Is getting fired a bad signal on the labor market? How the type of displacement affects unemployment durations

by

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Abstract: The aim of the paper is to examine the consequences of displacement on unemployment duration. Due to *ex-ante* asymmetric information about the productivity of unemployed applicants, prospective employers use the type of displacement as a costless signal about the ability of the worker when making hiring decisions. While individual layoffs might be caused by workers' low productivity, plant closures can be considered as exogenous. Consequently, individual layoffs might attach a negative productivity signal to the worker resulting in longer unemployment duration. To test this hypothesis I use administrative linked employer-employee data for Germany which not only contain daily information on unemployment duration but also allows to control for the type of displacement. I show that workers displaced due to plant closures and downsizings find indeed significantly faster a new job. I also find the strengths of the signaling effect to be proportional to the share of displaced workers.

Keywords: Signaling, Dismissals, Displacements, Unemployment duration, Germany, Asymmetric information

JEL-Code: J64, J65, J68

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1. Introduction

A large empirical literature has found several factors that affect unemployment durations, e.g. institutional settings (Hunt 1995), previous unemployment durations (Heckman and Borjas 1980), previous job tenure (Card and Sullivan 1988, Farber 1994), personal characteristics (Uysal and Pohlmeier 2011) or macroeconomic conditions (Abbring, Van den Berg et al. 2002). This paper argues that the type of displacement is another relevant factor. In case of ex-ante asymmetric information employers take into account the conditions on how previous employment ended when making hiring decisions. This paper is a contribution to the literature both on unemployment durations and on selection mechanisms in asymmetric information frameworks.

Based on the seminal work of Akerlof (1970) on market failures that occur on markets characterized by asymmetric information, Spence (1973) introduced the idea that employers and employees use signals in order to solve asymmetric information on the labor market. Gibbons and Katz (1991) developed a model in which the type of displacement is used as a signal in order to ex-ante screen applicants[1]. The line of reasoning is that an individual layoff is treated as a negative signal as it is likely the result of low revealed productivity. In contrast, displacements due to plant closures appear to be independent of workers' productivity. Therefore the model predicts higher wage losses and longer unemployment durations subsequent to individual layoffs. In an empirical application of their model Gibbons and Katz (1991) confirm their predictions as they find higher wage losses and longer unemployment durations for individual layoffs compared to displacements due to plant closures. The effects are stronger for white than for blue collar workers.

For both the US and European labor markets, a vast literature on earnings losses of displaced workers exists. The bulk of following literature that tests the model by Gibbons and Katz (1991) empirically investigates wage effect of job termination[2]. One reason to focus on wages is that most of the papers investigate the signaling effect for US labor market which is characterized by flexible wages, low employment protection and rather short periods of unemployment. In contrast, many European labor markets such as Germany have binding wage agreements between unions and employers' associations and high job security. Therefore, earnings losses in Europa are lower and almost negligible, whereas duration of the unemployment spell tend to be much longer (Gregory

[1] When it comes to screening of applicants, one might argue that employers rather rely on the certificate of employment as a signal of working quality as it is obligatory in Germany. There are reasons to be skeptical about the credibility of this signal: Due to a decision of the German Federal Court of justice, all certificates of employment need to be written in a benevolent manner and are therefore limited in their credibility. Thus, employers need to focus on further pieces of information.

[2] For a broad overview on literature and post-displacement wages see von Wachter (2009) or Couch and Placzek (2010).

and Jukes 2001). Therefore unemployment durations seem to be the more relevant outcome to measure a signaling effects of job terminations for the German labor market.

Besides Gibbons and Katz (1991), further studies have confirmed the signalling effect for the US and Canada (Doiron 1995, Stevens 1997, Hu and Taber 2010). However, empirical findings on the signaling effect are ambiguous. Krashinsky (2002) provides an alternative explanation for the relation of job termination and re-employment wages which is based on the firm size: Large firms tend to pay higher wages and are less likely to close compared to small firms. Therefore, workers displaced from small closing firms face smaller wage losses at the start of a new job in a small firm compared to workers displaced from large firms who find new employment in a small firm. Krashinsky (2002) re-estimates and confirms the findings of Gibbons and Katz (1991) but the signaling effect loses significance once he additionally controls for firm size effects. Other studies point to the existence of a recall bias in survey data (Evans and Leighton (1995) and Oyer (2004)), i.e. the respondent's memory of prior events tends to erode over time resulting in understatement of unemployment durations and wage effects. Therefore, results based on survey data may lack of reliability. Song (2007) takes the recall bias into account and controls for the pre- and post-displacement wage tenure profiles and also finds no significant results for the signaling hypothesis.

Another concern on the validity of previous research is given by Grund (1999), who tests the signaling effect on wages for Germany. Using survey data from Germany which include the reason for job determination, he does not find evidence for the signaling hypothesis. He argues plant closures are more likely to appear in distressed local labor markets characterized by high unemployment rates. Displaced workers thus face higher unemployment durations and lower bargaining power. Therefore unemployment durations of workers displaced due to plant closures can be longer than unemployment durations subsequent to individual layoffs due to economic conditions, even if the dismissed workers themselves are preferred towards individually displaced workers. As Grund's (1999) empirical analysis neglects local labor market conditions, the effects from the signal and the local labor market cannot be disentangled. He furthermore argues that differences in labor market institutions can explain the different findings for the United States and Germany.

While the bulk of literature focuses on wage losses there is only little empirical evidence for the signaling effect of job termination on unemployment duration, especially for Europe. Biewen and Steffes (2010) use the German Socio-Economic Panel (GSOEP) and include the reasons for unemployment in a Probit model with employment status as dependent variable but find not significant effect. Using administrative data from Denmark, Frederiksen, Ibsen et al. (2013) find the type of displacement to have a significant effect on unemployment durations. Average re-

employment probabilities are about 23%-30% higher in case of plant closures. They furthermore distinguish between plant closures and downsizings and find the strengths of the signaling effect to be proportional to the share of displaced workers, i.e. the higher the share of laid off workers, the weaker is the negative signal attached to a worker. The effect is significant across all occupational groups. For the German labor market, so far no comparable study on unemployment durations exists.

In what follows, I provide an empirical analysis using register-based data from the German Federal Employment Agency on daily frequency that includes information on the reasons for job termination. I can therefore focus on displaced workers and distinguish between individual layoffs, downsizings and plant closures. Including information on regional unemployment rates and firm sizes allow to control for local labor market conditions and firm size effects. Using a Cox proportional hazard model, I find evidence for the signaling hypothesis: Job seekers displaced due to plant closures and mass layoffs face shorter unemployment durations compared to individual layoffs. Moreover, my analysis does confirm previous findings that the strengths of the signaling effect is proportional to the share of displaced workers. In contrast to previous literature, I do not find heterogeneity in the signaling effect with respect to the education or occupation.

The paper is organized as follows: Section two gives a brief introduction on the institutional settings in Germany with regard to relevant aspects of employment protection legislation. Section three provides information on data and sample selection. Section four includes the estimation strategy, estimation results and sensitivity analysis. Section five concludes the paper.

2. Institutional Settings

Differences in labor market institutions might are a potential driver of heterogeneous findings in the signaling literature. In this section I illustrate how German employers face constraints in their decision on whom to lay off due to employment protection legislation.

In the German employment protection legislation different rules apply depending on the type of displacement (e.g. individual, collective, plant closures). Since 2004 employment protection legislation only applies to employees with job tenures of at least six months and to firms with at least ten workers.

Individual layoffs need to be justified, i.e. the employer needs to state a suitable reason for termination. According to law a displacement is considered to be justified in cases of lack of personal capabilities, long-lasting sickness, for economic reasons or personal misconduct, e.g. thievery.

With regard to collective displacements, the German employment protection legislation specifies social criteria which set up the ranking order of workers in case of a mass layoff. Four criteria need to be considered in the layoff process and restrict the employer's decision: (i) job-tenure within the firm, (ii) age, (iii) family responsibilities and (iv) disabilities (§1(3) Dismissal Protection Law). The employer has to rank workers according to these criteria. The workers with the lowest ranks should be dismissed first. Social criteria need to be considered also for individual layoffs if they take place for economic reasons.

In case of plant closures social criteria do not play a role since all employees become redundant at the same time. Therefore, layoffs due to plant closures should be independent of employees' characteristics and abilities.

Furthermore, employers have to consult works councils timely and extensively about imminent plant closures (§111 Industrial Constitution Law). Hence employees are informed about plant closures in advance and especially the more productive employees can leave their job for another position before the plant closure actually takes place.

After a lay-off has been reported, certain cancellation periods apply depending on job tenure within the firm. The periods range from one month for a tenure of two years to a maximum of seven months for job tenures of 20 years or more.

Since July 1st, 2003 employees who have received notification about their displacement are obliged to register as job seekers as soon as possible, but latest three months prior to the displacement. For late registrations a period of exclusion from benefits of one week can be imposed (§ 144 Abs. 6. Social Code III).

3. Data and Sample Selection

Data used for studies on the signaling effect are either survey data or register-based data of which both have their shortcomings. While surveys on displacements often contain information on the reason for job termination, the number of observations and the frequency tend to be low. Another shortcoming is lack of reliability due to a recall bias. Jacobson, LaLonde et al. (1993) argue that in case of surveys not all individuals participate in each wave. Summarizing, survey data mostly includes necessary variables but lacks of observations and is less reliable. In contrast, register-based datasets include a large number of observations and a rich set of variables, but lack information on the reason for job termination

3.1 German Administrative Data

I draw from a linked employer-employee data set that consists of two sources of administrative data from the Institute for Employment Research (IAB). This administrative data is highly reliable and is used by the employment agency to compute levels of unemployment benefits. The integrated employment biographies (IEB) contain all persons who were registered as job seekers and who received unemployment benefits (UB) and unemployment assistance (UA) as well as all individuals employed subject to social security contributions in the period from 1975 to 2010 on a daily frequency[3]. The IEB covers about 80% of the German workforce and provides rich information on employment status, wages and socio-demographic characteristics[4]. Moreover, I am able to distinguish between displacements, mutual agreements, end of fixed-term employment contracts and job leavers as this information is asked during registration as job seeker. For this analysis, I use a 5% sample of the IEB.

I match the employment biographies with IAB's Establishment History Panel (BHP) which provides information on firm size, wage and occupation of the establishments at annual frequency from 1975 to 2009.

To identify plant closures in the BHP I follow Hethey-Maier and Schmieder (2013). Previous studies so far have often focused on the establishment identification number (EID) to identify plant closures. One problem of proper identification is that focusing solely on a change of the EID in the data can be misleading since restructuring and relabeling of firms is often poorly measured. As the EID can change for several reasons, e.g. due to a change of ownership or legal form, plant closures might be incorrectly identified. To solve the problem of proper plant entry and exit identification, Hethey-Maier and Schmieder (2013) have developed a strategy that is based on the worker flow. They classify new EIDs either as new establishments, Spin-Offs, or ID changes based on whether the workers in a new establishment have worked together before or not. Therefore a new establishment is an establishment where the workforce consists largely of workers that have newly come together to the production process (either as a new firm or as part of an existing firm). Consequently, a plant closure is defined if a workforce that has been working together does not appear again in the following year. After a plant closure many establishments do not vanish but reappear with different ID or as one or more spin-offs. Their definition allows distinguishing

[3] For more information on the IEB see Dorner et al. (2010).

[4] The data lacks on information on periods of self-employment or inactivity, but as these people cannot be displaced they are not relevant for the sample.

between these different types of events. Furthermore is has the advantage that a change of the EID, a change of the owner or location will not lead to wrong identifications of plant closures.

3.2 Sample Selection

Since I assume that plant closures and downsizings are exogenous and unexpected shocks for the employees, the layoff needs to be initiated by the employer. Therefore, I focus on displacements, i.e. involuntary job losses, and exclude all other reasons for job termination such as mutual agreements, end of fixed-term employment contracts or voluntary job leavers. I restrict the sample to involuntary job losses (i.e. job displacements) that can appear either individually or on a collective base. Among collective displacements, I distinguish between plant closures and downsizings, i.e. a decrease of the workforce of at least of at least 20%[5] compared to the previous year.

To make sure that a firm's performance is independent of a single employee's productivity, I follow Schmieder, Von Wachter et al. (2010) and exclude observations from small firms with less than 50 employees in the year prior to the displacement. To ensure furthermore that closures and downsizings appear as unexpected events I follow Couch (2001) and Schmieder et al. (2010) and restrict the sample to workers with job tenures of at least two years in the previous job. Thereby I only observe employees in expectation of stable jobs and ensure that job changes do not happen on a voluntary base, as job mobility and on the job search effort decreases sharply after a few years within a job (Schmieder et al, 2010). In addition, I make sure to exclude premature dismissals in jobs with fixed-term contracts.

As unemployment benefits and unemployment assistance differ in their level of compensation, job seekers face different incentives for job search depending on the type of benefits. This is found to significantly influence unemployment durations (Hunt 1995). By excluding low-tenured workers, I make sure that all job seekers in the sample have worked long enough to be eligible for unemployment benefits. Finally, by defining a minimum tenure of two years I rule out that the dismissal is caused by an initial mismatch between worker and employer with respect to task, occupation or personality. I also exclude recalls, i.e. a worker is hired again by a previous employer after a layoff, since in these cases employers have already observed a worker's productivity during the previous job. Hence, there is considerably less asymmetric information and no more need for firms to ex-ante screen applicants.

[5] A mass layoff is defined as a downsizing of more than five workers for firms with between 20 and 60 employees, 10% or more than 25 employees for firm sizes between 60 and 500 employees and at least 30 employees for firms with 500 or more employees (§ 17 Dismissal Protection Act)

I treat workers as unemployed if the following criteria are fulfilled: (i) a person has to be absent from employment subject to social security contributions (ii) and a person has to be registered as job seeker at the employment agency and/or receive unemployment benefits. With this definition I make sure that the sample contains only people that are currently not employed and still interested in finding employment. I neglect other forms of employment such as marginal employment, apprenticeships or participation in active labor market programs. If a person does not find a job until the end of the sample period, the observation is right-censored. Workers that register as job seekers but do not become unemployed as they find a new job in time are excluded as well. In case of undocumented interruptions during unemployment, I exclude observations with a gap of more than 12 weeks. This is the maximum duration for sanctions by the employment agency, e.g. if job seekers refused job offers or did not apply for jobs. Gaps of longer durations happen for reasons unknowns and are therefore excluded from the sample.

For the downsizing variables, I compare the firm size, i.e. the workforce of a firm, with the size in the previous year. I define mass layoffs as small downsizings if the firm was reduced between 20 and 50 % and as severe downsizings if a reduction by more than 50% occurred in a firm that is not defined as closed.

I restrict the sample workers aged 25-55 years to circumvent selectivity issues regarding early retirement. The sample is furthermore restricted to the period 2004-2009, since as of July 2003 employees who have received notification about their displacement are obliged to register as job seekers as soon as possible. Otherwise, sanctions such as periods of exclusion from benefits can be imposed. I find data quality on this highly relevant question of the reason for job termination to be higher in this period. Employees with a sickness absence of more than six weeks are excluded from the sample as well.

I exclude all other types of changes in a firm structure other than firm closure, e.g. spin-offs or ID-changes. Otherwise a bias of the effect of plant closures might occur if the reference group contains events such as ID-changes.

I control for individual and household characteristics such as age, marital status, children, education as well as information on the last employment such as tenure within the last job, industry, occupation and firm size in the year prior to the displacement.

In order to control for local labor market conditions I use the labor market regions concept developed by Kosfeld and Werner (2011) who constructed 141 labor market regions for Germany

based on commuters' behaviors[6]. Even though the unemployment rate is a time variant covariate, I only include the rate during the transition from employment to unemployment. As I seek to separate the effect of local labor market conditions and the effect of the exogenous shock on unemployment durations at the moment when the shock appears, I only need to account for the unemployment rate at that particular time. I include the absolute rate of unemployment in the moment of the job loss.

In order to prevent potential bias caused by unobserved heterogeneity it is crucial to include relevant determinants of unemployment duration in the estimation results. One of these determinants are personal traits, often categorized as big five (Uysal and Pohlmeier 2011). A strand of literature that focuses on accounting for unobserved heterogeneity is the literature on matching. Since estimators of treatment affects usually rely on the untestable assumption of unconfoundedness, it's necessary to account for all relevant information. As Caliendo, Mahlstedt et al. (2014) find, outcomes of personal traits are often significantly relevant but can be substituted by including detailed labor market histories. Therefore I follow Caliendo et al. (2014) who propose a large set of covariates and include further information from the job seeker's labor market history, e.g. (a) the number of jobs, (b) number of unemployment spells (c) and duration of unemployment spells. Based on the idea that the effects are different for the short, medium and long term labor market history, I generate intervals with measures of the share of days spend in unemployment in the previous (a) 1-3 (b) 3-5 and (c) 5-7 years before the start of an unemployment spell.

I include these variables to control for state dependence, i.e. that the amount and duration of previous unemployment spells do affect how much time a person needs to leave the current unemployment spell. Having included this information, I can control for two different types of state dependence which Heckman and Borjas (1980) distinguish between: "Lagged state dependence" refers to the length of previous unemployment while "occurrence dependence" refers to the number of previous unemployment spells.

Table 1 in the Appendix contains a detailed list of all variables used in my estimations.

3.3 Descriptive Statistics

A main concern for my analysis is to detect selectivity between groups in my sample. This can emerge if certain types of select into certain firms and jobs. Hence, in this section I compare the four groups (plant closures, severe large downsizing, small downsizing, and individual layoffs) in

[6] The definition of LMR specify that the maximum commuting distance may not exceed 60 min. Kosfeld und Werner's delineation guarantee that on average more than 80% of jobs within a LMR are hold by employees that live within the same LMR.

my sample to with respect to the observable attitudes. Besides differences in observables, there might be further personal attributes which are relevant in the job search process but not covered in my analysis, such as bad performances in job interviews. However, I order to bias the effect of the type of displacement in an empirical analysis, this attributes would have to be unequally distributed with respect to the type of displacement, which is an unrealistic assumption.

The sample contains 15,526 persons. As Table 2 shows the average unemployment duration is 397 days or about 56 weeks. For the control variables, I use the information at the moment of displacement. Considering displacements I find that about 4.6% of layoffs were the results of plant closures. Collective dismissals account for 16.2% of displacement, of which 3.9% were severe and 12.3% small downsizings. About 80% of job seekers were laid-off in a year in which the firm size did not change significantly or was even increasing in size. Table 2 furthermore shows that a share of 21.5% of job seekers has no vocational training, whereas 70.6% successfully finished vocational training. Almost 8% held a university degree.

Table 2. Covariates in Relation to Signals

Sector	**Total**	**Plant Closure**	**Severe Downsizing**	**Small Downsizing**	**Individual**
Type of displacement (share)	1.000	.046	.039	.123	.791
Unemployment Duration (days)	397.146	352.575	375.514	396.062	400.978
Age	40.909	42.961	42.042	41.231	40.68
Male	.692	.707	.707	.686	.691
German	.879	.882	.878	.866	.882
Married	.592	.642	.618	.605	.585
Children (Dummy)	.542	.559	.547	.558	.538
Education					
No Vocational Training	.215	.222	.254	.241	.208
Vocational Training	.706	.730	.688	.684	.709
University Degree	.079	.048	.058	.075	.083
Tenure	2271.955	2751.261	2390.027	2306.131	2232.934
Firm size in y-1	411.884	166.895	260.190	269.237	455.873
Local Unemployment Rate	11.117	11.434	10.874	11.416	11.065
Number of previous Jobs	3.834	3.259	3.523	3.645	3.912

Share of days in unemployment (1-3 years)	.027	.011	.041	.031	.026
Share of days in unemployment (3-5 years)	.083	.041	.115	.102	.081
Share of days in unemployment (5-7 years)	.097	.055	.118	.118	.096
Occupation					
Agriculture	.005	.003	.005	.005	.005
Simple Manual	.268	.307	.329	.334	.252
Qualified Manual	.146	.191	.156	.155	.143
Technicians	.044	.047	.049	.049	.043
Engineers	.021	.016	.008	.022	.022
Simple Service	.188	.180	.183	.164	.193
Qualified Service	.020	.009	.016	.016	.021
Semi professionals	.024	.009	.018	.016	.027
Professionals	.006	.012	.006	.006	.006
Simple Clerks	.070	.056	.054	.048	.075
Qualified Clerks	.177	.145	.145	.163	.183
Managers	.026	.021	.026	.019	.028
Other	.003	.005	.005	.004	.003
Sector					
Agriculture	.004	.003	.002	.005	.005
Mining	.003	.002	.002	.001	.004
Manufacturing	.402	.454	.334	.398	.404
Energy	.006	-	.007	.002	.007
Construction	.050	.095	.046	.054	.047
Retail & Trade	.142	.133	.118	.096	.151
Hotels and Restaurants	.020	.010	.005	.011	.023
Transport and Communication	.063	.099	.044	.051	.063
Finance	.017	.003	.014	.014	.018
Real estate	.008	.002	.007	.008	.008

Research and Development	.113	.094	.104	.130	.112
Public administration, Education	.022	.030	.026	.021	.021
Health	.037	.013	.013	.012	.043
Other services	.027	.017	.030	.023	.028
Temporary Agency Employment	.085	.044	.255	.175	.065

Note: The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2009. The sample excludes tenure of less than two years as well as employments in companies of a firm size smaller than 50.

Between the different types of displacements, I see little difference in education. When I look at occupation, I find a higher share of people displaced due to plant closures who used to work in qualified manual jobs and as technicians. Individual layoffs seem to occur rather in service occupations and for occupations as simple clerks.

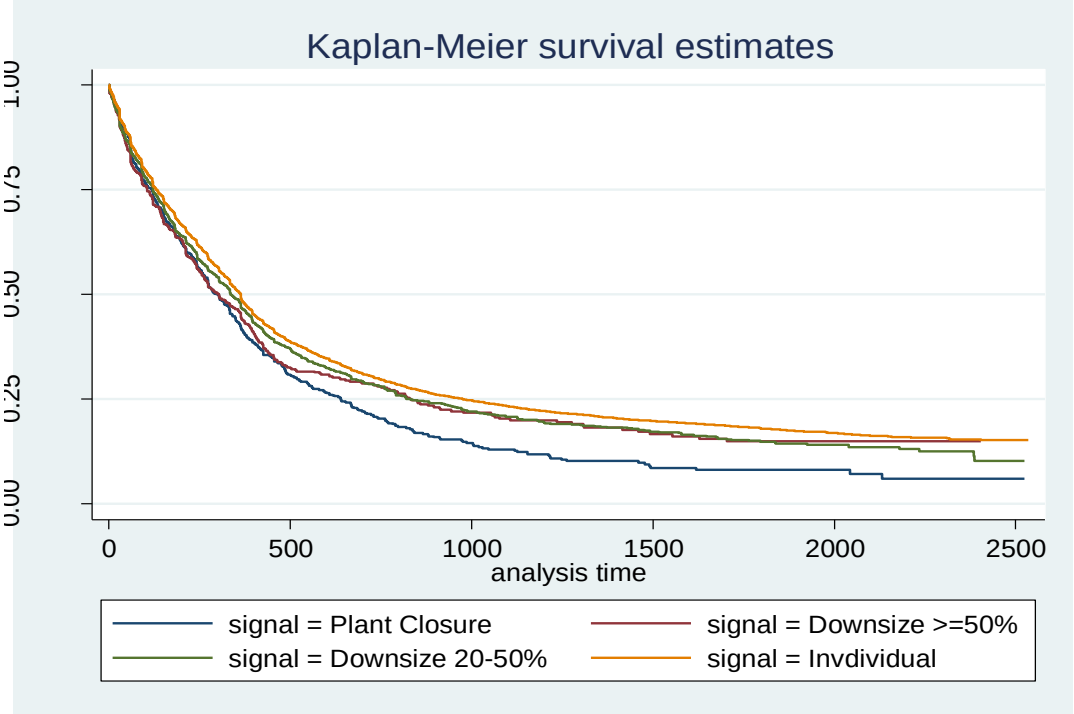
As discussed earlier, Krashinsky (2002) finds that the type of displacement is related to the firm size. The data shows that differences with respect to the type of displacement also exist in Germany. Table 2 states an average firm size of a closing firm to be 167 while people who were laid off individually are displaced from firms with on average 456 employees. Since I excluded all firms with less than 50 employees from the data, the actual average firm size for closing firms might be even smaller. It seems that small firm have a higher probability to close. The distribution of economic branches is relatively equal, with one exception of temporary agency work. This seems to a particularly large fraction in the group of workers affected by downsizing. Apparently, these workers are the first to be displaced while the core staff is retained by firms.

Displaced workers also differ in terms of the previous job tenure. For workers who have been displaced on an individual level the average job tenure is 2232 days, roughly six years. Keep in mind that I excluded observations with less than two years within the previous job. The values constantly increase with the share of displaced workers. For plant closures the average tenure is 2751 days or 7.5 years. Hence, if individual and mass layoffs are inevitable for economic reasons those employees who were the last to take up employment are the first to be dismissed. This might be caused by social criteria but also on the fact that high-tenure workers have more specific knowledge and are therefore harder to replace.

An interesting finding is that plant closures do not seem to occur in areas with high unemployment rates. The highest average unemployment rates are found in relation to plant closures. However, the differences between the groups are negligible.

As Table 2 shows the average unemployment duration is 352 days subsequent to a layoff due to a plant closure, 375 and 396 days respectively for severe and small downsizing and 400 days for individual layoffs. Similar results are provided in Graph I which shows Kaplan-Meier estimates for my sample.

Graph I. Kaplan-Meier survival estimates by type of displacement



Note: The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2009. The sample excludes tenure of less than two years as well as employments in companies of a firm size smaller than 50.

Obviously, unemployment durations are shorter after plant closures and downsizings. However, no conclusion about the validity of the signaling theory can be drawn here as the duration neglects further explanatory factors such as worker characteristics and local labor market conditions.

4. Estimation strategy and results

The aim of this paper is to test if and to what extend the termination of the last employment affects the transition into a new employment subject to social security contributions. My method of choice is to calculate hazard rates by using is a Cox proportional hazard rate model.

4.1 Estimation Strategy

For the estimations, I use a Cox proportional hazard model. The advantage of this semi-parametric approach is that I can fit survival models without making any assumption about the shape of the baseline hazard function. Therefore I avoid the risk of miss-specification in comparison to parametric models.

The underlying assumption of proportional hazard rate models is that the survival curves for two strata (in this case plant closures vs. individual layoffs) must have hazard functions that are proportional over time[7]. In the Cox model for person j the estimated hazard rate is

$$h\left(t|\mathbf{x}_\mathrm{j}\right) = h_0\left(t\right)\exp(x_j\beta) \qquad (1)$$

where β needs to be estimated from the data. In this model no further parameterization is necessary and no assumption about the baseline hazard $h_0(t)$ is required.

4.2 Estimation results

I test the signal variables in six specifications: Model I, III and V include only the dummy for plant closures as well as covariates introduced in section 3.2. In these estimations the reference group for the plant closure signal includes all kinds of displacements other than plant closures (i.e. individual layoffs and downsizings). In Model II, IV and VI I add dummies for small (20%-50%) and severe downsizings (≥50%). Here the reference group consists solely of individual layoffs. Model I and II include the sample selections as described in 3.2. In Model III and IV I reduce the minimum firm size in y-1 to 20 employees for sensitivity analysis. In Model V and VI I reduce the minimum tenure from two to one year.

Table 3 shows the results of the Cox regressions. As predicted by the signaling theory I find the coefficient of the plant closure dummy to be significant in all six estimations. The effect of a plant closure on re-employment in Model I is 35.1% (exp(0.301)) and 38.1% (exp(0.323) if I add the

[7] I test this graphically using log-log tests as well as the link test. For details on duration analysis and Cox proportional hazard model see Cleves et al. (2010).

dummies for downsizings (Model II). For layoffs due to plant closures I always find a highly significant positive effect on transitions into employment of between 29.6% and 38.2% in all six estimations. Displacements due to plant closures have higher job-finding probabilities compared to individual layoffs, ceteris paribus. Signals for downsizings are significant as well. As found in previous studies, the strength of the signal is proportional to the share of displaced workers: For large downsizings I find an average higher re-employment probability of between 19.9% and 13,7% while the effect for small downsizings is between 10.8% and 8.6%. The fact that plant closure signals are smaller in estimations without downsizings can be explained by the different reference groups: Models I, III, and VI include downsizings in the reference group while the only reference category in the other estimations are individual layoffs. A detailed estimation result for Model II can be found in Table 4 in the Appendix.

Table 3. Cox Proportional Hazard Rate Models with Closure and Downsizing Signals.

	Model I	Model II	Model III	Model IV	Model V	Model VI
Closure	0.301***	0.323 ***	0.260***	0.275***	0.303***	0.324***
	0.043	0.044	0.038	0.038	0.033	0.033
Downsizing (>50%)	-	0.159***	-	0.129***	-	0.182***
		0.054		0.045		0.044
Downsizing (20%-50%)	-	0.103***	-	0.083***	-	0.087***
		0.031		0.027		0.026
# observations	15,526	15,526	21,480	21,480	22,457	22,457

Notes: Model I and II include firms with a minimum size of 50 employees in the previous year, in model III and IV the minimum firm size is reduced to 20. In model V and VI the minimum tenure within the firm is reduced from two to one year. Standard errors below coefficients. The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2009. */**/*** denotes statistical significance at the 10/5/1 percent level.

The robustness of the findings supports the hypothesis that employers use the type of displacements as a signal for productivity. I can therefore confirm the findings of previous studies, that collective displacements give a higher probability of job re-entrance compared to individual layoffs. However, the extent to which the strengths of the signals differ is slightly higher in my estimations: Frederiksen et al. (2013) found the effect of the signal to be about 30% in case of plant closures while the effect is about 11% for severe and 3% for small downsizings.

Grund (1999) argues that the sudden and exogenous shock assumption might not hold for Germany since employers have to inform works councils at an early state and extensively about imminent plant closures (§111 industrial constitution law). Employees are therefore informed in advance and especially the most productive workers might find jobs in other firms soon. Assuming that plant closures are an exogenous shock that occur all of a sudden is indeed a strong assumption.

However, I do not need to assume that workers are completely uninformed about an imminent firm closure to gain reliable results. Particularly workers with high productivity will quickly find a new job on the external labor market, and therefore quit their job before a firm closure occurs. These workers are not part of my sample as I restrict the sample to involuntary job losses. The workers that remain in the sample are those who did not find a new job in time, either because they were not able to or because they did not know about the imminent closure. In this case the sample would contain a negative selection of workers affected by firm closures and the magnitude of the signaling effect would be underestimated. Estimation results of signaling effects would be negatively biased due to a selection bias. Hence, if I find a significant signaling effect of plant closures compared to individual layoffs, I shall expect that the actual (unbiased) effect is even larger.

Table 4. Cox Proportional Hazard Model (Model II)

	Coefficient	Std. Errors.
Plant Closure	0.323***	0.044
Downsizing >50%	0.159***	0.054
Downsizing 20-50%	0.103***	0.031
Local unemployment rate	-0.022 ***	0.002
Number of previous Jobs	0.013***	0.003
Share of days in Unemployment (1-3 years)	0.452***	0.164
Share of days in Unemployment (3-5 years)	-0.160**	0.068
Share of days in Unemployment (5-7 years)	-0.341***	0.056
Firm size >=250	-0.139***	0.022
Tenure (5-9 years)	-0.105***	0.026
Tenure (10-19 years)	-0.127***	0.033
Tenure (≥20 years)	-0.324***	0.069
Low Education (No Vocational training)	-0.288***	0.028
High Education (University)	0.111***	0.047
Age <30	1.011***	0.048
Age 30-39	0.818	0.033
Age 40-49	0.605	0.030
Agriculture	-0.343*	0.184
Qualified manual	0.062*	0.034
Technician	0.117**	0.053
Engineer	0.141**	0.080
Simple service	0.024	0.036
Qualified service	-0.168*	0.088
Semi professional	0.178*	0.081
Professional	0.221	0.164
Simple clerk	-0.020	0.047
Qualified clerk	0.012	0.036
Manager	0.124**	0.070
Other	0.099	0.178
Agriculture	0.171	0.177

Mining	-0.413*	0.215
Energy	-1.011***	0.188
Construction	0.323***	0.048
Retail & Trade	0.101***	0.034
Hotels and Restaurants	0.238***	0.032
Transport and Communication	0.149***	0.048
Finance	-0.064	0.087
Real Estate	-0.083	0.126
Research and Development	0.064*	0.039
Public Administration, Education	-0.240***	0.078
Health	0.174***	0.063
Other services	0.031	0.066
Temporary Agency Employment	0.289***	0.046
Male	0.063***	0.024
Married	0.037	0.024
Children	0.034	0.023
German	0.398***	0.037
#observations	15,526	

Notes: The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2009. The sample excludes tenure of less than two years as well as employments in companies of a firm size smaller than 50. */**/*** denotes statistical significance at the 10/5/1 percent level.

4.3 Robustness Checks

In order to validate the results I use the following specifications of my models as robustness checks. Model VII and VII includes direct job changes, i.e. workers that did register as jobseekers because they have received notification about their displacement, but found a new job in time and therefore did not become unemployed. Model IX and X include dummies for each labor market regions instead of using the local unemployment rate for the region in the month of job termination. Estimations in Models IX and include year dummies to control for time effects.

Table 5. Robustness Checks.

	Model VII	Model VIII	Model IX	Model X	Model XI	Model XII
Closure	0.492***	0.527***	0.283***	0.306***	0.305***	0.326***
	0.036	0.036	0.045	0.045	0.044	0.044
Downsizing (>50%)	-	0.319***	-	0.164***	-	0.145***
		0.044		0.055		0.055
Downsizing (20%-50%)	-	0.127***	-	0.106***	-	0.096***
		0.028		0.032		0.032
Region Fixed effects	-	-	Yes	Yes	-	-
Year Fixed Effects	-	-	-	-	Yes	Yes
# observations	18,846	18,846	15,526	15,526	15,526	15,526

Notes: Model VII and VIII included direct job changes without unemployment, Model IX and X include labor market region fixed effects. Model XI and XII include year fixed effects. The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2009. */**/*** denotes statistical significance at the 10/5/1 percent level.

Table 5 support previous results for plant closures and downsizings as the effects remain robust despite the specifications. Interestingly, the effects for closure and downsizings are even higher if direct job changes are included. This seems reasonable given that this might be the high-potential workers who easily found jobs on the external labor market and therefore did not become unemployed.

4.4 Heterogeneity in the Signaling Values

So far, my analysis shows the average effect on unemployment durations for a displacement from a closing or downsizing firm compared to individual layoffs. As a next step, I estimate models with interaction terms between plant closures and other covariates in order to test for heterogeneity in the magnitude of the closure signal. In their analyses Gibbons and Katz (1991) and Doiron (1995) find evidence for the signaling hypothesis for a sample of white-collar workers, while the effect for blue-collar workers remains insignificant. Their explanation for the heterogeneous effect is that blue-collar jobs are often covered by collective bargaining agreements that involve explicit layoff-by-seniority rules. Hu & Taber (2011) find the signaling effect only for male white workers, while effects are different and insignificant respectively for black and female workers.

In order to test for different magnitudes of the closure variable among different groups, I interact the closure variable with several covariates. Table 6 shows the estimation results with interaction terms. Note that I run a separate specification of Model II for each interaction term. Unlike former research on the topic I do not find heterogeneity for the signaling effect across educational levels. The only strong interaction is found for job tenure, i.e. the strengths of the signal increases with the tenure of the job. According to Gibbons and Katz (1991), the least productive employees should be the first to be displaced. Hence, high tenure can be seen as a signal for high productivity. However, individual layoffs after a long tenure are suspicious and should therefore only occur in particularly unproductive cases (Hut & Taber 2011). This especially holds in the presence of high employment protection for tenured workers as in Germany.

In addition I test whether the signaling effect differs with respect to the local unemployment rate. It is conceivable that employers in areas with low unemployment rates have less of a choice which applicant to choose. Thus, the signal should be of smaller magnitude. In contrast, higher unemployment rates should go along with a higher number of job applicants. Employers then have to pay more attention to costless signals such as the reason for termination of the last employment. Again, the interaction terms show no significance at all. Finally, I also test the heterogeneity in the

signaling effect with respect to firm size as proposed by Krashinksky (2002). Again, the signal is only significant on a 10% level. Some interaction terms with sectors are significant. Hotel and Restaurants show positive interactions with plant closures while the interaction effect is negative for real estate.

Table 6. Interaction between Plant Closure and Covariates

	Coefficient	Std. Error
Education		
Low Education	0.146	.094
High Education	0.224	.204
Occupation		
Simple manual	0.594	0.880
Qualified manual	0.692	0.882
Technician	0.303	0.898
Engineer	0.522	0.966
Simple service	0.699	0.882
Qualified service	1.317	0.947
Semi professional	0.142	0.918
Professional	1.310	1.132
Simple clerk	0.438	0.893
Qualified clerk	0.308	0.887
Manager	0.292	0.911
Other	0.401	1.126
Industry		
Agriculture	-0.209	0.591
Mining	0.009	0.354
Energy	-	-
Construction	-0.022	0.153
Retail & Trade	-0.237*	0.123
Hotels and Restaurants	0.760***	0.218
Transport, Storage and Communication	-0.039	0.161
Finance	0.471	0.792
Real Estate	-0.837***	0.148
Research and Development	-0.220	0.171
Public Administration, Education	-0.561	0.382
Health	-0.140	0.277
Other	-0.191	0.366
Temporary Agency Employment	-0.360	0.285
Tenure	0.000***	0.000
Local Unemployment Rate	0.026	0.919
Firm size	0.000*	0.000
# observations	26,854	

Notes: The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2009. The sample excludes tenure of less than two years as well as employments in companies of a firm size smaller than 50. */**/*** denotes statistical significance at the 10/5/1 percent level.

5. Conclusion

In this paper, I examine the effect of the type of displacement on unemployment duration as introduced by Gibbons and Katz (1991) for the German labor market. My intention is to test if and to what extent employers use the termination of the last employment as a costless signal in order to screen job applicants in an ex-ante asymmetric information framework. I distinguishe between displacements on an individual base, due to downsizings and due to plant closures. Theory predicts that individual layoffs are considered as a negative productivity signal compared to a dismissal due to a plant closure. I apply a Cox proportional hazard rate model to test whether the reason for displacement has a significant effect on unemployment durations. In contrast to previous research, I can distinguish between reasons for job termination in this administrative dataset.

My main result is that employers indeed use the reason for job termination as a signal to screen applicants. They prefer applicants that were laid off due to a plant closure or due to a downsizing of the firm towards applicants that were laid-off on an individual base. For layoffs due to plant closures I find a transition rate into employment to be 35% higher compared the reference group that was displaced through individual layoffs. Hence, I contribute to the empirical literature that seeks to explain unemployment durations by adding further information that should be accounted for in future studies.

However, I find the signaling effect to be of slightly higher extent than in similar studies who found values of up to 30%. I also confirm the findings by Frederiksen et al. (2013) stating that the strengths of the signaling effect is proportional to the share of displaced employees. However, the effects of downsizings are slightly higher for my sample. These specific results for the German labor market might emerge because employers do not have discretion with respect to whom to lay off not have since they have to meet certain criteria in a layoff process.

Although the data allows us to contribute on existing literature, I still face inadequateness that should be solved in further research. For example, the data does not cover a job seeker's search effort, e.g. the number of applications he/she wrote and the time spend for reading job advertisements which is of importance when unemployment durations are analyzed. As mentioned in Uysal and Pohlmeier (2011), personal characteristics such as consciousness and openness are important factors that can explain differences in unemployment duration. Furthermore, the effects of (career) networks in job search should not be neglected as they provide job seekers with a huge network with a higher number of job offers. I leave this for future research.

Appendix:

Table 1. List of Variables

Variable	Type	Explanation	Reference Category
Type of displacement	Categorical	Three dummy variables: plant closure, severe (≥50%) and small (20-50%) downsizing	Individual layoff
Last job tenure	Categorical	Three dummy variables: 5-9 years, and 10-19 years, ≥20 years	< 5 years
Age	Continuous	Three dummy variables: <30, 30-39,40-49	≥ 50
Firm size in the previous year	Categorical	One dummy for large firms (≥ 250 workers)	< 250 workers
Local unemployment rate	Continuous	Local unemployment rate in percent	N/A
Education	Categorical	Two dummy variables: High education (university degree), low education (no vocational training)	Vocational training
Occupation	Categorical	Thirteen dummy variables	Simple Manual
Sector	Categorical	Fifteen dummy variables	Manufacturing
Male	Categorical	One dummy variable	Female
German	Categorical	One dummy variable	No German Citizenship
Married	Categorical	One dummy variable	Not married
Children	Categorical	One dummy variable	No children
Durations of previous unemployment	Continuous	Three variables: Share of days spend in unemployment in the intervals of 1-<3, 3-<5, 5-<7 years before the beginning of the current unemployment spell	N/A
Number of previous jobs	Continuous	Number of jobs before the beginning of the current unemployment spell	N/A

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