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

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Abstract
Do high search costs affect the labour market outcomes of job seekers living far away from jobs? I randomly assign transport subsidies to unemployed youth in urban Ethiopia. Treated respondents increase job search intensity, and are more likely to find good employment. Subsidies also reduce participation in temporary work during job search. I explain these results with a dynamic model of job search, in which cash constraints cause workers to give up search too early. The predictions of the model closely match the trajectory of treatment effects over time, which I estimate using a weekly phone call survey.

Keywords: job search, spatial mismatch, unemployment, cash constraints, urban, transport
JEL Classifications: J64; C93; J61; O18

1 Introduction

Cash constraints can impose frictions on labour markets if job-seekers cannot optimally invest in finding employment (Card et al., 2007; Chetty, 2008; Bryan et al., 2014). These constraints are compounded in large sprawling cities, where high transport costs make it particularly difficult for those living far away from jobs to access employment. Young people in urban areas often spend long periods of time in unemployment while looking for jobs.

Density in cities should reduce the amount of time workers spend in unemployment before finding a good match (Moretti, 2014; Puga, 2010; Wheeler, 2006). Yet these benefits can be mitigated by high transport costs when cities are badly planned (Bertaud, 2004; Wasmer and Zenou, 2002; Cervero, 2001). The rapid growth of African cities has often not come with corresponding investments in infrastructure and well located housing stock, leading to congestion and an increasing number of people living on the outskirts of cities, far away from jobs. The resulting search costs are commonly thought to impose significant frictions on labour markets (Diamond, 1982; Pissarides, 2000; Acemoglu and Shimer, 1999). Yet there is relatively little empirical work establishing direct causal links between search costs and employment outcomes, particularly in developing countries. This paper aims to fill that gap.

I use a randomized controlled trial of transport subsidies to test whether young job seekers who live far from the centre of a large African city are constrained in their ability to search for jobs because of where they live. Subsidies covered the cost of transport to the centre, but no more, for two days each week for a pre-announced period of time.[1] In this way I exogenously reduce search costs without reducing the cost of commuting to work in the long run, such that impacts of the subsidies can be interpreted as evidence both for the existence of search frictions and for inequality in access to jobs due to place of living.

The programm is evaluated using detailed baseline and two endline surveys (four and ten months after baseline), conducted in Addis Ababa, Ethiopia. Short phone call interviews were conducted in every week between these surveys. I use this high frequency survey data to

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

track the trajectories of search intensity over all weeks.[2] The study is designed to compare and validate the impact of these subsidies in two different but both policy-relevant sub-populations. These two study populations were defined before the baseline survey began, they differ in terms of how they were originally surveyed. The *board* sample are active job seekers who were surveyed visiting the main vacancy boards, are well-educated and aspire to permanent professional jobs. These individuals are of particular interest to policy makers because they would be most likely to self-select in youth employment programmes. The second sample, the *city* sample, were found at home in slum areas of the city, are less well educated, and relatively detached from the labour market.

Four months after baseline, shortly after the transport treatment ended, treated individuals in the *board* sample are seven percentage points more likely to have permanent work (relative to a control mean of 19%). Treated individuals have jobs of higher quality, work in higher skilled sectors, and are more likely to work in the city centre. They are not significantly more likely to be working overall, these good jobs displace temporary work. I use a second endline, 6 months later, to show that these effects start to dissipate over time but are still partly persistent some time after the treatment ends.

Treated individuals in the *city* sample are more likely to be working (by nine percentage points relative to a control mean of 46%) at endline, they have jobs of higher average quality, and are less likely to be working as day labourers. There is no impact on having permanent employment among this group, since very few have the qualifications required for these jobs.

The positive impacts on employment seem to be driven directly by increased job search in response to subsidies. Weekly data from the phone surveys show that treatment has a significant impact on job search, and an even larger impact on search in city centre, and search through formal methods. I find no evidence that the subsidies change expectations or attitudes, nor that the treatment effects are driven by Hawthorne effects from the phone calls. I use a pure control group who received neither phone calls nor the subsides, and find no impact of the calls on job search or employment outcomes.

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

The trajectory of the effects on job search over time is hard to explain by a price incentive to search alone. Instead the evidence suggests that cash constraints play a role in impeding job search. Three key results support this argument. Firstly, the treatment effects grow over time after subsidies begin. The subsidies seem to prevent respondents from *giving up* job search over time as they run down their savings because cash constraints do not bite as hard. Secondly, treated individuals are still more likely to be searching *after* the treatment ends, suggesting that the subsidies allow them to retain cash on hand and thus to search for longer. Finally the treatment effects on search and employment are particularly strong among the relatively poor and cash constrained.

To explain the link between cash constraints and job search I develop a dynamic discrete-time model of risk averse and cash-constrained job seekers, who must choose whether or not to search for work in each period. Job search is costly in monetary terms so that choosing to search is risky: failing to find a permanent job after paying the costs of search is particularly unpleasant for some one with very low savings. Therefore job seekers stop searching once their savings fall below a certain critical value, and start to save up to search in future periods. I solve this model and simulate the dynamics of job search and the effects of subsidized search over time. Agents with lower search costs take longer to run down their savings, and search for work more regularly in the steady-state. The simulated dynamics match the estimated trajectories closely, and explain differences in these trajectories across the two samples.

These findings contribute to a large literature investigating the relationship between cash constraints and job search (Danforth, 1979; Chetty, 2008; Card et al., 2007; Acemoglu and Shimer, 1999). In these models unemployment insurance plays a role in allowing job seekers to smooth consumption while in unemployment. I contribute to this literature with an experiment in which search costs are monetary and can be subsidized directly.[3] In my model, individuals who are particularly poor or running out of savings have to stop searching. Rather than increasing the rate of exit from unemployment, binding cash constraints make it less likely that a job seeker will find a good job.

[3]The assumptions of my job search model reflect this. Existing work generally assumes utility functions that are separable in consumption and the (dis)utility of search. See Danforth (1979) for a discussion of this issue.

Further, I find that job seekers significantly reduce labour supply while receiving subsidies. This suggests a role for temporary or casual work in the job search process, by funding job search and consumption for the unemployed (Browning et al., 2007).[4] Temporary work seems relatively available in this setting: respondents in my sample often work at temporary jobs and search for better work in the same week. This suggests a segmentation of the labour market, whereby workers use lower quality wage work while queueing for better jobs (Lewis, 1954; Harris and Todaro, 1970; Fields, 1975).

Secondly, my results complement the literature on mobility constraints in developing countries. Migration, like job search, entails costs that can be too high or too risky to pay, especially for cash constrained individuals (Bryan et al., 2014; Ardington et al., 2009; Angelucci, 2015).[5] Just as migration costs impede the efficient allocation of labour across space, I find evidence for frictions *within* urban labour markets due to the distance between workers and jobs, which could be preventing optimal matching of workers to jobs (Diamond, 1982).[6]

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

My findings contrast with the existing literature that has found weak impacts of other active labour market policies on youth outcomes.[7] A simple cost benefit analysis suggests that the gains to treated job seekers in terms of increased earnings outway the costs of the subsidies

[4]Evidence on high-turnover for factory jobs in Ethiopia (Blattman and Dercon, 2015) lends support to this idea in a similar context.

[5]Jensen (2012) and Beam (2014) provide evidence on how information about jobs, in a rural context, can increase employment in urban jobs. Freeing up certain physical constraints in relation to housing (Field, 2007; Franklin, 2015) and access to electricity (Dinkelman, 2011) has been shown to allow the poor to increase labour supply.

[6]However, the general equilibrium effect of a scaled up version of my intervention would be minimal if reducing search costs improves labour market outcomes only at the expense of other job seekers (Crépon et al., 2013). The main focus of this paper is on the implications for those job seekers who have unequal access to employment opportunities.

[7]This is particular true for developing countries. See for example (Betcherman et al., 2004; Groh et al., 2012; Ibarraran et al., 2014).

provided.

The paper proceeds as follows: Section 2 discusses the setting and design of the experiment, while Section 3 discusses the data. Section 4 gives an overview of the theoretical framework, the full model is detailed in the appendix. I look at the main treatment effects on employment and job search in Sections 5 and 6, respectively. Section 7 tests and confirms some of the additional predictions of the cash constraints model. Section 8 concludes.

2 Setting and Experiment

2.1 Context

Addis Ababa is an ideal setting to study the effects of urban growth and transport costs on labour markets. The city's population has been doubling nearly every decade for the last 40 years, is now estimated to be 4.5 million, and is expected to grow to 12 million by 2024 (UN-Habitat, 2005). Addis Ababa has been the major destination city for rural-urban migration in Ethiopia, and is growing rapidly as a result. New migrants disproportinately find housing far from the city centre, while long term residents are being forced out of the inner city slums to make way for commerical redevelopment (Yntiso, 2008).

The youth, who are new entrants into the labour market and lack prior experience to signal their ability, find it particularly difficult to find employment. Youth unemployment is high (at 28%) and many and many recent college graduates spend years looking for their first job (Serneels, 2007; Broussard and Teklesellasie, 2012), partly because the secondary and tertiary education system has expanded enormously in recent years, increasing the supply of labour to formal sector work, but making it harder for firms to distinguish among applicants.[8]

The majority of white-collar jobs are found on job vacancy boards located in the centre of Addis Ababa, where formal firms post advertisements. The majority of formal firms are located in the centre of town. Data from a survey of the 500 biggest employers in the city, across all sectors, shows that 50% of formal sector private sector jobs are located within 3 kilometers of

[8] See an extensive literature on labour in Ethiopia (Mains, 2013; Serneels, 2007; Haile, 2005; Franklin, 2014) for an overview of the issues.

the centre. Most public sector work is even more concentrated in the centre.

Social networks are cited as a common method of finding work, but for individuals disconnected from the formal labour market jobs found in this way are likely to be of low quality, located in their local areas. Jobs in the centre pay more, and have better working conditions.[9] While everyone in this study had mobile phones, few had smartphones. Relatively few jobs are available online, only 20% of the sample report ever having used the internet for search.[10] Thus to find better employment, young people must travel to the city centre and make formal job applications.

Individuals in my sample live on average 6.8 kilometers from the city centre, some as far as 13 kilometers away.[11] Thus, for job seekers the costs of gathering information about jobs are high, and transport costs often comprise a high proportion of their weekly expenditures. Job seekers have limited budgets, no social welfare support, and must rely on the small amounts of money they receive from their parents, as well as temporary or casual labour, to make ends meet. Individuals in the study oscillate between searching for work, taking temporary work, or doing neither from week to week.

The costs of searching for a good job are not evenly distributed. They are likely to be higher for those living far away from the city, and for the youth and new migrants who have limited access to social networks that might help them find good work.[12] The poor are likely to find it particularly hard to pay to search. Thus some young people are at a disadvantage in their ability to share in the growth of Addis Ababa's economy.

[9] In a dataset of 2000 working adults of all ages from all over the city, I find that respondents who travel outside of their local area for work earn 95% more than those who work in their own homes, and 60% more than those who work within walking distance of home. Commuters are 50% more likely to have permanent jobs than those working in their local areas. These differences are no doubt driven in part by selection effects and the costs of commuting to these jobs. Yet most young people in the sample used for this study expressed an aspiration to permanent formal sector employment.

[10] The online job site (Ethiojobs.com) recruits mainly for highly specialised positions at NGOS and multinationals.

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

[12] Calculations using geo-referenced data from the 2007 census at the census enumeration area level show that young people and new migrants are also more likely to be living further from the centre of city.

2.2 Experimental design

I test for the relationship between search costs and labour outcomes by providing a cash transfer that could only be used for travel. Individuals were given money to cover the costs of transport if they arrived to collect it at a kiosk in the center of the city, nearby to the main job boards. The subsidy amount given was tailored to cover the transport costs of a return trip from respondents' place of living to the centre, using the fares charged on public transport.[13] The modal amount given was about a third of the median of daily wages.[14] There was little or no spare cash left over after covering the costs of transport.

A baseline face-to-face survey was conducted before the sample was split randomly into three groups: the treatment group who received the subsidies and were also called every week, and two control groups; one that received the same weekly phone calls, and one that was not called at all. This allows me to test for an impact of the phone calls on outcomes at endline. Furthermore, treated respondents were randomly divided into those receiving the subsidies for 8 weeks and those receiving them for 11 weeks.

In all 551 respondents received the phone calls, of whom a further 255 were offered the transport treatment. So out of the initial sample of 877, 326 were not contacted again until the endline. Randomization was done by stratifying the sample by a number of different baseline covariates, following Bruhn and McKenzie (2009).[15]

The treatment group were clearly informed about the nature of the subsidies, that they had been randomly selected, and for how long they would be able to collect the subsidies. The treatment group began to receive the subsidies in *Week 1*, one week after the end of the baseline survey. The phone survey also began in that week. By limiting the subsidies to no more than twice a week, the experiment was designed to limit the extent to which commuting costs were subsidized. Respondents knew in advance that the subsidies were offered for a limited

[13]The amount was enough money to make the trip by mini-bus which is the preferred mode of transport because of its flexibility and regularity. 58% of respondents said their main mode of travel was by mini-bus, 38% said that they used large government buses. These buses are cheaper but unreliable and only serve trunk routes. The average one way trip to the centre took 33 minutes by mini-bus.

[14]This was 15 Ethiopian Birr for a return trip, or just less than $1 per day. No one received less than 12 Birr or more than 20 Birr.

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

time only. Restricting collection times to before midday on week-days limited the use of the subsidies for recreational purposes. Proof of identification was required to limit fraud. The last phone calls were completed the following week (week 12). In total respondents could collect the money up to 22 times.

2.3 Transport Costs in Perspective

The cost of transport to and from the centre of the city is less than $1 a day, but it is a substantial cost to pay for unemployed youth in Addis Ababa. Reported expenditure from the baseline survey for this experiment confirms that search costs are high. A *single* trip by mini-buses costs 12% of median weekly expenditure for individuals in the sample (about $4). Weekly transport costs at baseline are on average $1.5, or 20% of median total expenditure. The cash provided by the experiment should be thought of as only a partial job search subsidy, as it does not include other costs associated with search.[16]

Job seekers find money to pay for these costs and other expenditure items from different sources. Some take casual labour jobs which pay about $10 a week on average. Others (about 50% of the sample) receive small amounts of money from their family. So the cash offered by the intervention, on average $1.5 per week or about 15% of temporary jobs, constitutes a significant transfer for many of the respondents. Individuals could collect up to 330 Birr ($17) over the course of the study.

3 Data

3.1 Two Sample Strategy

This study measures the impact of transport subsidies on two different but policy relevant sub-populations. This was done to mimic different ways in which active labour market policies might be targeted in this context, and allows me to verify and compare the results across two different target populations. These contrasting results could be useful to policy makers deciding

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

on the method of targeting for youth programs. The two samples differ in terms of the location in which they were first approached and invited to take part in the study. The distinction between the samples was made in the study design *ex ante* and is not the result of specification search.

Both samples comprise men and women aged 18-30 who were able and available to start a new job in Addis Ababa in the next 2 weeks, even if they were currently working at a temporary job. Only individuals living in neighbourhoods least 5km away from the center of Addis Ababa were included in the sample. One sample I call the *board* sample, the other the *city* sample:

BOARD SAMPLE: The board sample was drawn by randomly approaching individuals who were gathered around the job boards in the center of Addis Ababa. Since they were all visiting the job boards at the time of screening, they were, by definition, active job seekers. They were approached discreetly by enumerators and invited to participate in an interview nearby.[17]

They were on average more educated,[18] but were often more recent migrants, and had less of a secure footing in the city. This is a group who are most likely to participate in the growth of high-productivity employment in Ethiopia, and would be most likely to self-select into active labour market programs, such as unemployment centres or training programmes. However, they started out as job seekers but gave up search or became discouraged over the course of the study. Policy makers might aim to prevent these skilled individuals from from withdrawing from the formal labour market too soon.

CITY SAMPLE: This sample was randomly drawn by going door-to-door in small enumeration areas around the city. These areas were chosen randomly, stratified within the six urban districts on the periphery of the city. The survey sites are marked in Figure C.1. Respondents were interviewed at home. This sample were disproportionately likely to have given up job search and staying at home. While many looked for work intermittently, others were discouraged. They had on average lower education than those in the *board* sample, most had completed high-school but no more.

[17] Care was taken to prevent groups of friends being interviewed. Many thousands of young people visit the boards every day, so the possibility of surveying groups of friends, while present, is not large.

[18] Of this sample 72% had some kind of post-secondary education, about 43% had university degrees. This constrast with the figures of representative surveys of the city: the corresponding figures in 2012, are 22% and 10%, respectively.

This is a group that policy makers might consider to be chronically detached from the labour market, and thus vulnerable to related social problems, crime, and "scarring" from long term unemployment. If distance from employment was one of the factors contributing to their detachment from the labour market, active labour market policies or improved transport could help them to be re-integrated into the economic life of the city.

3.2 Phone Survey

In addition to face-to-face baseline and endline surveys, I used a weekly phone call survey to measure the trajectories of job search, employment, and treatment effects over time. These trajectories are used to identify mechanisms through which the treatment effects impact job search, by fully accounting for job seekers activities during the time of treatment. Each respondent in the phone survey was contacted on average 10.4 times, we reached about 400 individuals on average each week. The usefulness and accuracy of phone call surveys is studied in Garlick et al. (2015).

The phone calls were short: they would take around four minutes to complete. The questionnaire was short, giving only a handful of measures that can be used in the analysis of the phone call data. This has the advantaged of pre-committing me to testing the significance of just a few major outcomes. Most importantly, the only measure of job quality used in the survey was whether the job was permanent. I analyze more detailed endline data seperately to investigate other measures of job quality.

3.3 Balance and Attrition

Table D.1 in the Appendix presents test for balance on variety of job market outcomes and baseline covariates. There is balance across a wide range of measures, in the pooled sample, as well as in the two samples separately, and none of the variables used as outcomes for job search or employment at endline are statistically different across groups. When estimating the main treatment effects, I control for all variables showing any sign of imbalance at baseline.[19]

[19]Out of the 30 main variables tested for balance in D.1 only one is significantly different at the 10% level in either of the two sub-samples. Treated individuals in the control group are more likely to have graduated more

There is no difference in rates of attrition between individuals who were given the transport subsidies and those who were given the calls but not the subsidies. In addition, almost no baseline covariates are significant predictors of attrition at endline. Table 1 provides an overview of rates of attrition at various points of survey. Attrition is high in this sample, because this is a very mobile study population. Much of the attrition is due to respondents who changed phone numbers, or provided wrong numbers at baseline, before the treatment began.

We could not find 14% of the total sample at all after the baseline, and about 25% were not interviewed at the endline survey. However, among the phone call survey respondents, a large proportion (just less than half) of the total attrition took place between the first phone call surveys.[20] This sort of attrition is unlikely to be correlated with the transport treatment, since treatment did not start until *after* the first phone calls. The phone calls do seem to have reduced attrition, but only by about 4 percentage points (the effect is not significant). The main results of this paper are robust to using either control group (the "no calls" and the "no transport" group) in endline regressions.

Table D.3 shows the main determinants of whether a respondent was found at follow-up (in week 16). The first two columns show that the transport subsidies had no effect on attrition after controlling for receiving the calls. Columns (3)-(6) show that very little else impacted the probability of being found at follow-up. Attrition is particularly hard to predict for the *board* sample. Furthermore, I show that the sample is balanced on baseline variables between treatment and control *after* attrition, both in phone surveys and at the endline, in Table D.2. I conclude that the results in the paper are not driven by attrition.

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recently. Descriptive statistics show that recent graduates are more successful at finding good jobs, suggesting that this imbalane would actually lead to downward bias of my estimates.

[20] 524 individuals were enrolled in the phone survey, 8% were never reached by phone. Out of the 465 that were reached by phone at least once, on average 400 were reached each week, an average rate of attrition on the phone survey of about 15%.

3.4 Take up and use of the subsidies

Many, but not all, respondents came to collect the subsidies. In all 67% of respondents came to collect the transport money at least once, during the three month period. Those that never collected the money were either not interested in job search, had no need for the money, or could not be contacted or did not understand the instructions.[21]

However, conditional on taking up the money at least once, respondents continued to take it regularly. The proportion of individuals collecting the subsidies remained stable over the course of the study, as is demonstrated in Table C.3.[22] In each week about 55% of treated respondents took the money, 45% of respondents took it on both of the available days. Very little predicts take up of the transport subsidies. Individuals in the board sample were slightly more likely to collect, by about 9 percentage points.[23]

Self-reported measures of what respondents did after they collected the subsidies shed some light on how they were used. The *board* sample were very likely to use it to search: only 12% said they looked at the job boards "rarely" or "never" after collecting the transport money, 45% said they used the boards every single time they collected the money, compared to 25% of the *city* sample. The *city* sample were far more likely to have to have gone to enquire about work at firms directly. These differences across samples reflects the different search strategies used by job seekers.

4 Theoretical Framework

I outline a theoretical framework for interpreting the empirical results in the paper, as well as for understanding how the two different samples in my data respond to the randomized

[21] Suggestive evidence from endline survey questions about the intervention suggest that imperfect implementation may have contributed to low take up, as around 20% of treated respondents reported not recalling the offer to take up the transport subsidies in the first place. However, this result should be treated with caution. Respondents who had no interest in the subsidies may fail to recall the original offer to take them when asked about it over 4 months later.

[22] I plot the proportion as percentage of people who were eligible to collect in that week, because the proportion of individuals eligible to collect varies while the subsidies were phased out randomly.

[23] This difference seems to be driven by the higher concentration in the *board* sample of highly educated individuals and new migrants, who were more likely to collect. Because of the small samples, very few outcomes are found to be significant predictors of take up in multivariate regressions.

intervention in different ways. I argue that the results in this paper are partly driven by a cash constraint mechanisms, whereby job seekers find searcing for work risky when they are low on cash.

4.1 Brief outline of the model

The formal presentation of the model is relegated to Appendix A. The model is a dynamic model of job search with credit constraints, in which infinitely lived agents search for permanent jobs over many periods, with limited budgets and relatively high search costs. Agents can search and have temporary work simultaneously.

In the model unemployed job seekers can choose to pay a fixed monetary cost to search for a good job at the beginning of the period. They get a good job with a fixed probability, conditional on searching. If they do not have a permanent job, whether they searched or not, they get income from casual labour in their local area for one period with fixed probability. Searching for a permanent job is risky: the probability of finding work after paying the high search cost is low. Thus agents only search for jobs when they have a sufficient cash on hand, so that they maintain buffer stock savings (Deaton, 1991). The model shows that there is a unique cash threshold such that job seekers search if and only if they are above this level. The threshold can be high even for very low risk-aversion parameters, explaining why individuals give up search even when they enough cash on hand to cover the costs of search for at least a few weeks. Below this threshold, job seekers save up for job search, above it, they spend money on job search, such that they converge to a steady state level of savings.[24]

The assumptions of the model reflect the real data as closely as possible. Temporary work is often available in young peoples' local areas: unemployed individuals have temporary work in about 30% of weeks in this study. In the model, agents can take temporary work even if they searched for work at the start of the period: in reality individuals with temporary work are only slightly less likely to be searching than those without work. These jobs do not pay significantly less than permanent jobs at entry level, but provide highly irregular incomes, are

[24] I use 'savings' interchangeably with 'cash on hand' since few individuals earn interest. Changing this assumption does not alter the result.

unpleasant, are not valued by formal employers as work experience, and offer few opportunities for promotion. These jobs also do not require job search, almost all of them are found through referrals or social networks.

Good jobs are hard to find: the probability of finding permanent work conditional on paying the search costs are about 2% per week, just 19% of the control group find them. I calibrate the model simulations to match this reality. These jobs are an absorbing state in the dynamic model since separations are relatively rare in reality.[25]

I perform dynamic simulations of the model, with a group of job seekers with a distribution of initial savings similar to that in my data. The proportion of individuals who search declines in each period, as individuals start to run out of savings and reach the critical threshold. The proportion of individuals searching reaches a steady state, where agents' savings oscillate around the threshold.[26] The model predicts that lowering search costs reduces the critical threshold above which job seekers search for work, reduces the speed at which they run down their savings while searching, and increases the regularity with which agents search while savings are around the threshold level.

4.2 Predictions for the two samples

The model is designed to explain the results in both the *board* and the *city* sample. Individuals in both samples are hypothesised to be cash constrained, and thus will search more when search costs are lowered. Take up was similar and respondents reported using the subsidies to search for jobs, suggesting that both samples found the subsidies useful.

The two samples differ in their initial conditions and the types of jobs that are available to them. These differences generate different predictions for the trajectory of the impacts on job search over time, and the types of "good jobs" available to the two samples, respectively.

Different job search trajectories: Individuals who have savings around the critical cash

[25] Using data from a longer panel of job seekers I find that the only 27% of permanent job spells end within one year, compared to 70% temporary work spells, and that most of these permanent work spells ended because of workers quit (85%) rather than because they were laid off.

[26] When agents find temporary or casual work, they earn money that they will use to search again once they have savings above the threshold.

threshold at the start of the study will immediately be induced to search when subsidies begin, because the critical value falls as soon as search costs do. Job search would be affected relatively quickly after treatment begins, but the effect would not persistent after the subsidies end. This prediction most accurately fits the *city* sample, who were already discouraged (although many were searching intermittently) and so might be considered to start in the steady state around the savings threshold.

On the other hand, those sampled at the job boards started as active searchers. Many of them might be thought of as being above the critical cash threshold, with cash to go on searching for a while.[27] The model predicts that individuals who start with cash above the threshold search every week initially, so treated individuals would run down their savings more slowly, thus delaying the point at which transport costs become a binding constraint. Treatment would take longer to have an effect: only once agents begin to give up search would the two groups diverge in their behaviour. If some individuals in the *board* sample remain above that critical threshold throughout the period of the intervention, the treatment effects would be persistent after the intervention ends.

Different search methods: The intervention is designed to increase job search in the centre of town, bothby visiting the job boards and visiting the large formal firms located there. Because the *board* sample find the vacancy boards more useful for job search, we expect to see stronger effects on the use of the job boards by the *board* sample. Individuals in the *city* sample rely more on direct enquiry at firms or work sites as method of search.

Different job quality margins: Assuming that better jobs are found in the centre (at the job boards or otherwise), take up of the subsidies for the use of job search should lead to higher rates of good jobs. The *board* sample, who are more highly educated, have the ability to get highly sought after permanent jobs. By contrast, I find that the city sample are very unlikely to find permanent jobs at all.Instead, jobs in the centre of town, without formal contracts, but better working conditions, were available but still required intensive search to find.

[27]This does not mean that they are necessarily wealthier, as in reality individuals are likely to have different risk preferences and job search efficacy, which would imply different critical savings thresholds.

4.3 Working while searching

The model predicts that exogenously increasing the regularily of income for the unemployed in the form of cash grants or income from casual labour will increase the probability that an agent searches for work, because searching becomes less risky. If a searcher fails to find a permanent job, this is a less bad outcome when they can expect to at least have some income in the future periods. This theoretical result underscores the role of temporary work to pay for job search, and suggests that changes in job search behaviour could interact with the decision to take temporary work.

A limitiation of the model is that decision to take temporary work, when it is available, is not endogenised: agents always take the temporary work, because they earn income with zero cost in terms of time and effort. If, as in McCall (1996), part time or temporary work takes up time that could otherwise be spent searching, agents would face a trade-off between their cash needs and a desire to dedicate all of their time to search. At the margins, some job seekers will be less likely to take up work when search costs are lower, because their needs for cash from temporary work are less acute: they can afford to go on searching.[28]

This effect would not apply to individuals who are not searching: they would *always* take temporary work in order to save up to search. We might only expect the treatment to reduce temporary employment among individuals the *board* sample who start with enough cash to search in every week, and can afford to forego income to focus on search.

5 Impact on Employment

This paper tests two key hypotheses: firstly that reduced search costs increase job search, and that this increase in job search leads to improved employment outcomes. In this section I begin with the latter by testing for an impact of transport subsidies on employment outcomes at the two endline surveys, at week 16 (4 months after the baseline survey and about one month after the transport subsidies ended) and (conducted by phone) week 40, respectively.

[28] Browning et al. (2007) presents a model in which job seekers take temporary jobs for a certain amount of time, and quit them once they have earned enough income to go on searching for a ‘good” job, a little longer.

I estimate intention-to-treat (ITT) estimates on binary labour market outcomes. Following (McKenzie, 2012), my preferred specification is one where I include a lagged dependent variable to control for differences in baseline outcomes, and set of controls for baseline covariates. Specifically, for weeks 16 and 40, separately, I estimate:

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

Here Y_{ic} is the outcome for individual i in cluster c. All standard errors are clustered at the Woreda level (the lowest urban administrative unit in Ethiopia), of which there are 70 in my data.[29] T_i is the treatment variable dummy, X_{i0} a set of baseline controls, and Y_{i0} the outcome of the dependent variable at baseline (week 0).

This study was designed to contrast the impacts of the treatment in the two different samples. To estimate different effects for each sample separately, I run:[30]

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

Here λ_s provides an estimate of the treatment effect on sample s (for instance the *board* sample). α_s are group specific intercept terms.

In the appendix, I show that my estimates are robust to the use of a wide range of other estimators, without the lagged dependent term (Equation 3 below, labelled COV in the tables). I also I include a logistic regression version of Equation 3 (labelled LOG). X_{i0} could easily be replaced with a set of blocking dummies on which assignment to treatment was based, this specification is labelled in tables as (BLK) in the tables. I also estimate a version without the covariates or lagged dependent variable (Equation 4 below, labelled COV), and a first-difference estimator (Equation 5 below, labelled FD) which is equivalent to including individual fixed

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

[30]All results are robust to running equation 1 for each sample separately.

effects.

$$Y_{ic} = \alpha + T_i\lambda + X_{i0}\beta + \epsilon_{ic} \tag{3}$$

$$Y_{ic} = \alpha + T_i\lambda + \epsilon_{ic} \tag{4}$$

$$Y_{i16c} - y_{i0c} = \alpha + T_i\lambda + X_{i0}\beta + \epsilon_{ic} \tag{5}$$

5.1 Jobs and Permanent Jobs

Table 2 contains the main results for the paper, showing the impact of the transport subsidies on the probably of having a job, and having a permanent job, for both samples, and at both endline surveys. If job search leads to an increase in the probability of a good job offer, the subsidies should lead to higher quality jobs, and a higher probability of having a job, if individuals who otherwise would have had not work at all were induced to take an offer. However, if the effects are concentrated among individuals who might have had temporary work anyway, the effect on employment could be much smaller than the probability of having a good job.

Treatment increases the probability of finding permanent work by 7.8 percentage points for the *board* sample, over a mean of 19% in the control group. This translates into about a 40% increase in the probability of having a permanent job. This shows that the subsidies allowed these individuals to find they good jobs that the were looking for.

The treatment has an effect on the probability of having any work at all (Columns 1 and 2), which is significant at the 10% level for the pooled sample. However, this effect is driven entirely by the *city* sample who are significantly more likely to be employed at endline by about 9 percentage points (a 20% increase). This is perhaps because the quality of offers received were higher due to increased job search, leading individuals who were previously rejecting previous offers (available in their local areas) to accept jobs found closer to the centre.[31]

By contrast, the *board* sample are not significantly more likely to be working, suggesting that finding permanent jobs displaced work that would otherwise have been done in temporary

[31] I confirm in Section 5.3 that the quality of jobs is indeed higher for treated individuals, in both samples.

jobs. Indeed the impact of the treatment on having temporary work is negative (but not significant) for the *board* sample. The *city* sample are not more likely to find permanent work, but this is because it is difficult for this group to find permanent work at all, since many of these jobs require post-secondary education. In the control group of this sample, only 6.5% have permanent jobs at endline.[32] Results in the online appendix show that these results are robust a number of different specifications.[33]

These intent-to-treat estimates understate the extent to which the subsides were useful for particular individuals who used the subsidies. I estimate the treatment-on-the-treated effect of the subsides, by instrumenting for take up of the subsides by treatment assignment. The instrumental variable results suggest that among those who used them, subsidies increased the probability of having permanent work for the *board* sample by 14 percentage points, and having any work for the *city* sample by 15 percentage points respectively. Furthermore, I instrument the number of trips taken to the city by individuals.[34] These results are in the one appedix, Table D.6.

5.2 Persistence and Catch Up

Are these impacts on job outcomes persistent a further 6 months after the first endline survey? The most employable job seekers in the treatment group may have found good jobs while receiving the transport subsidies, but their equally employable counter-parts in the control group may have continued to search for work steadily in the following 6 months, getting similar quality jobs, albeit a while later. This would lead to the differences between treatment and control dissipating by the end of that 6 month period.

The effects would be persistent if employment and job search outcomes exhibit duration dependence. This would be the case if job seekers become less employable after being unem-

[32] Much of this is likely to over-reporting of permanent work: many of these respondents may not understood the definition of a permanent job.

[33] The results are presented in Tables D.5 and D.7 for permanent and temporary work, respectively. Generally, the inclusion of the lagged dependent variable improves the efficiency of the estimates. The results are significant at the 5% level for most of the main results in Table 2, but only at the 10% when the lagged variable is not included.

[34] I have limited power to estimate TOT estimates, as the sample must be restricted to only respondents who were in the phone call survey, in order to estimate the number of trips made to the centre by these individuals.

ployed for a long period of time (Kroft et al., 2013), or if job search becomes harder over time as psychological discouragement sets it.[35]

I provide evidence that the results have dissipated, but not entirely, 6 months later, suggesting some persistence. In Table D.8, I show that when surveyed 6 months later (at week 40) those who were treated among the *board* sample are now 3.3 percentage points (roughly 10%) more likely to have permanent work. This is displayed along side the impacts for week 16, showing that the coefficient has about halved over time. This effect is not significant in this small sample however, and should be treated with caution.[36]

5.3 Job Quality

Overall, I find evidence that both samples had significantly improved job quality, along different margins. These results suggest that respondents are not simply getting work faster by accepting inferior jobs. Here I look at the impacts on other measures of job quality and type at the *first* endline survey at 16 weeks into the study, in Table 3. Having restricted my attention only to the few outcomes that were in the phone survey, I now look at a number of additional measures of job quality in the endline survey. The key variables are described in the table notes.

The estimated impacts on many measures of job quality of significant at the 10% level in the pooled sample, but this again masks considerable heterogeneity across the samples in terms of the types of goods jobs individuals could get. The bottom rows of the table show the mean outcomes for individuals *with work* for each sample. For instance the treatment only improves the probability of having a job that required a degree for the *board* sample: notice that no one of control group in the *city* sample finds a job requiring a degree (Column 5).[37]

The *board* respondents are more likely to have found jobs that require at least a degree as a qualification.[38] They are also more likely to be working in the central business district (Column

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

[36] Robustness checks using alternative specifications for the results at week 40 are in Table D.8.

[37] Only 1.5% of this sample has a university degree.

[38] During focus group discussions run during the endline survey, many respondents attached special importance

10). For the treated *city* whose outside options often include work on construction sites or on the street, it is noteworthy that the treatment increased the probability of having a job in an office, and with a monthly salary.

I find no impact on the average wages of treated individuals and the control group. Although the coefficient is large for the *board* sample, it is not estimated precisely. I am unable to reject the Kolmogorov-Smirnov test of equality in distribution of wages (in levels or logs) between treatment and control groups.[39] In Addis Ababa, job quality is not always a good proxy for job quality of entry level jobs, since entry level wages tend to be similar across different occupations. Young people look for formal sector jobs because of the security that they provide, the regularity of pay, and opportunity for promotion. Indeed high salaries may be offered in low quality jobs to compensate workers for tougher and riskier working conditions.

In light of this, the lack of a large impact on earnings is perhaps not surprising, in the short run. However, I do find that treatment changed the composition of professions that young people worked in. When I rank those professions by average wage (across all ages and tenure, from existing national labour force data, where large wage gaps do exist), I find a distinct shift towards higher paying professions due to the treatment (see Figure C.4).

6 Impact on Job Search and Temporary Work

In this section I show that transport subsidies had an impact on job search. This establishes that job seekers were indeed constrained in their ability to search for work by the high costs of search, the key hypothesis of this paper. I find that the treatment affects search at the extensive, rather than intensive margin: it caused people to be more likely to do some search during the week, rather than increasing the number of days that job seekers spent searching per week.[40]

to the goal of finding work for which they were specifically trained.

[39] The results do not seem to be driven by selection effects: I use quantile regressions to show that the treatment has no impact on the probability that individuals in either sample are above various thresholds, or percentiles, of income.

[40] I suggest that this is because of falling returns to the number of days searched. Job seekers using the job boards liked to visit the boards once or twice in the week (new vacancies were not released so regularly that more frequent

Secondly, I argue that the increased job search activity lead to the improved employment outcomes presented in the previous section. Not only did individuals in both samples search more during the weeks of the study, they changed the nature of their job search: they were more likely to have visited the centre of the city while the subsidies were in place, and were more likely to have visited the job boards in the centre of the city. These changes in search methods explain how treated individuals were able to get permanent, better quality jobs.

I verify the richer predictions of the theoretical model discussed in Section 4. In particular I show that the impact of the subsidies increase over time consistent with a story of cash constraints leading active job seekers to give up search as they run down their savings. The *city* sample responds slightly faster to the treatment, perhaps because a greater proportion of individuals in the sample had already given up search, and the subsidies allowed them to begin searching again. I show that for the *board* sample the impacts on search are persistent even after the subsidies are no longer in place, providing additional evidence for cash constraints in this sample.

6.1 Estimation

Equation 6 estimates the average impact of the treatment on the propensity to search (λ), over all weeks of the study pooled together. Using equation 7, I then estimate the treatment effect in each week separately, by looking at the coefficients λ_t on the week dummy variables W_{it}.[41] These λ_t's allow me to plot the trajectory of effects over time.

$$y_{it} = \alpha_t + T_i\lambda + X_{i0}\beta + \epsilon_{it} \qquad \forall t \neq 0 \tag{6}$$

$$y_{it} = \alpha_t + \sum_t T_i W_{it}\lambda_t + X_i\beta + \epsilon_{it} \tag{7}$$

I interact treatment with a continuous week variable w, to estimate the trend of the treatment

trips would be beneficial) and these trips could be combined with visits to firms to lodge applications. Two days searching per week was the modal search intensity for those searching, and the treatment had no significant impact on this.

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

effect over time, estimating an intercept term, linear, quadratic and cubic trend terms, as in equation 8 and 9, below.

$$y_{it} = \alpha_t + T_i\lambda_0 + T_i w\lambda_1 + X_i\beta + \epsilon_{it} \tag{8}$$

$$y_{it} = \alpha_t + T_i\lambda_0 + T_i w\lambda_1 + T_i w^2\lambda_2 + X_i\beta + \epsilon_{it} \tag{9}$$

Figure 1 summarizes the impacts on job search for both the *board* sample (Column 1) and the *city* sample (Column 2). In Panel A, non-parametric (local polynomial regression) estimates of the probability of searching for employment as a function of time are presented. The weekly estimates of the difference between the treatment and control groups in each week are plotted in the Panel B of Figure 1. I overlay the linear and quadratic estimates of the trend of the treatment effect over time. Tables in the Appendix show all of these estimates.

6.2 Job search trajectories and methods

I find pronounced impacts of the transport subsidies on job search in the *board* sample. Job search declines over time, as job seekers become discouraged or run out of savings to search. The proportion of individuals searching (defined as the respondent having taken "some direct action to look for a job in the last 7 days") declines more slowly in the treatment group however (see Panel A, Col 1), so that the effect of the treatment on job search increases over time (Panel B, Col 1). The trajectory of the impact is linear over time with a significant upward trend.[42]

The nature of the transport subsidy, which required job seekers to arrive at the centre of the city, also had an impact on the method of job search used by job seekers.The treatment has an impact on the probability of visiting the job vacancy boards (see Figure C.8, with coefficient estimates in Table D.12 in the Appendix). These effects are more pronounced (well above 10

[42]Tables in the appendix confirm that this linear trend, weekly, and pooled treatment effects are highly significant for the *board* sample in Tables D.11, and D.10. The quadratic term is estiamted close to zero, and is not significant.

percentage points for later weeks of the study) than the effects on job search.[43]

The effects of the treatment on search are significant for a number of separate weeks for the *city* sample, and the effect is significant on average across all weeks pooled together (a 5.9 percentage points average increase, see Table D.11). I find no evidence of an upward linear or quadratic trend however: the effect seems to be relatively constant over time, and is evident after only 3 weeks of the study, sooner than in the *board* sample.

Furthermore, I show that job search methods seem to have changed for the *city* sample. The effect on the use of the job boards is less pronounced than in the *board* sample. Note that only 24% of the city sample use the boards, compared to 65% of the *board* sample, in the average week. There are a few weeks in which individuals in the *city* sample were significantly more likely to be using the boards, but this effect does not seem to have lasted even to the end of the treatment period, perhaps because these respondents experimented with using the boards while in the centre, but refined their strategy when they did not find it useful.

However, individuals in both samples are more likely to have to made trips to the centre of Addis Ababa, as shown in Figure 2. These effects are large: consistently above 10 percentage points after 5 weeks of the study.[44] While not all job seekers find the job boards useful because of the high average requirements for job seekers in terms of experience and education, individuals in the *city* sample could make trips to firms located at the centre of the city, to enquire about higher quality jobs than were available in their local areas.[45]

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

[44]This effect is robust to looking only at individuals who did not have jobs, allaying concerns that the effect is driven by individuals travelling to the centre of the city for work.

[45]While I have no direct evidence that all of these trips were used for job search, job search was by far the most common reason cited for visiting the centre, and the most common activity reported by treated respondents when asked what they did after collecting the transport money.

6.3 Interaction between work and search

In this section I look at the composition of the samples across different activities, while the subsidies were in place, to look at how temporary work interacts with job search. For ease of exposition and to reduce the noise of weekly estimates, I follow McKenzie (2012) and pool weekly observations into groups of 4 weeks, and the estimate the impact of treatment on the probability of searching in each of these *months*.[46] I can then graph changes in the sample composition over months, along with estimated treatment effects. I estimate:

$$y_{imt} = \alpha_t + \sum_m T_i M_{mi} \lambda_m + X_{i0}\beta + \epsilon_{it} \quad (10)$$

Figure 3 shows, for a number of outcomes, the control mean (the bars), the treatment effects estimated using Equation 10 above, placed relative to the control mean, with the confidence intervals drawn around the treatment effect. Coefficients for the monthly results for a range of outcomes are presented in Table D.14.

The results show that in the *board* sample the treatment group were 6.7 percentage points less likely to be working throughout the whole second month of the study. There was no impact on the rates of finding permanent employment in these early weeks: the effect on employment is driven completely by a reduction in temporary casual or other informal sector work.[47] These are jobs that do not last long, and the effect is only present for a few weeks, but provides strong evidence that active job seekers choose to reduce their supply to labour to temporary work because of the subsidies.

This result is consistent with the theory in Section 4. Treatment seems to have stopped

[46]This gives me power to more precisely estimate the treatment effects, with the small sample sizes that introduces volatility into the weekly estimates.

[47]To allay concerns that these months were chosen strategically to boost significance, I present complementary results where I *restrict* the sample to groups of four weeks, starting with the first four weeks of the study, and then iteratively move this window forward by one month. I re-estimate the treatment effects separately for each iteration. This provides a type of moving average monthly treatment effect, and clearly shows the trajectory of treatment effects. These estimates are shown for the pooled sample in Appendix Table D.15.

respondents from having to take up temporary work to pay for job search, because the costs of search were lower. They searched for good jobs rather than taking bad ones, and ended up with improved employment outcomes at endline.

I show that the reduction in employment is *not* driving the impact on job search for the *board* sample, who were more likely to be searching whether they had temporary work, or no work at all in the final month, and conditional on not working in month two (see Figure 3). So the effect is not driven by the reduction in employment, although does contribute to the effect in month 2. There was no effect on permanent work for the *board* sample until the final week of the study, perhaps because of the long application process of permanent jobs, which could result in a lag between the time of job search and finding those jobs in week 16.[48]

For the *city* sample the treatment effects on job search seem to be dampened by the fact that an increasing number of this sample had found good jobs over time (see Table D.13 showing a significant upward linear trend of the effect on employment). By the final months of the study, individuals in the *city* sample were more likely to be searching conditional on not working (see Figure 3).

I also plot the impact on the probability that a respondent is "discouraged" (not searching nor working), in Figure 3 (see Figure C.9 for weekly estimates). The treatment has a significant negative effect on discouragement in both samples. This is the combined effect of increasing employment and job search. This indicates that treatment did not just cause individuals who would otherwise have been active job seekers to find employment, but also to bring individuals who would otherwise have been doing nothing back into the labour market.

6.4 Persistence

I find a significant impact on searching for work even after subsidies ended, for the *board* sample. I do not find such an effect for the *city* sample. This is consistent with my assumption that the two groups differ in terms of initial buffers savings. The *city* sample appeared to start the study already around the savings threshold constraint, searching for work irregularly. The

[48] In the endline data, respondents with permanent jobs overwhelmingly reported finding those jobs in the previous few weeks- the treatment group were particularly likely to have found their jobs in the time since the last phone call in week 12: 28% versus 14% in the control group.

model predicts that they would search more regularly, but not save up more buffer savings while subsidies are in place. By contrast, the *board* sample has individuals with enough buffer savings to keep them intact until after the subsidies ended.

I exploit randomized variation in the timing of when subsidies end to compare individuals receiving subsidies in weeks 9-11 to those who finished the treatment earlier. I run the regression:

$$y_{it} = \alpha_t + T_i\lambda + P_{it}\delta + X_{i0}\beta + \epsilon_{it} \qquad\qquad \forall t \geq 8 \tag{11}$$

where P_{it} is equal to one only if participant i was receiving the treatment in the week t. Once the treatment period randomly ended for an individual, this treatment variable P_{it} "switches off", while T_{it} stays on.

The results are in Table 4. Panel A shows the simple effect of the subsidies on search at the endline, while Panel B represent the results from Equation 11. The coefficient on *Treat Ever* (λ) is large and highly significant while that on *Treat now* (δ) is not. This shows that there is no additional effect of receiving the treatment in the current period, over and above the effect of having received the treatment at any time. Individuals who stopped the treatment went on searching as intensely as those who were still receiving it during weeks in 9-11.

7 Robustness and Mechanisms

I have argued that the trajectory of impacts is not compatible with a mechanism whereby subsidies provide a temporary incentive to search alone. Such a mechanism would predict constant treatment effects over time, no persistence, and no impact on temporary work. My model of cash constraints more fully explains the trajectory of impacts over time. Here I present further evidence for this explaination, by testing whether individuals who are more cash constrained respond more to treatment, and look for evidence of other mechanisms, such as motivation, learning, or aspiration effects. While I cannot rule out all possible alternative explanations for the results, this suggests that cash constraints are a key mechanism.

7.1 Heterogeneous treatment effects

The treatment effects should be larger for individuals who are more cash constrained at the baseline. Since those who are very wealthy to start with should not be constrained by the costs of search. In a model without cash constraints, the impact of search costs should be independent of wealth.

I estimate differential treatment effects for individuals above and below median household wealth, expenditure and savings using Equation 2, and test for equality of these coefficients. For the *board* sample I find clear evidence that poorer individuals benefited more from the treatment than wealthier ones. Individuals from poorer households were more likely to find permanent jobs, or have any work at all, after receiving subsidies, while individuals with low savings at baseline saw a disproportionately large impact on the probability of being discouraged at endline.[49] The results for the *city* sample are less clear. However, I do find that those with low expenditure and savings seem to have benefited more from the treatment, the impacts on their probability of being discouraged are larger.

These heterogenous treatment effects hold for job search trajectories, as well as endline employment outcomes. The impact on the probability of searching during all weeks is higher for poorer individuals (see Table D.16 in the Appendix).[50] The coefficient for individuals from richer households is not significant, suggesting that the constraints of job search do not bind for those who are not cash constrained.

Finally, I find that those who had not worked in more than 4 months benefited far from the treatment than those who had spent less time out of the labour force. These results are in the Appendix, Table D.17.

[49]The effects on poorer households are large and significant, although I am not always able to reject the test of equality between the two groups in such small sub-samples.

[50]Here the F-stat on the difference between coefficients for rich and poorer individuals shows that this difference is significant at the 10% level.

7.2 Hawthorne effects

Using a group of respondents who were not called, nor given subsidies, I look for impacts of the phone call surveys on endline outcomes, to test whether the regular calls induced Hawthorne effects. The phone calls might have primed individuals to think that search was a worthwhile activity, or offered a higher promise of employment than it actually did, or just induced shame for not searching.

In Table D.19 in the Appendix "TE call" estimates the impact of receiving the phone calls on everyone who received them, "TE trans" is the impact of receiving the treatment, over and above the impact on the calls.[51] I find no significant impact of getting phone calls, across a range of specifications. The impact of the subsidies is still large and significant, although less precisely estimated with a smaller control group.

7.3 Other mechanisms

If the subsidies impacted outcomes related to motivation, expectations of future job offers, and perceptions of market wages, this could constitute a competing explaination for the trajectory of impacts. In Table D.20 in the Appendix I find very little evidence for treatment effects on a range of such outcomes. This is particularly true for the *board* respondents, unsurprisingly since these individuals were already very familar with search, and the job boards.[52]

In addition, I argue that the results were not driven by changes in commuting costs *to get to work*: many of the impacts on good quality jobs are only seen towards the end of the study period, probably because it takes a while to get good jobs because of the long application periods for formal jobs. So respondents would not have had time to benefit from lower commuting costs for long. Furthermore the subsidies had not been available for over a month at the time of the endline survey, the point at which I find large impacts on employment outcomes.

[51] All treated individuals were called.

[52] I do find a significant negative impact on perceptions of average and fair market wages, among the *city* sample. Job seekers in this sample had over-inflated expectations about the wages that they could earn in the market. Respondents said they expected to earn about 1500 Birr ($75) per month on average at baseline, when in reality, those that found jobs earned little over 1000 ($50). This result is consistent with the unemployed learning about the true value of jobs through additional search.

8 Conclusion

I conduct a randomized controlled trial of subsidized transport to study the impact of reduced search costs on job search and employment outcomes. Subsidies lead to an increase in job search intensity, a reduction in reliance on casual labour, and better employment outcomes four months later.

The paper considers the interplay between savings, consumption and temporary earnings in the job search process. The trajectory of impacts, estimated in this paper using high-frequency phone call data, are hard to explain only through a mechanism whereby reduced search costs provided a price incentive to search. I find evidence that cash constraints play a role, and develop a dynamic model of job search and buffer savings to explain this mechanism. The evidence for cash constraints is particularly strong for the *board* sample, a group of more active job seekers.

The results add to evidence from developing countries showing that cash constraints can limit mobility and access to employment opportunities. The returns to reducing search costs seem to easily outway the costs of the subsidies provided. In Appendix B I argue that the total cost per treated individual amount to about $9 on average, while the gains from finding permanent employment faster are estimated to be between $15 and $30, depending on assumptions. This does not include the non-monetary benefits of improved job quality. This suggests that job seekers would borrow to finance job search up front if they could.

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

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Tables and Figures for placement in text:

Table 1: Attrition by treatment status

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

Table 2: Effects of transport subsidies on employment (Work and Permanent Work) at both endlines

Outcome	Any Work		Permanent Work	
	(1)	(2)	(3)	(4)
Week	16	40	16	40
Panel A: Average Treatment Effects At Follow Up (Pooled Sample)				
All	0.067*	0.068*	0.043	0.017
	(0.034)	(0.039)	(0.027)	(0.033)
Control Mean	0.53	0.55	0.13	0.21
Observations	658	605	657	605
R^2	0.578	0.602	0.215	0.323
Panel B: Treatment Effects At Follow Up by Sample				
Board	0.046	-0.0096	0.078**	0.033
	(0.051)	(0.051)	(0.037)	(0.051)
City	0.088**	0.11	-0.0043	-0.0015
	(0.041)	(0.080)	(0.034)	(0.038)
Board Control Mean	0.58	0.65	0.19	0.31
City Control Mean	0.46	0.41	0.065	0.080
Observations	658	605	657	605
R^2	0.581	0.6104	0.221	0.324

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

Table 3: Effects of treatment on job quality and type at endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			log			in	pay			
	work	casual	ln wage	hours	degree	office	monthly	satisfied	formally	in city
Panel A: Impacts on work outcomes at week 16										
TE Pooled	0.062*	-0.022	0.051	3.74**	0.047**	0.070*	0.069*	0.061**	0.054*	0.059*
	(0.035)	(0.024)	(0.088)	(1.71)	(0.018)	(0.037)	(0.037)	(0.028)	(0.029)	(0.032)
Observations	658	596	356	656	596	596	596	596	596	596
R-squared	0.067	0.077	0.115	0.079	0.228	0.059	0.107	0.058	0.114	0.051
Panel B: Heterogeneous impacts on work at week 16 by Sample										
TE board	0.043	0.0026	0.091	2.53	0.075**	0.020	0.032	0.015	0.064	0.097**
	(0.051)	(0.025)	(0.11)	(2.34)	(0.033)	(0.052)	(0.053)	(0.045)	(0.049)	(0.042)
TE city	0.087*	-0.050	-0.0090	5.27**	0.014	0.13**	0.11**	0.11***	0.042*	0.015
	(0.044)	(0.042)	(0.15)	(2.34)	(0.011)	(0.050)	(0.049)	(0.029)	(0.023)	(0.046)
Observations	658	596	356	656	596	596	596	596	596	596
R-squared	0.067	0.079	0.116	0.080	0.230	0.063	0.108	0.062	0.114	0.053
Board mean	1	0.12	5.55	42.5	0.19	0.39	0.59	0.27	0.33	0.42
City mean	1	0.36	5.36	36.0	0	0.19	0.26	0.35	0.058	0.41

Results are from OLS regressions on endline outcomes at week 16, using a set of baseline covariate controls. Panel A shows results for the two samples pooled together. Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Dependent variables: (4) *hours*: Hours worked per work. (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 (8) *Formally:* The respondent has a job which he/she reports being very satisified with. (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 (10) *In city:* The job is performed in an area close to the centre of the city.

Control means in the lower rows show the mean of the outcome for those individuals who have jobs, for the two samples respectively.

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

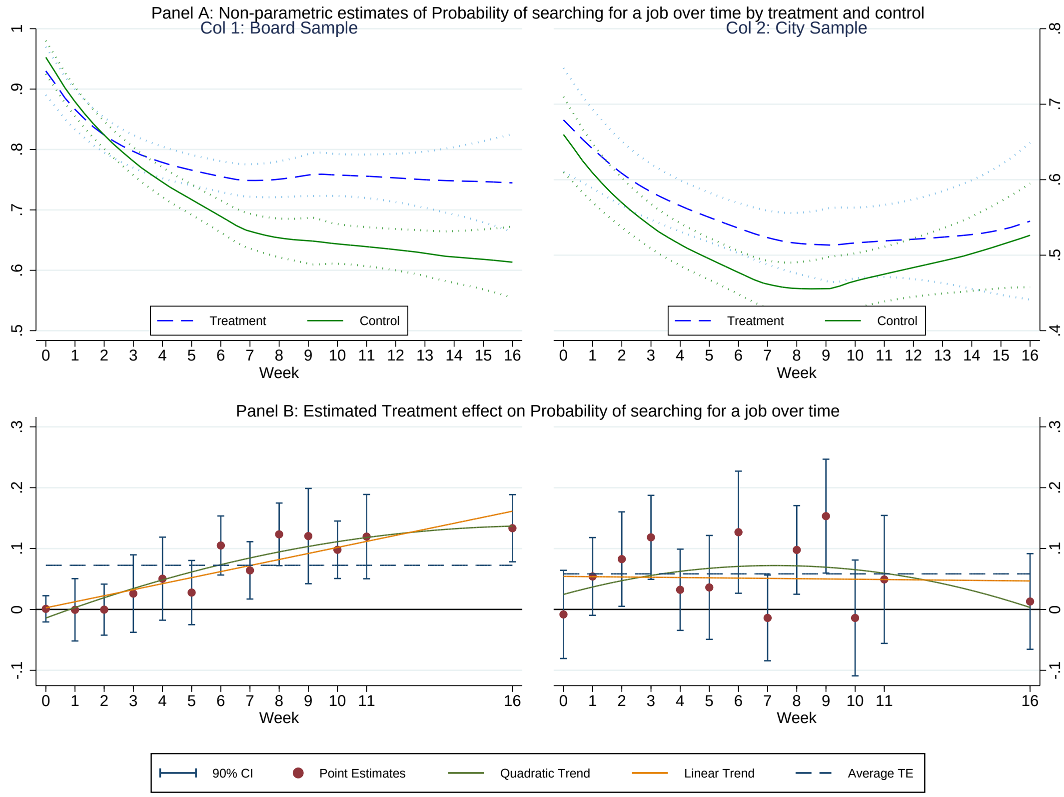


Figure 2: Impact on travelling to the city centre: Non-parametric trends & treatment effects over time

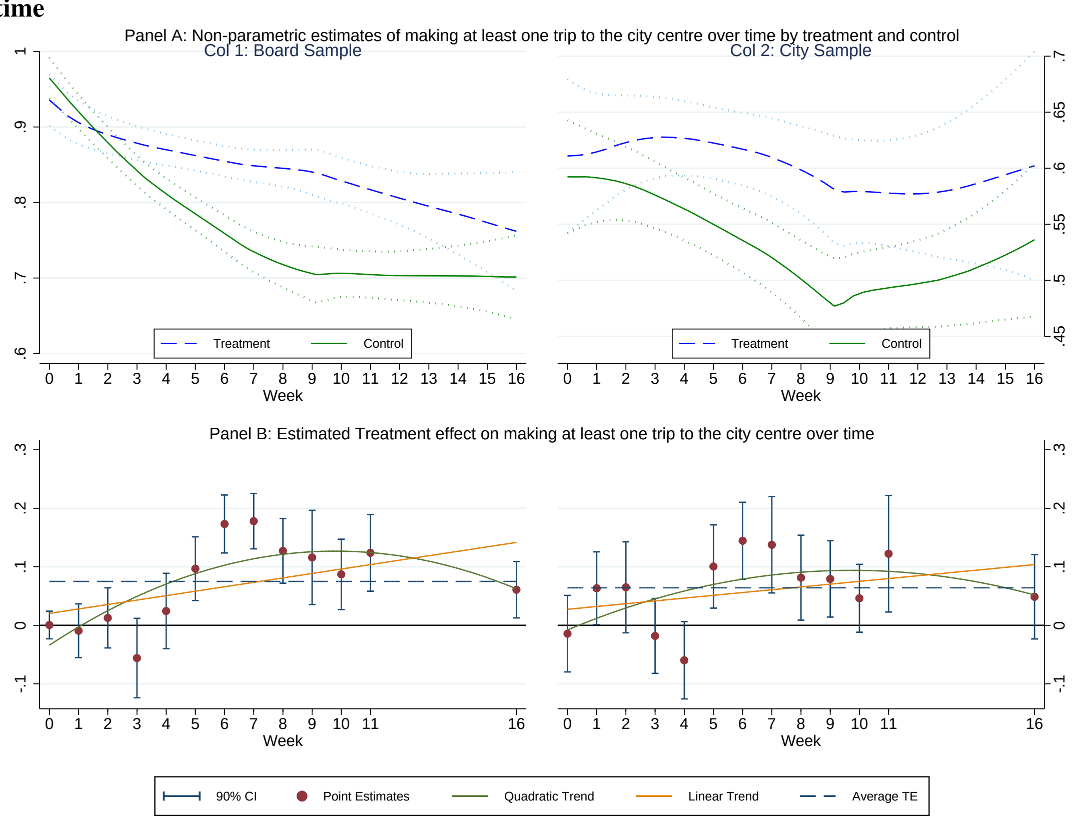
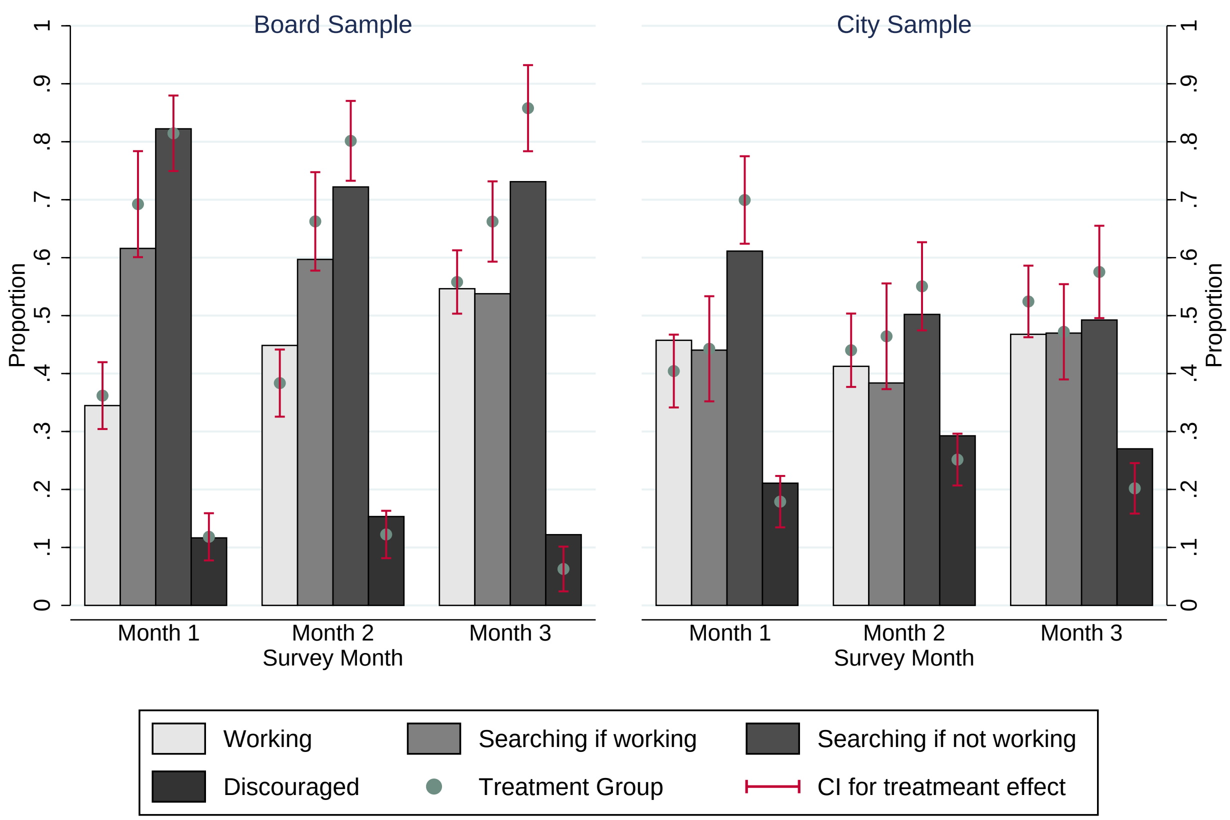


Figure 3: Control means and treatment effects on labour outcomes by month



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

Table 4: Persistence of treatment effects after subsidies have ended

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

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

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

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

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

Table 5: Heterogeneous effects on endline outcomes by respondent wealth

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

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

Material for Online Appendices

Appendix A Theory

I develop a model which considers the consumption and job search decisions of an unemployed cash- and credit-constrained job seeker. I use this model to explain the job search behaviour and the trajectory of treatment effects observed in the data. The model demonstrates why job seekers don't search for work when the returns to search are very high and they have enough cash on hand to pay the costs of search (at least once). I based the key assumptions of the model on observations from the data as well as qualitative insights from focus group discussions conducted during the baseline surveys.

The model describes the experience of an individual who is cash constrained, and who receives income from taking spells of temporary work in order to smooth consumption and earn money to pay search costs. In each week, she pursues a permanent job by choosing to pay a fixed monetary search cost.[53] The model incorporates search-on-the-job. Agents can search for work and have temporary employment at the same time.

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

After describing the main features of the model, I proceed as follows: Firstly, I provide stylized solutions to the stationary version of the model. The key prediction of the baseline model is that job seekers find it optimal to not search below a critical value of savings, and that this critical value can be many orders of magnitude larger than the cost of search itself.

Secondly, I simulate time series predictions of savings, consumption and search paths using the solved model. This allows me to predict stylized dynamics of job search behaviour over

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

time. I show that these simulated dynamics are consistent with those found in the data for a wide range of parameter values.

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

The model is similar in its mechanisms to buffer savings models of investment (Deaton, 1991; Bryan et al., 2014; Vereshchagina and Hopenhayn, 2009): in each period job seekers make the risky investment decision only when they have sufficient savings to do so, because borrowing is not possible. However, the model differs in it's dynamics: job seekers must continue to make the decision to search over and over again, the costs of searching low in earch period, but so are the expected returns. In most of the buffer savings/investment models agents who are able to save up to make investments 'survive': they continue as entrepeneurs or successful urban migrators. In my model some one with enough cash on hand to search expects to run down their savings while In this sense my model is an interested in the behaviour who is an active searcher, and the point at which he or she gives up search, as much as it is interested in someone saving up to search again. In this sense the flavour is similar to unemployment insurance models such as Chetty (2008) where unemployed job seekers search for work with cash on hand, but choose suboptimal outcomes when savings get low. I adapt this search framework by introducing a monetary search costs.

A.1 The Model

The model is presented in discrete time. As is the case in the surveys and empirical analysis, the time period under consideration is one week. The job seeker begins each week with personal savings x, which could be cash on hand or formal savings in the bank. She starts the week by deciding whether she will search for a permanent job and pay the corresponding cost of search p. The decision to search is binary, so that the cost of search is constant and corresponds to the costs of transport, photocopying, making applications and buying newspapers.[54] After the

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

decision to search is made and the outcome is observed, the job seeker chooses that period's consumption to maximize future expected utility.

Searching for work at the start of the period leads to a permanent job with probability σ. If a permanent job is found, unemployment ends, and the newly employed person receives income Y with certainty in the current period and in all future periods. For tractability, I make the simplifying assumption that permanent employment is an absorbing state: these jobs cannot be lost.[55] The permanently employed worker solves a kind of cake eating problem, augmented with a permanent income stream, with next period's consumption discounted by the factor β. The worker chooses consumption c in each period:

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

Someone who hasn't paid the cost of search cannot find a permanent job in that period. Individuals remain unemployed for the rest of the period if they have searched but failed to find work, or have not searched at all. Unemployed individuals can earn: with probability θ they earn a known income W, which is paid at the end of the period.[56] W could be interpreted as cash transfers from other family members but is more realistically thought of as income from temporary employment, earned for work done as a casual/temp labourer, for a family member, or in self-employment in the informal sector. Search is not required to get these temporary jobs.[57] The model can be adapted to model family income support directly, by adding a set amount to a job seekers finances each week. Similarly, the model assumes job seekers have cash on hand which does not earn interest. The models predictions are not qualitatively changed by the inclusion of small returns to savings, or cash the family.[58]

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

[55] In reality, few respondents do in fact leave or permanent jobs. They come with secure contracts and are highly desirable. My high-frequency sample contains only 3 cases of individuals leaving jobs they considered to be permanent.

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

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

[58] I discuss, in Section A.5, the impacts of guaranteed income streams on job search. In general, this modifications reduce the risk associated with searching, and thus increase search activity.

c, solving:

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

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

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

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

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

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

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

I am interested in when individuals choose to search for work. That is when $S(x) \geq F(x)$. The key insight of the model comes from Expression 14. $F(x)$ is monotonically increasing and concave, and $S(x)$ is a convex combination of $F(x-p)$ and $V(x)$. Therefore, as is the standard result for models of this kind (Bryan et al., 2014; Vereshchagina and Hopenhayn, 2009; Buera, 2009) $S(x)$ crosses $F(x)$ once from below.

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

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

critical value as:

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

The right hand side of this expression represents the expected benefit of searching and the left hand side the expected loss, relative to not searching. At low levels of savings $F(x) - F(x - p)$ becomes large and searching becomes very risky because marginal utility of future consumption is very high. The left Panel of Figure A.1 illustrates this intuition. $S(x)$ falls quickly as savings get lower.

The model implies a steady state level of savings at x^*. Below this level individuals do not search, but try to save up for search. Above it they run down their savings by paying the costs of search.

The model predicts that lowering the cost of search reduces the critical value x^*. Lowering search costs reduces the risk associated with searching. Define x^{*t} as the new critical value when the costs of search have been reduced. Lowering search costs also has implications for the dynamics of savings over time: job seekers run down their savings less slowly when costs are lower. These dynamic implications are discussed in detail in Section A.3.

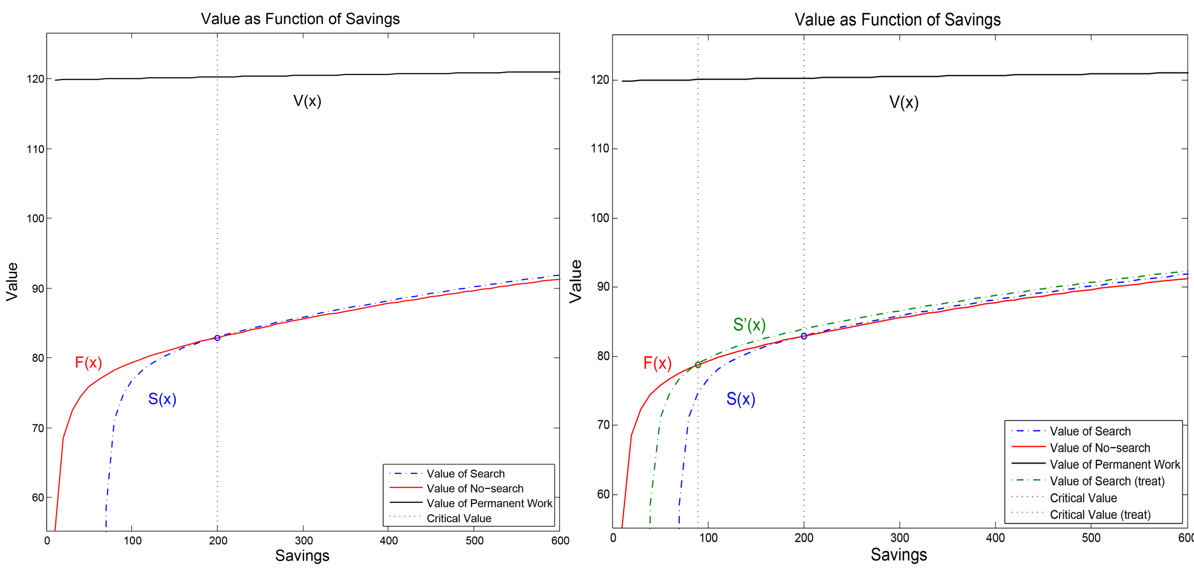
This model cannot be solved analytically, because the job seeker's optimal policy depends on wealth levels, which are in turn a function of her optimal decisions. I now turn to describe the numerical solutions of the model.

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

A.2 Solution to the Stationary Model

In this section, I solve the model numerically.[60] This happens in two steps. First I calculate the value of permanent employment, which is given simply by Expression 12. Secondly I use that value of permanent work to estimate the value of searching, not searching, and unemployment, and solve for these and the corresponding consumption levels in each state. In these solutions I use a power utility function of the form $u(c) = \frac{c^(1-\delta)-1}{1-\delta}$, where δ is a measure of constant relative risk aversion. I use log-utility $u(c) = ln(c)$ for the case of $\delta = 1$.

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



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

Figure A.1: Single Crossing Point of the Value of Searching and not Searching

The left panel of Figure A.1 shows the value for searching and not searching for different levels of savings, and the critical value above which it pays to search. I verify that the model implies that $S(x)$ crosses $F(x)$ once from below, and I solve for the critical value x^* at which an unemployed individual is indifferent between searching and not searching. In this case it is $x = 200$. Figure C.12 in the Appendix shows the optimal consumption paths for an individual who is forced to search, and an individual who cannot is forced not to search in the current period.[61]

I calculate the impact of reducing the cost of search (usually by a half or a third) on x^*. I call this x^{*t}. I then also calculate x^{*s}, the critical value for the case when search costs are reduced for just one period, before it revests back to the original search costs, so that the continuation value of unemployment remains unchanged.[62]

I then calculate the proportion of individuals that would be induced to search by the change in the critical value, given the distribution of savings in the data. That is, the proportion of individuals with savings $x^{*s} < x < x^*$ at baseline. This gives a rough idea of the static impact

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

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

of reduced searched costs due to the experimental treatment. See the right hand Panel of Figure A.1: reducing the search costs for one period shifts the value of search to left, such that (in this example) everyone with savings $90 < x < 200$ are induced by treatment to start searching in that period.

Can the model's predictions of the impact on x^* be reconciled with the impact of the randomized subsidy on the probability of someone searching for work? Here a provide a short example of how the model can be used to predict the impacts of the treatment used in the experiment: I solve the model using log-utility (the special case of CRRA utility with $\delta = 1$, $\beta = 0.9$, and the probability of finding informal work while unemployed to $\theta = 0.3$. Other parameter chooses are outlined in Table A.1.

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

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

In the baseline data savings are 874 at the average, with a standard deviation of about 1300, and a median of 400. Here 12.2% of the sample have savings between 360 and 600, and would be induced to search for one period according to the predictions of the model. This is in line with my experimental estimated treatment effect of 8% on the probability of searching for work.

A.3 Dynamic Simulations

The model can be used to predict search, saving and consumption behaviour over time, given a starting level of savings. In this section I simulate the behaviour over time of individuals who remain unemployed: agents search for permanent jobs but never find them. I use these predictions to confirm the dynamics of job search, among the unemployed observed in the data. The data shows that job seekers give up search over time after they start out searching, and that many job seekers oscillate between searching and not searching every few weeks. The proportion of individuals searching in the data stabilizes over time.

I simulate the dynamics of two populations, each intended to represent the *board* and *city* samples, respectively. I model the two populations differently only in terms of their initial condition: the distribution of cash on hand. The two samples could be thought to differ along other dimensions: risk aversion, efficacy of search effort, the value of outside options.

I simulate an initial savings level for 1000 individuals, for the *board* sample I simulate savings well above the critical threshold, while for the *city* sample I draw from a distribution centred around the critical value of savings under the original (control group) cost of search.[63] I then simulate a series of random temporary employment shocks for each individual in each time period. Using the optimal consumption and search decisions from the solution to the calibrated model, I look at the evolution of job search and savings over time.

Job seekers' savings converge to the critical x^*, above this level they search and run down their savings, below this they save up for search. This of course is happens immediately for the *city* sample whose simulated savings were already distributed around this critical level, while it happens more slowly for the *board* sample. In each case I calculate the proportion of individuals searching in steady state, given by s^*. s^{*t} gives the corresponding steady state proportion searching for work when the search costs are reduced.

In Figure A.2 I present the results of such a simulation with a representative agent with a log-utility function, $\theta = 0.3$, $\sigma = 0.03$, $\beta = 0.95$, and $p = 60$. The critical value at which someone gives up search is $x^* = 600$ for this calibration. The graph shows the proportion of individuals searching for work over time, for a control group and a treatment group with identical savings, but transport costs reduced by one third.

When search costs are lowered, respondents in the *city* sample immediately begin to search more regularly, converging to a higher steady state proportion of search. They spend more weeks searching, in steady-state, than they did with the higher search costs.

By contrast, for the board sample, the treated individuals run down their savings less slowly, so that initially there is no major difference for treated individuals. As both groups converge to the steady state proportion of searchers over time, the treated respondents are about 20% more likely to search in each period, in steady state.[64]

[63] I use a logNormal distribution for the simulated savings.

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

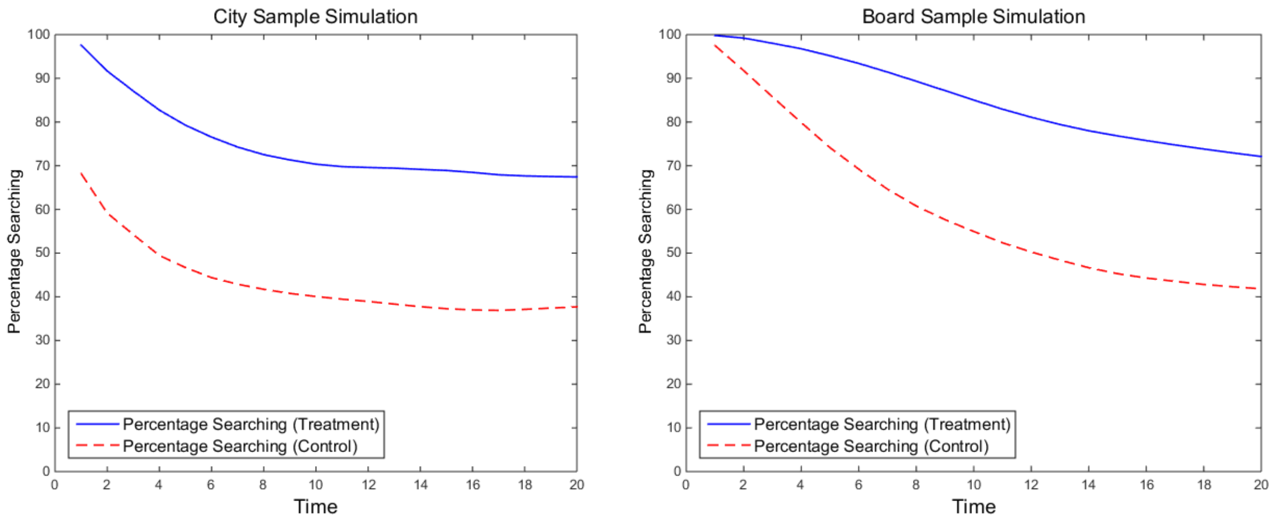


Figure A.2: Percentage of the sample searching for a job over time (simulation)

A.4 Calibrations

Here I repeat the analysis in the examples above for a range of parameter values. For each parameterization of the model I calculate x^* and s^*.[65] I look at the effect of reducing search costs from p to p_t (60 to 40 Birr) on each of these outcomes, either for just one period, or permanently. I show that the predictions of the model are broadly consistent with the data for a wide range of parameter values. The calibrations also illustrate interesting additional predictions of the model. Table A.1 shows the calibration choices.

The impact on x^* is slightly bigger when search costs are reduced for only one period, rather than permanently reduced ($x^{*t} \leq x^{*s}$), but this difference is usually small. I calculate the % induced to search (Col 4) in the static case by looking at the proportion of my sample who had savings between x^{*t} and x^*, and the impact on the proportion searching in steady with lowered search costs (s^{*t} -s^*).

In Table A.2 I look at the effect of risk aversion, and the probability of having temporary work (θ) on the results. I use power-utility, with different values for the constant rate of risk aversion δ. As expected, the critical value x^* is monotonically increasing in the risk aversion, job seekers are less likely to take the risk of searching. Similarly the steady state proportion of searchers falls with risk aversion. There is no clear relationship between high-levels of risk aversion and the predicted treatment effects (in Col (4) and Col (7)): when risk aversion is very high, hardly anyone searches, even with low transport costs.

[65] These are the critical value for savings and the steady-state proportion of job seekers, respectively.

Table A.1: Key parameters values for model calibrations

para-meter	description	value
Y	Weekly wage for permanent wages	400 Birr
W	Weekly wage in temporary work	320 Birr
p	Weekly cost of searching actively work	60 Birr
p_t	Subsidies cost of searching for work	40 Birr
σ	Probability of finding permanent work if searching	0.03
θ	Probability of offer of temporary work if without permanent work	(0.1-0.5)
β	Discount rate	(0.8-0.99)
$u(c)$	Utility function: power (CRRA) utility	
δ	CRRA for power utility function	(1-2.8)
Solves for:		
x^*	Critical value of savings below which agent does not search	
s^*	Steady state value probability of searching in each week	

These calibrations do two things. They show that the critical value x^* is highly sensitive to the cost of search: so that a significant proportion of individuals can be induced to search more, and for longer, by lower search costs.

Secondly they show that the treatment effects on jobs search found in the paper can be rationalized by the theoretical model of cash constraints. In fact, in many calibrations the predicted treatment effects are considerably larger than the treatment effects found in the paper. High rates of risk aversion are not needed to rationalize my results: calibrations with relatively low risk aversion parameters are more consistent with the proportion of job seekers searching in each week, and the estimated treatment effects.

The model seeks to explain the behaviour of a marginal job-seeker, for whom search costs are actually salient. The subsidies were not likely to be useful to all job seekers. In fact, on an average week, between 40% and 50% of the subsidies were collected during the experiment. If these were the only individuals in the sample that were truly cash constrained, the model would predict smaller impacts, in terms of ITT, than the calibrations imply. Thus the predicted effects over between 20 and 30 percentage points in Table A.2 would be very much in line with the experimental ITT estimates of reduced transport costs, which were about 10 percentage points.[66]

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

Table A.2: Risk aversion & job search: Calibration of the main outcomes & simulated treatment effects

(Agent with power utility function or log-utility for $\delta = 1$. $\beta = 0.95$, $p = 60$, $p_t = 40$, $Y = 400$, $W = 320$, $\sigma = 0.03$)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Statics				Steady State		
	x^*	x^{*t}	x^{*s}	induced	s^*	s^{*t}	s^{*t} -s^*
δ	$\theta = 0.1$						
1	480	320	300	7.6%	15.0%	33.7%	18.6%
1.2	640	440	400	9.0%	10.2%	23.8%	13.6%
1.4	840	560	540	8.0%	5.2%	19.0%	13.9%
1.8	1280	860	820	8.2%	1.5%	10.6%	9.1%
2	1500	1020	980	8.0%	0.5%	7.7%	7.3%
2.4	1820	1340	1320	2.2%	0.3%	2.8%	2.5%
2.8	1940	1660	1620	0.8%	0.3%	1.1%	0.8%
δ	$\theta = 0.3$						
1	400	260	240	5.2%	53.1%	76.5%	23.3%
1.2	520	340	320	11.6%	44.4%	71.0%	26.6%
1.4	640	440	400	9.0%	38.2%	64.3%	26.2%
1.8	920	640	580	8.0%	23.9%	54.2%	30.3%
2	1060	740	680	10.4%	19.3%	48.5%	29.2%
2.4	1360	960	900	7.6%	10.9%	37.0%	26.2%
2.8	1640	1180	1120	3.4%	6.7%	29.1%	22.3%
δ	$\theta = 0.4$						
1	380	260	240	5.0%	66.1%	83.3%	17.2%
1.2	480	320	300	7.6%	59.1%	80.6%	21.5%
1.4	600	400	380	12.0%	49.8%	76.7%	26.9%
1.8	820	580	540	7.6%	37.9%	66.3%	28.4%
2	960	680	620	6.6%	30.1%	60.8%	30.7%
2.4	1220	860	800	8.4%	20.5%	53.1%	32.6%
2.8	1480	1060	1000	4.0%	13.2%	44.2%	31.0%

A.5 Temporary Work and Job Search

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

In Table A.3 I look at the case with power utility, and look the predictions of the model for a range of values for θ, and for δ. The critical value decreases with the value of θ in these

Table A.3: Risk, income security, and job search: Solution for x^*

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

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

calibrations, except for when $\theta = 1$ and very high risk aversion.[67]

This result has important implications. It suggests that job seekers rely on temporary work for money to search for work. Temporary work insulates jobs seekers from risk while unemployed. The need to take temporary work seems symptomatic of cash constraints faced by job seekers.

This theoretical finding conforms with the key empirical result of Section 6.3, which showed that individuals who received transport subsidies were less likely to take temporary work while receiving the subsidies. In the model this is because the subsidies reduce reliance on temporary work and allow job seekers to concentrate on finding permanent jobs. [68]

As an extension the model allows me to simulate the effect of pure unemployment insurance. That is I set $\theta = 1$, but income while unemployed (now interpreted as indemnity payments rather than wages from temporary work) low at $W = 60 = p$. The model predicts a huge increase in the proportion of individuals induced to search. This suggests an important role for unemployment insurance to prevent job seekers from taking less desirable forms of work purely as a way to mitigate against risk (Browning et al., 2007).

A.6 Probability of Finding Work

The calibrations of the model simulate only the impact of reduced costs on job search. I do not attempt to simulate the impact of treatment on employment rates. On average, for the *board*

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

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

sample, the probability of finding a permanent job *per week* of search is about 2.5 percent, and in the simulations I calibrate σ to match that average return to job search. So in simulations where a 65% of the control group search for work in each week, 19% would end up with permanent work at endline, matching the empirical data by design.

It remains for me to show that the increased probability of job search induced by the treatment is consistent with the treatment effect on permanent work estimated in the paper. That is, given that I estimate that a week of searching for week leads to a permanent job with probability 2.5 percent, would the increase in the proportion of treated individuals searching in each week induced by the treatment lead to the increase in permanent employment of 7 percentage points (over a control mean of 19%) that was estimated at endline?

In each week, treated individuals are more likely to be searching for work by about 8%, over sixteen weeks to endline, relative to a control mean of 65%. This implies that the treatment increases the average number of weeks searched by about 1.5 weeks, over a control mean of about 9 weeks of search. It also leads to about 1.7 additional weeks of visiting the job boards. If returns to search are linearly and constantly related to the number of search activities, these averages would explain about a 2.9 percentage point increase in the probability of finding a job. Yet the actual effect is estimated to be about 7 percentage points. This seems to inadequately capture the size of the treatment effect found here.

However, the treatment seems to have effected the number of weeks that individuals searched, at the higher end of the distribution of job search intensity. I find that treated respondents are far more likely to have searched during *all* weeks of the study by a very large margin: among the board sample, treated respondents are about 50% more likely to have searched in all weeks of the study. The median number of trips to the boards among treated individuals is eighteen compared to twelve in the control group, for the full sample.

The returns to search may well exhibit non-linearities in the time spent searching. Noticing an important vacancy that all other job seekers missed might make all the difference. Job seekers are most likely to be successful if they've already made a few applications in recent weeks, so that the returns to search are highest when it is sustained and persistent. Thus shifting the right tail of distribution of search intensity, as the treatment did, is likely to have a larger impact an effect at the mean, which would explain the 7% treatment effect on permanent work estimated in the paper.

Appendix B Simple cost-benefit analysis

Are the costs of subsidizing transport worth the benefits to job seekers? Here, I try to calculate the total benefit and total cost per recipient of the transport program. This excercise is challenging: many of the benefits to job seekers are non-monetary: entry level jobs do not pay considerably more than entry level jobs, so the immediate gains in terms of life-long earnings are not observed here. I do not quantify the non-monetary benefits of better working conditions for those find better jobs. However, in terms of transfers given directly to the recipients of the program, the program was remarkably cheap. By revealed preference, the job seekers seem to value the permanent, formal jobs, for which they search. Individuals in the *board* sample even forego earnings in temporary work to concentrate on finding temporary jobs.

I look at the benefits to the *board* sample. Of board respondents, while about 70% collected the subsidies at least once, only about 50% of total subsidies were collected. This implies an average cost per individual of about \$9 in transport subsidies (50% of \$18, the average amount that could be collected, if all subsidies were collected across all weeks). The money was not spent if the money was not collected. I do not include administrative costs, assuming that these would be relatively low if transport costs were subsidized directly, and at scale. Was the cost worth the benefit per recipient?

At week 16, the treatment group were 7 percentage points more likely to have permanent jobs. Most of these respondents had only recently found those jobs, within the last month. At week 40 I estimated that the treatment group were still ahead by 3.5 percentage points, suggesting a rate of decay of half a percentage point per month. Assuming that the control group continue to catch up at the same rate, and integrating over the 14 months it would then take for them to do so completely, I estimate that the treatment group was on average half a month of additional permanent employment across the whole sample.

What is the value of this additional permanent employment? Assuming that permanent jobs pay the same as worse jobs forever, and the treatment only causes individuals who would have been relying on temporary work to have permanent work, then the value of permanent work comes from its regularity: workers with permanent jobs work every week, whereas those without it have temporary work about 50% of the time. This suggests that half a months permanent work translates into a gain of a quarter of a months salary, or \$30 in this context.

Furthermore, I find that permanent jobs pay far more in the long run. In sample of older workers, with higher wages, permanent jobs pay on average \$30 more per month than temporary jobs. Assuming that treated individuals are able to begin earning those higher wages exactly half a month earlier than the control group (assuming again that eventually the same proportion of the control group end up in permanent jobs), this would constitute an additional gain from the treatment of \$15.

This calculation suggests that the subsidies more than pay for themselves, using these partial equilibrium estimates, with a benefit of \$45 to a cost of just \$9 per respondent on average. However, because they were located in the centre, commuting to these costs would diminish the overall benefit. Half a month of commuting to the centre would cost approximately \$25 per month. These calculations not include the non-monetary benefits of the better permanent jobs, which were often safely, healthier and more prestigious. It also assumes that control respondents catch up to the treatment group completely within one year. If employment outcomes exhibit path dependence the treatment could remain ahead for considerably longer. For instance, if the treatment helped someone to find employment who otherwise would have become to discouarged and never found a good a job, the estimated benefits of the subsidies would be considerably larger.

Appendix C Charts and Images

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

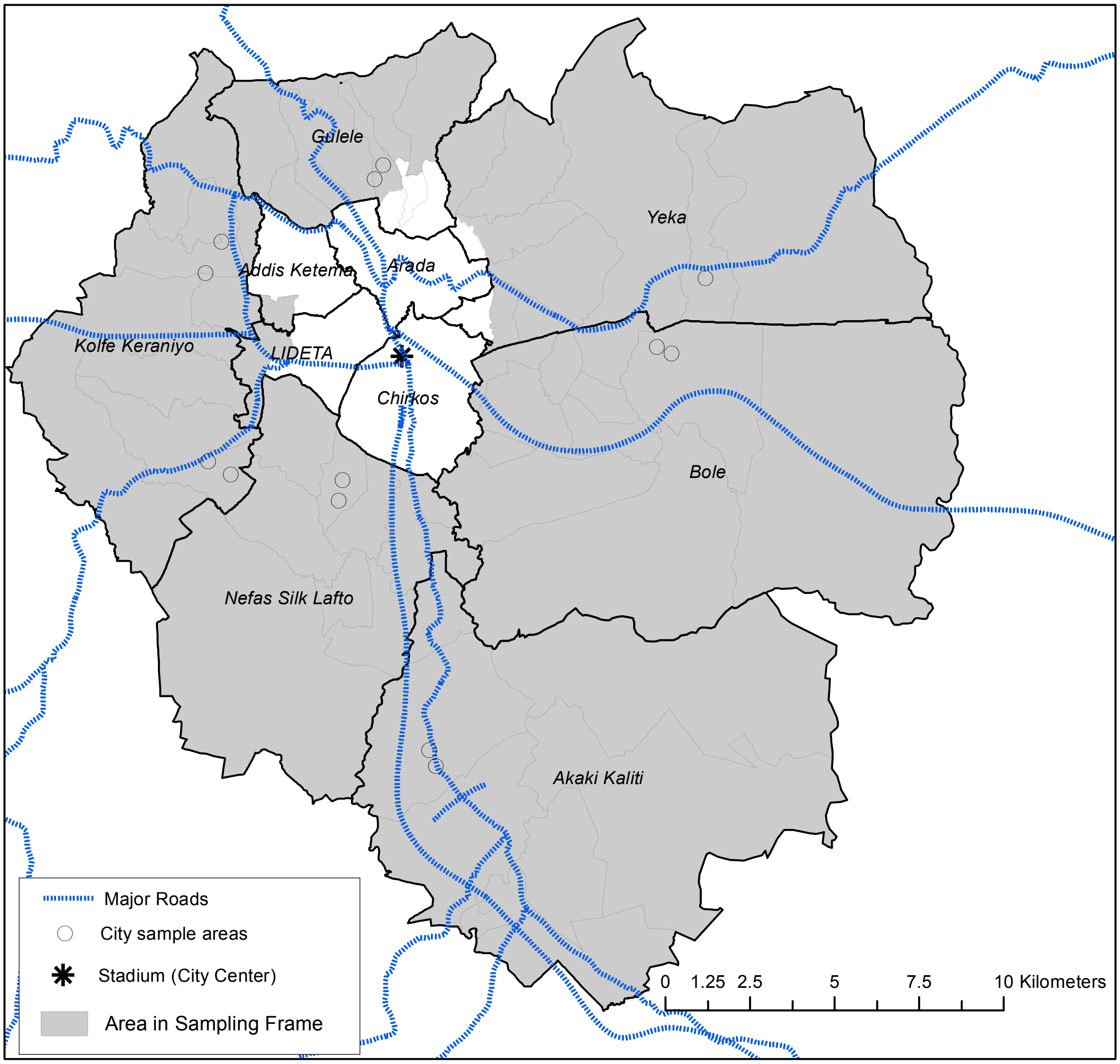


Figure C.2: Trajectory of Treatment effects across weeks in each sample

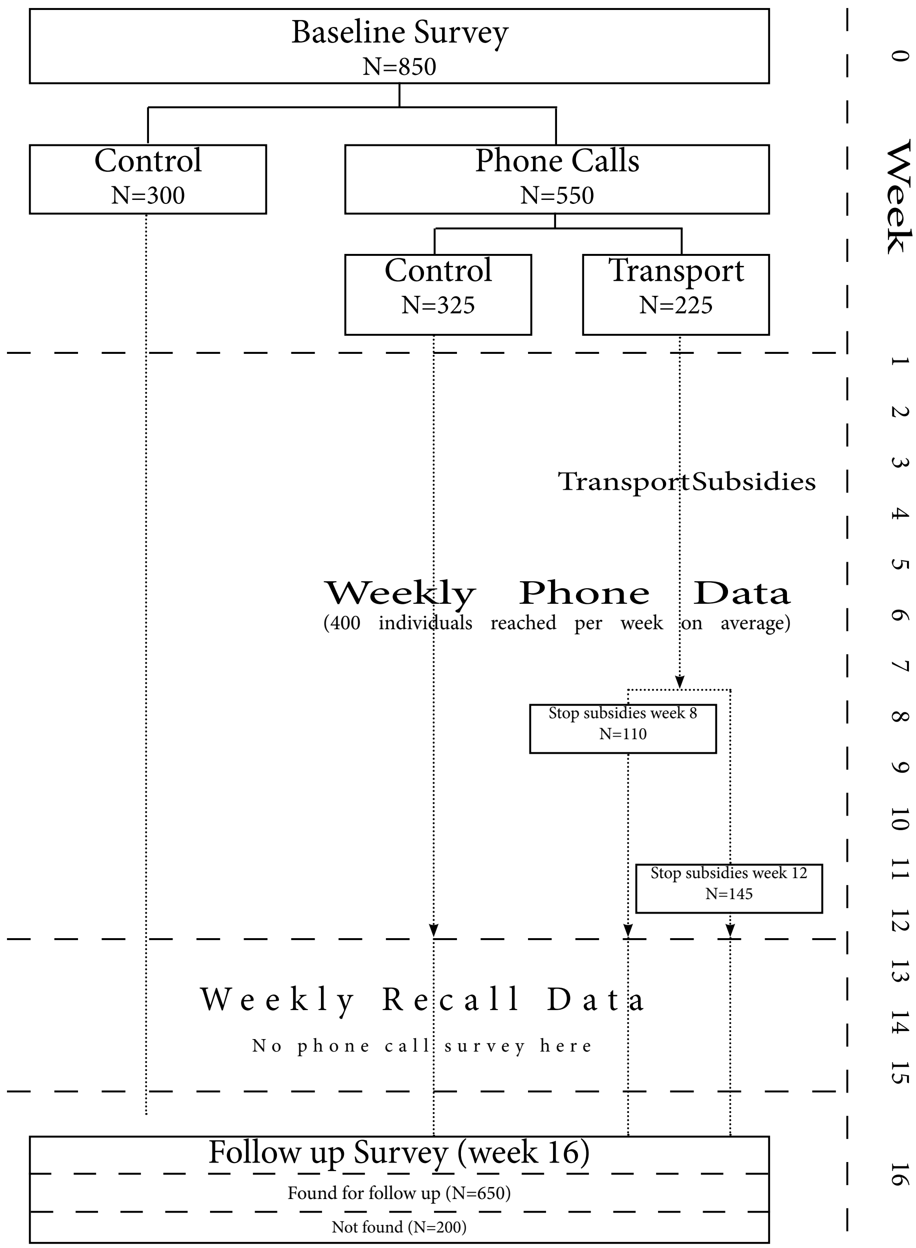


Figure C.3: Take up of the transport subsidy by week (among those eligible to receive it): local polynomial regression

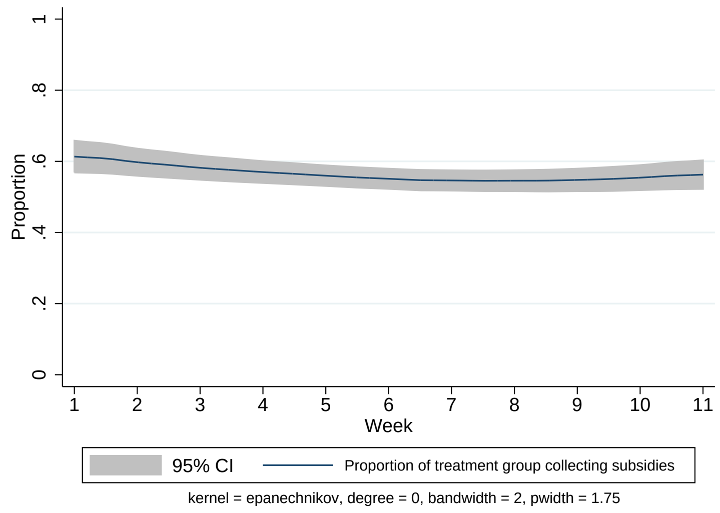


Figure C.4: Impact of treatment on distribution of occupations

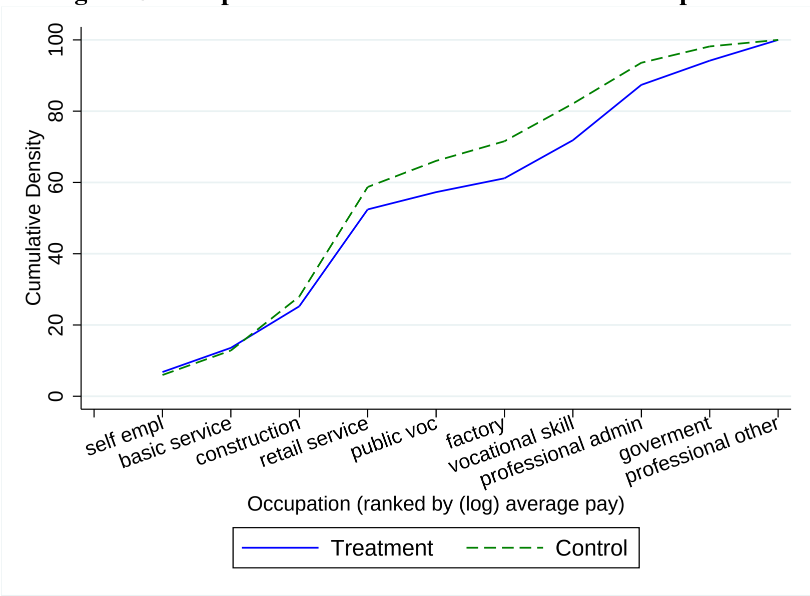


Figure C.5: Composition of the sample for each week by treatment and control: *Board Sample*

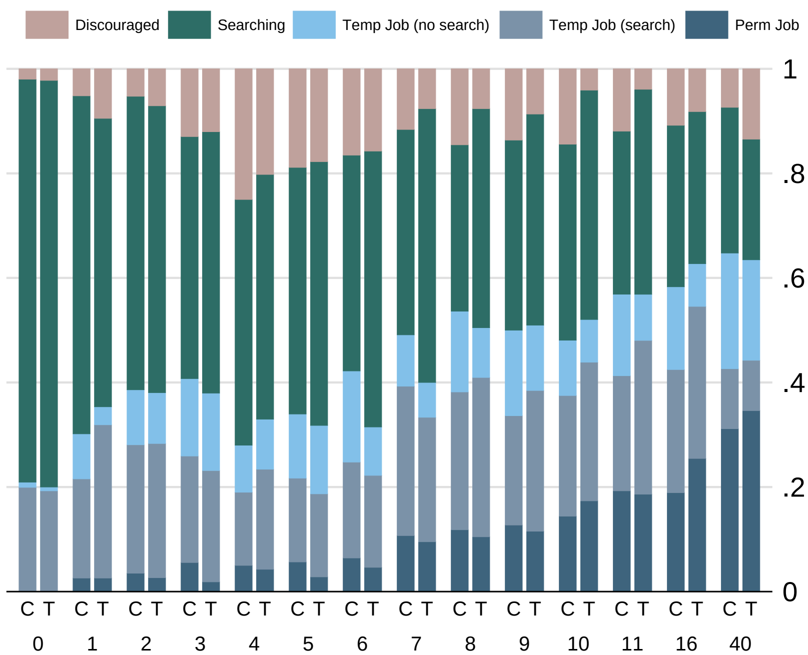


Figure C.6: Composition of the sample for each week by treatment and control: *City Sample*

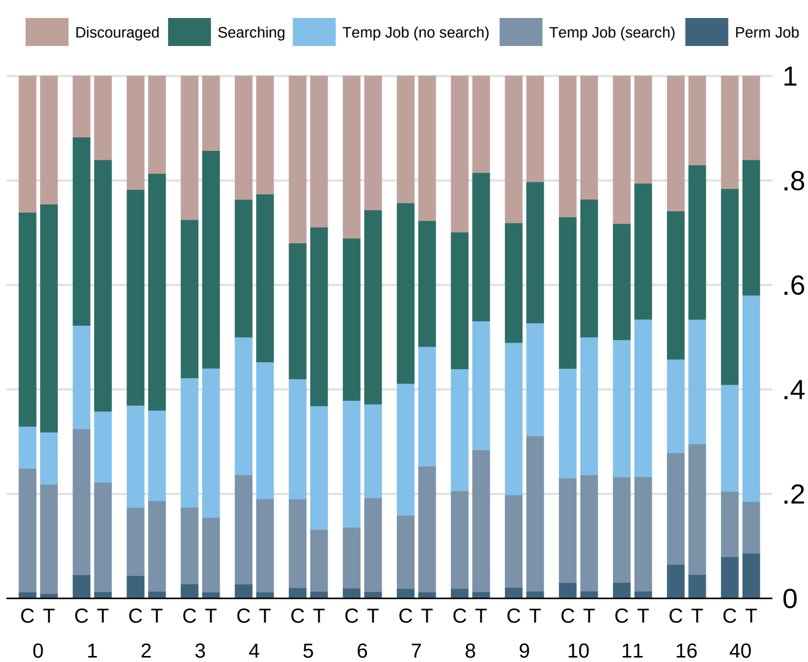


Figure C.7: Impact on having a job: Trends & treatment effects over time

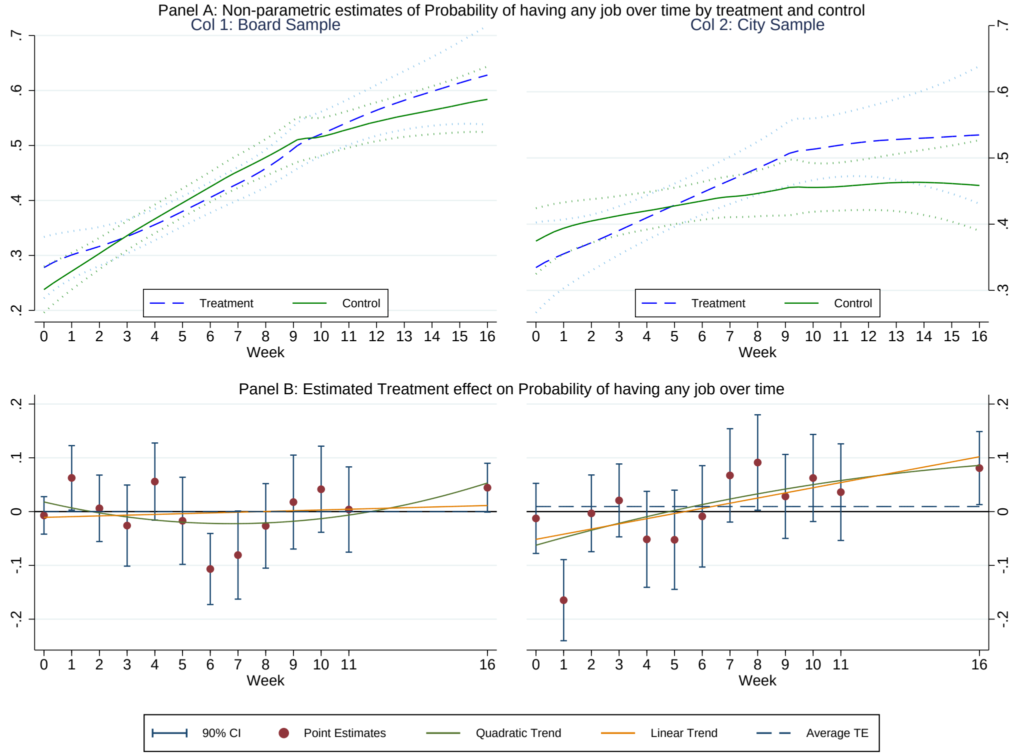


Figure C.8: Impact on search at the vacancy boards: Non-parametric trends & treatment effects over time

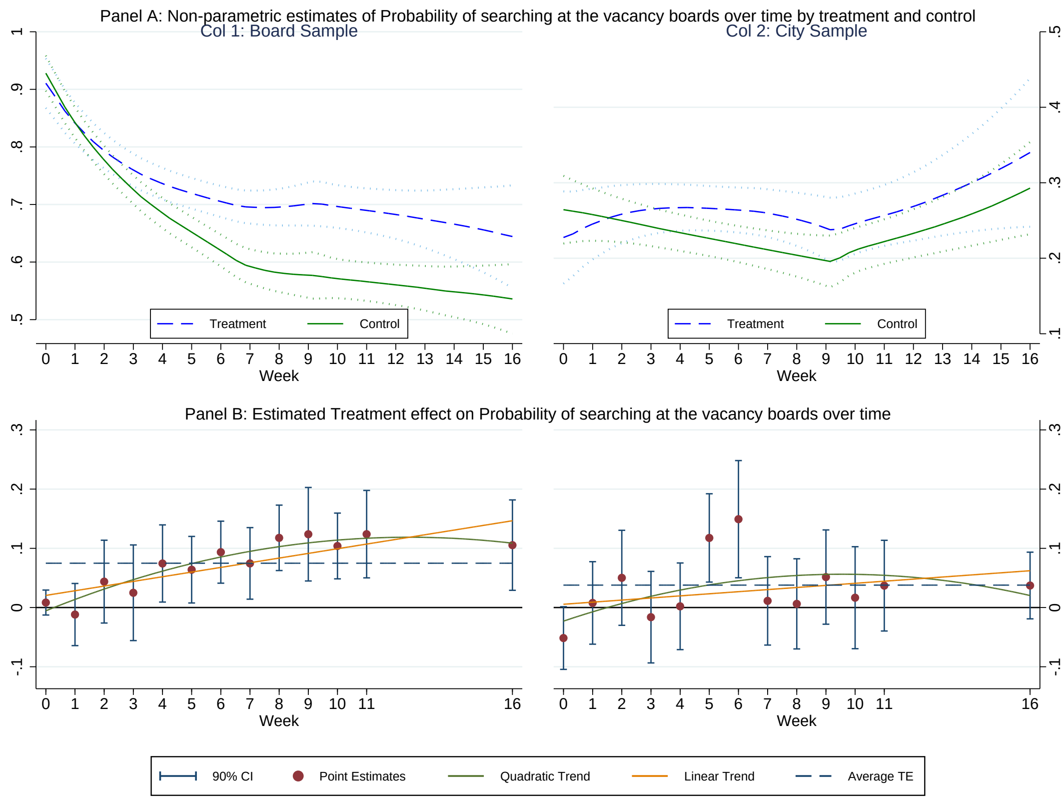


Figure C.9: Impact on discouragement: Trends & treatment effects over time

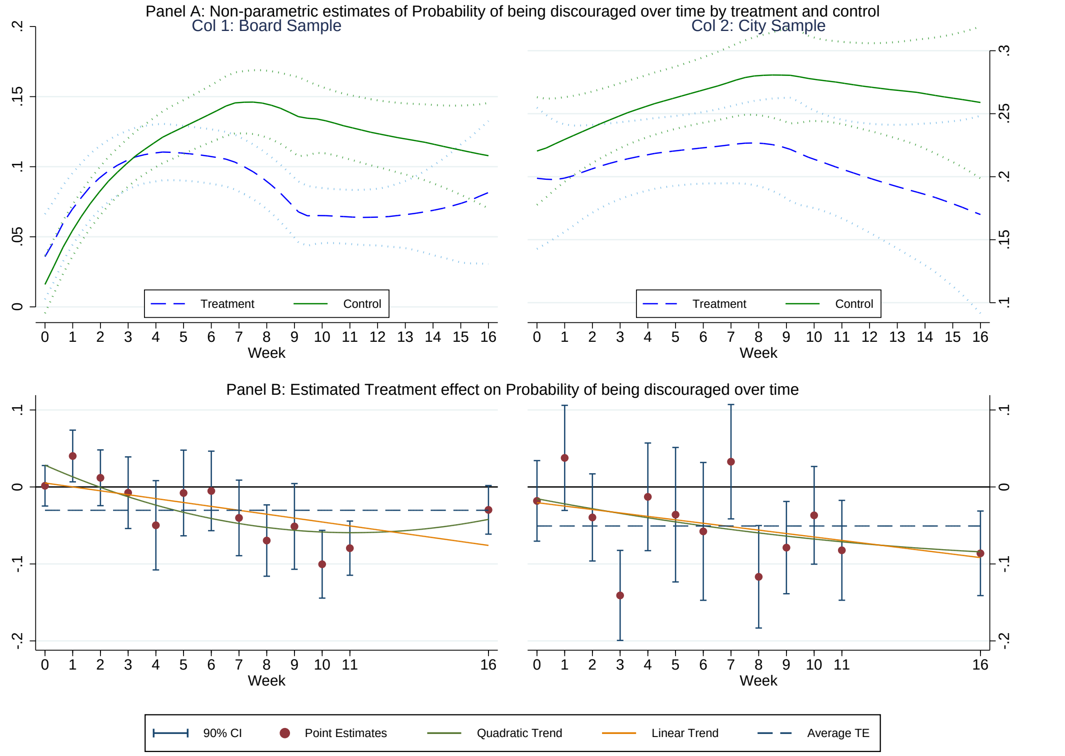


Figure C.10: Impact on having a temporary job and *not* searching for work in the same week: Non-parametric trends & treatment effects over time

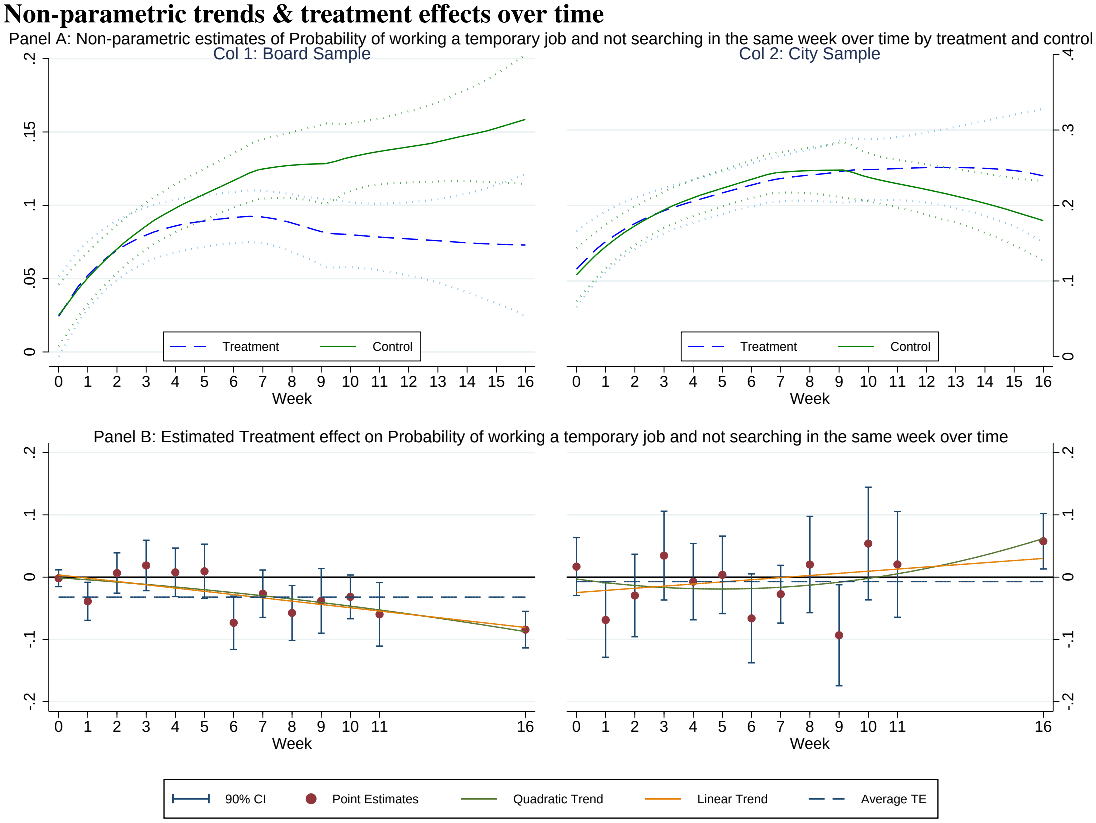
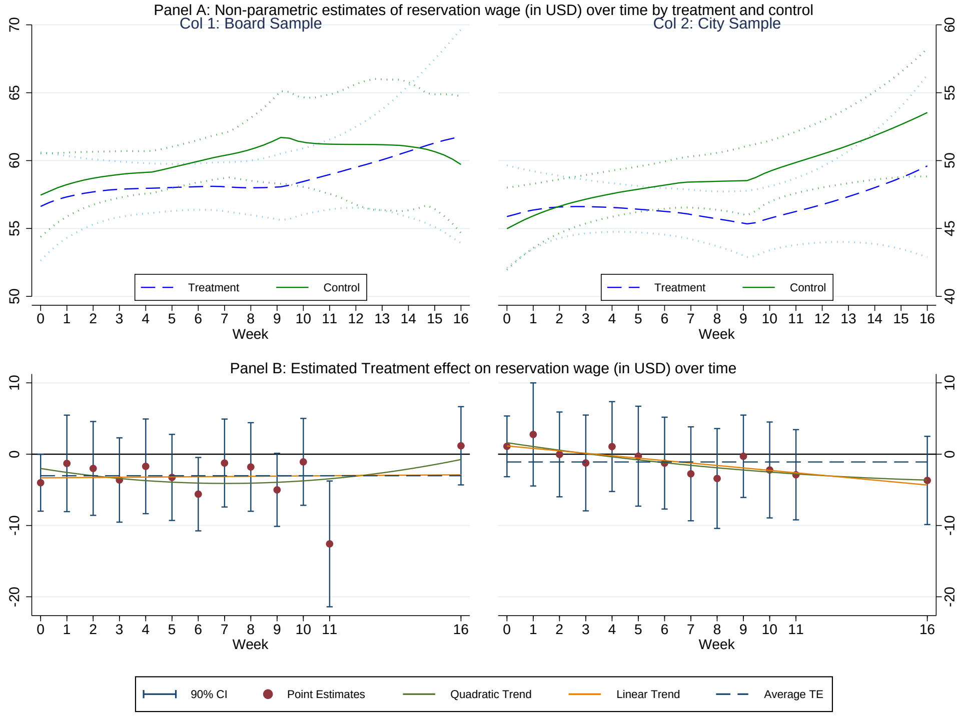


Figure C.11: Impact on reservation wage (in USD): Trends & treatment effects over time



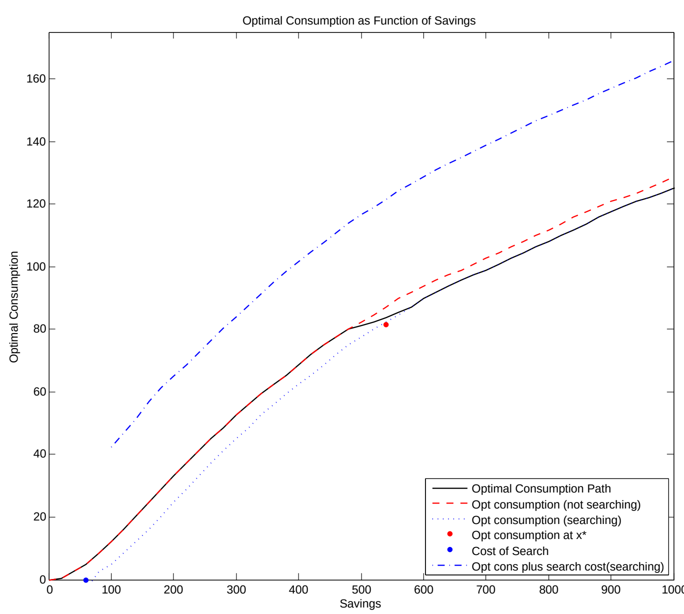


Figure C.12: Optimal consumption paths while searching, and while not searching

Appendix D Additional Tables

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

Panel A: Entire Sample at Baseline

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

Table D.2: Balance Table (cont): Test for balance across samples after attrition

Panel B: Sample resurveyed at Follow Up

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

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

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

Table D.3: Determinants of staying in the survey at first follow up (Week 16)

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

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

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

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

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

Table D.5: Effects of transport subsidies on having permanent employment at endline

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

[1] The dependent variable is a dummy variable equal to one if the individual reported having a permanent job, measured at endline (week 16). Results are from OLS regressions on endline outcomes.
[2] Panel A gives average ITT effect for the two samples together. Panel B shows results two different samples- "board" and "city"
[3] Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table D.6: Intrumental variables (TOT) impacts of treatment

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

[1] All results show instrumental variable (2SLS) estimates where random assignment to the treatment group is used as instrument for the dependent variable. The instrumented variable is labelled in the column header.
[2] Outcome 'Weeks Search' indicates t he number of weeks that a respondent searched for work from the time of the start of the subsidies until the endline survey. *City* respondents searched on average 7 out of the 14 weeks, while the *Board* sample search for 9.
[2] This tables uses treatment status as an instrument for 'takeup' (whether the respondent used the transport subsidies) and 'Centre trips' (number of trips to the city centre where the money could be collected) in Columns 1/3 and Columns 2/4, respectively.
[3] Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level

Table D.7: Effects of transport subsidies on having employment at endline

	Control Mean	(1) BAS	(2) LOG	(3) COV	(4) ANC	(5) BLK	(6) FD
Panel A: Average Treatment Effects At Follow Up (Pooled Sample)							
All	0.53	0.058*	0.059*	0.062*	0.064*	0.057*	0.081*
		(0.034)	(0.035)	(0.035)	(0.034)	(0.034)	(0.043)
Observations	657	657	657	657	657	657	657
R^2		0.003		0.066	0.078	0.159	0.062
Panel B: Treatment Effects At Follow Up by Sample							
Board	0.58	0.044	0.046	0.043	0.046	0.049	0.067
		(0.051)	(0.052)	(0.052)	(0.051)	(0.051)	(0.062)
City	0.46	0.076	0.075*	0.086*	0.088**	0.068	0.099*
		(0.046)	(0.044)	(0.044)	(0.041)	(0.041)	(0.057)
Observations	657	657	657	657	657	657	657
R^2		0.553		0.066	0.079	0.159	0.062

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

Table D.8: Effects of transport subsidies on having a permanent job at both endlines

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

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

Table D.9: Impacts on having employment at both endlines (weeks 16 & 40)

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

[1] The dependent variable is a dummy variable equal to one if the individual reported having work in the last 7 days, measured at endline (week 16). Results are from OLS regressions on endline outcomes.
[2] Panel A gives average ITT effect for the two samples together. Panel B shows results two different samples- "board" and "city"
[3] Standard errors are in parenthesis and are robust to correlation within clusters (Woredas within Addis Ababa) $*$ denotes significance at the 10%, $**$ at the 5% and, $***$ at the 1% level.

Table D.10: Week-specific treatment effects on searching for work

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

[1] The dependent variable is a dummy variable equal to one if the individual reported having a searched for job in the last week (week 16). Results are from OLS regressions on endline outcomes.

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

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

Table D.11: Trends in the treatment effects on searching for work over all weeks

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

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

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

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

Table D.12: Trends in the treatment effects on visiting the vacancy boards over all weeks

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

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

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

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

Table D.13: Trends in the treatment effects on having a job over all weeks

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

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

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

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

Table D.14: Monthly impacts of treatment on main job market outcomes

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

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

Table D.15: Iterative 4 week average treatment effects (one regression per coefficient)

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

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

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

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

Table D.16: Heterogeneous Effects on Job Search Trajectories Outcomes by Respondent Household Wealth (Board Sample)

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

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

Table D.17: Heterogenous effects on endline outcomes by respondent background

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

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

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

Table D.18: Heterogenous effects on endline outcomes by respondent education

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

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

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

Table D.19: 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

Table D.20: Impact of the subsidies on finances and aspirations at endline

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

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

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

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

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