Location, search costs and youth unemployment: A randomized trial of transport subsidies in Ethiopia

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Abstract

Do high costs of search affect the labour market outcomes of young job seekers living far away from the centre of cities? I randomly assign temporary and non-fungible transport subsidies to unemployed youth living in spatially dislocated areas of Addis Ababa, Ethiopia. Lowering transport costs increases the intensity of job search (during and after treatment), and increases the likelihood of finding permanent employment by 6 percentage points in the short run. Analysis of weekly phone call data show that search activity declines over time but the subsidies prevent this from happening in the treatment group. The subsidies reduce participation in temporary and informal work, suggesting an important role for alternative sources of income to support job seekers. I explain these results with a dynamic model of job search with savings, cash constraints and monetary search costs. The predictions of the model are quantitatively consistent with the estimated impacts on increased job search activity, and in turn the rates at which jobs are found. These results suggest that the cost of transport in large cities can lead to frictions in the matching of firms and workers and reinforce spatial inequality.

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1 Introduction

Young people in African cities often spend long periods of time in unemployment while trying to find satisfying livelihoods. This has long been a concern of researchers and policy-makers (Harris and Todaro, 1970; World Bank, 2012). Cash constraints create frictions in labour markets if job-seekers cannot optimally invest in finding employment (Card et al., 2007; Bryan et al., 2014). These constraints are compounded when transport costs make job search particularly expensive for those living far away from jobs in sprawling cities.

As African economies expand, the growth of cities is expected to create thicker labour markets (Marshall, 1890) and allow workers and firms to make better matches faster, and reduce the time spent in unemployment (Moretti, 2014; Puga, 2010; Wheeler, 2006). However the rapid growth of cities has often not come with corresponding investments in infrastructure and well located housing stock, leaving an increasing number of people living on the outskirts of cities, far away from access to employment opportunities.[1] The resulting increase in the costs of search could inhibit the creation of new and higher quality jobs (Pissarides, 2000; Acemoglu and Shimer, 1999) and leave particular individuals locked out of employment opportunities.

This paper studies whether young job seekers who live far from the centre of Addis Ababa are constrained in their ability to find jobs by their place of living. A randomized controlled trial of transport subsidies tests directly for an impact of search costs and location on job search outcomes. The subsidies were non-fungible (they could only be used for transport to the city centre) and provided for two days of travel for a pre-announced period of time (12 weeks). Subsidies covered entirely the costs of transport to the city centre but no more. In this way the subsidies exogenously reduced the costs of search *without* introducing wealth effects or reducing the costs of commuting to work in the long run. Since the treatment acts only to bring individuals living far away to a more a equal footing with those living closer in, the results should be interpreted as evidence both for the existence of search frictions and for inequality in access to jobs due to living location.

I find that individuals spend more time searching for work, and are more likely to have applied for jobs, and to have visited the central job boards during all weeks of the study. They are also less likely to be engaged in forms of temporary or casual work in the weeks when they are receiving treatment, suggesting that they substitute away from taking informal work to concentrate on finding good jobs. By the end of the treatment period they are significantly more likely to being working in good jobs.

The subsidy programme is evaluated using a detailed baseline and two endline surveys (4 and 10 months after baseline). A phone call survey was also conducted, whereby respondents were phoned every week for 3 months, and asked a series of short questions about labour market outcomes. I track the trajectories of search intensity over all weeks using this high-frequency data, and look at the timing of changes in employment outcomes.[2]

I used a two sample approach to compare and validate the impact of these subsidies on two different, but both policy-relevant sub-populations. The first (referred to as the *city* sample) surveyed unemployed people found at home in randomly selected slum areas of Addis Ababa. Most of the respondents in this sample were unskilled, worked in the informal sector, and were unqualified for white-collar jobs. The second sample (referred to as the *board* sample) was taken from individuals found at the main vacancies boards in Ethiopia. They were relatively

[1] This is especially true in cities where policy has made it particularly hard for the poor to live in the centre.

[2] There is very little evidence from high-frequency data on employment outcomes. For recent evidence from the US see Krueger and Mueller (2011).

well-educated, active job seekers, who aspired to highly sought-after professional jobs. I argue that the latter group are those more likely to self-select into participating in youth employment programmes.

Four months after baseline, shortly after the transport treatment ended, treated individuals in the *board* sample are 7 percentage points (relative to a control mean of 19%) more likely to have permanent work. They were not more likely to be working overall, but treated individuals had jobs of higher quality, in higher skilled sectors, and that were more likely to be located in central parts of the city.

By contrast, treated individuals in the *city* sample were not more likely to have found permanent jobs, but the treatment improved their labour market outcomes in other ways. They were more likely to be working (by about 8 percentage points relative to a control mean of 46%) at the endline, they had jobs of higher average quality, and were less likely to be working as day labourers. Furthermore these treatment effects are at least partly persistent. While there is some catch up, the control group among the *board* sample are still less likely, by 3.5 percentage points, to have permanent work 10 months after the baseline.

To explain these results I develop a dynamic discrete-time model of risk averse and cash-constrained job seekers, who must choose whether or not to search for work in each period, and, conditional on that decision, how much of their remaining savings to consume. Job search is costly in monetary terms, it enters the utility function directly such that the marginal utility-cost of search is higher for someone with lower savings. In this model search is risky: the future value of remaining unemployed, after failing to find a job, falls dramatically when someone has very low savings and has to spend a significant portion on searching for work. The model implies a single crossing point in the value of searching versus not searching so that workers with savings above this amount find it optimal to search. Individuals starting with wealth above this crossing point search actively and run down their savings until they no longer find it optimal to search in every period.[3]

I estimate this model numerically, for a wide range of parameter values. I am able to solve for (1) the critical value of savings required to search, (2) the number of weeks it takes a (relatively) wealthy individual to run down savings to that critical point, and (3) the proportion of workers who search for work in steady-state. I find that these predicted outcomes are largely consistent with their empirical moments estimated for the control group in my sample. I then simulate the effect of reduced transport costs on search and consumption activities, and so estimate the effect of treatment on each of the three model predictions mentioned above. The model predicts that lowering transport costs reduces the critical value at which one gives up search, reduces the time taken for job seekers to run down their savings, and results in a higher proportion of individuals searching for work in the steady-state.

The predictions of this cash constraints model are consistent with the estimated impacts of search costs on job search. The high-frequency data shows that the treatment effect on search exhibits a particular trajectory over time. In the control group the percentage of respondents searching for jobs declined over the course of the study as respondents gave up job search and fell into unemployment, or took up temporary jobs to make ends meet. Three key facts emerge. Firstly, the subsidies seem to have prevented respondents from *giving up* job search over time as they ran down their savings, or from skipping weeks of search once they no longer had the savings to search in every week. Secondly, treated individuals are still more likely to be searching *after* the treatment has ended, confirming a prediction of the model that the subsidies allowed

[3]Below that level of savings job seekers save up for job search.

them to hold on to savings and thus to search for longer. Finally I show that the treatment effects of the subsidies are particularly strong among the relatively poor and cash constrained.

By contrast I find no impact of the treatment on reservation wages, perceptions of the job market, or aspirations. While I cannot rule out that learning effects or other psychological mechanisms might play a role in explaining the results, the model of monetary search costs and cash constraints better explains the trajectory of impacts over time. Qualitative interviews with a number of young job seekers confirm that the treatment did not introduce the job seekers to a new search technology: almost everyone had used the job boards at least once before. In addition, I test for the impact of the phone calls on job search behaviour or endline job outcomes, by using a "pure control" group who received neither phone calls nor transport subsidies. I find no impact of the calls on job search behaviour or labour outcomes, which suggests that results are not driven by Hawthorne effects.[4]

These findings contribute to the literature in a number of ways. A large literature investigates the relationship between cash constraints and job search (Danforth, 1979; Chetty, 2008; Card et al., 2007; Acemoglu and Shimer, 1999). In these models unemployment insurance plays a role in allowing job seekers to smooth consumption while in unemployment.[5] I contribute to this literature with a model in which search costs are monetary and can be subsidized directly. Existing work generally assumes utility functions that are separable in consumption and the (dis)utility of search.[6] In my model, individuals who are particularly poor or running out of savings cannot simply search more. Rather than increasing the rate of exit from unemployment, binding cash constraints make it less likely that a job seeker will find a permanent job.

Monetary search costs are a likely to a fitting assumption for many developing countries where the poor spend high proportions of their earnings on transport. This setting, and my results, are most similar to literature looking at migration decisions in developing countries. Migration, like job search, may entail costs that are too high or risky to pay, especially for cash constrained individuals. In this context, providing subsidies to migration (Bryan et al., 2014) or increased liquidity (Ardington et al., 2009) allow young people to migrate to cities, which greatly increases employment and household earnings.[7]

The finding that search costs are creating frictions in labour markets suggests that reducing the costs of transport and improving access to information about jobs could have impacts on access to employment. Other studies have found relatively weak impacts of active labour market policies (Betcherman et al., 2004; Groh et al., 2012; Ibarraran et al., 2012). However, this experiment was conducted on a small scale. These effects could be partly displaced in general equilibrium if reducing search costs improves labour market outcomes at the expense of other job seekers (Crépon et al., 2013).

Still the costs of search fall are likely to fall particularly hard on individuals who live further

[4] The main results are robust to using the two control groups pooled together, or using each of the two control groups separately.

[5] Indeed the youth's inability to smooth consumption during long spells of unemployment is one of the concerns motivating this paper. Without adequate family support, young people can suffer extended periods of long periods of poverty in unemployment, and can have their life aspirations postponed if not severely blunted (Mains, 2013).

[6] See Danforth (1979) for a discussion of this issue.

[7] Jensen (2012) and Beam (2014) provide evidence on how information about jobs, in a rural context, can increase employment rates in urban jobs. However the literature has not addressed how rural-urban migrants, and other poor individuals living on urban peripheries, search for employment *within* cities, and the constraints that they face in doing so. There is some evidence from developing countries which suggests that freeing up certain physical constraints in relation to housing (Field, 2007; Franklin, 2012) and access to electricity (Dinkelman, 2011) allows the poor to increase labour supply. One of the mechanisms suggested for these effects is that individuals are constrained in their ability to search for jobs.

away from the city since the costs of acquiring information about jobs increases with distance (Ihlanfeldt, 1997). Individuals who suffer most acutely from a lack of access to cash to cover the costs of search are also likely to be particularly constrained. In this sense my results provide evidence for the spatial mismatch hypothesis (Kain, 1992; Holzer, 1991; Zenou, 2009), which has been applied and confirmed empirically in the context of large cities in the United States (Kling et al., 2007; Phillips, 2014) but, to my knowledge, never rigorously tested in developing country context until now (Banerjee et al., 2007; Verick, 2011). My results and theoretical model of cash constraints contribute to this literature by explaining the mechanisms through which search costs leave certain individuals at a distinct disadvantage in the labour market.

Finally my results shed light on the role of the informal sector in providing livelihoods for urban youth. The segmentation of African labour markets between formal and informal sectors has been well studied by economists, since at least Lewis (1954) and later Harris and Todaro (1970) and Fields (1975). Debates have raged about whether the informal sector provides a temporary livelihood for those queueing for better work (Serneels, 2007) or if it is a vibrant entrepreneurial sector in its own right (Maloney, 2004). I find evidence that young people use temporary work as means to finding better work, with "planned separations" from these temporary jobs (Browning et al., 2007). I show that improving access to formal sector employment opportunities (by reducing search costs) reduces labour supply to forms of temporary and casual work.[8] My theoretical model shows that reducing cash constraints by providing job seekers with regular income would diminish the risks of unemployment and allow them to search more intensively, while also mitigating the need to take up undesirable forms of temporary work.[9]

The paper proceeds as follows: Section 2 discusses the setting and design of the experiment, while Section 3 discusses the data and randomization. The results turn first to the impact of the subsidies on employment outcomes at endline only in Section 4. Section 5 looks at the trajectory of the impacts on job search over time using the phone survey. The theoretical Section 6 explains these impacts on job search with a model, which is used to generate further predictions, which are tested in turn in Section 7, which also investigates other potential mechanisms. Section 8 concludes.

2 Setting and Experiment

Addis Ababa is an ideal setting to study the effects of urban growth and transport costs on youth labour markets. The city's population has been doubling nearly every decade for last 40 years, and is now estimated to be 4.5 million, and is estimated to grow to 12 million by 2024 (UN-Habitat, 2005). Addis Ababa has been the major destination city for rural-urban migration in Ethiopia, and is one of the most rapidly growing cities in Africa as a result. Many of the new migrants can not access well positioned land in the centre of the city, and long term residents are also being forced out of the inner city slums to make way for new development.[10]

[8] An extensive literature on labour in Ethiopia (Mains, 2013; Serneels, 2007; Haile, 2005), the descriptive data collected for this paper, as well as anecdotal evidence from field work, all suggest that most young Ethiopians still aspire to wage employment in the formal sector. Public sector work is sought after, but not to the extent that it once was. Formal employment, particularly permanent jobs, provide surety, security and social prestige.

[9] Evidence on high-turnover for factory jobs in Ethiopia (Blattman and Dercon, 2015) lends support to this idea, in a very similar context: workers seem to have taken low skill jobs due to cash needs rather than aspiring to keep them long term.

[10] Compensation is usually poor and many of those displaced are suffering from having to move to dislocated areas where they no longer have access to their social networks and business links in the center of the city. Existing research documents the loss of income, and transport related problems of those living in worse locations within the city (Yntiso,

The labour market appears to suffer from many frictions, many of which are induced by distance. Rates of youth unemployment are high in Addis Ababa, at 28% in estimates of Broussard and Teklesellasie (2012). Yet firms still often express frustration at their inability to find the right skilled candidates. The secondary and tertiary education system has expanded enormously in recent years, increasing the supply of labour of formal sector work, but making it harder for firms to distinguish among applicants.

For job seekers the costs of gathering information about jobs are high, and transports costs often comprise a high proportion of their weekly expenditures. The majority of white-collar jobs are found on job boards, located in the center of the Addis Ababa. For job seekers living far away from the city center, finding a job for the first time or after a spell of unemployment is difficult, especially for those without savings or financial support. Many college graduates spend years looking for their first job (Serneels, 2007).

Young people search for work with limited budgets and savings (and no welfare support or unemployment insurance), and often eed to take up forms of temporary work to make ends meet, while looking for better, permanent work. These are jobs like casual labour or self-employment and work for the family, usually in and around their local areas. The better jobs are usually located in the centre of the city and require formal applications.

The costs of searching for a good job are not evenly distributed: they are considerably higher for those living far from the city centre, those that are particularly poor or find it hard to find other sources of income to support themselves, and new migrants who do not have access to the social networks that might otherwise allow to access employment opportunities. Thus some young people are at a disadvantage in their ability to share in the continued growth of Addis Ababa's economy.

2.1 Experimental design

I test for the relationship between search costs and labour outcomes by providing a cash transfer that could only be used for travel. Individuals were given money to cover the costs of the transport if they arrived to collect it at designated spot in the center of the city. The amount given out was enough to cover the costs of return trip from their place of living to the centre. We budgeted enough money to make the trip by mini-bus which is the preferred mode of transport because of its flexibility and regularity.[11] The average one way trip to the centre took 33 minutes by mini-bus.

The subsidy amount was tailored to the distance an individual travelled; using the transport costs for a trip from each respondents place of living, using the current fares in Addis Ababa, as surveyed by the enumeration team. The modal amount given was 15 birr for a return trip, or just less than $1 per day.[12] No one received less than 12 birr or more than 20 birr. There was little or no spare cash left over after covering the costs of transports. The money was collected from a makeshift kiosk near the main bus terminal and transport hub in the centre of Addis Ababa, which is close to some of the main job boards in the city.

The sample was assigned to treatment and control groups randomly, with the sample split

2008).

[11] 58% of respondents said their main mode of travel was in a mini-bus, 38% said that they used the large yellow government buses. These buses are cheaper but unreliable and uncomfortable. A negligible number used other modes of transport such as walking or getting lifts with acquaintances with cars. These mini-buses are similar to those used in many African countries, an overview of the industry can be found in Kumar and Barrett (2008).

[12] The US dollar - Birr Exchange stood at around 18 Birr to $1 at the beginning of the experiment.

into three groups: the treatment group who received the subsidies and were also called in every week, and two treatment groups; one that received the same weekly phone calls, and one that was not called at all.[13] This allows me to test both for an impact of the subsidies on trajectories over time with the phone data, and test for an impact of the phone calls at endline. Furthermore, treated respondents were randomly divided into those receiving the subsidies for 8 weeks and those receiving them for 11 weeks.

Immediately after the completion of the baseline data collection the sample was assigned to treatment and control groups for the purposes of the experiment. Randomization was done by stratifying the sample by a number of different baseline covariates, including gender and education. I followed the standard blocking procedure as suggested by Bruhn and McKenzie (2009). Within these strata, 30% of individuals were assigned to both the transport and the calls only groups. The remaining 40% were designated as pure controls. A more detailed discussion of the variables used to block the randomization is given in the data description section. Figure C.2 in the appendix gives an overview of the randomization design and timeline. 551 respondents received the phone calls, of them a further 255 were offered the transport treatment. 326 were not contacted again until the endline.

The treatment group began to receive the subsidies one week after the end of the baseline, which will be referred to as *Week 1* throughout the rest of the paper. Phone call surveys, which are described in more detail in section 3.2 were also begun in that week. The treatment group were clearly informed of the nature of the subsidies, that they had been randomly selected, and for how long they would be to collect them. They could collect the subsidies no more than twice a week and only before midday on any week-day, by showing an identification document and signing for the money.[14]

In the week before they were last allowed to collect the money, they were phoned and reminded that they would no longer be receiving the transport in the next week. The last respondents received their money in week 11 of the study. The last phone calls of the study were completed in week 12. In total respondents could collect the money *up to* 22 times.

2.2 Transport Costs in Perspective

The cost of transport to and from the centre of the city seems small, at less than $1 a day, but it is a substantial cost to pay for unemployed youth in Addis Ababa. Reported expenditure from the baseline survey for this experiment confirms that search costs are high. A *single* trip by mini-buses costing 9.50 Birr represents 12% of median weekly expenditure for individuals in the sample, which was just 80 Birr ($4). Weekly transport costs were on average 25 birr, or 20% of median total expenditure.

Job seekers found money to pay for these costs and other expenditure items from difference sources. Some took casual labour jobs which pay about 200 Birr a week on average. Others (about 50% of the sample) received money from their family; those that did received about 150 Birr per week on average. So the cash provided by the intervention, of up to 30 Birr per week, constitutes a significant transfer for many of the respondents.

This subsidies do not necessarily cover all of the costs of job search. Respondents reported that there are many other costs associated with search, such as paying for printing and photo-

[13] No one was assigned to just the transport treatment without getting the phone calls.

[14] This was designed to limit the use of the transport subsidy for recreational use, to make it useful for job seekers who would come in early to see the new job postings, and apply for jobs in the afternoon, but not individuals who wanted to take advantage of evening entertainment in the city center.

copying, buying clothes for interviews, renting or buying newspapers with information about vacancies, and sometimes paying firms to make applications.

Individuals who were offered the full 11 weeks of the program had the option to collect up to 330 Birr ($17) over the course of the study. If a respondent collected the subsidies at every opportunity, the monthly subsidy was worth about 10% of the monthly salary of a good permanent job, or 15% − 20% of some of the lower paid, informal jobs.

3 Data

Here I describe the data collection and the randomization procedure used in the experiment. I discuss the sampling strategy used for surveying, report on rates of attrition from the survey, and show tests of balance on baseline covariates after randomization.

Figure C.2 in the Appendix provide an overview of the timeline of the project. The experiment unfolded as follows: the baseline was conducted, finishing in week 0 of the study (10 April 2013), immediately after which individuals were assigned to treatment and control groups. Both the transport subsidies and the weekly phone calls began in the week after after baseline, week 1, and continued until week 12 of the study. Three weeks after the end of the phone call and transport treatment the endline survey was conducted (week 16). Respondents were interviewed in a random sequence at the endline survey, in order to avoid correlation between individual characteristics and time effects.

3.1 Sampling Strategy

This study uses a two sample approach: I study the effects of transport subsidies in two representative but distinct populations in Addis Ababa. This was done to verify the validity of the results across two different population relevant sub-populations, and to compare the nature of the impacts across heterogeneous individuals. This allows me comment on which individuals would most benefit from the subsidy.

From here on, I refer to the one sample as the *city* sample and the other as the *board* sample. The two samples, taken together without dividing respondents in this way, will be referred to as the *pooled* sample. The distinction between the two samples is central to the interpretation of the results because they differed in terms of the margins at which their employment outcomes could be improved.

Screening: Both samples comprise of men and women aged 18-30 who able and available to start a new job in Addis Ababa in the next 2 weeks. Individuals who had some kind of temporary work were included, but those who had no interest in taking a job other the one they already had were not. All individuals were screened on their place of living: only individuals living in neighbourhoods least 5km away from the center of Addis Ababa were included. See the map in Figure C.1 in the Appendix for an idea of the layout of Addis and the radius outside of which the sample was drawn. Individuals in the sample live, on average, 6.8km as the crow flies (sometimes considerably further by road) from the city center where the transport money was collected.

The individuals making up the two samples, both screened for eligibility, were found in the following ways:

City sample: This sample was randomly drawn by going door-to-door in small enumeration areas around the city. Addis Ababa is made of 10 subcities. Four of these, contained within a 5km radius from the centre of the city, were not included in the sample. The chosen sampling areas were stratified to be representative of the remaining 6 subcities located on the periphery of the city. led by selecting two Kebeles from that subcity.[15] Two enumerator teams then moved outward in different directions from the center of the chosen Kebeles, surveying about 60 individuals per Kebele. The survey sites are marked in Figure C.1. Respondents were interviewed at home, enumerators returned for interviews if they were not around at the time of the first visit.

Board sample: The board sample was drawn by randomly approaching individuals who were gathered in the areas around the job boards in the center of Addis Ababa. Although they were all interviewed in the center of the city, these respondents were screened on their place of living, all of them lived in same subcities used in the sampling for the city sample, ensuring that they lived at least 5km away from the center. Since they were all at the job boards at the time of screening, they were almost all, by definition, job seekers, and fit the screening criteria outlined above.

I find impacts of the subsides on both samples, but the two samples respond at very different employment margins: this difference reflects the different backgrounds of the two samples, and the employment opportunities that are available to them. While the characteristics of the *city* sample were similar on average to young people in representative survey data, the *board* sample is clearly a selective group. Notably, they were far more likely to be highly educated, they were more likely to be recent migrants from smaller towns or rural areas. Representative data from Addis Ababa in 2012 suggests that 22% of the same age cohort had some kind of post-secondary education, about 10% had university degrees. The corresponding figures for the board sample is 72% and 43% respectively. At endline they were far more likely to have permanent jobs, which the *city* sample were very unlikely to find.

I consider my two samples to be representative of two sizeable and policy relevant sub-populations: The *city* sample are those who are withdrawn from the formal labour market and could be induced to (re-)enter by lowered, while the *board* sample represents the group of individuals who are most likely to be part of the growing formal and high-productivity sector in Ethiopia, but may give up search too soon, or become discouraged by their inability to find a good match in the market. The *board* sample started out searching actively, but stood a high chance of becoming discouraged or running out of the funds required to keep pursuing employment. They are also a group most likely to be aided by youth program and active labour market policies initiated by governments or donors, since they would rely on youth to self-select into these programs.

3.2 Phone Survey

I used 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 treatment effects impact job search, by fully accounting for job seekers activities during the time of treatment. As described in the experimental design, I also randomly assigned some individuals in the control group to not receive the phone calls. In all 524 individuals were assigned to the phone call survey, and 4,510 interviews were conducted over 11 weeks, an average of just over 400 individuals contacted each week, and each respondent contacted on average 10.4 times.

[15]With a population of over half a million, this subcity is more populous than the next biggest subcity by more than 50%.

The phone calls were short: they would take between 2 and 4 minutes to complete. The questionnaire was short, giving only a handful of measures that can be used in the analysis of the phone call data. While this restricts the detail of the investigation that can be conducted, it has the advantaged of pre-committing me to testing the significance of just a few major outcomes. These are the outcomes that I analyse in detail throughout the paper, using more detailed endline surveys to investigate further where necessary. Most importantly, the only measure of job quality used in the survey was whether the job was a permanent job- this is the main outcome analysed at endline.

Since this outcome receives special attention throughout the analysis, it is useful to note that it was not hand-picked from a range of different quality measures in the phone survey.[16]

3.3 Tests of Balance

Table D.1 in the Appendix presents test for balance on variety of job market outcomes, focusing on the main employment outcomes that will be used to test for impact, and that were measured in the phone survey, and then respondent characteristics and other labour market outcomes.

Blocking and randomization was done for each sample separately, so that treatment is necessarily balanced across the two samples. I make sure to test for balance in the pooled sample, and in each sub-sample separately.

There is balance across a wide range of measures, in the pooled sample, and the two samples separately. Very few measures, and none of the variables used for the blocking or employment outcome variables are statistically different across groups. For the *board* sample individuals in the control group are more likely to be recent graduates (individuals who finished school, university or vocational training in the last 15 months). This is a group that may not have been searching for work for quite as long.

3.4 Attrition

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 are significant predictors of attrition at endline. Table 1 provides an overview of rates of attrition at various points of survey. Attrition is high in this sample.[17] I argue that much of the attrition is due to respondent who changed phone numbers, or provided poor contact information at baseline, before the treatment began.

We could not find 14% of the total sample at all after the baseline, and about 25% were not interviewed at the endline survey. However, among the phone call survey respondents, a large proportion (just less than half) of the total attrition took place between the first phone call surveys.[18] This sort of attrition is unlikely to be correlated with the transport treatment, since treatment did not start until *after* the first phone calls.

The phone calls do seem to have reduced attrition, but only by about 4% (the effect is not

[16]The nature of permanent work is discussed in more detail in Section A.

[17]This was because of the limited resources of the survey team (which required us to use phone calls as the main means of response during a time when the Ethiopian cellular network was very unreliable). This was also a very mobile study population: young people often left town, changed phone numbers, or did not want to respond, with short notice.

[18]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%.

significant). The main results of this paper are robust to using either control group (the "no calls" and the "no transport" group), so this is not of great concern.

Table 1: Attrition by treatment status

		Calls		
	Control	No Transport	Transport	Total
Never found	81	22	22	125
	24.85%	7.43%	8.63%	14.25%
Contacted by phone, not Endline	0	35	31	66
	0%	11.82%	12.16%	7.53%
Refused at Endline	9	12	7	28
	2.76%	4.05%	2.75%	3.19%
Interviewed at Endline	236	227	195	658
	72.39%	76.69%	76.47%	75.03%
Total	326	296	255	877
	100%	100%	100%	100%

The group who received the subsidies had almost identical rates of attrition (at all stages of data collection) to those receiving calls but not treatment. Table D.3 shows the main determinants of whether a respondent was found at follow up (in week 16). The first two columns show that the transport subsidies had no effect on attrition after controlling for receiving the calls. Columns (3)-(6) show that very little else impacted the probability of being found at follow up. I look at predictors of attrition for the *board* and *city* separately and find that attrition is particularly hard to predict for the *board* sample.

Furthermore, I show that the sample is balanced on baseline variables between treatment and control *after* attrition. Table D.2 Panel B shows balance after attrition to the endline survey, while Panel C shows balance among those ever reached for the phone call surveys. This shows that the actual samples used for estimating treatment effects are broadly balanced on covariances. All variables that are relatively strong predictors of attrition are used as covariates in estimating regressions as robustness checks. I conclude that the results in the paper are not driven by attrition.[19]

4 Main Results: Employment Outcomes

I estimate the impact the transport subsidies on the labour market outcomes of job seekers at the main endline survey, 16 weeks (4 months) after the baseline, and about one month after the end of the transport subsidy program itself. I then present results from the second endline survey (conducted by phone) 40 weeks after the baseline to look at the persistence of these effects long after the experiment ended.

I leave aside the impacts on job search for now. Section 5 will use high-frequency phone call data, and argue that the increased job search intensity over the weeks of the study is the main driving force behind the large job outcome impacts documented in this Section. In Section

[19]There is one notable exception to this. The *city* sample were particularly hard to track down at the second endline, 10 months later. The attrition in that sample is a problem. However, the results in this paper focus on the first endline. I discuss this issue further in Section 4.1.1.

7 I test additional predictions of the theoretical model (outlined in Section 6) and investigate mechanisms other than increased job search that may be driving the results.

All results are intent-to-treat (ITT) estimates on binary labour market outcomes, using difference and difference-in-difference OLS estimators. Standard errors are clustered at the Woreda level (the lowest urban administrative unit in Ethiopia), of which there are 70 in my data, suggesting that I do not have problems with too few clusters (Cameron et al., 2008).[20]

For robustness, I employ a series of different estimators, and present the results from all of these specifications here, to show that results are not sensitive to specification. I focus on the limited set of binary outcomes used in the phone survey. In this sense I tie my hands to only look at a small group of outcomes that were chosen before the study began. Later I turn to explore other employment outcomes from the endline survey in more detail.

Regressions on endline outcomes take the form:

$$y_i = \alpha + T_i\lambda + \epsilon_i \tag{1}$$

$$y_i = \alpha + T_i\lambda + X_{i0}\beta + \epsilon_i \tag{2}$$

$$y_{is} = \alpha_s + \sum_s T_i S_{si}\lambda_s + X_{i0}\beta + \epsilon_i \tag{3}$$

T_i is the treatment variable dummy. Equation (1) estimates the basic difference in means between the treatment and control group (this specification is labelled BAS in the tables). Equation 2 includes a set of individual covariate controls (COV), based on baseline outcomes, which also include basic individual characteristics, and especially those that exhibit any minor imbalance between treatment and control at baseline. X_{i0} could easily be replaced with a set of blocking dummy's on which assignment to treatment was based (see 3), this specification is labelled in tables as (BLK).

Equation 3 provides the basic form for estimating different treatment effects for different groups (or heterogeneous treatment effects) by baseline group S_i. Usually this is used to estimate treatment effects for the two samples, the *board* and *city* samples, but will be employed to estimate treatment effects by different education outcomes, or poverty levels at baseline.[21]

Further I estimate difference-in-difference style estimators, by looking at the impact of treatment in the change in labour market outcomes between baseline and endline, as in equation 4 below labeled (FD) throughout. The ANCOVA estimator (labelled ANC), in equation 5, is similar but looks at endline outcomes and includes a lagged dependent variable to account for differences the dependent variable at baseline in a flexible way.[22]

$$y_{i16} - y_{i0} = \alpha + T_i\lambda + X_{i0}\beta + \epsilon_i(t = 16) \tag{4}$$

$$y_{i16} = \alpha + y_{i0}\rho + T_i\lambda + X_{i0}\beta + \epsilon_i \tag{5}$$

[20]The Woreda system recent replaced the communist-era Kebele system in Addis Ababa. Woreda's were formed by the combination of 2, 3 or 4 former Kebele's into a large consolidated administrative units.

[21]These coefficients measure the size of the treatment effect for each category separately. A simple t-test can be used to test the difference in the size of the coefficients

[22]The ANCOVA estimator is more efficient that either difference in difference estimator or the standard POST estimator which ignores baseline outcomes (Frison and Pocock, 1992; McKenzie, 2012).

4.1 Jobs and Permanent Jobs

I have discussed the labour market for young urban Ethiopians briefly. I provide more detail on the types of jobs available and who finds them in the Appendix Section A. Briefly, job seekers look for good jobs. Among highly educated individuals, this means looking for permanent jobs.[23] For less skilled job seekers, this means looking for any formal sector or white collar work. Other forms of casual or temporary work are readily available. These are used for subsistence and are considered inferior.[24]

Therefore, if transport subsidies improve access to good job opportunities for treated individuals we would expect the following for treated respondents: 1) better quality jobs and more permanent jobs for those can get them, 2) an increase in the probability of having any work which may be larger or smaller than the effect on the probability of having a permanent job. It is possible that the impacts on having any work at all are zero, if the treatment only induces displacement of temporary work for permanent jobs.

In Table 2 shows the impact of subsidies on finding permanent work at endline: treatment increases the probability of finding a permanent job, among the board sample, from 19% among the control group, by about 7 percentage points, indicating about a 30% increase in the probability of having a permanent job. This is the central result of this paper. It shows that subsidies allowed active job seekers to find the jobs that they were looking for.

Table 2: Effects of transport subsidies on having permanent employment at endline

	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

The *city* sample are not more likely to find permanent work, but this is because it is difficult

[23] Permanent usually come with a written contract and an understanding that the job will be available to the employer indefinitely or for a set, but relatively long, period of time.

[24] I discuss these mechanisms in more detail in the model of Section 6.

for this group to find permanent work at all.[25]

I then look at the impact of the treatment on having any job at all. If job seekers had access to better employment opportunities we should see more individuals in work rather than unemployed. However, if the effects on "good jobs" were concentrated among individuals who would overwise have been working at temporary jobs, the effect on employment could be dampened.

I find that there is about a 6 percentage point increase in the probability of having employment at endline in the pooled sample, over a control mean of 53%. These results are concentrated among the *city* sample, for whom the effect is large (at around 8 percentage points). The effect is smaller and not statistically significant for the *board* sample.

Table 3: Effects of transport subsidies on having employment at endline

	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	657	657	657	657	657	657	657
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	657	657	657	657	657	657	657
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). 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.

4.1.1 Persistence and Dissipation

Are these impacts on job outcomes persistent a further 6 months after the first endline survey? Treated individuals were more likely to have jobs after 16 weeks, but the control group may have caught up over the proceeding 6 months by continuing to catch up. In this case the treatment effects will have dissipated after the control group have had long enough to search as much the treatment group did during the subsidy period. In this case the treatment would have had an impact on unemployment durations, an important finding, but nothing more than that. Alternatively, if job search intensity dropped off after a few months of search because job seekers became discouraged or ran out of the money required to search (for which I present considerable

[25] In fact the results are concentrated among individuals with universities degrees, to whom these permanent jobs are actually available. Results on heterogenous treatment effects in Section 7 discusses this in more detail. The effect is only present for individuals with degrees.

evidence in the next section) the control group might not catch up to the treatment group at all.

The treatment effects seem to be at least partly persistent. In Table 4 I show that when surveyed 6 months later (at week 40 of the project) those who were treated among the *board* sample are now 3.7 percentage points (roughly 10%) more likely to have permanent work. This is displayed along side the impacts for week 16, showing that the coefficient has roughly halved over time. The coefficient at 40 weeks is not statistically significant but is reasonably large.

Table 4: Impacts on having permanent work at both endlines (weeks 16 & 40)

Estimator	CM		Basic		Controls		First Diff	
			(1)	(2)	(3)	(4)	(5)	(6)
Week	16	40	16	40	16	40	16	40
Panel A: Average Treatment Effects At Follow Up (Pooled Sample)								
All	0.130	0.210	0.028	0.018	0.042	0.018	0.044*	0.017
			(0.027)	(0.038)	(0.026)	(0.033)	(0.026)	(0.034)
Obs			657	605	657	605	657	605
R^2			0.001	0.000	0.088	0.133	0.097	0.143
Panel B: Treatment Effects At Follow Up by Sample								
Board	0.190	0.310	0.068*	0.035	0.078**	0.033	0.078**	0.032
			(0.038)	(0.052)	(0.037)	(0.051)	(0.037)	(0.051)
City	0.065	0.080	-0.019	0.007	-0.004	-0.001	0.001	-0.002
			(0.032)	(0.037)	(0.034)	(0.038)	(0.033)	(0.042)
Obs			657	605	657	605	657	605
R^2			0.186	0.285	0.091	0.133	0.100	0.143

[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.

I look to see whether the effects on having any work are persistent in the *city* sample. Before doing so, it is important to note that there were problems with attrition for the *city* sample at 6 months, that were not present at the first endline or in the *board* sample in either survey. Attrition rates were high and resulted in a lack of co-variate balance at baseline for the sample that were found 6 months later.

In the basic difference specification, treated individuals in the the *city* sample were 17% more likely to be working at endline. However, this result should be read with caution: it coefficient is not stable when controlling for baseline covariates as illustrated in column (6). This is indicative of the fact that among the sample found at endline the treatment group were more likely to have had work at baseline, due to the attrition. Therefore the first difference (FD) estimates are the most trustworthy, suggesting that among the *city* sample the probability of working was increased by 10 percentage points (over a mean of 41%), although even this is not quite significant (these results are in Table D.5 in the Appendix).

The persistence of these effects suggests that reducing the duration of unemployment (or as it may be, transition into a first job) can have long lasting effects. Unemployment could lead to scarring if jobs seekers' skills worsen with time in unemployment, or if psychological factors make it hard for job seekers to resume looking for work after becoming discouraged or running out of savings.[26]

4.1.2 Job Quality

I now turn to look at the impacts on other measures of job quality and type at the *first* endline survey at 16 weeks into the study. Here I look at a number of measures that were not specified for inclusion into the phone survey. I test for whether the increase in employment for the *city* sample were in jobs of better quality, and whether the permanent jobs found by *boards* respondents are qualitatively different in other ways.

In Figure C.3 in the Appendix, I classify the jobs of all respondents working at follow up into occupational groups, rank those groups by average weekly salary earned at follow up, and plot the cumulative distribution among these occupations by treatment and control groups. The results clearly show a positive shift in the quality of jobs among the treated group.

Many job quality outcomes were significantly improved by the transport subsidies, as shown in Table 5. I look at a series of dummy variables indicating that a respondent has a job with a certain quality, all of which are in some ways proxies for the permanence, formality or desirability of work. The key variables are described in the notes to Table 5. For instance, treated respondents are 14 percentage points more likely to be working in office, as opposed to at another kind of other work site, and 4.3 percentage points more likely to have found the job through formal means (an application and a proper interview).

The results indicate that the *city* respondents are more likely to find jobs of better quality. They are not simply getting work faster by accepting inferior jobs. If I restrict the sample to just individuals who had work, and run the same regression, I confirm that, conditional on having a job, *city* respondents are more likely to have better jobs.

The *board* respondents, who are already likely to have jobs in office, or be paid by the month, do not see significant treatment effects on these variables. However, they are more likely to have found jobs that require at least a degree as a qualification. During focus group discussions run during the endline survey, many respondents attached special importance to the goal of finding work for which they were specifically trained.

I find no significant difference between the average wages of treated individuals and the control group. Although the coefficient is large for the *board* sample (9%) it is not estimated precisely. I am unable to reject the Kolmogorov-Smirnov test of equality in distribution of wages (in levels or logs) between treatment and control groups. This is not too surprising. The description of jobs presented in Appendix A shows that permanent jobs are desirable for their security. They do not actually pay more than other jobs for entry level positions.

It could be the case that treatment brought individuals who otherwise would not have been employed into work: they might have average expected earnings, which would bring down the average wage in the treatment group and downward bias the impacts on wages. I check for this using a Heckman (1979) selection model. Using both the two-step inverse Mills ratio and log-likelihood approaches to control for selection, I find similar coefficients on the impact on log incomes which are still not significant, although they are slightly larger.

[26] I discuss other issues related to the persistence of impacts on job search in Section 7 but otherwise these issues are beyond the scope of this paper.

Table 5: **Effects of treatment on job quality and type at endline**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			log			in	pay			
	work	casual	ln wage	hours	degree	office	monthly	satisfied	formally	in city
Panel A: Impacts on work outcomes at week 16										
TE Pooled	0.062*	-0.022	0.051	3.74**	0.047**	0.070*	0.069*	0.061**	0.054*	0.059*
	(0.035)	(0.024)	(0.088)	(1.71)	(0.018)	(0.037)	(0.037)	(0.028)	(0.029)	(0.032)
Observations	658	596	356	656	596	596	596	596	596	596
R-squared	0.067	0.077	0.115	0.079	0.228	0.059	0.107	0.058	0.114	0.051
Panel B: Heterogeneous impacts on work at week 16 by Sample										
TE board	0.043	0.0026	0.091	2.53	0.075**	0.020	0.032	0.015	0.064	0.097**
	(0.051)	(0.025)	(0.11)	(2.34)	(0.033)	(0.052)	(0.053)	(0.045)	(0.049)	(0.042)
TE city	0.087*	-0.050	-0.0090	5.27**	0.014	0.13**	0.11**	0.11***	0.042*	0.015
	(0.044)	(0.042)	(0.15)	(2.34)	(0.011)	(0.050)	(0.049)	(0.029)	(0.023)	(0.046)
Observations	658	596	356	656	596	596	596	596	596	596
R-squared	0.067	0.079	0.116	0.080	0.230	0.063	0.108	0.062	0.114	0.053

Results are from Difference OLS regressions on endline outcomes at week 16, using the simple (BAS) specification without covariates. Panel A shows results for the two samples pooled together.S tandard 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

Unusual Dependent variables: (5) *Degree*: Respondent has a job that required a degree as minimum qualification (6) *In Office:* Job is performed in an office, or formal business house- proxy for "white collar" work (7) *Pay Monthly:* Respondent is paid every month, usually according to set a contract (9) *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

4.1.3 Discussion: Better Jobs or More Jobs?

Subsidized transport improved job market outcomes for job seekers. The *board* sample were more likely to be working at permanent jobs, some instead of being unemployed, others instead of having temporary jobs. Thus the increase in employment among the *board* sample is smaller than the increase in the probability in finding permanent work. This would be expected if permanent work was the main outcome on which these individuals search, and there was no impact on the probability of having temporary work.

It is interesting to note that at the second endline the incidence of permanent jobs has increased in both the treatment and control groups. The incidence of temporary jobs has stayed constant among those who have not found permanent work. Thus the incidence of temporary has actually declined to 28% from 37% at week 16, in the control group. This reinforces the idea that job seekers in the *board* sample are taking temporary jobs when they have to, while looking for permanent jobs.

By contrast, the *city* sample seems to have found a better set of temporary work opportunities, ones that are still temporary but look more like formal employment. In this sample treatment seems to have an induced individuals to find better jobs when they otherwise would have remained without work, which leads to a significant increase in the employment rate for this sample. Coefficients for the impacts on job quality are usually slightly larger than the impact on having any work at all, although not much larger. As with the *board* sample, this suggests that the effect on employment is partly driven by individuals finding high quality jobs who otherwise would have been working at worse jobs.

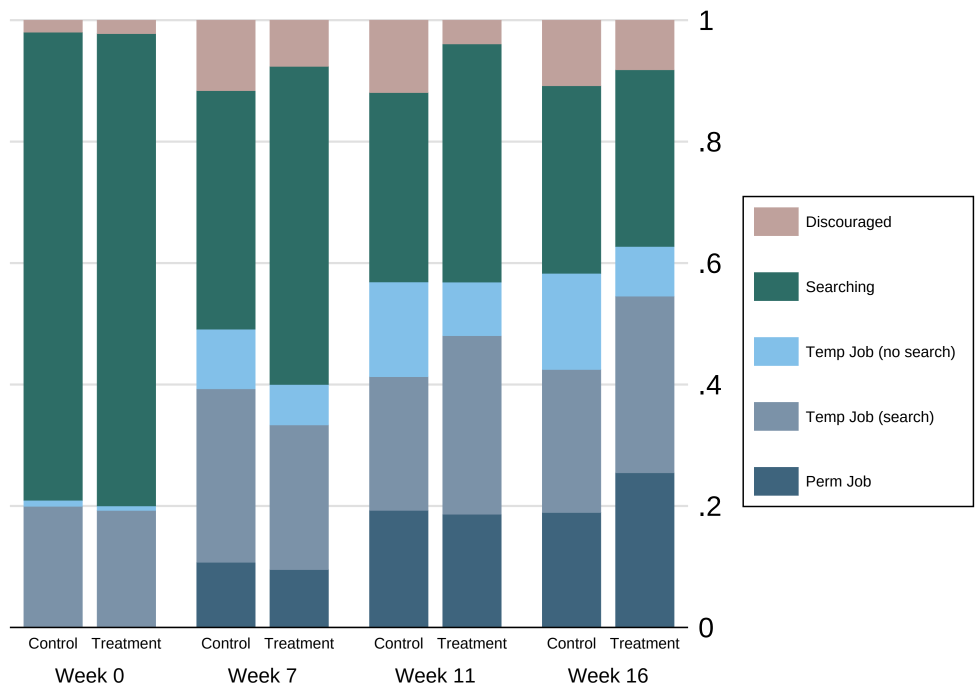
Firstly I turn to the next pressing question: what is the more proximate cause of the improved labour market outcomes documented in this section? I look at how job search responded to the transport subsidies.

5 Main Results: Job Search

This Section looks at the impacts of the transport subsidies on job search at both the intensive and extensive margins, as well as on methods of job search. These impacts on search activity appear to be driving the impacts on employment outcomes. In Section 6 I discuss the link between the magnitude of the impact in job search and the magnitude of the impact on employment, to argue that job search could explain the main results on permanent employment.

I focus on the trajectory of the impacts of subsidies over time. This illuminates the role of cash constraints in the job search process. I find the impacts are not constant over time, but increase as job seekers run down their savings and become discouraged. Section 6 fully outlines the theoretical mechanisms by which cash constraints could produce this trajectory.

Figure 1: Composition of sample by week and treatment status: Board sample

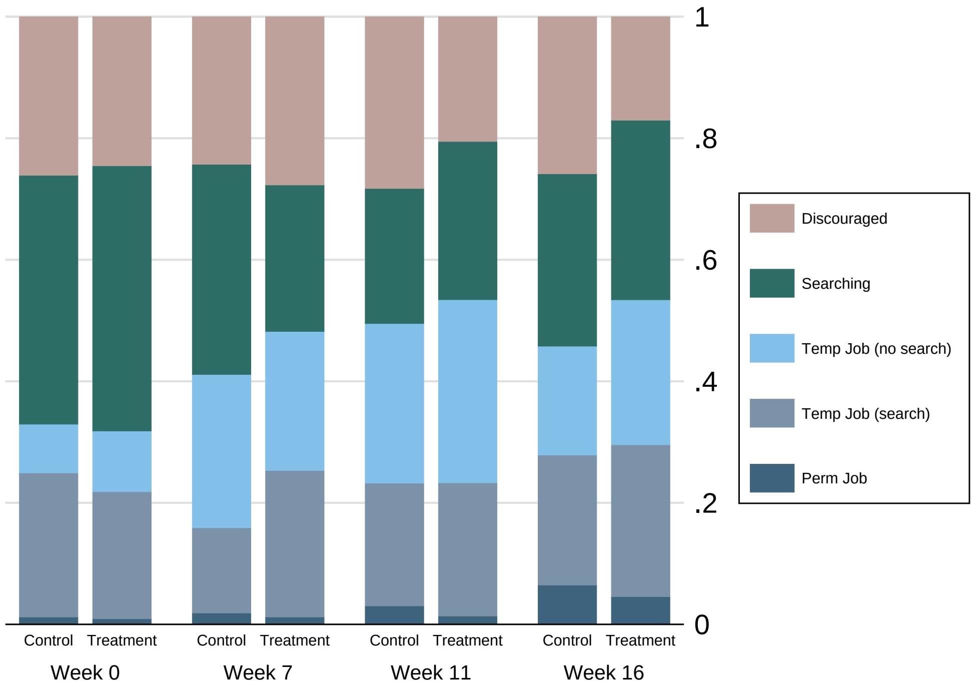


5.1 Overview: Composition Effects

Before turning to the impacts on specific job search outcomes, it is useful to look at how the treatment effected the composition of labour activities in the sample over time. I do this separately for the *board* (Figure 1) and *city* (Figure 2) samples. For each week, I show the percentage of individuals engaged in 5 distinct states of employment, for treatment and control individuals. For the sake of clarity, I present only a select number of weeks to provide an overview changing compositions, the Appendix provides similar graphs for all 16 weeks.

The 5 states plotted here are (from the top of the bar chart): Discouragement (not working or searching); Searching (but no work) Temporary work (not searching) Temporary work (but

Figure 2: Composition of sample by week and treatment status: City sample



also searching)[27], and Permanent work. In this ordering, the top of the blue bars in the graphs indicate the employment rate for the relevant group.

Week 0 shows the strong balance between treatment control in terms of the activities they were engaged in at baseline: most individuals were searching at this stage anyway, especially in the *board* sample. As the study goes on, more of the unemployed are likely to become discouraged (stop searching for work), while more of those with work are likely to give up looking for better jobs. These trends hold for both samples. The rate at which people give up job search is similar among those with, and those without temporary jobs.

The transport treatment, at each margin, pushes respondents away from discouragement, towards work, more permanent work, and increased job search intensity (regardless of employment status). This is true for most of the later periods of the study, after which the treatment had been running for a while and had time to take effect. A key focus of these results is how the subsidies had an effect sooner among the *city* sample than among the *board* sample.

Importantly the *board* sample seem to be *less* likely to be working in temporary jobs for the middle weeks of the study. As I discuss in Section 6, this is consistent with a theory of respondents substituting low quality temporary work in favour of more intensive search for jobs that they are actually interested in. If some job seekers are giving up looking for good permanent jobs by taking their outside options in temporary, or informal work, the treatment seems to have prevented this happening, at least temporarily. This effect goes away after the

[27] The distinction between Searching or not searching among temporary workers is important, as on the job search is extremely important, especially for individuals how do not consider their work to satisfactory or long term. If much temporary work is used as a means to short run subsistence, and perhaps to make money to search for other work, it is as interesting to look at job search in this group as those without work.

Table 6: Ordered logistic regression

	(1) All Weeks	(2) After Week 7	(3) Week 16 Only
Panel A: Effects across samples			
Effect for *boards*	0.20	0.42**	0.53***
	(0.14)	(0.18)	(0.19)
Effect for *city*	0.21	0.32*	0.30*
	(0.17)	(0.17)	(0.16)
Panel B: Effects in pooled Sample			
Pooled Effect	0.20*	0.37***	0.43***
	(0.11)	(0.12)	(0.13)
Obs (both panels)	5,011	2,202	658

Dep Var is a categorical variable: 1- Discouraged; 2- Temp work (no searching) 3- Searching (no work) 4 - Temp work (and searching); 5 - Permanent work. Log-odds coefficients are reported. All regressions include a full set of control variables. Standard errors are in parenthesis and are robust to correlation within clusters (70 Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

transport subsidies are removed.

I confirm that these trends are quantitatively significant. I employ an ordered logistic regression specified in equation 6 using the 5 job market outcomes are ranked ordinally in the order that they are presented.

$$P(j|X_{0i}, T_i) = P(\eta_j \leq y_i^\star \leq \eta_{j+1}) \tag{6}$$

where

$$y_i^\star = T_i\lambda + X_{0i}\beta + \epsilon_{it}$$

The results in Table 6 clearly show a statistically significant impact of the treatment on the ordered categorical variable, in the positive direction: away from discouragement to more job search and better jobs, for both samples. The effect is more pronounced for the *board* respondents.

5.2 Job Search Trajectories

How did the treatment impact the job search activity of recipients during the weeks that they were receiving it, and how did these impacts change over the course of the study? Did search intensity change change over time, and how and when did treated individuals diverge from the control group?

$$y_{it} = \alpha_t + T_i\lambda + X_{i0}\beta + \epsilon_{it} \qquad \forall t \neq 0 \tag{7}$$

$$y_{it} = \alpha_t + \sum_t T_i W_{it}\lambda_t + X_i\beta + \epsilon_{it} \tag{8}$$

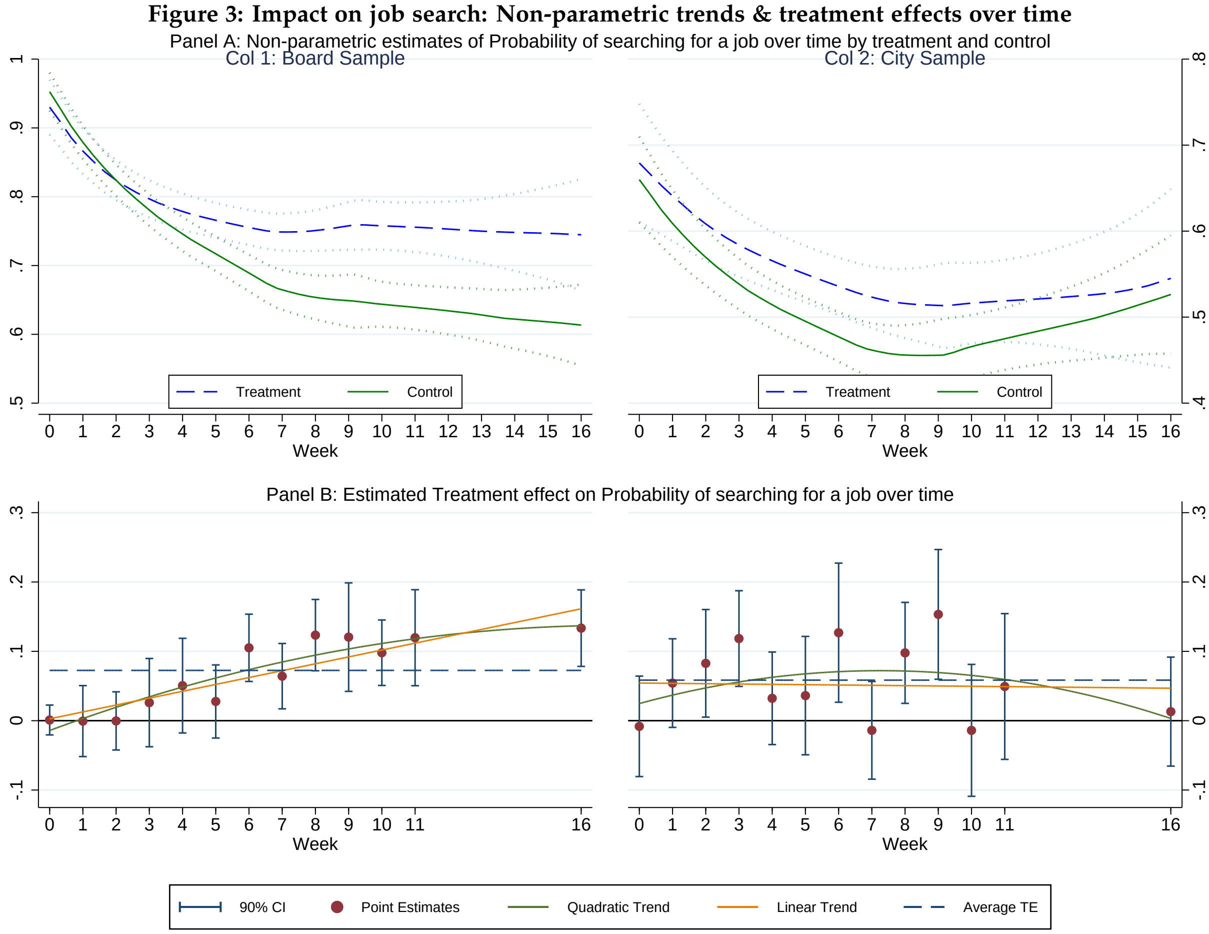
I begin by presenting estimates of the treatment effect on the propensity to search for work over time, looking at the 12 post-baseline surveys: 11 phone call surveys (denoted by week 1-11), and the final face-to-face survey (week 16). I estimate the average impact on the probability of searching for a job across all 12 weeks combined (equation 7). Using equation 8, I then estimate the treatment effect in each week separately.[28] I estimate the trend over time, estimating an intercept term, linear, quadratic and cubic trend terms, as in equation 9 and 10, below.

$$y_{it} = \alpha_t + T_i\lambda_0 + T_i w\lambda_1 + X_i\beta + \epsilon_{it} \tag{9}$$

$$y_{it} = \alpha_t + T_i\lambda_0 + T_i w\lambda_1 + T_i w^2\lambda_2 + X_i\beta + \epsilon_{it} \tag{10}$$

Figure 3 summarizes all of these results for both the *board* sample (Column 1) and the *city* sample (Column 2). In Panel A, non-parametric estimates of the probability of searching for employment as a function of time are presented. They show that search behaviour declined over time, as individuals either found employment or became discouraged and stopped searching for work. However, for both samples, the treated group clearly shows a different trajectory.

Figure 3: Impact on job search: Non-parametric trends & treatment effects over time



[28]In all specifications, "treatment" is defined as having received the transport subsidies as any point in the past, the treatment switches on in week 1, and does not "switch off". In later analysis, I exploit variation in when the subsidy treatment was ended for different individuals, and the fact that the treatments ended by at least week 11 for everyone (5 weeks before the follow up paper survey) to estimate the persistence of the treatment effects.

Table D.6 in the Appendix estimates (parametrically) these treatment effects week by week. I estimate one treatment effect coefficient for each week of phone data using equation 8. The control mean (CM) shows how the proportion of individuals searching for a job declined over time, but by considerably less for the treatment group, who were as much as 10% more likely to be searching in particular weeks of the study. I show results for the two samples pooled together, and for each separately.[29]

These weekly estimates of the impact of the treatment in each week are plotted in the Panel B of Figure 3, showing, for both samples separately, a clear upward trend in the treatment effect over time. For the *board* sample, these effects seem to increasing linearly with time, whereas for the *city* sample (in Column 2), these effects seem to have a more immediate effect at the beginning of the study, remaining constant with a decline towards the end (the effect is negligible in week 16).[30] Panel B also overlays the linear and quadratic estimates of the trend in the treatment effect over time.

The Appendix contains Tables that show the parameter values used in these plots. They show a significant linear trend for the *board* sample and a mostly constant effect *city* sample. The quadratic term for the *city* sample is negative, reflecting the decline in the effect in the final period, but is not statistically significant.

The results suggest unambiguously that individuals in both samples were more likely to search for jobs while receiving the transport subsidies, but the trajectory of these impacts differs slightly between the samples.[31] For the boards sample, who were initially more likely to be searching for employment, the impacts took some time to kick in, doing so only as individuals become discouraged. For the *city* respondents, the effect seems to have been more immediate, but less persistent through to the later weeks.

5.2.1 Search Methods

The nature of the transport subsidy, which required job seekers arrive at the centre of the city, also had an impact on the method of job search employed by job seekers. I find that the treatment had an impact on the probability of respondents searching for employment at the vacancy boards (see Figure C.6 and Table D.8 in the appendix). Again I find some heterogeneity by sample: the effects not as strong for the *city* sample, perhaps because the boards were never likely to be their preferred method of search.[32]

5.2.2 Search at the intensive or extensive margin

The evidence suggests that treatment did not induce job seekers to search more intensively during a given week.[33] The results presented in the Appendix Table D.11 show a positive impact of treatment on the average number of days spent searching for work (and days visiting

[29] Power is low for weekly-sample specific treatment effect estimates, so the pooled estimates more often statistically significant, but hide some heterogeneity between the two groups.

[30] I show, shortly, that this decline in search activity may be driven by these individuals finding better work.

[31] The impact on job search at week 16 shows that treated respondents were more likely to search *after* the subsidies ended. I return to this point in Section 7.

[32] Although there are individual weeks, early in the study, where the effect is significant, suggesting that the intervention "nudged" or at least encouraged respondents to try to check the boards, possibly with little tangible reward

[33] However, it is hard to be certain of this because the treatment induced an increase in search at the extensive margin. This may have induced job seekers who were likely to be relatively inactive job seekers to search, bringing down the average search intensity in the treatment group.

the boards), but these effects are driven entirely by the increase in proportion of individuals searching for work. Indeed, estimates not presented here, show that there was no significant impact in the number of days searched, conditional on an individual search at all.[34]

This suggests that the treatment had the effect of increasing search only at the extensive margin. This conforms with responses in qualitative interviews, that job search was only worthwhile if done for a few days a week. Checking the boards regularly, following up with applications, and responding to interviews required two or three days a week. Searching more was not adequate, doing more was subject to decreasing returns. Instead the treatment increased the probability that individuals decided to make that effort to undertake search for the week at all. This result is reflected in the assumptions of model developed in Section 6.

5.2.3 Effects on temporary employment

I find evidence that for the *board* sample, in intermediate weeks of the treatment period, there is a statistically significant drop in the proportion of individuals taking work (Figure C.7 and Table 7). There was no impact on the rates of finding permanent employment in these early weeks: the effect is driven completely by jobs in temporary casual or other informal sector work. These are jobs that do not last long, and the effect is only present for a few weeks, but provides strong evidence that active job seekers choose to reduce their supply to labour to temporary work because of the subsidies.[35]

These results suggest that temporary work acts as a source of cash while for job seekers looking for permanent work. They may do so to pay the costs of search directly, to smooth income while unemployed, or to generate buffer savings. Transport subsidies lower the financial burden of searching, and so reduce the need for job seekers to take temporary work. These issues are discussed in more detail in Section 6.4.1.

5.2.4 Impacts on search activities by Months

The small samples used to estimate the impact on job search in each week individually have limited power to detect significant treatment effects, despite the large coefficients estimated. In order to increase the precision of my estimates, I follow McKenzie (2012) and pool weekly observations together and estimate average effects for all weeks at once. I pool observations into sets of four consecutive weeks, or 3 successive *months*, which allows me to confirm the trajectories of the treatment effects, with considerably more power. Monthly results are presented in Table 7 using Specification 11:

$$\begin{aligned} y_{imt} &= \alpha_t + \sum_m T_i M_{mi} \lambda_m + X_{i0}\beta + \epsilon_{it} \\ y_{imt} &= \alpha_{st} + \sum_s \sum_m T_i M_{mi} S_{is} \lambda_{sm} + X_{i0}\beta + \epsilon_{it} \end{aligned} \tag{11}$$

The results emphasise the trajectories illustrated in the figures above. The treatment effects on search activity take some time to take effect, and grow over time for the *board* sample. The impacts are seen as early as the first month for the *city* sample, but seem to have diminished by

[34] Although, again, this could be because the individuals who were motivated to begin searching for work where ones that were not naturally inclined to search, and thus searched less when they were searching.

[35] For the *city* sample, there also seems to be evidence of an initial fall in employment rates, with a strong upward linear trend. However the negative impact on work for only one period may be an outlier and should not be interpreted with too much confidence.

the third month. For the final month, both samples are significantly less likely to be discouraged. For the *board* this is driven largely by increased search activity among the unemployed, for the *city* sample is a combination of increased search, and increased employment rates.

Table 7: Monthly impacts of treatment on main job market 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
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

To allay concerns that these months were chosen strategically to boost significance, I present complementary results where I *restrict* the sample to groups of four weeks, starting with the first four weeks of the study, and then iteratively move this window forward by one month. I re-estimate the treatment effects separately for each iteration. This provides a type of moving average monthly treatment effect, and clearly shows the trajectory of treatment effects. These estimates are shown for the pooled sample in Appendix Table D.13. These results confirm that the treatment starts to work slowly, and is strong and significant in the later weeks.

6 Theory

I develop a model which considers the consumption and job search decisions of an unemployed cash- and credit-constrained job seeker. 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 based 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 models describes the experience of an individual who is cash constrained, and who receives income from taking spells of temporary work in order to smooth consumption and earn money to pay search costs. In each week, he or she pursues a permanent job by choosing to pay a fixed monetary search cost.[36] The model incorporates search-on-the-job. Agents can search for work and have temporary employment at the same time.

The key intuition of the model is that a risk-averse job seeker with low levels of cash on hand finds it too risky to look for a job because the returns to search are low in expectation. The value of a permanent job is high, but probability of finding one is small: only 19% of my control group had permanent jobs after 15 weeks.

After describing the main features of the model, I proceed as follows: Firstly, I provide stylized solutions to the stationary version of the model. The key prediction of the baseline model is that job seekers find it optimal to not search below a critical value of savings, 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. This allows me to predict stylized dynamics of job search behaviour over time. I show that these predicted dynamics are consistent with those found in the data.

As well as providing an intuition for the descriptive trends observed in the data, the model is used to predict the effect of reducing the costs of search in the way that the experimental treatment did. I show that these predictions are consistent with the estimated treatment effects, in both the static and dynamic cases.

Thirdly, having described the key predictions that the model is able to produce, I summarize these predictions for a wide range of combinations of parameter choices. This shows that for a great number of plausible parameter values, the predictions of the model are consistent with the empirical results found in the paper. Finally, I generate new predictions using the model, which I test in detail in Section 7.

6.1 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. S/he 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

[36] 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.

costs of transport, photocopying, making applications and buying newspapers.[37]

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 in all future periods. Permanent employment is an absorbing state: these jobs cannot be lost. The permanently employed worker solves a kind of cake eating problem, augmented with a permanent income stream:

$$V(x) = \max_{0 \leq c \leq x+Y} u(c) + \beta V(x - c + Y) \tag{12}$$

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.[38] 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 jobs.[39]

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 solves:

$$F(x) = \max_{0 \leq c \leq x} u(c) + \beta(\theta U(x - c + W) + (1 - \theta)U(x - c)) \tag{13}$$

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.

The optimal consumption decision of course differs between searchers and non-searchers.[40] 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:

$$S(x) = \sigma V(x - p) + (1 - \sigma)F(x - p) \tag{14}$$

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

[37]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.

[38]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.

[39]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 from the decision to search.

[40]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.

searching, reflecting the job-seeker's decision to search at the beginning of each period.

$$U(x) = max\,\{S(x), F(x)\} \tag{15}$$

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 14. $F(x)$ 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:

$$\sigma(V(x^* - p) - F(x^*)) = (1-\sigma)(F(x^*) - F(x^* - p)) \tag{16}$$

The right hand side of this expression represents the expected benefit of searching and the left hand 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 4 illustrates this intuition. $S(x)$ falls quickly as savings get lower.

The model implies a steady state level of savings at x^* for most calibrations of the model. Below this level individuals 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 of searching. Define x^{*t} as the new critical value when the costs of search have been reduced permanently. 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. These dynamic implications are discussed in detail in Section 6.3.

This model cannot be solved analytically, because job seekers' optimal policy depends on wealth levels, which are in turn a function of their optimal decisions. I now turn to describe the numerical solutions of the model.

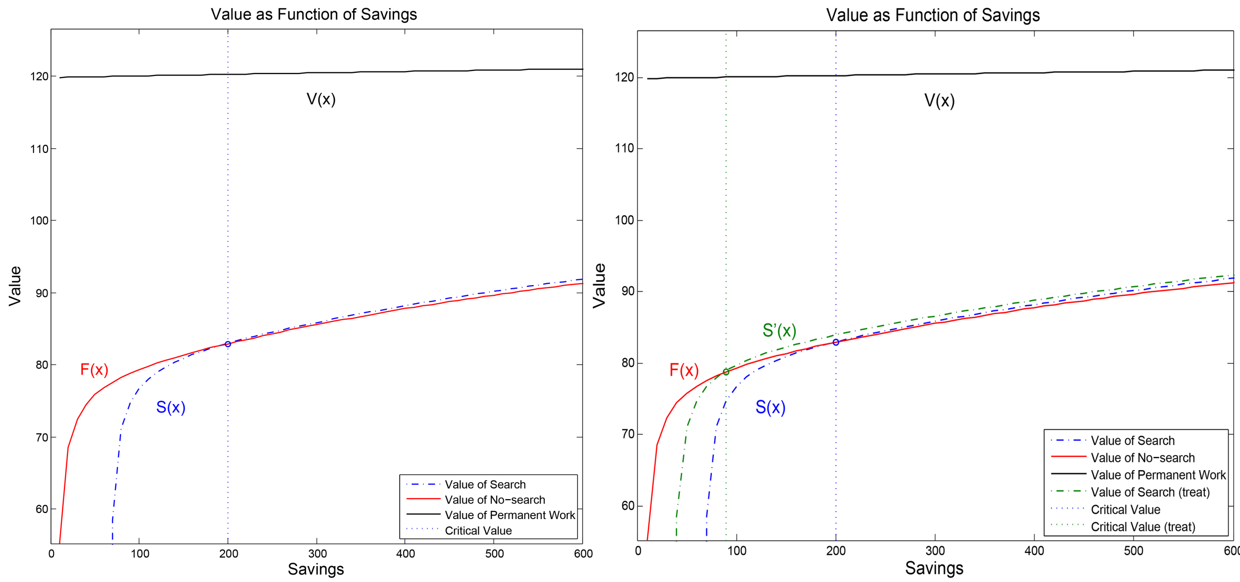
Note that by design the model can generate the 4 states of activities observed among individuals without permanent work in the data (see Figure 1): searching without working, searching and working, discouragement (not searching and not working), or working without searching.

6.2 Solution to the Stationary Model

In this section, I solve the model numerically.[41] This happens in two steps. First I estimate the value of of permanent employment, which is given simply by Expression 12. 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.

The left panel of Figure 4 shows the value for searching and not searching for different levels of savings, and the critical value above which it pays to search. I verify that the model implies

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



Left panel: baseline model. Right panel: Effect of lowered search costs (treatment)

Figure 4: Single Crossing Point of the Value of Searching and not Searching

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 $x = 200$. Figure C.10 in the Appendix shows the optimal consumption paths for an individual who is forced to search, and an individual who cannot search in the current period.[42]

I estimate the impact of *permanently* reducing the cost of search (usually by a half or a third) on x^*. I call this x^{*t}. I then also estimate x^{*s}, the critical value for the case when search costs are reduced for just one period, before it revests back to the original search costs, so that the continuation value of unemployment remains unchanged.[43]

I then estimate the proportion of individuals that would be induced to search by the change in the critical value, given the distribution of savings in the data. That is, the proportion of individuals with savings $x^{*s} < x < x^*$ at baseline. This gives a rough idea of the static impact of reduced searched costs due to the experimental treatment. See the right hand Panel of Figure 4: reducing the search costs for one period shifts the value of search to left, such that (in this example) everyone with savings $90 < x < 200$ are induced by treatment to start searching in that period.

I provide an illustrative example of how the model's predictions can be tested against the empirical findings presented in the paper before looking at the calibration results across a wide range of parameter choices.

[42]In this model consumption does not follow the 45 degree line at low levels of savings: because income is always uncertain, cash constrained individuals need to always be keep savings in hand in case no income is received in the following period.

[43]Unsurprisingly, x^{*s} is always (weakly) less than x^{*t}: people are more likely to search when the costs are search are set to rise again in the next period. However, the difference between the two solutions is usually relatively small. The treatment induced by this experiment reduced the cost of search temporarily, but for 12 weeks rather than one. Thus we might expect the effect in each week of this experiment to be somewhere between x^{*t} and x^{*s}.

6.2.1 Illustrative Example

Can the model's predictions of the impact on x^* be reconciled with the impact of the randomized subsidy on the probability of someone searching for work? For this example and all calibrations, I use the estimates from the data to parameterize wages, the cost of search and the average probability of finding permanent work. Table 8 show the chosen values for these parameters. Other parameters of the model are allowed to vary across calibrations. For this example, I set probability of finding informal work while unemployed to $\theta = 0.3$, and use utility given by a simple log-utility function, $u(x) = log(x)$, and a discount rate $\beta = 0.9$.

For these parameters a job seeker gives up searching when his/her savings falls below the critical value $x^* = 600$. This is 10 times the cost of search, and 50% more than a week's salary in a permanent job.

I simulate the effect of reducing the cost of search from 60 to 40 birr per week (a one third reduction). This changes the critical value to $x^{t*} = 360$. Reducing the cost of search for one period has the same effect $x^{s*} = 360$ in this simulation, although this is not the case in all calibrations.

In the baseline data savings are 874 at the average, with a standard deviation of about 1300, and a median of 400. Here 12.2% of the sample have savings between 360 and 600, and would be induced to search for one period according to the predictions of the model. This is not out of line with my experimental estimated treatment effect of 8% on the probability of searching for work.

6.3 Dynamic Simulations

The model can be used to predict, for a given starting level of savings, search, saving and consumption behaviour over time. I use these predictions to confirm the dynamics observed in the data. Most notably, the data shows that job seekers give up search over time after they start out searching, and that many job seekers oscillate between searching and not searching every few weeks. The proportion of individuals searching in the data stabilizes over time. In section B in the appendix, I discuss these patterns in greater detail.[44]

To show that the model replicates these patterns, I simulate job search behaviour and a series of random shocks for an individual starting with savings well above x^* and estimate the expected number of weeks it takes for that individual to run savings to below x^*. This is the number of weeks before an agent will switch from searching to not searching. I denote this as w^*. I then reestimate this value with search subsidy in place, and call this w^{*t}.

Secondly, I show that the model implies a steady state probability of searching for work in each period. I simulate an initial savings level for 1000 individuals, in such a way that it resembles the savings distribution in my sample at baseline. Further I simulate a series of random temporary employment shocks for each individual in each time period. Using the optimal consumption and search decisions from the solution to the calibrated model, I look at the evolution of job search and savings over time. Job seekers' savings converge to the critical x^*, above this level they search and run down their savings, below this they save up for search. This generates oscillating search behaviour from week to week. I find that the proportion of individuals searching stabilises around steady state given by s^*. s^{*t} gives the corresponding steady state proportion searching for work when the search costs are reduced.

[44] I do this using data from a new and enlarged phone call survey currently underway in Ethiopia.

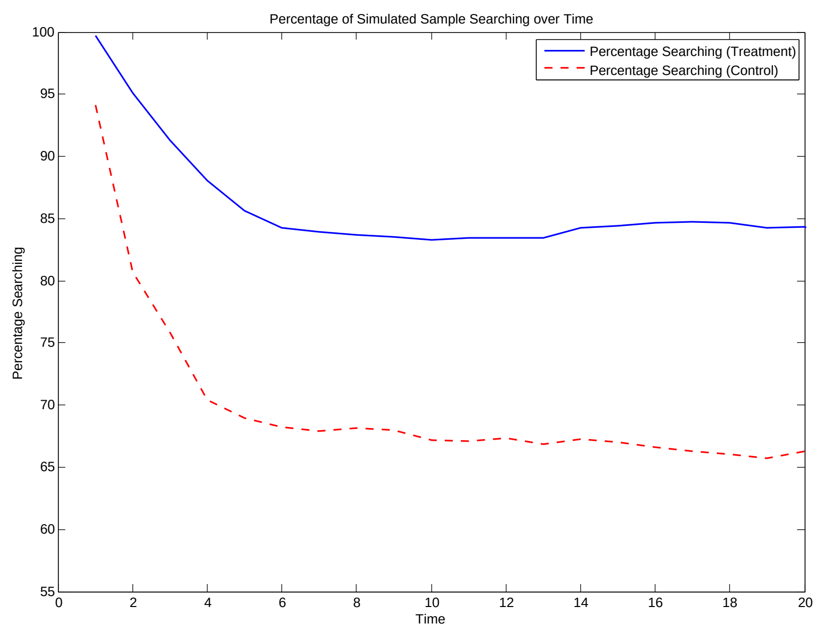
Treatment reduces the speed with which individuals run down savings, and increases the steady state proportion of individuals searching for work. Figure 5 shows a calibrated example of the proportion of the sample that are searching for work over time. These impacts on the dynamics of job search are often larger than those predicted by the static analysis.

6.3.1 Illustrative Example

For this example I use a representative agent with a log-utility function, $\theta = 0.3$, $\sigma = 0.03$, $\beta = 0.95$, and $p = 60$ (see Table 8 for a summary of the parameters). The critical value at which someone gives up search is $x^* = 600$ for this calibration. I then simulate initial savings for 1000 job seekers. Savings follow a logNormal distribution with mean 800 and variance 1000, which is closely resembles the distribution of savings at baseline in the data.

I generate time-series for each individual by simulate a series of income shocks and following the optimal consumption and search paths ascribed by the model. Figure 5 shows proportion of individuals in such a simulation searching for work over time, along with the corresponding trajectory for a treatment group for whom the costs of search are reduced by a third but initial savings are identical. Time series simulations of the model are consistent with the patterns observed in the data. The trajectories predicted in Figure 5 are similar to those estimated empirically in Figure 3 (Col 1). The calibration starts from Week 1, to correspond to the week in which subsidies started in the experiment.

Figure 5: Percentage of the sample searching for a job over time (simulation)



Almost all individuals start with savings above x^*, so that nearly 95% are searching for work in week 1. As a result the predicted *static* effect of reduced search costs in the first period is small. This mirrors the result from the experiment. However, job seekers who start out searching give up search over time. Someone starting with savings of 800 searches in every period to begin with. On average it takes such an individual 11 weeks to down savings below $x^* = 600$. When search costs are reduced by a third, this time taken to run down savings below x^* is extended to 19 weeks on average.

The model implies a steady state proportion of individuals searching for the treatment group that is significantly higher than the control group. In this calibration someone the control group searches about 68% of the time in steady-state. The treatment group is roughly 20% more likely to search in each period. This result is much larger than the effect implied by the static analysis.

Note that these effects on the steady state proportion of searchers are driven by two factors. Firstly there is the impact on the critical value of x^* which I have described in detail. Secondly the lower search costs lead to savings being run down slower. Even when an individual has savings close to the critical value this second factor is at play: an individual who waits for a positive income shock to increase savings above x^* can search for longer before savings dip below x^* again.

6.4 Calibrations

Here I repeat the analysis in the examples above for a range of parameter values. For each paramterization of the model I estimate the x^*, s^* and w^*.[45] I look at the effect of reducing search costs from p to p_t (60 to 40 Birr) on each of these outcomes. I show that the predictions of the model are broadly consistent with the data for a wide range of parameter values. The calibrations also illustrate interesting additional predictions of the model.

Table 8 shows the calibration choices. In the top panel I show the values that are constant across calibrations, in the lower the panel the range over which other parameters vary in the calibrations.

Table 8: 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 unemployment	(0.1-0.5)
β	Probability of offer of temporary work if unemployed	(0.8-0.99)
$u(c)$	Utility function: log/power utility	
δ	CRRA for power utility function	(1.2-2.8)
Solves for:		
x^*	Critical value of savings below which agent does not search	
w^*	Number of weeks it takes someone starting with x=1600 to run down savings to x^*	
s^*	Steady state value probability of searching in each week	

[45]These are the the critical value for savings, the steady-state proportion of job seekers, and the number of weeks that it takes for a job seeker to run down savings to x^* when starting with savings higher than x^*, respectively.

Table 10 presents the solution with different parameter values for β and θ for an agent with log-utility. The critical value decreases with risk aversion: unemployment individuals are more likely to search when the value future consumption more and thus are willing to sacrifice consumption in the short run for the chance to get a permanent job. The impact on x^* is slightly bigger when search costs are reduced for only one period, rather than permanently reduced ($x^{*t} \leq x^{*s}$), but this difference is usually small. As was the case in the illustrative examples, I calculate the % induced to search (Col 4) in the static case by looking at the proportion of my sample who had savings between x^{*t} and x^*.

Table 9: Patience & job search: Calibration of the main outcomes & simulated treatment effects
(Agent with log-utility. $p = 60$, $p_t = 40$, $Y = 400$, $W = 320$, $\sigma = 0.03$)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Statics				**Steady State**			**Discouragement**		
	x^*	x^{*t}	x^{*s}	induced	s^*	s^{*t}	s^{*t} -s^*	$\overline{w}$	$\overline{w}^t$	$\overline{w}^t - \overline{w}$
β	$\theta = 0.1$									
0.8	1140	620	620	12.2%	0.0%	1.5%	1.5%	4.27	6.66	2.39
0.9	700	420	400	10.0%	2.2%	14.8%	12.6%	8.03	12.97	4.94
0.95	480	320	300	7.6%	15.0%	33.7%	18.6%	15.05	21.66	6.61
0.99	280	220	180	9.6%	40.3%	61.8%	21.5%	30.09	43.94	13.85
β	$\theta = 0.3$									
0.8	1200	600	600	12.5%	0.0%	11.3%	11.3%	3.22	6.55	3.33
0.9	600	360	360	12.2%	19.4%	52.8%	33.5%	8.85	13.91	5.06
0.95	400	260	240	5.2%	53.1%	76.5%	23.3%	17.68	25.8	8.12
0.99	240	180	180	8.6%	84.7%	93.8%	9.1%	40.45	43.4	2.95
β	$\theta = 0.4$									
0.8	1280	600	600	13.3%	0.0%	14.9%	14.9%	3.41	6.59	3.18
0.9	620	360	340	14.1%	25.8%	61.2%	35.4%	8.64	13.13	4.49
0.95	380	260	240	5.0%	66.1%	83.3%	17.2%	18.84	30.21	11.37
0.99	240	180	160	8.8%	91.7%	96.3%	4.6%	41.26	45.1	3.84

In Table 10 I look at the effect of risk aversion on the results. I use power-utility, with different values for the constant rate of risk aversion δ. 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.

High rates of risk aversion are not needed to rationalize my results. In fact, calibrations with relatively low risk aversion parameters are more consistent with the job search behaviour and treatment effects estimated in the data. The parameters that most closely matches the empirical results are $\theta = 0.4$ and $\delta = 1.2$. The calibrations of the model are consistent with the estimated treatment effects in the data.

In these calibrations changes in the critical value x^* is highly sensitive the cost of search. For almost all parameter values the results are qualitatively similar to the results in the paper. Even for very low rates of risk aversion and impatient consumers, a significant proportion of individuals can be induced to search more, and for longer, by lower search costs.

In fact, the predicted treatment effects in the model are considerably larger than the treatment effect found in the paper, for a large number of calibrations. Of course, the model seeks to explain the behaviour of a marginal consumer for whom search costs are salient. The subsidies were not likely to be useful to all job seekers. In fact, on an average week, between 40% and 50% of the subsidies were collected. This could explain why the model predicts treatment effects that

Table 10: Risk aversion & job search: Calibration of the main outcomes & simulated treatment effects
(Agent with power utility function. $\beta = 0.95$, $p = 60$, $p_t = 40$, $Y = 400$, $W = 320$, $\sigma = 0.03$)

	x^*	x^{*t}	x^{*s}	induced	s^*	s^{*t}	s^{*t} -s^*	$\overline{w}$	$\overline{w}^t$	$\overline{w}^t - \overline{w}$
δ					$\theta = 0.1$					
1.2	640	440	400	9.0%	10.2%	23.8%	13.6%	12.9	20.89	7.99
1.4	840	560	540	8.0%	5.2%	19.0%	13.9%	10.94	18.29	7.35
1.8	1280	860	820	8.2%	1.5%	10.6%	9.1%	5.68	12.75	7.07
2	1500	1020	980	8.0%	0.5%	7.7%	7.3%	3.51	11.33	7.82
2.4	1820	1340	1320	2.2%	0.3%	2.8%	2.5%	1	5.85	4.85
2.8	1940	1660	1620	0.8%	0.3%	1.1%	0.8%	1	1	0
δ					$\theta = 0.3$					
1.2	520	340	320	11.6%	44.4%	71.0%	26.6%	15.75	25	9.25
1.4	640	440	400	9.0%	38.2%	64.3%	26.2%	13.82	23.74	9.92
1.8	920	640	580	8.0%	23.9%	54.2%	30.3%	10.61	20.26	9.65
2	1060	740	680	10.4%	19.3%	48.5%	29.2%	8.26	19.3	11.04
2.4	1360	960	900	7.6%	10.9%	37.0%	26.2%	5.99	15.06	9.07
2.8	1640	1180	1120	3.4%	6.7%	29.1%	22.3%	1	10.07	9.07
δ					$\theta = 0.4$					
1.2	480	320	300	7.6%	59.1%	80.6%	21.5%	18.07	27.31	9.24
1.4	600	400	380	12.0%	49.8%	76.7%	26.9%	14.42	24.62	10.2
1.8	820	580	540	7.6%	37.9%	66.3%	28.4%	12.36	22.39	10.03
2	960	680	620	6.6%	30.1%	60.8%	30.7%	10.47	19.91	9.44
2.4	1220	860	800	8.4%	20.5%	53.1%	32.6%	6.19	18.04	11.85
2.8	1480	1060	1000	4.0%	13.2%	44.2%	31.0%	4.77	14.26	9.49

are larger than the estimated effects, at least for high levels of risk aversion.[46] In this sense the model can be said to rationalize the large treatment effects induced by a small change in the cost of search.

6.4.1 Temporary work and job search

An important result from the calibrations deserves special mention: increasing the probability of getting temporary work (θ) while unemployed actually increases the probability that an unemployed person will search for work. This is a counter-intuitive finding, since the value of unemployment has been increased. In most job search models, this would be predicted to reduce incentives to find permanent work more quickly (Chetty, 2008). However, in this model temporary work provides income that allows for job search. Forward looking unemployed agents are more likely to pay the cost of job search for a given level of savings because search is less risky. For lower values of θ, search is made risky by the lower probability of getting income after that search decision has been made.

In Table 11 I look at the case with power utility, and look the predictions of the model for a range of values for θ, and for δ. The critical value decreases with the value of θ in these calibrations, except for when $\theta = 1$ and very high risk aversion.[47]

This result has important implications. It suggests that job seekers rely on temporary work

[46] In addition, it may be the case that I have over- or under-estimated the extent to which transport influence the cost of search. In calibrations performed here the treatment reduces the cost of search from 60 to 40 birr (the average subsidy amount was between 20 and 30 birr per week).

[47] Only in cases where θ is very close to 1 do job seekers become relatively indifferent between unemployment and permanent work, and reduce search effort. Here permanent work is still valuable when $\theta = 1$ because permanent work pays more than temporary work. When $Y = W$, $\theta = 1$, of course, no one searches for work for any calibration since permanent work is not preferred in any way.

Table 11: Risk and job search: Solution for x^*
(Power utility with $p = 60$, $\beta = 0.98$, $\sigma = 0.03$)

	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

for money to search for work, and as a method to deal with risk while unemployed. The need to take temporary work seems symptomatic of the cash constraints that job seekers face.

The model assumes that job search and temporary work are non-rival activities in terms of the time of the unemployed. However, in reality taking temporary work could seriously interfere with a job seekers ability to search for work. This would drive the result in the paper that the subsidies allow job seekers to take *less* temporary to concentrate on search.[48] In this setting an appropriate policy response, if subsidizing search directly were not feasible would be provide money to search for work directly.

I simulate the effect of pure unemployment insurance. That is I set $\theta = 1$, but income while unemployed (now interpreted as indemnity payments rather than wages from temporary work) low at $W = 60 = p$. The model predicts a huge increase in the proportion of individuals induced to search. This suggests that job search could be improved by providing a small income stream to remove the risks associated with unemployment.

6.5 The probability of finding permanent work

In this section I argue that the increases in permanent employment induced by the transport subsidies could plausibly have been induced by the increased rates of job search predicted by the model and estimated from the experiment. The model assumes that paying for job search leads to a constant probability of finding work. In reality the mechanism I have in mind is that increased job search leads to more information about good jobs, more applications to good jobs, and increased probability of receiving and accepting a job offer.

On average, for the *board* sample, the probability of finding a permanent job for *each week* of search is about 2.5 percent (leading to a 19% probability of permanent employment at endline) for the control group. In the model I use calibrations with $\sigma = 0.03$. In each period, treated individuals are more likely to be searching for work by about 8% for every week, over 16 weeks to endline, relative to a control mean of 65%. This implies that the treatment increases the average number of weeks of search by about 1.5 weeks over a control mean of about 9 weeks of search. It also leads to about 1.7 additional weeks of visiting the job boards. If returns to search are linearly and constantly related to the number of search activities, these averages would explain about a 2.9 percentage point increase in the probability of finding a job. The actual effect is estimated to be about 7 percentage points. This seems to inadequately capture the size of the

[48]The Ethiopian government has spent a lot of money on employment programs that have engaged the unemployed youth in forms of unskilled casual work, such as acting as parking attendants or building cobble-stoned streets. The results in this paper argue that for some responds searching for temporary work, these forms of work may provide some support, but that the funds may be better spent allowing job seekers to search, or by subsidizing search directly.

treatment effect found here.

However, I would argue that the returns to search exhibit non-linearities in the time spent searching. I find that the combined effect of the treatment on search across all weeks is that treated respondents are far more likely to have searched during *all* weeks by a very large margin. Among the board sample, treated respondents are about 50% more likely to have search in all (or all but one) weeks of the study. The median number of trips to the boards among treated individuals is 18 compared to 12 in the control group, for the full sample.

If noticing that one job vacancy that others missed makes all the difference, or an application is most likely to be successful if one has already made a few applications in recent weeks, then the marginal returns to search are highest when search is sustained and persistent over weeks. Then shifting the distribution of search persistence at the right tail, as the treatment did, is likely to have a larger impact than the impact on the mean would suggest, which would explain the 7% treatment effect on permanent work estimated in the paper.

6.6 Summary

The model generates the following results:

Risk Aversion: Job seekers give up search when savings fall below a critical value x^*, which is usually many multiples larger than the cost of search itself.

Discouragement: Job seekers who start with enough income to search ($x > x^*$) give up search quickly when the costs of search are high. Reduced search costs lead to savings being run down more slowly.

Persistence: Because savings are run down more slowly the model predicts that the treatment effects on search should be persistent for individuals who have not run down their savings to the steady state by the end of the experiment period. The keep buffer savings that allow them to search for longer.

Volatility: Individuals who have savings close to x^* alternative between searching and not searching, a pattern confirmed in the data. They save up to search and then run down their savings again when they search. In the model, subsidies lead to more weeks searching when individuals are at this steady state.

Temporary Work: Rather than discourage job search by making unemployment more attractive, improving the probability of temporary work facilitates job search and makes it more likely that an agent will search for a given level of savings.

Heterogeneity: The treatment effects should be larger for individuals who are more cash constrained at the baseline. Individuals who are very wealthy to start with, or receive income from their family, should not be constrained by the costs of search.

7 Mechanisms & Persistence

In this section, I provide further experimental evidence to corroborate the intuition and predictions of the theoretical model. I also investigate some of the possible alternative explanations for the results, such as effects on aspirations, motivation and learning about job search. I find no convincing evidence for any of these alternative explanations.

Estimation of heterogenous treatment effects show that individuals who came from poorer households, or had lower levels of savings at the start of the subsidy period, had significantly larger responses to treatment than wealthier individuals. In addition I confirm that the treatment effects were persistent, in the short run, after the subsidies had ended: confirming the prediction of the model that the treatment could allow job seekers to maintain their savings and thus search for longer even after the treatment ended.

The transport treatment, and the initial increase in job search among respondents, could have had a range of additional effects other than the price and income effects described here. The treatment may have changed the information set of respondents, by alerting them to the existence of the job boards. I argue that this unlikely, and find no statistical evidence for changes in respondents perceptions or information about search. The regular phone calls could have induced additional search by acting as reminders to search. I use a control group who received no phone calls to show that there is no evidence of Hawthorne effects of this kind. Similarly, I find no impact on reservation wages, attitudes or aspirations. I argue that while some of these alternative mechanisms may have been at play, the evidence points to the cash constraints story as the dominant explanation, for which there is the strongest evidence.

7.1 Heterogeneous Treatment Effects

I estimate differential treatment effects for individuals above and below median household wealth, expenditure and savings, and as in equation 3, and test for equality of these coefficients. The model would predict that wealthier individuals should not be effected by the subsidies. They receive enough income from their families to always have enough wealth to keep searching. In other words they'd be above the critical level x^* implied by the model. Poorer individuals for whom the costs of search are more salient, should benefit more.

For the *board* sample, who were more likely to respond to the treatment, I find clear evidence that poorer individuals benefited more from the treatment than wealthier ones. Individuals from poorer households were more likely to find permanent jobs, or any work at all, after receiving subsidies, while individuals with low savings at baseline saw a disproportionately large impact on the probability of being discouraged at endline. The effects on poorer households are large and significant, although I am not always able to reject the test of equality between the two groups in such small sub-samples.

The results for the *city* sample are less clear. I look baseline expenditure as a proxy for cash constraints, I find that those with low expenditure and savings seem to have benefited more from the treatment, they are particular less likely to be discouraged. The results by household wealth are not as strong.[49]

According to the model, poorer individuals should be more effected by the price of search. In Table 13 I show that the average impact on search, across all weeks, is higher for poorer individuals and the F-stat on the different between coefficients shows that this difference is significant at the 10% level. The impact on richer households is not significant on its own, suggesting that the constraints of job search do not bind for these individuals.

[49] However for this sample the household wealth measure may not be the best for measuring cash constraints: individuals who were living at home with their parents were more likely to appear wealthy, whereas individuals who were living alone may actually have been more cash constrained. To the extent that the ability to find a good job might be correlated with wealth and background, the analysis here could be confounded.

Table 12: **Heterogeneous effects on endline outcomes by respondent wealth**

	Board Sample			City Sample		
	(1) work perm	(2) work	(3) discouraged	(4) work perm	(5) work	(6) discouraged
Heterogeneous Treatment Effects by Household Wealth Index (Above/Below Median)						
poor hh	0.13**	0.12*	-0.002	-0.044	-0.027	0.005
	(0.060)	(0.062)	(0.043)	(0.035)	(0.064)	(0.063)
not poor hh	-0.008	-0.110	-0.074	0.017	0.19**	-0.20**
	(0.078)	(0.10)	(0.051)	(0.054)	(0.067)	(0.069)
R^2	0.085	0.088	0.038	0.076	0.086	0.108
Equality F-stat	1.320	3.460	1.150	1.040	5.300	5.160
Equality p-val	0.260	0.068	0.290	0.330	0.038	0.041
Heterogeneous Treatment Effects by Savings at Baseline (Above/Below Median)						
low savings	0.092**	0.027	-0.027	-0.005	0.074	-0.13**
	(0.044)	(0.064)	(0.038)	(0.032)	(0.064)	(0.057)
not low savings	0.029	0.091	-0.009	-0.030	0.098	0.006
	(0.092)	(0.12)	(0.063)	(0.075)	(0.13)	(0.11)
R^2	0.084	0.075	0.039	0.064	0.074	0.097
Equality F-stat	0.340	0.220	0.074	0.120	0.021	1.160
Equality p-val	0.560	0.640	0.790	0.730	0.890	0.300
Heterogeneous Treatment Effects by Expenditure at Baseline (Above/Below Median)						
low exp	0.074	0.064	-0.100***	-0.028	0.110	-0.16**
	(0.057)	(0.081)	(0.035)	(0.040)	(0.068)	(0.070)
not low exp	0.078	-0.004	0.047	0.019	0.026	0.014
	(0.067)	(0.078)	(0.059)	(0.044)	(0.097)	(0.074)
R^2	0.082	0.081	0.055	0.063	0.097	0.103
Equality F-stat	0.002	0.310	4.150	0.860	0.390	2.480
Equality p-val	0.970	0.580	0.046	0.370	0.540	0.140
Observations	368	369	369	289	289	289

[1] 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

7.1.1 Effects by unemployment duration

The model presented in this paper is stationary- the length of time that a person has been employed or searching for work does not have any effect on their future labour decisions, for a given level of savings. However, there are many ways that unemployment history could matter. If individuals with strong labour market attachment, who have been out of work for a shorter period of time, have more job contacts or think that they may be rehired by their industry, they may have less need for the job boards and official applications as a means of search. Therefore an individual with weaker ties to the labour market would benefit more from the treatment. On the other hand, an individual who has been discouraged from search by months of unemployed might be less motivated and thus respond less to treatment.

I categorize individuals as having been employed for a "long duration" if they have spent more than 4 months without work.[50] I find that those who had not worked in more than 4 months benefited far from the treatment than those who had spent less time out of the labour force. These results are in Appendix Table D.14. This seems to be the case for both samples, although the results are more striking for the *board* sample. This fits with the story that individuals with relatively weak labour market attachment rely the most on active (costly) job search,

[50]If they have not worked before, this means they haven't worked since graduating

Table 13: **Heterogeneous effects on job search trajectories by respondent household wealth (Board sample)**

	(1) searchnow	(2) searchnow	(3) searchnow
Treat*Poor HH	0.075***	-0.0097	-0.040
	(0.021)	(0.035)	(0.045)
Treat*Poor HH*Time		0.014**	0.036*
		(0.0056)	(0.020)
Treat*Poor HH*Timesq			-0.0021
			(0.0019)
Treat*Noot Poor HH	0.019	-0.014	0.024
	(0.025)	(0.045)	(0.058)
Treat*Not Poor HH*Time		0.0085	-0.018
		(0.0071)	(0.026)
Treat*Not Poor HH*Timesq			0.0025
			(0.0023)
Observations	2,768	2,768	2,768
R-squared	0.778	0.784	0.784
Equality F-stat	3.04	0.42	2.35
Equality p-val	0.081	0.52	0.13

[1] Results are from OLS regressions on endline outcomes. 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

and stand to gain the most from subsidies.

7.1.2 Effects by Education

Table D.15 in the Appendix looks at heterogenous treatment effects by education. While I find no evidence of differential impacts on search, education is crucial for the margin at which job seekers can improve their employment outcomes. Individuals with degrees are most likely to find permanent jobs, these jobs are out of reach for individuals with post-secondary education. The treatment has a large impact on the probability of finding permanent work for high educated individuals.

Individuals with diplomas seem to be have fared worse than those with degrees. They have some chance of finding good permanent jobs, but who may find it more difficult, or take longer to find these jobs. The treatment does not lead to higher employment rates for this group, but they are more likely to still be searching at endline. I find large impacts on finding jobs among individuals with only primary school, but these are not permanent jobs.[51]

7.2 Persistence

In this Section I test for whether the treatment effect of transport subsidies persisted after the subsidies were removed. The end of subsidy date differed from individual to individual by randomize design. If the treatment effects were due purely to a change in the relative price of search, we would expect the effects is dissipate immediately after the subsidies end. However the model of Section 6 predicts that treatment prevents individuals from running down their

[51] They do seem to be better, more formal jobs, in areas further away from respondents' place of living.

savings, and thus could leave some individuals in a position to keep searching after the subsidies end. This would lead to persistent treatment effects.

In the Table 14, I replicate the week 16 (endline) specific treatment effect in Panel A, to show that the effect on search is still present 5 weeks after the treatment ended.

Table 14: Persistence of treatment effects after subsidies have ended

	(1) work	(2) work perm	(3) searchnow	(4) searchboards	(5) discouraged	(6) days search
Panel A: Average Impacts at Follow up Survey (Week 16 only)						
Treated ever	0.061*	0.040	0.076*	0.068	-0.051*	0.042
	(0.034)	(0.026)	(0.041)	(0.044)	(0.029)	(0.14)
R-squared	0.065	0.081	0.059	0.084	0.073	0.075
Panel A: Average Impacts over weeks 8 to 12						
Treated now	-0.050	0.002	-0.022	-0.006	0.024	-0.200
	(0.047)	(0.027)	(0.047)	(0.040)	(0.029)	(0.20)
Treated ever	0.061	0.006	0.10***	0.085**	-0.082***	0.37**
	(0.040)	(0.025)	(0.037)	(0.040)	(0.023)	(0.17)
R-squared	0.059	0.099	0.066	0.130	0.090	0.046
Panel C: Heterogenous Impacts at Follow up Survey (Week 16 only)						
Treated ever board	0.043	0.080**	0.12**	0.094	-0.019	0.200
	(0.051)	(0.037)	(0.054)	(0.069)	(0.033)	(0.17)
Treated ever city	0.087*	-0.005	0.023	0.045	-0.096*	-0.140
	(0.044)	(0.033)	(0.064)	(0.046)	(0.050)	(0.22)
R-squared	0.067	0.095	0.063	0.107	0.082	0.077
Panel D: Heterogenous Impacts over weeks 8 to 12						
Treated now board	-0.022	-0.011	-0.066	-0.017	0.021	-0.410
	(0.063)	(0.046)	(0.051)	(0.062)	(0.028)	(0.30)
Treated now city	-0.076	0.022	0.030	-0.010	0.029	0.038
	(0.076)	(0.018)	(0.087)	(0.051)	(0.052)	(0.24)
Treated ever board	0.023	0.028	0.15***	0.13**	-0.066***	0.66***
	(0.054)	(0.040)	(0.048)	(0.059)	(0.023)	(0.24)
Treated ever city	0.11*	-0.023	0.039	0.034	-0.100**	0.010
	(0.058)	(0.024)	(0.056)	(0.053)	(0.043)	(0.21)
R-squared	0.062	0.110	0.076	0.178	0.095	0.059
Observations	2202	2202	2202	2202	2202	2202

[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 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
[4] Two types of treatment effects are presented: "on" denotes having received the treatment at any time in the past or currently. "here" indicates the impact of the treatment being available in that specific week.

Next I exploit the randomized variation in when treatment ended, with some individuals stopping the program in week 8, three weeks before the others ended it. In each week 9-11 I can compare those who were still receiving treatment to those who had finished it. I create a dummy variable P_{it} equal to one only if participant i was eligible to receive the treatment in the week t. Once the treatment period ended for an individual, this treatment variable "switches off", while T_{it} stays on. In estimates presented here I estimate the impact on T_{it} as the treatment effect of *ever* receiving treatment compared to P_{it}, the effect of receiving subsidies *now*, in week

t.

$$y_{it} = \alpha_t + T_i\lambda + P_{it}\delta + X_{i0}\beta + \epsilon_{it} \qquad\qquad \forall t \geq 8 \tag{17}$$

Panel B represent the results from Equation 17. The coefficient on *Treat Ever* (λ) is large and highly significant while that on *Treat now* (δ) is not. This shows that there is no additional effect of having the treatment in current period, for the last few weeks of the study (weeks 9, 10 and 11). Individuals who have stopped the treatment go on searching as intensely as those who are still receiving it. This shows that the treatment effects are significant, as predicted by the model of cash constraints. These results hold when the results are estimated for the two sample separately, as presented in Panels C and D.

7.3 Alternative Explanations

The results presented thus far are consistent with a story of credit constraints preventing poor job seekers from being able to invest in job search at an individually optimal level. The effect of the treatment works by lowering the cost of search relative to remaining unemployed, allowing job seekers to take the risk of searching when they are lowing on savings, and thus allowing them to retain savings to search for longer.

The evidence that the treatment effects were persistent after the end of the subsidies would seem to support this theory. However, there are a number of alternative explanations that could be driving these results that deserve further investigation. This Section will consider some of these competing explanations for the results in this paper, not just for the results on the persistence on the impacts, but also explanations other than those related to the price of search, such as those involving learning, aspirations or beliefs that might be driving the results.

The transport subsidy may have increased job search in the short run simply by nudging, or hinting to respondents that they should search, or reminding them to do so on a regular basis. Then, if a short period of search lead to learning about the nature of job search, or more information about the availability of jobs, or the salaries that they pay, this could lead to respondents searching more after the end of the subsidies because of changes in their information set. Searching more may make someone a "better" job seeker, because they learn how to go about doing it.

Further, since the treatment reduced participation in temporary work during the weeks of the study, it could be the case that individuals were less likely to get "stuck" in temporary jobs, that were hard to leave because of the short run benefits of the pay that they offered. This fits into a category of "scarring" explanations. The treatment may have prevented discouragement by keeping job seekers looking for work for slightly longer. If behaviour is persistent, and discouragement is a mental state that is hard to break out of, this would lead to treated individuals searching more after the end of the treatment period.

The decision to search for a permanent job is one not taken lightly. It is time consuming, and involves a certain fixed cost in getting acquainted with the market, preparing a CV and applications, and keeping up with vacancies, possibly while freeing oneself up from other work obligations, such as in temporary employment. Treated individuals may have built up that critical level of search intensity, that could carry through into later periods.

7.3.1 Hawthorne Effects

Could the weekly phone calls that were given to all treated individuals (and half of the control group) be causing improved job outcomes? The phone calls might have primed individuals to think that search was a worthwhile activity, or offered a higher promise of employment than it actually did, or just induced shame in respondents for not searching. Furthermore, it may have induced false reporting at endline because of the pressure of being asked questions so often.

This experiment was designed to test this mechanism, by randomly assigning half of the control group to not get the phone calls. The main results in this paper are robust to pooling this split control group together or looking at each separately.[52]

If the individuals who were not given subsidies but were called experienced better employment outcomes than those were not contacted at all, this would be cause for concern. It would suggest that the calls had some independent impact on job search that is confounding the main results. It test for an impact of the phone calls by looking at endline outcomes, with data for both individuals who were called during the study, and those who were not.

Table 15: 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					
TE trans	0.096**	0.081	-0.059*	0.053	0.034
	(0.048)	(0.055)	(0.030)	(0.045)	(0.033)
TE call	-0.029	0.00085	0.011	0.011	-0.010
	(0.049)	(0.047)	(0.044)	(0.053)	(0.035)
Panel B: Average Impacts at Endline by Sample					
TE trans boards	0.13*	0.10	-0.050	0.060	0.10**
	(0.072)	(0.093)	(0.044)	(0.059)	(0.046)
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	-0.0037	0.0072	0.043	-0.028	-0.067
	(0.069)	(0.075)	(0.044)	(0.064)	(0.055)
TE call city	-0.071	-0.012	-0.035	0.064	0.057*
	(0.072)	(0.052)	(0.087)	(0.087)	(0.029)
Obs	658	658	658	658	657

[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 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

In Table 15 I estimate the coefficient on a dummy variable indicating that respondents had received the phone calls. This coefficient estimates the impact of the calls, while the usual coefficient on "trans" estimates the *additional* impact of receiving the subsidies, over and above the effect of the calls. I find that, at endline, there are few if any statistically significant differences between those with phone calls to those who did not receive them, across a range of specifications. The impact of the subsidies is still large and significant. In this regression, the coefficient on subsidies compares individuals who received the subsidies to individuals who were called

[52]For the search trajectories, of course, we do not have choice: the impacts are estimated only by looking at the sampe who were called.

(it does not include those were not called).[53]

7.3.2 Reservation Wages

Standard search-theoretic models in which reservation wages are endogenous, would be ambiguous about the impact of subsidies on unemployment durations. Lowered searched costs could lower reservation wages thus increasing time in unemployment. This reservation wage model is not entirely applicable to Ethiopia. While respondents may be rejecting job offers, they are far more likely to be doing so the basis of the type of work offered. Specifically, they are after more prestigious, stable jobs.

The result in this paper are consistent with individuals holding out for better quality, permanent, jobs, but I find no impact of the subsidies on reservation wages. Figure C.9 shows no significant change in reservation wages throughout the survey, nor at the endline survey. There is an increase in reservation wages for the treated *boards* individuals of about 6 percentage points at the endline (week 16), but the increase is not statistically significant, even when the sample is restricted to those who who haven't found work. The size of the effect is consistent with the difference in wages between permanent and temporary jobs, suggesting that job seekers decided to pursue those jobs, and adjusted their wages up accordingly.

I do find a significant negative impact on perceptions of average and fair market wages, among the *city* sample. Job seekers in this sample had over-inflated expectations about the wages that they could earn in the market.[54] This result is consistent with the unemployed learning about the true value of jobs through additional search. I find no other effects on other variables such as optimism, expectations about the probability of finding work, or general well-being.

8 Conclusion

This paper looks at the impact of high search costs on labour market outcomes for cash constrained youth in Addis Ababa, Ethiopia. The job market in this city is characterized by high levels of unemployment, and a growing supply of labour wanting to work in those professions. This growth has been driven by rapid urbanization as well as the the enormous expansion of the secondary and tertiary education system in Ethiopia.

The labour market is plagued by search frictions. Gathering information about job vacancies and applying for those vacancies is time consuming and expensive. But the costs are particularly high for finding the highly sought after jobs that are in short supply. These are the permanent jobs that are found predominantly at the job boards near the center of town. Job seekers must make decisions between paying the costs to search for the jobs that they really want, or taking temporary working that is more easily available closer to home.

I test whether these high costs of job search cause poor labour market outcomes for disadvantaged youth living in particularly dislocated parts of the city. A randomly selected group of

[53]One competing hypothesis could still explain these results; which is that phone calls, in combination with the transport subsidies, together induced the transport group to search more intensively, but without the phone calls, the transport treatment alone could induce increased search effort. I cannot reject this outright, since budgetary and sample constraints prevented me from assigning some individuals to a transport treatment group, without the phone call.

[54]respondents said they expected to earn about 1500 birr on average at baseline, when in reality, those that found jobs earned little over 1000. There was no such discrepancy for the *board* sample, who seemed to understand the market better to begin with.

individuals were given a weekly transport subsidy covering the costs of two return trips from their place of living in around Addis Ababa to the center of town where the vacancy information boards are located.

My split sample approach allows me to compare how job seekers with different backgrounds, looking for different types of jobs, respond differently to reduced job search costs. The *board sample* is comprised of active job seekers, often of high educational attainment, surveyed in areas around the vacancy boards where they were searching for work. These are respondents who are most likely to self-select into any youth employment programme initiated by government or NGOs.

Four months after participants were first surveyed, individuals in *both* samples receiving the transport money are positively impacted in their labour market outcomes, but these impacts differ across the samples, in line with the types of work available to different types of job seekers. I show that *board* sample participants were more likely to find permanent work, particularly in the professions they want to work in, while those in *city* sample are more likely to be working generally, and the work they are doing tends to more formal and less likely to be part time, or casual. Furthermore the transport subsidies increase job search intensity, for those with and without work, throughout the study.

This paper supports the hypothesis that labour market frictions are constraining the ability of the young and unemployed to enter the labour market. "Flattening" spatial distance seems to have improved their access to employment opportunities that might otherwise have been denied to them, as a direct result of their place of living and financial constraints.

The results suggest that labour markets could be made more efficient, as well as accessible and equitable, to a growing and aspirant urban population by policies that reduce the costs of finding work. This could be done either through improved and subsidized transport for the poor, or more direct measures to make access to information about vacancies and employers more readily available such as online or mobile-phone based matching services or job search assistance programs. The paper also highlights the vulnerability of the youth while in unemployment, and suggests that access to more reliable sources of income could significantly ease the transition from school to work.

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Appendix A What are jobs like, and who finds them?

Over the 16 weeks that I follow my survey of job seekers I observe enormous changes in their lives and job market outcomes.

Of the 658 individuals interviewed in the endline survey, 359 of them were working, compared to only 168 of those individuals at the baseline survey just 16 weeks earlier. Only 183 of the individuals working at the endline had ever held a any kind of job before in their lives. And among the 186 individuals working at baseline (28% of the sample) less than one third kept those jobs through to the endline 4 months later. A similar proportion were no longer working at the endline, and the remaining 40% of were working in different jobs. Half of those who weren't initially working, had found work by the endline. These transitions provide evidence of considerable volatility of the labour outcomes of individuals in this population. The high-frequency data shows even more volatility in week to week employment outcomes.

Table A.1: Descriptions of job market outcomes and characteristics by individuals characteristics

	% of sample	Job	Perm job	Casual job	Monthly Wage	Hourly wage	Hours work	Paid Monthly	In Office	Firm Size	Dissatisfied	Referral	Board
Full Sample	100%	54.6%	14.3%	10.7%	1287	10.9	40.4	25.5%	8.72%	71.6	22.3%	13.6%	16.4%
Panel A: By Sample													
City	44%	48.1%	5.88%	15%	1107	11.5	37	15.4%	2.2%	51.5	16.6%	16.5%	2.93%
Board	56%	59.6%	20.9%	7.12%	1401	10.6	42.6	34.1%	14.2%	85.7	26.8%	11.1%	27.9%
Panel B: By Gender													
Male	78.1%	58.4%	14.4%	12.9%	1367	11.8	40	23.7%	8.84%	73.9	25.5%	15.9%	18.1%
Female	21.9%	41%	13.9%	3.03%	882	6.72	42.5	31.8%	8.33%	60.6	11.1%	5.3%	10.6%
Panel C: By Period of Migration to Addis Ababa													
Born AA	36.2%	49.6%	9.66%	10.2%	1178	11.9	36.8	18.1%	6.19%	64.4	14.7%	16.8%	8.85%
Since Birth	19.5%	51.6%	10.2%	13.3%	1496	10.3	43.1	21.7%	5.83%	49.1	24.2%	11.7%	7.5%
Last 5 Yrs	24%	62%	16.5%	12.2%	1175	9.22	43.5	30.9%	9.35%	67.2	27.8%	13.7%	23%
Last 1 Yr	20.2%	57.5%	24.1%	7.21%	1419	12.2	39.9	37.8%	16.2%	113	27.6%	9.01%	33.3%
Panel D: By Education Level													
Grades 0-9	19.3%	49.6%	8%	14.7%	1180	9.53	41	13.8%	3.45%	38.2	15.2%	12.1%	6.03%
Secondary	22.8%	51%	9.52%	10.9%	1326	11.2	40.4	19.7%	1.46%	54.2	23.8%	19.7%	7.3%
Vocational	9.44%	55.7%	9.84%	8.47%	1078	8.95	41.1	25.4%	5.08%	38.8	26.2%	20.3%	11.9%
Diploma	22.8%	59.9%	12.9%	14.5%	1261	10.6	40	31.3%	10.7%	82.8	21.8%	14.5%	20.6%
Degree	25.7%	55.1%	26.5%	5.63%	1439	12.2	41.4	34.5%	20.4%	126	24.6%	4.93%	31%
Panel E: By Year of last Education attendance													
Last 1 Yr	37.2%	53.1%	17.2%	8.56%	1293	12.9	39.5	29.3%	13.5%	104	24.9%	9.01%	22.1%
13-36 Months	27%	58.8%	15.3%	13%	1212	9.79	41.8	29.6%	8.02%	75.3	23.7%	14.8%	17.3%
+ 3 Years	35.8%	52.8%	10.2%	11.4%	1343	9.92	40.3	18%	4.27%	35.2	18.7%	17.5%	9.48%

Table A.2 provides a picture of the jobs available in Addis Ababa. This is not meant a representative sample of the labour market in Addis Ababa, rather it provides a picture of entry level jobs found by young people. It gives an overview of what jobs are like, what attitudes are to different types of labour, and who gets what types of jobs.

The first two Columns *Job* and *Perm Job* give the percentage of respondents have jobs or permanent jobs, respectively, whereas the later columns give the average statistics for respondents of a certain type who *have employment*. These results are broken down by job seeker characteristics in the various Panels. So for individuals working in construction, of course everyone has a job, but only 6.45% of these jobs are permanent, and 38.2% were found via a referral. For individuals born in Addis Ababa (Born AA) 49.3% had jobs, and 18.3% of the jobs found by these individuals were found at the job boards.

A few notable statistics are facts mentioning in Table A.2. *Boards* individuals with jobs are far more likely to have found them at the vacancy boards, or got them by applying for through formal channels (getting the job with an interview). Many still find out about their jobs through social networks, but far fewer than those in the *city sample*. But as the panel describing jobs by the method that was used to find them shows, the jobs found at the boards look a lot better. They are more likely to be permanent, pay more, and often require formal applications.

Table A.2: Descriptions of job market outcomes and characteristics by job type

	% of sample	Perm job	Casual job	Monthly Wage	Hourly wage	Hours work	Paid Monthly	In Office	Firm Size	Dissatisfied	Referral	Board
All Jobs	100%	26.3%	19.9%	1287	10.9	40.4	47.4%	16.2%	71.6	40.9%	25.2%	30.5%
Panel A: By Job Activity												
Construction	29.5%	5.56%	41.1%	1388	12.6	37.2	12.2%	1.11%	25.8	57.8%	38.9%	13.3%
o/ Daily Labour	6.23%	0%	68.4%	813	9.35	28.8	10.5%	5.26%	67	57.9%	21.1%	0%
Factory Work	6.23%	26.3%	10.5%	860	5.35	47.9	78.9%	5.26%	249	42.1%	15.8%	26.3%
Basic Services	23.6%	18.1%	8.22%	935	9.48	43.5	58.9%	5.48%	25.9	47.9%	23.3%	28.8%
Vocational	11.5%	17.1%	2.86%	1389	14.9	39.1	40%	5.71%	52.5	25.7%	37.1%	20%
Civil Service	5.57%	94.1%	0%	1458	8.42	42.2	82.4%	88.2%	206	47.1%	0%	94.1%
o/ Skilled	17.4%	47.2%	9.43%	1459	11.1	41.1	83%	49.1%	104	32.1%	11.3%	60.4%
Panel B: By Job status												
Permanent	27.7%	100%	0%	1575	10.2	45.5	85.1%	43.2%	139	27.7%	9.46%	63.5%
Temporary	45.7%	0%	0%	1216	9.27	40.8	51%	11%	62.8	47.7%	29%	29.7%
Casual	18.9%	0%	100%	1162	13.7	34.2	7.81%	4.69%	50.4	56.3%	32.8%	3.13%
Self Empl	7.67%	0%	0%	1053	13.1	36.7	19.2%	0%	9	38.5%	30.8%	11.5%
Panel C: By Method job was found												
At Boards	37.4%	48%	2.04%	1397	8.55	45.3	85.7%	39.8%	140	36.7%	1.02%	100%
Networks	62.6%	12.2%	25.6%	1195	11.3	38.5	31.7%	7.32%	40.8	50.6%	45.7%	0%
Panel D: By Job Hiring Method												
Formally	48.7%	53.2%	3.9%	1271	7.75	45.6	94.8%	40.3%	107	31.2%	0%	79.2%
Referral	51.3%	8.64%	25.9%	1296	11.9	38.3	22.2%	2.47%	30.6	50.6%	100%	1.23%
Panel E: By Job Education Requirement												
None	51.3%	8.45%	31.7%	1223	12.8	36.1	22.5%	2.82%	38.1	56.3%	34.5%	6.34%
Secondary	32.9%	25.3%	17.6%	1087	7.95	43.5	63.7%	18.7%	84.8	39.6%	16.5%	39.6%
Degree	15.9%	59.1%	0%	1685	11.3	41.6	79.5%	54.5%	139	38.6%	4.55%	84.1%

Panel D provides a breakdown of labour outcomes by respondent education level. Surprisingly, better educated individuals in my sample do not seem to earn considerably more than those without higher education. While those with degrees do earn more, the difference is not especially large. Evidence from representative surveys of Ethiopia suggest that wages grow with tenure for educated respondents. However, those with degrees are far more likely to have permanent employment, and to have found their jobs formally or at the job boards. Indeed, jobs that have holding a degree as requirement for employment are overwhelmingly 87.2% advertised at the job boards, and require formal applications.

Table A.2 shows detailed descriptions of job outcomes broken down by the different types of jobs observed in the sample. A few key facts are worth mentioning.

Permanent jobs: Individuals who found permanent jobs clearly earn a little more than other types of jobs, but the differences is small, particularly when looking at hourly wages instead of total monthly wages. Permanent jobs offer more hours of work per week,[55] and are undoubtedly less volatile in terms of the work being available from week to week: looking at the high frequency data, very few individuals (11%) holding down a permanent job had spells of unemployment (weeks when they worked one week, but then not the next) whereas 50% of those among those holding temporary jobs had spells of unemployment.

Construction work: One of the most striking and perhaps surprising findings of the survey data is the dominance of construction jobs as a means to make a living for young people in Addis Ababa. In the baseline survey about 25% of respondents and 60% of young men who

[55]In an economy where many young workers consider themselves under-employment, in the sense of wanting more hours of work (Broussard and Teklesellasie, 2012), this is a sought after characteristic of a job.

had work were working in construction[56]. Almost half of these jobs were casual labour jobs (individuals were paid daily, or piece rate salaries) and few were considered permanent jobs. Very few construction jobs were found on the job boards, they tended to be found by going to visit worksites, or hearing about them through social networks.

Interestingly, the wages paid in construction are surprisingly high. On average, these wages were hardly lower than much sought after civil service jobs, with only Other Skilled (non-government jobs usually in specialized occupations such as lawyers or teachers) paying higher hourly wages on average. This may reflect the high premium paid for the kind of difficult labour done in construction, and the enormous demand for this kind of work in the middle of the construction boom currently happening in Addis Ababa. Yet individuals working on construction sites were more likely (by 15pp) to be dissatisfied with their work, and more likely to be searching (by 12pp) for work while working, when compared to all other jobs. When asked what job they expected to work at, in 6 months time, less than half of all construction workers anticipated still working in construction. These construction jobs are exactly the kind that I have in mind as the kind used for temporary spells of income while workers are looking for better employment opportunities.

The public sector: Government jobs are sought after by the youth in Addis Ababa.[57] I distinguish between civil service jobs, usually office and administrative jobs, which are more prestigious and considered routes to a middle class life (Mains, 2013) from any other kind of government employment. In my baseline survey one third of all individuals with degrees expected to find work in government civil service jobs in the next six months. However, work in this sector is hard to find, and by the follow up survey only 15% of those with degrees were still expecting this type of work, and only 4% had found a civil service position. Discouragement set in quickly. Civil service jobs are almost all permanent positions in large government departments, and are almost exclusively found at the job boards. They are far more likely to be given after a formal job interview, and none were given on the basis of referral alone. However, claims of highly inflated civil service wages appear to be vastly overstated. In fact government jobs pay less than other jobs after controlling for education.

Yet, while not everyone is satisfied with civil service jobs, they are far less likely to be dissatisfied with these jobs than other permanent jobs on average. This satisfaction seems to be drive forces other than the wages paid by these jobs: government employees are more less likely to be dissatisfied with their jobs, but *more* likely to be dissatisfied with wages they are paid in these jobs.

Appendix B Description of Search Behaviour over Time

This section looks into the search behaviour of job seekers over time. Here I use a new dataset of regular phone call interviews from ongoing fieldwork with a larger sample of 4000 job-seekers, over more than 35 weeks. I decompose the decline in job search over time into three different dynamics. 1) individuals who were always searching in every week start transitioning between searching and not searching on a regular basis. 2) Individuals who were searching on and off, do so less often and 3) Some individuals who were searching infrequently give up entirely (without finding a permanent job).

The patterns in this new dataset are similar patterns in the data used for the experiment. An individual not searching transitions to searching from one week to the next with probability 26% on average, with the probability of transitioning back to not searching is higher, at 33%. This is a high rate of oscillation, which the theoretical model explains through the movements around the steady-state critical value of savings. The percentage of sample searching in each week, among

[56] almost no women were working in construction

[57] For a detailed history of the civil service in Ethiopia see (Mains, 2013).

those who have not found permanent work, drops off from 65% to 35% of individuals searching in a each week over this 36 week period.

This drop off is driven by two effects: individuals who were searching in every week switching to searching only infrequently and individuals who were searching on and off searching less frequently than they were before. In the early weeks of the study, individuals searching in one week were observed searching in the next with probability of 75%. In the last month of the study, the probability of sticking with search in consecutive weeks falls to 62%.

I divide weeks into six distinct 6 week intervals. In the early intervals I see many individuals that search in every week of that interval (46% of the sample, in fact). In the final 6 weeks period, only 17% of individuals are searching in every week. In the interpretation of the theoretical model, that 17% are those who still above the critical threshold x^*. The drop off happens quickly: in weeks 31-36 only 18.7% are searching in every week.

Table B.1: Job Search consistency over time

	% Weeks Searched	*Never Search*	*Some Search*	*Always Search*
Weeks 1-6	64.2%	18.3%	35.5%	46.1%
Weeks 7-12	57.2%	21.4%	44.3%	34.3%
Weeks 13-18	55.1%	23.0%	45.6%	31.4%
Weeks 19-24	47.8%	26.9%	50.0%	23.1%
Weeks 25-30	44.9%	33.0%	44.1%	22.9%
Weeks 31-36	38.7%	40.2%	41.2%	18.7%

By contrast, the number of people who *never search* increases far more slowly.In the model these are the individuals who are permanently discouraged: their incomes never reach above x^*. After 18 weeks only 26% of the sample don't search at all for 6 weeks, up from 18% at baseline. Few individuals give up searching entirely in the early weeks. However after 30 weeks discouragement really sets in, where 40% are never searching.

So the proportion of individuals who transition between not searching and searching seems to increase over time at first, as individuals who previously had enough income to search run out of savings and start having to oscillate between not searching and searching. After almost 5 months some of those who are moving between not searching and searching begin to give up completely, possibly because their family stops giving them income.

Appendix C Charts and Images

Figure C.1: Map of Addis Ababa showing sampling frame and selected EAs

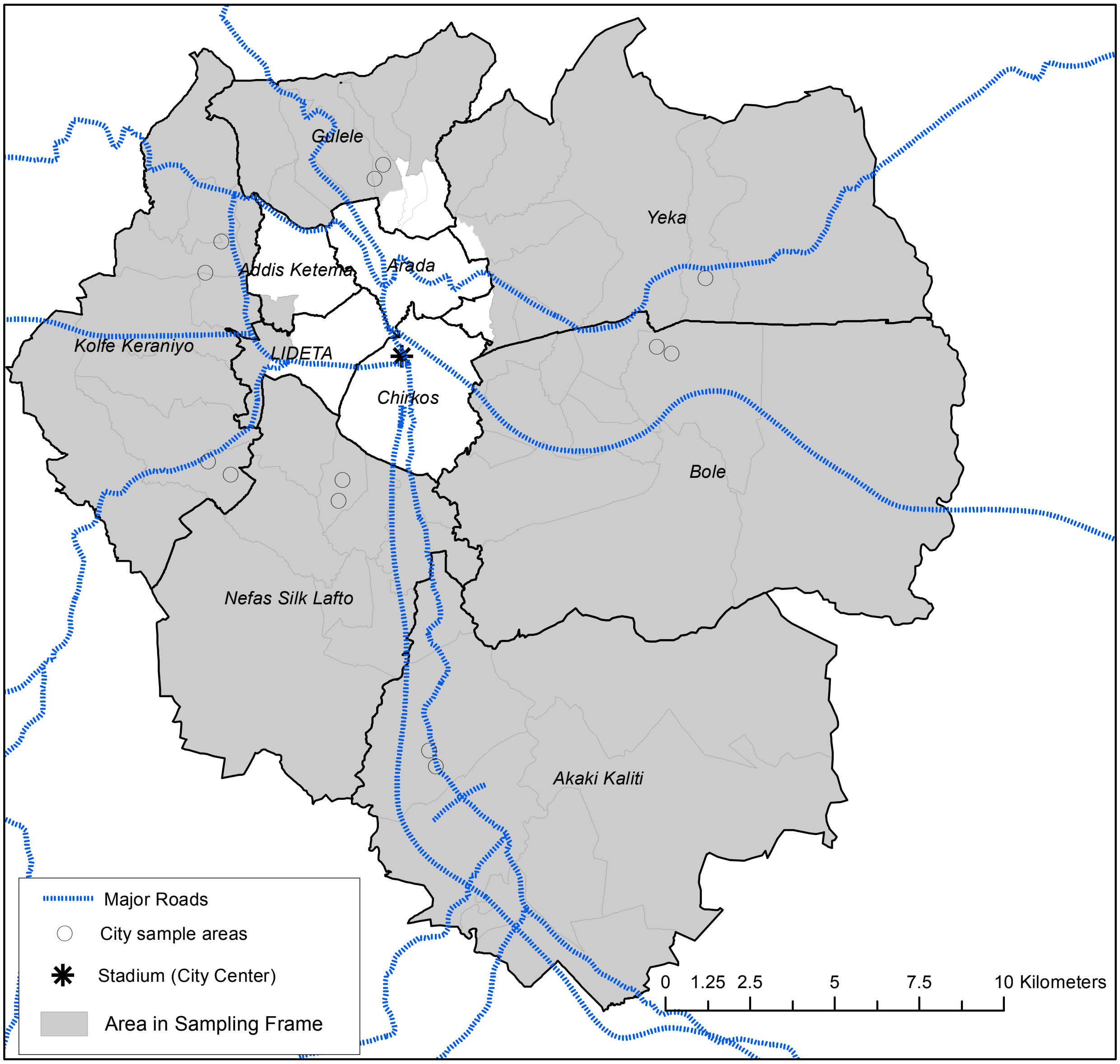


Figure C.2: Trajectory of Treatment effects across weeks in each sample

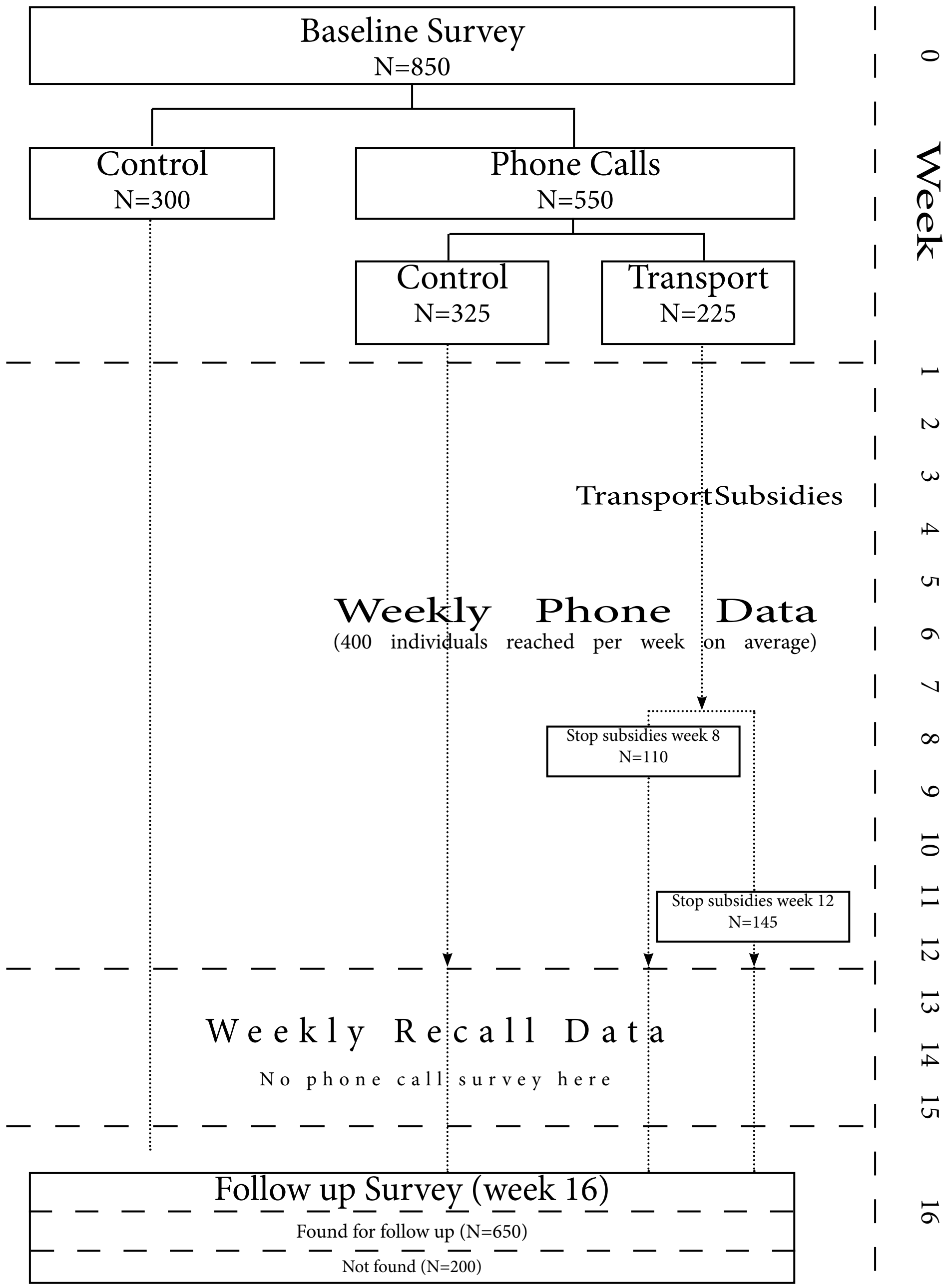


Figure C.3: Impact of treatment on distribution of occupations

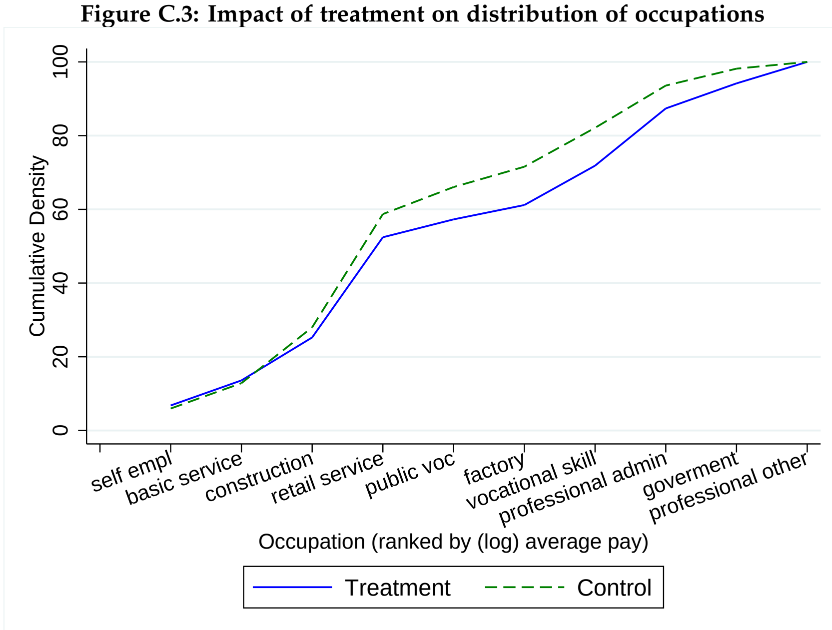


Figure C.4: Composition of the sample for each week by treatment and control: ***Board Sample***

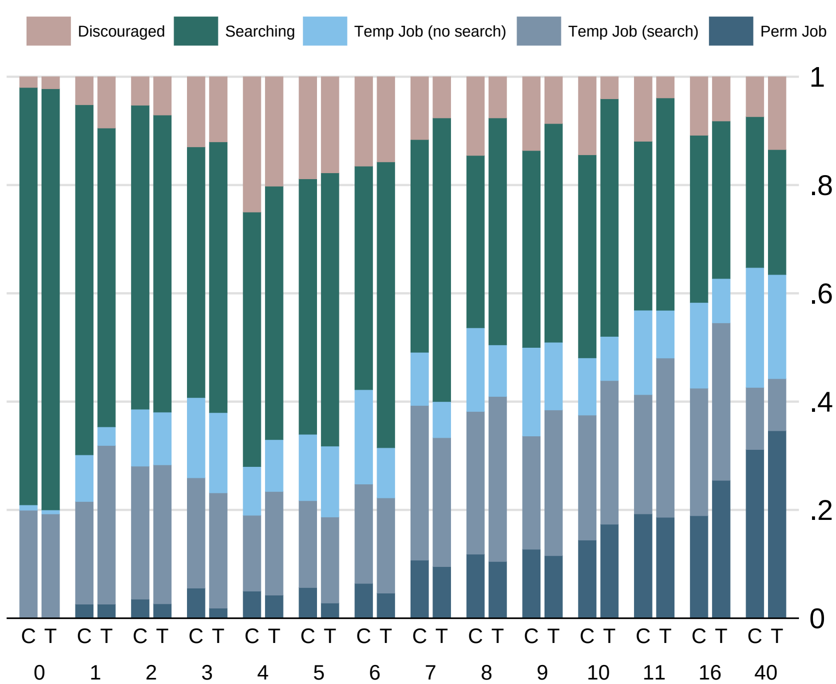


Figure C.5: Composition of the sample for each week by treatment and control: *City Sample*

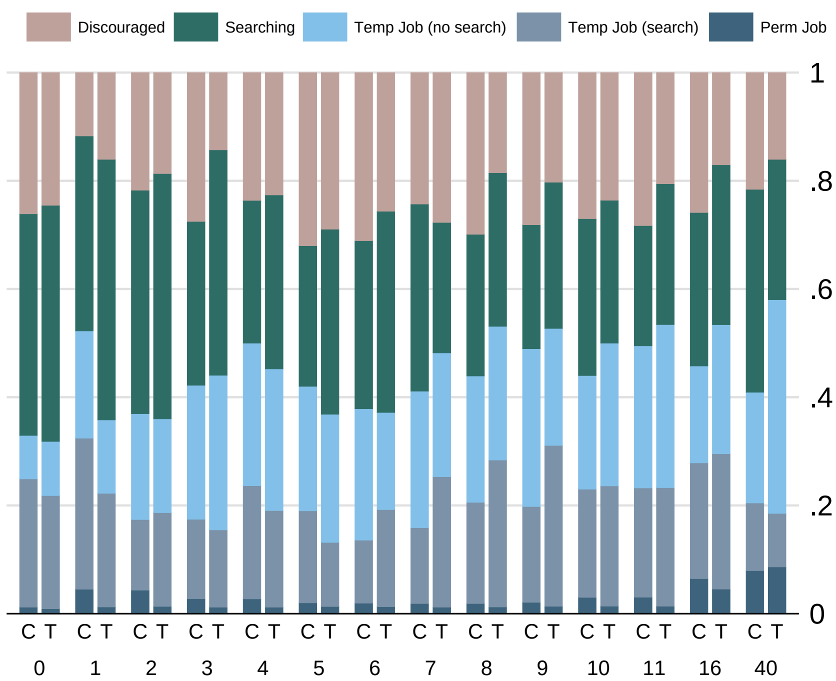


Figure C.6: Impact on visiting the job boards: Trends & treatment effects over time

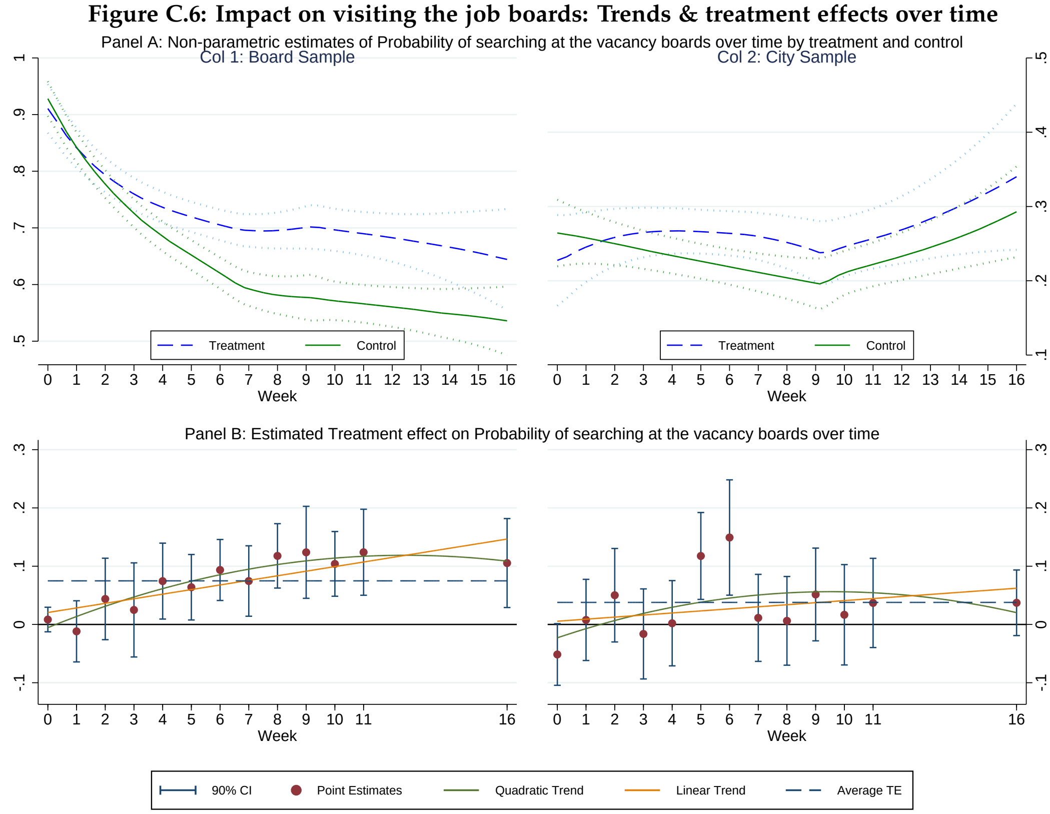


Figure C.7: Impact on having a job: Trends & treatment effects over time

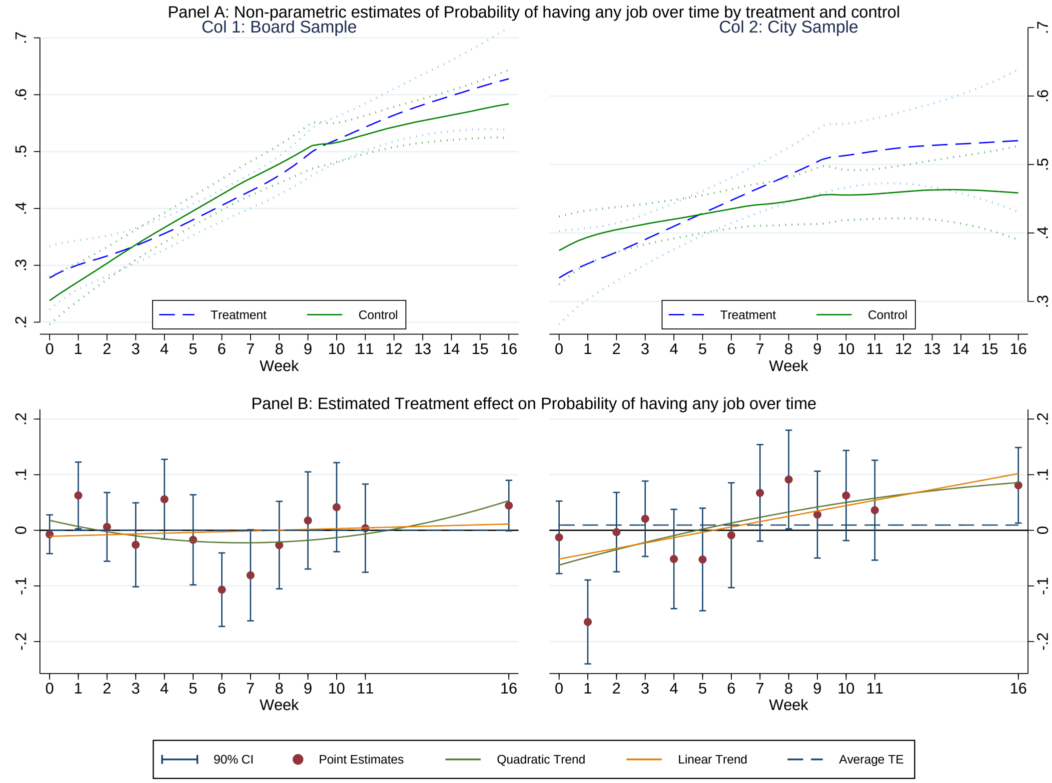


Figure C.8: Impact on discouragement: Trends & treatment effects over time

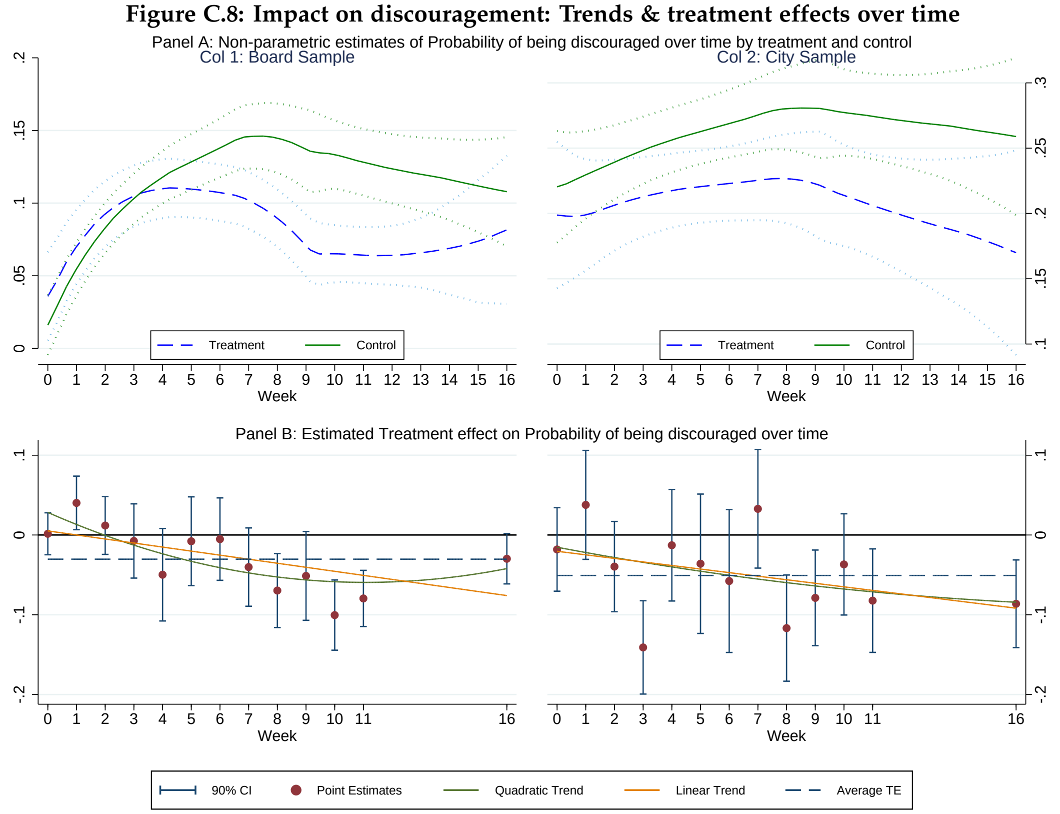
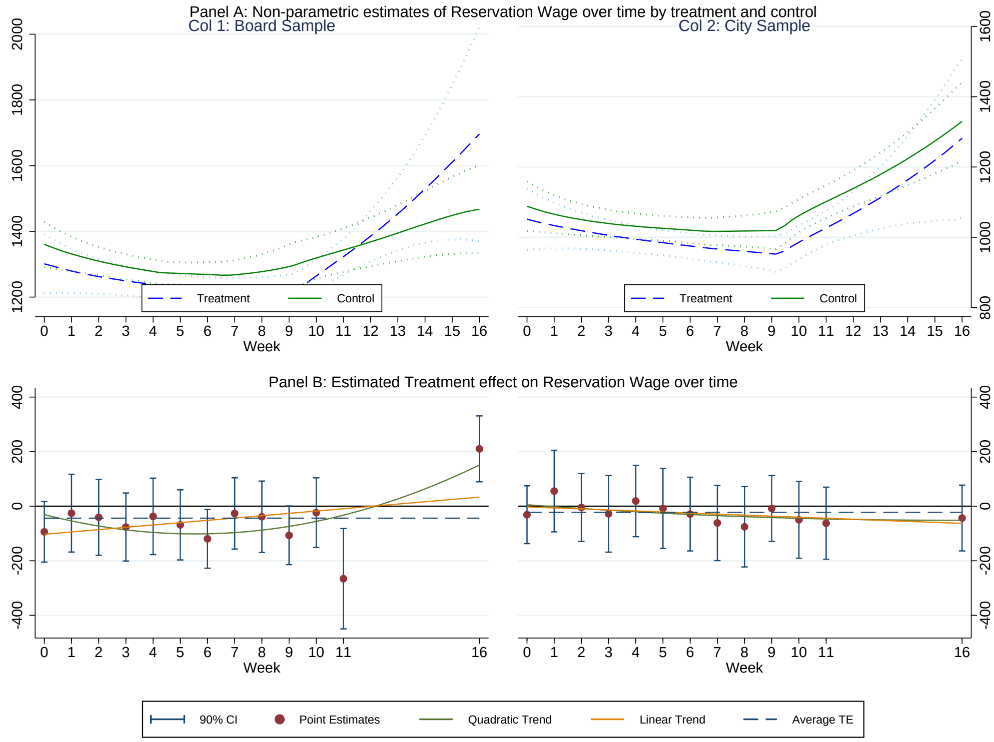


Figure C.9: Impact on reservation wage: Trends & treatment effects over time



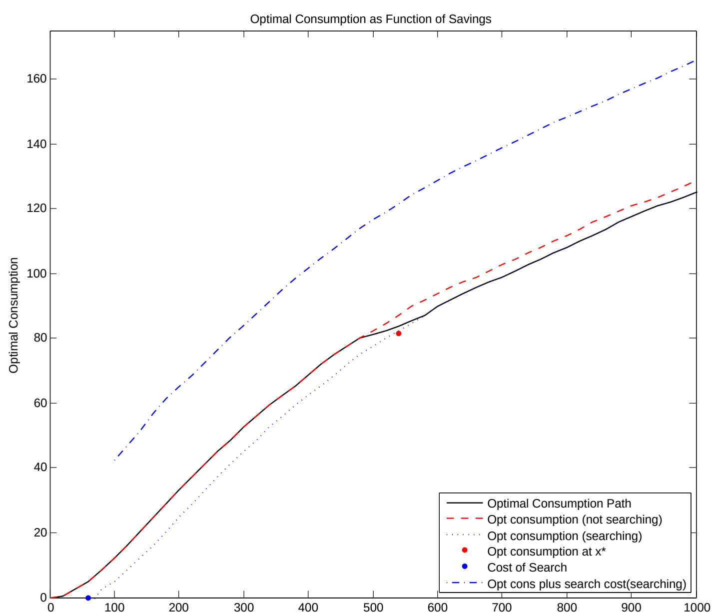


Figure C.10: Optimal consumption paths while searching, and while not searching

Appendix D Additional Tables

Table D.1: Test for balance in full sample and within board and city samples

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 D.2: Balance Table (cont): Test for balance across samples 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
Visited 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
Visited 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 D.3: Determinants of staying in the survey at first follow up (Week 16)

	(1)	(2)	(3)	(4)	(5)
		Full Sample		Board Sample	City Sample
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
Prob > F	0.70	0.71	0.15	0.64	0.0017

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.4: Determinants of staying in the survey at second follow up (Week 40)

	(1)	(2)	(3)	(4)	(5)
		Full Sample		Board Sample	City Sample
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
Prob > F	0.70	0.71	0.15	0.64	0.0017

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D.5: Impacts on having employment at both endlines (weeks 16 & 40)

Estimator	CM		Basic		Controls		First Diff	
			(1)	(2)	(3)	(4)	(5)	(6)
Week	16	40	16	40	16	40	16	40
Panel A: Average Treatment Effects At Follow Up (Pooled Sample)								
All	0.530	0.550	0.058*	0.063	0.062*	0.066*	0.081*	0.063
			(0.034)	(0.039)	(0.035)	(0.040)	(0.043)	(0.047)
Obs			657	605	657	605	657	605
R^2			0.003	0.003	0.066	0.074	0.062	0.105
Panel B: Treatment Effects At Follow Up by Sample								
Board	0.580	0.650	0.044	-0.013	0.043	-0.012	0.067	0.030
			(0.051)	(0.049)	(0.052)	(0.051)	(0.062)	(0.057)
City	0.46*	0.41*	0.076	0.17***	0.086*	0.17***	0.099*	0.110
			(0.046)	(0.053)	(0.044)	(0.057)	(0.057)	(0.079)
Obs			657	605	657	605	657	605
R^2			0.553	0.586	0.066	0.080	0.062	0.106

[1] The dependent variable is a dummy variable equal to one if the individual reported having work in the last 7 days, 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.

Table D.6: Week-specific treatment effects on searching for work

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.820	0.004	0.970	0.640	-0.000	0.009
		(0.028)			(0.017)	(0.057)
week 1	0.750	0.024	0.840	0.660	-0.000	0.054
		(0.033)			(0.050)	(0.045)
week 2	0.700	0.039	0.820	0.550	0.000	0.083
		(0.037)			(0.042)	(0.067)
week 3	0.570	0.073*	0.690	0.450	0.026	0.12**
		(0.041)			(0.061)	(0.051)
week 4	0.550	0.043	0.630	0.470	0.051	0.032
		(0.042)			(0.067)	(0.052)
week 5	0.540	0.034	0.650	0.430	0.028	0.036
		(0.043)			(0.055)	(0.069)
week 6	0.520	0.12**	0.610	0.430	0.11*	0.130
		(0.053)			(0.059)	(0.091)
week 7	0.620	0.033	0.740	0.500	0.065	-0.014
		(0.039)			(0.049)	(0.058)
week 8	0.560	0.11***	0.650	0.460	0.12***	0.098**
		(0.033)			(0.047)	(0.047)
week 9	0.520	0.14**	0.610	0.420	0.120	0.15**
		(0.055)			(0.081)	(0.067)
week 10	0.590	0.051	0.670	0.500	0.098**	-0.015
		(0.043)			(0.046)	(0.075)
week 11	0.530	0.092	0.620	0.430	0.120	0.049
		(0.055)			(0.077)	(0.078)
week 16	0.580	0.079*	0.610	0.530	0.13**	0.012
		(0.041)			(0.053)	(0.063)
Obs	(5,752)		(5,752)			

[1] The dependent variable is a dummy variable equal to one if the individual reported having a searched for job in the last week (week 16). Results are from OLS regressions on endline outcomes.
[2] CM denotes the mean out the dependent variable for the control group. TE denotes the estimate treatment effect in that specific week, estimated by interacting the treatment variable with week dummy variables.
[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level.

Table D.7: Trends in the treatment effects on searching for work over all weeks

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	0.066***	0.029	0.007	0.073***	0.003	-0.015	0.059*	0.061**	0.035
	(0.021)	(0.021)	(0.025)	(0.027)	(0.028)	(0.026)	(0.034)	(0.030)	(0.044)
Treat X Time		0.0050*	0.015		0.0100***	0.018**		-0.001	0.010
		(0.0026)	(0.0089)		(0.0037)	(0.0086)		(0.0035)	(0.017)
Treat X TimeSq			-0.001			-0.001			-0.001
			(0.00054)			(0.00055)			(0.00099)
CM	0.590			0.680			0.490		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.652	0.686	0.686	0.652	0.686	0.686	0.652	0.686	0.686

[1] For each sampled (Pooled, Board, and City) results from the following models are presented: (a) the constant average treatment effect over all weeks (b) a linear trend in the treatment effect (with the intercept given by "trans" (c) a quadratic function with linear, quadratic and intercept terms.

[2] Dependent Variable is a dummy variable equal to one if the individual reported having take some step to look for work in the last 7 weeks.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table D.8: Trends in the treatment effects on visiting the vacancy boards over all weeks

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	0.058**	0.015	-0.011	0.075**	0.019	-0.009	0.038	0.011	-0.014
	(0.025)	(0.025)	(0.024)	(0.029)	(0.034)	(0.033)	(0.041)	(0.036)	(0.036)
Treat X Time		0.0058**	0.018**		0.0080*	0.021*		0.003	0.014
		(0.0025)	(0.0086)		(0.0042)	(0.011)		(0.0022)	(0.014)
Treat X TimeSq			-0.001			-0.001			-0.001
			(0.00056)			(0.00077)			(0.00082)
CM	0.430			0.600			0.230		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.576	0.620	0.620	0.576	0.620	0.620	0.576	0.620	0.620

[1] For each sampled (Pooled, Board, and City) results from the following models are presented: (a) the constant average treatment effect over all weeks (b) a linear trend in the treatment effect (with the intercept given by "trans" (c) a quadratic function with linear, quadratic and intercept terms

[2] Dependent Variable is a dummy variable equal to one if the individual reported having visited the job vacancy boards in the last 7 weeks.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table D.9: Trends in the treatment effects on discouragement over all weeks

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	-0.040**	-0.008	0.007	-0.030	0.005	0.029*	-0.051	-0.024	-0.021
	(0.019)	(0.021)	(0.017)	(0.024)	(0.024)	(0.017)	(0.032)	(0.037)	(0.030)
Treat X Time		-0.0047**	-0.011*		-0.0051**	-0.016*		-0.004	-0.005
		(0.0018)	(0.0067)		(0.0022)	(0.0082)		(0.0031)	(0.011)
Treat X TimeSq			0.000			0.001			0.000
			(0.00045)			(0.00054)			(0.00074)
CM	0.190			0.130			0.260		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.237	0.241	0.241	0.237	0.241	0.241	0.237	0.241	0.241

[1] Dependent variable is a dummy variable equal to one if the individual reported having not worked and not searched for work in the last 7 days. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] CM denotes the mean out the dependent variable for the control group. TE denotes the estimate treatment effect in that specific week, estimated by interacting the treatment variable with week dummy variables.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table D.10: Week-specific treatment effects on the number of days searched per week in each week

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	2.060	0.010	2.060	2.050	-0.100	0.160
		(0.12)			(0.089)	(0.24)
week 1	2.150	0.200	2.510	1.770	0.170	0.160
		(0.13)			(0.20)	(0.16)
week 2	2.010	0.140	2.240	1.740	0.030	0.260
		(0.16)			(0.20)	(0.24)
week 3	1.570	0.32**	1.840	1.290	0.220	0.41**
		(0.15)			(0.21)	(0.20)
week 4	1.410	0.130	1.550	1.290	0.230	0.029
		(0.14)			(0.22)	(0.18)
week 5	1.510	0.091	1.800	1.210	0.026	0.150
		(0.14)			(0.18)	(0.22)
week 6	1.400	0.45*	1.620	1.170	0.57*	0.250
		(0.23)			(0.33)	(0.27)
week 7	1.880	-0.250	2.460	1.270	-0.390	-0.160
		(0.23)			(0.43)	(0.14)
week 8	1.730	0.170	2.080	1.370	0.240	0.047
		(0.25)			(0.46)	(0.15)
week 9	1.400	0.30*	1.640	1.130	0.250	0.34*
		(0.16)			(0.25)	(0.19)
week 10	1.610	0.50**	1.830	1.380	0.80**	0.088
		(0.22)			(0.34)	(0.19)
week 11	1.400	0.38**	1.610	1.170	0.63**	0.011
		(0.18)			(0.27)	(0.18)
week 16	1.810	0.060	1.910	1.680	0.220	-0.120
		(0.14)			(0.17)	(0.22)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is the number of days an individual reported searching for work out of the last 7 days. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two sub-samples.

[2] CM denotes the mean out the dependent variable for the control group. TE denotes the estimate treatment effect in that specific week, estimated by interacting the treatment variable with week dummy variables.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table D.11: Trends in the treatment effects on the number of days searched in the last week

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	0.18**	0.120	0.054	0.24*	0.030	-0.063	0.110	0.24**	0.200
	(0.088)	(0.084)	(0.090)	(0.13)	(0.12)	(0.094)	(0.12)	(0.11)	(0.15)
Treat X Time		0.007	0.039		0.028**	0.071*		-0.019*	-0.003
		(0.0089)	(0.028)		(0.013)	(0.036)		(0.0098)	(0.042)
Treat X TimeSq			-0.002			-0.003			-0.001
			(0.0017)			(0.0022)			(0.0025)
CM	1.670			1.930			1.390		
Obs	4,949	5,690	5,690	4,949	5,690	5,690	4,949	5,690	5,690
R^2	0.481	0.502	0.503	0.481	0.503	0.503	0.481	0.503	0.503

[1] Dependent Variable is the number of days an individual reported searching for work in the last 7 weeks.. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] CM denotes the mean out the dependent variable for the control group. TE denotes the estimate treatment effect in that specific week, estimated by interacting the treatment variable with week dummy variables.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table D.12: Trends in the treatment effects on having a job over all weeks

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	0.004	-0.030	-0.020	-0.000	-0.012	0.016	0.009	-0.052	-0.065
	(0.029)	(0.030)	(0.032)	(0.038)	(0.040)	(0.042)	(0.043)	(0.043)	(0.047)
Treat X Time		0.0052*	0.001		0.002	-0.011		0.0097**	0.015
		(0.0029)	(0.011)		(0.0040)	(0.016)		(0.0041)	(0.013)
Treat X TimeSq			0.000			0.001			-0.000
			(0.00062)			(0.00096)			(0.00070)
CM	0.450			0.460			0.450		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.493	0.478	0.478	0.493	0.478	0.478	0.493	0.478	0.478

[1] For each sampled (Pooled, Board, and City) results from the following models are presented: (a) the constant average treatment effect over all weeks (b) a linear trend in the treatment effect (with the intercept given by "trans" (c) a quadratic function with linear, quadratic and intercept terms

[2] Dependent Variable is a dummy variable equal to one if the individual reported having a any kind of paid work in the last 7 weeks.

[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table D.13: Iterative 4 week average treatment effects (one regression per coefficient)

	(1) work	(2) work perm	(3) searchnow	(4) searchboards	(5) discouraged	(6) days search	N
weeks 0-3	-0.019	-0.012	0.034	0.0090	-0.011	0.19*	1227
	(0.034)	(0.016)	(0.026)	(0.033)	(0.023)	(0.10)	
weeks 1-4	-0.0044	-0.0091	0.042	0.025	-0.037	0.17	1191
	(0.035)	(0.016)	(0.029)	(0.034)	(0.025)	(0.11)	
weeks 2-5	-0.013	-0.011	0.041	0.040	-0.038	0.15	1186
	(0.039)	(0.016)	(0.032)	(0.034)	(0.030)	(0.12)	
weeks 3-6	-0.028	-0.0060	0.057	0.081**	-0.024	0.20	1175
	(0.039)	(0.018)	(0.036)	(0.039)	(0.035)	(0.14)	
weeks 4-7	-0.028	-0.0068	0.050	0.080**	-0.016	0.063	1194
	(0.040)	(0.019)	(0.036)	(0.038)	(0.033)	(0.11)	
weeks 5-8	-0.011	-0.0027	0.087***	0.080**	-0.044	0.11	1208
	(0.040)	(0.022)	(0.032)	(0.038)	(0.029)	(0.14)	
weeks 6-9	0.012	-0.0025	0.10***	0.076**	-0.058**	0.080	1194
	(0.040)	(0.024)	(0.031)	(0.038)	(0.029)	(0.17)	
weeks 7-10	0.028	-0.0017	0.12***	0.090**	-0.081***	0.33**	1161
	(0.039)	(0.025)	(0.032)	(0.037)	(0.027)	(0.13)	
weeks 8-11	0.023	-0.0062	0.11**	0.100**	-0.077***	0.41***	1141
	(0.040)	(0.028)	(0.042)	(0.038)	(0.026)	(0.14)	
weeks 9-12	0.026	-0.0081	0.092**	0.099**	-0.084***	0.45***	757
	(0.042)	(0.031)	(0.044)	(0.040)	(0.026)	(0.15)	

[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 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 D.14: Heterogenous effects on endline outcomes by respondent background

	Board Sample			City Sample		
	(1) work perm	(2) work	(3) discouraged	(4) work perm	(5) work	(6) discouraged
Heterogeneous Treatment Effects by Duration of Unemployed (Long = 4 months+)						
long duration	0.11**	0.14**	-0.086**	-0.008	0.100	-0.19**
	(0.051)	(0.064)	(0.041)	(0.026)	(0.098)	(0.088)
not long duration	0.058	-0.063	0.059	-0.016	0.046	-0.008
	(0.063)	(0.081)	(0.049)	(0.053)	(0.088)	(0.057)
R^2	0.085	0.086	0.054	0.062	0.087	0.114
Equality F-stat	0.310	3.920	5.320	0.022	0.120	3.070
Equality p-val	0.580	0.053	0.025	0.890	0.740	0.100
Heterogeneous Treatment Effects by Migration Status (Migration to Addis since birth)						
birth migrant	0.089*	0.053	-0.010	-0.010	0.030	-0.061
	(0.046)	(0.055)	(0.040)	(0.053)	(0.097)	(0.069)
not birth migrant	0.049	0.001	-0.086*	-0.011	0.110	-0.120
	(0.084)	(0.14)	(0.051)	(0.039)	(0.079)	(0.084)
R^2	0.080	0.075	0.037	0.061	0.075	0.091
Equality F-stat	0.150	0.130	1.400	0.001	0.300	0.230
Equality p-val	0.700	0.720	0.240	0.980	0.590	0.640
Heterogeneous Treatment Effects by Experience						
experience	0.076	0.002	-0.015	-0.037	0.100	-0.046
	(0.076)	(0.095)	(0.047)	(0.046)	(0.079)	(0.055)
not experience	0.083	0.069	-0.033	0.035	0.037	-0.19**
	(0.059)	(0.070)	(0.049)	(0.034)	(0.11)	(0.085)
R^2	0.080	0.075	0.035	0.065	0.075	0.095
Equality F-stat	0.004	0.280	0.063	1.810	0.160	2.050
Equality p-val	0.950	0.600	0.800	0.200	0.690	0.180
Observations	368	369	369	289	289	289

[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 D.15: Heterogenous effects on endline outcomes 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 D.16: Impact of the subsidies on finances 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.