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Conference Paper Do neighbors help finding a job? Social networks and labor market

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2016: Demographischer Wandel -Session: Social Networks and Inequality, No. E18-V2

Provided in Cooperation with:

Verein für Socialpolitik / German Economic Association

Suggested Citation: Jahn, Elke Jutta; Neugart, Michael (2016) : Do neighbors help finding a job? Social networks and labor market, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2016: Demographischer Wandel - Session: Social Networks and Inequality, No. E18-V2, ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft, Kiel und Hamburg

This Version is available at: https://hdl.handle.net/10419/145476

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Do neighbors help finding a job? Social networks and labor market outcomes after plant closures

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January 2016

Abstract

Social networks may affect individual workers' labor market outcomes. Using rich spatial data from administrative records, we analyze whether neighbors' employment status influences an individual worker's employment probability after establishment closure and, if hired, his wage. Our findings suggest that a 10 percentage point higher neighborhood employment rate increases the probability of having a job after six months by 0.8 percentage points and daily earnings by 1.7 percent. The neighborhood effect seems not to be driven by social norms but information transmission via neighborhoods and, additionally, via former co-worker networks.

Keywords: social networks, job search, neighborhood, employment, wages, plant closures JEL-Classification: J63, J64, R23

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1 Introduction

Individuals are embedded in social networks and the question arises to which extent this influences their decisions and economic situation. In the labor market finding a job may not only be a function of individual characteristics and the vacancies posted by firms. Social networks may influence search behavior or channel information on vacancies to the job searchers. It has been known since the seminal work by Granovetter (1995) that workers typically use personal networks when searching for jobs. While there has been substantial theoretical work on social networks (see, e.g., the surveys by Ioannides and Loury 2004; Jackson et al. 2008; Topa and Zenou 2015), empirically we know less about the role of social networks for labor market outcomes.

In this paper we try to answer the question to which extent the neighborhood in which a job seeker lives affects his labor market outcomes, and through which economic mechanisms this happens. Our empirical analysis draws on a rich administrative dataset that comprises the universe of workers in 23 self-contained labor market regions in Germany. The neighborhoods are constructed by geo-referencing the places of residence of workers within grids of one-square kilometer size. The identification idea for estimating a causal effect running from a neighborhood's employment rate on an individual worker's probability of finding a job rests on the assumption that the worker is placed "randomly" into a grid after a job loss which was beyond his control. Workers having lost their jobs get "treatments" of varying degree by living in neighborhoods which differ in the share of employed workers.

While, as most other studies, we do not directly observe the actual contacts an individual worker has in his social network, our approach can address various other intricately difficult issues when it comes to identifying a social network effect. As argued by Manski (1993) common factors affecting the employment status of an individual and his social network may flaw estimates of a social network effect. By focusing on workers who lost their jobs because of firm closures we may reasonably exclude that the social network drove the job loss. Then, as long as the displaced worker does not share unobserved characteristics with other individuals in his neighborhood,

the employment rate of the neighborhood should be uncorrelated with the residual. We address the issue with a rich set of control variables for the displaced worker. Nevertheless, it may be the case that workers chose to live in a specific neighborhood in the past. They may have selected themselves into particular neighborhoods for reasons we do not observe but could be related to employment relevant characteristics of a neighborhood. While we cannot rule out such behavior, our identification employs an approach that should reduce the risk of mismeasurement on those grounds. The grids, the spatial unit of our analysis which we use to define a neighborhood, do not align to naturally grown neighborhoods. This should add randomness to the residence of the displaced workers. Furthermore, the smallness of our one square kilometer grids may have prevented households to always exactly find a place of residence in a particular neighborhood in the past further adding randomness to the choice of residence. Finally, the self-contained labor markets, as we will explain in more detail later on, are defined as labor market regions where workers can commute. Restricting ourselves to those 23 self-contained labor markets allows to control for shifts in the relevant labor demand of the job searchers living in a particular neighborhood of a commuting area. Thus, it will help to avoid falsely attributing a higher likelihood of a worker finding a job to a higher neighborhood employment rate when it is actually driven by a shift in the labor demand in the regional labor market.

We expect that higher employment rates in a neighborhood increase the probability of finding a job all else equal. The literature suggests three mechanisms through which this may happen. The relevant social network may provide information on job vacancies that not-connected workers may not get (Topa, 2001; Calvó-Armengol and Jackson, 2007). Secondly, the network may help potential employers to overcome a problem of asymmetric information. As firms will typically have difficulties to assess the true productivity of applying workers, referrals may provide valuable information to the firm and make it more likely that workers get hired who know someone already working in the firm to which they apply (Montgomery, 1991; Simon and Warner, 1992). Thirdly, one may observe faster transitions back into employment not because the social network provides information, but rather because it shapes social norms (Akerlof, 1980; Agell, 1999). Workers living in neighborhoods with high employment rates may derive a negative utility from not being employed as one's status does not comply with the socially prevalent. Similarly, a neighborhood of relatively high unemployment may provide for an environment where being unemployment is the rule and, therefore, low search efforts comply with what other people do. Our empirical analysis tries to shed light on which of those mechanisms is more likely to explain our finding that social network effects matter for the job finding probability.

Very early contributions to the empirical literature on neighborhood effects were made by Henderson et al. (1978) who found that average class ability positively affected educational achievements of Canadian students, and by Datcher (1982) who could attribute a substantial fraction of the racial differences in education and earnings to poorer neighborhoods from which blacks come. Detailed spatial information on U.S. residential neighborhoods is used in Bayer et al. (2008), and similarly for a German metropolitan region in Hawranek and Schanne (2014), to show that workers coming from the same residential location tend to cluster at work locations which is consistent with local referral effects. Building on a similar identification strategy as Bayer et al. (2008), Hellerstein et al. (2011) find evidence for residential hiring networks, and Schmutte (2015) finds positive wage effects of higherquality neighborhoods measured by paid wage premiums. Again using neighborhoods, and additionally former schoolmates, as the supposedly relevant social network, Markussen and Røed (2015) show that social insurance takeup is contagious. The work probably closest to our analysis is the study by Hellerstein et al. (2015). They analyze the effect of residential neighborhoods on labor market outcomes with U.S. data focusing on the business cycle for workers' job finding probability. Their unit of analysis are Census tracks for which they develop various measures of neighborhood network strengths. Those neighborhood measures are then shown to drive the job finding rates of workers living in the neighborhoods. The social network is also defined as the neighborhood in which persons live in a range of studies that look into the labor market outcomes of refugees that were, due the specific rules of a country's authority, assigned to particular regions. Beaman (2012) studies, for example, the labor market outcomes of refugees resettled into various U.S. cities. Similar analyses can be found in Edin et al. (2003) for Sweden or in Damm (2009) for Denmark. Social networks consisting of former co-workers is the starting point in Cingano and Rosolia (2012), Glitz (2013), Saygin et al. (2014). Here, the idea is that information on vacancies may come from workers with whom the displaced worker shared some time working jointly at the closing firms. The study by Cingano and Rosolia (2012) is based on an Italian dataset, the one by Glitz (2013) rests on German data, and Saygin et al. (2014) employ Austrian data. All of them find significant effects of the employment rate among the former co-worker network on the job findings probability of the displaced workers.

Using German administrative data, our point of departure is, as in some of the previous studies, also the residential neighborhood, though at arguably smaller grid sizes. Besides evaluating the role of the neighborhood's employment rate we try to distinguish between social norms as one potential cause of our findings, and information transmission. Moreover, we try to better understand if information travels in neighborhoods or through co-worker networks. Finally, rather than looking into specific groups of people as has been done in the studies on the resettlement of refugees, we evaluate the labor market outcomes for the universe of German workers as a function of their neighborhood employment rates.

In our most favored specification we estimate that a ten percentage point increase in the employment rate of the neighborhood increases the probability of being employed after six months by about 0.8 percentage points. We, furthermore, provide evidence that neighbors belonging to similar sociodemographic groups matter for finding a new job more easily. Running regressions of daily earnings on the neighborhoods' employment rates also reveals statistically significantly positive effects. A ten percentage point higher employment rate of the neighborhood increases the daily earnings of those who found a job after half a year by 1.7%. We interpret the positive effect as pointing towards an information transmission channel being at work rather than a social norm effect driving the results on job finding rates. Regarding the question whether a former co-worker network provides additional information on vacancies with respect to neighbors, our results suggest that an average firm is much more likely to hire a worker from a particular neighborhood if it already employs a former co-worker. Thus, information seems not only to travel through neighborhoods but is, in addition, provided by those who were also formerly employed at the same closing firm.

We proceed by introducing our econometric model and identification strategy in Section 2. Section 3 gives information on our data set. In Section 4 we present our results. The last section concludes.

2 Empirical model and identification

We estimate a linear probability model

$$e_{i,t+1} = \alpha + \delta er_{i,t} + \theta \log(n_{i,t}) + \boldsymbol{\beta} \boldsymbol{x}_{i,t} + \epsilon_{i,t} \tag{1}$$

where $e_{i,t+1}$ is an indicator variable for individual *i* that takes the value of one if the individual found a job six months after the displacement, $e_{i,t}$ is the employment rate of the residential neighborhood at the start of the unemployment spell of individual *i*, $n_{i,t}$ is the number of citizens at the place of residence, $\boldsymbol{x}_{i,t}$ is a vector of controls including worker characteristics, indicator variables for the year of dismissal and the regional labor markets, and $\epsilon_{i,t}$ are unobserved determinants.

We are mostly interested in an estimate of δ . This parameter estimate may be interpreted as causal if there are no common factors affecting the employment probability of an individual and his social network. For various reasons this is likely to be the case in our analysis. First, we restrict the analysis to workers who have been displaced because of plant closures. By construction the job loss becomes exogenous to the behavior of the worker which, as we are interested to find out, could otherwise be a function of his social network. Then, displaced workers are "treated" by the varying employment rates of the neighborhoods in which they live. To the extent that a worker who lost his job does not share unobserved characteristics with other individuals in his neighborhood, the employment rate of the neighborhood should be uncorrelated with the residual. A rich set of sociodemographic characteristics for the displaced worker that we use as controls reduces the likelihood of omitted variables. Nevertheless, it may be the case that our "treated" workers have chosen at some time in the past their places of residence because they wanted to locate close to their friends and acquaintances for reasons that our control variables do not cover. While we cannot rule out such behavior one may argue that even if such sorting processes happened in the past, finding the appropriate house or apartment may not always have worked out within the unit of our analysis, i.e. one square kilometer. It seems likely to us that, if occurred at all, there was some randomness in the choice of the place of residence. Finding a house or apartment close by may not always have been possible within the same neighborhood but rather have worked out for adjacent neighborhoods only. Furthermore, the geo-referenced grids which define our neighborhoods are not tailored along "natural" borders such as streets, rivers or the like. They rather cut irregularly through naturally defined neighborhoods adding to the randomness of the treatment of the displaced workers. Finally, our analysis rests on selfcontained labor market regions which are defined on the basis of workers' residences in commuting distance to potential employers. To this end we are able to control for common shocks to the relevant regional labor market of a displaced worker that influence the job finding rates. For all those reasons we are confident to employ a reasonable identification strategy.

3 Data and descriptive statistics

Our analysis draws on administrative data assembled by the Institut für Arbeitsmarkt- und Berufsforschung in Nürnberg. We have information on a stock of approximately 1.3 million workers from 23 selected labor market regions for the years 2007, 2008, and 2009. Those labor market regions are self-contained in the sense that they were constructed as commuting areas for the workers living therein. Technically, those in total 141 labor market regions are computed using factor analyses on commuting distances between German districts imposing a maximum commuting time of 60 min one way (c.f. Kosfeld and Werner, 2012). From those self-contained regional labor markets we selected the three largest, the ten smallest and ten medium sized markets for our analysis. All regional labor markets are in West Germany for reasons of homogeneity, see Figure 1.

On average, there were more than a million workers living in the three metropolitan labor market areas, about 160 tsd. in the 10 urban, and slightly more than 37 tsd. living in the 10 rural labor market areas, c.f. Figure 1. The metropolitan labor market areas are split up into more than 4,600 neighborhoods of the size of one square kilometer each. The urban labor market areas contain a little bit more than 1,700 neighborhoods and the rural about 620 neighborhoods on average. For the analysis we have only considered neighborhoods with a population size larger than 50. In 2007, 16,851 firms were closing down, and 18,147 and 18,303 in the two consecutive years, respectively. The sum of formerly full-time workers, on which we base our analysis, displaced over the course of the three years amounts to almost 90,000 observations. On average, each plant displaced five workers when closing down. Those displaced workers lived in about 8 tsd. different neighborhoods at the time of the plant closures. There were four displaced workers per neighborhood at an average population per neighborhood of about 550 workers.

Figure 2 shows the histogram of neighborhood sizes. There are a few relatively large neighborhoods in the sample. On average almost 9 out of 10 workers were employed. Again, the distribution is skewed with a few neighborhoods having considerable lower employment rates, see Figure 3. Overall it occurs, however, that there is ample variation in the neighborhood employment rates to draw on.

In Table 2 we present more information on the almost 90 tsd. displaced workers for whom we want to know what drives their job finding probability. 50.2% of them were full-time employed after 6 months of losing the job due to a firm closure, and almost 60% had any kind of job after six months. In addition, we can draw on a rich set of information on the sociodemographic characteristics of the workers including, among other things, past earnings and unemployment spells.

Finally, we are interested in which neighborhoods the displaced workers have their places of residence. To this end, Figure 4 plots the number of different places of residence the displaced worker from a particular plant live in. Each dot represents a closing plant of a particular size. Would all displaced workers from that plant live in different neighborhoods, the dot would lie on the 45 degree line. Not all dots do so, implying that there is considerable variation in the neighborhoods in which displaced workers from a particular plant live in.

4 Results

4.1 Basic regression

Table 3 assembles our main results for three different specifications of the linear probability model introduced with eq. (1). The dependent variable indicates whether a displaced worker is employed six months after having lost his job. The parameter estimate we are most interested in is the one on the neighborhood's employment rate while having included a rich set of control variables for worker characteristics, and year and labor market region dummies. In Model (2) we add the logarithm of the size of the neighborhood, and in Model (3) we additionally include an interaction term of the indicator variables for the years and labor market regions in order to account for potential labor market region specific business cycle effects. For all three models we get a positive effect of the neighborhoods' employment rates on the probability of holding a job half a year of the firm closure. Including the log of the population of a neighborhood slightly decreases the size of the estimate of the neighborhood employment rate whereas the estimate of the effect of the size of the neighborhood itself is not statistically different from zero. For Model (3) with the largest set of control variables, we estimate that a ten percentage point increase in the neighborhood employment rate increases the probability of being employed after six months by 0.8 percentage points. Given that every second displaced worker has found a job after six months our estimated relative change amounts to 1.6%. The size of the neighborhood employment effect on the re-employment probability of displaced worker in our study is in the range of what has been found by others, at least those, that can partly be compared. In particular, Hellerstein et al. (2015) find for their weighted measure of the Census track employment rate that an interquartile change raises the employment probability in their sample by 1.9% which is the upper bound of their estimates.

4.2 Mechanisms

4.2.1 Social norms?

As we explained earlier on, the existing literature suggests that neighborhoods may have an effect on an individual's job finding rate by providing information through friends and acquaintances who live nearby or by changing the worker's preferences through a social norm effect. One way that possibly allows to rule out one of the two channels is to look into the effect of the neighborhood employment rate on the daily earnings of those workers employed after six months. The underlying idea is the following: if social norms are at work, then higher residential employment reduces reservation wages and consequently wages on the new job should be lower. Displaced workers comply to a social norm of one having to work for his living and will be inclined to accept jobs although they may pay less. If, on the other hand, information transmission is at work, reservation wages are likely to increase with the residential neighborhood employment rate as the job searcher can rightly expect more information on vacancies and job offers to arrive. Consequently wages on the new job should be positively correlated with the employment rate of the residential neighborhood. Table 4 shows the results of a regression with the daily earnings of those workers being full-time employed after six months as the dependent variable. Restricting ourselves to the fulltime employed in this set of regressions seems to make sense as we can only draw on daily earnings. If we had part-time workers included daily earnings would in addition vary with the type of the part-time contract. The right hand side variables are exactly the same as already used for the regression in Table 3. For Model (3) with the largest set of controls we get a statistically positive coefficient on the neighborhood employment rate. Following our reasoning outlined above this suggests that the effect of the neighborhood employment rate increasing a displaced worker's employment rate after six months could be driven through the provision of information by employed neighbors. Thus, neighbors do not only seem to help the displaced workers finding a new job by informing them on vacancies. It also occurs that the job seekers profit from sizable wage gains as more information comes in. A ten percentage point increase in the neighborhood employment rate raises the log daily earnings by 0.0238. On average this is a 1.7% increase in daily earnings.

4.2.2 Composition of the neighborhood network

We turn to the question of whether the effect of a neighborhood's employment rate on finding a new job differs along major sociodemographic groups now. It may be the case that contacts are more likely among similar workers and, therefore, information would more likely travel among subgroups, as well as referrals would be more likely among members of those groups. Table 5 shows the results of a set of regressions were we include two neighborhood employment rates, one which matches the sociodemographic characteristic of the worker and an another one relating to the sociodemographic group to which the worker does not belong to.

It occurs that workers in the neighborhood with similar sociodemographic characteristics are the relevant ones. For the neighborhood's employment rate of the respective sex we get an estimate of the same size as in the basic regressions. Interestingly, the employment rate of the opposite sex in the neighborhood does not affect an individual's probability of finding a job. Similarly, nationality and education are of importance. Again, it is the employment rate of the workers in the neighborhood having the same citizenship which is driving the job finding probability whereas the employment rate of the workers with other citizenship is irrelevant. The same interpretation applies to the employment rates split along the educational dimension. For more deeply evaluating a possibly heterogeneous effect along age we considered a 10-year gliding window centered at the age of a worker. Now, a ten percentage point increase in the neighborhood's employment rate of one's age group increases the probability of having a job half a year later by 4.9 percentage points. Moreover, there are signs of crowding out effects of job opportunities. As the employment rate in a neighborhood of the other age groups to which an individual does not belong to increases, this worker's employment chances deteriorate substantially. A ten percentage point increase in the other age group's employment rate lowers the probability of having a job half a year after the displacement by 3.7 percentage points.

4.2.3 Co-worker effects

So far our results point towards information transmission as the predominant effect of the network. It is, however, still an open question whether information travels through the neighborhood only, or if there is, in addition, a former co-worker network driving the results. We will attempt to shed light on this issue now.

We do not have information on all the former co-workers with whom a displaced worker shared some working history before the plant closed down. However, we know about all the workers who lost their job at the time of plant closure. It occurs to us that a reasonable short-cut to a variable on co-workers' networks seems to be to assume that the displaced workers of a particular plant shared the same co-workers. Then, if there is a former coworker effect in addition to a neighborhood effect, one should observe when comparing two workers living in the same neighborhood that a worker is more likely to end up in a firm that already employs a former co-worker than in another firm that does not employ a former co-worker.

In order to evaluate such an additional effect arising through information transmission among former co-workers we adapt an estimation strategy proposed by Kramarz and Skans (2014). In particular, we estimate a linear model for the probability that individual i starts working in plant j

$$E_{i,n(i),j} = \beta_{n(i),j} + \gamma A_{i,j} + \epsilon_{i,j} \tag{2}$$

where $E_{i,n(i),j}$ is an indicator variable that takes the value of one if an individual *i* from neighborhood *n* is working in plant *j*, $A_{i,j}$ is an indicator variable capturing whether a former co-worker from the closing plant of individual *i* already works in plant *j*, and $\beta_{n(i),j}$ is a neighborhood-plant specific factor taking into account that an individual *i* coming from neighborhood *n* ends up in plant *j*. The specific factor takes into account our network effect arising from the residential neighborhood, i.e. information transmission through employed workers living close by. Then, the estimate on γ informs on how much more likely it is that an average plant hires an individual from neighborhood *n* who has a former co-worker working for it, than an individual who does not have a former co-worker at the plant. If there is no co-worker effect we expect $\gamma = 0$.

Estimation of an eq. (2) would require a dataset for every possible combination of a worker with a hiring plant. After half a year about 53,200 workers found a job in one of 40,700 firms in our sample. Combining those two figures would give a dataset with more than two billion lines. Even slicing through the data along the 23 self-contained labor market regions, thereby assuming that workers could only have been hired by one of the firms in the region, yields a dataset too large to be estimated with the plant-neighborhood fixed effects $\beta_{n(i),j}$. A fixed effect transformation helps to arrive at an estimable model. To this end all cases are dismissed where there is no within plantneighborhood variation in A. Then, one calculates the fraction of workers with former co-workers in a plant who were hired by that particular plant:

$$R_{nj}^{link} = \frac{\sum_{i}^{n(i),j} E_{i,n(i),j} A_{i,j}}{\sum_{i}^{n(i),j} A_{i,j}} = \beta_{n,j} + \gamma + \tilde{u}_{n,j}^{link}.$$
(3)

Similarly, one determines the fraction of workers hired from a neighborhood by a plant where no former co-worker has been working already:

$$R_{nj}^{noLink} = \frac{\sum_{i}^{n(i),j} E_{i,n(i),j}(1 - A_{i,j})}{\sum_{i}^{n(i),j} (1 - A_{i,j})} = \beta_{n,j} + \tilde{u}_{n,j}^{noLink}.$$
 (4)

Finally, the difference between the two ratios eliminates the plant-neighborhood

effect and gives an estimate of γ . It is computed as the fraction of those hired by a plant from a neighborhood among those with a former co-worker in the plant minus the fraction of those hired by the plant from the same neighborhood among those without a former co-worker in that same plant.

Table 6 summarizes the estimates of γ for all the 23 labor market regions. Comparing the likelihoods of an average plant hiring from a neighborhood with and without a former co-worker already employed in that plant reveals that it is more likely to be hired by such a firm out of a specific neighborhood if that firm already employs a worker from the closing firm. All the estimates of γ are significantly larger than zero and indicate a roughly 2 percentage point higher likelihood that an average firm hires from a particular neighborhood if it has at least one former co-worker already employed. For all those estimates we assumed that displaced workers only look for jobs within one of the self-contained labor market areas. There are 1,194 plant-neighborhood pairs with variation in A left for rural areas, 9,313 for the urban labor market areas, and 47,146 for the metropolitan labor market areas. While the fixed effect transformation washed out plant-neighborhood specific effects it may still overestimate a co-worker effect to the extent that former co-workers live in the same neighborhood. However, an alternative specification of the indicator variable A taking into account that a former co-worker already working in a new plant should not live in the same neighborhood did not substantiate such a conjecture γ .

4.3 Robustness

In Table 7 we present the results of various robustness tests. First of all, one may be concerned about the linear probability model estimated so far given that the dependent is an indicator variable. Model (1) replicates the analysis using a probit model which yields essentially the same results as the linear probability model. We get a marginal effect of 0.08. We also ran a placebo experiment by randomizing the assigned neighborhood employment rates. As one would expect the estimated coefficient on the neighborhood employment rate is statistically not significantly different from zero anymore.

Apart from the log of the population of the neighborhood which becomes significantly different from zero now, there are no changes to be observed for the other control variables. The fact, that the population size turns negative may be related to neighborhood sizes being correlated with the neighborhood employment rate. Indeed we find some negative correlation across the neighborhoods but re-running our most preferred specification (Model (3) from Table 3) with an interaction term included did not change our main results. Next, we changed our definition of being employed after six months after plant closure to full-time employed workers only, not arriving at different results. Model (4) includes for each of the displacing plants a fixed effect thereby substituting the worker plant specific variables we used earlier. Again, the estimated parameter stays robust. Restricting ourselves to the cases where workers were displaced by plants having had more than 10 employees increases the estimated parameter by about 50%. Note, however, that the number of observations used drops substantially now. Moreover, we substituted the linear specification of the neighborhood employment rate with a more flexible one where we included dummy variables for the size of the neighborhood employment rates. Again, we find that higher neighborhood employment rates increase a worker's probability of having a job half a year after his displacement. Finally, we changed the dependent variable looking into the employment status after 12 and 18 months. It occurs that the effects of the neighborhood employment rate on being reemployed after 12 and 18 months are somewhat smaller than the effect after six months.

5 Conclusion

Social networks may affect individual workers' labor market outcomes. We evaluated to which extent the employment rate among the neighbors of a worker who lost his job after a plant closure affects his employment status six months after the displacement. We find that a 10 percentage point higher employment rate in the neighborhood increases the probability of having a job six months after the displacement by 0.8 percentage points. Moreover, higher employment rates in the neighborhood do not only help workers to find jobs. They also profit from higher earnings. On average their daily earnings are higher by 1.7% if the neighborhood's employment rate is 10 percentage points higher. The positive effect of the neighborhood employment rate on the daily earnings suggests that the neighborhood effect runs through information provision of the social network rather than via a social norm effect. There is, moreover, strong evidence that the neighborhood effect is driven by the employment rate of the sociodemographic group in the neighborhood to which the respective worker looking for a job belongs to. Further analyses suggest that information on vacancies does not to only travel via the neighborhood but also through a former co-worker network to a substantial amount. We can show that it is much more likely that an average firm hires a worker from a particular neighborhood if that firm already employs a former co-worker.

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Figure 1: Self-contained labor market regions



Legend



Figure 2: Histogram of neighborhood sizes

Figure 3: Histogram of neighborhood employment rates



Figure 4: Number of different neighborhoods displaced workers live in by size of closing plant



		Years	
	2007	2008	2009
Displacements and closing plants			
Number of closing plants	16,851	18,147	18,303
Mean size of closing plants	4	5	5
Number of displacements (previously full-time)	27,467	31,360	30,982
Neighborhood characteristics			
Number of neighborhoods	7,740	8,101	8,022
Mean number of full-time displacements in neighborhood	4	4	4
Mean share of displacements in neighborhood	0.014	0.015	0.016
Mean population in neighborhood	557	546	537
Mean employment rate in neighborhood	0.883	0.887	0.890
Labor market region (LMR)			
Number of labor market regions	23	23	23
Metropolitan	3	3	3
Urban	10	10	10
Rural	10	10	10
Mean labor force in labor market region			
Metropolitan	1,130,879	1,141,051	1,112,597
Urban	160,338	163,095	160,369
Rural	37,464	37,623	37,100
Mean number of neighborhoods in labor market region			
Metropolitan	4,696	4,677	4,662
Urban	1,712	1,727	1,724
Rural	623	622	623
Share of all workers displaced in a labor market region			
Metropolitan	0.011	0.012	0.013
Urban	0.010	0.011	0.012
Rural	0.009	0.011	0.012

Table 1: Sample statistics - neighborhoods and labor market region

Variables	Mean	St. de
Employment status		
Full-time employed after 6 months	0.502	0.500
Full-time employed after 12 months	0.529	0.499
Full-time employed after 18 months	0.530	0.499
Employed after 6 months	0.592	0.491
Employed after 12 months	0.640	0.480
Employed after 18 months	0.655	0.475
Male	0.620	0.485
Non German	0.177	0.382
Share of age group		
Age 16-30	0.295	0.456
Age 31-45	0.401	0.490
Age 46-65	0.304	0.460
Share of skill group		
Low skilled	0.177	0.382
Medium skilled	0.591	0.492
High skilled	0.231	0.422
Share of occupational group		
Occ: Manufacturing	0.269	0.444
Occ: Gastronomy, health, and social services	0.197	0.398
Occ: Commercial and business-related services	0.358	0.480
Occ: IT and natural sciences	0.025	0.155
Occ: Protecting, logistic, and cleaning services	0.151	0.358
Worked and lived in same LMR	0.838	0.369
Sectoral shares		
Construction sector	0.110	0.313
Manufacturing sector, general	0.059	0.236
Manufacturing sector, metal	0.039	0.194
Manufacturing sector, transport	0.004	0.063
Service sector	0.774	0.418
Agricultural sector	0.014	0.116
Real daily wage	61.52	36.96
Tenure in years, past 5 years	2.228	1.716
Unemployed, past 5 years (dummy)	0.477	0.499
Number of jobs, past 5 years	2.427	3.252
Observations	89.809	

Table 2: Sample statistics - displaced workers

	(1)	(2)	(2)
	(1)	(2)	(3)
Variables	baseline	+ pop	+ dissyear*lmr, plant controls
Employment rate	0.102^{**}	0.081^{**}	0.081^{**}
	(0.022)	(0.0217)	(0.027)
(Log) neighborhood size		-0.003	-0.003
		(0.002)	(0.002)
Dissyear FE	Υ	Y	Y
Lmr FE	Υ	Υ	Υ
Lmr*Dissyear FE	Ν	Ν	Υ
Observations	89,809	89,809	89,809
R-squared	0.034	0.034	0.035

Table 3: Employment probability after six months

Source: IEB, Robust standard errors in parentheses

** p<0.01, * p<0.05, + p<0.1

Notes: Further control variables are gender, citizenship, education, age groups (16-30, 31-45, 46-65), occupation, whether worker lives and works in same labor market region, real daily wage on previous job, tenure in past five years before dismissal, incidence of unemployment in past five years before dismissal, plant size at day of closure, and sector of closing firm.

	(.)	(-)	(-)
	(1)	(2)	(3)
Variables	baseline	+ pop	+ dissyear*lmr, plant controls
Employment rate	0.176^{**}	0.239^{**}	0.238^{**}
	(0.032)	(0.047)	(0.047)
(Log) neighborhood size		0.009^{*}	0.009*
		(0.004)	(0.004)
Dissyear FE	Υ	Υ	Y
Lmr FE	Υ	Υ	Υ
Lmr [*] Dissyear FE	Ν	Ν	Υ
Observations	$51,\!880$	51,880	$51,\!880$
R-squared	0.410	0.411	0.412

 Table 4: Earnings effect

Source: IEB, robust standard errors in parentheses

** p<0.01, * p<0.05, + p<0.1

Notes: Further control variables are gender, citizenship, education, age groups (16-30, 31-45, 46-65), occupation, whether worker lives and works in same labor market region, real daily wage on previous job, tenure in past five years before dismissal, incidence of unemployment in past five years before dismissal, plant size at day of closure, and sector of closing firm.

	(1)	(2)	(3)	(4)
Different sex	-0.004			
	(0.041)			
Same sex	0.088*			
	(0.043)			
Different nationality		-0.013		
		(0.013)		
Same nationality		0.084^{**}		
		(0.026)		
Different education			-0.015	
			(0.021)	
Same education			0.078^{**}	
			(0.022)	
Different cohort				-0.366**
				(0.034)
Same cohort				0.486^{**}
				(0.029)
Observations	$89,\!809$	87,080	89,809	89,806
R-squared	0.035	0.035	0.035	0.038

Table 5: Composition of neighborhood

Source: IEB, robust standard errors in parentheses

** p<0.01, * p<0.05, + p<0.1

Notes: Further control variables are gender, citizenship, education, age groups (16-30, 31-45, 46-65), occupation, whether worker lives and works in same labor market region, real daily wage on previous job, tenure in past five years before dismissal, incidence of unemployment in past five years before dismissal, plant size at day of closure, and sector of closing firm.

	Rural	Urban	Metropolitan	All
γ	0.012^{*}	0.025**	0.019**	0.020**
	(0.005)	(0.002)	(0.001)	(0.001)
R_{nj}^{noLink}	0.018^{**}	0.010^{**}	0.001^{**}	0.003^{**}
U U	(0.003)	(0.001)	(0.000)	(0.000)
R_{nj}^{Link}	0.030^{**}	0.035^{**}	0.021^{**}	0.023^{**}
	(0.005)	(0.002)	(0.001)	(0.001)
Observations	1,194	$9,\!313$	$47,\!146$	$57,\!653$

Table 6: Co-worker effect

Notes: Source: IEB, standard errors in parentheses, ** p<0.01, * p<0.05, + p<0.1.

VARIABLES	(1) Probit	(2) Placebo	(3) Full time	(4) Firm FE	$\begin{array}{c} (5) \\ \mathrm{Firm\ size} > 10 \end{array}$	(6) Quintile Er	(7) 12 months	(8) 18 months
Employment rate	0.080**		0.083^{**}	0.086^{*}	0.155**		0.059**	0.067**
(Log) neighborhood size	(0.021) -0.003+	-0.006**	-0.002 -0.002	-0.004 -0.004	(0.003 -0.003 (0.003	-0.003	-0.006**	-0.006* -0.006
Random employment rate	(200.0)	(100.0) 0.019 (2.016)	(200.0)	(cuu.u)	(200.0)	(200.0)	(100.0)	(600.0)
Employment rate 2nd quintile		(orn·n)				0.016^{*}		
Employment rate 3rd quintile						(0.013^{*})		
Employment rate 4th quintile						(0.005) 0.025^{**}		
Fundovment rate 5th cuintile						(0.005) 0.020*		
Annual trac and arout forder						(0.008)		
Observations	89,809	89,809	89,809	89,809	24,631	89,809	89,809	89,809
R-squared	0.026 (a)	0.035	0.056	0.018	0.049	0.035	0.030	0.028
Dissyear FE	Υ	Υ	Υ	Υ	Υ	Υ	Υ	Υ
Lmr FE	Υ	Υ	Υ	Υ	Υ	Υ	Υ	Υ
Lmr*Dissyear FE	Υ	Υ	Υ	Υ	Υ	Υ	Υ	Υ
Number of firms				53,301				
Source: EB, Robust standard errors in parentheses								
$**n - 0.01 * n - 0.05 \pm n - 0.1$								

Table 7: Robustness

27

** p<0.01, * p<0.05, + p<0.1</p>
(a) Pseudo R-squared

worker lives and works in same labor market region, real daily wage on previous job, tenure in past five years before dismissal, incidence of unemployment in past five years before dismissal, plant size at day of closure, and sector of closing firm. Notes: Further control variables are gender, citizenship, education, age groups (16-30, 31-45, 46-65), occupation, whether