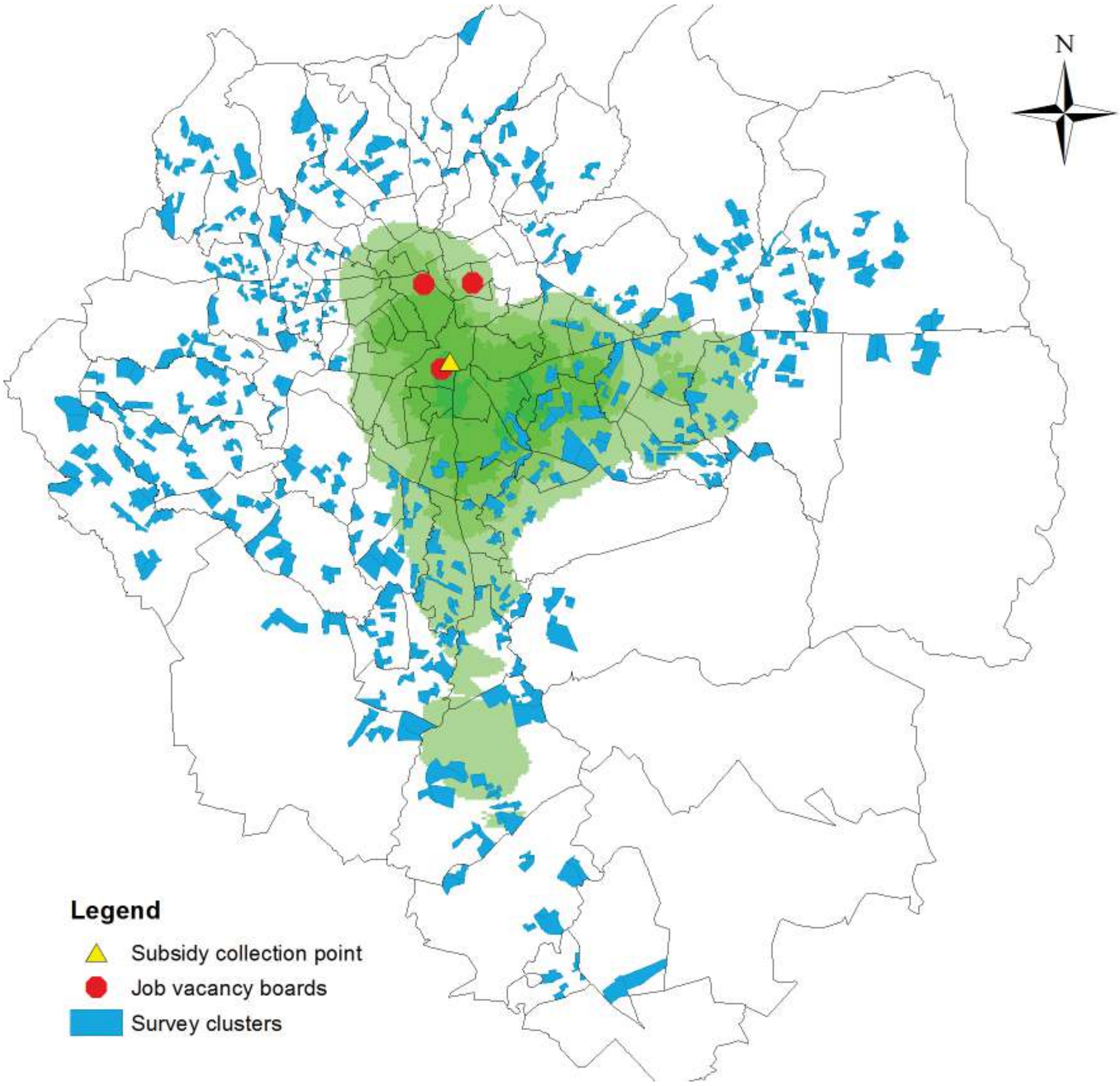
Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City

Material For Online Publication

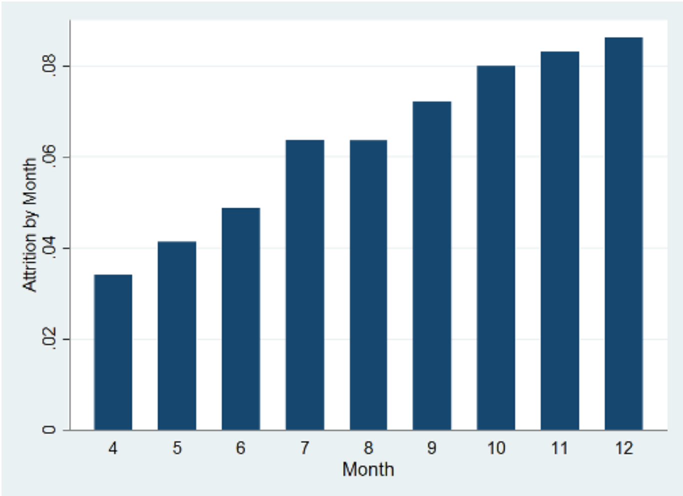
A.1 Additional Figures and Tables

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



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

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



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

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

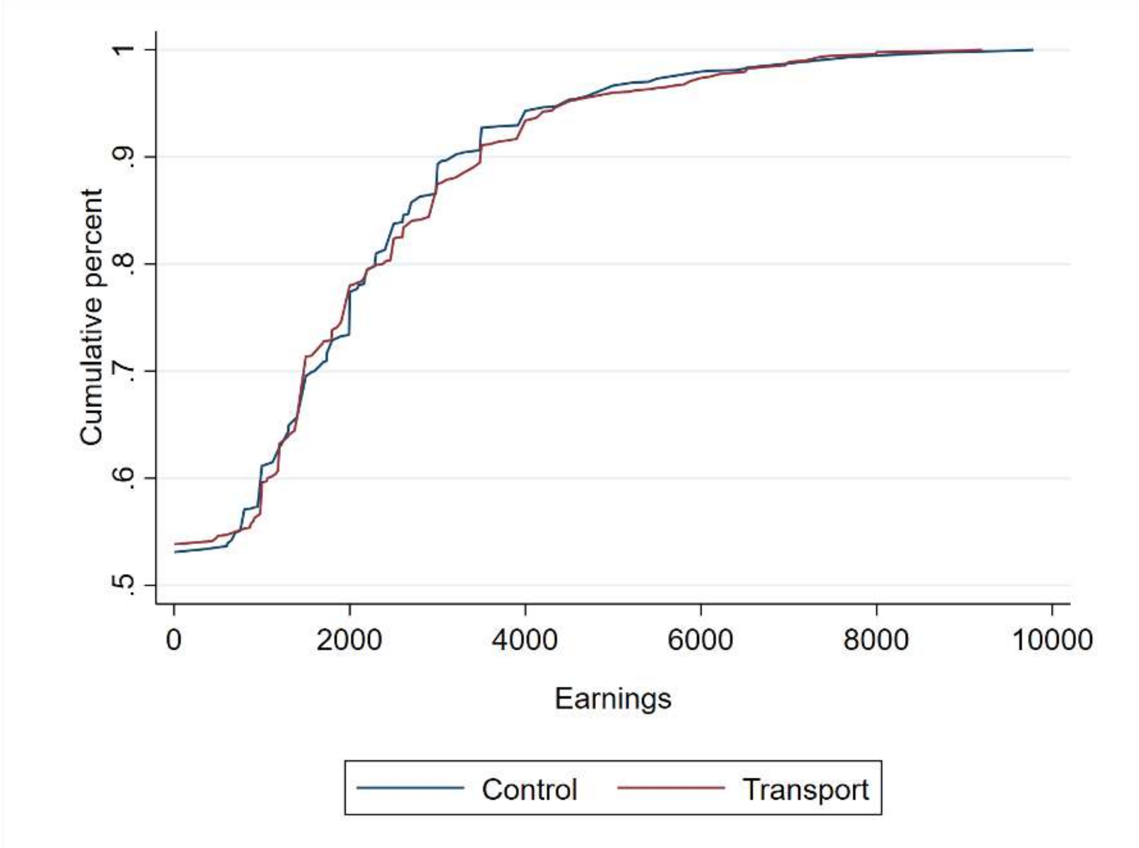
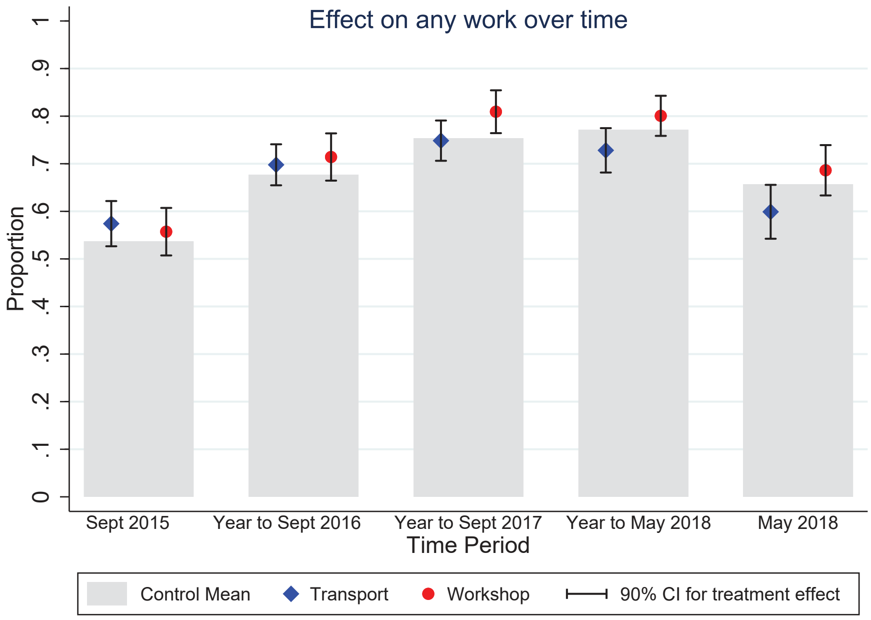
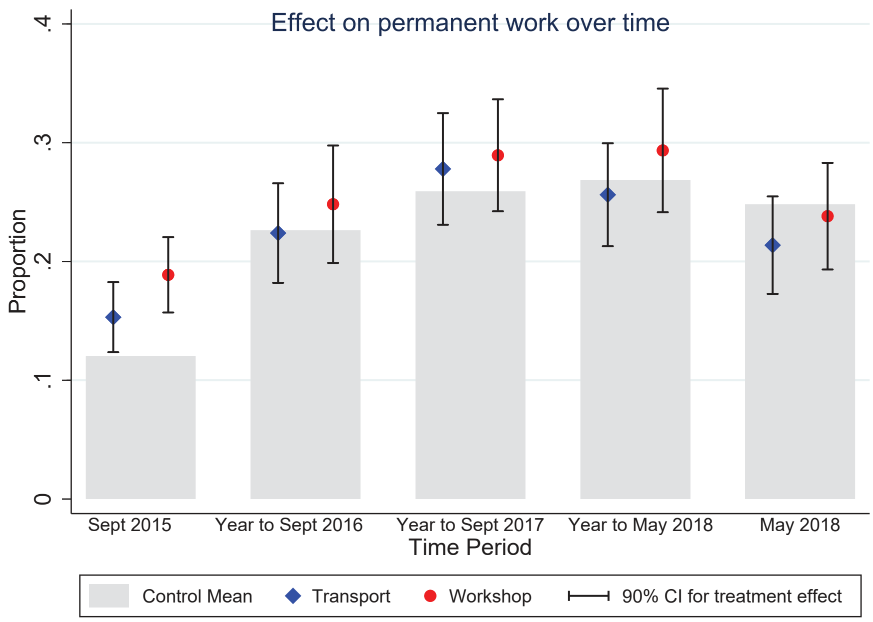


Figure A.4: **Impact trajectories by year: Employment**



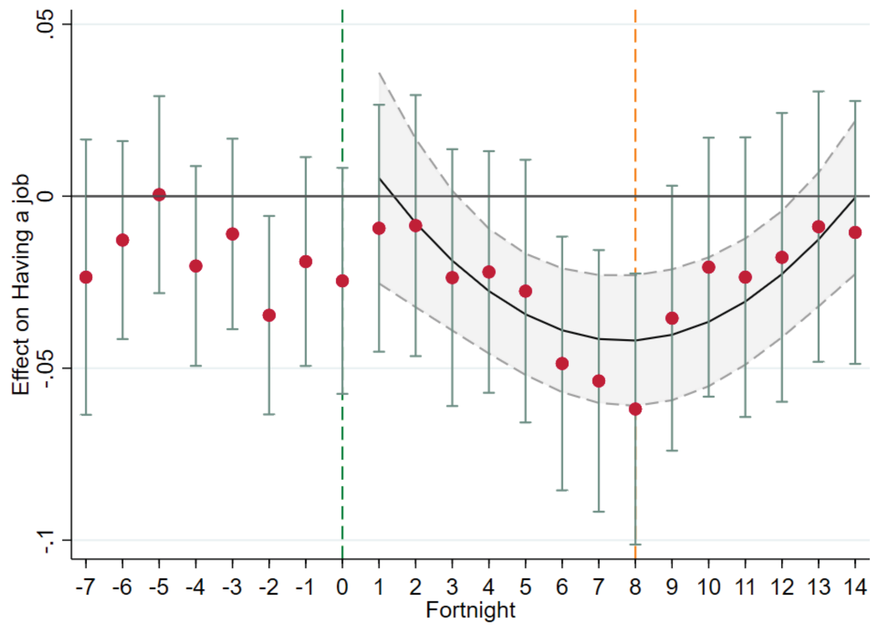
Note: This Figure combines data from our two endlines with recall data collected at the time of our second endline in May 2018. Specifically, Sept 2015 refers to the first endline survey, May 2018 refers to the second endline survey. The three time periods in between represent recall data collected in May 2018, for which respondents were asked to recall whether they were employed during the time periods denoted on the x-axis.

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



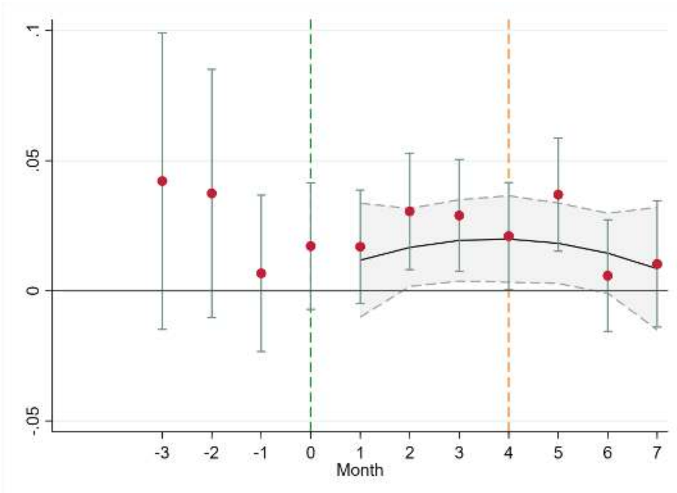
Note: This Figure combines data from our two endlines with recall data collected at the time of our second endline in May 2018. Specifically, Sept 2015 refers to the first endline survey, May 2018 refers to the second endline survey. The three time periods in between represent recall data collected in May 2018, for which respondents were asked to recall whether they were employed during the time periods denoted on the x-axis.

Figure A.6: **Impact trajectories (fortnightly) of the transport treatment: Employment**



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

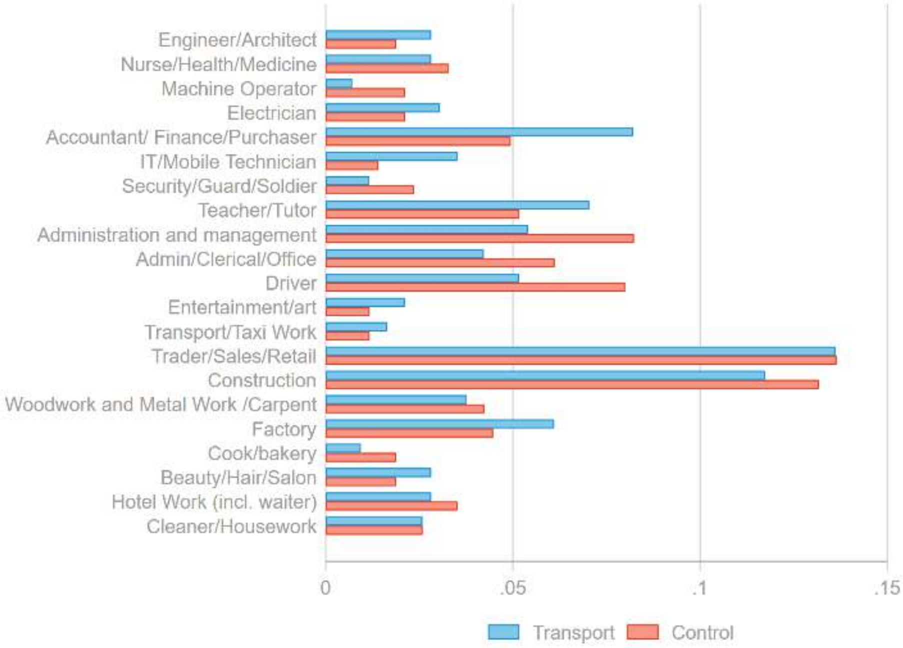
Figure A.7: **Impact trajectory (fortnightly) of the transport treatment: Travelled to city centre**



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

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

(a) **Transport Subsidy**



(b) **Job Application Workshop**

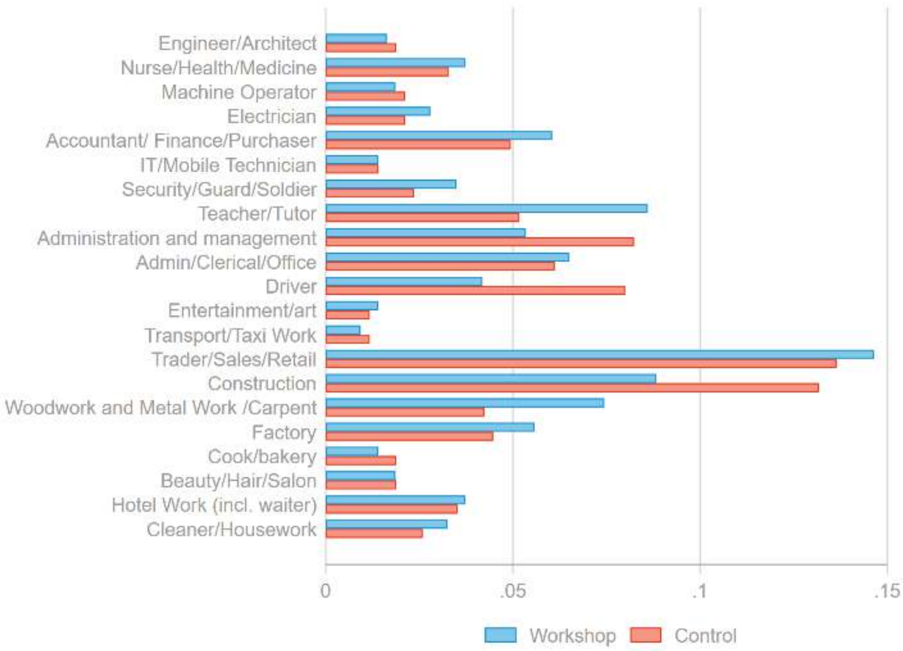


Table A.1: **Summary statistics of the tests administered in the job application workshop**

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

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

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

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

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

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

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

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

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

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

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

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

Outcome	Control Mean	SD	Transport	Workshop	N	F-test P
	(1)	(2)	(3)	(4)	(5)	(6)
Degree	0.18	0.39	0.01	-0.01	3049	0.347
			(0.63)	(0.74)		
Vocational	0.43	0.49	0.01	0.01	3049	0.717
			(0.82)	(0.59)		
Diploma or degree	0.25	0.43	0.00	-0.01	3049	0.557
			(0.94)	(0.68)		
Worked (7d)	0.31	0.46	-0.01	-0.02	3049	0.881
			(0.61)	(0.56)		
Searched for work (7d)	0.50	0.50	-0.01	0.00	3049	0.804
			(0.83)	(0.96)		
Female	0.52	0.50	0.00	0.00	3049	0.968
			(0.98)	(0.96)		
Born outside of Addis Ababa	0.37	0.48	0.01	-0.01	3049	0.530
			(0.72)	(0.84)		
Amhara	0.46	0.50	-0.01	-0.06	3049	0.078
			(0.87)	(0.11)		
Oromo	0.26	0.44	-0.00	0.02	3049	0.489
			(0.88)	(0.59)		
Worked in the last 6 months	0.46	0.50	-0.00	-0.01	3049	0.659
			(0.99)	(0.67)		
Married	0.20	0.40	0.01	-0.03	3049	0.131
			(0.81)	(0.26)		
Lives with parents	0.52	0.50	-0.01	0.01	3049	0.451
			(0.79)	(0.66)		
Ever had permanent job	0.13	0.34	0.00	-0.01	3049	0.370
			(0.84)	(0.56)		
Searched for work last 6 months	0.75	0.43	-0.01	0.00	3049	0.606
			(0.67)	(0.89)		
Age	23.44	3.00	0.06	0.05	3049	0.934
			(0.70)	(0.78)		
Years since school	42.30	273.93	6.40	-13.78	3045	0.128
			(0.71)	(0.37)		
Search frequency	0.57	0.31	-0.01	0.00	3049	0.782
			(0.75)	(1.00)		
Work frequency	0.34	0.38	-0.00	0.00	3049	0.846
			(0.94)	(0.90)		
Self-employed	0.05	0.22	-0.00	-0.00	3049	0.636
			(0.97)	(0.66)		
Casual labourer	0.06	0.23	-0.01	-0.01	3049	0.880
			(0.39)	(0.53)		
Work satisfaction	0.09	0.28	0.00	0.00	3049	0.881
			(0.79)	(0.91)		
Total savings	2279.23	6203.56	407.17	-160.84	3049	0.094
			(0.23)	(0.59)		
Reservation wage	1327.22	1235.30	72.65	13.61	3021	0.306
			(0.28)	(0.83)		
Distance city centre	5.92	2.24	0.22	0.30	3049	0.887
			(0.65)	(0.58)		
Trips to the city centre (7d)	1.83	2.03	0.03	0.03	3045	0.991
			(0.84)	(0.86)		
Has formal job	0.06	0.23	0.02	0.02	3049	0.789
			(0.17)	(0.15)		

Uses CV in applications	0.28	0.45	0.01	0.02	3049	0.659
			(0.61)	(0.41)		
Expected no. job offers	1.46	2.09	0.15	-0.04	2864	0.292
			(0.43)	(0.86)		
Aspired wage	5583.33	5830.85	300.29	402.24	2883	0.743
			(0.37)	(0.29)		
No. job contacts	6.74	9.63	-0.67	0.20	3014	0.384
			(0.51)	(0.87)		
Present biased	0.12	0.33	0.02	0.02	2067	0.814
			(0.42)	(0.35)		
Future biased	0.08	0.27	-0.03	0.00	2067	0.063
			(0.17)	(0.92)		
Life satisfaction	4.20	1.85	-0.03	-0.05	3045	0.892
			(0.87)	(0.78)		

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

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

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

Table A.8: **Predictors of attrition**

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

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

	Transport	Workshop
	(1)	(2)
Female	-.004	-.042
	(.042)	(.042)
Age	-.002	.004
	(.008)	(.006)
Married	.041	.033
	(.056)	(.045)
Lives with parents	-.033	.052
	(.054)	(.047)
Amhara	.054	-.006
	(.047)	(.041)
Oromo	.006	-.004
	(.051)	(.044)
Born outside Addis Ababa	.062	.070
	(.046)	(.046)
Degree	.038	-.037
	(.063)	(.052)
Years since school	-.00009	-.0001
	(.00008)	(.00008)*
Worked (last 7 days)	.105	.046
	(.048)**	(.048)
Searched for work (last 7 days)	-.057	-.068
	(.060)	(.038)*
Work frequency (before treatment)	-.039	-.012
	(.081)	(.054)
Search frequency (before treatment)	.254	.214
	(.072)***	(.065)***
Wage work (last 6 months)	-.019	-.074
	(.055)	(.048)
Search frequency (last 6 months)	-.036	-.010
	(.065)	(.056)
Ever had permanent job	-.072	-.086
	(.058)	(.058)
Const.	.407	.524
	(.211)*	(.178)***
Obs.	600	654
F statistic	2.513	3.059
P-value *F* test	.004	.001

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

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

Outcome	2015 Control mean (1)	2015 Transport (2)	2015 Workshop (3)	2015 Equality (pval) (4)	2018 Control mean (5)	2018 Transport (6)	2018 Workshop (7)	2018 Equality (pval) (8)
Wage earnings	739.230	65.879	3.363	0.30	1,216.811	30.916	299.469**	0.02
		(63.864)	(65.667)			(102.352)	(121.383)	
		[0.295]	[1.000]			[1.000]	[0.025]	
Log of Monthly Wages	7.203	0.011	0.008	0.96	7.643	0.028	0.163***	0.01
		(0.060)	(0.058)			(0.051)	(0.049)	
		[0.612]	[1.000]			[1.000]	[0.006]	
Wages winsorized at 99th percentile	722.772	66.574	2.845	0.27	1,197.949	12.452	250.128**	0.02
		(57.745)	(60.700)			(93.336)	(104.700)	
		[0.295]	[1.000]			[1.000]	[0.025]	
Wages winsorized at 95th percentile	653.629	83.209*	29.747	0.30	1,145.810	16.161	194.265**	0.05
		(45.703)	(50.351)			(83.298)	(89.690)	
		[0.224]	[1.000]			[1.000]	[0.031]	
Wages winsorized at 90th percentile	611.747	73.312*	32.876	0.38	1,070.490	9.553	151.441*	0.07
		(40.651)	(45.034)			(74.224)	(77.242)	
		[0.224]	[1.000]			[1.000]	[0.031]	

Note. This table shows impacts on both endline 1 (2015) and endline 2 (2018) wage earnings under different definitions of the outcome variable. We estimate impacts using Equation (1), weighting each observation by the inverse of the probability of being sampled. In row 1, we reproduce the impacts on our main measure of wage earnings. In row 2, we show the impact on the log of earnings. In rows 3, 4 and 5, we show results for wage earnings winsorized at the 99th, 95th, and 90th percentiles of the distribution.

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

Outcome	2015 Control mean (1)	2015 Transport (2)	2015 Workshop (3)	2015 Equality (pval) (4)	2018 Control mean (5)	2018 Transport (6)	2018 Workshop (7)	2018 Equality (pval) (8)
Wage earnings	739.230	65.879	3.363	0.30	1,216.811	30.916	299.469**	0.02
		(63.864)	(65.667)			(102.352)	(121.383)	
		[1.000]	[1.000]			[1.000]	[0.023]	
Total earnings (with profits)	971.395	10.994	76.754	0.39	1,811.911	-101.236	405.842**	0.00
		(74.959)	(85.239)			(135.372)	(160.515)	
		[1.000]	[1.000]			[1.000]	[0.023]	
Total earnings (with winsorised profits)	953.008	50.224	101.370	0.52	1,774.559	-119.727	335.022**	0.00
		(72.377)	(84.440)			(127.018)	(148.209)	
		[1.000]	[1.000]			[1.000]	[0.023]	

Note. This table shows impacts on both endline 1 (2015) and endline 2 (2018) wage earnings under different definitions of the outcome variable. We estimate impacts using Equation (1), weighting each observation by the inverse of the probability of being sampled. In row 1, we reproduce the impacts on our main measure of wage earnings. In row 2, we show impacts on total earnings, which are defined as wage earnings plus monthly profits from self-employment. This measure includes some values of profits that are large outliers. In row 3, we show impacts on total earnings (wage earnings plus profits from self employment), winsorising profits at the 99th percentile to remove outliers.

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

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

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

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

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

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

Table A.14: Regression Discontinuity Estimates

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

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

Table A.15: **Job search**

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

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

Table A.16: **Family indices**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
Job Quality	0.523 (.573) [1]	0.485 (.632) [1]	-0.835	0.952	2841
Financial Outcomes	0.190 (.236) [1]	0.144 (.211) [1]	-0.532	0.836	2841
Expectations and Aspirations	-0.137 (.749) [1]	0.0470 (.63) [1]	0.0400	0.788	2134
Mobility	0.637 (.53) [1]	0.591 (.599) [1]	-0.559	0.936	2836
Education and Skills	-0.473 (.77) [1]	-1.144 (.892) [1]	0.376	0.419	2841
Wellbeing	0.0540 (.164) [1]	0.182 (.154) [1]	-0.119	0.451	2837
Networks	-0.234 (.362) [1]	-0.286 (.387) [1]	0.00400	0.887	2833

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

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

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

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

Table A.18: **Financial outcomes**

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

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

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

Outcome	Transport	Job App. Workshop	Control Mean	F	N
No. job offers expected in next 4 months	-0.00600 (.143) [1]	0.270 (.154)* [.367]	1.383	0.0757	2641
Monthly reservation wage	8.791 (82.503) [1]	-86.57 (73.081) [.367]	1799	0.286	2480
Wage aspiration (monthly) in 5 years	689.8 (700.32) [1]	706.5 (817.628) [.367]	6237	0.985	2607
Expected no. weeks before obtaining perm. job	1.468 (4.323) [1]	-5.010 (3.345) [.367]	32.20	0.0923	1347

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

Table A.20: **Mobility**

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

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

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

Outcome	Transport	Job App. Workshop	Control Mean	F	N
In full-time education	-0.00700 (.008) [.777]	0.00100 (.01) [1]	0.0210	0.386	2841
In part-time education	-0.0480 (.02)** [.11]	-0.0330 (.023) [.52]	0.138	0.453	2841
In informal training	-0.00900 (.016) [.777]	-0.0100 (.015) [.696]	0.0470	0.951	2841
Graduated from any school over past 12 months	0.0120 (.017) [.777]	-0.0130 (.016) [.696]	0.0770	0.121	2841
Graduated (vocat. school) over past 12 months	0.0160 (.011) [.45]	0.00700 (.01) [.696]	0.0240	0.380	2841
Graduated (training course) over past 12 months	0 (.014) [1]	-0.0230 (.012)* [.475]	0.0440	0.0730	2841

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

Table A.22: **Psychological outcomes**

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

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

Table A.23: **Social networks**

Outcome	Transport	Job App. Workshop	Control Mean	F	N
Job Search Network	-0.298 (.422) [1]	-0.529 (.411) [.778]	5.182	0.552	2841
No. people with permanent jobs among those with whom you share job info	0.118 (.212) [1]	0.121 (.233) [.778]	2.178	0.987	2528
Can access a guarantor if needed for a job over the next month	-0.00500 (.054) [1]	-0.0660 (.054) [.778]	1.244	0.235	2504
No. of meetings of voluntary associations attended over past 30 months	0.0100 (.061) [1]	0.00900 (.063) [.802]	0.119	0.985	2841

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

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

	(1)	(2)	(3)	(4)
	Dep. var: Wage earnings in 2018			
Employment (2015)	364.7	56.44	137.9	-71.01
	(368.0)	(322.9)	(318.5)	(290.6)
Hours worked (2015)	-8.596	-8.940*	-2.308	-3.800
	(5.880)	(5.158)	(5.033)	(4.392)
Wage earnings (2015)	0.336**	0.303*	0.243	0.217
	(0.157)	(0.166)	(0.160)	(0.161)
Permanent work (2015)	1,207***	862.0***	909.2***	671.1***
	(259.0)	(267.1)	(245.5)	(256.6)
Longest work spell (2015-2018)		71.43***		66.57***
		(7.876)		(6.959)
Constant	848.2***	469.7***		
	(100.4)	(92.61)		
Baseline controls	NO	NO	YES	YES
Observations	651	651	651	651
R-squared	0.114	0.206	0.062	0.148

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

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

Baseline covariate	Covariate = 0			Covariate = 1			Transport	Workshop
	Control mean	Transport	Workshop	Control mean	Transport	Workshop	Equality (pval)	Equality (pval)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Above high school	826.4	16.8	485.0**	1,755.5	53.9	38.9	0.84	0.06
		(125.2)	(189.9)		(154.4)	(134.0)		
		[1.000]	[0.031]		[1.000]	[0.751]		
Male	905.5	-40.1	127.6	1,564.3	106.3	500.1**	0.43	0.14
		(111.5)	(106.3)		(165.1)	(229.3)		
		[1.000]	[0.090]		[1.000]	[0.160]		
Active searcher	1,096.7	3.9	358.3*	1,363.7	63.2	245.3	0.74	0.66
		(125.7)	(183.5)		(146.2)	(171.0)		
		[1.000]	[0.049]		[1.000]	[0.367]		
Ever had permanent job	1,160.5	40.7	360.2***	1,687.4	-38.9	-244.7	0.83	0.11
		(102.8)	(131.0)		(360.5)	(346.8)		
		[1.000]	[0.027]		[1.000]	[0.527]		
Lives close to the centre	1,171.5	41.1	424.1**	1,278.1	51.9	135.0	0.96	0.22
		(138.3)	(182.0)		(150.6)	(145.1)		
		[1.000]	[0.036]		[1.000]	[0.527]		
Born in Addis Ababa	1,217.3	-206.8	136.0	1,216.5	175.7	395.9**	0.08	0.31
		(154.8)	(183.7)		(141.2)	(168.8)		
		[1.000]	[0.114]		[1.000]	[0.160]		
Uses CV/Certificates	1,053.7	-4.0	307.8**	1,912.1	178.3	252.8	0.48	0.86
		(110.3)	(136.6)		(238.5)	(284.8)		
		[1.000]	[0.036]		[1.000]	[0.527]		
Present bias	1,234.1	87.8	456.8***	1,358.3	-83.1	-141.2	0.65	0.07
		(115.5)	(147.8)		(358.4)	(289.3)		
		[1.000]	[0.022]		[1.000]	[0.643]		
Job Search Network	1,031.4	102.3	266.8*	1,402.3	-25.7	347.0*	0.56	0.76
		(132.0)	(143.2)		(166.4)	(209.2)		
		[1.000]	[0.049]		[1.000]	[0.301]		

Note. This table shows differential treatment effects by individual baseline characteristics on earnings at the second endline (2018) of the workshop and transport treatments. We estimate heterogenous treatment effects in a saturated model where we interact the treatment with dummies for baseline covariate =0, and for baseline covariate =1. Otherwise the model is the same as the one 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. 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.

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

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

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

A.2 Sensitivity to attrition

A.2.1 Impacts on earnings at the second endline

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

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

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

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

[57] We predict earnings using our main set of baseline covariates with a linear regression model for the non-attrited control group.

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

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

Outcome	Control mean (1)	ITT Estimate Coeff (2)	Std. Err. (3)
Predicted earnings	1,531.1	250.3**	109.2
Predicted earnings +/- 0.25 SDs	1,545.6	222.0**	109.2
Predicted earnings +/- 0.5 SDs	1,560.1	193.6*	109.3
Mean control earnings +/- 0.25 SDs	1,574.6	187.0*	109.6
Mean control earnings +/- 0.5 SDs	1,649.0	45.6	110.9
Extreme cases			
95th / 5th percentile	2,168.0	-687.9***	138.7
Max/min	5,295.6	-4,265.6***	460.5

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

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

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

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

A.2.2 Impacts on employment outcomes at the first endline

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

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

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

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

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

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

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

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

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

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

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

A.3 Indirect effects on the untreated

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

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

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

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

where T_i identifies individuals who have been assigned to the transport treatment, while C_i identifies individuals who have not been assigned to the transport treatment.[59] S_{20c} is a dummy variable for individuals living in a cluster where 20% of individuals were offered the transport treatment. Thus, β_{20} captures the difference in outcomes between

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

untreated individuals in these clusters and untreated individuals in clusters where nobody was treated. Further, γ_{20} measures the difference in outcomes between treated individuals in S_{20c} clusters and untreated individuals in untreated clusters (the other βs and γs follow the same definition for different levels of saturation).

For both interventions, we find no significant difference, on average, between untreated individuals living in treated clusters and untreated individuals in pure control clusters (Table A.34).[60] Behind this average result, however, we find some evidence that the indirect effects of the transport treatment depend on the level of saturation (Table A.35). In clusters with 40 percent saturation, we document a positive indirect effect of the transport treatment on formal and permanent work. On the other hand, individuals in clusters with 90 percent saturation are 5.6 percentage points less likely to be in permanent employment than individuals in pure control clusters.[61] They are not, however, less likely to be in formal employment. Due to the limitations outlined above, we can only interpret these results as tentative evidence of local spillovers. Given the small sample sizes and the number of tests run in Tables A.35 and A.36, this evidence should be interpreted with caution.

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

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

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

Outcome	Transport Spill	Job App. Workshop Spill	Control Mean	F	N
Worked	-0.0460 (.034) [1]	0.0280 (.053) [1]	0.537	0.541	2841
Hours worked	-2.326 (1.842) [1]	0.541 (2.524) [1]	25.56	0.745	2835
Formal work	0.0140 (.02) [1]	0.0570 (.038) [1]	0.172	0.929	2841
Perm. work	0.00600 (.019) [1]	0.0120 (.027) [1]	0.120	0.0927	2841
Currently self employed	-0.0150 (.019) [1]	-0.0160 (.029) [1]	0.102	0.301	2841
Total earnings (with profits)	-41.10 (89.847) [1]	13.46 (103.598) [1]	971.4	0.417	2802
Satis. with work	-0.0170 (.024) [1]	0.0440 (.048) [1]	0.231	0.482	2841

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

Table A.35: **Spillover effects of the transport treatment on the untreated (by randomised level of cluster saturation)**

	20%	40%	75%	90%	*F(p)*
Worked	-0.0880	-0.0200	0	0.0360	0.407
	(0.047)*	(0.038)	(0.076)	(0.079)	
Hours worked	-4.758	-0.681	-1.242	4.067	0.305
	(2.546)*	(2.327)	(3.469)	(4.819)	
Formal work	-0.0140	0.0640	0.0410	-0.0230	0.156
	(0.024)	(0.032)**	(0.065)	(0.061)	
Perm. work	-0.0230	0.0660	0.0350	-0.0550	0.005***
	(0.023)	(0.030)**	(0.046)	(0.025)**	
Self-employed	-0.0250	0	-0.00600	-0.00900	0.908
	(0.024)	(0.030)	(0.054)	(0.046)	
Monthly earnings	-105.8	34.40	-51.61	108.8	0.639
	(110.970)	(124.195)	(244.436)	(179.350)	
Satis. with work	-0.0340	0.0120	-0.0170	-0.00800	0.775
	(0.029)	(0.040)	(0.056)	(0.072)	

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

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

	20%	40%	75%	90%	*F(p)*
Worked	0.0200	0.0660	0.0110	0.0450	0.810
	(0.081)	(0.051)	(0.045)	(0.034)	
Hours worked	-1.578	0.467	-1.206	1.365	0.732
	(4.317)	(2.691)	(2.269)	(1.828)	
Formal work	0.0190	0.0410	0.0860	0.0580	0.709
	(0.049)	(0.040)	(0.042)**	(0.021)***	
Perm. work	-0.0150	0.0200	0.0540	0.0390	0.451
	(0.038)	(0.025)	(0.033)*	(0.022)*	
Self-employed	0.0550	-0.0420	-0.0140	-0.0330	0.286
	(0.051)	(0.031)	(0.016)	(0.019)*	
Monthly earnings	-18.00	-66.08	-26.20	26.74	0.889
	(212.853)	(120.739)	(128.430)	(77.728)	
Satis. with work	0.0550	-0.0130	0.0130	-0.00900	0.801
	(0.065)	(0.054)	(0.036)	(0.037)	

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

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

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

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

A.4.1 A hazard model framework

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

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

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

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

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

A.4.2 Search success in a signal-processing framework

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

$$x_{if} \sim \mathcal{N}(0, 1); \tag{7}$$
$$\varepsilon_{if} \sim \mathcal{N}(0, \sigma^2). \tag{8}$$

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

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

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

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

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

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

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

The firm hires if and only if:

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

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

Two implications follow from this:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

larger in the control group.[66]

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

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

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

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

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

A.4.4 Introducing an observable covariate

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

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

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

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

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

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

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

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

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

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

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

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

A.4.5 Predictions: Applying the model framework

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

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

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

after job-search support is withdrawn.

Prediction 2 (search intensity vs. search efficacy): 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.

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

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

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