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Abstract

Using a search theoretical model, we analyse the effects of the information flow via social networks (friends, relatives and other personal contacts) by comparing monetary and non-monetary outcomes in obtaining jobs via networks versus formal methods. Propensity-score matching on survey data from the low-skilled unemployed is used to identify causal effects. The analysis takes into account unobserved heterogeneity by applying Rosenbaum bounds. Because of the potential ambiguity when comparing outcomes in accepted jobs, we also examine the effectiveness of job searches using social networks as a source of information compared to not using networks. We find no evidence for causal effects on monetary outcomes and, at best, only weak evidence for effects on non-monetary job outcomes.

Zusammenfassung

Ausgehend von einem suchtheoretischen Modell analysieren wir die Effekte des Informationsflusses über soziale Netzwerke auf dem Arbeitsmarkt indem wir monetäre und nicht-monetäre Erträge aus Beschäftigung vergleichen, die über soziale Netzwerke und formale Wege gefunden wurden. Um kausale Effekte zu identifizieren wenden wir Propensity-Score-Matching auf Erhebungsdaten für geringqualifizierte Arbeitslose an. Mit Hilfe von Rosenbaum-Bounds können wir unbeobachtete Heterogenität in der Analyse berücksichtigen. Da der Vergleich nach Methode der Job-Findung irreführend sein kann, untersuchen wir auch den Effekt der reinen Jobsuche über soziale Netzwerke, unabhängig von der Methode der Job-Findung. Die Analyse zeigt keine Effekte sozialer Netzwerke für monetäre Erträge aus Beschäftigung und im besten Fall sehr schwache Hinweise auf kausale Effekte für nicht-monetäre Erträge.

JEL classification: J64, J31

Keywords: unemployment, social networks, job search behavior

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1 Introduction

As has been proposed, for example, by Rees (1966) and Granovetter (1974), many jobs are found through contacting social networks, i.e., asking friends, relatives and other personal contacts. Because job searches can be costly, both in terms of financial resources and time, job seekers may access their social network to receive better and faster information compared to more “formal” search strategies such as replying to employment ads or using public or private employment services. In the literature, the faster information flow within social networks and the higher quality of information provided is expected to result in both faster transitions into new jobs and transitions into better jobs (Granovetter 1974, 1995). However, the empirical research provides mixed evidence for the effect of using social networks on the quality of jobs obtained. Some papers report positive effects, such as increased wages or job satisfaction, whereas others report no or even negative effects (for overviews see Mouw 2003; Ioannides and Datcher Loury 2004; Granovetter 1995).

In this paper, we analyse the effect of social networks on the returns to job search. Following a search theoretical analysis proposed by Montgomery (1992) and extended by Franzen and Hangartner (2006), we distinguish between two theoretical mechanisms, one for monetary and one for non-monetary job outcomes. We add to the existing literature in three important regards. First, using propensity score matching, we show that for the low-skilled job seekers, there seems to be positive returns to job searches via networks, albeit not on wages but on non-monetary characteristics. Second, we show that these positive effects on both job satisfaction and on the probability of obtaining a permanent employment contract might be explained by unobserved heterogeneity, a problem common to estimating the causal effects of networks from cross-sectional data (Mouw 2003, 2006). Using Rosenbaum bounds, we show that even a low level of unobserved heterogeneity is sufficient to cast doubt on the causal interpretation of significant differences between jobs found via networks and jobs found by other means. Third, from Montgomery´s model (1992), we derive the proposition that not only obtaining a job via networks, but also merely engaging in job searches via networks, can raise (reservation) wages and therefore influence monetary returns to the job search. Testing this implication using propensity score matching, however, indicates no such effect on either monetary or non-monetary outcomes.

The paper proceeds as follows: In Section 2, we discuss two mechanisms for monetary and non-monetary job search outcomes and derive our hypotheses. The data and the methods used to test our hypotheses are presented in Section 3. Section 3.1 describes the data, and Section 3.2 introduces propensity score matching as a method for estimating the causal effect of networks on job outcomes. We also show how propensity score matching can be used to evaluate the effects of unobserved heterogeneity on these estimates. In Section 4, we present our empirical analysis; we first discuss the success of the propensity score matching in eliminating the influence of all observed covariates (Section 4.1), and then we present estimated causal effects of job searches through social networks and discuss their sensitivity to unobserved covariates and different operationalisations of the theoretical concept “search through social networks” (Section 4.2). In Section 5, we present our conclusions.

2 Theoretical Mechanisms and Hypotheses

To analyse the returns to the job search strategy, we rely on Montgomery`s (1992) re-formulation of Granovetter’s (1974) seminal thesis in terms of sequential job search theory[1] (for an overview regarding search theory, see Rogerson et al. 2005). The theory assumes that in every search period, a job seeker has to decide whether to accept or to refuse a job offer, if such a job offer is received at all. A job seeker will make this decision based on her reservation wage, meaning that she will only accept a job where the respective wage offer exceeds a certain threshold. If a job offer is not accepted, the job seeker continues searching. Job offers are received during a search period with a certain probability. This probability, or job offer arrival rate, depends upon various characteristics of the job seeker, e.g., the level of human capital. It might also depend on the search methods used by the individual. In Montgomery`s search theoretical model, the influence of two different job search methods mainly depend on a) whether the job offer arrival rates and b) the wage offer distribution differ for each method (e.g., Mouw 2003: 873 f).

The Granovetter version of Montgomery`s theoretical model assumes that social networks relay more job offers per search period (i.e., the job offer probability is higher) than do formal job search methods but that the wage distributions are equal for both methods (for empirical evidence supporting this assumption, see Koning et al. 1997). However, comparing the wages for the jobs that were located via networks and the jobs that were located through formal search methods might not be as straightforward as one would expect. Montgomery (1992) points out that if a job seeker has located her current job via formal methods, this does not exclude the possibility that additional job offers from her social network could have increased her reservation wage. Suppose that all or most job seekers use both formal searches and their social networks to collect information, and suppose that the use of personal contacts in a job search increases the probability of receiving a job offer in a given period. To understand the implications of this model, let us consider an extreme example[2]. We assume that in a given search period, an individual almost always receives a job offer through personal contacts (job offer rate close to 1) and almost never receives a job offer through formal search methods (job offer rate close to 0). As Montgomery explains, this situation can lead to a seemingly counter-intuitive empirical result:

“In the period in which an offer is accepted, an individual accepting a job through a [formal method] is thus likely to have received two offers. An individual accepting a

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[1] Montgomery uses a sequential job search model, but similar predictions can be derived from an extensive job search model (Mouw 2003: Appendices A and B) as well.

[2] This example is adapted from Montgomery 1992: 590.

job through a [personal contact], on the other hand, is likely to have received only one offer. Because the expected highest offer increases as the number of offers rises, the use of a [personal contact] implies a lower expected wage" (Montgomery 1992: 590).

Franzen and Hangartner (2006) extended Montgomery`s work with regard to non-monetary job search outcomes. In addition to the wage distribution, they assume the existence of what they call "job adequacy distribution." This distribution is different for jobs located via formal versus network searches. This difference occurs because network contacts have better information on specific job characteristics that is inaccessible through a formal search (e.g., workload and colleagues) as well as better information on the preferences of the job seeker. Network contacts can filter potential vacancies with regard to the job seeker`s preferences and the working conditions associated with the respective employer. Thus, formally, the job adequacy distribution of network jobs second-order stochastically dominates the distribution of jobs found through formal search methods. In this case, expected outcomes regarding the non-monetary aspects of jobs will be higher for network jobs than for jobs found via formal search channels (c.f. Montgomery 1992: 592)[3].

In this analysis, we will focus on a specific population, namely, on low qualified and/or long-term, unemployed job seekers. In addition to the obvious characteristic of having lower levels of human capital, this group might be different from other populations in regards to the type of social network in which they are embedded. One major difference is that the social network of unemployed job seekers might contain a relatively low number of employed persons. This situation presents a disadvantage because employed contacts are a major source of information in job searches (cf. Calvó-Armengol and Jackson 2004). If the unemployed are, to some extent, cut off from this source, there are fewer advantages to using their social network for a job search. However, a less effective network is not automatically an ineffective network. Whether using one`s social network for a job search is effective for the low qualified and/or long-term unemployed compared to using formal methods is an open question and, as such, subject to the empirical analysis presented below. Therefore, in the population of low-skilled unemployed job seekers, the same two theoretical mechanisms can be assumed to be at work.

[3] In their empirical analysis, Franzen and Hangartner (2006) focus on university graduates. Regarding the effects on non-monetary outcomes, they find positive effects for several indicators of job adequacy, which they see as an indication for the positive effect of the networks on non-monetary outcomes. For monetary outcomes, their results indicate that jobs found through social networks pay, on average, approximately 5 percent less than jobs found by other means. According to our interpretation of the Montgomery 1992 model, this percentage difference can be seen - in combination with Franzen and Hangartner´s assumptions - as an indication of a positive effect for searches via social networks. In contrast, even if Franzen and Hangartner (2006: 363) do interpret this negative difference to be consistent with the Montgomery model, they also state that "searching via social contacts has no monetary advantage" (Franzen and Hangartner 2006: 361).

Mechanism one, which Montgomery attributes to Granovetter (1974), states that a job search via social networks leads to faster information flow, i.e., more job offers per search period. However, even if offers arrive more quickly through social networks, the wages attached to these offers do not differ from those offers obtained through formal search methods. If a positive effect on the job offer arrival rate and, ultimately, on wages truly exists, in terms of empirical estimation, this effect implies that we should observe the seemingly paradoxical situation of lower wages in jobs located through networks. Our first hypothesis, therefore, is as follows:

Hypothesis 1: The use of social networks in a job search has a positive effect on wages, (paradoxically) indicated by the lower wages in jobs found via social networks.

Mechanism two states that a job search via social networks leads to better information regarding the non-monetary aspects of a job, i.e., it accesses more detailed, broader and more precise information that is not accessible via formal search channels. Consequently, the average job quality of jobs found through networks is higher than it is for jobs found through other means, leading us to the second hypothesis:

Hypothesis 2: The use of social networks in a job search has a positive effect on job quality, indicated by the higher job quality of jobs found via social networks.

What could make interpreting our empirical results difficult is that these hypotheses are grounded on very specific assumptions regarding (unobservable) wage and job quality distributions. As Montgomery (1992) notes, the expected sign when comparing wages (or other post-hiring job outcomes) for jobs found via both search methods depends on these assumptions, i.e., do formal and network search access the same wage distributions, and what is the form of these distributions (e.g., normal or lognormal) (e.g., Mouw 2003: 891)? For example, we could follow Lin (1982), (cf. Montgomery 1992: 586) and argue that the wage distribution for both search methods differs, with wages for jobs located through social networks being, on average, higher. Then, a positive effect of social networks on wages would correspond to a positive wage differential (see Montgomery 1992: 592). Additionally, if, empirically, there is only one job offer in each search period, a near-zero wage differential is also consistent with a positive effect on the information flow (see Montgomery 1992: 590). Therefore, finding results that are inconsistent with our theoretical prediction might either mean that social networks have no effect on job search outcome or that there is an effect, but our theoretical model is based on empirically unsupported assumptions.

Because of this ambiguity, Mouw (2003) generally believes that in cross-sectional data, “comparing the wages of accepted job offers is a misleading way to determine the effectiveness of job search methods if workers use multiple methods of job search” (Mouw 2003: 870). Following Montgomery`s advice (1992: 593), he argues that for social networks to have a causal effect on wages, two conditions should hold: a specific indicator of network structure (e.g., network size) should be positively correlated with the probability that the accepted job was found via personal contacts and, at the same time, should be positively correlated with the wages for this accepted job. Because no viable network characteristics are available in our data, we propose a different way to manage this problem. We argue that in addition to comparing wages in jobs found via social networks versus formal methods, a second comparison can be made that is not subject to the above ambiguity. We therefore compare wages for individuals who used networks as a job search strategy with those who did not use it, irrespective of how the current job was ultimately located. In this case, any positive effect on the job offer arrival rate (mechanism one) or the information quality (mechanism two) should be confined to the group of network searchers. With this focus on job searches, we circumvent the problem that the job finding method might be endogenous using Montgomery's (1992) definition.

3 Data and Methods

3.1 Data

In many population surveys, the number of low-skilled unemployed job seekers is low, with the additional problem that this is particularly true for the subset of low-skilled unemployed that successfully re-enter employment. In the following analysis, we use survey data that were originally collected to evaluate the success of a pilot project regarding in-work benefits in Germany (Krug 2009, 2010). This survey focuses specifically on low-skilled or long-term unemployed workers that re-entered employment. Interviews were conducted with formerly unemployed persons that started work with or without a benefit between January 2001 and August 2002 (regional pilot project phase) and between September 2002 and March 2003 (nationwide implementation). For our analysis, we focus on the subsample of approximately 1100 low-skilled and/or long-term unemployed individuals.

Another advantage of this survey is that it contains a variety of information on the persons re-entering employment, ranging from objective data on socio-demographic characteristics, employment history, household context, and individual and household income, to subjective information on attitudes about life and employment as well as job satisfaction. The survey includes extensive information on job search behaviour during unemployment and information on how the accepted job was found. However, one of the downsides of the survey is that because it was not intended to analyse network effects, we have to limit the scope of our analysis to whether a person used networks to search for a job; we are unable to assess the effect of certain network characteristics.

Because of the wide range of information, we can address several aspects regarding the monetary and non-monetary returns to job search and can distinguish between objective and subjective indicators. For objective indicators for monetary returns, we used the respondent's monthly and hourly wages in euro. Because the interviews were conducted early after leaving unemployment, the wages can be regarded as starting wages. Monthly wages will reflect whether a job is only part-time, while hourly wages will indicate the expected productivity in the new job. As a subjective indicator, we used the question, "How satisfied have you been with your earnings?" measured on a four-point scale, which we dichotomised to “not satisfied” and “satisfied.” To analyse the effect of the networks on non-monetary returns, we used questions on general job satisfaction and task satisfaction as subjective indicators (again dichotomised to "not satisfied" and "satisfied"). For more objective indicators, we used information on whether the employment contract was fixed-term or permanent and information on the employment stability, where stability is measured by the right censored duration of the job and employment status at the time of the interview.

3.2 The matching estimator for causal effects

We use propensity score matching (PSM) to estimate the returns of a job search through social networks. For the following analysis, let net_F be a dummy treatment indicator with $net_F = 1$ if the accepted job was found with the help of one`s network, and $net_F = 0$ if the job was found through other search channels. Furthermore, let jso be a variable representing monetary or non-monetary job search outcomes in the new job. Following Rubin’s Causal Model (RCM, see Rubin 1974; Holland 1986; Gangl 2010; Sobe 1995), two potential versions of the outcome variable have to be distinguished, depending on how the job was located:

$$jso = \begin{cases} jso^0, & if \;\; net_F = 0 \\ jso^1, & if \;\; net_F = 1 \end{cases} \tag{1}$$

Within this framework, one important causal effect is the average treatment effect on the treated (ATT):

$$\delta = E(jso^1 \mid net_F = 1) - E(jso^0 \mid net_F = 1) \tag{2}$$

Equation 2 compares the expected outcome in treatment status (“network job”) for those who received the treatment with the so-called counterfactual, which is the expected outcome that the same persons would have experienced if they had not received the treatment (“formally found job”). Outcome variables can be continuous (i.e., hourly wages), or they can be binary (i.e., job satisfaction).

The counterfactual expectation in equation 2 can be replaced by a factual expectation of the job search outcomes for persons in jobs found through formal methods, given covariates $\mathbf{x}$ (Holland 1986):

$$\delta = E_{\mathbf{x}}\left(E(jso^1 \mid net_F = 1, \mathbf{x}) - E(jso^0 \mid net_F = 0, \mathbf{x})\right) \tag{3}$$

A nonparametric estimator for the causal effect under conditional mean independence is the matching estimator (Rosenbaum and Rubin 1985, 1983; Heckman et al. 1998; Morgan and Harding 2006), which estimates the ATT by matching persons in a network job to persons in a formally found job with identical vectors of pre-treatment covariates $\mathbf{x}$.

The matching estimator is given by a weighted difference in means, with I_1 and I_0 indicating the persons in network jobs and regular jobs, respectively, and CS denoting the region of common support in the propensity score distributions of both groups:

$$\hat{\delta} = \frac{1}{n_1} \sum_{i \in I_1 \cap CS} jso_i^1 - \frac{1}{n_1} \sum_{i \in I_1 \cap CS} \sum_{j \in I_0 \cap CS} w(i,j)\, jso_j^0 \quad (4)$$

The number of individuals with network jobs within the region of common support is n_1, and $w(i,j)$ is the weight given to observation j when matched to observation i. Depending on the choice of $w(i,j)$, different versions of the matching estimators can be constructed. We use single-nearest neighbour matching (SNNM) without replacement, i.e., observation j is chosen as a match for observation i when j is closest to i in terms of the absolute distance of propensity scores $|P(\mathbf{x}_i) - P(\mathbf{x}_j)|$. This algorithm is chosen because the sensitivity analysis (see below) is only possible when using SNNM.

To avoid any matches where $P(\mathbf{x}_j)$ is both the nearest neighbour and very far from $P(\mathbf{x}_i)$, a maximum level of acceptable distance (calliper) has to be set. Because the covariates are balanced nonparametrically, a weighted difference in means gives the causal effect of networks on the job search outcomes that are measured as dummy or continuous variables. If the outcome variable is a function of time, as is the case when analysing employment stability, then we can combine the appropriate event history analysis - here, for example, a Cox regression - with the matching approach (Rubin 1973; Hujer et al. 1998; Ho et al. 2007). This combination is achieved by performing the regression using observations within the CS only, where $\lambda_k(t)$ denotes the hazard function for individual k, t denotes the time in employment, and $a(t)$ denotes the baseline hazard:

$$\lambda_k(t) = \alpha(t) \exp(net_{F\,k}\gamma + \mathbf{x}_k \beta), \; k \in CS, \quad (5)$$

The covariates and the treatment indicator, net, are included as independent variables, and $\exp(\gamma)$ gives the effect of the networks on employment stability in the form of a hazard ratio.

Note that the problem of unobserved heterogeneity arises if the influential variables cannot be included in the vector of the covariates $\mathbf{x}$. For example, an unobserved variable such as ability might influence wages and is simultaneously correlated with the chances of finding a job through networks. This correlation could be due to homophily (McPherson et al. 2001), where high-ability job seekers have a network consisting of high-ability persons, who in turn refer higher-wage jobs. If longitudinal data are available, such unobserved fixed confounders can be taken into account by applying a panel fixed-effect estimator (e.g., Mouw 2006). Because only cross-sectional data are available to us, as an alternative, we perform a sensitivity analysis based on propensity score matching. Although a PSM on cross-sectional data cannot directly control for unobserved heterogeneity, it allows us to perform a sensitivity analysis to determine how strong the influence of any unobserved variables must be to cast doubt on the causal interpretation of network effects. The sensitivity analysis assumes that there is a relevant, unobserved confounder u that should have been included in the estimation of the propensity score ($u=1$ for high-ability persons, $u=0$ for low-ability persons):

$$P(x_i) = P(net_F = 1 \mid \mathbf{x}_i, u_i) = \left(1 + \exp(-(\mathbf{x}_i'\boldsymbol{\beta} + \eta\; u_i)\right)^{-1} \tag{6}$$

Using a method developed by Rosenbaum (2002), we can vary the influence of this hypothetical variable (represented by the odds ratio (OR) e^{η}, where η is the respective coefficient), and determine whether any estimated effects are still significant. This test is a “worst-case scenario” (DiPrete and Gangl 2004: 15) because an unobserved relevant confounder u may not really exist, and the test assumes that the unobserved confounder leads to better job outcomes for every matched pair. However, the sensitivity test provides a good idea of how robust the results are with respect to any unobserved heterogeneity.

Because analysing differences in the wages for accepted jobs located with and without networks (treatment indicator net_F) can lead to ambiguous results (see the discussion in Section 2), we supplement our analysis by estimating the effect of an alternative treatment indicator, net_S. The indicator equals one ($net_S = 1$) for job seekers that use networks as one of their search strategies (irrespective of how the job was eventually found) and $net_S = 0$ for job seekers that do not use networks (irrespective of how the job was eventually found).

4 Empirical Analysis

4.1 Estimating the propensity score

To ensure conditional independence, the logistic regression that estimates the propensity must include all relevant covariates. Contrary to regression analysis, we must focus only on those factors that are simultaneously correlated with the probability of taking a job found through networks and with the respective monetary or non-monetary labour market outcome. Factors that influence only labour market outcomes are not necessary to ensure unbiased estimation, but they can enhance the precision of the estimates. Additionally, factors that are only weakly correlated with either the treatment or the outcome should be excluded because they have limited use in reducing bias and can inflate the variance of the estimator (Imbens 2004).

To control for all necessary covariates, we identify three types of factors that are potentially important for finding a job through networks and for returns to job search. First, we control for differences in the job search behaviour between the two groups. How many search strategies and what different search strategies an unemployed person uses will influence whether, in the end, the accepted job comes from her social network, and these strategies will also influence (reservation) wages through the number of alternative job offers received during a search period. We also control for overall unemployment duration to account for a decline in reservation wages after longer periods of unemployment, and we control for any unemployment compensation received during that time. We use the survey question of whether a respondent turned down a low-wage job offer as an indicator of a high reservation wage. We control for whether the newly employed received an in-work benefit because the prospect of receiving such a wage subsidy might have an influence on reservation wages.

Second, we take into account homophily in social network development. Homophily is the tendency of individuals to primarily interact with those who are similar to them. For example, well-qualified persons might have well-qualified friends. If well-qualified friends are more helpful in locating an open position than are poorly qualified friends, and well-qualified people receive higher average wages, irrespective of how the job was found, then this could result in a spurious correlation between wages and obtaining a job through networks. Because we do not have information on the characteristics of a person's friends, we control for several key dimensions of homophily (e.g., McPherson et al. 2001).

Third, we control for factors that influence the individual's degree of access to social capital. The better the job seeker`s ability to access social capital is, the more likely she will find a job through social networks. We identify health limitations, family income and employment-family structure as factors that could simultaneously influence one`s access to social capital and the possibility of realising high returns to job search (Boisjoly et al. 1995). We also include a dummy variable for East and West Germany to control for regional differences in the chances of finding a job.

Table 1 shows the logistic regression using all of the available covariates. In Model 1, there are only a few covariates with significant effects on obtaining a job through networks (c.f., Lin 1999: 472, Fn2). Among all of the covariates, only job search behaviour has a significant effect on whether an accepted job was found through networks. Intuitively, the more search methods one uses, the less likely it is that the accepted job will be found through networks. Additionally, whether one actively uses networks as a search method has a high influence on whether the accepted job was found through networks. This statement is not tautological because there are also persons who did not search actively through networks, but the obtained job was still referred to them by persons from their network. Other search channels do not influence the chances of obtaining a job through networks, except for "waiting for job offers from the employment agency." Model 2 restricts the covariates to those with a p-value lower than 0.6 because, similar to regression analysis, many irrelevant variables only inflate the standard errors of the matching estimator. A likelihood ratio test that compares Models 1 and 2 shows that the eliminated variables have no significant contribution. The estimation of the propensity score is therefore based on Model 2.

Table 1
Logistic regression to estimate the propensity score

	(1) Before Matching		(2) Before Matching		(3) After Matching		(4) After Matching	
	Full Model		Restricted Model		Restricted Model		Full Model	
	Odds Ratio	(stand. error)	Odds Ratio	(stand. error)	Odds Ratio	(stand. error)	Odds Ratio	(stand. error)
Job search behavior								
Search effort (number of different search strategies used)	0.645***	(0.095)	0.634***	(0.066)	1.100	(0.144)	1.226	(0.228)
Job seeker placed ad in a newspaper	1244	(0.334)	1.276	(0.309)	0.935	(0.283)	0.832	(0.287)
Asked friends, neighbors, relatives	4.689***	(1.418)	5.007***	-1381	1.090	(0.377)	0.941	(0.364)
Asked caseworker in employment agency	1.171	(0.319)	1.157	(0.287)	0.981	(0.300)	0.884	(0.291)
Computer search in database of employment agency	1.224	(0.339)	1.236	(0.299)	0.827	(0.255)	0.703	(0.248)
Unsolicited application	1.344	(0.397)	1.368	(0.374)	0.935	(0.308)	0.849	(0.307)
Waited for job offers from the employment agency	1.857***	(0.393)	1.977***	(0.348)	0.917	(0.204)	0.801	(0.216)
Other search strategies	0.941	(0.215)					0.796	(0.233)
Ever turned down employment offer because wage was too low? (Ref.: no, never)	0.871	(0.204)	0.891	(0.207)	0.896	(0.251)	0.868	(0.249)
Unemployment duration	1.011	(0.009)	1.008	(0.009)	0.998	(0.011)	0.999	(0.011)
Unemployment duration (squared)	1.000*	(0.000)	1.000*	(0.000)	1.000	(0.000)	1.000	(0.000)
Unemployment compensation before employment (Ref.: unemployment insurance)								
Unemployment benefit	0.986	(0.203)	0.983	(0.196)	1.188	(0.295)	1.140	(0.297)
No compensation	0.741	(0.184)	0.767	(0.184)	1.221	(0.371)	1.184	(0.375)
Social benefits	0.687	(0.175)	0.729	(0.178)	1.108	(0.335)	1.030	(0.326)
In-work benefit (Ref.: yes)	1.027	(0.179)					0.831	(0.180)
Dimensions of homophily								
Born in Germany (Ref.: yes)	1.081	(0.219)					1.105	(0.287)
Sex	1.038	(0.182)					0.947	(0.212)
Age	0.997	(0.014)					1.005	(0.018)
Formal education (none)								
Lower secondary school	0.923	(0.263)					0.960	(0.350)
Secondary school	1.120	(0.361)					1.107	(0.458)
Intermediate school.	1.080	(0.315)					1.073	(0.396)
Upper secondary school	1.057	(0.351)					0.818	(0.337)
Formal qualification (R: none)								
Skilled worker/technical training	0.985	(0.183)	0.958	(0.167)	0.815	(0.186)	0.852	(0.205)
Vocational training/master craftsman/technician	0.772	(0.181)	0.777	(0.164)	0.828	(0.218)	0.900	(0.268)
Job experience (years)	0.999	(0.014)					0.989	(0.017)
Family important (yes)	1.031	(0.196)					0.932	(0.221)
Leisure important (yes)	1.092	(0.183)					1.106	(0.233)
Work important (yes)	1.000	(0.155)					1.055	(0.203)

Table 1 continued

	(1) Before Matching		(2) Before Matching		(3) After Matching		(4) After Matching	
	Full Model		Restricted Model		Restricted Model		Full Model	
	Odds Ratio	(stand. error)	Odds Ratio	(stand. error)	Odds Ratio	(stand. error)	Odds Ratio	(stand. error)
Access to social capital								
Living with partner (married or not married)	1.247	(0.238)	1.272	(0.234)	0.948	(0.216)	0.943	(0.225)
Children in Household	0.910	(0.071)	0.916	(0.065)	1.031	(0.092)	0.994	(0.098)
Net household income before employment	1.046	(0.038)	1.048	(0.037)	1.025	(0.046)	1.034	(0.048)
Net household income before employment (squared)	0.999	(0.001)	0.999	(0.001)	0.999	(0.001)	0.999	(0.001)
Health problems (Ref.: none)								
Health problems without impact on employment chances	1.043	(0.283)					0.756	(0.240)
Health problems with impact on employment chances	1.053	(0.281)					1.434	(0.530)
West Germany (Ref.: yes)	1.184	(0.228)	1.113	(0.195)	0.970	(0.224)	0.942	(0.242)
Pseudo R2	0.0613		0.0598		0.0048		0.0116	
Prob > chi2	0.0001		0.0000		10000		10000	
N	1107		1119		524		520	

Likelihood ratio (LR)- Test of model 1 vs. model 2 (based on 1107 cases from model 1): LR chi2(15) = 1.99, Prob > chi2 =1.00; * p<0.10, ** p<0.05, *** p<0.01

Models 3 and 4 perform a simple multivariate test to determine whether matching eliminated the influence of the covariates. The test re-estimates Models 1 and 2 after the propensity score matching took place. As observed in Model 3, all formerly significant covariates become insignificant. Additionally, Model 4 ensures that in the matched sample, the formerly irrelevant variables remain irrelevant. A more detailed analysis of the influence of covariates before and after the matching can be found in in the Appendix. In this Table, we show that after matching the persons in network and regular jobs, there are no significant bivariate differences in the means for any of the covariates used in the matching procedure. Having established the quality of our matching procedure, we can use the matched sample to estimate the causal effect of the networks on returns to job search.

4.2 The causal effect of networks on returns to job search

Table 2 reports the results from SNNM without replacement, with a very strict calliper of 0.005. First, let us consider the unadjusted differences in average job outcomes between persons in network jobs and in regular jobs. Persons who obtained jobs through networks seem to have, on average, better job outcomes. Because the differences are measured before controlling for any covariates, that is, without considering differences in the composition of the job seekers, they are what a “naïve” observer might see when comparing network jobs with others. We see that jobs obtained through networks tend to be characterised by a monthly wage of approximately 100 euro higher than other jobs; hourly wages are, on average, approximately 80 eurocents higher. However, this difference in wages is not reflected by differences in wage satisfaction, which is approximately 5 percent, but is not statistically significant.

Assuming the conditional independence assumption holds, the matching procedure eliminates all compositional differences between job seekers ending up in network or regular jobs. Differences in the means after the matching can therefore be interpreted as causal effects. From Table 2, we can see that the empirical evidence does not support our theoretically derived prediction of a negative difference. The monetary returns point estimates basically remain positive after controlling for the covariates, but they become smaller and statistically insignificant. Thus, we must conclude that there appears to be no causal effect of using networks on the monetary returns to job search.

With regard to non-monetary outcomes before the matching, persons who located their job through networks tend to be more satisfied with their jobs and their specific job tasks. Additionally, persons who obtained their job through networks are significantly more likely to be employed with a permanent contract, with a difference of approximately 16 percent. As is indicated by a hazard ratio well below 1, network jobs tend to be significantly more stable.

Table 2
Unadjusted differences and causal effects of networks on job search outcomes: job finding via networks (net_F)

	Before matching			After matching		
	Unadjusted difference	**Standard error**	**Number of treated / controls**	**Causal effect**	**Standard error**	**Number of treated / controls**
Hypothesis 1: Monetary outcome						
Monthly gross wages (euro)	99.28**	43.62	216 / 654	23.22	54.48	195 / 195
Hourly gross wages (euro)	0.782*	0.442	215 / 652	0.738	0.722	194 / 194
Satisfied with wage (Dummy, 1 if yes)	0.053	0.034	285 / 834	0.008	0.044	262 / 262
Hypothesis 2: Non-monetary outcomes						
Satisfied with job (Dummy, 1 if yes)	0.077***	0.027	285 / 834	0.065**	0.032	262 / 262
Satisfied with task (Dummy, 1 if yes)	0.046*	0.028	285 / 834	0.023	0.033	262 / 262
Permanent contract (Dummy, 1 if yes)	0.159*	0.092	285 / 834	0.198*	0.109	262 / 262
Employment stability (Hazard ratio from Cox-Regression)	0.693***	0.091	281 / 829	0.798	0.135	259 / 259

Single nearest neighbor matching, no replacement, caliper 0.005; * p<0.10, ** p<0.05, *** p<0.01; propensity score matching performed in Stata using psmatch2 (Leuven, Sianesi 2003).

In contrast to monetary outcomes, effects on non-monetary outcomes partly remain significant even after controlling for observed covariates. The share of employees who are satisfied with their job is 6.5 percent higher in network jobs than in regular jobs. In contrast, persons in network jobs are not more likely to be satisfied with the specific tasks in their jobs, and the effect of employment stability in network jobs also disappears. Additionally, there is a significant negative effect of 20 percent on the likelihood of locating a job with a permanent contract. This finding indicates that, at least for some non-monetary outcomes, Hypothesis 2 is supported by the data.

However, we still must address the problem of unobserved heterogeneity. To do so, we simulate an unobserved variable and vary its influence on obtaining a network or a regular job. The result is shown in Table 3. The second column repeats the causal effects from Table 2. Going from left to right, we first simulate a situation with no unobserved heterogeneity, which provides the results already reported in Table 2, where there are no significant effects on monetary returns and significant effects on job satisfaction and permanent contracts. Next, we assume that we have unobserved heterogeneity of former job seekers in network and regular jobs (reflected by variable u in equation 8). This unobserved heterogeneity can be due to unobserved abilities that influence wages or to whether employers only offer a fixed-term contract. The heterogeneity might also come from a personal characteristic, such as optimism, which can influence job satisfaction. If we assume that such a characteristic has only a small influence on whether job seekers end up in a network job (OR=1.1), we see that the p-values for the effect on wages become even larger. As far as the non-monetary returns are concerned, the effect on job satisfaction would still be significant with a p-value of 0.07, whereas the effect on fixed-term contracts, even if initially quite large, becomes insignificant. Under the assumption of a medium influence (OR = 1.2) of the unobserved variable u, the effect on job satisfaction becomes insignificant as well. Unfortunately, because employment stability is measured as a right-censured duration variable, the sensitivity analysis is not applicable.

Because the positive effects of networks on non-monetary outcomes are very sensitive to the influence of unobserved heterogeneity, we must interpret the evidence for Hypothesis 2 carefully. The observed differences might reflect causal effects, but if there is an important unobserved variable with only a small or medium influence, these differences must be regarded as spurious and not causal. Of course, our sensitivity analysis does not inform us as to whether such an important variable exists. Regardless, one cannot consider that the evidence in support of Hypothesis 2 is very strong.

Table 3
Sensitivity test of the causal effects towards unobserved heterogeneity

	Causal effect	P-values for the causal effect, assuming …			
		… no unobserved heterogeneity	… a low level (OR=1.1) of unobserved heterogeneity	… a medium level (OR=1.2) of unobserved heterogeneity	… a high level (OR=1.3) of unobserved heterogeneity
Hypothesis 1: Monetary outcome					
Monthly gross wages (euro)	23.22	0.405	0.631	0.806	0.911
Hourly gross wages (euro)	0.738	0.219	0.421	0.627	0.790
Satisfied with wage (Dummy, 1 if yes)	0.008	0.465	0.389	0.217	0.108
Hypothesis 2: Non-monetary outcomes					
Satisfied with job (Dummy, 1 if yes)	0.065	0.030	0.070	0.134	0.221
Satisfied with task (Dummy, 1 if yes)	0.023	0.281	0.433	0.510	0.375
Permanent contract (Dummy, 1 if yes)	0.198	0.046	0.126	0.256	0.418
Employment stability (Hazard ratio from Cox-Regression)	0.798	n.a.	n.a.	n.a.	n.a.

Single nearest neighbor matching, no replacement, caliper 0.005; * p<0.10, ** p<0.05, *** p<0.01; n.a.: not available; p-values calculated in Stata using rbounds (Gangl 2004) for continuous outcomes and mhbounds (Becker and Calliendo 2006) for dichotomous outcomes.

In addition to the problem of unobserved heterogeneity, our estimation strategy has to cope with the aforementioned ambiguity of any effects on wages. The observation of no significant difference between wages in network and regular jobs does not prove the absence of a causal effect. To address this problem, we conducted a propensity score matching with a different treatment indicator. The treatment is now defined as using networks as a search strategy versus not using them. In this case, the control group consists of persons that potentially used any search strategy other than networks. Because of the small number of individuals in the control group, we had to broaden the calliper to 0.01 for single nearest neighbour matching without replacement. Table 4 shows no significant effect for search through networks on either hourly or monthly wages. Note that the point estimates are close to zero or even negative. This result indicates that using networks as a job search strategy, at least for the low-skilled unemployed, does not seem to lead to higher reservation wages. The reason for this result can either be that networks do not increase the number of job offers for this population or that the number of job offers, contrary to job search theory, do not influence reservation wages.

Table 4
Unadjusted differences and causal effects of networks on job search outcomes: job search via networks (net_S)

	Before matching			After matching		
	Unadjusted difference	**Standard error**	**Number of treated / controls**	**Causal effect**	**Standard error**	**Number of treated / controls**
Hypothesis 1: Monetary outcome						
Monthly gross wages (Euro)	-99.925**	50.563	716 / 146	-37.213	71.297	137 / 137
Hourly gross wages (Euro)	0.082	0.513	713 / 146	-0.002	0.457	137 / 137
Satisfied with wage (Dummy, 1 if yes)	0.028	0.040	924 / 183	0.065	0.054	169 / 169
Hypothesis 2: Non-monetary outcomes						
Satisfied with job (Dummy, 1 if yes)	-0.022	0.032	924 / 183	0.012	0.042	169 / 169
Satisfied with task (Dummy, 1 if yes)	-0.022	0.033	924 / 183	0.006	0.042	169 / 169
Permanent contract (Dummy,1 if yes)	0.046	0.109	924 / 183	0.071	0.142	169 / 169
Employment stability (Hazard ratio from Cox-Regression)	0.809	0.110	916 / 182	0.723*	0.139	168 / 168

Single nearest neighbor matching, no replacement, caliper 0.01; * p<0.10, ** p<0.05, *** p<0.01; propensity score matching performed in Stata using psmatch2 (Leuven and Sianesi 2003).

In addition to these effects on monetary outcomes, we also present the effects of a job search through networks on non-monetary returns in Table 4. There are no effects for a job search through networks on any of the non-monetary outcomes, except for employment stability. Because significance is indicated merely at the 10 percent level and because we have no theoretical explanation for this effect, it should be interpreted with caution.

5 Conclusions

The aim of this paper was to test the effect of social networks on the monetary and non-monetary outcomes of a job search for formerly unemployed, low-skilled job seekers. After controlling for several observed covariates, we found that networks had no effects on monetary outcomes such as wages or wage satisfaction. This result does not change if we use a sensitivity analysis to take into account the existence of unobserved differences in reservation wages. Acknowledging Mouw`s (2003) warning that finding a job via social networks might be endogenous, we also analyse the effect of a search through networks in general instead of focusing on the effect of how the accepted job was found. The results from this specification do not indicate any significant effects from using social networks on wages. In contrast to our theoretical expectations, social networks do not seem to transmit more or faster information than a formal job search. This result could be due to the specific situation of the unemployed in Germany. All of the unemployed in Germany must register at their local employment agency to receive unemployment benefits. Because employment agencies also deliver placement services and inform the unemployed about job vacancies, more and faster information might flow via this formal channel. This hypothesis is consistent with Pellizzari (2010), who argues that social networks are only as effective as local employment agencies are ineffective.

Similar results are found for the non-monetary outcomes of a job search, even if, at first, the results from the propensity score matching show significant effects for social networks on two of our four non-monetary outcomes (job satisfaction and permanent contract, but not for task satisfaction and employment stability). A sensitivity analysis, however, shows that neither the positive effect on obtaining a permanent contract nor the positive effect on job satisfaction is very robust. If we assume the existence of unobserved heterogeneity with a small or medium influence, then the positive effects disappear. In addition, we took into account the potential endogeneity of job finding method by replacing it with job search method. Our results indicate that there is no effect of information flow via social networks on either monetary or non-monetary outcomes.

Faced with these results, one might ask why job seekers engage in job searches via social networks at all. An answer to this question is beyond the scope of this paper. However, scholars are discussing other advantages of networks in addition to information flow (cf. Sandefur and Lauman 1998). For example, in addition to information, influence is discussed as a potentially important mechanism by which networks might be helpful in a job search (Lin 1999). However, Davern (1999) as well as Davern and Hachen (2006) find no evidence for either the effects of information or the effects of influence, at least for the information/influence indicators they used. Another mechanism might be connected to social solidarity or social enrichment. For example, in their case study, Fernandez et al. (2000) found no evidence for informational benefits, similar to the results presented here. They did find, however, that recruitment via social contacts leads to social enrichment in the work place. They also found that employers benefit from their employee`s social networks because the pool of applicants is enriched. They even found that using referrals significantly reduces screening costs because referrals were more appropriate for the job at application. This finding draws attention to the fact that the benefits from searches via social networks are not restricted to job seekers alone, but might also, or even primarily, exist for the employing firms (cf. Rebien 2010; Holzer,1996). Indeed, one way to look at our results is that job search via social networks mainly measures potential effects of networks on information flow. In contrast, job finding via social networks measures information flow as well as other mechanisms, for example social enrichment. Here benefits from social networks can only be realized, if a job seeker actually accepts the job found via her network, explaining positive and significant effects from job finding via networks that disappear when we switch to job search via social networks.

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Appendix

Table
Balancing table for the sample before and after propensity score matching

Variable	Sample (before or after matching)	Accepted job located through … Social network	Accepted job located through … Formal job search	Standardized bias (%)	p-value
Search effort (number of different search strategies used)	Before	5.98	6,20	-15,90	0,02
	After	6.13	6,05	5,40	0,52
Job seeker placed ad in a newspaper	Before	0.12	0,17	-12,80	0,07
	After	0.13	0,13	1,10	0,90
Asked friends, neighbors, relatives	Before	0.92	0,81	33,40	0,00
	After	0.91	0,89	5,60	0,46
Asked caseworker in employment agency	Before	0.85	0,88	-10,20	0,13
	After	0.87	0,86	2,20	0,80
Computer search in data base of employment agency	Before	0.83	0,87	-11,50	0,09
	After	0.86	0,87	-2,10	0,80
Unsolicited application	Before	0.89	0,92	-9,30	0,16
	After	0.90	0,89	2,60	0,78
Waited for job offer from employment agency	Before	0.44	0,39	10,40	0,13
	After	0.43	0,44	-0,80	0,93
Ever turned down employment offer because wage was too low?	Before	0.10	0,12	-5,00	0,47
	After	0.11	0,12	-4,90	0,58
Unemployment duration	Before	20.13	24,42	-16,20	0,03
	After	20.91	20,70	0,80	0,92
Unemployment duration (squared)	Before	911.69	1482,90	-18,50	0,01
	After	974.13	946,85	0,90	0,89
Unemployment insurance benefit	Before	0.28	0,23	10,50	0,12
	After	0.27	0,29	-6,10	0,50
Unemployment benefit	Before	0.44	0,45	-1,30	0,85
	After	0.45	0,43	3,80	0,66
No unemployment compensation	Before	0.14	0,15	-2,40	0,73
	After	0.14	0,13	1,10	0,90
Social benefits	Before	0.14	0,17	-8,60	0,22
	After	0.15	0,14	1,10	0,90
No formal qualification	Before	0.36	0,33	6,40	0,35
	After	0.36	0,32	7,20	0,41
Skilled worker/technical training	Before	0.47	0,47	-0,20	0,98
	After	0.46	0,48	-4,60	0,60
Vocational training/master/craftsman/ technician	Before	0.17	0,20	-7,60	0,28
	After	0.18	0,19	-2,90	0,74
Living with partner (married or not married)	Before	0.29	0,24	12,40	0,07
	After	0.28	0,28	-0,90	0,92
Children in Household	Before	1.03	1,14	-9,90	0,16
	After	1.09	1,04	4,50	0,60
Net household income before employment	Before	13.30	12,96	4,90	0,48
	After	13.14	13,11	0,50	0,96
Net household income before employment (squared)	Before	217.79	219,87	-0,70	0,92
	After	213.70	216,27	-0,90	0,90
West Germany (Ref.: yes)	Before	0.64	0,62	5,10	0,46
	After	0.63	0,64	-0,80	0,93

Note: reported p-value is from a test for the equality of means; standardized bias computed as difference in means divided by the average between the respective variances of the covariate. There is no critical value but as a rule values of below 4-5 can be considered as indicating satisfactory balance (e.g. Caliendo and Hujer, 2006).

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