increase from the control level). This effect is significant at the 10 percent level, but has a q-value of 0.20 once we account for multiple comparisons. 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. 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.

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

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.[47]

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.15 to A.22 of the empirical appendix).[48] 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.[49]

4.2 Who benefits the most from the interventions?

Sub-group analyses: In this section, we conduct a series of sub-group analyses, to test for heterogeneous treatment effects. Our Pre-Analysis Plan specified a set of covariates for heterogeneity tests; for each dimension of heterogeneity, we run the following specification:

$$\begin{aligned} y_{ic} = \sum_{v=1}^{m} \left[\beta_{0v} + \sum_{f} \left(\beta_{vf} \cdot \texttt{treat}_{fic} + \gamma_{vf} \cdot \texttt{spillover}_{fic} \right) \right] \cdot I\left(x_{ic,pre} = v\right) \\ + \alpha \cdot y_{ic,pre} + \boldsymbol{\delta} \cdot \boldsymbol{x}_{ic0} + \mu_{ic}, \end{aligned} \tag{5}$$

where $x_{ic,pre}$ is a categorical variable with values $1, ..., m$ corresponding to the m groups of interest (*e.g.* male and female in the case of gender), and $I\left(x_{ic,pre} = v\right)$ is a indicator variable that takes the value of 1 when $x_{ic,pre}$ is equal to v. The coefficients β_{vf} estimate the effect of treatment f for group v. As before, we calculate adjusted standard errors that correct for multiple hypotheses within our main family of six employment outcomes, for each dimension of heterogeneity tested. Table A.14 in the Appendix shows this excercise

[47] 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.19).

[48] 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.15 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.

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

for heterogeneity by education, for instance.[50]

Given our results in Section 4.1, we focus our attention on the probability of having a permanent job, and of having a formal job, at endline. Table 3 shows our results for permanent work across all of the main dimensions of heterogeneity in our pre-analysis plan; Table A.11 in the Online Appendix repeats the exercise for formal work. In each case, we order our covariate categories such that the first category listed refers to what one might generally think of as the 'more disadvantaged' group — that is, respondents with lower savings, respondents having never had a permanent job, respondents with low search intensity, women, respondents born outside of Addis, respondents living further from the city centre, respondents having never used CVs/certificates in job search, respondents with a smaller job search network, respondents exhibiting present bias and respondents exhibiting present bias without anticipating revision. Similarly, we order our three education categories in increasing order (high school, then vocational, then diploma/degree), and interpret lower education levels as defining more disadvantaged subsamples.

We report separate hypothesis tests for treatment equality across treatments. For the probability of permanent work, we find significant heterogeneity of the transport treatment by educational level (where treatment effects are concentrated among those having only a high school education) and by gender (where effects are concentrated among women). For the workshop treatment, we find significant heterogeneity in effects by our incentivised measure of present bias (effects are concentrated among the present biased), and by distance (where we find larger effects for those living closer to the city centre). For the probability of formal work, we find significant heterogeneity only for the effect of the workshops by gender (where effects are, again, concentrated among women).

Individual differences between estimated treatment effects are generally not large — and, therefore, we do not detect many differences in these separate hypothesis tests. However, estimated treatment effects are generally larger for the 'more disadvantaged' subsample than for the 'less disadvantaged'. Across Table 3 and Table A.11, we have a total of 48 pairwise comparisons.[51] We can use these 48 pairwise comparisons to perform an omnibus non-parametric sign test of whether, in general, our treatments were more likely to help disadvantaged subgroups. We find that the more disadvantaged subgroup has a

[50] Tables A.23 and A.24 do the same for job search intensity at baseline, and work experience at baseline, respectively.

[51] That is, 12 comparisons for each treatment, and comparisons both in the probability of having a permanent job and of having a formal job. For education, we make a pairwise comparison between 'high' and 'vocational', then a second comparison between 'vocational' and 'diploma/degree'.

larger coefficient in 35 cases; this implies a p-value of 0.012 for the null hypothesis that the treatments benefit more disadvantaged and less disadvantaged groups equally.[52]

This sign test — and the following stratification exercise — should be seen as exploratory, in the sense that they are prompted by results from our pre-specified hypotheses, and seek to generalise the insights generated from the regressions (see Olken (2015)). Nonetheless, we interpret this result as showing that, in general, our treatments tended to favour the inclusion of subgroups who are traditionally disadvantaged in the labour market.

A stratification exercise: To further formalise and test this claim — and to synthesise across our multiple different pre-specified dimensions — we run a stratification exercise, following Abadie et al. (2017). To run this estimation, we stratify by predicted probability of endline employment. In a first stage, we use a logit estimation to predict endline employment using a large vector of baseline covariates. We report first-stage coefficients in Table A.10 in the Online Appendix; significant predictors of endline employment relate primarily to previous work experience. This generates a 'high-employment' group (having an endline employment rate of about 69% in the control group) and a 'low-employment' group (having an endline employment rate of about 47% in the control group). Respondents in the low-employment group have lower self-reported reservation wages, work for lower wages and more often work with temporary contracts; they also search less frequently, are less educated and have lower savings at endline.[53] These correlations provide a stark contrast with one narrative of urban labour markets in developing countries — namely, that urban youth unemployment largely comprises the middle-class queueing for good jobs. Rather, the correlations confirm that our low-employment group is relatively disadvantaged and relatively excluded.

We then use a 'split sample' method to estimate treatment heterogeneity between our high-employment and low-employment groups. Specifically, following Abadie et al. (2017), we randomly split our control group into a prediction sample and an estimation sample.

[52] We use a re-randomisation algorithm to obtain the distribution of this statistic under the null hypothesis that our treatments were no more likely to help a disadvantaged group (Young, 2017). This approach allows here for potential correlation between different pairwise tests. If we test separately for permanent employment and for formal employment, we respectively obtain p-values of 0.111 and 0.005. If we ignore the possibility of correlation between different pairwise tests, the test statistic has a binomial distribution: this would then imply that $p = 0.5^{48} \cdot \sum_{k=35}^{48} {}^{48}\mathcal{C}_k \approx 0.001$. If we use the binomial distribution to test separately for permanent employment and for formal employment, we respectively obtain p-values of approximately 0.076 and 0.003.

[53] We also test for treatment balance between high-employment and low-employment groups. We do this by regressing a dummy for the high-employment group on dummies for the transport and workshop treatments, using our probability weights and geographical clustering. The balance test passes comfortably: we obtain p-values of 0.97, 0.39 and 0.61 respectively for balance on transport, balance on workshops, and balance across both treatments jointly.

We use the former to estimate the coefficients of our first-stage logit model of endline employment; we then use the estimation sample, along with all respondents in the treatment groups, to estimate heterogeneous effects. We repeat this process and average our coefficients in order to obtain our split-sample estimates; we then nest the entire estimation in a bootstrap, allowing for geographic clustering.[54] We estimate for the same outcome variables reported in Table 2 earlier; for further context, we also test for effects on temporary employment, self-employment, and earnings if employed.

Table 4 shows our results. We find starkly different patterns between high-employment and low-employment groups. We find large and significant effects on total employment for the low-employment group (an increase in the employment rate of 10 percentage points from each treatment, on a control-group mean of 38%); this is significantly different to the high-employment group — where, as in the earlier estimations, we find no treatment effect. We find significant effects on both formal work and permanent work for the low-employment group; in the case of the workshop treatment, we formally reject a null of equal effects for formal work ($p = 0.048$) and are close to rejecting the same null for permanent work ($p = 0.145$). We find a significant increase in total earnings from the workshop treatment on the low-employment group; in general.[55]

These stratification results are robust to several alternative specifications. First, we check the robustness of our main estimates to using different subsets of predictor covariates; specifically, we drop each of the 26 predictor covariates in turn, then each of the 325 possible pairs of 26 covariates, then each of the 2600 possible triples of 26 covariates.[56] Our results are robust to these alternative specifications, as Figure A.20 shows. Second, we check that our conclusions are robust to using a leave-one-out estimator rather than the split-sample algorithm (results available on request). Finally, in Table A.12, we repeat the exercise stratifying by predicted probability of permanent employment (*i.e.* rather than by probability of any employment). Patterns remain broadly similar — in particular, we find that the effects on formal work and on permanent work are driven by the low-employment group. These results are also summarised in Figure A.5.

[54] Following Abadie et al. (2017), we use 100 replications for the inner loop and 1000 replications for the outer/bootstrap loop.

[55] We do not find individually significant effects on hours worked or on work satisfaction — though, for both of these outcome variables and both treatments, we find suggestive evidence that effects are larger for the low-employment group than for the high-employment group ($p = 0.104$ and $p = 0.034$ for hours worked; $p = 0.089$ and $p = 0.119$ for job satisfaction).

[56] Dropping more than three covariates at a time proved computationally infeasible — for example, ${}^{26}\mathcal{C}_4 = 14950$.

To understand these results, note that the impacts of the main interventions on overall employment can be entirely decomposed into the effects on permanent work, temporary work, and self-employment. The large and significant impacts on employment for the low-employment group seem to be driven almost entirely by impacts on permanent work. The coefficient on temporary work is very close to zero. By contrast, the impact of the transport workshop seems to be driven by an increase in temporary work, which is of a similar magnitude to the impact on permanent work (although the coefficient on temporary work is not significant). This suggests that the transport intervention seems to have been useful for finding temporary jobs as well as permanent ones, where the improved quality of applications was only useful for permanent jobs — for which such formal applications are required.

4.3 Indirect effects on the untreated

In this section, we study the outcomes of untreated job-seekers who live close to program participants. The benefits of the interventions can extend to this group if the young job-seekers who are offered the programs share information, job referrals or resources with friends and acquaintances in the same neighbourhood.[57] Some eligible respondents living in clusters assigned to treatment are not offered the program.[58] In Table 2 (columns 4 and 5) we find no significant difference between untreated individuals living in geographical clusters assigned to one of the two interventions and untreated individuals in pure control clusters.[59]

However, we find that this masks some evidence of spillovers that differ according to how many neighbours received the transport intervention. We also randomly vary the proportion of treated individuals in the clusters that receive the subsidies. This allows us

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

[58] The proportion of untreated individuals was fixed at 20% in the clusters that received the job application worshop and randomly varied between 10% and 80% across those that received the transport subsidy.

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

to run a regression of the form:

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

T_i identifies individuals who have been assigned to the transport treatment, while C_i identifies individuals who have not been assigned to the transport treatment.[60] S_{20c} is a dummy variable for individuals living in a cluster where 20% of individuals were offered the transport treatment. Thus, β_{20} captures the difference in outcomes between untreated individuals in these clusters and untreated individuals in clusters where nobody was treated. Further, γ_{20} measures the difference in outcomes between treated individuals in S_{20c} clusters and untreated individuals in untreated clusters.

We find that the indirect effects of the transport treatment depend on the level of saturation, shown in Table A.28 in the appendix.[61] We document a positive indirect effect on formal and permanent work among control individuals in clusters with 40 percent saturation. We also document that untreated individuals in clusters with 90 percent saturation are 5.6 percentage points less likely to be in permanent employment than individuals in pure control clusters.[62] They are not, however, less likely to be in formal employment.

5 Mechanisms: How did treated individuals get better jobs?

In this section, we explore the mechanisms through which treated individuals obtain better jobs, by studying high-frequency data on job search decisions over the course of the study. Before presenting our main empirical results, and to fix ideas, we build a stylised model of dynamic job search in discrete time. This is formally presented in the Appendix and discussed intuitively here. In this model, an individual searches for a permanent job with a formal labour contract. To obtain such a job, the individual must first search for vacancies, in order to identify suitable positions to which she or he can apply.[63] Searching for

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

[61] Similarly, Table A.29 shows the spillover effects of having treated neighbours on treated respondents, by level of randomized saturation. We find some suggestive evidence that the treatment effects on formal work are driven by clusters where the randomized saturation was higher (*i.e* where more people received the treatment), although we are unable to reject that the treatment effects are equal across levels of randomized saturation.

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

[63] A vacancy is suitable if the candidate fits the required qualifications and thus has a positive chance of getting the job. In this simple model, workers do not have preferences over different permanent jobs, and therefore no reservation wage.

information about such vacancies is costly. If a suitable vacancy is found, the job seeker applies for the job. Conditional on applying, the probability of getting the job depends on the precision of the signal that a job seeker provides. We assume that the likelihood of getting a job is strictly increasing in the quality of the signal.[64] In this simple set-up, there are three phases in the job search process: (i) searching for vacancies (search intensity), (ii) making applications, and (iii) converting applications into job offers on the basis of the quality of signals (search efficacy). Individuals choose their optimal search intensity and then apply for suitable jobs, while employers decide whether to make offers on the basis of the signals they receive.

How do we expect our two interventions to affect this process? The workshop aims to improve the quality of the signal, and therefore increase the probability of getting a job, conditional on applying. The transport intervention reduces the cost of searching for vacancies, therefore making it possible for job-seekers to see more job vacancies. This could increase the probability of finding a good job through different mechanisms. First, reduced transport costs increase the number of suitable and desirable jobs seen by the job-seeker, and, if the cost of filing an application is relatively low, this will lead to an increase in the number of applications made. Second, if job-seekers are constrained in the number of job applications they can make (for example, because filing an application entails additional costs), reduced transport costs allow for a more selective job search and increase the likelihood of converting a job application into an offer without increasing the number of applications made. Third, job search can increase the precision of the information job-seekers have about vacancies they already know about. As discussed in detail in the Appendix (Section A.1.2), this can reduce the number of applications that job seekers make conditional on their search effort, but increase the probability of each application being successful. Overall, the transport treatment can be expected to increase search efforts (gathering information about vacancies), but its impact on the number of applications made is, *a priori*, ambiguous.

Our simple framework captures in a parsimonious way a number of important features of the labour market for permanent formal employment. First, as dismissal costs are higher in this market, firms will screen candidates more carefully and may be reluctant to hire risky applicants (Lazear, 1998). This suggests that the precision of signals and the ability to target applications to suitable jobs are going to play a key role, which we reflect in the model. Further, the model makes the simplifying assumption that active search effort is directed only towards finding permanent, formal jobs. As such, search for temporary work is not modeled, and the interventions only improve the chance of finding these good jobs.

[64] Behind this assumption is the idea that each employer seeks a minimum quality threshold and that, with a less informative signal, an applicant with the required quality is less likely to demonstrate to the employer that they are above the required threshold.

This captures in a simple way the idea that in a segmented labour market frictions operate very differently in different sectors (see Section 2, as well as Fields (1975) and Blattman and Dercon (2016)). Further, it reflects features specific to our setting — for example, the fact that the vacancies advertised on the job boards are typically formal, stable positions that require applicants to undergo a substantive screening process.[65]

Finally, the framework suggests that treatment effects will be strongest for those groups that experience the largest changes in the probability of hearing about a vacancy and in the probability of receiving a job offer after making an application. The change in these probabilities can differ across groups because (i) some groups increase search effort more than others in response to the interventions, or (ii) different groups have differnet marginal returns to improvements in search effort and signal quality. In particular, since we assume that search effort and signal quality have diminishing returns, we expect the latter of these two effects to be stronger for individuals who, in the absence of treatment, would have low search intensity, low search efficacy, and hence low attachment to the labour market. The next sections investigate these hypotheses empirically, finding more evidence for the second of these channels.

5.1 Effects on job-search intensity

In order to investigate how the two interventions impact job-search intensity, we rely on the high-frequency data from phone interviews. This allows a detailed analysis of how job-seekers' search behaviour evolves over the course of the intervention. In particular, we estimate the fortnightly impact of each intervention on search intensity, using equation 2.

We find that the transport intervention increases the intensity of search. In the fortnights when the transport voucher is available, treated individuals are about 12.5 percent more likely to have taken any active step to find 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 search for a vacancy at the job boards – the place where good, 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

[65] There could be complementarities between search in the two sectors, such that increased search for permanent work or improved signals could also raise the probability of finding temporary work. For instance, the transport intervention, by facilitating travel to the city center, could expand the social networks of job-seekers. This, in turn, could provide expanded access to information about informal opportunities. In general, we are not able to reject the null hypothesis that the treatments had no impact on the probability of finding temporary or informal work.

centre of the city for a number of months while the subsidies are in place (see Figure A.19).[66] These findings help to explain why the increase in search intensity translates into the effects on permanent and 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). This is notable and consistent with the hypothesis that financial constraints prevent job-seekers from incresing search effort: if the workshop did have a positive impact on the motivation of job-seekers, this was not enough to induce them to search more intensively.[67]

We note that more frequent search activity among respondents in the transport group does not necessarily translate in a higher number of job applications. This result, together with the findings on search efficacy that we present next, is consistent with the hypothesis that increased search efforts allow employees to be more selective about the applications they make. We present this result, as well as impacts on a range of job search indicators identified by our pre-analysis plan, in the the online appendix (Table A.26).

5.2 Effects on search efficacy

To test the effects of the two treatments on search efficacy, we estimate their impact on the ratio of offers received to applications made. This information is available in the endline survey (in reference to the past 12 months). The results reveal a significant impact of the workshop on the ratio of offers to applications for permanent jobs. Control individuals receive on average an offer for a permanent job every 7.2 applications. The workshop brings this down to one offer every 5.2 applications (see Table A.26 in the online Appendix).[68] The effect is more pronounced among the lowest-educated workers (those who drive the treatment effects documented in the previous section). Control individuals with only a high school degree receive an offer for a permanent job every 10.5 applications. Our two interventions bring this down to about one offer every 4.6 applications, a very large impact (see Table A.27 in the appendix).

[66] 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 sought jobs that did not require long commutes.

[67] We find no impacts on other meaures and methods of job search.

[68] In these regressions the dependent variable is the ratio of job offers to job applications. This ratio is not defined when a subject makes zero job applications. It is important to note that the probability that a subject makes zero job applications is not statistically different across experimental groups, which rules out potential concerns about sample selection.

Given that neither treatment has a significant impact on the total number of applications made, this suggests that the workshop increased the *quality* of applications. The transport treatment, on the other hand, is likely to have worked by increasing the pool of vacancies from which job seekers could choose and hence boosting the likelihood of a good match. This is consistent with the mechanisms hypothesised: by allowing workers to search more broadly and target their applications more effectively (the transport subsidy) and by increasing the quality of their signals (the workshop), the interventions increase the chances that each application results in an offer.[69] Crucially, this suggests that scaled up versions of these programmes are unlikely to increase congestion in the number of applicatoins received for each vacancy, which could cause externalities on hiring firms.

5.3 Disaggregating mechanisms by endline employment probability

The earlier discussion shows that, *on average*, the transport treatment increased search intensity, whereas both treatments increased search efficacy. From these results, one might be tempted to take these average effects as descriptive of the sample as a whole. To test this, we repeat our earlier stratification exercise, to characterise how responses to treatment differ between the high-employment and low-employment subsamples.

Figure 3 shows that the transport treatment increased job search intensity, measured here as the total number of trips to job vacancy boards after the interventions began, across different values of predicted employment. It is notable that the low-attachment group searches considerably less than those with higher predicted employment, so that the relative increase in job search is relatively constant across the distribution of labour market attachment.[70]

[69] As a robustness check on these results, we investigate whether our interventions might increase search effectiveness by inducing workers to search for easier-to-get jobs and we find no evidence supporting this hypothesis. First, we use self-reported data on reservation wages and find that treated individuals report being willing to work for the same wages as control individuals. This holds on average, as well as within educational categories (see Figure A.10 and Table A.18 in the appendix). Second, in the endline survey we ask individuals whether they stopped searching for some occupation in the previous 12 months. We find that individuals in the three experimental groups are equally likely to give up searching for at least one type of occupation and to stop searching for white collar jobs. Finally, in Figures A.11a and A.11b (online appendix) we compare the probability of working in a number of different occupations across the three groups. Occupation profiles look similar for the three experimental arms, with a slight shift towards white collar jobs for individuals in the transport and workshop group. Overall, this evidence suggests that treated individuals look for similar occupations and are willing to work for similar wages.

[70] Unsuprisingly, we find no impact of the workshop on search intensity, regardless of labour market attachment.

In contrast, both treatments increase search efficacy for the low-employment group; we estimate that, for the low-employment group, both transport and workshop treatments increase the ratio of offers to applications by approximately 8-9 percentage points (on a control group mean of 12 percentage points). Figure 4 shows how the effects change over the distribution of labour market attachment. We find no effect on search efficacy for either treatment on the high-employment group.[71] This result provides important nuance to our earlier result on mechanisms: it shows that anonymity and distance present very different kinds of costs depending on individuals' degree of labour market exclusion.

These results also help us to understand the heterogeneous results in Section 4.2, which shows the impacts of the interventions to be concentrated among those with low labour market attachment. This was not because they see larger increases in job search intensity than the high-attachment group. Rather it seems that they experience higher marginal returns to increased job search: because of relatively low search intensity, and perhaps a lack of good job oppurtunities through other mechanisms, they experience large gains from expanding their pool of vacancies when they search more intensity. For the workshop, our results suggest that the returns to improved signals are larger for individuals with low attachment.

6 Anonymity or Distance: Further discussion on informational and spatial constraints

In this section, we provide additional evidence on the nature of the constraints relaxed by our interventions. The section constitutes exploratory work that goes beyond the pre-analysis plan, aiming to shed additional light on the mechanics of the impacts.

6.1 Informational constraints

What can we learn from our experiment about the nature of informational constraints? Our workshop sought to improve job-seekers' communication skills, and to provide a tangible signal of ability (the certificates). We argue that these interventions operated through the

[71] We can strongly reject a null hypothesis that the workshop treatment has common effects across groups: $p = 0.015$. We are close to rejecting equality for the transport treatment: $p = 0.147$.

same channel: of improving the clarity of signals to employers. Section 5 already showed that the workshop did not work by increasing job search intensity. Similarly, the workshops' effects cannot be explained through a simple certification mechanism: while the take-up of the certificates was very high (80% of attending participants collected them), only 42% of the recipients who made at least one job application showed the certificate to their prospective employer.[72] Instead, we interpret our results as evidence for the effects of improving the information content of job applications.

To show how the workshop helped workers to signal their abilities, we begin by running a regression of individual test scores on a rich set of covariates (including demographic characteristics, educational attainment, and prior work history); we obtain a measure of predicted test scores for all the job-seekers in our sample (see Table A.30). We then investigate how predicted skills correlate with the labour market outcome that was most impacted by the intervention — that is, the likelihood of permanent work — in different experimental groups. Since the test score regression includes only variables that are easily signalled to employers through a job application, the predicted score is a good proxy of precisely the skills that our workshop should have helped workers to signal.

In the control group, we find no association between high predicted test scores and better labour market outcomes. For those in the workshop treatment, however, the pattern is noticeably different: our intervention causes both a positive shift and a positive tilt in the relationship between predicted test scores and the probability of permanent work.[73] The effect is concentrated among low-educated job-seekers — the group that experienced the strongest treatment effects (see Figure 5 for a graphical representation).[74] This is consistent with the idea that the labour market is least effective in sorting talent among those workers who lack formal certification of specific skills.

As further suggestive evidence, we employ a regression discontinuity design. The certificates issued to respondents reported test scores in discrete bands. The original test score was not reported on the certificates and was never disclosed to study participants. 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 and significant discrete improvements in employment

[72] This is unlikely to be caused by poor comprehension, as participants report having understood the information presented in the certificates well, and certificate use is correlated with test scores.

[73] Consistent with our expectations, this effect appears to be unique to the job application workshop; we do not find similar impacts from the transport treatment.

[74] The underlying regression results are reported in Table A.32, where we bootstrap standard errors to perform inference with a generated regressor.

prospects at band cut-offs.[75] We estimate a separate model for each cut-off, implementing a local linear regression to control for raw test scores on either side of the cut-off.[76] We find no significant improvements at band cut-offs. Notably, this is the case for the two main outcomes our interventions have impacted (permanent work and formal work). Figure A.12 in the online appendix illustrates this result by showing the effect of being above the median aggregate test score on having a permanent job.[77] These discontinuity results are consistent with our earlier results on predicted test scores: we interpret our results as indicating that the workshops helped job-seekers to improve their ability to signal skills, rather than merely operating through a simple certification mechanism.

6.2 Spatial constraints

Finally, we provide some additional evidence on the ways in which a transport subsidy like ours can change the spatial patterns of job search and employment in an urban labour market. In the absence of the treatments, respondents in our control group are less likely to search for a formal job the further they live from the city centre (Figure A.13). We also find that the incidence of informal employment is significantly higher the further respondents live from the city centre. The tansport treatment halves the gap in formal employment betweeen individuals living within and beyond the median distance from the centre of the city. Among those who live far from the centre the transport intervention increases formal employment by 7.1 percentage points (48% increase), compared to 4.6 percentage points (a 23% increase) amongst those living closer (Figure A.16).

We do not find evidence that employment and permanent employment rates differ with distance from the city centre, perhaps because everyone in our sample lives at least 2.8 km from the centre. This pattern may also be moderated by the spatial sorting of workers (e.g. some relatively rich neighbourhoods are in the outskirts of the city). Indeed, we find that our predicted employment measure increases with distance from the city centre, as does average education.

[75] We perform this analysis for the aggregate score (a summary measure of all test results) and the Raven test score, since we find that these measures have the strongest predictive power for endline employment outcomes.

[76] Scores below the median were lumped together into a bottom band, while individuals scoring above the median are divided into five bands (one for each of the higher deciles of the test score distribution). We use the optimal bandwidth selection rule suggested by Imbens and Kalyanaraman (2012), but results are consistent with different bandwidth selections.The optimal bandwidth selection is performed using the Stata command provided by Nichols (2007). The selected bandwidth differs across test outcomes and band cut-offs. For example, the optimal bandwidth for the aggregate test score at the 50th percentile is 0.62, where the median of the aggregate score is 4.843 with a standard deviation of 0.88. In cases where the selected bandwidth is larger than the reported test score band itself, we check that the results are robust to restricting the bandwidth to the range within the marks band.

[77] The full set of results, using the optimal bandwidth and other selected bandwidths, is available on request.

However, we do notice that self-employment (which, for many is an occupation of last resort) increases significantly with distance from the city centre in the control group. Figure 6 shows that upon receiving the transport treatment, respondents who reside far from the centre are significantly less likely to be self-employed, and become as likely to be self-employed as individuals who live close.[78] Overall, we interpret these results as suggestive evidence that spatial barriers influence the occupational structure of urban labour markets, and that enhancing mobility can help unraveling such spatial patterns.

7 Conclusions

The results in this paper contribute to the growing consensus that, on average, active labour market policies in developing countries are unlikely to have large effects on whether or not respondents have a job. However, in contrast to earlier findings, results in this paper nonetheless suggest that frictions matter for explaining problems of labour market exclusion. In particular, our results highlight two dimensions of heterogeneity: heterogeneity in job quality, and heterogeneity in job-seeker. In doing so, our results show large potential gains from programmes that are carefully targeted — particularly for helping job-seekers to access permanent employment in a segmented labour market, and particularly for assisting job-seekers with low labour market attachment.

Specifically, our results build upon previous work in three key dimensions. First, we shed light on how frictions operate in urban labour markets in developing countries. A simple lack of face-to-face interactions between workers and firms does not appear to be a primary constraint. Rather, workers search strategically for job vacancies and exercise considerable discretion over the jobs to which they apply. Binding constraints for job seekers seem to be the number of vacancies from which they can choose, and their inability to adequately signal their abilities to employers for which they are well suited. For this reason, the two interventions studied in this paper outperform direct-matching job fair interventions — both in this context (Abebe et al., 2017) and elsewhere (Groh et al., 2015). Second, we show how frictions may operate in the allocation of formal and permanent jobs, which seem particularly hard for young people to find — even while the market for low-quality, temporary, jobs may work relatively efficiently. Third, effects of the interventions are strongest among the most disadvantaged socio-demographic groups, who experience both an increase in job quality (more permanent and more formal jobs) and gains in overall employment and earnings. This is an important result, which reveals that even if this type

[78] Regression results with differential effects of distance (either linear or quadratic) corroborate this conclusion: thanks to the treatment, the effect of distance on the probability of self-employment is statistically indistinguishable from zero (see Figure A.15).

of interventions may not lead to sizable gains in overall employment— as documented in the literature (Crépon and van den Berg, 2016; McKenzie, 2017) — they can improve equality of opportunity by helping those excluded from the labour market.

Finally, while our study is designed to investigate the potential efficiency gains generated by the interventions, some hypotheses can be advanced. First, increased competition among workers, as generated by the treatments, can help employers either to find more suitable candidates, or to find them faster (Marimon and Zilibotti, 1999; Galenianos et al., 2011; Hsieh et al., 2013). Further, a better match between the skills that workers have and the skills that are required by their jobs may help to reduce worker turnover — a key policy concern in Ethiopia and other developing countries (Blattman and Dercon, 2016). Our experiment cannot directly measure such aggregate welfare gains, partly because it is not possible to identify indirect treatment effects outside of well-defined study areas — a common challenge for all studies of this kind. However, we find evidence suggesting that overall efficiency in the labour market improves as a result of the interventions. In particular, we document that the job application workshop enables the labour market to better separate low-skilled and high-skilled workers, removing information frictions that may lead firms to make the wrong hires. We also find that the transport subsidy improves the allocation of labour by giving workers who live out of town the opportunity to access salaried jobs, rather than having to fall back on self-employment. These findings suggest that job search assistance can help to reduce labour market distortions, and may produce aggregate efficiency gains.

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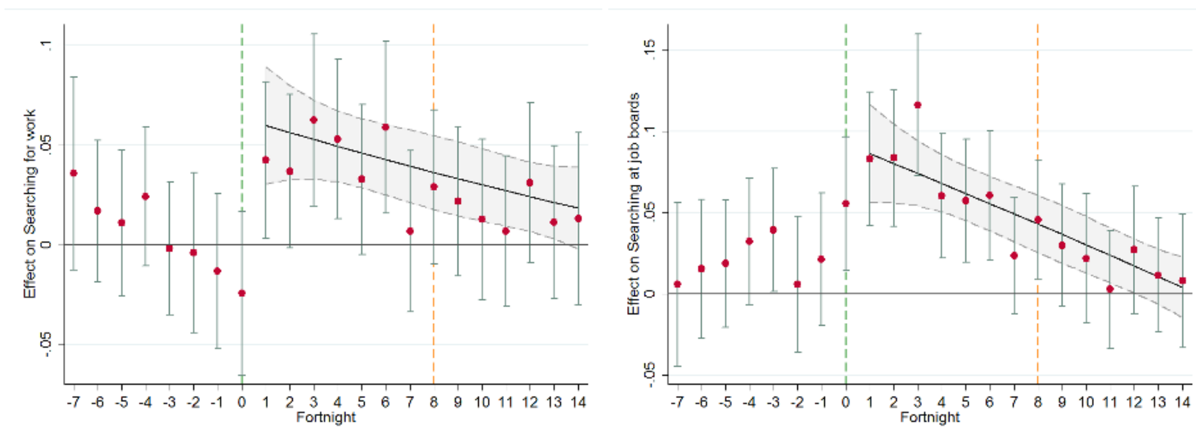
Figures and Tables

Figure 1: **Impact trajectory of the transport treatment**

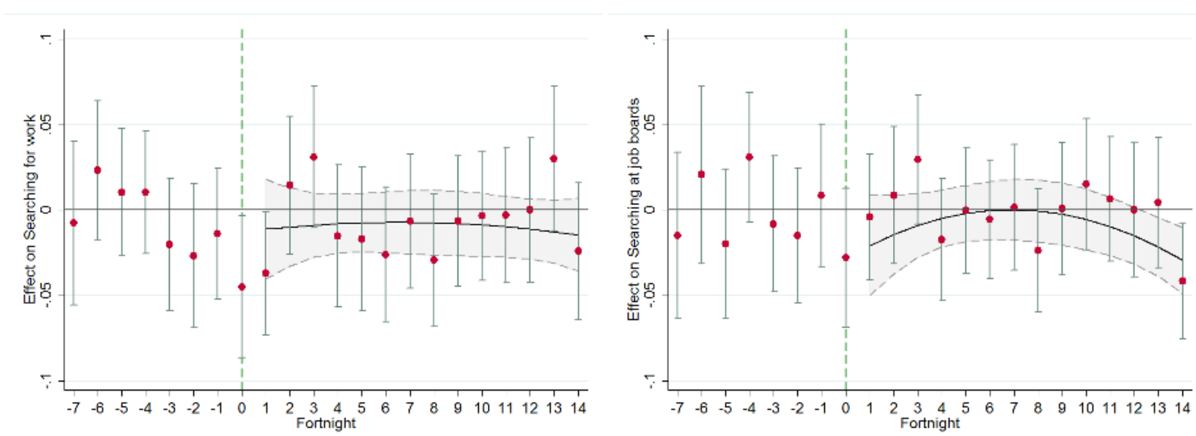
(a) Impact on search (any active step) (b) Impact on searching at the job boards



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: **Impact trajectory of the application workshop: Searching for work**

(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: **Impacts on *search intensity* by predicted probability of good work**

(a) Transport (b) Workshop

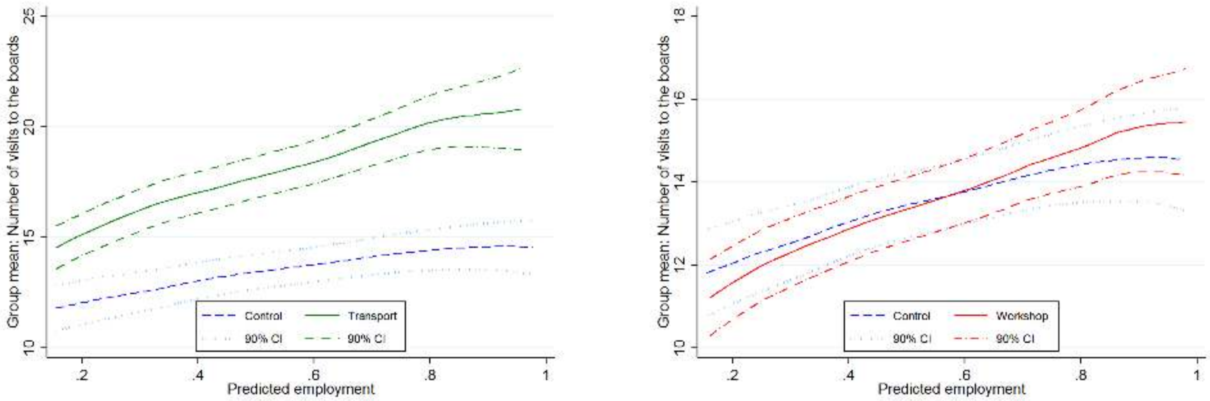


Figure 4: **Impacts on *search efficacy* by predicted probability of employment**

(a) Transport (b) Workshop

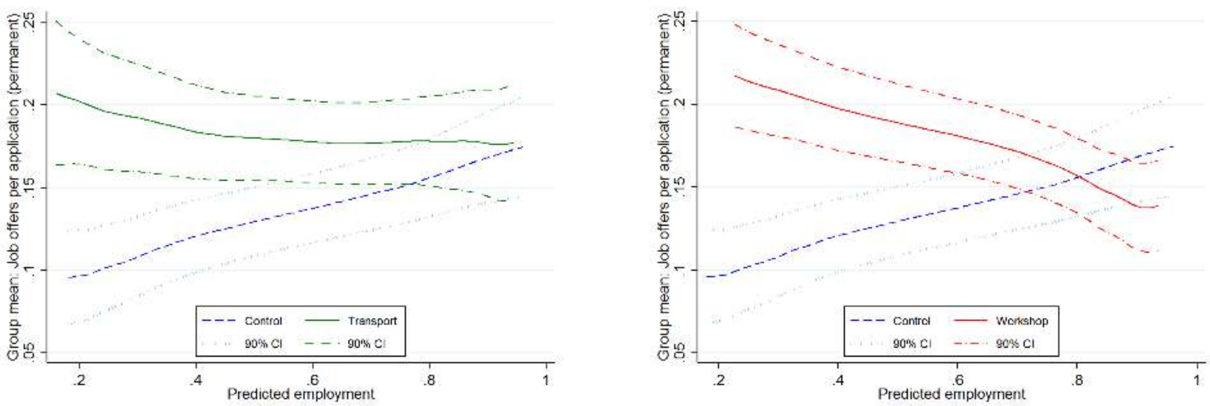


Figure 5: **Predicted grades and permanent work**
(job-seekers with at most a high school degree)

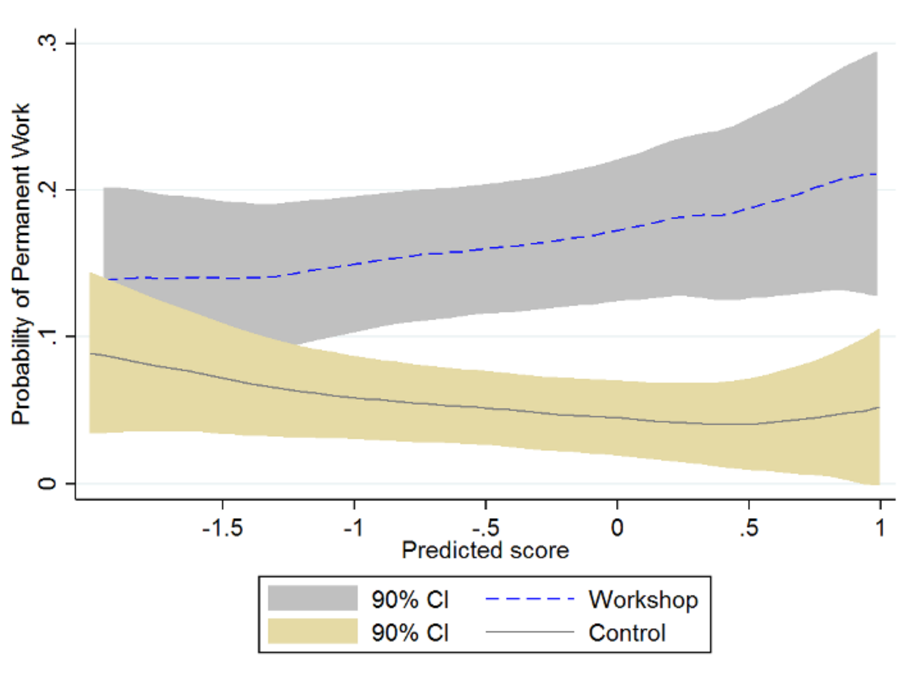


Figure 6: **Relationship between distance and self-employment: Impact of transport subsidies**

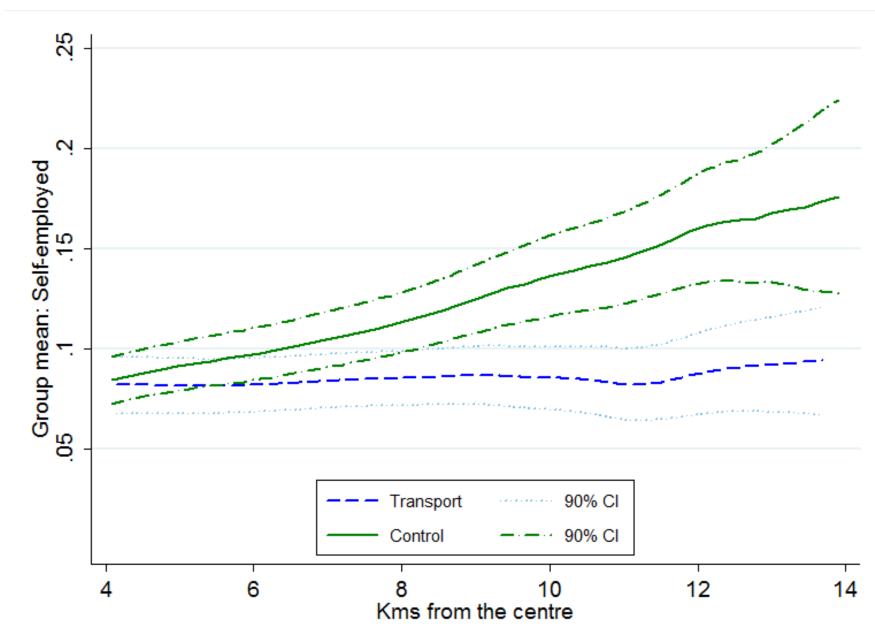


Table 1: **Treatment Assignment**

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: **Employment outcomes**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
Worked	0.0400 (.029) [.297]	0.0200 (.031) [.698]	-0.0460 (.034) [1]	0.0320 (.053) [1]	0.537	0.515	3790
Hours worked	0.159 (1.57) [.951]	-0.0780 (1.551) [1]	-2.358 (1.861) [1]	0.578 (2.546) [1]	25.57	0.870	3783
Formal work	0.0550 (.019)*** [.026]**	0.0530 (.02)*** [.018]**	0.0140 (.02) [1]	0.0600 (.038) [1]	0.172	0.937	3790
Perm. work	0.0340 (.018)* [.179]	0.0690 (.02)*** [.003]***	0.00700 (.019) [1]	0.0150 (.026) [1]	0.120	0.0900	3790
Monthly earnings	2.344 (73.914) [.951]	61.55 (84.583) [.698]	-43.60 (90.083) [1]	12.31 (103.749) [1]	971.4	0.433	3737
Satis. with work	0.00100 (.027) [.951]	0.0230 (.027) [.698]	-0.0190 (.024) [1]	0.0500 (.047) [1]	0.231	0.493	3790

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: 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 4: Heterogeneous effects by predicted probability of endline employment

	Worked	Hours worked	Formal work	Permanent work	Earnings	Satisfaction with work
High employment group:						
Transport	-0.019	-2.324	0.064	0.013	-131.779	-0.043
	(0.034)	(1.995)	(0.033)*	(0.029)	(143.085)	(0.040)
Workshop	-0.053	-3.082	0.013	0.038	-174.617	-0.017
	(0.035)	(2.059)	(0.030)	(0.030)	(150.041)	(0.036)
Low employment group:						
Transport	0.095	2.155	0.051	0.045	113.641	0.032
	(0.040)**	(2.076)	(0.024)**	(0.022)**	(83.427)	(0.031)
Workshop	0.103	3.207	0.088	0.088	276.794	0.057
	(0.042)**	(2.276)	(0.027)***	(0.024)***	(106.232)***	(0.035)
Obs.	2178	2173	2178	2178	2147	2178
Control group means:						
High employment group	0.712	32.565	0.295	0.241	1643.8	0.311
Low employment group	0.414	19.924	0.152	0.097	659.9	0.168
Tests of common effects:						
Transport (p)	0.022**	0.104	0.741	0.334	0.123	0.089*
Workshop (p)	0.003***	0.034**	0.048**	0.145	0.011**	0.119
Tests among 'low employment':						
Zero effects (p)	0.018**	0.343	0.003***	0.000***	0.031**	0.226
Common effects (p)	0.845	0.632	0.194	0.119	0.129	0.522

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A.1 Theoretical framework

The main insights behind our experimental design and testing strategy are best illustrated with a standard search model (*e.g.* Mortensen (1970), McCall (1970)), adapted to our setting. We consider an individual searching for their preferred form of employment which, in the context of our study, is a permanent job with a formal labor contract. This form of employment is preferred because it provides higher employment security and comes with various benefits.

A.1.1 Main model

To obtain such a job, the individual must first identify suitable positions to which (s)he can apply. During each period the job seeker chooses an intensity of search s at cost c. The probability of finding a suitable position to which to apply is $\Pr(a = 1 \mid s)$ with $\frac{\partial \Pr(a=1 \mid s)}{\partial s} \geq 0$: searching more intensely increases the probability of applying.

Conditional on applying ($a = 1$), the probability of getting the job depends on the precision θ of the signal that the job seeker provides to the potential employer; we denote this $\Pr(w = 1 \mid \theta)$ where $w = 1$ indicates that the individual gets the job. We assume that $\Pr(w = 1 \mid \theta) > 0$ and that $\Pr(w = 1 \mid \theta)$ increases in θ. Behind this assumption is the idea that each employer seeks a minimum quality threshold and that, with a less informative signal, an applicant is less likely to demonstrate to the employer that they are above the required threshold.[79] The job seeker searches for a job in each period until finding one, after which he or she receives the continuation value W of having secured a permanent job.

The optimal search strategy of the job seeker can thus be expressed by the following Bellman equation:

$$V(c,\theta) = \max_{s \geq 0} -cs + \beta \left[\Pr(a|s) \Pr(w|a,\theta) W + (1 - \Pr(a|s) \Pr(w|a,\theta)) V(c,\theta)\right]$$

where β is the discount factor and $V(c,\theta)$ is the value function, which depends on the search cost c and the signal precision θ. Within each period, the optimal solution is the value of search s^* that satisfies the first order condition:[80]

$$-c + \frac{\partial \Pr(a|s)}{\partial s} \beta \Pr(w|a,\theta) \left(W - V(c,\theta)\right) = 0 \tag{7}$$

[79] This does not imply that a more accurate signal provided by the applicant is always above the threshold. It only means that, without a more accurate signal, the probability of getting a job offer is lower than without the signal. The magnitude of the increase in the probability of getting the job depends on the proportion of applicants that are revealed to be above the employer's threshold by the more informative signal.

[80] The second order condition for a maximum is

$$\frac{\partial^2 \Pr(a|s)}{\partial s^2} \beta \Pr(\theta|a) \left(W - V(c,\theta)\right) < 0$$

which requires $\frac{\partial^2 \Pr(a|s^*)}{\partial s^2} < 0$ locally at s^* since, by definition, $\beta \Pr(w|a,\theta) > 0$ and $W > V(c,\theta)$.

Since by construction $\beta > 0$, $\Pr(w|a,\theta) > 0$ and $W > V(c,\theta)$ (having a good job is preferred to searching for a good job), there exists a non-zero search level s^* only if $\frac{\partial \Pr(a|s^*)}{\partial s} > 0$ (search strictly increases the probability of hearing about a suitable application). Inserting the optimal search level s^* into the Bellman equation and solving for $V(c,\theta)$ yields:

$$V(c,\theta) = \frac{-cs^* + \beta \Pr(a|s^*) \Pr(w|a,\theta) W}{1 - \beta(1 - \Pr(a|s^*) \Pr(w|a,\theta))} \tag{8}$$

Replacing $V(c,\theta)$ in (7) yields an equation that can numerically be solved for s^*:

$$\underbrace{\frac{\partial \Pr(a|s^*)}{\partial s} \Pr(w|a,\theta)}_{\text{Increase in probability of getting the job}} \times \underbrace{\beta \left(W - \frac{\beta \Pr(a|s^*) \Pr(w|a,\theta) W - cs^*}{1 - \beta(1 - \Pr(a|s^*) \Pr(w|a,\theta))} \right)}_{\text{Discounted net value of getting the job}} = c \tag{9}$$

Equation (9) has an intuitive interpretation. It says that the marginal net returns to search have to equal the marginal cost of search. Figure A.21 provides an example.[81] The horizontal line represents the right hand side c; the downward sloping curve depicts how the left-hand side varies with s. The optimal choice of s is where the two curves intersect.

The transport intervention is designed to decrease the marginal cost of job search c. This motivates the job-seeker to search harder (Figure A.22), so that the marginal benefit and marginal cost of search are equalised. As a result of higher search effort, the job-seeker will be more likely to hear about a vacancy and hence more likely to obtain good employment (Figure A.23). Further, we note that while decreasing the cost of search should not change search efficacy in this simple framework, in a richer model higher search efforts could also enable the job-seeker to better target her applications, with a potential positive effect on search efficacy.[82]

Prediction 1 *The transport intervention increases the intensity of job search and the probability of getting a good job.*

The job application workshop is designed to increase signal quality θ. This will have a direct effect on search efficacy $\Pr(w|a,\theta)$. Further, higher search efficacy increases the returns to job search. The job-seeker will increase search intensity in response, as long as he or she can find the resources to finance the additional job search. The combination of a higher search efficacy and (possibly) higher search intensity translates into an increase in the probability of finding good employment (Figure A.24).

Prediction 2 *The job application workshop increases search efficacy, that is, the probability of getting an offer conditional on applying. It also increases the probability of getting a good job.*

[81] We set the default values of $\Pr(w|a,\theta) = 0.1$, $\beta = 0.9$, $c = 1$, and $W = 100$. Furthermore, we let $\Pr(a|s) = \frac{1}{1+e^{-s}}$ and hence $\frac{\partial \Pr(a|s)}{\partial s} = \frac{e^{-s}}{(1+e^{-s})^2}$. Note that as long as $\frac{\partial \Pr(a|s)}{\partial s} > 0$ and $\frac{\partial \Pr(a|s)}{\partial s} \Pr(w|a,\theta)\beta W - c > 0$ (the *gross* marginal returns to search is higher than the marginal cost), the left hand side of (9) decreases with s, guaranteeing a unique solution.

[82] We explore this point in an extension of the model in the next subsection.

The strength of the treatment effect for a particular group of subjects will depend on (i) the change in the probability of hearing about a vacancy and (ii) the change in search efficacy. As we assume that search effort and signal quality have diminishing returns on these probabilities, the effect of the same increase in effort and signal quality will be larger for individuals who, in the absence of treatment, would have lower search intensity and lower search efficacy. A second reason for heterogeneity is that some groups may increase search effort more than other groups in response to the interventions. However, we expect this to be a second-order effect. We thus predict that, overall, groups with poor attachment to the labour market for good jobs will benefit from the interventions the most.

Prediction 3 *The treatment effects are stronger for workers who, without the intervention, would have a low probability of good employment.*

A.1.2 Targeted job applications

So far we have assumed that increased search s automatically results in an increased probability of applying for a job; that is, that $\frac{\partial \Pr(a=1 \mid s)}{\partial s} \geq 0$. We now extend the model to relax this assumption; specifically, we allow for workers to choose their applications optimally.

To do this, we now allow the possibility that a worker submits multiple applications in each period. We denote n as the number of job openings identified by search, and make the natural assumption that, on average, higher search intensity translates into a larger number of job openings being identified. We now normalize s such that $\mathbb{E}(n \mid s) = s$. (It therefore follows that, if $\mathbb{E}(a \mid n)$ is the expected number of applications for n job openings, the expected number of applications for a given search intensity is $\mathbb{E}(a \mid s) = s \cdot \mathbb{E}(a \mid n)$.)

Second, we introduce differences in the value v of each job prospect to a job seeker. We denote v as the net value of a given job prospect; that is:

$$v = \Pr(w = 1 \mid \theta) \cdot W - c.$$

We assume that v is a continuous random variable with mean $\overline{v}$, *pdf* $f(v)$ and *cdf* $F(v)$. Note that v may take both negative and positive values. A job seeker who identifies n job openings observes n values v independently drawn from $F(v)$.

With these assumptions, we now can discuss the targeting of applications by job seekers. We consider two cases of interest: one in which each job seeker is constrained in the number of applications that she or he can file each period; and one in which treatment generates more precise information about the value v of each job opening.

A.1.2.1 Case 1: Constrained number of applications

To illustrate what happens with constraints in the number of filed applications, we consider the simple case where the number of applications is fixed (that is, we let $a = k$ where k is an exogenously determined constant). In each period, a job seeker who searches with intensity s identifies n job openings. To each opening corresponds a draw v from the distribution

$F(v)$. The optimal strategy for the job seeker is to apply to the k positions with the highest v (or to all n positions in case $n \leq k$).

By searching more intensively, the job seeker increases the size of the sample from which are selected the k job values with the highest job values v. Since v is continuous — that is, on the assumption that $F(v)$ is strictly monotone for $F(v) \in (0,1)$ — the expected value of the kth order statistic is strictly increasing in n. (This is straightforward — and follows, for example, from Theorem 14 in Alexander et al. (1974, p.257).) This yields the following prediction:

Prediction 1 *If workers are constrained in the number of applications they can file, the transport intervention and job application workshops need not increase the number of applications. But job applications will be better targeted and hence result in a higher probability of getting a permanent job.*

A.1.2.2 Case 2: Information about the value of job openings

We now consider what happens if a treatment increases the precision of the information that job seekers obtain about each job opening. This may arise either because they have direct access to job-specific information by traveling to the job boards, or because, thanks to the job application workshop, they can better evaluate the chance of being offered a job requiring qualifications that they either have or do not have.

Formally, let v^e denote the expected value of a job opening to a job seeker and let $v^e = \overline{v} + \rho(v - \overline{v})$, where $\rho \in [0,1]$ measures the precision of the information available to the job seeker. When $\rho = 0$, the job seeker has no job specific information and $v^e = \overline{v}$ – all job openings look the same. When $\rho = 1$, the job seeker has complete information: $v^e = v$. Note that, with this parameterization, job seekers have unbiased expectations in the sense that $E[v^e] = \overline{v}$.

We assume that $\overline{v} > 0$. When $\rho = 0$, job seekers apply to all job openings. In this case, the number of job applications a is simply the number of job openings n identified by the job seeker. Hence higher search intensity s leads to a larger number of applications (as in the main model above).

When $\rho = 1$, job seekers apply to all job openings with a positive value $v > 0$. Hence the probability of applying to each individual job opening is $\Pr(v > 0) = 1 - F(0)$ and the number of job applications is $a = n\,(1 - F(0)) \leq n$. In the intermediate case where for $0 < \rho < 1$, the probability of applying is $\Pr(v^e > 0) = \int_0^\infty f(\overline{v} + \rho(v - \overline{v}))dv$ and the number of job applications is $a = n \cdot \int_0^\infty f(\overline{v} + \rho(v - \overline{v}))dv \leq n$. It follows that, for a given search intensity s, the number of application *falls* when information precision ρ increases. This is because as ρ increases, a mean-preserving spread of the distribution of v^e around its positive mean $\overline{v}$ forces an increasing proportion of realizations of v^e below 0.

At the same time, however, better information raises the expected value of searching. To see this, first note that for $\rho = 0$, $\mathbb{E}(v \,|\, s; \rho = 0) = n\overline{v}$. In contrast, for $\rho = 1$, $\mathbb{E}(v \,|\, s; \rho = 1) = n\,(1 - F(0)) \cdot \mathbb{E}(v \,|\, v > 0)$. To show that $\mathbb{E}(v \,|\, s; \rho = 1) > \mathbb{E}(v \,|\, s; \rho = 0)$, let us decompose $\overline{v}$

as follows:

$$\overline{v} = F(0) \cdot \mathbb{E}(v \,|\, v \leq 0) + [1 - F(0)] \cdot \mathbb{E}(v \,|\, v > 0).$$

Given that $\mathbb{E}(v \,|\, v \leq 0) < 0$, it follows that:

$$\overline{v} < (1 - F(0))\, \mathbb{E}(v \,|\, v > 0).$$

The same reasoning can be applied to intermediate situations in which $0 < \rho < 1$. We have $\mathbb{E}(v \,|\, s; \rho) = n \left[1 - \int_{-\infty}^{0} f(\overline{v} + \rho(v - \overline{v}))dv\right] \cdot \mathbb{E}(v \,|\, \overline{v} + \rho(v - \overline{v}) > 0)$. By analogy to the above, we have:

$$\begin{aligned} \overline{v} = & \left(\int_{-\infty}^{0} f(\overline{v} + \rho(v - \overline{v}))dv\right) \cdot \mathbb{E}\left[v \,|\, \overline{v} + \rho(v - \overline{v}) \leq 0\right] \\ & + \left(1 - \int_{-\infty}^{0} f(\overline{v} + \rho(v - \overline{v}))dv\right) \cdot \mathbb{E}\left[v \,|\, \overline{v} + \rho(v - \overline{v}) > 0\right]. \end{aligned} \tag{10}$$

Given that $\mathbb{E}\left[v \,|\, \overline{v} + \rho(v - \overline{v}) \leq 0\right] < \mathbb{E}\left[v|\overline{v} + \rho(v - \overline{v}) > 0\right]$ as long as $\rho > 0$, it follows that

$$\overline{v} < \left(1 - \int_{-\infty}^{0} f(\overline{v} + \rho(v - \overline{v}))dv\right) \mathbb{E}\left[v|\overline{v} + \rho(v - \overline{v}) > 0\right].$$

Hence we have shown that, when treatment increases information about job suitability ρ, there is a fall in $\mathbb{E}(a \,|\, s)$: job seekers better target their job search. We also have shown that, for a given s and thus a given $\mathbb{E}(n)$, an increase in ρ leads to an increase in $\mathbb{E}(v \,|\, s)$. This in turn increases the value of search and thus results in a higher value of s^* and thus of $\mathbb{E}(n)$. Hence the effect of treatment on the unconditional number of job applications $\mathbb{E}(a)$ is ambiguous – it depends on model parameters and on the specific distribution of v. These findings can be summarized as follows:

Prediction 2 *If a treatment improves a job seeker's ability to predict the value of each job opening, it should increase the intensity of search and thus the number job openings on which the job seeker collects information. But it need not increase the number of applications. Because search is better targeted, the probability of getting an offer conditional on applying (i.e., the job conversion rate) should increase with treatment.*

A corollary of the above prediction is that the effect on a of a treatment that reduces search cost depends on whether this treatment simultaneously increases the precision of the search. If it does not, the predictions of the main model apply: the number of applications increases with treatment; if it does, the revised prediction 6 applies.

A.2 The timing of the employment effects

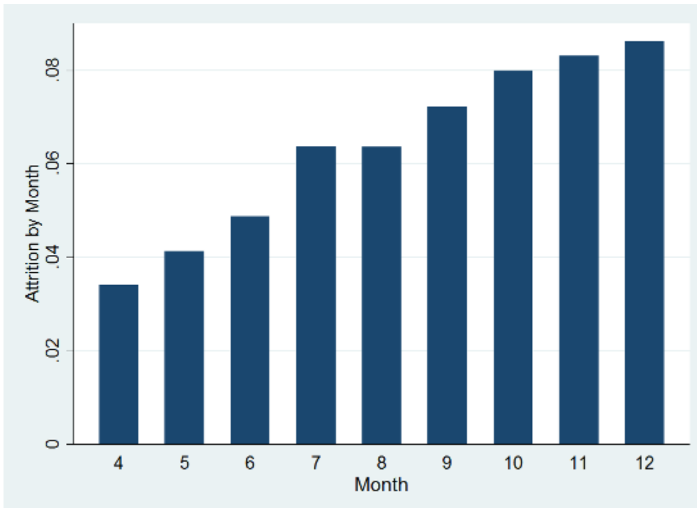
The high-frequency data contains information about the employment outcomes of study participants. This allows us to assess the timing of the impacts on permanent employment.[83] We find that the effects on permanent work take place towards the end of the study period. Specifically, in the last fortnight of the phone survey the difference in permanent employment between individuals in the workshop and control group is about 1.8 percentage points. This goes up to 6.9 percentage points by the time of the endline survey (or 6.4 percentage points, if we only look at individuals who were interviewed both in the endline survey and in the last wave of the phone survey). The widening difference in permanent employment rates between the two groups is the result of two separate effects. First, individuals in the workshop group are about (a significant) 3.1 percentage points more likely to transition to permanent employment between the two surveys.[84] Second, individuals in the workshop group are (an insignificant) 1.6 percentage points less likely to discontinue a permanent job in the same time period. We report these results in Figure A.17 and Tables A.33 and A.34. Further, we find that study participants temporarily decrease the amount of work they take during fortnights when the transport subsidy is available (Figure A.18). The effect is driven by a reduction of work in self-employment. This effect is similar to that reported in Franklin (2017).

[83] In the phone survey we do not have a question about the formality of employment. We are thus unable to analyse the timing of the effects on formal employment.

[84] This figure is obtained from a regression where the dependent variable is dummy that takes the value of one if a respondent switches from not having permanent work at the end of the phone survey to having permanent work in the endline survey. Alternatively, we can use endline information about the date when workers started their current job to construct a dummy variable capturing permanent jobs found between the two surveys. When we regress this alternative dependent variable on the treatment dummies, we obtain a very similar coefficient (about 2.7 percentage points) for the job application workshop.

A.3 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: **Heterogeneous impacts by gender**

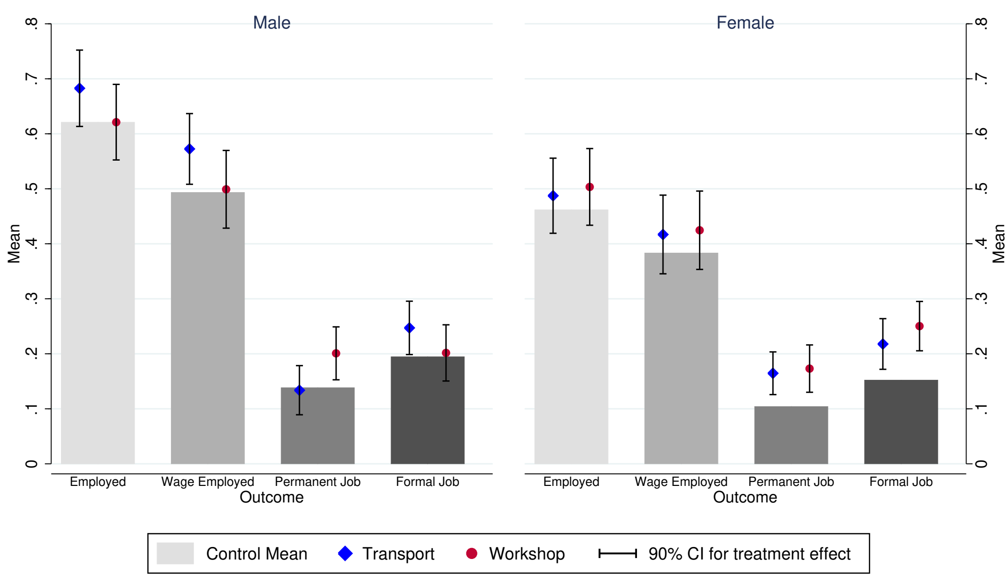


Figure A.3: **Heterogeneous impacts by education**

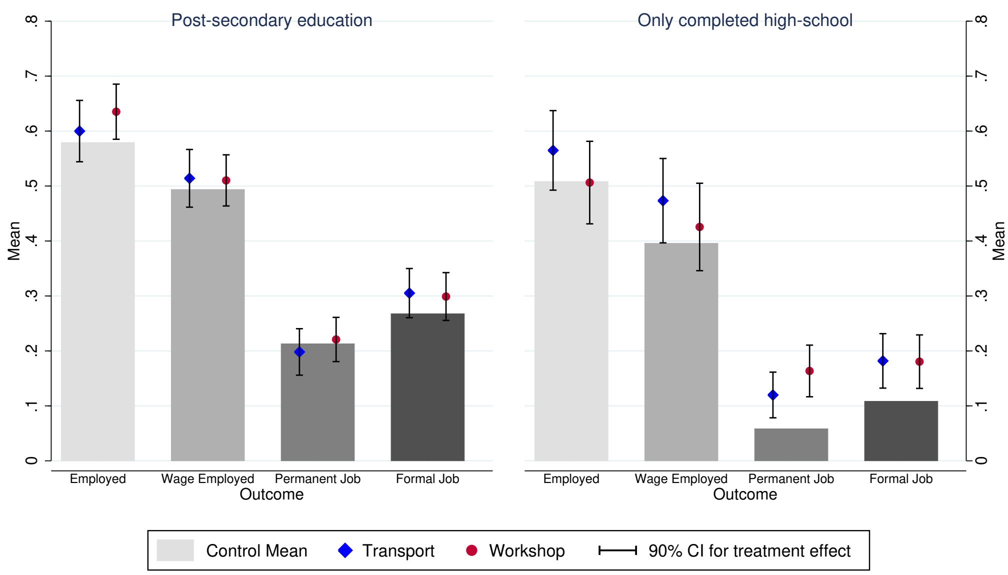


Figure A.4: **Heterogeneous impacts by empoyment**

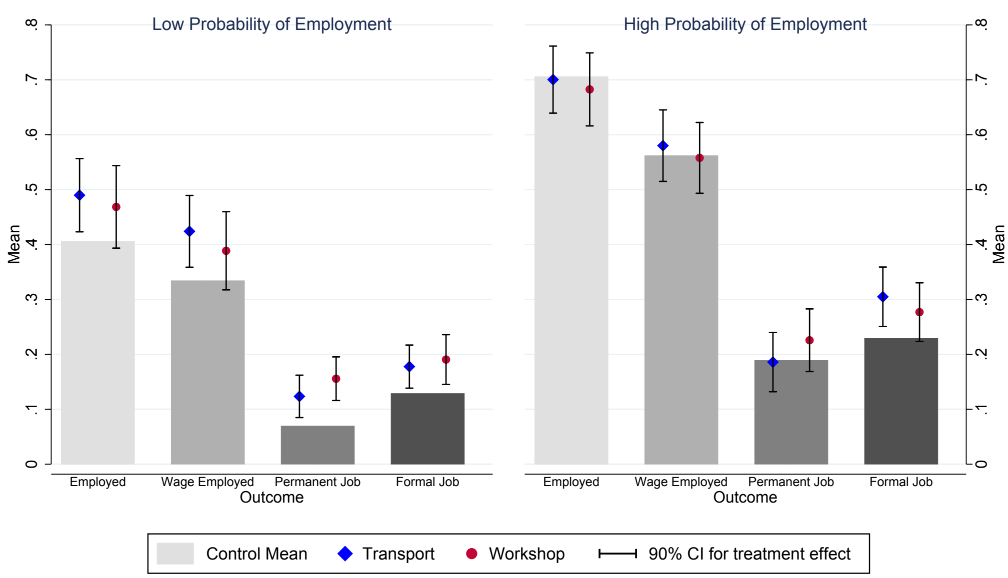


Figure A.5: **Heterogeneous impacts by predicted formal work**

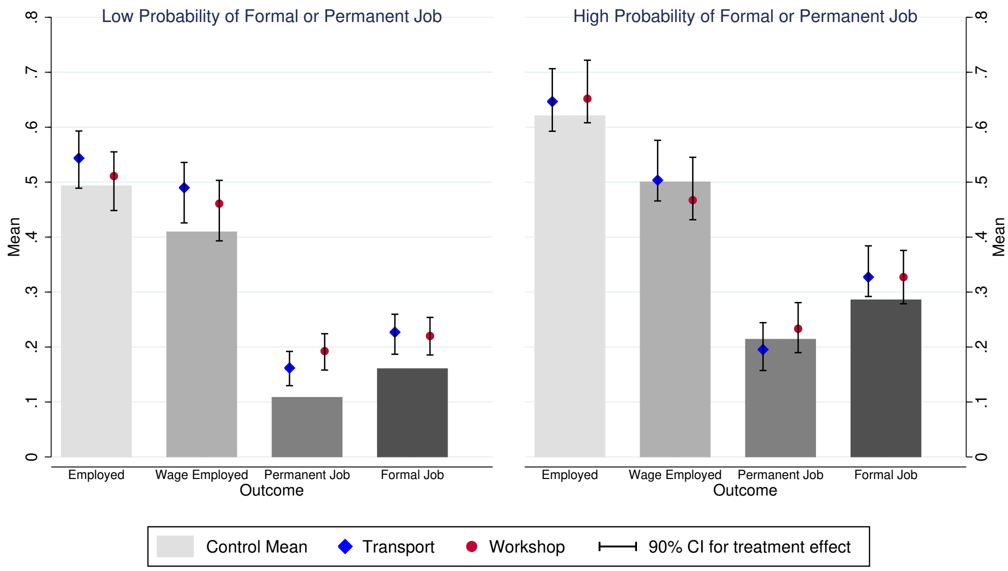


Figure A.6: **Heterogeneous impacts by certificates at baseline**

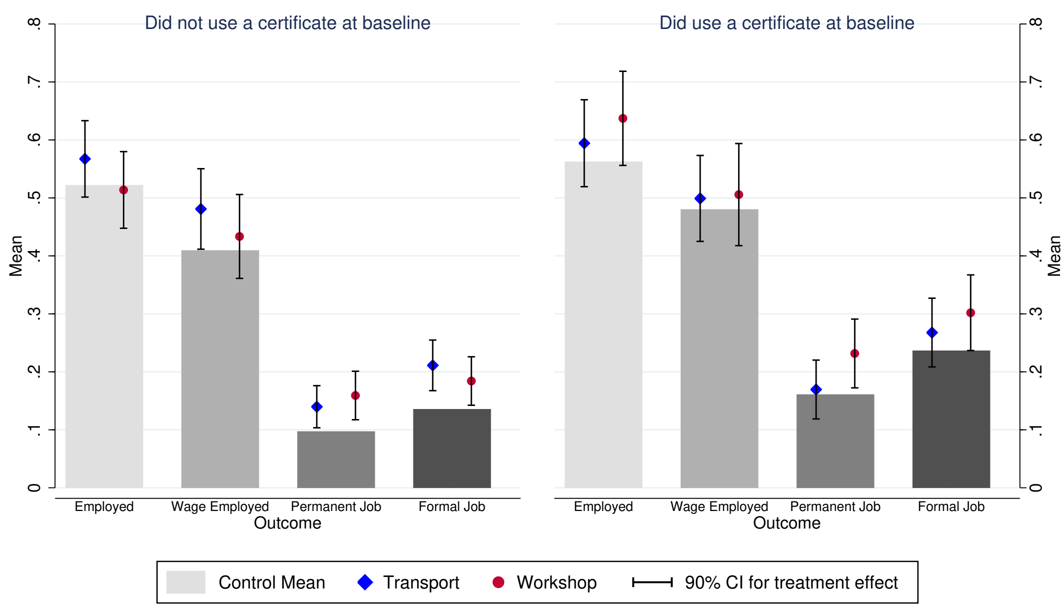


Figure A.7: **Heterogeneous impacts by work experience at baseline**

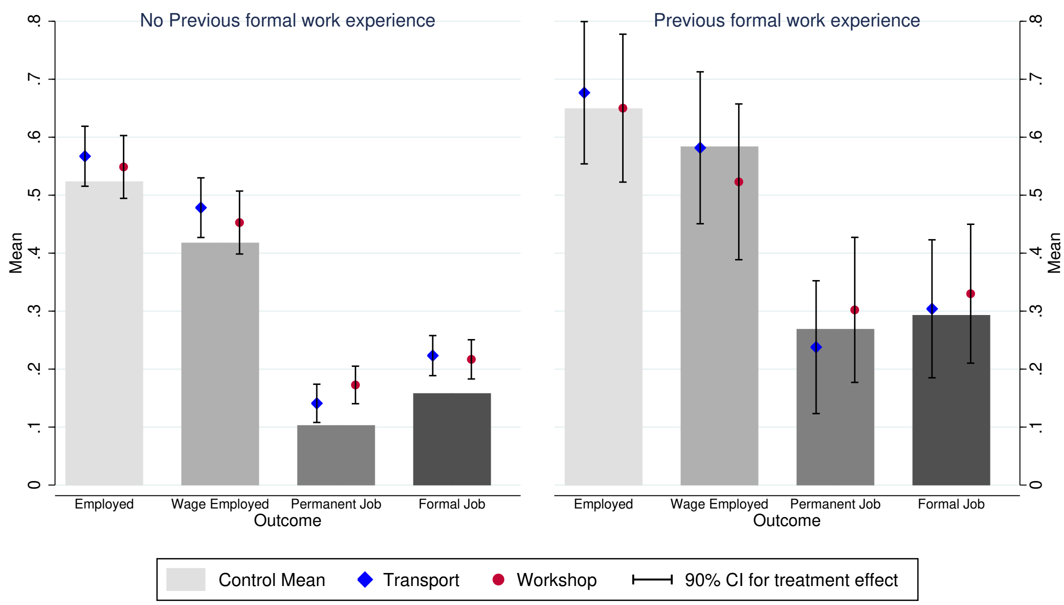
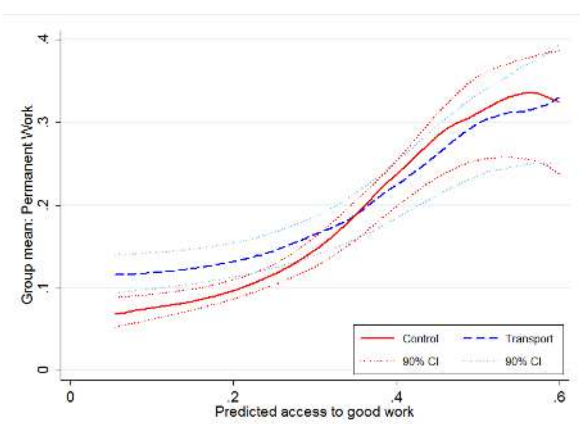


Figure A.8: **Treatment effects by predicted probability of good work**
Non-parametric analysis

(a) Transport



(b) Workshop

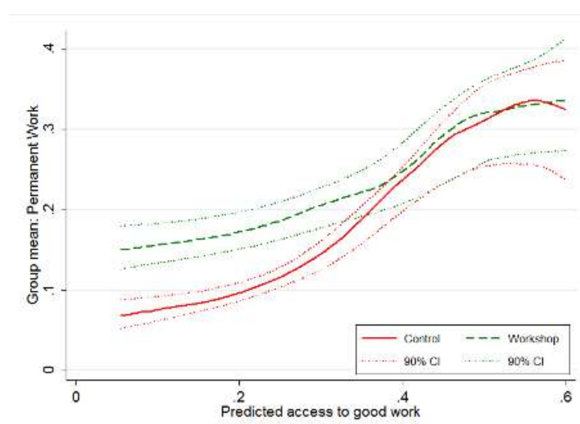


Figure A.9: **Shape of treatment effects on formal work by predicted probability of good work**

(a) Transport

(b) Workshop

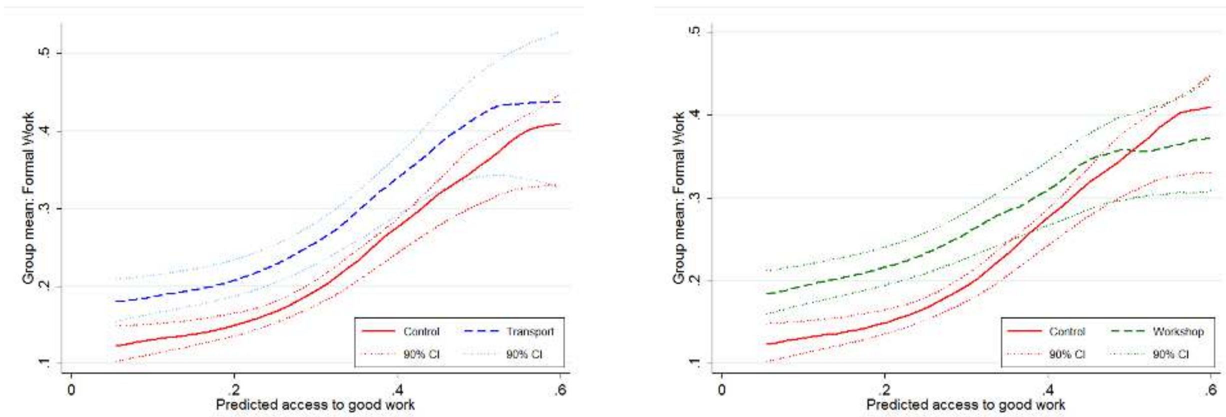


Figure A.10: **Heterogeneous impacts by education: Reservation wages**

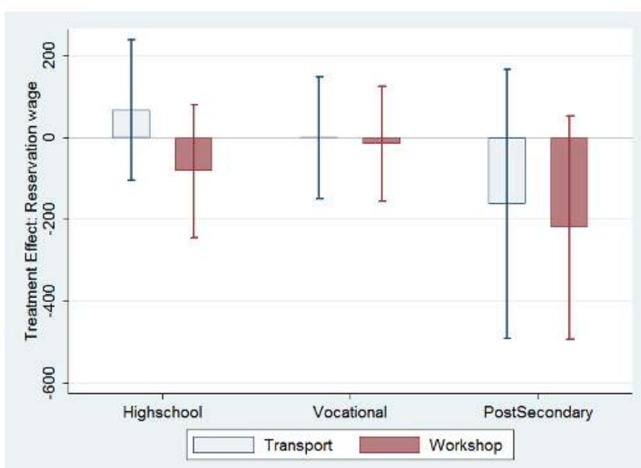
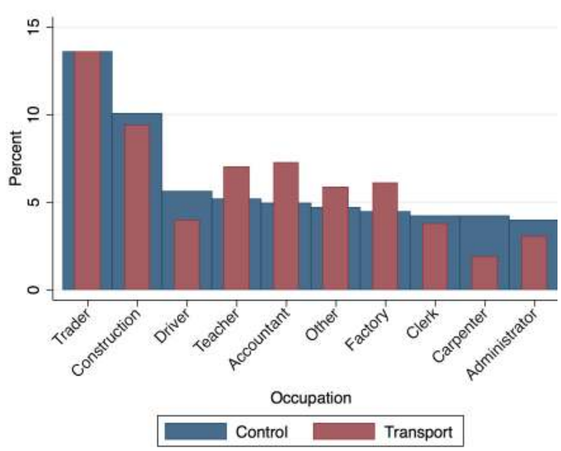


Figure A.11: **Most common occupations**

(a) **Transport Subsidy**



(b) **Job Application Workshop**

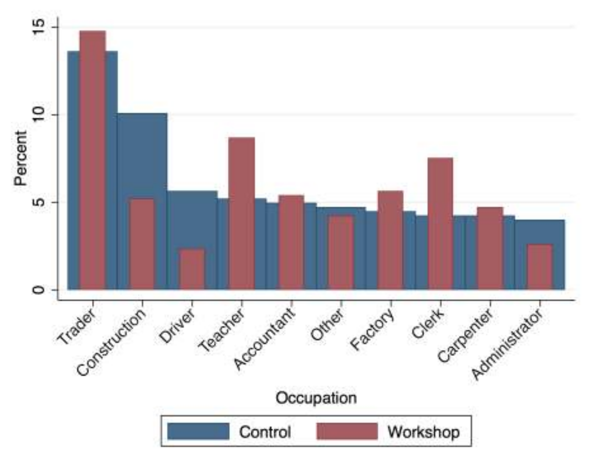


Figure A.12: **Aggregate test scores and permanent work: Impact of scoring above the median**

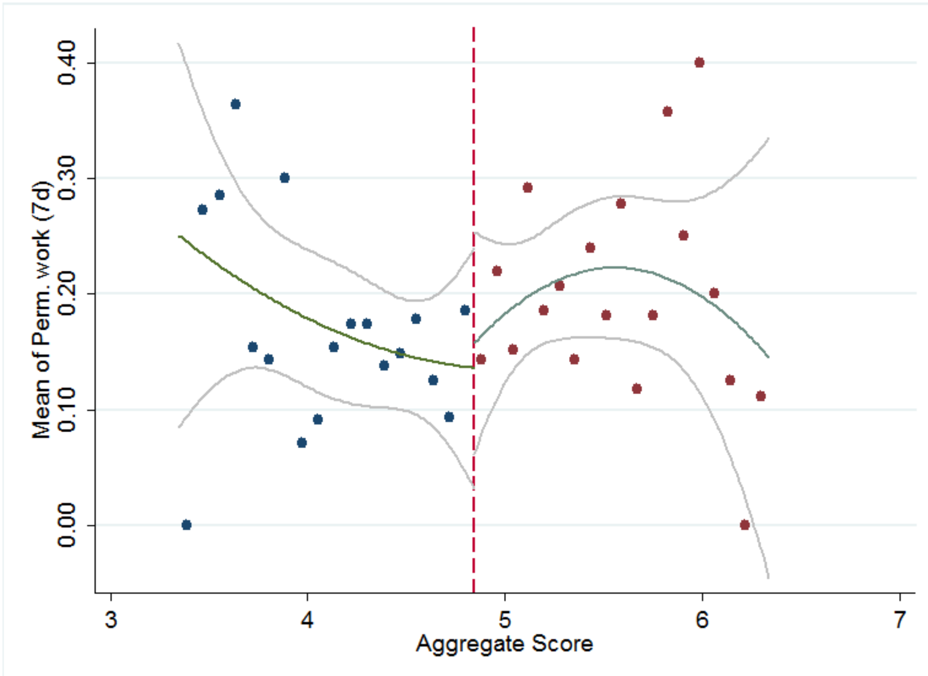


Figure A.13: **Relationship between job search at boards by distance from centre: Impact of transport subsidies**

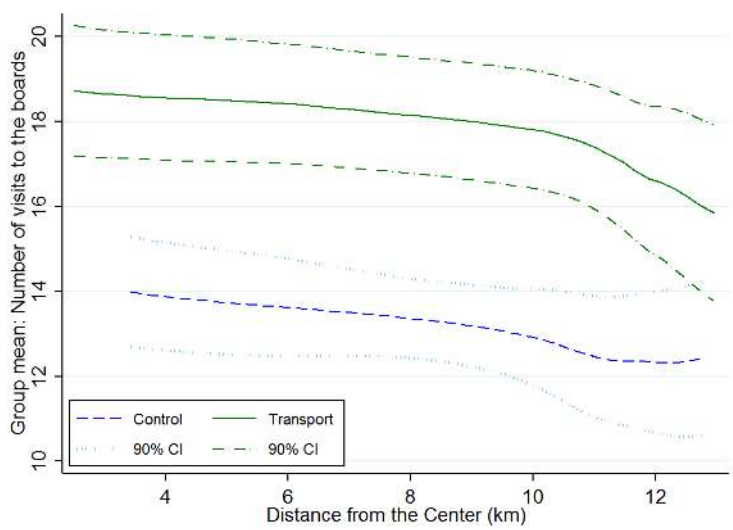


Figure A.14: **Relationship between distance and self-employment: Impact of the job application workshop**

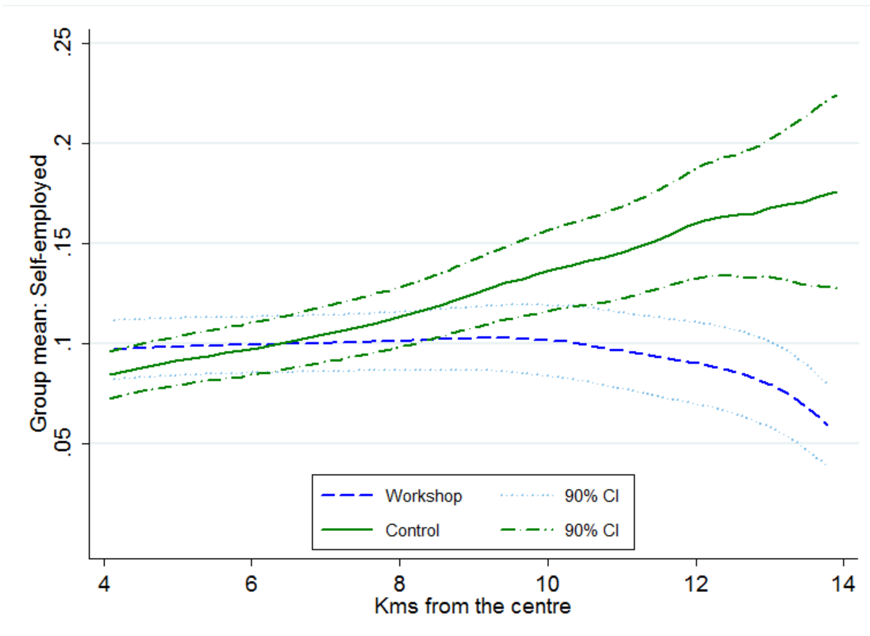


Figure A.15: **Heterogeneous impacts by distance from the centre: Self-employment**

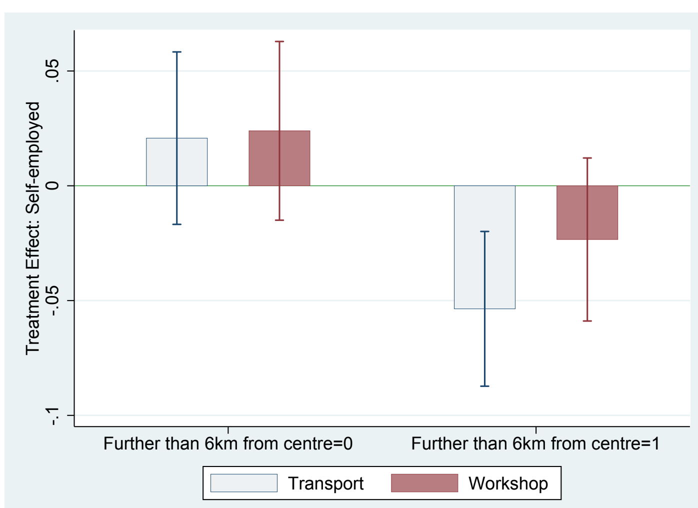


Figure A.16: **Heterogeneous impacts by distance from the centre: Formal work**

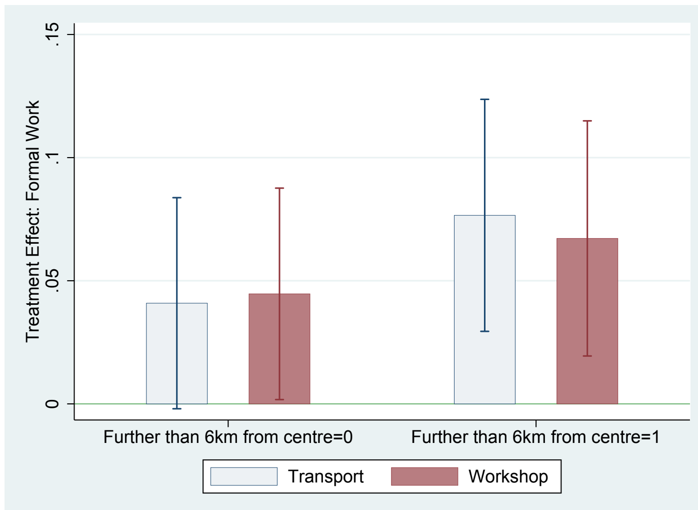
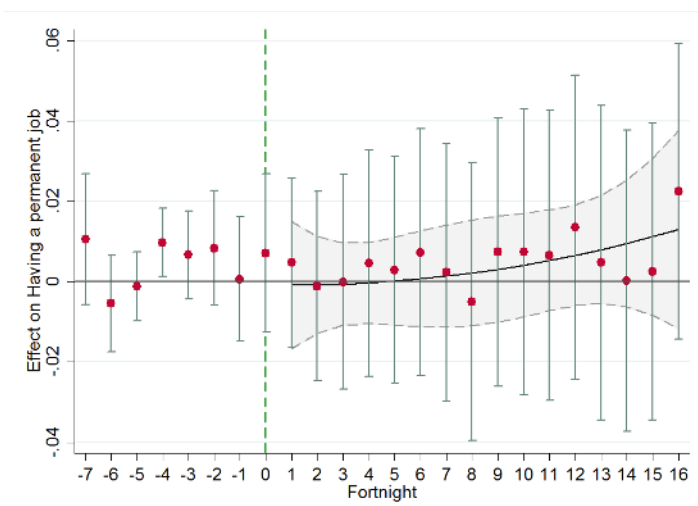


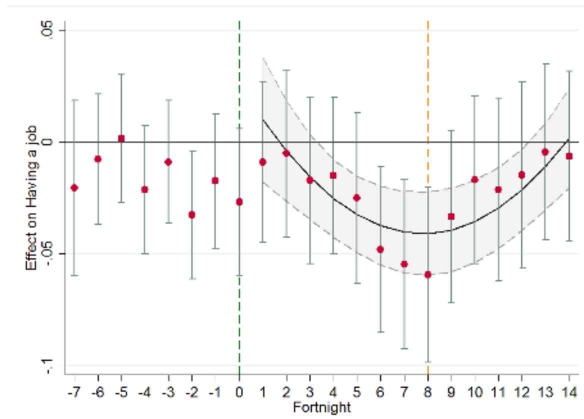
Figure A.17: **Impact trajectory of the job application workshop on permanent employment**



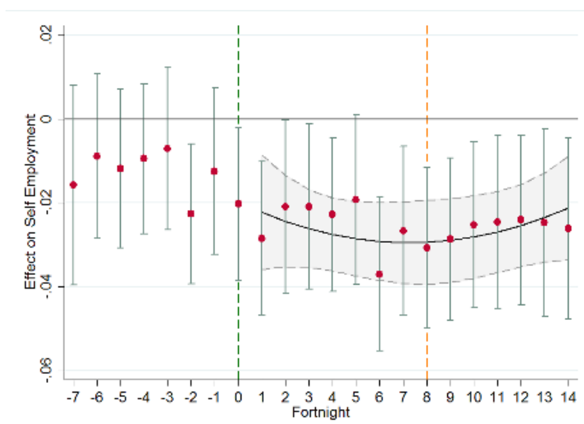
The green dotted line indicates the fortnight when the workshop takes place.

Figure A.18: **Impact trajectory of the transport treatment on other employment outcomes**

(a) Impact on Employment

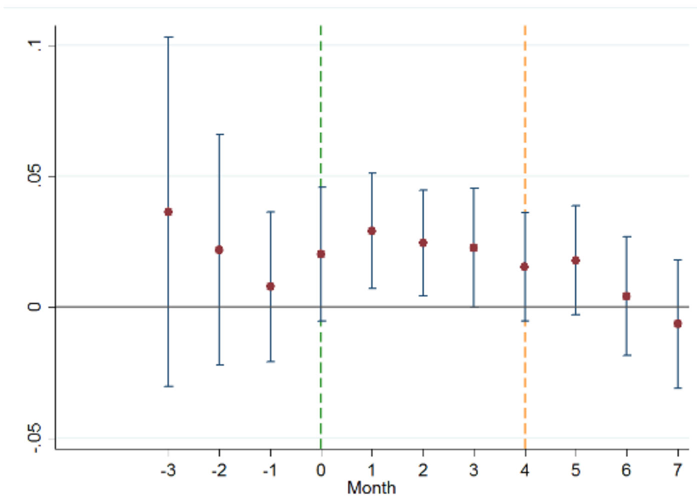


(b) Impact on Self-Employment



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.

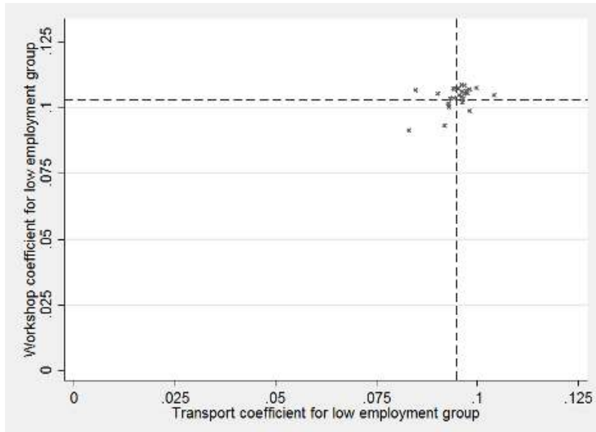
Figure A.19: **Impact trajectory of the transport treatment: Travelled to city centre**



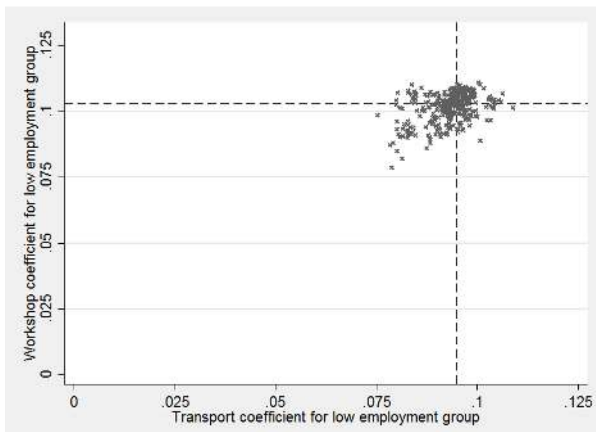
The green dotted line indicates the month when the treatment begins.
The orange dotted line indicates the month when the treatment ends.

Figure A.20: **Robustness of stratification to predictor covariates**

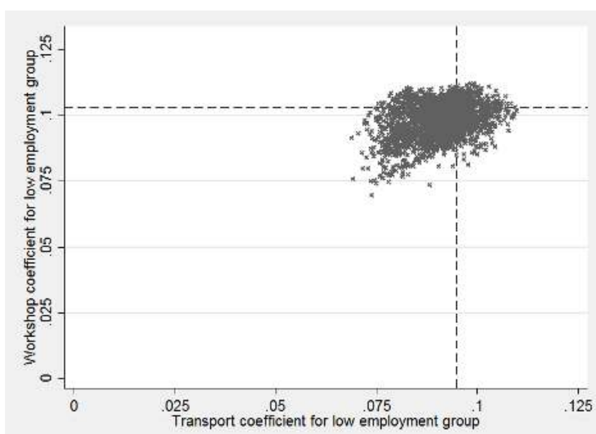
(a) Effects of dropping each of the 26 predictor covariates



(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 4; 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: $(0.095, 0.103)$.

Figure A.21: **Optimal search**

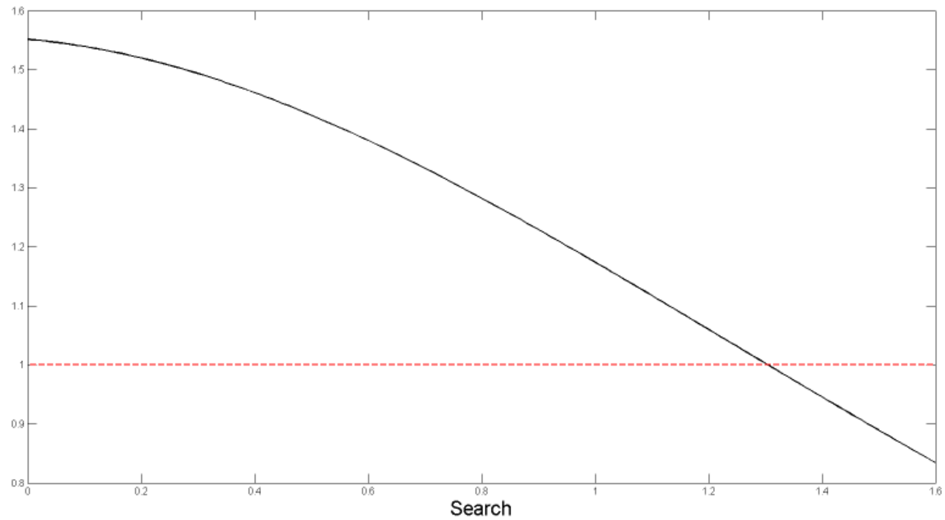


Figure A.22: **Optimal search as search costs change**

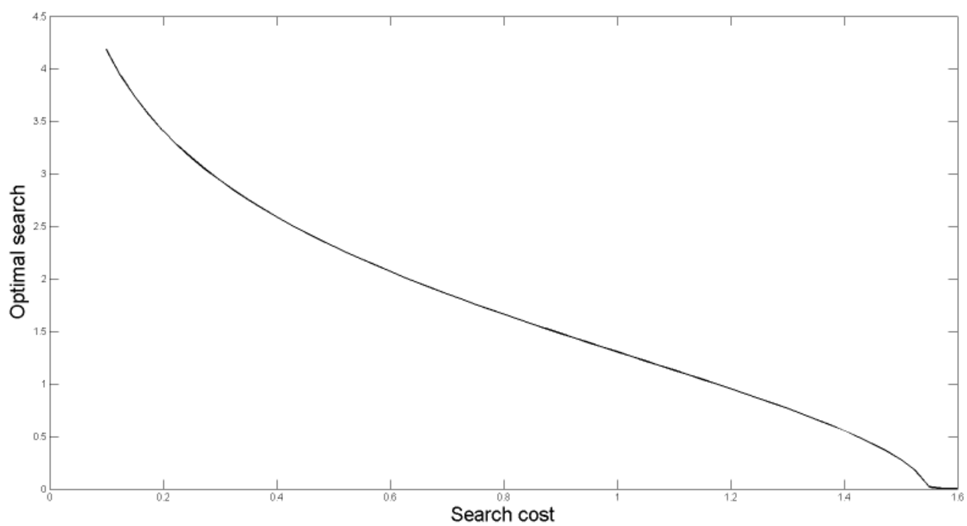


Figure A.23: **Good employment as search costs change**

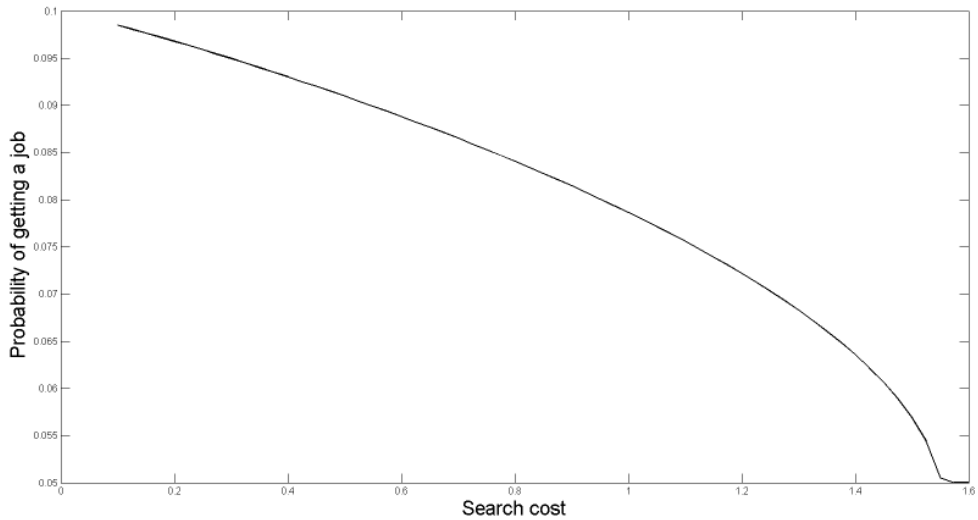


Figure A.24: **Good employment as search efficacy changes**

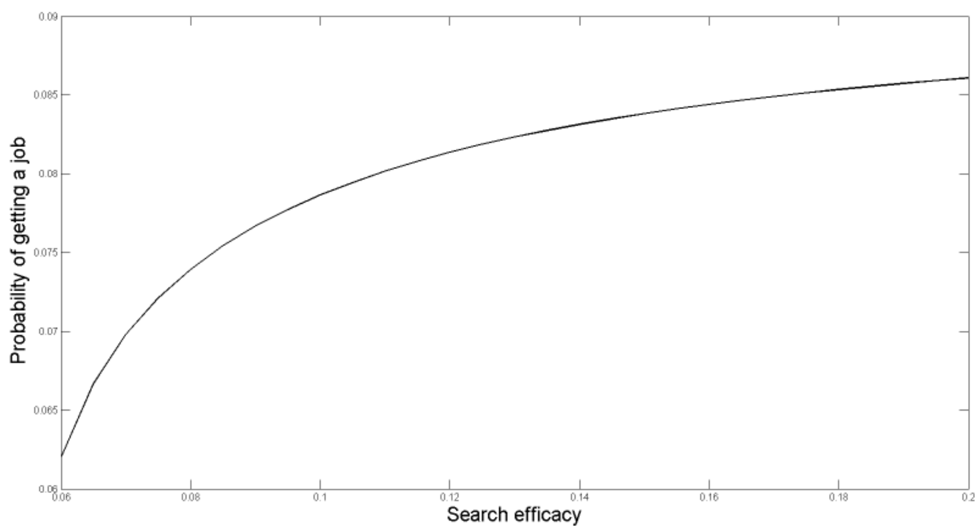


Table A.1: **Summary statistics of the tests administered in the job application workshop**

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**

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 take-up**

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

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

Table A.10: Logit of endline work on baseline covariates in the control group (estimated coefficients)

Baseline: Age	0.010
	(0.048)
Baseline: Dummy: Female	-0.161
	(0.210)
Baseline: Dummy: Married	-0.477
	(0.282)*
Baseline: Dummy: Respondent is ethnically Oromo	0.214
	(0.255)
Baseline: Dummy: Respondent is ethnically Amhara	0.138
	(0.219)
Baseline: Dummy: Born outside of Addis	0.273
	(0.243)
Baseline: Dummy: Vocational education (reference: high school)	0.156
	(0.224)
Baseline: Dummy: Diploma (reference: high school)	-0.455
	(0.366)
Baseline: Dummy: Degree (reference: high school)	0.871
	(0.351)**
Baseline: Years since finished formal education	0.029
	(0.049)
Baseline: Dummy: Respondent lives with mother or father	0.505
	(0.236)**
Baseline: Distance to city centre	0.014
	(0.043)
Baseline: Number of trips to central Addis Ababa in last 7 days	-0.061
	(0.048)
Baseline: Search frequency	0.129
	(0.337)
Baseline: Dummy: Searched for work in the last 6 months	0.488
	(0.244)**
Baseline: Number of applications for permanent jobs made in the last 12 months	0.336
	(0.189)*
Baseline: Contract worker	0.780
	(0.446)*
Baseline: Casual worker	0.993
	(0.445)**
Baseline: Temporarily employed	1.277
	(0.281)***
Baseline: Self-employed	1.932
	(0.469)***
Baseline: Dummy: Respondent has work experience in a permanent job	0.577
	(0.305)*
Baseline: Dummy: Respondent has certificates used for job applications	-0.702
	(0.332)**
Baseline: Dummy: Respondent has a CV used for job applications	0.398
	(0.325)
Baseline: Dummy: Savings is above the median	0.143
	(0.211)
Baseline: Total expenditure in last 7 days	0.000
	(0.000)
Baseline: Reservation wage	0.000
	(0.000)
Constant	-1.694
	(1.045)
Obs.	744
Pseudo-R^2	0.120

Table A.11: 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	≤ 5.8 Km		> 5.8 Km	≤ 5.8 Km		> 5.8 Km	≤ 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

Table A.12: Heterogeneous effects by predicted probability of permanent employment at endline

	Worked	Hours worked	Formal work	Permanent work	Temporary work	Self-employment	Earnings	Employed earnings	Satisfaction with work
High-employment group:									
Transport	0.003	-0.716	0.035	-0.038	0.057	-0.023	-94.042	-124.850	-0.013
	(0.034)	(1.858)	(0.035)	(0.032)	(0.038)	(0.021)	(139.960)	(193.051)	(0.036)
Workshop	0.035	1.064	0.021	-0.000	0.012	0.006	-29.109	-118.777	0.033
	(0.036)	(2.095)	(0.033)	(0.033)	(0.037)	(0.023)	(142.121)	(192.056)	(0.036)
Low-employment group:									
Transport	0.066	0.595	0.064	0.061	0.010	-0.018	36.718	-39.903	0.005
	(0.038)*	(2.012)	(0.025)**	(0.020)***	(0.038)	(0.022)	(89.675)	(157.407)	(0.034)
Workshop	0.034	-0.058	0.071	0.097	-0.053	-0.011	110.734	177.204	0.018
	(0.040)	(2.053)	(0.024)***	(0.023)***	(0.039)	(0.023)	(104.575)	(191.068)	(0.035)
Obs.	2178	2173	2178	2178	2178	2178	2147	1240	2178
Control group means:									
High-employment group	0.649	29.261	0.330	0.282	0.272	0.092	1479.231	2285.533	0.274
Low-employment group	0.481	23.379	0.121	0.059	0.326	0.095	840.068	1752.401	0.206
Tests of common effects:									
Transport (p)	0.157	0.579	0.503	0.003***	0.349	0.865	0.381	0.698	0.655
Workshop (p)	0.987	0.672	0.218	0.008***	0.115	0.579	0.378	0.230	0.727
Tests among 'low employment':									
Zero effects (p)	0.230	0.937	0.004***	0.000***	0.215	0.715	0.570	0.477	0.879
Common effects (p)	0.442	0.747	0.787	0.192	0.100*	0.713	0.473	0.230	0.744

Table A.13: **Effects on main outcomes by gender**

	Transport			Job Application Workshop			Control Mean		
Outcome	Male	Female	*F(p)*	Male	Female	*F(p)*	Male	Female	N
Worked	0.0580 (.042) [.988]	0.0240 (.042) [1]	0.569	-0.00500 (.043) [1]	0.0370 (.042) [1]	0.460	0.621	0.462	2841
Hours worked	-0.310 (2.299) [1]	0.227 (2.293) [1]	0.871	-0.779 (2.162) [1]	0.221 (2.323) [1]	0.759	28.50	23	2835
Formal work	0.0430 (.029) [.988]	0.0630 (.028)** [.194]	0.638	0.00500 (.031) [1]	0.0900 (.027)*** [.011]**	0.0427	0.195	0.152	2841
Perm. work	-0.00700 (.027) [1]	0.0590 (.024)** [.194]	0.0558	0.0620 (.029)** [.136]	0.0700 (.026)*** [.045]**	0.834	0.138	0.104	2841
Self-employed	-0.0210 (.029) [1]	-0.0170 (.02) [1]	0.908	-0.00300 (.03) [1]	-0.00200 (.02) [1]	0.980	0.128	0.0786	2841
Monthly earnings	-20.85 (142.199) [1]	16.00 (69.94) [1]	0.816	63.64 (151.875) [1]	57.30 (86.198) [1]	0.971	1389	601	2802
Satis. with work	-0.0240 (.04) [1]	0.0150 (.034) [1]	0.457	0.0180 (.043) [1]	0.0240 (.035) [1]	0.916	0.285	0.184	2841

Note. In this table we report, separately for each gender, the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on primary employment outcomes. These are obtained by OLS estimation of equation (5), 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 columns 3 and 6 we report the *p*-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for men and women. In the last two columns we report the mean outcome for men and women in the control group.***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table A.14: **Effects on main outcomes by education**

	Transport				Job Application Workshop				Control Mean			
Outcome	Hi Sch.	Voc.	Dip/Deg	*F(p)*	Hi Sch.	Voc.	Dip/Deg	*F(p)*	Hi Sch.	Voc.	Dip/Deg	N
Worked	0.0520 (.044) [1]	0.0370 (.043) [1]	-0.0210 (.045) [1]	0.518	-0.00700 (.046) [1]	0.0620 (.035)* [.669]	0.0340 (.047) [1]	0.408	0.508	0.564	0.609	2841
Hours worked	0.475 (2.416) [1]	-0.729 (2.014) [1]	-0.931 (2.225) [1]	0.898	-2.205 (2.365) [1]	2.106 (1.807) [1]	3.446 (2.293) [.993]	0.204	24.80	27.10	25.80	2835
Formal work	0.0710 (.029)** [.175]	0.0370 (.033) [1]	0.00700 (.046) [1]	0.536	0.0690 (.029)** [.167]	0.0350 (.03) [1]	-0.00200 (.045) [1]	0.401	0.108	0.216	0.370	2841
Perm. work	0.0590 (.025)** [.175]	-0.00600 (.03) [1]	-0.0340 (.043) [1]	0.0795	0.106 (.028)*** [.003]***	0.00800 (.026) [1]	0.00600 (.047) [1]	0.0225	0.0583	0.169	0.300	2841
Monthly earnings	91.75 (99.124) [1]	-128.0 (102.803) [1]	-153.6 (246.833) [1]	0.242	70.08 (111.817) [1]	63.43 (121.759) [1]	-9.763 (261.076) [1]	0.960	780	1057	1657	2802
Satis. with work	0.00900 (.035) [1]	-0.0240 (.037) [1]	-0.00700 (.042) [1]	0.751	0.00400 (.036) [1]	0.0420 (.038) [1]	0.0550 (.045) [1]	0.578	0.225	0.238	0.245	2841

Note. In this table we report, separately for each education category, the *intent-to-treat* estimates of the direct of the transport intervention and the job application workshop on primary employment outcomes. These are obtained by OLS estimation of equation (5). 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 columns 3 and 6 we report the p-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for individuals with different levels of education. In the last three columns we report the mean outcome in the control group for the different education categories.***p< 0.01, **p<0.05, *p<0.1.

Table A.15: **Family indices**

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

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

Table A.16: **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.17: **Financial outcomes**

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

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

Table A.18: **Expectations, aspirations, reservation wages**

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

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

Table A.19: **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.20: **Education and training**

Outcome	Transport	Job App. Workshop	Spillover 1	Spillover 2	Control Mean	F	N
In full-time education	-0.00700 (.008) [.777]	0.00100 (.01) [1]	0.00300 (.011) [1]	0.0330 (.022) [.203]	0.0210	0.387	2841
In part-time education	-0.0480 (.02)** [.11]	-0.0330 (.023) [.52]	-0.0140 (.026) [1]	-0.0200 (.031) [.466]	0.138	0.453	2841
In informal training	-0.00900 (.016) [.777]	-0.0100 (.015) [.696]	-0.00700 (.016) [1]	-0.0430 (.013)*** [.008]***	0.0470	0.951	2841
Graduated (in past 12m)	0.0120 (.017) [.777]	-0.0130 (.016) [.696]	0.0150 (.02) [1]	-0.0180 (.023) [.453]	0.0770	0.121	2841
Graduated from vocational degree (in past 12m)	0.0160 (.011) [.45]	0.00700 (.01) [.696]	0.00500 (.012) [1]	0.00300 (.016) [.729]	0.0240	0.380	2841
Graduated from training (in past 12m)	0 (.014) [1]	-0.0230 (.012)* [.475]	0.0190 (.016) [1]	-0.0280 (.012)** [.061]*	0.0440	0.0730	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 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.21: **Psychological outcomes**

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

Note. In this table we report the *intent-to-treat* estimates of the direct and indirect effects of the transport intervention and the job application workshop on 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.22: **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	-0.601	-0.612	-0.425	5.182	0.464	2807
	(.372)	(.348)*	(.377)	(.468)			
	[1]	[.504]	[.724]	[1]			
Number of people with permanent jobs in job network	0.118	0.121	-0.0680	0.394	2.178	0.987	2528
	(.212)	(.233)	(.246)	(.306)			
	[1]	[.778]	[1]	[1]			
Can access guarantor for job (in next month)	-0.00500	-0.0660	-0.0240	-0.00400	1.244	0.235	2504
	(.054)	(.054)	(.06)	(.068)			
	[1]	[.504]	[1]	[1]			
No. meetings of voluntary associations attended (past 30d)	0.0100	0.00900	-0.0330	-0.0540	0.119	0.984	2841
	(.061)	(.063)	(.069)	(.062)			
	[1]	[.802]	[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 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.23: **Effects on employment outcomes by job search at baseline**

	Transport			Job Application Workshop			Control Mean		
Outcome	Lo Search Intens.	Hi Search Intens.	*F(p)*	Lo Search Intens.	Hi Search Intens.	*F(p)*	Lo Search Intens.	Hi Search Intens.	N
Worked	0.0370 (.043) [1]	0.0380 (.044) [1]	0.988	0.0520 (.044) [.507]	-0.0130 (.043) [.677]	0.285	0.514	0.559	2841
Hours worked	-1.319 (2.416) [1]	1.110 (2.306) [1]	0.486	0.194 (2.484) [.677]	-0.610 (2.157) [.677]	0.816	25.60	25.60	2835
Formal work	0.0740 (.029)** [.158]	0.0330 (.028) [1]	0.338	0.0650 (.03)** [.216]	0.0380 (.03) [.507]	0.556	0.149	0.194	2841
Perm. work	0.0410 (.026) [1]	0.0190 (.027) [1]	0.563	0.0950 (.03)*** [.021]**	0.0410 (.027) [.507]	0.191	0.106	0.134	2841
Monthly earnings	68.71 (109.409) [1]	-64.03 (98.611) [1]	0.353	172.9 (129.422) [.507]	-42.94 (115.688) [.677]	0.234	921	1021	2802
Satis. with work	0.0260 (.038) [1]	-0.0290 (.037) [1]	0.298	0.0570 (.039) [.507]	-0.0120 (.036) [.677]	0.180	0.208	0.254	2841

Note. In this table we report heterogeneous treatment effects for individuals that were searching above and below the median number of weeks in the three months after being surveyed and before the start of the interventions. For each group, we report the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by OLS estimation of equation (5). 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 columns 3 and 6 we report the p-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for individuals with different levels of education. In the last three columns we report the mean outcome in the control group for the different education categories. ***$p< 0.01$, **$p<0.05$, *$p<0.1$.

Table A.24: **Effects on employment outcomes by experience in permanent employment**

	Transport			Job Application Workshop			Control Mean		
Outcome	Never Perm. Job	Had Perm. Job	*F(p)*	never Perm. Job	Had Perm. Job	*F(p)*	Never Perm. Job	Had Perm. Job	N
Worked	0.0400 (.031) [1]	0.0300 (.074) [1]	0.905	0.0200 (.033) [1]	0.00200 (.077) [1]	0.827	0.523	0.649	2841
Hours worked	-0.420 (1.686) [1]	3.580 (4.232) [1]	0.369	-0.398 (1.675) [1]	1.349 (4.313) [1]	0.707	25.20	29.10	2835
Formal work	0.0590 (.02)*** [.052]*	0.00700 (.071) [1]	0.495	0.0540 (.021)*** [.057]*	0.0330 (.072) [1]	0.782	0.158	0.293	2841
Perm. work	0.0360 (.02)* [.586]	-0.0290 (.069) [1]	0.379	0.0710 (.02)*** [.004]***	0.0300 (.075) [1]	0.598	0.103	0.269	2841
Monthly earnings	-44.30 (75.873) [1]	391.7 (256.399) [.73]	0.0954	70.40 (85.236) [1]	-34.48 (227.816) [1]	0.654	928	1348	2802
Satis. with work	-0.00300 (.028) [1]	0.00300 (.077) [1]	0.940	0.0210 (.028) [1]	0.0200 (.079) [1]	0.985	0.223	0.305	2841

Note. In this table we report heterogenous treatment effects for individuals with and without experience in permanent employment. For each group, we report the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by OLS estimation of equation (5). 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 columns 3 and 6 we report the *p*-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for individuals with different levels of education. In the last three columns we report the mean outcome in the control group for the different education categories. ***$p< 0.01$, **$p<0.05$, *$p<0.1$.

Table A.25: **Effects on employment outcomes by savings**

	Transport			Job Application Workshop			Control Mean		
Outcome	Below Med.	Above Med.	*F(p)*	Below Med.	Above Med.	*F(p)*	Below Med.	Above Med.	N
Worked	0.114 (.059)* [.328]	0.0150 (.031) [.597]	0.117	0.100 (.062) [.181]	-0.0100 (.034) [.544]	0.110	0.537	0.537	2841
Hours worked	4.863 (3.12) [.356]	-1.581 (1.745) [.421]	0.0574	3.782 (3.238) [.27]	-1.617 (1.711) [.299]	0.132	24.90	25.80	2835
Formal work	0.0980 (.039)** [.169]	0.0390 (.023)* [.328]	0.218	0.0780 (.042)* [.164]	0.0420 (.022)* [.164]	0.451	0.190	0.166	2841
Perm. work	0.0490 (.037) [.369]	0.0240 (.02) [.369]	0.532	0.0480 (.038) [.259]	0.0720 (.022)*** [.012]**	0.565	0.149	0.110	2841
Monthly earnings	248.8 (143.984)* [.328]	-79.72 (90.923) [.421]	0.0596	298.7 (145.379)** [.164]	-21.62 (99.364) [.544]	0.0710	925	987	2802
Satis. with work	0.0310 (.053) [.597]	-0.0130 (.029) [.597]	0.448	0.106 (.049)** [.164]	-0.00800 (.03) [.544]	0.0381	0.219	0.235	2841

Note. In this table we report heterogenous treatment effects for individual with baseline savings above and below the median. For each group, we report the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by OLS estimation of equation (5). 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 columns 3 and 6 we report the p-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for individuals with different levels of education. In the last three columns we report the mean outcome in the control group for the different education categories. ***p< 0.01, **p<0.05, *p<0.1.

Table A.26: **Job search**

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

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

Table A.27: **Effects on search outcomes by education**

	Transport				Job Application Workshop				Control Mean			
Outcome	Hi Sch.	Voc.	Dip/Deg	*F(p)*	Hi Sch.	Voc.	Dip/Deg	*F(p)*	Hi Sch.	Voc.	Dip/Deg	N
Applied to temporary jobs	0.494	0.209	-0.0130	0.646	-0.0400	0.0520	-0.174	0.919	0.887	1.380	1.710	2832
	(.326)	(.308)	(.711)		(.227)	(.258)	(.658)					
	[1]	[1]	[1]		[1]	[1]	[1]					
Applied to permanent jobs	-0.230	0.190	0.184	0.369	0.0550	-0.219	0.135	0.653	0.854	1.940	4.390	2827
	(.21)	(.358)	(.858)		(.227)	(.293)	(.831)					
	[1]	[1]	[1]		[1]	[1]	[1]					
Interviews/Applications	-0.0440	-0.0160	-0.0340	0.888	-0.0770	0.0160	-0.0100	0.329	0.337	0.371	0.347	1584
	(.05)	(.038)	(.044)		(.045)*	(.04)	(.044)					
	[1]	[1]	[1]		[1]	[1]	[1]					
Offers/Applications	0.0570	-0.0490	-0.0370	0.288	0.0100	-0.0250	0.0280	0.651	0.263	0.291	0.177	1586
	(.061)	(.051)	(.036)		(.059)	(.055)	(.043)					
	[1]	[1]	[1]		[1]	[1]	[1]					
Interviews/Applications (Perm)	0.0210	-0.00100	-0.0290	0.821	0.00900	0.0210	0.00400	0.971	0.287	0.351	0.323	1240
	(.072)	(.046)	(.048)		(.064)	(.048)	(.051)					
	[1]	[1]	[1]		[1]	[1]	[1]					
Offers/Applications (Perm)	0.114	0.0140	-0.0350	0.109	0.120	-0.00800	0.00600	0.0756	0.0957	0.181	0.158	1238
	(.061)*	(.043)	(.041)		(.045)***	(.045)	(.051)					
	[1]	[1]	[1]		[.277]	[1]	[1]					
Interviews/Applications (Temp)	-0.0900	-0.00800	-0.0790	0.473	-0.122	0.0150	0.00200	0.210	0.375	0.397	0.390	986
	(.063)	(.054)	(.059)		(.064)*	(.056)	(.066)					
	[1]	[1]	[1]		[1]	[1]	[1]					
Offers/Applications (Temp)	-0.0400	-0.0520	0.00900	0.702	-0.0810	-0.0260	0.0990	0.145	0.362	0.381	0.211	986
	(.073)	(.058)	(.056)		(.081)	(.062)	(.063)					
	[1]	[1]	[1]		[1]	[1]	[1]					
Uses CV for applications	-0.0280	0.104	0.0300	0.00890	0.0290	0.0690	0.0380	0.727	0.192	0.376	0.686	2841
	(.037)	(.037)***	(.053)		(.042)	(.036)*	(.053)					
	[1]	[.187]	[1]		[1]	[1]	[1]					
Uses certificates	-0.00200	0.105	0.0780	0.162	0.0690	-0.00100	0.0920	0.279	0.296	0.533	0.610	2841
	(.047)	(.051)**	(.06)		(.058)	(.056)	(.056)*					
	[1]	[1]	[1]		[1]	[1]	[1]					

Note. In this table we report, separately for each education category, the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on job search outcomes. These are obtained by OLS estimation of equation (5). 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 columns 3 and 6 we report the *p*-value from F-tests of the null hypotheses that transport subsidies and the job application workshop, respectively, have the same effect for individuals with different levels of education. In the last three columns we report the mean outcome in the control group for the different education categories. ***p< 0.01, **p<0.05, *p<0.1.

Table A.28: **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.29: **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.30: **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.31: **Predicted skills and employment outcomes: All job-seekers**

	Work	Permanent work	Formal work	Earnings
	(1)	(2)	(3)	(4)
Predicted score	.053 (.022)**	.047 (.017)***	.063 (.019)***	352.393 (102.618)***
Workshop	.036 (.029)	.034 (.021)	.041 (.022)*	56.538 (115.825)
Predicted score * workshop	-.022 (.029)	-.018 (.023)	-.044 (.028)	-35.074 (141.522)
Const.	.560 (.022)***	.171 (.016)***	.222 (.014)***	1132.593 (87.763)***
Obs.	1463	1463	1463	1448

Note. In each column we report the results of an ordinary least squares regression of the outcome in the column heading on predicted grades, a dummy for being invited to the job workshop and the interaction of these two variables. In parentheses, we report standard errors obtained through a bootstrapping procedure for generated regressors. The sample includes all individuals in the control group and in the job application group. ***p< 0.01, **p<0.05, *p<0.1.

Table A.32: **Predicted skills and employment outcomes: Job-seekers with at most a high-school degree**

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

Note. In each column we report the results of an ordinary least squares regression of the outcome in the column heading on predicted grades, a dummy for being invited to the job workshop and the interaction of these two variables. In parentheses, we report standard errors obtained through a bootstrapping procedure for generated regressors. The sample includes all individuals with at most a high school degree in the control group and in the job application group. ***p< 0.01, **p<0.05, *p<0.1.

Table A.33: **When did the impacts of treatment take place?**

	(1)	(2)
Transport	.013	.025
	(.021)	(.020)
Job App. Workshop	.018	.064
	(.020)	(.021)***
Control mean	.151	.177
Obs.	2496	2496
Sample	Last phone interview	Endline interview

Note. In this table we report the *intent-to-treat* estimates of the effects of the two treatments on permanent employment. The sample is restricted to individuals who are interviewed both in the last wave of the phone survey and in the endline survey. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters.***p< 0.01, **p<0.05, *p<0.1

Table A.34: **When did the impacts of treatment take place? Transitions in and out of permanent employment across surveys**

	Out of permanent employment	Into permanent employment	New permanent employment
Transport	.007	.019	.019
	(.017)	(.016)	(.007)**
Job App. Workshop	-.016	.031	.027
	(.014)	(.016)*	(.008)***
Obs.	2496	2496	2496

Note. In this table we report the *intent-to-treat* estimates of the effects of the two treatments on transitions in and out of permanent work between the last phone survey and the endline survey. Column three reports effects on the probability of finding a new permanent job between the two surveys, using endline data on the date of the start of employment. The sample is restricted to individuals who are interviewed both in the last wave of the phone survey and in the endline survey. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters.***p< 0.01, **p<0.05, *p<0.1

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