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Coworker networks in the labour market☆

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ABSTRACT

This paper studies the effect of coworker-based networks on individual labour market outcomes. I analyse how the provision of labour market relevant information by former coworkers affects the employment probabilities and, if hired, the wages of male workers who have previously become unemployed as the result of an establishment closure. To identify the causal effect of an individual worker's network on labour market outcomes, I exploit exogenous variation in the strength of these networks that is due to the occurrence of mass-layoffs in the establishments of former coworkers. The empirical analysis is based on administrative data that comprise the universe of workers employed in Germany between 1980 and 2001. The results suggest a strong positive effect of a higher employment rate in a worker's network of former coworkers on his re-employment probability after displacement: a 10 percentage point increase in the prevailing employment rate in the network increases the re-employment probability by 7.5 percentage points. In contrast, there is no evidence of a statistically significant effect on wages.

1. Introduction

In many economic situations, individuals do not act autonomously but as members of social networks. This observation has encouraged substantial theoretical and empirical research on such networks and their role in society.[1] In the labour market, social networks are likely to play an important role, primarily by facilitating the exchange of information about potential job opportunities and by reducing uncertainty about workers' and firms' characteristics. In this context, one of the key questions of interest is whether and to what extent the social network a worker is embedded in affects his or her labour market outcomes.

Using data describing the entire work histories of the universe of workers in four large metropolitan areas in Germany, I define a given worker's network as the group of all coworkers with whom he worked together in the same establishment in the past. The focus on former coworkers is motivated by the observation that in many cases in which a worker finds a job through a social contact, this contact is work-related. For example, in Granovetter's famous study of the job search behaviour of professional, technical and managerial workers in Boston (Granovetter, 1995), 69 percent out of the 56 percent of workers who found their job through a personal contact indicated that the contact was known from a work situation (compared to only 31 percent who indicated that the contact was a relative or friend). In addition, coworkers are likely to possess good knowledge of the specific abilities of a given worker and are more aware of potential job openings than, for example, neighbours, friends or family members who, although wanting to help, often lack the attachment to the relevant labour market segment (see Antoninis, 2006). Both these properties should make coworkers particularly valuable social contacts when looking for a new job. Finally, and in contrast to most other network definitions, coworkers in the same establishment typically know each other. This is not trivial since in many studies of network effects actual personal contact between individual network members, a prerequisite for a

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[1] For overviews of the theoretical literature on social networks, see, for example, Goyal (2007) or Jackson (2008). Environments in which the role of social networks has recently been studied empirically include schools (e.g. Cipollone and Rosolia, 2007, or Calvó-Armengol et al., 2009), universities (e.g. DeGiorgi et al., 2010), individual establishments (e.g. Bandiera et al., 2009, or Mas and Moretti, 2009), law firms (Oyer and Schaefer, 2012), and sports (Guryan et al., 2009).

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network-based exchange of information, is not self-evident.

In the empirical analysis of this paper, I examine in detail the role coworker-based networks play for individual labour market outcomes, focussing on a sample of male workers who were displaced from their jobs as the result of an establishment closure. In the main specification, I study how the prevailing employment rate in a displaced worker's network of former coworkers at the time of displacement – as a measure of network quality – affects his re-employment probability and wage rate in the year after displacement. To account for unobserved group level shocks and potential sorting into networks based on observable and unobservable characteristics, I include a comprehensive set of control variables that capture a worker's permanent characteristics, past employment history, and network size, as well as a full set of fixed effects for the closing establishments, hence comparing the post-displacement outcomes of workers who are displaced from the same establishment. However, since variation in the employment rates of workers' networks arises from different past work histories, it may still be that a relationship between the network employment rate and a worker's own post-displacement outcomes is due to unobserved factors shared between members of the same network rather than the exchange of job-relevant information through the network. To deal with this problem, I employ a novel instrumental variable strategy that exploits past mass-layoffs as exogenous shocks to the employment rate in a worker's network. After separating from each other, some of a displaced worker's former coworkers may themselves become unemployed as the result of a mass-layoff. Under the assumption that, conditional on observable characteristics, the extent to which a coworker network is affected by such mass-layoffs is exogenous to any unobserved factors determining a worker's post-displacement outcomes, mass-layoffs can serve as a valid instrument for the prevailing employment rate in a network.

My empirical results show that coworker-based networks are an important feature of the labour market. Being embedded in a higher quality network in terms of the prevailing employment rate has a positive effect on the employment probability in the year after displacement but no effect on starting wages in the new job. According to results from the main instrumental variable specification, a 10 percentage point increase in the employment rate of a worker's network of former coworkers at the time of displacement increases the re-employment probability in the following year by 7.5 percentage points. This effect is robust to the inclusion of establishment/education group fixed effects as well as a large number of additional robustness checks. To provide evidence for the exogeneity of the instrumental variable, I perform a number of placebo estimations which show that contemporaneous mass-layoffs are unrelated to past labour market outcomes and that future mass-layoffs are unrelated to current labour market outcomes. Among the group of former coworkers, female coworkers, coworkers from the same age cohort as the displaced workers, and coworkers with whom prior interaction was more intensive are particularly important for post-displacement employment outcomes. Finally, an analysis looking at the longer run effects reveals that the positive impact on the employment probability of a displaced worker only persists for the first year after displacement.

This analysis contributes to the growing empirical literature on the role of social networks in the labour market. In this literature, most studies exploit survey data and directly examine how the use of informal hiring methods is related to workers' labour market outcomes.[2] While the evidence is not unambiguous, a majority of studies suggest a positive role of informal job finding methods for workers' labour market outcomes. In the absence of direct survey information on job finding methods and social interactions, an alternative set of empirical studies have employed a variety of network definitions likely to proxy for such interactions to indirectly test for the presence of network effects in the labour market. These network definitions comprise such diverse social groups as neighbours (e.g. Topa, 2001; Weinberg et al., 2004; Bayer et al., 2008; Schmutte, 2015; Hellerstein et al., 2011; Damm, 2014), individuals with the same (ethnic) origin (e.g. Munshi, 2003; Edin et al., 2003; Beaman, 2012; Dustmann et al., 2016), close friends (Cappellari and Tatsiramos, 2015), family members (Kramarz and Skans, 2014), freshmen hallmates (Marmaros and Sacerdote, 2002), and fellow war veterans (Laschever, 2003). The overall evidence from these studies, which typically relate the employment status of a worker to the prevailing employment rate in his network, points towards a positive role of social networks in the labour market.

The work most closely related to the present analysis is a study by Cingano and Rosolia (2012) who use matched employer-employee data for the two Italian provinces of Treviso and Vicenza over the period 1975 to 1997 to examine the response of unemployment duration to the employment rate and size of a worker's group of former coworkers. Their main result shows that a one standard deviation increase in the network employment rate reduces unemployment duration by about 8 percent (around 3 weeks for the average spell), a magnitude comparable to the findings of a more recent analysis for Austria by Saygin et al. (2014). Analyzing the heterogeneity across network contacts, Cingano and Rosolia (2012) further show that stronger ties, as measured by the joint tenure with the same employer in the past, as well as geographically and technologically closer contacts exert a particularly strong influence on employment outcomes.

While the present analysis shares, both in terms of data preparation and empirical model specification, a number of commonalities with, in particular, the analysis by Cingano and Rosolia (2012), it makes several novel contributions to the still nascent literature on the role of coworker-based networks in the labour market. First, it replicates key features of the Italian study in the alternative setting of Germany, where establishments, and hence coworker networks, tend to be larger, and where, in the empirical analysis, closing establishments pertain to both manufacturing and non-manufacturing sectors of the economy. Second, it provides additional heterogeneity analysis, in particular regarding the role of ethnic similarity and contact intensity between workers. Third, and most importantly, it employs a different identification strategy. Rather than relying exclusively on a comprehensive set of past and contemporaneous control variables to capture differences in workers' skill sets in an OLS framework, this analysis exploits mass-layoffs as exogenous shocks to the employment rate in a given worker's network of former coworkers in an IV framework to identify the network effects of interest.[3] In doing so, my study thus also speaks to the question of how employment shocks propagate through social networks as predicted by the theoretical models of, for example, Topa (2001) and Calvó-Armengol and Jackson (2004).

The remainder of the paper is organised as follows. In the next section, I sketch the theoretical framework underlying the empirical

[2] Recent examples of such studies are Weber and Mahringer (2008) for Austria, Goel and Lang (2010) for Canada, Caliendo et al. (2011) for Germany, Frijters et al. (2005) for the UK, Kugler (2003); Loury (2006) and Brown et al. (2016) for the US, Pellizzari (2010) for a selection of European countries, and Bentolila et al. (2010) for both the US and Europe. For a comprehensive summary of the literature on the use of referrals in the labour market, see Ioannides and Loury (2004) and Topa et al. (2011).

[3] There are a number of related studies that focus on coworker relationships and their role in the labour market. In an experimental setting, Beaman and Magruder (2012) investigate whether social networks help firms screen their job applicants and provide evidence that if the pay of the worker who makes the referral is linked to the performance of the referred worker, individuals shift their choice of referrals towards coworkers and away from relatives. Åslund et al. (2014) study how the origin/ethnicity of managers affects hiring patterns in Swedish establishments and document that, conditional on hiring from their pool of former coworkers, ethnic similarity between managers and coworkers increases the latter's probability of being hired. Using Swedish matched employer-employee data, Hensvik and Skans (2016) study networks based on previous coworker relationships to systematically test Montgomery's (Montgomery, 1991) employee referral model. Finally, Colussi (2015) assesses the role of coworker-based networks for immigrants' labour market outcomes in Italy.

analysis, show its empirical implementation, and explain the identification strategy. Section 3 describes the data source and sample preparation, and provides descriptive evidence on the main features of coworker-based networks in Germany. Section 4 presents the empirical results, including robustness and placebo tests, as well as heterogeneity results for different subgroups of former coworkers. Section 5 concludes.

2. Empirical strategy

2.1. Main estimation equation

The empirical analysis in this paper builds on a theoretical framework in which the primary role of social networks in the labour market is to increase the arrival rate of job offers (see, for example, Calvó-Armengol and Jackson (2004), and Wahba and Zenou (2005)).[4] The basic information transmission process can be summarized as follows. In the first phase of each period, agents hear about a new job opportunity and the wage associated with it with an exogenous probability. If the agent is unemployed, he will accept the job. If the agent is employed and the new wage offer does not dominate the agent's current wage, he will pass the information about the new job on to one (or several) of his unemployed contacts. If an unemployed contact receives more than one job offer, he will accept the one that offers the highest wage. Given this type of information flow, unemployed agents who are embedded in a large network characterised by a high employment rate should be more likely to find a new job and should receive higher wages than agents embedded in a small network characterised by a low employment rate.[5]

Testing the main predictions of this model requires, in a first step, a definition of what constitutes a network and based on which criteria two agents can be considered connected. The central assumption in this paper is that two workers are directly connected if and only if both workers worked together in the same establishment at some point in the past five years, during the so-called network building phase.[6]

To estimate the causal effect of the prevailing employment rate in a worker's network on his labour market outcomes, I focus on a setting that provides a natural starting point to study this relationship: establishment closures that push groups of workers from the same establishments into unemployment and onto the job market. Relative to more general job transitions, focussing on establishment closures as a source of job loss has the advantage of restricting the analysis to comparable workers who come out of the same set of establishments and who simultaneously start using their social networks in order to look for a new job, ruling out potential selection bias due to differential on-the-job search behaviour of workers with different network characteristics. The specific prediction that arises in this set-up is that displaced workers embedded in a large network with a high employment rate at the time of the establishment closure should be more likely to find a new job and earn higher wages in this job than displaced workers embedded in a small network with a low employment rate.

To test these predictions and be able to compare my results to those in the existing literature, I adopt the specification proposed by Cingano and Rosolia (2012) and estimate the following linear-in-means model, where the dependent variable y_{it+1} is either an indicator variable taking the value one if a worker is observed working in the year after the establishment closure, or, if he is working, the log daily wage in the new job:

$$y_{it+1} = \alpha + \beta_1 ER_{it} + \beta_2 \log NS_{it} + \gamma' \mathbf{x}_{it} + \varepsilon_{it}. \qquad (1)$$

ER_{it} and NS_{it} represent the employment rate and network size of displaced worker i's network of former coworkers at the time of displacement t, and $\mathbf{x}_{it}$ is a comprehensive set of both individual and average network characteristics. The latter comprise the mean age of all former coworkers and its square, the share of former coworkers with medium and high educational attainment, the share of former coworkers that are women, and the share of former coworkers that are immigrants. As individual characteristics, I include a worker's educational attainment, potential experience and its square, immigrant status, tenure in the closing establishment, and the last log wage observed in the closing establishment. In addition, to describe a worker's employment history, I add dummies for the modal 3-digit industry in which the displaced worker has worked in the past, the number of years employed, the average annual wage growth, the average establishment size, and the number of distinct employers to the vector of individual regressors, all measured over the network building phase. This extensive set of individual control variables is meant to account for a large part of individual-level heterogeneity in the sample.

2.2. Identification

As is well known in the literature on social interaction effects, identification of the parameters of interest can be difficult because of the potential endogeneity of the network employment rate and size regressors in Eq. (1) (e.g. Moffitt, 2001).[7] In the present context, such endogeneity could arise as a result of common unobserved group level shocks that affect the outcomes of all workers who formerly worked together. It could also be the result of correlated unobservables due to either workers' sorting into particular establishments based on unobservable characteristics or common latent skills that were accumulated when workers worked together in the same establishment. For example, under sorting of more able workers into the same establishments, workers with high unobserved ability will tend to have a network consisting of other high ability workers, potentially leading to a spurious positive correlation between the outcomes of the displaced worker and the employment rate prevailing in his network. On the other hand, more able workers are likely to have worked in larger establishments and, thus, will tend to have a larger network size. Both the employment rate and the network size regressors in Eq. (1) are therefore potentially endogenous, leading to biased OLS estimates of both β_1 and β_2. The direction of the biases is a priori undetermined and depends on the correlations between the regressors and the unobserved error term ε_{it}, as well as their correlation with each other.[8]

To overcome these issues, I include, in a first step, fixed effects for the closing establishments in the estimation of Eq. (1) which account

[4] Alternatively, social networks may connect unemployed workers with potential employers by means of referrals, providing information about job match quality that both parties would otherwise not have (see, for example, the referral models by Montgomery (1991), and Simon and Warner (1992)).

[5] Note that the unambiguous prediction regarding the exit rate from unemployment is based on the assumption that an unemployed worker accepts any job offer. Workers, however, may increase their reservation wage in response to an increase in their job offer arrival rate, so that the net effect on unemployment duration is a priori ambiguous. However, under standard assumptions regarding the shape of the wage offer distribution, the relationship between the exit rate from unemployment and the job offer arrival rate is non-negative (see van den Berg, 1994).

[6] Alternative definitions of the length of the network building phase lead to similar results; see Section 4.1.1.

[7] Note that the reflection problem (Manski, 1993) often encountered when estimating linear-in-means models such as Equation (1) does not constitute a problem in the present context, since the groups of former coworkers generally differ across displaced workers due to the latter's heterogenous employment histories (see Bramoullé et al. (2009) and DeGiorgi et al. (2010).

[8] Consider a simplified version of the empirical model in Eq. (1): $y_i = \alpha + \beta_1 ER_i + \beta_2 NS_i + \varepsilon_i$. The asymptotic OLS bias of $\hat{\beta}_1$ can be shown to be $\text{plim}_{N\to\infty}\left(\hat{\beta}_1^{OLS} - \beta_1\right) = \frac{\sigma_{NS}^2 \sigma_{\varepsilon,ER} - \sigma_{NS,ER}\sigma_{\varepsilon,NS}}{\sigma_{NS}^2 \sigma_{ER}^2 - \sigma_{NS,ER}^2}$ where $\sigma_{NS}^2 = Var(NS_i)$ and $\sigma_{\varepsilon,NS} = Cov(\varepsilon_i, NS_i)$, and σ_{ER}^2, $\sigma_{\varepsilon,ER}$ and $\sigma_{NS,ER}$ are defined analogously (see, for example, Frölich (2008)). Since the denominator of the right-hand side term is positive, the sign of the bias is determined by the sign of the difference in the numerator. Note that the partial correlation between the employment rate and the network size variables in the data ($\sigma_{NS,ER}$) is 0.168.

for any unobserved shocks at the time of displacement that may be specific to the workers of a given establishment. Identification is then coming from variation in the employment rate and size of the coworker networks across workers who are being displaced from the same establishment.

However, even after conditioning on establishment fixed effects and a comprehensive set of control variables, it may still be that the relationship between a worker's outcomes and the employment rate and size of his network is a reflection of common latent skills shared by these workers. To deal with this issue, I exploit the occurrence of mass-layoffs as exogenous shocks to the employment rate in a network. Suppose a displaced worker's network at the time of displacement consists of NS_{it} coworkers with whom he had worked during the network building phase. After separating from each other, some of these former coworkers may themselves have been part of a mass-layoff. Following the literature, I define a mass-layoff to occur if a large establishment's workforce declines by at least 30 percent from one year to the next (see, for example, Jacobson et al., 1993, or von Wachter et al., 2009). Under the assumption that these mass-layoffs are unrelated to the displaced workers' unobserved skill sets, they can serve as exogenous shocks to their network of former coworkers.

Following this reasoning, I construct for each displaced worker a measure of the share of his former coworkers who were themselves part of a mass-layoff after separating from the displaced worker. I then use this share as an instrument for the network employment rate ER_{it} in Eq. (1). More precisely, let E_{it-s} be the number of former coworkers who are employed in year $t-s$, and let M_{it-s} be the number of these coworkers who, after separation from displaced worker i, were in that year part of a mass-layoff in a large establishment with more than 50 employees. Then, letting t denote the year of the establishment closure that pushes displaced worker i into unemployment, the instrumental variable is calculated as

$$Z_{it}=\sum_{s=1}^{5}\frac{M_{it-s}}{E_{it-s}}.$$

In the construction of the instrument, I use the number of *working* coworkers in the denominator rather than their overall number to avoid any variation in the instrument stemming from differences in the number of former coworkers who are employed and thus at the risk of being (mass-)laid off. Furthermore, I focus on mass-layoffs in large establishments as these are more likely to represent exogenous shocks. Finally, I ensure that in the rare cases in which a given coworker experiences more than one mass-layoff after separation, only the last layoff is counted.[9]

The identifying assumption required to obtain a consistent estimate of the parameter β_1 is that, conditional on the set of control variables (including the network size), the unobserved error term is mean independent of the instrument, $E[\varepsilon_{it}|Z_{it}, NS_{it}, \mathbf{x_{it}}]=E[\varepsilon_{it}|NS_{it}, \mathbf{x_{it}}]$.[10] While one may be concerned that this exclusion restriction is violated unconditionally since a higher occurrence of mass-layoffs among a worker's former coworkers in the past may indicate a generally weaker labour market for this worker today, I argue that the conditioning on establishment fixed effects (which implicitly control for highly specific industry characteristics) and detailed individual characteristics ensures that co-displaced workers face the same labour market conditions upon displacement and that past mass-layoffs affect these workers' relative post-displacement outcomes only through their effect on the prevailing network employment rates. In Section 4.1.2, I provide evidence in support of the identifying assumption by means of a series of placebo tests in which I relate contemporaneous values of the instrument to past labour market outcomes and future values of the instrument to contemporaneous labour market outcomes. Note that in the absence of an additional instrument, the network size variable $\log(NS_{it})$ only serves as a control variable in the estimation and that $\widehat{\beta}_2$ therefore has no causal interpretation. Much of the discussion of the empirical results will thus focus on $\widehat{\beta}_1$, which measures the effect of a network's employment rate on a displaced worker's labour market outcomes, holding the overall network size constant.

3. Data and descriptives

The data used in the analysis derive from administrative records and comprise the universe of workers in Germany who are subject to social security contributions.[11] The observations in the sample are recorded annually on the 30th of June, and span more than two decades, from 1980 to 2001. Each record contains a unique worker and establishment identifier as well as information about some key background characteristics of a worker, such as occupation, industry, citizenship, and education. To improve the consistency of the education variable, I apply the imputation algorithm suggested by Fitzenberger et al. (2006). From this data base, I construct a panel data set of all establishments operating in one of the four largest metropolitan areas in Germany: Hamburg, Cologne, Frankfurt, and Munich. These metropolitan areas are relatively large, covering, on average, an area of around 2150 square miles and a population of 2.9 million individuals in 1995, distributed over, on average, 8 counties (*Kreise*) and 200 municipalities (*Gemeinden*). I do not include Berlin which is a special case due to German unification in 1990.

The base years for my analysis are the years 1995 and 1996. The motivation for choosing these particular years it that they provide a sufficiently long pre- and post-displacement period which allows controlling for the labour market histories of the workers as well as studying longer-run effects. I obtain a list of all establishments that exist in those years but do not exist anymore in the following year (8.7 percent of all active establishments). From this set of establishments, I select those that had between 5 and 50 workers in the last year of business (13.2 percent of the sample of closing establishments) and for whom the maximum share of displaced workers who end up working together in another establishment in the year after the establishment closure is smaller than 50 percent, leaving a total of 1814 establishments in the sample. Let me briefly justify these sample restrictions. I exclude very small establishments since these are often family-run and provide insufficient variation in network structure within establishments. I exclude establishments with more than 50 employees to keep the sample of displaced coworkers operational and reduce the scope for unreasonably large networks of former coworkers. Overall, 30.7 percent of all active establishments have between 5 and 50 employees in the years considered, employing in total 28.4 percent of all workers. The reason for excluding establishments whose majority of the workforce continuous to work together in a new establishment after the closure is to avoid including establishments that simply change their legal status, for example through mergers or takeovers, in which case they would receive new identifiers and hence appear as new establishments in the data (for a similar procedure, compare Schmieder et al., 2009).

[9] The empirical results are robust to alternative ways of constructing the instrument, such as $\sum_{s=1}^{5}M_{it-s}/\sum_{s=1}^{5}E_{it-s}$, or $\sum_{s=1}^{5}M_{it-s}/\sum_{s=1}^{5}L_{it-s}$, where L_{it-s} is the number of former coworkers employed in large establishments in period $t-s$.

[10] Considering again the simplified version of the empirical model, the asymptotic bias of the IV estimator of $\widehat{\beta}_1$ can be shown to be $\text{plim}_{N\to\infty}\left(\widehat{\beta}_1^{IV}-\beta_1\right)=\frac{\sigma_{NS}^2\sigma_{\varepsilon,Z}-\sigma_{NS,Z}\sigma_{\varepsilon,NS}}{\sigma_{NS}^2\sigma_{ER,Z}-\sigma_{NS,ER}\sigma_{NS,Z}}$. Under the conditional mean independence assumption and the additional assumption that the error term is linear in the set of control variables, so that $E[\varepsilon_i|NS_i]=\theta+\gamma NS_i$, the instrumental variable estimate $\widehat{\beta}_1^{IV}$ will be consistent since $\sigma_{\varepsilon,Z}=\gamma\sigma_{NS,Z}$ and $\sigma_{\varepsilon,NS}=\gamma\sigma_{NS}^2$ (see Frölich, 2008).

[11] In 2001, 77.2% of all workers in the German economy were covered by social security and are hence recorded in the data (Bundesagentur, 2004). The main groups not included in the data are civil servants, the self-employed, and military personnel.

In the next step, I collect information about all male workers who are working in these establishments in their last year of existence and who have been present in the German labour market during the entire network building phase. In practice, this means that a worker must have been observed for the first time in the data at the latest in the first year of the network building phase, $t-5$, ensuring a better comparability of co-displaced workers with respect to their network characteristics. The resulting group of workers constitutes the sample of displaced workers. I focus on male workers as these are more likely to remain attached to the labour market and try to find a new job after displacement. Women, for instance, may decide to withdraw entirely from the labour force or move fertility decisions forward as a result of their displacement. There may also be peer effects in fertility among former coworkers, as documented by Asphjell et al. (2014), which would further complicate the analysis and interpretation of the empirical results. For each displaced worker included in the final sample, I then collect information on all other workers he has ever worked with in the same establishment.[12]

For the construction of the network-related variables, I only consider those coworkers who worked together with a given displaced worker during the network building phase, defined as the 5-year period prior to the corresponding base year, but not including it. So, for example, the network building phase for those workers who were displaced in 1995 extends from 1990 to 1994. Consequently, other contemporaneously displaced workers are not included in the set of coworkers. The motivation for this restriction is that these workers become unemployed at the same time as the displaced worker and should therefore be unable (and unwilling) to provide any information about new job opportunities. I restrict the network building phase to the preceding five years based on the assumption that the strength of a link between two workers depreciates over time: any link between two workers that was last active more than 5 years ago (by means of working together in the same establishment) has ceased to provide any information to the displaced worker in the year of displacement. Finally, I trim the estimation sample by dropping those displaced workers with a network size above the 95th percentile.[13]

Table 1
Summary Statistics - Worker Sample.

Number	10,916
Share Foreign	13.8
Average Age	40.0
Share in	
Hamburg	25.3
Cologne	26.7
Frankfurt	24.5
Munich	23.6
Share in	
1995	44.3
1996	55.7
Educational Attainment	
Share missing education	2.1
Share low education	8.7
Share medium education	81.0
Share high education	8.2
Industry	
Agriculture	1.2
Construction	15.3
Manufacturing, low tech	9.4
Manufacturing, basic	15.7
Manufacturing, high tech	4.2
Communications, transport & utilities	10.2
Wholesale	13.7
Retail	7.3
Prof., med. and business services	15.1
Education & Welfare	0.8
Public administration	0.1
Other services	6.9

Note: The table reports descriptive statistics of the sample of male workers who become unemployed as the result of an establishment closure in the Hamburg, Cologne, Frankfurt and Munich metropolitan area in the years 1995/1996. The sample comprises 1814 establishments with between 5 and 50 employees in the year of their closure.

Table 1 shows descriptive statistics for the sample of displaced male workers. Overall, there are 10,916 workers who become unemployed as the result of 1814 establishment closures in the Hamburg, Cologne, Frankfurt and Munich metropolitan areas in 1995 and 1996. Around 13.8 percent of these workers are foreign citizens. Most of the displaced workers in the sample have medium education which in the German context refers to vocational training. Around 10.8 percent do not have vocational training or have missing information about their educational attainment, and about 8.2 percent of workers have university education. In terms of sectoral composition, large numbers of displaced workers worked in basic manufacturing, construction, and professional, medical and business services in their last job.

Table 2 provides information about some key summary statistics for the period before the establishment closure (top panel), the year after the establishment closure (middle panel), and the instrumental variable (bottom panel). During the network building phase, the average number of distinct coworkers a displaced worker worked with was 133. This average figure conceals the fact that the (trimmed) network size distribution is strongly right-skewed, with a 10th percentile of 9, a median of 43, and a 90th percentile of 379. The average employment rate in the networks of former coworkers in the years of displacement – the main variable of interest in the empirical analysis – is 58.4 percent with a standard deviation of 18.0 percent, varying between 35.1 percent at the 10th percentile and 79.1 percent at the 90th percentile. On average, a displaced worker worked 3.0 years with his former coworkers although there is substantial variation, reaching from one year at the 10th percentile to 7 years at the 90th percentile. Between 1980 and 1995/1996, the average displaced worker was employed for about 12.4 years, working in 3.5 different establishments and spending around 3.4 years in each of them. The average tenure in the establishment that eventually closes down in either 1995 or 1996 is somewhat larger, about 5.1 years. The average daily wage earned in the closing establishments is around 88.1 euros.[14] During the 5-year long network building phase, displaced workers are observed working 4.4 years in, on average, 2.1 distinct establishments with typically around 64.8 employees. Finally, while the average rate of annual wage growth of 26.7 percent is driven by a few extreme outliers and therefore not particularly meaningful, the median annual wage growth during the network building phase is 1.7 percent.

The middle panel of Table 2 shows workers' employment and wage outcomes in the first year after their displacement. Overall, 71.6 percent of displaced workers are working again in the year after

[12] Note that while the sample only includes closing establishments in the Hamburg, Cologne, Frankfurt, and Munich areas, the construction of the network and employment history variables is based on prior jobs in all of Germany.

[13] The distribution of network sizes across displaced workers is strongly right-skewed, with a median of 48, a mean of 508 and a 95th percentile of 1416 former coworkers. The trimming of the data avoids results being driven by extreme outliers in the network size variable. However, the empirical results are also robust to using the full sample of displaced workers.

[14] Wage records are right-censored at the social security contribution ceiling, which affects 6.4 percent of observations in 1995/1996. I impute these wages by first estimating a tobit model with a standard set of socio-economic wage determinants (gender, citizenship, education, region and industry) and then adding a random error term to the predicted value of each censored observation, ensuring that the imputed wage lies above the censoring threshold (see Gartner (2004) for details). I proceed correspondingly for all censored wage observations in later years of the sample.

Table 2
Summary statistics - before and after the establishment closure.

	Mean	Standard Deviation	10th Percentile	50th Percentile	90th Percentile
Before closure					
No. of coworkers (last 5 years)	132.6	228.1	9	43	379
Share of coworkers working in t (in %)	58.4	18.0	35.1	60.0	79.1
Duration of cowork	3.0	2.9	1	2	7
Overall work experience	12.4	4.2	6	14	17
No. of establishments worked at	3.5	2.0	1	3	6
Establishment tenure	3.4	3.6	1	2	8
Tenure in closing establishment	5.1	4.9	1	3	15
Last wage in closing establishment	88.1	44.9	49.6	77.7	135.1
Last log wage in closing establishment	4.37	0.47	3.90	4.35	4.91
No. of years working (last 5 years)	4.4	1.0	3	5	5
No. of establishments worked at (last 5 years)	2.1	1.0	1	2	3
Average establishment size (last 5 years)	64.8	109.3	8.0	29.0	154.0
Annual wage growth (last 5 years) (in %)	26.7	1958.9	-4.3	1.7	20.4
After closure					
Share DW working in t+1	71.6	45.1	0	1	1
Wage in t+1	87.6	42.8	51.0	77.4	134.3
Log wage in t+1	4.38	0.44	3.93	4.35	4.90
Instrumental variable					
Share of coworkers in mass-layoffs (in %)	4.2	10.9	0.0	0.0	10.0
Residual share (establishment FE) (in %)	0.0	9.0	−6.1	−0.2	3.0
Residual share (establishment/education FE) (in %)	0.0	8.4	−5.1	−0.0	2.4

Note: Sample comprises 10,916 displaced male workers who had coworkers who were not themselves displaced workers. The instrumental variable is the share of working former coworkers who, after separation, worked in a large establishment (>50 employees) and separated from that establishment as part of a mass-layoff. Statistics on the residual shares are obtained by regressing the instrument on the specified fixed effects and summarizing the residuals.

displacement, earning, on average, a daily wage of 87.6 euros which is about the same as the average wage in the sample prior to displacement.

The bottom panel of Table 2 provides some summary statistics for the instrumental variable used in the analysis. On average, 4.2 percent of a displaced worker's working former coworkers were laid off from an establishment with more than 50 employees after they separated from the displaced worker. As indicated by the standard deviation of 10.9 percent, there is quite some variation in this share across displaced workers, reaching from 0 percent at the 10th percentile to 10.0 percent at the 90th percentile (and 21.2 percent at the 95th percentile). In the second row in the bottom panel, I eliminate variation in the average share of laid-off coworkers that exists between different closing establishments by regressing these shares on a full set of fixed effects for the closing establishments and then summarizing the predicted residual shares. Importantly, the standard deviation only decreases by 17.4 percent to 9.0 percent, indicating that most of the variation in the instrumental variable exists within rather than between establishments. This is important because I control for establishment fixed effects in the analysis and rely on differential shocks to the networks of workers that belong to the same closing establishment. As the last row shows, even within groups of workers with the same education level who are being displaced from the same establishment, there is still variation in the share of laid-off coworkers. The relatively small loss in identifying variation after the inclusion of comprehensive sets of fixed effects provides some first suggestive evidence that these mass-layoffs may indeed be exogenous events.

4. Empirical results

4.1. Main results

In this section, I investigate how the prevailing employment rate in a displaced worker's network of former coworkers at the time of displacement affects his re-employment probability and wages one year later. As pointed out before, in the absence of a suitable instrument, I will view the network size variable as an additional control variable whose estimated impact on labour market outcomes can generally not be given a causal interpretation. The first three columns in Table 3 show the results of a linear probability model for being employed based on Eq. (1). Column (1), which includes all the individual and average network characteristics but no establishment fixed effects, shows a small positive association between the employment rate in the network and the probability of being employed one year after displacement.[15] Including a full set of fixed effects for the closing establishments in column (2) reduces the coefficient to a statistically not significant 0.051. However, as discussed in Section 2, in the presence of endogeneity of the employment rate and network size control variable, the estimated OLS parameters are likely to be biased. Column (3) shows the results when the networks' employment rates are instrumented with the shares of coworkers who were themselves part of a mass-layoff after separation from the displaced workers. As the summary of the first stage regression at the bottom of the table shows, there is a strong negative effect of this share on a network's employment rate in the year of displacement: a 10 percentage point increase in the share of laid-off coworkers during the network building phase reduces their employment rate in 1995/1996 by 1.3 percentage points, with an F-statistic for the excluded instrument of 65.1 and a partial R-squared of 0.0141. Fig. 1 shows the corresponding relationship between the residualized network employment rates and shares of coworkers affected by mass-layoffs. Reassuringly, the first stage relationship does not appear to be driven by individual outliers. Exploiting the variation in employment rates that is driven by these mass-layoff shocks, the second-stage results indicate a strong positive effect of the network employment rate on the re-employment probability of a displaced worker: a 10 percentage point increase in the employment rate, which corresponds to about half a standard deviation (compare Table 2), leads to a 7.5 percentage point higher probability of being employed in the first year after displace-

[15] An unconditional OLS regression that only includes the network employment rate and the network size as regressors yields significant estimates of 0.118 (0.027) and 0.007 (0.004), respectively.

Table 3
Employment and Wage Effects.

	(1)	(2)	(3)	(4)	(5)	(6)
		Employment Probability			Log Wages	
	OLS		IV	OLS		IV
Employment rate coworkers	**0.073****	**0.051**	**0.753***	**0.054****	**0.067***	**0.174**
	[0.031]	**[0.048]**	**[0.387]**	**[0.023]**	**[0.036]**	**[0.264]**
Log number of coworkers	0.002	0.004	−0.015	0.006	0.008	0.005
	[0.005]	[0.007]	[0.013]	[0.004]	[0.005]	[0.009]
Tenure in closing establishment	−0.001	−0.001	0.000	0.001	0.001	0.001
	[0.002]	[0.002]	[0.002]	[0.001]	[0.001]	[0.001]
No. of years working (last 5 years)	0.043***	0.040***	0.050***	0.009*	0.004	0.006
	[0.005]	[0.006]	[0.008]	[0.005]	[0.005]	[0.007]
Average establishment size (last 5 years)	0.000	0.000	0.000	0.000	0.000	0.000
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Annual wage growth (last 5 years)	−0.000***	−0.000***	−0.000***	−0.082***	−0.062***	−0.064***
	[0.000]	[0.000]	[0.000]	[0.019]	[0.019]	[0.019]
Last log wage in closing establishment	0.020	0.024	0.021	0.697***	0.666***	0.666***
	[0.012]	[0.016]	[0.016]	[0.024]	[0.035]	[0.035]
Average age coworkers	−0.002	−0.005	−0.005	0.003	0.021***	0.020***
	[0.010]	[0.012]	[0.012]	[0.006]	[0.008]	[0.008]
Average age coworkers squared	0.000	0.000	0.000	0.000	−0.000***	−0.000***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Share coworkers medium education	−0.011	0.010	−0.006	0.080***	0.028	0.025
	[0.032]	[0.042]	[0.043]	[0.024]	[0.030]	[0.030]
Share coworkers high education	−0.110*	−0.137*	−0.156*	0.203***	0.061	0.06
	[0.064]	[0.080]	[0.082]	[0.055]	[0.063]	[0.063]
Share female coworkers	−0.054*	−0.079**	−0.074*	−0.011	−0.103***	−0.102***
	[0.030]	[0.037]	[0.038]	[0.024]	[0.029]	[0.029]
Share immigrant coworkers	−0.040	−0.044	0.059	−0.003	−0.041	−0.026
	[0.041]	[0.059]	[0.084]	[0.031]	[0.039]	[0.053]
Establishment fixed effects		yes	yes		yes	yes
Observations	10,916	10,661	10,659	7,789	7,435	7,433
Number of groups		1,559	1,558		1,375	1,374
1st stage statistics employment rate coworkers						
Share coworkers in mass-layoffs			−0.131***			−0.138***
			[0.016]			[0.021]
F-stat 1st stage			65.1			42.7

Note: Additional controls included in all specifications are the displaced worker's potential experience and its square, education level and immigrant status, as well as dummies for the number of different employers and dummies for the main sector of activity during the network building phase. The instrumental variable for the coworker employment rate is the share of working former coworkers who, after separation, worked in a large establishment (>50 employees) and separated from that establishment as part of a mass-layoff. Standard errors are robust and clustered at the closing establishment level.
*denotes statistical significance at the 10% level.
**at the 5% level.
***at the 1% level.

ment (p-value 0.052). This effect is larger in magnitude than the main estimate in the study by Cingano and Rosolia (2012), who, based on an OLS specification similar to that in column (2) of Table 3, find that a 10 percentage point increase in the network employment rate increases the probability of being employed 9 months after displacement by 1.8 percentage points. Since the German data are recorded annually, the duration between displacement and the first observed post-displacement period is typically less than one year; 6 months on average under the assumption that establishment closures are uniformly distributed over time. Linearly extrapolating the results for 12 and 9 months, separately reported in Cingano and Rosolia (2012), to a horizon of 6 months would imply a 2.4 percentage point higher employment probability in response to a 10 percentage point increase in the network employment rate. Note that my estimate does not lend itself to calculating a traditional "social multiplier" by applying the transformation $1/(1-\widehat{\beta}_1)$ since it measures the effect of a change in the network employment rate on the probability of finding a job for *unemployed workers* rather than the effect on the overall employment probability in the network (compare Rege et al., 2012).[16]

[16] For illustration, suppose the initial employment rate in a network is given by ER^* and consider an exogenous increase in this rate by P percentage points. The estimate of β_1 then suggests that besides the direct effect, an additional share $\beta_1 P(1-ER^*-P)$ of the network members find a job as the result of social interaction effects, where

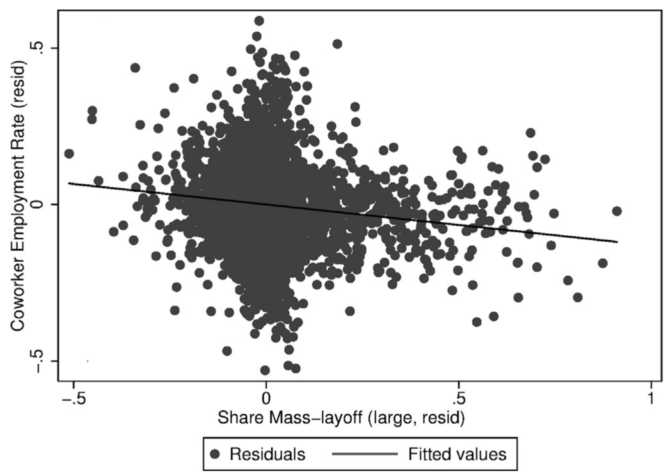

Fig. 1. First-stage relationship. Note: The plot shows the first-stage relationship between the residualized network employment rates and the residualized shares of working former coworkers who, after separation, worked in a large establishment (> 50 employees) and separated from that establishment as part of a mass-layoff (corresponding to column (3) of Table 3).

Columns (4) to (6) in Table 3 show the corresponding results for the log wages in the new jobs of the displaced workers. While in the OLS regressions, there is some indication of a small positive association between the network employment rate and workers' starting wages, there is no evidence of a causal effect of a higher network employment rate on the starting wages after accounting for potential endogeneity. The absence of a significant effect on starting wages can be rationalised within the theoretical framework laid out in Section 2 if workers do not adjust their reservation wages in response to changes in their job offer arrival rate due to shocks to their coworker networks, either because these changes are unanticipated or because workers do not expect them to be persistent. The absence of a causal effect of the network employment rate on entry wages also speaks against potential alternative mechanisms that could lead to a positive association between average network employment rates and individual-level employment probabilities, such as leisure complementarities (Jenkins and Osberg, 2004) or social norms (Akerlof, 1980; Stutzer and Lalive, 2004). If these channels were the main drivers of individuals' job search behaviour, workers with higher prevailing employment rates in their networks should set lower reservation wages in order to increase their chances of escaping unemployment, and thus end up working in lower-paid jobs.

The results reported in Table 3 – particularly for the employment estimations – provide some suggestive evidence for a downward bias of the OLS estimates relative to the IV estimates.[17] As discussed in Section 2, this could be driven by the two network regressors' endogeneity and their correlation with each other. However, due to the nature of the instrumental variable, both estimates are also likely to capture different types of effects. The IV estimate is based on variation in a network's employment rate due to mass-layoffs of workers whereas the variation for the OLS estimate is driven by more diverse determinants of workers' employment statuses, including retirement, health or family reasons. Since laid-off workers are presumably quite attached to the labour market, they are more likely to keep a job information to themselves rather than pass it on than, for example, retired former coworkers or former female coworkers on maternity leave. If still unemployed, laid-off former coworkers may also be directly competing with displaced workers for information about new job opportunities. As a result, a given change in the network employment rate due to mass-layoffs is likely to have a bigger effect on an individual worker's labour market outcomes than a corresponding change due to alternative reasons.

4.1.1. *Robustness checks*

Table 4 summarizes the results from a number of robustness checks for the main result of a positive effect of the employment rate in a worker's network on his re-employment probability. The corresponding results for the starting wages, which I do not report here explicitly, consistently show small and statistically not significant effects. In column (1), I condition on establishment/education fixed effects rather than just establishment fixed effects as in the baseline specification. By doing so, I compare the employment outcomes of workers who are displaced from the same establishment and who have the same education level prior to their displacement. This specification leads to an estimate of 0.658 (p-value 0.101), broadly comparable in magnitude to the baseline result of 0.753 (compare column (3) of Table 3).

Column (2) shows the results from an estimation without any worker- or network-specific control variables (apart from network size and establishment fixed effects). Given that a number of control variables are highly statistically significant in the baseline IV regression, the fact that the point estimate on the network's employment rate hardly changes in the absence of these control variables indicates that the instrument is not systematically related to these observable characteristics, providing some suggestive evidence for its exogeneity also with respect to unobservable characteristics.

Columns (3) presents results using an alternative definition of the instrumental variable where I extend the range of establishments that are considered for the construction of the mass-layoff instrument to those with more than 10 employees (rather than using only large establishments with more than 50 employees). As these mass-layoffs occur more often, the first stage is substantially stronger. However, the point estimate of 0.601 is still similar in magnitude to the estimate in the baseline specification.

In column (4), I add the average log wage among the group of former coworkers at the time of displacement as a further control variable to capture an additional dimension of the quality of a displaced worker's network. Again, results remain largely unchanged.

According to the information transmission process outlined at the beginning of Section 2, employed agents forward any job information they do not need themselves to one of their unemployed contacts. From the perspective of a recently displaced worker, this implies that unemployed two-link away contacts–those workers who worked together with one of the former coworkers during the network building phase but never with the displaced worker himself–are competitors for available job information. In column (5), I include the employment rate and log number of each displaced worker's two-link away contacts as additional regressors, instrumenting the employment rate with the share that was part of a mass-layoff in a large establishment during the network building phase (constructed in the same way as for the direct contacts). Contrary to the theoretical prediction, the employment rate among the two-link away contacts has no significant effect on a displaced worker's probability of working in the year after displacement. However, due to a weaker first stage, standard errors are quite large so one cannot rule out fairly substantial positive or negative effects with certainty. Reassuringly, though, the point estimate of the coefficient for the employment rate of the direct contacts remains relatively unaffected, with a value of 0.677 (p-value 0.103).

(*footnote continued*)
$(1 - ER^* - P)$ is the share of the remaining unemployed workers in the network. Thus the total effect of the increase in the network employment rate is given by $P + \beta_1 P(1 - ER^* - P)$, which implies a social multiplier of $1 + \beta_1(1 - ER^* - P)$. An initial employment rate of 70 percent that is exogenously increased by 10 percentage points would then, for example, lead to a moderate social multiplier of 1.15, given the estimate of $\widehat{\beta}_1 = 0.753$ reported in column 3 of Table 3.

[17] Accounting for conditional heteroskedasticity and the clustering of the error terms, a generalized Hausman test for the consistency of the OLS estimator yields a Chi-squared test statistic of 3.388 (p-value of 0.0657) for the employment regression and 0.164 (p-value of 0.6857) for the wage regression.

Table 4
Robustness Checks Employment.

	(1) Establishment/Education Fixed Effects	(2) No Controls	(3) Using Mass-layoffs in Medium and Large Establishments	(4) Wage Control	(5) Competitors	(6) Network Size Control
Employment rate coworkers	**0.658**	**0.644***	**0.601****	**0.829***	**0.677**	**0.856****
	[0.402]	**[0.379]**	**[0.260]**	**[0.436]**	**[0.415]**	**[0.417]**
Log number of coworkers	−0.012	−0.008	−0.011	−0.007	−0.005	
	[0.014]	[0.015]	[0.010]	[0.011]	[0.015]	
Average log wage coworkers in t				−0.070		
				[0.048]		
Employment rate two-link away contacts					−0.240	
					[0.452]	
Log number two-link away contacts					−0.005	
					[0.012]	
Number of coworkers (in 1000)						−0.101
						[0.140]
Number of coworkers (in 1000) squared						0.082
						[0.104]
Observations	10,007	10,659	10,659	10,502	10,642	10,659
Number of groups	1,822	1,558	1,558	1,528	1,555	1,558
F-Stat/Kleibergen-Paap statistic 1st stage	67.1	49.5	142.0	59.0	26.8	59.1

Note: For a list of the additional control variables included (apart from column (2)), see Table 3, column (3). Apart from columns (3) and (4), the instrumental variable for the coworker employment rate is the share of working former coworkers who, after separation, worked in a large establishment (>50 employees) and separated from that establishment as part of a mass-layoff. In column (1), a full set of establishment/education group fixed effects is included. In column (2), the only control variables included are the network size and a full set of establishment fixed effects. In column (3), the instrument is calculated for mass-layoffs in medium and large establishments (>10 employees). In column (4), the average wage of the coworkers in the network is included as an additional control variable. In column (5), the employment rate and log number of a displaced worker's two-link away contacts are included as additional control variables and the employment rate instrumented with the share of two-link away contacts who worked in a large establishment (>50 employees) and separated from that establishment as part of a mass-layoff. In column (6), actual numbers of coworkers (in 1000) and their square are used rather than their log to control for the overall network size. Standard errors are robust and clustered at the closing establishment level. A (***) at the 1% level.
*denotes statistical significance at the 10% level.
**at the 5% level.

Finally, in column (6), I check the robustness of my results to the functional form specification of the network size control variable. Rather than including the log number of coworkers, I include a quadratic function that allows for a non-monotonic relationship between network size and the re-employment probability of the displaced worker. In this case, the estimated parameter is slightly larger than in the baseline specification but still of comparable magnitude.[18] Overall, the results in Table 4 show a robust positive effect of the network employment rate on the re-employment probability of a displaced worker, with the baseline estimate of 0.753 being roughly the midpoint of the range of estimates obtained across the alternative specifications.

In further unreported heterogeneity analysis, I investigate the impact of the network employment rate on the re-employment probability of different demographic subgroups of displaced workers. However, due to small sample sizes, there is not enough variation to statistically distinguish the estimated effects for these different subgroups. Taken at face value, however, the point estimates suggest a stronger network effect for low-educated, young and immigrant workers, in line with survey evidence and existing studies analysing the role of networks in the labour market (see Topa et al., 2011).

[18] I have also allowed for alternative lengths of the network building phase with similar results. For example, allowing workers to remain connected to their former coworkers for up to eight years after separation yields a point estimate in the IV employment regression of 0.836 (0.408); extending the network building phase even further to ten years gives an estimate of 0.653 (0.446).

4.1.2. Placebo tests

The identification of the causal effect of the network employment rate on a displaced worker's post-displacement labour market outcomes hinges upon the assumption that any unobserved individual-level characteristics that may affect these outcomes directly are conditionally mean independent of the instrument. To support the validity of this assumption, I carry out two placebo tests. In the first test, I replace the actual post-displacement outcomes in my estimations with the corresponding outcomes observed prior to the network building phase. If mass-layoffs of a displaced worker's coworkers during the network building phase are exogenous events, they should be uncorrelated with any unobserved permanent individual characteristics that may affect labour market outcomes directly, and therefore uncorrelated with a displaced worker's past employment outcomes.

Columns (3) and (4) of Table 5 show the corresponding results, where the dependent variable is the displaced worker's employment status in 1989 (for displacement year 1995) and 1990 (for displacement year 1996). I report both the reduced form effect of the instrument, and the second-stage results from the standard IV estimation. For comparison, I report the corresponding estimates from the baseline specification (compare Table 3) in columns (1) and (2). Focussing on the reduced form first, column (3) shows that, as required for the validity of the instrument, there is no significant relationship between the share of laid-off coworkers and the displaced worker's past employment probability, with an estimate of 0.017 which is close to zero and statistically not significant. This compares to a significant coefficient of −0.099 (0.050) in the baseline specification reported in column (1), which in itself is an interesting parameter

Table 5
Placebo Tests.

	(1) Benchmark	(2)	(3) Placebo Past	(4)	(5) Placebo Future
	Reduced Form	IV	Reduced Form	IV	Reduced Form
Employment rate coworkers		**0.753*** **[0.387]**		**−0.127** **[0.333]**	
Share coworkers in mass-layoffs	**−0.099**** **[0.050]**		**0.017** **[0.044]**		
Future share coworkers in mass-layoffs					**−0.056** **[0.048]**
Observations	10,659	10,659	10,659	10,659	10,622
Number of groups	1,558	1,558	1,558	1,558	1,552
1st stage statistics					
Share coworkers in mass-layoffs		−0.131*** [0.016]		−0.131*** [0.016]	
F-stat 1st stage		65.1		65.1	

Note: The dependent variable in these estimations is the employment status in the year before the start of the network building phase, t−6 (columns (3) and (4)), or the employment status in the year after displacement, t+1 (columns (1), (2) and (5)). Included control variables are the same as in Table 3, column (3). Columns (1) and (2) refer to the benchmark model (compare Table 3). The instrumental variable used in columns (2) and (4) for the coworker employment rate is the share of working former coworkers who, after separation, worked in a large establishment (>50 employees) and separated from that establishment as part of a mass-layoff. Column (5) shows reduced form results using the future share of working former coworkers who worked in a large establishment (>50 employees) and separated from that establishment as part of a mass-layoff between t+1 and t+5. Standard errors are robust and clustered at the closing establishment level.
*denotes statistical significance at the 10% level.
**at the 5% level.
***at the 1% level.

as it reflects how the effect of an employment shock in one part of the network propagates through connected parts of the network in subsequent periods.[19] As a result of the close to zero reduced form estimate for the past employment outcomes in column (3), the second-stage coefficient of -0.127 reported in column (4) is also close to zero. The corresponding unreported placebo results for wages show effectively zero reduced form coefficients both in the benchmark case and for past wages, and hence small and insignificant second-stage coefficients.

The second placebo test speaks to the validity of the exclusion restriction and here, in particular, the assumption that past mass-layoffs of former coworkers have no direct effect on workers' post-displacement outcomes. This assumption would be violated if, after controlling for establishment fixed effects and individual characteristics, past mass-layoffs continued to reflect current worker-specific labour market conditions, a possible scenario if mass-layoffs were driven by secular declines in the demand for the specific skills shared by connected workers. However, if that was the case, future mass-layoffs among former coworkers should likewise proxy for declining skill demand and thus have a similar effect on current labour market outcomes. To test for such symmetry, I change the time period over which I construct the instrumental variable and estimate the relationship between a displaced worker's employment status in the year after displacement (t+1) and the share of former coworkers displaced as the result of a mass-layoff during the subsequent period t+1 to t+5.[20] The reduced form results in column (5) of Table 5 show a substantially smaller and not significant effect of future mass-layoffs on current outcomes which speaks against the hypothesis that mass-layoffs of coworkers are simply a reflection of displaced workers' individual labour market prospects.

Overall, the results from the two placebo tests indicate that there is no relationship between either the instrumental variable and past labour market outcomes of the displaced workers or between the future instrumental variable and their current labour market outcomes, lending support to the assumption that, conditional on observables, the underlying mass-layoffs are exogenous events unrelated to the displaced workers' unobserved characteristics and labour market conditions.

4.2. Heterogeneity across information providers

By focussing on the overall employment rate in a worker's network of former coworkers, I have so far implicitly assumed that each contact is equally likely to transmit labour market relevant information to the displaced workers. However, it is conceivable that some coworkers are better providers of job information than others, either because they are more likely to hear about relevant job offers or because they are more likely to forward this information to the displaced worker.

Focussing on the latter aspect first, it is well known from sociological evidence that individuals tend to associate with other individuals who are similar in terms of socio-economic characteristics (e.g. McPherson et al., 2001). If such homophily is reflected in a higher propensity to exchange job information, the employment rate of coworkers who are similar to the displaced workers should have a larger effect on the latter's labour market outcomes than the employment rate of dissimilar coworkers. To test this prediction, I define similarity in terms of either age, gender, education or nationality, and split the overall employment rate in worker i's network at the time of displacement, $ER_{it} = E_{it}/NS_{it}$, into the share of workers who are similar, S_{it}/NS_{it}, and the share of workers who are dissimilar, D_{it}/NS_{it}, in the

[19] In an extension, I broke down the instrument in the reduced form baseline specification (Table 5, column (1)) into mass-layoffs that occurred in the two years prior to displacement and mass-layoffs that occurred three to four years before displacement. While statistically not different, the corresponding estimates of -0.115 (0.062) and -0.052 (0.096) point towards more recent mass-layoffs as being more detrimental to post-displacement employment outcomes, which in turn suggests that localised employment shocks are dynamically transmitted through the coworker network, fading out as their impact on the prevailing network employment rates diminishes over time.

[20] Note that if a given coworker is subject to more than one mass-layoff between t+1 and t+5, I use the earlier one for the construction of the instrumental variable.

Table 6
Heterogeneity Across Information Providers (1).

	(1) Age Cohort	(2) Gender	(3) Education	(4) Nationality
ER - same characteristic	**1.133***** **[0.429]**	**0.629*** **[0.371]**	**0.645*** **[0.383]**	**0.668*** **[0.386]**
ER - different characteristic	**0.568** **[0.391]**	**1.154*** **[0.604]**	**1.031**** **[0.458]**	**0.974**** **[0.461]**
Observations	10,659	10,659	10,659	10,659
Number of groups	1,558	1,558	1,558	1,558
Kleibergen-Paap statistic 1st stage	33.3	36.0	32.1	32.0
P-value equality coefficients	0.03	0.28	0.15	0.29

Note: For a list of the additional control variables included, see Table 3, column (3). Group-specific employment rates are calculated by splitting the overall employment rate in a network E/NS into subcomponents S/NS and D/NS, where S and D are the number of employed workers in each subgroup, and S+D=E. The instruments are constructed accordingly. In column (1), coworkers are distinguished by their age cohort, defined by +/- five years of the displaced worker's age. In column (2), coworkers are distinguished by their gender. In column (3), coworkers are distinguished by their education level, either without vocational training (or missing information), with vocational training, or with college education. In column (4), coworkers are distinguished by their citizenship status, either German or immigrant. Standard errors are robust and clustered at the closing establishment level.
*denotes statistical significance at the 10% level.
**at the 5% level.
***at the 1% level.

Table 7
Heterogeneity Across Information Providers (2).

	(1) Industry (3-digit)	(2) Geographic Proximity	(3) Contact Intensity	(4) Firm Size
ER - same characteristic	**0.384** **[0.386]**	**0.881**** **[0.407]**		
ER - different characteristic	**1.108**** **[0.486]**	**0.677*** **[0.390]**		
ER - above median			**0.942**** **[0.421]**	**0.689*** **[0.400]**
ER - below median			**0.704*** **[0.389]**	**0.816**** **[0.405]**
Observations	10,659	10,659	10,659	10,659
Number of groups	1,558	1,558	1,558	1,558
Kleibergen-Paap statistic 1st stage	37.1	32.4	31.6	32.2
P-value equality coefficients	0.06	0.28	0.16	0.54

Note: For a list of the additional control variables included, see Table 3, column (3). Group-specific employment rates are calculated by splitting the overall employment rate in a network E/NS into subcomponents S/NS and D/NS, where S and D are the number of employed workers in each subgroup, and S+D=E. The instruments are constructed accordingly. In column (1), coworkers are distinguished by the three-digit industry they work in in t (or, for the unemployed former coworkers, the last three-digit industry they worked in). In column (2), coworkers are distinguished based on whether or not they work in the same county as the displaced worker in t (or, for the unemployed former coworkers, the last county they worked in). In column (3), coworkers are distinguished based on whether they worked strictly more or less than the median duration of joint employment spells with former coworkers in the sample. In column (4), coworkers are distinguished based on whether at the time of cowork with the displaced worker, they worked in an establishment that was strictly larger or smaller than the median establishment size in which the displaced worker experienced his joint employment spells. Standard errors are robust and clustered at the closing establishment level. A (***) at the 1% level.
*denotes statistical significance at the 10% level.
**at the 5% level.

characteristic in question, where $S_{it} + D_{it} = E_{it}$.[21] Table 6 reports the corresponding results. Column (1) shows that coworkers who are of the same age cohort (defined as within +/- five years of the displaced worker's age) have indeed a relatively larger effect on the re-employment probability of a displaced worker. In contrast, columns (2) to (4) suggest that female coworkers, coworkers with a different education level and coworkers of a different nationality have a stronger effect on a displaced worker's employment outcome after displacement. This may seem surprising. However, it is important to keep in mind that these differential effects between different groups of coworkers are derived conditional on having worked together at the same employer. It may well be that homophily in social interaction leads to a clustering of different groups of workers in certain establishments, but that once workers interacted with each other on a daily basis at the workplace, differences in education, gender or nationality are actually conducive to the exchange of information. Also, note that none of the differences in coefficients reported in columns (2) to (4) is statistically significantly different from zero.

In Table 7, I differentiate subgroups of coworkers in terms of their presumed ability to provide relevant information. In columns (1) and (2), I assess whether coworkers who most recently worked in the same three-digit industry and regional area (county), respectively, have a

[21] The instrumental variables are constructed accordingly: $Z_{it}^{g} = \sum_{s=1}^{5} (M_{it-s}^{g}/E_{it-s})$, where $g \in (D, S)$.

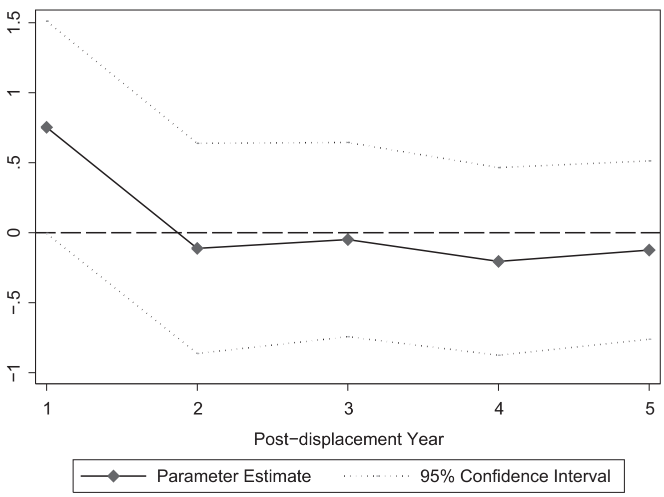

Fig. 2. Persistence of employment effect.
Note: The graphs shows the estimated marginal effects of the network employment rate on a displaced worker's employment status in the years following displacement, estimated in separate regressions based on the specification in column (3) of Table 3.

particularly strong effect on the re-employment probability of a displaced worker. Workers who have moved to a different industry than the one in which the displaced worker was last employed may be able to provide more novel information about job opportunities relative to workers who remain active in the same industry as the displaced worker. The significant difference of the coefficients in column (1) seems to support this hypothesis. On the other hand, workers who are still working in the same county as the displaced worker appear to be more valuable as a source of information, possibly because the displaced workers are primarily searching for new jobs in the geographical vicinity of their last job. In column (3), I distinguish coworkers based on the contact intensity they had with the displaced worker, using the duration of cowork as a proxy of such intensity. As expected, the effect of the group of coworkers with whom the displaced worker worked for more than the median duration in the sample is larger than for those with whom he worked less than the median duration. Finally, in column (4), I allow for the fact that a connection to a coworker that was established while working in a large establishment may be less strong than a connection to a coworker that was established during a joint employment spell in a small establishment, as workers may be constraint regarding the overall number of social contacts and the intensity with which they can maintain them. The results suggest that indeed contacts acquired in small establishments are more valuable than those acquired in large establishments, although for this estimation in particular, the estimated parameters are not statistically different from each other.

4.3. Persistence of effects

Given the strong positive effect of the prevailing employment rate in a given worker's network on his employment probability in the year after displacement, it is interesting to investigate how persistent this effect is over time. For this reason, I estimate the baseline model separately for each of the five post-displacement years covered by the data. Fig. 2 shows the corresponding results. The profile of the estimated coefficients shows that the positive effect on a displaced worker's employment status declines rapidly over time. Already from the second post-displacement year onwards, there is no more discernable difference in the employment rates of workers who were part of a strong network at the time of displacement and workers who were part of a weak network. This is maybe not too surprising given the expected strong labour market attachment of the sample of male workers in this study. The primary effect of coworker-based networks thus appears to be a more rapid transition out of unemployment rather than a persistent improvement of employment outcomes. The absence of a persistent effect can also be viewed as further evidence against possible endogeneity concerns regarding the use of past mass-layoffs as an instrumental variable. If these mass-layoffs were to proxy for undesirable unobserved worker attributes, one would expect the in that case upward-biased positive effects on employment to be longer lasting.[22]

5. Conclusion

This paper diverges from much of the existing literature on the role of social networks in the labour market by focussing on networks of former coworkers. Coworkers are likely to play an important role in the exchange of labour market relevant information between individuals. As opposed to most other acquaintances, coworkers are likely to possess good knowledge of a given worker's specific skills and be more aware of job opportunities appropriate for the worker in question. Using data on the universe of workers in four large metropolitan areas in Germany, I show that the strength of a worker's network has a sizeable effect on the probability of finding a new job after being displaced as the result of an establishment closure. The empirical analysis systematically takes account of unobserved correlated group level shocks and individual sorting into establishments, allowing a causal interpretation of the estimated effects. This is achieved through the inclusion of a comprehensive set of fixed effects and control variables, and, most importantly, the utilization of exogenous shocks to the networks in the form of mass-layoffs as instrumental variables.

This novel identification strategy, which could be readily applied in other network-settings as well, successfully deals with the endogeneity of the key regressor of interest, for which I find compelling evidence in a standard OLS regression. According to the baseline IV specification, a 10 percentage point increase in the employment rate of a displaced worker's network of former coworkers increases the probability of finding a job in the year after displacement by 7.5 percentage points. In contrast, there is no evidence of a positive effect of a stronger network on the starting wages of workers, suggesting that workers do not adjust their reservation wages to unexpected changes in the rate at which job offers arrive through their networks. The positive employment effects are relatively short-lived, robust to changes in specification and alternative definitions of the key variables involved, and do not seem to be driven by possible correlations between the instrument and unobserved characteristics or labour market conditions of workers.

While this study focuses exclusively on male workers in order to isolate as well as possible the job search dynamics after an exogenous employment shock, negative and statistically insignificant IV estimates for the sample of women suggest that for them the role of coworker-based networks may be different. Whether this is due to a higher propensity to drop out of the labour force entirely after displacement, the potentially confounding role of peer effects in fertility among female coworkers (as shown by Asphjell et al. (2014), or a fundamentally different role of coworkers in women's social networks is an interesting question left for future research.

Overall, the results of this study suggest that a strong network of coworkers provides valuable information about labour market opportunities and can serve as a useful resource to accelerate transition out of unemployment in times of economic distress. More generally, the findings also imply that social networks act as amplifiers of economic shocks, which could partly explain the substantially larger employment response of, for instance, low-skilled workers or immigrants to business cycle fluctuations. A detailed analysis and quantitative assessment

[22] As in the case of the baseline specification, the effect of changes in the network employment rate on wages is small and statistically not significant throughout the five year period after displacement.

of this potentially important role of social networks for aggregate employment dynamics is left for future research.

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