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Location, Search Costs and Youth Unemployment: Experimental Evidence from Transport Subsidies

[Search Costs and Urban Labour Markets]

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Abstract

Do high search costs affect the labour market outcomes of job seekers living far away from jobs? I randomly assign transport subsidies to unemployed youth in urban Ethiopia. Treated respondents increase job search intensity, and are more likely to find good, permanent, jobs. Subsidies also induce a short-term reduction in temporary work. I use a high-frequency phone call survey to track the trajectory of search behaviour over time to show that the subsidies significantly increased job search intensity, and the use of formal search methods. The evidence suggests that cash constraints cause young people to give up looking for good jobs too early.

Young people in urban Africa spend long periods of time in unemployment or underemployment while looking for good jobs. Dense labour markets in cities should reduce the time taken for workers to find good matches (Puga, 2010; Wheeler, 2006), but these benefits can be mitigated by high transport

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costs when cities are badly planned (Wasmer and Zenou, 2002; Cervero, 2001). The rapid growth of African cities has often not come with corresponding investments in transport infrastructure or well located housing, leading to congestion and an increasing number of people living on the outskirts of cities, far away from jobs. Search costs are commonly thought to impose significant frictions on labour markets (Diamond, 1982; Mortensen and Pissarides, 1994; Acemoglu and Shimer, 1999). Cash constraints could compound these frictions in poorly planned and sprawling cities, if high transport make it particularly costly for the poor to optimally invest in job search. Yet there is relatively little empirical work establishing causal links between search costs and employment outcomes, particularly in developing countries.

I use a randomised controlled trial of transport subsidies to test whether young job seekers who live far from the centre of a large African city are constrained in their ability to search for jobs because of where they live. Subsidies cover the cost of transport to the centre, but no more, in the way that a bus-fare subsidy programme would.[1] In this way, I exogenously reduce search costs, but for only two days each week for a pre-announced period of time, so that I do not reduce the cost of commuting to work in the long run. Therefore, the positive impacts of the subsidies on employment outcomes can be interpreted as evidence both for the existence of search frictions and for inequality in access to jobs due to place of living.

The intervention is evaluated using detailed baseline and two endline surveys (four and ten months after baseline), in Addis Ababa, Ethiopia. Short phone call interviews were conducted in every week between these surveys. I use this high-frequency survey data to track the trajectories of search intensity over all weeks.[2] The study is designed to compare and validate the impact of these subsidies in two subpopulations that differ in ways that aim replicate different targeting mechanisms that would likely be used to enrol youth into employment programs.

Respondents in the *board* sample were surveyed while they were visiting the main job vacancy boards.

[1] The transport subsidies were worth about one-third of daily wages for low-skilled work at the time of the experiment.

[2] Work on unemployment and job search using high-frequency data is relatively rare. For an example from the US see Krueger and Mueller (2012).

They begin as active job seekers but give up searching as cash runs low. They are well-educated and search for better quality, permanent jobs. These individuals are of particular interest to policy makers because they are most likely to self-select into youth employment programmes. The *city* sample were found at home in slum areas of the city, are less well educated, and try to find formal work rather than the kinds of casual jobs available in their local areas. They have been out of school for longer, and are relatively detached from the labour market compared to the *board* sample. They begin cash constrained, and search infrequently.

Four months after baseline, after the transport treatment ended, treated individuals in the high-skilled *board* sample are seven percentage points more likely to have permanent work (relative to a control group mean for this sample of 19%). Treated individuals have jobs of higher quality, they work in higher-skilled sectors, and are more likely to work in the city centre. They are not significantly more likely to be working overall, because good jobs displace temporary work. In the lower-skilled *city* sample, I find significant impacts on total employment and formal employment, as well as job quality. I use a second endline, six months later, to show that these effects start to dissipate over time but are somewhat persistent.

The weekly survey data shows that treatment has a significant impact on search intensity and an even larger impact on formal search in the city centre. Active job seekers reduce participation in temporary informal work during the times when they are searching. I find no evidence that the subsidies change expectations or attitudes, nor that the weekly phone calls induce Hawthorne effects. I use a pure control group who receive neither phone calls nor the subsides, and find no impact of the calls on search or employment.

The evidence suggests that cash constraints play a role in impeding job search. The trajectory of the impacts on job-search is difficult to explain by a price effect alone, for three key reasons. Firstly, among respondents who started as active job seekers and those who begin with relatively more cash on hand (predominant in the board sample), the effects of subsidies grow over time. The subsidies seem to prevent them from giving up job search as cash constraints start to bind. Secondly, treated individuals are still more likely to be searching *after* the treatment ends, suggesting that they are able to save the cash from the subsidies and thus search for longer. Finally, the treatment effects on search and employment are particularly strong among the relatively poor and cash constrained.

I outline a framework of job search with cash constraints, in which agents who must pay the costs of search repeatedly give up search as cash grows low. This framework predicts different trajectories of impact on job search between agents who start with enough cash to search and those who start with binding cash constraints. These types correspond roughly to the *board* and *city* samples, respectively.[3]

These findings contribute to a large literature showing how cash constraints can cause job-seekers to give up search and take up bad jobs too early (Card *et al.*, 2007; Basten *et al.*, 2014; Acemoglu and Shimer, 1999). In standard models unemployment insurance plays the role of allowing the unemployed to smooth consumption. I contribute to this literature with an experiment in which search costs are monetary and can be subsidised directly.[4] In my setting, individuals who are running out of cash give up looking for a good job, and thus either remain stuck in unemployment or take up temporary, informal work. I find that job seekers significantly reduce participation in temporary work while receiving subsidies, whereas those in the control group move in and out of temporary jobs often. This suggests a role for temporary or casual work in the job search process, by funding job search and consumption in the absence of unemployment insurance (Browning *et al.*, 2007).[5]

Secondly, this paper complements the literature on mobility constraints in developing countries. Migration, like job search, entails costs that can be too high or too risky to pay for cash constrained individuals (Bryan *et al.*, 2014; Ardington *et al.*, 2009; Angelucci, 2015).[6] Just as migration costs impede the efficient allocation of labour across space, I find evidence for frictions *within* urban labour markets due to the distance between residences and employment centres, which could be preventing

[3]In the online Appendix I present a formal model of this framework, which is calibrated to fit the observed labour market conditions and predicts remarkably similar job search trajectories to those estimated in the data.

[4]Existing work assumes utility functions that are separable in consumption and the (dis)utility of search. This leads to cash constraints lead job seekers to accept bad jobs too quickly. See Danforth (1979).

[5]Theories of segmented labour markets argue that workers use lower quality wage work to smooth consumption while queuing for better jobs (Lewis, 1954; Harris and Todaro, 1970; Fields, 1975). Evidence of factory work being used as last-resort employment lends support to this idea in a similar context (Blattman and Dercon, 2015).

[6]Jensen (2012) and Beam (2016) provide evidence on how information about jobs, in a rural context, can increase employment in urban jobs.

optimal matching of workers to jobs (Diamond, 1982).

The results have implications for the equality of access to employment opportunities, since the costs of search fall particularly hard on the poor and those living far away from the city centre. My results provide evidence for the spatial mismatch hypothesis (Kain, 1992; Holzer, 1991; Zenou, 2009), which has been studied empirically in large cities in the United States (Kling *et al.*, 2007; Phillips, 2014) but, to my knowledge, never rigorously tested in a developing country context until now.

Finally, my findings contrast with the existing literature that has found weak impacts of other active labour market policies on youth outcomes, particularly in developing countries (Betcherman *et al.*, 2004; Groh *et al.*, 2012; Ibarraran *et al.*, 2014). I estimate that the expected internal rate of return to the transport subsidies is overwhelmingly positive, but argue that individuals may not be able to borrow against future earnings in order to search. The findings suggest a role for interventions to provide job search assistance or unemployment insurance in urban Africa.

What are the implications of search frictions for the efficient functioning of the labour market? This paper argues that, in the context of severe cash constraints, search costs could be preventing potential good matches between workers and firms from happening. The paper does not claim that a scaled up subsidised transport program would not have displacement effects in equilibrium (Crépon *et al.*, 2013). However, treated respondents in the experiment were able to search for jobs, and meet firms that they otherwise would not have. The results show that these firms hired some of those that they met, presumably because they were judged to be the more productive candidates. In the conclusion to the paper, I reflect further on these questions and use representative data from large African cities to show how distance from the city centre is highly correlated with residents' occupations.

1 Setting and Experiment

1.1 Context

Addis Ababa is an ideal setting to study the effects of urban growth and transport costs on labour markets. The city's population has been doubling nearly every decade for the last 40 years, is now estimated to be 4.5 million, and is expected to grow to 12 million by 2024 (UN-Habitat, 2005). New migrants live disproportionately far from the city centre, and long term residents are being forced out of the inner city to make way for redevelopment (Yntiso, 2008).

The youth, who are new entrants into the labour market and lack prior experience to signal their ability, find it particularly difficult to find employment. Youth unemployment is high (at 28%) and many recent college graduates spend years looking for their first permanent job.[7] The majority of white-collar jobs are found on job vacancy boards located in the centre of Addis Ababa, where formal firms post advertisements, and most formal firms are located in the centre of town. Data from a survey of the 500 biggest employers in the city, across all sectors, shows that 50% of formal sector private sector jobs are located within 3 kilometers of the centre. Public sector work is even more concentrated in the centre.[8]

Jobs are often found through social networks, but for individuals in this study these tend to be low quality jobs, located in their local areas. Jobs in the centre of the city pay more, and have better working conditions.[9] While everyone in the study has a mobile phone, few had smart-phones, and few jobs are available online.[10] Thus to find better employment, young people must travel to the city centre and make formal applications.

[7] See an extensive literature on labour in Ethiopia (Serneels, 2007; Broussard and Teklesellasie, 2012; Mains, 2013; Haile, 2005; Franklin, 2014).

[8] Data on formal firms comes from Abebe *et al.* (2014).

[9] Most respondents in this study expressed an aspiration to permanent formal sector employment.

[10] Only 20% of the sample report ever having used the internet for search. The online job site (Ethiojobs.com) recruits mainly for specialised positions at NGOs and multinationals.

1.2 Experimental Design

I test for the relationship between search costs and labour outcomes by providing a cash transfer that could be used only for travel. Individuals received money to cover the costs of transport if they arrived to collect it at a kiosk in the centre of the city, nearby to the main job boards. In this way, the experiment closely resembles a transport fare reduction program, with one the minor difference that job seekers must have cash on hand to pay for their first single one-way trip. While transport costs are relatively high in this context, no respondents appear to have too little cash to make that first trip.[11] The cash amount given was tailored to cover the transport cost of a return trip from respondents' place of living to the centre, using the fares charged on public transport. The modal amount given was about one-third of the median of daily wages.[12] There was little or no spare cash left over after covering the costs of transport.

Each of the *board* and *city* samples were split randomly into three groups after the baseline survey: the treatment group who received the subsidies and were also called weekly, and two control groups; one that received the same weekly phone calls, and one that did not. This design allows me to test for an impact of the phone calls on endline outcomes. Furthermore, treated respondents were randomly divided into receiving the subsidies for either eight or eleven weeks. Out of the initial sample of 877, 552 respondents received the phone calls, of whom 258 were offered the transport treatment, while 325 were not contacted again until the endline. Randomisation was done by stratifying the sample using baseline covariates, following Bruhn and McKenzie (2009).[13]

The treatment group were clearly informed about the nature of the subsidies, and that they had been randomly selected to receive them. The treatment group began to receive the subsidies in week 1, one week after the end of the baseline survey. The phone survey also began in that week. By limiting the subsidies to no more than twice a week, the experiment was designed to limit the extent

[11] The first percentile of average weekly expenditure per person is twice as large as a single bus fare. When respondents were asked why they did not take up the subsidies, no one responded that they could never afford to travel.

[12] This was 15 Ethiopian Birr for a return trip or \$0.75 per day. The amount was enough money to make the trip by mini-bus which is the preferred mode of transport. The average one-way trip to the centre took 33 minutes by minibus.

[13] The following variables were used to stratify the randomisation: *Gender, Completed Grade 10, Diploma, Degree, Currently Employed, Work Experience, Age, Currently Searching for a job.*

to which commuting costs were subsidised. Respondents knew in advance that subsidies were offered for a limited time only. Restricting collection times to before midday on weekdays limited use for recreational purposes. Proof of identification was required to limit fraud. The last phone calls were completed one week after the last subsidy (week 12). In total respondents could collect the money up to 22 times.

1.3 Search Costs in Perspective

Individuals in my sample live on average 6.8, and some as far as 13, kilometres from the city centre.[14] Thus, for unemployed youth in Addis Ababa, the costs of gathering information about jobs are high. A *one-way* trip by mini-bus to the centre costs less than \$1, but this constitutes 12% of median weekly expenditure for individuals in the sample at baseline. Respondents spend more than 20% of median total expenditure on transport.

Job seekers have limited budgets, no social welfare support such as unemployment insurance, and must rely on temporary or casual jobs, which pay about \$10 a week on average, to fund job search and consumption. Individuals in the study oscillate between searching, taking temporary work, or doing neither from week to week. This suggests a tension between committing to searching full time and having to find short-term sources of income. Others (about 50% of the sample) rely on small amounts of money from their families to survive. So the cash offered by the intervention, on average \$1.5 per week or about 15% of wages for temporary work, constitutes a significant transfer for many of the respondents. Still, this should be thought of as only a partial job search subsidy, as it does not include other costs associated with search.[15]

The costs of searching for a good job are not evenly distributed. They are likely to be higher for those living far away from the city, and for the youth and new migrants who have limited access to social networks that could provide other routes to work.[16] The poor are likely to find it particularly hard to

[14] This is too far to walk to the centre. Very few individuals in this sample report doing so.

[15] Respondents reported other search costs, such as paying for printing, eating out of the home while travelling, renting newspapers with information about vacancies, and even paying firms to make applications.

[16] Calculations using geo-referenced data from the 2007 census at the census enumeration area level show that young

pay to search. Thus some young people are at a disadvantage in their ability to share in the growth of the urban economy.

2 Data

2.1 Two Sample Strategy

This study measures the impact of transport subsidies on two different but policy relevant sub-populations. This was done to mimic different ways in which active labour market policies might be targeted, and allows me to verify and compare the results across two different target populations. The two samples differ in terms of the location in which they were first approached and invited to take part in the study. The distinction between the samples was made in the study design; it wasn't selected *ex post*. The main conclusions drawn from comparing these two samples hold up when performing traditional sub-group analysis, by education or poverty, for instance. Both samples comprise men and women aged 18-30 who were available to start a new job in Addis Ababa in the next two weeks, even if they had temporary work. Only individuals living in neighbourhoods least five kilometres away from the center of Addis Ababa were included. One sample I call the *board* sample, the other the *city* sample:

Board sample: The board sample was drawn by randomly approaching individuals who were gathered around job vacancy boards in the city centre. Since they were all visiting the boards at the time of screening, they were, by definition, active job seekers. They are on average more educated (see Table 1), were often more recent migrants to Addis, and had less of a secure footing in the city. This is a group who are most likely to participate in the growth of high-productivity employment in Ethiopia, and would be most likely to self-select into active labour market programs, such as job-centres or training programmes. More than half had graduated from school or college in the last year, and only 23% had been looking for full-time work for more than 12 months. They start out as active

people and new migrants are also more likely to be living further from the centre of city.

job seekers, but search effort among the control group quickly diminishes over time.

Table 1: Education levels in the Experiment and Representative Data

	Board Sample	City Sample	Pooled Sample	Addis Ababa (2012 Data)
Degree	44%	1%	24%	10%
Other post-secondary	37%	23%	31%	25%
No highschool	5%	40%	21%	30%

City sample: This sample was drawn by going door-to-door in randomly selected enumeration areas on the periphery of the city. The survey sites are marked in Figure B1. Respondents were interviewed at home. They have on average lower education than those in the *board* sample and Addis Ababa as a whole: most have completed high-school but no more (see Table 1). This is a group that policy makers might consider to be chronically detached from the labour market, and thus vulnerable to related social problems, crime, and "scarring" from long term unemployment. They might be thought of as individuals who are liquidity constrained: they mostly search for work intermittently while doing informal work, while some are completely discouraged. About half have been out of school for more than 3 years. If distance from employment was one of the factors contributing to their detachment from the labour market, active labour market policies or improved transport could help them to resume search for better employment.

2.2 Phone Survey

In addition to face-to-face baseline and endline surveys, I use a weekly phone call survey to measure the trajectories of job search, employment, and treatment effects over time. These trajectories are used to identify mechanisms through which the subsidies impact job search, by fully accounting for job seekers' activities during the time of treatment. Each respondent in the phone survey was contacted on average 10.4 times.[17] Conditional on being reached in the first phone call, attrition from the phone survey was low: only 5% of the sample answered fewer than 50% of the calls. The phone questionnaire is short (calls took around four minutes to complete), and focusses on a small set of limited set of

[17]The usefulness and accuracy of phone call surveys is studied in Garlick *et al.* (2015).

outcome measures. This has the advantaged of pre-committing me to testing the significance of just a few major outcomes, and reducing bias from multiple hypothesis testing. Most importantly, the only measure of job quality in the survey is whether the job is permanent. I analyse more detailed face-to-face endline data separately only after establishing impacts on the primary outcomes.

2.3 Balance and Attrition

I test for balance on a variety of job market outcomes and baseline covariates (Table C1, Appendix). There is balance across a wide range of measures, in the pooled sample, as well as in the two samples separately, and none of key outcomes variables are statistically different across groups.[18]

Table 2: Attrition at Endline (week 16)

Panel A:Summary of Sources of Attrition (Pooled Sample)

		Phone Survey		
	No Call	Control	Subsidies	Total
Never found	81	22	22	125
	24.92%	7.48%	8.53%	14.25%
Contacted by phone only	0	35	31	66
	0%	11.90%	12.02%	7.53%
Refused at Endline	9	12	7	28
	2.77%	4.08%	2.71%	3.19%
Interviewed at Endline	235	225	198	658
	72.31%	76.53%	76.74%	75.03%
Total	325	294	258	877
	100%	100%	100%	100%

Panel B: Impact of Treatment and Calls on Response

	(1)	(2)	
	Pooled Sample	*Board Sample*	*City Sample*
Transport	0.0021	-0.0058	0.0043
	(0.0330)	(0.0516)	(0.0414)
Call	0.0422	0.0380	0.0634
	(0.0355)	(0.0467)	(0.0584)
Observations	877	877	

There is no difference in rates of attrition between individuals who were given the transport subsidies and those who were given the calls but not the subsidies. In addition, almost no baseline covariates

[18] When estimating the main treatment effects, I control for all variables showing any sign of imbalance at baseline.

are significant predictors of attrition at endline. Table 2 provides an overview of rates of attrition at various points of survey. Attrition is high in this sample, because this is a very mobile study population. I do not find 14% of the total sample at all after the baseline, and about 25% are not interviewed at the endline survey. However, among the phone call survey respondents, just less than half of total attrition takes place before the first phone call surveys.[19] This attrition is due to respondents who changed numbers or provided wrong numbers before the treatment began. This sort of attrition is unlikely to be correlated with the transport treatment, since treatment only starts after the first phone calls.

Panel B of Table 2 shows that there is no significant effect of subsidies, nor of the phone calls, on attrition. Further details are found in Appendix Table C3, showing very few significant determinants of whether a participant responds at follow-up (in week 16). I conclude that the results in the paper are not driven by attrition.

2.4 Take Up and Use of the Subsidies

Many, but not all, treated respondents collected the subsidies. In all, 66% collected the transport money at least once, during the three month period. Those that never collected the money were either not interested in job search, had no need for the money, or could not be contacted about the intervention.[20] Conditional on taking up the money at least once, respondents continue to take it regularly. The proportion of individuals collecting the subsidies remains stable over the course of the study.[21] In each week about 55% of treated respondents take the money, 45% take it on both of the available days. Very little predicts take up of the transport subsidies. The *board* sample were more likely to collect, but the difference is not significant.

Self-reported measures of what respondents did after collecting the subsidies shed some light on how

[19] 524 individuals were enrolled in the phone survey, 8% were never reached by phone. Out of the 465 that were reached by phone at least once, on average 400 were reached each week, an average rate of attrition on the phone survey of about 15%.

[20] Suggestive evidence from endline survey questions about the intervention suggest that imperfect implementation may have contributed to low take up, as around 20% of treated respondents reported not recalling the offer to take up the transport subsidies in the first place.

[21] See the Appendix, Figure B3.

they were used. In the *board* sample only 12% said they looked at the job boards "rarely" or "never" after collecting the transport money; 45% said they looked at the vacancy boards every single time they collected the money, compared to 25% of the *city* sample. However, the *city* sample were far more likely to have to have gone to inquire about work at firms directly. These differences across samples reflects the different search strategies used by job seekers, which in turn reflects on different jobs available to them.

3 Theoretical Framework

I argue that the results in this paper are best explained by a story of cash constrained individuals who must pay the costs of searching repeatedly, and give up search when cash on hand grows low. The trajectory of impacts over time are not well explained by a static framework where the subsidies increase job search purely through a price effect. Here I outline the intuition of dynamic job search decisions under cash constraints, to explain why the unemployed give up search when they run short of cash, even though they may have enough cash on hand to search at least once. I suggest a number of tests that follow from the cash constraints story, which guide the interpretation of the results on job search. This framework is not used to explain what kinds of jobs people get if they search more intensively, which is largely determined by the kind of jobs respondents are qualified to get.

In this setting, young people search for good jobs, while having irregular forms of informal or temporary work available to them. Each week individuals must decide whether to pay the costs of traveling to the centre to search for work. Good jobs are hard to find, and the probability of finding one after paying the search costs are low.[22]

Young unemployed people start to look for work with an endowment of savings, which is depleted by search costs. As cash runs low, search becomes riskier. Temporary work (such as casual manual labour) may be available at times, but such income streams are highly uncertain. Thus risk-averse

[22] The probability of finding permanent work conditional on paying the search costs are about 2% per week, just 19% of the control group find them.

individuals give up search when they start to run short on cash because wish to maintain buffer savings, even if they have enough cash to pay the costs of search for at least one more week.[23] Indeed individuals do appear to hold precautionary savings.[24] An unemployed person will search again briefly, after receiving some cash from temporary work or family support, until cash is depleted again.

This framework explains the volatile job search behaviour observed in the data: job seekers often transition from week to week from searching for work, while sometimes finding temporary or casual work, sometimes searching and working simultaneously. Those who start with surplus cash on hand search regularly until they run low on cash, while those who face binding cash constraints search infrequently. In the Online Appendix I present a formal exposition of this framework.[25] I simulate the model with different starting conditions (in terms of starting cash relative to the steady state) to generate different predictions about the trajectory of job search over time, and the trajectory of treatment effects. These testable implications would not follow from a static model of a change in the cost of the search, or other behavioural explanations related to nudging or motivation.

Testable Implications: Firstly, job seekers who start with enough cash on hand also start as active job seekers. They continue to search for a few weeks without subsidies. In these weeks, treated respondents pick up the cash after making trips that they would have made anyway. They experience the subsidies, temporarily, as a cash transfer, keep cash on hand and thus give up search later than if they had not received the subsidies. Thus, a sample of active searchers will not be impacted by the subsidies immediately after the experiment begins; the control and treatment groups should only diverge in search behaviour gradually over time as individuals in the control group give up searching at a faster rate. I argue, and test, that this best characterises the *board* sample (who are more recent graduates, and begin as active job seekers) and those who begin with relatively more cash on hand.

By contrast, individuals who have been looking for work for longer, start with less cash on hand,

[23] Individuals are not so literally cash-constrained that they can never afford to travel: subsidies allows them to pay to travel to the centre without risk because they know they will be immediately reimbursed.

[24] At endline survey individuals in the control group who are not working but have stopped searching still spend over 100 Birr ($5) per week on personal items, over 50% have cash on hand at the time of the interview, and a further 34% have cash in the bank.

[25] I write and calibrate a dynamic model of job search with credit constraints, in which infinitely lived agents search for permanent jobs over many periods, and can search and do temporary work simultaneously.

and therefore face binding cash constraints, and exhibit volatile search behaviour. The treatment effects should show earlier in the experiment since subsidies immediately remove those constraints. Respondents can immediately search more without the risks implied when search was costly. I show that this best characterises the search behaviour and treatment effects in the *city* sample, who are generally poorer, have been looking for work for longer, and are less frequent job-seekers.

Second, the framework generates the prediction that the treatment effects should be persistent for those who are start as active job-seekers. Active job seekers run out of cash during the course of the study. But when they are treated some will manage to hold onto buffer savings right until the end of the study, because they can search for free. These individuals would be able to continue to search more intensively even after the subsidy is removed.

Third, we should expect heterogeneous impacts according to initial levels of poverty, because especially wealthy individuals will be less cash constrained and thus less likely to be impacted. They may still search for work regularly, and collect subsidies, but won't search more than they would have without subsidies, especially in the early weeks.

Finally, this framework sheds light on the relationship between casual labour and search for good jobs. Job seekers may rely on temporary jobs to survive when they are short on cash. They may also take temporary work to save money so that they can resume search in the future (Browning *et al.*, 2007).[26] When the latter strategy is common, we might expect a reduction in participation in temporary work while the subsidies are in place.

Credit markets for job search: Given the high returns to finding good work, why are job seekers not able to borrow from friends and family to search? Information asymmetries may make it difficult for a job seekers to accurately assess their own probability of finding a good job and therefore make it risky to borrow against that. It is even harder for a family member to assess that probability and the corresponding risk of investing. Such lending agreements would be subject to numerous principle-agent problems, especially since search effort is so hard to verify, and not all young people would

[26] The formal model discusses this point in more depth. Working at a temporary job may also limit an individuals ability to search for work simultaneously (McCall, 1996).

actually use such loans to search. Finally, job seekers are from very poor families that may face liquidity cash constraints of their own.[27]

4 Impact on Employment

I show that high search costs cause worse labour market outcomes by showing that treated respondents have significantly better employment at endline. For endline outcomes I estimate intention-to-treat (ITT) impacts on employment outcomes. Following McKenzie (2012), my preferred specification includes a lagged dependent variable to control for differences in baseline outcomes, and set of controls for baseline covariates. Specifically, for both endline surveys, separately, I estimate:

$$Y_{ic} = \alpha + Y_{i0}\rho + T_i\lambda + X_{i0}\beta + \epsilon_{ic} \tag{1}$$

Here Y_{ic} is the outcome for individual i in cluster c.[28] T_i is the treatment variable dummy, X_{i0} a set of baseline controls, and Y_{i0} the outcome of the dependent variable at baseline (week 0). The main results are robust to a battery of alternative specifications, by estimating Equation 1 without Y_{i0}, X_{i0} or both (pure difference in means), or by controlling for strata (dummy variables) used in the randomisation (see Tables C5 and C6 in the Appendix). To estimate different effects λ_s for either sample or subgroup s, estimating the control group mean for each sample α_s, I run:

$$Y_{isc} = \alpha_s + Y_{i0}\rho + \sum_s T_i S_{si}\lambda_s + X_{i0}\beta + \epsilon_{ic} \tag{2}$$

[27] Certainly, many families do provide financial support to job seekers in this sample, but job search can take many months. Young people often express anxiety that this support will run out, and often face pressure to give up looking for a permanent job to start earning money in the informal sector, in order to start providing money to their family more immediately.

[28] All standard errors are clustered at the Woreda level (the lowest urban administrative unit in urban Ethiopia, of which there are 70 in my data), to avoid correlations between cluster-level shocks and treatment. Results are robust to using unclustered standard errors.

Table 3: Effects of transport subsidies on Employment Outcomes at endline (Week 16)

	(1)	(2)	(3)	(4)
	Permanent Work		*Any Work*	
Pooled	0.043		0.067*	
	(0.027)		(0.034)	
Board		0.078**		0.046
		(0.037)		(0.051)
City		-0.0043		0.088**
		(0.034)		(0.041)
Observations	657	657	658	658
R^2	0.215	0.221	0.578	0.581
Pooled Control Mean	0.13		0.53	
Board Control Mean		0.19		0.58
City Control Mean		0.065		0.46

The dependent variable is a dummy variable equal to one if the individual reported having a job (columns (1) and (2)) or a permanent job (columns 3) and (4)) , measured at both endlines. Results are from OLS regressions on endline outcomes, with the ANCOVA specification, which includes a control for dependent variable at baseline. Panel A gives average ITT effect for the two samples together. Panel B shows results from the two different samples- "board" and "city". Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level.

4.1 Jobs and Permanent Jobs

Table 3 contains the main result, showing the impact of the transport subsidies on the probability of having a job, and having a permanent job, at the main face-to-face endline survey at Week 16. Treatment increases the probability of finding permanent work by 7.8 percentage points for the *board* sample, over a mean of 19% in the control group (a 40% increase in the probability of having a permanent job). This shows that the subsidies allowed these individuals to find the good jobs that they were looking for.

In the *city* sample treatment significantly increases the probability of employment by about 9 percentage points (a 20% increase, over a control mean of 53%). Temporary and informal work is common in this sample, so the fact that treatment increased overall employment, together with improved job quality (Section 4.2) suggests that subsidies increased the probability of finding an acceptable job offer, resulting in a shift out of unemployment. By contrast, the *board* sample are not significantly more likely to be working, because the effect on permanent jobs displaces work that would otherwise have been done in temporary or casual jobs.[29] The average effect on employment across the two samples is large and positive but only significant at the 10% level.[30]

The *city* sample are not more likely to find permanent work, probably because it is difficult for this group to find permanent work at all: only 6.5% of the control group have permanent jobs at endline. Most of these jobs require post-secondary education and only 1.5% of this sample has a degree. I confirm that the treatment effects on permanent work are concentrated among respondents with degrees (See Table C10 in the Online Appendix.)

4.2 Job Quality

The results to this point have focussed only on outcomes that were included in the phone survey. I now study a broader range of job quality measures from the endline survey. Table 4 shows that transport subsidies significantly improve job quality, for both samples. This suggests that respondents are not simply getting work faster by accepting inferior jobs. To account for multiple tests performed, I provide q-values to control for false discovery across the 7 seven outcomes for each row of estimates.[31] The estimated impacts on many measures of job quality are significant at the 10% level in the pooled sample, but this again masks considerable heterogeneity across the samples in terms of the types of good jobs individuals could get. The *board* respondents are more likely to be working in the central business district and more likely to have jobs that require at least a degree as a qualification.[32] For the *city* sample, whose outside options often include work on construction sites or on the street, it is noteworthy that the treatment increased the probability of having a job in an office, with a monthly salary, and having acquired that job by formal application. The coefficients estimated here are larger

[29] I find the impact of the treatment on having temporary work is negative (although not significant) for the *board* sample. The result is not presented here.

[30] Results in the Appendix, Table C6, show that these results are robust to a number of different specifications. The coefficient on the pure different in means estimate is slightly smaller than the other specifications: this is due to an equally small and insignificant difference in employment outcomes in the baseline. Including covariates controls for that imbalance.

[31] To calculate q-values I use the the procedure suggested by Benjamini and Hochberg (1995) and implemented by Anderson (2008) to control the number of false rejections.

[32] During focus group discussions run during the endline survey, many respondents with University degrees attached special importance to the goal of finding work for which they were specifically trained. By contrast, it seems impossible for respondents in the *city* sample to a job requiring a degree.

Table 4: Treatment Effects on Job Quality at Endline

	(1) log wage	(2) degree	(3) in office	(4) pay monthly	(5) satisfied	(6) formally	(7) in city
Panel A: Impacts on work outcomes at week 16							
All	0.051	0.047**	0.070*	0.069*	0.061**	0.054*	0.059*
(se)	(0.088)	(0.018)	(0.037)	(0.037)	(0.028)	(0.029)	(0.032)
[q-val]	[0.566]	[0.081]	[0.081]	[0.081]	[0.081]	[0.081]	[0.081]
Observations	356	596	596	596	596	596	596
R-squared	0.115	0.228	0.059	0.107	0.058	0.114	0.051
Control Mean	5.47	0.06	0.16	0.24	0.16	0.12	0.22
Panel B: Heterogeneous impacts by sample on work outcomes at week 16							
Board	0.091	0.075**	0.020	0.032	0.015	0.064	0.097**
(se)	(0.11)	(0.033)	(0.052)	(0.053)	(0.045)	(0.049)	(0.042)
[q-val]	[0.699]	[0.088]	[0.739]	[0.739]	[0.739]	[0.456]	[0.088]
City	-0.0090	0.014	0.13**	0.11**	0.11***	0.042*	0.015
(se)	(0.15)	(0.011)	(0.050)	(0.049)	(0.029)	(0.023)	(0.046)
[q-val]	[0.952]	[0.273]	[0.042]	[0.056]	[0.002]	[0.132]	[0.87]
Observations	356	596	596	596	596	596	596
R-squared	0.116	0.230	0.063	0.108	0.062	0.114	0.053
Board mean	5.55	0.11	0.22	0.34	0.15	0.19	0.25
City mean	5.36	0	0.09	0.12	0.16	0.03	0.19

Results are from OLS regressions on endline outcomes at week 16, using a set of baseline covariate controls. Panel A shows results for the two samples pooled together. Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level. Dependent variables: (2) *Degree*: Respondent has a job that required a degree as minimum qualification (3) *In Office:* Job is performed in an office, or formal business house- proxy for "white collar" work (4) *Pay Monthly:* Respondent is paid every month, usually according to set a contract (5) *Satisfied:* The respondent has a job with which he/she reports being very satisfied. (6) *Formally:* The job was acquired through an official application and interview process (this excludes referral from a friend or family, or jobs given after just a conversation with the employer (7) *In city:* The job is performed in an area close to the centre of the city. Control means in the lower rows show the mean of the outcome for those individuals who have jobs, for the two samples respectively. FDR q-values are given in square brackets using the method of Benjamini and Hochberg (1995).

than those on overall employment.

I find no significant impact of treatment on average wages, although the coefficient is large and positive for the *board* sample.[33] This result is not particularly surprising. Entry level wages tend to be similar across different occupations and types of work, while the wage returns to job tenure are high.[34] Furthermore, high salaries may be offered in low-quality jobs to compensate workers for tougher and riskier working conditions. Not all good jobs found in the city centre actually take place in the centre, even if firms have headquarters based there and use the central vacancies boards, and most formal

[33] I am unable to reject the Kolmogorov-Smirnov test of equality in distribution of wages between treatment and control groups. The results do not seem to be driven by selection effects: quantile regressions show that the treatment has no impact on the probability that individuals in either sample are above various percentiles of income.

[34] I rank professions by average wage (estimated from representative labour force data, where large wage gaps do exist), and find a distinct shift towards higher paying professions due to the treatment (see Figure B4).

firms do cover the costs of transport for their permanent workers. Still, some workers may face net pay decreases after paying the cost of commuting to formal jobs. Many seem happy to accept this in return for secure, formal jobs with higher earnings in the long run.

4.3 Persistence and Catch Up

Are these impacts on job outcomes persistent a further six months after the first endline survey? The most employable job seekers in the treatment group may have found good jobs while receiving the subsidies, but their equally employable counterparts in the control group may have continued to search for work steadily in the following six months, gradually closing the gap in (good) employment. Alternatively, the effects will be persistent if employment and job search outcomes exhibit duration dependence: if job seekers become less employable after being unemployed for an extended period, or if job search becomes harder over time as discouragement sets in.[35]

Table 5: Persistence of Employment Effects at Second Follow-up

	(1)	(2)	(3)	(4)
	Permanent Work		*Any Work*	
Pooled	0.017		0.068*	
	(0.033)		(0.039)	
Board		0.033		-0.0096
		(0.051)		(0.051)
City		-0.0017		0.108
		(0.038)		(0.080)
Observations	605	605	605	605
R^2	0.323	0.324	0.602	
Pooled Control Mean	0.13		0.53	
Board Control Mean		0.19		0.58
City Control Mean		0.065		0.46

The dependent variable is a dummy variable equal to one if the individual reported having a job (columns (1) and (2)) or a permanent job (columns 3) and (4)) , measured at both endlines. Results are from OLS regressions on endline outcomes, with the ANCOVA specification, which includes a control for dependent variable at baseline. Panel A gives average ITT effect for the two samples together. Panel B shows results from the two different samples- "board" and "city".] Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level.

[35] Also, getting a job could require a certain persistent regularity of job search that is a hard to maintain without income support. The control group never major to get above the required threshold of search intensity, while the subsidies allow the treated group to do so.

I provide evidence that the results have dissipated but not entirely, 6 months later, suggesting some persistence. In Table 5, I show that when surveyed 6 months later (at week 40) those who were treated among the *board* sample are now 3.3 percentage points (roughly 10%) more likely to have permanent work: the coefficient has about halved over six months.[36].

5 Impact on Job Search and Temporary Work

How were job seekers constrained by the costs of transport? I find that treated respondents are more likely to search (regardless of whether they are working), and less likely to take up forms of temporary or informal work. In particular, the subsidies lead to increased search in the centre of town and at job boards where vacancies for formal, permanent jobs are posted. Equation 3 estimates the pooled average treatment effect across all weeks. I interact treatment with a continuous week variable w, to estimate the trend of the treatment effect over time, estimating an intercept term, linear, and quadratic trend terms, as in equation 4 and 5, below.

$$y_{it} = \alpha_t + T_i\lambda + X_{i0}\beta + \epsilon_{it} \qquad \forall t \neq 0 \tag{3}$$

$$y_{it} = \alpha_t + T_i\lambda_0 + T_i w\lambda_1 + X_i\beta + \epsilon_{it} \tag{4}$$

$$y_{it} = \alpha_t + T_i\lambda_0 + T_i w\lambda_1 + T_i w^2\lambda_2 + X_i\beta + \epsilon_{it} \tag{5}$$

Equation 6 estimates the treatment effect in each week separately, by looking at the coefficients λ_t on the week dummy variables W_{it}.[37]

$$y_{it} = \alpha_t + \sum_t T_i W_{it}\lambda_t + X_i\beta + \epsilon_{it} \tag{6}$$

[36] The results for the *city* sample should be treated with caution. While the coefficient on employment is large it is not significant in this smaller sample, and for the *city* sample I find high rates of attrition that attrition is correlated with treatment status for this sample. The same problem is not present for the *board* sample (see Table C4)

[37] In all specifications, "treatment" is defined as having received the transport subsidies at any point in the past, the treatment switches on in week 1, and does not "switch off".

Figure 1 plots the trajectory of λ_ts over time, overlaid with the average effect, as well as linear and quadratic trend estimates for both the *board* sample (Column 1) and the *city* sample (Column 2), in Panel B.[38] Panel A shows local polynomial regression estimates of the probability of searching for employment as a function of time.

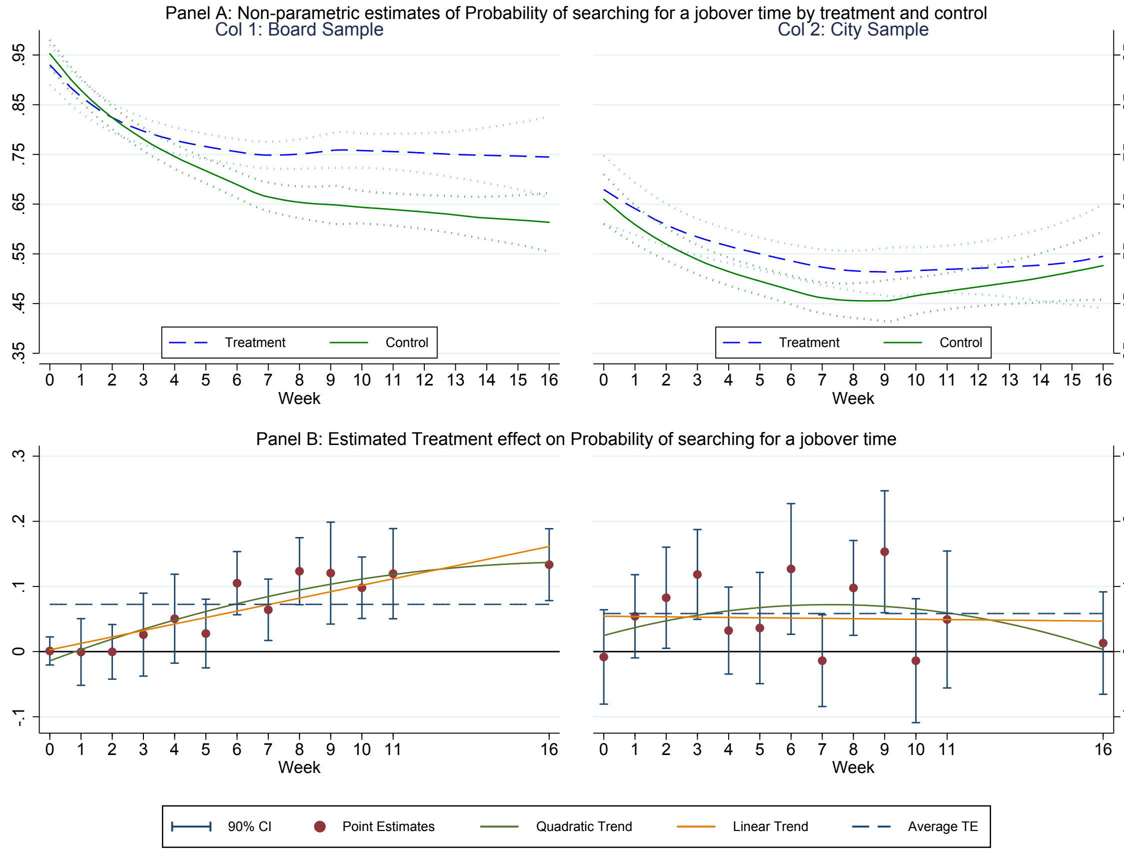
5.1 Job search trajectories

Figure 1 shows the impact trajectories on job search for each week of the study. While the subsidies were in place those in the *board* sample are 7.3 percentage points more likely to search, while those in the *city* sample are 5.9 percentage points more likely to search, across all weeks pooled together. However, the samples differ in the timing and trajectory of the impacts, as is clear from Figure 1. Those in *board* sample give up job search gradually over time: the treatment effects are significant for the later weeks of the experiment, but not immediately. The upward linear trend term is large and highly significant. In the *city* sample the effects are more immediate, and relatively constant over time while subsidies are still in place.

I argue that these results are driven by the story of cash constraints, according to which cash-on-hand influences future job search trajectories. I estimate heterogeneous treatment effects after pooling the two samples together. In Figure 2 shows that respondents who start out as active searchers (those who were searching for work at baseline) respond more slowly to the treatment as search activity declines over time in the treatment group. In the cash constraints framework, they have not yet begun to bump up against those constraints, and experience the treatment as a cash transfer in the early weeks. Treated respondents keep on searching without cost, thus delaying the point at which they give up job search, and the treatment effects to grow with time, as those in the control give up. Those who began the study as infrequent-searchers (assumed to already be liquidity constrained) respond to treatment immediately, but the treatment effect declines when subsidies end (Figure 2, Column B). Consistent with this story, I show that poorer respondents respond more immediately to

[38] For the sake of brevity the point estimates in the Figures (both the weekly coefficients, and linear and quadratic trend estimates) are not presented here, but are available on request.

Figure 1: Impact on job search: Non-parametric trends & treatment effects over time

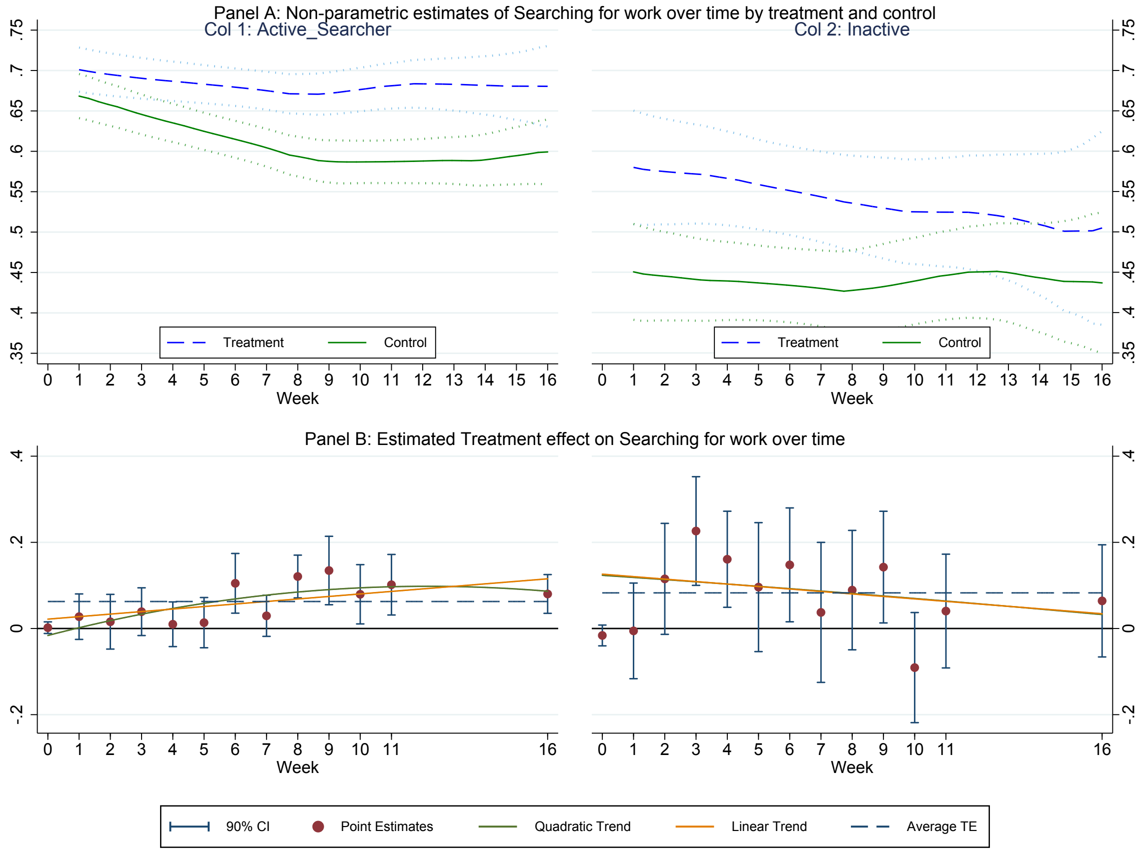


treatment, while the effects take longer for wealthier individuals (see Figure B5 in the Appendix). The composition of the *board* sample more closely resembles those active job-seekers: they were all visiting the job boards at baseline (by design), and do seem to be more cash liquid: they spent on average 40% more in the first week of the study. By contrast, the *city* sample start as less active job seekers, only 64% were looking for work and only 25% had recently visited the job boards or the city centre to look for work, at baseline.

Finally, transports subsidies increase job search at the formal job vacancy boards and at the city centre (see Figures B7 and B8 in the Appendix). For the *boards* sample the impact on searching for work at the job board are larger (well above 10 percentage points for later weeks of the study) than the effects on job search.[39] For both samples the impact on having a visited the centre of the city are

[39]This suggests that the treatment induced individuals who would have been searching anyway to change their method of search, as well as inducing individuals to start searching at the boards who would otherwise not have been searching at all.

Figure 2: Job Search Trajectories among "Active" versus "Inactive" searchers at baseline



larger than the impact on searching at all.

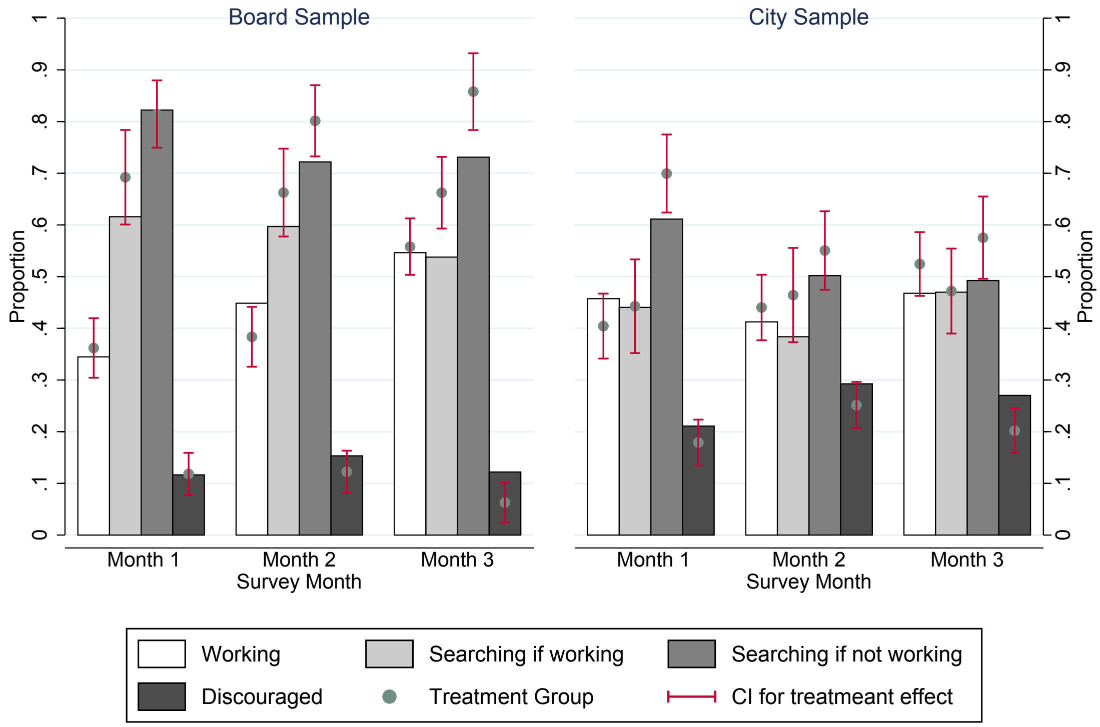
5.2 Temporary Work During Job Search

To better understand the interplay between work and search, I pool weekly observations into groups of 4 weeks, and estimate the impact of treatment on the probability of engaging in different activites: searching, working or both, in each of these *months*.[40] I then plot, for each month, the proportion of the control group in these different states, along with estimated treatment effects, with confidence intervals, in Figure 3.

In the *board* sample the treatment group are 6.7 percentage points less likely to be working (at

[40]This gives me power to more precisely estimate the treatment effects. Coefficients are presented in Table C8. To allay concerns that these months were chosen strategically to boost significance, I re-estimate the impacts in groups of four weeks , starting with the first four weeks of the study, and then iteratively moving this window forward by one month (Table C9).

Figure 3: Control means and treatment effects on labour outcomes by month



Note: Here, the bars represent the proportion of the control group engaged in each activity, while the points with confidence intervals represent the means for the treatment group, with confident intervals taken from regression estimates of the impact of the treatment (relative to the control group). Two conditional categories are shown: for instance "Searching if working" shows the proportion of employed individuals who were searching for work. This representation shows the impacts of the treatment on job search, independent of its impact on employment.

temporary jobs) throughout the whole second month of the study. This is consistent with the theory that job seekers rely on temporary work to cover the costs of searching for permanent work, such that reduced search costs should lead to reduced reliance on temporary work for these individuals, which in turn could allow more time to invest doing job search.[41] I do not find the same effects for the *city* sample, perhaps because they had already begun to find better temporary jobs by the same point in time.[42] The subsidies are not inducing an increase in job search only through the channel of reduced temporary work: Treated respondents are more likely to be searching whether they had temporary work, or no work at all.[43]

[41]The effect on employment is driven completely by a reduction in temporary casual or other informal sector work and is not persistent after the subsides end.

[42]The impact on employment in this sample trends upwards throughout the experiment (see Figure B6): this could be dampening any reduction in less desirable forms of temporary labour.

[43]There is a negative effect on being "discouraged" (not searching nor working), in Figure 3, suggesting that treatment brings individuals who would otherwise have been doing nothing back into the labour market.

6 Evidence for Cash Constraints

The trajectory of impacts in Figure 1 is not compatible with a mechanism whereby subsidies provide a static incentive to search. Such a mechanism would predict constant treatment effects over time, no persistence, and no impact on temporary work. Instead, I provide further evidence that cash constraints explain the results.

6.1 Persistence

A reduction in search costs should allow active job seekers to search consistently throughout the experiment and keep buffer savings intact. This would allow them to continue to search more actively even after subsidies end. I randomly varied the end week of the subsidies among treated individuals in order to compare individuals receiving subsidies in weeks 9-11 to those who randomly finish the treatment in week 8.

$$y_{it} = \alpha_t + Treat_i\lambda + Treat_i * Available_{it}\delta + X_{i0}\beta + \epsilon_{it} \qquad \forall t \geq 8 \tag{7}$$

Estimates from Equation 7 are presented in Columns 3 and 4 of Table 6. $Available_{it}$ is equal to one only if subsidies were available to individual i in week t. Once the treatment randomly ended for an individual this variable "switches off". Columns 1 and 2 show the simple effect of the subsidies on search at the endline, five weeks after subsides ended for everyone.

The coefficient on *Treatment* (λ) is large and significant while that on *Treat*Available* (δ) is not. The effects on job search are persistent: individuals who stopped the treatment went on searching as intensely as those who were still receiving it during weeks in 9-11. And treated respondents were still more likely to be searching by week 16. Again, these results mask heterogeneity between individuals with different starting conditions. Section 3 discussed how effects should not be persistent for those that start with binding cash constraints: they do not build up savings over time, but simply search more while subsidies are in place. Figure 2 confirms that the persistence findings hold only for

Table 6: Persistence of Impacts on Search Behaviour

	(1)	(2)	(3)	(4)
	Search Week 16	Search-boards Week 16	Search Week 8 on	Search-boards Week 8 on
Treated	0.076*	0.0680	0.10***	0.085**
	(0.041)	(0.044)	(0.037)	(0.040)
Treated*Treatment Available			-0.0220	-0.00630
			(0.047)	(0.040)
R-squared	0.0590	0.0840	0.0660	0.130
Observations	658	658	2202	2202

Columns (1)-(2) show impacts of the treatment on job search for those who were treated at the endline (week 16). Columns (3)-(4) study weeks 8-12, during which the subsidies were discountinued randomly at different times, so that some individuals still have the subsidies available, while a random subset do not. The interaction with "Available"' estimates the effect of *still* receiving the subsidies, on top of the effect of having been included in the treatment group originally. Each coefficient gives the estimate for the treatment effect of *transport* with the sample restricted to the weeks denoted in the first column. Standard errors are in parenthesis and are robust to correlation within clusters. $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

those who began as active job-seekers (predominant in the *board* sample). In addition, the results are persistent for wealthier individuals, who started with more cash on hand, but not for poorer individuals (Figure B5 in the Appendix.)

6.2 Heterogeneous Treatment Effects by Cash-constraints

In a model without cash constraints, the effects of search costs should be independent of wealth. I construct an index to capture cash constraints, comprised of household wealth, savings, and expenditure, at baseline.[44] I estimate differential treatment effects for individuals above and below the median of this index using Equation 2.

Consistent with the proposed mechanism, I find that treatment effects are larger for individuals who are more cash constrained at the baseline. Individuals from poorer households were more likely to find permanent jobs, or have any work at all, after receiving subsidies.[45] During the weeks in which the subsidies were available (Weeks 1-12) poor households search significantly more in response to treatment than non-poor households (Column 1 in Table 7), although both groups were positively affected.[46]

[44] The results are similar looking at these different measures in turn.
[45] Results are presented for the two samples pooled together but I find similar patterns in the two samples separately.
[46] I find similar take up rates are similar among poor and not-poor households.

Table 7: Heterogeneous effects by poor/non-poor on Job Search

	(1)	(2)	(3)	(4)
	Search Weeks 1-12	Permanent Work Week 16	Work Week 16	Discouraged Week 16
Heterogeneous Treatment Effects in Pooled Sample				
Poor HH	0.14***	0.069*	0.099*	-0.050
	(0.018)	(0.038)	(0.054)	(0.039)
Not-Poor HH	0.058**	-0.034	0.0011	-0.052
	(0.023)	(0.048)	(0.067)	(0.049)
Observations	4,510	657	658	658
R-squared	0.619	0.152	0.576	0.220
Poor=Not-Poor F-stat	7.20	2.87	1.29	0.00038
Poor=Not-Poor p-val	0.0073	0.091	0.26	0.98

Household poverty is defined as being below the median for a household poverty index comprising household wealth (assets), individual savings (cash on hand) at baseline, and individual expenditure at baseline. Results are robust to using different poverty measures. Results are from OLS regressions on endline outcomes. Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level. Column (1) reports the results pooling observations across weeks 1 to 12 of the study, while the subsidies were in place. Results are consistent when estimating linear impact trajectories, or week-specific treatment effects. Columns (2-4) reports the results for the main face-to-face endline survey at week 16, 1 month after the subsidies ended.

6.3 Alternative Mechanisms

I test for behavioural explanations that could be driving the results. The regular calls may have induced Hawthorne effects, by nudging or priming job seekers to think about searching more, or by updating their beliefs about the returns to search. Using a group of respondents who were randomly not called, nor given subsidies, I look for impacts of the phone call surveys on endline outcomes. In Table 8 “Calls” shows the impact of receiving the phone calls on everyone who received them, “Treated” is the additional effect of receiving the subsidies. I find no significant impact of getting phone calls, across a range of endline outcomes.[47]

The trajectory of impacts might be explained by some interaction of effects of the treatment on motivation, expectations of future job offers, and perceptions of market wages. In Table C11 (Appendix) I find very little evidence for treatment effects on a range of such outcomes.[48] All respondents in

[47] All treated individuals were called. The impact of the subsidies is still large and significant, although less precisely estimated with this smaller control group.

[48] I do find a significant negative impact on perceptions of market wages, among the *city* sample, who begin with over-inflated expectations about the wages that they can earn in the market. They expect to earn 1500 Birr ($75) per month on average at baseline, when in reality, those that found jobs earned little over 1000 ($50).

Table 8: Impact of the phone call survey on outcomes at endline

	(1) searchnow	(2) searchboards	(3) discouraged	(4) work	(5) work perm
Panel A: Average Impacts at Endline (Week 16)					
TE trans	0.095*	0.084	-0.059*	0.051	0.031
	(0.048)	(0.054)	(0.030)	(0.045)	(0.032)
TE call	-0.027	-0.0046	0.010	0.014	-0.0036
	(0.050)	(0.046)	(0.044)	(0.053)	(0.033)
R-squared	0.021	0.073	0.037	0.016	0.047
Panel B: Average Impacts at Endline (Week 16) by Sample					
TE trans boards	0.13*	0.11	-0.050	0.057	0.099**
	(0.073)	(0.090)	(0.044)	(0.060)	(0.045)
TE trans city	0.050	0.053	-0.073*	0.048	-0.045
	(0.061)	(0.053)	(0.042)	(0.067)	(0.037)
TE call boards	0.00024	-0.0028	0.042	-0.023	-0.056
	(0.071)	(0.073)	(0.044)	(0.064)	(0.052)
TE call city	-0.071	-0.012	-0.035	0.064	0.057*
	(0.072)	(0.052)	(0.087)	(0.087)	(0.029)
R-squared	0.025	0.074	0.040	0.018	0.055
Observations	658	658	658	658	657

Dependent Variables are listed at the top of each column. Results are from OLS regressions on phone survey outcomes, with different treatment effects estimated as the average of groups of 4 weeks. Each coefficient gives the estimate for the treatment effect of *transport* with the sample restricted to the weeks denoted in the first column. The total number of observation used all regressions in each *row* is given in the last column (N). Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

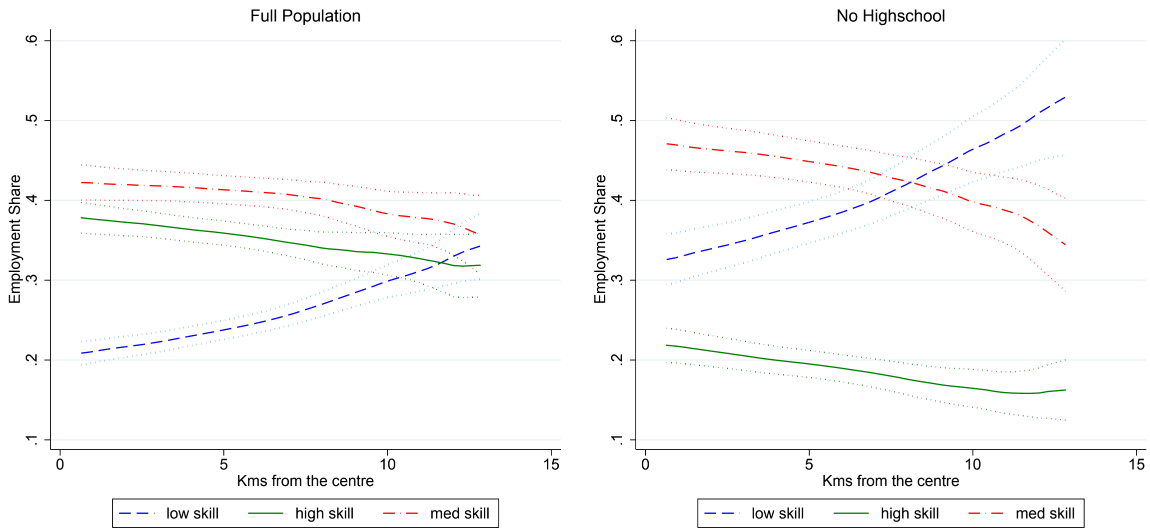
the *board* sample were already very familiar with the job boards as a method of job search, so the treatment did not teach them about a new search technology.

Finally, the results do not seem to be driven by changes in the cost of commuting *to get to work*: many of the impacts on good quality jobs are evident only towards the end of the study, most likely because the job application process for formal jobs takes some time. Respondents would not have had time to benefit from lower commuting costs for long, yet they are still more likely to be in formal jobs long after subsidies ended.

7 Conclusion

This paper uses evidence from randomised transport subsidies to show that search costs impose significant constraints on young people trying to find good work. Simple internal rate of return

Figure 4: Job quality by distance from the centre of Addis (2013 LFS data)



calculations suggest that the returns to job search are considerably higher than the costs of the subsidies, on average.[49] The individual returns to search may just be highly heterogeneous and difficult to observe by both job-seekers and their potential creditors. So while it may not be individually rational for risk-averse job seekers to invest in search, a social planner may wish to subsidise all young people to search more, because of the high returns to those who will find a match.

Do these search frictions have implications for the efficient working of labour markets? This paper cannot show that improvements in labour outcomes for individuals in the study did not displace other workers (Crepon et al, 2013). However, a number of descriptive findings suggest that frictions play an important role in labour markets and could be influencing the efficient allocation of workers to jobs. High productivity matches might not be happening when workers are locked out of good formal jobs due to high search costs. Using geocoded census data for Addis Ababa, Nairobi, and Kampala, I show that informality increases with distance from the city centre: the share of jobs that are informal increases significantly by 2.4%, 2.1% and 1.2% per km from the centre of those cities, respectively.[50]

[49] I argue that the total cost per treated individual amount to about \$9 on average, while the gains from finding permanent employment faster are estimated to be between \$15 and \$30, depending on assumptions. This does not include the non-monetary benefits of improved job quality, nor the considerable wage gains differentials paid in permanent jobs after a few years of tenure, but rather focuses just on the increased regularity of employment in permanent work.

[50] These regressions exclude all agricultural labour on the periphery of the city. The coefficients are much larger when I include agricultural informal work in the regressions.

On average someone living out the outskirts of one of these cities is about 24% more likely to be in informal self-employment than wage employment. In Figure 4 looks at the proportion of different types of employment plotted as a non-parametric function of distance to the centre of the city, using a geo-coded Labour Force Survey (2013) from Ethiopia. The proportion of workers in low-skilled occupations increases drastically with distance, while high-skilled work decreases.[51] These relationships are stronger among the youth, and hold when comparing only young individuals with the same educational attainment.[52]

The results from the experiment show that at least a part of the reason that young people are caught in these low-skill jobs is due to the costs of job search: the treatment makes up a significant fraction of the observed difference in high-quality employment across space. So while the workers in this sample may have displaced other workers, that does not imply no gains in efficiency due to match quality. When firms met these workers, they hired some of them, suggesting that they were judged to be productive than the usual pool of applicants. Thus, cash constraints appear to be leading workers to choose lower productivity occupations. This in turn could affect the proportion of good-jobs offered by firms by lowering the value of good vacancies (Acemoglu and Shimer, 2000). An interesting avenue for future research would be to study, experimentally, the impact of improved match quality on firm productivity, wage setting, and recruitment.

Furthermore, the nature of unemployment in urban Africa suggests an outsized role for search frictions in determing the length of school-to-work transitions. In the phone panel young people move in and out of work regularly, and often quit non-permanent jobs after only a few weeks, all while looking for work. Census data shows that unemployment remains high even for people over 30. In my sample, more than 27% of job seekers who find permanent jobs took more than 4 years to find them. Before they do so, high-skilled workers use low-skill jobs for temporary income in order to pay high search costs, resulting in high turnover, which is a common complaint among firms. Search frictions seem to

[51] Here I define low-skill employment as any job involving construction, casual labour, domestic work, or work for family labour. High-skill includes all full-time employment for government, NGOs or permanent positions in the private sector. Medium skill is the residual category. All calculations exclude agricultural work, which is uncommon even on the outskirts of cities, but could be affecting the results.

[52] See the right-hand panel of Figure 4, and Figures B9 and B10 in the Online Appendix.

be contributing significantly to the time it takes for workers to find a good match, and resulting in numerous short work spells throughout early careers.

My results suggest a role for social insurance to support the unemployed during job search. Labour markets could be made more efficient, as well as more accessible and equitable to a growing and aspirant urban population, by policies that reduce the costs of finding work. This could be done through improved and subsidised transport for the poor, including new light rail, bus rapid transit, and urban road upgrades. Online or mobile-phone based matching services or job search assistance programs could play a direct role in improving matching between workers and firms. The results underscore the importance of encouraging denser, affordable urban housing, so that poor job seekers can live closer to jobs and thus more readily access opportunity in growing cities.

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Material for Online Appendices:

Appendix A Theory

I develop a model which considers the consumption and job search decisions of an unemployed cash-constrained job seeker, who cannot borrow to search. I use this model to explain the job search behaviour and the trajectory of treatment effects observed in the data. The model demonstrates why job seekers don't search for work when the returns to search are very high, and they have enough cash on hand to pay the costs of search (at least once). I base the key assumptions of the model on observations from the data as well as qualitative insights from focus group discussions conducted during the baseline surveys. The model describes the experience of an individual who is cash constrained, and who receives income from taking spells of temporary work to smooth consumption and earn money to pay search costs. In each week, she pursues a permanent job by choosing to pay a fixed monetary search cost.[53] The model incorporates search-on-the-job since agents can search for work and have temporary employment at the same time.

The key intuition of the model is that job search is risky for a job-seeker with low levels of cash on hand. The value of a permanent job is high, but the probability of finding one is small (in the data only 19% of my control group had permanent employment after 15 weeks). So the high chance of remaining unemployed after paying a high search cost, and thus being left with low savings, deters investment in job-search.

After describing the main features of the model, I proceed as follows: Firstly, I provide stylised solutions to the stationary version of the model. The key prediction of the baseline model is that job seekers find it optimal not to search when they have savings below a critical level and that this critical value can be many orders of magnitude larger than the cost of search itself. Secondly, I simulate time series predictions of savings, consumption and search paths using the solved model. I can then predict dynamics of job search behaviour over time, to explain how agents at different initial states of job

[53]For the sake of simplicity, the model is framed in terms of the decisions of a job seeker from the *board* sample, in search of permanent work. The model could easily be applied to the *city* sample, in search of a higher quality job. In the model the two samples differ according to their initial savings levels and search behaviour. I return to this distinction later in this section.

search react to changes in search costs. Thirdly, having described the key predictions generated by the model, I summarise these predictions for a wide range of combinations of parameter choices. I look at the role of risk aversion and income regularity on altering job search behaviour, and to generate further predictions that are tested empirically in the paper. I show that these simulated dynamics are consistent with those found in the data.

The model is similar in its mechanisms to buffer savings models of investment (Deaton, 1991; Bryan *et al.*, 2014; Vereshchagina and Hopenhayn, 2009): in each period job seekers make the risky investment decision only when they have sufficient savings to do so because borrowing is not possible. However, the model differs in its dynamics: job seekers must continue to make the decision to search over and over again, the costs of searching may be low in each period, but so are the expected returns.[54]

A.1 The Model

The model is presented in discrete time. As is the case in the surveys and empirical analysis, the time period under consideration is one week. The job seeker begins each week with personal savings x, which could be cash on hand or formal savings in the bank. She starts the week by deciding whether she will search for a permanent job and pay the corresponding cost of search p. The decision to search is binary so that the cost of search is constant and corresponds to the costs of transport, photocopying documents, making applications and buying newspapers.[55] After the decision to search is made and the outcome is observed, the job seeker chooses that period's consumption to maximise future expected utility.

Searching for work at the start of the period leads to a permanent job with probability σ. If a permanent job is found, unemployment ends and the newly employed person receives income Y with certainty in the current period and all future periods. For tractability, I make the simplifying assumption that permanent employment is an absorbing state: these jobs cannot be lost.[56] The permanently

[54] In most of the buffer savings/investment models agents who are able to save up to make investments permanently transition to a new state: they become successful entrepreneurs or successful urban migrators. In my model someone could save up to search for work, then run down their savings again while still not finding a job, resulting in multiple investment cycles.

[55] The assumption of a binary search cost is motivated by the fact that, conditional on searching, days of search per week are relatively constant. Most job seekers make two trips to the centre to look for work, corresponding to key days when new information about vacancies is published. The majority of variation in search activity is between rather than within weeks.

[56] In reality, few respondents do in fact leave or permanent jobs. They come with secure contracts and are highly

employed worker solves a kind of cake eating problem, augmented with a permanent income stream, with next period's consumption discounted by the factor β. The worker chooses consumption c in each period:

$$V(x) = \max_{0 \leq c \leq x+Y} u(c) + \beta V(x - c + Y) \tag{A.1}$$

Someone who hasn't paid the cost of search cannot find a permanent job in that period. Individuals remain unemployed for the rest of the period if they have searched but failed to find work, or have not searched at all. Unemployed individuals can earn: with probability θ they earn a known income W, which is paid at the end of the period.[57] W could be interpreted as cash transfers from other family members but is more realistically thought of as income from temporary employment, earned for work done as a casual/temp labourer, for a family member, or in self-employment in the informal sector. Search is not required to get these temporary jobs.[58] The model can be adapted to model family income support directly, by adding a set amount to a job seekers finances each week. Similarly, the model assumes job seekers have cash on hand that does not earn interest. The model's predictions are not qualitatively changed by the inclusion of small returns to savings, or cash received from the family.

Let $U(x)$ be the value of being unemployed at the start of the period with savings x. An individual who chooses not to search for a job remains unemployed and chooses consumption c, solving:

$$F(x) = \max_{0 \leq c \leq x} u(c) + \beta(\theta U(x - c + W) + (1 - \theta)U(x - c)) \tag{A.2}$$

So $F(x)$ is the value of the decision not to search. A job seeker who has failed to find work faces the same problem, but has already spent p on search. Therefore $F(x - p)$ gives the value of having searched but failed to find work.

desirable. My high-frequency sample contains only 3 cases of individuals leaving jobs they considered to be permanent.

[57] This income is only realised in the next period, earned income increases the value of unemployment in the future, but cannot alleviate credit constraints in the current period if savings are already close to the zero-lower-bound.

[58] I take the arrival of these forms of temporary work to be random occurrences: the job seeker has no choice about whether to take the job (there is no leisure cost to taking temporary work) and the probability of getting this work is independent of the decision to search.

The optimal consumption decision, of course, differs between searchers and non-searchers.[59] Consumption when searching (c^s) is inevitably lower than consumption after not choosing to search c^{ns} because the searching individual has lower savings after paying the cost of search, and does not risk running down savings any further.

Using expressions for the value of permanent work and the value of failing to find permanent work, I write the value of searching for work at the beginning of the period:

$$S(x) = \sigma V(x - p) + (1 - \sigma)F(x - p) \tag{A.3}$$

Here, σ is the probability of finding permanent employment. I can now write an expression for the value of unemployment, which is given by the envelope of the value of searching and not searching, reflecting the job-seeker's decision to search at the beginning of each period.

$$U(x) = max\,\{S(x), F(x)\} \tag{A.4}$$

I am interested in when individuals choose to search for work. That is when $S(x) \geq F(x)$. The key insight of the model comes from Expression A.3. $F(x)$ is monotonically increasing and concave, and $S(x)$ is a convex combination of $F(x - p)$ and $V(x)$. Therefore, as is the standard result for models of this kind (Bryan *et al.*, 2014; Vereshchagina and Hopenhayn, 2009; Buera, 2009) $S(x)$ crosses $F(x)$ once from below.

I define x^* as the critical value for which $S(x^*) = F(x^*)$. This is the level of savings below which an individual gives up looking for work. At these levels search becomes prohibitively risky, job seekers trade off the benefits of search against the costs of reducing their buffer savings (Deaton, 1991). The result is further illuminated by rewriting the expression for this critical value as:

$$\sigma(V(x^* - p) - F(x^*)) = (1 - \sigma)(F(x^*) - F(x^* - p)) \tag{A.5}$$

The right hand side of this expression represents the expected benefit of searching and the left hand

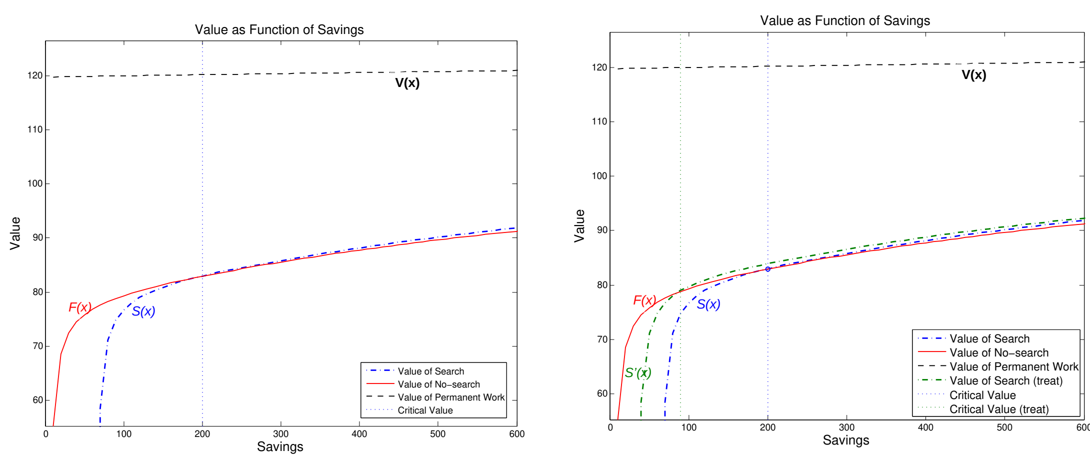
[59] In each case, the model is solved using an optimal control decision which solves the stochastic Euler equation equating the marginal utility of consumption in the current period with expected margin utility of consumption in the future period.

side the expected loss, relative to not searching. At low levels of savings $F(x) - F(x-p)$ becomes large and searching becomes very risky because marginal utility of future consumption is very high. The left Panel of Figure A1 illustrates this intuition. $S(x)$ falls quickly as savings get lower.

The model implies a steady state level of savings at x^*. Below this level individuals do not search but try to save up for search. Above it, they run down their savings by paying the costs of search. The model predicts that lowering the cost of search reduces the critical value x^*. Lowering search costs reduces the risk associated with searching. I define x^{*t} as the new critical value when the costs of search have been reduced. Lowering search costs also has implications for the dynamics of savings over time: job seekers run down their savings less slowly when costs are lower.

A.2 Solution to the Stationary Model

In this section, I solve the model numerically.[60] This happens in two steps. First I calculate the value of permanent employment, which is given simply by Expression A.1. Secondly I use that value of permanent work to estimate the value of searching, not searching, and unemployment, and solve for these and the corresponding consumption levels in each state. In these solutions I use a power utility function of the form $u(c) = \frac{c^{(}1-\delta)-1}{1-\delta}$, where δ is a measure of constant relative risk aversion. I use log-utility $u(c) = ln(c)$ for the case of $\delta = 1$.



Left panel: baseline model. Right panel: Effect of lowered search costs (treatment)
Figure A1: Single Crossing Point of the Value of Searching and not Searching

The left panel of Figure A1 shows the value for searching and not searching for different levels of

[60]The model is solved by iterating over the value function, using a grid for the state space (savings) and re-calculate the value function until it converges. This is done in Matlab.

savings, and the critical value above which it pays to search.[61] I verify that the model implies that $S(x)$ crosses $F(x)$ once from below, and I solve for the critical value x^* at which an unemployed individual is indifferent between searching and not searching. In this case it is $x = 200$. I calculate the impact of reducing the cost of search (usually by a half or a third) on x^*. I call this x^{*t}. See the right hand Panel of Figure A1: reducing the search costs for one period shifts the value of search to left, such that (in this example) the critical value below which respondents do not search falls from 200 to 90. In a very static prediction, everyone who is cash-constrained to begin with (cash below 200 but above 90) will start searching immediately, until they run down cash on hand to the new steady-state level.

A.3 Dynamic Simulations

The model can be used to simulate search, saving and consumption behaviour over time, given a starting distribution of savings. These predictions conform to the behaviour observed among the control groups (constrained and unconstrained) in the data. I then simulate the experiment by predicting the effect of changing the costs of search from 60 to 40 birr per week.

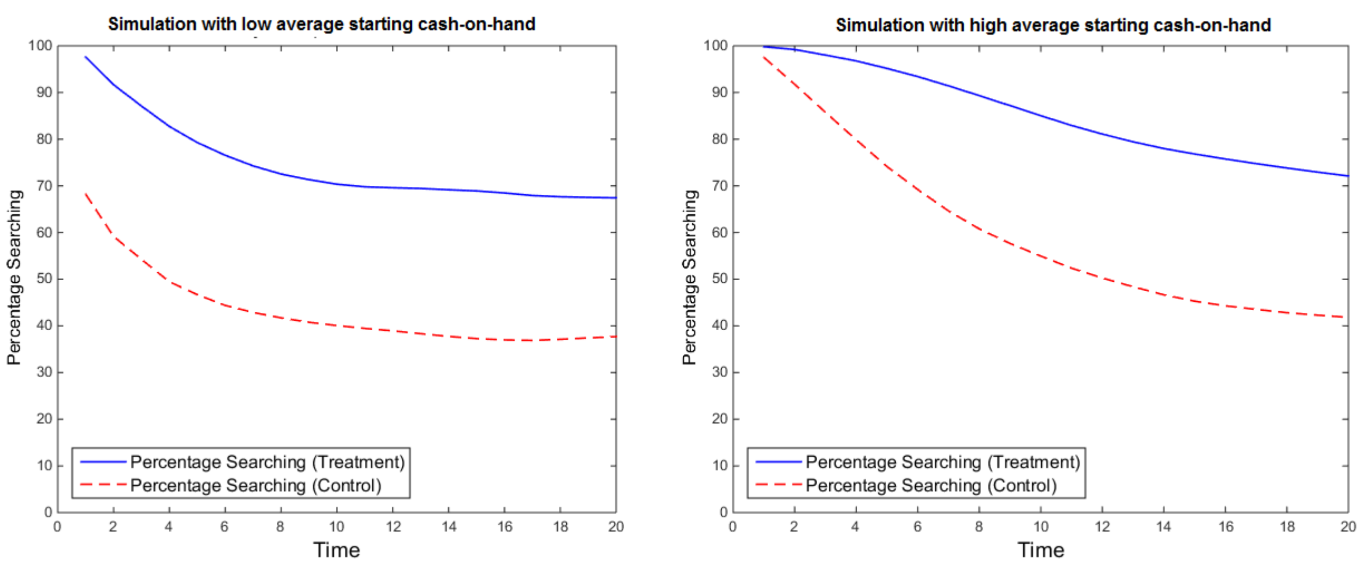


Figure A2: Percentage of the Sample Searching for a Job Over Time (Simulation)

I perform the simulations for two different types of populations, who differ only in terms of their initial savings: a group who are not cash constrained (starting with savings distributed well above x^*, and a

[61] Similarly I can determine optimal consumption paths, which are not presented here for the sake of brevity.

group who start out with binding cash constraints (savings distributed around x^*).[62] As predicted by the model, the first group start out searching in every period, but give up search once they run down their savings to x^*. The second group start out oscillating between searching and not searching every few weeks. In Figure A2 I present the results of simulations, showing the proportion of individuals searching for work over time, for 20 periods, both with and without reduced search costs.

Firstly, in the group simulated to start with binding cash constraints: when search costs are lowered treated respondents immediately begin to search more regularly. This is shown in Panel A of Figure A2. They spend more weeks searching, in steady-state, than they do with the higher search costs. By contrast, for simulations with higher initial cash on hand, most people continue to search actively, but treated individuals run down their savings more slowly. Initially there is little difference in trajectories for treated and untreated individuals. But as the control group ran out of cash, the treated respondents keep searching. Eventually both treated and untreated converge to steady state. Treated respondents are about 20% more likely to search in each period, in steady state, regardless of initial cash on hand. In this way, the effects of experimentally lowering transport costs are not just static. Dynamic effects come from job seekers running down their savings more slowly, as well as searching more regularly in steady-state because paying to search is less costly when search costs are lower.

Table A1: Key Parameters Values for Model Calibrations

Parameter	Description	Value
Y	Weekly wage for permanent wages	400 Birr
W	Weekly wage in temporary work	320 Birr
p	Weekly cost of searching actively work	60 Birr
p_t	Subsidies cost of searching for work	40 Birr
σ	Probability of finding permanent work if searching	0.03
θ	Probability of offer of temporary work if without permanent work	(0.1-0.5)
β	Discount rate	(0.8-0.99)
δ	CRRA for power utility function	(1-2.8)

[62] I use a log-utility function, and set $\theta = 0.3$, $\sigma = 0.03$, $\beta = 0.95$, and $p = 60$. The critical value at which someone gives up search, in the control group, is $x^* = 600$ for this calibration. I perform simulations of 1000 representative job-seekers, for each case. A simulate a random series of cash shocks due to spells of temporary employment.

Table A2: Risk, and Cash Flow While Unemployment: Solution for x^*

	δ **(Risk Aversion)**					
θ	1.2	1.4	1.8	2	2.4	2.8
0.1	350	450	650	900	1500	2050
0.2	300	400	550	800	1400	1850
0.4	300	350	500	750	1300	1700
0.6	300	350	450	800	1250	1600
0.8	300	350	450	750	1200	1550
1	250	300	450	750	1150	1600

Power utility with $p = 60$, $\beta = 0.98$, $\sigma = 0.03$

A.4 Calibrations

I calibrate the model for a range of parameter choices, in each case calculating the critical value x^* and the critical value under experimentally reduced search costs x^{*t}, as well as simulating job search over time I show that the predictions of the model are broadly consistent with the data for a wide range of parameter values (Table A1 shows the calibration choices). Table A3 shows the effect of increased risk aversion on the critical values. As expected, the critical value x^* is monotonically increasing in the risk aversion, job seekers are less likely to take the risk of searching. Similarly the steady state proportion of searchers falls with risk aversion: s^* shows the proportion of the sample that search in each period, in steady-state, in the benchmark case, while s^{*t} gives the proportion under experimentally reduced search costs.

The results show that the treatment effects on job search in the paper can be rationalised by the theoretical model of cash constraints. In all calibrations the critical value x^* is highly sensitive to the cost of search, even when people do have cash on hand, they give up search when the costs are high. High rates of risk aversion are not needed to rationalise my results: calibrations with relatively low risk aversion parameters are more consistent with the proportion of job seekers searching in each week, and the estimated treatment effects. In fact, in many calibrations the predicted treatment effects are considerably larger than the treatment effects found in the paper, even with low risk aversion parameters.[63]

Simulation of pure unemployment insurance: Table A2 looks at the effect of changing the

[63]This is perhaps because not all job seekers are constrained by search costs (about 40% of respondents did not take up the subsidies, after all); the model seeks to explain the behaviour for those marginal job seekers for whom cash constraints are binding.

probability of finding temporary work. The result is somewhat counter-intuitive. In this model, more regular income leads to *more* job search because that income reduces the risks associated with job search. Forward looking unemployed agents are more likely to pay the cost of job search if they know it is likely that they will still earn income in the future. In other models, without monetary search costs, this would be predicted to reduce incentives to find permanent work more quickly (Chetty, 2008). The critical value decreases with the value of θ (the probability of finding temporary work, or receiving other financial support), except for the cases in which θ is very close to 1, at which point job seekers become relatively indifferent between temporary work and permanent work, and face lower incentives to search.[64] This allows me to simulate the effect of pure unemployment insurance. That is I set $\theta = 1$, but income while unemployed (now interpreted as indemnity payments to cover search costs, rather than wages from temporary work) I set to be low, at $W = 60 = p$. The model predicts a huge increase in the proportion of individuals induced to search. This suggests an important role for unemployment insurance to prevent job seekers from taking less desirable forms of work purely as a way to mitigate against risk (Browning *et al.*, 2007).

Table A3: Risk aversion & Job search: Calibration & Simulated Treatment Effects
(Agent with power utility function or log-utility for $\delta = 1$. $\beta = 0.95$, $p = 60$, $p_t = 40$, $Y = 400$, $W = 320$, $\sigma = 0.03$)

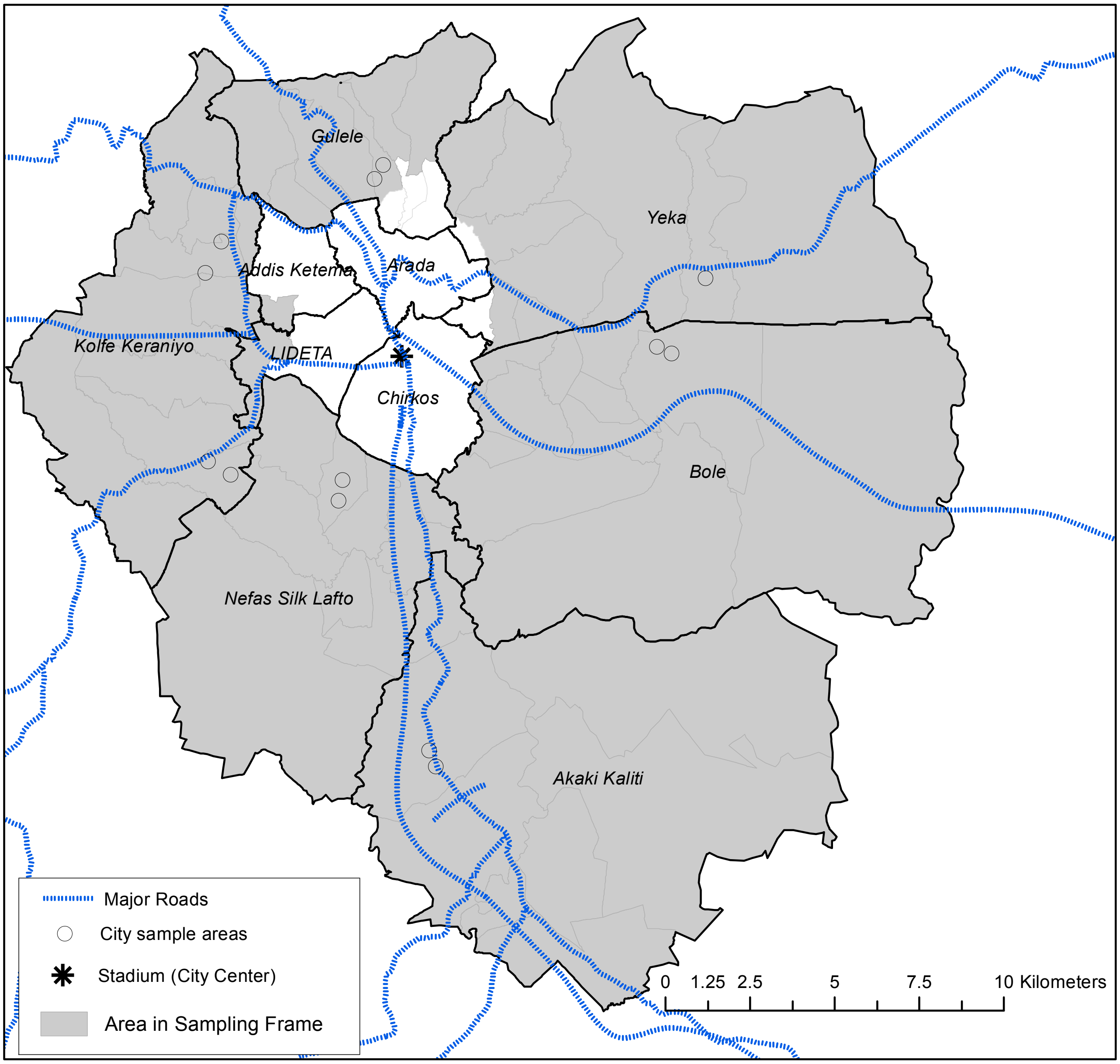
	(1)	(2)	(3)	(4)	(5)	(6)
	Statics			**Steady State**		
	x^*	x^{*t}	induced	s^*	s^{*t}	s^{*t} -s^*
δ			$\theta = 0.3$			
1	400	260	5.2%	53.1%	76.5%	23.3%
1.2	520	340	11.6%	44.4%	71.0%	26.6%
1.4	640	440	9.0%	38.2%	64.3%	26.2%
1.8	920	640	8.0%	23.9%	54.2%	30.3%
2	1060	740	10.4%	19.3%	48.5%	29.2%
2.4	1360	960	7.6%	10.9%	37.0%	26.2%
2.8	1640	1180	3.4%	6.7%	29.1%	22.3%

This result has important implications. It suggests that job seekers rely on temporary work for money to search for work. Temporary work insulates jobs seekers from risk while unemployed. The need to take temporary work seems symptomatic of cash constraints faced by job seekers. This theoretical finding conforms with the key empirical result of Section 5.2, which shows that individuals who receive transport subsidies are less likely to take temporary work while receiving the subsidies.

[64] When $Y = W$, $\theta = 1$, of course, no one searches for work for any calibration since permanent work is not preferred in any way.

Appendix B Charts and Images

Figure B1: Map of Addis Ababa with Sampling Frame and Selected EAs



Job vacancy boards are located close to "Stadium": the city center where the transport subsidy money could be collected.

Figure B2: Study Timeline and Randomisation Procedure

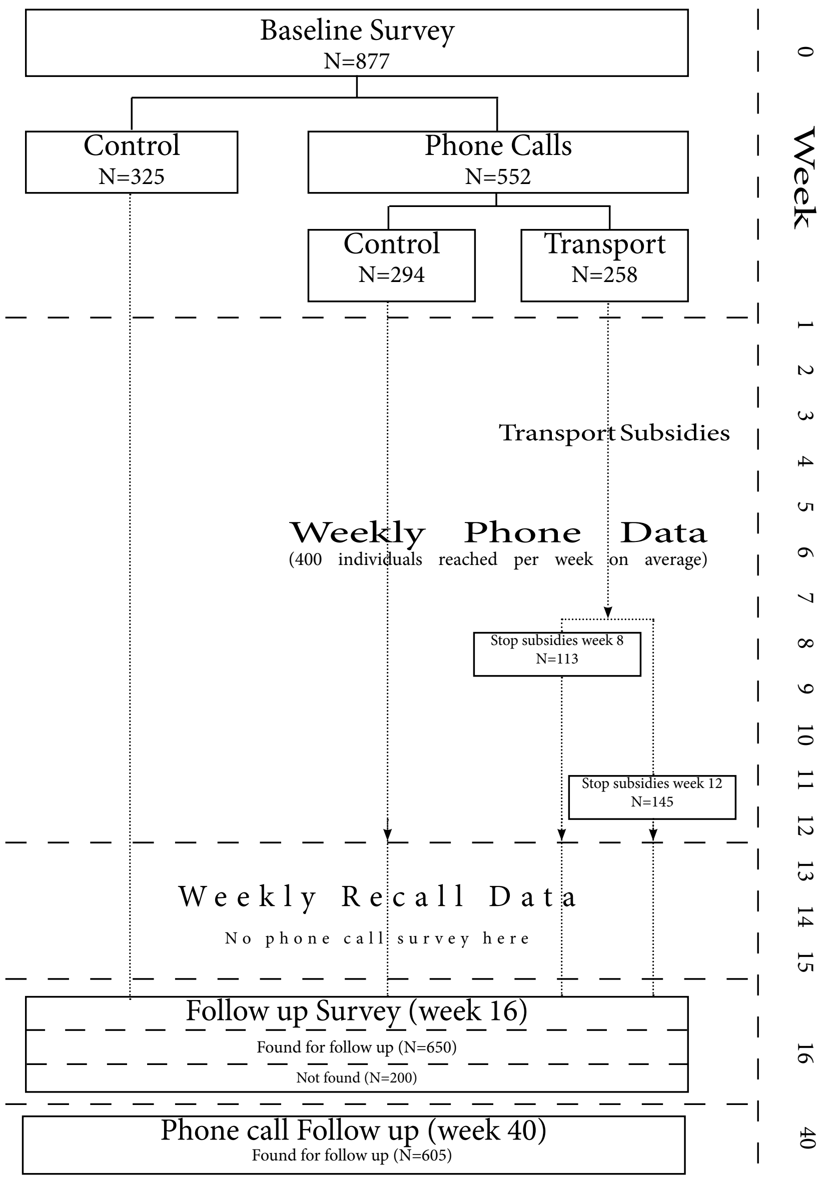


Figure B3: Take Up of the Subsidies by Week: Local Polynomial Regression

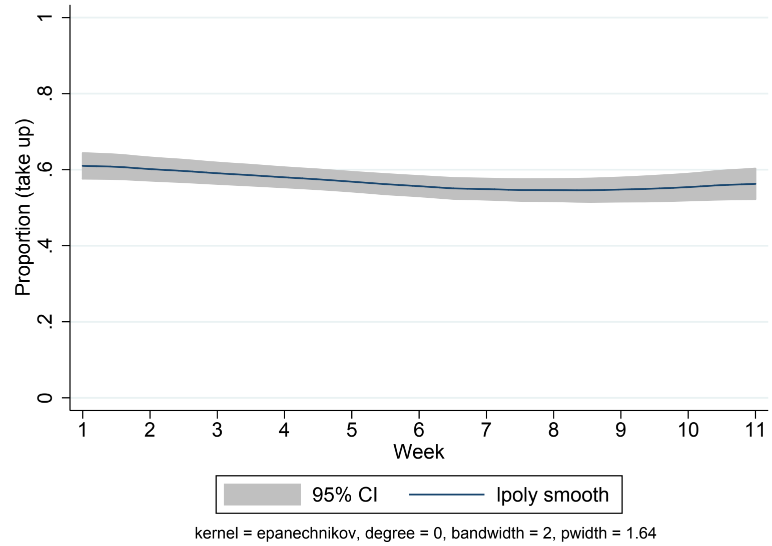


Figure B4: Impact of Treatment on Distribution of Occupations

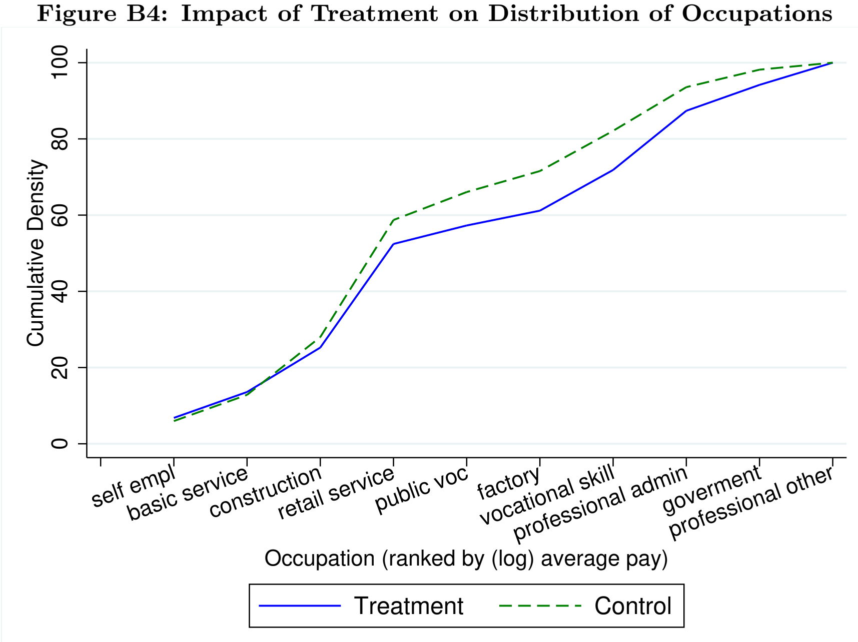


Figure B5: Job Search Trajectories among Poor and Not-poor Households

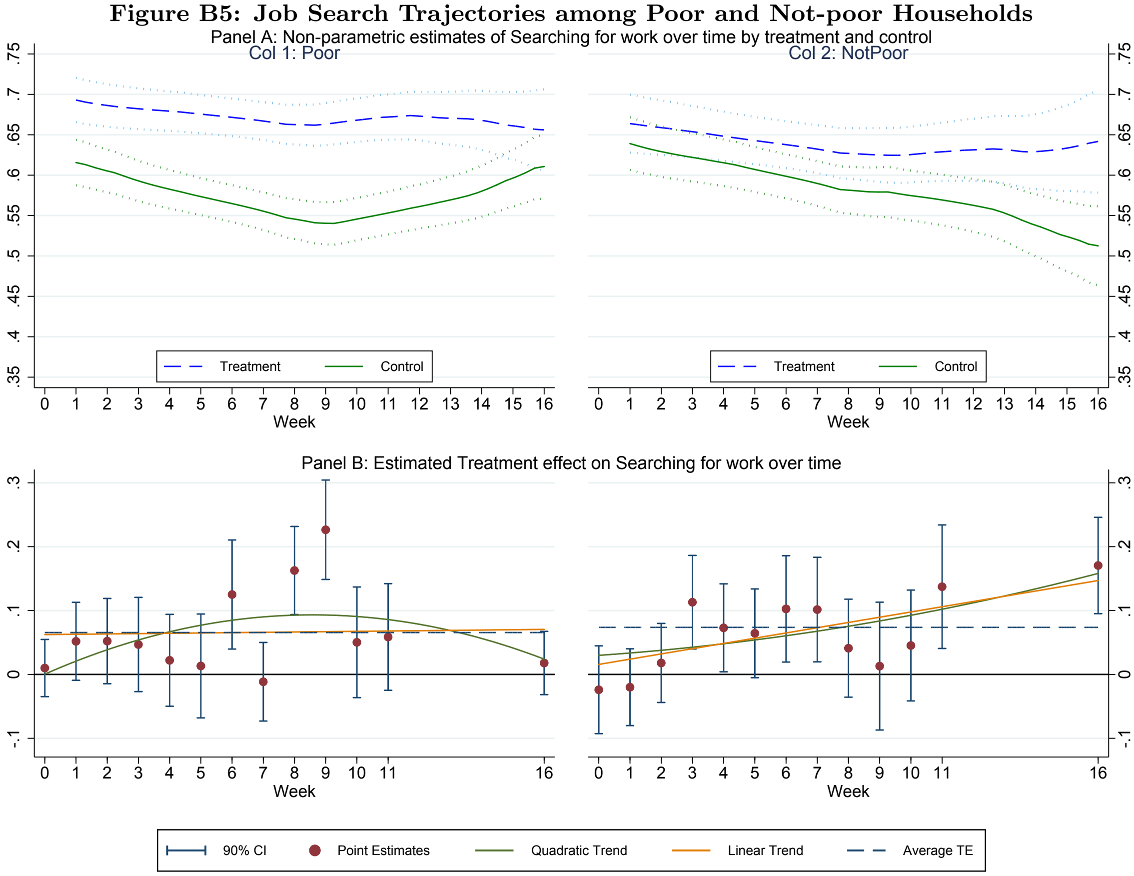


Figure B6: Impact on Employment: Trends & Treatment Effects Over Time

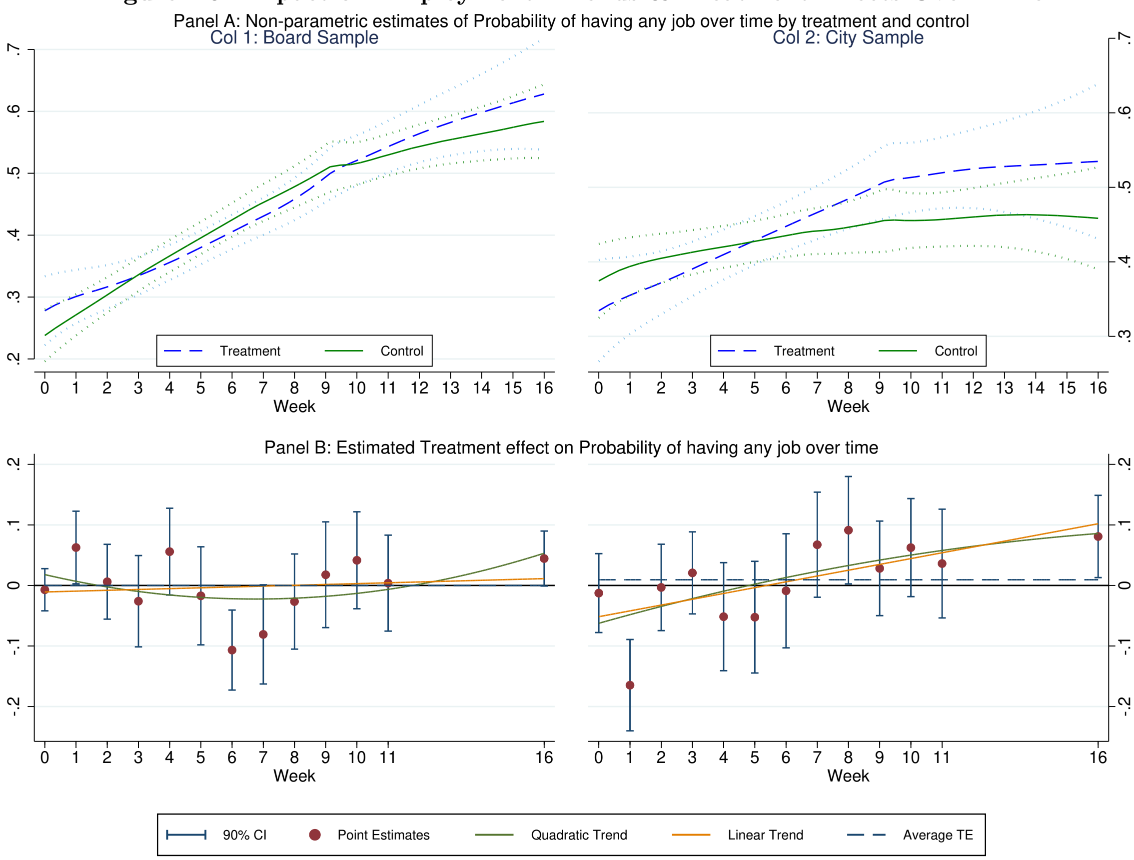


Figure B7: Impact on Search at Vacancy Boards: Trends & Treatment Effects Over Time

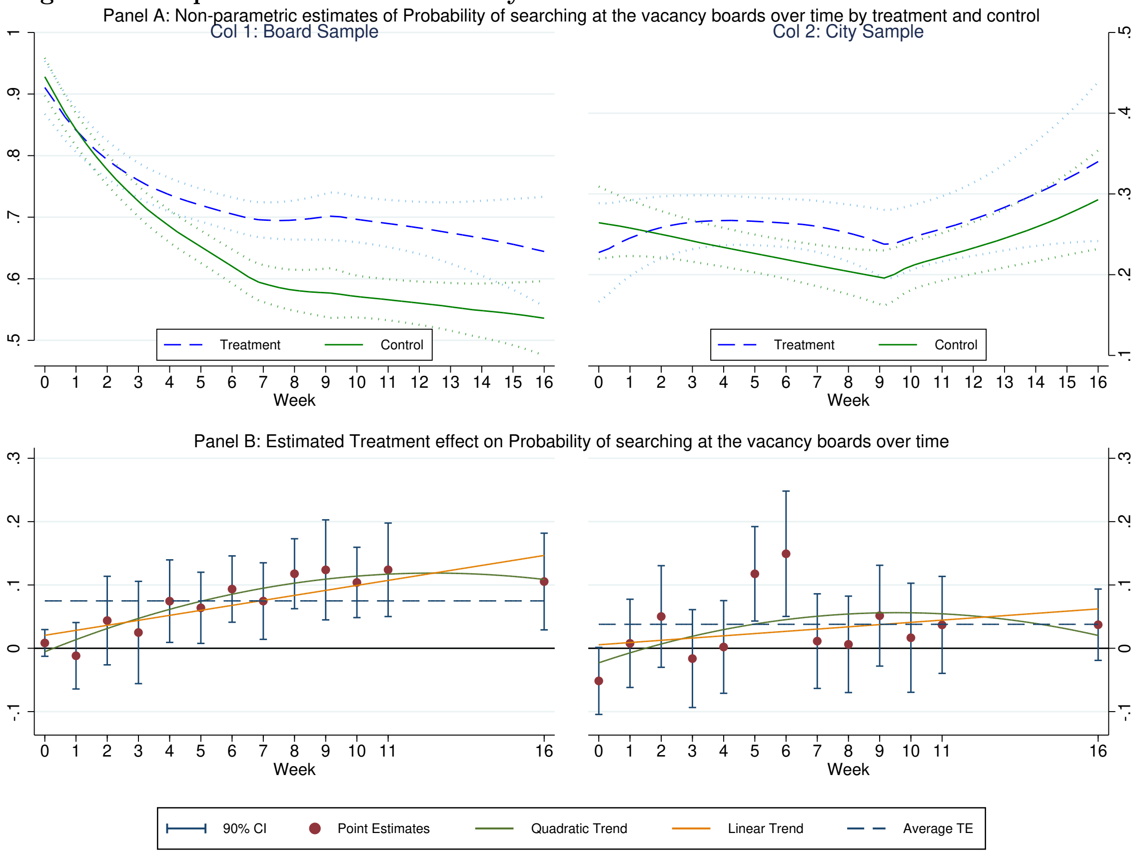


Figure B8: Impact on Travelling to the City Centre: Trends & Treatment Effects Over Time

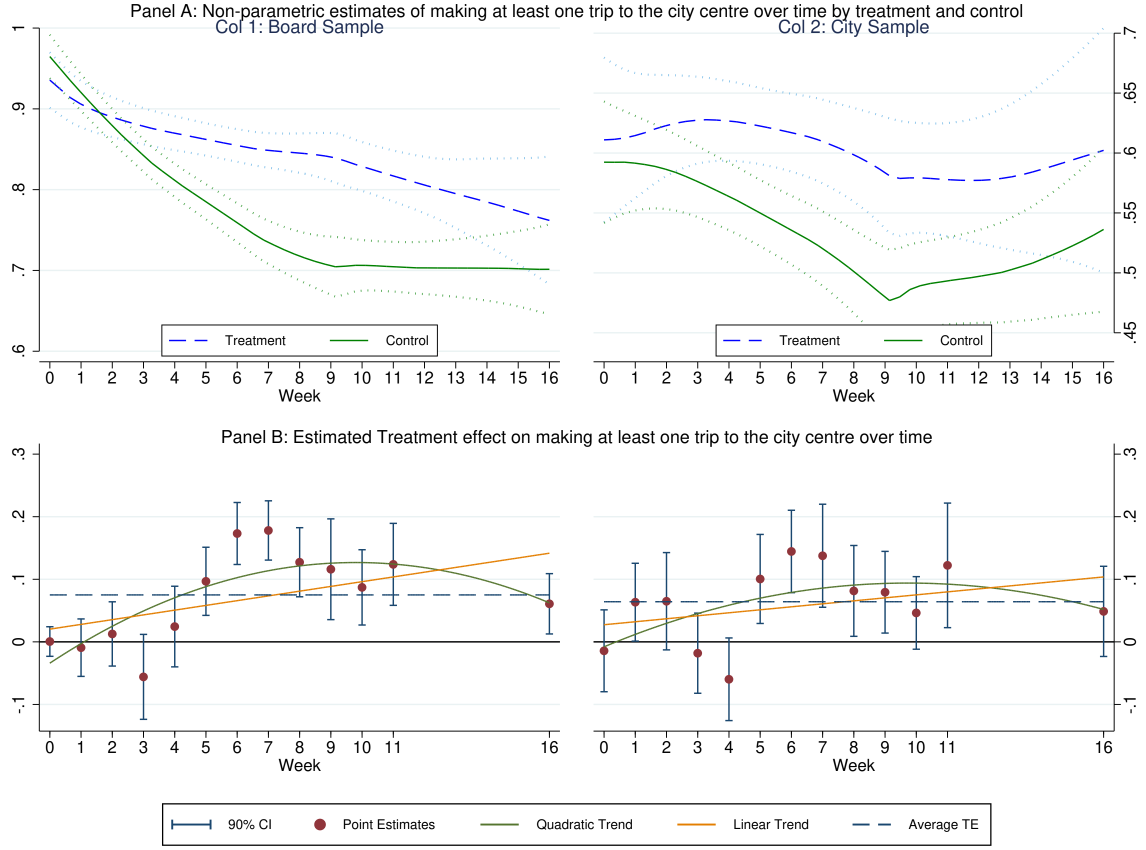


Figure B9: Job Quality by Distance from the Centre of Addis Youth Only (2013 LFS data)

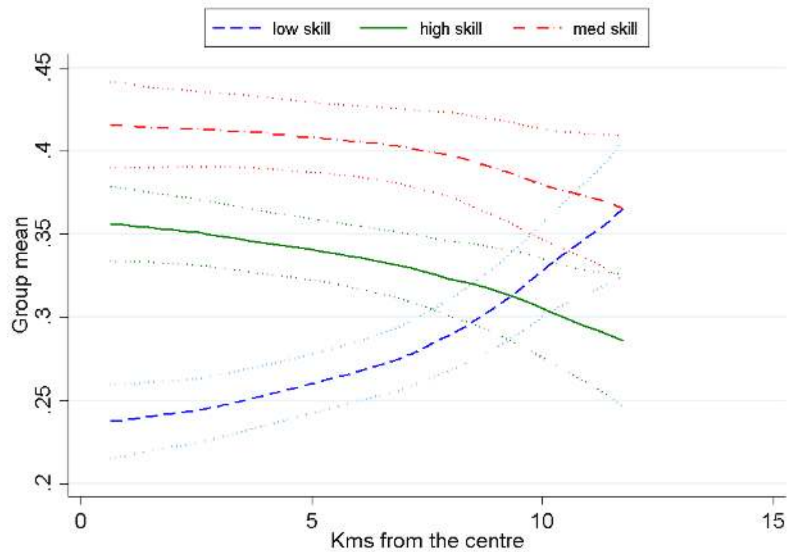
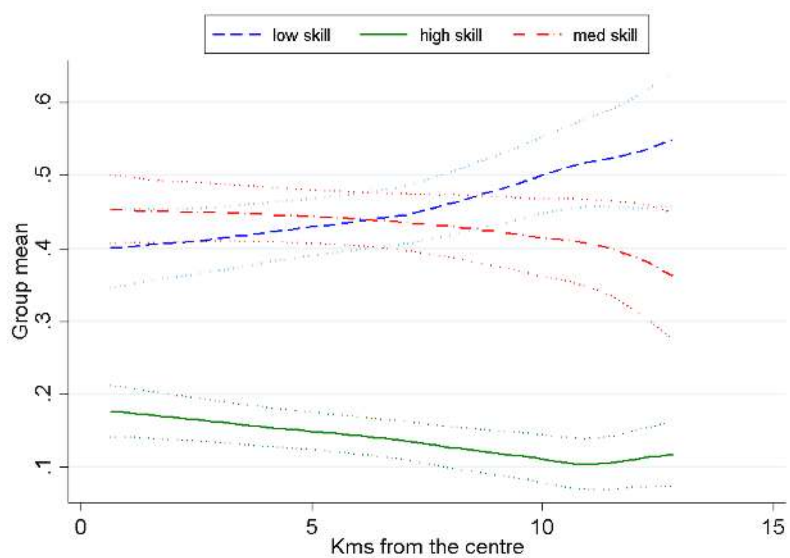


Figure B10: Job Quality by Distance from the Centre of Addis without Highschool Youth (2013 LFS data)



Appendix C Additional Tables

Table C1: Tests for Balance

Panel A: Entire Sample at Baseline

	Full Sample			**Boards Sample**			**City Sample**		
	treat	cont	p-val	treat	cont	p-val	treat	cont	p-val
Sample	.539	.54	.982	1	1		0	0	
Work	.256	.258	.934	.201	.201	.983	.319	.326	.892
Permanent Work	.0039	.0065	.643	0	0		.0084	.014	.642
Searching	.829	.829	.98	.971	.973	.912	.664	.66	.935
Visisted Boards	.624	.628	.902	.964	.958	.765	.227	.242	.744
Discouraged	.12	.129	.713	.0216	.018	.794	.235	.26	.609
Hours Worked	7.38	6.06	.197	6.89	5.15	.207	7.95	7.13	.588
Construction	.0891	.0905	.95	.0935	.0749	.497	.084	.109	.454
Female	.217	.223	.848	.129	.132	.948	.319	.33	.838
Diploma	.205	.183	.431	.302	.287	.749	.0924	.0596	.238
Degree	.236	.242	.853	.432	.44	.866	.0084	.0105	.845
Finish Gr 10	.783	.788	.858	.928	.955	.232	.613	.593	.703
Age	23.7	23.4	.162	23.9	23.6	.27	23.5	23.2	.371
Household Size	3.52	3.48	.8	2.76	2.89	.414	4.41	4.18	.321
Head of HH	.225	.223	.952	.302	.263	.392	.134	.175	.311
Amhara	.453	.496	.252	.446	.494	.343	.462	.498	.51
Oromo	.318	.3	.612	.388	.356	.509	.235	.235	.996
Orthodox	.705	.721	.652	.712	.698	.752	.697	.747	.303
Muslim	.101	.113	.595	.0432	.0719	.244	.168	.161	.869
Lives with Family	.256	.268	.706	.367	.383	.739	.126	.133	.844
Born out of Addis	.612	.612	.997	.813	.814	.971	.378	.375	.959
Recent Grad	.345	.401	.123	.468	.551	.0989	.202	.225	.613
Work Experience	.523	.499	.517	.417	.389	.571	.647	.628	.719
Weeks w/o Work	37.6	40.4	.409	37.3	34.4	.43	38	47.4	.1
HH Wealth index	-.0149	.0143	.695	-.112	-.0166	.382	.0985	.0506	.628
Own Room	.229	.223	.853	.23	.201	.472	.227	.249	.636
Kms from center	6.15	6.33	.467	6.4	6.86	.282	5.85	5.71	.481
Weekly expenditure	179	152	.0352	202	174	.115	152	128	.152
Money from fam	84.9	75.1	.395	113	105	.657	52	39.6	.371
Reservation Wage	1225	1282	.355	1326	1398	.379	1106	1146	.668
Observations	258	619		139	334		119	285	

Table C2: Tests for Balance (cont): Balance after Attrition

Panel B: Sample resurveyed at Follow Up

	Full Sample			*Boards Sample*			*City Sample*		
	treat	cont	p-val	treat	cont	p-val	treat	cont	p-val
Sample	.556	.563	.859	1	1		0	0	
Work	.242	.263	.579	.182	.205	.616	.318	.338	.739
Permanent Work	.0051	.0065	.824	0	0		.0114	.0149	.812
Searching	.828	.826	.946	.964	.969	.787	.659	.642	.778
Visisted Boards	.631	.65	.647	.955	.954	.971	.227	.259	.571
Discouraged	.116	.126	.723	.0273	.0193	.632	.227	.264	.514
Hours Worked	6.84	6.45	.741	6	5.45	.724	7.9	7.74	.93
Construction	.0859	.087	.963	.0818	.0656	.58	.0909	.114	.554
Female	.207	.224	.632	.127	.135	.839	.307	.338	.601
Diploma	.202	.185	.606	.282	.278	.94	.102	.0647	.269
Degree	.247	.252	.899	.436	.44	.947	.0114	.01	.914
Finish Gr 10	.818	.807	.727	.927	.961	.165	.682	.607	.227
Age	23.8	23.6	.301	23.8	23.7	.653	23.9	23.4	.326
Household Size	3.45	3.43	.869	2.68	2.88	.275	4.42	4.12	.299
Head of HH	.258	.25	.838	.336	.282	.296	.159	.209	.325
Amhara	.449	.509	.164	.445	.498	.356	.455	.522	.29
Oromo	.348	.302	.242	.409	.34	.206	.273	.254	.736
Orthodox	.717	.737	.6	.709	.71	.979	.727	.771	.425
Muslim	.0859	.102	.518	.0455	.0734	.321	.136	.139	.947
Lives with Family	.242	.261	.619	.345	.363	.749	.114	.129	.711
Born out of Addis	.616	.622	.893	.791	.803	.79	.398	.388	.877
Recent Grad	.328	.389	.139	.455	.541	.131	.17	.194	.637
Work Experience	.495	.511	.708	.391	.409	.743	.625	.642	.786
Weeks w/o Work	39	40	.788	37.7	35.3	.564	40.6	46.1	.417
HH Wealth index	-.0276	.0254	.547	-.171	.0025	.165	.152	.0549	.422
Own Room	.247	.224	.511	.264	.208	.247	.227	.244	.763
Kms from center	5.98	6.45	.106	6.09	6.94	.0709	5.85	5.8	.852
Weekly expenditure	183	155	.0422	206	166	.0327	156	140	.476
Money from fam	96.2	77.9	.197	123	107	.42	62.7	40.8	.236
Reservation Wage	1227	1288	.379	1323	1400	.434	1108	1145	.693
Observations	198	460		110	259		88	201	

Panel C: Sample Recontacted (at least once) in the Phone Surveys

	Full Sample			*Boards Sample*			*City Sample*		
	treat	cont	p-val	treat	cont	p-val	treat	cont	p-val
Sample	.557	.558	.982	1	1		0	0	
Work	.245	.264	.57	.197	.219	.62	.305	.322	.751
Permanent Work	.0042	.0062	.737	0	0		.0095	.014	.736
Searching	.823	.839	.587	.97	.967	.872	.638	.678	.484
Visisted Boards	.629	.655	.489	.962	.952	.641	.21	.28	.175
Discouraged	.122	.122	.986	.0227	.0222	.974	.248	.248	.999
Hours Worked	7.15	6.25	.407	6.62	5.32	.359	7.82	7.42	.812
Construction	.097	.093	.861	.0985	.0778	.485	.0952	.112	.647
Female	.232	.227	.886	.136	.137	.985	.352	.341	.843
Diploma	.207	.186	.507	.295	.289	.892	.0952	.0561	.196
Degree	.253	.246	.832	.447	.433	.796	.0095	.0093	.988
Finish Gr 10	.793	.795	.945	.932	.963	.168	.619	.584	.552
Age	23.7	23.4	.328	23.8	23.6	.397	23.5	23.3	.58
Household Size	3.51	3.49	.864	2.79	2.92	.45	4.43	4.21	.388
Head of HH	.224	.223	.988	.303	.256	.316	.124	.182	.185
Amhara	.456	.492	.364	.447	.504	.286	.467	.477	.867
Oromo	.333	.308	.49	.402	.356	.372	.248	.248	.999
Orthodox	.705	.725	.565	.705	.711	.892	.705	.743	.471
Muslim	.105	.114	.744	.0455	.0704	.333	.181	.168	.778
Lives with Family	.262	.273	.752	.364	.381	.729	.133	.136	.957
Born out of Addis	.62	.612	.822	.818	.811	.865	.371	.36	.84
Recent Grad	.359	.403	.253	.477	.556	.14	.21	.21	.988
Work Experience	.506	.506	.997	.409	.396	.806	.629	.645	.777
Weeks w/o Work	37.3	40.6	.349	37.7	33.9	.328	36.8	49	.0489
HH Wealth index	-.0057	.0321	.643	-.114	-.0028	.336	.131	.0761	.623
Own Room	.224	.211	.693	.227	.2	.529	.219	.224	.916
Kms from center	6.17	6.39	.41	6.38	6.93	.22	5.91	5.72	.383
Weekly expenditure	177	149	.0397	202	169	.0929	146	123	.222
Money from fam	90.4	74	.18	117	102	.39	56.5	38.7	.235
Reservation Wage	1207	1252	.448	1321	1370	.544	1064	1104	.64
Observations	237	484		132	270		105	214	

Table C3: Attrition: Determinants of Response at Endline (Week 16)

	Full Sample			Board Sample	City Sample
	(1)	(2)	(3)	(4)	(5)
trans board	0.016	-0.0058	-0.012	-0.013	
	(0.044)	(0.052)	(0.052)	(0.050)	
trans city	0.034	0.0044	0.0025		-0.00075
	(0.045)	(0.041)	(0.038)		(0.037)
call board		0.038	0.042	0.045	
		(0.047)	(0.047)	(0.048)	
call city		0.063	0.078		0.083
		(0.058)	(0.055)		(0.056)
sample board	0.070	0.088	0.072		
	(0.042)	(0.068)	(0.073)		
Grade 0-9			-0.083	-0.083	-0.11
			(0.055)	(0.098)	(0.13)
Secondary			0.0045	0.043	-0.037
			(0.048)	(0.052)	(0.14)
Vocational			0.11*	0.033	0.12
			(0.064)	(0.11)	(0.13)
Diploma			-0.036	-0.044	-0.049
			(0.047)	(0.051)	(0.18)
household wealth i			0.015	0.0022	0.033
			(0.015)	(0.020)	(0.027)
hhsize			-0.0020	-0.0074	0.0024
			(0.011)	(0.015)	(0.014)
female			0.026	0.0048	0.038
			(0.037)	(0.058)	(0.047)
headofhh			0.12***	0.082	0.20**
			(0.046)	(0.058)	(0.077)
living relatives			-0.014	-0.017	0.0098
			(0.039)	(0.046)	(0.091)
amhara			-0.0044	-0.017	-0.0012
			(0.034)	(0.053)	(0.050)
orthodox			0.052	0.035	0.062
			(0.036)	(0.047)	(0.062)
birth migrant			0.013	-0.081	0.061
			(0.042)	(0.063)	(0.056)
age			0.0097*	0.0046	0.014*
			(0.0055)	(0.0091)	(0.0076)
experience			-0.012	0.0044	-0.025
			(0.034)	(0.042)	(0.050)
work			-0.0019	-0.025	0.015
			(0.034)	(0.044)	(0.051)
work perm			0.075		0.076
			(0.18)		(0.19)
married			0.019	-0.056	0.046
			(0.040)	(0.067)	(0.053)
Constant	0.71***	0.67***	0.40**	0.72***	0.26
	(0.034)	(0.060)	(0.16)	(0.23)	(0.21)
Observations	877	877	877	473	404
R-squared	0.006	0.009	0.045	0.028	0.076
F-test	0.36	0.53	1.41	0.85	5.76
P-value	0.70	0.71	0.15	0.64	0.0017

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Dep Var is = 1 if respondent interviewed at baseline respondended at endline.

Table C4: Attrition: Determinants of Response at Second Endline (Week 40)

	Full Sample			Board Sample	City Sample
	(1)	(2)	(3)	(4)	(5)
trans board	0.018	-0.014	-0.012	-0.0091	
	(0.039)	(0.046)	(0.047)	(0.049)	
trans city	0.063	0.012	0.0085		0.017
	(0.075)	(0.085)	(0.083)		(0.087)
call board		0.055	0.054	0.055	
		(0.053)	(0.052)	(0.051)	
call city		0.11	0.12		0.12
		(0.075)	(0.075)		(0.075)
sample board	0.11***	0.15**	0.19***		
	(0.037)	(0.059)	(0.067)		
Grade 0-9			-0.040	-0.18**	-0.13
			(0.065)	(0.087)	(0.22)
Secondary			-0.031	-0.019	-0.15
			(0.061)	(0.079)	(0.22)
Vocational			0.0082	0.015	-0.076
			(0.073)	(0.11)	(0.24)
Diploma			-0.064	-0.049	-0.23
			(0.051)	(0.055)	(0.23)
household wealth i			0.00091	-0.012	0.043
			(0.018)	(0.022)	(0.037)
hhsize			0.013	0.013	0.0082
			(0.0098)	(0.020)	(0.011)
female			0.043	-0.082	0.11*
			(0.045)	(0.081)	(0.060)
headofhh			0.031	-0.046	0.15
			(0.056)	(0.069)	(0.092)
living relatives			-0.039	-0.061	0.011
			(0.044)	(0.057)	(0.076)
amhara			0.0066	0.0048	-0.0076
			(0.030)	(0.041)	(0.050)
orthodox			0.015	0.046	-0.020
			(0.036)	(0.047)	(0.059)
birth migrant			-0.0088	-0.074	0.0065
			(0.048)	(0.066)	(0.072)
age			0.00031	0.0012	0.0037
			(0.0051)	(0.0084)	(0.0062)
experience			0.010	0.029	-0.023
			(0.032)	(0.050)	(0.047)
work			0.024	0.065	-0.0052
			(0.040)	(0.048)	(0.062)
work perm			-0.047		-0.037
			(0.16)		(0.18)
married			0.020	0.0097	-0.0096
			(0.048)	(0.070)	(0.070)
Constant	0.62***	0.56***	0.48***	0.72***	0.54*
	(0.030)	(0.050)	(0.16)	(0.22)	(0.30)
Observations	877	877	877	473	404
R-squared	0.013	0.019	0.029	0.035	0.041
F-test	0.46	1.12	1.15	1.99	4.90
P-value	0.63	0.36	0.32	0.027	0.0037

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Dep Var is = 1 if respondent interviewed at baseline respondended at endline.

Table C5: Alternative Specifications: Impact on Permanent Work (week 16)

	Control Mean	(1) BAS	(2) LOG	(3) COV	(4) ANC	(5) BLK	(6) FD
Panel A: Average Treatment Effects At Follow Up (Pooled Sample)							
All	0.13	0.028	0.027	0.042	0.043	0.032	0.044*
		(0.027)	(0.024)	(0.026)	(0.026)	(0.026)	(0.026)
Observations	657	657	657	657	657	657	657
R^2		0.001		0.088	0.098	0.151	0.097
Panel B: Treatment Effects At Follow Up by Sample							
Board	0.19	0.068*	0.046*	0.078**	0.078**	0.073*	0.078**
		(0.038)	(0.028)	(0.037)	(0.037)	(0.040)	(0.037)
City	0.06	-0.019	-0.036	-0.004	-0.002	-0.020	0.001
		(0.032)	(0.054)	(0.034)	(0.032)	(0.026)	(0.033)
Observations	657	657	657	657	657	657	657
R^2		0.186		0.221	0.230	0.276	0.218

[1] The dependent variable is a dummy variable equal to one if the individual reported having a permanent job, measured at endline (week 16). Results are from OLS regressions on endline outcomes.
[2] Panel A gives average ITT effect for the two samples together. Panel B shows results two different samples- “board” and “city”
[3] Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level
[4] Specifications: BAS- a version without the covariates or lagged dependent variable (Equation C.2 below). LOG- A logistic regression version of Equation C.2. COV- Baseline covariates, equation C.1. BLK- replaces covariates with a set of blocking dummies on which assignment to treatment was based. FD- first-difference estimator (Equation C.3).

$$Y_{ic} = \alpha + T_i\lambda + X_{i0}\beta + \epsilon_{ic} \tag{C.1}$$

$$Y_{ic} = \alpha + T_i\lambda + \epsilon_{ic} \tag{C.2}$$

$$Y_{i16c} - y_{i0c} = \alpha + T_i\lambda + X_{i0}\beta + \epsilon_{ic} \tag{C.3}$$

Table C6: Alternative Specifications: Impact on Employment (week 16)

	Control Mean	(1) BAS	(2) LOG	(3) COV	(4) ANC	(5) BLK	(6) FD
Panel A: Average Treatment Effects At Follow Up (Pooled Sample)							
All	0.53	0.058*	0.059*	0.062*	0.064*	0.057*	0.081*
		(0.034)	(0.035)	(0.035)	(0.034)	(0.034)	(0.043)
Observations	658	658	658	658	658	658	658
R^2		0.003		0.066	0.078	0.159	0.062
Panel B: Treatment Effects At Follow Up by Sample							
Board	0.58	0.044	0.046	0.043	0.046	0.049	0.067
		(0.051)	(0.052)	(0.052)	(0.051)	(0.051)	(0.062)
City	0.46	0.076	0.075*	0.086*	0.088**	0.068	0.099*
		(0.046)	(0.044)	(0.044)	(0.041)	(0.041)	(0.057)
Observations	658	658	658	658	658	658	658
R^2		0.553		0.066	0.079	0.159	0.062

[1] The dependent variable is a dummy variable equal to one if the individual reported having done work in the last 7 days, measured at endline (week 16).
[2] Panel A gives average effect for the two samples together. Panel B shows results two different samples- “board” and “city”.
[3] Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level.
[4] Specifications are outlined in Table C5 in Equations C.2, C.2 and C.1.

Table C7: Intrumental Variables (TOT) Treatment Effects

Outcome	Any Work		Permanent Work		Weeks Search	
	(1)	(2)	(3)	(4)	(5)	(6)
Instrumented Variable	takeup	Centre trip	takeup	Centre trip	takeup	Centre trip
Panel A: Instrumental Variable Results for Board Sample						
Treatment on treated	0.086	0.043	0.14**	0.083**	2.20**	0.45
	(0.093)	(0.048)	(0.065)	(0.035)	(0.94)	(0.29)
Observations	369	224	368	223	369	224
R^2	0.078	-0.031	0.060	-0.348	0.089	0.349
First stage beta	0.564***	0.881*	0.564***	0.881*	0.564***	0.881*
First stage SE	0.031	0.476	0.031	0.476	0.031	0.476
Kleibergen-Paap Wald F	150	15.3	155	16.9	152	16.5
Panel B: Instrumental Variable Results for City Sample						
Treatment on treated	0.15*	0.074	-0.014	-0.030	1.82**	0.92*
	(0.080)	(0.058)	(0.055)	(0.034)	(0.75)	(0.51)
Observations	289	173	289	173	178	173
R^2	0.090	-0.269	0.103	-0.015	0.127	-0.132
First stage beta	0.568***	1.379*	0.568***	1.379*	0.568***	1.379*
First stage SE	0.035	0.707	0.035	0.707	0.035	0.707
Kleibergen-Paap Wald F	127	3.27	129	3.30	167	3.22

[1] All results show instrumental variable (2SLS) estimates where random assignment to the treatment group is used as instrument for the dependent variable. The instrumented variable is labelled in the column header.

[2] Outcome 'Weeks Search' indicates t he number of weeks that a respondent searched for work from the time of the start of the subsidies until the endline survey. *City* respondents searched on average 7 out of the 14 weeks, while the *Board* sample search for 9.

[2] This tables uses treatment status as an instrument for 'takeup' (whether the respondent used the transport subsidies) and 'Centre trips' (number of trips to the city centre where the money could be collected) in Columns 1/3 and Columns 2/4, respectively.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C8: Monthly Treatment Effects on Main Labour Outcomes

	(1) work	(2) work perm	(3) searchnow	(4) searchboards	(5) discouraged	(6) days search
Panel A: Average Impacts By Month						
month 1	-0.018	-0.017	0.040*	0.033	-0.012	0.19*
	(0.024)	(0.012)	(0.023)	(0.023)	(0.018)	(0.097)
month 2	-0.024	-0.009	0.070***	0.084***	-0.034*	0.110
	(0.024)	(0.012)	(0.024)	(0.023)	(0.018)	(0.098)
month 3	0.038	0.012	0.083***	0.081***	-0.064***	0.28***
	(0.023)	(0.012)	(0.023)	(0.023)	(0.018)	(0.096)
R-squared	0.487	0.141	0.648	0.545	0.231	0.471
Observations	5,011	5,010	5,011	5,011	5,011	4,949
Panel B: Average Impacts By Month and Sample						
board month 1	0.018	-0.009	0.011	0.033	0.007	0.170
	(0.033)	(0.016)	(0.032)	(0.031)	(0.025)	(0.13)
board month 2	-0.067**	-0.013	0.074**	0.090***	-0.024	0.130
	(0.033)	(0.016)	(0.032)	(0.031)	(0.025)	(0.13)
board month 3	0.021	0.028*	0.11***	0.11***	-0.054**	0.47***
	(0.031)	(0.016)	(0.031)	(0.029)	(0.024)	(0.13)
city month 1	-0.048	-0.021	0.069**	0.009	-0.039	0.200
	(0.036)	(0.018)	(0.035)	(0.033)	(0.027)	(0.14)
city month 2	0.028	-0.004	0.058*	0.070**	-0.043	0.055
	(0.036)	(0.018)	(0.035)	(0.034)	(0.027)	(0.14)
city month 3	0.058*	-0.014	0.043	0.033	-0.072***	0.036
	(0.035)	(0.017)	(0.034)	(0.033)	(0.027)	(0.14)
R-squared	0.492	0.159	0.651	0.572	0.235	0.478
Observations	5,011	5,010	5,011	5,011	5,011	4,949

[1] Dependent Variables are listed at the top of each column. Results are from POST-OLS regressions on endline outcomes,
[2] Analysis excludes the follow up survey, just restricting analysis to the sample contacted in the phone surveys, with Month 1 defined as weeks 1-4, Month 2 as weeks 5-8 and Month 3 as weeks 9-12.
[3] Panel A gives average ITT effects across the full sample. Panel B estimates different coefficients for the two subsamples.
[4] Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa)
$*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C9: Iterative 4 Week Average Treatment Effects on Main Labour Outcomes

	(1) work	(2) work perm	(3) searchnow	(4) searchboards	(5) discouraged	(6) days search	N
weeks 0-3	-0.019	-0.015	0.037	0.021	-0.011	0.22**	1227
	(0.033)	(0.016)	(0.026)	(0.033)	(0.023)	(0.098)	
weeks 1-4	-0.0025	-0.013	0.045	0.036	-0.037	0.20*	1191
	(0.035)	(0.016)	(0.028)	(0.035)	(0.025)	(0.10)	
weeks 2-5	-0.016	-0.015	0.044	0.050	-0.037	0.17	1186
	(0.040)	(0.017)	(0.029)	(0.034)	(0.029)	(0.11)	
weeks 3-6	-0.034	-0.010	0.058*	0.089**	-0.023	0.22*	1175
	(0.041)	(0.018)	(0.034)	(0.038)	(0.034)	(0.13)	
weeks 4-7	-0.038	-0.011	0.050	0.088**	-0.012	0.075	1194
	(0.040)	(0.019)	(0.034)	(0.037)	(0.032)	(0.10)	
weeks 5-8	-0.019	-0.0070	0.084***	0.086**	-0.039	0.12	1208
	(0.040)	(0.021)	(0.030)	(0.038)	(0.028)	(0.13)	
weeks 6-9	0.0060	-0.0072	0.095***	0.077**	-0.052*	0.080	1194
	(0.041)	(0.023)	(0.029)	(0.037)	(0.029)	(0.17)	
weeks 7-10	0.027	-0.0047	0.10***	0.085**	-0.075***	0.32**	1161
	(0.040)	(0.024)	(0.031)	(0.036)	(0.027)	(0.13)	
weeks 8-11	0.025	-0.0064	0.090**	0.088**	-0.071***	0.37**	1141
	(0.042)	(0.027)	(0.042)	(0.038)	(0.026)	(0.14)	
weeks 9-12	0.030	-0.0055	0.066	0.082*	-0.075***	0.40**	757
	(0.043)	(0.030)	(0.045)	(0.041)	(0.026)	(0.16)	

[1] Dependent Variables are listed at the top of each column. Results are from OLS regressions on phone survey outcomes, with different treatment effects estimated as the average of groups of 4 weeks.
[2] Each coefficient is estimated with a separate regression. Each coefficient gives the estimate for the treatment effect of *transport* with the sample restricted to the weeks denoted in the first column. The total number of observation used all regressions in each *row* is given in the last column (N)
[3] Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C10: Heterogenous Effects at Endline by Respondent Education

	(1) work	(2) work perm	(3) searchnow	(4) searchboards	(5) discouraged	(6) work satisfied
Average Treatment Effects At Follow Up (Pooled Sample)						
Pooled Sample	0.057*	0.030	0.082*	0.080*	-0.054*	0.029
	(0.034)	(0.026)	(0.041)	(0.044)	(0.029)	(0.032)
Heterogeneous Treatment Effects by Education Level Completed						
Grades 0-9	0.16**	0.044	-0.130	-0.015	0.003	0.22***
	(0.079)	(0.060)	(0.11)	(0.11)	(0.085)	(0.064)
Secondary	-0.066	-0.051	0.14*	0.110	-0.022	0.018
	(0.084)	(0.043)	(0.081)	(0.086)	(0.052)	(0.061)
Diploma	-0.044	-0.013	0.17**	0.091	-0.095**	-0.056
	(0.075)	(0.046)	(0.079)	(0.079)	(0.046)	(0.061)
Degree	0.23***	0.15**	0.067	0.110	-0.074	-0.001
	(0.073)	(0.074)	(0.080)	(0.071)	(0.049)	(0.066)
Observations	658	657	658	658	658	596
R-squared	0.021	0.055	0.022	0.030	0.020	0.022
Mean of Dependent Variable for Control Group by Education Level						
Grades 0-9	0.390	0.040	0.620	0.280	0.220	0.110
Secondary	0.440	0.090	0.570	0.380	0.190	0.170
Diploma	0.490	0.110	0.650	0.530	0.120	0.200
Degree	0.410	0.180	0.700	0.620	0.080	0.140
All Levels	0.440	0.110	0.640	0.470	0.140	0.160

[1] Results are from OLS regressions on endline outcomes, details of the specifications titled are in the REF
[2] Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C11: Effects on Finances, Expectations and Aspirations at Endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	log of:								
	total savings	formal savings	total money	expenditure	fair wage	market wage	job prospects	kept occ pref	expected offers
Panel A: Impacts on Aspirations at week 16									
TE Ave	0.029	-0.120	-0.068	0.064	-0.048	-0.036	-0.054	0.085*	-0.061
	(0.20)	(0.23)	(0.12)	(0.062)	(0.043)	(0.048)	(0.041)	(0.049)	(0.32)
Heterogeneity by Sample									
TE board	0.160	-0.340	-0.064	0.087	0.030	0.025	-0.056	0.065	0.150
	(0.28)	(0.28)	(0.17)	(0.087)	(0.057)	(0.064)	(0.054)	(0.063)	(0.31)
TE city	-0.130	0.200	-0.073	0.038	-0.14**	-0.110	-0.050	0.110	-0.300
	(0.28)	(0.29)	(0.17)	(0.093)	(0.058)	(0.070)	(0.063)	(0.076)	(0.59)
Panel B: Heterogenous Impacts on Aspirations by work status week 16									
TE work	0.260	-0.150	0.160	-0.043	-0.040	-0.016	-0.095*	0.090	-0.300
	(0.23)	(0.26)	(0.24)	(0.086)	(0.056)	(0.063)	(0.052)	(0.065)	(0.34)
TE no work	-0.370	-0.037	-0.180	0.150	-0.065	-0.070	-0.010	0.084	0.210
	(0.30)	(0.43)	(0.15)	(0.10)	(0.077)	(0.078)	(0.072)	(0.080)	(0.55)
Heterogeneity by Sample									
TE work-board	0.360	-0.350	0.037	0.003	0.087	0.092	-0.064	0.077	0.067
	(0.32)	(0.31)	(0.32)	(0.11)	(0.084)	(0.089)	(0.072)	(0.078)	(0.41)
TE no work-board	-0.250	-0.360	-0.130	0.200	-0.057	-0.079	-0.047	0.065	0.270
	(0.41)	(0.66)	(0.22)	(0.16)	(0.095)	(0.088)	(0.089)	(0.12)	(0.52)
TE work-city	0.110	0.220	0.390	-0.120	-0.22***	-0.16*	-0.15**	0.120	-0.840
	(0.31)	(0.40)	(0.27)	(0.16)	(0.060)	(0.082)	(0.064)	(0.12)	(0.55)
TE no work-city	-0.500	0.330	-0.240	0.110	-0.073	-0.061	0.026	0.100	0.150
	(0.44)	(0.46)	(0.18)	(0.13)	(0.12)	(0.13)	(0.11)	(0.11)	(0.95)
N	440	225	286	590	594	594	658	450	571

[1] Dependent Variables are listed at the top of each column. Results are from OLS regressions on phone survey outcomes, with different treatment effects estimated as the average of groups of 4 weeks.

[2] Each coefficient gives the estimate for the treatment effect with the sample restricted to the weeks denoted in the first column. The total number of observation used all regressions in each *row* is given in the column (N)

[3] Standard errors are in parenthesis and are robust to correlation within clusters (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level.