Location, Search Costs and Youth Unemployment: The Impact of a Randomized Transport Subsidy in Urban Ethiopia

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Abstract

How are young people living far away from the centre of cities constrained in their ability to find good jobs by high costs of search? I test for the impact of search costs on labour market outcomes for unemployed youth in spatially dislocated areas of urban Addis Ababa, Ethiopia. Some job seekers were randomly assigned to receive a transport subsidy twice a week, covering the costs of travel from the outskirts to the city center where information about vacancies is available. I find that receiving the transport subsidies increases the likelihood of finding permanent employment by 7 percentage points, 4 months after baseline. This result can be explained by a simple framework where getting information about jobs has monetary costs, the youth are cash constrained but have an outside of option of spells of temporary or casual work. Search subsidies should increase search intensity through both price and income effects; the response should be large for those that are severely cash constrained. I use a novel high-frequency data set gathered with weekly phone calls, which allows me to measure impact trajectories over time. I show that the subsidies increase search intensity, mainly by preventing job seekers from giving up search over time. The treatment effects are particularly large among cash poor respondents, and respondents reduce labour supply at temporary jobs when subsidies are available. In addition the impacts persist for a few weeks after the transport subsidies ended, when the price effect would no longer have been at work. Using another phone survey 6 months after the subsidies ended, I find evidence that the treatment effects partly dissipate as the control group catches up, but there is still a persistent effect on job outcomes: the gains are not completely transitory. I test, but find no evidence, for priming or Hawthorne effects due to the regular phone calls.

1 Introduction

High rates of urban youth unemployment in African cities have long been of concern to researchers and policy makers (Harris and Todaro, 1970; World Bank, 2012). Those young people

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who are fortunate enough to find sustainable livelihoods may still spend a long time in unemployment and forms of underemployment while searching for jobs and otherwise trying to make ends meet.

As African economies grow and many new jobs are created in cities, rural-urban migration is seen as an effective, if sometimes risky, way for the poor to improve living standards (Bryan et al., 2014; Harris and Todaro, 1970). And as cities grow urban density creates thicker labour markets (Marshall, 1890), which should allow workers and firms to make better matches faster, and reduce employment durations for job seekers (Moretti, 2014; Duranton, 2008).

However the rapid growth of cities has has not always come with a corresponding development of infrastructure and well located housing stock. This could leave an increasing number of people living on the outskirts of cities, far away from access to employment opportunities. High costs of search related to the cost of transport in sprawling cities could generate labor market frictions that 1) reduce the quality of matches and thus inhibit the creation of new jobs, and of specialized and high quality jobs (Pissarides, 2000; Acemoglu and Shimer, 1999) and 2) leave particular individuals locked out of employment opportunities because search is particularly costly.

This paper tests whether young job seekers are constrained in their ability to find jobs by their place of living, among a population of unemployed youth who live far away from the center of Addis Ababa, Ethiopia. I hypothesize that when search is costly, financial constraints make it challenging, and at times risky or even impossible, for someone who is cash and credit-constrained to search for jobs. This could lead to them giving up looking for work too soon, under-investing in search, becoming discouraged, or remaining in unemployment for long periods of time.

These constraints are likely to fall particularly heavily on the poor, new urban migrants, those living particularly far away from the city center, and without the social networks in their local communities (Bayer et al., 2008) that might otherwise have provided information about good opportunities. Spatial mismatch between jobs and poor job seekers could be creating barriers to access for the poor, and new urban migrants, which could be systematically excluding these individuals from the labour market. This implications for equality of access to employment opportunities, and could limit the extent to which growth is 'pro-poor' (Ravallion and Chen, 2003).

This paper uses randomized control trial to test directly for an impact of the costs of transport on job search outcomes. I provided transport subsidies for travel to city center of Addis Ababa, which 1) were non-fungible: could only be used for transport to the city center, 2) were provided for only two days per week, 3) entirely covered the costs of getting to the center of the city but no more, 4) lasted for a short (12 weeks) and pre-announced period of time. In this way the subsidies exogenously reduced the costs of search without introducing effects on wealth or reducing commuting costs.

Many of the highly desirable, often in the formal sector, jobs are available at large job vacancy boards located in the center of the city, to which individuals used the subsidies to travel. Since the treatment acts only to bring individuals who were living further away closer to the city, on an equal footing with individuals living closer, one could interpret an impact of the subsidies act as evidence both for the existences of search frictions and for inequality in access to jobs.[1]

There is existing evidence on the role of place of living on employment opportunities in de-

[1]Of course, the existence of these frictions could be leading to worse matches in this labour market. This is an issue which this paper does not claim to address.

veloping country contexts. However, most of this work has focused on the migration decision, which like, job search, may entail some costs, which may be too high or too 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 men to make the migration to cities, which greatly increases employment and household earnings. Further Jensen (2012) and Beam (2012) 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.[2]

The literature on the spatial mismatch hypothesis (Zenou, 2009) addresses this question in the context of large cities in the United States. A number of mechanisms have been suggested for spatial mismatch hypothesis including high commuting costs, neighborhood effects, red-lining of jobs, and job search costs (Gobillon et al., 2007). This paper focuses on the last of these: If the costs acquiring information increase with distance (Ihlanfeldt, 1997),[3] those living far away from jobs, who cannot afford to be rents closer to the centre, could be at a distinct disadvantage. Evidence suggests that the costs of job search fall disproportionately on minority-group workers in the United Sates (Ihlanfeldt and Sjoquist, 1998; Holzer et al., 1994). Experimental evidence from American cities from the Moving to Opportunity program (Jeffrey R. Kling et al., 2007) and a transport subsidy program not dissimilar to the one used in this paper (Phillips, 2014) lend support to this idea.

This hypothesis has been applied in African contexts (see Banerjee et al. (2007); Verick (2012) for applications to South Africa), but never rigorously tested. This paper aims to provide the first evidence of its kind on the impact of place of living, and the costs of search, on labour market outcomes in African cities.

However the mechanisms by which search costs inhibit job search are not always fully explained. I argue that high transport costs alone could not generate barriers to employment access, these costs become salient in the presence of poverty, and this paper suggests an important role for cash and credit constraints. The literature on unemployment insurance from developed countries predicts that job search intensity and unemployment durations can be influenced by cash constraints (Danforth, 1979; Chetty, 2008; Card et al., 2007; Acemoglu and Shimer, 1999).[4]

Yet the job search literature generally assumes that search is not costly in monetary terms,[5] and that job seekers simply lower their reservation wages when they run out of cash. This paper studies an environment where the main costs of search are monetary, such that individuals who are particularly poor or running out of savings cannot simply search harder. This is likely to hold true in many developing countries where the poor spend high proportions of their earnings on transport. In theoretical terms, being low on cash raises the marginal disutility of job search,

[2]Although some evidence from developing countries suggests that providing 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 about less constrained in their ability to search for jobs.

[3]particularly in markets where other channels and institutions for the dissemination of information are poorly developed (as is the case for Ethiopia, where respondents have to travel to physical job boards just to find out about vacancies.

[4]Indeed the youth's inability to smooth consumption during long spells of unemployment is one of the motivating concerns of this paper. Without adequate family support, the youth can suffer extended periods of long periods of poverty in unemployment, and can have their life aspirations postponed if not severely blunted (Mains, 2012; White, 2001).

[5]In other words utility functions are separable in consumption and (disutility of search costs on) leisure

because individuals are so close to subsistence and the marginal utility of consumption is high.

Further, I try to 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). Particular debates have raged about whether this informal sector acts as a residual to the formal sector where workers queue for formal sector jobs (Serneels, 2007) or if it represents a vibrant entrepreneurial sector in its own right (Maloney, 2004). This paper aims to fill a gap in this literature by looking at the impact of improved formal sector employment opportunities on labour supply in the informal sector. My results provide evidence that some job seekers use short spells of informal sector work to support themselves during job search, with "planned separations" from these jobs (Browning et al., 2007).[6]

I evaluate these transport subsidizes using detailed baseline and **two** endline surveys (4 and 10 months later), as well as a novel phone call survey, for which respondents were phoned every week for 3 months, and asked a series of short questions about labour market outcomes. This regular phone call data allows me to track the trajectories of search intensity over the weeks, and look at when individuals found jobs or gave up looking for a job. 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.[7]

I used a two sample approach to test the impact of these subsidies on two different, but both policy-relevant sub-populations. The first (referred to as the *city* sample) was taken from unemployed people found at home in randomly selected slum areas of Addis Ababa, where most of the respondents were relatively unskilled and had limited employment opportunities in white-collar jobs and mostly worked in the informal sector. The second sample (referred to as the *board* sample) was taken from individuals found at the main vacancies boards in Ethiopia, who were mainly well-educated, active job seekers, who aspire to sought-after professional jobs.

The results provide clear evidence for transport costs as a search friction and barrier to employment access. Treated individuals in both samples spend more time and effort on job search during the weeks of the phone call survey, they are less likely to give up job search, and more likely to be searching even after the treatment ends. They more likely to have made some active step to find work, 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 the temporary or casual work in the weeks when they are receiving treatment, suggesting that they substitute away from informal work in response to opportunities in the formal sector.

After 4 months, shortly after the transport treatment ended, individuals in the *board* sample[8] are 7 percentage points (over a mean of 19%) to have permanent work. These jobs are of higher quality, in higher skilled sectors, and more likely to be located in central parts of the city (as opposed to in respondents' local areas).

By contrast, those in the *city* sample seem to have very little chance of getting the highly sought after permanent jobs, but the treatment improved their access to employment in other ways: they are both more likely to be working (by about 8 percentage points over a mean of

[6] An extensive literature on Ethiopia (Mains, 2012; Serneels, 2007; Haile, 2005), as well as the descriptive data collected for this paper, and anecdotal evidence from field work, would suggest that most (but not all) young Ethiopians still aspire to wage employment in the formal and (although to a lesser extent than before) the public sector. These jobs, particularly "permanent" jobs, provide surety, security and social prestige.

[7] This suggests that the phone calls, at least, did not have priming or Hawthorne effects, and possibly hints that these highly constrained but motivated individuals cannot simply moved to action by 'nudging'.

[8] Recall that the board sample were those interviewed at the job boards. For a more detailed description of the samples see section 3.

46%) at the endline, and the jobs that they find are on average higher quality and 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 are still less likely, by 3.5 percentage points, to have permanent work 10 months after the baseline (and end of the treatment). I discuss reasons for why these results could have persisted for so long after treatment.

I look for evidence for the mechanisms through which the treatment allows respondents to search more intensively. I find that the treatment effects exhibit a particular trajectory: the percentage of respondents searching for jobs declines over the of the course of the study as respondents give up job search to fall into unemployment, or take up temporary jobs to make ends meet. The subsidies seem to have prevented respondents from *giving up* job search, or from skipping weeks of search. Furthermore, treated individuals are still more likely to be searching *after* the treatment has ended, suggesting that the subsidies allow them to hold on to savings and thus to search for longer.

I take this to be indicative of the presence of cash constraints and indeed confirm that the effects are particularly concentrated among individuals who were poor, or otherwise low on cash or savings, at the baseline. I find no evidence for an impact of the treatment on attitudes, reservation wages or aspirations of the job seekers.

I argue that results in this paper are consistent with a simple framework where job search is costly in monetary terms, job seekers have limited savings and are credit constrained. In this setting, poorer individuals find it harder to search intensively because they face higher marginal costs of search, and cannot borrow against future earnings to smooth consumption and spend more on search. Lowering the costs of job search increases the intensity and duration of job search activities through three main (possibly interrelated) channels 1) **Price effect:** changing the relative price of search in weeks when transport is received 2) **Wealth effect:** lowering total spending on search which alleviates cash constraints, lowers the marginal disutility of additional search, and prevents the running to down of savings so that this effect could persist over weeks, and 3) **Time effect:** the alleviation of cash needs could decrease the need for respondents to take temporary or informal work, which impose a large time constraint could impair their ability to search for better work.

The results in this paper have implications for policies that either reduce the costs of finding jobs and improve information flows about jobs directly through transport upgrades, subsidies, or active labour market policies, or the provision of safety nets for the unemployed (such as cash grants or access to credit, to prevent them from underinvesting in job search). At less than $2 per week per respondent, the intervention was designed to be a low cost program, which could be easily scalable: to a larger transport subsidy program, a job search assistance grant, or even a job placement service. Given that other studies (Betcherman et al., 2004; Groh et al., 2012; Ibarraran et al., 2012) find weak impacts of relatively expensive Active Labour Market Policies, wage subsides and employment training, this study suggests that relatively cheap interventions aimed at removing labour market frictions could be considered as complements to other labour market fixes.

2 Setting and Experiment

The growing urban inequality, high rates of youth unemployment (as high as 28% in estimates of Broussard and Teklesellasie (2012)), the urban sprawl of Addis Ababa, in conjunction with descriptive and anecdotal evidence of considerable barriers to access of information about jobs,[9] provides the motivation for studying the role transport costs in the lives of young job seekers.

This is a market plagued by matching frictions: unemployment is high, while firms still complain about the difficulties of finding skilled workers.[10] In this market, young people search for work with limited budgets and savings (and no welfare support, unemployment insurance or social security net to speak of), and are forced to take up forms of temporary, casual or low quality work (sometimes in self-employment) usually in and around their local areas, while looking for better work. These better jobs are usually located in the centre of the city and require formal applications. In the absence of permanent employment, workers prefer not to take temporary work, they would rather be searching for "good" work.

The city of Addis Ababa has been growing rapidly, "its population has nearly doubled every decade. In 1984 the population was [1.4 million], in 1994 it was [2.1 million] and it is currently thought to be 4 million. UNHABITAT estimates that this number will continue to rise, reaching 12 million in 2024." (UNHABITAT, 2003). A good discussion of recent urban developments in Addis Ababa is given in Yntiso (2008). Addis Ababa has been the major arrival city for rural-urban migration in Ethiopia, and is one of the most growing rapidly cities in Africa as a result (UN-HABITAT, 2005). Many of the new migrants can no longer access well positioned land in the centre of the city, and long term residents are also increasing being forced out of the inner city slums to make way for new development (UN-HABITAT, 2005).[11] A map of Addis Ababa is given in the appendix, figure B.2. The central four sub-cities (not shaded grey) are a good guide to the size of the city as recently as a 30 years ago.

So for those living far away from the city center, finding a job for the first time or after a spell of unemployment, tends to be difficult, especially for those without savings or financial support. Many of the job seekers in my sample were individuals that had a graduated the previous summer, 10 months before the baseline survey, and had still failed to find a first job, while claiming to have searching, on and off, for that whole time. Individuals who had never had work before had been without work for one whole year, on average. Others had been searching for well over a year.

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. Many of the jobs on these boards are cross-posted in newspapers available for rent near and around the job boards. Most individuals find out about these types of jobs by travelling to the boards.[12]

[9]Section 2 and A in the appendix provide a detailed discussion of unemployment and job search in Addis Ababa, and the institutions governing the job search process.

[10]We conducted qualitative interviews with over 25 firms of different sizes in the city of Addis Ababa to discuss their methods of hiring This paper cannot, and does not set out to, prove that search frictions of this kind play a non-negligible role in the high rates of unemployment. Still the structure off labour market suggests that this may be the case: firms find screening costly, and suffer from extremely high turnover. Hence they prefer to hire on a temporary basis. Job seekers search for permanent work, and are willing to move jobs if they find better options, but are not able to search at an optimal intensity or for the optimal time. Thus the correct matches less likely, and firms continue to prefer to offer short term contracts.

[11]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 (Yntiso, 2008). Existing research documents the loss of income, and transport related problems of those living in worse locations within the city (Yntiso, 2008).

[12]It is puzzling why there isn't a market for job vacancy information to be delivered to areas other than the center. The information on the boards is not centralized in any way, with different boards and newspapers being run independently. Job seekers want to get access to the full set of available vacancies, and thus are willing to travel to look it all. Since new information is released almost every day, and applying quickly increases the chance of getting jobs, it wold seem that there are returns to visiting the boards regularly. The costs of collecting this decentralized information and disseminating

While the vacancies on job boards are freely accessible, the newspapers, which often contain different or more up to date jobs, cost money to rent (very few people pay the prohibitively high cost of actually buying the newspapers). Applications often come with a fee, and some privately run labour brokers charge money to put job seekers in touch with work. The main cost for job seekers, however, are the costs of transport to travel to the centrally located job boards. This costs are unevenly distributed, however. In a city of over 4 million people (in the greater urban area), and with continuous settlement for up to 10km in any direction from the city center, some individuals have enormous distances to travel to have access to this job information, and the costs of transport are high and have been growing. A detailed discussion of the costs of transport, relative to the money available to the respondents, is a given in the next section 2.2.

In my data, rates of employment increase over time, both as respondents find the good jobs they want, and as cash needs become more salient and workers have to take temporary work. Similarly job seekers search less at both the intensive and extensive margin (even for those without jobs) as they become discouraged or run out of the money required to search.

2.1 Experimental design

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 to the center by the proffered type of mass public transport available. Addis Ababa is serviced by a fleet of large orange buses, run and subsidized by the government. While these are buses are very cheap, they are uncomfortable, overcrowded, and arrive less frequently than the other main form of transport available, the mini-bus taxi. These mini-buses are similar to those used in many African countries, an overview of the industry can be found (Kumar and Barrett, 2008). Given that most young people prefer to use mini-buses instead of buses, we budgeted enough money to make the trip by mini-bus.[13] The modal amount given was 15 birr (no one received less than 12 or more than 20 birr) for a return trip, or just less than $1 per day.[14]

The amount was chosen to not exceed the costs of the travel by enough to entice individuals to collect the money and return home with no other purpose to the trip. Indeed a few individuals, who made no effort to search for a job during the course of the study, did initially collect the money perhaps hoping to receive more than they were offered, but soon stopped coming for the money when they realised that there was little profit to be made on each trip. Thus the intervention was designed to impact individuals who had reasons to travel, in most cases to search for work, and were constrained by the costs of travel at the margin.

The sample was assigned to treatment and control groups randomly, with the sample split into three groups: the pure control group, a control group who did not receive the transport program but did get weekly phone calls, and the treatment group who received both phone calls and the transport treatment.[15] Immediately after the completion of the baseline data collection

it, on a regular basis, seems to high to be a profitable service for job seekers who have very little money to spend on job search and for whom the marginal benefit of finding out about one more vacancy, is very low. Indeed I spoke to one entrepreneurial service trying to do exactly this, by allowing paying subscribers a small fee to be able to phone in and et information about job vacancies, but respondents using his service complained that the information was incomplete and not up to date enough to be worthwhile to them. The phone service was understaffed and hard to get through to.

[13] At the endline survey, when the subsidies were no longer being handed out, 58% of respondents said their main mode of travel was in a mini-bus, 38% said that they used a bus, while a negligible number used other modes of transport such as walking or getting lifts with acquaintances with cars. Travel on buses, on average, took 10 minutes longer than a mini-bus trip, which had an average one way trip time of 33 minutes.

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

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

on April 4, 2013 (the baseline took 16 days from March 19 to April 4), 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, education and baseline covariates. I followed the standard blocking procedure as suggested by Bruhn and McKenzie (2009). Then within these strata, 30% of each strata 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 randomize is given in the data description section. Figure B.1 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.[16]

This facilitated the roll out of the treatment to the first half of the treatment group in the week beginning 8 April 2013, 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 3.3 were also begun in that week (1). Respondents were phoned on the preceding weekend, informing them that they had randomly been selected to receive a transport subsidy program, that would completely cover their transport costs for two days per week. They were told for how long this transport money would be handed out. They would receive the treatment by arriving at the center of Addis Ababa, to a bus terminal and hub, near where the main job boards in the city are located, showing their identification,[17] signing for the amount of money that they could collect, and then receiving the specified money. The amount specified was tailored to the distance an individual travelled; using the transport costs for a trip from each respondents place of living, using the current rates in Addis Ababa, as surveyed by the enumeration team. They could sign for the money twice a week, and no more, and needed to collect the money before midday.[18]

A makeshift kiosk was set up in a public recreational space next to the central bus terminal, and the transport money was handed out to the selected respondents by a single individual, from the first week right up until the 11th week of the study. Respondents were randomly divided into those receiving the treatment for 8 weeks and those receiving it for 11 weeks, and they were informed about how long they would get money in advance. In the week before the intervention ended they were told, either when they collected the money or by phone, 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 Costs of Transport in Perspective

The costs of transport seem small, but for the unemployed and poor of Addis Ababa, they do present a significant barrier to searching for work. Average weekly expenditure per respondent in the sample was 127 birr at the baseline survey, but this masks considerable heterogeneity. Median Expenditure was 80 birr, 70 birr among those without some kind of employment (usually

[16] Practically, however, it should be noted that the blocking had to be done on a pre-entry data set. The full baseline (paper) survey could not be entered in time for the randomization to be done for treatment to begin timeously. Thus a few key blocking variables were entered by hand in order for the blocking to be done. There were a few mistakes in the pre-entry of the data, which lead to some individuals being assigned to the wrong strata, but since these errors appear to have happened at random, this has not upset the balance on baseline variables

[17] Almost everyone in Addis Ababa has some kind of identity card, provided to them by their kebele or woreda (the lowest local government administrative unit) either where they live or in the place that they are from

[18] 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

temporary). Some of those without employment drew this expenditure either from meager savings (only 22% of the sample had any savings at all), but in the most cases survive on money from their parents, transfers which averaged 154 birr per week, among the 50% of the sample who reported getting money from parents).[19] Even those with some kind of employment earned, at the median, only 138 birr per week.

Thus, a single trip by mini-buses costing 9.50 birr represents, in my sample, 12% of median weekly expenditure. At baseline, respondents were travelling to the center twice a week. Two return trips by bus would cost 13% of median expenditure, or around 23% by mini-bus. Indeed, in the baseline survey, transport costs were on average 25 birr, or 20% of total expenditure. The transfer provided by the intervention, of up to 30 Birr per week, thus provides a significant transfer for many of the respondents, but one that was non-fungible. Individuals who were offered the full 11 weeks of the program, had the option collect up to 330 Birr, or $17 over the course of the study.

3 Data

The project timeline, figure B.1 in the appendix also provides an overview of the data collection and sampling of the study, starting in week 0 with the baseline survey. The first section of this chapter (3.1) describes this baseline survey after which the sample randomly allocated to treatment and control. Regular phone calls to a subset of respondents were conducted from week 1 until week 11, as described in section 3.3. Three Weeks after the end of the phone call and transport treatments, the endline survey began, with the majority of respondents re-interviewed in detail 16 weeks after the baseline survey was conducted. Because of the short time frame of the project, and a rapidly changing job market outcomes in our sample, respondents were reinterviewed in a *random* order to prevent bias from recall as a result of being interviewed in different weeks, because a purely random amount of time would have passed since the end of the treatment for all respondents.[20] Section 3.5 describes in detail the endline survey and issues of attrition.

3.1 Sampling Strategy

This study is comprised of complementary representative samples of *two* different populations from Addis Ababa. The distinction between the two samples is central to the analysis of this paper and will be used throughout. From here on, I refer to the one sample as the *city* sample and the other as the *board* sample for reasons that will become clear shortly. The two samples full sample, taken together without dividing respondents in this way, will be referred to as the *pooled* sample.

Screening: Both samples comprise of individuals age 18-30, made of men and women, who were available for work, and would be able to start a new job in Addis Ababa in the next 2 weeks if one was offered to them. Individuals who had some kind of work were not excluded, but the screening process was designed to exclude all respondents with permanent employment, those with jobs that they were simply not working at in the week of surveying and those who were

[19]The section describing the sample following this provides a more detailed description of the lives and budgets of these job seekers.

[20]Although, as outlined in 2.1, randomized variation in the week in which the intervention ended was intentionally introduced in order evaluated the persistence of treatment effects.

only interested in working outside of Ethiopia. It also excluded anyone engaged in full time education, work in the home, or with disabilities making them unable to work. In addition, all individuals in both samples were screened on their place of living: only individuals living in neighbourhoods and small satellite towns at least 5km away from the center of Addis Ababa were sampled. See the map in figure B.2 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 7 small enumeration areas around Addis Ababa. These enumeration areas were stratified by sub-city (10 large administrative units in Addis Ababa). The 4 central subcities of Addis Ababa, all completely contained in a 5km radius from the center point of the city, were removed from the sample. The 6 more distant subcities were all sampled, with one Kebele (local government unit) chosen from each subcity. The most populous, *Kolfe Keraniyo* was over-sampled by selecting two Kebeles from that subcity.[21] 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 B.2.

***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 sampled for the city sample, ensuring that they lived on average 5km away from the center. Since they were all by definition looking at the job boards, they would all fit the screening criteria above, since they would be job seekers.

The two sample approach was used to ensure the impacts of the two treatments could be tested and compared in the two samples to see for whom the intervention was most appropriate. For instance the *board* sample who were active seekers might need the money to continue their job search while the rest had no need for it. Alternatively, the *city* sample might have been less active job seekers precisely because of their monetary constraints, and would be most effected at the margin.

During the piloting of the questionnaire, it was revealed that the standard approach of door-to-door sampling, given time and budget constraints which prevented us returning on multiple days to find individuals, meant that we were undersampling highly educated individuals and individuals who were seeking work through formal channels into the permanent employment at the job boards.[22] While some individuals found at home were searching for work, many were doing so informally in their local areas. Discouragement and idleness were common among the individuals sampled in this way. By sampling at the job boards we were easily able to find individuals who were actively engaged in formal job search, althouh, as will become clear soon, not all of them did so every week, or could afford to continue doing so as the study progressed.

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

[22]It appeared that well educated job seekers who were interested in visiting the job boards were rarely at home when their households were approached for interviews.

3.2 Representativeness

As a result of two sample approach, and the predominance of highly educated respondents found at the board, my sample does over-sample individuals who have some kind of the tertiary education. In total my sample comprises 43% who have a diploma or a degree, and a full 23% of the sample with a degree. 80% of my sample has a grade 10 or above.

By comparison survey data from the Ethiopian Statistics Agency, suggests that the population of the same cohort of unemployed individuals living in Addis Ababa, has 22% with some kind of post-secondary education, with just less than 10% having some kind of degree, but a further 9.6% of that that total age cohort (including those not available for work) still identifying as students.[23]

Surveying at the job boards meant that we over-sampled highly skilled and educated individuals, who had already made the effort to visit the boards, relative to the total population of Addis Ababa. Although they were still many individuals without high levels of education at the boards, they were under-represented. Still, the sample gathered from the board can be considered to be representative of the average active young job seeker in Addis Ababa.

However, highly educated young job seekers are a non-neglible contingent of the Addis Ababa youth population, a contingent that is growing in size every year. The *boards* sample provides a representative sample of educated and motivated job seekers, who are possibly the group most likely to respondent to job search assistance programs. The *city* sample represents a better random sample of the young unemployed people of Addis, albeit one that under-represents the highly educated that dominate the other sample.

In terms of other important youth demographics the sample is roughly representative of the population of Addis. 48% of the sample is ethnically Amaharic compared to 49% of the population of Addis Ababa (UNHABITAT, 2003). The sample slightly overrepresents the Oromifa ethnic group (29% of the sample compared to 18% of the population), probably because of the sampling in the outskirts of Addis, which are closer to the Oromia province which surrounds the city.

In this sense, average treatment effects estimated in here, are not argued to be Average Treatment Effects of an intervention of this type, when applied to any person in Addis Ababa. Rather I seek to estimate and compare treatment effects and descriptive outcomes across two representative samples, both of relevant and dominant populations of the city.

3.3 Phone Survey

In order to measure trajectories and test for changes in job market outcomes and job search behaviour during the weeks during which the treatment was being implemented, a phone survey was conducted to gather high-frequency data on job seekers immediately after the baseline survey was complete, and up until 3 weeks before the endline survey was conducted. To test for motivational or Hawthorne effects of regular phone calls about job search, we restrict the phone call program to a sub-sample of individuals. In all 551 individuals were assigned to the phone call survey, and were to be reached by the phone numbers that were recorded during the baseline survey.

The phone survey was conducted by two skilled enumerators who were provided with cell phones and airtime and attempted to call each of the chosen respondents on a weekly basis.

[23]Own calculations, from the CSA Urban Employment/Unemployment Survey

They were told to phone the same individuals on the *same day* of the week each week. Since the questions asked focused on activities in the last 7 days (since the last phone), so that for an individual who was reached by phone call every week, there should be a complete record of their weekly activities for the entire 11 weeks. The phone calls took on average 4-5 minutes in the first weeks of the survey, with familiarity bringing down that time to about 2 minutes. Still there were weeks when not every individual could be reached, and there were some respondents who could not be reached by phone, because they had given the wrong number. In all 4,510 interviews were conducted over 11 weeks, an average of just over 400 individuals contacted each week, with 465 individuals contacted at least once during the survey. Contacted individuals were contacted on average 10.4 times during the study. About 100 individuals who were assigned to the phone survey were never reached by phone, although some were later found in the endline survey.

Again, figure in the appendix provides a useful overview of the design of the trial, including a visualization of the phone call surveys, the sample involved and the weeks during which the phone calls were conducted.

Importantly, costs of mobile credit, time constraints and patience of our respondents all limited the length of the interview that could be conducted in the phone call surveys. As a result only 8-12 questions were administered during this survey, 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. In addition, most of the measures are binary, which prevents someone analysing this data from handpicking variable definitions that produce significant treatment effect estimates.

Most notably, the key variable to be measured in the phone call survey was the "Permanent Job" outcome, which was chosen as the primary question about job quality to be the focus of the study. This was because it was the variable that was clearly the most sought after property of a job among job seekers when this was discussed in focus group discussions at the baseline survey, and from basic data in the baseline survey. A permanent job, may not imply better wages or hours, but promises long term security and less risk of loosing employment in the future.[24] Figure C.1 presents a one page version of the questionnaire used for this survey.

3.4 Test of Balance

Table C.1 in the appendix presents test for balance on variety of job market outcomes, focusing initially on the main employment *outcomes* from the phone call survey. This is followed by balance tests for the main respondent characteristics, and other labour market outcomes.

Importantly, because I am working with two separate samples, with different characteristics on average, and the extent to which I look at treatment effects in each sample separately. Furthermore randomization was stratified, as outlined in previous section, on baseline characteristics, but this was done *separately* for each sample.[25].

The following variables were used to stratify the randomization, in each of the two samples:

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

[25] This was because the distribution of baseline variables was so different across the two samples that it made little sense to stratify by the same variables. For example, nearly half of the *boards* sample had degrees, making it a very useful variable on which to block for that sample. However only three individuals in the other sample had degrees, meaning that using this status to stratify would serve no purpose

boards Gender, Diploma, Degree, Currently Employed, Work Experience, Age

cities Gender, Completed Grade 10, Currently Employed, Currently Searching for a job, Age

To show that attrition did not have differential impacts on the composition of the treatment and control groups I present balance tests for the sample that were resurveyed at the follow up survey. I discuss the attrition problems of my sample in more detail in the next section, but this table gives at least a first check that attrition did not significantly impact on the balance of the characteristics between random and control. This is suggestive evidence, that at least on observables, those that attritted from the treatment group were not significantly different from those that did not. I check for balance on observables among the group that were reached for the phone call survey, to show that attrition from the phone call survey is not effecting balance either. These are presented in additional balance tables presented in the appendix, table C.2.

The first variable on which I test for balance is the sample dummy variable, for being the *board* sample. Randomization was done separately for each sample but in such a way that the treatment and control group are made up of an almost identical proportions of the two different samples. This makes it possible to test for average treatment effects with the *pooled* sample, although in all specifications, I control for *sample* as a robustness check. I then proceed to test for balance on the main labour market outcomes at baseline, and find no significant differences.

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 blocking variables or major outcome variables, are statistically different across groups. In fact only one variable is statistically significant at the 10% level, out of 30 variables tested: individuals in the control group are more likely to be recent grads (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. This heterogeneity is only evident, however, among the Boards sample.[26]

This balance holds after attrition to the final endline survey, in Panel B and attrition for the phone call surveys, in Panel C. This gives assurance that the actual samples used for estimating treatment effects (both at endline (B) and weekly (C)) are broadly balanced on covariances. There are a handful of notable exceptions, which are discussed in the more detail in the section on attrition. All variables that do exhibit notably differences at baseline, are used as covariates in estimating regressions as robustness checks.

3.5 Attrition

Attrition was high, for a survey of such a short duration. This was in large part due to the methods used to recontact respondents, which was done primarily via phone, during a time when the Ethiopian mobile network was highly unreliable.[27] Table 1 shows the rates of attrition at varies points of survey: 14% of the total sample could not be found at all after the baseline survey, and about 25% were not found at the endline survey.

[26]This lack of balance might be expected to bias estimates of treatment effects *downwards* since my descriptive statistics suggest that recent grads are more likely to still be searching for work at endline, and they are more likely to find permanent employment in the endline survey.

[27]We were careful to list 2, sometimes 3 phone numbers per respondent, including a number of a next of kin, but there were still mistakes in the phone numbers given, and the turnover of phone contracts made numbers subject to change. In addition, budgetary constraints limited the amount of time and money I could spend tracking down respondents at the endline outcomes, especially for the *board* sample for whom I didn't have detailed information about place of living (having just surveyed them at the boards)

A large proportion (just less than half) of the total attrition took place between the first phone call surveys, which is measured for the phone call respondents. This sort of attrition may have implications for the representativeness of the sample (as we might imagine that highly mobile youth, or those without good access to mobile technology, would be more likely to leave the sample), but it is very unlikely to be correlated with the transport treatment, since treatment did not start until *after* the first phone calls.

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%
Found at Endline	236	227	195	658
	72.39%	76.69%	76.47%	75.03%
Total	326	296	255	877
	100%	100%	100%	100%

Indeed the transport treatment does not seem to have had any impact on attrition rates. Attrition is a problem for the estimation of treatment effects only if it effects the probability of the attrition, and if attrition is correlated with key outcomes measures. In this case it seems that attrition was different for the treatment group.

The phone call survey seems to have marginally improved the probability of finding a respondent, because we were more closely tracking these individuals, and some would inform the phone call enumerators if they were likely to move town or change phone numbers, allowing us to stay in touch for longer. For the paper endline follow up survey, the rates of making contact are only marginally higher than the pure control group. 76.5% versus 72.3%. This difference in rates of attrition is not statistically significant.

Among the group of individuals receiving the phone calls, the group getting the transport looks uncannily similar to the group receiving phone calls but not transport money, in terms of rates of attrition. Since so much of the analysis will be performed just looking at this group (getting phone calls) it is assuring that the treatment did effect the rates of attrition.

Furthermore, the balance tables presented for the sample reached at the endline (see table C.1: Panel B) and the sample reached at least once during the phone calls (see table C.1: Panel C), showing very few deviations from balance after attrition, suggesting that the type of individuals that couldn't be recontracted, did not differ along observable characteristics from those that did, between the treatment and control groups. One notable exception to this is the *Kms for the center* variable, which was balanced at baseline, but not among those at endline, among the *board* sample. It seems that treated individuals living further from the center were *more* likely to attrit.*On average, those living far away were more likely to attrit.* This seems counter-intuitive, since one would expect the transport subsidies to encourage youth living far away to come to the center more, making the more likely to be present near the city for the endline survey.

4 Main Results: Employment Outcomes

In this section I present the main results of the impact of the subsidies on employment outcomes. I focus on the impacts of the treatment on outcomes 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. Results are also presented for the second endline survey (conducted by phone) 40 weeks after the baseline to look at the persistent of these effects long after the subsidies have been removed.

The impact of the subsidies on job search behaviour is delayed until the next section 5, in which I use the 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. I also document the margins at which job search activities increase, and the trajectories that those impacts take over time. The section after that 6 looks further evidence on the mechanisms driving the results, including heterogeneity of treatments effects by individual background, and the persistence of the treatment on job search outcomes.

All results in this section (and in all proceeding sections unless otherwise stated) are intent-to-treat (ITT) estimates on binary labour market outcomes, using difference and difference-in-difference OLS estimators. All standard errors are clustered at the Woreda level (the lowest urban administrative unit in Ethiopia)[28], of which there are 70 in my data, suggesting that I do not have problems with too few clusters (Cameron et al., 2008). Generally, clustering standard errors inflates standard errors only marginally, because treatment was at the individual level and is largely uncorrelated within clusters.

I focus on these main binary outcomes[29] for both the high-frequency analysis and the detailed endline survey, using other measures from the endline to look at more detailed at job quality, including wages and hours, only once the main results are established. In this way, the high-frequency questionnaire provides a form of an informal pre-analysis plan, hopefully assuring the reader that the labour market outcomes chosen weren't selected from many, for the purposes of finding statistical significance.

For robustness, I employ at series of different estimators, and present the results from all these specifications here, to show that results are not sensitive to specification. Regressions on endline outcomes, estimating the difference between the treatment and control groups outcomes, take the form:

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

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

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

where T_i is the treatment variable dummy. and equation 1 estimates the basic (BAS) difference in means between the treatment and control group. 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 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).

[28]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.

[29]The subjective question about job types, quality and subjective perceptions of jobs is presented in section 4.1.1 (on job quality) and section 6.2 (which looks at the main mechanisms of the treatment effects)

Equation 3 provides the basic form for estimating different treatment effects for different groups (or heterogenous 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.[30]

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, which can be estimated with or without additional controls. In most specifications I use controls, as in equation 4 below labeled (FD) throughout. The ANCOVA estimator (labelled ANC), is similar, but looks at endline outcomes and cincludes a lagged depdent variable to account for difference in baseline outcomes in a flexible way. The ANCOVA estimator, in equation 5 below, is more efficient that either difference in difference estimator or the standard POST estimator which ignores baseline outcomes (Frison and Pocock, 1992; McKenzie, 2012).

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

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

4.1 Jobs, and Permanent Jobs

The labour market for young, relatively well educated, urban Ethiopians is one in which job seekers try to find good, "permanent" jobs that do not necessarily pay much better immediately, but offer job security and opportunities for promotion (over-all, an expectation life time earnings). Since some forms of a temporary or casual work are more readily available, individuals may not consider finding non-permanent work as a success, unless that job is of particularly good quality. This suggests that improved labour market opportunities could lead to: 1) individuals who would have been working without treatment, being more likely to be in good quality jobs 2) those that would not have been working because opportunities to work were unsatisfactory to be more likely to be working because they've found better options, and 3) those that would have been oscillating between forms of temporary work should be more likely to be working because either tye've found pernmanet work, or the forms of temporary work to which they have access are improved.

With this setting in mind, I look first at the impact of the transport subsidies on finding a permanent job at the endline, as the primary outcome for job quality[31] and find that the 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 experiment, suggesting that these active job seekers were able to find the jobs they were initially after. This result is consistent across all 6 specifications, and significant at the 5% level in most of them. The interested reader can refer to Appendix A for a more detailed discussion of permanent jobs. These 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.

It is illuminating that the *city* sample are not more likely to find permanent work, but this finding is driven in large part by the fact that it is so difficult for this group to find permanent work. Results presented later on this paper will show that the results are further concentrated

[30]These co-efficients 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

[31]Also, this was the only measure of job quality to included in the phone call survey, based on baseline surveys and qualitative work, it was decided, ex-ante, that this was the most important outcome on which to focus

Table 2: Effects of transport subsidies on having Permanent Employment at endline

	CM	(1) BAS	(2) LOG	(3) COV	(4) ANC	(5) BLK	(6) FD
Panel A: Average Treatment Effects At Follow Up (Pooled Sample)							
All	0.130	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	0.190	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	0.060	-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] Dep Var is a dummy variable equal to one if the individual reported having worked at a permanent job in the last 7 days, measured at endline (week 16). Results are from OLS regressions on endline outcomes, details of the specifications titled are in the REF

[2] Panel A gives average ITT effect for everyone. Panel B presents coefficients the two different samples-"board" and "city"-separately

[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] Column CM gives the control mean for the dependent variable for the relevant sample

among the group of individuals with university degrees, to whom these kinds of the permanent jobs are available.[32]

I then look at the impact of the treatment on having a job at all. These results are likely to be partly obscured by the fact that some of the work available is considered inferior (and respondents might actual prefer to be unemployed and searching than taking these spells of temporary work). However, we should still see that if employment opportunities have been improved by the treatment that, ceteris paribus, a treated individual should be more likely to be working.

Indeed, 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 *boards* sample. This suggests that some respondents in that *city* sample with low motivation and job search intensity at baseline, were induced to go and out search for employment at endline. I provide further discussion on these findings at the end of this section.

Are these impacts on job outcomes persistent a further 6 months after the first baseline survey, when I collected another round of data on these respondents via phone? It could be that treated individuals simply searched more intensely during the treated period and thus are more likely to have good jobs after 16 weeks, but the control group could have caught up over the proceeding 6 months by continuing to search at the same intensity. In this case the treatment effect would disappear completely if one looked long enough after the treatment ended. This would not

[32]The 6% of individuals in the *city* sample who say that they found permanent employment, are mostly likely to be a group of individuals who consider their work permanent, but not in the way that the *board* sample do, whereby there is a clear contract ensuring that the job secure for some time

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

	CM	(1) BAS	(2) LOG	(3) COV	(4) ANC	(5) BLK	(6) FD
Panel A: Average Treatment Effects At Follow Up (Pooled Sample)							
All	0.530	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	0.580	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	0.460	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] Dep Var 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, details of the specifications titled are in the REF

[2] Panel A gives average ITT effect for everyone. Panels B and CS estimate effects for different groups. (B): The two different samples- "board" and "city"

[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 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)
Observations			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)
Observations			657	605	657	605	657	605
R^2			0.186	0.285	0.091	0.133	0.100	0.143

[1] Dep Var 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, details of the specifications titled are in the REF

[2] Panel A gives average ITT effect for everyone. Panels B and CS estimate effects for different groups. (B): The two different samples- "board" and "city"

[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

render the large treatment effects uninteresting, as employment durations would still have been reduced for the treatment group. In fact it would be surprising if there was *not* catch up, at least to some extent.[33]

Still, 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 alone-side the impacts for week 16, showing how the coefficient has roughly halved over time. The coefficient at 40 weeks is not statistically significant, because of the small samples sizes, but is of reasonably large magnitude, and intuitively consistent with the larger estimate at week 16: 25% of the control group found permanent jobs between week 16 and week 40, 19% of the treatment group were the same. This difference in rates of finding permanent work *between* the surveys, at least, is statistically significant. Unsurprisingly there is no impact on permanent

Table 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)
Observations			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)
Observations			657	605	657	605	657	605
R^2			0.553	0.586	0.066	0.080	0.062	0.106

[1] Dep Var 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, details of the specifications titled are in the REF

[2] Panel A gives average ITT effect for everyone. Panels B and CS estimate effects for different groups. (B): The two different samples- "board" and "city"

[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

work among the *city* sample, who still have very a low probability of finding permanent work. However, I can look at the impact on having any employment at all, in this sample. Before doing so, I should remind the reader of the problems associated with attrition among this *city* sample after 6 months. These were documented in the data section: these problems did not apply to *board* sample were just as likely to be found by phone after 10 months as they were face-to-face after 4. In particular, high attrition rates in the city sample result in a lack of co-variate balance at baseline for the sample that were found 6 months later. Most notably the

[33]If the most productive of the treatment group were helped into finding work by the subsidies for instance, their equally productive counter-parts in the control group would be quite likely to find jobs too over the following months, while those who hadn't found work by week 16 in the treatment group would continue to struggle to find work after the subsidies ended

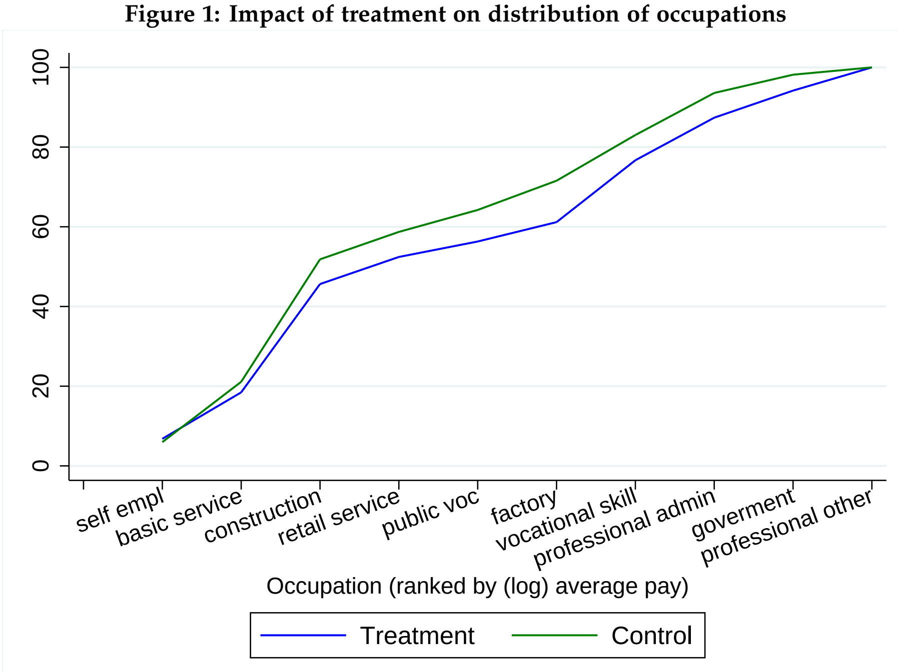
treatment group found then were significantly *more* likely to be working at baseline, such that the first difference and difference-in-difference estimates are considerably different from the simple difference specifications. Thus results should be read with caution, and if anything, the FD estimates 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 significant with the small sample (see table 5. There seems to be no significant impact on the probability of having any work at all for the *board* sample after 40 weeks.

4.1.1 Job Quality

Treated individuals in the *board* sample are more likely to have found permanent jobs, which is possibly the most sought after job quality characteristic for young people in Addis Ababa. This was the only question asked about job quality in the phone call surveys (including in the 40 week second follow up phone survey). 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 experiment to see if the additional jobs (permanent or otherwise) are of better quality on average, which is what we would expect in access to the labour market had been improved by the treatment.

In Figure 1 below, I classify the jobs of all respondents working at follow up into similar 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.

Figure 1: Impact of treatment on distribution of occupations



I find that on average the wage of jobs found by treated job seekers look no different to those in the control group. 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 perhaps not surprising, given the results presented in section A on the nature of work and permanent work, which indicated that permanent jobs were not desirable because of a pay differential, but because of the security and nature of the work.[34]

However, other job quality outcomes were significantly impacted by the transport subsidies, as shown in Table C.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 and desirability of work. The key variables are described in the notes to Table C.5. For instance, they are 14 ppts more likely to be working in office, as opposed to some kind of other work site, and 4.3 ppts more likely to have found the job through formal means (application and proper interview).

The results indicate that the *city* respondents are more likely to find jobs of better quality, indicating that they are not simply taking work faster and accepting inferior work. 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. This suggests that the treatment helped respondents to find better jobs.

Boards 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 be have found jobs that require at least a degree as a qualification. Given that most respondents prioritized (during focus group discussions) finding employment in the occupation for which they studied or trained, this seems like a positive outcome.

4.1.2 Discussion

Subsidized transport improved job market outcomes for job seekers. The results suggests that the effect on the *board* sample is partly a substitution away from unemployment and other forms of temporary work into permanent work, but without inducing that many individuals to take temporary work that they otherwise wouldn't have done (indeed the proportion of *board* individuals doing temporary work is almost identical between treatment and control). 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. This is further corroborated by the fact that there aren't significant impacts (although the coefficients are positive) on the the quality of jobs found by these respondents: many of them would already have been able to access office jobs, but not necessarily permanent ones.

An interesting observation that perhaps supports the evidence on the role of temporary jobs, and substitution away from these forms of work (whenever possible) is that at week 40, the second follow up, while a higher proportion of individuals were likely to be employed among the *board* sample (see table 4), the vast majority of that increase comes in the form of permanent work. There are in fact *fewer* individuals taking temporary work 40 weeks later (down to 28% from 37% at endline). The proportion doing temporary work among this without permanent work, however, was largely unchanged.

By contrast, the *city* sample seems to have found a better set of temporary work opportunities, ones that are temporary but look *more* like formal employment. Because these work opportunities are, on average, better than the ones they had before the subsidies, they are more likely to have take work at the time of the endline. The coefficients of the impacts on job quality are

[34] Certainly, during the course of fieldwork, I met and spoke to many unemployed men who were engaged in sometimes hazardous or stressful casual labourer, but often at considerably higher salaries than were available in more formal work.

usually larger, but not much larger, than the impact on having any work at all, so some of the *city* sample respondents working at high quality jobs might have had some low quality temporary job at endline under the counterfactual with subsidies, but the majority would simply not have worked because the oppurunities we're good enough.

The persistent impacts of the two main effects found at the first endline up until 40 weeks later, raises some interesting questions. The fact that the results have partially dissipated over time is not surprising, as the control was likely to catch. However the continued advantage of the treated individuals after all this time suggests that reducing the duration of unemployment (or, as it may be transition into a first job) may matter, not just instrumentally, but because taking too long to find work can have longer last effects. I discuss this, and other mechanisms, later in section 6.

Firstly I turn to a more pressing question: what is the more proximate cause of the improved labour market outcomes documented in this section? Undoubtedly the subsidies themselves did not cause better jobs: I look at how job search responded to the transport subsidies, and this could have lead to the sharp improvements in labour market outcomes after 4 months.

5 Main Results: Job Search

This section seeks to explain how the treatment group managed to find more, and better, jobs. I look at how job search decisions, at the intensive and margins, and the method of search was impacted by the subsidies. In general the subsidies were designed to allow more job search in the center of Addis Ababa, which for many people is the main place where job search happens, although it is important to note that the subsidies do not cover job search in places close to the respondents' homes.

In hypothesize that reduced transport costs should increase job search activity for treated respondents. These effects could be at the intensive margin, or at the extensive margin (whether someone decides to search on a given day or week) if job search required a minimum amount of money or effort to have any efficacy, and make it worthwhile. These can be classified into three mechanisms:

(S1) Price effect Increasing the probability and intensity of search in all weeks *that treatment is given* equally because the price of search has been lowered relative to the potential benefits of finding a good job.

(S2) Income effect Increasing the probability of searching for a job particularly for those who are cash constrained. In the same way that a cash grant would, the intervention lowers the amount of money spent on search and increases disposable cash, which should in turn lower the marginal disutility for a job seeker with very little money. In the extreme, an individual with no money is would be unable to travel to the center without the subsidies.

(S3) Savings effect Prolonging the length of job search (delaying giving up search) because lower costs of search allow job seekers to hold onto savings, which they can use to search in later periods (while the subsidies are still in place *and* when they have ended, via the effect **(S2)** above.

Furthermore, the theory would suggest that job seekers might change their behavior with regards to jobs that they take/accept in response to lowered search costs:

(W1) They are less likely to take short spells of temporary work because they do not this work to fulfill cash needs that are generated (partly) by the costs of job search

(W2) Forward looking job seekers might be more picky about taking certain types of work- temporary jobs in particular- because they anticipate being able to search for longer

In this section I look at the evidence for these different impacts on search work behavior while the treatment is being administered. I have find evidence for all of these channels at work: more job search and less temporary work for treated respondents. However, for the *boards* sample I find more evidence for (S2) and (S3), since the impacts of the treatment start to take effect in later weeks of the study, once discouragement (giving up job search, or having weeks without searching) sets in.

I have already shown that for the board sample, for whom high quality permanent jobs are available, there is some evidence for (W2) above, since they are equally likely to be working at endline but more likely to have found permanent work. In this chapter I show that (W1) also seems to be at work: respondents are less likely to be doing temporary work during weeks when the subsidy is available; this effect disappears once the subsidy is removed.

5.0.3 How searching leads to jobs

Increased job search intensity, I would argue, is linked directly to increased rates of (good) employment at the endline (4 months later) by an increased rate of good job offers. This could be because individuals are more likely to search in each week, that they search more during each week of search, that they substitute cheap forms of job search (say perhaps around the local area without requiring travel) to more costly search in the center of town, or that they keep up their job search for longer before giving up, or taking temporary work that reduces their ability to search while they work. This leads to more high quality job offers, which in turn should lead more individuals to 1) accept jobs, and 2) have good quality of jobs at endline.

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 endline) for the control group. As I show in this section, the treatment increases the average number of weeks of search by about 1.3 weeks over a control mean of about 9 weeks of search (and about 1.5 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, where the actual effect is estimated to be about 7 percentage points.

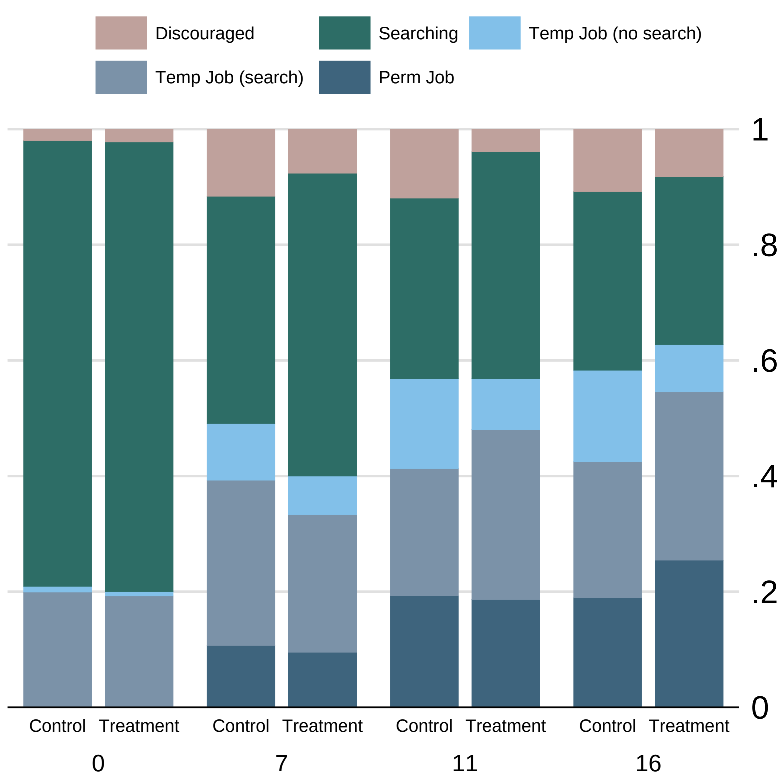
But the impacts of the treatment on the distribution of search intensity reveals more than just the average number of days or weeks of search. Importantly, I show that the combined effect of the treatment on search across all weeks is that treated respondents are far more likely to have searched during *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 the returns to job search are non-linear, such that searching for a job continuously for a long stretch of time, greatly increases the probability of finding a job relative to searching in an irregular fashion, this increased probability of searching for all the weeks could be driving the full 7 percentage point impact on the probability of finding permanent work.

5.1 Overview: Composition Effects

Before looking directly at job search responded to the reduction in the job search costs, a simple graphical analysis provides some insights into how labour market outcomes changed over time in the sample, by looking at the composition of the different groups across labour market categories for each week. I do this separately for the *board* (5.2) and, in the appendix city (5.2) samples. For clarity sakes, I present only a select number of weeks to provide an overview changing compositions, the appendix provides similar graphs for all 16 weeks for a more detailed story. For each week (1-16, and for week 40), stacked bar graphs show the percentage of both the control (C) and treatment (T) group separately, classified into one of five *distinct* categories, starting from the top of each bar: 1- Discouraged (not working or searching); 2- Searching (but no work) 3- Temporary work (not searching) 4 - Temporary work (but also searching)[35], 5 - Permanent work. In this ordering, the top of the blue bars in the graphs below indicate the employment rate for the relevant group at that time.

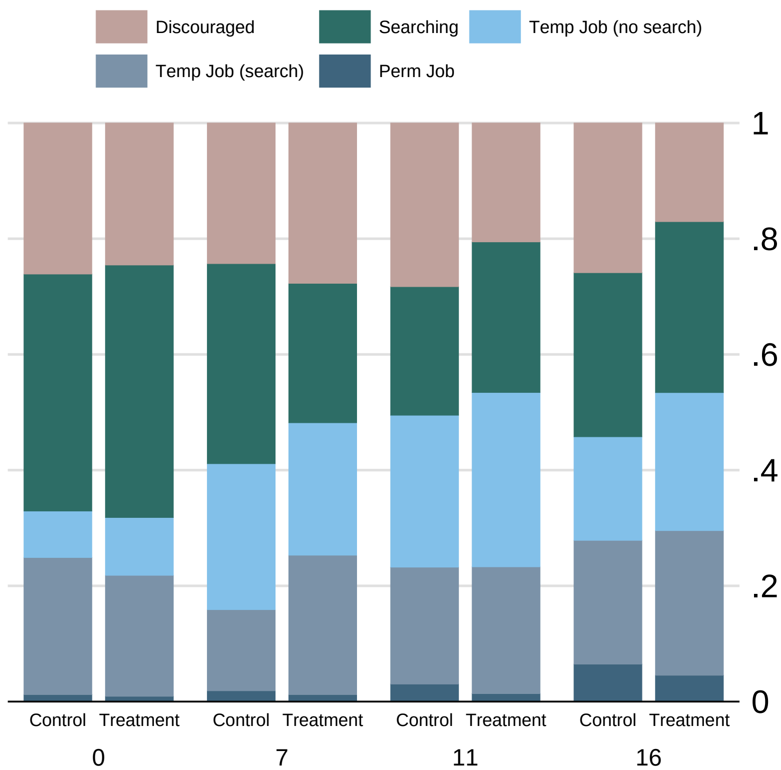
Figure 2: Composition of the sample for each week by treatment and control: *Board Sample*



Some basic trends in the composition of the the sample are evident. Among the boards sample almost everyone was searching for work at the baseline, even among this with temporary work.

[35]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.

Figure 3: Composition of the sample for each week by treatment and control: ***City Sample***



However, 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 work. These trends hold for both samples, and rate at which people give up job search seems similar among those with to those without jobs. Discouragement sets in as some individuals give up searching, but then falls as more individuals find more employment.

I discuss the trends in the main variables over time individually in section 5.2. For now it seems evident that the transport treatment, at each margin, pushes respondents away from discouragement, both into work, more permanent work, and into 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 to look at how long it took for the treatments to take effect, and how they took effect sooner among the *city* sample, than the *boards*.

One notable exception is the *board* sample, who seem to be less likely to be working for the middle weeks of the study. As I discuss in 5.2, this is consistent with a theory of respondents substituting low quality temporary work in favour of more intensive job search that they are actually interested in. In addition, as also discussed in greater detail shortly, treated individuals are less likely to be engaged in temporary work, while also *not* searching for a job at the same time. If taking temporary work in a profession that is not one's own, and then giving up trying to find a better job, can be considered as a form of discouragement, the treatment seems to have also prevented discouragement in this way, especially if job search is made more challenging

Table 6: Ordered Logistic Regression: Effect of treatment on labour market status

	(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

[1] 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

[2] Log-odds coefficients are reported

[3] All regressions include a full set of control variables.

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

or more time constrained while working. This effect seems to go away after by the time the transport subsidies are removed.

Among the *boards* sample, the number of people in permanent work gradually grows over the weeks. The total percentage of people working is given by the top of the blue *Temp Work (No search) bar*, and shows relatively little difference between treatment and control in the *boards* sample, aside from the weeks 5-7, as already discussed. Importantly, for the *board* sample, week 16 shows an increased probability of having a permanent job, a difference that I investigate in more detail in the next section.

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

where

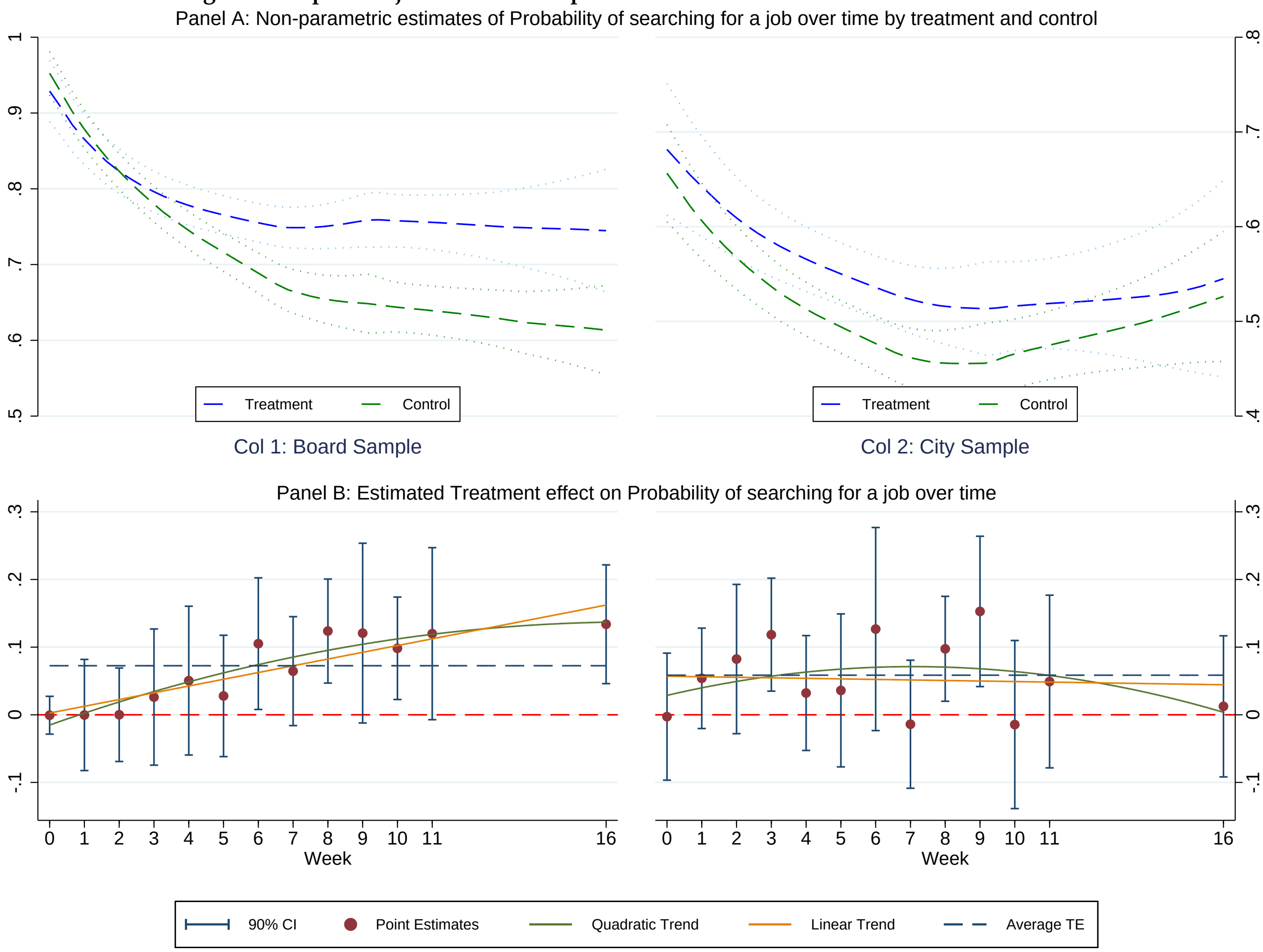
$$y_i^\star = T_i\lambda + X_{0i}\beta + \epsilon_{it}$$

I assess the impact of the transport treatment on the *distribution* of individuals across employment categories. I assume an ordinal ranking of job market outcomes equivalent to the one outlined for the tables above, where workers transition away from discouragement towards job search, employment and eventually permanent work. I estimate an ordered logistic model, specified by equation 6 for the two samples separately, the pooled samples, and for both the final week of the study, and all of the later weeks combined. The results clearly a show statistically significant impact of the treatment on ordered categorical variable, in the positive direction: of more job search, and better jobs, for both samples. The effect seems bigger, however, for the *boards* respondents.

5.2 Job Search Trajectories

How did the treatment impact the job market 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 over time?

Figure 4: Impact on job search: Non-parametric trends and treatment effects over time



$$y_{it} = \alpha_t + \sum_t T_i W_{it} \lambda_t + X_i \beta + \epsilon_{it}$$
$$y_{ist} = \alpha_{st} + \sum_s \sum_t T_i W_{it} S_{is} \lambda_{st} + X_i \beta + \epsilon_{it} \tag{7}$$

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 as week 1-11), and the final paper survey (week 16). As with each key job market outcome variable, I estimate the average impact on the probability of searching for a job across all 12 weeks combined. Using equation 7 outlined in the specifications section, I then estimate the treatment effect in each week separately. I estimate the trend over a time, estimating an intercept term, linear, quadratic and cubic trend terms, as in equation 8, given below.[36].

$$y_{it} = \alpha_t + T_i\lambda_0 + T_i w\lambda_1 + X_i\beta + \epsilon_{it}$$
$$y_{it} = \alpha_t + T_i\lambda_0 + T_i w\lambda_1 + T_i w^2\lambda_2 + X_i\beta + \epsilon_{it} \tag{8}$$
$$y_{ist} = \alpha_{ts} + \sum_s T_i S_{si}\lambda_s + X_{i0}\beta + \epsilon_{it} \qquad \forall t \neq 0 \tag{9}$$

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".[37]

Figure 4 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, showing how 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. Table C.6 in the appendix estimates (parametrically) these treatment effects over time, presenting both the control means (CM) of the dependent variable over all the weeks, and the linearly estimated treatment different between treatment and control from equation 7. This 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 particularly weeks during the study. I show results for the two samples pooled together, and for each separately.[38]

These weekly point estimates of the impact of the treatment in each week are plotted in the Panel B of Table 4, showing, for the board sample separately, a clear and persistent 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 an effect more immediately, at the beginning of the study period, and then remain at similarly high levels, with a decline towards the end (it is negligible in week 16) [39]. Panel B also overlays the linear and quadratic estimates of the trend in the treatment effect over time, confirming a mostly upward linear, and constant (flat), trajectory for the *board* and *city* sample, respectively. The quadratic term for the *city* sample is negative due, but is not statistically significant.

The estimates of the coefficients on these trajectory parameters are presented in Table in the appendix, and show for the *board* sample a statistically insignificant quadratic term, but a highly significant upward linear term, whereas for the *city* sample, I identify a (significantly) negative quadratic term. I also plot the average treatment effect across all 15 weekly observations, which is estimated and presented in the first row of Table , and is estimated using the 9 specification with the usual set of covariates [40].

[36]Cubic function estimates are largely not presented here, since they added little explanatory power to the trajectory estimates

[37]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

[38]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

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

[40]The results are similar for the ANCOVA, DD and FD estimators as well

The results suggest unambiguously that individuals in both samples were more likely to search for jobs while, and after, receiving the transport subsidies, but the trajectory of these impacts differ slightly between the samples. 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 more and more individuals become discouraged. For the *city* respondents, the effect seems to have been more immediate, but less consistent or persistent during the following weeks.

Indeed, it does look as if some individuals in the *city* sample were induced to begin job search, when they were not engaged in search at the time of the first interview. For the boards individuals, who were all searching for jobs to begin with, the treatment prevented the onset of discouragement, or encouraged the resumption of search activity after short periods of discouragement.

In the appendix similar tables and figures estimate similar impact trajectories for other search outcome variables from the phone call surveys. I find that the treatment had similar impacts on the probability of respondents searching for employment at the vacancy boards (see Figure B.5, Table C.8, Table C.9), with the difference between the two samples even more marked: among the boards sample the treatment effect is significant at the 10% for almost all individuals week after week, despite the small sample. The effect is not significant for the *city* sample, perhaps the boards were never likely to be their preferred method of search[41].

Search at the intensive or extensive margin: While selection effects make it hard to estimate whether job seekers searched more *intensively* after getting the transport subsidies (since the treatment induced respondents to search more, evidence suggests that it did not. The results presented in the appendix as tables C.11 and C.12 show a positive impact of treatment on the average number of days spent searching for work (and days visiting the boards in Table B.7), but are not so large that they could not be accounted for by the increase in the proportion of individuals searching for work. Indeed, estimates not presented here, show that there was no significant impact in the number of days searched, per individual that was searching[42].

Effects on employment over all weeks: Of course, the impacts of treatment on search intensity would be interpreted differently depending on whether they were driven by lower levels of discouragement (if the control group were not working or searching), or if they were driven by treated individuals putting off taking a job, such that the control group were more likely to be working, and thus likely to be searching. More importantly I am interested in whether the transport treatment allowed job seekers to find employment faster, or if they were able to avoid taking unwanted and demanding casual labour to support due the treatment.

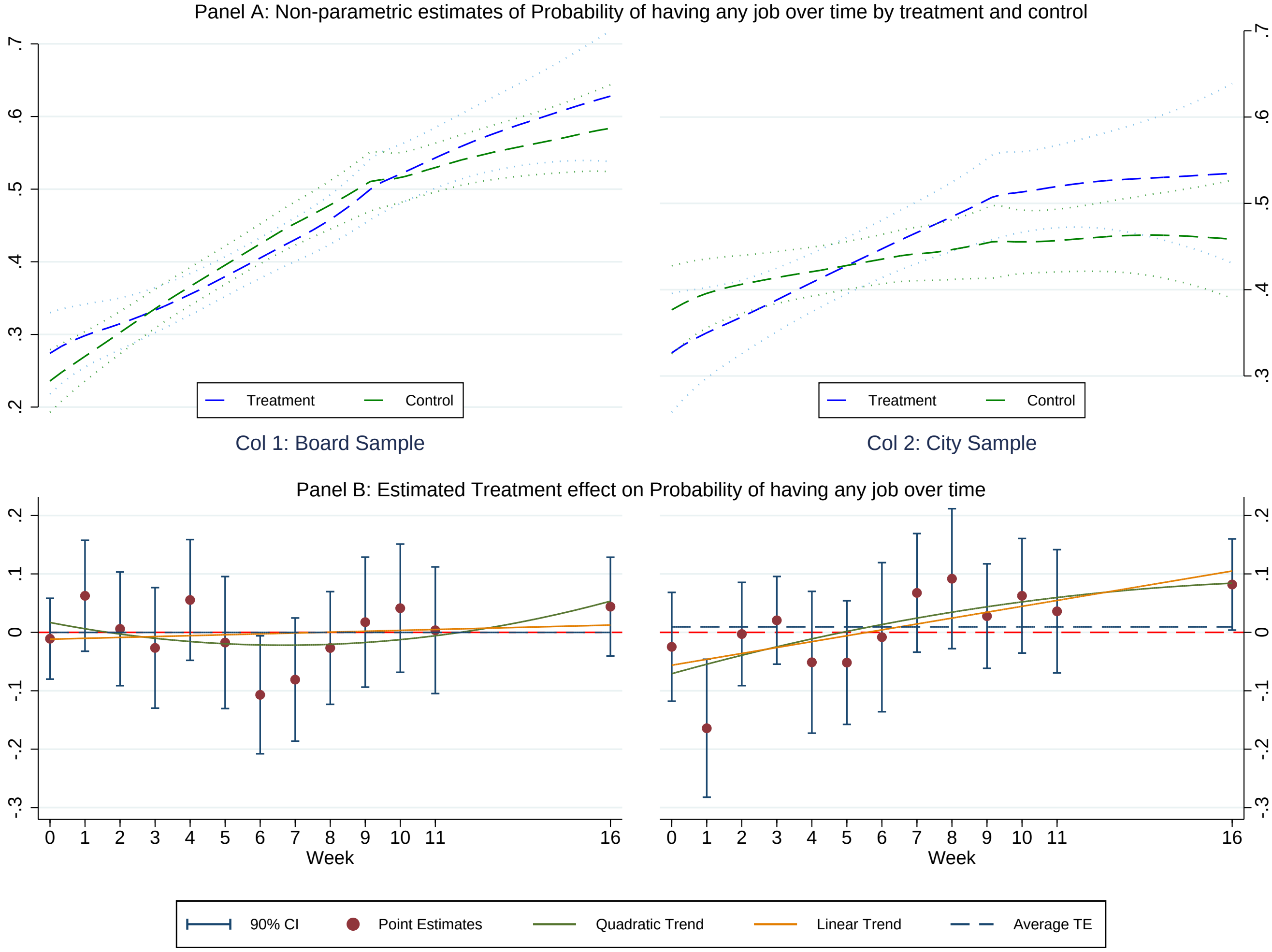
I find that for most weeks, the employment rates of the treated group are statistically indistinguishable from the control group for the boards group. These results are plotted in Figure 5 and presenting in tables in the appendix. However, there is evidence of a small dip in the middle of the study period, around the time when many individuals stopped searching for work, when the treatment group were less likely to be working. This was a time when most workers still only had casual or temporary work, and while only one of the coefficients is significant, there seems to be a clear and then rise again in the employment rate, hinting that treated individuals were able to avoid taking a temporary work, which might otherwise forced them to give up job search.

For the *city* sample, there also seems to be evidence of an initial fall in employment rates, with

[41] 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

[42] 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

Figure 5: Impact on having a job: Non-parametric trends and treatment effects over time



a strong upward linear trend, although the treatment group is only more likely to be working in the final week (16).

I find that the treatment does indeed prevent discouragement (see Table C.10, and Figure B.8 and Table C.10 in the appendix). For the *city* respondents these large and significant results seem driven both by an increased rate of employment, and higher rates of search among the unemployed, whereas for the *boards* the result is driven mostly by respondents.

The results in this section confirm the picture presented in figures and at the beginning of the results section showing the impact of treatment on the distribution of employment status. Treatment caused a gradual shift into better quality jobs (as presented in the previous section on endline outcomes), shifted the unemployed towards for jobs, as well as the employed who still wanted to find better employment, and lead to higher employment rates among the *city* respondents (in jobs that were of higher quality).

5.2.1 Impacts on search activities by Months

The tables and plots of weekly treatment effects presented in this section provide an insight into the trajectories that treated and untreated job seekers take. However, in small samples (often only 300-400 individuals were contacted by phone each week), large coefficients are often not statistically significant and subject to some random error which conceals evidence of an upward trend in the treatment effects. Yet the presence of these effects over a number of weeks suggest that they are unlikely to be purely due to random error. To confirm this, I pool observations into sets of consecutive four weeks, creating 3 successive *months*, to show clearly the increasing size and significance of the coefficients over time, for some of the outcome measures. Using these monthly treatment effects allows me to confirm the trajectories of the treatment effects, with considerably more power.

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

These results are presented, just focusing on the core labour market outcomes, in Table 7 using specification MON. The Results emphasis the trajectories illustrated in the figures above, with the treatment effects on search activity taking some time to take effect, and growing over time for the *boards* sample, whereas the impacts are seen as early as the first month for the *city* sample, but seem to have diminished by the third month. However, for the final month, both samples are significantly less likely to be discouraged. For the *boards* 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. The pooled results show many more significant results, simply due to the added power for pooling the samples together.

To allay fears that these months were chosen strategically to boost significance, I present complementary results were I *restrict* the sample weeks to groups of four weeks, starting with the first four weeks of the study, and iteratively move this window forward by one month and re-estimate the treatment effects using the basic COV estimator used before estimator. This provides a type of moving average monthly treatment effect, and shows the trajectory of treatment effects. These estimates are shown for the pooled sample in Table C.20, but the sample specific estimates are presented in the appendix. These results confirm that the treatment starts to work slowly, and is strong and significant for all the later groups of weeks. The coefficients are very similar to those presented for the corresponding monthly groupings of weeks, although not identical because of the inclusion of covariates which are estimated slightly differently in the restricted samples.

6 Mechanisms & Persistence

The results presented thus far show that treated individuals responded to transport subsidies by increasing their job search activities during the weeks that the subsidies were handed out. I have argued that these impacts can be explained by framework where highly cash constrained individuals are able to search for work more regularly and intensively, as they are buffered against the shocks of weekly consumption needs, and because they are able to save their cash while receiving subsidies and thus to search for jobs for longer. I use a heterogenous treatment effects analysis to provide additional evidence that the effects are be driven mostly by alleviating

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	-0.022	-0.016	0.044*	0.035	-0.014	0.17*
	(0.024)	(0.012)	(0.024)	(0.023)	(0.018)	(0.097)
month 2	-0.020	-0.007	0.075***	0.088***	-0.036**	0.090
	(0.024)	(0.012)	(0.024)	(0.023)	(0.018)	(0.098)
month 3	0.027	0.006	0.092***	0.090***	-0.073***	0.36***
	(0.029)	(0.014)	(0.028)	(0.028)	(0.022)	(0.12)
Panel B: Average Impacts By Month and Sample						
board month 1	0.011	-0.010	0.011	0.025	0.006	0.100
	(0.033)	(0.016)	(0.032)	(0.031)	(0.025)	(0.13)
board month 2	-0.059*	-0.013	0.077**	0.085***	-0.026	0.066
	(0.033)	(0.016)	(0.032)	(0.031)	(0.025)	(0.13)
board month 3	0.016	0.011	0.10***	0.11***	-0.073**	0.50***
	(0.039)	(0.019)	(0.038)	(0.036)	(0.029)	(0.15)
city month 1	-0.048	-0.018	0.072**	0.014	-0.038	0.220
	(0.036)	(0.018)	(0.035)	(0.033)	(0.027)	(0.14)
city month 2	0.029	-0.001	0.061*	0.074**	-0.044	0.070
	(0.036)	(0.018)	(0.035)	(0.033)	(0.027)	(0.14)
city month 3	0.041	-0.011	0.064	0.039	-0.065**	0.150
	(0.043)	(0.021)	(0.042)	(0.040)	(0.033)	(0.17)
Obs	5,011	5,011	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 (subcities within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

at a cash constraint, by showing the the impacts are largely concentrated among individuals who were poorer, or had less cash on hand at baseline. Still I look at few alternative mechanisms that could driving the results, by looking at the impacts of the subsidies on a variety of other outcomes, including attitudes, aspirations and reservation wages.

In the second part of this section I look at the persistence of the treatment effects, 6 months later.

6.1 Heterogenous Treatment Effects

This paper argues that access to jobs is constrained for certain individuals because they are cash constrained, have weak labour market attachment, or because they are recent migrants with weaker social networks. If these mechanisms are the constraints that are driving the impact of the transport subsidies, one should see that the estimated treatment effects are larger among those for whom the constraints are particularly large. To the extent that the variation in the constraint of interest, I can test for this: for instance by looking at treatment effects for individuals who were more or less cash constrained, separately, based on baseline measurements that proxy for cash constraints.

Firstly I estimate treatment effects for those above and below median household wealth, expenditure and savings measures, and for each sample separately. I find evidence that poorer individuals benefited more from the treatment than those who were not. Indeed individuals in the *board* sample who lived in wealthier households, and had high levels of savings, seem not to have benefited from the transport treatment at all in terms of finding permanent jobs. The results are strongly concentrated among poorer individuals. I do not find this same result for permanent work among those with low savings at baseline, but this group saw a disproportionately large impact on discouragement (there doesn't seem to be one for those with higher expenditure), partly because this group is more likely to be searching at endline as a result of the transport subsidies.

Table 8: Heterogenous Effects on Endline (week 16) 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
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
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
Observations	368	369	369	289	289	289

[1] Results are from OLS regressions on endline outcomes, details of the specifications titled are in the

[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

The results for the *city* sample are more mixed. It would see that individuals from wealthier households were helped more by the subsidies: 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. When I look baseline expenditure as a proxy for cash constraints, I find that those with low expenditure seem to have benefited more from the treatment.

In all, the results suggest that the transport subsidies were particularly helpful to individuals who were cash constrained at baseline, which fits with the theory that these are main constraints which the transport subsidy alleviated.

Secondly, I categorize individuals as having been employed for a "long duration" if they have spent more than 4 months without work.[43] 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. This seems to be the case for both samples, although the results are more striking for

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

the *boards* sample. This fits with the theory that individuals with relatively weak labour market attachment rely the most on active (costly) job search, and those stand to gain the most from subsidies.

I find that there is little heterogeneity by work experience in either sample, except that individuals that were initially inexperienced seem less likely to fall into discouragement after the 4 months.

Finally, I look at the impacts of the subsidies by education. I would be generally ambivalent about whether the highly educated would respond more or less to the treatment. There is little evidence that in this sample that educated individuals.

There seems to be little evidence of heterogeneity in search effort by education during the weeks of the phone call survey, all education levels seem to increase search intensity. However, the efficacy of job search could be different for different education levels, because some individuals are more employable than others.

The results suggest that the transport treatment has been most beneficial to those with higher education in terms of finding permanent jobs. This is because individuals with degrees are those most likely to be able to access permanent jobs, many of which require degrees as minimum standard. Thus impacts on finding permanent jobs were concentrated among those with degrees. In other words the subsidies were designed in such a way that they help individuals to *access* permanent work by visiting job boards, and thus were most likely to help individuals who were most likely to be able to use that access to find the jobs: those with degrees.

Table 9: Heterogenous Effects on Endline (week 16) 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

Individuals with diplomas and high school finishers do not seem to have benefited in terms of job outcomes. However, they are still far more likely to be searching for employment, suggesting that they are induced to search more because of the subsidies, but have not yet managed to find work, perhaps because the greatest constraint to them finding jobs is their lack of appropriate skills.

Interestingly individuals without high school degrees do seem to be more likely to have found work after receiving subsidies. It could be the case that the subsidies induced those with high school certificates and more used the subsidies to search more for hard to get permanent jobs- only those with degrees were more successful though- but for those without high-school degrees who have no intention for applying for jobs requiring high skill levels, the subsidies simply allowed them to search more, and over a greater geographical area, for temporary jobs, with which treated individuals were more satisfied at endline.

6.2 Attitudes, Aspirations and Reservation Wages

The results presented thus far, would be consistent with a story of credit constraints preventing poor job seekers from being able to invest in job search at an individually optimal level, with the change in the costs of search lowering that barrier to entry into the labour market, and making the returns to search higher relative to the outside option of temporary employment or doing nothing.

However, the persistence of these effects suggests a more nuanced story. For the credit constraint story to explain the persistence of the impacts, the treatment would have had to increase youth capital stocks, such as savings, allowing them to go on searching for longer.

Alternatively, the treatment may have induced some behavioural or learning impacts, whereby discouragement "scars" the unemployed; the transport subsidies prevent job seekers from slipping into dejection and pessimism, which means that they are less likely to be discouraged some time later. Another theory would be that the period of high search intensity, or at least as the results suggest, a *longer* period of sustained job search, teachers job seekers something about the nature of the job market, wages, and how to find find employment, which makes them in turn more likely to keep searching, particularly at the job boards, if that information is positive

Another story could also explain the persistence of the search intensity. 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. I have already argued temporary work was *reduced* by the treatment in the early weeks of the study. Indeed, many respondents, in focus group discussions, reported searching very intensely in bursts, but then becoming discouraged and ending their job search indefinitely. If the transport subsidy changed the calculus, at the margin, of entering into one of these phases of intensive job search, the treated individuals would be more likely to still be engaged in one of these phases at the endline.

This section will attempt to investigate possible *behavioural and financial* channels through which the treatment could be operating, hopefully to explain some of the main results found thus far.

A more simplistic theory would be that the estimated impacts are due simply to hawthorne effects or priming effects: the regular phone calls and attention given to the treated job seekers makes them feel the need to either falsely report increased search intensity, or to actually search more intensely because they are being observed. It is to this hypothesis that I turn to first.

6.2.1 Hawthorne Effects

The possible existence of Hawthorne effects presents a challenge to the results presented here so far, since the phone calls may have induced behavioural changes that are separate to the price effects of the transport subsidies. I am less worried about reporting bias in this context: the impacts on reported job search are clear in the weekly data which includes only individuals who were called. For the same reason, the impacts on job search trajectories are robust to this issue. Endline results could thus be biased by the fact that half of the control were not called, when all of the treatment group were. If repeated phone calls reinforced the idea that individuals should be searching for work, this might have induced increased job search or motivation in unobservable ways.

Experimental variation in the sample selected to participate in the phone call study allows me to test if the phone calls had a significant impact on endline outcomes, and if the subsidies had an impact at endline, independent of the calls. By controlling for both receipt of the phone calls, as well as the transport subsidies, I am able to isolate the impact of the calls from that of the subsidies, on endline employment outcomes. The results are presented in Table 10.

Table 10: 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

I find that, at endline, there are few if any statistically significant difference between those with phone calls to those who did not receive them, across a range of specifications. I estimate the effect of the phone calls, while simultaneously controlling for assignment to the transport treatment to confirm the treatment effect of the transport *relative to the other individuals who received the phone calls but not the transport*.

Thus, after controlling for the effect of transport, the phone call respondents do not look significantly different to those that didn't get the calls. This improves confidence that estimating the treatment effects of the transport at endline by pooling all of the controls (with and without calls) was legitimate, and the results found there are not driven by any effects of the phone calls.

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.

6.2.2 Savings and Money

In order to investigate whether the president impacts on job search are due to long term wealth effects of the transport money, I test for any impacts of the treatment on endline financial variables. I find no impacts. I look at current weekly expenditure on all goods, expenditure on transport (not presented here), money received in financial support (as a measure of dependence), and money in savings, in total and formal savings (in the bank).[44] I find no evidence that the transport subsidies improved respondents financial positions at endline, suggesting that this is not the cause of the persistent job search intensity.

Of course, many of these outcomes are undoubtedly effected by work status, which is an endogenous outcome that may have been impacted by job search. These these results must be viewed with caution, but I look at the different impacts on finanicial outcomes between those that did, and those did not have work at endline.

Furthermore I look at expenditure during the weeks of the transport subsidy. This results are presented in the appendix in Table C.22, and suggest that there was no consistent impact of subsidies on expenditure in those weeks. So if there were no changes in long term financial status due to the treatment, one might imagine the respondents spending all of the money that they were given at the time, on more trips to the center, and increased search intensity, without saving any of it. Indeed Figure B.10 in the appendix suggests that respondents did increase the number of trips they made to the city center as a result of the intervention.

6.2.3 Reservation Wages

Standard search theory suggests that reduced search costs ought to have an ambiguous impact on employment, or rates of finding jobs, because reduced costs both increase search intensity, but could also increase reservation wages, and thus delay entry to the job market. I test the assumption of this argument by estimating the impact of the treatment on reservation wages. I find little evidence of changes in reservation wages induced by the treatment. Figure B.11, in the appendix 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. No other coefficients in other weeks are significant either.

However, I have already argued that the reservation wage model of job search is not entirely applicable to the Ethiopian context, particularly for first time job seekers. There is a relatively small wage premium for higher education, and across different types of jobs. Certain types of jobs and occupations may come with them a promise of higher pay in the future, but job seekers are still not searching for first time jobs on the basis of pay per se. So while respondents may indeed be receiving job offers and rejecting them, they are likely not to be doing so on the basis of the wage offer, but rather on the type of employment being offered; namely whether the job is permanent, secure and/or respectable or white-collar.

[44] One might expect respondents who received the transport subsidies to save them. But if the transport subisdies allowed respondents to take less temporary work and thus had less income (which seems to be the case among the *boards* respondents, then this would not be the case.

Indeed the estimated increase in the reservation wage induced by treatment is a about 220 birr on average, not too different from the difference between the wages offered in permanent and temporary jobs. This results is consistent with the treated individuals preferring to search more intensively for permanent jobs, and thus adjusting their wage expectations up to reflect that preference.

However, I do some see impact of the treatment on perceptions of market wages among those in the *city* sample, perhaps reflecting some learning about the distribution from increased job search. So while, reservation wages stayed mostly constant for these individuals, their views of what they thought the average wage in the current market was, and what they thought was a *fair wage* for the available work would be, fell significantly as a result of the treatment. I hypothesize that this is the result of learning about the prevailing market wage among a group who searched more intensively. Indeed the greatest impacts of the treatment are among those who did find find work. Expectations about the prevailing market wages were simply too high, with most respondents from the *city* sample saying that they could expect to earn just over 1500 Birr per month in their chosen professions at baseline, when in reality those that found jobs were usually earning little over 1000 Birr.

6.3 Persistence

I have shown, in the section 4, that that there is some evidence that the treated individuals are more likely to have jobs 7 months after the treatment ended. They are not just quicker to enter work (by 4 months), but the control group still haven't caught up by 7 months later. This could be due to a number of a mechanisms driving the persistence of impacts.

(P1) If finding a good job requires the kind of regular and sustained job search. Given the poverty of the respondents and irregular cash needs, the control group never get this chance: the treatment isolated job seekers against shocks

(P2) Respondent who give up job search at some point could be less likely to pick up job search again some weeks later, so that the control group might not catch up because they are for likely to have fallen into discouragement. This could be the case, for instance, if respondents have a small window in which to find their dream job, if they run out of money during that time, they have to settle for other work

(P3) Having to take the temporary work mentioned in (W1) could prevent individuals from continuing to search, if they are time inconsistent: once they are earning at temporary jobs it's hard to quit, forego earnings temporarily to find a better job

(P4) If unemployment causes "scarring" in the sense that skills deteriorate with time out of the labour force, meaning that the control group are less employable because they do not find employment quickly enough.

(P5) If treatment effects on search intensity are persistent, through a mechanism related to (S3) above: namely that individuals are able to stay liquid and thus search for longer because of they are able to maintain their savings

(P6) There are encouragement or learning effects which keep respondents searching for longer even after the financial incentives have been removed.

If none of the impacts above were at work, or were all too weak, we might expect that treatment and control respondents who did not find good jobs after 4 months would return to similar levels of job search intensity and thus the control group would eventually "catch up" to the treatment group, as the productive individuals in the control group who did not find jobs immediately, slowly do during the weeks after treatment.

I will present some evidence for a number of these mechanisms being at work, and some evidence that others seem to be less prevalent than others. However, I prefer to remain largely agnostic about the main driving factors, as I have neither the data, nor the power, to sufficiently disentangle the effects.

6.3.1 Persistence of Search Impacts

One way that the treatment effects on job outcomes could have persisted for so long, is if the treatment group continued to search more intensely even after the subsidized ended, as suggested in point (P5) above. I test for this now: Because treatment durations differed from individual to individual randomly (some ended treatment in week 8, others in week 11), this endline survey was between 5 and 8 weeks after the treatment ended for treated individuals. One might expect any impact on employment to be persistent some time after the treatment as ended (someone getting a job to keep it for at least a few weeks, while the control may still not have found treatment), but for other behavioural outcomes such as job search effort it is not clear whether these impacts would persist after the treatment ended. If reducing transport costs simply increases the marginal benefit of search relative to other uses of time, such as leisure, then one would expect the impacts to end of the subsidies end. However, if the impacts are persistent, a different theory of change would be needed. In this section, I show that these impacts on search behaviour are persistent, at least until the end of the study.

Tables presented in previous sections, particularly on endline (week 16) outcomes, have already provided some evidence that the impacts of the transport program on job search are persistent. In the weekly treatment effect tables, the coefficient on week 16 (the endline) survey showed a significant effect on the endline probability of searching for a job (C.6), or searching at the job boards (C.8), mainly among the *board* sample. The *city* sample is marginally, but not significantly, more likely to be searching for work.

Specifications used until this point have considered individual i as treated (T_{it}) in week t if the person was offer the treatment at any time $\leq t$ (currently *or* in the past). I now estimate a new specification where I estimate the additional impact of having the treatment in that specific week, over and above the effect of being in the treatment group at all.

The dummy variable P_{it} is 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 "on", compared to the treatment effect of having treatment "here" for P_{it}.

$$y_{it} = \alpha_t + T_i\lambda + P_{it}\delta + X_{i0}\beta + \epsilon_{it} \qquad \forall t \geq 8 \qquad \text{(PERS)}$$

In this way, 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. Thus in each week 9-11 I can compare those who were still receiving treatment to those who had finished it. I test whether the treatment effects estimated thus far are dominated by the periods in which

Table 11: 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)						
TE on	0.061*	0.040	0.076*	0.068	-0.051*	0.042
	(0.034)	(0.026)	(0.041)	(0.044)	(0.029)	(0.14)
Panel A: Average Impacts over weeks 8 to 12						
TE here	-0.050	0.002	-0.022	-0.006	0.024	-0.200
	(0.047)	(0.027)	(0.047)	(0.040)	(0.029)	(0.20)
TE on	0.061	0.006	0.10***	0.085**	-0.082***	0.37**
	(0.040)	(0.025)	(0.037)	(0.040)	(0.023)	(0.17)
Panel C: Heterogenous Impacts at Follow up Survey (Week 16 only)						
TE on 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)
TE on 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)
Panel D: Heterogenous Impacts over weeks 8 to 12						
TE here 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)
TE here 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)
TE on 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)
TE on 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)
Obs	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.

individuals were actually receiving treatment, or if the treatment effects are similar (or in fact, even greater) in weeks after the treatment has ended, to when it was going on.

In the Table 11, I present the week 16 (endline) specific treatment effect in Panel, to show the persistence some weeks after the end of the programme when no one was still being treated, followed in Panel B with the PERS estimates for the later weeks of the survey (weeks 8 onwards). In Panel B, the coefficient given by *TE on* estimates the average difference in the dependent variable between the treatment and control group in the later weeks of the survey. The coefficient on *TE here* tests for an additional impact among those who are currently receiving the treatment (P_{it}). If the treatment effects are not persistent at all (they fall back to zero as soon as the treatment ends) the coefficient on *TE here* should be large and significant, accounting for all of the difference between treatment and control estimated thus far.

Instead, the opposite seems to be true. For the primary search variables on which I an impact of the treatment has already been hold the coefficient is large and significant 16 weeks after treatment ended (in Panel A) and in Panel B, the coefficients on *TE here* are consistently close to zero and not significant. This suggests no drop off in the increased search activity after the treatment ended for some individuals in weeks 9, 10 and 11. Further, treated individuals are considerably less likely to be discouraged after the treatment ends.

Panel *C* and *D* provide the same estimates of persistence. The standard impacts of the transport (*TE on*) in Panel C are familiar: more permanent jobs for the boards sample, more employment generally for the city sample, and more search activity among the boards individuals.[45] Among the boards sample, the impacts on increased search activity are persistent and strong. The impact of the treatment on reducing discouragement is persistent among the *city* sample, but this is less surprising since this is likely due to an increase in overall employment, rather than increases in search behaviour among the unemployed. Once again, the results seem to be driven from *having received at any time* the treatment, rather than currently being able to collect it.

7 Conclusion

This paper looks at the impact of high search costs on labour market outcomes for cash constrained youth in Addis Ababa, Ethiopoia. 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, due to the enormous expansion of the secondary and tertiary education system in Ethiopia.

It is also a market plagued by serious search frictions, in which 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 (often white collar jobs) that are found predominantly at the job boards near the center of town. These jobs pay more and are more secure. Job seekers therefore have to decide between looking for and/or taking temporary work, often in their local areas, often found through social contacts and which are easier to find, or spending a lot of time and effort searching for the jobs they really aspire to.

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 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. These transport subsidies were offered from between 8 to 11 weeks, also chosen randomly.

I take a split sample approach, surveying two different types of unemployed youth in Addis Ababa, and thus allowing 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. The *city sample* is made up of individuals often of lower educational attainment, that were taking fewer active steps to find work, generally relying on less formal methods of job search.

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 are found to increase job search intensity, for those with and without work, throughout the study. These impacts are persistent some time after the program was ended. Some results on the impacts of the treatment suggest that these persistent

[45] Recall that we did not find more search activity in the last month of the intervention in the city sample, the treatments were significant only in the middle of the study.

effects are not due to wealth effects (participants do not have more savings or expenditure at any point in the study), nor does the treatment seem to have effected aspirations or perceptions in sustained away. Although these results should be treated with caution.

The results found here support 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 them, as a direct result of their place of living and financial constraints. This suggests the idea of a spatial mismatch story in the large African capital, of Addis Ababa.

This suggests that labour markets could be made more efficient and equitable to the growing and aspirant urban population by reducing the costs of finding work, either through improved and subsidized transport for the poor, or more direct measures to make access to information about vacancies and employers more readily accessible.

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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, only 111 of them had been working when interviewed at the baseline, and only 55 had kept the sample jobs that had been working at at baseline (of the 186 working at baseline). 57 individuals had been working at baseline, but where no longer at the endline survey. Half of those who weren't initially working, had found work by the endline. And these transitions over 16 weeks miss the considerable changes that occurred during the intervening weeks.

Some individuals found very similar to work to work that they had done in past; they moved from one construction site to another, or found another short term contract in a factory near Addis Ababa. Others found completely new jobs in occupations that they had not worked in before, either because they had finally had a successful application for a job they had always wanted, or because they gave up searching for one type of work and tried something new. In some other cases, they had to settle for low pay, low skilled work, My sample represents a

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%
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%
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%
By Period of Migration to Addis Ababa (or born in 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%
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%
By Year of last Education attendence													
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%

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 the types of first, or entry level jobs. found by young people. Yet it still gives an overview of what jobs are like, what attitudes are to different types of labour, and who gets what types of jobs. The job market outcomes for different types of respondents, as well as the characteristics of different types of jobs found by respondents in the sample are described in detail. The first two descriptive statistics *job* and *Perm Job* simply give the percentage of a certain time of respondent have jobs or permanent jobs, respectively, whereas the later columns give the average statistics for respondents of a certain type who *have employment*. 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: 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. These jobs just seem to be hard to actually get.

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%
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%
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%
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%
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%
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%

The panel on education levels show the returns to education in Addis Ababa. Surprisingly the better individuals in my sample, at least at these first jobs, 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. 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.

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 afford more hours per work,[46] 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. Overall, individuals finding permanent

[46]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

worked on average more weeks over the 16 weeks of the study than those in temporary jobs or having to find casual work on a regular basis.

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 had work were working in construction[47]. Almost half of these jobs were casual labour jobs (individuals were paid daily, or piece rate salaries) and none of them were considered permanent jobs.

At the endline survey, things have not changed significantly. Table A.2 shows that 72 individuals had construction jobs, making 29% of all of the jobs. In addition, a further 6% of the sample were in other daily labour jobs, which often involved similar low skilled, hard labour. Only 8% of these construction jobs were considered permanent[48], and very people were recruited formally. Interestingly, very few of these jobs were found on the job boards, they tended to be found by going to visit worksites, or hearing about them through social networks.

And most interestingly, the wages paid in construction tend to be 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.

The government sector: Government jobs, and the civil service, are sought after by the youth in Addis Ababa. For a detailed history of the civil service in Ethiopia see (**?**). I distinguish between civil service jobs, usually office and administrative jobs, which are (or perhaps, used to be) highly prestigious, and routes to a middle class life (**?**), from any other kind of government employment, which may not be quite as sought after. 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

Yet, while not everyone is satisfied with civil service jobs, but they are far less likely to be dissatisfied with these jobs than other permanent jobs on average. However, 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.

I would speculate, based on the characteristics of these jobs, and from discussions with numerous individuals in my sample, some of whom were working in civil service jobs, that the preference for government jobs in due in large part from the permanence of these jobs.

B Appendix B: Charts and Images

[47] almost no women were working in construction

[48] These permanent construction jobs were usually jobs for managers or highly skilled machine operators, who kept the same job one construction company for some time

Figure B.1: Trajectory of Treatment effects across weeks in each sample

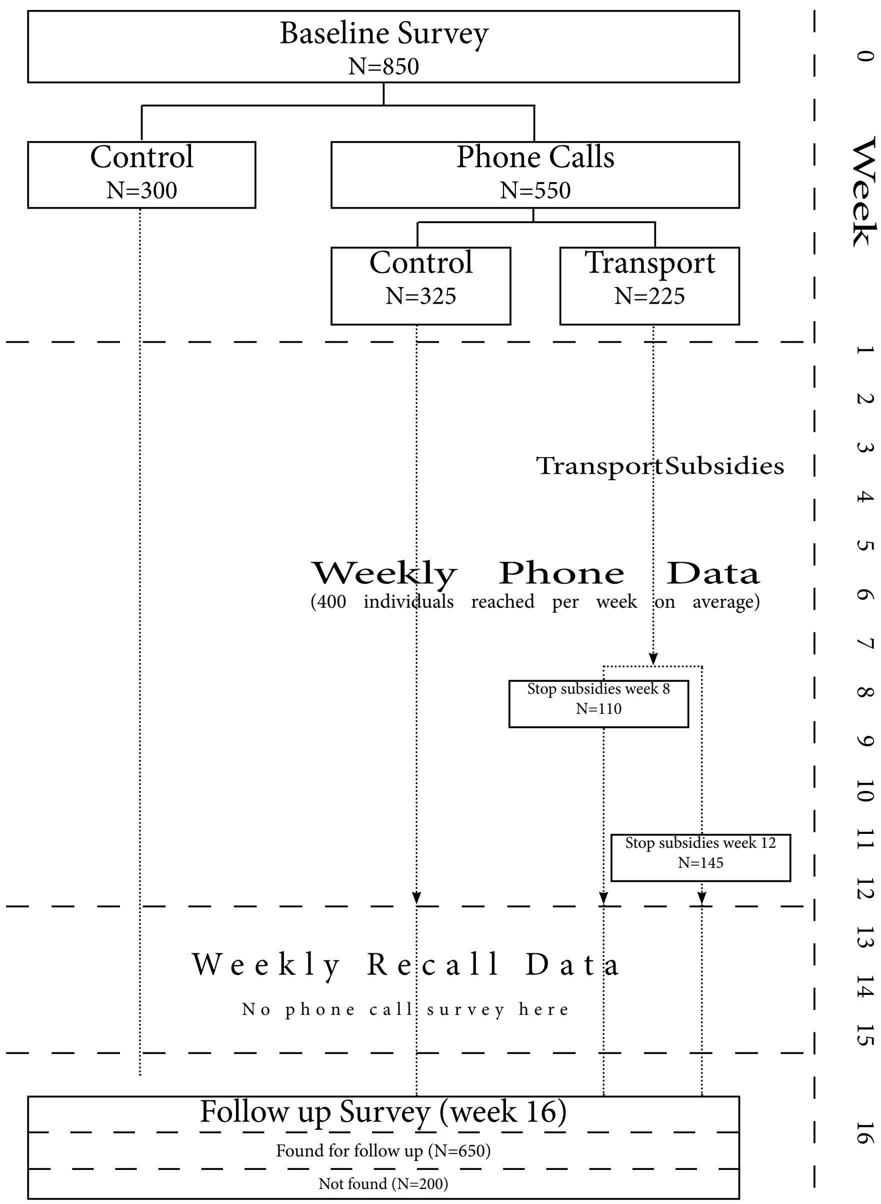


Figure B.2: Map of Addis Ababa showing sampling frame and selected EAs

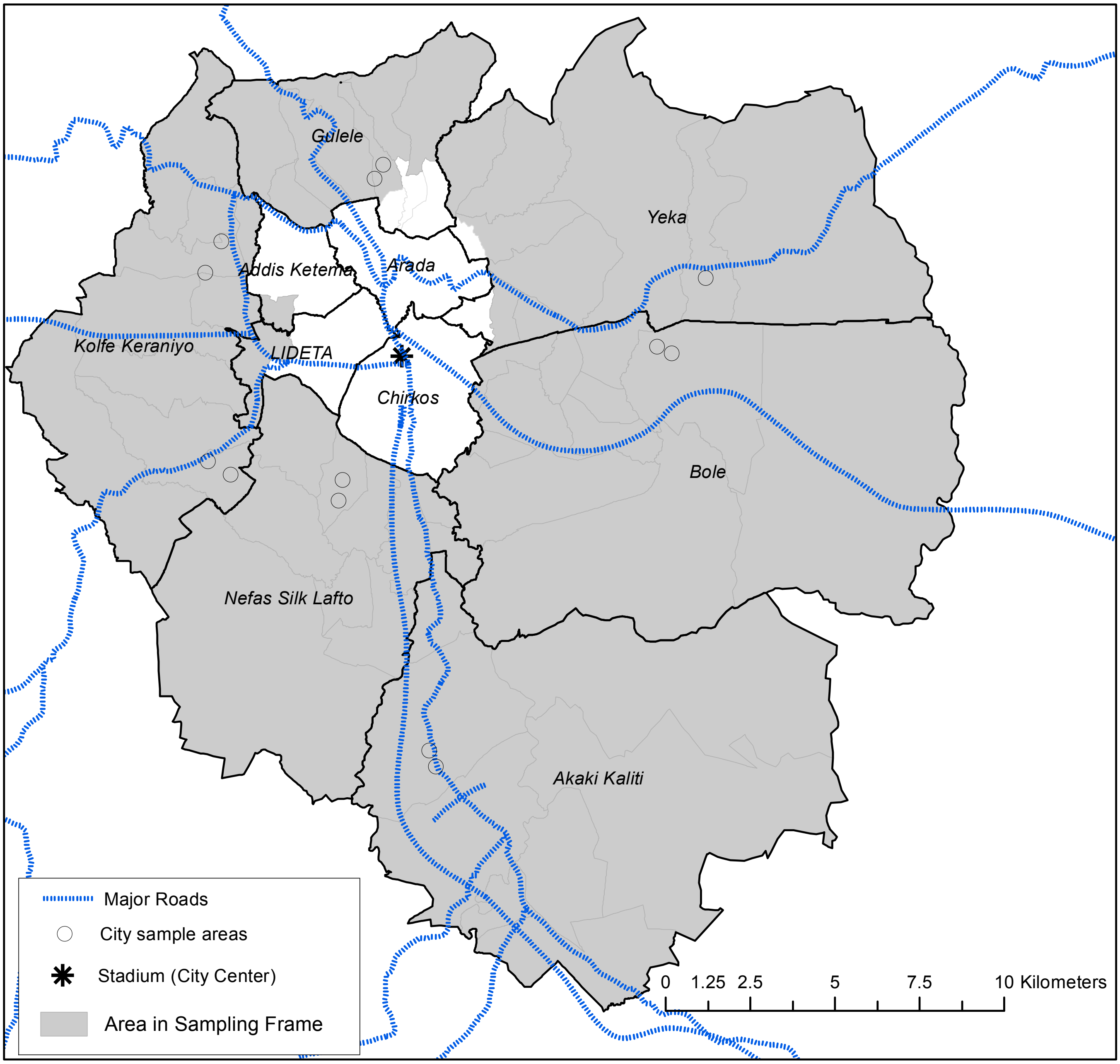


Figure B.3: Composition of the sample for each week by treatment and control: *Board Sample*

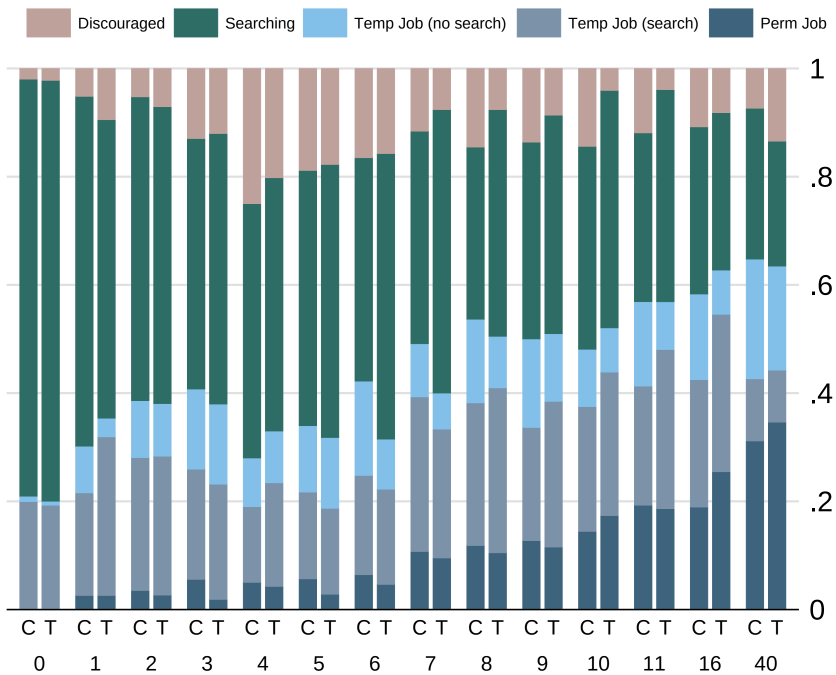


Figure B.4: Composition of the sample for each week by treatment and control: *City Sample*

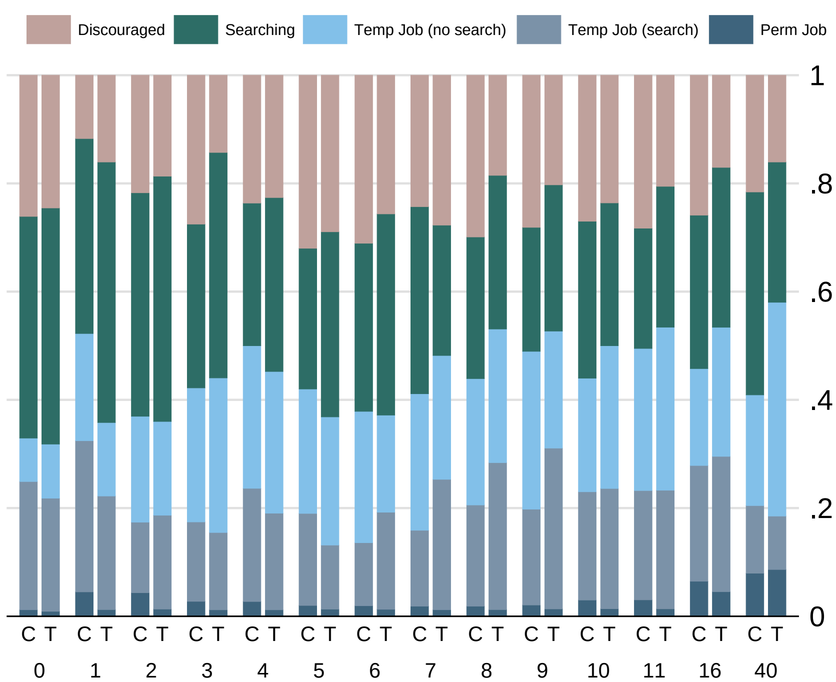


Figure B.5: Impact on visiting the job boards: Non-parametric trends & treatment effects over time

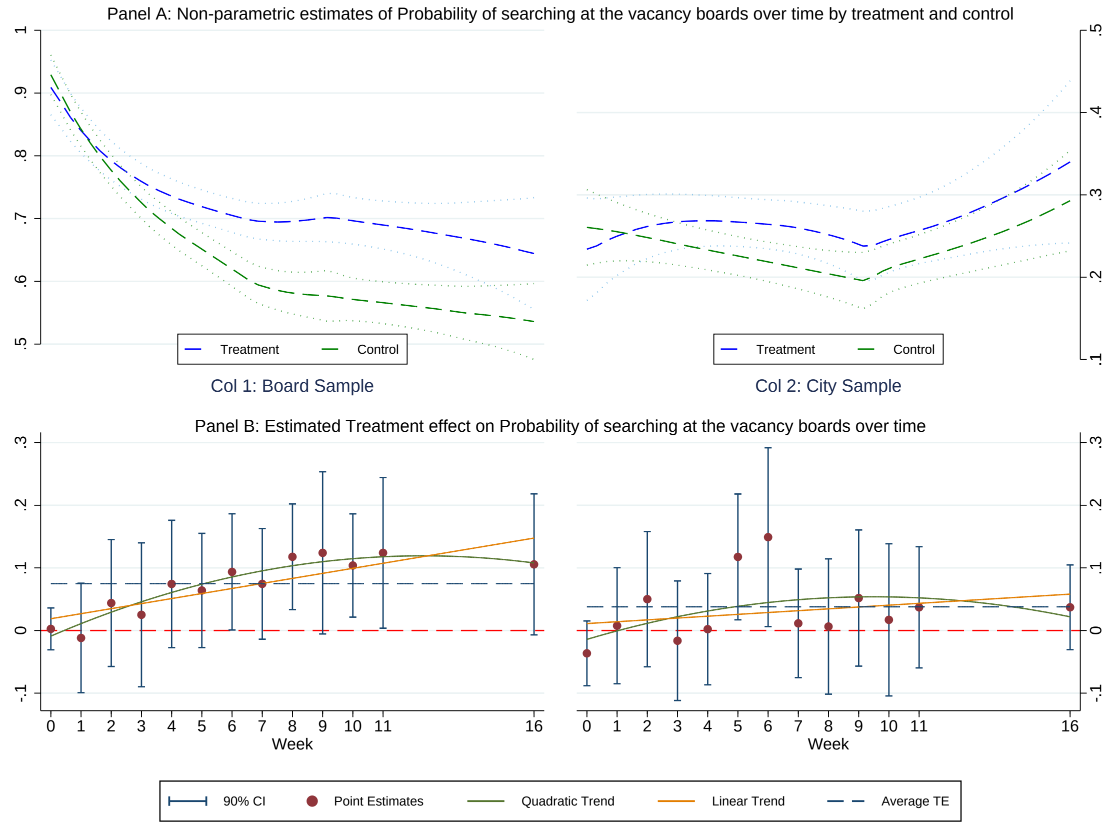


Figure B.6: Impact on days searching for work: Non-parametric trends & treatment effects over time

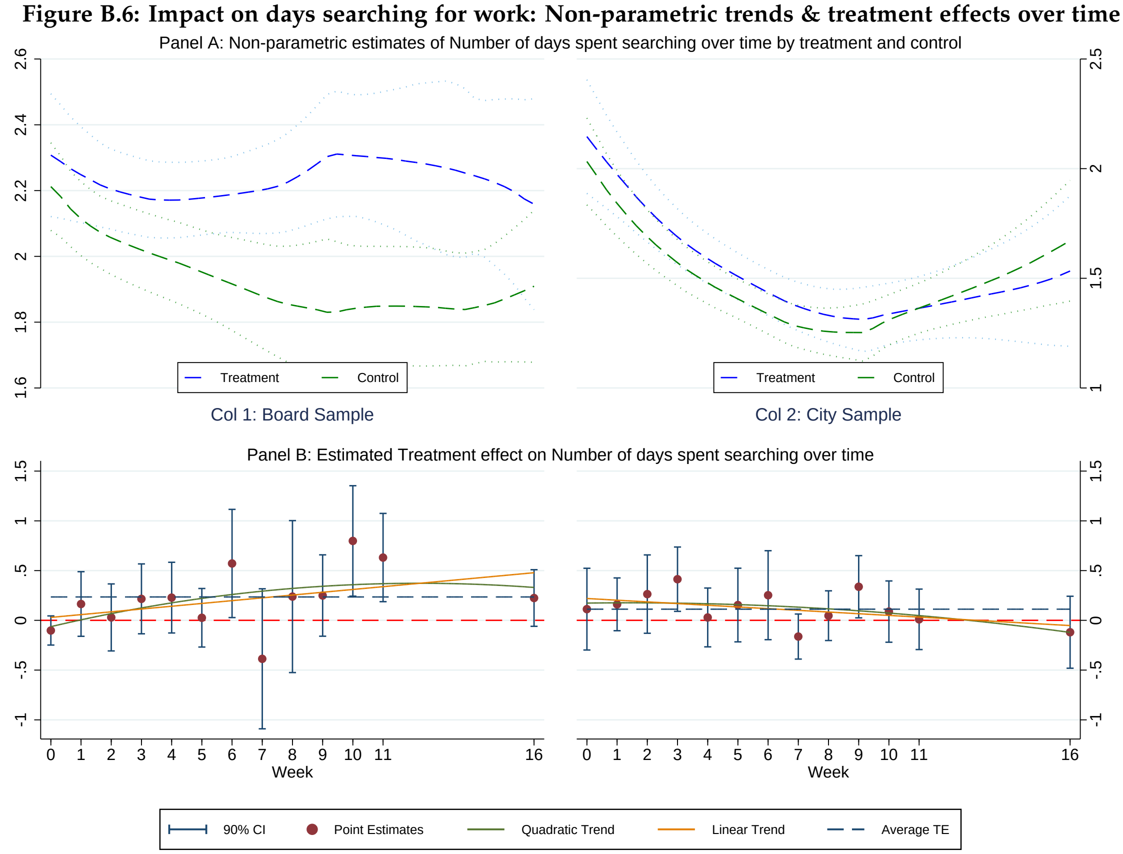


Figure B.7: Impact on days at the vacancy boards: Non-parametric trends & treatment effects over time

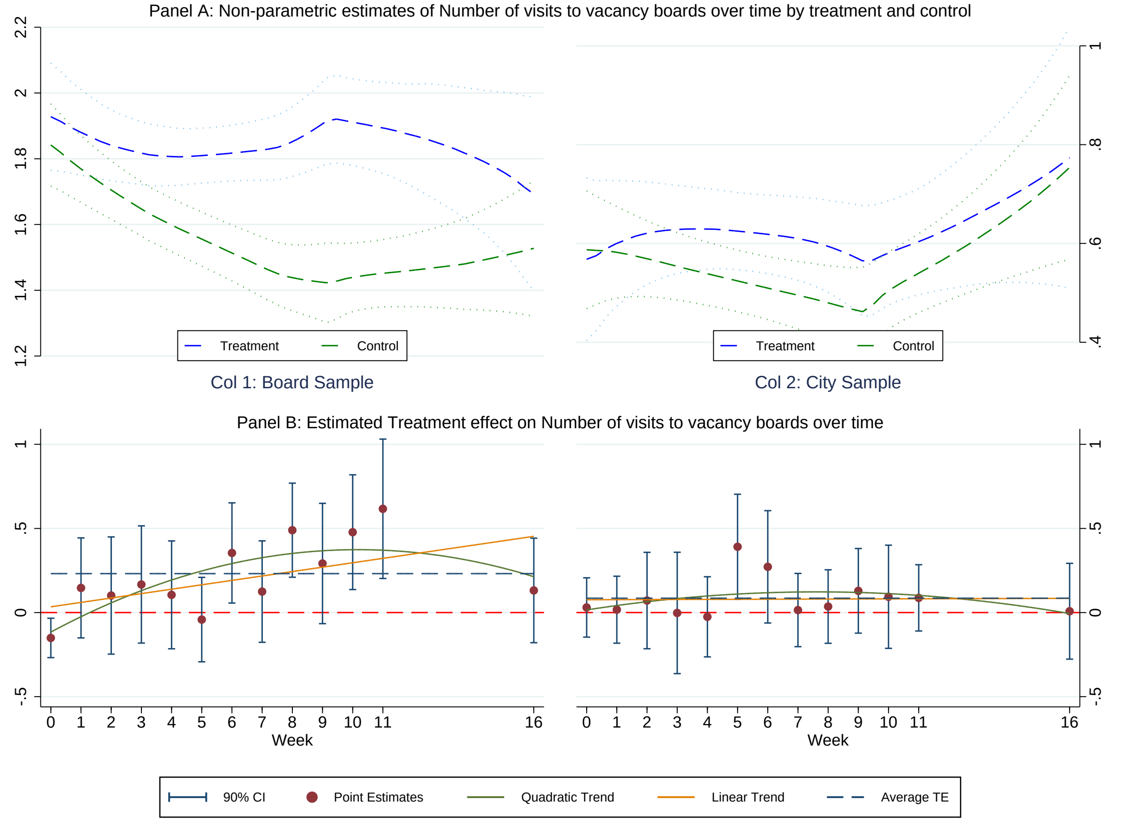


Figure B.8: Impact on discouragement: Non-parametric trends & treatment effects over time

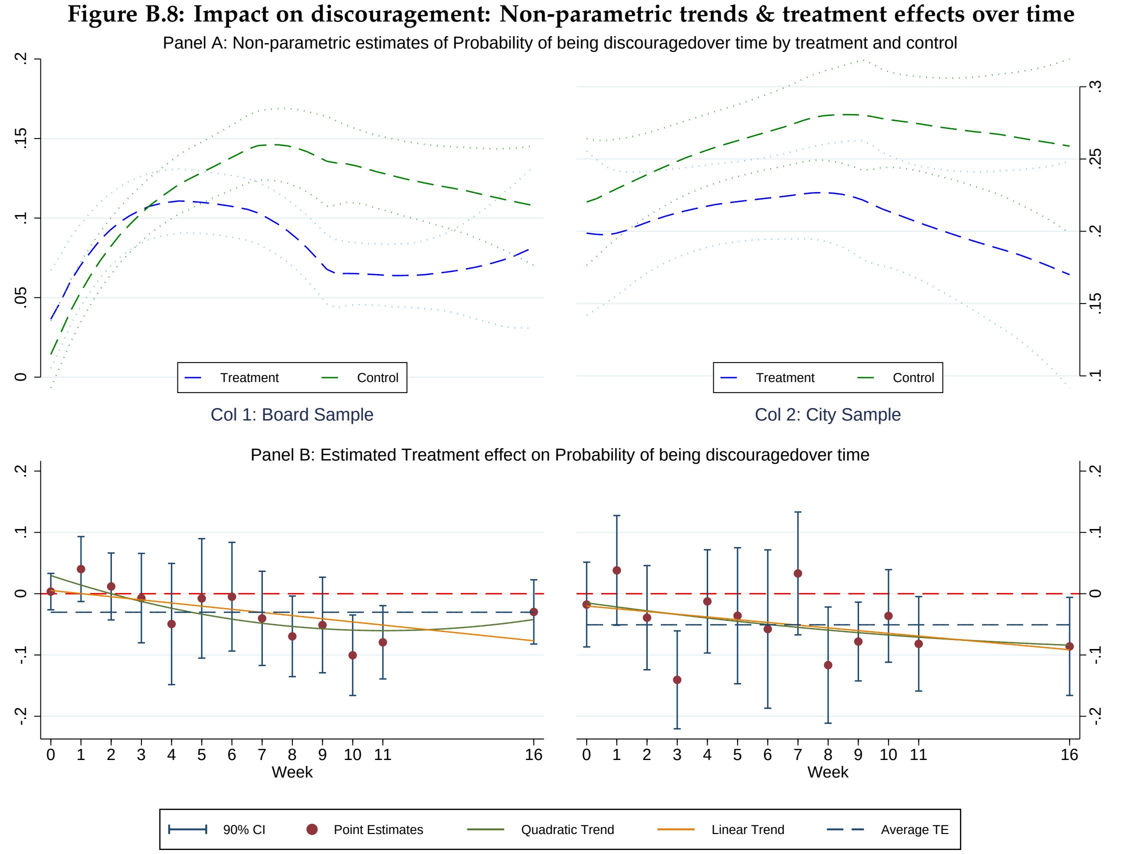


Figure B.9: Impact on Permanent Employment: Non-parametric trends & treatment effects over time

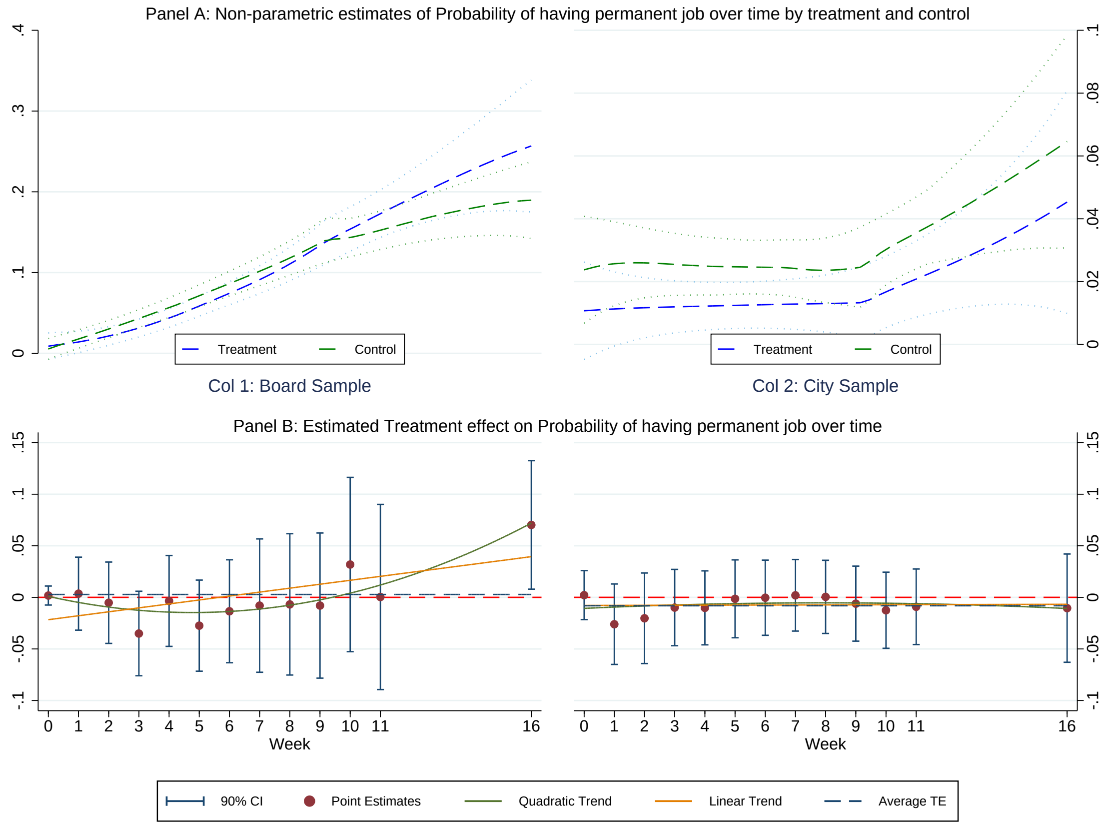


Figure B.10: Impact on Trips to central Addis: Non-parametric trends & treatment effects over time

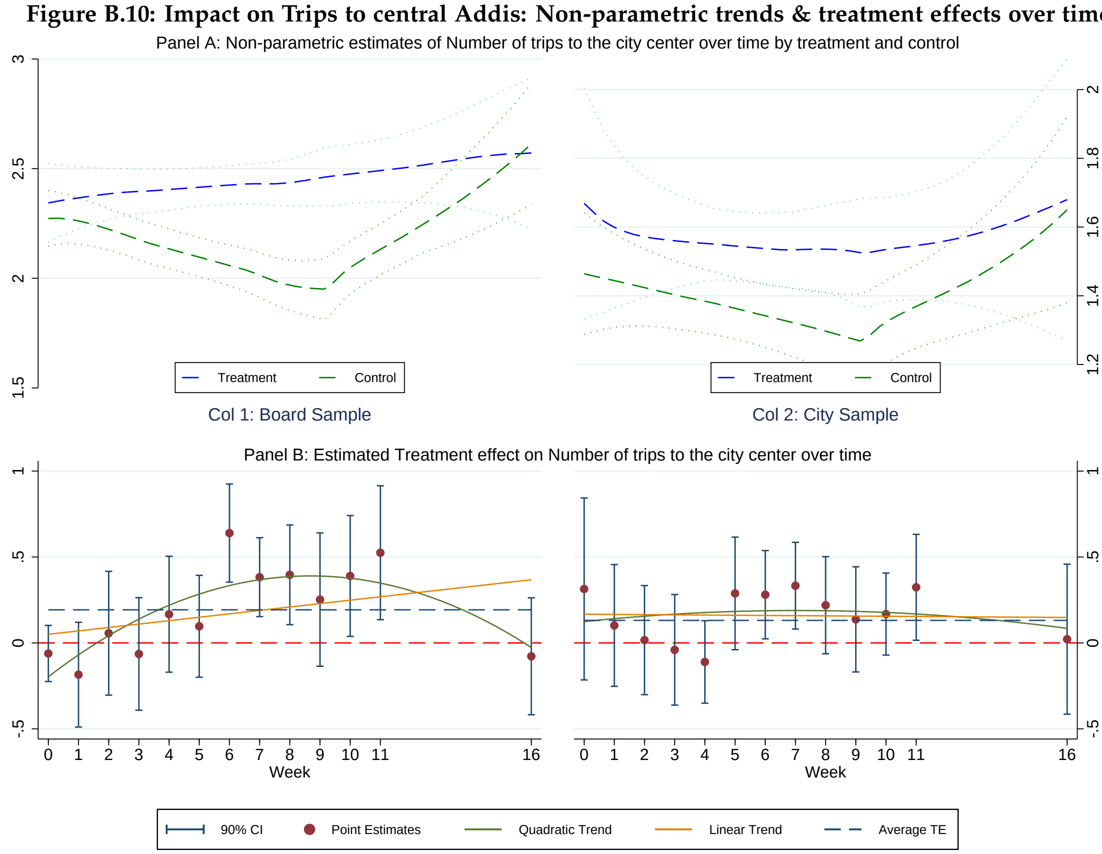
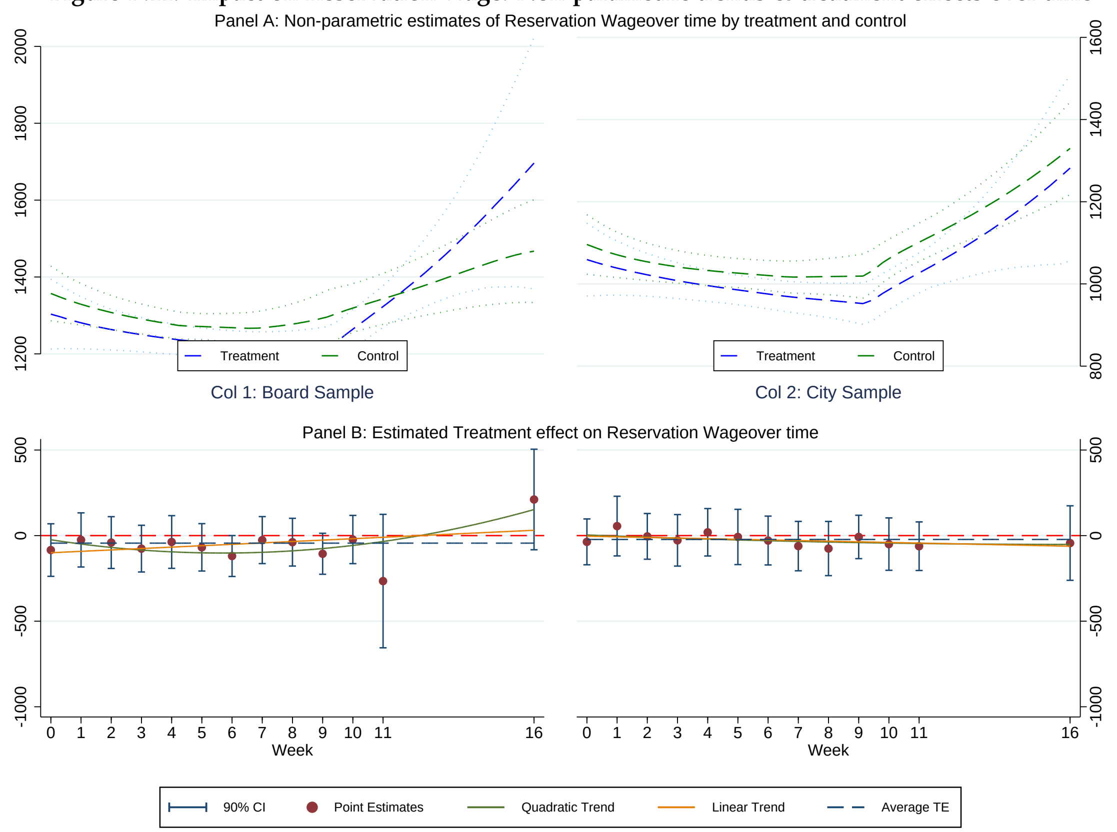


Figure B.11: Impact on Reservation Wage: Non-parametric trends & treatment effects over time



C Appendix C: Further Tables

Table C.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	diff	p-val	treat	cont	diff	p-val	treat	cont	diff	p-val
Sample	.539	.54	-8.2e-04	.982	1	1	0		0	0	0	
Work	.256	.258	-.0027	.934	.201	.201	8.4e-04	.983	.319	.326	-.007	.892
Permanent Work	.0039	.0065	-.0026	.643	0	0	0		.0084	.014	-.0056	.642
Searching	.829	.829	7.0e-04	.98	.971	.973	-.0018	.912	.664	.66	.0042	.935
Visisted Boards	.624	.628	-.0044	.902	.964	.958	.0059	.765	.227	.242	-.0152	.744
Discouraged	.12	.129	-.0091	.713	.0216	.018	.0036	.794	.235	.26	-.0244	.609
Hours Worked	7.38	6.06	1.32	.197	6.89	5.15	1.74	.207	7.95	7.13	.82	.588
Construction	.0891	.0905	-.0013	.95	.0935	.0749	.0187	.497	.084	.109	-.0247	.454
Female	.217	.223	-.0059	.848	.129	.132	-.0022	.948	.319	.33	-.0105	.838
Diploma	.205	.183	.0229	.431	.302	.287	.0147	.749	.0924	.0596	.0328	.238
Degree	.236	.242	-.0059	.853	.432	.44	-.0085	.866	.0084	.0105	-.0021	.845
Finish Gr 10	.783	.788	-.0054	.858	.928	.955	-.027	.232	.613	.593	.0205	.703
Age	23.7	23.4	.312	.162	23.9	23.6	.302	.27	23.5	23.2	.324	.371
Household Size	3.52	3.48	.038	.8	2.76	2.89	-.134	.414	4.41	4.18	.236	.321
Head of HH	.225	.223	.0019	.952	.302	.263	.0387	.392	.134	.175	-.041	.311
Amhara	.453	.496	-.0425	.252	.446	.494	-.048	.343	.462	.498	-.0361	.51
Oromo	.318	.3	.0173	.612	.388	.356	.0322	.509	.235	.235	2.1e-04	.996
Orthodox	.705	.721	-.0151	.652	.712	.698	.0146	.752	.697	.747	-.0499	.303
Muslim	.101	.113	-.0123	.595	.0432	.0719	-.0287	.244	.168	.161	.0067	.869
Lives with Family	.256	.268	-.0124	.706	.367	.383	-.0163	.739	.126	.133	-.0073	.844
Born out of Addis	.612	.612	1.3e-04	.997	.813	.814	-.0014	.971	.378	.375	.0027	.959
Recent Grad	.345	.401	-.0557	.123	.468	.551	-.0833	.0989	.202	.225	-.0229	.613
Work Experience	.523	.499	.0241	.517	.417	.389	.028	.571	.647	.628	.019	.719
Weeks w/o Work	37.6	40.4	-2.75	.409	37.3	34.4	2.93	.43	38	47.4	-9.4	.1
HH Wealth index	-.0149	.0143	-.0292	.695	-.112	-.0166	-.0953	.382	.0985	.0506	.0479	.628
Own Room	.229	.223	.0057	.853	.23	.201	.0296	.472	.227	.249	-.0222	.636
Kms from center	6.15	6.33	-.181	.467	6.4	6.86	-.456	.282	5.85	5.71	.142	.481
Weekly expenditure	179	152	26.9	.0352	202	174	28.8	.115	152	128	24.8	.152
Money from fam	84.9	75.1	9.83	.395	113	105	7.69	.657	52	39.6	12.4	.371
Reservation Wage	1225	1282	-57	.355	1326	1398	-71.6	.379	1106	1146	-39.6	.668
Observations	(258)	(619)			(139)	(334)			(119)	(285)		

Table C.2: Table C.1 (cont): Test for Balance across Samples and after Attrition

Panel B: Sample resurveyed at Follow Up

	Full Sample				*Boards Sample*				*City Sample*			
	treat	cont	diff	p-val	treat	cont	diff	p-val	treat	cont	diff	p-val
Sample	.556	.563	-.0075	.859	1	1	0		0	0	0	
Work	.242	.263	-.0206	.579	.182	.205	-.0228	.616	.318	.338	-.0201	.739
Permanent Work	.0051	.0065	-.0015	.824	0	0	0		.0114	.0149	-.0036	.812
Searching	.828	.826	.0022	.946	.964	.969	-.0055	.787	.659	.642	.0173	.778
Visisted Boards	.631	.65	-.0187	.647	.955	.954	8.8e-04	.971	.227	.259	-.0314	.571
Discouraged	.116	.126	-.0099	.723	.0273	.0193	.008	.632	.227	.264	-.0364	.514
Hours Worked	6.84	6.45	.392	.741	6	5.45	.55	.724	7.9	7.74	.161	.93
Construction	.0859	.087	-.0011	.963	.0818	.0656	.0162	.58	.0909	.114	-.0235	.554
Female	.207	.224	-.0168	.632	.127	.135	-.0079	.839	.307	.338	-.0315	.601
Diploma	.202	.185	.0172	.606	.282	.278	.0038	.94	.102	.0647	.0376	.269
Degree	.247	.252	-.0047	.899	.436	.44	-.0038	.947	.0114	.01	.0014	.914
Finish Gr 10	.818	.807	.0117	.727	.927	.961	-.0341	.165	.682	.607	.0749	.227
Age	23.8	23.6	.255	.301	23.8	23.7	.13	.653	23.9	23.4	.416	.326
Household Size	3.45	3.43	.0285	.869	2.68	2.88	-.202	.275	4.42	4.12	.296	.299
Head of HH	.258	.25	.0076	.838	.336	.282	.0545	.296	.159	.209	-.0499	.325
Amhara	.449	.509	-.0592	.164	.445	.498	-.0526	.356	.455	.522	-.0678	.29
Oromo	.348	.302	.0463	.242	.409	.34	.0693	.206	.273	.254	.019	.736
Orthodox	.717	.737	-.0198	.6	.709	.71	-.0013	.979	.727	.771	-.0439	.425
Muslim	.0859	.102	-.0163	.518	.0455	.0734	-.0279	.321	.136	.139	-.0029	.947
Lives with Family	.242	.261	-.0184	.619	.345	.363	-.0175	.749	.114	.129	-.0157	.711
Born out of Addis	.616	.622	-.0056	.893	.791	.803	-.0122	.79	.398	.388	.0097	.877
Recent Grad	.328	.389	-.0608	.139	.455	.541	-.086	.131	.17	.194	-.0236	.637
Work Experience	.495	.511	-.0159	.708	.391	.409	-.0184	.743	.625	.642	-.0168	.786
Weeks w/o Work	39	40	-1.04	.788	37.7	35.3	2.45	.564	40.6	46.1	-5.58	.417
HH Wealth index	-.0276	.0254	-.053	.547	-.171	.0025	-.174	.165	.152	.0549	.097	.422
Own Room	.247	.224	.0236	.511	.264	.208	.0551	.247	.227	.244	-.0165	.763
Kms from center	5.98	6.45	-.463	.106	6.09	6.94	-.854	.0709	5.85	5.8	.0442	.852
Weekly expenditure	183	155	28.5	.0422	206	166	39.4	.0327	156	140	15.2	.476
Money from fam	96.2	77.9	18.3	.197	123	107	16.4	.42	62.7	40.8	21.9	.236
Reservation Wage	1227	1288	-61.1	.379	1323	1400	-77	.434	1108	1145	-37	.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	diff	p-val	treat	cont	diff	p-val	treat	cont	diff	p-val
Sample	.557	.558	-8.9e-04	.982	1	1	0		0	0	0	
Work	.245	.264	-.0197	.57	.197	.219	-.0215	.62	.305	.322	-.0177	.751
Permanent Work	.0042	.0062	-.002	.737	0	0	0		.0095	.014	-.0045	.736
Searching	.823	.839	-.0161	.587	.97	.967	.003	.872	.638	.678	-.0395	.484
Visisted Boards	.629	.655	-.0263	.489	.962	.952	.0103	.641	.21	.28	-.0709	.175
Discouraged	.122	.122	4.6e-04	.986	.0227	.0222	5.1e-04	.974	.248	.248	-4.5e-05	.999
Hours Worked	7.15	6.25	.901	.407	6.62	5.32	1.3	.359	7.82	7.42	.398	.812
Construction	.097	.093	.0041	.861	.0985	.0778	.0207	.485	.0952	.112	-.0169	.647
Female	.232	.227	.0048	.886	.136	.137	-6.7e-04	.985	.352	.341	.0113	.843
Diploma	.207	.186	.0208	.507	.295	.289	.0066	.892	.0952	.0561	.0392	.196
Degree	.253	.246	.0073	.832	.447	.433	.0136	.796	.0095	.0093	1.8e-04	.988
Finish Gr 10	.793	.795	-.0022	.945	.932	.963	-.0311	.168	.619	.584	.0349	.552
Age	23.7	23.4	.233	.328	23.8	23.6	.244	.397	23.5	23.3	.22	.58
Household Size	3.51	3.49	.0272	.864	2.79	2.92	-.131	.45	4.43	4.21	.223	.388
Head of HH	.224	.223	4.9e-04	.988	.303	.256	.0475	.316	.124	.182	-.0584	.185
Amhara	.456	.492	-.036	.364	.447	.504	-.0567	.286	.467	.477	-.01	.867
Oromo	.333	.308	.0255	.49	.402	.356	.046	.372	.248	.248	-4.5e-05	.999
Orthodox	.705	.725	-.0206	.565	.705	.711	-.0066	.892	.705	.743	-.0382	.471
Muslim	.105	.114	-.0082	.744	.0455	.0704	-.0249	.333	.181	.168	.0127	.778
Lives with Family	.262	.273	-.0111	.752	.364	.381	-.0178	.729	.133	.136	-.0022	.957
Born out of Addis	.62	.612	.0087	.822	.818	.811	.0071	.865	.371	.36	.0116	.84
Recent Grad	.359	.403	-.0442	.253	.477	.556	-.0783	.14	.21	.21	-7.6e-04	.988
Work Experience	.506	.506	1.3e-04	.997	.409	.396	.0128	.806	.629	.645	-.0163	.777
Weeks w/o Work	37.3	40.6	-3.3	.349	37.7	33.9	3.79	.328	36.8	49	-12.2	.0489
HH Wealth index	-.0057	.0321	-.0378	.643	-.114	-.0028	-.112	.336	.131	.0761	.0549	.623
Own Room	.224	.211	.0129	.693	.227	.2	.0273	.529	.219	.224	-.0053	.916
Kms from center	6.17	6.39	-.221	.41	6.38	6.93	-.546	.22	5.91	5.72	.19	.383
Weekly expenditure	177	149	28.6	.0397	202	169	32.8	.0929	146	123	23.3	.222
Money from fam	90.4	74	16.4	.18	117	102	15.4	.39	56.5	38.7	17.8	.235
Reservation Wage	1207	1252	-45.7	.448	1321	1370	-49.6	.544	1064	1104	-40.3	.64
Observations	(237)	(484)			(132)	(270)			(105)	(214)		

Table C.3: Determinants of Staying in the last at Follow Up (Week 16)

	(1)	(2)	(3)	(4)	(5)
				Board Sample	City Sample
	atfu	atfu	atfu	atfu	atfu
trans board	0.016	-0.0058	-0.011	-0.012	
	(0.044)	(0.052)	(0.052)	(0.051)	
trans city	0.034	0.0044	0.0032		0.0013
	(0.045)	(0.041)	(0.038)		(0.039)
call board		0.038	0.041	0.048	
		(0.047)	(0.047)	(0.047)	
call city		0.063	0.079		0.085
		(0.058)	(0.055)		(0.056)
sample board	0.070	0.088	0.076		
	(0.042)	(0.068)	(0.074)		
Grade 0-9			-0.10*	-0.033	-0.18
			(0.053)	(0.11)	(0.12)
Secondary			-0.0097	0.087	-0.082
			(0.050)	(0.058)	(0.14)
Vocational			0.10	0.069	0.092
			(0.064)	(0.11)	(0.13)
Diploma			-0.039	-0.034	-0.052
			(0.047)	(0.051)	(0.18)
household index n			0.016	0.00074	0.034
			(0.015)	(0.020)	(0.027)
hhsize			-0.0022	-0.0093	0.00058
			(0.011)	(0.015)	(0.014)
female			0.026	0.014	0.042
			(0.037)	(0.056)	(0.047)
headofhh			0.12***	0.084	0.19**
			(0.046)	(0.058)	(0.080)
living relatives			-0.014	-0.016	0.014
			(0.039)	(0.046)	(0.090)
amhara			-0.0039	-0.021	-0.0011
			(0.034)	(0.053)	(0.049)
orthodox			0.052	0.039	0.060
			(0.036)	(0.047)	(0.063)
birth migrant			0.014	-0.085	0.062
			(0.043)	(0.064)	(0.057)
age			0.0072	0.0099	0.0047
			(0.0064)	(0.0090)	(0.0088)
experience			-0.017	0.025	-0.039
			(0.033)	(0.040)	(0.050)
work			-0.0016	-0.031	0.013
			(0.034)	(0.044)	(0.052)
work perm			0.081		0.089
			(0.18)		(0.18)
married			0.015	-0.044	0.036
			(0.040)	(0.068)	(0.053)
sincegrad years			0.0059	-0.020*	0.017**
			(0.0062)	(0.011)	(0.0073)
Constant	0.71***	0.67***	0.45***	0.62***	0.46*
	(0.034)	(0.060)	(0.16)	(0.23)	(0.22)
Observations	877	877	877	473	404
R-squared	0.006	0.009	0.046	0.035	0.084
F-test	0.36	0.53	1.56	0.91	5.28
Prob > F	0.70	0.71	0.084	0.58	0.0026

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

Table C.4: Determinants of Staying in the last at Second Follow Up (Week 40)

	(1)	(2)	(3)	(4)	(5)
				Board Sample	City Sample
	atfu	atfu	atfu	atfu	atfu
trans board	0.018	-0.014	-0.013	-0.0092	
	(0.039)	(0.046)	(0.047)	(0.049)	
trans city	0.063	0.012	0.0080		0.015
	(0.075)	(0.085)	(0.083)		(0.087)
call board		0.055	0.055	0.054	
		(0.053)	(0.052)	(0.051)	
call city		0.11	0.12		0.11
		(0.075)	(0.075)		(0.075)
sample board	0.11***	0.15**	0.19***		
	(0.037)	(0.059)	(0.067)		
Grade 0-9			-0.025	-0.19**	-0.099
			(0.065)	(0.094)	(0.23)
Secondary			-0.020	-0.026	-0.13
			(0.066)	(0.085)	(0.23)
Vocational			0.015	0.0089	-0.060
			(0.075)	(0.11)	(0.24)
Diploma			-0.061	-0.050	-0.23
			(0.052)	(0.055)	(0.23)
household index n			0.00058	-0.011	0.043
			(0.018)	(0.023)	(0.036)
hhsize			0.014	0.013	0.0092
			(0.0098)	(0.020)	(0.012)
female			0.043	-0.084	0.11*
			(0.045)	(0.082)	(0.059)
headofhh			0.033	-0.046	0.15
			(0.056)	(0.069)	(0.097)
living relatives			-0.039	-0.061	0.0083
			(0.043)	(0.057)	(0.074)
amhara			0.0062	0.0055	-0.0077
			(0.030)	(0.041)	(0.050)
orthodox			0.016	0.046	-0.019
			(0.036)	(0.047)	(0.059)
birth migrant			-0.0097	-0.074	0.0058
			(0.049)	(0.065)	(0.073)
age			0.0023	0.00030	0.0090
			(0.0058)	(0.0087)	(0.0077)
experience			0.014	0.026	-0.014
			(0.034)	(0.050)	(0.050)
work			0.024	0.066	-0.0039
			(0.039)	(0.047)	(0.061)
work perm			-0.052		-0.044
			(0.17)		(0.18)
married			0.023	0.0078	-0.0041
			(0.049)	(0.073)	(0.071)
sincegrad years			-0.0046	0.0032	-0.0094
			(0.0069)	(0.011)	(0.0083)
Constant	0.62***	0.56***	0.44**	0.73***	0.43
	(0.030)	(0.050)	(0.17)	(0.23)	(0.34)
Observations	877	877	877	473	404
R-squared	0.013	0.019	0.029	0.035	0.043
F-test	0.46	1.12	1.15	1.88	5.58
Prob > F	0.63	0.36	0.33	0.036	0.0020

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

Table C.5: Effects of treatment on Job Quality and Type at Endline (BAS)

	(1) work	(2) casual	(3) ln wage	(4) hours	(5) degree	(6) in office	(7) pay monthly	(8) satisfied	(9) formally	(10) 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)
Heterogeneity 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)
Obs	658	596	356	656	596	596	596	596	596	596

[1] Results are from Difference OLS regressions on endline outcomes, details of the specifications can be found in the section on heterogeneity. Column (1) presents average ITT effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

[2] 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

[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 C.6: Effects of treatment on Job Search in each week

	(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] 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. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

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

Table C.7: Trends in the of treatment on Job Search 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 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.8: Effects of treatment on visiting the vacancy boards

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.650	-0.016	0.960	0.260	0.003	-0.037
		(0.024)			(0.021)	(0.033)
week 1	0.550	0.015	0.820	0.260	-0.012	0.008
		(0.037)			(0.053)	(0.056)
week 2	0.500	0.056	0.720	0.240	0.044	0.050
		(0.046)			(0.062)	(0.066)
week 3	0.420	0.015	0.630	0.220	0.025	-0.016
		(0.047)			(0.070)	(0.058)
week 4	0.410	0.041	0.550	0.280	0.074	0.002
		(0.042)			(0.062)	(0.054)
week 5	0.410	0.086**	0.590	0.220	0.064	0.12*
		(0.041)			(0.055)	(0.061)
week 6	0.390	0.12**	0.560	0.200	0.094	0.15*
		(0.051)			(0.056)	(0.087)
week 7	0.420	0.051	0.640	0.200	0.074	0.011
		(0.041)			(0.054)	(0.053)
week 8	0.390	0.069	0.560	0.210	0.12**	0.006
		(0.042)			(0.051)	(0.066)
week 9	0.370	0.099*	0.540	0.180	0.120	0.052
		(0.054)			(0.079)	(0.066)
week 10	0.420	0.072	0.630	0.210	0.10**	0.017
		(0.045)			(0.050)	(0.074)
week 11	0.380	0.090*	0.550	0.180	0.12*	0.037
		(0.048)			(0.073)	(0.059)
week 16	0.430	0.072*	0.540	0.290	0.110	0.037
		(0.043)			(0.068)	(0.041)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is a dummy variable equal to one if the individual reported having visited the job vacancy boards 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] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.9: Trends in the Effects of treatment on visiting the vacancy boards

	(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 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.10: Effects of treatment on Discouragement in each week

	(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 take some step to look 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] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.11: Effects of treatment on number of days searched in the last 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 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] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.12: Trends in the Effects of treatment on 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] 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 the number of days an individual reported searching for work in the last 7 weeks.
[3] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.13: Effects of treatment on Having a job in each week

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.260	-0.013	0.200	0.330	-0.013	-0.012
		(0.037)			(0.042)	(0.060)
week 1	0.410	-0.048	0.300	0.520	0.063	-0.16**
		(0.047)			(0.058)	(0.072)
week 2	0.380	0.003	0.390	0.370	0.006	-0.003
		(0.041)			(0.059)	(0.054)
week 3	0.410	-0.007	0.410	0.420	-0.026	0.021
		(0.040)			(0.063)	(0.046)
week 4	0.400	-0.007	0.280	0.500	0.056	-0.051
		(0.047)			(0.063)	(0.074)
week 5	0.380	-0.036	0.340	0.420	-0.017	-0.052
		(0.047)			(0.069)	(0.064)
week 6	0.400	-0.063	0.420	0.380	-0.11*	-0.008
		(0.048)			(0.061)	(0.078)
week 7	0.450	-0.012	0.490	0.410	-0.081	0.068
		(0.047)			(0.064)	(0.062)
week 8	0.490	0.030	0.540	0.440	-0.027	0.092
		(0.047)			(0.059)	(0.073)
week 9	0.500	0.022	0.500	0.490	0.018	0.028
		(0.044)			(0.068)	(0.054)
week 10	0.460	0.053	0.480	0.440	0.042	0.062
		(0.045)			(0.067)	(0.060)
week 11	0.530	0.021	0.570	0.490	0.004	0.036
		(0.047)			(0.066)	(0.064)
week 16	0.530	0.060*	0.580	0.460	0.044	0.082*
		(0.035)			(0.051)	(0.047)
Obs	(5,752)		(5,752)			

[1] 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. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.
[2] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.14: Trends in the Effects of treatment on having a job in each week

	(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 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.15: Effects of treatment on having a Permanent job in each week

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.006	0.002	0.000	0.013	0.002	0.003
		(0.0083)			(0.0056)	(0.015)
week 1	0.035	-0.014	0.026	0.045	0.004	-0.026
		(0.018)			(0.022)	(0.024)
week 2	0.039	-0.013	0.035	0.043	-0.005	-0.020
		(0.019)			(0.024)	(0.027)
week 3	0.041	-0.025	0.056	0.028	-0.035	-0.010
		(0.018)			(0.025)	(0.022)
week 4	0.038	-0.007	0.050	0.027	-0.003	-0.010
		(0.018)			(0.027)	(0.022)
week 5	0.039	-0.018	0.057	0.020	-0.027	-0.001
		(0.019)			(0.027)	(0.023)
week 6	0.042	-0.008	0.064	0.019	-0.013	-0.000
		(0.020)			(0.030)	(0.022)
week 7	0.064	-0.002	0.110	0.019	-0.008	0.002
		(0.023)			(0.039)	(0.021)
week 8	0.069	-0.001	0.120	0.019	-0.007	0.000
		(0.024)			(0.042)	(0.022)
week 9	0.078	-0.004	0.130	0.021	-0.008	-0.006
		(0.025)			(0.043)	(0.022)
week 10	0.088	0.017	0.140	0.030	0.032	-0.013
		(0.029)			(0.051)	(0.022)
week 11	0.120	0.003	0.190	0.030	0.000	-0.009
		(0.032)			(0.055)	(0.022)
week 16	0.130	0.033	0.190	0.065	0.070*	-0.010
		(0.027)			(0.038)	(0.032)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is a dummy variable equal to one if the individual reported having a permanent job in the last week. Column (1) presents average effects across the full sample, while Column (2) estimates different coefficients for the two subsamples.

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

Table C.16: Trends in the Effects of treatment on having a Permanent job in each week

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	-0.002	-0.015	-0.004	0.003	-0.022	0.001	-0.008	-0.008	-0.010
	(0.017)	(0.014)	(0.011)	(0.025)	(0.020)	(0.015)	(0.022)	(0.018)	(0.016)
Treat X Time		0.002	-0.003		0.004	-0.007		0.000	0.001
		(0.0016)	(0.0058)		(0.0026)	(0.010)		(0.0014)	(0.0028)
Treat X TimeSq			0.000			0.001			-0.000
			(0.00037)			(0.00064)			(0.00019)
CM	0.071			0.110			0.033		
Obs	5,010	5,751	5,751	5,010	5,751	5,751	5,010	5,751	5,751
R^2	0.164	0.158	0.159	0.164	0.159	0.160	0.164	0.159	0.160

[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 permanent job in the last 7 weeks.

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

Table C.17: Effects of treatment on being Discouraged in each week

	(1) Pooled Effects		(2) Effects by Sample			
	Pool CM	Pool TE	Board CM	City CM	Board TE	City TE
week 0	0.130	-0.011	0.018	0.270	0.003	-0.029
		(0.022)			(0.018)	(0.041)
week 1	0.084	0.043	0.052	0.120	0.040	0.038
		(0.029)			(0.032)	(0.054)
week 2	0.130	-0.012	0.053	0.220	0.012	-0.039
		(0.029)			(0.033)	(0.052)
week 3	0.200	-0.069*	0.130	0.280	-0.007	-0.14***
		(0.035)			(0.044)	(0.049)
week 4	0.240	-0.026	0.250	0.240	-0.050	-0.013
		(0.040)			(0.060)	(0.051)
week 5	0.250	-0.020	0.190	0.320	-0.008	-0.036
		(0.044)			(0.059)	(0.067)
week 6	0.240	-0.030	0.170	0.310	-0.005	-0.058
		(0.046)			(0.054)	(0.079)
week 7	0.180	-0.008	0.120	0.240	-0.040	0.033
		(0.038)			(0.047)	(0.061)
week 8	0.220	-0.092***	0.150	0.300	-0.070*	-0.12**
		(0.034)			(0.040)	(0.058)
week 9	0.200	-0.065**	0.140	0.280	-0.051	-0.078**
		(0.032)			(0.047)	(0.039)
week 10	0.210	-0.074**	0.140	0.270	-0.10**	-0.036
		(0.031)			(0.040)	(0.046)
week 11	0.200	-0.083***	0.120	0.280	-0.079**	-0.082*
		(0.028)			(0.036)	(0.047)
week 16	0.170	-0.054*	0.110	0.260	-0.030	-0.086*
		(0.029)			(0.032)	(0.049)
Obs	(5,752)		(5,752)			

[1] Dependent Variable is a dummy variable equal to one if the individual reported having no job, and having made no attempt to find a job 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] Standard errors are in parenthesis and are robust to correlation within clusters (70 kebeles within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table C.18: Trends in the Effects of treatment on being having temporary work and *not* searching in each week

	(1) Pooled Samples			(2) Board Sample			(3) City Sample		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Treat	-0.022	-0.015	-0.008	-0.035*	-0.004	-0.007	-0.006	-0.028	-0.008
	(0.016)	(0.017)	(0.017)	(0.019)	(0.019)	(0.017)	(0.027)	(0.029)	(0.031)
trans trend		-0.001	-0.004		-0.0044**	-0.003		0.004	-0.005
		(0.0020)	(0.0070)		(0.0020)	(0.0076)		(0.0037)	(0.012)
trans trendsq			0.000			-0.000			0.001
			(0.00043)			(0.00049)			(0.00071)
CM	0.180			0.130			0.230		
Obs	5,011	5,752	5,752	5,011	5,752	5,752	5,011	5,752	5,752
R^2	0.216	0.209	0.209	0.216	0.210	0.210	0.216	0.210	0.210

[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 no job, and having made no attempt to find a job in the last 7 days.

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

Table C.19: Trends in the Effects of treatment on being Discouraged in each week

	(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] 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 no job, and having made no attempt to find a job in the last 7 days.

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

Table C.20: 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 C.21: Heterogenous Effects on Endline (week 16) 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
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
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
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 C.22: Impact of the Subsidies on Finances and Aspirations at endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	log of:								
	savings tot	savings form	money total	expenditure	fair wage	market wage	job prospects	kept occ pref	offers exp
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

Figure C.1: Phone Survey Questionnaire

	Phone Survey Questions	Codes
p1	**During the last 7 days were you engaged in any kind of productive activity such as work for payment, family gain or production for own consumption?**	01 = Yes 02 = No - - > Skip to Question p5
p2	**Was this work short term/temporary or casual work?**	01 = Yes, it is short-term work 02 = No - - > Skip to Question p3
p2_1	*Check if the respondent was engaged in short term work the last time they were surveyed (jobj1_1= 1 if first phone interview, p2_1=1 if is a later interview)* *Ask If* ***YES:*** **Is this the same work that you were engaged in last week/the last time we interviewed you (was it with the same employer)?**	01 = Yes, I have not changed jobs 02 = No, it is not the same, I have taken work with a different employer
p3	**Was this work full-time, permanent paid employment?**	01 = Yes, it is full-time permanent work 02 = No
p3_1	*If yes, check if the respondent was engaged in permanent employment the last time they were phone surveyed (using existing data spreadsheets).* *If yes:* **Is this the same permanent employment you had last week/the last time we interviewed you (was it with the same employer)?**	01 = Yes, I have not changed jobs 02 = No, it is not the same, I have taken work with a different employer
p4	**Are you satisfied with this job, or are you still hoping for another job?**	01 = Yes, I am satisfied 02 = No, I want other work
p4_1	**How many hours did you work in the last 7 days?**	*Write down the number of days worked*
p4_2	**How much do you think you have earned in the last 7 days?** *If they are being paid on a monthly basis calculate the effective weekly rate by dividing by four*	*Write down the estimated amount earned*
p5	**Have you been searching for work in the last 7 days?** *Ask even if they have already found a full time job.*	01 = Yes 02 = No - - > Skip to Question p6
p5_1	**How many days of the last 7 days have you searched for work?**	*Write down the number of days searched*
p5_2	**How many of hours of the last working day (***ask about Friday if it's the weekend),* ***did you spend searching for work?***	*Write down the number of hours searched*
p5_3	**On how many days of the 7 days did you visit a (or any) job board?**	*Write down the number of days at job boards*
p6	**When we last spoke to you said you expected to find employment working as _____** (*use most recent existing data on res oppucation),* **are you still interested in this kind of work?**	01 = Yes, I still expect to take this kind of work 02 = No, I am now expecting to take different work work
p6_1	*Ask about the most recent job type they have mentioned, if they just changed the job they are interested in, ask about that:* **You said you would take a job at the monthly wage of _______** (*use the most recent reservation wage number of the existing data).* **Would you still work at this rate?**	01 = Yes, I would still take that wage -- > skip to p6_3 02 = No, that wage is now too low
p6_2	**If this wage is too low, what is the lowest wage you would now accept for this work?**	*Write down the lowest wage they would work for now (then skip to question p7)*
p6_3	**If you would still work for this amount, would you work for 100 birr less? 200 birr less?** *Enumerator: keep going down until the person says NO and then write down the amount at which the last said YES*	*Write down the amount where they said YES, hen skip to question p7)*
p7	**How much do you think you have spent in cash during the last 7 days?**	*Write down the total amount of cash spent*
p8	**How many times have you travelled into central Addis Ababa in the last 7 days (including this trip)?**	*Write down the number of trips to Addis*