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## Plant-level employment development before collective displacements: Comparing mass layoffs, plant closures, and bankruptcies

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# Plant-level Employment Development before Collective Displacements: Comparing Mass Layoffs, Plant Closures, and Bankruptcies\*

## Abstract

To assess to what extent collective job displacements can be regarded as unanticipated exogenous shocks for affected employees, we analyze plant-level employment patterns before bankruptcy, plant closure without bankruptcy, and mass layoff. Utilizing administrative data covering 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 much shorter shadow of death preceding bankruptcy. Our analysis of worker flows underlines that 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: D22, J65, L2*

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## 1. Introduction

This paper analyzes firms' employment patterns prior to collective job displacement events, i.e. mass layoffs and firm exits with and without bankruptcy, in order to assess whether these events occur suddenly and unexpectedly or whether one can observe employment developments that point to an upcoming displacement event. A systematic and long lasting plant-level employment reduction prior to exit constitutes a “shadow of death”.<sup>1</sup> While a sudden and unanticipated shock precludes workers but also creditors and the managers of suppliers, and customers from taking preventive actions, a shadow of death opens up the opportunity to predict upcoming closures and to react strategically.

Regarding the shadow of death, a growing body of literature has found that employment and productivity usually decrease already several years before a firm's ultimate shutdown (e.g. Griliches and Regev 1995 for Israel, Troske 1996 for the US, Bellone et al. 2006 and Blanchard et al. 2014 for France, Carreira and Teixeira 2011 for Portugal, Almus 2004 and Fackler et al. 2014 for Germany). It is important to understand this pre-event process properly as displaced workers might face serious problems in case they are laid off suddenly. In contrast, if workers are laid off during a well-planned close-down of a firm it might be easier for them to find a new job and for local employment agencies to adjust to the process more smoothly thereby mitigating potential drawbacks for workers and local labor markets.

The above mentioned studies were not able to distinguish between business failure and voluntary shutdown, the latter being driven e.g. by the presence of more profitable alternatives. Recognizing this gap, the industrial organization literature calls for an analysis of involuntary closures (e.g. Carreira and Teixeira 2011:338). Firm exits due to bankruptcies can unambiguously be regarded as failure whereas closures may occur due to very different

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<sup>1</sup> 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.

reasons that are not necessarily related to a firm's profitability (Müller and Stegmaier 2015). Addressing this issue empirically, Bates (2005) and Headd (2003) find that a non-negligible share (about one third) of closed firms was evaluated as successful by their owners. This suggests that previous studies on the shadow of death may have mixed up very different phenomena. Against this background the main contribution of our study is to compare pre-exit employment patterns before closures with and without bankruptcy thereby analyzing the shadow of death prior to involuntary exit for the first time.<sup>2</sup> We have a clear conjecture 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 on average longer prior to closures compared to bankruptcies.

Our study further contributes to the literature by analyzing worker flows. By examining an employer's hiring behavior as well as separations, we shed light on whether firms exit the market after an intended reduction of economic activity or struggle against a negative development and try to continue their operations. In the former case, one would argue that an upcoming closure is foreseeable for the workforce (and stakeholders outside the firm) whereas in the latter case, there is no reason for workers to expect their employers shutting down the plant.

These insights obtained from the analysis of employment patterns before collective displacements have important implications regarding the literature on job displacement (e.g. Jacobsen et al. 1993), i.e., to what extent collective job displacements can be regarded as unanticipated exogenous shocks for affected employees. The literature on job displacement, which analyzes the fortunes of workers who unexpectedly lost their job due to events outside

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<sup>2</sup> A more detailed assessment of the identification of bankruptcies, closures due to other reasons, and mass layoffs is provided in Section 4. Detailed information on the legal background of bankruptcies, closures without bankruptcy, and mass layoffs can be found in Section 3.

of their individual control, has long been aware of the fact that a shadow of death may obscure analyses that utilize plant closures (or mass layoffs) as an exogenous shock in order to determine wage or employment effects for affected workers among others.<sup>3</sup> The reason is that a shadow of death – if it is observable for a firm’s employees – opens up the opportunity for strategic behavior, e.g. selective worker attrition, which is particularly crucial for the interpretation of the frequently reported negative labor market outcomes of displaced workers (von Wachter 2010). Many studies show large short-term earnings losses followed by a long catch-up process (e.g. Jacobsen et al. 1993). Despite large heterogeneities these earnings loss profiles are one among few settled facts in this literature.<sup>4</sup> But 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 to do with the fact that researchers use different definitions of displacement or different displacement events. Mass layoffs<sup>5</sup> and plant closures are the two events typically considered but 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).

As stated above, 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. Ex-post tests for anticipation applying *worker-level* data compare changes in labor market outcomes for workers separating from dying and continuing plants (e.g. Schwerdt 2011).<sup>6</sup> Looking at the *plant-level*, in contrast, reveals whether there is ex-ante observable information that workers

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<sup>3</sup> Other studies use displacements as proxies for 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).

<sup>4</sup> See von Wachter (2010) for a discussion of heterogeneities in results and its sources.

<sup>5</sup> The job displacement literature typically approximates a mass layoff by an employment reduction of at least 30 percent in plants that had at least 50 employees before this employment reduction (e.g. Jacobsen et al. 1993, Schmieder et al. 2010). A more detailed definition is provided in Section 4.

<sup>6</sup> Worker-level studies do not distinguish between different displacement events and are impeded by identification issues related to unobserved worker heterogeneity potentially influencing decisions, e.g. about whether to separate early.

can use to infer upcoming displacement. Hence, instead of focusing on differences in actual behavior, we test whether workers' chances to act strategically differ by displacement event and provide novel evidence on the appropriateness of these events to serve as a proxy for exogenous, i.e. unanticipated, job displacement.

To conclude, what is the aim of our work? Our main contribution is the provision of the first systematic comparison of employment developments including the in- and outflows of workers 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 worker attrition, differs between the three event types. Anticipation possibilities offer scope for strategic reactions also for agents outside the firm (creditors, other contractors, social security agencies). Therefore our contribution goes well beyond the issue of selective worker attrition.

Our results show substantial differences between plants closing due to bankruptcy and other plant closures. Bankruptcies seem to be much more surprising for employees and, possibly, also for their employers. Compared to the employment development of the control group, closures shrink by a higher percentage than bankruptcies in any but the last of the five years prior to ultimate shut down. Interestingly, plants facing mass layoffs experience a long-lasting and monotone employment *increase* before the event. With respect to worker flows, it is remarkable 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.



## 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. Hethy-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. Müller 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 is obviously no issue 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.<sup>7</sup> The mere existence of a shadow of death challenges a crucial assumption made in the displacement literature. It puts into question whether becoming displaced is really outside of the worker's individual control.

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 establishes a trade-off between within- and between plant selectivity. We claim 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. Our paper sheds light on whether there is scope for within plant selectivity for closures and bankruptcies, too. 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 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 typical displaced worker, 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.<sup>8</sup> We therefore think that to what extent between plant selectivity matters depends on the research question at hand.

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

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<sup>7</sup> 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.

<sup>8</sup> Eliason and Storrie (2006:837) and Hijzen et al. (2010b:266) argue similarly.

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. Fackler et al. (2014) find that employment reductions prior to plant closures come along with changes in the workforce composition. In particular, they report that the shares of high-skilled and female employees and the median age of the workforce increase before closure. Schwerdt (2011) finds selective attrition and recommends including separations up to two quarters before plant closure in 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. Taken together, the results of these studies are ambiguous regarding the question whether early leavers are those who leave first because they have better outside options in the labor market or whether they are less skilled and dispensable workers who are laid off first. At the same time, most of these studies agree that there is selective worker attrition going on before firm closures.

## **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 length of the shadow of death. 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,<sup>9</sup> 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 difference in differences approach combining matching with difference in differences as in Hijzen et al. (2010b) is arguably the most appropriate technique to be used though it still not assures that the parallel trend assumption is fulfilled. Before turning to this critical assumption, we point out that 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

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<sup>9</sup> 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.

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.

The parallel trend assumption fails if workers reaction to upcoming displacement differs by their earnings trends. E.g. the costs of job displacement will be over-estimated if workers with above average earnings growth tend to leave early. 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 that is least foreseeable and therefore least prone to pre-event selectivity clearly is important.

### **3. Institutional Background**

This section briefly describes the legal backgrounds of bankruptcies, closures, and mass layoffs and their consequences for workers and firms. The procedure for a bankrupt firm is regulated in the Insolvency Code (*Insolvenzordnung*). According to the Insolvency Code, both debtors and creditors are entitled to file for bankruptcy. If the debtor is a corporation (e.g. a limited liability company or a public limited company), the debtor's management is obliged to file as soon as the company is bankrupt; for any other type of debtor (i.e. natural persons or business partnerships) as well as all creditors, no mandatory filing rule exists. After filing for

bankruptcy the court usually appoints an interim bankruptcy administrator. Most importantly, this administrator has to cope with two questions. First, he has to validate whether one of the prescribed legal causes to file for bankruptcy – illiquidity or over-indebtedness – applies and second, he will check if the firm has sufficient assets to cover the costs of bankruptcy procedures. If the firm's assets are sufficient and one of the causes for bankruptcy applies, the court will open regular bankruptcy proceedings. If the causes do not apply, the request to initiate bankruptcy proceedings will be rejected by the court. Finally if the firm is either illiquid or over-indebted but the remaining assets are insufficient to cover the costs of the legal procedures, the court will also reject the opening of a formal bankruptcy procedure.

If the employer does not pay his employees' wages before the opening of formal bankruptcy proceedings, employees face the risk of losing these wage claims during the bankruptcy. §165 of the Social Code III (*Sozialgesetzbuch*) therefore offers a guarantee scheme for affected employees (*Insolvenzgeld*) that almost fully compensates unpaid wages. If workers apply for this compensation, their salary claims against their employer are subrogated to the German Federal Employment Agency. Thus, workers are secured in case of their employer's bankruptcy and wage claims against the employer subsist so that there is no incentive for employers to file for bankruptcy in order to save on wages.

In addition, the opening of formal bankruptcy proceedings shortens statutory periods of notice and employees can therefore be laid off more quickly. While statutory periods of notice for regular dismissals vary according to workers' tenure and last up to seven months<sup>10</sup> a worker can be laid off in the course of a bankruptcy within a maximum of three months. However, employment protection legislation, which applies to all establishments with more than 10 employees, is not affected by the opening of bankruptcy proceedings and consequently, there is also no difference between bankruptcies and closures without bankruptcy.

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<sup>10</sup> Collective agreements or individual contracts may provide for even longer periods of notice.

Beyond that, there are further regulations that apply to companies with more than 20 employees. The Works Constitution Act (*Betriebsverfassungsgesetz*, WCA hereafter) prescribes in §111 that in firms with more than 20 employees<sup>11</sup> the employer has to inform the works council<sup>12</sup> in a timely manner about a so-called “establishment alteration” (*Betriebsänderung*), i.e., planned alterations that may entail substantial drawbacks for the staff. Such alterations can be the reductions of operations, the closure of an establishment<sup>13</sup> or important departments of an establishment, a mass layoff, or changes in the organization among others.

In case of an establishment alteration, which applies to mass layoffs, closures, and bankruptcies if the legal requirements are fulfilled, the employer has to negotiate with the works council about a possible reconciliation of interests. If bankruptcy proceedings have been commenced, a reconciliation of interest can be held dispensable by the local labor court if the administrator files a respective motion. However, the works council can neither force the employer to abstain from the intended alteration of the business nor to agree to a specific reconciliation of interests (e.g. a specific list of redundant employees selected according to some social criteria) as alterations of the firm are generally seen as part of the freedom to conduct a business. Therefore, the WCA provides a second step where the employer and the works council have to negotiate a social plan, an agreement to alleviate the economic effects of the changes in business for affected employees.<sup>14</sup> Contrary to the reconciliation of interests, a social plan may be legally set in force. Hence, due to these regulations there is a strong incentive for employers with more than 20 employees not to lay off a huge number of workers

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<sup>11</sup> As §111 refers to firms rather than establishments, establishment alterations may also apply to establishments with less than 20 employees if they are part of a firm with more than 20 employees.

<sup>12</sup> Workers are entitled to establish a works council if an establishment has at least five employees, but the law imposes no automatism. The WCA guarantees works councils a number of legal rights including rights on information, consultation, and co-determination (see Addison 2009 for details).

<sup>13</sup> The closure of an establishment is legally defined as the final termination of the objective and the organization of an establishment (Kania 2012).

<sup>14</sup> In practice reconciliations of interests and social plans are often negotiated simultaneously.

at once but to reduce employment more smoothly if reduction of economic activities or the shutdown of a plant is foreseeable. If the company has not more than 20 workers and no works council, there is no similar procedure. It could even be the case that the employer is not obliged to inform his workers about upcoming changes.

Taken together, the consequences for workers and firms are remarkably similar for mass layoffs, bankruptcies, and closures without bankruptcy. Differences are that periods of notice prior to dismissals and negotiations with works councils can be shortened in case of bankruptcies.

#### **4. 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). The BHP contains the entire population of German establishments employing at least one worker subject to social security since 1975.<sup>15</sup> The data aggregates employers' compulsory worker-level social security notifications at the plant level and refers to the 30<sup>th</sup> of June of each year. It includes information on plant age and size, workforce composition (such as age, educational and vocational qualification, occupational group, or share of females), worker in- and outflows, identifiers for administrative districts (*Kreise*) and sectors (5-digit), and a unique plant identifier. Major advantages of the BHP are that it covers all industries, size, and age classes and that it can be considered very reliable since it is based on mandatory social security notifications. Shortcomings are that it does not contain information on plants that do not have employees who are liable to social security and that some potentially relevant variables, such as sales or profits, are not included.

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<sup>15</sup> 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.



Data availability and measurement issues are closely related to selectivity problems. It is generally difficult to actually identify mass layoffs and plant closures in administrative data. Plant exit is associated with a plant ID vanishing from the data. But 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 IDs as true closures where, after the ID disappeared, 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 mainly identified using administrative data on *Insolvenzgeld*. Note that employees are not only eligible for receiving *Insolvenzgeld* if formal bankruptcy proceedings are opened, but also if the court rejects the opening of a bankruptcy procedure or even if both debtors and creditors do not file for bankruptcy although the firm is illiquid or over-indebted. Hence, we employ information on bankruptcy procedures (i.e. opening or rejection of opening due to insufficient assets) Data on *Insolvenzgeld* are collected by the 610 local branches of the Federal Employment Agency (BA). These data have the same unique plant ID that identifies plants in the BHP. One major advantage of these data is that the BA staff is required to actively monitor local bankruptcy processes and to store information on (upcoming) bankruptcies even if there are no applications for *Insolvenzgeld*. We additionally make use of social security announcements, that are legally required if a firm dismisses employees due to its bankruptcy, and of publicly available bankruptcy announcements made by the local courts, but this adds only marginally to the *Insolvenzgeld* data.<sup>16</sup>

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<sup>16</sup> More detailed information on the identification of bankruptcies and several descriptive statistics are provided by Müller and Stegmaier (2015).

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 clustered worker outflow<sup>17</sup> 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 IDs as small exits.<sup>18</sup> 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),<sup>19</sup> i.e. an employment reduction of between 30 and 80 percent within one year in plants that previously had at least 50 employees.<sup>20</sup> 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.<sup>21</sup> We

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<sup>17</sup> 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.

<sup>18</sup> Our robustness checks show that using all small deaths (as in Fackler et al. 2014) makes little difference.

<sup>19</sup> We are grateful to Johannes Schmieder for providing us with the necessary codes.

<sup>20</sup> German law also provides a legal definition for mass layoffs (§17 KschG). Since this event cannot be detected in any data available up to date, scholars typically follow the above mentioned approach, which goes back to Jacobsen et al. (1993).

<sup>21</sup> The events are mutually exclusive with respect to a specific year. Mass layoffs may, however, precede 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

look at plants having the last pre-event observation in the year 2007. Strictly speaking, the event takes place at some point between June 30<sup>th</sup> of 2007 and June 29<sup>th</sup> 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 crisis 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.<sup>22</sup> 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.<sup>23</sup> 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 36,276 closures, 4,767 bankruptcies, and 317 mass layoffs.

We compare the pre-event employment development of plants facing one of the three displacement events in 2007 with a control group of plants that did not experience any of the three events in 2007. The control group may thus contain both plants with the same and other displacement events occurring earlier or later. This is 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 entire 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 insights.

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bankruptcies of the event-cohort 2007 are preceded by mass layoffs occurring in the period of observation between 2002 and 2006.

<sup>22</sup> 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.

<sup>23</sup> 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.

The outcome variables of interest in the following empirical analysis are 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 30<sup>th</sup> (the reference date in the BHP) of a given year but not on June 30<sup>th</sup> of the previous year. Analogously, separations are defined as all workers that were not employed in a given plant on June 30<sup>th</sup> of a given year but on June 30<sup>th</sup> of the previous year.<sup>24</sup> Following previous studies on worker flows, e.g. Burgess et al (2001) or Davis and Haltiwanger (1999), churning (or excess worker flows) is defined as 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 net employment adjustment thereby representing simultaneous job creation and destruction (Davis and Haltiwanger 1999:2717). Following Davis and Haltiwanger (1999:2718f), we calculated symmetric accession-, separation-, and churning rates between two periods  $t$  and  $t-1$ , thus dividing each of the three measures by average employment in  $t$  and  $t-1$ .

## 5. Descriptive Evidence

Table 1 shows some descriptive statistics for the four groups of plants that are included in our analysis (bankruptcies, closures, mass layoffs, and the control group) in the base year 2002. With respect to plant size, it can be seen that bankruptcies are about 60 percent larger than plants in the control group.<sup>25</sup> Closures are smaller than the other three groups of plants whereas plants facing mass layoffs are by far largest, which is not surprising given the definition of mass layoffs described above. Regarding plant age, there are no substantial

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<sup>24</sup> Unfortunately, it is not possible to distinguish between voluntary quits and layoffs.

<sup>25</sup> The fact that bankruptcies are larger than closures is not surprising since only firms with a certain organizational structure, i.e., limited liability or stock corporation, are legally required to file for bankruptcy (see Section 3).

differences between the four groups of plants.<sup>26</sup> A similar picture applies to the sectoral composition. Between 11 (closures) and 21 percent (mass layoffs) of the plants belong to the manufacturing sector. The share of plants belonging to the construction sector is highest for bankruptcies (20 percent) whereas it is very low for mass layoffs with only 3 percent. The share of plants in the service sector is lowest for bankruptcies (62 percent) and between 75 and 77 percent for the other three groups. Worker flow measures are depicted in Table 2.<sup>27</sup> It can be seen that accession, separation, and churning rates are highest for mass layoffs followed by bankruptcies whereas both closures and plants in the control group have comparably low worker flow rates.

Figure 1 depicts the development of plant size relative to the base year. It can be seen that plants in the control group grew continuously during the whole period of observation. One can further see that compared to the reference year 2002, bankrupt plants grew until 2004, had the same employment level as in the base year in 2006, and a strong employment reduction only in the last year. Closures, by contrast, had a rather constant employment level from 2002 to 2004 and reduced their employment level continuously between 2004 and 2007. These employment reductions become increasingly larger as exit approaches, which is in line with previous evidence by Fackler et al. (2014). For mass layoffs, one can see a comparably strong employment increase until 2006, followed by a reduction in the year prior to the event. 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).

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<sup>26</sup> Note that plant age is censored at 27 years. The reason is that for those plants that already existed in 1975, it is not clear whether they were founded in 1975 or earlier. The figures reported in Table 1 might therefore underestimate true plant age.

<sup>27</sup> Note that the base year for the worker flows is 2003 (and not 2002) because these measures refer to the period between two reference dates (i.e. June 30<sup>th</sup> 2002 and June 30<sup>th</sup> 2003). To make sure that we use the same sample of plants for both employment levels and worker flows, i.e., plants that already existed in 2002 but not necessarily in 2001, we have to skip one year at the beginning of the observation period when considering worker flows. The number of plants in Table 2 is slightly lower than in Table 1 because the calculation of the worker flow measures requires that plants are observed in both 2002 and 2003. Plants that do not report having employees on June 30<sup>th</sup> of a given year cannot be included in our flow sample in that year and the following year.

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. Taking this point of view, we find a substantial and long-lasting shadow of death for closures, a moderate shadow of death for bankruptcies (since employment declines substantially only in the last year) and no shadow of death preceding mass layoffs.

The developments of the worker flow rates relative to the base year are depicted in Figures 2-4. In Figure 2, one can see that accession rates decreased by about 20-30 percent for all groups of plants during the period of observation. For the control group, the accession rate remains rather constant from 2005 onwards and it further decreases slightly for closures and bankruptcies. For mass layoffs, there is a small peak in 2006 and a drop in the last year.

Looking at the separation rates relative to the base year (Figure 3), one can see a continuous decline only for the control group. The separation rate for closures is slightly lower than in the base year until 2006 and increases moderately in the last year. The same pattern applies to mass layoffs. For bankruptcies, the separation rate does not differ substantially from the base year until 2006 and in the last year, it rises strongly by about 60 percent.

The churning rates relative to the base year (Figure 4) show rather similar developments for all four groups. Between 2003 and 2007, it decreases by around 20 percent for closures and the control group and by about 10 percent for bankruptcies. For mass layoffs, the churning rate reaches its minimum in 2005 where it is 14 percent lower than in the base year and increases slightly in the next two years. Taken together, the descriptive analysis of the worker flow measures reveals that remarkable differences in the developments between the four groups can be found only with respect to the separation rate, which is in line with the development of the employment levels (Figure 1). However, one has to keep in mind that there are substantial permanent differences in the worker flow rates as shown in Table 2. The

overall picture that emerges from the analysis of levels and developments of the worker flow measures will be discussed below.

## **6. Econometric Analysis**

### **6.1 Estimation Approach**

We aim at assessing differences in the scope for workers' anticipation of upcoming layoff events. Economic literature argues explicitly that workers are lacking credible information about the true economic state of the firm (e.g. Freeman and Lazear 1995). The reason is that firms may conceal this information from workers in order to use it strategically. But anticipation can only be driven by information available to workers and we therefore estimate regression models which include only variables that can be considered both relevant and observable from an individual worker's point of view. 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, respectively. As right-hand-side variables we include year dummies, time-invariant dummies for the three displacement events, and interaction terms between the year dummies and the event dummies. In this regression model, the time-invariant event dummies measures the difference in the base-year (i.e. 2002)<sup>28</sup> between each of the three treatment groups and the 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 indicators describe how differences between control and treatment groups evolve over time. We additionally include dummies for two-

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<sup>28</sup> 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.

digit industries, 30 administrative districts (*Regierungsbezirke*), and nine plant size classes<sup>29</sup> in order to compare affected plants with the average plant within the same industry, region, and size-class (referring to the base-year 2002). Although we think that these control variables are most relevant from an individual worker's point of view, we also conducted a robustness test estimating regressions with plant fixed effects in order to control for all time-invariant plant characteristics and to reduce heterogeneity as much as possible. A major disadvantage of fixed effects estimations is that they do not allow us to identify permanent differences between the four groups but only developments over time. These patterns are, however, almost identical to the results presented below.

## 6.2 Employment Regressions

Estimation results for our employment regressions are presented in Table 3. The coefficients of the event dummies indicate that, in the reference year 2002 (conditional on industry, region, and plant size class), closures are about 2 percent smaller than plants in the control group.<sup>30</sup> Bankrupt plants are about 9 percent larger than plants in the control group and, as expected, plants facing mass layoffs are 25 percent larger than plants in the control group. The regression results in Table 3 further show that plants in the control group grew by about 13 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 the control group. Have bankrupt plants been larger than plants in the control group in 2002, they lost about 11 percent compared to the control group until 2006. The most severe

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<sup>29</sup> 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.

<sup>30</sup> This is computed as  $(\exp(-0.0249)-1)*100\%$ . In the text, all effects of the employment regressions are reported this way.



employment reductions, however, are faced by closures. Here, the relative decline in employment amounts to 18 percent between 2002 and 2006.<sup>31</sup> 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 a reduction in the last year. 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. 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.<sup>32</sup>

The long-run shrinking process of closures without bankruptcy may occur because 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. Moreover, 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 (for a discussion see Müller and Stegmaier 2015). Hence, disinvestment strategies may also happen in the absence of economic difficulties. Contrarily to closures, bankruptcies

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<sup>31</sup> 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 differences are that Dustmann and Meghir (2005) do not 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.

<sup>32</sup> Note that all differences in the developments (i.e. the coefficients of the interaction terms) between the four groups are statistically significant at the 1 percent level.

reduce employment at a much smaller scale. A comparison of the employment development for bankruptcies and closures therefore suggests that the latter group contains planned exits following long-run shrinking strategies while bankrupt plants try to stay in business at a given scale and shut down with a huge employment drop and many unpaid bills.

For mass layoffs, we think that the long run increase in employment and therefore in economic activity cannot be interpreted as warning signals or hints why the growth path of these plants was interrupted later. Potential reasons for this pre-event employment growth could be an increased hiring of workers due to some temporary peak in the plants' order situation or these plants may experience idiosyncratic shocks (e.g. important consumers terminate cooperation), which interrupts the plants' growth process and forces employers to reduce their employment level substantially. Not least because mass layoffs are much more costly for the employer than stepwise employment reductions (e.g. because of social plans, see section 3) 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.

### **6.3 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 4 that both plants facing mass layoffs and bankruptcies in 2007 have a higher accession rate than the control group in the base year (more precisely between the reference dates in 2002 and 2003). The difference is about 8 percentage points for bankruptcies and 23 percentage points for mass layoffs. For closures, by contrast, the accession rate in the base year hardly differs 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, *inter alia*, that employment fluctuations decrease as plants become older (see also the results on separations and churning below). Comparing the developments of the accession rates over time, there are hardly any economically and statistically significant differences between the four groups 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 the control group.<sup>33</sup>

Turning to the separation rate, our results 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 10 percentage points and smallest for closures with two percentage points. 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.<sup>34</sup> 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.

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 Table 3 is achieved with an accession rate (separation rate) comparable

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<sup>33</sup> 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.

<sup>34</sup> At the beginning of the observation period, the developments of the separation rate in the four groups are often not significantly different from each other. At the end of the observation period, the differences become statistically significant except between closures and mass layoffs.

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 regression.

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 (e.g. because of employment protection legislation and voluntary quits), 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 hires, 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. Unfortunately, we are not able to investigate whether 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. The signal is strong because there is no reason why a firm that is going to exit the market soon should continue hiring workers to such a large extent.

The churning rate regressions reveal that, in the reference year, bankruptcies and mass layoffs have substantially higher churning rates than the control group because of their substantially higher accession and separation rates. The difference is about 7 percentage points for

bankruptcies and 28 percentage points for mass layoffs. For closures, by contrast, the churning rate in the base year does not differ between treatment and control group and, thus, there is no indication of management action going against the shrinking process. Looking at the development over time, the churning rate for the control group decreases somewhat. The coefficients of the interaction terms reveal that there are hardly any systematic and statistically significant differences in the developments between the four groups.

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, our results 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 typically yield smaller earnings losses than displacement of high tenure workers, the latter being traditionally in the focus of the job displacement literature. With respect to closures without bankruptcy, it is hard to test whether they are planned, but the fact that the employment reductions in closing plants, in contrast to bankruptcies, do not come along with increased churning points at strategic shrinking rather than a struggle for life followed by an unintended collapse.

## 6.4 Robustness

We have performed 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, which produces almost identical results. 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 the control group to plants that did not experience any of the three displacement events until 2010 which did not alter any of our insights. Third, we additionally controlled for plant size 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 and obtained remarkably similar results. 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 30<sup>th</sup> 2008 and June 29<sup>th</sup> 2009), reveals again very similar results.

Fifth, we restricted the sample to plants with at least 10 employees in 2002 since one might argue that employment growth and worker flow measures cannot be interpreted meaningfully for very small plants. What is more, very small plants might be different from an average plant in many dimensions. This restriction reduced our sample by more than 75 percent. The results of the employment regressions, however, are still very similar with even stronger employment reductions for bankruptcies, in particular in the last year, and much stronger reductions for closures in all years. With respect to the worker flow measures, the differences between closures and bankruptcies become somewhat less pronounced but are still visible. The results for mass layoffs are hardly affected. Sixth, to make sure that the observed

shrinking processes are not driven by plants that were extraordinarily large at the beginning of the observation period and laid off an extremely huge number of workers before finally leaving the market, we dropped all plants above the 95<sup>th</sup> percentile of the 2002 plant size distribution for each displacement event. Running this outlier-test did not alter any of our insights.

Seventh, we ran our regressions separately for the secondary (manufacturing and construction) and the tertiary sector (services).<sup>35</sup> For the tertiary sector we still find the same patterns as in our main specification for each of the three events. The same applies to closures and bankruptcies in the secondary sector. For mass layoffs in the secondary sector we do not find that employment increases prior to the event and there is even an employment reduction in the last year (but much smaller than for closures and bankruptcies). Accordingly, the worker flow patterns for mass layoffs in the secondary sector also differ somewhat. However, one has to note that our mass layoff sample in the secondary sector comprises only 74 plants whereas the respective number for the tertiary sector is 243. In addition, we ran another robustness test excluding the construction sector since large construction sites may be assigned an own plant ID that disappears as soon as construction is finished. However, excluding the construction sector does not affect our results. Taken together, we conclude that our insights are robust over several different specifications and sample restrictions.

## **7. 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

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<sup>35</sup> Investigating the shadow of death with respect to productive efficiency and sunk costs for French firms (but without being able to distinguish between different types of firm exit) Blanchard et al. (2014) find that firm exit in the service sector occurs more suddenly than in manufacturing.

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.<sup>36</sup>

For mass layoffs we see a long run increase in employment and therefore in 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. Closures without bankruptcy reduce the number of workers over a long time span, which implies that the consequences of closures might be heavily underestimated in the public debate as it is often concerned with the final employment levels. 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 to strategic shrinking rather than a struggle for life followed by an unintended collapse. Moreover, leaving the market after repaying debts normally 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 compensate their high separation rate with a high accession rate. Our reading of this result is that these plants try to stay in business at a given scale and shut down with a huge employment drop and many unpaid bills. Taken together, we conclude 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.

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<sup>36</sup> Our results are robust to the choice of the control group and we do not dwell on this distinction in the discussion.



Now what can we learn 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. Detailed worker level analyses of the selection processes coming along with the plant level employment patterns reported in this paper are highly desired and leave room for further research. 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.

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 317 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, as German law prescribes negotiations on a reconciliation of interests and the setup of a social plan in case of mass layoffs (as described in detail in Section 3) firms usually cannot lay off 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. 2010a, 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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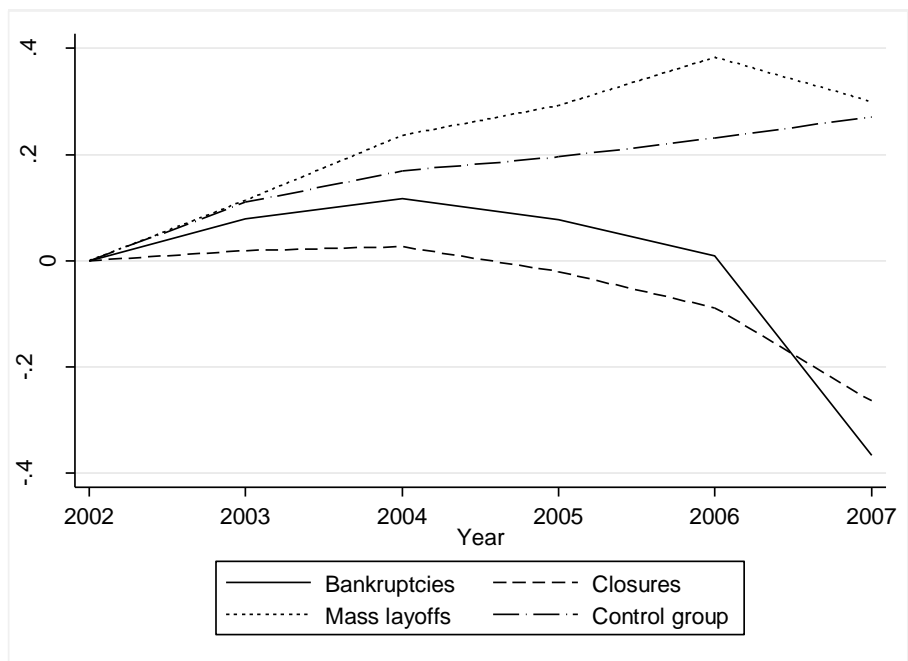
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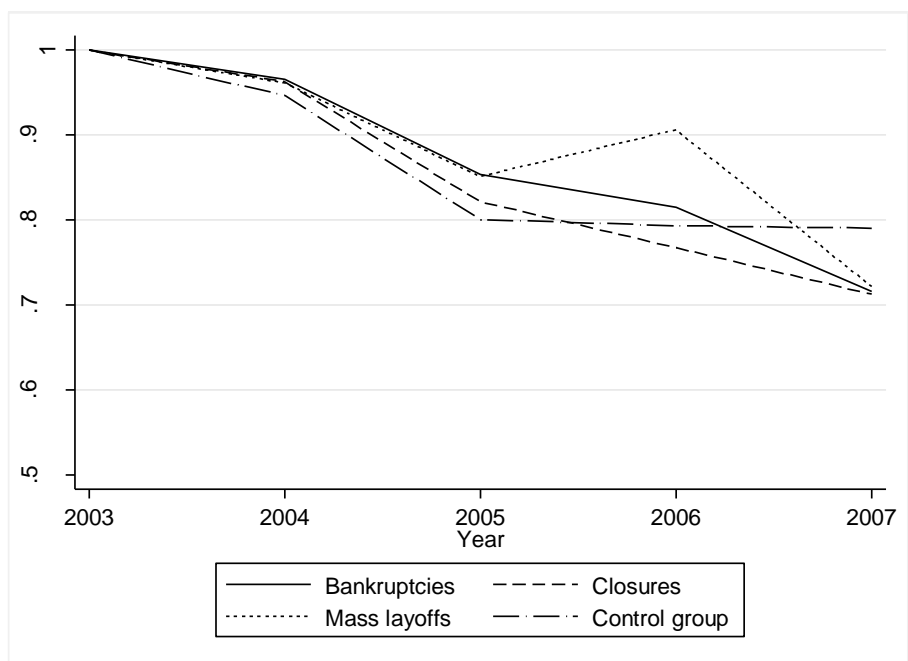
## Figures and Tables

Figure 1) Log number of employees, difference to the base year 2002



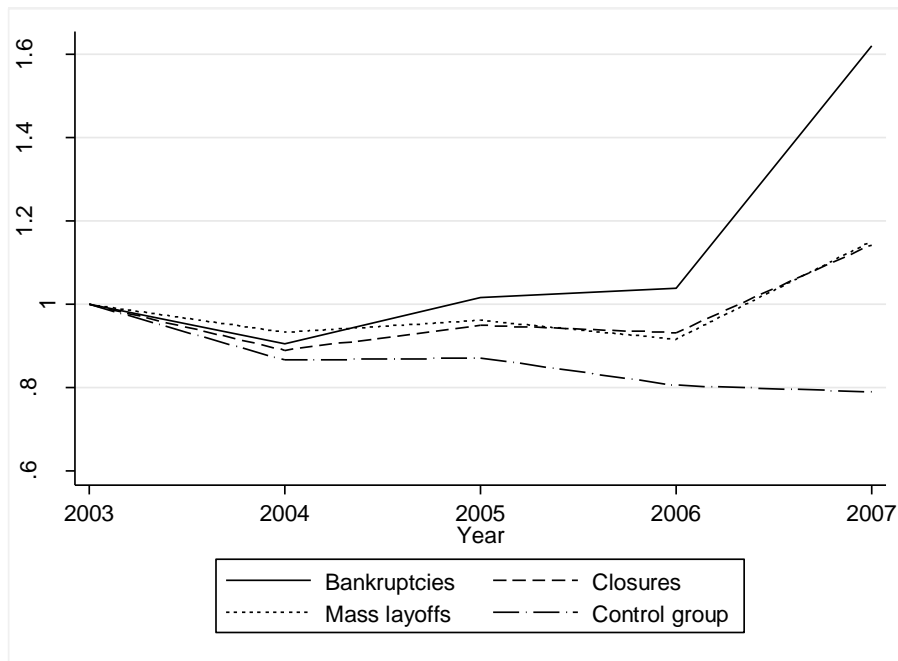
Notes: BHP years 2002-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; plants that already existed in 2002.

Figure 2) Accession rate relative to the base year 2003



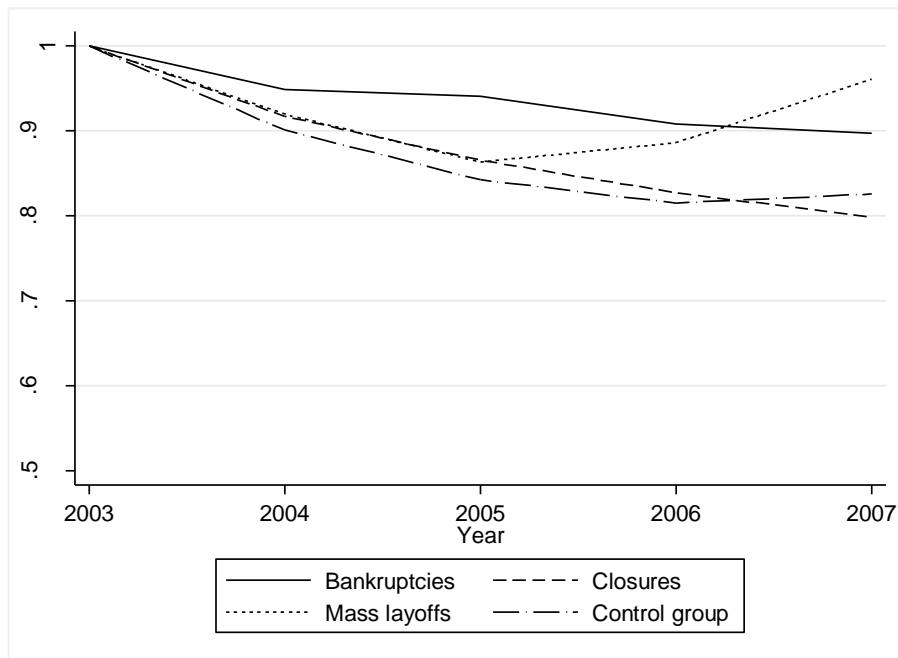
Notes: BHP years 2003-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; plants that already existed in 2002.

Figure 3) Separation rate relative to the base year 2003



Notes: BHP years 2003-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; plants that already existed in 2002.

Figure 4) Churning rate relative to the base year 2003



Notes: BHP years 2003-2007, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; plants that already existed in 2002.

Table 1) Plant characteristics in 2002 (means)

	Bankruptcies	Closures	Mass layoffs	Control group
Log no. of employees	1.8282 (1.2706)	0.9962 (0.9537)	4.3656 (1.0016)	1.3283 (1.2371)
Plant age (years)	10.9432 (9.7525)	11.6708 (9.6805)	12.4953 (10.0981)	11.3869 (9.8102)
Manufacturing (dummy)	0.1771 (0.3817)	0.1093 (0.3120)	0.2082 (0.4067)	0.1342 (0.3408)
Construction (dummy)	0.1995 (0.3997)	0.1168 (0.3212)	0.0252 (0.1571)	0.1168 (0.3212)
Services (dummy)	0.6235 (0.4846)	0.7739 (0.4183)	0.7666 (0.4237)	0.7490 (0.4336)
No. of plants	4,767	36,276	317	1,471,359

Notes: BHP, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; standard deviations in parentheses.

Table 2) Worker flow measures in 2003 (means)

	Bankruptcies	Closures	Mass layoffs	Control group
Accession rate	0.3145 (0.3459)	0.2340 (0.3333)	0.4493 (0.3390)	0.2311 (0.3114)
Separation rate	0.2964 (0.3134)	0.2459 (0.3369)	0.3484 (0.2228)	0.2313 (0.3079)
Churning rate	0.3444 (0.4281)	0.2487 (0.4473)	0.5812 (0.3955)	0.2553 (0.4195)
No. of plants	4,576	34,437	316	1,272,271

Notes: BHP, West Germany, private sector, w/o agriculture, forestry, hunting, and mining; standard deviations in parentheses.



Table 3) Regression results – employment levels

Variable	Log (number of employees)
Bankruptcy (dummy)	0.0884 (15.45)***
Closure (dummy)	-0.0249 (-10.03)***
Mass layoff (dummy)	0.2245 (16.76)***
Year 2002 (reference)	---
Year 2003 (dummy)	0.0372 (94.66)***
Year 2004 (dummy)	0.0775 (159.03)***
Year 2005 (dummy)	0.0845 (153.23)***
Year 2006 (dummy)	0.1027 (169.83)***
Year 2007 (dummy)	0.1255 (194.39)***
Year 2003 × bankruptcy	-0.0008 (-0.11)
Year 2004 × bankruptcy	-0.0004 (-0.04)
Year 2005 × bankruptcy	-0.0404 (-3.94)***
Year 2006 × bankruptcy	-0.1141 (-9.90)***
Year 2007 × bankruptcy	-0.4915 (-31.80)***
Year 2003 × closure	-0.0339 (-13.60)***
Year 2004 × closure	-0.0643 (-20.80)***
Year 2005 × closure	-0.1156 (-32.39)***
Year 2006 × closure	-0.1940 (-49.21)***
Year 2007 × closure	-0.3896 (-83.50)***
Year 2003 × mass layoff	0.0763 (3.50)***
Year 2004 × mass layoff	0.1581 (4.42)***
Year 2005 × mass layoff	0.2080 (5.04)***
Year 2006 × mass layoff	0.2807 (6.25)***
Year 2007 × mass layoff	0.1744 (3.70)***
2-digit industry (dummies)	Included
Plant size in 2002 (dummies)	Included
Region (dummies)	Included
Intercept	0.7233 (227.49)***
R-squared	0.7925
No. of observations	7,241,051

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 4) Regression results – worker flows

Variable	Accession rate	Separation rate	Churning rate
Bankruptcy (dummy)	0.0778 (15.38)***	0.0515 (11.24)***	0.0733 (11.74)***
Closure (dummy)	-0.0045 (-2.49)**	0.0166 (9.23)***	-0.0014 (-0.58)
Mass layoff (dummy)	0.2348 (12.60)***	0.0956 (7.84)***	0.2795 (12.98)***
Year 2003 (reference)	---	---	---
Year 2004 (dummy)	-0.0116 (-33.63)***	-0.0312 (-89.83)***	-0.0257 (-57.49)***
Year 2005 (dummy)	-0.0447 (-132.67)***	-0.0298 (-84.66)***	-0.0407 (-88.49)***
Year 2006 (dummy)	-0.0459 (-134.94)***	-0.0450 (-127.74)***	-0.0477 (-102.78)***
Year 2007 (dummy)	-0.0461 (-134.45)***	-0.0488 (-138.31)***	-0.0450 (-95.60)***
Year 2004 × bankruptcy	0.0011 (0.18)	0.0028 (0.49)	0.0077 (1.09)
Year 2005 × bankruptcy	-0.0012 (-0.20)	0.0348 (5.83)***	0.0204 (2.75)***
Year 2006 × bankruptcy	-0.0121 (-2.02)**	0.0566 (9.13)***	0.0167 (2.22)**
Year 2007 × bankruptcy	-0.0429 (-7.13)***	0.2331 (28.36)***	0.0106 (1.33)
Year 2004 × closure	0.0030 (1.32)	0.0040 (1.74)*	0.0048 (1.67)*
Year 2005 × closure	0.0030 (1.37)	0.0174 (7.37)***	0.0071 (2.39)**
Year 2006 × closure	-0.0084 (-3.88)***	0.0283 (11.76)***	0.0048 (1.60)
Year 2007 × closure	-0.0211 (-9.62)***	0.0842 (31.18)***	-0.0047 (-1.53)
Year 2004 × mass layoff	-0.0057 (-0.33)	0.0079 (0.68)	-0.0208 (-1.43)
Year 2005 × mass layoff	-0.0220 (-1.20)	0.0165 (1.37)	-0.0386 (-2.38)**
Year 2006 × mass layoff	0.0038 (0.21)	0.0158 (1.26)	-0.0185 (-1.10)
Year 2007 × mass layoff	-0.0787 (-4.99)***	0.1014 (7.29)***	0.0222 (1.37)
2-digit industry (dummies)	Included	Included	Included
Plant size in 2002 (dummies)	Included	Included	Included
Region (dummies)	Included	Included	Included
Intercept	0.2190 (199.15)***	0.1913 (177.39)***	0.2094 (138.30)***
R-squared	0.0268	0.0303	0.0321
No. of observations	5,647,163	5,647,163	5,647,163

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.

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