SEARCHING WITH FRIENDS

Stefano Caria*, Simon Franklin†, Marc Witte‡

October 2016

PRELIMINARY – PLEASE DO NOT CITE

Abstract

We study the effects of information sharing about vacancies among young job-seekers. To do this, we track the information-sharing partners of the young job-seekers who take part in an RCT of a transport subsidy program in Addis Ababa, Ethiopia. Treated individuals search for work more often as a result of this intervention. However, we find that their information-sharing partners reduce their job search efforts compared the partners of untreated jobseekers. This is not because they now acquire more information about vacancies from their treated friends. Instead, we document *less* information sharing between treated individuals and their job search partners. We present suggestive evidence that this may be because cooperation in job search becomes harder when one job-seeker has access to more resources than the other.

*University of Oxford
†London School of Economics
‡University of Oxford. Corresponding author: marc.witte@economics.ox.ac.uk

1 Introduction

We study information sharing about job opportunities in the social networks of young jobseekers. The exchange of information in social networks shapes the labour market (Calvo-Armengol and Jackson, 2004), influences the take-up of social programs and products (Marianne Bertrand, 2004, Banerjee et al., 2013, Cai et al., 2015), and fosters the adoption of innovations (Conley and Udry, 2010, Beaman et al., 2015). While much of the literature on the indirect effects of social programs has focused on geographical neighbours (Topa, 2001, Conley and Topa, 2007, Topa et al., 2009), we study indirect effects in a network of close friends spread across a large African city.

We exploit a unique dataset that tracks the close social ties of individuals assigned to the treatment and control groups in a randomized study of a transport subsidy program for young jobseekers in Addis Ababa, Ethiopia. Abebe et al. (2016) report the the main effects of the program. In particular, before the intervention was rolled out, we selected a random sample of "seed" individuals in the treatment and control groups of the original study. We asked the "seeds" to name the people in the city with whom they often exchanged information about jobs. We then tracked and surveyed a random sample of these named contacts, locating people living in all parts of the city. This sample of contacts ("peers") enables us to estimate the indirect effects of the transport subsidy by comparing the outcomes of the peers of treated seeds with those of the peers of untreated seeds.

We find that the peers of transport subsidy recipients have a significantly lower likelihood of searching for employment along multiple dimensions, compared to the peers of untreated recipients. Overall job search goes down, and job search strategies that require transport to the city centre are chosen less frequently. We find no evidence of indirect effects on employment outcomes and some limited evidence of more positive beliefs.

The negative indirect effect on job search behaviour is driven by the sample of job contacts that are actively looking for a job at baseline. We find no evidence that these job contacts are more likely to exchange information about available vacancies with the seeds. To the contrary, seeds participating in the job search assistance program interact less frequently with close friends who searched actively at baseline. This is true for different types of interaction, including the exchange of information about vacancies. We also find a 10 percentage point, yet statistically insignificant, decrease in the probability of employment for close friends who were active searchers at baseline.

Why does information sharing decrease? One possibility is that the job search assistance intervention increases inequality between pairs of friends and that inequality makes it harder to sustain cooperation in job search. In line with this hypothesis, we find that the negative indirect effects are stronger for pairs of friends who have similar levels of expenditure at baseline. Ultimately, we interpret this as evidence that job search takes place as a collabo-

rative activity: close friends help each other find jobs, in particular when they are similar to each other.

We make three key contributions. First, we show that job search of unemployed youths works in groups. Rather than looking for work by themselves, the young people in our urban context collaborate while searching for employment and share information about vacancies with each other. In particular, the bulk of these information is publicly available information posted on vacancy boards on central squares in the city. Job search in such a context differs from receiving private and rivalrous referrals from employed friends, which the literature has mainly focussed on (Bentolila et al., 2010, Burks et al., 2015, Topa, 2011).

Second, we show that interventions can reduce the social capital of untreated individuals who are socially connected to program participants. A recent literature has emphasized the importance of information sharing for the decision to adopt new products (Banerjee et al., 2013, Cai et al., 2015) and the importance of peer support to achieve goals (Breza and Chandrasekhar, 2015). The importance of social networks in the labour market has also been emphasized by both economists and sociologists ((Granovetter, 1973, Ioannides and Datcher Loury, 2004, Beaman and Magruder, 2012). We find that these important social links can be lost when only one individual in a pair of close friends is offered to participate in a social intervention.

A related literature has identified negative psychological consequences for neighbors of individuals who receive cash transfers (Haushofer et al., 2015) and an erosion of social preferences following the introduction of formal insurance (Cecchi et al., 2016). Our results show that the negative effects on social networks are stronger when the ties had similar expenditure levels at baseline, highlighting that the inequality generated by non-universal interventions is a possible mechanism leading to the deterioration of social capital.

Third, we document that close friends are not geographically clustered. We further show that a job search assistance intervention has significant indirect effects on the search behavior of the close friends of program participants. On the other hand, Abebe et al. (2016) do not find evidence of indirect effects among the neighbors of the same program participants. These findings have important implications for how researchers should think about social networks and spillovers in an urban context. Recent studies have focused on social interaction within the confines of urban neighborhoods (such as in recent work of Topa et al. (2009) and Schmutte (2015)). To develop a more comprehensive view of the role that networks play in the economy of cities, this literature should be complemented by new studies that track social connections across space (Kramarz and Skans, 2014).

The remainder of this paper is structured as follows: First, section **??** reviews the related literature. Section 2 presents the randomised intervention, the data sources, and the covariate balance tests. Section 3 lists theoretical explanations for spillover effects of rivalrous goods. In section 4, we present our empirical strategy, and in section 5, the empirical results

are displayed. Section 7 concludes.

2 Context

2.1 The Ethiopian Economy

The randomised experiment whose data this paper uses was set up in Addis Ababa, Ethiopia, in April 2014 and has been completed in September 2015. The choice of Addis Ababa was driven by research considerations: Ethiopia has recently experienced strong economic growth, with an average growth rate of 10% over the last ten years, making the country the second-fastest growing in Sub-Saharan Africa (World Bank, 2014). At the same time, the Ethiopian economy is still characterised by a high rate of youth unemployment, especially in the urban context. Despite a widely reported shortage of qualified employees by Ethiopian firms in the manufacturing and service sector,[1] the unemployment rates are particularly high for young people that graduated from high school and above. This points to a matching problem between the supply and demand side of the urban Ethiopian labour market. A number of studies hypothesises about the causes of this mismatch: Under- or mis-qualification of graduates, information asymmetries (such as grade inflation or insufficient degree documentation), or spatial barriers to employment (such as costs of public transport).[2]

This paper measures the potential impact of the latter: By using data from a randomised job search assistance experiment that handed out transportation subsidies to a sample of approximately 800 young unemployed from suburban Addis Ababa, we look at spillover effects of this subsidy on a range of outcomes of a sample of randomly picked close friends ("peers") of the main intervention sample. These outcomes cover social interactions between peers and seeds, job search and employment outcomes as well as the peers' aspirations and attitudes. We also report the results for a range of secondary outcomes (educational, financial, job application outcomes) in section 5.

2.2 The Experimental Context

This paper estimates the peer effects of a randomised transport subsidy, which was one intervention of the "Youth Job Search Assistance" randomised control trial (RCT) conducted in 2014 and 2015 by the Centre for the Study of African Economies (CSAE) at the University of Oxford and the Ethiopian Development Research Institute (EDRI). In the context of this RCT, 4400 young unemployed from suburban Addis Ababa, aged 18 to 29, were interviewed during a baseline survey in April 2014 and subsequently were randomly allocated to three

[1] This became evident during qualitative interviews with Ethiopian firm managers in the planning stage of the RCT.

[2] A summary of these different points can be found in (World Bank, 2007).

different intervention groups and one control group. The three different interventions were 1) a job application and CV training workshop in collaboration with the local university, 2) job fairs where randomly chosen participants met with a sample of medium to large firms, and 3) the above-mentioned transport subsidy. This paper solely focusses on the spillover and network effects of the transport subsidy, while the overall results for the RCT can be found in Abebe et al. (2016).

2.2.1 The Transport Subsidy

One main motivation behind the job search assistance through a transport subsidy for young suburban unemployed is the fact that information on job opportunities and vacancies, both in the formal and informal sector, is surprisingly centralised in Addis Ababa. Most jobs are promoted on specific "job boards" located at various central locations in the city centre, where both public and private employers can announce vacancies (Franklin, 2014). This leads to a situation where unemployed individuals can increase their chances of finding a suitable job by frequently travelling to the city centre. For the intervention sample of unemployed youths living several kilometres outside the city centre, this entails significant transportation costs which often deter unemployed from targeted job search altogether. In a first pilot of the transport subsidy experiment, Franklin (2014) found positive and significant short-run employment impacts for the recipients after a very similar transport subsidy intervention.

For the transport intervention approximately 800 individuals were randomly picked to receive a monetary subsidy supposed to cover transport expenditures between the participant's house and the city centre. However, the subsidy was unconditional in the sense that it was provided in cash and it was not followed whether participants actually spend the money for transportation expenses. Since participants could collect the subsidy up to three times per week from an office located in the city centre of Addis Ababa and eligible participants are living at a non-negligible distance from the centre (at least 2 kilometres), it is very likely that participants incurred some expenditure to claim the subsidy. The "per pick-up" amount of the subsidy was calculated based on the public transportation costs of exactly the distance between the participant's residence and the city centre and hence varied by participant, ranging from 15 to 30 Ethiopian Birr (ETB) with a median of 20 ETB, which is roughly equivalent to the range from 50p to 1 GBP and a median of 70p. The duration of the subsidy was randomised over batches of participants (between 13 and 21 weeks). The intervention started in late September 2014, with participants phasing in on a weekly basis and the last ones beginning their intervention in mid-October 2014. The intervention ran out in February 2015 for the latest batch of participants.

The impact of the transport subsidy intervention on the recipients' outcomes is reported in Abebe et al. (2016). The authors find no impact on overall employment, the hours worked,

permanent work, monthly earnings, self-employment , or work satisfaction, all measured during a follow-up survey in August 2015. However, there is a 5.5 percentage point increase in formal work for the subsidy recipients (at a 5% level). This effect holds for both males and females if the results are split up by gender. Additionally, there is a 6.6 percentage point increase in permanent employment for female beneficiaries (at a 5% level). There are no effects on job application behaviour. However, the authors find relatively strong effects of the transport subsidy on job search, which is measured every fortnight: Job search at the vacancy boards increases significantly during the first eight fortnights after treatment, by approximately 26 percentage points (which corresponds to a five-fold increase over the control group mean). Overall job search does not increase quite as much, but still goes up by 5 percentage points or 12.5% during the first four months after the onset of the intervention (both at 5% level).

2.2.2 The Network Survey

Even though the detailed sources of data and survey instruments we used will be presented in section 2.2.3 below, this paragraph is supposed to give a brief overview of the way the data on job contact networks was elicited. In addition to the individual baseline survey (July 2014) of the 800 transport intervention participants and 800 control group individuals, we conducted a network survey of randomly selected job contacts of these transport subsidy treatment and control participants (called "seed" individuals for this purpose): 165 transport subsidy recipients and control group seeds from 59 different suburban neighbourhoods were randomly selected and asked about their "city-wide job network connections". In particular, these job contacts could reside anywhere in Addis Ababa (hence city-wide) and were defined as individuals with whom the seeds regularly (at least once per 30 days) held conversations about jobs.[3] This led to a sample of approximately 1000 listed city-wide job contacts whose place of residence was scattered all over the city of Addis Ababa. Of this sample of 1000 "close ties" or "peers",[4] a random subsample of approximately 600 was selected for interviews as part of the network survey. These 600 city-wide job contacts form our main network sample, for which we evaluate spillover effects on a range of different outcome variables, such as employment indicators, job search behaviour, personal aspirations, and attitudes.

It is not *a priori* obvious why we can expect spillover effects from an intervention whose goal it is to increase employment by providing participants with a subsidy to overcome spatial

[3] These individuals did not have to satisfy the eligibility criteria of the main intervention sample, namely being between 18 and 29 years old and unemployed, even though a large proportion of the network sample is (as will be seen in the covariates in section 2.3). We stratified the randomised selection of these contacts by the main intervention eligibility criteria.

[4] We will use the expressions "job contacts", "close ties", "friends" or "peers" interchangeably throughout this paper.

hurdles between their residence and the location of information on vacancies. In particular, when looking at outcome variables that are rivalrous goods for each participant (e.g. a suitable vacancy, information about a vacancy, a firm's contact data), it is not necessarily evident why a (myopic) individual should share these data with one or several job contacts. However, when taking elements of homophily, reputation, or reciprocity into the equation and keeping in mind that the spatial externalities prevent job information to flow freely, it is thinkable that rational individuals face incentives to share these information within their network. A detailed discussion of these mechanism will follow in chapter 3.

2.2.3 Data sources

The different analyses we run in this paper rely on a range of different data sources and questionnaires. In summary, the data sources used for estimating the spillover effects of the transport subsidy and for analysing the job contact networks are the following:

1. **Main baseline survey of the RCT** ($N \simeq$ 1,800), April-July 2014
    - $\sim$ 800 individuals = transport subsidy recipients; $\sim$ 1000 = control group
    - of which we randomly picked 165 "seed" individuals
2. **Network survey (baseline) of city-wide peers** ($N = 596$), September 2014
3. **6 month follow-up of city-wide peers** ($N = 540$, 9% attrition), March/April 2015
4. **Transport subsidy intervention data** (October 2014-January 2015)
    - amount per day, pick-up days and time, for all treatment group seed individuals

2.2.4 City-wide social networks

The network roster which was used to ask the 165 seed individuals (transport subsidy recipients and control group participants) about their city-wide job contacts does not present respondents a limited list of choices and is not restricted to any eligibilty criteria apart from being a resident of Addis Ababa. The survey instrument reads as follows:

> "With whom in Addis Ababa do you regularly share information about job opportunities? List all people and fill in information about that person X."

Further items of this listing elicit more information on the characteristics of the network (relation of links, questions on borrowing and lending, travel to city-centre etc.). The approximately 1,000 "close ties" listed in this way did *not* have to satisfy any of the eligibility

criteria of the main intervention sample from which the seeds were drawn, namely being between 18 and 29 years old, with at least a high-school degree and without permanent employment. Of those 1,000 listed "close ties", a random subsample of approximately 600 was interviewed with the network baseline questionnaire. This network baseline survey contains *inter alia* sections on socioeconomic characteristics, labour market and job search data, behavioural and aspirational data, as well as time use and expenditure data. When looking at the final sample of 596 city-wide job contacts, it is interesting to see that more than half of them do satisfy the eligibility criteria of the main baseline sample even without imposing any sampling criteria.

Let us define this set of city-wide job contacts as follows:

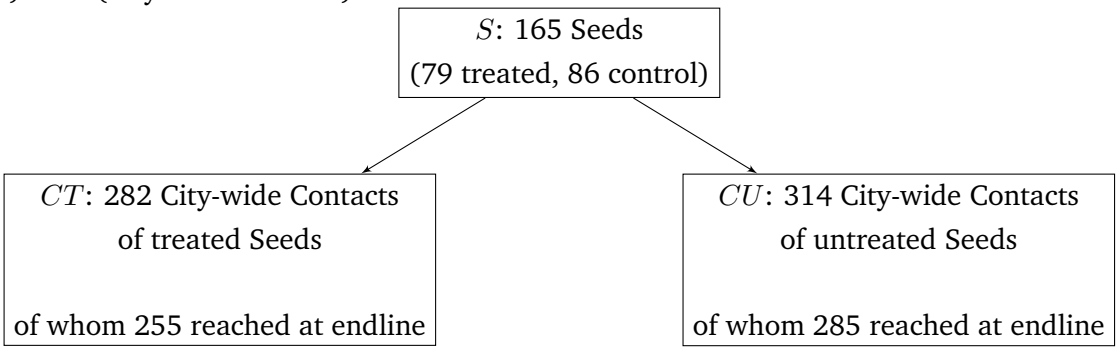
$$C = \{\text{City-wide contacts}\}$$

Similarly, the set of seed individuals from the main baseline survey is defined as:

$$S = \{\text{Seed individuals}\}$$

2.2.5 Summary

All individuals that are part of subsequent analyses are displayed in the following diagram. All individuals are part of one (and one only) of the two sets of individuals, S (Seed individuals) or C (City-wide contacts).



As mentioned above, more than half of the city-wide contacts meet the eligibility criteria (age 18-29, at least high school education, no permanent employment) of the main baseline survey (see section 2.3).

2.3 The networks, summary statistics and balancing tests

In table 1, it is evident that despite the fact that the transportation subsidy was randomised over the 165 seed individuals, some few statistically significant baseline differences between seed individuals receiving the transport subsidy (**T**reatment) and those that do not (**C**ontrol) remain. The most striking of these differences is the share of females in the treatment group,

which is significantly higher than in the control group. It should be noted, however, that the sample of 165 seed individuals we are using in this paper is only a subsample of all project participants that were randomly allocated to treatment or control groups, so these mean differences might vanish for the whole sample of approximately 1800 intervention participants and non-participants.[5] For some of the differences that are on the verge of significance, for example some of the ethnicity indicators, cases where the indicator variable takes the value of one become too few to present meaningful comparisons.[6] When looking at spillover effects of the transport intervention on to the network job contacts of the seeds, we do not evaluate the seeds' outcomes themselves. Therefore, non-balanced baseline characteristics for seed individuals are not as worrisome, but can be controlled for in chapter 5.

Table 2 shows the summary statistics and covariate balance for the city-wide network sample ("peers") by the treatment status of their seeds. This full sample of these city-wide network contacts is not restricted to meet the eligibility criteria of the seeds. Again, since the seeds were allocated to treatment and control group randomly, and the peers again chosen randomly from a random sample of seed individuals, one would expect to see covariate balance throughout. For most covariates, this baseline balance holds well. The only exceptions on a 5% or stronger level are found for the covariates whether the individual was born outside of Addis Ababa, had permanent work in the past seven days (at baseline), and (at 10% level) had searched the vacancy boards during the month before the baseline. We will control for all unbalanced (on a 10 % or stronger level) baseline covariates in our analysis.

Table 3 finally shows the summary statistics and covariate balance for the city-wide peers of treated and untreated seeds, collapsed by seed. This means that if a seed individual has several city-wide contacts, the table displays the mean of the characteristics of these contacts by seed. Again, some of the significant differences of tables 1 and 2 persist.

2.4 Network Maps

Figure 1 shows the distribution of treated and untreated seeds on a map of Addis Ababa, while figure 2 adds to this the peers of treated and untreated seeds.[7] It becomes evident that the seed individuals, both treated and untreated, are clustered in certain areas of the map, which correspond to the enumeration areas that were used for the random allocation of the transport subsidy intervention. Moreover, there are no seed individuals who live in the centre of Addis Ababa, located in the northwestern part of the map, as this was one of the

[5] Looking at the direct intervention impacts on outcomes of primary recipients of the intervention is not part of this paper. We focus on spillover effects alone.

[6] For instance, in the case of ethnicity 3 (*Tigre*), we are comparing seven *Tigre* individuals in the treatment group to 2 *Tigre* individuals in the control group

[7] This map also displays administrative boundaries for the city's administrative districts that do not adequately represent the extent of urban sprawl in Addis Ababa, which is why some dots lie outside the outer black lines

seeds' original eligibility criteria. The job contacts of the seeds, however, are distributed over the whole range of the map, with no particular clustering pattern showing up and with some peers living very close to the city centre.

Figure 3 displays a kernel density estimate of the distance between seeds and peer. It shows that the mode of the distance between the two job contacts is approximately 2 km, with a mean of 5.75 km and a median of 1.82 km. This means that more than 50% of pairs live more than 25 minutes of walking distance apart, based on normal walking times and calculated "as the crow flies". In the reality of a densely populated urban agglomeration in Sub-Saharan Africa, which very rarely has a street connecting two locations in a straight line, the walking times between the two individual places of residence should be much higher over the whole distribution.

In a next step, table 4 displays a range of interaction variables between the job contacts at baseline by treatment status of the seed. First of all, the variables are all balanced at 10% level, further indicating that the randomisation was successful. Moreover, we can see that the interaction between the peers and seeds is quite intense: On average in the past month (before baseline), they spoke about 12-13 times and half of the sample travelled to the city centre together. In addition to that, more than 80% of peers ever received information about jobs from their seeds, and in return more than 95% of seeds received job information from their peers. Cleary these are very relevant networks for the exchange of job information, which is partly a consequence of the way these job contacts were sampled. However, the symmetry of the information exchange is equally striking, with 80% of all pairings exchanging information in both ways. In terms of the geographic distribution of the job contacts, we find that seeds on average live about 8km from the city centre of Addis Ababa, and that the mean distance between the two contacts is 5.75k (as elaborated before), with both variables being balanced across treatment status. Interestingly, if we define local neighbourhoods according to Ethiopia's Central Statistical Agency's (CSA) enumeration areas, we find that only 5% of the sample of peers and seeds live within the same neighbourhood.

The fact that some of the city-wide contacts C live in the city centre of Addis Ababa might have some implications for the expected spillover impact of the seeds' potential transport subsidy on them. Since the transport subsidy was mainly designed to enable recipients to travel to the city centre more frequently, where *inter alia* information on job opportunities are relatively freely available (vacancy boards), spillover effects at least on aspects directly linked to vacancy boards could be expected to a lesser extent. However, there are other ways through which the subsidy might impact on the seeds behaviour: E.g. the sheer fact that the seed receives the subsidy induces her to travel to the centre regularly and use the time spent in the centre for job search, whereas a peer living in the centre permanently has no additional incentive to spend more time on job search. If now the seed gathers relevant and shareable information, spillover effects on the peer living in the centre are thinkable. In any

case, we will run regressions on the subsample of peers living outside the city centre in the mechanism section of the analysis.

3 Theoretical background

Many of the outcome variables of the spillover analysis can be classified as rivalrous goods. This is certainly the case for three broad categories of employment-related "spillover goods": Referrals to actual jobs, information about jobs, and access to information about jobs (e.g. via a contact person within a firm). Other variables, however, do not impose a direct rivalry between the seed and the peer: E.g. the contact's job search behavior and intensity, the contact's expectations about jobs available and aspiration outcomes, the contact's job application behaviour etc. do not stand in direct competition to the seed's behaviour in these areas. This makes intervention spillovers on these variables more likely. Before thinking about which behavioural traits might induce a seed not to prevent any sort of spillover of rivalrous goods to her city-wide contacts, it is important to notice that the seed's reluctance (or willingness) to share - say - information of available jobs will depend on both the seed's own employment status (after the intervention) and the extent of potential competition for jobs between the seed and her peer (e.g. based on their similarity in endowments, qualifications etc.). For example, an employed seed should *ceteris paribus* be more likely to share information about job opportunities with her peer, and a seed with an engineering degree should *ceteris paribus* be more willing to pass on information about vacancies for secretarial posts to her job contacts.

However, even without thinking about structural socioeconomic differences between seed and contact that might make spillover effects on a range of job-related outcome variables more likely, there are demographic and economic reasons going beyond a purely rational (or myopic) approach which suggest positive peer effects. Homophily, as developed originally by Lazarsfeld et al. (1954), describes the habit of individuals to associate with others that are similar to them. In the context of the transport subsidy intervention, one could imagine seeds who receive the treatment being more likely to pass on certain beneficial aspects of that treatment to individuals that are closely comparable on a range of certain indicators. More generally speaking, the spillover effects of the job search assistance could be quite different for similar or dissimilar job search pairs; a hypothesis that we test in section 6.

In addition to that, the seed's reputation in her city-wide network of job contacts is another reason why potentially rivalrous goods could spill over on to the peers. The job contacts are defined as individuals to which the seed is speaking to about jobs at least once in 30 days. During the course of the transport intervention, which on average lasts about four months, a treatment group seed can be expected to talk to the peers a minimum of four times. As a consequence, one would expect these job contacts to be aware of the seed receiving the transport subsidy and to develop expectations about the subsidy's impact on the seed. Sim-

ilarly, it is imaginable that a situation where job contacts are aware of the seed's treatment status creates an implicit obligation for the seed to share at least some of the benefits of the intervention's benefits. Since the actual subsidy payments are calculated such that they do not vastly exceed the actual costs of travel from the seed's home to the city centre, this sharing of benefits can be assumed not to take place as an exchange of money, but rather of information or network access. A seed not complying with this benefit sharing rule might lose her reputation in her network and consequently be cut off from future exchanges in the job network that are beneficial for her.

This last aspect relates to the third reason of why peer effects of the transport intervention can be expected: In a job network where beneficial exchanges are multi-directional and happen over a longer horizon, a seed might pass on rivalrous information to the city-wide peer in the context of reciprocity. In order not to be excluded from future exchanges in the job network (or in order to increase her chances of receiving purely rivalrous goods through the network, e.g. a job), the seed will pass on rivalrous goods through the network. This is also in line with evidence from Kremer and Miguel (2007), who find that for goods with a low private, but high public value, diffusion through networks does barely take place. Job-related information usually have a high private, but low public value, which suggest we should expect higher network diffusion.[8]

4 Empirical Strategy

Estimating the spillover effects

This paper estimates the spillover effects of the transport subsidy intervention on a range of outcomes of the seeds' city-wide contacts (C). While many of these outcome variables are job-search or employment-related, such as job search behaviour and intensity or different employment indicators, types and terms of employment, other outcomes are indicators of the contact's aspirations, reservation wage, beliefs and attributes. This list is not exhaustive and will be extended, e.g. by looking at the spillover effects of the subsidy on network-related outcomes of the peer-seed pairs, such as forms and intensity of information exchange or frequency of interactions.

In general, there are two principle empirical approaches for estimating the effects of a randomised intervention on an outcome variable (McKenzie, 2012): The difference-in-differences estimator (DiD) or an analysis of covariances (ANCOVA).[9] The main advantage of the DiD estimator over a simple ex-post mean comparison is the substantial gain in power.

[8] It is possible to present the arguments of this section in a stylised risk-sharing type of model à la Angelucci and De Giorgi (2009), where individuals share rivalrous goods through their network as a form of future insurance against long-run unemployment. For now, this will be left for later versions of the paper.

[9] Either estimator has its relative merits and drawbacks, which is explained in detail in McKenzie (2012).

The DiD estimator uses data for (at least) two time periods, one before and one after the intervention. Since the endline questionnaire of the network survey is similar to the baseline questionnaire and contains the exact same variable definitions almost throughout, we can use the DiD estimator to estimate the spillover effects of the intervention, expressed as follows:

$$y_{i,t} = \beta_0 + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 (Treat_i * Post_t) + \beta_4 X_{i,t} + \varepsilon_{i,t} \qquad \text{for} \qquad i \in C, \quad (1.1)$$

where

$$Treat_i = \begin{cases} 1 & \text{if } s(i) \text{ received transport subsidy} \\ 0 & \text{if } s(i) \text{ did not receive transport subsidy} \end{cases}$$

and

$$Post_t = \begin{cases} 1 & \text{if an observation is from the follow-up survey } (t = 1) \\ 0 & \text{if an observation is from the baseline survey } (t = 0). \end{cases}$$

y_{it} is the outcome of interest of contact i at time t, which denotes the two time periods baseline ($t = 0$) and follow-up survey ($t = 1$), $Treat_i * Post_t$ is the interaction term of the treatment indicator and the post-intervention period, and X_{it} is a vector of control variables. The coefficient of interest is β_3 on the interaction term, displaying the effect of a seed's treatment on the post-intervention outcomes of the city-wide job contacts. Including the vector of covariates X_{it} into the regression does not only control for potentially unbalanced baseline outcomes, but can also help increasing power by explaining more of the variation in the outcome variables. It is important to note, however, that β_4, the coefficient on X_{it}, is by design restricted to be the same in the two periods pre- and post-intervention. Plausibly, this cannot be justified for all potential covariates. Only exogenous time-invariant characteristics (e.g. gender) or exogenous covariates that can be assumed to have a time-invariant, constant impact (e.g. marital status) should be included here, if required.[10]

Considerable gains in power can be achieved by using analysis of covariance (ANCOVA) instead of the DiD or simple post mean comparisons. The basic idea is that one can condition on a variable that is correlated with the outcome of interest and hence reduce the variance of the treatment estimator. In the ANCOVA case for randomised control trials, conditioning takes place on pre-intervention values of the outcome variable (see equation 1.2).

The expression for our ANCOVA estimator run on the follow-up period $t = 1$ is:

$$y_{i,t=1} = \beta_0 + \beta_1 \bar{y}_{i,t=0} + \beta_2 Treat_{i,t=1} + \beta_3 X_{i,t=0} + \varepsilon_{i,t} \qquad \text{for} \qquad i \in C, \qquad (1.2)$$

where

$$Treat_{i,t=1} = \begin{cases} 1 & \text{if } s(i) \text{ received transport subsidy} \\ 0 & \text{if } s(i) \text{ did not receive transport subsidy} \end{cases}$$

[10] The ANCOVA estimator will not have similar restrictions as it controls for baseline outcomes only.

Again, y_{it} is the outcome of interest of person i at round t (pre- or post-intervention), $\bar{y}_{i,t=0}$ refers to mean of the outcome variable for individual i in the pre-treatment baseline survey, and $X_{i,t=0}$ is a vector of pre-treatment baseline controls. Including the vector of pre-treatment baseline covariates can control for these variables in case of baseline imbalances among them, and/or increase power of the estimate by reducing the variance of the error term $\varepsilon_{i,t}$.

Frison and Pocock (1992) show that the ANCOVA estimator is weakly more efficient than the DiD estimator of equation 1.1. In the present case of a single baseline and follow-up survey, the ratio of the DiD variance to the ANCOVA variance is $2/(1+\rho)$ (McKenzie, 2012). As an example, if the autocorrelation ρ between the outcome variables was zero, effectively making the outcome a mean plus noise, a DiD estimation requires twice the sample size as ANCOVA to obtain the same power. As mentioned before, we will run equations 1.1 and 1.2 for a range of different outcome variables which in theory might have very different autocorrelations. However, since in an experimental setting like the present, for any value of ρ ANCOVA is more efficient (and hence has a higher power), we will use the ANCOVA estimator only.[11]

The unit of randomisation of the experiment and our analysis is the seed individual (strictly speaking, the seeds are a random subsample of the transport subsidy recipients and control group participants, who in turn received treatment randomly). Hence, we cluster the standard errors of all regressions presented here on the seed level.[12]

5 Results

In this section, we will discuss the spillover effects of the job search assistance program. We will present our results on separate groups of different outcome variables: First, we look at search-related outcomes, such as the search behaviour in the past week and month, or the different search strategies chosen (e.g. search at vacancy boards, search at work sites etc.). Second, we present the results for a range of outcomes that are supposed to capture expectations about the local job market, such as the number of job offers expected as well as the reservation and aspiration wage. Next, we look at a range of psycho-social outcomes of the peers that capture different dimensions of individual aspirations and beliefs. Afterwards, we look at whether the social interactions between seeds and peers have changed due to the transport subsidy treatment. And finally, we report effects on a range of different secondary

[11] When running our analysis with DiD regression 1.1 (results not reported in this draft), we reassuringly obtain very similar results to our ANCOVA estimator.

[12] Alternatively, it is also justifiable to cluster standard errors by "cluster", i.e. enumeration area, or to cluster two-way by enumeration area and seed individual. Our approach of clustering by seed individual is the most conservative strategy.

outcomes, such as employment outcomes, education outcomes, or financial outcomes.

Additionally, this section will mostly display our preferred regression specification, which is the regression on the full sample of non-attrited peers, controlling for the significantly different baseline covariates.

5.1 Job-search-related outcomes

Table 5 displays the results of regression 1.2 on recent job search behaviour, i.e. in the past seven days prior to the endline.[13] The interaction coefficient displays the treatment effect of being a contact of a treated seed. As table 5 shows, recent job search and recent vacancy board search have both decreased by 7 percentage points, respectively. This is a decrease equivalent to 41% or 21%, respectively, compared to the endline search levels of the peer control group.[14] We can compare these decreases to the results of the transport subsidy intervention on the main beneficiaries from whom the seeds were randomly sampled. During the four months after receiving the subsidy for the first time, the beneficiaries increased their job board search five-fold (by 26 pp.) and their overall job search by 12.5% (or 5 pp.; all previously mentioned in section 2 and taken from Abebe et al. (2016)). The discrepancy between the positive first-order results on job search and the negative spillover effects is stark; these spillover effects are actually very large decreases.

Continuing with table 5, the recent job search within the peers' social networks decreases by 3 percentage points, however on a statistically insignificant level. This is very interesting in so far as the randomly distributed transport subsidy allowed the seeds to travel to the city centre of Addis Ababa for free, where all of the job vacancy boards are located. Even though the subsidy was never framed as enabling vacancy board search for the seeds, the analysis of the main intervention suggests that treated participants were approximately 26 percentage points more likely to search at the job search boards within four months after the beginning of the job search assistance (cf.Abebe et al. (2016)). One interpretation of the significant negative spillover effects on job board search (but not on social network job search) of the peers is that treated seeds might have shared the information on the vacancy boards with their peers, leading to a decrease in the peers' own board search intensity (since she already received all necessary information through the seed). Other mechanisms are possible, too, e.g. the treated seed might have found the information on the vacancy boards quite useless and passed on this knowledge to the peers - a hypothesis we will test later in this section.

In a further attempt to describe the impact of the transport subsidy on the peers' job search behaviour, we look at different search strategies chosen by the peers in the past month (before the endline survey), i.e. over a longer time horizon. Table 6 displays the results for a

[13] As mentioned before, we obtain very similar results in terms of coefficient sign, size and significance when running the DiD specification instead of the ANCOVA specification.

[14] 17% / 34% of the peers of untreated seeds search the boards/ search overall at endline

range of search strategies. Again, we can observe a strong shift away from vacancy board job search in the past month that is even larger in economic and statistical significance than for the seven-day period. Besides, the search at work agencies and at central locations (such as central squares in the city) goes significantly down by approximately 2-4 percentage points, further supporting the idea that peers of treated seeds were taking advantage of their seeds' free transportation to the city centre. Search strategies that *do not* depend on commuting to the centre of Addis Ababa are not affected by the transport subsidy: Neither job search in the social networks, nor at work sites (such as construction sites that can be found everywhere in the city, not just in the centre), or searching the internet for jobs decrease significantly. Overall, the shift in search strategies lines up with the initial idea that job search activities can be substituted between the peers and the (treated) seeds.

5.2 Job market expectations

One channel through which the spillover effects on job search can be explained is discouragement of peers by the information they receive from their treated seeds. For instance, the seeds receiving the transport subsidy, and consequently increasing their vacancy board search by 25 pp. as well as their overall job search by 12.5% (cf. Abebe et al. (2016)), could share sobering or even discouraging information about the Addis Ababa job market with their peers, who in turn downwards-adjust their search behaviour. We can test this idea by looking at a range of work-related expectations and aspirations and testing whether these differ between peers of treated and untreated seeds.

Table 7 shows that there is no significant treatment effect on the job offers expected in the next four months, the reservation wage, the aspired wage in five years, and the overall life satisfaction for peers of treated seeds. These results strongly suggest that there is no change in job market expectations for the peers of treated seeds; most of the variation is explained by pre-intervention baseline levels of the dependent variables.

5.3 Psycho-social outcomes: Attitudes and beliefs

We also collect detailed data on the respondents' beliefs and attitudes about life. Looking at a range of these variables, one can further test whether the peers of treated seeds change their outlook on life as a consequence of the information received from their job contacts. In table 8, we can see that in general the intervention led to more positive beliefs about the control over their own life of treated peers: All variables intended to capture the locus of control of the respondents change to what corresponds to having more control.[15] We interpret this as

[15]We are currently working on creating more sophsticated aspiration indices that are in line with the psychological literature.

further evidence that the peers were neither discouraged by the fact that their seeds received the transport subsidy nor by the information they received from them.

5.4 Social interactions between seeds and peers

We next look at whether the social interaction indicators between peers and seeds that were strongly balanced at baseline change as a consequence of the transport subsidy intervention. Ex ante, it is likely that some activities should decrease after the intervention, such as common travel to the city centre, shared travel expenses (since the treated seed now receives a subsidy), whereas other indicators could be expected to increase (e.g. shared information about jobs). Surprisingly, table 9 only shows very limited social interaction treatment effects: There is weak evidence pointing towards less common travel to the city centre and a strong negative effect on shared travel expenses. However, there are no changes in the exchange of information either from seed to peer or vice versa, which is surprising given the previously described strong changes in job search behaviour of the peers of treated seeds. We suspect that this is due to only a part of the sample of (treated) seeds sharing job information with their peers and thus carrying most of the job search effect, and we will look into this hypothesis in more detail in section 6.

5.5 Further outcomes

To wrap up this section on our main results, we report the findings for the following five group of outcome variables in tables 10-14: employment and employment quality outcomes, education outcomes, job application outcomes, financial outcomes, and mobility outcomes.

While we do not find any statistically significant results on employment and employment quality outcomes in table 10, the increases in wage employment (over six months), work satisfaction, and office work (in last seven days) of 4, 5 and 4 percentage points, respectively, are economically meaningful and make displacement effects of the peers through treated seeds very unlikely. Similarly, we find no spillover effects on education in table 11.

Table 12 shows that peers of treated seeds did not change their job application behaviour (type of job applied to, application strategies) compared to peers of untreated seeds, as the coefficients are insignificant statistically and economically, and most of the variation in the dependent variable can be explained by pre-intervention application behaviour.

When looking at financial outcomes in table 12, unsurprisingly no significant treatment effects emerge - as these effects would have been assumed to be subordinate to employment effects anyways. Finally, table 14 displays the effects on mobility outcomes. Here, we find a significant decrease (of 20% compared to the mean at baseline) in the number of trips to the city centre that a peer of a treated seed has made in the past seven days. This is in line with our main findings on job search behaviour: The transport subsidy has made the trips for the

friends of subsidy recipients less necessary, which is reflected in a negative spillover on job search behaviour in the centre as well as the travel to the centre.

6 Mechanisms

Our results from the previous section show that as a consequence of the transport subsidy treatment for the seed individuals, their job-contact peers reduce their overall job search effort and shirt their job search activities from commuting-based job search towards different, more stationary activities. One simple explanation of this decrease in job search activities of peers could depend on the seeds' employment status after the intervention. For example, if the subsidy recipients were *less likely* to be employed after the intervention, they could as a consequence be less likely to pass on information about vacancies at their place of work to their peers. However, this mechanism is unlikely to be behind our results, since the seeds were sampled not to be in permanent employment before the start of the intervention. More rigorously, we can rule out any explanation of our spillover results involving a reduction in seed employment, since (as described in section 2), Abebe et al. (2016) do not find a negative impact of the transport subsidy on any employment outcomes. The only significant increase in any of the employment outcomes is formal employment by 5.5 pp. If anything, a (small) positive change in formal seed employment after the intervention should increase the job information flow to the peers and not reduce it.

6.1 Active baseline job-seekers

Another explanation of our negative spillover results involves the composition of the peer sample in our study. As opposed to the seed individuals, the peers were simply surveyed as "job contacts" of the seeds and did not have to be actively looking for a job at baseline. It is possible to think of scenario where only the peers who (similar to their seeds) are active job seekers are affected by the fact that some of their job contacts start receiving job search assistance, while the sample of non-searching peers does not change their behaviour to such an extent. Evidence along these lines would point towards a situation where job search collaborations between seeds and peers at baseline get altered through the subsidy intervention. In order to look into this hypothesis in more detail, we split our spillover sample into those peers who already use the job vacancy boards at baseline and those who don't.[16]

[16]To be precise, we split the sample into those individuals who did not visit the vacancy boards once in the week before the baseline survey and those who visit the boards at least once. Importantly, we can also split the sample based on (overall) job search in the week before the baseline survey and find very similar results that support the same interpretations. The important differentiation is between peers who are active job-seekers at baseline and those who are not.

We begin by looking at our main results on job search, shown in table 15. The uneven columns (1), (3) and (5) show the result for those peers that do not use the job vacancy boards at baseline, whereas the even columns (2), (4) and (6) show the results for baseline vacancy board users. It becomes apparent that the negative spillover results of the transport subsidy on the job search of social contacts of the subsidy recipients are almost entirely driven by those peers who do travel to the vacancy boards before the intervention. The coefficients on overall job search, board search, and even social network job search are large and - at least in the first two cases - statistically significantly negative. For people who searched for a job intensively at baseline, in particular at the job board, the fact that someone in their social (and job search) network started receiving a subsidy had a negative impact on their job search.
We continue by looking at the different search mechanisms in table 16 and we can again find a stronger decrease in job board and city centre search for the group of active baseline searchers. The difference in coefficients on search at work sites and agencies follows this pattern, but we fail to reject that they are equal to zero.

The stronger decrease in job search for the sample of active baseline searchers could be explained by several hypotheses, all of which have information exchanges between seed and peer at the core: The patterns seem suggestive of either the substitutability of job search activities away from the peer (who still actively looked for a job at baseline) to the seed, whose job search is now subsidised. Apart from that, the discouragement of the job searching peers by the information received from the subsidised peers seems less likely, since the negative spillovers are born by those peers who already use the job boards at baseline, hence already have access to the vacancies posted there and presumably a pretty got impression of the city's job market.We can test these two different mechanisms in more detail, first by looking at the communication between peers and seeds, split up again by peer baseline search status.

By comparing the uneven (peers not searching at baseline) with the even (peers searching at baseline) columns, we can see in table 17 that somewhat surprisingly, the interaction between treated seeds and peers that are baseline searchers decreases substantially in almost all dimensions: The peers spent less time with their seeds (-16 pp.), travel to the centre less often (-20 pp.), share travel expenses less often (–19 pp.), give less information about jobs to the seed (-21 pp.), and similarly *receive* less info (-12 pp). These effects are very large and - apart from the last one - statistically significant. As opposed to what we hypothesised before, the subsidy did not increase the interactions between the treated seeds and their actively job searching friends, but rather substantially cut down communication and relations between the job contact pairs. Interestingly, there is some evidence suggesting the opposite mechanism for those peers that were not active job seekers at baseline: Their interaction with their treated seeds increased in some dimensions (time spent, information received, money exchange) at economically significant, but mostly statistically insignificant levels.

These findings clearly speak against the complementarity of job search between seeds and peers, and instead of supporting the idea of information exchange within these pair of job seekers, our results point towards a rupture of the job search relationship. The subsidy seems to have altered the initial relationship between the seeds and peers, which was characterised by frequent meetings and very high levels of information exchange. After the receiving the subsidy, the seeds sharply decreased their interaction with those peers who, just like them, were actively looking for a job at baseline, potentially using the subsidy to improve their own job search position and breaking a previously existing 'job search cooperation' with the peers.

Before we go on testing this particular story, we have another look at our outcomes describing the peers' attitudes and beliefs as well as expectations about the job market, in order to comprehensively reject any theory of discouragement of the peers. In table 18, we cannot see clear patterns of differences in the spillover effects for baseline searchers or non-searchers. Both groups seem to have an increased feeling of control over their own life, while the other variables barely change. Only the baseline seekers are slightly more likely to claim that life achievements are primarily a question of luck, but this difference does not seem large enough to be evidence of a systematic discouragement of the job-seeking peers through the sharing of vacancy board information. If anything, this particular result could reflect the fact that the seeds randomly received the transport subsidy, which to the peers could (quite rightly) look like evidence for the importance of luck.

Lastly, when looking at expectations and aspirations of the peers by baseline search status, there is also no evidence of a discouragement of the job-seeking peers: The expectations about offers and wages remain constant, and if anything life satisfaction increases more strongly for the baseline job searchers. To sum up, the negative spillover results on peers' job search, in particular for those peers who were active job seekers at baseline, cannot be explained by a loss of heart of the treated seeds' job contacts. Neither the positive results on beliefs and attitudes nor the decrease in information exchange between job-seeking peers and seeds support this story. Instead, the evidence points towards a rupture of the job search pairs, potentially with the subsidised seed breaking the initially existing cooperative job search arrangements between seeds and peers.

6.2 Similarity of seed and peer

In order to test the hypothesis of the transport subsidy destroying the job search cooperation between seeds and peers, we first look at the similarity of the job search pairs. As described in section 2.3, the peers were not restricted to meet the eligibility criteria of the transport subsidy sample from which our seeds were randomly drawn,[17] but still ended up looking

[17]Namely: Between 18-29 years of age, at least a high school degree, not in permanent employment, living outside of the city centre of Addis Ababa.

very much like them in many dimensions. However, certain differences between seeds and peers exist, in particular for variables capturing economic status and job search activities. The idea that prior to the transport subsidy intervention seeds and their peers cooperated during job search (as suggested by the strong baseline interactions within these pairs, which itself is of course a consequence of the sampling design) is based to a certain extent on the similarity between seeds and peers. Before the interventions, peers and seeds were relatively equal in their (job search) endowments and and consequently had a relatively equal exchange of information on vacancies, the job market etc. By randomly handing out transport subsidies to the seeds, we introduce an asymmetry into the job search contacts and hence make them unequal job search partners, which eventually leads to the rupture of the cooperation and the better endowed seed continues searching on her own.

If this hypothesis holds, we can expect the negative spillover effects on job search for the peers to be stronger for those peers who (at baseline) are more similar to their peers. We can test this by creating different measures of similarity between peer and seed. For now, we limit ourselves to a measure of initial economic status: We calculate the absolute difference in baseline expenditure between seed and peer. We then split the sample (of peers who do job search at baseline) into those above and below the median difference in expenditure between seed and peer.

Table 20 shows our main results on job search activities for the sample of baseline job seekers, split between those who are similar (uneven columns (1), (3), (5)) and dissimilar (even columns (2), (4), (6)) to their seeds in baseline expenditure. The results support our hypothesis of an asymmetry induced by the transport subsidy: For those peer-seed pairing that were more similar at baseline in economic status, the spillover effects of the intervention are much more negative, reducing job search and board search by almost two thirds and also decreasing the search within networks (insignificantly due to limited power caused by small sample sizes).

When looking at table 21, it becomes clear that baseline economic similarity can explain the decrease in search of peers, but not necessarily the decrease in communication: Even though more similar pairs travel to the city less frequently and exchange less money, the information flows decrease almost equally between similar and dissimilar peer-seed pairings.

7 Conclusion

We study information sharing about job opportunities in the social networks of young job-seekers in Addis Ababa, Ethiopia. In particular, this paper estimates the spillover effects of a randomised job search assistance program and looks how the social network of job seekers is affected by the intervention. The job search assistance program was designed as a transport subsidy to overcome spatial barriers in the job search process of young suburban unemployed

individuals (Abebe et al., 2016). These job seekers are geographically disconnected from centralised labour market information, in particular on the job vacancy boards in the city centre of Addis Ababa. We select a random sample of individuals in the control and treatment group of the original experiment (the "seeds"). For each seed, we obtain a list of the individuals with whom the seed frequently exchanges information about job opportunities (the "friends" or "peers"). We randomly sample friends of treated seeds, as well as friends of untreated seeds. As a consequence, we have an exogenous variation in the composition of the contact network and can attribute any changes in the peers' outcomes to the receipt of the subsidy through the peer.

We find that peers in the network of individuals that received the transport have a significantly lower likelihood of searching for employment along multiple dimensions, compared to the peers of untreated individuals. Most importantly, overall ob search decreases significantly and search strategies shift from commuting-based strategies towards different activities. In line with this, overall travel to the city is lower, and correspondingly search strategies requiring transport are chosen less frequently. In addition to that, there is suggested evidence of more positive beliefs and attitudes about life for peers of treated seeds. In connection with unchanged job market expectations and aspirations, this rules out discouragement effects on the peers' side and points toward the transmission of information from treated seeds to peers. In general, the impact of the transport subsidy diffuses considerably beyond the level of the individual that randomly got allocated to the transport subsidy group into the network of her peers. Importantly, this suggests that a transport subsidy is not simply a remedy for an individual that is disconnected from relevant information on job opportunities, but has considerable impact on peers' job search efforts, too.

The negative spillover effect on job search is driven by the sample of job contacts that are active job seekers at baseline. We do not find any evidence that these job contacts are more likely to exchange information about job opportunities with the seeds. To the contrary, seeds participating in the job search assistance program interact less frequently with close friends who searched actively at baseline. This is true for different types of interaction, including the exchange of information about vacancies. We also find a 10 percentage point, yet statistically insignificant, decrease in the probability of employment for close friends who were actively looking for a job at baseline.

Why do we find this decrease in information sharing and social interactions? One possibility is that the job search assistance intervention increases inequality within the pairs of friends and that this inequality makes it harder to sustain cooperative job search. In support of this idea, we find some qualified evidence that the negative indirect effects are stronger for pairs of friends with similar levels of baseline expenditure. Ultimately, we interpret this as evidence that job search activities take place collaboratively: close friends help each other find jobs, in particular when they are similar to each other.

This paper contributes to the literature on job network effects of randomised employment interventions and on spatial job networks. The results of our study imply that the benefits of randomised interventions designed to lower the costs of job search are different from what the analysis of only the direct beneficiaries suggests. Dismantling spatial barriers to centralised job information can reduce the social capital of untreated individuals who are socially connected to program participants. In line with the recent literature on the importance of information sharing on technology adoption (such as Banerjee et al. (2013)) and of peer support on achieving one's goals (Breza and Chandrasekhar, 2015), we find that an intervention that asymmetrically only targets one person in a pair of closely tied friends can disrupt this relationship and as a consequence reduce social capital. Our results show that the negative effects on social networks are stronger when the ties had similar expenditure levels at baseline, highlighting that the inequality generated by non-universal interventions is a possible mechanism leading to the deterioration of social capital.

Finally, we show that close friends are not geographically clustered, bearing implications for the way researchers should think about relevant social networks and interactions in an urban setting. Rather than looking at social interactions and network effects that are restricted to occur at the level of city blocks or neighborhoods (Topa et al. (2009), Schmutte (2015)), the literature should develop a more comprehensive view of the role that networks play in the economy of cities and try to capture social connections across space (Kramarz and Skans, 2014).

Ultimately, our findings are also important news for policy-makers who are interested in the costs or benefits of policy interventions, in particular interventions supposed to activate job search behavior of certain groups: Only estimating the direct program effects can considerably underestimate the true social costs and benefits of the program and hence lead to incorrect decisions about its implementation. The impact on social networks, and hence social capital, can be very different from what the direct program evaluation suggests.

References

Abebe, G., S. Caria, M. Fafchamps, P. Falco, S. Franklin, and S. Quinn (2016). Curse of anonymity or tyranny of distance? the impacts of job-search support in urban ethiopia. (2016-10).

Angelucci, M. and G. De Giorgi (2009). Indirect effects of an aid program: How do cash transfers affect ineligibles' consumption? *The American Economic Review*, 486–508.

Banerjee, A., A. G. Chandrasekhar, E. Duflo, and M. O. Jackson (2013). The diffusion of microfinance. *Science 341*(6144).

Beaman, L., A. BenYishay, J. Magruder, and A. M. Mobarak (2015). Can network theory based targeting increase technology adoption? Technical report, Mimeo.

Beaman, L. and J. Magruder (2012). Who gets the job referral? evidence from a social networks experiment. *The American Economic Review 102*(7), 3574–3593.

Bentolila, S., C. Michelacci, and J. Suarez (2010). Social contacts and occupational choice. *Economica 77*(305), 20–45.

Breza, E. and A. G. Chandrasekhar (2015, May). Social networks, reputation and commitment: Evidence from a savings monitors experiment. Working Paper 21169, National Bureau of Economic Research.

Burks, S. V., B. Cowgill, M. Hoffman, and M. Housman (2015). The value of hiring through employee referrals. *The Quarterly Journal of Economics*.

Cai, J., A. De Janvry, and E. Sadoulet (2015). Social networks and the decision to insure. *American Economic Journal: Applied Economics 7*(2), 81–108.

Calvo-Armengol, A. and M. O. Jackson (2004). The effects of social networks on employment and inequality. *The American Economic Review 94*(3), 426–454.

Cecchi, F., J. Duchoslav, and E. Bulte (2016). Formal insurance and the dynamics of social capital: Experimental evidence from uganda. *Journal of African Economies 25*(3), 418–438.

Cingano, F. and A. Rosolia (2012). People i know: job search and social networks. *Journal of Labor Economics 30*(2), 291–332.

Conley, T. G. and G. Topa (2007). Estimating dynamic local interactions models. *Journal of Econometrics 140*(1), 282–303.

Conley, T. G. and C. R. Udry (2010). Learning about a new technology: Pineapple in ghana. *The American Economic Review 100*(1), 35–69.

Crépon, B., E. Duflo, M. Gurgand, R. Rathelot, and P. Zamora (2013). Do labor market policies have displacement effects? evidence from a clustered randomized experiment. *The Quarterly Journal of Economics 128*(2), 531–580.

Dustmann, C., A. Glitz, U. Schönberg, and H. Brücker (2016). Referral-based job search networks. *The Review of Economic Studies 83*(2), 514–546.

Fafchamps, M., A. Vaz, and P. C. Vicente (2012). Voting and peer effects: Experimental evidence from mozambique! Technical report, Mimeo, Oxford University.

Franklin, S. (2014). Search frictions and urban youth unemployment: Evidence from an experiment in subsidized transport.

Frison, L. and S. J. Pocock (1992). Repeated measures in clinical trials: analysis using mean summary statistics and its implications for design. *Statistics in medicine 11*(13), 1685–1704.

Granovetter, M. S. (1973). The strength of weak ties. *American Journal of Sociology 78*(6), 1360–1380.

Haushofer, J., J. Reisinger, and J. Shapiro (2015). Your gain is my pain: Negative psychological externalities of cash transfers. Technical report, Working Paper.

Ioannides, Y. M. and L. Datcher Loury (2004). Job information networks, neighborhood effects, and inequality. *Journal of economic literature 42*(4), 1056–1093.

Kramarz, F. and O. N. Skans (2014). When strong ties are strong: Networks and youth labour market entry. *The Review of Economic Studies 81*(3), 1164–1200.

Kremer, M. and E. Miguel (2007). The illusion of sustainability*. *The Quarterly Journal of Economics 122*(3), 1007–1065.

Kuzubas, T. U., A. Szabo, et al. (2014). Multiple job search networks: Theory and evidence from indonesia. Technical report, Working Paper, Bogazici University.

Lazarsfeld, P. F., R. K. Merton, et al. (1954). Friendship as a social process: A substantive and methodological analysis. *Freedom and control in modern society 18*(1), 18–66.

Marianne Bertrand, Sendhil Mullainathan, E. S. (2004). A behavioral-economics view of poverty. *The American Economic Review 94*(2), 419–423.

McKenzie, D. (2012). Beyond baseline and follow-up: The case for more t in experiments. *Journal of Development Economics 99*(2), 210–221.

Schmutte, I. M. (2015). Job referral networks and the determination of earnings in local labor markets. *Journal of Labor Economics 33*(1), 1–32.

Topa, G. (2001). Social interactions, local spillovers and unemployment. *The Review of Economic Studies 68*(2), 261–295.

Topa, G. (2011). Labor markets and referrals. *Handbook of social economics 1*, 1193–1221.

Topa, G., P. Bayer, and S. Ross (2009). Place of work and place of residence: Informal hiring networks and labor market. *Journal of Political Economy 116*, 1150–1196.

World Bank, T. (2007). *Ethiopia - Urban Labor Markets : Challenges and Prospects*. World Bank Publications.

World Bank, T. (2014). *World Development Indicators 2014*. World Bank Publications.

List of Figures

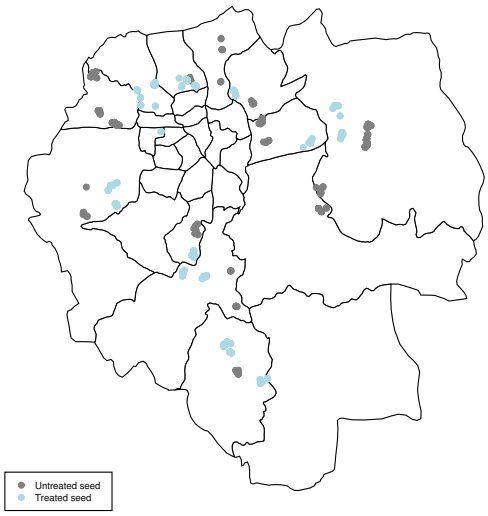


Figure 1: Distribution of treated and untreated seeds in Addis Ababa

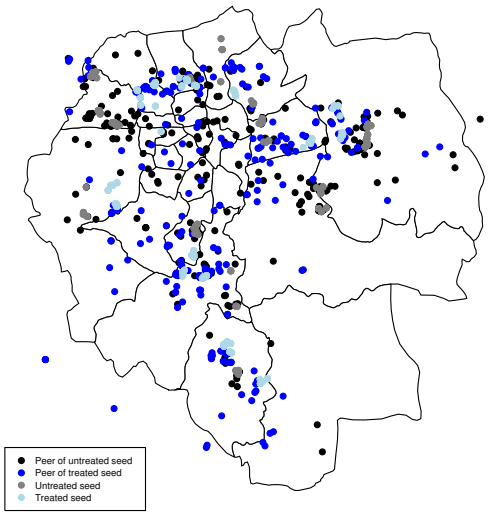


Figure 2: Distribution of treated and untreated seeds and peers of treated and untreated seeds in Addis Ababa

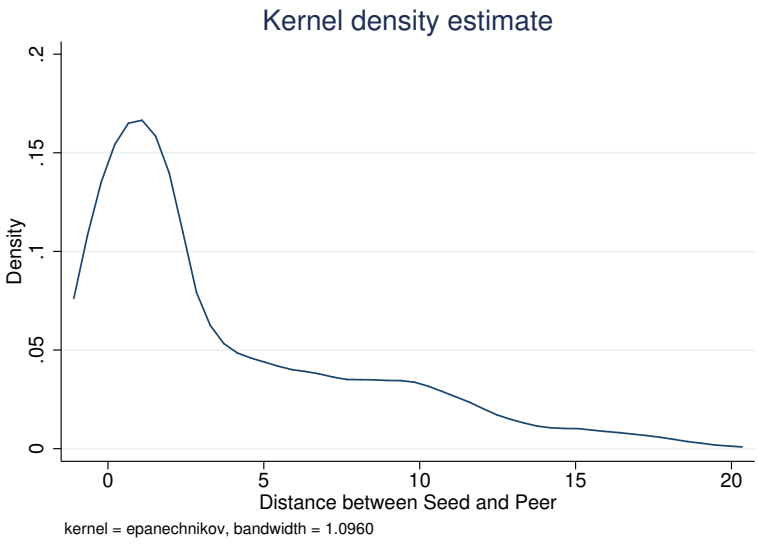


Figure 3: Kernel density estimate of the distance between peers and seeds (in km)

List of Tables

Table 1: Balance of covariates for seeds, by treatment status, full sample

	results					
	C	T	t-stat	Pval	N1	N2
Number of peers	3.67	3.63	-.12	.91	86	79
Age	23.85	23.51	-.73	.46	86	79
Female	.45	.61	1.99	.05	86	79
High school	.33	.24	-1.21	.23	86	79
Preparatory school	.03	.03	-.36	.72	86	79
Vocational school	.4	.42	.29	.77	86	79
Diploma	.07	.05	-.52	.61	86	79
University	.17	.27	1.41	.16	86	79
Business	.24	.2	-.5	.62	46	46
Vocational	.43	.3	-1.29	.2	46	46
Humanities	.13	.07	-1.05	.3	46	46
Worked (7 d)	.38	.37	-.22	.83	86	79
Return job	.05	.03	-.73	.47	86	79
Wage empl (6 m)	.51	.47	-.55	.58	86	79
Self empl (6 m)	.13	.1	-.54	.59	86	79
Ever worked for pay	.67	.49	-2.38	.02	86	79
Searched job (7 d)	.55	.51	-.51	.61	86	79
Searched job (6 m)	.77	.72	-.67	.5	86	79
Ever searched wage job	.33	.38	.72	.47	86	79
Plans to start business	.02	.01	-.51	.61	86	79
Potential job	.03	.04	.11	.92	86	79
Searched boards (7 d)	.35	.23	-1.72	.09	86	79
Ever searched boards	.69	.76	1.05	.29	86	79
Lives with parents	.51	.56	.58	.56	86	79
Last year of education	2002.36	2002.7	.75	.46	86	79
Years since school	3.64	3.3	-.75	.46	86	79
Permanently empl	0	0	.	.	86	79
Temporarily empl	.2	.24	.66	.51	86	79
Casual worker	.07	.01	-1.88	.06	86	79
Casual worker	.04	.03	-.2	.84	86	79
Contract worker	.03	.06	.84	.4	86	79
Self-employed	.08	.05	-.8	.43	86	79
Amhara	.53	.49	-.53	.6	86	79
Oromo	.21	.28	1.03	.31	86	79
Tigre	.02	.09	1.81	.07	86	79
Guraghe	.17	.08	-1.93	.05	86	79
Mixed	.01	.01	.06	.95	86	79
Other	.05	.05	.12	.9	86	79

Table 2: Balance of covariates for network contacts, by treatment status

	results					
	C	T	t-stat	Pval	N1	N2
Age	25.64	25.01	-.88	.38	313	281
Female	.36	.42	1	.32	314	282
Degree	.16	.21	1.07	.29	314	282
Vocational Training	.25	.28	.67	.5	314	282
Post-Secondary Degree	.83	.78	-1.01	.31	314	282
Born outside Addis	.47	.31	-2.78	.01	314	282
Amhara	.49	.49	-.08	.94	314	282
Oromo	.19	.25	1.49	.14	314	282
Worked (7d)	.5	.49	-.29	.77	314	282
Permanent work (7d)	.12	.2	2.24	.03	314	282
Satisfied with work	.2	.2	-.27	.79	314	282
Written agreement	.2	.22	.44	.66	314	282
Applied to temporary jobs	.23	.19	-.87	.39	314	282
Applied to permanent jobs	.25	.26	.35	.73	314	282
Uses CV for applications	.25	.26	.35	.72	314	282
Uses certificates	.27	.3	.62	.54	314	282
Received job by interview	.12	.16	1.11	.27	314	282
Office work (7d)	.15	.2	1.29	.2	314	282
Offers expected (next 4m)	1.75	1.57	-.88	.38	265	255
Life satisfaction (0-10)	4.44	4.49	.27	.79	312	282
Expenditure (7d)	528.95	549.44	.28	.78	303	261
Savings	11503.95	11177.91	-.15	.88	83	74
Monthly earnings	2320.52	2307.99	-.02	.98	155	134
Reservation wage	2266.7	2262.86	-.01	.99	312	276
Aspiration wage (in 5y)	13782.62	8787.04	-.76	.45	305	270
Trip to center (7d)	2.33	2.39	.23	.82	305	261
Works away from home	.83	.86	.78	.44	314	282
In full-time education	.06	.07	.43	.67	314	282
In part-time education	.13	.17	1.17	.25	314	282
In informal training	.08	.11	1.16	.25	314	282
Searched job (7d)	.38	.3	-1.49	.14	314	282
Searched job (6m)	.54	.47	-1.22	.22	314	282
Ever searched wage job	.78	.73	-1.36	.17	314	282
Searched boards (7d)	.24	.21	-.62	.54	314	282
Searched boards (1m)	.47	.56	1.86	.06	314	282
Searched in networks (7d)	.34	.27	-1.31	.19	314	282
Married	.19	.2	.25	.8	314	282
Lives with parents	.39	.48	1.55	.12	314	282
Years since school	5.57	4.84	-1	.32	280	243

Table 3: Balance of covariates by contact status and collapsed by seed

	results					
	C	T	t-stat	Pval	N1	N2
Age	25.57	25.19	-.43	.67	86	79
Female	.37	.45	1.32	.19	86	79
Degree	.13	.26	2.56	.01	86	79
Vocational Training	.3	.3	0	1	86	79
Post-Secondary Degree	.86	.74	-2.42	.02	86	79
Born outside Addis	.41	.31	-1.67	.1	86	79
Amhara	.52	.55	.56	.58	86	79
Oromo	.18	.25	1.62	.11	86	79
Worked (7d)	.5	.54	.81	.42	86	79
Permanent work (7d)	.11	.22	2.47	.01	86	79
Satisfied with work	.22	.18	-1	.32	86	79
Written agreement	.21	.27	1.26	.21	86	79
Applied to temporary jobs	.23	.21	-.41	.69	86	79
Applied to permanent jobs	.24	.3	1.27	.21	86	79
Uses CV for applications	.25	.27	.23	.82	86	79
Uses certificates	.29	.34	.94	.35	86	79
Received job by interview	.11	.19	1.98	.05	86	79
Office work (7d)	.11	.23	2.78	.01	86	79
Offers expected (next 4m)	2.06	1.6	-1.66	.1	85	77
Life satisfaction (0-10)	4.52	4.44	-.39	.69	86	79
Expenditure (7d)	470.8	565.88	1.2	.23	85	78
Savings	10514.22	10625.26	.05	.96	46	47
Monthly earnings	2270.69	2180	-.21	.84	66	65
Reservation wage	2092.31	2265.21	.57	.57	86	79
Aspiration wage (in 5y)	11545.87	8539.1	-.63	.53	85	77
Trip to center (7d)	2.34	2.47	.52	.6	85	78
Works away from home	.83	.85	.44	.66	86	79
In full-time education	.05	.06	.17	.87	86	79
In part-time education	.11	.16	1.33	.19	86	79
In informal training	.08	.13	1.7	.09	86	79
Searched job (7d)	.4	.33	-1.27	.21	86	79
Searched job (6m)	.53	.48	-.8	.42	86	79
Ever searched wage job	.77	.74	-.77	.44	86	79
Searched boards (7d)	.26	.22	-.74	.46	86	79
Searched boards (1m)	.5	.57	1.42	.16	86	79
Searched in networks (7d)	.36	.3	-.99	.32	86	79
Married	.17	.21	.73	.47	86	79
Lives with parents	.42	.49	1.19	.24	86	79
Years since school	5.07	4.8	-.42	.67	83	77

Table 4: Interaction and distance between peers and seeds, by treatment status

	results					
	C	T	t-stat	Pval	N1	N2
Spoken (30d)	11.99	13.11	.87	.38	308	281
Travel (30d)	.5	.5	-.01	1	310	282
Info from seed (ever)	.82	.85	.6	.55	310	282
Info to seed (ever)	.97	.94	-1.09	.28	310	281
Lent/borrowed (ever)	.5	.46	-.65	.51	310	282
Distance from Seed to center	8.02	7.83	-.23	.82	314	282
Distance between Seed and Peer	6.87	4.47	-1.61	.11	314	280
Same neighborhood	.04	.05	.23	.82	314	282

Table 5: ANCOVA regression on job search behaviour outcomes

	(1) Searched job (7d)	(2) Searched boards (7d)	(3) Searched in networks (7d)
interaction	-0.0693	-0.0696**	-0.0279
	(0.114)	(0.027)	(0.543)
search_rsof_bl	0.00970		
	(0.844)		
migrant_birth_rsof_bl	-0.0121	-0.0510*	-0.0139
	(0.790)	(0.087)	(0.756)
permanent_work_rsof_bl	-0.121**	-0.0367	-0.174***
	(0.022)	(0.314)	(0.001)
new_search_boards_bl	0.118**	0.0901**	0.122***
	(0.021)	(0.014)	(0.010)
search_boards_rsof_bl		0.0849*	
		(0.066)	
search_network_bl			0.00100
			(0.983)
Constant	0.304***	0.135***	0.309***
	(0.000)	(0.000)	(0.000)
Observations	540	540	540

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: ANCOVA regression on job search

	(1) Searched boards (1m)	(2) Searched in network (1m)	(3) Searched at work sites (1m)	(4) Searched at agency (1m)	(5) Searched at central locations (1m)	(6) Searched internet (1m)
interaction	-0.165**	-0.0714*	-0.00784	-0.0276	-0.0196**	0.0281
	(0.019)	(0.100)	(0.803)	(0.196)	(0.040)	(0.318)
new_search_boards_bl	0.193***	0.0121	0.0552*	0.00866	0.0129	0.00946
	(0.000)	(0.816)	(0.062)	(0.683)	(0.196)	(0.714)
migrant_birth_rsof_bl	-0.0102	0.0459	-0.0237	0.0221	-0.0154*	-0.0150
	(0.859)	(0.272)	(0.446)	(0.364)	(0.090)	(0.593)
permanent_work_rsof_bl	-0.00421	-0.168***	-0.0930***	-0.0392	-0.0144**	0.121**
	(0.948)	(0.006)	(0.009)	(0.158)	(0.013)	(0.020)
new_search_boards_bl	0					
	(.)					
new_search_network_bl		-0.0676				
		(0.175)				
search_sites_bl			-0.0649			
			(0.322)			
search_agency_bl				0.139***		
				(0.000)		
search_location_bl					-0.0161**	
					(0.015)	
search_internet_bl						0.131***
						(0.002)
Constant	0.435***	0.757***	0.201***	0.0604***	0.0244**	0.0494*
	(0.000)	(0.000)	(0.004)	(0.008)	(0.016)	(0.057)
Observations	540	540	540	540	540	540

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: ANCOVA regression on job market expectations

	(1)	(2)	(3)	(4)
	Offers expected (next 4m)	Reservation wage	Aspiration wage (in 5y)	Life satisfaction (0-10)
interaction	-0.0518	-203.9	-1494.0	0.284
	(0.824)	(0.363)	(0.364)	(0.218)
expect_offer_rsof_bl	0.0848*			
	(0.087)			
migrant_birth_rsof_bl	-0.0405	-190.3	-1700.3	0.121
	(0.865)	(0.406)	(0.222)	(0.559)
permanent_work_rsof_bl	-0.0944	820.7**	3630.6**	0.312
	(0.683)	(0.024)	(0.047)	(0.129)
new_search_boards_bl	0.226	-136.1	277.7	-0.239
	(0.244)	(0.493)	(0.856)	(0.264)
res_wage_rsof_bl		0.588***		
		(0.000)		
aspiration_rsof_bl			0.424***	
			(0.006)	
life_satisfaction_rsof_bl				0.228***
				(0.000)
Constant	1.431***	1366.7***	6847.7***	3.797***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	450	499	514	537

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: ANCOVA regression on psycho-social indicators

	(1) Life course: independence	(2) Life achievements: luck	(3) Doubts own abilities during difficulties	(4) Possibilities dependent on social circumstances	(5) Little control over own life
interaction	0.583***	0.159	-0.166	-0.0827	-0.372***
	(0.000)	(0.207)	(0.177)	(0.294)	(0.005)
a1_26_rsof_bl	0.106**				
	(0.017)				
migrant_birth_rsof_bl	-0.0507	0.0273	0.130	0.116	0.140
	(0.718)	(0.817)	(0.220)	(0.197)	(0.244)
permanent_work_rsof_bl	0.0533	-0.266*	-0.314**	-0.217*	-0.233*
	(0.704)	(0.065)	(0.013)	(0.073)	(0.060)
new_search_boards_bl	0.0561	-0.233*	-0.139	0.248**	-0.0419
	(0.624)	(0.070)	(0.242)	(0.012)	(0.672)
a1_28_rsof_bl		0.121***			
		(0.003)			
a1_32_rsof_bl			0.125**		
			(0.012)		
a1_33_rsof_bl				-0.0190	
				(0.667)	
a1_35_rsof_bl					0.0926*
					(0.066)
Constant	2.708***	2.656***	2.478***	3.420***	2.604***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	485	485	485	485	485

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: ANCOVA regression on social interactions between peer and seed

	(1) Spent time with seed (30d)	(2) Travel to Addis (30d)	(3) Shared travel expenses (30d)	(4) info to seed (6m)	(5) info from seed (6m)	(6) lent/borrowed (ever)
interaction	0.0224	-0.0403	-0.0713	-0.0525	-0.00313	0.0367
	(0.673)	(0.368)	(0.112)	(0.375)	(0.959)	(0.374)
migrant_birth_rsof_bl	-0.0678	-0.0369	-0.0306	-0.0477	-0.0452	0.0495
	(0.178)	(0.430)	(0.500)	(0.370)	(0.446)	(0.310)
permanent_work_rsof_bl	-0.0348	-0.0432	-0.0775*	-0.0180	-0.0594	-0.125**
	(0.538)	(0.352)	(0.066)	(0.750)	(0.287)	(0.012)
new_search_boards_bl	0.0100	0.0100	0.0318	0.0336	0.0130	0.0303
	(0.821)	(0.787)	(0.377)	(0.496)	(0.789)	(0.494)
Constant	0.750***	0.267***	0.243***	0.464***	0.488***	0.230***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	490	540	540	540	540	540

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: ANCOVA regression on employment outcomes

	(1) Worked (7d)	(2) Wage empl (6m)	(3) Permanent work (7d)	(4) Temporarily empl	(5) Casual worker	(6) Contract worker	(7) Self-employed	(8) Satisfied with work	(9) Written agreement
interaction	-0.0116	0.0402	-0.0143	0.0312	-0.00994	-0.0211	-0.0151	0.0511	0.00686
	(0.793)	(0.368)	(0.709)	(0.485)	(0.419)	(0.201)	(0.610)	(0.228)	(0.878)
work_rsof_bl	0.276***								
	(0.000)								
migrant_birth_rsof_bl	-0.0178	0.0330	-0.00264	-0.0274	-0.0176	0.0324	-0.0370	-0.0299	-0.0412
	(0.668)	(0.473)	(0.938)	(0.504)	(0.144)	(0.115)	(0.200)	(0.492)	(0.280)
permanent_work_rsof_bl	0.0878*	0.0249	0	-0.0943**	-0.0121	-0.0382***	-0.0221	0.0388	0.143*
	(0.058)	(0.610)	(.)	(0.025)	(0.374)	(0.001)	(0.461)	(0.561)	(0.076)
new_search_boards_bl	0.0271	0.0819**	0.0881***	-0.0181	0.00332	-0.00761	-0.0562**	-0.00977	0.154***
	(0.509)	(0.038)	(0.009)	(0.657)	(0.800)	(0.568)	(0.031)	(0.809)	(0.000)
work_wage_6months_rsof_bl		0.299***							
		(0.000)							
work_temp_rsof_bl				0.239***					
				(0.000)					
work_cas_rsof_bl					0.0189				
					(0.676)				
work_contract_rsof_bl						0.00257			
						(0.956)			
selfemployed_rsof_bl							0.380***		
							(0.000)		
work_satisfaction_rsof_bl								0.237***	
								(0.000)	
written_agreement_rsof_bl									0.294***
									(0.000)
Constant	0.538***	0.447***	0.142***	0.194***	0.0351**	0.0422**	0.142***	0.242***	0.154***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.013)	(0.016)	(0.000)	(0.000)	(0.000)
Observations	540	540	540	540	540	540	540	540	540

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: ANCOVA regression on education outcomes

	(1) In full-time education	(2) In part-time education	(3) In informal training
interaction	0.00180	-0.00241	0.0298
	(0.938)	(0.951)	(0.298)
fulltime_education_rsof_bl	0.415***		
	(0.000)		
migrant_birth_rsof_bl	-0.0114	0.00429	-0.00880
	(0.567)	(0.914)	(0.745)
permanent_work_rsof_bl	0.00215	0.0855	-0.0336
	(0.952)	(0.108)	(0.179)
new_search_boards_bl	-0.0137	0.0622**	0.00895
	(0.556)	(0.044)	(0.742)
parttime_education_rsof_bl		0.149**	
		(0.015)	
informal_training_rsof_bl			0.0271
			(0.559)
Constant	0.0620***	0.0940***	0.0578**
	(0.006)	(0.006)	(0.012)
Observations	540	540	540

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: ANCOVA regression on job application outcomes

	(1) Applied to temporary jobs	(2) Applied to permanent jobs	(3) Uses CV for applications	(4) Uses certificates
interaction	0.0182	-0.00609	0.0269	0.0132
	(0.613)	(0.886)	(0.465)	(0.706)
apply_temp_rsof_bl	0.0702			
	(0.116)			
migrant_birth_rsof_bl	0.0124	-0.0136	-0.0403	-0.0101
	(0.752)	(0.742)	(0.259)	(0.778)
permanent_work_rsof_bl	-0.0609	0.0432	0.0411	0.0338
	(0.184)	(0.457)	(0.422)	(0.509)
new_search_boards_bl	0.0999***	0.106***	0.0611	0.0752**
	(0.005)	(0.008)	(0.109)	(0.047)
apply_perm_rsof_bl		0.278***		
		(0.000)		
cv_application_rsof_bl			0.354***	
			(0.000)	
cert_application_rsof_bl				0.356***
				(0.000)
Constant	0.113***	0.142***	0.107***	0.103***
	(0.001)	(0.000)	(0.001)	(0.001)
Observations	540	540	540	540

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: ANCOVA regression on financial outcomes

	(1) Expenditure (7d)	(2) Savings	(3) Monthly earnings
interaction	32.14	1027.7	199.2
	(0.604)	(0.926)	(0.765)
expenditure_rsof_bl	0.246***		
	(0.000)		
migrant_birth_rsof_bl	-21.89	11256.8	558.8
	(0.716)	(0.356)	(0.308)
permanent_work_rsof_bl	51.80	1651.4	-502.0
	(0.498)	(0.900)	(0.458)
new_search_boards_bl	-16.19	2753.6	370.9
	(0.792)	(0.815)	(0.478)
saving_rsof_bl		3.212***	
		(0.002)	
earnings_rsof_bl			0.811***
			(0.002)
Constant	429.8***	4658.4	824.1
	(0.000)	(0.515)	(0.203)
Observations	540	130	227

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: ANCOVA regression on mobility outcomes

	(1) Trip to center (7d)	(2) Works away from home
interaction	-0.496*	-0.00756
	(0.059)	(0.814)
travel_rsof_bl	0.214***	
	(0.000)	
migrant_birth_rsof_bl	-0.227	-0.0923***
	(0.363)	(0.007)
permanent_work_rsof_bl	0.460	-0.0525
	(0.117)	(0.273)
new_search_boards_bl	0.140	-0.0103
	(0.560)	(0.730)
work_away_rsof_bl		0.266***
		(0.000)
Constant	2.490***	0.674***
	(0.000)	(0.000)
Observations	482	540

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: ANCOVA regression on job search behaviour outcomes, by baseline search status

	(1)	(2)	(3)	(4)	(5)	(6)
Active baseline job seeker?	No	Yes	No	Yes	No	Yes
	Searched job (7d)	Searched job (7d)	Searched boards (7d)	Searched boards (7d)	Searched in networks (7d)	Searched in networks (7d)
Treatment effect	-0.0272	-0.230**	-0.0366	-0.183**	-0.00235	-0.129
	(0.568)	(0.014)	(0.260)	(0.014)	(0.963)	(0.133)
Baseline: Born outside Addis	-0.0341	0.0979	-0.0523	-0.0195	-0.0318	0.0613
	(0.460)	(0.363)	(0.108)	(0.808)	(0.498)	(0.535)
Baseline: In permanent employment	-0.163***	0.0864	-0.0446	-0.00634	-0.228***	0.0792
	(0.004)	(0.523)	(0.233)	(0.955)	(0.000)	(0.556)
Baseline: Searched boards ever	0.109**	0	0.0867**	0	0.125**	0
	(0.039)	(.)	(0.019)	(.)	(0.013)	(.)
Control for baseline outcome	YES	YES	YES	YES	YES	YES
Constant	0.301***	0.454***	0.123***	0.345***	0.311***	0.408***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.005)
Observations	417	123	417	123	417	123

p-values in parentheses. Standard errors clustered by seed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 16: ANCOVA regression on job search, by baseline search status

Active baseline job seeker?	(1) No	(2) Yes	(3) No	(4) Yes	(5) No	(6) Yes	(7) No	(8) Yes	(9) No	(10) Yes	(11) No	(12) Yes
	Searched boards (1m)	Searched boards (1m)	Searched in network (1m)	Searched in network (1m)	Searched at work sites (1m)	Searched at work sites (1m)	Searched at agency (1m)	Searched at agency (1m)	Searched at central locations (1m)	Searched at central locations (1m)	Searched internet (1m)	Searched internet (1m)
Treatment effect	-0.135*	-0.219**	-0.0691	-0.0478	0.0207	-0.0762	-0.0137	-0.0630	-0.00466	-0.0542**	0.0303	0.0165
	(0.090)	(0.017)	(0.158)	(0.547)	(0.501)	(0.345)	(0.522)	(0.305)	(0.521)	(0.038)	(0.296)	(0.813)
Baseline: Searched boards ever	0.129**	0	-0.00137	0	0.0203	0	-0.0189	0	0.00102	0	-0.0333	0
	(0.026)	(.)	(0.983)	(.)	(0.539)	(.)	(0.426)	(.)	(0.900)	(.)	(0.217)	(.)
Baseline: Born outside Addis	0.00596	-0.0202	0.0651	-0.0392	-0.0266	0.0143	0.0226	0.0468	-0.00297	-0.0447**	-0.0344	0.0603
	(0.928)	(0.823)	(0.164)	(0.644)	(0.380)	(0.867)	(0.375)	(0.476)	(0.714)	(0.039)	(0.236)	(0.419)
Baseline: In permanent employment	0.0139	0.0193	-0.229***	0.188	-0.0985***	0.00845	-0.0233	-0.0768	-0.00844	-0.0241	0.104**	0.354**
	(0.844)	(0.894)	(0.001)	(0.126)	(0.003)	(0.949)	(0.419)	(0.370)	(0.141)	(0.204)	(0.045)	(0.037)
Control for baseline outcome	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constant	0.413***	0.723***	0.754***	0.888***	0.124	0.302***	0.0536**	0.101**	0.0120	0.0739**	0.0619**	0.0611
	(0.000)	(0.000)	(0.000)	(0.000)	(0.142)	(0.001)	(0.023)	(0.022)	(0.190)	(0.036)	(0.023)	(0.160)
Observations	417	123	417	123	417	123	417	123	417	123	417	123

p-values in parentheses. Standard errors clustered by seed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 17: ANCOVA regression on social interactions between peer and seed, by baseline search status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Active baseline job seeker?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
	Spent time with seed (30d)	Spent time with seed (30d)	Travel to Addis (30d)	Travel to Addis (30d)	Shared travel expenses (30d)	Shared travel expenses (30d)	info to seed (6m)	info to seed (6m)	info from seed (6m)	info from seed (6m)	lent/borrowed (ever)	lent/borrowed (ever)
Treatment effect	0.0808	-0.158	0.0196	-0.198**	-0.0258	-0.187**	0.0148	-0.211**	0.0506	-0.123	0.0770*	-0.0766
	(0.143)	(0.102)	(0.655)	(0.026)	(0.560)	(0.033)	(0.817)	(0.028)	(0.457)	(0.192)	(0.069)	(0.391)
Baseline: Born outside Addis	-0.0218	-0.204**	-0.00481	-0.102	-0.00136	-0.0986	0.0136	-0.213**	0.00389	-0.181*	0.0651	0.0413
	(0.695)	(0.028)	(0.921)	(0.238)	(0.977)	(0.259)	(0.802)	(0.024)	(0.951)	(0.053)	(0.207)	(0.656)
Baseline: In permanent employment	-0.0671	0.102	-0.0693	0.152	-0.101**	0.0914	-0.0415	0.182	-0.0994	0.225*	-0.0944*	-0.246***
	(0.288)	(0.479)	(0.127)	(0.253)	(0.023)	(0.439)	(0.530)	(0.190)	(0.107)	(0.097)	(0.097)	(0.008)
Baseline: Searched boards ever	0.0230	0	-0.0278	0	0.00204	0	-0.00930	0	-0.0282	0	0.00286	0
	(0.626)	(.)	(0.509)	(.)	(0.960)	(.)	(0.871)	(.)	(0.606)	(.)	(0.954)	(.)
Control for baseline outcome	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constant	0.709***	0.847***	0.230***	0.386***	0.214***	0.362***	0.413***	0.645***	0.450***	0.617***	0.203***	0.345***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	375	115	417	123	417	123	417	123	417	123	417	123

p-values in parentheses. Standard errors clustered by seed.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18: ANCOVA regression on beliefs and attitudes, by baseline search status

Active baseline job seeker?	(1) No Life course: independence	(2) Yes Life course: independence	(3) No Life achievements: luck	(4) Yes Life achievements: luck	(5) No Doubts own abilities during difficulties	(6) Yes Doubts own abilities during difficulties	(7) No Possibilities dependent on social circumstances	(8) Yes Possibilities dependent on social circumstances	(9) No Little control over own life	(10) Yes Little control over own life
Treatment effect	0.404***	1.113***	0.0569	0.476*	-0.196	0.0391	-0.0744	-0.119	-0.273**	-0.577**
	(0.005)	(0.000)	(0.698)	(0.051)	(0.166)	(0.872)	(0.440)	(0.386)	(0.039)	(0.032)
Baseline: Born outside Addis	-0.0497	-0.188	0.0151	-0.0175	0.117	0.141	0.0462	0.407***	0.204*	-0.0157
	(0.748)	(0.408)	(0.918)	(0.935)	(0.278)	(0.559)	(0.663)	(0.005)	(0.100)	(0.949)
Baseline: In permanent employment	0.0468	-0.0251	-0.279*	-0.158	-0.419***	0.0906	-0.228*	0.0425	-0.298**	0.281
	(0.771)	(0.929)	(0.090)	(0.637)	(0.002)	(0.758)	(0.085)	(0.856)	(0.021)	(0.350)
Baseline: Searched boards ever	0.157	0	-0.217	0	-0.0815	0	0.120	0	-0.148	0
	(0.223)	(.)	(0.141)	(.)	(0.558)	(.)	(0.282)	(.)	(0.182)	(.)
Control for baseline outcome	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constant	2.938***	1.946***	2.712***	2.288***	2.314***	2.789***	3.249***	4.390***	2.546***	2.618***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	369	116	369	116	369	116	369	116	369	116

p-values in parentheses. Standard errors clustered by seed.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 19: ANCOVA regression on expectations and aspirations, by baseline search status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Active baseline job seeker?	No	Yes	No	Yes	No	Yes	No	Yes
	Offers expected (next 4m)	Offers expected (next 4m)	Reservation wage	Reservation wage	Aspiration wage (in 5y)	Aspiration wage (in 5y)	Life satisfaction (0-10)	Life satisfaction (0-10)
Treatment effect	-0.102	0.0789	-164.8	-342.1	-385.0	-4495.4	0.115	0.774*
	(0.704)	(0.847)	(0.519)	(0.222)	(0.752)	(0.392)	(0.667)	(0.072)
Baseline: Born outside Addis	-0.0353	-0.154	-237.1	48.84	-955.8	-3474.7	0.127	-0.0974
	(0.903)	(0.677)	(0.357)	(0.855)	(0.279)	(0.484)	(0.588)	(0.773)
Baseline: In permanent employment	-0.120	-0.00696	863.3**	482.1*	5107.2***	-1402.5	0.369*	-0.148
	(0.626)	(0.988)	(0.045)	(0.099)	(0.008)	(0.579)	(0.098)	(0.767)
Baseline: Searched boards ever	0.289	0	-110.7	0	-903.8	0	-0.0814	0
	(0.213)	(.)	(0.670)	(.)	(0.365)	(.)	(0.733)	(.)
Control for baseline outcome	YES	YES	YES	YES	YES	YES	YES	YES
Constant	1.494***	1.450***	1355.1***	1373.1***	6071.5***	9087.4**	3.921***	3.100***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.031)	(0.000)	(0.000)
Observations	355	95	384	115	395	119	414	123

p-values in parentheses. Standard errors clustered by seed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: ANCOVA regression on job search for baseline searchers, by similarity of peer and seed economic status

	(1)	(2)	(3)	(4)	(5)	(6)
Similar baseline expenditure of peer and seed?	Yes	No	Yes	No	Yes	No
	Searched job (7d)	Searched job (7d)	Searched boards (7d)	Searched boards (7d)	Searched in networks (7d)	Searched in networks (7d)
Treatment effect	-0.367***	-0.0876	-0.264**	-0.0354	-0.187	-0.0612
	(0.002)	(0.515)	(0.011)	(0.738)	(0.148)	(0.620)
Baseline: Born outside Addis	0.0531	0.110	0.0847	-0.150	-0.0224	0.168
	(0.697)	(0.486)	(0.497)	(0.136)	(0.872)	(0.252)
Baseline: In permanent employment	0.0868	0.149	0.00860	-0.0363	0.0218	0.142
	(0.706)	(0.420)	(0.963)	(0.768)	(0.923)	(0.448)
Control for baseline outcome	YES	YES	YES	YES	YES	YES
Constant	0.521***	0.358***	0.380***	0.271***	0.623***	0.0855
	(0.000)	(0.002)	(0.000)	(0.004)	(0.002)	(0.652)
Observations	66	57	66	57	66	57

p-values in parentheses. Standard errors clustered by seed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 21: ANCOVA regression on job search strategies for baseline searchers, by similarity of peer and seed economic status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Similar baseline expenditure of peer and seed?	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
	Searched boards (1m)	Searched boards (1m)	Searched in network (1m)	Searched in network (1m)	Searched at work sites (1m)	Searched at work sites (1m)	Searched at agency (1m)	Searched at agency (1m)	Searched at central locations (1m)	Searched at central locations (1m)	Searched internet (1m)	Searched internet (1m)
Treatment effect	-0.218*	-0.216	-0.175	0.113	-0.0584	-0.0410	-0.155**	0.0565	-0.0502	-0.0673	0.125	-0.0497
	(0.076)	(0.131)	(0.165)	(0.366)	(0.614)	(0.747)	(0.031)	(0.564)	(0.140)	(0.179)	(0.261)	(0.481)
Baseline: Born outside Addis	-0.140	0.125	-0.0651	-0.0387	0.0815	-0.0768	0.115	-0.0332	-0.0465	-0.0471	0.00846	0.192*
	(0.332)	(0.341)	(0.601)	(0.760)	(0.530)	(0.544)	(0.306)	(0.719)	(0.138)	(0.179)	(0.924)	(0.075)
Baseline: In permanent employment	-0.0233	0.116	0.0867	0.355***	0.199	-0.220***	-0.00869	-0.137*	-0.00762	-0.0435	0.150	0.572**
	(0.920)	(0.514)	(0.699)	(0.000)	(0.302)	(0.003)	(0.951)	(0.078)	(0.700)	(0.282)	(0.418)	(0.025)
Control for baseline outcome	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constant	0.799***	0.617***	1.033***	0.604*	0.362***	0.235*	0.151**	0.0271	0.0629	0.0929	0.0824	0.00536
	(0.000)	(0.000)	(0.000)	(0.061)	(0.010)	(0.072)	(0.019)	(0.495)	(0.144)	(0.170)	(0.158)	(0.914)
Observations	66	57	66	57	66	57	66	57	66	57	66	57

p-values in parentheses. Standard errors clustered by seed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 22: ANCOVA regression on social interactions between peer and seed, by similarity of peer and seed economic status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Similar baseline expenditure of peer and seed?	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
	Spent time with seed (30d)	Spent time with seed (30d)	Travel to Addis (30d)	Travel to Addis (30d)	Shared travel expenses (30d)	Shared travel expenses (30d)	info to seed (6m)	info to seed (6m)	info from seed (6m)	info from seed (6m)	lent/borrowed (ever)	lent/borrowed (ever)
Treatment effect	-0.0949	-0.181	-0.240*	-0.171	-0.250**	-0.137	-0.183	-0.241*	-0.170	-0.120	-0.207*	0.0524
	(0.426)	(0.213)	(0.060)	(0.166)	(0.039)	(0.231)	(0.123)	(0.092)	(0.148)	(0.414)	(0.072)	(0.675)
Baseline: Born outside Addis	-0.183	-0.212	-0.273**	0.0709	-0.287**	0.0940	-0.314**	-0.102	-0.367***	-0.00319	-0.106	0.177
	(0.143)	(0.133)	(0.027)	(0.527)	(0.017)	(0.434)	(0.012)	(0.458)	(0.002)	(0.982)	(0.369)	(0.217)
Baseline: In permanent employment	0.157	0.00823	0.236	0.135	0.117	0.157	0.298*	0.0771	0.327*	0.192	-0.286***	-0.0994
	(0.336)	(0.970)	(0.266)	(0.426)	(0.517)	(0.344)	(0.083)	(0.737)	(0.052)	(0.390)	(0.003)	(0.527)
Control for baseline outcome	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constant	0.859***	0.820***	0.486***	0.260**	0.476***	0.214**	0.700***	0.577***	0.694***	0.535***	0.465***	0.181**
	(0.000)	(0.000)	(0.000)	(0.011)	(0.000)	(0.017)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.044)
Observations	62	53	66	57	66	57	66	57	66	57	66	57

p-values in parentheses. Standard errors clustered by seed.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$