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Does social capital affect wages? A panel data analysis of causal mechanisms

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Abstract

Many studies document the positive association between accessed social capital and wages. It is widely accepted that the underlying relationship is causal. However, most studies use cross-sectional data, and only a few test causal mechanisms. In our analysis, we first test a broad range of social capital indicators by applying fixed-effects panel data regression to a sample of currently employed and a sample of newly employed individuals. Second, we test reservation wages, network search, being offered a job without prior job search, and the number of job interviews as some of the theoretical mechanisms put forward to explain positive social capital effects. Overall, we find no empirical evidence for wage effects of the social capital measure and no evidence that any of the proposed mechanisms are empirically relevant.

Keywords: social capital; wages; social networks; mediation; panel data; causal path; mechanism

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1. Introduction

Social capital is unequally distributed among members of society (McPherson et al. 1982; van Tubergen and Voelker 2015; Benton 2016). Portes (1998:6–7) defines social capital as “the ability of actors to secure benefits by virtue of membership in social networks or other social structures”. According to theorists such as Coleman (1988) and Lin (2000), social capital is productive in that it helps individuals achieve goals that would otherwise be unattainable, only partially attainable, or would require considerably more effort to achieve. Theoretical and empirical assessments of the benefits of social capital have been presented in many research areas, where much research has also been devoted to the importance of social capital in the job search process, especially with regard to increasing wages (Castilla et al. 2013). This comprehensive literature can be broadly divided into research focusing on so-called mobilized social capital on the one hand and accessed social capital on the other (Lai et al. 1998). Whereas mobilized social capital refers to the actual use of social contacts to further one’s interests, accessed social capital refers to the resource opportunity structure for the potential use of social capital irrespective of its actual use, i.e., to the social resources embedded in routine networks. In this context, one important field of research focuses on whether and how social capital is useful in obtaining higher-wage jobs. Whereas the literature on mobilized social capital and wages is inconsistent, ranging from positive effects to no effects to negative effects, research on accessed social capital consistently reports positive effects on wages and only a few null effects (Chen 2011).

This paper aims to contribute to the existing literature on accessed social capital in several important ways. First, we aim to test a broad range of theoretically distinct social capital measures: the number of weak and strong ties in the network (Granovetter 1973), the number of employed friends (Calvo-Armengol and Jackson 2007), the availability of useful social resources (van der Gaag and Snijders 2005) and access to influential social positions (Lin and Dumin 1986). With this approach, we go well beyond the social capital variables that have so far been used in the literature to analyze wage effects. Second, we also investigate causal mechanisms. To do this, we estimate the indirect effects of social capital via intermediate outcomes not yet tested systematically in the literature. These intermediate outcomes are increased reservation wages (Mouw 2003), actively asking the network for help (Marin 2011), job finding without active job search (McDonald 2015), and invitations to job interviews (e.g., Fernandez and Weinberg 1997). To test our hypotheses, we use longitudinal data from a panel study and account for unobserved heterogeneity bias by way of fixed effects regression, something not typically done in the literature.

The remainder of this paper proceeds as follows. Section 2 discusses theoretical mechanisms and derives our hypotheses on wage effects. Section 3 reviews the empirical literature on accessed social capital and wages. Section 4 discusses some limitations of current practices. Section 5 introduces the data used in this article and discusses measures and analytical samples as well as the use of fixed effects regressions

as our estimation method. Section 5 presents the results of our analysis. Finally, section 6 addresses the limitations of our study and draws some conclusions from our analysis.

2. Theoretical considerations and research hypotheses

Nan Lin (2000) conceptualizes social capital as the quality and quantity of social resources an actor can access through his or her location in a social network. In contrast to individual resources, social resources are not held by the actors themselves but by members of their social networks. In job search, such “second order resources” (Flap and Voelker 2001) can be used to obtain higher-wage jobs than would be possible based on the job seeker’s individual resources alone. According to the model of multiple job search methods (Montgomery 1992; Mouw 2003; Krug 2012), there are two main pathways by which social capital can increase wages. One pathway is that social capital increases a job seeker’s probability of receiving a job offer. More job offers means that job seekers can be more selective regarding which offers they accept, especially in terms of wages. The literature mainly assumes social capital to have a positive impact on job offer probability by providing access to two important social resources, information and influence (Trimble and Kmec 2011). Well-embedded job seekers are likely to receive information not only via formal job search methods but also from personal contacts (Granovetter 1974). Information disseminates via networks much faster than via other sources (Lai and Wong 2002); networks may also contain details not available elsewhere if personal contacts have “knowledge about the precise job requirements and procedure for applying” (Lin, Ensel, and Vaughn 1981:394). Furthermore, information received from personal contacts is often judged as especially trustworthy (Granovetter 2005). According to Montgomery (1991), firms can use referrals as information signaling the high ability of the applicant, especially if the referring incumbent employee is highly valued in the firm. Second, in addition to merely providing information, members of the job seeker’s social network can sometimes exert influence on the firm’s decision-making process when looking to fill a vacancy. For example, Goldberg (1982) argued that firms sometimes use nepotistic hiring practices to grant nonmonetary benefits to their employees.

A second pathway works directly via wage differences between jobs obtained with or without the support of one’s social network. Again, information and influence are the resources most commonly mentioned in this context. Employers are usually only incompletely informed about applicants’ productivity and work motivation, which causes them to pay lower starting wages until that information is revealed on the job (Jovanovic 1979). Saloner (1985) and Simon and Warner (1992) assume that employers use referrals via personal contacts to reduce this uncertainty even before hiring the applicant, thus allowing them to pay higher wages from the start. Another explanation for higher wages found in the literature is that personal contacts will exert their influence in such a way such that job seekers will be offered jobs for which they are not fully qualified (Beaman and Magruder 2012).

However, according to the literature, different aspects of the social network are associated with a higher or lower potential to provide social resource information and influence. According to Granovetter (1973; Wiese and Tutic 2015), tie strength plays an important role in successfully searching for a job. Strong ties, he argues, are highly motivated to provide social support but are unlikely to move in circles that are different from one's own and, thus, are unlikely to convey information that one does not already possess. Weak ties, however, provide job seekers with access to nonredundant information. Burt (1992) argues that weak ties are useful because they are more likely to connect networks that would otherwise be separated. In addition, Lin (2001) argues that the strength of weak ties might be primarily attributable to their higher probability of linking job seekers to people in high-status positions. Therefore, our first hypothesis is the following.

Hypothesis 1a: Job seekers with more weak ties will obtain higher wages.

Other theoretical positions point out the advantages of strong ties. Wegener (1991) argues that strong ties are useful, especially for low-status job seekers, and Bian (1997) argues that strong ties are very helpful for reasons other than the transmission of information: "strong ties of trust and obligation may be more advantageous in accessing influence, which is generally more costly and difficult to obtain" (Bian 1997:367). Aral and Van Alyster (2011), in contrast, have argued that strong ties transmit a greater volume of information than weak ties. Thus, even if the proportion of nonredundant information via strong ties might be small, the higher volume of information in total leads to more nonredundant information via strong ties than via weak ties. Therefore, we can expect that strong ties provide advantages with respect to wages, too, and our second hypothesis is:

Hypothesis 1b: Job seekers with stronger ties will obtain higher wages.

Whereas sociological research focuses on tie strength as a correlate of social resources, economists have pointed out the importance of network members' employment status, especially for passing on information. For example, in the theoretical model developed by Calvo-Armengol and Jackson (2007), it is employed network members who are more likely to be well informed concerning job vacancies. In addition, compared to unemployed contacts, they are less likely to utilize this information for themselves and, thus, more likely to pass it on. Therefore, the third hypothesis we test is the following:

Hypothesis 1c: Job seekers with more employed personal contacts will obtain higher wages.

According to Lin and Dumin (1986), the amount of information and influence-based resources embedded in the social network depends on the number of network members with high-prestige occupations (such as doctors and architects). However, the occupations that are helpful for job-finding depend on the job seeker's own occupation and/or intention to change occupations. Therefore, Yakubovich (2005) argues that contact with employers can be particularly helpful. They either make the hiring decision or refer the job seeker to other employers. As our fourth hypothesis, we are therefore testing the following proposition:

Hypothesis 1d: Job seekers with more influential network members will obtain higher wages.

Instead of focusing on the correlates of information and influence, other researchers propose focusing on the availability of social resources in the job seeker's social network (van der Gaag and Snijders 2005). According to this view, individuals can use their social capital to obtain instrumental support; in the case of job search, for example, this could take the form of receiving job leads or getting help with applications.

Hypothesis 1e: Job seekers with more job search resources embedded in their network members will obtain higher wages.

In addition to which aspect of social capital will influence wages, an important question is how this influence comes about. Several ideas have been proposed, some of which we can test in this article. First, starting with reservation wages, Montgomery (1992) and Mouw (2003) hypothesized that job seekers' wage gains through social networks are due to higher reservation wages. The reservation wage is defined as the minimum acceptable wage, set by the job seeker, where all wage offers below this threshold are rejected and the job seeker continues the job search (Mortensen 1986). Montgomery (1992) argued that those individuals with more social capital will be aware of their network's ability to provide them with better chances to obtain high-wage job offers. As a consequence, these job seekers will set higher reservation wages, i.e., they will become more demanding with regard to what jobs they accept and when to refuse and continue searching. Therefore, our first mechanism hypothesis is as follows.

Hypothesis 2a: Reservation wages mediate the effect of social capital on wages.

According to economic job search theory, job seekers will decide which job search method to use based on their expectations that this method will yield more and better job offers (Lai and Wong 2002). It follows that having a high amount of social capital will increase the probability of using one's network for job search in the sense of actively asking friends and acquaintances for job leads. Being asking for help, in turn, is one of the key factors for network members to actually provide such help. According to Marin (2011), a key obstacle for network members to provide help during job search is that they are insecure as to whether the job seeker actually wants or needs their help. From this perspective, asking for job leads will increase the likelihood of receiving job search support that results in a job offer. For example, Trimble O'Connor (2013) found that if asked, almost all contacts are able to provide help (92 percent). Therefore, our next hypothesis is the following.

Hypothesis 2b: Asking for help mediates the effect of social capital on wages.

In contrast to the preceding argument, Granovetter posited that one of the main advantages of networks might be that they allow individuals to find new jobs without actually asking for job leads (Granovetter 1995:33,145), a phenomenon often called "nonsearching" (McDonald 2015) or "the invisible hand" of social capital (Lin and Ao 2008). Proponents of this perspective argue that those who receive unsolicited job leads must, by definition, have better and more resourceful networks than others (McDonald and

Elder 2006). The reason is that anyone can ask their contacts for job leads, but only those select few with especially high social capital will receive offers from their network without asking for them (e.g., McDonald, Benton, and Warner 2012). Thus, our third hypothesis is the following:

Hypothesis 2c: Nonsearch mediates the effect of social capital on wages.

Finally, most employers use job interviews to screen applicants. Social capital theorists often assume that personal contacts from the network are helpful in obtaining job interviews, either because incumbent employees of the firm are putting in a good word or because they help job seekers write more appropriate resumes (Fernandez and Weinberg 1997). If job seekers are more likely to get a job interview, they might be able to become more selective regarding the jobs they accept, thus choosing higher-wage jobs. Our next hypothesis is therefore the following.

Hypothesis 2d: The number of job interviews mediates the effect of social capital on wages.

3. State of current empirical research

Among those studies relying on accessed social capital, most focus on either the amount of weak or strong tie capital or both. Weak-tie social capital is often measured via an individual's membership in voluntary organizations. Such studies consistently report positive wage effects (Boxman et al. 1991; Tassier 2006; Knight and Yueh 2008; Ruiter and De Graaf 2009; Hermann and Kopasz 2011; Matsunaga 2015). Other measures for weak tie social capital are rare. Obukhova and Zhang (2017) find that among the five persons who could have helped in finding a job, weak ties measured as the number of distant contacts do not impact wages, whereas if measured as the the number of acquaintances a positive effect on wages is observed.

Authors have applied various measures to investigate the effect of strong-tie social capital on wages. Hermann and Kopasz (2011) use two indicators of strong-tie social capital and find positive effects for the intensity of contact with friends and for the intensity of contact with relatives (the latter only for men). Knight and Yueh (2008) also find positive effects of the number of strong ties on wages, whereas Tassier (2006) finds no effect of the number of persons with whom important matters were discussed but positive effects of the (estimated) number of friends' friends. Obukhova and Zhang (2017) find no effect for the number of close contacts and marginally significant but negative effects of the number of friends.

In contrast to tie strength, the number of employed friends has been the focus of empirical research only in two studies that we are aware of. Analyzing panel data from a general population sample, Capellari and Tatsiramos (2015) found that the number of employed persons among an individual's three most important contacts has a positive effect on wages. In contrast, Mouw (2003) analyzed data on individuals in urban areas and found no wage effect of the proportion of unemployed among the three closest friends.

The results for accessed influential positions are equally scarce and focus on occupational characteristics only. Behtoui (2007) used a position generator (Lin and Dumin, 1986) to construct a measure that combines several characteristics of the occupations accessible to the individual into one single factor, finding that this measure of social capital significantly influences the probability of being in the highest wage category. Analyzing a sample of 438 youths, Behtoui (2016) finds that the average status of their contacts has a positive impact on a combined measure of wages and job prestige. We are not aware of research that analyzes the effect of the availability of specific social support resources on wages, be they job-search related or other resources.

We are also not aware of studies that analyze the causal connection between accessed social capital, any of our four mechanism variables (reservation wages, nonsearch, network activation and the number of job interviews) and wages, although researchers have presented evidence for parts of the respective causal chains. For example, Caliendo et al. (2011) analyze the effects of network size on reservation wages and find them significantly positive. In contrast to our analysis, Caliendo et al. (2011) do not, however, present estimates of effects on actual wages or control for unobserved confounders. Mouw (2003) only presents circumstantial evidence from a fixed-effects regression by arguing that the observed null effect of job-finding via personal contacts is due to the differencing out of reservation wages.

Regarding nonsearch, empirical research finds that wages are higher for those who located their job without actively searching for a job (McDonald 2015). It has been argued that the reason is that those job seekers who have more social capital are more likely to find their job without actively looking for one, but we are not aware of a direct test of this proposition, i.e., one based on network or social capital information.

With respect to asking for help in job search, Marin (2011) finds that contacts hesitate to provide information on vacancies if they are unsure whether job seekers are actually seeking such information. Bonoli and Turtschi (2015) find that job seekers who searched through their personal contacts during the seven days prior to a survey interview were more likely to find a job. Krug and Rebien (2012) find, however, that those who searched through their network at any point during their unemployment did not receive higher wages compared to those who only used formal methods.

Barbulescu (2015) finds that network size has no impact on the number of job interviews. In contrast, compared to formal searching, searching via contacts is more likely to result in job interviews. Furthermore, job offers received via contacts are more likely to be accepted, indicating superior job quality (Obukhova and Lan 2013). However, Franzen and Hangartner (2006) find that individuals who secure their jobs via their networks experience a lower number of job interviews, possibly due to shorter job search durations. Fernandez and Weinberg (1997) find that not only do referred applicants time their applications better, they are also more likely to be granted a job interview. Again, we are not aware of research on the causal connection among social capital, job interviews and wages.

4. Limitations of current empirical research

Studies on accessed social capital predominantly report positive wage coefficients, mostly interpreting this finding as evidence for causal effects. However, these studies also have limitations that open them up to criticism (see also Mouw 2003 and Chen 2011).

First, several authors have argued that from a methodological perspective, homophily in network formation is an alternative explanation for positive wage coefficients. Homophily is defined as “the principle that a contact between similar people occurs at a higher rate than among dissimilar people” (McPherson et al. 2001:416). If individuals tend to associate with similar others, a positive correlation between their characteristics and the characteristics of individuals in the network arises, even if networks play no role in their job search (Mouw 2003; Shalizi and Thomas 2011; Angrist 2014). According to Mouw (2006), accounting for unobserved heterogeneity is one way to prevent homophily from biasing causal estimates in wage regressions. He proposed, among others, fixed-effects panel data regression as a particularly suitable method to achieve this goal. However, none of the social capital studies cited above account for unobserved confounders by way of fixed-effects regression. Therefore, the question remains open whether unexplained confounders that induce homophily in social networks are responsible for the observed positive wage coefficients. In this article, we contribute to the literature by testing the wage effect of social capital by way of fixed effects regressions.

Second, most studies are based on observations from individuals who are employed at the time of the survey. Such analyses are in danger of confounding the social capital effects on wages with effects in the reverse direction (cf., Ruiter and DeGraaf 2008:430). If individuals have already been employed for several years at the time of the survey, the amount of accessed social capital could be a consequence instead of a determinant of their wages. For example, those who obtained jobs with higher wages usually have a higher probability of having more employed friends or high-status contacts. This relationship could result from their job (e.g., through frequently meeting with their clients) or simply because their higher income makes them more attractive to network with. Consequently, among the employed, a positive but spurious correlation between social capital and wages can exist at the time of the survey but be absent during job search. To avoid such reverse causality bias, we test our hypotheses not only on a sample of currently employed workers but also on a sample of newly employed workers. In the later sample, we can use the panel data to measure social capital as well as the covariate *before* workers entered their new job.

Third, a major disadvantage of the accessed social capital approach is that it focuses on the structure of the network and on resources that have the potential to further job seekers’ interests (Campbell et al. 1986:100), but the degree to which job seekers actually use these resources to further their interests is unclear. Thus, another limitation of previous research is the lack of studies that test the causal mechanisms connecting social capital to job search outcomes, in our case to wages. We take a first step toward closing this gap by testing four mechanism hypotheses that researchers have proposed to explain

the wage benefits derived from social capital. According to Goldthorpe (2001), such tests could further our understanding of the causal nature of the relationship because it helps us to explain how the effect is brought about. Accounting for causal mechanisms therefore strengthens our trust in the results of statistical correlation analyses. If, for example, we were to find a positive wage coefficient of social capital, the simultaneous finding of a significant indirect effect via any of the mechanisms will increase, and its absence will reduce our confidence in the causal interpretation of the coefficient.

5. Data and Method

To test our hypotheses, we use the German Panel Study "Labour Market and Social Security" (PASS). PASS is a large-scale household panel survey conducted by the Institute for Employment Research (Trappmann et al. 2019). At the time we composed this article, twelve waves (2007–2018) were available for research. PASS uses a dual frame for sampling: The first subsample is drawn from national registers of welfare benefit recipients, and the second subsample is a probability sample of the German residential population. Both samples have been refreshed since the start of the panel to represent new population entries and to regain statistical power lost due to attrition. The questionnaire focuses on labor market-related issues and on transfer payments as well as on the material and social conditions of households and on social inclusion.

5.1 Measures

Our main dependent variable is logarithmized hourly wages. There are two kinds of focal independent variables, i.e., accessed social capital and mechanism variables. Starting with the social capital variables, our first indicator focuses on access to weak ties, measured as the number of active memberships in five different types of voluntary organizations, an operationalization common in the literature (see, e.g., Ruiter and DeGraaf 2003; Krug and Prechsl 2019; Krug et al. 2019). Respondents could choose all applicable options from a list comprising organization types unions, political parties, church communities, clubs, or a residual category.

Our second measure of accessed social capital is the natural log number of close friends or family members outside the household¹. The number of close friends measures a part of the personal network that is characterized by frequent contact, high density (i.e., redundancy) of ties, a high degree of homophily and a large proportion of relatives. This operationalization is also rather common in the literature (see, e.g., Hermann and Kopasz 2011; Krug and Prechsl 2019; Krug et al. 2019).

¹ To avoid negative values, we added 1 before logarithmizing the number of strong ties. Thus, a value of 0 in the logarithmized variable reflects 0 strong ties. We used the logarithm because we assume that the effect of an additional strong tie decreases with the overall number of strong ties in the network. However, we also conducted our analysis without logarithmizing the variable and the results did not differ substantially.

The third measure aims to capture the benefits obtained from contact with individuals who are gainfully employed. We use information gathered from a name generator² regarding the three closest confidants outside the household to construct our measure. More specifically, our measure is the total number of employed individuals among the three closest friends and thus ranges from zero to three. A similar measure has been used in the studies by Capellari and Tatsiramos (2015) and Mouw (2003).

The fourth measure of accessed social capital is based on a position generator (Lin and Dumin 1986) and focuses on contact with influential social positions. Instead of collecting information on occupational positions, the generator was modified to reflect positions especially helpful during job search (see Wolf 2009). We focus on three questions asking the respondent about leisure-time contacts with persons who might potentially be in a position to exert influence on an employer's hiring decision. These persons include those who are involved in hiring decisions themselves, those who are business owners or those who earn more than 3,000 euros net income per month. We use a sum score of the three items ranging from zero (the respondent has no leisure-time contacts with anyone in any of the three positions) to three (the respondent has leisure-time contacts with persons in all three positions).

The fifth measure focuses on the job search resources available to the individual through the social network. We use information from a so-called resource generator, a measurement concept developed by van der Gaag and Snijders (2005) and adapted to the context of job search (see Wolf 2009). The focus is on the whole network, irrespective of tie strength. The resource generator asks for resources potentially available to respondents, irrespective of whether these resources are actually called upon during job search. We construct a sum score of three items that are all concerned with support related to job-finding prospects (whether the respondent knows anyone who would provide information about new jobs, help with applications, and recommendations to prospective employers).

Both the measures of weak-tie and strong-tie social capital as well as the number of employed friends are common in the literature. To the best of our knowledge, however, our measures for contact with influential social positions and available job search support have not been applied in the context of the wage effects of accessed social capital.

The second type of focal independent variables consists of the respective theoretical mechanisms: 1) the log of the monthly reservation wage; 2) a binary indicator that takes the value 1 for all individuals in newfound employment, who in the previous wave asked friends and relatives for job leads ("From where have you gathered information on jobs during the past four weeks? Asked family and friends?"); 3) a further binary indicator for nonsearchers, i.e., for those who entered a new job without prior job

² The name generator is an instrument for obtaining network data, originally developed by Burt (1984) and applied in many general population surveys since then. In our survey, the interviewees were asked a stimulus question (name generator) to think about the person outside the household who is most important to them. Subsequently, the interviewees were asked about several characteristics of this person (gender, education, employment status) and their relationship with this person (frequency of conflicts, intimacy). The procedure was also conducted for the second and third most important person.

searching (“In the past four weeks, have you been looking for a job?”); The indicator takes the value 0 for all other individuals, including nonsearchers; 4) the number of job interviews in the wave prior to job-finding, top-coded at 5 interviews (“Have you attended any job interviews during the past four weeks? If so, please tell me how many.”). If individuals obtained their job without searching, the number of interviews was set to 0³.

In addition to the two kinds of focal independent variables, several other independent variables are included in the analysis as controls. In the cross-sectional regression, we control for time constant as well as time constant variables. In the fixed-effects regressions, time constant variables are omitted as well as age which is collinear with wave in fixed effects regressions with yearly panel data⁴. Controls slightly differ between the samples of currently and newly employed individuals.

5.2 Analytical samples

In our study, we distinguish between two analytical samples that we constructed from the data, a sample of all currently employed and a sample of all newly employed individuals. Because we need longitudinal information to estimate fixed effects regressions (see section 5.3), in both samples, we eliminate all individuals with only one observation in the respective observation period.

The first sample consists of all currently employed workers, pooled over all available waves of the panel survey. The availability of waves depends on the respective social capital measure in all 12 waves, but we cannot use the first wave because it does not contain the crucial control variable “current employment duration”, leaving us with waves 2-12. Information on employed confidants and contact with influential persons is only available in waves 3 and 5, and information on job search support in waves 3, 5, 9 and 12. Consequently, the sample of currently employed workers distinguishes three subsamples containing 48,112, 4,022 and 6,678 observations. In these samples of currently employed, all variables in the regression models are obtained from the wave in which they were originally collected.

The second sample consists only of newly employed individuals. We identified all transitions from employment, registered unemployment or other nonemployment states (such as maternity leave) into a new job, regardless of whether someone actively searched for a job. To avoid our independent variables being already influenced by the dependent variable, we measure wages in wave $t+1$, i.e., after individuals entered a newfound job. The social capital measures, the mechanism variables and the controls are taken from wave t , i.e., the wave immediately before job seekers entered a newfound job. Ideally, we would

³ In the PASS survey, only those who actively searched for a job are asked their number of job interviews. Therefore, there is no information on the number of job interviews for non-searchers. For most job seekers, the number should be 0, because both questions refer to the same period (“prior four weeks”), and only in rare cases should individuals who had a job interview consider themselves as not having actively searched for a job.

⁴ We also implemented the fixed-effects regression including age, specified as either age and age squared or age in categories of 10 years, the results did not deviate from those reported below.

even attempt to measure social capital at wave t-1 to better reflect the temporal succession of cause, mediator and effect. However, building our sample this way would impose too many restrictions, leading to even fewer observations. The current approach, however, biases the analysis toward spurious correlations between social capital and the mechanism variables⁵.

Similar to the case of currently employed, three subsamples arise, where sample sizes depend on the respective social capital variables. Each individual observation is created by collapsing information from two waves. The dependent variable is taken from time t+1 and all independent variables from time t. An exception is the independent “current employment duration”, which needs to be measured at t+1 to account for differences in the duration of the newfound job. For weak and strong tie social capital this again leaves us with 11 waves and 4552 observations. For employed confidants and influential contacts this design leads to 130 observations (waves 3 and 5) and for job search support to 297 observations (waves 3, 5 and 9).

The restrictions imposed upon the second sample are necessary to avoid reverse causality and to test the causal mechanisms. However, the sample size drastically declined, especially for the social capital variables measured only in two or three waves. To avoid any further loss of cases, e.g., due to item nonresponse, and thus reduced precision in our estimates, we multiply impute missing data (Little and Rubin 2002) in the sample of newly employed workers. As recommended in the methodological literature, in addition to the variables from our analysis, we include auxiliary variables, both time constant and time changing, to be able to impute the dependent variable (Hippel 2007). These additional auxiliary variables are the following: the Big 5 personality traits, gender, education and sample as time-constant variables, and life satisfaction, social status measures (siops and isei), job finding via networks, firm size, as well as concurrent values of household income, number of friends and active organizational memberships as time-changing variables. In addition, we accounted for the panel character of the data. The implementation of this procedure is not standard in most imputation software, including the mi (multiple imputation) procedure in stata, which we used here. Therefore, as recommended in the literature (Lloyd et al. 2013), to account for the correlation of variable values from one individual, we transformed the data from the “long” to the “wide” format for the imputation, and then back again for the analysis.

⁵ Given that for a mediation effect to arise, a correlation not only between social capital and the mechanism variable but also between the latter and wages is necessary. Therefore, the consequences of our approach for our empirical results should be limited.

5.3 Method

As noted previously in this article and criticized by various authors (Mouw 2003; Chen 2011), the standard estimation strategy in the literature on social capital and wages does not account for unobserved confounders. However, such unobserved confounders can be an alternative explanation for the literature's main finding of positive wage effects. To examine the role of unobserved confounders, for both the currently and newly employed workers, in the first step we start with a pooled linear regression that treats the data as if cross-sectional. Wages is the dependent variable, and social capital is the focal independent variable of the regression. In the second step, we conduct fixed effects panel data regressions (Halaby 2004; Brüderl and Ludwig 2015). The comparison will show us the degree to which empirical evidence, at least in our study, but presumably in general, is biased by unobserved but wage-relevant differences between those with higher and lower social capital.

By focusing on intraindividual change, fixed effects regression accounts not only for observed but also for unobserved confounders, as long as they are constant over the observation period. A statistically significant coefficient in a fixed effects regression indicates that a within-person change between waves in the focal independent variable is associated with a within-person change in the dependent variable. This within estimate presents a stronger argument for causality than do mere static comparisons between persons with different levels of the focal dependent and independent variables (between variations) by differencing out all unobserved confounders, if they are constant over the observation period.

In a third step we are testing the proposed causal mechanisms, which is only possible in the sample of newly employed workers. A variable is usually accepted to lie on the causal path if it is measured after the cause⁶ and before the effect variable and if its inclusion in the regression will reduce the size of the presumed causal variable's coefficient (Baron and Kenny 1986). For example after estimating an effect of weak ties on wages introduction of reservation wages should reduce the effect of weak ties on wages. Therefore, for each social capital variable, we conduct a stepwise inclusion of all mechanism variables and compare the resulting regression coefficients. For weak- and strong-tie social capital, we can use fixed-effects regressions to conduct the test. For the other three social capital measures, the sample size is very low, and therefore, we conduct the test based on the OLS linear regression.

6. Results

In this section, we present the results of our empirical analysis. We do this separately for the samples of currently and newly employed workers. We also report on a number of robustness checks we conducted.

⁶ See also footnote 4.

6.1 Wage effects of social capital for currently and newly employed workers

Starting with currently employed workers, Table 1 presents the results from a pooled linear regression analysis that treats the data as cross-sectional and thus does not control for unobserved confounders. We estimate a separate regression model for each social capital variable because the sample sizes differ. What we find is consistently positive and statistically significant coefficients for all social capital variables. Because the dependent variable is logarithmized, the coefficients can be approximately interpreted as percentage point increases or decreases. For example, for weak tie social capital (Table 1, Model 1), we find that for every additional kind of organization in which an individual is an active member, wages increase by approximately 6.4 percent. This value is quite large and similar to what other studies have reported; for example, Ruiters, DeGraaf (2003) find a coefficient of 0.046. For strong ties (Table 2, Model 2), measured as the log number of close contacts outside the household, we find that a 1 percent increase in strong ties is associated with a ca. 0.001 percent increase in wages. This value is quite small but statistically significant.

Table 1: Wage coefficients of various social capital measures for currently employed workers: pooled linear regression

	Model 1 ^{a)}	Model 2 ^{a)}	Model 3 ^{b)}	Model 4 ^{b)}	Model 5 ^{c)}
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Weak Ties	0.064*** (0.0030)				
Strong ties		0.001* (0.0003)			
Employed confidants			0.030*** (0.0082)		
Contact to Influential persons				0.051*** (0.0071)	
Job search support					0.033*** (0.0073)
Time varying covariates	yes	yes	yes	yes	yes
Time constant covariates	yes	yes	yes	yes	yes
Constant	1.394*** (0.0371)	1.423*** (0.0376)	1.375*** (0.146)	1.433*** (0.145)	1.490*** (0.0853)
Observations	48,112	48,112	4,022	4,022	11,225
Persons	10,369	10,369	2,011	2,011	4,432
R-squared	0.247	0.239	0.294	0.301	0.290

*Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: pooled linear regression estimation samples: currently employed in a) waves 2-12, b) waves 3 and 5, c) waves 3, 5, 9 and 12; dependent variable: log wages (at time t); time varying controls: tenure, age, household size, marital status, cumulated unemployment duration, dummies for waves (all at time t); time constant controls: gender, education, sample dummy. For full tables see Appendix Table A1.*

Regarding the number of employed among the three closest friends, wages statistically significantly increase by 3 percent for every additional employed friend, which is similar to the value of 3.4 percent reported by Cappellari and Tatsiramos (2015) in their linear pooled regression. Wages increase by 5.1 percent for every additional kind of influential person the respondent recently had contact with and by ca. 3.3 percent for every additional job search resource available to the respondent. All three coefficients are statistically significant (Table 2, Models 3-5).

Table 2: Wage coefficients of various social capital measures for currently employed workers: fixed effects regression

	Model 1 ^{a)}	Model 2 ^{a)}	Model 3 ^{b)}	Model 4 ^{b)}	Model 5 ^{c)}
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Weak Ties	-0.001 (0.0029)				
Strong ties		0.000 (0.0003)			
Employed confidants			-0.004 (0.0087)		
Contact to Influential persons				0.013 (0.0085)	
Job search support					-0.002 (0.0068)
Time varying covariates	yes	yes	yes	yes	yes
Time constant covariates	no	no	no	no	no
Constant	2.396*** (0.0208)	2.394*** (0.0207)	2.529*** (0.104)	2.500*** (0.103)	2.424*** (0.0385)
Observations	48,112	48,112	4,022	4,022	11,225
Persons	10,369	10,369	2,011	2,011	4,432
R-squared	0.098	0.098	0.095	0.095	0.135

*Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: fixed effects regression; estimation samples: currently employed in a) waves 2-12, b) waves 3 and 5, c) waves 3,5,9 and 12; dependent variable: log wages (at time t); controls: tenure, household size, marital status, cumulated unemployment duration, dummies for waves (all at time t); age not included due to collinearity with (yearly) waves in fixed effects estimation. For full tables see Appendix Table A2.*

Thus far, our results are in line with the empirical literature on social capital and wages. For those measures that had already been tested in the literature, we obtain similar results. For the measures that had not been tested in the previous literature, we find that the results are in line with theoretical expectations. However, this is no longer the case once we control for unobserved confounders. The fixed effects regression coefficients presented in Table 2 are all close to zero and statistically insignificant. For example, the wage coefficient for weak-tie social capital is only -0.001, and the coefficient for

strong-tie capital is 0.000. Thus, we conclude that for currently employed individuals, the data do not support hypotheses 1a to 1e.

Turning to the newly employed workers, we again start with pooled linear regressions that treat the data as if cross-sectional. Similar to the currently employed, regression coefficients are quite large and statistically significant, even in the subsamples where the number of observations is lower. An exception is the coefficient for strong ties, which is statistically insignificant.

Table 3: Wage coefficients of various social capital measures for newly employed workers: pooled linear regression

	Model 1 ^{a)}	Model 2 ^{a)}	Model 3 ^{b)}	Model 4 ^{b)}	Model 5 ^{c)}
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Weak Ties	0.040*** (0.0110)				
Strong ties		0.007 (0.0112)			
Employed confidants			0.130** (0.0421)		
Contact to Influential persons				0.129*** (0.0372)	
Job search support					0.090* (0.0378)
Time varying covariates	yes	yes	yes	yes	yes
Time constant covariates	yes	yes	yes	yes	yes
Constant	1.410*** (0.115)	1.425*** (0.118)	0.642 (0.834)	0.618 (0.848)	0.786# (0.459)
Observations	4,552	4,552	130	130	297
Persons	2,025	2,025	65	65	148
R-squared	n.a.	n.a.	n.a.	n.a.	n.a.

*Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: pooled linear regression estimation samples: newly employed in a) waves 1-11, b) waves 3 and 5, c) waves 3,5,9 and 12; multiply imputed data, R^2 not available; dependent variable: log wages (at time $t+1$); time varying controls: tenure in new job (at time $t+1$), age labour market status before new job (employed, unemployed, out of labour force, other) household size, marital status current unemployment duration, cumulated unemployment duration, dummies for waves (all at time t); time constant controls: gender, education, sample dummy. For full tables see Appendix Table A3.*

As in the case of currently employed workers, applying fixed-effects regression to the sample of newly employed workers produces regression coefficients that are statistically insignificant and close to zero. However, as shown in Table 4, we can only conduct this test for weak and strong tie social capital. For the other measures, the coefficients would be based on only a few cases where social capital measures actually change over time. Thus, we conclude that for newly employed individuals, the data do not support hypotheses 1a and 1b. In the sample of newly employed workers, the only available evidence

regarding hypotheses 1c to 1e is potentially biased. Therefore, we cannot draw definitive conclusions, but the evidence from fixed effects regressions on currently employed workers and the limited evidence from fixed effects regressions on newly employed workers casts serious doubt on the causal interpretation of the coefficients from the cross-sectional regression. As we will see, the mediation analyses will reinforce our skepticism.

Table 4: Wage coefficients of various social capital measures for newly employed workers: fixed effects regression

	Model 1 ^{a)}	Model 2 ^{a)}
	Coef. (Std.)	Coef. (Std.)
Weak Ties	0.000 (0.0187)	
Strong ties		0.009 (0.0152)
Time varying covariates	yes	yes
Time constant covariates	no	no
Constant	2.194*** (0.0771)	2.176*** (0.0848)
Observations	4,552	4,552
Persons	2,025	2,025
R-squared	n.a.	n.a.

*Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: fixed effects regression; estimation samples: newly employed in a) waves 1-11; multiply imputed data, R^2 not available; dependent variable: log wages (at time $t+1$); controls: tenure in new job (at time $t+1$) labour market status before new job (employed, unemployed, out of labour force, other) household size, marital status current unemployment duration, cumulated unemployment duration, dummies for waves (all at time t); age not included due to collinearity with (yearly) waves in fixed effects estimation. For full tables see Appendix Table A4.*

6.2 Mediation analyses of the wage effects of social capital for newly employed workers

Turning now to the causal mediation analysis, we will base the test on fixed-effects regressions for weak and strong ties and on pooled linear regressions for employed confidants, contact with influential persons and available job search support. Starting with weak ties, model 1 in Table 5 repeats the regression results reported earlier (Table 4 Model 1), whereas Models 2 to 5 conduct a stepwise inclusion of reservation wages, the dummy for asking for job leads, the dummy for finding a job without active search and the number of job interviews as potential intervening variables⁷. Because the wage coefficient of weak ties is close to zero to begin with, the failure of any of the mechanisms to act as mediators is not surprising. However, the results are still informative. For reservation wages, we find a strong positive

⁷ Whether we include the mechanism variables stepwise or estimate separate models for each individual variable does not change the results substantially.

coefficient, even if it is only marginally statistically significant at the 10% level. If anything, the social capital coefficient in Model 2 is increasing instead of decreasing. Thus, even if reservation wages have the expected positive impact on actual wages, social capital has no (or even a slightly negative) effect on reservation wages.

Table 5: Mediation analysis for the wage effect of weak ties: fixed effects regression

	Model 1	Model 2	Model 3	Model 4	Model 5
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Weak Ties	0.000 (0.0187)	0.002 (0.0187)	0.002 (0.0187)	0.002 (0.0187)	0.002 (0.0186)
Reservation Wages		0.054# (0.0288)	0.054# (0.0288)	0.054# (0.0286)	0.054# (0.0285)
Asking for help			-0.010 (0.0224)	-0.008 (0.0296)	-0.008 (0.0295)
“Non-Search”				0.005 (0.0396)	0.002 (0.0394)
No. job interviews					-0.007 (0.0120)
Time varying covariates	yes	yes	yes	yes	yes
Time constant covariates	no	no	no	no	no
Constant	2.194*** (0.0771)	1.815*** (0.214)	1.822*** (0.215)	1.824*** (0.215)	1.823*** (0.215)
Observations	4,552	4,552	4,552	4,552	4,552
Persons	2,025	2,025	2,025	2,025	2,025
R-squared	n.a.	n.a.	n.a.	n.a.	n.a.

*Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: fixed effects regression; estimation samples: newly employed in waves 1-11; dependent variable: log wages (at time $t+1$); controls: tenure in new job (at time $t+1$) labour market status before new job (employed, unemployed, out of labour force, other) household size, marital status current unemployment duration, cumulated unemployment duration, dummies for waves (all at time t); age not included due to collinearity with (yearly) waves in fixed effects estimation.*

Asking the social network for job leads has no effect on wages, and for this reason alone, it cannot act as a mediator, irrespective of how social capital is measured (including measures not tested in our study). The same is true for job-finding without prior active search efforts and the number of job interviews; in both cases, the wage regression coefficients are close to zero and statistically insignificant. Regarding job interviews and asking for job leads, the point estimates are even negative, suggesting a slightly negative association between the mechanism variables and wages in our data.

Table 6: Mediation analysis for the wage effect of strong ties: fixed effects regression

	Model 1	Model 2	Model 3	Model 4	Model 5
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Strong Ties	0.009 (0.0152)	0.011 (0.0150)	0.011 (0.0150)	0.011 (0.0150)	0.011 (0.0151)
Reservation Wages		0.055# (0.0288)	0.055# (0.0289)	0.054# (0.0286)	0.055# (0.0286)
Asking for help			-0.010 (0.0224)	-0.008 (0.0296)	-0.009 (0.0296)
“Non-Search”				0.005 (0.0398)	0.001 (0.0396)
No. job interviews					-0.007 (0.0121)
Time varying covariates	yes	yes	yes	yes	yes
Time constant covariates	no	no	no	no	no
Constant	2.176*** (0.0848)	1.790*** (0.216)	1.797*** (0.217)	1.799*** (0.217)	1.797*** (0.217)
Observations	4,552	4,552	4,552	4,552	4,552
Persons	2,025	2,025	2,025	2,025	2,025
R-squared	n.a.	n.a.	n.a.	n.a.	n.a.

*Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: fixed effects regression; estimation samples: newly employed in waves 1-11; dependent variable: log wages (at time $t+1$); controls: tenure in new job (at time $t+1$) labour market status before new job (employed, unemployed, out of labour force, other) household size, marital status current unemployment duration, cumulated unemployment duration, dummies for waves (all at time t); age not included due to collinearity with (yearly) waves in fixed effects estimation.*

For strong ties, the pattern documented in Table 6 is almost identical to that in Table 5, which is not surprising given that both regressions differ only with respect to the social capital variable.

For employed confidants, our mediation analysis has to rely on pooled linear regression. However, as we will see, even if we assume the respective wage coefficient to be biased, the results are still informative, even if in cross-sectional analysis, unobserved confounders can also bias mediation tests. Such bias would lead to what might be called spurious mediation effects. That is, including the mechanism variable might change the coefficient, but only because it is correlated to the same unobserved confounder as the social capital variable. Therefore, if we observe changes in the social capital coefficient, the reason could be unobserved confounders or true mediation effects. In contrast, if we do not observe any changes, a mediation effect is unlikely. As we can see, the latter is what we observe from Tables 7 to 9, where the coefficient of all remaining social capital variables, i.e. employed confidants, influential persons and job search support, exhibit almost no changes, once the potential mediators are included in the regression.

Table 7: Mediation analysis for the wage effect of employed confidants: pooled linear regression

	Model 1	Model 2	Model 3	Model 4	Model 5
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Employed confidants	0.130** (0.0421)	0.126** (0.0418)	0.125** (0.0430)	0.126** (0.0434)	0.120** (0.0435)
Reservation Wages		0.146 (0.149)	0.143 (0.150)	0.140 (0.151)	0.133 (0.150)
Asking for help			0.038 (0.107)	0.047 (0.119)	0.051 (0.119)
“Non-Search”				0.028 (0.159)	0.050 (0.163)
No. job interviews					0.041 (0.0458)
Time varying covariates	yes	yes	yes	yes	yes
Time constant covariates	yes	yes	yes	yes	yes
Constant	0.642 (0.834)	-0.183 (1.398)	-0.147 (1.400)	-0.149 (1.400)	-0.039 (1.423)
Observations	130	130	130	130	130
Persons	65	65	65	65	65
R-squared	n.a.	n.a.	n.a.	n.a.	n.a.

*Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: pooled linear regression estimation samples: newly employed in waves 3 and 5; dependent variable: log wages (at time $t+1$); time varying controls: tenure in new job (at time $t+1$), age labour market status before new job (employed, unemployed, out of labour force, other) household size, marital status current unemployment duration, cumulated unemployment duration, dummies for waves (all at time t); time constant controls: gender, education, sample dummy.*

Table 8: Mediation analysis for the wage effect of influential contacts: pooled linear regression

	Model 1	Model 2	Model 3	Model 4	Model 5
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Influential contacts	0.129*** (0.0372)	0.128*** (0.0369)	0.127*** (0.0369)	0.129*** (0.0370)	0.121** (0.0393)
Reservation Wages		0.158 (0.144)	0.155 (0.146)	0.163 (0.148)	0.157 (0.148)
Asking for help			0.035 (0.104)	0.015 (0.116)	0.019 (0.117)
“Non-Search”				-0.071 (0.164)	-0.051 (0.170)
No. job interviews					0.032 (0.0457)

	Model 1	Model 2	Model 3	Model 4	Model 5
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Time varying covariates	yes	yes	yes	yes	yes
Time constant covariates	yes	yes	yes	yes	yes
Constant	0.618 (0.848)	-0.264 (1.374)	-0.231 (1.382)	-0.222 (1.378)	-0.140 (1.408)
Observations	130	130	130	130	130
Persons	65	65	65	65	65
R-squared	n.a.	n.a.	n.a.	n.a.	n.a.

*Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: pooled linear regression estimation samples: newly employed in waves 3 and 5; dependent variable: log wages (at time $t+1$); time varying controls: tenure in new job (at time $t+1$), age labour market status before new job (employed, unemployed, out of labour force, other) household size, marital status current unemployment duration, cumulated unemployment duration, dummies for waves (all at time t); time constant controls: gender, education, sample dummy.*

In all, for none of the five social capital variables did any of the four mediation tests indicate support for the existence of a mediation effect. This means that there is no wage effect of social capital via reservation wages, asking for help, non-search and/or the number of job interviews. Therefore, hypotheses 2a to 2d do not find support in the data.

6.4 Robustness checks

As a robustness check, we tested whether our results depend on institutional context. According to the thesis of double embeddedness, the value of social capital is contingent on institutional and cultural context (Nee, and Ingram 1998). Critics might argue that our results are specific to Germany, which is a country that relies more on educational credentials than social contacts. However, it is not possible to fully compare our results to empirical evidence from different national institutional contexts because to the best of our knowledge, ours is the only study that tests social capital effects by applying fixed effects regression. Nevertheless, we can address the question of context to some degree by distinguishing different labor market contexts within Germany.

One way to do this is to distinguish between workers in East and West Germany. Even thirty years after the German reunification, important cultural differences and differences regarding the labor market remain (Schnabel 2016). We interacted the social capital measures with a dummy for East and West Germany in a fixed effects regression on currently employed individuals, but found no statistically significant interaction effects (see Table A5 in the appendix).

Similarly, effects might depend on sector of employment. Because public employers are more visible and more accountable to the public, they are more subject to regulations that prohibit the use of nonmeritocratic employee selection criteria, such as network contacts (Bills et al. 2017). Interacting social capital with a sector dummy (see Table A6 in the appendix) also lead to statistically insignificant interaction effects for all social capital variables.

Table 9: Mediation analysis for the wage effect of job search support: pooled linear regression

	Model 1	Model 2	Model 3	Model 4	Model 5
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Job search support	0.090* (0.0378)	0.090* (0.0393)	0.090* (0.0392)	0.091* (0.0394)	0.090* (0.0396)
Reservation Wages		0.125 (0.0931)	0.125 (0.0935)	0.126 (0.0940)	0.124 (0.0942)
Asking for help			-0.0013 (0.0706)	-0.0081 (0.0854)	-0.0061 (0.0859)
“Non-Search”				-0.020 (0.105)	-0.010 (0.110)
No. job interviews					0.016 (0.0348)
Time varying covariates	yes	yes	yes	yes	yes
Time constant covariates	yes	yes	yes	yes	yes
Constant	0.786# (0.459)	0.045 (0.838)	0.047 (0.840)	0.053 (0.838)	0.072 (0.841)
Observations	297	297	297	297	297
Persons	148	148	148	148	148
R-squared	n.a.	n.a.	n.a.	n.a.	n.a.

*Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: pooled linear regression estimation samples: newly employed waves 3,5,9 and 12; dependent variable: log wages (at time $t+1$); time varying controls: tenure in new job (at time $t+1$), age labour market status before new job (employed, unemployed, out of labour force, other) household size, marital status current unemployment duration, cumulated unemployment duration, dummies for waves (all at time t); time constant controls: gender, education, sample dummy.*

7. Discussion of limitations and conclusions

In this paper, we analyzed wage effects of five different kinds of social capital indicators, weak and strong tie social capital, employed confidants, contact with influential positions and job search support. The indicators measure theoretical concepts that are frequently referred to when arguing about the advantages derived from social capital during job search.

The advantage of our study is that we were able to account for unobserved time-constant confounders and test our hypotheses both on current and newly employed workers. In addition, we also tested three potential causal mechanisms, i.e., reservation wages, asking the network for job leads, job-finding without prior search and the number job interviews for newly employed workers. In all these tests, we found no empirical support for causal wage effects of any of the social capital variables and no evidence supporting the importance of any of the causal mechanisms for any of the social capital variables.

Ours is one of the few studies to present empirical evidence against the wage effect of social capital, despite testing more social capital variables than most studies in the field. One explanation for the null effects of social capital is to point to the institutional context. While cultural and institutional differences are certainly important, we do not believe that they explain our results. Arguably, Germany has institutional characteristics that differ from those in other countries, but as in other countries, personal contacts match a high share of job seekers (Krug 2013; Brenzel et al. 2016). Furthermore, similar to most other countries, there is consistent empirical evidence that social networks help in finding a job (Brandt 2006; Krug et al. 2019, Gërxhani and Kosyakova 2020), and there is inconsistent evidence regarding wage effects (see Dustman et al. 2015 for positive and Glitz 2017 for null effects). We argue that the main reason for our null finding is that in contrast to most other studies, we controlled for unobserved confounders. As we have seen, in our cross-sectional analysis, we observed a pattern that is very similar to that in the rest of the empirical literature, namely, significant wage effects of social capital, measured in various ways. It is only after controlling for unobserved confounders that the null effects emerged. Because we are not aware of any data from other countries that allow for a similar research design, a comparison of our fixed-effects results with results from other countries is not possible. Until such data become available, the question of whether the observed null effects of social capital extend to other national institutional contexts must remain open.

Of course, we also acknowledge the limitations of our analysis, most of which, however, also apply to prior studies. For example, due to the use of large-scale survey data, we were not able to measure structural aspects of social networks such as network composition or density. Similarly, we rely on the concept of accessed social capital. Even if it seems likely that those who are in possession of social capital will use it during job search, we do not measure the actual flow of any social resources. Further research should therefore focus on the activation of social capital. Until then, the main conclusion of our analysis is that access to social capital does not create advantages for job seekers or job changers in terms of wages. Of course, this does not mean that social capital plays no role in the labor market in general or for job seekers specifically. For example, empirical evidence with the same data we used points toward positive causal effects of weak tie social capital and general social support on the job-finding of the long-term unemployed (Krug et al. 2019).

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Appendix

Table A1: Wage coefficients of various social capital measures for currently employed workers: pooled linear regression

	Model 1 ^{a)}	Model 2 ^{a)}	Model 3 ^{b)}	Model 4 ^{b)}	Model 5 ^{c)}
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Weak Ties	0.0635*** (0.0030)				
Strong ties		0.001* (0.0003)			
Employed confidants			0.030*** (0.0082)		
Contact to Influential persons				0.051*** (0.0071)	
Job search support					0.033*** (0.0073)
Age	0.041*** (0.0018)	0.041*** (0.0018)	0.051*** (0.0073)	0.048*** (0.0072)	0.042*** (0.0040)
Age squared	-0.000*** (0.0000)	-0.000*** (0.0000)	-0.001*** (0.0000)	-0.001*** (0.0000)	-0.000*** (0.0000)
Gender	-0.132*** (0.0048)	-0.137*** (0.0048)	-0.137*** (0.0163)	-0.125*** (0.0162)	-0.135*** (0.0095)
Education (R:CASMIN 1)					
CASMIN 2	0.110*** (0.0055)	0.117*** (0.0055)	0.075*** (0.0186)	0.066*** (0.0186)	0.095*** (0.0110)
CASMIN 3	0.425*** (0.0069)	0.442*** (0.0069)	0.374*** (0.0227)	0.346*** (0.0230)	0.390*** (0.0137)
Household size	0.008*** (0.0021)	0.010*** (0.0022)	0.003 (0.0077)	0.003 (0.0076)	0.005 (0.0044)
Married (R: yes)	0.044*** (0.0058)	0.048*** (0.0058)	0.057** (0.0200)	0.055** (0.0199)	0.015 (0.0113)
Current employment duration	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Cumulated unemployment duration over life course	-0.003*** (0.0001)	-0.003*** (0.0001)	-0.002*** (0.0005)	-0.002*** (0.0005)	-0.002*** (0.0002)
Sample (Dummy)	-0.007*** (0.0009)	-0.008*** (0.0009)	-0.283*** (0.0236)	-0.270*** (0.0238)	-0.248*** (0.0123)
Wave 2	0.006 (0.0136)	0.005 (0.0137)			
R: Wave 3					
Wave 4	-0.010 (0.0124)	-0.007 (0.0124)			
Wave 5	0.097*** (0.0117)	0.098*** (0.0117)	0.128*** (0.0160)	0.127*** (0.0159)	0.124*** (0.0145)
Wave 6	0.102*** (0.0117)	0.105*** (0.0117)			
Wave 7	0.121*** (0.0117)	0.122*** (0.0117)			
Wave 8	0.142*** (0.0117)	0.142*** (0.0118)			
Wave 9	0.172*** (0.0115)	0.174*** (0.0116)			0.224*** (0.0138)
Wave 10	0.195*** (0.0116)	0.196*** (0.0117)			
Wave 11	0.234***	0.233***			

	Model 1 ^{a)}	Model 2 ^{a)}	Model 3 ^{b)}	Model 4 ^{b)}	Model 5 ^{c)}
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Wave 12	(0.0114) 0.247*** (0.0117)	(0.0114) 0.248*** (0.0117)			0.263*** (0.0144)
Constant	1.394*** (0.0371)	1.423*** (0.0376)	1.375*** (0.146)	1.433*** (0.145)	1.490*** (0.0853)
Observations	48,112	48,112	4,022	4,022	11,225
Persons	10,369	10,369	2,011	2,011	4,432
R-squared	0.247	0.239	0.294	0.301	0.290

Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: pooled linear regression estimation samples: currently employed in a) waves 2-12, b) waves 3 and 5, c) waves 3, 5, 9 and 12; dependent variable: log wages (at time t); time varying controls: tenure, age, household size, marital status, cumulated unemployment duration, dummies for waves (all at time t); time constant controls: gender, education, sample dummy.

Table A2: Wage coefficients of various social capital measures for currently employed workers: fixed effects regression

	Model 1 ^{a)}	Model 2 ^{a)}	Model 3 ^{b)}	Model 4 ^{b)}	Model 5 ^{c)}
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Weak Ties	-0.000 (0.0029)				
Strong ties		0.000 (0.0003)			
Employed confidants			-0.004 (0.0087)		
Contact to Influential persons				0.013 (0.00846)	
Job search support					-0.002 (0.0068)
Household size	0.014*** (0.0039)	0.014*** (0.0039)	0.018 (0.0165)	0.019 (0.0165)	0.016* (0.0067)
Married (R: yes)	0.023# (0.0126)	0.023# (0.0126)	0.063 (0.0571)	0.062 (0.0570)	0.030 (0.0202)
Current employment duration	-0.000*** (0.0001)	-0.000*** (0.0001)	-0.000 (0.0004)	-0.000 (0.0004)	-0.000** (0.0001)
Cumulated unemployment duration over life course	-0.000 (0.0007)	(0.0001) (0.0007)	-0.005 (0.0048)	-0.005 (0.0047)	0.000 (0.0011)
Wave 2	-0.026** (0.0082)	-0.026** (0.0081)			
R: Wave 3					
Wave 4	0.031*** (0.0080)	0.031*** (0.0080)			
Wave 5	0.144*** (0.0089)	0.144*** (0.0089)	0.144*** (0.0122)	0.142*** (0.0123)	0.147*** (0.0099)
Wave 6	0.170*** (0.0094)	0.170*** (0.0094)			
Wave 7	0.195*** (0.0098)	0.195*** (0.0098)			
Wave 8	0.217*** (0.0103)	0.217*** (0.0103)			
Wave 9	0.244***	0.244***			0.257***

	Model 1 ^{a)}	Model 2 ^{a)}	Model 3 ^{b)}	Model 4 ^{b)}	Model 5 ^{c)}
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Wave 10	(0.0110) 0.269***	(0.0110) 0.269***			(0.0135)
Wave 11	(0.0116) 0.291***	(0.0116) 0.291***			
Wave 12	(0.0121) 0.311***	(0.0121) 0.311***			0.317*** (0.0166)
Constant	2.396*** (0.0208)	2.394*** (0.0207)	2.529*** (0.104)	2.500*** (0.103)	2.424*** (0.0385)
Observations	48,112	48,112	4,022	4,022	11,225
Persons	10,369	10,369	2,011	2,011	4,432
R-squared	0.098	0.098	0.095	0.095	0.135

Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: fixed effects regression; estimation samples: currently employed in a) waves 2-12, b) waves 3 and 5, c) waves 3,5,9 and 12; dependent variable: log wages (at time t); controls: tenure, household size, marital status, cumulated unemployment duration, dummies for waves (all at time t); age not included due to collinearity with (yearly) waves in fixed effects estimation.

Table A3: Wage coefficients of various social capital measures for newly employed workers: pooled linear regression

	Model 1 ^{a)}	Model 2 ^{a)}	Model 3 ^{b)}	Model 4 ^{b)}	Model 5 ^{c)}
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Weak Ties	0.040*** (0.0110)				
Strong ties		0.007 (0.0112)			
Employed confidants			0.130** (0.0421)		
Contact to Influential persons				0.129*** (0.0372)	
Job search support					0.090* (0.0378)
Current employment duration	0.001 (0.0007)	0.001 (0.0007)	0.001 (0.0075)	-0.002 (0.0076)	0.002 (0.0021)
Previous labour market status (R:Employed)					
Unemployed	-0.157*** (0.0197)	-0.159*** (0.0198)	-0.236* (0.109)	-0.205# (0.105)	-0.240** (0.0822)
Out of labour force	-0.126*** (0.0364)	-0.124*** (0.0365)	-0.184 (0.209)	-0.330 (0.243)	-0.274* (0.111)
Other	-0.126** (0.0428)	-0.128** (0.0430)	-0.339 (0.206)	-0.574** (0.210)	-0.195 (0.156)
Household size	0.018** (0.0066)	0.020** (0.0066)	0.015 (0.0367)	0.005 (0.0383)	0.014 (0.0280)
Married	0.017 (0.0202)	0.018 (0.0203)	0.079 (0.110)	0.115 (0.108)	0.002 (0.0745)
Current unemployment duration	0.000 (0.0004)	0.000 (0.0004)	0.002 (0.0021)	-0.000 (0.0018)	0.001 (0.0018)
Cumulated unemployment duration	-0.002*** (0.0002)	-0.002*** (0.0002)	-0.001 (0.0011)	-0.001 (0.0011)	-0.001 (0.0007)
Age	0.041***	0.040***	0.076#	0.085#	0.069**

	Model 1 ^{a)}	Model 2 ^{a)}	Model 3 ^{b)}	Model 4 ^{b)}	Model 5 ^{c)}
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Age squared	(0.0059) -0.000***	(0.0059) -0.000***	(0.0448) -0.001#	(0.0450) -0.001*	(0.0223) -0.001**
Gender	(0.0001) -0.075***	(0.0001) -0.078***	(0.0005) -0.265**	(0.0005) -0.170#	(0.0003) -0.180**
Education (R:CASMIN 1)	(0.0155)	(0.0155)	(0.0918)	(0.0906)	(0.0600)
CASMIN 2	0.071*** (0.0177)	0.074*** (0.0177)	0.138 (0.101)	0.118 (0.104)	0.102# (0.0617)
CASMIN 3	0.334*** (0.0251)	0.343*** (0.0248)	0.432*** (0.117)	0.428*** (0.116)	0.396*** (0.107)
Sample (Dummy)	-0.018*** (0.0052)	-0.019*** (0.0053)	-0.071 (0.0885)	-0.050 (0.0928)	-0.053 (0.0609)
Wave 1	-0.043 (0.0479)	-0.045 (0.0479)			
Wave 2	-0.080# (0.0420)	-0.081# (0.0420)			
R: Wave 3					
Wave 4	0.043 (0.0417)	0.044 (0.0418)			
Wave 5	0.101** (0.0380)	0.101** (0.0380)	0.097 (0.0865)	0.081 (0.0875)	0.192* (0.0747)
Wave 6	0.114** (0.0356)	0.115** (0.0357)			
Wave 7	0.112** (0.0377)	0.110** (0.0377)			
Wave 8	0.175*** (0.0382)	0.172*** (0.0382)			
Wave 9	0.204*** (0.0367)	0.203*** (0.0367)			0.380*** (0.0787)
Wave 10	0.263*** (0.0350)	0.264*** (0.0350)			
Wave 11	0.272*** (0.0369)	0.271*** (0.0369)			
Constant	1.410*** (0.115)	1.425*** (0.118)	0.642 (0.834)	0.618 (0.848)	0.786# (0.459)
Observations	4,552	4,552	130	130	297
Persons	2,025	2,025	65	65	148
R-squared	n.a.	n.a.	n.a.	n.a.	n.a.

Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: pooled linear regression estimation samples: newly employed in a) waves 1-11, b) waves 3 and 5, c) waves 3,5,9 and 12; multiply imputed data, R^2 not available; dependent variable: log wages (at time $t+1$); time varying controls: tenure in new job (at time $t+1$), age labour market status before new job (employed, unemployed, out of labour force, other) household size, marital status current unemployment duration, cumulated unemployment duration, dummies for waves (all at time t); time constant controls: gender, education, sample dummy.

Table A4: Wage coefficients of various social capital measures for newly employed workers: fixed effects regression

	Model 1 ^{a)}	Model 2 ^{a)}
	Coef. (Std.)	Coef. (Std.)
Weak Ties	0.000 (0.0187)	
Strong ties		0.009 (0.0152)
Current employment duration	0.002* (0.0008)	0.002* (0.0008)
Previous labour market status (R:Employed)	-0.083***	-0.082***
Unemployed	(0.0232) -0.044	(0.0232) -0.045
Out of labour force	(0.0390) -0.057	(0.0390) -0.057
Other	(0.0545) 0.013	(0.0544) 0.013
Household size	(0.0145) 0.039	(0.0146) 0.039
Married	(0.0434) 0.001	(0.0433) 0.001
Current unemployment duration	(0.0007) -0.003	(0.0007) -0.003
Cumulated unemployment duration	(0.0017) -0.144**	(0.0017) -0.146**
Wave 2	(0.0486) -0.0948*	(0.0486) -0.0958*
R: Wave 3	(0.0478)	(0.0478)
Wave 4	0.051 (0.0416)	0.051 (0.0416)
Wave 5	0.169*** (0.0387)	0.169*** (0.0387)
Wave 6	0.189*** (0.0410)	0.189*** (0.0410)
Wave 7	0.235*** (0.0418)	0.235*** (0.0418)
Wave 8	0.333*** (0.0455)	0.333*** (0.0454)
Wave 9	0.392*** (0.0453)	0.393*** (0.0453)
Wave 10	0.451*** (0.0452)	0.451*** (0.0451)
Wave 11	0.491*** (0.0471)	0.492*** (0.0470)
Constant	2.194*** (0.0771)	2.176*** (0.0848)
Observations	4,552	4,553
Persons	2,025	2,025
R-squared	n.a.	n.a.

Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: fixed effects regression; estimation samples: newly employed in a) waves 1-11; multiply imputed data, R^2 not available; dependent variable: log wages (at time $t+1$); controls: tenure in new job (at time $t+1$) labour market status before new job (employed, unemployed, out of labour force, other) household size, marital status current unemployment duration, cumulated unemployment duration, dummies for waves (all at time t); age not included due to collinearity with (yearly) waves in fixed effects estimation.

Table A5: Interaction effects social capital and East / West Germany (Fixed-effects regression, currently employed workers)

	Model 1	Model 2	Model 3	Model 4	Model 5
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Employed in East Germany (R: West Germany)	-0.090* (0.0449)	-0.091* (0.0441)	-0.077 (0.0685)	-0.132** (0.0477)	-0.120 (0.0952)
Weak ties	-0.000 (0.0032)				
Interaction	-0.001 (0.0078)				
Strong ties		0.000 (0.0003)			
Interaction		0.000 (0.001)			
Employed confidants			0.001 (0.0097)		
Interaction			-0.016 (0.0165)		
Contact to Influential persons				0.010 (0.009)	
Interaction				0.025 (0.0202)	
Job search support					-0.003 (0.0084)
Interaction					0.004 (0.0147)
Time varying covariates	yes	yes	yes	yes	yes
Time constant covariates	no	no	no	no	no
Constant	2.399*** (0.0236)	2.397*** (0.0234)	2.495*** (0.106)	2.483*** (0.105)	2.383*** (0.0455)
Observations	48,112	48,112	4,022	4,022	11,225
Persons	10,369	10,369	2,011	2,011	4,432
R-squared	0.098	0.098	0.150	0.153	0.136

*Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** p<0.001, ** p<0.01, * p<0.05, # p<0.10; estimation method: fixed effects regression; estimation samples: currently employed in a) waves 2-12, b) waves 3 and 5, c) waves 3,5,9 and 12; dependent variable: log wages (at time t); controls: employed in east Germany, tenure, household size, marital status, cumulated unemployment duration, dummies for waves (all at time t); age not included due to collinearity with (yearly) waves in fixed effects estimation.*

Table A6: Interaction effects social capital and employment in public service (Fixed-effects regression, currently employed workers)

	Model 1	Model 2	Model 3	Model 4	Model 5
	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)	Coef. (Std.)
Public service	0.090*** (0.0263)	0.090*** (0.0268)	0.130 (0.0820)	0.129 (0.0795)	0.163** (0.0503)
Weak ties	0.003 (0.0033)				
Interaction	-0.004 (0.0070)				
Strong ties		0.000 (0.0003)			
Interaction		-0.000 (0.0006)			
Employed confidants			-0.000 (0.0091)		
Interaction			-0.011 (0.0183)		
Contact to Influential persons				0.020* (0.0094)	
Interaction				-0.009 (0.0174)	
Job search support					0.003 (0.007)
Interaction					-0.020 (0.0134)
Time varying covariates	yes	yes	yes	yes	yes
Time constant covariates	no	no	no	no	no
Constant	2.349*** (0.0222)	2.349*** (0.0223)	2.450*** (0.111)	2.421*** (0.111)	2.297*** (0.0413)
Observations	48,112	48,112	4,022	4,022	11,225
Persons	10,369	10,369	2,011	2,011	4,432
R-squared	0.136	0.136	0.151	0.154	0.178

Data source: Panel "Labour Market and Social Security", own calculations; robust standard errors in parentheses; *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, # $p < 0.10$; estimation method: fixed effects regression; estimation samples: currently employed in a) waves 2-12, b) waves 3 and 5, c) waves 3,5,9 and 12; dependent variable: log wages (at time t); controls: public service employment, tenure, household size, marital status, cumulated unemployment duration, dummies for waves (all at time t); age not included due to collinearity with (yearly) waves in fixed effects estimation.