Wage Losses after Job Displacement: Productivity Depreciations or Lost Firm Rents?*

Daniel Fackler,[a] Steffen Mueller,[b] and Jens Stegmaier[c]

– Preliminary draft. Please do not quote –

Abstract

Workers may experience reduced wages after job displacement because of productivity losses or lost firm rents. We disentangle both channels by running worker fixed-effects regressions of wages and firm wage premia, respectively, for workers displaced due to employer bankruptcy compared to a matched control group of non-displaced workers. Premia are measured as firm effects from a two-way fixed-effects approach as described in Abowd, Kramarz, and Margolis (1999). Using German administrative data we find that wage losses are small, fade away after few years, and barely increase with pre-displacement tenure. Losses in firm wage premia are of the same magnitude as overall wage losses, indicating that wage losses are driven by losses in firm rents rather than productivity depreciations.

Keywords: job displacement, returns to tenure, specific human capital, firm rents

JEL Classification: J31, J63, J65

[a] Halle Institute for Economic Research (IWH), Kleine Märkerstraße 8, D-06108 Halle (Saale), Germany, E-Mail: Daniel.Fackler@iwh-halle.de.

[b] Corresponding author, Halle Institute for Economic Research (IWH) and University of Magdeburg, Kleine Märkerstraße 8, D-06108 Halle (Saale), Germany, E-Mail: Steffen.Mueller@iwh-halle.de.

[c] Institute for Employment Research (IAB), Regensburger Str. 104, D-90478 Nürnberg, Germany, E-Mail: Jens.Stegmaier@iab.de.

* The authors thank participants of the research seminars of IAB and Nuremberg University for helpful comments. This study is based on customized data. We therefore thank staff members of the IAB and the German Federal Employment Agency for their indispensable support concerning data provision. We also thank Regina Riphahn, Thorsten Schank, and Richard Upward for valuable comments and Katharina Heisig for excellent research assistance. We gratefully acknowledge financial support by the German Research Foundation (DFG) under the project “Wage and employment effects of bankruptcies”.

1. Introduction

Structural change, import competition, or economic downturns may render formerly viable businesses unviable and cause firms to lay off workers or to close entirely. Previous literature on the costs of job displacement has documented severe long-run earnings and wage losses of displaced workers compared to non-displaced workers (e.g. Jacobson et al. 1993 or Couch and Placzek 2010 for the US, Hijzen et al. 2010 for the UK, Huttunen et al. 2011 for Norway, Schmieder et al. 2010 for Germany). [1] While this has become a stylized fact, our understanding of *why* displaced workers' wages are reduced is fragmentary. Knowledge about the reasons for wage losses is however key to understand consequences of job displacement for affected individuals and the economy and may help to design adequate policy responses.

The most prominent candidate explanation for wage losses after displacement is productivity losses induced by depreciations of specific human capital (e.g. Jacobson et al. 1993: 686). [2] It has been repeatedly shown for the US that wage losses increase sharply with pre-displacement tenure (e.g. Topel 1991), which is generally in line with wage losses being driven by human capital depreciations. Another strand of explanations for wage losses after changing employers considers pre-displacement rents captured by workers. Search models (see Rogerson and Shimer 2011 for a review) and other models of imperfect competition in the labor market, e.g. models of monopsony (Manning 2011) or more general rent-sharing approaches relating firm profits to wages (recently summarized in Card et al. 2016), imply that workers and employers share rents and, hence, that losses after job displacement may to some extent include rent losses. A long-standing body of empirical literature documents wage premia related to firm size or industry (Krueger and Summers 1988, Brown and Medoff 1989,

[1] For surveys of the job displacement literature, see e.g. von Wachter (2010), OECD (2013), or Carrington and Fallick (2014).

[2] Productivity losses associated with the job match (Jovanovic 1979) are an alternative explanation that will be discussed below.

Katz et al. 1989) and more recent literature shows that permanent firm-specific premia are a major component of workers' wages (Abowd et al. 2003, Card et al. 2013, henceforth CHK).

Against this background, we contribute to the literature by exploring the relative importance of productivity-related versus firm wage premia-related explanations. Importantly, pre-displacement tenure and pre-displacement wage premia can be expected to be positively correlated as workers employed by high-paying firms are more likely to stay with these firms. Hence, the frequently reported tenure-loss gradient in wage losses is probably not or only partly driven by human capital depreciations.

Wage losses mainly driven by productivity depreciations are bad news for the economy and reduce productivity gains associated with resource reallocation between firms and sectors (e.g. Davis and Haltiwanger 2014). If wage losses can be attributed to human capital depreciations, public unemployment insurance may increase aggregate efficiency by stimulating specific investments (Acemoglu and Shimer 2000). Contrary if losses in firm premia drive wage losses, these losses are purely private and productivity losses are less of a concern.

We use high-quality administrative German data and are able to utilize a direct measure of plant closure in administrative data whereas previous studies had to rely on worker flows to proxy closures. This measure also enables us to include small firms and thus to provide a more complete picture of job turnover and displacement costs associated with it. We employ an event-study approach to explain wage losses and losses in firm premia, the latter being measured as the firm effect from a two-way fixed-effects approach as described in Abowd, Kramarz, and Margolis (1999, henceforth AKM). In particular, we will investigate whether there are losses in AKM wage premia and compare them to overall wage losses in order to asses to what extent wage losses can be explained by premia losses. What is more, we will investigate whether losses in wages and firm wage premia depend on pre-displacement tenure.

Our results show that wage losses are moderate, amounting on average to four percent in the short run and one percent in the long run. In contrast to earlier US studies, they are only weakly related to pre-displacement tenure. For instance, six years after displacement, the difference in wage losses between low-tenure and high-tenure workers amounts to less than two percentage points and is statistically not significant. Losses in firm rents are also moderate and their magnitude is comparable to overall wage losses, indicating that wage losses after job displacement in Germany can be explained by losses in firm rents rather than human capital depreciations, which implies that workers are reemployed by lower paying firms but without losing productivity.[3] These results cast doubt on the importance of firm-specific human capital in Germany.

2. Theory and evidence

In the following, we will distinguish between two broad channels potentially explaining wage losses after job displacement: tenure-related (productivity) losses and losses in firm rents. We do not further investigate to what extent human capital is specific to tenure in firms, sectors (e.g. Neal 1995, Parent 2000, Dustmann and Meghir 2005), or occupations (e.g. Shaw 1984, Kambourov and Manovskii 2009). Our estimates for tenure-related wage losses may thus be a weighted average of all these different channels. The decisive feature we are interested in and that these channels share is that specific human capital and therefore productivity is destroyed. In the following, we will briefly outline standard theories relating wages to firm tenure and present evidence from papers utilizing job displacement to estimate tenure-related losses.[4] We then continue with presenting the basic concept of wage premia and corresponding empirical evidence.

[3] Heining et al. (2017) is the only study we are aware of that relates displacement costs to losses in employer pay premia as measured by AKM firm effects. They report that pay premia losses are higher in economic downturns and vice versa.

[4] Carrington and Fallick (2014) provide a more in-depth review.

Becker (1962) suggests that workers accumulate human capital that can increase general or firm-specific productivity, the latter being destroyed if the worker-firm match ends. ***Firm-specific capital*** can alternatively be understood as a specific set of general skills in a world where firms differ in the weight they assign to various combinations of skills (Lazear 2009). Regardless of whether workers invest in purely firm-specific human capital or in a specific combination of general skills, firm-specific human capital and firm-specific productivity rise with firm tenure and so do displacement costs.[5]

Deferred compensation as implied by the models of Salop and Salop (1976) and Lazear (1979, 1981) is an alternative explanation for wage losses rising with firm tenure. In these models, employers wish to reduce worker mobility or worker misbehavior, respectively, by offering contracts with steep wage-tenure profiles. Workers are paid below their productivity in the beginning of their career and above productivity in the end. Assuming that human capital is completely (mostly) firm-specific, employers have to bear (part of) the costs of firm-specific human capital investments. As it sets incentives to stay at the firm, deferred compensation can be seen as a pay policy of firms that secures firm-specific human capital investments. Thus, wage losses also reflect productivity losses and implications are similar to those of human capital theory. For our purpose, we therefore subsume them under our concept of tenure-related productivity losses.

Job matching theory provides a competing explanation by assuming that there is a productivity component specific to a certain worker-firm match (Jovanovich 1979). Workers will find better matches the longer they are in the labor market. Matching quality may also increase within a firm because as time goes by and employers gain more insights regarding

[5] In Lazear's model, productivity losses do not only depend on the amount of on-the-job human capital investments but additionally on the thickness of the market for those skills and the idiosyncrasy of the skill combination demanded by the worker's initial firm. The main difference to Becker's model in terms of wage losses following involuntary separation is that workers can use parts of their human capital investments at subsequent employers.

their workers' skills, they can assign tasks that better match their workers' qualifications. High tenure therefore signals a superior match and, again, the implications are similar to that of human capital theory: high-tenure workers lose more productivity after job displacement than low-tenure workers. It is also not unlikely that low-tenure workers even improve their match quality after displacement.

Early papers for the US employ the Displaced Worker Survey to infer returns to tenure from job displacement (e.g. Addison and Portugal 1989a, Kletzer 1989) and show that pre-displacement tenure is valued more in the pre- than in the post-displacement job, pointing to firm-specific productivity components related to tenure. Addison and Portugal (1989b: 289) use a quadratic term in pre-displacement tenure and report that wages decline linearly by 1.6 log points per year of pre-displacement tenure. Topel (1991) also shows that wage losses rise sharply with pre-displacement tenure. In his study, the change in log weekly wages for workers with more than 20 years of tenure is 0.439 whereas it is 0.095 for workers having at most five years of tenure. Farber (1993) estimates that each year of tenure increases earnings losses by one percent and Neal (1995) roughly confirms this for a sample of sector stayers. One major shortcoming of these papers is, however, that the Displaced Worker Survey does not contain a control group of non-displaced workers. Based on the Panel Study of Income Dynamics (PSID) but again abstaining from using a control group, Topel (1990) presents smaller wage losses but still a clear tenure-loss gradient. Based on event-study models with worker fixed effects and a control group of non-displaced workers, Davis and Wachter (2011) report a higher net present value of earnings losses for US workers with six years of pre-displacement tenure compared to three years. Using German data, Orlowski and Riphahn (2009) show that returns to tenure are small, while Dustmann and Meghir (2005) find somewhat larger returns to tenure for young workers.

A second strand of theories is concerned with labor market imperfections as a source of rents. Search models (e.g. Rogers and Shimer 2011) imply that worker mobility is costly for both the firm and the worker. This means that an ongoing employment relationship entails rents. These rents are shared between firms and workers according to the relative bargaining position of both parties. Similarly, theories of monopsony (Manning 2011) assume that employers have wage setting power because labor supply to the firm is not perfectly elastic, the latter being driven by worker preferences and other costs associated with changing employers. Being displaced from an employer with low monopsony power and reemployed by an employer with high monopsony power will lead to wage cuts despite unchanged worker productivity. Note that we will not discriminate between these explanations for rents when speaking about rents as the sources of wage loss.

Empirical literature indeed finds permanent ***firm-specific wage premia*** – i.e. wage differentials for observationally equivalent workers reflecting e.g. efficiency wages or rent sharing – that are usually associated with certain firm characteristics, including size (Brown and Medoff 1989, Oi and Idson 1999) and sector (Krueger and Summers 1988, Katz et al. 1989). If job displacement causes changes in these firm characteristics, workers may lose the corresponding premia. Clearly, firms going bankrupt will typically not be the highest-paying firms (and we test that) but they may still pay some premia. Motivated by the desire to disentangle unobservable worker and firm components in wages, AKM proposed a two-way fixed-effects estimator. This estimator decomposes wages into four components, namely time-varying observable worker characteristics, a worker fixed effect (including both observable and unobservable characteristics that do not vary over time), a firm fixed effect, and a residual, respectively. The firm effect from this decomposition is the systematic part of a worker's wage that is common to all workers of a firm regardless of their individual characteristics.[6] Firms with high AKM firm effects can thus be considered as high-paying firms while those with low firm effects are low-paying firms. Workers losing job in a high-paying firm and being forced to accept a new job in a lower paying firm will experience a wage cut though being as productive as before.

We will use AKM type firm effects as a summary measure for firm rents that may include e.g. effects of size, industry, location, firm performance, and rent-sharing practices or efficiency wages. An extensive literature surveyed in Card, Cardoso, Heining, and Kline (2016) shows that premia are related to firm performance but only to a modest extent. It therefore seems fair to conclude that a considerable component of wage premia is non-competitive in a sense that – comparing firms with similar performance – workers in some firms are able to extract a higher share of rents than equally productive workers in other firms and that these rents are largely unrelated to productivity.[7]

Against the background of the literature outlined in this section, we will now investigate whether there are losses in wages and AKM wage premia and whether they depend on pre-displacement tenure. What is more, we will compare losses in wages and AKM wage premia in order to assess to what extent wage losses can be explained by premia losses and to gauge whether wage losses are accompanied by productivity depreciations or merely reflect losses of unproductive rents.

3. Data and measurement issues

For our empirical analysis we make use of a large administrative linked employer-employee dataset for Germany based on social security records. This dataset combines worker-level information from the Integrated Employment Biographies (IEB) and plant-level information from the Establishment History Panel (BHP). The data are provided by the Institute of

[6] E.g. Card, Cardoso, Heining, and Kline (2016) find that wage premia apply equally to workers with different educational levels within the same firm.
[7] Mueller (2017) provides corroborative results based on German data.

Employment Research (IAB) of the German Federal Employment Agency (BA). For the identification of job displacements, we use novel administrative bankruptcy data, which are described in detail by Mueller and Stegmaier (2015). More detailed information on the bankruptcy data is also provided in the Data Appendix.

The IEB contain daily worker level information on employment subject to social security as well as on benefit receipt, registered job search, and participation in active labor market policies.[8] One major advantage of the IEB is that employers are obliged to report the data, which makes it very reliable. For instance, the data does not suffer from recall bias or (selective) non-response that both often plague studies based on survey data. The IEB provide detailed information on employment biographies over a long time span since employment and benefit receipt are recorded from 1975 onwards.[9] The data further include several personal and job-related characteristics, such as year of birth, education, occupation, and a unique plant identifier.

The BHP contains the entire population of German plants employing at least one worker who is subject to social security. The data are annual and refer to June 30th of each year. They are created by aggregating the underlying social security records from the Employment History (BeH), which is also part of the IEB, at the plant level. The BHP contains information on industry, location, number of employees, composition of the workforce, and wage structure (for more detailed information, see Spengler 2008).[10]

Our main outcome variable will be the log daily wage.[11] As the data lacks information on hours worked, we follow the literature and consider only wage spells of male workers in full-

[8] Self-employed individuals and civil servants are not included in the IEB.
[9] For the description of a two percent random sample from the IEB, the Sample of Integrated Labour Market Biographies (SIAB), see Antoni et al. (2016).
[10] In contrast to the publicly available version of the BHP our data contain all plants rather than a 50 percent sample.
[11] In order to ensure consistency of the worker and plant level data, the wage information in our analysis refers to June 30th of each year. Wages are top-coded at the contribution limit to the social security system. Several

time employment.[12] For the measurement of firm specific wage premia, we make use of firm fixed effects from a two-way fixed-effects model as in AKM provided by CHK (more detailed information is provided in the Data Appendix). The firm fixed effect obtained from a two-way fixed-effects model serves as a proxy for pay differentials that originate solely in plant characteristics but not in worker characteristics. In order to gauge the part of wage losses that is not due to losses in firm wage premia we additionally use the log daily wage minus the respective firm fixed effect as an outcome variable.

One major advantage of looking at job displacements due to bankruptcies is that it allows us to reliably identify displacements from small plants, which is particularly important regarding the question at hand as it has been shown that firm wage premia vary considerably with firm size (as shown by AKM, for example). The bankruptcy data allow us to observe displacement events directly whereas previous studies that made use of mass layoffs or closures had to rely on statistical approximations as described in the following. A mass layoff is typically defined as an employment reduction by at least 30 percent in plants with at least 50 employees (e.g. Jacobson et al. 1993, Schmieder et al. 2010), which excludes displacements from small plants by definition. Plant closures can be identified based on vanishing plant IDs. Since plant IDs can also disappear due to other events, such as changes of ownership or legal form, several studies make use of worker flows to identify ‘true’ closures (e.g. Hethey-Maier and Schmieder 2013 for Germany). The major disadvantage of the worker flow approach is that it cannot be applied meaningfully for very small plants. Bankruptcy information, by contrast, makes it possible to reliably identify displacements from small plants, too.

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researchers solve this problem by applying an imputation procedure (as described by Gartner 2005, for example). As e.g. Schmieder et al. (2010) we abstain from imputing wages as the induced noise is too large in fixed effects estimations. Since non-displaced workers are more likely to be affected by top-coding than displaced workers (around four percent of observations in the treatment group and 15 percent in the control group are affected), wage losses might be understated. Moreover we find that the share of top-coded observations varies hardly with tenure.

[12] Restricting on full-time work is a minor restriction for male workers. In the year 2010, for instance, the average worker in our sample is part-time employed on five out of 365 days.

Moreover, the legal entity to go bankrupt is the entire firm and not only single plants. Observing bankruptcy in our plant-level dataset therefore means that, in the case of multi-plant firms, the entire firm filed for bankruptcy. Previous studies observed plant closure instead of bankruptcy and may therefore in many cases look at multi-plant firms shutting down one or several local production facilities but continue to exist as a firm. This is a potential problem as (part of) the workforce of closing plants may be transferred to other production sites of the very same firm and this may lead to an underestimation of adjustment costs because firm-specific human capital would not be lost if a displaced worker just switches to another plant within the same firm. Data on bankruptcies avoid this potential bias. Another difference between closures with and without bankruptcy is the length of the pre-exit employment decline being much longer lasting in the former (Fackler et al. 2016), which in turn suggests more scope for worker anticipation and strategic behavior in closures without bankruptcy.

The sample used for our analysis contains all male employees who lost their job due to their employer's bankruptcy in 2007 and a ten percent sample of those who are not affected by collective displacements in that year, i.e., workers who are affected by plant closures or mass layoffs[13] in 2007 are also excluded from the control group. We don't use earlier data because the bankruptcy data are incomplete for years prior to 2007 and we don't look at displacements occurring later as we need sufficient post-displacement periods to evaluate long-run effects. From these data we construct a yearly panel. The period of observation comprises the years 2002-2013, i.e., we follow displaced workers five years prior to and six years after displacement. Our treatment group also includes 'early leavers', i.e. workers separating from a plant within one year before the plant's final shutdown. The sample is further restricted to workers in the Western German private sector plants without agriculture and mining (in the

[13] As in Schmieder et al. (2010), mass layoffs are defined as employment reductions by at least 30 percent in plants with at least 50 employees. We are grateful to Johannes Schmieder for providing us with the necessary codes.

year before displacement) who are 20-55 years old and have at least three years of tenure before displacement. The tenure restriction implies that plants have to be at least three years old and further implies that employers had time to learn about worker productivity, which helps to ensure that wages reflect worker productivity. The same sample restrictions are applied to the control group of non-displaced workers.

4. Empirical analysis

4.1 Descriptive statistics

Some descriptive statistics of our sample (referring to the year before displacement, i.e. 2006) are provided in Table 1. With respect to age and working experience there are only minor differences between treatment and control group. It can also be seen that displaced workers have on average lower tenure than non-displaced workers but differences between treatment and control group within tenure classes are small. Displaced workers seem to have on average lower skill levels than non-displaced workers. Looking at average plant size reveals that workers in the treatment group are employed in smaller plants than workers in the control group, which is in line with small firms' higher bankruptcy risk, as reported for example by Mueller and Stegmaier (2015). It can further be seen that high-tenure workers are employed in larger plants than low-tenure workers. This pattern is in line with previous evidence on labor turnover and firm size (e.g. Brown and Medoff 1989 or Winter-Ebmer and Zweimueller 1999), which has revealed lower quit rates in large firms. With respect to the sectoral composition it is worth noting that displaced workers are far more often employed in the construction sector and less often in manufacturing compared to their non-displaced counterparts. Moreover, employment relationships seem to be somewhat more stable in the secondary sector, i.e. manufacturing and construction, than in the tertiary sector as the probability of working in trade, logistics, or other services decreases with tenure. We will check whether our results are driven by sectoral composition or firm size and present corresponding evidence in the next section.

The development of log daily wages by tenure class is depicted in Figure 1. It can be seen that displaced workers have on average lower wages than their non-displaced counterparts, which is in line with the descriptive statistics presented in Table 1, and that wages are higher for high-tenure workers. Moreover, low-tenure workers in both treatment and control group have somewhat higher wage growth than high-tenure workers after the displacement year 2007. The descriptive evidence in Figure 1 further reveals that displaced workers experience a pre-displacement wage dip, something that has also been found in other job displacement studies such as Jacobson et al. (1993). After displacement, interestingly, there are hardly any wage losses for displaced workers.

One concern with job displacement studies in general is that too high pre-displacement wages may both cause job displacement and wage losses after displacement. Against this background, Figure 2 highlights that firm effects are higher for surviving firms implying that while dying firms may pay some premia they certainly pay less premia than the overall market. Claiming that job displacement studies are flawed by excessive wage premia causing displacement and high long-lasting losses seems therefore not to be justified.

As being employed by a high-paying firm increases the likelihood of staying with that firm, it is reasonable to assume that high-tenure workers are more likely to be employed in high-wage firms. Figure 2 illustrates this point. The pre-displacement firm fixed effect rises monotonically with pre-displacement tenure. In the year 2006, for example, displaced workers with more than 10 years of tenure are employed in firms paying on average 14 percent more than firms in the treatment group employing low-tenure workers with less than five years of tenure. After job displacement, this wedge is reduced markedly as low-tenure workers move to higher-paying firms while high-tenure workers accept jobs at firms paying lower premia than their previous employer. In the following, we will investigate these patterns in greater detail in a multivariate setting.

4.2 Econometric approach

In order to investigate displaced workers' losses in wages and firm wage premia compared to the control group of non-displaced workers, we estimate various specifications of the following model:

$$Y_{it} = \alpha_i + \beta X_{it} + \sum_{t=-4}^{6} \gamma_t T_t + \sum_{t=-4}^{6} \delta_t D_i T_t + \varepsilon_{it}$$

In this equation, Y_{it} is an outcome variable of interest, i.e. the logarithm of individual *i's* daily wage in period *t,* an AKM firm fixed effect, or the difference between the two, respectively. The latter represents that part of the log wage that is solely related to (time-varying and -invariant) individual characteristics and reflects the part of the wage that - according to the AKM decomposition - is unrelated to employer characteristics. α_i is an individual fixed effect and X_{it} a vector of time-varying controls including a 4th order polynomial of age. β represents the corresponding vector of coefficients. Further time-varying control variables are not included as one has to make sure that the included control variables are not themselves affected by the treatment (e.g. von Wachter 2010). T_t is a dummy variable representing the year relative to the displacement, which occurs in $t=0$, and the corresponding coefficient vector γ_t measures the development of the outcome variable Y_{it} over time in the control group. D_i is a time-invariant dummy variable that identifies the treatment group and δ_t thus measures the development of Y_{it} in the treatment group relative to the control group. ε_{it} is an idiosyncratic error term.

To make treatment and control group better comparable and to ensure that both groups have similar wage growth paths before displacement, which is crucial to evaluate the causal effect of job displacement, we apply a matching approach, more precisely one-to-one nearest neighbor propensity score matching. For the computation of the propensity score we include CHK firm fixed effects and the difference between log wage and CHK firm fixed effects for the years 2002-2006 as well as various individual and firm-level characteristics referring to the year 2006, including age, experience, tenure, and duration in benefit receipt as 4^{th} order polynomials, dummies for education and occupation (the latter according to the classification by Blossfeld 1987), two-digit industry dummies, a 4^{th} order polynomial of firm size, and dummies for federal states. Balancing tests show that our matching procedure works quite well. The mean of the standardized bias is reduced from 17.2 to 2.3 and the median from 9.8 to 1.3. T-tests reveal that significant differences in the means (at the five percent level) remain for only 16 out of more than 90 variables.

In order to investigate to what extent losses in wages or firm premia vary with pre-displacement tenure we interact T_t as well as D_iT_t with dummies for pre-displacement tenure (measured in the displacement year 2007), i.e. the development of Y_{it} is allowed to vary with pre-displacement tenure in both the treatment and the control group. To put it differently, we compare displaced and non-displaced workers having similar tenure in 2007. The main reason is that wage growth depends on tenure and comparing, e.g. displaced low-tenure workers with non-displaced high-tenure workers is likely to overstate the recovery of the former.

One crucial assumption made in the AKM model is that firm effects are indeed additive and homogenous across all workers employed in the same plant. If firm effects differ within plants by worker skill or tenure, our analysis of CHK effects yields biased results. CHK show that firm effects are indeed very homogenous across deciles of person effects which supports the assumption of additive relative firm effects being unrelated to skill. Similar conclusions are drawn by Card, Cardoso, and Kline (2016) for Portugal. Macis and Schivardi (2016) as well as Bonhomme et al. (2015) also report no or negligible deviations from the simple additive AKM model using samples of Brazilian and Swedish data, respectively.

4.3 Results

Results for log wages without tenure interactions estimated with the model described in Section 4.2 are presented in the first column of Table 2. We find long-run wage losses in the order of 1.3 percent with a 95 percent confidence interval ranging from 0.1 to 2.5 percent. Short-run losses amount to four percent so that there is wage convergence over time. Between the years t-2 and t-1, the treatment group loses 1.6 percent relative to the control group. This negative pre-displacement dip confirms a classic result of previous studies (e.g. Jacobson et al. 1993). However, there are no significant differences between treatment and control group in earlier years, i.e. between t-4 and t-2. Taken together, our results show that overall wage losses are not large.[14]

The results of the specification with tenure interactions are presented in Table 3. The upper part of Table 3 shows the results for workers having at least three (i.e. our minimum tenure restriction) and at most five years of tenure. For these rather low-tenured workers (in our sample the average pre-displacement tenure in the year 2006 is ten years) we find short-term losses of 2.7 percent and no loss after six years indicating substantial recovery in wages. Differences between low-tenure workers and workers having more than five years (starting at five years and one day) and at most ten years of tenure are depicted by the interaction terms of the time dummies and the respective dummy for pre-displacement tenure. Short-term losses are (insignificantly) larger by about 1.4 percentage points and long run losses by 2.1 percentage points. The differences between high- (more than ten years of tenure) and low-

[14] Using mass layoffs instead of bankruptcies and looking on high-tenure workers displaced from plants with at least 50 employees, Schmieder et al. (2010) report losses in the daily wage of about 7 Euros in t+1 and 8 Euros in t+6. If we use wages instead of log wages as the dependent variable we find similar losses workers displaced from plants with at least 50 employees.

tenure workers are very similar, which indicates that losses do not further increase with pre-displacement tenure. These results demonstrate that the relationship between pre-displacement tenure and wage losses is positive but rather weak. This is in line with previous findings by Orlowski and Riphahn (2009) who also report small returns to tenure in Germany.

Workers may gain and lose wage components depending on whether they move to a lower or higher paying firm after displacement. As mentioned above, we use firm fixed effects from a two-way fixed-effects model as described by AKM estimated by CHK. These fixed effects can be interpreted as proportional pay differences between firms. If firm A has a firm fixed effect being ten log points higher than that of another firm B, a worker in firm A earns ten percent more than the very same worker would have earned in firm B.[15]

Results from regressions with firm premia as dependent variable are presented in the second column of Tables 2 and 3, respectively. The results in Table 2 show that on average, workers go to lower-paying firms after becoming displaced. The initial loss in firm wage premia amounts to 3.3 percent which is moderate in absolute terms but substantial compared to the total wage loss of four percent as reported in the first column of Table 2. The loss gradually declines over the years following displacement but there is still a significant difference of -1.2 percent observable in t+6. However, convergence might be partly driven by selectivity in the missing firm fixed effects in later years (as discussed in the Data Appendix) rather than by displaced workers climbing up the premia ladder again.

Table 3 (column 2) presents the corresponding regression results with tenure interactions. For low-tenure workers we find a short-term loss in firm wage premia of 2.7 percent and no significant loss in the long run. Workers having five to ten years of tenure experience similar short- and long-term losses that do not differ statistically significant from that of low tenure

[15] The underlying assumptions and why they can be expected to hold in our data is discussed in detail by Card et al. (2013).

workers. High-tenure workers lose 1.3 (1.6) percentage points in firm rents directly after job displacement (six years after) relative to low-tenure workers but these differences are also insignificant. Compared to the control group the short-run wage losses associated with firm wage premia are four percent for high-tenured workers and their long-run losses amount to around two percent.[16]

Comparing losses in wages and firm wage premia after job displacement (Table 2) reveals that wage losses seem to occur almost fully due to losses in firm wage premia, which is further illustrated by the estimations presented in the last column of Table 2 where we used the difference between the log daily wage and the respective firm fixed effect as dependent variable, i.e. that part of the log wage that is unrelated to firm characteristics and thus reflecting solely characteristics of the individual worker. In particular, it can be seen that wage losses that are unrelated to firm characteristics are very close to zero and never statistically significant. Except for the pre-displacement dip, a similar picture applies to the model with tenure interactions. Although there are some differences visible across tenure groups, they are statistically not significant which again points to rather small returns to tenure and therefore no important role for firm-specific human capital.

These insights still hold when we run various robustness tests.[17] It is sometimes argued that bankruptcies and closures in the construction sector and in some sectors with seasonal demand (e.g. hotels, restaurants, and the tourism industry) are used strategically to not pay workers during months with low demand. We therefore excluded the construction sector and other sectors with seasonal demand in a robustness test, which does not affect our main insights. A similar picture also applies if we restrict our sample to the manufacturing sector. Overall we conclude that our main insights are robust to sector choice. In another robustness test we excluded plants with less than ten workers in 2006 which yields remarkably similar

[16] These figures are obtained by adding up the respective coefficients.
[17] Results are available on request.

results. In order to check whether our results are driven by sectoral or occupational changes after displacement we included dummies for two-digit industries and occupational groups (according to Blossfeld 1987) as time-varying controls, which did not affect our results. In our main specification we considered all individuals as being treated who leave the plant within one year prior to plant closure. Our results hold if we shorten this time frame to six months. Our conclusions are also unchanged if we change the (number of) dummy categories measuring pre-displacement tenure, change our worker age restriction from 20-55 years to 30-50 years, or drop the worker age polynomial.

5. Conclusions

We analyzed reasons for wage losses after job displacement and, in doing so, distinguished between tenure-related explanations and losses in firm wage premia. We argued that tenure-related losses reflect losses in worker productivity, e.g. specific human capital or match-specific productivity. By construction of the Abowd et al. (1999) technique, losses in firm rents do not reflect individual worker productivity. Whether workers lose rents or productivity has important implications for adequate policy responses to job loss and for welfare considerations.

Using high-quality administrative data for Germany we showed that wage losses of full-time employed male workers are moderate on average, amounting to four percent in the short run and one percent in the long run. We further showed that, in line with previous studies for Germany (, Orlowski and Riphahn 2009) but in contrast to earlier studies for the US (e.g. Topel 1991), wage losses are only weakly related to pre-displacement tenure. For instance, six years after displacement, the difference in wage losses between low-tenure and high-tenure workers amounts to less than two percentage points and is statistically not significant. In order to investigate to what extent wage losses are driven by losses in firm wage premia we made use of firm fixed effects from a two-way fixed-effects regression estimated by Card et al.

(2013). We found that losses in firm rents are also moderate and that their magnitude is comparable to overall wage losses. Although pre-displacement tenure and firm wage premia are positively correlated, our multivariate analysis does not reveal that high-tenure workers experience significantly higher losses in firm rents. Taken together, our results indicate that wage losses after job displacement in Germany can be explained by losses in firm rents rather than human capital depreciations.

These results are not in line with theories explaining wage-tenure profiles with specific human capital, match effects, and deferred compensation. That specific human capital and match effects play – if at all – a minor role implies that displaced workers do not lose productivity. A consequence of this finding is that productivity gains that are assumed to be generated when resources are reallocated from failing and shrinking plants to expanding and newly founded plants (Syverson 2011, Davis and Haltiwanger 2014) are unlikely to be cancelled out by productivity losses of workers losing their jobs in the course of economic restructuring. Hence, if low-productivity firms shrink or exit, more job turnover is likely to increase aggregate productivity. What is more, our results cannot empirically support efficiency arguments for unemployment insurance that are based on the notion that unemployment insurance may stimulate specific human capital investments (Acemoglu and Shimer 2000).[18]

In many studies, wage losses are found to be permanent. This is a longstanding puzzle in the job displacement literature as reemployed displaced workers should catch up to non-displaced workers because wages increase faster at the beginning of an employment relationship, which in turn is predicted by human capital theory and a stylized fact in labor economics. One candidate explanation for the non-catch-up is the loss in firm pay premia following job

[18] One may draw this implication from Acemoglu and Shimer (2000) as they look at workers' decisions to accept highly specific and therefore highly productive jobs, which depends on the generosity of unemployment insurance in their model. However, they model specialization of a job with the job's capital intensity. Hence, using the argumentation of Acemoglu and Shimer (2000) to build an efficiency argument for unemployment insurance based on specific *human* capital investments implies assuming that employers' decision to invest in specific *human* capital follows a similar logic than the decision to invest in specific *physical* capital.

displacement since the premia loss is permanent unless displaced workers are able to climb up the pay premia ladder again by switching employers. We find that a total short-run wage loss of four percent for the average worker comes along with a loss in premia of three percent. The wage recovery of around three percentage points over the subsequent five years (relative to the control group) is associated with a recovery in wage premia of two percentage points. Hence, being moderate in absolute terms, the importance of premia losses in explaining total wage losses and wage recovery is substantial. Our findings provide a potential explanation for the long-lasting wage losses frequently reported in the job displacement literature.

Policy makers may further take away from this study that wage losses are attributable to lost premia and are therefore purely private and that there is no reason to expect aggregate efficiency gains from compensating workers for their private losses. However, the social implications of job loss – e.g. effects on health and mortality (e.g. Black et al. 2015, Sullivan and von Wachter 2009) or spillover effects on family members (e.g. Marcus 2013, Rege et al. 2011) – go well beyond productivity losses. What is more, earnings losses do additionally depend on reduced working hours, which is not captured by our study. The lack of evidence for adjustment costs in terms of productivity losses should therefore not be confused with the social costs of job displacement and firm turnover.

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Data Appendix

New bankruptcy data

The bankruptcy data that are used to identify displacements are mainly based on information on *Insolvenzgeld*, a compensation scheme each employee who has not received his wage due to employer bankruptcy is eligible to. These data are collected by the 610 local branches of the Federal Employment Agency (BA) and have the same unique plant ID that identifies plants in the IEB and 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, but this adds only marginally to the *Insolvenzgeld* data. More detailed information on the identification of bankruptcies as well as various descriptive statistics is provided by Mueller and Stegmaier (2015).

In our definition of bankruptcy as a job displacement event we only include bankruptcies for plants that vanish from the BHP, which excludes plants that survived bankruptcy or are still in the bankruptcy process but not yet closed. For vanishing plant IDs with more than ten employees we additionally make use of worker flow information as described in Hethey-Maier and Schmieder (2013) to make sure that employees of these plants do not end up in subsidized employment ('*Auffanggesellschaften*'), which is very unlikely for plants having less than ten employees. More specifically, we consider only 'atomized deaths' (Hethey-Maier and Schmieder 2013), i.e., vanishing plant IDs where less than 30% of the workers end up together under the same new plant ID in the next year.

Firm wage premia

For the measurement of firm specific wage premia, we make use of firm fixed effects from a two-way fixed-effects model (as in AKM) of log wages estimated and provided to us by CHK. More detailed information on the data is provided by Card et al. (2015). Identification of AKM effects requires exogenous mobility between firms. CHK carefully discuss this assumption and present evidence supporting it. In their two-step procedure, CHK first estimate firm effects from a sample of workers who switch plants and control for year effects and age polynomials interacted with educational attainment. They use these estimates to compute the person effects for stayers in a second step. Note that tenure is not among the covariates. If there are returns to tenure it is possible that firm effects are biased upwards for firms experiencing an outflow of high tenured workers. This is because (omitted) returns to tenure reduce the gains (or increase the losses) from changing to a new employer. We do not think, however, that this issue is of prime importance for our analysis of premia losses and pre-displacement tenure for four reasons. First, workers changing plants are mostly young low tenure workers. Second, CHK control for age and therefore should at least partly capture tenure effects. Third, returns to tenure are rather small in Germany (e.g. Riphahn and Orlowski 2009). Finally, even if the firm effect would be biased upwards for a plant with high-tenure worker outflow, the firm effect would be too large for all workers of that plant including low tenure workers and therefore the bias is unlikely to generate a spurious correlation between tenure and premia and, thus, to alter the interpretation of our results. We make use of CHK estimates for the period 2002-2009 and extrapolate them until 2013. There are no estimates available for plants established after 2009, which leads to the exclusion of observations of workers moving to new firms. To gauge the importance of this potential problem for the interpretation of *long run* effects, we report the share of non-missing observations over time by tenure category in Appendix Table A1. Independent of tenure and affiliation to treatment or control group, this share is around 98 percent prior to displacement. Due to switching employers following job displacement, it drops more in the treatment group (84 to 88 percent non-missing in year 2013) than in the control group (91 to 93 percent). As young firms pay on average lower premia – in our sample, plants founded before 2002 pay 13 percentage points higher premia than plants founded later – and because displaced workers are more likely than non-displaced workers to go to a young firm, the somewhat stronger attrition in the treatment group may lead to an underestimation of long-run premia losses after job displacement. Importantly, differences in attrition across tenure groups are small so that comparisons of firm premia loss *differentials* across tenure classes should be less biased by attrition. In any case, long-run effects are less reliable than short-run effects.

In order to be able to compare losses in wages and firm wage premia, the sample that is used for our analyses contains only observations with non-missing firm fixed effects. As this may induce some kind of selectivity, we re-ran our log wage regressions using all observation rather than only those with non-missing firm fixed effects. The overall wage losses in this full sample are somewhat higher, which is not surprising as those plants with missing firm fixed effects are mainly those founded after 2009 and because newly founded firms pay lower premia (as stated above). However, the tenure gradient in wage losses (what we are mainly interested in) remains widely unchanged.

Figure 1: Log wages by pre-displacement tenure

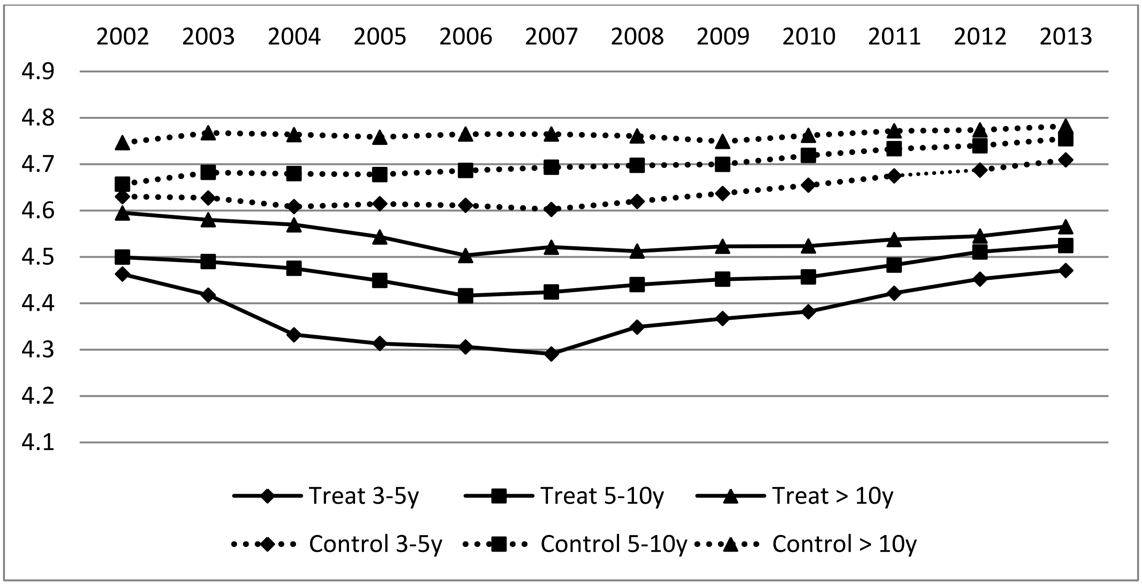

Figure 2: Firm fixed effects by pre-displacement tenure

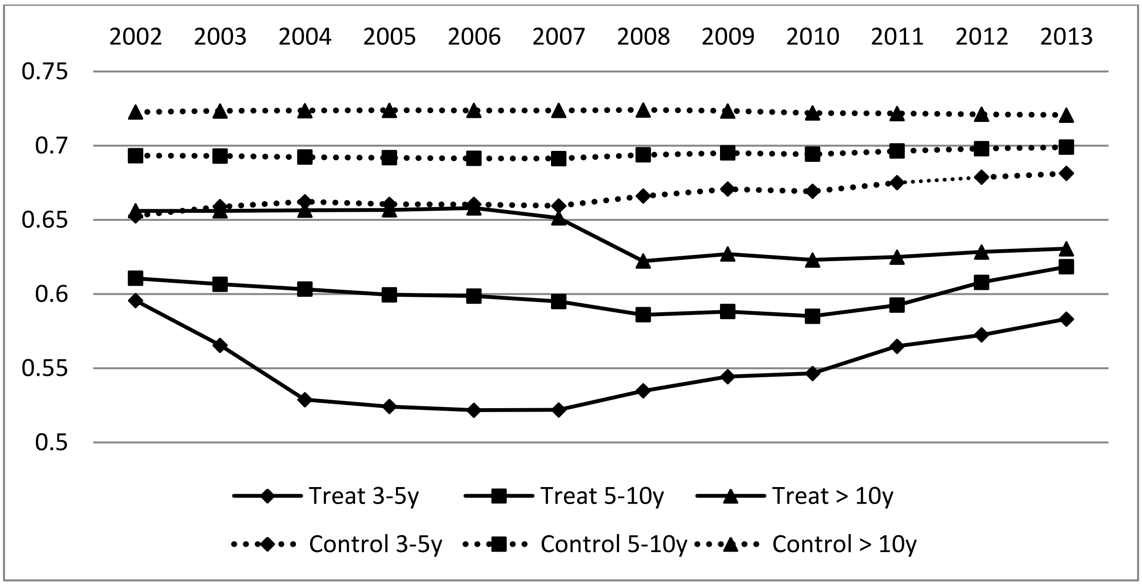

Table 1: Descriptive worker-level statistics (year 2006, standard deviations in parentheses)

	Treatment group				Control group			
Tenure (years)	Full Sample	3 - 5	> 5 - 10	> 10	Full Sample	3 - 5	> 5 - 10	> 10
Age (years)	41.0014 (8.1846)	38.3317 (8.9727)	39.9335 (8.4164)	43.8289 (6.3519)	40.5163 (8.1846)	37.1724 (8.9211)	38.3737 (8.2717)	43.5297 (6.6249)
Experience (years)	16.3442 (8.0324)	12.5957 (8.1760)	14.8714 (7.8070)	20.2884 (6.2745)	16.6468 (8.1296)	12.3811 (8.2040)	13.7758 (7.5368)	20.6020 (6.6183)
Tenure (years)	9.0052 (6.5299)	2.9252 (0.5623)	6.0617 (1.3210)	15.9405 (5.4212)	10.6958 (7.3863)	3.0291 (0.5843)	6.2877 (1.3937)	17.1992 (6.0999)
Low skilled occupation (dummy)	0.4156 (0.4929)	0.3994 (0.4899)	0.3960 (0.4892)	0.4456 (0.4971)	0.3718 (0.4833)	0.3660 (0.4817)	0.3653 (0.4815)	0.3792 (0.4852)
Medium skilled occupation (dummy)	0.4480 (0.4973)	0.4692 (0.4992)	0.4541 (0.4980)	0.4277 (0.4948)	0.4454 (0.4970)	0.4385 (0.4962)	0.4449 (0.4970)	0.4485 (0.4973)
Highly skilled occupation (dummy)	0.1313 (0.3377)	0.1266 (0.3327)	0.1437 (0.3509)	0.1223 (0.3277)	0.1805 (0.3846)	0.1923 (0.3941)	0.1873 (0.3902)	0.1704 (0.3760)
No. of employees per plant	212.8147 (429.0664)	41.8710 (89.6078)	187.9582 (404.0343)	351.6529 (532.0842)	2006.8860 (6528.5580)	791.1887 (3336.2550)	1443.0530 (5117.2030)	2929.3570 (8143.3940)
Manufacturing (dummy)	0.2746 (0.4463)	0.2314 (0.4218)	0.2883 (0.4531)	0.2903 (0.4540)	0.5411 (0.4983)	0.3974 (0.4894)	0.4963 (0.5000)	0.6324 (0.4821)
Construction (dummy)	0.3902 (0.4878)	0.2604 (0.4390)	0.3514 (0.4775)	0.5151 (0.4999)	0.0941 (0.2920)	0.0948 (0.2929)	0.0896 (0.2856)	0.0976 (0.2967)
Trade, logistics (dummy)	0.2405 (0.4274)	0.3314 (0.4708)	0.2547 (0.4358)	0.1656 (0.3718)	0.2107 (0.4078)	0.2750 (0.4465)	0.2337 (0.4232)	0.1674 (0.3733)
Services (dummy)	0.0947 (0.2929)	0.1769 (0.3817)	0.1056 (0.3075)	0.0290 (0.1678)	0.1541 (0.3611)	0.2328 (0.4226)	0.1804 (0.3845)	0.1026 (0.3035)
No. of workers	6,650	1,690	2,442	2,518	468,817	82,521	172,257	214,039

Table 2: Losses in log wages and firm wage premia

Period (relative to displacement)	Log daily wage	Firm fixed effect	Log daily wage minus firm fixed effect
t-4	-0.0011 (0.0037)	0.0000 (0.0012)	-0.0011 (0.0035)
t-3	0.0000 (0.0030)	-0.0002 (0.0015)	0.0002 (0.0027)
t-2	-0.0004 (0.0032)	-0.0001 (0.0015)	-0.0003 (0.0028)
t-1	-0.0164 (0.0041)***	-0.0001 (0.0015)	-0.0163 (0.0039)***
t	-0.0252 (0.0041)***	-0.0051 (0.0019)***	-0.0201 (0.0037)***
t+1	-0.0399 (0.0048)***	-0.0332 (0.0032)***	-0.0067 (0.0038)*
t+2	-0.0351 (0.0050)***	-0.0308 (0.0034)***	-0.0043 (0.0039)
t+3	-0.0325 (0.0053)***	-0.0305 (0.0037)***	-0.0019 (0.0044)
t+4	-0.0220 (0.0056)***	-0.0224 (0.0040)***	0.0004 (0.0047)
t+5	-0.0097 (0.0059)	-0.0143 (0.0041)***	0.0046 (0.0051)
t+6	-0.0132 (0.0060)**	-0.0115 (0.0042)***	-0.0017 (0.0053)
No. of observations	107,626	107,626	107,626
R-squared	0.0163	0.0028	0.0149

Notes: Fixed effects regressions for the matched sample; robust standard errors in parentheses; ***, **, * denotes significance at the 1, 5, or 10 percent level, respectively; outcome difference between treatment and control group is normalized to zero in t-5 (via worker fixed effects) and coefficients show differences between treatment and control group; firm fixed effect as estimated by Card et al. (2013).

Table 3: Losses in log wages and firm wage premia interacted with pre-displacement tenure

Period (relative to displacement)	Log daily wage	Firm fixed effect	Log daily wage minus firm fixed effect
t-4	0.0093 (0.0101)	-0.0011 (0.0070)	0.0103 (0.0084)
t-3	0.0006 (0.0105)	-0.0021 (0.0087)	0.0028 (0.0075)
t-2	-0.0036 (0.0115)	-0.0023 (0.0087)	-0.0013 (0.0082)
t-1	0.0044 (0.0116)	-0.0024 (0.0087)	0.0068 (0.0084)
t	-0.0065 (0.0135)	-0.0062 (0.0091)	-0.0003 (0.0107)
t+1	-0.0266 (0.0136)**	-0.0267 (0.0105)**	0.0001 (0.0096)
t+2	-0.0191 (0.0148)	-0.0298 (0.0113)***	0.0107 (0.0101)
t+3	-0.0156 (0.0149)	-0.0225 (0.0115)**	0.0069 (0.0110)
t+4	0.0004 (0.0160)	-0.0147 (0.0116)	0.0151 (0.0124)
t+5	0.0055 (0.0148)	-0.0116 (0.0118)	0.0172 (0.0113)
t+6	0.0028 (0.0152)	-0.0075 (0.0117)	0.0103 (0.0116)
t-4 * tenure >5-10	-0.0064 (0.0115)	0.0009 (0.0071)	-0.0073 (0.0101)
t-3 * tenure >5-10	0.0069 (0.0113)	0.0010 (0.0087)	0.0059 (0.0086)
t-2 * tenure >5-10	0.0100 (0.0124)	0.0013 (0.0087)	0.0087 (0.0093)
t-1 * tenure >5-10	-0.0106 (0.0140)	0.0015 (0.0087)	-0.0121 (0.0114)
t * tenure >5-10	-0.0154 (0.0150)	0.0003 (0.0093)	-0.0157 (0.0125)
t+1 * tenure >5-10	-0.0135 (0.0158)	-0.0025 (0.0116)	-0.0110 (0.0116)
t+2 * tenure >5-10	-0.0189 (0.0170)	0.0020 (0.0125)	-0.0210 (0.0121)*
t+3 * tenure >5-10	-0.0222 (0.0173)	-0.0052 (0.0129)	-0.0169 (0.0131)
t+4 * tenure >5-10	-0.0263 (0.0186)	-0.0030 (0.0134)	-0.0233 (0.0148)
t+5 * tenure >5-10	-0.0161 (0.0177)	0.0070 (0.0135)	-0.0231 (0.0141)
t+6 * tenure >5-10	-0.0208 (0.0183)	0.0063 (0.0136)	-0.0271 (0.0149)
t-4 * tenure > 10	-0.0180 (0.0115)	0.0013 (0.0070)	-0.0194 (0.0101)*
t-3 * tenure > 10	-0.0079 (0.0113)	0.0026 (0.0087)	-0.0106 (0.0086)
t-2 * tenure > 10	-0.0018 (0.0122)	0.0028 (0.0087)	-0.0046 (0.0090)
t-1 * tenure > 10	-0.0376 (0.0124)***	0.0030 (0.0087)	-0.0406 (0.0095)***
t * tenure > 10	-0.0291 (0.0144)**	0.0014 (0.0093)	-0.0305 (0.0117)***
t+1 * tenure > 10	-0.0182 (0.0149)	-0.0133 (0.0112)	-0.0049 (0.0109)
t+2 * tenure > 10	-0.0195 (0.0161)	-0.0047 (0.0120)	-0.0148 (0.0114)
t+3 * tenure > 10	-0.0184 (0.0166)	-0.0146 (0.0124)	-0.0038 (0.0127)
t+4 * tenure > 10	-0.0276 (0.0176)	-0.0158 (0.0127)	-0.0117 (0.0140)
t+5 * tenure > 10	-0.0202 (0.0172)	-0.0135 (0.0130)	-0.0067 (0.0137)
t+6 * tenure > 10	-0.0179 (0.0173)	-0.0164 (0.0129)	-0.0015 (0.0138)
No. of observations	107,626	107,626	107,626
R-squared	0.0136	0.0060	0.0124

Notes: Fixed effects regressions for the matched sample; reference: tenure 3-5 years; robust standard errors in parentheses; ***, **, * denotes significance at the 1, 5, or 10 percent level, respectively; outcome difference between treatment and control group is normalized to zero in t-5 (via worker fixed effects) and coefficients show differences between treatment and control group; firm fixed effect as estimated by Card et al. (2013).

Table A1: Share of non-missing firm fixed effects

	Treatment group			Control group		
Tenure (years)	3 - 5	> 5 - 10	> 10	3 - 5	> 5 - 10	> 10
2002	0.9538	0.9780	0.9844	0.9649	0.9746	0.9734
2003	0.9724	0.9812	0.9835	0.9788	0.9743	0.9736
2004	0.9821	0.9820	0.9846	0.9861	0.9743	0.9738
2005	0.9845	0.9820	0.9852	0.9851	0.9742	0.9737
2006	0.9820	0.9815	0.9847	0.9846	0.9741	0.9736
2007	0.9617	0.9654	0.9713	0.9837	0.9739	0.9735
2008	0.9657	0.9703	0.9889	0.9817	0.9731	0.9735
2009	0.9686	0.9711	0.9914	0.9807	0.9726	0.9735
2010	0.9316	0.9328	0.9508	0.9602	0.9589	0.9617
2011	0.9039	0.8927	0.9127	0.9423	0.9445	0.9501
2012	0.8662	0.8630	0.8997	0.9263	0.9310	0.9382
2013	0.8380	0.8455	0.8751	0.9100	0.9195	0.9276