

# Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City

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

*Key words:* Labour markets, Job search, Information, Transport, Cities, Development, Ethiopia, Jobs, Wages.

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## 1. AN EXPERIMENT TO HELP YOUTH FIND BETTER JOBS

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

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

To investigate which of these two competing views is more accurate, we run an experiment with two parallel treatment arms. The first intervention—aimed at reducing the cost of job search—is a *transport subsidy*. Participants are reimbursed, up to three times a week, for the cost of a bus fare from their place of residence to the centre of the city, where they can find information about jobs and visit firms. The second intervention—aimed at improving the ability to signal skills—is a *job application workshop*. We certify young people's general skills using a mix of standardized personnel selection tests. Further, we offer orientation on how to signal skills in job applications and job interviews. The experiment is conducted with a representative sample of over 3,000 young people in Addis Ababa, Ethiopia. We evaluate these interventions with two endline surveys taking place 8 months and 4 years after the end of treatment, respectively.

We find starkly different results from the two interventions. The transport intervention increases job-search intensity and increases the probability of having a formal job 8 months after treatment. However, 4 years after treatment, these effects have dissipated completely. In other words, lowering the search cost gets young workers a formal job faster, but it does not change their long-term employment outcomes. The job application workshop, in contrast, shows long-lasting effects. In the short run, it increases the probability of permanent as well as formal work *without* increasing the intensity of job search. Four years after treatment, the workshop shows a large positive impact on earnings, amounting to a 25% increase over the control group mean. These earning gains are particularly impressive when contrasted with the trajectory of individuals in the control group. While it is relatively easy for control individuals to find work—they reach a 70% employment rate by the second endline—higher salaries remain out of their reach: among respondents in the control group, wages only grow at roughly the rate of inflation.

These findings show that while both the inability to signal skills and the cost of job search are significant temporary barriers to formal employment, only the inability to signal skills impacts labour market outcomes in the long run. To explain why this may be the case, we use a simple theoretical framework and several supporting empirical results. In the framework, firms and workers meet each other at a frequency determined by the intensity of workers' job search. When they meet, the firm observes a noisy signal of match quality (*i.e.* the suitability of the worker's skills for the position) and then decides whether to offer a formal employment contract. Policies that subsidize job search—such as the transport intervention—increase the rate at which firms

and workers meet each other. As a result, workers get formal jobs faster. However, when job-search support is withdrawn, the control group progressively catches up and thus the treatment effect dissipates over time. Further, these policies do not change firms' ability to identify suitable workers and thus leave match quality unchanged. On the other hand, policies that improve the information job seekers convey about themselves—such as the job application workshop—help workers to find formal jobs, but also enable firms to target their offers more effectively, and can thus improve match quality. Higher match quality will in turn translate into higher earnings, possibly with a delay due to wage-setting frictions. Unlike the employment effects, the match quality and earnings effects will persist over time: the control group has access to a noisier matching technology and is thus not able to close the gap in employment quality.

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

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

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

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

time of the second endline, we observe a 32% earnings gap between control individuals who had permanent work experience at the beginning of the study and those who did not; this gap is eliminated for young people in the workshop group.

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

Further, this is the first study that directly compares the impacts of two different active labour market interventions and, in doing so, is able to quantify the relative importance of two types of labour market frictions. In line with [Franklin \(2018\)](#) and [Phillips \(2014\)](#), who study the short-term impacts of transport subsidies on non-representative samples, we find confirmation that search costs are a significant barrier to job search. However, in our representative sample these effects are weak and ultimately short lived. These findings also complement a recent literature showing that transport subsidies have persistent effects when they connect rural workers to urban jobs ([Bryan \*et al.\*, 2014](#)): such interventions relax constraints that are likely to be different from those at play in a population already exposed to an urban labour market like ours.<sup>3</sup>

Our study overcomes some of the shortcomings in the recent experimental literature on active labour market interventions in developing economies (as reviewed *e.g.* by [McKenzie \(2017\)](#)). First, as mentioned above, we work with a large representative sample that we follow up to 4 years after the intervention. In comparison, other studies often rely on populations of youth selected along a particular economic dimension (*e.g.* whether they have been searching for work, or are part of a specific government program), and they typically document short-term impacts only. Second, we have low attrition, even in the 4-year follow-up survey.<sup>4</sup> Third, we follow a

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

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

pre-analysis plan that specifies all of our main outcomes of interest.<sup>5</sup> This enables us to formally control for multiple hypotheses testing—all of our main results are robust to this correction—and it eliminates concerns about selective reporting. Fourth, we combine face-to-face survey data with a high-frequency phone questionnaire. This enables us to document the mechanisms through which job seekers find better jobs and to analyse their immediate response to each intervention in a way that recall data would not permit. Lastly, we are able to study the key issue of match quality by using data on long-run earnings and employment duration, which few studies in this literature have been able to do.

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

## 2. INTERVENTIONS AND CONCEPTUAL FRAMEWORK

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

In many developing countries, where informal employment is readily available, employment rates are often high by international standards and workers typically work longer hours compared to developed countries (Feng *et al.*, 2017; Bick *et al.*, 2018). However, available jobs are often of poor quality: they offer limited formal protections, low tenure security, and slow wage growth (Banerjee and Duflo, 2007; AfDB, 2012; Lagakos *et al.*, 2018), and job separations are more frequent than in developed countries (Donovan *et al.*, 2018). These challenges are particularly severe for young workers.

The labour market in Addis Ababa, the growing capital city of Ethiopia where this study is conducted, exemplifies these broad trends. First, informal work is very common and very often temporary and unstable. While 65% of the young individuals in the control group of our study are employed by the time of the second endline survey, only 25% have a formal job with an open-ended contract (throughout the rest of the article, we define a job that has a written contract with the employer as a *formal job*, and a job that has an open-ended contract with no fixed duration as a *permanent job*). This is representative of the outcomes of young people under the age of 30 in the city; older workers, on the other hand, have rates of formal permanent employment that are 40% higher than those of the under 30s (see [Supplementary Appendix Table A.3](#)). Second, real wage growth is weak, particularly for informal and short-term jobs. In our sample, the earnings of control group workers do not grow in real terms in the 3 years between the two endline surveys. Individuals who are employed at both endlines experience some real wage growth over the same period, but this is about twice as high for people in formal and permanent employment compared to workers in informal or temporary jobs. Stable jobs with formal contracts are thus highly sought by young Ethiopians.<sup>7</sup> Third, worker turnover rates are high, pointing specifically to poor match

5. This plan was registered at [www.socialscisearch.org/trials/911](http://www.socialscisearch.org/trials/911).

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

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

quality. For example, in a sample of 500 local employers, we find that churning (defined as hires and separations over and beyond those required by adjustments in firm size) accounts for about 65% of all worker flows—a higher figure than what [Kerr \(2018\)](#) recently reports for South Africa. Worker turnover is also the most commonly reported HR problem among firms in the same survey.<sup>8</sup>

It is unclear what precisely prevents high-quality matches from forming. While causal evidence on this question is scarce, several pieces of descriptive evidence suggest that search frictions can be a major driver of low match quality in developing countries' labour markets. The cost of job search constitutes a first likely source of friction. In Addis Ababa, for example, a significant amount of information about jobs is disseminated through job vacancy boards located in the centre of the city.<sup>9</sup> Effective job search thus requires frequent trips to the centre of town to consult this information. In addition, job seekers need to spend money to buy newspapers, print CVs and cover letters, and travel to employers for job applications and interviews. As a result, the median baseline job-search expenditure for an active job-seeker amounts to about 16% of his or her overall expenditure.<sup>10</sup> These costs are larger for people who live farther away from the city centre. In our baseline, we document that living 10 km closer to the centre of the city is associated with visiting the job boards 6.7 more times in a year (0.4 of a standard deviation) and making 1.9 more applications to permanent jobs (0.5 of a standard deviation).

A second potential source of friction relates to information about skills. In Addis Ababa, firms often mention that the recent expansion of the higher education system has made it more challenging to identify high-ability candidates. Further, career advice or job search assistance is almost completely lacking from high school and university curricula. Many young job seekers are thus not familiar with the process and the standards of job applications. For example, while firms report valuing a well-written CV, 41% of the study participants who have applied for at least one job in the last 6 months have not prepared a CV to support their applications. In the absence of good signals about skills and ability, firms often resort to selecting workers on the basis of previous work experience or job referrals ([Serneels, 2007](#); [Caria, 2015](#)).<sup>11</sup> This puts young people at a disadvantage, as they have little work experience and less extensive networks,<sup>12</sup> but is also inefficient for firms. Anecdotally, managers often complain about the poor quality of job applications and express a demand for job-search training to be implemented as part of the education system.

In light of these challenges, we devised two interventions to reduce the cost of job search and help workers to signal their abilities to employers. Among the available options, we chose two relatively low-cost interventions that could be easily implemented in other contexts. In the rest of this section we describe these interventions and we then use a simple theoretical framework

8. The firm survey is described in detail in [Abebe et al. \(2017\)](#). We observe similar patterns of turnover and churning using the 1-year phone panel survey that we collected for this study (and which we discuss in detail in Section 3 below). Average employment spells among job seekers in this panel are short (72% of jobs are terminated within the first 3 months) and irregular (temporary workers did not work on average 12% of the weeks since they got the job, compared to only 2% for permanent workers), and job insecurity is high (in 82% of job terminations, the worker is unable to find another job right away).

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

10. This goes up to 25% for job seekers who report searching 6 days a week.

11. Fifty-six percent of firms report that for blue collar positions they only consider candidates with sufficient work experience, and 63% of firms use this selection method for white collar positions.

12. Fifty-five percent of the participants in our study report having less than 1 year of work experience and only 16% have ever worked in a permanent job.



to motivate a number of testable predictions about the effects of these interventions in a labour market where job search is costly and firms are uncertain about worker skills.

## 2.2. *Treatment 1: The job application workshop*

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

The intervention takes place over 2 days. On the first day, participants take a series of personnel selection tests. On the second day, they attend the orientation session. The intervention was administered by the School of Commerce of Addis Ababa University, between September and October 2014. The School of Commerce has a reputation for reliable personnel selection services; many firms screen applicants using tests developed, and sometimes administered, by the School of Commerce.<sup>14</sup>

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: (1) a Raven matrices test, (2) a test of linguistic ability in Amharic, (3) a test of mathematical ability and (4) a "work-sample" test. The results of the tests are presented in a certificate, which job seekers can use in support of their job applications. The certificates are officially issued by the School of Commerce and the Ethiopian Development Research Institute.<sup>15</sup> The certificates explain the nature of the tests and report the relative grade of the individual for each test, and an aggregate measure of performance. We report relative performance using bands: a band for the bottom 50% of the distribution and then separate bands for individuals in the upper deciles of the distribution: 50–60%, 60–70%, 70–80%, 80–90%, and 90–100%. This enables us to investigate the effects of the information disclosed in the certificates in a regression discontinuity framework.

We chose the tests on the basis of the results of several qualitative interviews with firm managers in the city.<sup>16</sup> 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 (Beaman *et al.*, 2013; Dal Bó *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

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

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

15. Participants collect the final certificates from the School of Commerce, after all testing sessions are completed. To minimize 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.

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

(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 organizational 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 [Supplementary Appendix Table A.1](#).<sup>17</sup> Per person, the intervention cost about 35 USD, including fixed costs related to developing the tests. Excluding these fixed costs, the sum is 18.2 USD—a figure in line with other recent information interventions (Dammert *et al.*, 2015; Bassi and Nansamba, 2017).

### 2.3. *Treatment 2: The transport subsidy*

Individuals in this treatment group are offered a subsidy to cover the cost of travelling to the city centre. The subsidy takes the form of a cash transfer that is conditional on visiting a disbursement point, located in an office in the centre of Addis Ababa. The centre of the city is where most employers are located ([Supplementary Figure A.1](#)). Further, the office is located close to the major job vacancy boards and to a central bus station, from which buses leave to destinations all around Addis Ababa. Recipients are required to attend in person, and to show photographic ID on each visit. Each recipient can collect cash once a day, up to three times a week. The daily amount is sufficient to cover the cost of a return bus fare from the participant’s area of residence at baseline to the disbursement point. We calibrate the subsidy to allow participants to travel on minibuses. Study participants can in principle walk to the office or use less expensive large public buses—an inferior means of transport that is crowded and infrequent—and save a part of the transfer. Qualitative evidence suggests that this is not common. Further, we do not find that individuals in this treatment group increase their savings during the weeks of the intervention. To access the subsidy, job seekers need to have (or borrow) enough cash to make the first journey—which in our setting is almost always the case.<sup>18</sup>

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

The median subsidy available on a given day is equal to 20 Ethiopian Birr (1 USD at the exchange rate at the beginning of the intervention). This equals about two-thirds of the median weekly expenditure on job search at baseline, and 10% of overall weekly expenditure. The

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

18. While job seekers have little cash-on-hand, our data show that most of them have at least enough to pay for one journey, in the knowledge that this money will be reimbursed. About 95% of job seekers in our sample have at least 15 ETB in savings, while 75% of job seekers have at least 10 ETB available as cash-on-hand or at home. See [Franklin \(2018\)](#) for further discussion of this issue.

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

20. We tried to minimize 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.



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.<sup>21</sup> The intervention was implemented between September 2014 and January 2015.

#### 2.4. Conceptual framework

To guide intuition about the likely effects of these two treatments—and the mechanisms by which such effects might operate—we now discuss a simple conceptual framework (which the [Supplementary Appendix](#) outlines in detail). We are particularly interested to explore how our interventions can affect (1) the probability of formal employment<sup>22</sup> and (2) the quality of a match between employer and employee. In order to focus on the direct impacts on these outcomes, we deliberately do not allow behavioural responses through reservation wages, and we abstract away from general equilibrium considerations. The framework is thus stylized. In Section 5, we present some evidence showing that reservation wages are indeed not affected by our interventions. Further, we come back to the issue of general equilibrium effects in Section 6.

The framework is built around two key labour market frictions: (1) it takes time for a worker to find a vacancy ([Rogerson et al., 2005](#)) and (2) firms make offers on the basis of match quality, but observe match quality with noise ([Farber and Gibbons, 1996](#); [Altonji and Pierret, 2001](#); [Kahn and Lange, 2014](#); [Pallais, 2014](#)). To capture the latter friction, our framework assumes that when an unemployed worker is matched to a job vacancy, the firm observes a signal about match quality. This signal comprises both (1) true match quality (specific to a worker-firm pair) and (2) idiosyncratic noise. The firm thus faces a signal-processing problem and will use Bayes' rule to form a posterior belief about the quality of a prospective match. Uncertainty about match quality is likely to be costly in this context: poor hiring decisions may decrease productivity or lead the firm to fire the worker (which requires the firm to pay severance pay) and screen other candidates. We thus allow firms to be moderately risk averse in their hiring preferences: *ceteris paribus*, firms prefer applicants with tighter signals and will hire the applicant only if the firm's expected utility exceeds some threshold.

To capture the first friction, we assume that job search take place over multiple periods of time and that, in every period, job seekers find a vacancy with a probability that is less than one.

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

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

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

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

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

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

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

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

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

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

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

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

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

### 3. EXPERIMENTAL DESIGN AND ESTIMATION STRATEGY

#### 3.1. *The sample*

To obtain our experimental sample, we began by drawing a random selection of geographic clusters from the list of Ethiopian Central Agency (CSA) enumeration areas.<sup>24</sup> 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 minimize potential spillovers between clusters, our sampling method ensured that we did not select any directly adjacent clusters.

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

How does our sample compare to the youth population of Addis Ababa? The [Supplementary Appendix](#) shows that individuals in our experiment are on average more educated than the overall

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

25. We initially completed baseline interviews with 4,388 eligible respondents. Before assigning treatments, we attempted to contact all of them by phone and dropped individuals who could not be reached after three attempts over a period of 1 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 6 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). [Supplementary Table A.4](#) shows how many individuals were dropped from the sample at each point and the reasons for them being dropped.

youth population ([Supplementary Table A.2](#)).<sup>26</sup> 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 ([Supplementary Table A.3](#)). Overall, we estimate that about 20% of all youth in Addis Ababa would be eligible for our study.

### 3.2. *Data collection: face-to-face and the phone survey*

We collected data on study participants through both face-to-face and phone interviews. We completed baseline face-to-face interviews between May and July 2014 and endline face-to-face interviews between June and August 2015; we then completed long-term follow-up interviews, by phone, in May 2018. These interviews recorded information about the socio-demographic characteristics of study participants, their education, work history, finances, expectations, and attitudes. The bulk of survey focused on labour market outcomes. Throughout the article, we measure wages using a 1-month recall period as reported by respondents.<sup>27</sup> We also collected an incentivized measure of present bias. We did not inform study participants at baseline that some of them would be offered job search assistance.

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

### 3.3. *Randomization*

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](#)).<sup>29</sup> In addition, we implemented a randomized saturation design, whereby we varied the proportion of sampled individuals in treated clusters who were offered treatment. We randomly assigned individuals within each treated cluster to a treatment or a control group.<sup>30</sup> This was done by blocking individuals within clusters by their education level, and implementing a simple re-randomization rule. The overall assignment to treatment is outlined in [Table 1](#). The randomized saturation rule is used to look at the spillover effects of the intervention through social networks. We do not focus on the results from this design in the article. Instead we discuss this design, and the main results, in [Supplementary Appendix Section A.3](#).

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

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

28. [Franklin \(2018\)](#) shows that high-frequency phone surveys of this type are reliable, in the sense of not generating Hawthorne effects.

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

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

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

### 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 [Supplementary Table A.6](#). For each baseline outcome of interest, we report the  $p$ -values for a test of the null hypothesis that all experimental groups are balanced. We cannot reject this null for any of the variables analysed.

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

### 3.5. Take-up

Take-up is substantial for both treatments. About 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

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

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

subsidies for at least 2 weeks tend to be dedicated users. Conditional on ever collecting the money, 74% of respondents take it at least once a week over the course of the entire study, with an average of 16 collections in total.

Further, 61% of individuals who are invited to the job application workshop attend it. 80% of those attending later collect the certificates from the School of Commerce. Take-up rates do not vary substantially with observable covariates.<sup>33</sup>

### 3.6. Estimation strategy

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

$$y_{ic} = \beta_0 + \sum_f [\beta_f \cdot \text{treat}_{fic} + \gamma_f \cdot \text{spillover}_{fic}] + \alpha \cdot y_{ic,pre} + \delta \cdot \mathbf{x}_{ic0} + \mu_{ic}, \quad (1)$$

where  $y_{ic}$  is the endline outcome for individual  $i$  in cluster  $c$  and  $\mathbf{x}_{ic0}$  is the vector of baseline covariate values that were used for re-randomization and blocking.  $\text{treat}_{fic}$  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  $\text{spillover}_{fic}$  is a dummy that identifies control individuals residing in clusters assigned to treatment  $f$ . Thus,  $\gamma_f$  captures the spillover effects of treatment  $f$ . We report the estimates of these spillover effects in [Supplementary Appendix A.3](#). We correct standard errors to allow for correlation within geographical clusters, and we use sampling weights to obtain average treatment effects for the eligible population as a whole.<sup>34</sup>

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

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

$$y_{itc} = \sum_f \sum_{w=S_f}^{E_f} [\beta_{fw} \cdot \text{treat}_{fic} \cdot d_{wit} + \gamma_{fw} \cdot \text{spillover}_{fic} \cdot d_{wit}] + \alpha_t \cdot y_{itc,pre} + \delta \cdot \mathbf{x}_{ic0} + \eta_t + \mu_{itc}, \quad (2)$$

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

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

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



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

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  $\beta_{fw}$  and  $\gamma_{fw}$ :

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

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

#### 4. TREATMENT IMPACTS

In this section, we discuss the main impacts of our interventions (and, in doing so, we test Prediction 1 from our model). We follow a detailed pre-analysis plan, registered at [www.socialscienceregistry.org/trials/911](http://www.socialscienceregistry.org/trials/911). The plan describes the empirical strategy, the outcome variables of interest, the definition of these variables, the subgroup analysis, and our approach to multi-hypothesis testing and attrition. We focus the discussion on six outcomes that we pre-specified as primary. These outcomes—which describe employment, employment quality, and earnings—are the typical targets of active labour market policies. In the remainder of this section, we focus entirely on primary pre-specified outcomes, measured at both 1-year and 4-year follow-ups.<sup>39</sup>

##### 4.1. Short-run impacts

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

However, our interventions are successful in increasing job seekers' chances of attaining *good jobs* in the short run, as indicated by the impacts on two key indicators of job quality: whether

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

37. For example, for an individual assigned to receive the transport treatment from week 15 of the study onwards, the dummy  $d_{0it}$  is equal to 1 in week 15 and to 0 in all other weeks. Similarly, for an individual who starts treatment in fortnight 15, we set  $d_{-1i14}=1$ , and  $d_{5i20}=1$ , and so on. Note that because interventions ran for different lengths of time, the number of fortnights for which we will be able to estimate the treatment effect relative to the start fortnight of the treatment will differ by treatment. In the notation above  $S_f$  denotes the earliest fortnight for which we will be able to estimate a treatment effect for treatment  $f$ .  $E_f$  denotes the final fortnight.

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

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

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

TABLE 2  
Impacts on employment outcomes

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

Notes: In this table, we report the *intent-to-treat* estimates of the direct effects of the transport intervention and the job application workshop on primary employment outcomes. These are obtained by ordinary least squares (OLS) estimation of equation (1), weighting each observation by the inverse of the probability of being sampled.  $N = 2,201$  for 2015 results, and  $N = 2,018$  for 2018 results. Because we did not follow-up with the spillover groups in 2018, we are unable to include the individuals in the spillover groups in the 2018 regressions. For consistency, we drop the spillover observations from the 2015 regressions as well. Results for 2015 are qualitatively unchanged when those observations are included. Below each coefficient estimate, we report the *s.e.* in parentheses and a *q*-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. *q*-values are obtained using the sharpened procedure of Benjamini *et al.* (2006). We do this for the data from the first endline in 2015 (Columns 1–4) and then for the second endline in 2018 (Columns 5–8). For each endline and each outcome, we report the mean outcome for the control group and the *p*-value from an *F*-test of the null hypothesis that transport subsidies and the job application workshop have the same effect. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

work is formal (in the sense of having a written contract), and permanent (in the sense of not having a specified end date). As we foreshadowed, both characteristics are highly sought by 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% (raising the share of workers in permanent employment by 6.9 percentage points from a level of 12% 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%. The effect is statistically significant at the 1% level and remains highly significant after correcting for multiple comparisons. The transport treatment, on the other hand, had a modest impact on permanent employment, which is significantly smaller than the effect of the workshop. We also find that both interventions increase workers' chances to have a formal job by about 30%. Only 17% of the control group has a formal job at endline and both programmes increase that figure by 5 percentage points. The effects are robust to the multiple comparison correction and to bounding exercises making various assumptions about the distribution of outcomes for missing observations, including the use of Lee Bounds (see [Supplementary Appendix Section A.2](#)).

#### 4.2. Long-run impacts

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

Additional checks (available in the [Supplementary Appendix](#)) show that the earnings effect of the workshop is robust to estimating the effect on log wages and to winsorizing at the top of the distribution to eliminate outliers ([Supplementary Table A.10](#)). Further, quantile regressions show that the effects are large and significant across the distribution of earnings ([Supplementary Table A.12](#)).<sup>41</sup> Figure 1 corroborates this conclusion by showing a clear rightward shift of the earnings distribution for workshop participants compared to the control group. A Kolmogorov–Smirnov test of the equality of the distributions produces a *D*-statistic of 0.0647 (with a *p*-value of 0.115 for the two-sided test, and 0.058 for the one-sided test where the alternative hypothesis is that earnings are higher in the treatment group compared to the control).<sup>42</sup> In [Supplementary Appendix Section A.2](#), we show that the effect of the workshop on earnings is not driven by differential rates of attrition: the result is robust to a number of assumptions about the distribution of outcomes for missing observations.

Four years after the intervention, formal employment rates in the workshop group are very similar to those of the control group. Between 2015 and 2018, the treatment effect on this variable has dropped by a significant 5 percentage points ( $p = 0.068$ ). This finding is consistent with our stylized framework, which predicts that control individuals eventually catch up in terms of the probability of having a formal job. Further, the workshop does not have long-term impacts on overall employment rates or on permanent employment. Second-endline employment rates among treated individuals are an insignificant 2.9 percentage points higher than in the control

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

42. These results are for 2018 wage earnings—our main variable of interest. This variable does not include profits from self-employment and assigns a value of 0 to all individuals that do not have a wage-paying job. Throughout the rest of the article, we will refer to this variable as “wage earnings” or simply as “earnings.” Our results are robust to using several alternative definitions of this outcome variable—in particular, (i) a broader measure of earnings which we obtain by summing wage earnings and profits from self-employment and (ii) a conditional measure of wage earnings that assigns a missing value to all individuals that do not have a wage-paying job. Throughout the rest of the article, we will refer to the first variable as ‘total earnings’ and to the second variable as “wages.” [Supplementary Table A.11](#) shows that impacts on total earnings are, if anything, larger than the effects on wage earnings alone. Similarly, [Supplementary Table A.13](#) shows quantile regressions for total earnings in 2018 which confirm the results for wage earnings reported in [Supplementary Table A.10](#). For total earnings, we document significant effects from the 45th percentile and up. Finally, we can reject the equality of the distribution of total earnings in the workshop and control groups using a Kolmogorov–Smirnov test ( $p$ -value of 0.066 for the two-sided test). If we separately consider only profits from self-employment, we find no effects something that is not surprising, given the substantial noise in self-employment profits, and given that our intervention was directed solely at improving access to wage employment. Similarly, we also find no extensive-margin effect on the probability of self-employment either. We report results on wages in the following section.

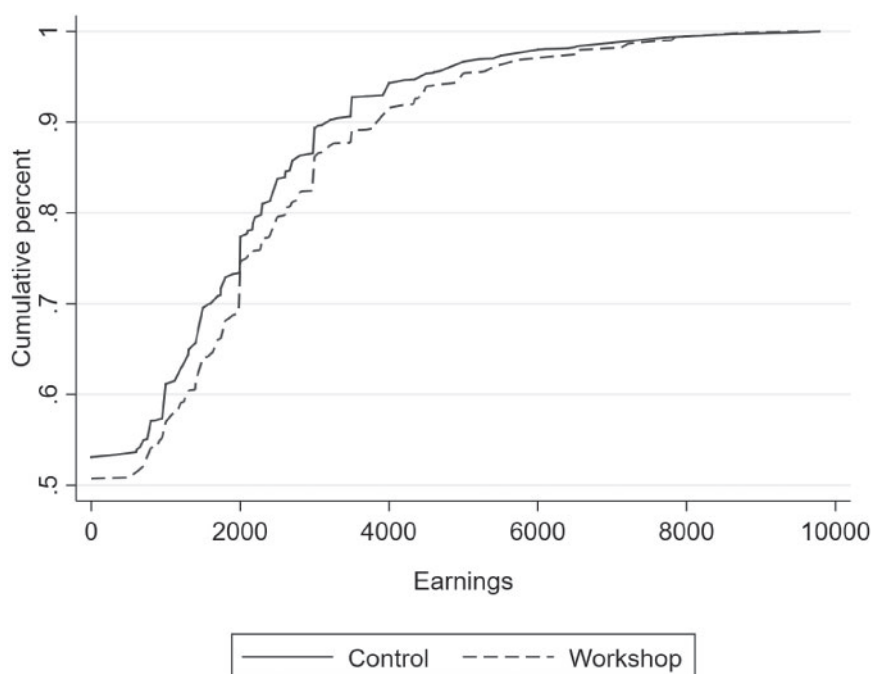


FIGURE 1

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

group. Permanent employment rates have also equalised, which is not surprising, given that by the second endline the correlation between formal and permanent work is high.<sup>43</sup>

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

43. As we have already discussed, improved rates of finding formal jobs will not automatically translate into higher employment rates in a setting where informal work is readily available. However, in the first endline we also document an increase in permanent work. The stability offered by permanent employment could enable treated individuals to work for more weeks each year, compared to control individuals, and thus to be more likely to work on any given week. We have some suggestive empirical evidence of this in the recall data plotted in [Supplementary Figure A.4](#): the employment impacts of the workshop grew from 2 percentage points 8 months after treatment, to a significant 5 percentage points in the second year after the intervention, and then decreased again to 2.7 points. Further, we show in [Supplementary Figure A.5](#) that the effect on permanent employment gradually decreased over time. We do not have recall data on formal employment.

effect is significant at the 10% level but is not robust to the correction for multiple comparisons and we thus do not interpret it further. Overall, these findings are in line with our stylized framework, which predicts no long-run effects of the transport subsidy, since it does not lead to an increase in match quality, as discussed in the next section.

## 5. MECHANISMS

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

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

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

**5.1.1. Search intensity.** We find that the transport intervention causes people to search for work more frequently, while the workshop does not lead to any change in search effort. We show this by estimating the fortnightly impact of each intervention on the probability of searching for work using equation 2. When the transport subsidy is available, treated individuals are about 12.5% more likely to look for work than control individuals (a 5 percentage point effect over a control mean of 40%, as shown in Figure 2a). This effect decreases linearly after the end of the transport intervention. We also find that when the transport subsidy is available, treated individuals are about 9 percentage points more likely to visit the job vacancy boards, where formal jobs are typically advertised (see Figure 2b). This is an increase of nearly 30% over a control mean of 28%.<sup>45</sup> Finally, treated respondents are more likely to travel to the centre of the city while the subsidies are in place (see [Supplementary Figure A.7](#)).<sup>46</sup> These findings help to explain why the increase in search intensity translates into the effects on formal work discussed above: most formal jobs, regardless of firm location, are advertised at the central job boards, while informal jobs are generally not.

The job application workshop, on the other hand, does not affect the likelihood of searching for a job (Figure 3) or the number of job applications sent ([Supplementary Table A.15](#)). This is notable and consistent with the hypothesis that financial constraints prevent job seekers from

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

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

46. By the time of the endline interview, we cannot find significant effects on the number of trips to the centre of the city made in the previous 7 days. Consistently with this, we do not find significant effects on whether individuals work outside of their *woreda* (a broadly defined administrative unit within the city). This is likely to be because workers choose jobs that do not require long commutes.

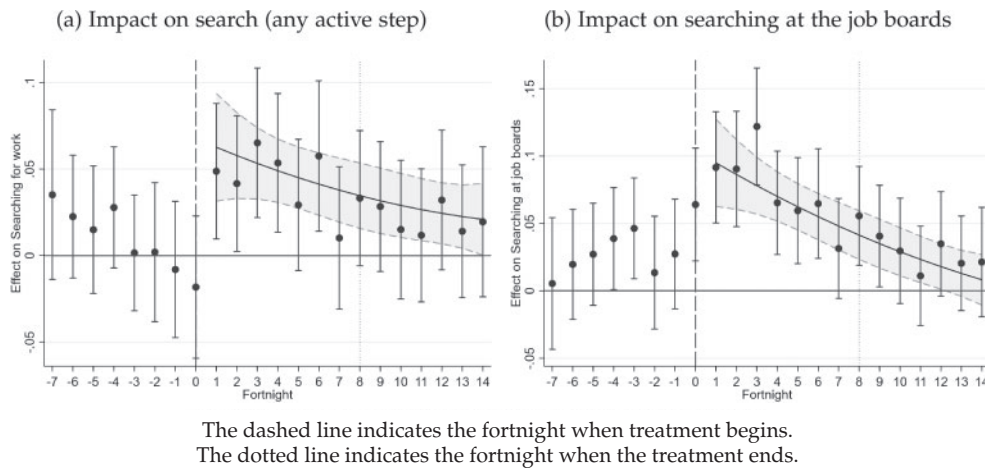


FIGURE 2

Fortnightly impacts of the transport treatment on job search. (a) Impact on search (any active step). (b) Impact on searching at the job boards.

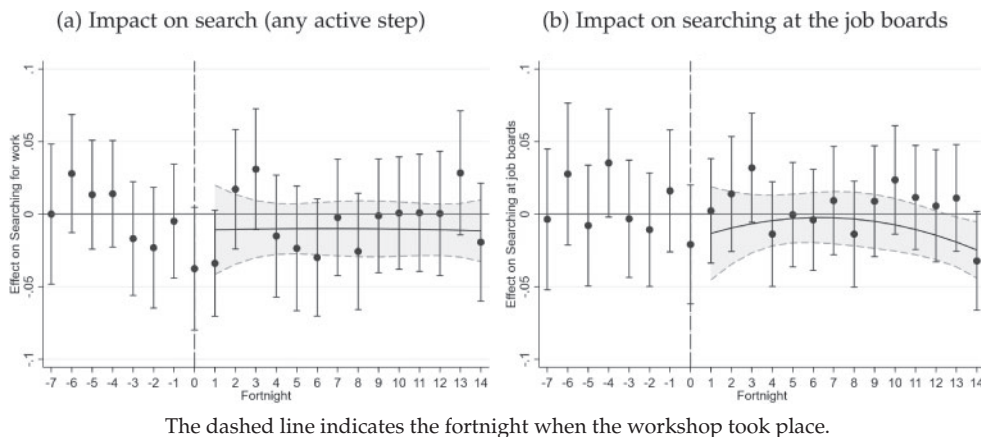


FIGURE 3

Fortnightly impacts of the job application workshop on job search. (a) Impact on search (any active step) and (b) Impact on searching at the job boards.

increasing search effort: if the workshop did motivate job seekers to search harder, they would appear to lack the resources to do so.<sup>47</sup>

**5.1.2. Search efficacy.** We also find evidence that the workshop increases search efficacy. First, the results above show that individuals in the workshop treatment are significantly more likely to obtain formal and permanent jobs while doing the same amount of job search as individuals in the control group. This is consistent with the prediction of our framework: the workshop does not relax the constraints that prevent individuals from intensifying job search,

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



but it makes firms more likely to extend a job offer to treated individuals. To quantify this effect, we compute the conversion rate of applications to offers for permanent jobs. In [Supplementary Table A.15](#), we show that the workshop improves this conversion rate in the time period between the baseline and the first endline survey 8 months after treatment). People in the control group receive an average of one offer for a permanent job every 7.2 applications. The workshop brings this down to one offer every 5.2 applications. The magnitude of the effect is meaningful, but our estimates are noisy: the effect is significant at the 10% level and has a  $q$ -value above standard levels of significance.

Second, we leverage the fact that our certificates report test scores in discrete bands and make no mention of the candidate's precise test score.<sup>48</sup> This allows us to study the impact of being placed in a higher band, while controlling for the precise test score, in a regression discontinuity framework. If our workshop treatment operated primarily through a certification mechanism, we would expect large discrete improvements in employment prospects at band cut-offs. We perform this analysis for the aggregate score (a summary measure of all test results) and, to maximize power, we normalize this score and pool the data for all discontinuities together.<sup>49</sup> We find that being placed in a higher band generates a large, but noisily estimated increase in earnings in 2018 (see [Supplementary Table A.14](#)). When we use the optimal bandwidth ([Imbens and Kalyanaraman, 2012](#)), we find that being just above the cut-off leads to a large increase in earnings of 0.37 standard deviations, which is marginally insignificant ( $p = 0.146$ ). We then explore robustness to the use of bandwidths that are respectively half and twice the optimal values. We find that the effect is as large as 0.46 standard deviations and is significant at the 5% level when we use the larger bandwidth. Overall, while somewhat noisy, this evidence suggests that the certification element of the intervention contributes to the overall treatment effect. Further, it is consistent with our prediction that the workshop increases search effectiveness by providing information about skills.<sup>50</sup>

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

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

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

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

51. In addition to investigating each outcome in a family separately, we use a standard “omnibus” approach: we construct an index for each family and test whether the index is affected by our treatments (see [Supplementary Table A.16](#)). 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.

TABLE 3  
Impacts on match quality

Outcome	Control mean (1)	N (2)	ITT Estimates		Equality pval (5)
			Transport Coeff (3)	Workshop Coeff (4)	
Wages (conditional on a wage job)	2,580.479	1,041	81.660 (161.627)	562.640*** (188.542)	0.012
Longest tenure in months (conditional on any jobs)	12.276	1,361	0.551 (0.664)	1.128* (0.678)	0.378
Longest tenure (unconditional)	10.132	1,751	0.213 (0.561)	1.258** (0.628)	0.067
Uses skills in current job (unconditional)	0.282	2,016	0.032 (0.040)	0.082** (0.040)	0.211
Promoted in current job (unconditional)	0.092	2,016	0.007 (0.016)	0.007 (0.016)	0.991

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

interventions work directly through the hypothesized channels of job search intensity and skills signalling.

## 5.2. Prediction 3: Did match quality increase?

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

Second, we show positive impacts on two proxies of match quality: employment duration and skills use. Employment duration is often considered the most effective indicator of match quality as a longer tenure shows that both the firm and the employee value the match. To measure employment duration, in the second endline survey we collect information on the longest spell of work with a single employer that study participants have completed. We find that the duration

of this work spell significantly increases by about 10 percent when young people are offered the job application workshop. Second, we collect information about whether individuals work in jobs where they make regular use of skills they have acquired in previous jobs or at school. This captures a different dimension of match quality—the effective sorting of skills and tasks. We find that individuals who receive the workshop are eight percentage points more likely to work in jobs where they employ their existing skills (conditional on having a job). We find no such evidence of improved match quality for the group receiving the transport subsidy.

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

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

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

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

In the second step, we use mediation analysis to quantify the share of the treatment effect on 2018 earnings that can be accounted for by the initial change in match quality. Following the recommendations by Acharya *et al.* (2016), we compute the Average Controlled Direct Effect (ACDE) of the workshop on long-run earnings, fixing the potential mediators of interest (the three proxies of match quality). The ACDE captures the impact of an intervention when a particular mediator is not allowed to respond to treatment and thus, by construction, cannot drive the

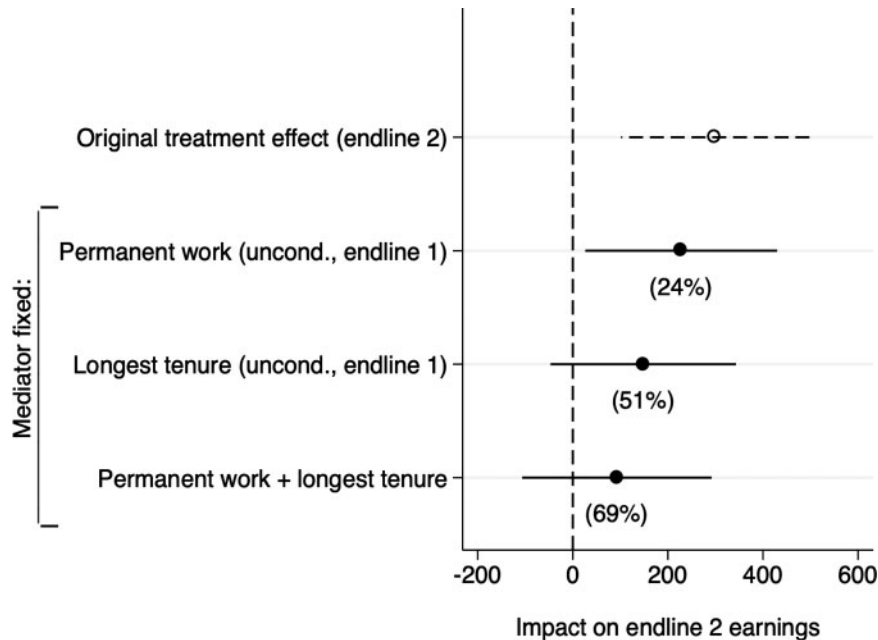


FIGURE 4

#### Mediation analysis: job application workshop

*Notes:* This figure reports coefficient estimates and 90% confidence intervals of the impact of the job application workshop on endline 2 earnings. The first row reports the original treatment effect on endline 2 earnings. The following rows report the Average Controlled Direct Effect (ACDE) of the intervention, obtained by fixing the mediator indicated in the row's name (Acharya *et al.*, 2016). We can assess the importance of a given mediator by comparing the original treatment effect to the ACDE. To facilitate the comparison, we report below each coefficient the share of the original treatment effect that is accounted for by the mediator. We consider the following mediators: permanent work at endline 1 and the length of the longest employment spell (which captures employment spells that started just after endline 1, and was measured with a retrospective question in endline 2). In the last row, we also report an estimate of the ACDE obtained by including both mediators.

treatment effect on the outcome of interest.<sup>52</sup> We can thus assess the importance of a given mediator by comparing the original treatment effect to the ACDE: if the mediator accounts for a large share of the impact of the intervention, the ACDE will be much smaller than the original treatment effect. We show these comparisons in Figure 4. We find that a large share of the earnings impacts (69 percent) can be explained by the changes in our proxies for match quality. When looking at each proxy individually, we find that the length of the longest employment spell alone mediates 51% of the earnings effect. Further, permanent work at endline 1 can explain 24% of the long-run effect on earnings. Overall, this analysis is consistent with the prediction of our model: the earnings effects are driven by the improvements in match quality.

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

TABLE 4  
Impacts on 2018 wage earnings by baseline characteristics

Baseline covariate	Covariate = 0			Covariate = 1			Transport	Workshop
	Control mean (1)	Transport (2)	Workshop (3)	Control mean (4)	Transport (5)	Workshop (6)	Equality (pval) (7)	Equality (pval) (8)
Above high school	826.4	15.9 (124.9) [1.000]	467.6** (188.0) [0.036]	1,755.5	36.9 (153.8) [1.000]	41.1 (134.4) [0.839]	0.91	0.07
Male	905.5	-46.9 (111.3) [1.000]	126.3 (106.6) [0.077]	1,564.3	96.7 (164.7) [1.000]	480.1** (227.5) [0.222]	0.44	0.15
Active searcher	1,096.7	-4.7 (127.3) [1.000]	347.1* (181.2) [0.045]	1,363.7	56.0 (145.4) [1.000]	239.5 (170.5) [0.479]	0.74	0.68
Ever had permanent job	1,160.5	33.1 (102.8) [1.000]	356.8*** (130.4) [0.036]	1,687.4	-52.3 (360.2) [1.000]	-298.2 (343.9) [0.632]	0.82	0.08
Lives close to the centre	1,171.5	28.8 (138.0) [1.000]	407.5** (181.1) [0.036]	1,278.1	51.6 (150.2) [1.000]	136.9 (146.4) [0.632]	0.91	0.25
Predicted endline earnings (above the median)	806.8	66.7 (114.7)	439.4*** (151.8)	1853.2	-61.3 (215.5)	24.6 (211.9)	0.546	0.087

*Notes:* This table shows differential treatment effects by individual baseline characteristics on earnings at the second endline (2018) of the workshop and transport treatments. We estimate heterogeneous treatment effects in a saturated model where we interact the treatment with dummies for baseline covariate = 0, and for baseline covariate = 1. Otherwise, the model is the same as the model presented in equation (1). We weight each observation by the inverse of the probability of being sampled. Below each coefficient estimate, we report the *s.e.* in parentheses and a *q*-value in brackets. We correct standard errors to allow for arbitrary correlation at the level of geographical clusters. Columns (1)–(3) show the results for the sub-sample with the baseline covariate = 0, while columns (4)–(6) show the results for the sub-sample where the covariate = 1. For example, row (1), column (1) shows the control mean for individuals who did *not* study at a tertiary level (826.4 Birr) and row (1), column (3) shows the treatment effect of the workshop for this group (467.6). We do this for five main baseline characteristics. In the last row, we show the results where we split the sample by predicted earnings using a range of baseline covariates. For this row, standard errors are derived using bootstrap methods. See Section 5.3 for additional discussion. Finally, in columns (7) and (8) we test for the equality of the treatment effects between the “covariate=0” and “covariate=1” group, for the transport and workshop treatment, respectively. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

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

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

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

disadvantaged category.<sup>53</sup> For example, job seekers without tertiary education experience an effect of about 60% of the control mean, and the effect of the workshop is significantly larger for this less educated group than it is for individuals who have some tertiary education.

To summarize across multiple pre-specified dimensions, we report in the final row of Table 4 an “endogenous stratification” exercise suggested by Abadie *et al.* (2018). Since this analysis was not included in the pre-analysis plan, it should be seen as an aggregation exercise: it is prompted by the results from pre-specified hypotheses, and seeks to generalise the insights from these regressions. To implement this approach, we stratify by predicted earnings at endline. In a first stage, we use a linear regression to predict endline (2018) earnings using our pre-specified baseline covariates. We then use a “split sample” method to estimate treatment heterogeneity between high predicted earnings and low predicted earnings individuals (Abadie *et al.*, 2018). The results show that the effect for the low-predicted-earnings group is large, and substantially larger than for those with high predicted earnings (indeed, we can reject the null hypothesis that the effects are equal between groups:  $p=0.087$ ). The estimated effect size for the low-predicted-earnings group is about 50% of the control mean.

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

These results imply that the workshop reduces the earnings premium of observable correlates of ability such as education or experience. This reduction in the earnings premium is evidence that better signals reduce statistical discrimination, as predicted by our framework. It is also a natural measure of the value of the information provided. Specifically, we find that the earnings premium of vocational education is reduced by a significant 83% thanks to the workshop, while the premium of having a degree decreases by 33% (not significant). Further, the premium of previous permanent work experience is fully erased—but the effect is imprecisely estimated ( $p=0.110$ ). Supplementary Table A.26 outlines these results. Overall, the earnings gap between the groups with low predicted earnings and high predicted earnings (on the basis of baseline covariates) drops from 130% to 48% (last row of Table 4). These findings illustrate the large equity gains generated by helping young workers access the labour market through improved signalling.

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



TABLE 5  
Impacts on 2015 permanent employment by baseline characteristics

Baseline covariate	Covariate = 0			Covariate = 1			Transport	Workshop
	Control mean (1)	Transport (2)	Workshop (3)	Control mean (4)	Transport (5)	Workshop (6)	Equality (pval) (7)	Equality (pval) (8)
Above high school	0.058	0.063** (0.025) [0.027]	0.109*** (0.028) [0.001]	0.213	-0.011 (0.025) [1.000]	0.010 (0.024) [0.371]	0.04	0.01
Male	0.104	0.064*** (0.023) [0.027]	0.074*** (0.026) [0.004]	0.138	-0.005 (0.027) [1.000]	0.063** (0.029) [0.068]	0.05	0.77
Active searcher	0.108	0.035 (0.024) [0.076]	0.084*** (0.028) [0.003]	0.134	0.030 (0.029) [1.000]	0.052* (0.030) [0.087]	0.89	0.45
Ever had permanent job	0.103	0.042** (0.020) [0.041]	0.073*** (0.019) [0.001]	0.269	-0.040 (0.069) [1.000]	0.032 (0.076) [0.371]	0.28	0.60
Lives close to the centre	0.117	0.001 (0.022) [0.240]	0.031 (0.025) [0.046]	0.124	0.053* (0.028) [0.435]	0.110*** (0.027) [0.001]	0.14	0.03
Predicted endline earnings (above the median)	0.078	0.053** (0.022)	0.087*** (0.025)	0.202	-0.018 (0.028)	0.018 (0.031)	0.022	0.061

Notes: This table shows differential treatment effects by individual baseline characteristics on permanent employment at the first endline (2015) of the workshop and transport treatments. See the notes to Table 4 for more details.

TABLE 6  
Impacts on 2015 formal employment by baseline characteristics

Baseline covariate	Covariate = 0			Covariate = 1			Transport	Workshop
	Control mean (1)	Transport (2)	Workshop (3)	Control mean (4)	Transport (5)	Workshop (6)	Equality (pval) (7)	Equality (pval) (8)
Above high school	0.108	0.071** (0.029) [0.021]	0.071** (0.029) [0.020]	0.268	0.031 (0.028) [0.505]	0.026 (0.026) [0.571]	0.36	0.26
Male	0.152	0.065** (0.028) [0.021]	0.093*** (0.027) [0.004]	0.195	0.044 (0.030) [0.505]	0.006 (0.031) [1.000]	0.63	0.04
Active searcher	0.153	0.070** (0.027) [0.021]	0.061** (0.027) [0.021]	0.195	0.036 (0.030) [0.505]	0.046 (0.032) [0.410]	0.43	0.74
Ever had permanent job	0.158	0.062*** (0.021) [0.017]	0.055*** (0.021) [0.016]	0.293	-0.012 (0.069) [0.505]	0.033 (0.070) [0.927]	0.32	0.76
Lives close to the centre	0.155	0.052** (0.026) [0.027]	0.039 (0.026) [0.034]	0.194	0.040 (0.028) [0.505]	0.063** (0.028) [0.136]	0.76	0.53
Predicted endline earnings (above the median)	0.101	0.071*** (0.025)	0.082*** (0.026)	0.249	0.021 (0.031)	-0.003 (0.030)	0.196	0.023

Notes: This table shows differential treatment effects by individual baseline characteristics on formal employment at the first endline (2015) of the workshop and transport treatments. See the notes to Table 4 for more details.

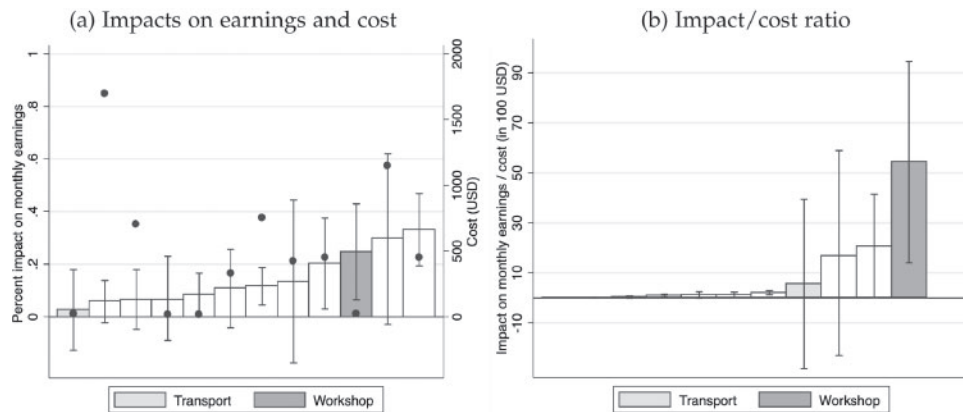


FIGURE 5

### Comparison with other Active Labor Market Policies in developing countries

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

## 6. DISCUSSION

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

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

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

### 6.2. General equilibrium, congestion, and signal inflation

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

Our framework predicts that the workshop increases efficiency even though it does not increase overall labour demand. Indeed, our framework assumes complete displacement in hiring: each

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

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

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

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

The empirical results from our study population offer some evidence that all workers have good match quality for at least some jobs. First, workers have different strengths: 44% of the workers in the job application workshop score high for at least one skill; only 10% of them are in the lower half of the distribution for all tested skills. By reporting results for each test, the certificates thus enable workers to demonstrate their particular skills. Second, the earnings gains generated by the workshop are broad based: as shown in Section 4.2 and Figure 1, the earnings of the treated stochastically dominate those of the controls, a finding confirmed by quantile regressions shown in [Supplementary Table A.13](#). While we cannot establish that no workers would be harmed by scale-up, the evidence suggests that the share of low-ability workers with nothing to gain from improved signals is small.

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

Finally, the workshop may allow job seekers to oversell their skills, making it harder for employers to screen candidates and thereby reducing match quality.<sup>55</sup> This is not what we find. If treated workers had misrepresented their skills at hiring, they should be laid off during the 45-day probation period required by Ethiopian law for all formal jobs. Instead, we find that treated

55. We thank an anonymous referee for this observation.

workers work in the same job for longer. Furthermore, they are offered better conditions when they move to another job. Even if job seekers could have fooled one employer about their skills, they cannot fool all of them.

## 7. CONCLUSION

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

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

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

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### Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

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