Labor Turnover before Plant Closure: 'Leaving the Sinking Ship' vs. 'Captain Throwing Ballast Overboard'

GUIDO SCHWERDT

CESIFO WORKING PAPER NO. 2252
CATEGORY 4: LABOUR MARKETS
MARCH 2008

CESifo *Working Paper No. 2252*

Labor Turnover before Plant Closure: 'Leaving the Sinking Ship' vs. 'Captain Throwing Ballast Overboard'

Abstract

Involuntary job loss in administrative data is commonly identified by focusing on mass-layoffs or plant closures. However, such events usually do not happen without prior knowledge, which potentially leads to selection in the labor turnover of distressed firms. We find that workers separating from closing plants up to 2 quarters before closure are associated with significantly lower displacement costs and on average significantly higher pre-closure earnings levels as opposed to ultimately displaced workers. Furthermore, our results indicate that displaced workers with high pre-closure earnings experience significantly lower reductions in future employment probabilities. These findings suggest that compositional differences cause estimated displacement costs to differ between early leavers and ultimately displaced workers. Focusing exclusively on the latter group would lead to a serious overestimation of displacement costs.

JEL Code: J63, J65, C21, C23.

Keywords: plant closure, labor turnover, exact-matching, employer-employee data.

Guido Schwerdt
Ifo Institute for Economic Research
at the University of Munich
Poschingerstrasse 5
81679 Munich
Germany
schwerdt@ifo.de

February 22, 2008
I would like to thank Barbara Petrongolo, Andrea Ichino, Steve Pischke as well as seminar participants at the LSE, EUI and CESifo for insightful comments.

1 Introduction

Displaced workers have been the subject of an extensive literature.[1] Introduced in the seminal paper by Jacobson et al. (1993), the standard specification to measure the effect of displacement is borrowed from the evaluation literature: the labor market performance of displaced workers (*treatment* group) is compared to the one of non-displaced workers (*control* group). Typically, this type of analysis requires administrative data, as long-term earnings and employment information must be available for displaced as well as non-displaced workers. A well-known challenge in these studies is the identification of involuntary job separations. The most popular strategy to overcome this difficulty is to focus on displacement-generating events such as mass-layoffs or plant closures (the limit case of a mass-layoff). Separations observed at the moment of such events are assumed to be the result of an exogenous shock and, therefore, independent of a worker's quality. Thus displaced workers should be a random sample of the workforce.

However, as plant closures typically do not happen as a complete surprise to either management or workers[2], it seems realistic to assume that the ultimate shutdown of an establishment is preceded by a period in which both workers and management have time to react strategically. Knowledge of future distress will influence both firms' firing- as well as workers' quitting-decisions. The firm might choose to retain its most productive workers[3], while workers with relatively better labor market opportunities might choose to avoid displacement and quit before closure. As a consequence of this selection process the average cost of separation might also vary relative to the closure of a plant. However, as presumably both mechanisms, "workers leaving the sinking ship" and "the management throwing ballast overboard", are

[1] See Kletzer (1998), Fallick (1996) and Farber (1999) for reviews of the literature on displaced workers.

[2] Advance notice legislation is the most obvious reason why information on impending lay-offs becomes available beforehand. See Addison and Portugal (1987), Jones and Kuhn (1995) and Ruhm (1994) for studies that investigate the effects of advance notice regulations.

[3] Several studies such as Farber and Gibbons (1996), Felli and Harris (1996) and Altonji and Pierret (2001) show that learning about workers' abilities occurs and that it influences the firm's employment decisions.

at work simultaneously[4], post-separation outcomes of early leavers might on average be better, equal or worse compared to post-separation outcomes of ultimately displaced workers.

In this study we investigate the labor turnover process in closing plants as well as differences in post-separation outcomes based on matched employer-employee data for the universe of Austrian workers. In particular, we test empirically three key propositions linked to the selection hypothesis in the labor turnover process before plant closure. Firstly, we investigate whether post-separation outcomes differ significantly between early leavers and ultimately displaced workers. Secondly, if the selection hypothesis is correct, the group of early leavers might be associated with a different level of average productivity compared to ultimately displaced workers. We test this proposition by means of estimating pre-closure earnings regressions. Finally, we investigate the relationship between pre-closure earnings and the effect of displacement in order to understand whether differences between early leavers and ultimately displaced workers in terms of pre-closure earnings can explain differences in post-separation outcomes.[5]

Although the literature on displaced workers is vast, few studies have so far empirically examined the labor turnover process in dying establishments. One recent paper analyzing changes in the composition of worker flows prior to displacement is that of Pfann and Hamermesh (2001). This study tests a model of two-sided learning using personnel data from Fokker Aircraft that cover the paths of layoffs and voluntary quitting through its bankruptcy. The basic idea of the model is, that parties to an employment relationship may learn about each other's intentions about ending the relationship by forming expectations based on the other party's prior behavior that ended similar relationships. Empirically Pfann and Hamermesh (2001) find that

[4]For previous evidence on this, see Pfann and Hamermesh (2001) and Lengermann and Vilhuber (2002).

[5]One should note that this study places itself in the tradition of papers, which take the plant closure itself as a given. A small distinct literature explicitly investigates the link between movements of workers and the causes of plant closure and downsizing (See Abowd et al. (2005) and Carneiro and Portugal (2003)). However, while this issue is not directly addressed here, the results of this study emphasize its importance and also provide "food for thought" for researchers working on this topic.

learning does occur. In particular, they find that workers with a lower firing probability during the closure process have longer job tenure, are males, have higher educational attainment, have technical/vocational schooling, are married, have taken more internal and external training courses and have a higher job evaluation. On the other hand, workers with lower quit propensities are between 35 and 50 years old, have longer tenure and are less well educated. In another paper, Lengermann and Vilhuber (2002) extend the signalling-model of Gibbons and Katz (1991) by introducing the idea that better workers may seek to avoid being viewed as being of average quality by leaving the firm prior to displacement, while those of lesser quality have an incentive to wait until displacement occurs. Using unemployment insurance records for the state of Maryland and proxying for worker quality by employing a measure derived from individual fixed effects stemming from wage regressions,[6] they find evidence for high-skilled workers leaving as well as firms laying off low skilled workers in periods before displacement. Bowlus and Vilhuber (2002), in another study, test the implications of a partial equilibrium search model with notice on impending displacement. Using data from US universal wage records, they find evidence that workers leaving a distressed firm before a mass-layoff have higher re-employment wages as opposed to ultimately displaced workers.

These findings foster the concern that focusing on the ultimately displaced workers might lead to biased estimates of the effect of displacement. A concern that, however, has been long recognized by the displacement literature. The standard approach to overcome this potential problem is usually to include all separations happening within a certain time window before the displacement generating event.[7] A strategy that faces the trade-off between neglecting early leavers and including a considerable amount of normal workforce turnover. While the choice of a time window has been quite ad-hoc in previous studies, we go beyond the existing literature by providing a rationale

[6]Following the technique pioneered by Abowd et al. (1999).

[7]Jacobson et al. (1993) focused on separators whose firms' employment in the year following their departure was more than 30% below their max in the 1970's. Bender et al. (2002) choose a rigid time window of two years before plant closure. Eliason and Storrie (2004) introduce a flexible time window, that varies with plantsize, of up to three years.

for the choice of a particular window. The key assumption that guides us in this exercise is that post-separation outcomes should be indistinguishable between separations from closing plants and separations from non-closing plants if observed separations in closing plants are completely unrelated to the upcoming shut-down of the establishment. Applying this selection criterion reveals that only separations up to 2 quarters before closure should unequivocally be regarded as early leavers.

Moreover, we exploit the size of our available data set to increase the comparability between displaced and non-displaced workers by employing an exact-matching selection algorithm for adequate control subjects. We then extend the standard specification of Jacobson et al. (1993) by allowing for heterogeneous displacement effects between early leavers and ultimately displaced workers. Our findings show that early leavers have significantly better post-separation labor market prospects, both in terms of employment probability as well as earnings, as opposed to ultimately displaced workers. Moreover, pre-closure earnings regressions reveal that early leavers are associated with significantly higher pre-closure earnings even conditional on several individual and plant characteristics. Ultimately, we show that displaced workers belonging to the upper part of the pre-closure earnings distribution are associated with significantly higher post-separation employment probabilities.

Taken together, a picture emerges that is more in line with the "workers leaving the sinking ship" mechanism suggesting that compositional differences cause estimated displacement costs to be significantly lower for early leavers as opposed to ultimately displaced workers. Focusing exclusively on the latter group would therefore lead to a serious overestimation of displacement costs.

The paper proceeds as follows. Section 2 discusses the common definition and practice of measurement of displacement effects and formulates the key propositions tested in this paper. In Section 3 we describe the data and the sample selection. Section 4 provides descriptive evidence on pre-closure characteristics and post-separation outcome variables of separators from closing establishments. Estimation methods to test the main hypotheses and results

are presented in Section 5. Section 6 concludes.

2 Definition and Measurement of Displacement Effects

The formal definition of displacement costs proposed in Jacobson et al. (1993) is given by

$$E(y_{it}|D_{i,s}=1, I_{i,s-p}) - E(y_{it}|D_{i,v}=0 \; \forall \; v, I_{i,s-p}), \tag{1}$$

where y_{it} denotes the earnings of worker i at date t and $D_{i,s}=1$ if worker i was displaced at date s (and $D_{i,s}=0$ otherwise). The information available at date $s-p$ is given by $I_{i,s-p}$ and p is sufficiently large that the events that eventually lead to displacement would not have begun by date $s-p$.

The most straightforward specification of a statistical model to estimate earnings losses corresponding to the definition in equation (1), that is presented in Jacobson et al. (1993), reads as follows

$$y_{it} = \alpha_i + \gamma_t + \sum_{k \geq -m} D_{it}^k \delta_k + \epsilon_{it}. \tag{2}$$

This model represents workers' earnings histories (y_{it}) and identifies displacement costs with a subset of the model's parameters (δ_k). The specification allows the pooling of information for workers displaced at different periods, by introducing a set of dummy variables for the number of quarters before and after worker's separation, D_{it}^k, where $D_{it}^k = 1$ if, in period t, worker i had been displaced k quarters earlier (or, if k is negative, worker i was displaced $-k$ quarters later). Moreover, worker's earnings depend on some controls for calendar time effects (γ_t) and individual fixed effects (α_i).

Taking this model to the data involves several difficulties. First, it typically requires administrative data in order to obtain information on long-term labor market outcomes of displaced as well as non-displaced workers. The use of administrative data, however, normally implies the shortcoming of having no information about the cause of an observed separation. The most

popular strategy to overcome this problem is to focus on separations occurring at the moment of displacement-generating events such as mass-layoffs or plant closures, which can be identified in matched employer-employee data by reductions in plant-/firm-level employment. To cope with the possibility of displacements happening prior to the identified displacement generating event, it is common practice to include all separations observed within a certain time window before the actual event. Note, therefore that this standard specification encompasses two types of displaced workers: "early leavers" (those who separated before the displacement generating event) and "ultimately displaced workers" (those who remained employed until the bitter end). Hence, the set of dummy variables identifying displaced workers in equation (2) could be decomposed into $D_{it}^k = UD_{it}^k + EL_{it}^k$, where UD_{it}^k and EL_{it}^k have an identical interpretation as D_{it}^k with the additional distinction that UD_{it}^k identifies ultimately displaced workers, while EL_{it}^k identifies early leavers.

Incorporating this definition in equation (2) results in the following expression

$$y_{it} = \alpha_i + \gamma_t + \sum_{k \geq -m} (UD_{it}^k + EL_{it}^k)\delta_k + \epsilon_{it}. \tag{3}$$

This paper now proposes the simple idea that displacement effects ($\delta's$) vary according to the timing of separation relative to the closure of a plant. In particular, displacement effects are different for early leavers and ultimately displaced workers. Making this distinction is motivated by economic theory. Given advance knowledge about the upcoming event, a search model of the labor market implies that such knowledge lowers the value of a given employment relationship as the probability of ending in unemployment increases.[8] This, in turn, lowers the worker's reservation wage and increases a worker's search intensity. If workers are heterogeneous with respect to their outside opportunities, then workers with better labor market prospects might engage more intensively in on-the-job search, receive more job offers

[8]The search framework is typically used in studies examining the effect of advance notice of job-displacement. See Ruhm (1994), Friesen (1997) and Bowlus and Vilhuber (2002).

and consequently have higher quit rates.[9] On the other hand, a negative demand shock for the firm's product results in reductions in the demand for labor. If firms have discretion on whom to lay off and private knowledge about workers' "true" productivity, the firm chooses to lay off less productive workers first, who are presumably associated with relatively bad labor market prospects. In sum, both mechanisms suggest that a selection on individual characteristics exists in the labor turnover of distressed firms. Empirical evidence presented in Pfann and Hamermesh (2001) and Lengermann and Vilhuber (2002) supports this selection hypothesis. Consequently, displacement effects of early leavers and ultimately displaced workers might vary due to this selection process based on workers' characteristics.

We formulate this potential implication of selection in the turnover process of closing plants as a testable proposition:

Proposition 1 *Displacement effects vary according to the timing of separation relative to the closure of a plant. In particular, workers separating early in the closure process face different displacement costs as opposed to ultimately displaced workers:*

$$\delta^{EL} \neq \delta^{UD},$$

where, omitting any subscript indicating the distance to separation, δ^{EL} and δ^{UD} refer to the effect of displacement for early leavers and ultimately displaced workers, respectively.

Proposition 1 states the first key hypothesis this study aims to test. However, even if observed displacement effects differ between early leavers and ultimately displaced workers, these differences could be due to reasons other than selection based on workers' characteristics. While previous studies have investigated differences between early leavers along various dimensions, we limit our focus to differences in pre-closure earnings. Acknowledging the limited capability of earnings to proxy for workers' productivity, we nevertheless

[9]This study takes the point of view that any separation -whether a layoff or a quit- should be included in the treatment group if the separation is related to the upcoming closure. A distressed firm might, for instance, enforce wage cuts. A worker, who would have normally remained in the firm, might therefore quit.

expect earnings to be positively correlated with individual productivity.[10] Hence, if selection based on productivity-related worker characteristics takes place, we would expect to see differences between early leavers and ultimately displaced workers in terms of pre-closure earnings. However, as presumably both mechanisms, firms' laying off less productive workers and workers with better labor market prospects quitting, are at work simultaneously, the two channels might offset each other in such a way that on average no differences in pre-closure earnings exist.

Thus, we expect to see differences in average pre-closure earnings only if one selection mechanism "dominates" the other. To test for this form of dominance, we formulate the following testable proposition:

Proposition 2 *Average pre-closure earnings of displaced workers vary according to the timing of separation relative to the closure of a plant. In particular, workers separating early in the closure process are associated with different levels of average pre-closure earnings compared to ultimately displaced workers.*

Ultimately, differences in average pre-earnings levels between early leavers and ultimately displaced workers can serve only as a potential explanation for differences in post-separation outcomes between these groups, if pre-closure earnings levels are related to the effect of displacement. Consequently, it remains to test whether displacement effects are correlated with pre-closure earnings levels:

Proposition 3 *Pre-closure earnings are correlated with the effect of displacement. In particular, workers belonging to the upper part of the pre-closure earnings distribution are associated with different costs of displacement as opposed to workers' positioned at the lower end of the distribution.*

Testing the validity of the latter two propositions could shed light on the link between selection in the labor turnover process before plant closure and differences in displacement effects relative to plant closure.

[10]The standard assumption that labor earns its marginal product might be violated for several reasons such as implicit incentive contracts or union bargaining.

3 Data Description

The data stems from the *Austrian social security database* (ASSD). The data set includes the universe of private sector workers in Austria covered by the social security system. All employment records can be linked to the establishment in which the worker is employed. It contains detailed information on individuals' employment and earnings histories as well as certain individual characteristics. Daily employment and monthly earnings information is extremely reliable, because social security tax payments for firms as well as benefits for workers hinge on these data.[11] Monthly earnings are top-coded, which applies to approximately 10% of workers. We transformed monthly gross earnings in daily wages dividing them by effective employment duration in each month of observation. Furthermore, the data includes information on employers such as geographical location, industry and size of the establishment.

The data set covers the period from 1978 until 1993 at a quarterly frequency, where the 10th of February, May, August and November serve as reference dates for the data collection. This setup implies that an individual is recorded as employed in a given quarter only if she is employed at the corresponding reference date. We concentrate on workers employed in the period 1982 to 1988 - who are in the risk set for a plant closure in this period; this allows us to observe the workers in detail 5 years prior to bankruptcy and 5 years afterwards.

The ASSD contains no direct information on plant closures. Following best practice in the displacement literature we identify a plant closure by the disappearance of a plant identifier. Each establishment has an employer social security number. Hence, a shutdown of an establishment in the data occurs when the employer identifier ceases to exist. As the unit of analysis is a plant as opposed to a firm, the possibility remains that a disappearance of an establishment identifier reflects re-organization or takeovers. To avoid including these "false plant deaths" we impose the following restriction: A plant is coded as a closing plant at the reference date t if two conditions

[11] See Hofer and Winter-Ebmer (2003) for a description of the data set.

are satisfied: (i) The plant identifier disappears during the three months following the reference date t (not observed anymore at $t+1$) and does not re-emerge during the following year[12]. (ii) Less than 50% of the employees of an establishment find a new employment relationship under the same, new establishment identifier.[13] The latter condition minimizes the inclusion of "false plant deaths", but might eliminate also some true plant closure, where large groups of workers move "together" from one dying firm into the same new firm.[14]

The sample selection follows closely the one applied in Ichino et al. (2007). The sample contains workers who fulfill the following conditions, at least at one of the quarter reference dates from 1982 to 1988: (i) Workers from plants not belonging to the construction and tourism industry. (ii) Workers from plants that once had at least 5 workers between 1978 and 1988. (iii) Blue and white collar workers with at least one year of tenure. (iv) Workers between 35 and 55 years of age.

The first two criteria are meant to exclude seasonal employment and establishments without basically any dependent employees. The latter two criteria should ensure that all workers present similar legal requirements for layoff. Low tenure workers and older workers might be easier to layoff due to probation periods or early retirement regulations.

The setup described above allows us to identify 4,703 closing plants between 1983 and 1988. Table 1 shows the incidence of plant closure by quarter and year. It reveals a clear seasonal pattern of plant closures. Almost one third of all closures occur in the last quarter of a year. The number of closures per year increases slightly during the 1980s. The distribution of plant closures over the nine federal states of Austria is displayed in Table 2. Almost one third of all closures happen in Vienna, the biggest and economically

[12]This condition is set to one year, because the plant identifiers are assigned anew after two years.

[13]Workers from such establishments are coded as "ambiguous" and are neither in the treatment nor the control group.

[14]This might be especially relevant in the European context, because of legal requirements before mass-layoffs such as "social plans". In this case the displacing firm might have gone through extraordinary efforts to secure re-employment of its workers at other firms.

most powerful province of Austria.

The upper panel of figure 1 plots the total number of employees in all plants closing between 1985 and 1988 against quarters relative to closure.[15] While total employment decreases over all three years before closure, it becomes apparent that the number of separations increases sharply in the last year before closure. In fact the number of employees more than halves from 28296 one year before closure to 12126 workers just before the closure. This drastic decline suggests that some of these separations are related to the upcoming closure of the establishments.

The bottom panel shows two examples of employment trends at the plant level before closure. Broadly speaking we observe two types of closing establishments in the data. Type A, represented by the lower left figure, shows no or a slow decline in total employment before closure. Type B (lower right figure) is characterized by sharp stepwise downsizing in the quarters just before closure. Especially the latter type gives reason to believe that displacement (or closure-related separations) happens even several quarters before the ultimate closure.

Finally, it is worth noting another point at this stage. The figures on total employment in figure 1 are based on a generated variable that counts all employees in the social security records associated with the respective plant identifier. However, not all employees fulfill the selection criteria outlined above. Moreover, as the final analysis is conducted based on an exact matching procedure (see section 5), some workers, although fulfilling the above criteria, could not be matched to a control and, therefore, are not included in the empirical analysis. The dotted lines in the lower panel of figure 1 indicate the number of workers included in the empirical analysis. Notably, the number of workers included in the empirical analysis shows a more stable pattern before closure than total employment does. This reflects that a significant number of separations before closure include low tenure workers or workers not in the age group between 35 and 55.

[15]Note that total employment in figure 1 refers only to a subset of the 4,703 closing plants. Namely to all plants closing between 1985 and 1988 for which information on plantsize is available for all 12 quarter before closure.

4 Descriptive Statistics on Separations before Plant Closure

It is common practice in the displacement literature to include also separations happening within a certain time-window prior to the displacement-generating event. While this reduces the possibility of neglecting early leavers, it increase at the same time the chances of including a considerable amount of normal workforce turnover. Thus, we first analyze separations happening before plant closure to detect potential patterns that might distinguish plant-closure-related separations from normal turnover. In the following, we therefore present various descriptive statistics for different separators groups distinguished by the timing of the separation relative to the closure of the plant.

Figure 2 shows changes in average workforce characteristics in all closing plants before closure. All variables are held constant at their level three years before closure. Any variation, therefore, stems from changes in the composition of the workforce.[16] The top left panel reveals that the share of female workers remains relatively stable at around 49% during quarters 12 to 4 before closure, but increases during the last year before closure by 6 percentage points. This indicates that early leavers are mainly men. Furthermore, early leavers are also mainly blue collar workers, which can be seen from the top right panel. The share of white collar workers in dying establishments jumps up by 12 percentage points in the last year of existence. Before this period, the share of white collars is steadily declining.

The higher share of blue-collar workers might be explained by institutional factors. In particular, the legislation on advance notice varies for blue and white collar workers in Austria. Depending on age and tenure, blue collar workers receive an advance notice of displacement up to two weeks before dismissal. White collar workers, on the other hand, receive such a notice between 1.5 and 5 months before dismissal.[17] Hence, if economic difficulties

[16] New hires are not included. Hence, compositional changes are solely induced by separations.

[17] See OECD (1993) for an overview of employment protection legislation in several OECD countries including Austria.

make downsizing necessary, it is less difficult to layoff blue-collar workers.

The middle panels show average experience and job tenure in days. Average experience rises up to the fourth quarter before closure by 30 days reflecting the fact that these very early separators have below average experience levels. During the last year of the plant's existence, more experienced workers tend to leave the plant, so that average experience again declines by 20 days. Average tenure, on the other hand, increase over the entire three year period before closure. However, while average tenure grows by around 110 days from quarter 12 to quarter 4 before closure, the increase in tenure almost vanishes to only 6 days during the last year before closure. Recall that tenure refers to the level three years before closure and that newly hired worked as well as workers with less than one year of tenure are not included in this average tenure measure. Hence, the initial increase in average tenure is not surprising as a correlation between the probability of leaving the firm and the tenure level is economically intuitive. Models including firm-specific human capital, heterogeneous job-matches or wage-seniority would imply such a correlation.[18] This makes it the more interesting to see that workers leaving shortly before plant closure are not characterized by below average tenure levels.

Average age is plotted in the lower left panel. It decreases slightly over the entire pre-closure period. No different pattern is apparent during the last year before closure. Hence the observed decrease in the average work experience and the flattening of the increase in tenure during this period is not a mere by-product of an age-effect in the sense that older workers are leaving in increasing numbers shortly before closure.

Descriptive statistics on daily earnings can be seen in the bottom right panel. Average daily earnings in euros at their level 3 years before closure are plotted against time relative to closure. Initially average earnings increase slightly by 30 cents from quarter -12 to quarter -4. Thereafter, up until closure, earnings drop by 80 cents, which roughly corresponds to a 2.5 per cent earnings drop. This indicates that early leavers are associated with higher average earnings compared to ultimately displaced workers.

[18] See Becker (1975), Jovanovic (1979) and Lazaer (1981) for examples of such models.

In sum, the key findings of these descriptive statistics are that separators leaving a dying establishment up to two quarters before closure are predominately men and blue collar workers. Moreover, they are associated with unconditional higher daily earnings.

To analyze the short-run effect of early separation we focus on the labor market status of separators in the first quarter after leaving the closing plant. As earnings data is available, we are able to evaluate a new employment relationship based on the associated daily wage. That is, we classify the new job relative to the previous job. In particular, we categorize the employment status in the first post closure quarter according to three different states: (i) not employed, (ii) employed with a lower wage, (iii) employed with a higher or equal wage.

One advantage of looking at the directions of separations is that it provides some evidence on the cause of separation, namely on whether the employment relationship ends because of a layoff or quit. Typically it is impossible to distinguish between these two causes in non-survey data. However, when observing individuals employed in a higher wage job immediately after separation it seems likely that these individuals quit their previous job. On the other hand, observing an individual accepting a lower wage or not being employed might indicate a layoff.

Figure 3 displays the percentage of workers ending up in either of the three states in the first quarter after separation by separation groups. First, notice that the distribution over the 3 outcomes varies quite a bit in the quarters -12 to -6 before closure with the results for quarter -10 being an outlier. However, as these separations occur at least one-and-a-half years before the closure of the plant, it is unlikely that a huge fraction of them is related to the closure event.

Starting from quarter -6 until quarter -1 a downward trend in the percentage of separators not employed immediately after separation becomes apparent. While 66 % of all separators leaving at quarter -6 end up not being employed in the next quarter, only 44 % of the separators leaving the distressed establishment in quarter -1 share the same fate. However, among those who stayed until the end 59% end up in non-employment in the first

quarter after plant closure.

Analogously, the fraction of separators immediately accepting a lower paid job increases until quarter -1 (up to 35 %) and then drops back for ultimately displaced workers (18 %). Interestingly, no such pattern exists for workers finding a higher paid job immediately.

This already provides some first evidence that in the short run early leavers perform better compared to ultimately displaced workers. To investigate this aspect further, we conduct a survival analysis. Figure 4 plots the Kaplan-Meier estimates for survival in non-employment after separation by quarter of separation relative to plant closure.[19] The graph reveals that while there appears to be no significant difference in terms of search time between ultimately displaced workers and early leavers leaving the closing plant in quarter -4 and -3, separators in quarters -2 and -1 find new employment more quickly. 75 % of early leavers leaving at -1 manage to find a new job within 2 quarters after separation and only 10 % of this group remain non-employed within the first 4 years after separation. In contrast, among the ultimately displaced workers around 30 % remain non-employed during the first 2 quarters and still roughly 15 % during the first 4 quarters after after closure.

To understand how overall employment probabilities change by quarter of separation relative to plant closure, figure 5 shows average employment by separator groups in the 16 quarters before and 20 quarters after separation. While prior to separation no significant differences exist, the employment probabilities of late early leavers (d=-1 and d=-2) dominate the respective probabilities of the other three groups in the first 20 quarters after separation.

Finally figure 6 provides unconditional evidence on the evolution of nominal log daily earnings, conditional on being employed, by separation groups. Obviously, changes in this measure may occur because of changes in real earnings, in inflation and because the set of employed workers may change. The evolution of earnings is qualitatively very similar for all separation groups.

[19] In light of the descriptive results presented in figure 2 and figure 3, which reveal especially interesting patterns in the last year before closure, we focus henceforth on separations happening during this period.

Over time, nominal daily earnings increase strongly, mainly reflecting growth in real earnings and inflation. Three aspects are particularly worth mentioning: firstly, at all quarters the level of earnings is the lowest for the group of ultimately displaced workers. However, the difference with any other group is always quite small, never exceeding more than .1 log points. Secondly, all groups have a spike in the evolution of wages directly after separation. This clearly reflects selectivity as the workers who are able to find a new job immediately are probably also the more productive ones. Thirdly, no higher earnings loss due to separation is obvious for ultimately displaced workers as opposed to early leavers conditional on being employed.

5 Estimation and Results

Borrowed from the evaluation literature, the seminal study of Jacobson et al. (1993) introduced the idea of studying the effects of displacement in a difference-in-difference setup. This way the effects of an involuntary job-loss are not identified by a simple pre/post comparison, but by the difference in differences when compared to pre/post outcomes of an adequate control group. The post outcome of the control group should conceptually serve as an estimate for the counterfactual outcome that would have occurred in the absence of displacement. To account for any remaining heterogeneity in the composition of the displaced and the non-displaced and to isolate the pure effect of displacement, individual fixed effects are included in the analysis to capture any time-invariant differences.

We go beyond this approach by employing an exact matching algorithm to further increase the comparability of treated and control subjects. Selection of a control group based on exact matching is feasible in this study given the enormous size of our data set. One advantage of exact matching is the creation of a common support for the treatment and control group. That is, we extract from the administrative records only those controls for a given treated, who have identical (or almost identical) characteristics. The characteristics with which we perform exact matching are gender, age, broad occupational status, industry and region of the employer. Moreover, we con-

duct almost exact matching based on quartile groups on continuous variables such as firm size and average daily earnings one year prior to displacement. Figure 7 visualizes how the matching algorithm works. Note two further points: (i) Besides being not employed in a closing plant, a valid control has to fulfill also the sample selection criteria described in section 3. (ii) The matching is performed at the last quarter the treated was observed being employed in the closing plant.

Identifying Early Leavers

Before turning to the estimation of displacement effects, we exploit this setup by comparing post-separation outcomes of early separators from closing plants with those of separators from surviving plants. This provides a test for the validity of including early leavers in the displacement group. The rationale behind this exercise is that if observed separations prior to the closure of a plant were due to "normal" labor turnover, which is not related to the upcoming plant closure, then post-separation outcomes should be indistinguishable from post-separation outcomes of separations happening in non-closure plants.

Equation 4 presents an empirical model to measure differences in post-separation outcomes between separators from closure and non-closure plants:

$$Y_{it} = K_{it}^{1,20} \tilde{D}_i^d \delta^d + K_{it}^{1,20} \kappa + \alpha_i + \theta_t + \epsilon_{it}. \tag{4}$$

Y_{it} represents the outcome variable of interest, α_i is an individual-specific fixed effect, θ_t captures the effect of calendar time and ϵ_{it} is an error term uncorrelated with all variables appearing on the right side of the equation. $K_{it}^{1,20}$ indicates the period relative to separation. For simplicity we don't estimate a single parameter for each quarter k relative to closure, but rather restrict our attention to the average effect over the first 5 years after separation. The dummy variable $K_{it}^{1,20}$ takes the value one if the separation happened up to 20 quarters before ($0 < k \leq 20$) and zero otherwise.

Separators from closing plants are identified by a dummy variable $\tilde{D}_i^d$. The dummy $\tilde{D}_i^d$ takes the value one if individual i separated from a closing

plant. The superscript d indicates the quarter of separation relative to the closure of the plant. We estimate equation (4) separately for all separations happening up to 4 quarters before the plant is last observed in the data. That is, separately for d= -1, -2, -3 and -4.

The control groups are selected based on the matching algorithm presented in figure 7. For each separator from a closing plant only separators from non-closing plants with almost identical characteristics are selected as controls.[20] The quality of this matching procedure is shown in table 3. For all 4 pairs of treatment and control groups mean differences in observed characteristics are extremely small. Only tenure and plantsize show somewhat larger differences. However, a difference in average tenure of up to 200 days is still relatively small compared to overall average values of around 2500 days and standard deviations of around 1700. While tenure is not a matching variable, treated and controls have been matched based on quartiles of the plantsize distribution. Yet, for all groups the average plantsize is consistently higher for separators from closing plants as compared to separators from non-closing plants. However, should the on average larger plantsize in the control groups significantly worsen the comparability between separators from closing plants and separators from non-closing plants, then at least it seems plausible to assume that this would influence results for all 4 groups more or less equally. Moreover, equation (4) includes additionally individual fixed effects to capture any remaining time-invariant differences between separators from closing plants and separators from non-closing plants.

Table 4 presents the results from estimating equation (4). The upper panel of table 4 shows estimation results with an employment dummy as dependent variable, while the lower panel shows estimation results with log daily earnings conditional on employment as the outcome variable. Controlling for individual fixed effects and calendar time effects, row 2 of table 4 reveals negative separation effects in terms of employment probabilities for

[20]If downsizing occurs also in non-closing plants and selective labor turnover matters for the survival probability of a plant, then including these separators in the control group might bias the results. However, our main results of this exercise remain qualitatively unchanged when restricting potential controls to include only separators from non-closing plants with quarter-on-quarter employment reductions of no greater than 30%.

all four groups. Estimated separation effects range from -.37 to -.42 indicating a common loss in terms of employment probabilities in the first 5 years after separation of around 40 percentage points.

The estimated interaction effect $K^{1,20}\tilde{D}^d$ can be seen in row 1 of table 4. The results reveal a significant effect of separating from a closure plant that goes beyond the isolated effect of separation for early leavers separating in d equal to -1 or -2. While separators leaving closing plants 3 and 4 quarters before closure are indistinguishable from normal separations, the estimated coefficients indicate a reduced loss in terms of employment probabilities for early leavers separating 1 or 2 quarters before closure of 9.4 and 7.2 percentage points, respectively. In terms of daily earnings no significant differences between separators from closing and non-closing plants can be found.

The results of this exercise provide evidence that at least a high fraction of all separations happening during the closure process of a plant are directly related to the upcoming closure and, therefore, should be included in the treatment group in the analysis of displacement effects. Given the results presented above, we feel confident in including at least all separations happening up to two quarters before closure into the displacement group.

Displacement Costs and Time of Separation

We can now define more specifically a dummy variable identifying early leavers. Let EL_i take the value one if individual i is observed working in a closing plant in the two last quarters before the plant closes ($-2 \leq d < 0$, but who is not employed at the plant at the very last quarter ($d = 0$) the plant is observed in the data and takes the value zero otherwise. Analogously we (re-)define the dummy variable D_i to identify all workers separating due to a plant closure. This includes the above defined group of early leavers as well as ultimately displaced workers.

With this notation in mind we are now able to test Proposition 1. Equation (5) defines a model to measure the effects of displacement that allows for heterogeneous displacement effects:

$$Y_{it} = K_{it}^{1,20}\kappa + K_{it}^{1,20}D_i\delta + K_{it}^{1,20}D_iEL_i\gamma + K_{it}^{1,20}EL_i\xi + \alpha_i + \theta_t + \epsilon_{it}. \quad (5)$$

We again measure these effects separately for employment probabilities and earnings. Y_{it} denotes the outcome variable of interest. As before, α_i is an individual-specific fixed effect, θ_t captures the effect of calendar time, $K_{it}^{1,20}$ identifies the 5 years time period after separation and ϵ_{it} is an error term uncorrelated with all right-hand-side variables.

Equation (5) extends the model defined in equation (4) by the two interaction effects $K_{it}^{1,20}D_iEL_i$ and $K_{it}^{1,20}EL_i$. The latter effect is supposed to capture any systematic difference between early leavers and their matched controls that goes beyond the isolated effect of $K_{it}^{1,20}$. The coefficient γ, that is associated with the interaction effect $K_{it}^{1,20}D_iEL_i$, is our key parameter interest. It measures the additional effect of being an early leaver that goes beyond the common effect of displacement δ.

Note, there's another important difference in the estimation of equation (5) in comparison to equation (4). The control group consists now of any matched controls, who are employed at a non-closure plant at the last quarter the corresponding treated was last observed working for the closing plant. This does not restrict future employment patterns of the control group in any way. Neither does it restrict the control to separate within the next quarter as well (as it did in the comparison with normal turnover presented above), nor does it restrict a control to a continuously employed worker as is the case in many displacement studies.[21] In this study we take the point of view that an adequate control should not be restricted in any way to proxy for the counterfactual outcome in the absence of displacement. A control should be distinguishable from a treated only insofar that the control does not suffer from displacement due to a plant closure. A control might, however, lose the job due to other reasons.

As before, the selection of adequate controls is based on the exact matching algorithm presented in figure 7. Table 5 presents evidence on the quality of the matching. Again the matching procedure worked well. Differences in

[21] See for example Jacobson et al. (1993).

means between displaced and non-displaced workers are small, with somehow larger differences in tenure and plantsize.

Displaced early leavers are associated with on average 228 days of tenure less than their matched controls. This difference is, however, only about 0.13 standard deviations. Moreover, if tenure is associated with more stable employment and higher earnings, the worse matching for early leavers in terms of tenure should (if at all) downward-bias the effect of displacement for early leavers as opposed to ultimately displaced workers.

Moreover, displaced workers are on average employed in smaller plants compared to their matched control subjects. The matching procedure based on quartiles did not work too well here. However, one should keep in mind that our estimation strategy does not solely rely on selecting adequate controls based on matching certain characteristics, but additionally includes individual fixed effects to capture remaining time-invariant confounding factors. Hence, it seems very unlikely that this difference in average plantsize between early leavers and their matched controls could ultimately drive our results.

Table 5 also reveals differences between early leavers and ultimately displaced workers. As already seen in the descriptive statistics, the group of early leavers consists more of men and blue collar workers compared to the group of ultimately displaced workers. Moreover, while both groups of displaced workers were employed at the same closing plants, the early leavers stem to a significantly larger proportion from big establishments. However, part of the difference in plantsize is precisely due to the downsizing before closure as plantsize is measured at the moment of separation.

Table 6 presents the results of estimating equation (5). Column 1 shows estimated coefficients from a regression with an employment dummy as dependent variable. The estimate for δ, which can be seen in row 4, reveals that the overall effect of displacement in terms of employment probability is estimated to be -0.23, implying a reduction in post-displacement employment probability of 23 percentage points in the first 5 years after displacement. This effect goes beyond the pure time effect $K_{1,20}$ of -.07, which represents the dissolution of employment relationships present even in the absence of

displacement. While no systematic differences can be found between early leavers and their matched controls, the additional effect of leaving early is estimated to be highly significant at around 0.07. This implies that early leavers face a 7 percentage points higher employment probability as opposed to ultimately displaced workers.

Column 2 presents analogous difference-in-difference estimation results with log daily earnings (conditional on being employed) as dependent variable. Focusing on the key parameters of interest in column 2, we find a common earnings loss due to displacement of 6 percent, but a significant 1.2 percentage points lower loss for early leavers. Column 3 shows earnings results with a less restrictive sample selection. Similar to the selection criteria applied in other studies, we assign zero earnings for individuals not employed in a given quarter, but include only observations with positive earnings within a calendar year. Hence, this "unconditional" earnings measure captures also earnings losses through short-term non-employment, which increases the estimated common loss of displacement to 61 percent. The loss for early leavers is now 14 percentage points lower.

In sum, table 2 reveals that the cost of displacement is significantly lower for early leavers compared to ultimately displaced workers. The difference in displacement effects might be explained by compositional differences between these two groups. Section 4 already provided descriptive evidence on differences in average workers characteristics, which fosters the conjecture that a selection process has set in during the closure procedure. Moreover, previous studies have also found evidence for the presence of selection in the labor turnover process before plant closure.[22]

However, workers and management have a competing agenda. Highly productive workers might leave a distressed plant to avoid ultimate displacement, whereas low productivity workers might be the first to be laid-off when a negative demand shock makes downsizing necessary. Hence, it remains an empirical challenge to answer how average productivity varies between early leavers and ultimately displaced workers.

[22]See Lengermann and Vilhuber (2002) and Pfann and Hamermesh (2001).

Pre-Closure Earnings and Time of Separation

To test proposition 2 we estimate a model of pre-separation daily earnings. However, note two important caveats of interpreting observed earnings differentials as differences in productivity: First, the use of earnings as a measure of worker productivity is based on the underlying assumption that wages are equal to the marginal products of labor. Various characteristics of actual labor markets, such as discrimination, union bargaining, signalling and mismatch, may result in violations of this assumption. Secondly, our measure of daily earnings does not reflect differences in labor input in terms of hours worked. Nevertheless, earnings remain the best available proxy for a worker's productivity given the data in hand.

Equation (6) presents a model of pre-separation earnings,

$$ln(w_{it}) = EL_i\lambda + X'_{it}\beta + \theta_t + \epsilon_{it}, \tag{6}$$

where the dependent variable $ln(w_{it})$ represents log daily earnings, EL_i takes the value one if individual i is an early leaver and takes the value zero if individual i is an ultimately displaced worker, X_{it} a set of control variables, θ_t captures the effect of calendar time and ϵ_{it} is an error term uncorrelated with all right-hand-side variables.

Table 7 presents the results of estimating equation (6). All regressions control for calendar time effects as well as for relative distance to the closure of the plant. The latter variable is an important control as earnings might be contaminated due to the economic ill-being of the employer. Column 1 represents the results from regressing log daily earnings on the early leaver dummy. The estimated coefficient λ is negative at -0.05 and highly significant. This unconditional evidence suggests that early leavers are associated with 5% lower daily earnings in the 17 quarters before displacement. Including personal characteristics such as age, gender, broad occupation and tenure pushes up the estimate to 0.72.[23]

Including plant characteristics such as plantsize, industry and location of

[23]The estimated earnings differential between men and women of -0.56 in column 2 most likely reflects the typical higher share of part-time work among women.

the plant drives down the estimated coefficient for early leavers again, as can be seen in column 3. However, the estimate remains significant and positive at 0.41. Finally, estimating a Tobit specification accounting for top-coding in the earnings data does not change the results significantly.

In sum, all specifications reveal significantly higher pre-closure earnings levels for early leavers. We take this as evidence that proposition 2 is correct.

Displacement Costs and Pre-Closure Earnings

To understand how higher average pre-closure earnings affects displacement effects, we estimate a displacement effect model allowing for heterogeneous displacement effects along the pre-separation earnings distribution. For simplicity we focus on quartile groups. That is, we allow for different displacement effects for each quartile. The model is specified as follows:

$$Y_{it} = K_{it}^{1,20}\kappa + K_{it}^{1,20}D_i\delta + \sum_{q=1}^{3} K_{it}^{1,20}D_i Qrt(q)_i\phi^q + \alpha_i + \theta_t + \epsilon_{it}. \qquad (7)$$

Equation (7) extends the model defined in equation (5) by the term $\sum_{q=1}^{3} K_{it}^{1,20}D_i Qrt(q)_i\phi^q$, where $Qrt(q)_i$ is a dummy variable taking the value one if individual i belongs to the q^{th} quartile of the pre-separation earnings distribution and the parameter ϕ^q measures the additional displacement effect for individuals belonging to the q^{th} quartile relative to the baseline effect of the omitted category represented by the 4^{th} quartile of the pre-separation earnings distribution.

To understand how these distributional differences affect the estimation results of equation (7), we estimate equation (7) separately for early leavers, ultimately displaced workers and the two groups jointly. Table 8 presents the estimation results separately for employment (column 1-3) and earnings conditional on being employed (columns 4-6).

In terms of employment probabilities, significant losses exist for the baseline category of workers belonging to the highest quartile of the pre-separation earnings distribution. This can be seen in row 1. The estimates ran0ge from

-.11 for the early leavers sample up to -.19 for the ultimately displaced workers sample. Based on the combined sample, high earnings workers are estimated to face a reduction of 16 percentage points in their post-separation employment probabilities. The interaction terms reveal that displacement costs are significantly higher in terms of future employment for low earnings workers. The workers belonging to the lowest quartile of the pre-separation earnings distribution suffer the most. They face an additional reduction in employment probabilities of 11 percentage points. Workers in the second quartile also endure an additional loss of 5 percentage points compared to high earnings workers. Workers in the third quartile suffer no significant additional loss. Note that the pattern of displacement effects between quartiles is very similar when estimating equation 7 based on the early leavers and the ultimately displaced workers sub-samples separately.

While these results clearly suggest that above median earnings workers suffer significantly less in terms of future employment, the results on earnings in column 4 to 6 show a reversed pattern. Here, it appears that high earnings workers lose the most as can be seen from the positive and significant coefficients in column 4. This pattern is also confirmed in the sub-sample regressions shown in column 3 and 6.

However, while these results seem striking, they have to be interpreted carefully. Recall that the estimations on log daily earnings only include observations with positive earnings. Hence, only those separators that successfully found new employment after their separation are included. This, however, leads to compositional differences within groups. In particular, these results seem to suggest that those workers who are successful in finding a new job are also the more productive workers. As low pre-separation-earnings workers are associated with significantly lower employment probabilities as opposed to high pre-separation-earnings workers, the results are not clear-cut as differences in post-separation earnings might be entirely driven by selection within these groups. Moreover, the Austrian labor market is highly regulated and wage setting is not at all flexible. This is particularly true at the lower end of the wage distribution, where generous social security regulations implicitly constitute minimum wages. Hence, it is not surprising that conditional on

re-employment earnings losses basically don't occur at the lower end of the pre-closure earnings distribution.

Results in column 7 to 9 report earnings losses on an "unconditional" earnings measure. As this measure captures earnings losses caused by short-term non-employment, most interaction effects with quartile groups become smaller and insignificant.[24] This reveals that results on earnings losses caused by a job loss strongly depend on the underlying earnings measure.

Regarding proposition 3, we therefore conclude, that - while no conclusive evidence based on earnings exists - displacement costs in terms of employment probabilities vary clearly with the level of pre-closure earnings. In particular, the findings suggest that workers with above median pre-closure earnings are associated with significantly lower losses in terms of future employment probabilities as opposed to below median workers.

6 Conclusion

In this paper our first task was to analyze job separations happening before plant closure. We find that early leavers separating up to two quarters before plant closure, are associated with significantly better post-separation labor market outcomes as opposed to separators from non-closing plants. Earlier separations from closing plants are, however, indistinguishable from normal turnover. This finding is particularly important for the economic literature that utilizes plant closures to identify involuntary and exogenous job losses in administrative data. As plant closures usually do not happen without prior notice, management and workers adjust their expectations about the value of a given employment relationship in response to the arrival of such information. Hence a negative shock that ultimately leads to closure might cause separations from dying plants even before the ultimate shutdown. While the empirical literature has acknowledged this by focusing on all separations within a certain time window prior to plant closure, the choice of that window often appears to be quite arbitrary. Facing the tradeoff be-

[24] With the exception of the interaction effects of the second quartile in the early leavers sample and the third quartile in the joint sample.

tween neglecting early leavers and including a significant amount of normal workforce turnover, the comparison with separators from surviving plants in terms of post-separation outcomes provides a good guideline for choosing a particular time window. Our results suggest that at least all separations up to 2 quarters before closure should be included in the treatment group of displaced workers.

Given this identification of early leavers, we tested three propositions related to the selection hypothesis in the labor turnover process before plant closure. A clear picture emerged: early leavers suffer significantly less from separating from a closing plant compared to ultimately displaced workers. They suffer less especially in terms of future employment probabilities. Moreover, early leavers are associated with significantly higher pre-closure earnings levels conditional on several individual and firm characteristics. Finally, displacement costs (in terms of future employment probabilities) are significantly lower for workers with higher pre-closure earnings.

These findings are in line with the hypothesis that prior knowledge about the upcoming plant closure induces both management and workers to react in terms of their firing and quitting decisions. As a consequence, selection based on workers' characteristics occurs: firms laying off less productive workers, while workers with better outside options quit. However, on average early leavers appear to be more productive as suggested by higher average pre-closure earnings. As displacement costs in terms of future employment are lower for high-earnings workers, the observed difference in displacement effects between early leavers and ultimately displaced workers could be explained by compositional differences between these groups that result from a selection in the turnover process before plant closure.

We believe that these results are relevant for the literature on worker displacement. A key implication of these findings is that any study utilizing plant closures as a quasi-experiment is well advised to include also separations occurring before the ultimate shutdown. We propose a procedure to identify early leavers based on a comparison with normal turnover that goes beyond the standard, ad-hoc method of including all separations happening within a certain time-window before closure. According to our results, focusing solely

on ultimately displaced workers would lead to serious overestimation of the cost of displacement as it appears that those who left before the closure are on average the more productive workers. While this study takes the plant closure as a given, this result also raises the question of causality and emphasizes the importance of a better understanding of the causal link between worker flows and the closure of firms.

References

Abowd, J. M., Kramarz, F., and Margolis, D. N. (1999). High wage workers and high wage firms. *Econometrica*, 67(2):251–333.

Abowd, J. M., McKinney, K. L., and Vilhuber, L. (2005). The link between human capital, mass layoffs, and firm deaths. mimeo, U.S. Census Bureau, LEHD and Cornell University.

Addison, J. and Portugal, P. (1987). The effect of advance notification of plant closings on unemployment. *Industrial and Labor Relations Review*, 41(1).

Altonji, J. G. and Pierret, C. (2001). Employer learning and statistical discrimination. *Quarterly Journal of Economics*, 116:313–350.

Becker, G. S. (1975). *Human Capital*. New York: Columbia University Press, 2nd edition.

Bender, S., Dustmann, C., Margolis, D., and Meghir, C. (2002). Worker displacement in france and germany. In Kuhn, P. J., editor, *Losing work, moving on : international perspectives on worker displacement*, pages 375–470. Kalamazoo: Upjohn Institute for Employment Research.

Bowlus, A. and Vilhuber, L. (2002). Displaced workers, early leavers, and re-employment wages. Technical paper TP-2002-18, LEHD, U.S. Census Bureau.

Carneiro, A. and Portugal, P. (2003). Wages and the risk of displacement. CETE Discussion Papers 0308.

Eliason, M. and Storrie, D. (2004). The echo of job displacement. Working Papers in Economics 135, Göteborg University.

Fallick, B. (1996). A review of the recent empirical literature on displaced workers. *Industrial and Labor Relations Review*, 50:5–16.

Farber, H. S. (1999). Mobility and stability: The dynamics of job change in labor markets. In Ashenfelter, O. and Card, D., editors, *The Handbook of Labor Economics*, volume 3. Elsevier Science.

Farber, H. S. and Gibbons, R. (1996). Learning and wage dynamics. *Quarterly Journal of Economics*, 111:1007–1048.

Felli, L. and Harris, C. (1996). Learning, wage dynamics and firm-specific human capital. *Journal of Political Economy*, 104:838–868.

Friesen, J. (1997). Mandatory notice and the jobless durations of displaced workers. *Industrial and Labour Relations Review*, 50(4):652–666.

Gibbons, R. S. and Katz, L. F. (1991). Layoffs and lemons. *Journal of Labor Economics*, 9(4):351–380.

Hofer, H. and Winter-Ebmer, R. (2003). Longitudinal data from social security records in austria. *Schmollers Jahrbuch*, 123:587–591.

Ichino, A., Schwerdt, G., Winter-Ebmer, R., and Zweimüller, J. (2007). Too old to work, too young to retire? CESifo Working Paper Series 2118.

Jacobson, L. S., LaLonde, R. J., and Sullivan, D. G. (1993). Earnings losses of displaced workers. *American Economic Review*, 83(4):685–709.

Jones, S. and Kuhn, P. (1995). Mandatory notice and unemployment. *Journal of Labor Economics*, 13(4):599–622.

Jovanovic, B. (1979). Job matching and the theory of turnover. *Journal of Political Economy*, 87(5):972–90.

Kletzer, L. (1998). Job displacement. *Journal of Economic Perspectives*, 12(1):115–136.

Lazaer, E. (1981). Agency, earnings profiles, productivity, and hours restrictions. *American Economic Review*, 71(4):606–20.

Lengermann, P. A. and Vilhuber, L. (2002). Abandoning the sinking ship: The composition of worker flows prior to displacement. Technical paper TP-2002-11, LEHD, U.S. Census Bureau.

OECD (1993). *Employment Outlook*. Paris.

Pfann, G. A. and Hamermesh, D. (2001). Two-sided learning, labor turnover and worker displacement. IZA Discussion Papers 308.

Ruhm, C. (1994). Advance notice, job search, and postdisplacement earnings. *Journal of Labor Economics*, 12(1):1–28.

Appendix

Table 1: Plant closures per quarter between 1983 and 1988

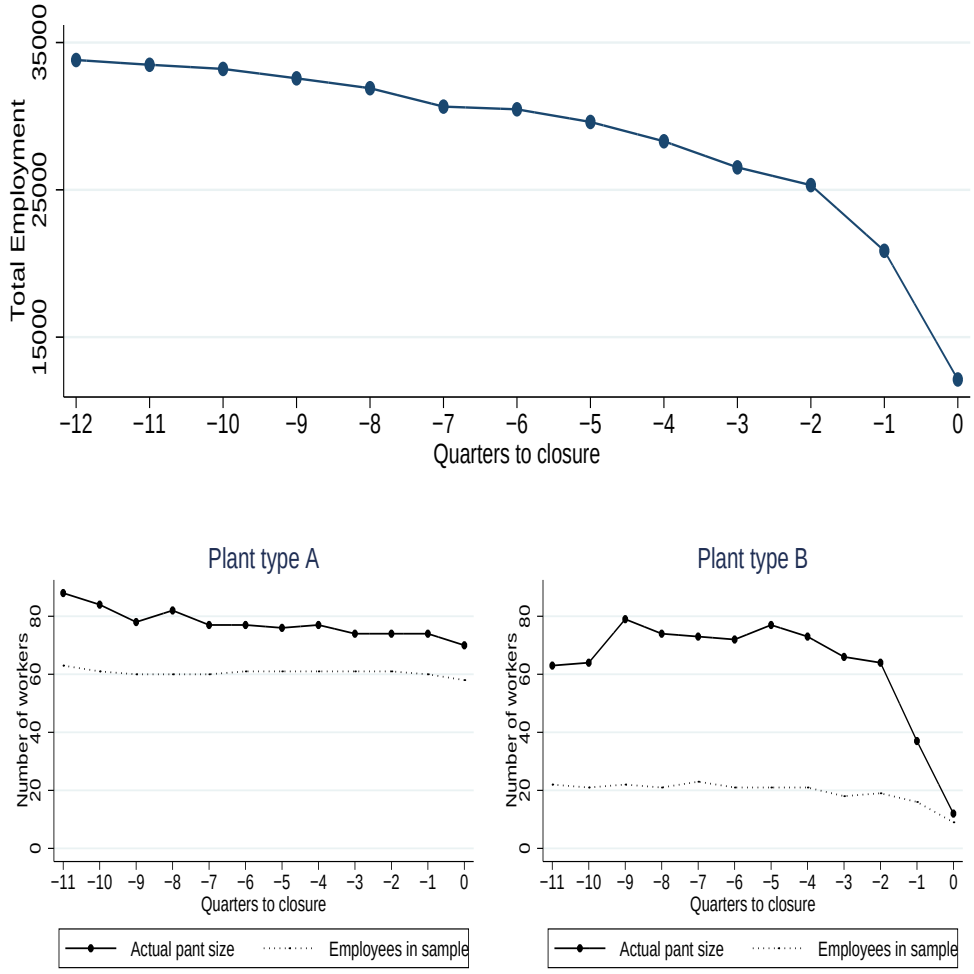
Year of	Quarter of plant closure				
plant closure	1	2	3	4	Total
1983	167	184	151	243	745
1984	174	188	145	224	731
1985	151	184	182	246	763
1986	199	185	178	251	813
1987	176	197	176	294	843
1988	175	182	166	285	808
Total	1,042	1,120	998	1,543	4,703

Table 2: Plant closures by federal state and year

Region	Year of plant closure						
	1983	1984	1985	1986	1987	1988	Total
Wien	250	236	253	258	276	255	1,528
Niederoestereich	144	125	146	119	155	152	841
Burgenland	21	14	14	17	19	19	104
Oberoesterreich	98	92	83	117	102	108	600
Steiermark	60	79	81	81	76	70	447
Kaernten	41	39	32	53	48	38	251
Salzburg	43	53	55	52	62	55	320
Tirol	56	54	65	71	55	57	358
Vorarlberg	29	23	26	37	30	35	180
Total	742	715	755	805	823	789	4,629

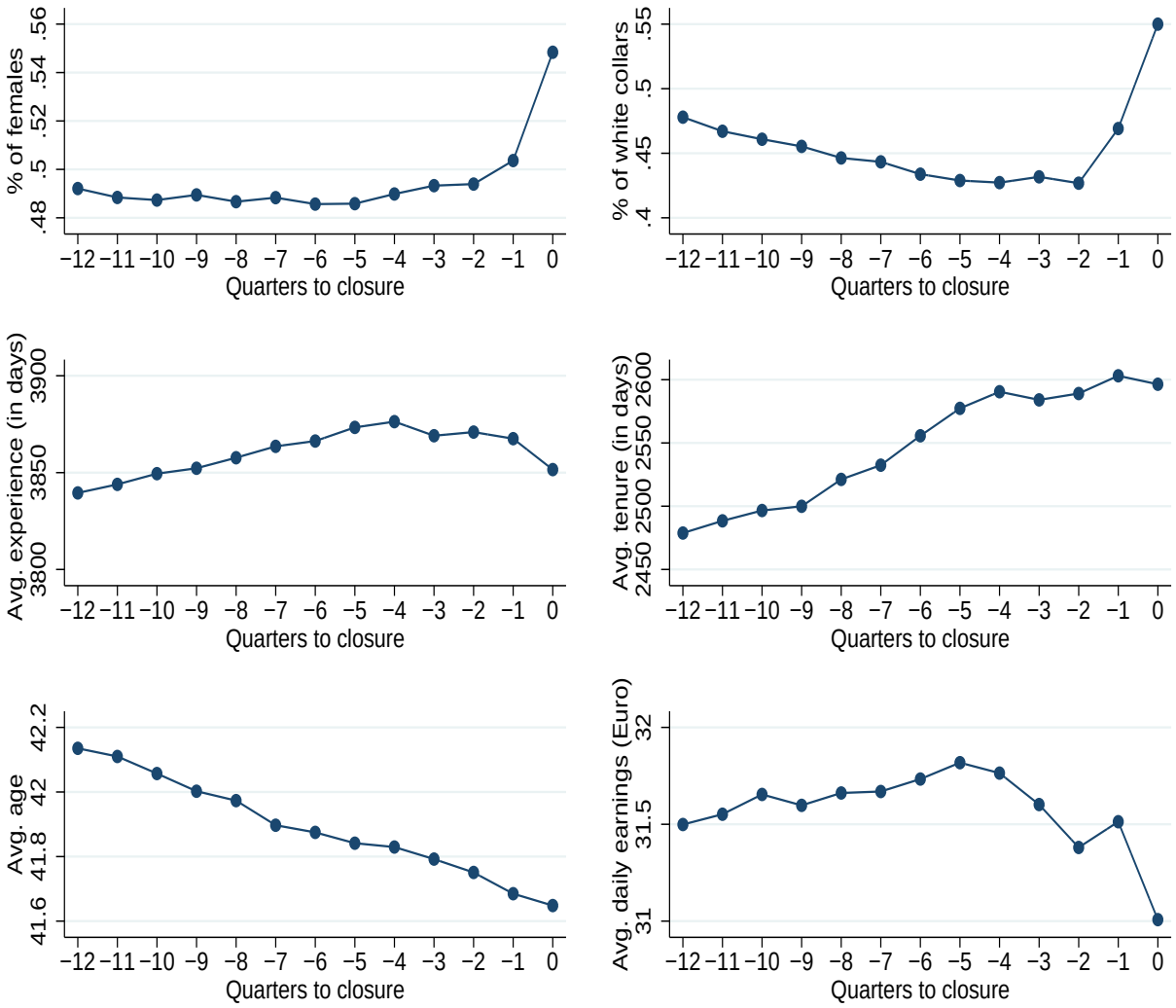
Note: For 74 *establishments no information on the location is available.*

Figure 1: Employment changes in closing plants



Note: The upper panel shows total employment in all plants closing between 1985 and 1988 relative to closure. The lower panel shows employment and the number of employees fulfilling the selection criteria before closure in two representative plants.

Figure 2: Evolution of Average Workforce Characteristics in Closing Plants before Closure



Note: All variables refer to their respective level 12 quarters before closure.

Figure 3: Employment Status in the 1st quarter after separation by quarter relative to plant closure

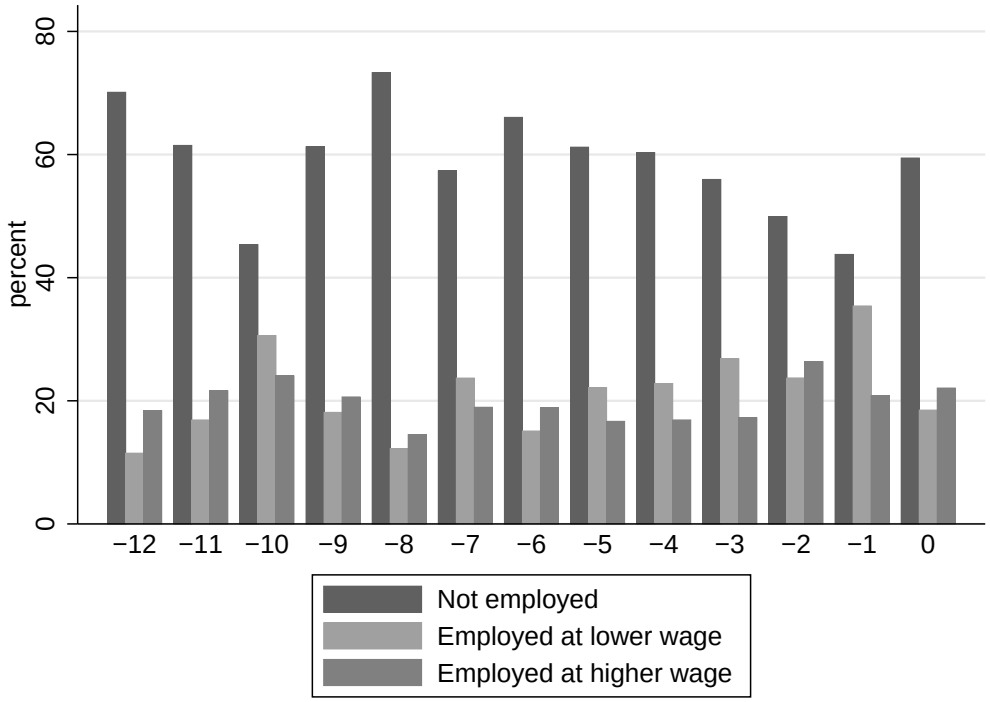


Figure 4: Kaplan-Meier estimates for survival in non-employment after separation by separation-quarter relative to plant closure.

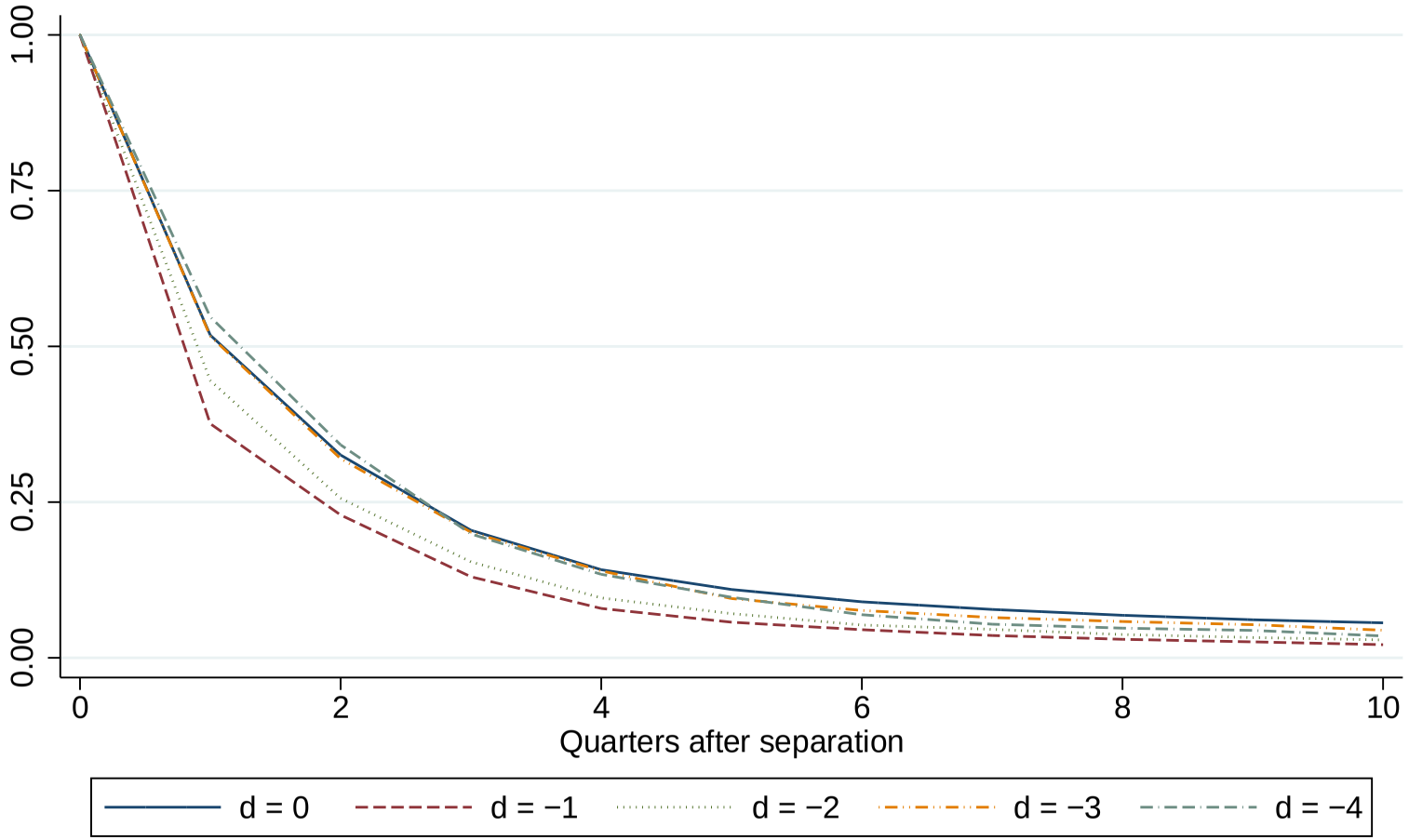


Figure 5: Average employment by separator group

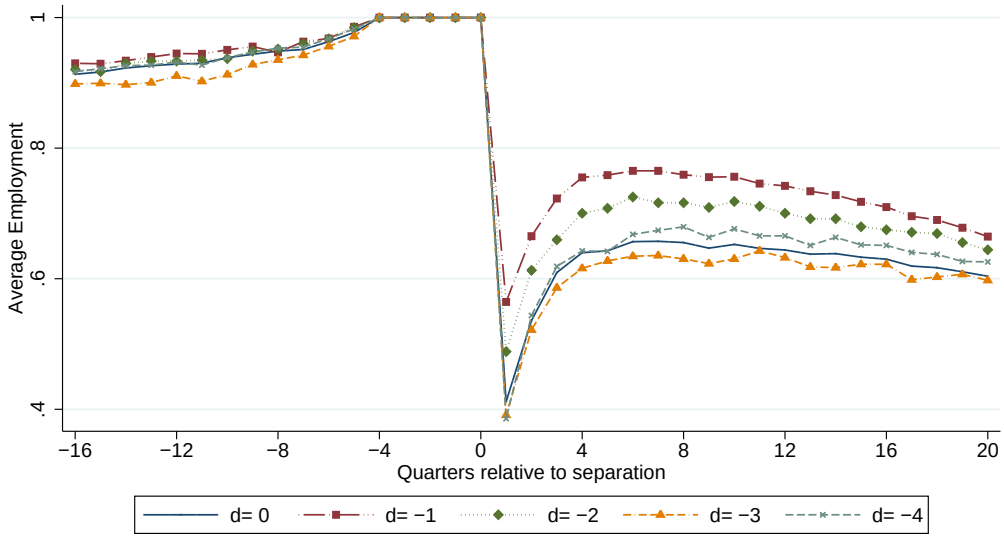


Figure 6: Average log daily earnings by separator group conditional on employment

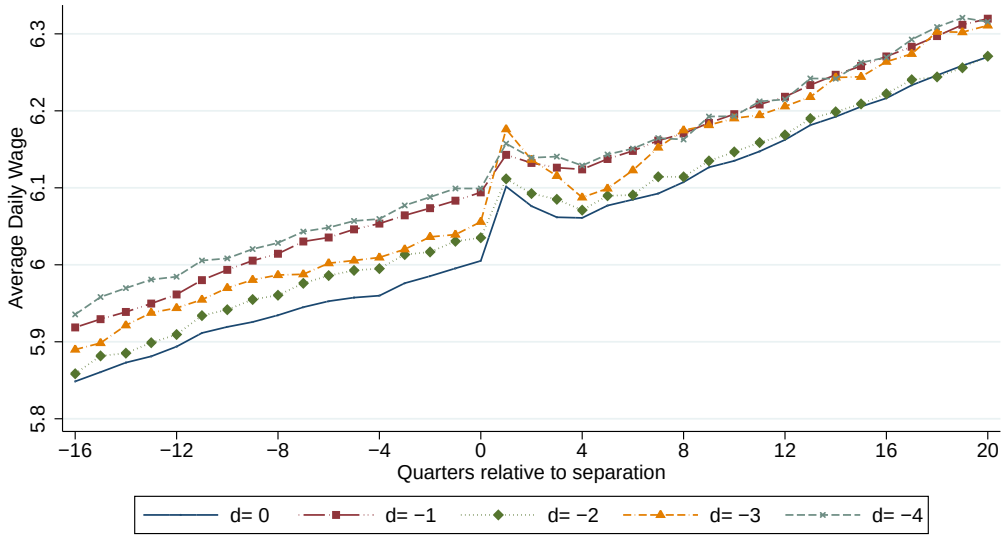


Figure 7: The Matching Algorithm

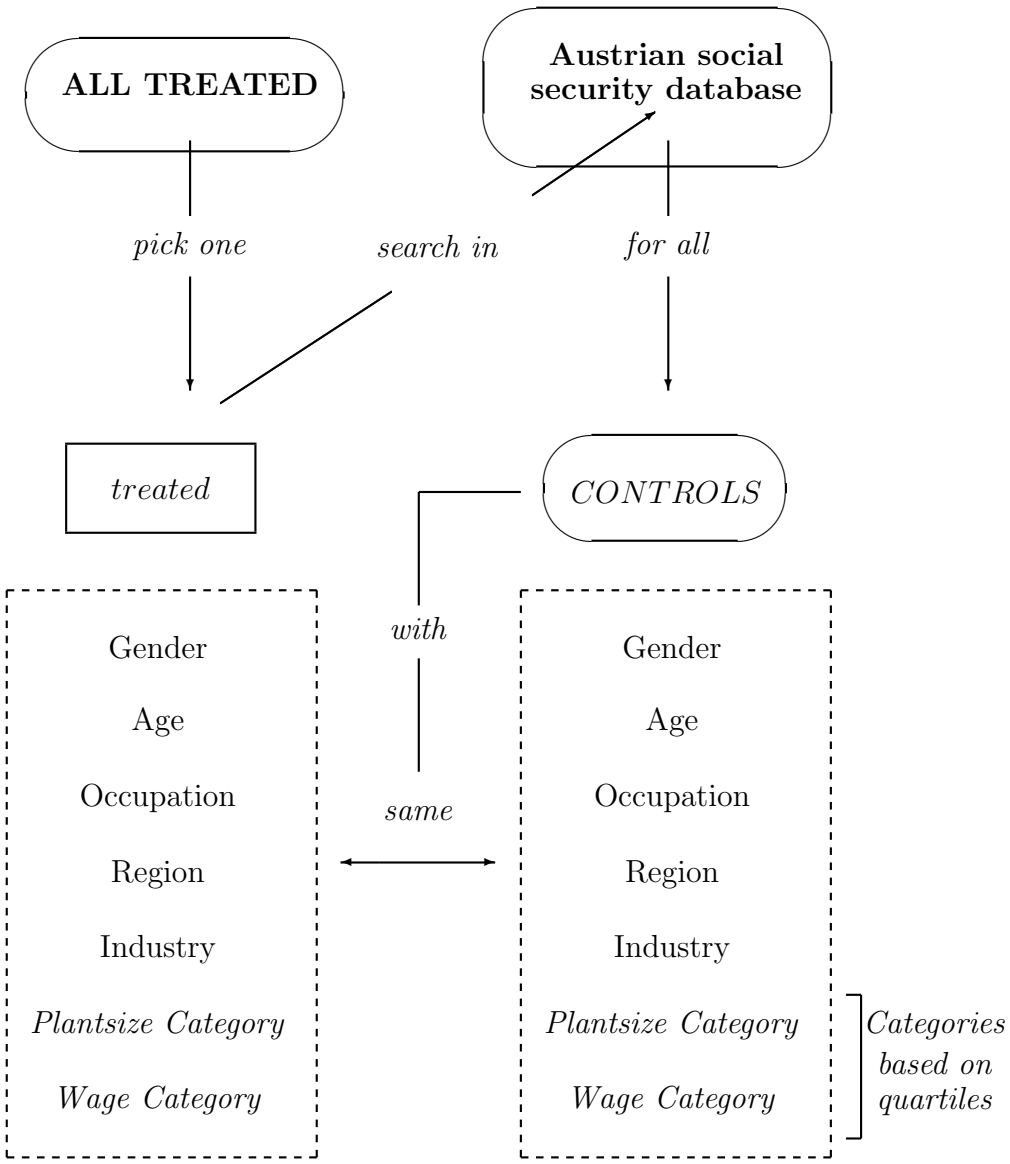


Table 3: Matching quality 1: Weighted averages for separators from closing and non-closing plants by distance to closure

	$d=-1$		$d=-2$		$d=-3$		$d=-4$	
Separation from	PC	NPC	PC	NPC	PC	NPC	PC	NPC
Female	.39	.39	.41	.41	.42	.42	.37	.37
Blue Collar	.57	.57	.7	.7	.41	.41	.43	.43
Age (years)	43	43	44	44	44	44	43	43
Tenure (days)	2780	2459	2755	2465	2354	2530	2654	2452
Experience (days)	4532	4337	4371	4230	4233	4202	4168	4124
Daily Earnings (euro)	37	36	33	33	38	37	39	38
Plantsize	64	143	110	160	56	94	103	150

Note: Sample averages of pre-separation characteristics for separations from closing (PC) and non-closing (NPC) plants and by distance to closure (d) in quarters. All variables are measured at the quarter immediately before separation. Earnings are are in nominal terms.

Table 4: Comparison with "normal" Turnover

	Employment			
	$d=-1$	$d=-2$	$d=-3$	$d=-4$
$K^{1,20}\tilde{D}^d$	.094	.072	.048	.014
	(.017)**	(.022)**	(.031)	(.03)
$K^{1,20}$	-.38	-.403	-.418	-.374
	(.013)**	(.017)**	(.024)**	(.023)**
Time dummies	yes	yes	yes	yes
Fixed effects	yes	yes	yes	yes
Const.	.953	.897	.928	1.009
	(.025)**	(.029)**	(.14)**	(.064)**
Obs.	75237	48763	24924	25296
R^2	.481	.498	.511	.515
F statistic	103.694	78.143	36.133	39.15
	Daily Earnings			
	$d=-1$	$d=-2$	$d=-3$	$d=-4$
$K^{1,20}\tilde{D}^d$	-.001	.017	.008	-.014
	(.012)	(.017)	(.024)	(.022)
$K^{1,20}$	-.031	-.067	-.051	-.056
	(.01)**	(.013)**	(.02)**	(.018)**
Time dummies	yes	yes	yes	yes
Fixed effects	yes	yes	yes	yes
Const.	5.863	5.827	5.73	5.922
	(.015)**	(.016)**	(.065)**	(.04)**
Obs.	55377	34117	18153	18347
R^2	.896	.896	.889	.901
F statistic	126.435	80.265	40.429	56.463

Note: Dependent variable is employment in the top panel and log daily earnings conditional on employment in the bottom panel. $K^{1,20}$ is an identifier for the first 20 quarters after separation and $\tilde{D}^d$ is a dummy that identifies separations from a closing plant. Regressions are run separately for different groups of separations distinguished by the relative distance to plant closure (d). All regressions control for individual fixed effects and for calendar time effects. Robust standard errors in parentheses.

Table 5: Matching quality 2: Weighted averages by displacement status and distance to closure

	ultimately displaced		*early leavers*	
	displ	non-displ	displ	non-displ
Female	.53	.53	.4	.4
Blue Collar	.4	.4	.68	.68
Age (years)	44	44	44	44
	(5.7)	(5.7)	(5.8)	(5.8)
Tenure (days)	2916	2900	2876	3104
	(1755)	(1701)	(1726)	(1671)
Experience (days)	4420	4402	4423	4430
	(1178)	(1180)	(1065)	(1070)
Daily Earnings (euro)	34	34	35	35
	(16)	(16)	(14)	(14)
Plantsize	21	37	116	200
	(42)	(98)	(148)	(260)

Note: Sample averages of pre-separation characteristics, by displacement status and by distance to closure. All variables are measured at the quarter immediately before separation. Earnings are in nominal terms. Standard deviations in parentheses.

Table 6: Displacement Effects

	Employment	**Daily Earnings**	
		conditional on employment	unconditional
	(1)	(2)	(3)
$K^{1,20}*D*EL$	.071	.012	.139
	(.006)**	(.005)*	(.019)**
$K^{1,20}*D$	-.228	-.06	-.614
	(.004)**	(.004)**	(.013)**
$K^{1,20}*EL$	-.001	-.015	-.00007
	(.002)	(.001)**	(.006)
$K^{1,20}$	-.068	.012	.792
	(.002)**	(.001)**	(.008)**
Time dummies	yes	yes	yes
Fixed effects	yes	yes	yes
Const.	.984	5.73	6.302
	(.022)**	(.013)**	(.052)**
Obs.	6540163	5740536	6124850
R^2	.459	.914	.177
F statistic	1107.077	4678.701	2535.624

Note: Dependent variable is an employment dummy in column 1 and log daily earnings in columns 2 and 3. Results in column 2 are based on observations with positive earnings within a quarter, while in column 3 all observations with positive earnings within a calendar year are included. EL is a dummy variable identifying early leavers, $K^{1,20}$ is an identifier for the first 20 quarters after separation and D is a displacement dummy. All regressions control for calendar time and individual fixed effects. Robust standard errors in parentheses.

Table 7: Pre Closure Earnings

	OLS	OLS	OLS	Tobit
	(1)	(2)	(3)	(4)
EL	.049	.072	.041	.042
	(.007)**	(.005)**	(.005)**	(.001)**
Age		-.0002	-.0008	-.0007
		(.0005)	(.0004)*	(.0001)**
Female dummy		-.561	-.5	-.517
		(.006)**	(.006)**	(.002)**
Tenure		.00004	.00004	.00004
		(1.62e-06)**	(1.56e-06)**	(4.48e-07)**
White collar		.326	.348	.373
		(.006)**	(.006)**	(.002)**
Plant size			.00004	.00004
			(1.00e-05)**	(1.92e-06)**
Industry dummies	no	no	yes	yes
Location dummies	no	no	yes	yes
Distance to closure	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Const.	5.488	5.589	5.579	5.603
	(.051)**	(.042)**	(.056)**	(.042)**
Obs.	264881	263436	263436	263436
R^2	.033	.482	.533	
F statistic	98.685	418.997	312.409	

Note: Dependent variable is always log daily earnings. EL is a dummy variable identifying early leavers. All regressions control for calendar time effects as well as for the relative distance to the ultimate closure of the plant. Robust standard errors in parentheses.

Table 8: Displacement effects by quartile of pre-closure earnings distribution

	Employment			**Daily Earnings**					
				conditional on employment			unconditional		
	ALL	UD	EL	ALL	UD	EL	ALL	PC	EL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$K^{1,20}*D$	-.158	-.189	-.111	-.155	-.176	-.126	-.553	-.633	-.437
	(.006)**	(.008)**	(.009)**	(.004)**	(.006)**	(.006)**	(.018)**	(.024)**	(.026)**
$QRT(1)*K^{1,20}*D$	-.113	-.105	-.119	.273	.308	.217	-.013	.024	-.056
	(.009)**	(.011)**	(.014)**	(.008)**	(.01)**	(.011)**	(.027)	(.035)	(.041)
$QRT(2)*K^{1,20}*D$	-.05	-.044	-.071	.103	.115	.085	-.048	-.013	-.119
	(.008)**	(.012)**	(.012)**	(.006)**	(.009)**	(.008)**	(.025)	(.035)	(.036)**
$QRT(3)*K^{1,20}*D$	.009	.006	-.0003	.055	.055	.049	.058	.063	.02
	(.008)	(.011)	(.012)	(.006)**	(.008)**	(.007)**	(.024)*	(.034)	(.034)
$K^{1,20}$	-.07	-.081	-.055	.007	.008	.004	.791	.738	.863
	(.002)**	(.002)**	(.003)**	(.001)**	(.002)**	(.001)*	(.007)**	(.01)**	(.011)**
Time dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes
Fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Const.	.982	.963	1	5.739	5.716	6.506	6.313	6.244	6.195
	(.022)**	(.022)**	(.007)**	(.013)**	(.013)**	(.004)**	(.052)**	(.052)**	(.02)**
Obs.	6540163	3543734	2996429	5740536	3107894	2632642	6124850	3316486	2808364
R^2	.46	.473	.44	.918	.915	.923	.177	.184	.168
F statistic	1050.498	636.304	460.768	4160.534	2060.109	2604.618	2380.725	1235.344	1270.239

Note: Dependent variable is an employment dummy in columns 1 to 3 and log daily earnings in columns 4 to 6. $K^{1,20}$ is an identifier for the first 20 quarters after separation, D is a displacement dummy and QRT is an identifier for the quartile of the pre-separation distribution, where the number in brackets refers to the respective quartile. Regressions are based either on the full sample (ALL), on a sub-sample including ultimately displaced workers and matched controls (UD) or on a sub-sample including early leavers and matched controls (EL). All regressions control for individual fixed effects and calendar time effects. Robust standard errors in parentheses.

CESifo Working Paper Series

for full list see www.cesifo-group.org/wp
(address: Poschingerstr. 5, 81679 Munich, Germany, office@cesifo.de)

2188 Sándor Csengődi and Dieter M. Urban, Foreign Takeovers and Wage Dispersion in Hungary, January 2008

2189 Joerg Baten and Andreas Böhm, Trends of Children's Height and Parental Unemployment: A Large-Scale Anthropometric Study on Eastern Germany, 1994 – 2006, January 2008

2190 Chris van Klaveren, Bernard van Praag and Henriette Maassen van den Brink, A Public Good Version of the Collective Household Model: An Empirical Approach with an Application to British Household Data, January 2008

2191 Harry Garretsen and Jolanda Peeters, FDI and the Relevance of Spatial Linkages: Do third Country Effects Matter for Dutch FDI?, January 2008

2192 Jan Bouckaert, Hans Degryse and Theon van Dijk, Price Discrimination Bans on Dominant Firms, January 2008

2193 M. Hashem Pesaran, L. Vanessa Smith and Takashi Yamagata, Panel Unit Root Tests in the Presence of a Multifactor Error Structure, January 2008

2194 Tomer Blumkin, Bradley J. Ruffle and Yosef Ganun, Are Income and Consumption Taxes ever really Equivalent? Evidence from a Real-Effort Experiment with Real Goods, January 2008

2195 Mika Widgrén, The Impact of Council's Internal Decision-Making Rules on the Future EU, January 2008

2196 Antonis Adam, Margarita Katsimi and Thomas Moutos, Inequality and the Import Demand Function, January 2008

2197 Helmut Seitz, Democratic Participation and the Size of Regions: An Empirical Study Using Data on German Counties, January 2008

2198 Theresa Fahrenberger and Hans Gersbach, Minority Voting and Long-term Decisions, January 2008

2199 Chiara Dalle Nogare and Roberto Ricciuti, Term Limits: Do they really Affect Fiscal Policy Choices?, January 2008

2200 Andreas Bühn and Friedrich Schneider, MIMIC Models, Cointegration and Error Correction: An Application to the French Shadow Economy, January 2008

2201 Seppo Kari, Hanna Karikallio and Jukka Pirttilä, Anticipating Tax Change: Evidence from the Finnish Corporate Income Tax Reform of 2005, January 2008

2202 Walter Krämer and André Güttler, On Comparing the Accuracy of Default Predictions in the Rating Industry, January 2008

2203 Syed M. Ahsan and Panagiotis Tsigaris, The Efficiency Loss of Capital Income Taxation under Imperfect Loss Offset Provisions, January 2008

2204 P. Mohnen, F. C. Palm, S. Schim van der Loeff and A. Tiwari, Financial Constraints and other Obstacles: Are they a Threat to Innovation Activity?, January 2008

2205 Sascha O. Becker and Mathias Hoffmann, Equity Fund Ownership and the Cross-Regional Diversification of Household Risk, January 2008

2206 Pedro R. D. Bom and Jenny E. Ligthart, How Productive is Public Capital? A Meta-Analysis, January 2008

2207 Martin Werding, Ageing and Productivity Growth: Are there Macro-level Cohort Effects of Human Capital?, January 2008

2208 Frederick van der Ploeg and Steven Poelhekke, Globalization and the Rise of Mega-Cities in the Developing World, February 2008

2209 Sara Biancini, Regulating National Firms in a Common Market, February 2008

2210 Jin Cao and Gerhard Illing, Liquidity Shortages and Monetary Policy, February 2008

2211 Mathias Kifmann, The Design of Pension Pay Out Options when the Health Status during Retirement is Uncertain, February 2008

2212 Laszlo Goerke, Tax Overpayments, Tax Evasion, and Book-Tax Differences, February 2008

2213 Jun-ichi Itaya and Heinrich W. Ursprung, Price and Death, February 2008

2214 Valentina Bosetti, Carlo Carraro and Emanuele Massetti, Banking Permits: Economic Efficiency and Distributional Effects, February 2008

2215 Assar Lindbeck, Mårten Palme and Mats Persson, Social Interaction and Sickness Absence, February 2008

2216 Gary E. Bolton and Axel Ockenfels, The Limits of Trust in Economic Transactions - Investigations of Perfect Reputation Systems, February 2008

2217 Hartmut Egger and Peter Egger, The Trade and Welfare Effects of Mergers in Space, February 2008

2218 Dorothee Crayen and Joerg Baten, Global Trends in Numeracy 1820-1949 and its Implications for Long-Run Growth, February 2008

2219 Stephane Dees, M. Hashem Pesaran, L. Vanessa Smith and Ron P. Smith, Identification of New Keynesian Phillips Curves from a Global Perspective, February 2008

2220 Jerome L. Stein, A Tale of Two Debt Crises: A Stochastic Optimal Control Analysis, February 2008

2221 Michael Melvin, Lukas Menkhoff and Maik Schmeling, Automating Exchange Rate Target Zones: Intervention via an Electronic Limit Order Book, February 2008

2222 Raymond Riezman and Ping Wang, Preference Bias and Outsourcing to Market: A Steady-State Analysis, February 2008

2223 Lars-Erik Borge and Jørn Rattsø, Young and Old Competing for Public Welfare Services, February 2008

2224 Jose Apesteguia, Steffen Huck, Jörg Oechssler and Simon Weidenholzer, Imitation and the Evolution of Walrasian Behavior: Theoretically Fragile but Behaviorally Robust, February 2008

2225 Walter Krämer, Long Memory with Markov-Switching GARCH, February 2008

2226 António Afonso and Christophe Rault, What do we really Know about Fiscal Sustainability in the EU? A Panel Data Diagnostic, February 2008

2227 Sergey M. Kadochnikov and Igor M. Drapkin, Market Structure, Technological Gap and Vertical Linkage Effects from Foreign Direct Investment, February 2008

2228 Guglielmo Maria Caporale, Davide Ciferri and Alessandro Girardi, Fiscal Shocks and Real Exchange Rate Dynamics: Some Evidence for Latin America, February 2008

2229 Scott Alan Carson, Geography and Insolation in 19th Century US African-American and White Statures, February 2008

2230 Wolfgang Buchholz and Jan Schumacher, Discounting and Welfare Analysis Over Time: Choosing the η, February 2008

2231 M. Hashem Pesaran, Christoph Schleicher and Paolo Zaffaroni, Model Averaging in Risk Management with an Application to Futures Markets, February 2008

2232 Wilhelm Kohler, Offshoring: Why Do Stories Differ?, February 2008

2233 Stefan Bach, Giacomo Corneo and Viktor Steiner, Effective Taxation of Top Incomes in Germany, 1992-2002, February 2008

2234 Robert S. Chirinko, σ: The Long And Short Of It, February 2008

2235 Volker Grossmann and Holger Strulik, Should Continued Family Firms Face Lower Taxes than other Estates?, February 2008

2236 Guido Tabellini, The Scope of Cooperation: Values and Incentives, February 2008

2237 Heinrich W. Ursprung and Christian Wiermann, Reputation, Price, and Death: An Empirical Analysis of Art Price Formation, March 2008

2238 Hans Fehr and Christian Habermann, Private Retirement Savings in Germany: The Structure of Tax Incentives and Annuitization, March 2008

2239 Joseph Francois and Ian Wooton, Market Structure and Market Access, March 2008

2240 Hiroyuki Kasahara and Beverly Lapham, Productivity and the Decision to Import and Export: Theory and Evidence, March 2008

2241 Gary E. Bolton and Axel Ockenfels, Does Laboratory Trading Mirror Behavior in Real World Markets? Fair Bargaining and Competitive Bidding on EBay, March 2008

2242 Atsushi Oshima, B. Ravikumar and Raymond Riezman, Entrepreneurship, Organization Capital and the Evolution of the Firm, March 2008

2243 Walter Krämer and Sebastian Schich, Large-Scale Disasters and the Insurance Industry, March 2008

2244 Leif Danziger, Adjustment Costs, Inventories and Output, March 2008

2245 Anne van Aaken, Lars P. Feld and Stefan Voigt, Power over Prosecutors Corrupts Politicians: Cross Country Evidence Using a New Indicator, March 2008

2246 Hans-Christian Heinemeyer, Max-Stephan Schulze and Nikolaus Wolf, Endogenous Borders? The Effects of New Borders on Trade in Central Europe 1885-1933, March 2008

2247 Johannes Becker and Clemens Fuest, Tax Competition – Greenfield Investment versus Mergers and Acquisitions, March 2008

2248 Giorgio Bellettini and Hubert Kempf, Why not in your Backyard? On the Location and Size of a Public Facility, March 2008

2249 Jose Luis Evia, Roberto Laserna and Stergios Skaperdas, Socio-Political Conflict and Economic Performance in Bolivia, March 2008

2250 Bas Jacobs and A. Lans Bovenberg, Optimal Taxation of Human Capital and the Earnings Function, March 2008

2251 Jan-Egbert Sturm and Timo Wollmershäuser, The Stress of Having a Single Monetary Policy in Europe, March 2008

2252 Guido Schwerdt, Labor Turnover before Plant Closure: 'Leaving the Sinking Ship' vs. 'Captain Throwing Ballast Overboard', March 2008