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Labor Market Signaling and Unemployment Duration: Evidence from Germany

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Labor Market Signaling and Unemployment Duration: Evidence from Germany

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 we use administrative linked employer-employee data for Germany which not only contain daily information on unemployment duration but also allows us to control for the type of displacement. We show that workers displaced due to plant closures and downsizings find indeed significantly faster a new job. We also find the strengths of the signaling value 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 to affect unemployment durations, e.g. institutional settings (Hunt, 1995; Von Wachter et al., 2012), previous unemployment spells (Heckman & Borjas, 1980; Heckman, 1981) previous employment spells (Card & Sullivan, 1988, Faber, 1994), personal characteristics (Uysal & Pohlmeier, 2011) or macroeconomic conditions (Abbring et al., 2002). This paper argues that 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 duration and on selection mechanisms in asymmetric information frameworks. The topic gained our attention for two reasons. On the one hand laid-off workers in Germany invest considerable amounts of time and money to file a lawsuit in case a layoff is believed to be unfair. Plenty of these lawsuits end in a mutual agreement with redundancy payments and are at least not labeled as displacements. We want to evaluate if any of this effort pays off with regard to re-employment probabilities or if there is no relation between the reason for displacement and the following unemployment spell. On the other hand, if employers had a preference for certain workers based on previous job termination, this information should be included in empirical studies that seek to explain unemployment durations.

Based on the seminal work of Akerlof (1970), who identified reasons for market failure 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. The basic idea is that an individual layoff is treated as a negative signal as it is likely the result of low revealed productivity. In contrast a displacement due to plant closures appears to be independent of a worker's productivity. Therefore the model predicts higher wage losses and longer unemployment durations subsequent to individual layoffs.

When it comes to the screening of applicants, one might argue that employers 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.

The bulk of literature that tests the model by Katz & Gibbons (1991) empirically investigates the wage effect of job termination. For a broad overview on literature und post-displacement wages see Von Wachter (2009). One reason for the focus on wages is that most of the papers that investigate the signaling effect focus on the US, a labor market that is characterized by high flexibility that has hardly experienced long periods of unemployment in the past. Usually, the channel in which the effect of signals would be observed is the wage. In contrast Germany, like many European labor markets, has binding wage agreements between unions and employers' associations and therefore huge differences in the duration of unemployment are found.

In an empirical application using the 1984-1986 Displaced Workers Survey (DWS), Gibbons and Katz (1991) find higher wage losses for individual layoffs compared to layoffs due to plant closures as predicted by their model. The effects are stronger for white collar workers than for blue collar workers. Doiron (1995) tests the hypothesis for Canadian Survey data and confirms the results of wage losses for white-collar workers while effects for blue-collar workers remain insignificant. Using survey data for the U.S., Stevens (1997), Song (2007) and Krashinsky (2002) confirm larger and persistent wage losses for individually laid off workers. Burda & Mertens (2001) use information on job termination from survey data to estimate the displacement reasons of a register-based sample. They find that displaced workers face higher wage loss than non-displaced workers. Weynandt (2014) uses Austrian Social Security Register to test differences in re-employment wages among individual layoffs, downsizings and layoffs due to plant closures and confirms the findings of Gibbons and Katz (1991) for Austria.

However, there are other contributions that cast doubts on the findings of Katz and Gibbons (1991). Krashinsky (2002) provides an alternative explanation for wage differences based on adverse selection and 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 when beginning a new job in a small firm than workers displaced through layoffs from large firms who find new employment in a small firm. In a first estimation he confirms the findings of Gibbons and Katz (1991) but the effect loses significance once he controls for firm size effects in his estimations. Evans and Leighton (1995) and Oyer (2004) point to the existence of a recall bias in survey data such as the DWS, i.e. the respondent's memory of prior events tends to erode over time resulting in understatement of unemployment durations. Therefore, results based on survey data may lack of reliability. Song (2007) takes the recall bias into account and control for the pre- and post-displacement wage tenure profiles and also finds no significant

results for the signaling hypothesis. Grund (1999) uses survey data from Germany which include the reasons for job determination. He does not find evidence for the signaling hypothesis and points to an explanation already given in Gibbons and Katz' (1991) study: Workers displaced in plant closings are more likely to be located in distressed local labor markets characterized by high unemployment, i.e. they face higher unemployment durations and lower bargaining power due to local labor market conditions. As the signaling and the local labor market effect point in opposite directions, the local labor market effect can neutralize or even outbalance the signaling effect. As a result, unemployment durations of workers displaced due to firm closures can be longer than unemployment durations subsequent to individual layoffs, even if the dismissed workers themselves are preferred towards individually displaced workers. As Grund's (1999) empirical analysis neglects local unemployment 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 why the findings for the United States and Germany differ.

While the bulk of literature focuses on the wage losses there is only little empirical evidence for the signaling effect of job termination on unemployment duration, especially for Europe. In the empirical application of their own model, Gibbons and Katz (1991) confirm their predictions using data from the 1984-1986 Displaced Workers Survey (DWS). They find unemployment durations for individual layoffs to be 25% longer compared to displacements due to plant closures. The effects are stronger for white collar workers as blue-collar jobs are often covered by collective bargaining agreements that involve explicit layoff-by-seniority rules. Further literature that tests the signaling effect on unemployment durations is relatively rare. Biewen and Steffes (2010) use the same survey data as Grund (1999) for Germany and include the type of displacement in a Probit model with unemployment status as dependent variable and find only limited evidence for the stigma effect of displacements. Using administrative data from Denmark, Frederiksen et al. (2013) find the type of displacement to have a significant effect on unemployment durations. Depending on the model, average re-employment probabilities are about 23%-30% higher in case of plant closures. The effect is significant across all occupational groups. They find the strengths of the signaling value to be proportional to the share of displaced workers. For the German labor market, no comparable study on unemployment durations exists.

An important shortcoming of previous literature is the lack of reliable data that allows distinguishing between collective and individual displacements. In order to test the signaling theory empirically, a sample of displaced worker is required, i.e. other reasons for job termination such as job leavers need to be excluded from the sample. Data used for studies 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 the recall bias. In addition Jacobson 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 dataset include a large number of observations and a rich set of variables, but lacks information on the reason for job termination.

In what follows, we provide an empirical analysis which uses register-based data of daily frequency which includes information on the reasons for job termination and on firm size. Thus, we can focus on displaced workers and distinguish between individual layoffs, downsizings and plant closures. We solve for the shortcomings of previous studies by including regional unemployment rates and firm sizes as covariates and by using interaction terms with the type of displacement. We can therefore control for local labor market conditions and firm size effects. Using a Cox proportional hazard model, we find evidence for the signaling hypothesis. Job seekers displaced through plant closures and mass layoffs face shorter unemployment durations compared to individual layoffs. Furthermore, our analysis does confirm previous findings that the strength of the signaling value is proportional to the share of displaced workers.

The paper is organized as follows: Section two gives a brief introduction on the institutional settings in Germany considering employment protection and unemployment benefits. 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

In Germany, different rules apply for the three kinds of displacement (individual, collective, plant closings) due to the German employment protection legislation. 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.

Gibbons and Katz (1991) stress that besides observed productivity, institutional settings such as seniority rules may have influenced the employers' decision on whom to lay off to a large extend. Therefore the true signaling effect may actually be larger than in their estimations. As Grund (1999) argues, differences in labor market institutions are one reason why the findings of signaling effect differ between the US and Germany. Thus we give arguments in how far German employers are not free in their decision on whom to lay off due to strict employment protection legislation.

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 personal misconduct, e.g. thievery, lack of personal capabilities, mostly long-lasting sickness or for economic reasons. To justify a displacement for lack of personal capabilities jurisdictions and case law set high standards. As a consequence, most employers dismiss workers for economic reasons to dissolve an employment relationship even though the true reason lies in the productivity of the worker (Jahn, 2009).

Regarding collective displacements, the German employment protection legislation specifies social criteria determining the rank order in which workers can be dismissed. 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 worker with the lowest rank must be dismissed first. These rules apply also for individual layoffs if they take place for economic reasons.

In case of plant closures where all employees become redundant, social criteria do not play a role. Therefore, displacements in a plant closure are completely independent of employees' characteristics and abilities.

Furthermore, employers have to consult work councils timely and extensively about imminent plant closings (§111 Industrial Constitution Law). Hence employees are informed about plant closings very early and especially the more productive employees have the possibility to get a new job in another firm before the plant closing 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

3.1 German Administrative Data

We use a linked employer-employee data set that consists of two sources of administrative data from the German Federal employment agencies' Institute of Employment Research (IAB).

The administrative data is highly reliable and is used by the employment agency to compute levels of social security contributions and unemployment benefits. The integrated employment biographies (IEB) contain all individuals that were registered as job seekers and who received UB and UA as well as all individuals employed subject to social security contributions in the period from 1975 to 2010 on a daily frequency¹. The IEB covers about 80% of the German workforce and provides rich information on employment status, wages and socio-demographic characteristics². Furthermore, we are able to distinguish between displacements, mutual agreements, fixed-term contracts and job leavers as this information is asked during registration as job seeker. For our analysis, we use a 5% sample of the IEB.

We match the employment biographies with the establishment history panel (BHP) which provides information on firm size, wage and occupation of the establishments at an annual frequency.

When firm closures are identified in the data it can create large biases to focus simply on a change of the establishment identification number (EID) since restructuring and relabeling of firms is often poorly measured. As the EID can change for other reasons than plant closures, e.g. due to a change of ownership or legal form, plant closures can be identified without a corresponding event to happen in the firm. To solve the problem of proper plant entry and exit identification, Hethy and Schmieder (2013) have developed a strategy that focuses on the workforce as an indicator. They classify new EIDs either as new establishments, Spin-Offs, or ID changes based on whether the workers in a new establishment come from the previous EID. 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 same firm in the following year. Their definition has the advantage that the change of the EID, a change of the owner or of location will not lead to wrong identifications of plant closures.

3.2 Sample Selection

We focus on involuntary job losses (i.e. job displacements) that can appear either individually or on a collective base. Among collective displacements, we distinguish between firm closures

¹ For more information on the IEB see Dorner et al. (2010).

² The data lacks on information on periods of self-employment or inactivity, but as these people cannot be displaced they are not relevant for our sample.

and downsizings, i.e. a decrease of the workforce of more than 20%³. Since we treat plant closures and downsizings as exogenous and unexpected shocks for the employees, the layoff needs to be initiated by the employer. Therefore, we focus on displacements and exclude all other reasons for job termination such as mutual agreements, fixed-term contracts or voluntary job leavers. To test our model we make sure that a firm's performance is independent of a single employees' productivity. Thus we follow Schmieder et al. (2010) and exclude observations from small firms with less than 50 employees in the year prior to the beginning of an unemployment spell in order to have a reasonable threshold for downsizing. To make sure that downsizings appear as shocks and are therefore unexpected we follow Couch (2010) and Schmieder et al. (2010) and restrict our sample to workers with job tenures of at least three years in the previous job. Thereby we only observe employees in expectation of stable jobs and ensure that job changes are really not on a voluntary base, as in Germany job mobility and on the job search behavior decreases sharply after a few years of job tenure (Schmieder et al, 2010).

Moreover, UB and UA differ in their level of compensation and therefore set different incentives for job search. According to Hunt (1995) these differences in incentives influence unemployment durations significantly. By excluding low-tenured workers we make sure that all job seekers in the sample have worked long enough to be eligible for UB. Finally, by defining a minimum tenure of three years we rule out that the dismissal is due to a mismatch between worker and employer. Therefore we can make sure that individual layoffs are seriously based on productivity and not due to a bad matching considering task, occupation or personality. We also exclude recalls, e.g. an employee is hired again by the prior employer after a layoff, from the sample since in these cases employers have already observed a worker's ability during the previous job. Hence, there is no more asymmetric information situation and the usage of a signal to screen applicants would become pointless.

We 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. With this definition we make sure that our sample contains only people that are currently not employed and still interested in finding employment. We 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. For cases of undocumented

³ A mass layoff is defined as a downsizing of more than five workers force 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.

interruptions in the data we follow Kruppe (2009) and Kruppe et al. (2008) and use seven days as the maximum tolerated interruption length. Observations with undocumented interruptions of more than seven days are excluded from the sample.

For the downsizing variables, we focus on the firm size, i.e. the workforce of a firm and calculate the difference to the previous year. We define downsizings as small if the firm was reduced in size by between 20 and 50 percent and as severe if a reduction by more than 50 percent occurred in a firm that is not defined as closed.

We restrict our sample to men aged 20-55 years to circumvent selectivity issues regarding female labor force participation and early retirement. 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. The reason for job termination is asked during the registration as a job seeker. Consequently, we restrict our sample to the period 2004-2010. Employees with a sickness absence of more than six weeks are excluded from the sample.

The control variables we include are individual variables such as age, marital status (dummy), children in the household (dummy), education (two dummies) and annual dummies as well as information about the last work such as tenure (two dummies), industry (eleven dummies) occupation (thirteen dummies) a change in occupation compared to the last job (dummy) and firm size in the previous year (two dummies). In order to control for local labor market conditions we use the labor market regions concept developed by Kosfeld and Werner (2011) who constructed 141 labor market regions for Germany based on commuter's behaviors. We include the unemployment rate of the previous employer's labor market region in the month of the layoff in our model and the squared term as we expect a non-linear relation. Even though the unemployment rate is a time variant covariate, we only include the rate during the transition from employment to unemployment. As we 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, we only need to account for the unemployment rate at that time.

One of the main concerns we face is how we can treat the problem of unobserved heterogeneity in our sample that might bias our estimation results. A strand of literature that focuses on accounting for unobserved heterogeneity is the literature on matching. Since estimators of treatment effects usually rely on the untestable assumption of unconfoundedness, one has a strong interest to account for as many relevant variables as possible. In this paper we are also concerned to include as much relevant information on personal traits and a person's

previous employment biography as possible since we seek to separate the effect of the displacement for other unobserved effects that could bias our estimates. Therefore we follow Caliendo et al. (2014) who propose a large set of variables and include further information from the job seeker's employment 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, we generate intervals with measures of the share of days spend in unemployment in the previous (a) 4-5 (b) 6-7 and (c) 8-10 years before the start of an unemployment spell.

Having included this wide range of information from labor market biographies and labor market conditions we hope to have excluded all bias in our estimations potentially caused by observed heterogeneity as far as possible. Table 6 contains a detailed list of all variables used in our estimations.

3.3 Descriptive Statistics

Our sample contains 32.300 persons. As Table 1 shows a share of 21.4% of all observations is right-censored. The average unemployment duration is 390 days or about 55 weeks. For the control variables, we use the information at the moment of dismissal.

Table 1 shows that a share of 17% of job seekers has no vocational training, whereas 72% successfully finished vocational training. Almost 10% hold a university degree. Considering displacements we find that about 5% of layoffs were the results of plant closures. Collective dismissals account for 12% of displacement, of which 3.5% were small and 8.5% severe downsizings. More than 80% of our job seekers were laid-off in a year where the firm did not change significantly or was even increasing in size.

- Table 1 about here –
- Table 2 about here -

As Table 2 shows the average unemployment duration is 400 days subsequent to plant closures, 422 and 421 days respectively for severe and small downsizing and 385 days for individual layoffs. Obviously, unemployment durations are longer 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 individual characteristics and local labor market conditions.

4. Estimation strategy and results

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

4.1 Estimation Strategy

For our estimations, we use a semi parametric Cox model. The advantage of this semi-parametric approach is that we can still fit survival models without making any assumption about the shape of the baseline hazard function. Thus we 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 our case plant closures vs. individual layoffs) must have hazard functions that are proportional over time⁴. In the Cox model for person j the estimated hazard rate is

$$h(t|\mathbf{x}_j) = h_0(t) \exp(x_j\beta) \quad (2)$$

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

4.2 Estimation results

We test our signal variables in six specifications: Model I, III and V include a dummy for plant closures besides further covariates. The reference group for the plant closure signal in this estimation are all kinds of displacements other than plant closures (i.e. individual layoffs and downsizings). In Model II, IV and VI we add dummies for small (20%-50%) and severe downsizings ($\geq 50\%$). Here, the reference group consists only of individual layoffs. Model I and II include the sample selections as described in 3.2. In Model III and IV we reduce the minimum firm size to 30 employees. In Model V and VI we reduce the minimum tenure from three to two years.

Table 3 shows the results from our Cox regressions. As predicted by the signaling theory we 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 $\exp(0.096)=7.6\%$ and 8.5% ($\exp(0.103)$) if we

⁴We 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).

add the dummies for downsizings (Model II). We always find a highly significant positive effect on transitions into employment of between 7% and 11% in all six estimations. Displacements due to plant closings have higher job-finding probabilities compared to individual layoffs, *ceteris paribus*. Also, our dummies for downsizings are significant, but to a weaker extend. As found in previous studies, the strength of the signal is proportional to the share of displaced workers: For small layoffs we find an average higher re-employment probability of between 3.5% and 4.5% while the effect for large downsizings is between 6% and 8.5% (Model I).

- Table 3 about here –

The robustness of our findings supports the hypothesis that employers use the type of displacements as a signal for productivity.

We can therefore confirm the findings of previous studies, for instance Frederiksen et al. (2013) that collective displacements give a higher probability of job re-entrance compared to individual layoffs. However, the extent to which the effects differ is lower in our case: 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. One explanation for the differences between the Danish and the German findings is that Denmark has far more relaxed employment protection regulations. In contrast to Germany, employers in Denmark have discretion with respect to whom to lay off. As mentioned in Section 2, German employers have to meet social criteria such as age, tenure and family responsibilities and are thus not free in their decision to lay-off according to productivity. Therefore it makes sense that we do find the type of displacement to be significant in our estimations, as it is a useful signal for employers, but that we find differences in the magnitude of the signal caused by institutional settings.

Grund (1999) argues that the sudden and exogenous shock assumption of the signaling model might not hold for Germany since employers have to inform work councils at an early state and extensively about plant closings (§111 industrial constitution law). Hence, employees are informed in advance and especially the most productive workers will find jobs in other firms soon while less productive workers remain in the closing firm. If this self-selection process would take place our sample contained mainly low productive displaced workers and hence not represent the actual difference between average retained workers and average displaced workers. The real magnitude of the plant closure effect might then be underestimated in our estimations. However, if this was true and our exogenous shock assumption did not hold, our estimation results would still show the lower range of the signaling effect.

Table 4 shows the coefficients for the entire set of covariates of Model II. As expected, the effect of local labor market conditions is significant; reemployment probabilities decrease with an increasing local unemployment rate. Considering the firm size, we find significant non-linear effects, stating that a layoff from small firms reduces reemployment probabilities compared to the reference group of firms with more than 2000 employees whereas displacements for medium size firms increase the probability. We can see that high previous job tenures are treated negatively, as employment probabilities decrease with tenure. Seniority rules and higher firm specific human capital levels lead to higher wage levels for high tenured employees. Hence, as Frederiksen et al. (2006) argue, jobs seekers have a higher reservation wage and have to invest more time for job search. This effect should be larger for job seekers with levels of firm specific human capital that is not transferable across jobs (Violante, 2002; Kambourov & Manovskii, 2009).

- Table 4 about here -

Regarding education our results proof that high education results in higher probabilities for re-entries into employment. Compared to our reference group of job seekers without vocational training, a degree from a university or school of applied science raises employment probabilities by 11%, while applicants with vocational training have a 22% higher reemployment probability.

Being married and having children increases job-entrance probability as well as being a German Citizen. As far as industries are concerned, construction, transportation and mail provide good opportunities to quickly find new employment, compared to the reference group service. More time for job search is necessary in industries such as energy. Considering the share of days spend in unemployment we surprisingly find that the previous 3 to 5 years are of less importance than the other two intervals. As expected, the higher the share the longer the time one has to expect to spend in unemployment. We can see similar findings for the amount of unemployment spells whereas a higher number of jobs increase re-entry probabilities. Having changed one's occupation does not show any significant results in our estimations.

4.3 Heterogeneity in the Signaling Values

So far, our 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, we 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.

In order to test for different magnitudes of the closure variable among occupational groups, we interact our closure variable with occupational dummies. The interaction effects from our estimations are insignificant, i.e. we do not find heterogeneity of the signaling values across occupational groups. For educational groups we do find significant interaction effects with plant closures on the five percent level. Compared to the reference group of workers without vocational training who have not been dismissed from a closing plant, we find lower expected labor market outcomes for workers with vocational training dismissed due to a plant closure. For unemployed with a university degree, the interaction effect is not significant.

- Table 5 about here -

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, especially in the presence of high employment protection for tenured workers in Germany. Therefore, we expect a high tenure of the last job in combination with a plant closure to be considered as a positive signal. Interestingly, our estimations do not confirm this hypothesis. All coefficients of the interaction terms between tenure levels and closure/downsizing signals are insignificant.

In addition we test whether the signaling effect differs with respect to local labor market conditions. 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. We generate interaction terms between our closure dummies and dummies for the local unemployment rate ($\leq 5\%$, $5\% - 10\%$, $10\% - 20\%$ and $\geq 20\%$). Again, our interaction terms show no significance at all.

Following the argument of Krashinsky (2002) we add interaction terms of our closure variable with firm size dummies (250-1999 and ≥ 2000 with 50-249 as reference group) to our model but again find no significant interaction effect. We cannot confirm the findings that firm size has an effect on the closure signal, as Krashinsky (2002) found in his analysis. For the industry we find significance for a few dummies. Only for the energy sector, we find evidence for a positive

interaction effect with our closure variable. In general, we only find little empirical evidence for heterogeneity in the signaling effect.

5. Conclusion

In this paper, we examined the effect of the type of displacement on unemployment duration using the signaling approach introduced by Gibbons and Katz (1991) for the German labor market. Our intention was 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. We distinguished between displacements on an individual base, due to downsizings and due to plant closures. Theory predicts that the individual layoffs are considered as a negative productivity signal compared to a dismissal due to a plant closing. We applied a Cox proportional hazard rate model to test whether the reason for displacement has a significant effect on unemployment duration. In contrast to previous research, we can distinguish between reasons for job termination as well as for changes in the firm size.

Our main result is that employers indeed use the reason for job termination as a signal. 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 plant closures we find a transition rate into employment to be 7-11% higher compared the reference group that was displaced through individual layoffs. However, we find the signaling effect to be of smaller extent than in similar studies who found values of more than 20%. We also confirm the findings by Frederiksen et al. (2013) stating that the signaling value is proportional to the share of displaced employees. However, the difference in the signaling effect between large mass layoffs and firm closures is much smaller than in previous studies. These specific results for the German labor market can be explained by the social criteria in a layoff process that employers have to meet and thus do not have discretion with respect to whom to layoff.

Our results also give impulses for re-thinking policy designs aimed at job seekers. During or shortly after a firm closes down, job seekers are often supported by special trainings and programs in order to buffer effects from mass-layoffs. As our results show, job seekers who were laid off in a plant closure do indeed need a little more time to find a subsequent employment compared to individual layoffs. Nevertheless, if we control for a group of covariates we find that these workers have higher re-employment probabilities. Therefore they actually need less support than job seekers with equal attributes who were laid off individually or due to a collective displacement. But as our results also show, local unemployment is an important determinant of

unemployment durations. Thus, in regions with low unemployment rates, special support subsequent to plant closures should be reviewed. For regions who suffer from high unemployment the support should be kept, even though our results make clear that workers that were laid off in plant closures are not the ones who need the most urgent support.

Although our data allows us to contribute on existing literature, we still face inadequateness that should be solved in further research. For example, our data does not cover a job seeker's search effort, i.e. 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. We leave this for future research.

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Tables and Figures

Table 1. Selected Descriptive Statistics

| | Mean | Std. Dev. |
|--|--------|-----------|
| # observations | 32.300 | |
| Duration unemployment (days) | 390.9 | 415.53 |
| Transition to employment | 78.61 | |
| Right-censoring | 21.39 | |
| Local unemployment rate (%) | 10.74 | 4.88 |
| Tenure (days) | 4022 | 2801 |
| Plant status | | |
| Closed | 4.78 | |
| Downsize >50% | 3.45 | |
| Downsize 20-50% | 8.33 | |
| Individual Layoff | 83.43 | |
| Socio-economic characteristics | | |
| No vocational training | 17.06 | |
| Vocational training | 72.26 | |
| University | 10.28 | |
| Agriculture | 0.96 | |
| Simple manual | 20.70 | |
| Qualified manual | 23.88 | |
| Technician | 4.32 | |
| Engineer | 3.14 | |
| Simple service | 21.60 | |
| Qualified service | 1.56 | |
| Semi professional | 1.71 | |
| Professional | 1.00 | |
| Simple clerk | 4.27 | |
| Qualified clerk | 10.66 | |
| Manager | 5.88 | |
| Other | 0.32 | |
| Biographic information | | |
| Age (years) | 40.46 | 8.59 |
| Share of days spend in Unemployment during the previous: | | |
| 4-5 years | 0.048 | 0.13 |
| 6-7 years | 0.075 | 0.18 |
| 8-10 years | 0.066 | 0.16 |

| | | |
|-------------------------------|-------|------|
| Number of Jobs | 2.96 | 2.49 |
| Number of Unemployment Spells | 2.79 | 2.50 |
| German | 87.70 | |
| Marital Status | 52.09 | |
| Children (Dummy) | 62.86 | |

Notes: The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2010. The sample excludes tenure of less than three years as well as employments in companies of a firm size smaller than 50. Variables are dummies, otherwise units come in parenthesis.

Table 2: Average Duration of Unemployment Spells in Days for Individual (constant or upsizing firm) and collective Displacements (Plant Closure, Downsizing).

| | Plant Closure | Downsizing (severe) | Downsizing (small) | Individual displacement |
|-----------------|---------------|------------------------|-----------------------|----------------------------|
| Duration (days) | 400.37 | 422.11 | 421.84 | 385.03 |
| Std. Dev. | 416.23 | 447.56 | 432.94 | 412.14 |

Note: The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2010. The sample excludes tenure of less than three years as well as employments in companies of a firm size smaller than 50.

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

| | Model I | Model II | Model III | Model IV | Model V | Model IV |
|----------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|
| Closure | 0.096*** 0.030 | 0.103*** 0.030 | 0.100*** 0.027 | 0.106*** 0.027 | 0.074*** 0.027 | 0.080*** 0.027 |
| Downsizing (>50%) | - | 0.082** 0.036 | - | 0.059* 0.033 | - | 0.079** 0.031 |
| Downsizing (20%-50%) | - | 0.044* 0.024 | - | 0.034* 0.021 | - | 0.040* 0.021 |
| # observations | 32,300 | 32,300 | 38,853 | 38,853 | 41,057 | 41,057 |

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 30. In model V and VI the minimum tenure within the firm is reduced from three to two years. Standard errors below coefficients. The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2010. */**/** denotes statistical significance at the 10/5/1 percent level.

Table 4. Cox Proportional Hazard Model (Model II)

| | Coefficient | Std. Errors. |
|---------------------------------|-------------|--------------|
| Plant Closure | 0.103*** | 0.030 |
| Downsizing >50% | 0.082** | 0.036 |
| Downsizing 20-50% | 0.044* | 0.024 |
| Local unemployment rate | -0.013** | 0.005 |
| Local unemployment rate squared | 0.003* | 0.000 |
| Firm size 50-199 | -0.110*** | 0.016 |
| Firm size 200-1999 | 0.055*** | 0.017 |

| | | |
|--|-----------|-------|
| Tenure (3-5 years) | 0.144*** | 0.022 |
| Tenure (5-10 years) | 0.122*** | 0.018 |
| Vocational training | 0.225*** | 0.019 |
| University | 0.1120** | 0.031 |
| Age below 30 | 0.212*** | 0.022 |
| Age above 50 | -0.377*** | 0.021 |
| Simple manual | 0.039 | 0.082 |
| Qualified manual | 0.034 | 0.082 |
| Technician | -0.130 | 0.088 |
| Engineer | -0.184 | 0.092 |
| Simple service | 0.060 | 0.081 |
| Qualified service | -0.246** | 0.099 |
| Semi professional | -0.182* | 0.098 |
| Professional | -0.217* | 0.111 |
| Simple clerk | -0.135 | 0.088 |
| Qualified clerk | -0.184** | 0.084 |
| Manager | -0.229*** | 0.087 |
| Other | 0.344** | 0.150 |
| Agriculture | 0.117 | 0.082 |
| Mining | -0.226** | 0.112 |
| Energy | -0.863*** | 0.137 |
| Construction | 0.202*** | 0.027 |
| Retail & Trade | -0.058* | 0.023 |
| Transport & Mail | 0.154*** | 0.027 |
| Finance | -0.336*** | 0.073 |
| Manufacturing | -0.021 | 0.020 |
| Health | -0.020 | 0.049 |
| Other | 0.304*** | 0.308 |
| Change in occupation | -0.014 | 0.013 |
| Share of days in Unemployment (4-5 years) | -0.017 | 0.058 |
| Share of days in Unemployment (6-7 years) | -0.115*** | 0.073 |
| Share of days in Unemployment (8-10 years) | -0.340*** | 0.044 |
| Number of previous jobs | 0.234 | 0.014 |
| Number of previous unemployment spells | -0.209*** | 0.013 |
| Marital Status | 0.044*** | 0.014 |
| Children (Dummy) | 0.095*** | 0.015 |
| German | 0.177*** | 0.022 |
| #observations | 32,300 | |

Notes: The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2010. The sample excludes tenure of less than three years as well as employments in companies of a firm size smaller than 50. The data sets used are a 5% random sample of the IEB and an annual version of the BHP, 2004-2010. */**/** denotes statistical significance at the 10/5/1 percent level.

Table 5. Estimations with Interaction Terms of Educational Level and Plant Closures.

| | Coefficient | Std. Errors. |
|--|-------------|--------------|
|--|-------------|--------------|

| | | |
|------------------------------|---------|-------|
| Closure | 1.22*** | 0.060 |
| Vocational Training | 1.39*** | 0.024 |
| University | 1.41*** | 0.033 |
| Closed # educ | | |
| Closed * Vocational Training | 0.85** | 0.047 |
| Closed * University | 0.88 | 0.072 |

Note: The reference group contains displacements from constant and upsizing firms. The data sets used are a 5% random sample of the SIAB and an annual version of the BHP, 2004-2010. */**/** denotes statistical significance at the 10/5/1 percent level..

Table 6. Overview of all 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 | Two dummy variables: 3-5 years and 5-10 years | More than 10 years |
| Age | Categorical | Two dummy variables: younger than 30 and older than 50 | 30-50 years |
| Firm size in the previous year | Categorical | Two dummy variables: 200-2000, >2000 | 50-200 |
| Local unemployment rate | Continuous | Two Variables: Normal and squared term | N/A |
| Education | Categorical | Two dummy variables: university degree, Vocational training | No vocational training |
| Occupation | Categorical | Twelve dummy variables | Agriculture |
| Change in Occupation | Categorical | One dummy variable | No change in occupation |
| Sector | Categorical | Ten dummy variables | Service |
| 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 4-5, 6-7, 8-10 years before the beginning of the current unemployment spell | N/A |

| | | | |
|--|------------|--|-----|
| Number of previous unemployment spells | Continuous | Number of unemployment spells 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 |
