Shadow of Death and the Exogeneity of Job Displacements:

Comparing Mass Layoffs, Plant Closures, and Bankruptcies*

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Abstract

To assess the scope for worker anticipation of upcoming job loss, we analyze employment patterns before bankruptcy, plant closure without bankruptcy, and mass layoff. Utilizing administrative data covering the universe of all West German private sector plants, we find no systematic employment reductions prior to mass layoffs, a strong and long-lasting reduction prior to closures, and a weaker shadow of death preceding bankruptcy. As the difference between bankruptcies and closures is driven by higher worker flows into the former, bankruptcies seem to struggle for survival while closures follow a shrinking strategy. We conclude that the scope of worker anticipation of upcoming job loss is smallest for mass layoffs and largest for closures without bankruptcy.

Keywords: job displacement, shadow of death, plant closure, bankruptcy, mass layoff

JEL Classification: L2, D22, J65

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1. Introduction

This paper analyzes firms' employment patterns prior to collective job displacement events, i.e. mass layoffs and firm exit, in order to assess whether these events occur suddenly and unexpectedly or whether we can observe employment developments that point to an upcoming displacement event. With respect to market exit, the main purpose of our analysis is to reveal whether firms die suddenly or whether exit is preceded by a long-lasting and piecewise shut down of economic activity. A systematic and long lasting plant-level employment reduction prior to exit constitutes a "shadow of death".[1] While a sudden and unanticipated shock precludes workers from taking preventive actions, a shadow of death opens up the opportunity to predict upcoming closures and to react strategically. The scope for strategic behavior, e.g. selective worker attrition, is in particular crucial for the interpretation of the frequently reported negative labor market outcomes of displaced workers (von Wachter 2010), a topic of outstanding relevance given the high unemployment rates in the United States and many European countries in recent years.

The literature on job displacement analyzes the fortunes of workers who lost their job due to events outside of their individual control. Besides describing the short run impact of such events on workers, it also tells a lesson about how forgiving labor markets are in the longer-run, e.g. how long an exogenous negative shock has consequences on individual earnings. Many studies show large short-term earnings losses followed by a long catch-up process (e.g. Jacobsen et al. 1993). Despite the mounting evidence on job displacement, this pattern is one among few settled facts in this literature.[2]

[1] The term „shadow of death" was introduced by Griliches and Regev (1995) to describe the phenomenon that productivity declines already several years before closure. Other studies (e.g. Almus 2004) used this term with respect to declining employment before closure.

[2] See von Wachter (2010) for a discussion of heterogeneities in results and its sources.

The scale of the short-term loss and whether and to what extent it persists varies to a considerable extent between studies (see e.g. von Wachter 2010). This unsatisfying situation has a lot to do with the fact that researchers have to make a number of important choices with respect to which workers and which displacement events to analyze. Choices at the worker level include age, gender, and tenure. Mass layoffs and plant closures are the two displacement events typically considered. Changes in one of the worker or event characteristics generally affect results. While there is some consensus on how to define worker characteristics (male workers younger than a certain age in stable employment relations), the choice of the appropriate displacement event and measurement problems associated with it are disputed questions since it is not fully understood what kind of selectivity processes are at work (von Wachter 2010:95).

Against this background, what is the aim of our work? Our main contribution is the provision of the first systematic comparison of employment developments prior to three job displacement events: mass layoff, plant closure, and bankruptcy. We detect pre-displacement periods that are likely to be affected by selective worker attrition and analyze whether the magnitude of the shadow of death, and therefore the scope for anticipation and selective pre-event layoffs, differs between the three event types. Of course, we are not the first to observe that a "shadow of death" may obscure analyses that utilize plant closures or mass layoffs as some exogenous event. Our analysis, however, compares three events and provides novel evidence on the appropriateness of these events to serve as a proxy for exogenous, i.e. unanticipated, job displacement. To better understand the processes behind the changes in employment levels, the paper additionally investigates the in- and outflow of workers.

On top of our contribution to labor economics, we add to the industrial organization literature. Typically, studies on the "shadow of death" analyze closures without being able to distinguish

between bankruptcies (involuntary closures) and closures without bankruptcy.[3] Recognizing this gap, the industrial organization literature calls for an analysis of involuntary closures (e.g. Carreira and Teixeira 2011:338). By comparing closures and bankruptcies, we provide first insights in pre-exit employment patterns of involuntary closures. We have a clear prior regarding differences in pre-exit patterns between bankruptcies and closures without bankruptcy. As leaving the market after repaying debts requires some form of long- or medium-run planning, we assume that the shadow is longer prior to closures compared to bankruptcies. Our analysis of worker flows provides additional insights on whether firms show a hiring behavior that suggests that they try to continue their economic activity.

Our administrative panel data cover all West German plants with at least one worker subject to social security payments. We find substantial differences between plants closing due to bankruptcy and plants closing for other reasons. Bankruptcies seem to be much more surprising for employees and, possibly also, for their employers. Compared to the employment development of an appropriate control group of survivors, closures shrink by a higher percentage than bankruptcies in any but the last of the five years prior to ultimate shut down. The most striking difference, however, exists between mass layoffs at the one hand and closures and bankruptcies at the other hand. Plants facing mass layoffs experience a long-lasting and monotone employment *increase* before the event. With respect to the worker flows, it is striking that bankruptcies have a higher amount of churning (excess worker flows) than the control group in each of the five years under investigation, which is not true for closures. This points to an intended and controlled shrinking strategy for closures without bankruptcy and to an unintended collapse for bankruptcies trying to keep a certain level of economic activity as long as possible thus compensating their higher level of separations by new hires. We finally utilize these results to discuss the appropriateness of the three layoff

[3] Mueller and Stegmaier (2015) discuss voluntary and involuntary closures in more detail.

events to serve as a proxy for exogenous job loss. Our results have implications for the analysis of post-displacement outcomes (Jacobsen et al. 1993) and studies using displacements as exogenous career interruptions to identify, e.g., returns to human capital components (Dustmann and Meghir 2005), fertility decisions (Del Bono et al. 2012), or intergenerational aspects of displacements (Oreopoulos et al. 2008).

2. The Exogeneity of Job Displacement

2.1 Within and Between Plant Selectivity

In an analysis about the consequences of job displacements, the choice of the displacement event may heavily influence the selection of displaced workers and thereby both internal validity and comparability with results of other studies. Generally, there can be selectivity within and between plants. *Between plant selectivity* occurs if workers displaced during mass layoffs differ (in observed or unobserved terms) from workers laid off due to plant closure or bankruptcy for reasons associated with sorting into different plant types. Sorting on the basis of unobserved worker characteristics, for instance, includes that risk-prone workers might chose more often firms that exhibit a higher failure risk (Winter-Ebmer 2001). Card et al. (2013) report for Germany that matching of high-wage workers with high-wage firms is a substantial and increasing phenomenon. One important dimension of between plant selectivity is related to firm size in the sense that larger firms are able to attract workers with better (unobservable) skills (Brown and Medoff 1989; Winter-Ebmer and Zweimüller 1999). The typical definition of a mass layoff is a sudden 30 percent drop in employment within plants having more than 50 employees (Jacobsen et al. 1993). By definition, this captures workers of rather large plants, only. Contrarily, closing plants are typically small. Hethey-Maier and Schmieder (2013) report for Germany that 83 percent of all establishment ID's vanishing

from administrative data belong to plants with less than four employees. Mueller and Stegmaier (2015) show that 83 percent of all bankruptcies occur in plants having no more than ten employees.

Within-plant selectivity consists of two components. On the one hand, it addresses the managerial decision whom to layoff during a mass layoff and this issue is obviously solved at the time of plant closure. On the other hand, within-plant selectivity also includes selective worker attrition in the years before the event, i.e. the shadow of death. The shadow of death phenomenon is not restricted to mass layoffs and has, in fact, been typically analyzed with respect to plant closures.[4] The mere existence of a shadow of death questions a crucial assumption made in the displacement literature. It questions whether becoming displaced is really outside of the workers individual control. Or with the words of Lengermann and Vilhuber (2002:1): "it is disputable whether the displacement really comes as a complete surprise".

When it comes to the discussion about whether mass layoffs or plant closures (be they with or without bankruptcy) are the better proxy for unexpected and exogenous job loss, it is sometimes argued that using plant closures avoids within plant selectivity but comes with the disadvantage of looking mostly at workers displaced from small plants (von Wachter 2010:95, Eliason and Storrie 2006:833). Obviously, this argumentation establishes a trade-off between within- and between plant selectivity. We argue that neither the advantage nor the disadvantage is a priori clear. First, if the shadow is more pronounced before closures than before mass layoffs, arguing that closures are less prone to within plant selectivity may not be justified. Second, looking at workers displaced from small plants is no disadvantage *per se*. If one is interested in the fate of workers having been in stable employment at a "good" and

[4] Fackler et al. (2014) provide evidence for Germany and a sound literature overview. Using different data sets, Fackler et al. (2014) and Almus (2004) compare employment developments of exiting and surviving plants in Germany and report lower employment growth in closing plants already several years prior to market exit.

typically high-paying firm, using small plants could indeed be inappropriate given their higher closure propensity, higher labor turnover, and lower wages. However, if one is interested in the consequences for the majority of displaced workers, one has to look also at small plants, as it is clear that mass layoffs, as defined in the job displacement literature, cover only a small fraction of displaced workers.[5] We therefore think that to what extent between plant selectivity matters depends on the research question at hand. Our paper sheds light on whether there is scope for within plant selectivity for closures and bankruptcies, too.

To be sure, we are not the first to observe that pre-exit mobility threatens the proper estimation of the causal effect of job loss. Pfann and Hamermesh (2008) explicitly argue that an upcoming shut down may be anticipated by workers and managers and report that workers staying until the end possess a particular high amount of firm-specific human capital. Lengermann and Vilhuber (2002) discuss strategic pre-exit behavior of workers and firms and report changes in the skill content of job and worker flows prior to displacement. Schwerdt (2011) finds selective attrition and recommends including separations up to two quarters before plant closure into the treatment group. He additionally reports better labor market outcomes of early leavers, something that has also been found by von Wachter and Bender (2006) and Couch and Placzek (2010). Eliason and Storrie (2006:848), however, find that early leavers may have worse outcomes. Dustmann and Meghir (2005) include workers leaving prior to closure in their sample of displaced workers. Del Bono et al. (2012) estimate the effect of plant closure on fertility decisions, carefully discuss pre-closure patterns at the firm level, and conclude that selectivity is a minor issue for their sample of young women.

[5] Eliason and Storrie (2006:837) and Hijzen et al. (2010:266) argue similarly.

2.2 Implications of Selectivity for Estimation and Interpretation

Pre-exit selectivity has implications for both the external and the internal validity of job displacement studies. External validity is limited as the final sample of displaced workers is a non-representative subsample of all workers affected by the displacement. As the treatment group should include so called 'early leavers', i.e. separations driven by the upcoming displacement event, our study helps to define the treatment group by showing differences in the scope for anticipation between different layoff events. What is more, the aggregate impact of job displacement events on, say, a region or a country is underestimated as many affected workers are not taken into account.

But what is actually the threat to internal validity for a state-of-the-art analysis of the consequences of exogenous job loss? Differences in observable characteristics between finally displaced workers and non-displaced workers can be addressed within a standard OLS regression framework. The true challenges are differences in unobserved worker characteristics and differences in potential earnings trends. Being aware of these problems, various empirical techniques have been used, in particular, matching and fixed effects (i.e. difference in differences with panel data). While the latter approach directly addresses permanent differences between workers,[6] the weak conditional independence assumption essential for the matching estimator does this by requiring that, after matching, treatment and control group workers have the same potential outcomes in the absence of the treatment. In turn, the identifying assumptions of both methods are violated if treatment and control group have different potential earnings trends. A conditional differences in differences approach combining matching with differences in differences as in Hijzen et al. (2010) is arguably the

[6] There may still be an influence of unobserved firm differences. To mitigate this, e.g. von Wachter et al. (2009) include employer fixed effects and thereby compare displaced workers with non-displaced colleagues staying at the firm. Similarly, Jacobson et al. (1993) interact time dummies with pre-displacement firm characteristics when comparing within the group of (former) colleagues. In fact, the choice of the control group depends on what one wants to know. Possible are e.g. comparisons with all other workers or with non-separators, only.

most appropriate technique to be used though it still not assures that the parallel trend assumption is fulfilled. What is more, this technique requires choosing a concrete pre-treatment period for matching. Here, the researcher faces a trade-off: by going back too far one excludes a considerable number of young plants and by going back not far enough one runs the risk of matching on firm characteristics that are themselves affected by the upcoming closure. Our study helps in making this decision by detecting the length of the shadow of death.

To tackle the unobserved trend problem, Jacobsen et al. (1993) include worker specific time trends, which, however requires a second transformation of the data exacerbating the influence of measurement error on results. Applying a more aggregated trend correction, von Wachter et al. (2009) interact year dummies with workers' pre-displacement industry affiliation to mitigate the problem of different industry earnings trends. While these corrections are useful and *may* mitigate the consequences of the shadow of death, one can never hope to assess whether they *really* do. Given the limitations of any econometric approach to tackle this issue and given the sizeable impact of these corrections on final results (see e.g. figures 2 and 3 in Jacobsen et al. 1993), choosing the displacement event least prone to pre-event selectivity is important.

3. Data

We make use of the German Establishment History Panel (BHP) provided by the Institute of Employment Research (IAB) of the German Federal Employment Agency (BA). Our data contains the entire population of West German establishments employing at least one worker

subject to social security.[7] The data aggregates employers' compulsory worker-level social security notifications at the plant level and refers to the 30th of June of each year. It includes information on plant age and size, workforce composition, worker in- and outflows, regional and sectoral information, and a unique plant identifier.

Data availability and measurement issues are closely related to selectivity problems. It is generally difficult to actually identify mass layoffs and plant closure in administrative data. Plant exit is associated with a plant ID vanishing from the data. The disappearance of a plant ID can be due to very different reasons, including takeovers and changes of ownership or legal form. To better proxy true closures, extension files based on the work of Hethey-Maier and Schmieder (2013) are available. Hethey-Maier and Schmieder (2013) use worker flows between plant IDs and, intuitively, consider only those vanishing plant ID's as true closures where, after the ID vanished, workers are dispersed over a number of different plants. Contrary, if the largest part of a vanishing plant ID's workforce ends up in the same successor ID, one would conclude that the plant did not cease to exist. Bankruptcies are identified using administrative data from three sources: information on *Insolvenzgeld*, a compensation scheme each employee who has not received his wage due to employer bankruptcy is eligible to, social security announcements that are legally required if a firm dismisses employees due to its bankruptcy, and publicly available bankruptcy announcements made by the local courts (see Mueller and Stegmaier 2015 for more detailed information on the identification of bankruptcies).

We now clarify the exact definition of what we treat as a closure, a bankruptcy, or a mass layoff. A closure is a vanishing plant ID *without bankruptcy information* where the maximum

[7] The publicly available version of the BHP is e.g. described in Gruhl et al. (2012). Our version differs only in that it contains all plants rather than a 50 percent sample.

clustered worker outflow[8] of the closing plant makes up less than 30% of the workforce of the closing plant (i.e. we use "atomized deaths" as defined in Hethey-Maier and Schmieder 2013). For plants having less than 4 workers when observed the last time, the concept of clustered worker flows is not meaningful. As the bulk of vanishing plant ID's refers to such plants, dropping them seems, however, inappropriate. We decided to treat small exits as true exits if either the workforce splits up into different successor plants (which is impossible for one-worker plants and quite restrictive for two-worker plants) or if the successor is larger than the closing firm. This definition makes it unlikely that we treat continuations under different ID as small exits.[9] A bankruptcy is a plant for which we have bankruptcy information *and* for which the plant ID vanishes from the data. Flow measures are not needed. Finally, a mass layoff is defined as in Schmieder et al. (2010),[10] i.e. an employment reduction of between 30 and 80 percent within one year in plants that previously had at least 50 employees.[11] As in Schmieder et al. (2010) we computed a complete cross-flow matrix of employees and, for the definition of mass layoffs, require that less than 20 percent of displaced workers end up under the same new plant ID. We also require that the plant is not experiencing employment increases of more than 30 percent in the year prior and after the mass layoff.

In the empirical analysis, we will compare plants subject to one of the three mutually exclusive events (closure, bankruptcy, mass layoff) with a control group defined below.[12] We look at plants having the last pre-event observation in the year 2007. [13] Strictly speaking, the

[8] A clustered worker flow denotes workers moving from the same predecessor plant to the same successor plant between two consecutive years. The largest cluster of all clustered outflows from a predecessor is its maximum clustered outflow.
[9] Our robustness checks show that using all small deaths (as in Fackler et al. 2014) makes little difference.
[10] We are grateful to Johannes Schmieder for providing us with the necessary codes.
[11] German law also provides a legal definition for mass layoffs (§17 KschG). Since this event can't be detected in any data available up to date, scholars typically follow the above mentioned approach, which goes back to Jacobsen et al. (1993).
[12] The events are mutually exclusive with respect to a specific year. Mass layoffs may, however, lead to closure or bankruptcy in future years. As this may be interesting for the evaluation of within-plant selectivity in closures and bankruptcies, we checked the importance of this phenomenon. Less than one percent of all closures and bankruptcies of the event-cohort 2007 are preceded by mass layoffs occurring between 1999 and 2006.
[13] Our bankruptcy information is incomplete for earlier years.

event takes place at some point between June 30th of 2007 and June 29th of 2008. We chose 2007 as this is the earliest year for which we have reliable bankruptcy information and because 2007 should be the least affected by the global economic crises reaching Germany around the third quarter of 2008. Our main insights are, however, unchanged if we replicate our analyses for plants with the last pre-event observation in 2008.[14] We study the employment patterns during the last five years before the displacement event, i.e., we restrict our analysis to plants that are at least five years old in 2007 and therefore already existed in 2002. The same restriction applies to the control group.[15] Our analysis refers to the western German private sector without agriculture and mining (both sectors are heavily influenced by national or European legislation). In the event cohort of the year 2007, our sample comprises 39,902 closures, 5,020 bankruptcies, and 320 mass layoffs.

The outcome variables of interests in the following empirical analysis are the logarithm of a plant's number of employees as well as the accession-, separation- and churning-rate in order to investigate the worker flows behind the employment changes. Accessions are defined as all workers that were employed in a given plant on June 30th (the reference date in the BHP) of a given year but not on June 30th of the previous year. Analogously, separations are defined as all workers that were not employed in a given plant on June 30th of a given year but on June 30th of the previous year.[16] Churning (or excess worker flows) is the sum of accessions and separations minus the change in total employment between two reference dates and describes the amount of worker flows that goes beyond an adjustment in the number of employees. Following Davis and Haltiwanger (1999:2718f), we calculated symmetric accession-,

[14] Fackler et al. (2013) argue that it is difficult to reliably identify closures close to the current edge of the data. As the current version of the BHP ends in 2010, we therefore don't consider closures later than 2008.
[15] The observation period of five years is in line with a previous study on the „shadow of death" for Germany by Fackler et al. (2014). Restricting the shadow to even shorter periods obviously makes little sense as a shadow of death for a, say, three year old plant is not a really meaningful measure.
[16] Unfortunately, it is not possible to distinguish between voluntary quits and layoffs.

separation-, and churning rates between two periods t and t-1, thus dividing each of the three measures by average employment in t and t-1.

4. Empirical Analysis

Our main goal is to assess differences in the scope for workers' anticipation of upcoming layoff events. Anticipation can only be driven by information available to workers and it therefore makes little sense to run a standard regression and to present partial effects, which are – of course – never observable to workers. Taking the viewpoint of the individual worker, we therefore only analyze unconditional changes in the respective outcome variables compared to the average plant in the economy and, second, compared to plants that have been very similar a certain amount of years prior to the event.

In what follows, we compare the pre-event development of plants facing one of the three displacement events in 2007 with a control group of plants that did not experience the respective event in 2007. The control group for each of the three events may thus contain both plants with other displacement events in 2007 and plants with the same event occurring earlier or later. This was done because we think that from an employee's point of view, the relevant comparison group comprises all other plants (with similar characteristics) and not only those that do not experience any of the three events during the period of observation. However, we conducted a robustness test restricting the control group to plants that never experienced any of the three events until 2010 which did not alter our main insights.

We estimate OLS regressions for the period 2002 to 2007 with the dependent variable being the natural logarithm of the number of employees, the accession-, separation-, or churning-rate. As explanatory variables we include year dummies, a time-invariant event dummy, and interaction terms between the year dummies and the event dummy. In this regression model,

the time-invariant event dummy measures the difference in the base-year (i.e. 2002)[17] between treatment and control group. Year dummies capture the employment evolution in the control group and thus account for any aggregate employment patterns, e.g. due to business cycle fluctuations. The interaction terms between year dummies and the time-invariant event indicator describe how differences between control and treatment group evolve over time.

Since it is not perfectly clear which is the relevant comparison group from an employee's point of view, we decided to estimate two specifications of the regression model described above. In the first specification we do not control for any further variables and thus compare plants facing a displacement event in 2007 with the average plant that did not experience the respective event in 2007. In the second specification, we choose a more restrictive control group comparing affected plants only with the average plant within the same industry, region, and size-class (referring to the base-year 2002). For this specification, we performed exact matching on two-digit industries, 30 administrative districts (*Regierungsbezirke*), and nine plant size classes[18] and estimated our regression models for the matched sample.[19]

4.1 Employment regressions

Estimation results for our employment regressions are presented in Tables 1a and 1b. Table 1a shows the results from our first specification which compares employment developments in plants facing one of the three displacement events in 2007 with all other plants that do not face the respective event in 2007. The coefficients of the event dummies indicate that, in the

[17] Note that in the worker flow regressions, the reference year is 2003 (and not 2002) because the flow measures refer to the period between two reference dates.

[18] The plant size classes are 1-4, 5-9, 10-19, 20-49, 50-99, 100-199, 200-499, 500-999, 1000 and more employees.

[19] In order to compare treated plants with the average non-treated plant in the same industry, region, and size-class we estimate weighted regressions assigning a weight equal to one to each observation in the treatment group and weighted each observation in the control group with the ratio of treated and non-treated observations in the respective cell (i.e. the combination of industry, region, size-class and year). This weighting scheme corresponds to the matching estimator described by Angrist (1998)

reference year 2002, closures are about 28 percent smaller than plants in the control group.[20] Interestingly, bankrupt plants are about 65 percent larger than plants in the respective control group and, as expected, plants facing mass layoffs are by far larger (on average about 20 times as large) than plants in the control group. The regression results in Table 1a further show that plants in the control group grew by about 17-19 percent between 2002 and 2007.

Looking at the coefficients of the interaction terms between the year and event dummies, one can see that bankruptcies experienced an increasingly worse employment development than their control group. Have bankrupt plants been larger than plants in the control group in 2002, they lost about 17 percent compared to their control group until 2006. The most severe employment reductions, however, are faced by closures. Here, the relative decline in employment amounts to 26 percent between 2002 and 2006.[21] In the last period before the event relative employment reductions in bankrupt plants are slightly larger than in closed plants whereas the relative employment reductions in the years before are always larger in closed plants. For mass layoffs, our estimates of the interaction terms between the year dummies and the event dummy show an employment increase until the year 2006, followed by reduction in the year prior to the event. It is important to note that this drop is likely to be driven by the definition of mass layoffs requiring that there is no employment increase of more than 30 percent prior to the mass layoff (and after it). This restriction does not apply to the control group.

So far we documented employment trends relative to the average plant in the economy. One could argue that workers don't look at a control group before making strategic moves. It is possible that they just look how their plant fares relative to the past. The regression results in

[20] This is computed as (exp(-0.3343)-1)*100%. In the text, all effects of the employment regressions are reported this way.

[21] Dustmann and Meghir (2005:89) report that the first obvious employment drop in closing firms is between year minus one and year minus two. Two potential reasons for these conflicting results are that Dustmann and Meghir (2005) do no confront closures with a control group and that they do not employ any worker flow measures to validate whether a vanishing plant ID is likely a closure or not.

Table 1a show that plants in the control group continuously grew between 2002 and 2007 so that the evolution of closures and bankruptcies may not look that negative in absolute terms as it does in relative terms. Looking at the employment development in absolute terms (i.e. the sum of the coefficients of the year dummies and the interaction terms with the event dummy) one can see that bankruptcies grew by about four percent between 2002 and 2005, had a small employment drop in 2006 (about three percent) and a strong employment reduction only in the last year. Closures, by contrast, reduced their employment level in each of the five years under investigation which is in line with previous evidence by Fackler et al. (2014). In 2003 and 2004, the employment reductions are rather small but still significant and become increasingly larger as exit approaches.

The regression results for the matched sample are presented in Table 1b and reveal a similar picture as those for the non-matched sample. Plants facing a mass layoff in 2007 increase their employment level until 2006 with a small reduction between 2006 and 2007. The employment level in bankrupt plants, however, increases relative to the matched control group until 2005, decreases slightly in 2006 followed by a strong employment reduction by almost 50 percent (compared to 2002) in 2007. Closures reduce their employment level relative to the control group in each of the five years thus showing a long-lasting shadow of death.

Taken together, the results of the employment regressions show that the three events under examination can be clearly ordered by the magnitude of the shadow of death. While there are no employment reductions preceding mass layoffs, the shadow is moderate for bankruptcies and substantial and long-lasting for closures.

4.2 Worker flow regressions

In a next step, we estimated regressions for the worker flow measures described above. Starting with the accession rate, one can see from Table 2a, which shows the results for the non-matched sample, that both plants facing mass layoffs and bankruptcies in 2007 have a higher accession rate than their respective control group in the base year (more precisely between the reference dates in 2002 and 2003). The difference is about eight percentage points for bankruptcies and 22 percentage points for mass layoffs. For closures, by contrast, the accession rate in the base year does not differ between treatment and control group. The coefficients of the year dummies, which capture the evolution in the control group, show a declining accession rate which seems to be nearly constant from 2005 onwards. This might indicate that employment fluctuations decrease as plants become older. Comparing the developments over time between the respective treatment and control groups, one can see that there is no economically significant difference until 2006. Only in 2007, the accession rate decreases for each of the three events with the largest drop for mass layoffs and the smallest for closures. Despite this drop in the last period, both bankruptcies and mass layoffs still have a considerably higher accessions rate than their respective control group.[22] Again, we obtain very similar results for the matched sample (Table 2b).

Turning to the separation rate (Tables 3a and 3b), our results for the non-matched sample in Table 3a show that in the base year, the separation rate is higher in the treatment than in the control group for each of the three events. The difference is largest for mass layoffs with 11 percentage points and smallest for closures with one percentage point. The evolution in the control group again suggests that employment fluctuations decrease on average as plants get older. The coefficients of the interaction terms show that the separation rate for each of the three events increases relative to the control group, in particular in the last pre-event period.

[22] The difference between treatment and control group in a given year equals the sum of the coefficients of the time-invariant event-dummy and the respective interaction term between year and event-dummy.

This effect is strongest for bankruptcies with 23 percentage points between 2003 and 2007 and moderate for closures and mass layoffs with 8 and 10 percentage points, respectively. The results for the matched sample in Table 3b reveal very similar developments.

Looking at the results for accessions and separations jointly, the picture that emerges is in line with the results of the employment regressions presented above. Although the separation rate for bankruptcies increases considerably already in 2005 and 2006, their accession rate remains on such a high level that employment decreases only slightly. Note that the separation rates for closures are lower than for bankruptcies and mass layoffs. The long-run shrinking of closures as reported in Tables 1a and 1b is achieved with an accession rate (separation rate) comparable to (slightly above) the control group's levels. The major difference to bankruptcies is that closures seem to undertake no efforts to stabilize employment levels by increased hiring. Except for the last year, mass layoffs always have a higher accession than separation rate which is consistent with the results from the employment regressions.

The results for the churning rate regressions are presented in Tables 4a and 4b and the results are again very similar for the matched (Table 4b) and the non-matched sample (Table 4a). In the reference year, bankruptcies and mass layoffs have substantially higher churning rates than their respective control groups because of their substantially higher accession and separation rates. For bankruptcies the difference is 9 percentage points in the non-matched sample and 7 percentage points in the matched sample. The respective figures for mass layoffs are 33 and 22 percentage points. For closures, by contrast, the churning rate in the base year hardly differs between treatment and control group for both the matched and the non-matched sample and, thus, there is no indication of management action going against the shrinking process. Looking at the developments over time, the churning rate for the control groups decreases somewhat over time. The coefficients of the interaction terms reveal that

there is hardly any systematic difference in the development between any of the three treatment groups and their respective control group.

Note that our main insights do not change when we perform a number of robustness tests (results are available upon request). First, our sample may contain a considerable number of plants that had just been founded at the beginning of the observation period in 2002. In order to mitigate the influence of presumably higher employment fluctuations associated with plant foundation, we restricted our sample to plants that have been founded in 1999 or earlier, i.e., that were at least three years old in 2002. Second, one might argue that the control group should not contain plants facing other displacement events or even the same event occurring earlier or later than in 2007. We therefore restricted each control group to plants that did not experience any of the three displacement events until 2010. Third, we restricted the control group in the matched sample to plants in the same size class not only in 2002 but also in 2001 and to 2000 to make sure that treated and non-treated plants had comparable growth paths before 2002. Fourth, replicating our analyses for plants facing a displacement event in 2008, an event-cohort that may already be affected by the Great Recession (note that an event in 2008 means that the event took place between June 30th 2008 and June 29th 2009), reveals very similar results.

5. Discussion and conclusions

We now turn to the overall picture that emerges for each of the three events when looking at all four outcomes jointly. The key question is whether the ultimate occurrence of the displacement event was foreseeable for workers. To shed light on this, we analyzed the

evolution of employment levels and worker flows, i.e., accessions, separations, and churning, before the event.[23]

For mass layoffs we see a long run increase in employment and therefore economic activity and a higher amount of worker flows. We think that despite the higher amount of worker flows, these developments cannot be interpreted as warning signals or hints why the growth path of these plants was interrupted later. Not least because mass layoffs are much more costly than stepwise employment reductions[24] one can hardly believe that a sudden collapse resulting in a mass layoff after continuous growth in the years before was foreseen by the relevant actors.

Closures reduce the number of workers over a long time span and there can be several reasons for long-run shrinking. First, some plants may experience idiosyncratic shocks exerting a long-lasting negative influence on profits (e.g. important consumers terminate cooperation). In many cases, management will have to react by reducing the scale of operation and, for some plants, the shrinking strategy will not help to avoid closure. Second, some business plans turn out to be not profitable and, e.g. in the sense of the passive learning model of Jovanovic (1982), employers decide to disinvest. Disinvestment may take time due to employment protection regularities or because parts of the plant generate a mark-up over variable costs and carry on until replacement investments become necessary. Third, many closures are voluntary exits and often do not reflect a failure of the business activity *per se* but, e.g., retirement decisions or situations where the firm owner built up more profitable alternatives (Mueller and Stegmaier 2015). Hence, disinvestment strategies may also happen in the absence of economic difficulties.

[23] Our results are robust to the choice of the control group and we don't dwell on this distinction in the discussion.

[24] In case of an upcoming mass layoff the employer is legally required (according to §17 KschG) to inform the German Federal Employment Agency. Inter alia, this notification procedure provides works councils with additional rights and is therefore often not in the interest of the employer.

It is hard to test whether (part of the) closures are planned, but the fact that the employment reductions in closing plants, in contrast to bankruptcies, do not come along with increased excess worker flows (churning) points at strategic shrinking rather than a struggle for life followed by an unintended collapse. To put it differently, bankruptcies have a high accession rate to compensate their substantial amount of separations while closures' accession rate is too low to even compensate for their comparably low separation rate. As the firm arguably has more control over the accession rate than over the separation rate, we would expect firms that intend to stay in business to have a high accession rate when the separation rate is high. One may argue that there is an upper bound to the accession rate, e.g. due to limited capacities of firms to search, to administer hirings, and to train new employees. If this is true, high separation rates may drive firms out of business even if management tries to stay in. Contrarily, firms having a moderate separation rate but an even lower accession rate obviously intend to shrink and this is exactly what we observe for closures. However, the most convincing argument for a planned exit strategy might be that these plants exit the market after repaying their debts, which in many cases requires a planned exit strategy. Whether the closure was intended or not can be important for the workers' scope for strategic reaction if management explicitly communicates upcoming disinvestment strategies or shutdown. In any case, the key point for our paper is that workers have a chance to observe negative business prospects prior to closures.

Contrarily to closures, bankruptcies reduce employment at a smaller scale and have a higher amount of worker flows. Their accession rate remains on a very high level in all the five years under investigation and is high enough to largely compensate for their higher amount of separations (except for the last year). Unfortunately, we are not able to investigate whether these separations are voluntary quits or layoffs but we think that the higher level of accessions points at a struggle to defend a certain production level before finally experiencing a sudden

collapse. Although we also find an increasing separation rate for bankruptcies as exit approaches, we think that their constantly high accession rate serves as a strong signal for these firms' employees that their employers intend to stay in business. A comparison of the results for bankruptcies and closures therefore suggests that the latter group contains planned exits following long-run shrinking strategies while bankrupt plants tried to stay in business at a given scale and shut down with a huge employment drop and many unpaid bills. Our reading of the results therefore is that the scope for worker anticipation and selective pre-event layoffs is lowest for mass layoffs, moderate for bankruptcies, and high for closures without bankruptcy.

One may object that workers take the high churning rate in mass layoffs and bankruptcies as a negative signal for the plant's economic situation. However, we show that the churning rate in plants facing mass layoffs or bankruptcies is higher than in the control group already five years before the respective event and hardly changes as the displacement event approaches. The absence of any systematic changes in the churning rate that could clearly be attributed to an upcoming displacement event makes us conclude that this cannot be regarded as a clear indicator for economic distress leading to a displacement event in the near future.

The higher churning rate is nevertheless interesting in its own right as it may reflect an employer's inability to build up a stable workforce with a sufficient stock of specific human capital which may be a competitive disadvantage that finally leads to bankruptcies or mass layoffs. In addition, the higher turnover in plants facing mass layoffs or bankruptcies may result in a higher share of low-tenure workers in such plants. Displacement of such workers would be evaluated differently than displacement of high tenure workers, the latter being traditionally in the focus of the job displacement literature.

Now what can we learn from our results about the appropriateness of the three events to serve as a proxy for unanticipated displacements? Putting all the pieces together, we conclude that

there is scope for within plant selectivity not only for mass layoffs. Although it remains true that mass layoffs open the opportunity to selectively lay off workers while this is not possible in closing plants at the time of closure, the existence of a shadow of death before closures and bankruptcies (but not before mass layoffs) shows that selective attrition may simply take place earlier there. This could have consequences not only for the timing of the selection process (selection prior to the event vs. selection at the time of the event) but also for the structure of the selection process (employee-driven selection vs. employer driven selection). We argue that especially the potentially long run shrinking process in the case of closures provides the employees with more scope for strategic behavior, whilst unexpected mass layoffs should rather be connected with employer-driven selection processes. Another implication of the existence of severe pre-exit employment reductions is that the total consequences of plant exit in terms of job losses may be underestimated when looking only at employment levels in the year of exit. If not all of the separating workers found a new job, total job loss exceeds final employment levels.

When using mass layoffs instead of closures or bankruptcies, one should keep in mind that the strict definitions necessary to properly approximate mass layoffs leaves us with just 320 plants. It is hard to argue that this subsample of plants leads to a sample of displaced workers being representative for the population of displaced workers or even for the population of workers being displaced from 'stable employment at a good firm', which has sometimes been postulated as the goal of displacement studies. In addition, German law prescribes the setup of a 'social plan' when the number of laid off workers exceed a certain threshold (§112 and § 112a *BetrVG [Works Constitution Act]*). This threshold depends on firm size but is low enough to capture our mass layoff definition. The social plan regulates whom to layoff. Among workers doing similar tasks, the firm has to select workers according to some social criteria (e.g. the propensity of laid off workers to find a new job [§112 (4)]). These

regularities hinder firms to select on the basis of productivity alone and it is therefore even harder for the economist to understand the type of selectivity and to interpret results on, say, estimated wage losses after displacement.

In order to investigate the fate of workers displaced from small and medium sized plants, which is indispensable to obtain a more complete picture of the consequences of involuntary job loss, one has, of course, to use closures or bankruptcies. This topic is of particular importance given the disproportionately strong contribution of small firms to overall job creation and destruction (e.g. Hijzen et al. 2010, Fuchs and Weyh 2010). Concerning the choice between closures and bankruptcies, our results suggest that using bankruptcies instead of all closures is the superior alternative as they seem to be less prone to pre-exit selection processes.

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Tables

Table 1a) Regression results – log (number of employees)

	Bankruptcies	Closures	Mass Layoffs
Event (dummy)	0.5017 (27.22)***	-0.3343 (-65.43)***	3.0381 (54.08)***
Year 2002 (reference)	---	---	---
Year 2003 (dummy)	0.0682 (129.23)***	0.0708 (131.53)***	0.0682 (129.45)***
Year 2004 (dummy)	0.1144 (178.31)***	0.1189 (181.45)***	0.1144 (178.67)***
Year 2005 (dummy)	0.1340 (184.45)***	0.1412 (190.22)***	0.1338 (184.57)***
Year 2006 (dummy)	0.1622 (203.02)***	0.1734 (212.20)***	0.1616 (202.74)***
Year 2007 (dummy)	0.1898 (219.67)***	0.2101 (237.71)***	0.1875 (217.29)***
Year 2003 × event	-0.0189 (-2.15)**	-0.0794 (-28.78)***	0.0352 (1.45)
Year 2004 × event	-0.0270 (-2.63)***	-0.1261 (-37.77)***	0.1178 (3.28)***
Year 2005 × event	-0.0943 (-8.25)***	-0.1979 (-52.31)***	0.1572 (3.81)***
Year 2006 × event	-0.1950 (-15.57)***	-0.2992 (-72.52)***	0.2225 (4.96)***
Year 2007 × event	-0.5954 (-38.09)***	-0.5107 (-108.57)***	0.1144 (2.43)**
Intercept	1.3265 (1323.21)***	1.3306 (1305.65)***	1.3275 (1326.41)***
R-squared	0.0030	0.0085	0.0042
No. of observations	7,563,779	7,514,491	7,563,779

Notes: BHP years 2002-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; regressions are based on the sample of plants that already existed in 2002; ***, **, * denotes significance at the 1, 5, or 10 percent level, respectively; t-values in parentheses; standard errors are clustered at the plant level.

Table 1b) Regression results – log (number of employees), matched sample

	Bankruptcies	Closures	Mass Layoffs
Event (dummy)	0.0535 (2.85)***	0.0066 (1.29)	0.0399 (0.68)
Year 2002 (reference)	---	---	---
Year 2003 (dummy)	0.0149 (12.12)***	0.0467 (79.06)***	-0.0955 (-16.02)***
Year 2004 (dummy)	0.0364 (22.87)***	0.0906 (123.34)***	-0.1196 (-13.35)***
Year 2005 (dummy)	0.0153 (8.51)***	0.1002 (119.51)***	-0.1815 (-9.37)***
Year 2006 (dummy)	0.0104 (5.30)***	0.1116 (115.84)***	-0.1680 (-10.21)***
Year 2007 (dummy)	-0.0002 (-0.10)	0.1234 (119.72)***	-0.1464 (-11.30)***
Year 2003 × event	0.0344 (3.88)***	-0.0551 (-19.94)***	0.2000 (7.92)***
Year 2004 × event	0.0509 (4.90)***	-0.0977 (-29.15)***	0.3543 (9.47)***
Year 2005 × event	0.0243 (2.10)**	-0.1566 (-41.22)***	0.4767 (10.37)***
Year 2006 × event	-0.0436 (-3.45)***	-0.2367 (-57.04)***	0.5582 (11.57)***
Year 2007 × event	-0.4041 (-25.73)***	-0.4232 (-89.61)***	0.4603 (9.53)***
Intercept	1.7718 (441.82)***	0.9889 (901.39)***	4.3143 (230.46)***
R-squared	0.0089	0.0151	0.0428
No. of observations	6,191,484	6,982,948	628,847

Notes: BHP years 2002-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; regressions are based on the sample of plants that already existed in 2002; weighted regressions with a weight equal to one for each observation in the respective treatment group and weights equal to the ratio of treated and non-treated observations in the respective cell (i.e. the combination of industry, region, size-class, and year) in the control group; ***, **, * denotes significance at the 1, 5, or 10 percent level, respectively; t-values in parentheses; standard errors are clustered at the plant level.

Table 2a) Regression results – accession rate

	Bankruptcies	Closures	Mass Layoffs
Event (dummy)	0.0830 (16.21)***	0.0026 (1.42)	0.2177 (11.43)***
Year 2003 (reference)	---	---	---
Year 2004 (dummy)	-0.0102 (-29.84)***	-0.0104 (-29.80)***	-0.0102 (-29.79)***
Year 2005 (dummy)	-0.0441 (-131.68)***	-0.0442 (-130.22)***	-0.0440 (-131.69)***
Year 2006 (dummy)	-0.0464 (-137.11)***	-0.0463 (-134.84)***	-0.0463 (-137.23)***
Year 2007 (dummy)	-0.0475 (-139.21)***	-0.0471 (-136.01)***	-0.0476 (-139.58)***
Year 2004 × event	0.0045 (0.74)	0.0053 (2.33)**	-0.0033 (-0.19)
Year 2005 × event	0.0037 (0.61)	0.0059 (2.70)***	-0.0200 (-1.08)
Year 2006 × event	-0.0065 (-1.09)	-0.0040 (-1.81)*	0.0065 (0.36)
Year 2007 × event	-0.0367 (-6.06)***	-0.0155 (-7.07)***	-0.0741 (-4.67)***
Intercept	0.2314 (851.09)***	0.2315 (839.55)***	0.2317 (852.95)***
R-squared	0.0056	0.0054	0.0055
No. of observations	5,861,676	5,820,813	5,861,676

Notes: BHP years 2003-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; regressions are based on the sample of plants that already existed in 2002; ***, **, * denotes significance at the 1, 5, or 10 percent level, respectively; t-values in parentheses; standard errors are clustered at the plant level.

Table 2b) Regression results – accession rate, matched sample

	Bankruptcies	Closures	Mass Layoffs
Event (dummy)	0.0826 (16.09)***	-0.0117 (-6.40)***	0.2139 (11.05)***
Year 2003 (reference)	---	---	---
Year 2004 (dummy)	-0.0074 (-16.35)***	-0.0134 (-29.33)***	-0.0009 (-0.33)
Year 2005 (dummy)	-0.0398 (-89.78)***	-0.0502 (-111.95)***	-0.0289 (-9.67)***
Year 2006 (dummy)	-0.0393 (-85.32)***	-0.0546 (-119.72)***	-0.0121 (-4.06)***
Year 2007 (dummy)	-0.0385 (-80.49)***	-0.0561 (-118.94)***	-0.0118 (-3.08)***
Year 2004 × event	0.0016 (0.26)	0.0084 (3.63)***	-0.0127 (-0.72)
Year 2005 × event	-0.0007 (-0.11)	0.0120 (5.39)***	-0.0350 (-1.84)*
Year 2006 × event	-0.0138 (-2.29)**	0.0044 (1.98)**	-0.0274 (-1.50)
Year 2007 × event	-0.0458 (-7.54)***	-0.0064 (-2.91)***	-0.1097 (-6.63)***
Intercept	0.2320 (579.36)***	0.2458 (682.52)***	0.2391 (91.39)***
R-squared	0.0217	0.0063	0.1124
No. of observations	4,764,463	5,386,696	456,267

Notes: BHP years 2003-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; regressions are based on the sample of plants that already existed in 2002; weighted regressions with a weight equal to one for each observation in the respective treatment group and weights equal to the ratio of treated and non-treated observations in the respective cell (i.e. the combination of industry, region, size-class, and year) in the control group; ***, **, * denotes significance at the 1, 5, or 10 percent level, respectively; t-values in parentheses; standard errors are clustered at the plant level.

Table 3a) Regression results – separation rate

	Bankruptcies	Closures	Mass Layoffs
Event (dummy)	0.0647 (13.94)***	0.0144 (7.83)***	0.1165 (9.31)***
Year 2003 (reference)	---	---	---
Year 2004 (dummy)	-0.0311 (-90.94)***	-0.0313 (-90.10)***	-0.0311 (-91.01)***
Year 2005 (dummy)	-0.0296 (-85.42)***	-0.0301 (-85.68)***	-0.0295 (-85.07)***
Year 2006 (dummy)	-0.0440 (-126.67)***	-0.0448 (-127.39)***	-0.0438 (-126.02)***
Year 2007 (dummy)	-0.0455 (-129.64)***	-0.0476 (-134.31)***	-0.0445 (-126.33)***
Year 2004 × event	0.0042 (0.74)	0.0040 (1.75)*	0.0068 (0.58)
Year 2005 × event	0.0353 (5.93)***	0.0173 (7.30)***	0.0157 (1.30)
Year 2006 × event	0.0569 (9.17)***	0.0276 (11.53)***	0.0147 (1.18)
Year 2007 × event	0.2289 (28.20)***	0.0786 (29.71)***	0.0993 (7.14)***
Intercept	0.2318 (862.15)***	0.2316 (850.07)***	0.2320 (864.13)***
R-squared	0.0046	0.0043	0.0035
No. of observations	5,861,676	5,820,813	5,861,676

Notes: BHP years 2003-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; regressions are based on the sample of plants that already existed in 2002; ***, **, * denotes significance at the 1, 5, or 10 percent level, respectively; t-values in parentheses; standard errors are clustered at the plant level.

Table 3b) Regression results – separation rate, matched sample

	Bankruptcies	Closures	Mass Layoffs
Event (dummy)	0.0478 (10.23)***	0.0177 (9.57)***	0.0532 (4.06)***
Year 2003 (reference)	---	---	---
Year 2004 (dummy)	-0.0345 (-57.45)***	-0.0292 (-66.17)***	-0.0309 (-9.18)***
Year 2005 (dummy)	-0.0334 (-50.21)***	-0.0270 (-60.88)***	-0.0296 (-5.13)***
Year 2006 (dummy)	-0.0506 (-78.40)***	-0.0395 (-87.06)***	-0.0594 (-13.92)***
Year 2007 (dummy)	-0.0542 (-83.35)***	-0.0414 (-89.17)***	-0.0553 (-12.01)***
Year 2004 × event	0.0076 (1.33)	0.0020 (0.86)	0.0064 (0.52)
Year 2005 × event	0.0391 (6.53)***	0.0142 (5.99)***	0.0153 (1.14)
Year 2006 × event	0.0634 (10.18)***	0.0222 (9.21)***	0.0300 (2.25)**
Year 2007 × event	0.2367 (29.12)***	0.0724 (27.20)***	0.1046 (7.56)***
Intercept	0.2488 (454.95)***	0.2282 (669.64)***	0.2979 (82.93)***
R-squared	0.0621	0.0070	0.0358
No. of observations	4,764,463	5,386,696	456,267

Notes: BHP years 2003-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; regressions are based on the sample of plants that already existed in 2002; weighted regressions with a weight equal to one for each observation in the respective treatment group and weights equal to the ratio of treated and non-treated observations in the respective cell (i.e. the combination of industry, region, size-class, and year) in the control group; ***, **, * denotes significance at the 1, 5, or 10 percent level, respectively; t-values in parentheses; standard errors are clustered at the plant level.

Table 4a) Regression results – churning rate

	Bankruptcies	Closures	Mass Layoffs
Event (dummy)	0.0889 (14.02)***	-0.0070 (-2.86)***	0.3255 (14.65)***
Year 2003 (reference)	---	---	---
Year 2004 (dummy)	-0.0237 (-53.27)***	-0.0238 (-52.78)***	-0.0236 (-53.25)***
Year 2005 (dummy)	-0.0389 (-85.33)***	-0.0391 (-84.47)***	-0.0388 (-85.18)***
Year 2006 (dummy)	-0.0459 (-99.51)***	-0.0461 (-98.37)***	-0.0458 (-99.40)***
Year 2007 (dummy)	-0.0438 (-93.42)***	-0.0435 (-91.48)***	-0.0437 (-93.27)***
Year 2004 × event	0.0084 (1.18)	0.0057 (1.97)**	-0.0246 (-1.68)*
Year 2005 × event	0.0222 (2.98)***	0.0084 (2.83)***	-0.0409 (-2.52)**
Year 2006 × event	0.0169 (2.24)**	0.0064 (2.15)**	-0.0189 (-1.13)
Year 2007 × event	0.0127 (1.58)	-0.0034 (-1.12)	0.0270 (1.63)
Intercept	0.2555 (698.00)***	0.2558 (688.80)***	0.2557 (699.69)***
R-squared	0.0023	0.0021	0.0022
No. of observations	5,861,676	5,820,813	5,861,676

Notes: BHP years 2003-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; regressions are based on the sample of plants that already existed in 2002; ***, **, * denotes significance at the 1, 5, or 10 percent level, respectively; t-values in parentheses; standard errors are clustered at the plant level.

Table 4b) Regression results – churning rate, matched sample

	Bankruptcies	Closures	Mass Layoffs
Event (dummy)	0.0737 (11.60)***	-0.0033 (-1.33)	0.2236 (9.80)***
Year 2003 (reference)	---	---	---
Year 2004 (dummy)	-0.0248 (-44.63)***	-0.0242 (-41.66)***	-0.0296 (-14.97)***
Year 2005 (dummy)	-0.0413 (-71.31)***	-0.0395 (-65.50)***	-0.0494 (-11.09)***
Year 2006 (dummy)	-0.0484 (-80.24)***	-0.0470 (-76.26)***	-0.0556 (-12.02)***
Year 2007 (dummy)	-0.0460 (-75.49)**	-0.0457 (-71.38)***	-0.0398 (-10.62)***
Year 2004 × event	0.0095 (1.33)	0.0061 (2.09)**	-0.0184 (-1.23)
Year 2005 × event	0.0246 (3.29)***	0.0088 (2.93)***	-0.0305 (-1.79)*
Year 2006 × event	0.0193 (2.55)**	0.0074 (2.44)**	-0.0087 (-0.50)
Year 2007 × event	0.0015 (1.84)*	-0.0013 (-0.43)	0.0257 (1.50)
Intercept	0.2707 (518.82)***	0.2521 (530.21)***	0.3618 (83.62)***
R-squared	0.0141	0.0016	0.1030
No. of observations	4,764,463	5,386,696	456,267

Notes: BHP years 2003-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; regressions are based on the sample of plants that already existed in 2002; weighted regressions with a weight equal to one for each observation in the respective treatment group and weights equal to the ratio of treated and non-treated observations in the respective cell (i.e. the combination of industry, region, size-class, and year) in the control group; ***, **, * denotes significance at the 1, 5, or 10 percent level, respectively; t-values in parentheses; standard errors are clustered at the plant level.