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Explaining Wage Losses after Job Displacement: Employer Size and Lost Firm Rents*

Abstract

Why does job displacement, e.g., following import competition, technological change, or economic downturns, result in permanent wage losses? The job displacement literature is silent on whether wage losses after job displacement are driven by lost firm wage premiums or worker productivity depreciations. We therefore estimate losses in wages and firm wage premiums. Premiums 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, on average, fully explained by losses in firm wage premiums and that premium losses are largely permanent. We show that losses in wages and premiums are minor for workers displaced from small plants and strongly increase with pre-displacement firm size, which provides an explanation for the large and persistent wage losses that have been found in previous studies mostly focusing on displacement from large employers.

Keywords: job displacement, wages, firm size, firm rents

JEL Classification: J31, J63, J65

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1. Introduction

Technological 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] These results imply that labor reallocation, e.g. induced by the rise of import competition (Autor et al. 2013, Dauth et al. 2014) or the use of industrial robots (Acemoglu and Restrepo 2017), has substantial adverse effects for affected workers.

Unfortunately, our understanding of *why* displaced workers' wages are reduced is fragmentary, despite decades of high-quality research on this topic. In particular, there is no study testing explicitly whether wage losses reflect losses in firm rents or in worker productivity. If lower post-displacement wages reflect lower worker productivity, potentially positive effects of labor reallocation on aggregate productivity are likely to be reduced, which is not the case if wage losses of displaced workers are due to losses in firm rents. Investigating the reasons for wage losses after displacement is also important because it allows gauging the importance of employer related wage components, thereby shedding light on the wage formation process. Another shortcoming of the literature is that most studies look at displacements from larger employers only, i.e., a group of employers known to invest more in firm-specific human capital (e.g., Barron et al. 1987, Hu 2003) and to pay above-average wage premiums (Abowd, Kramarz, and Margolis 1999, henceforth AKM). Hence, the focus on larger employers is not only yielding an incomplete picture of the true costs of labor

[1] For surveys of the job displacement literature, see, e.g., von Wachter (2010), OECD (2013), or Carrington and Fallick (2017).

reallocation in an economy, it likely overstates them and thereby provides a potential explanation why displaced workers are often found to experience substantial wage losses.

Against this background, we investigate to what extent wage losses after job displacement can be explained by losses in firm-specific wage premiums. We utilize a measure of firm wage premiums that is purged from observed and unobserved differences in worker quality, which allows us to interpret the pre-post displacement difference in the employer wage component as a difference in the firm-specific remuneration policy, including wage premiums associated with firm size or industry, for example. We further estimate wage losses after displacement from large and small employers in order to shed light on whether the high wage losses found in previous studies reflect a firm size wage differential, i.e., whether losses for workers displaced from larger employers occur because they get re-employed by smaller, lower-paying firms.

The most prominent candidate for an explanation of wage losses after displacement are productivity losses induced by depreciations of specific human capital (e.g., Kletzer 1998) or by destroyed job matches that emerged during the employment relationship (Jovanovic 1979). Rent losses, as an alternative explanation, have not been analyzed before but have been mentioned, e.g., in the seminal paper by Jacobsen et al. (1993:686). In addition to more general rent-sharing approaches relating firm performance to wages (recently summarized in Card, Cardoso, Heining, and Kline 2016), an important strand of theories is concerned with labor market imperfections as a source of rents. Search models (e.g., Rogerson 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. An important distinction to the explanations stressing human capital or match productivity is that workers do not lose productivity while losing premiums, which implies different consequences of job displacement and different policy responses. For instance, offering training programs to displaced workers may be more appropriate if displacement causes productivity losses.

Empirical literature finds permanent firm-specific wage differentials that are usually associated with certain firm characteristics, including size (Brown and Medoff 1989, Oi and Idson 1999) and industry (Krueger and Summers 1988, Katz et al. 1989). More recent literature shows that permanent firm-specific wage premiums are a major component of workers' wages (Abowd et al. 2002, Card et al. 2013, henceforth CHK). If job displacement causes changes in firm characteristics, workers may lose the corresponding premium. In particular, it can be regarded as a stylized fact that large firms pay higher wages than their smaller counterparts and that such firm size wage differentials still exist when (observed and unobserved) worker characteristics are taken into account (see, e.g., Brown and Medoff 1989, AKM, Troske 1999, Winter-Ebmer and Zweimüller 1999). Common explanations for these size-wage differentials include poorer working conditions in large firms, i.e., compensating wage differentials,[2] higher efficiency wages in larger firms due to higher monitoring costs, higher surplus generated by larger firms that is shared with employees (e.g., Brown and Medoff 1989, Troske 1999), and monopsony, where the firm size wage differential reflects an upward-sloping labor supply curve (Green et al. 1996). Since the job displacement literature has often focused on displacements from larger employers, an obvious explanation for wage

[2] Brown and Medoff (1989), for example, do not find any evidence that the firm size wage differential reflects a compensating wage differential, and a literature survey by Wagner (1997) also points to superior working conditions in larger firms. Hence, it appears unlikely that working conditions can explain why larger firms pay higher wages.

losses that has not been investigated so far is that displaced workers lose firm wage premiums associated with larger firm size.[3] To fill this research gap, our paper investigates how losses in wages and firm wage premiums are related to employer size.

We use high-quality administrative social security data from Germany and novel administrative bankruptcy data that allow us to directly observe plant shutdowns, whereas previous studies using administrative data had to rely on worker flows to proxy closures or mass layoffs. In contrast to most of the previous literature, this enables us to identify displacements from small plants and thus to provide a more complete picture of job turnover and displacement costs. Including small plants allows us to test whether wage and firm wage premium losses depend on employer size. We employ an event-study approach to explain wage losses and losses in firm wage premiums, the latter being measured as the firm effect from a two-way fixed-effects estimation as described in AKM. In particular, we will investigate whether there are losses in AKM firm wage premiums and compare them to overall wage losses in order to asses to what extent wage losses can be explained by premium losses.

Our results show that wage losses for workers who lost their jobs due to bankruptcies between 2007 and 2009 amount, on average, to six percent in the short run and three percent after five years, which is moderate compared to wage losses found in related studies. The magnitude of losses in firm rents 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, e.g., human capital depreciations, which implies that workers are re-employed by lower-paying employers but without losing productivity.[4] Splitting up the losses by pre-displacement employer size

[3] Krashinsky (2002) compares losses in weekly earnings for laid-off workers and workers displaced via plant closure. Using NLSY survey data on 937 workers, he demonstrates that earnings losses increase with pre-displacement firm size, but he does not distinguish between wage premiums or human capital depreciations driving (size-related) losses.

[4] Card, Cardoso, Heining, and Kline (2016) and Hirsch and Mueller (2017) show that AKM firm fixed effects are only weakly related to firm performance.

reveals that displacement from small firms causes only minor wage losses, whereas being displaced from a larger employer comes along with wage losses of more than 11 percent and even more than 15 percent in large manufacturing plants. Importantly, rent losses increase with pre-displacement plant size as well, and for workers displaced from larger employers, we find that they make up more than two thirds of the total wage loss. The latter suggests that specific human capital plays a minor role in explaining wage losses even for workers displaced from larger plants that typically undertake more investments in specific human capital (e.g., Barron et al. 1987, Hu 2003). We further show that changes in employer size can largely explain the higher losses of workers displaced from larger plants. Analyzing the role of the Great Recession, we find higher wage and firm wage premium losses for workers who lost their jobs in 2008 and 2009 than for workers displaced in 2010, and we show that this can be partly explained by the fact that larger and better-paying firms more often went bankrupt during the crisis.

By analyzing the role of firm rents in explaining the wage losses of displaced workers, we shed light on one of the core puzzles in the job displacement literature, i.e., the lack of earnings recovery of displaced workers even over long time spans. By showing that firm wage premiums earned at the previous employer are an important driver of wage losses and that premium losses are mainly permanent, we are able to explain why wage losses have a permanent component. Our results further imply that structural change may not impose severe burdens on workers if change is hitting mostly small plants and that burdens are higher if larger plants and manufacturing plants are affected. Our work has direct implications for other strands of the literature looking at adjustment costs workers face as a consequence of economic shocks, e.g., the emerging literature on the effects of industrial robots (Acemoglu and Restrepo 2017, Dauth et al. 2017) or import competition (Autor et al. 2013, Dauth et al. 2014).

2. Data and Measurement

For our empirical analysis, we use 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 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 provided in the Appendix.

The IEB contains daily worker-level information on employment subject to social security and on benefit receipt, registered job search, and participation in active labor market policies.[5] One major advantage of the IEB is that employers are obliged to report the data, which makes it very reliable. For instance, the data do not suffer from recall bias or (selective) non-response, both of which often plague studies based on survey data. The IEB provides detailed information on employment biographies over a long time span, since employment and benefit receipt are recorded from 1975 onwards.[6] 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

[5] Self-employed individuals and civil servants are not included in the IEB.
[6] 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).

industry, location, number of employees, composition of the workforce, and wage structure (for more detailed information, see Spengler 2008).[7]

Our main outcome variable will be the log daily wage.[8] Because the data lack information on hours worked, we follow the literature and consider only the wages of male workers in full-time employment.[9] To measure firm-specific wage premiums, we use firm fixed effects from an AKM two-way fixed-effects model, as estimated by CHK (see Appendix for more detailed information). This estimator decomposes wages into four components

$$\log(wage_{it}) = \Psi_{J(i;t)} + x'_{it}\beta + \alpha_i + r_{it}$$

i.e., a firm fixed effect $\Psi_{J(i;t)}$, where J denotes the firm at which worker i is employed in year t; time-varying observable worker characteristics $x'_{it}\beta$, where x_{it} includes year dummies; and a third-order polynomial of worker age interacted with education, a worker fixed effect α_i (including both observable and unobservable characteristics that do not vary over time), and a residual r_{it}, respectively. The firm effect from this decomposition is the systematic part of a worker's wage, which is common to all workers of her firm regardless of their individual characteristics.

[7] In contrast to the publicly available version of the BHP, our data contain all plants rather than a 50 percent sample.

[8] Following CHK, we calculated average daily wages at the main employer for each individual in each year. The main employer is the establishment where an individual received the highest total amount of earnings from full-time employment in a given year. To obtain average daily wages, we summed up all earnings from full-time employment at the main employer and divided it by the number of days spent in full-time employment in the respective establishment. Wages are top-coded at the contribution limit to the social security system. Several researchers solve this problem by applying an imputation procedure (as described by Gartner 2005, for example). Like, 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 (approximately four percent of observations in the treatment group and 14 percent in the control group are affected), wage losses might be understated. In the matched sample used for regression analyses (see below), the share of top-coded observations amounts to approximately four percent in both the treatment and control group. Because the share of top-coded observations also increases with plant size, size-wage differentials might be understated as well.

[9] Restricting on full-time work is a minor restriction for male workers. The share of displaced workers in part-time or marginal employment after job loss is approximately 5.5 percent, and the respective figure for the control group is 2 percent.

CHK-type firm effects are a summary measure for firm rents that may include, e.g., rent-sharing practices, monopsony power, and efficiency wages. They can be interpreted as proportional pay differences between firms. If firm A has a firm fixed effect that is ten log points higher than that of another firm B, a worker in firm A earns approximately ten percent more than the very same worker would have earned in firm B. The underlying assumptions and why they can be expected to hold in our data are discussed in the Appendix.

As stated above, we use novel administrative bankruptcy data to identify job displacements. One major advantage of looking at job displacements due to bankruptcies is that it allows us to observe displacement events directly, which in turn makes it possible to reliably identify displacements from small plants.[10] 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 of 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 in 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 as well.

Moreover, the legal entity to go bankrupt is the entire firm, not just 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

[10] It should be noted that bankruptcy as a reason for job loss does not particularly affect outcomes of displaced workers, since our results for workers displaced from larger plants are very close to those of previous studies making use of mass layoffs (see also Section 3.4)

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 we would not expect any loss in firm wage premiums here. 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 latter (Fackler et al. 2016), which in turn suggests greater 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 between 2007 and 2009. We construct a control group of non-displaced workers for each of the three displacement cohorts by drawing a five percent random sample of all male workers who are unaffected by collective displacements in the respective year, i.e., workers who are affected by plant closures or mass layoffs[11] are also excluded from the control group. We do not use earlier data, because the bankruptcy data are incomplete for years prior to 2007, and we do not look at displacements occurring later than 2009, as we need sufficient post-displacement periods to evaluate long-run effects. From these data, we construct a yearly panel comprising the years 2002-2014, i.e., we follow displaced workers five years prior to and 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 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 it further implies that employers had time to learn about worker productivity, which helps ensure that wages reflect worker productivity. To ensure a close labor market attachment, it is further required that

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

individuals be observed in our data in all five years before displacement. The same sample restrictions are applied to the control group of non-displaced workers.

To be sure, one has to take into account that displaced workers in our sample are likely to be affected by the Great Recession, particularly those displaced in 2008 and 2009. Previous literature for the US has shown that workers who are displaced during recessions face even higher earnings losses (Davis and von Wachter 2011, Couch et al. 2011), which makes the consequences of job loss faced by workers displaced during recessions an even more important issue. We will further address this question below by comparing the wage losses of the 2008 and 2009 displacement cohorts with those of workers displaced in 2010 (not included in our main sample due to the shorter post-displacement period), whose re-employment prospects are arguably unaffected by the Great Recession.

3. Empirical Analysis

3.1 Descriptive Statistics

Some descriptive statistics of our sample (referring to the year before displacement, denoted *t-1* in the following) are provided in Table 1. The respective figures are also reported by four size classes. Pre-displacement plant size is measured three years prior to job loss (i.e., in *t-3*) to avoid the measure being influenced by pre-bankruptcy downsizing (Fackler et al. 2016). With respect to age and working experience, there are only minor differences between the treatment and control group. It can also be seen that displaced workers have, on average, lower tenure and lower skill levels than non-displaced workers. Comparing worker characteristics across plant size classes reveals that workers in larger plants are slightly older and have more working experience and tenure. Workers in larger plants are also more often found in both high- and low-skilled occupations, whereas the share of workers in medium-skilled occupations is higher in small plants.

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). 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.[12] Moreover, the share of workers in the manufacturing sector is larger in the upper size classes, and workers in smaller plants are more often found in the tertiary sector. The literature on the consequences of international trade (Autor et al. 2013, Dauth et al. 2014) and industry robots (Acemoglu and Restrepo 2017) for workers and the literature on sector specific human capital (Yi et al. 2017) are both very much concerned with the fate of manufacturing workers. Therefore, we will also present evidence focusing on the manufacturing sector in the next section.

The development of log daily wages by pre-displacement plant size is depicted in Figure 1a for displaced and in Figure 1b for non-displaced workers. It can be seen that displaced workers in all size classes have, on average, lower wages than their non-displaced counterparts and that wages are higher in larger plants, thus confirming the frequently reported positive relationship between firm size and wages. With respect to wage growth, only minor differences between plant size classes are observable. The descriptive evidence in Figure 1a 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), and that this dip is somewhat more pronounced in larger plants. After displacement, wage losses are only observable for workers displaced from the three upper plant size classes whereas workers displaced from the smallest plants even experience wage gains.

[12] The share of workers displaced from manufacturing plants in the seminal paper by Jacobson et al. (1993) is approximately twice as large (70 percent) as that in our sample (34 percent).

Corresponding evidence on firm wage premiums is presented in Figures 1c and 1d. It can be seen that already before displacement, displaced workers are employed in lower-paying employers than non-displaced workers. One potential objection against job displacement studies in general is that too high pre-displacement wages may cause both job displacement and high wage losses after displacement. The evidence presented in Figures 1c and 1d lends no support to this objection. Descriptive evidence in Figures 1c and d also confirms a positive relationship between premiums and firm size. Moreover, it can be seen that displaced workers' premium losses increase with pre-displacement plant size. In the following, we will investigate these patterns in greater detail in a multivariate regression.

3.2 Econometric Approach

Our empirical strategy combines a difference-in-differences approach with leads and lags, which is the workhorse model of job displacement studies, with propensity score matching. To investigate displaced workers' losses in wages and firm wage premiums compared to the control group of non-displaced workers, we estimate various specifications of the following model:

$$Y_{it} = \alpha_i + \beta X_{it} + \sum_{t=-4}^{5} \gamma_t T_t + \sum_{t=-4}^{5} \delta_t D_i T_t + \varepsilon_{it}$$

In this equation, Y_{it} is an outcome variable of interest, i.e., the logarithm of individual *i's* daily wage in period *t* or a firm wage premium, respectively. α_i is an individual fixed effect (not to be confused with the person-effect from the AKM model described above), and X_{it} is a vector of time-varying controls, including a 4th order polynomial of age. β represents the corresponding vector of coefficients. 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 timeinvariant 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 investigate the extent to which losses in wages or firm premiums vary with pre-displacement employer size, we interact T_t as well as D_iT_t with dummies for pre-displacement plant size to allow the development of Y_{it} to vary with pre-displacement plant size in both the treatment and the control group. To put it differently, we compare displaced and non-displaced workers having worked in a plant of similar size. The main reason is that wage *growth* in the control group may depend on plant size (although descriptive evidence in Figure 1 does not reveal substantial differences). Note that plant size is computed over all workers (except marginally employed workers), i.e., it includes part-time and female workers as well as those who do not meet the sample restrictions. As mentioned above, pre-displacement plant size is measured three years prior to displacement to avoid the measure being influenced by pre-bankruptcy downsizing.

To make the treatment and control groups more comparable and to facilitate both groups having similar counterfactual wage growth paths, which is crucial to evaluate the causal effect of job displacement in our difference-in-differences model, we apply a matching approach, more precisely, one-to-one nearest neighbor propensity score matching separately by displacement cohorts and plant size classes.[13] For the computation of the propensity score, we include log daily wages and firm wage premiums for the periods *t-5* to *t-1* as well as various individual and plant-level characteristics referring to *t-1*. These covariates include: age, experience, tenure, and duration in benefit receipt as 4th order polynomials; dummies for education and occupation (the latter according to the classification by Blossfeld 1987); two-digit industry dummies, a 4th order polynomial of plant size, and dummies for labor market

[13] To ensure that our results for larger plants are not driven by a very few extraordinarily large plants, we excluded the top five percent of the worker-level plant size distribution.

regions (as classified by Kropp and Schwengler 2011). Balancing tests show that our matching procedure works. The mean and median of the standardized bias are below 3 percent in 9 of the 12 subsamples. Only for the largest plant size class are the respective figures greater than 3 percent, but they never exceed 5.1.[14] In Appendix Table A1, which shows means of selected variables for the matched sample, it can be seen that there remain hardly any significant differences between the treatment and control groups after matching, both for the entire sample and within plant size classes.

3.3 Results

Results for log wages (without plant size interactions) estimated with the model described in Section 3.2 are presented in Figure 2 and the first column of Table 2. The wages of displaced workers slightly decrease relative to non-displaced workers before job loss and the difference amounts to a maximum of 0.9 percent in the last year before job loss. This negative pre-displacement dip confirms a classic result of previous studies (e.g., Jacobson et al. 1993) and is rather weak in our sample. We find short-run losses after displacement amounting to six percent in the first year after job loss (i.e., in *t+1*) and long-run wage losses on the order of three percent in year *t+5* so that there is post-displacement wage convergence over time.[15] Our results therefore show that average wage losses are not large compared to those found in previous studies.[16] To inform the literature about the consequences of international trade (Autor et al. 2013, Dauth et al. 2014) and industry robots (Acemoglu and Restrepo 2017), as well the literature on sector specific human capital (Yi et al. 2017), which are both very much concerned with the fate of manufacturing workers, the results for workers displaced from manufacturing plants are presented in Appendix Figure A1. Wage losses after the bankruptcy

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[14] According to Caliendo and Kopeinig (2008), a standardized bias below five percent can be regarded as sufficient.

[15] Note that 93 percent of all displaced workers who become re-employed within the period of observation (79 percent of all displaced workers in our sample) find their first post-displacement job by year *t+1*. Hence, the convergence pattern is not driven by workers returning to work after several years of unemployment or inactivity.

[16] We compare our results with those of related studies below.

of manufacturing plants are considerably higher, amounting to ten percent in the short run and five percent in the long run. Hence, labor reallocation in the manufacturing sector is more expensive for workers.

The results from regressions with CHK firm wage premiums as the dependent variable are presented in Figure 2 and the second column of Table 2. The results show that, on average, workers go to lower-paying firms after becoming displaced. The initial loss in firm wage premiums amounts to six percent, and the long-run loss is approximately four percent. Hence, premium losses are very similar to overall wage losses. One major contribution to the literature is therefore that premium losses, on average, fully explain wage losses after employer bankruptcy in Germany. The pattern is very similar in the manufacturing sector (Appendix Figure A1), where the premium loss amounts to 8.5 percent in the short run and six percent in the long run.

The role of employer size

We document above that wage losses are moderate on average, which seems to stand in some contrast to the overall perception in the literature reporting substantially higher losses (e.g., Jacobsen et al. 1993, Couch and Placzek 2010, Schmieder et al. 2010). We now focus on pre-displacement plant size to provide first evidence for the wage loss after being displaced from small plants and to uncover whether the abovementioned discrepancy with previous studies regarding the magnitude of wage losses can be explained by those studies focusing on large employers only.

We present results for four broad size categories, i.e., plants having 1-9, 10-19, 20-99, and more than 100 employees, respectively. Our results in Figure 3 and the first column in Table 3 show a monotone and steep size gradient in wage losses. Short-run losses for workers displaced from plants with 100 or more employees are approximately three times larger than losses after displacement from small plants with less than ten employees, which corresponds to losses of 11.3 versus 3.8 percent, respectively. The manufacturing sector again stands out with higher losses, but it has a similar size gradient in wage losses, ranging from 15.7 to 5.7 percent in the year after job loss (Appendix Figure A2). Importantly, even five years after displacement, the size gradient in wage losses is still substantial.

This positive relationship between wage losses and pre-displacement employer size may originate from size-specific wage premiums or stronger investments in firm-specific human capital in large plants (Barron et al. 1987, Hu 2003). Descriptive evidence in Figure 1 clearly hints at larger plants paying higher premiums and on displacement from larger plants causing higher premiums losses. Employing our standard fixed effects regression framework with plant size interactions, we demonstrate in Figure 4 and Table 3 (second column) that losses in firm wage premiums indeed increase with pre-displacement plant size. Workers displaced from plants with less than ten employees experience short-run premium losses of 3.2 percent, whereas the respective figure for the largest plants amounts to 8.7 percent. Losses in manufacturing (Appendix Figure A3) are again more pronounced, amounting to 5.3 and 11.2 percent, respectively. Long-run premium losses amount to 1.8 versus 7.0 percent in the full sample and to 2.9 versus 8.4 in manufacturing.

The results reported in Table 3 can also be used to gauge how much of the wage loss is explained by wage premiums in small versus large plants. Workers displaced from large plants experience wage losses of 11.3 (7.6) percent in the short (long) run, of which losses in premiums make up for 77 (92) percent. In small plants, the premium losses make up for 84 percent of the total short-run wage loss, whereas in the long run, the premium loss slightly exceeds the wage loss. In the manufacturing sector, wage losses are fully explained (or even exceeded) by premium losses in small and medium-sized plants, whereas wage losses slightly exceed premium losses in large plants but premium losses still make up more than two thirds of total wage losses. One immediate conclusion from these findings is that the strong positive relation between long-run wage loss and pre-displacement employer size can be best explained by wage premiums and less by human capital depreciations rising with pre-displacement employer size.

We now take a deeper look into whether this size-loss gradient in firm wage premiums can be explained by changes in employer size before versus after displacement by additionally controlling for plant size in our size interacted regression. For this purpose, we include dummy variables for nine plant size classes as time-varying controls. The results for firm wage premiums – presented in Figure 5 – show that the strong size-loss gradient presented in Figure 4 is sharply reduced when actual plant size is controlled for. However, the difference between the smallest and the largest plants stays statistically significant.[17]

Part of this convergence in premium loss patterns when controlling for actual plant size may be driven by workers being displaced from large plants downgrading in size, but the opposite may also be at work. Appendix Table A2 supports this conjecture:[18] half of the workers displaced from large plants move to smaller size classes, while half of the workers displaced from small plants actually upgrade in employer size.[19] To round out the picture, we directly show the premium losses for workers coming from large versus small plants split up by all four possible destination size classes, the latter being measured in *t+1*. Appendix Figure A5 shows that premium losses are indeed decreasing with destination employer size.[20] Interestingly, workers who become re-employed in smaller plants in the year after job loss have persistently higher premium losses over the entire period of observation. Workers

[17] Note that the upward peak for large firms in the displacement year visible in Figure 5 can be explained by plants moving to a lower size class (i.e., downsizing) while – by construction – leaving premiums unchanged. Due to size-premium differentials, premiums are then relatively high compared to other plants in the lower size classes.

[18] For this analysis, we compared pre- with post-displacement plant size, the latter being measured in *t*+1. We therefore considered only displaced workers who are re-employed by *t+1*.

[19] Krashinsky (2002:90) reports similar patterns for the US.

[20] For completeness, Appendix Figure A4 shows the corresponding wage losses.

displaced from large plants but staying in large plants face much lower premium losses than those moving to smaller plants. Accordingly, workers displaced from small plants lose generally less and may even gain substantially by moving to large employers.

To sum up, we documented in detail that workers displaced from larger plants have higher wage losses *because* they move to smaller employers and lose firm wage premiums being strongly associated with the gap in employer size between their pre-displacement and post-displacement employers.

The persistence in wage losses

In many studies, wage losses after displacement are found to be permanent. This is a longstanding puzzle in the literature, as re-employed displaced workers should catch up to non-displaced workers, because wages typically increase faster at the beginning of an employment relationship (e.g., Dustmann and Meghir 2005). One very intuitive candidate explanation for the non-catch-up is the loss in firm pay premiums following job displacement, since the premium loss is permanent unless displaced (non-displaced) workers climb up (fall down) the pay premiums ladder via subsequent employer changes. Recall that we do not restrict the control group to stay with the same employer after year *t*, which opens up the full range of labor market transitions from *t+1* onwards, including, e.g., displacement or on-the-job search.

Approximately 93 percent of displaced workers in our sample who become re-employed within the period of observation started their first post-displacement job already by the end of the year following displacement. The share of re-employed workers changing post-displacement employers remains very high in the treatment group, with, on average, approximately 20 percent in each of the years following displacement. In the control group, less than ten percent of workers change employers, and this figure is constant over time.

Convergence in wage premiums would require either displaced workers climbing up the ladder or non-displaced workers falling down. We show in Figure 1d that non-displaced workers barely lose premiums over time, and thus, any convergence must come from displaced workers climbing up. Figure 1c shows that workers displaced from small and medium-sized plants indeed slightly catch up over time and that the implied convergence amounts to approximately two percentage points. Workers displaced from large plants re-enter the labor market in better-paying plants than their small-plant counterparts but experience no premium increases over time. Averaged over all pre-displacement size classes, displaced workers gain 2.2 percentage points in premiums between *t+1* and *t+5*, while non-displaced workers gain only 0.4 percentage points in the same time interval.

Overall, our findings are very much in line with those of Jung and Kuhn (2017), who also show a lack of downward convergence for the control group and only mild upward convergence after displacement in the US. Moreover, their finding of very strong losses concentrated among a rather small group of workers fits well with our results presented in Appendix Figures A4 and A5, where we show that workers moving from large to small employers face strong losses, whereas other groups, particularly workers displaced from very small plants, experience only minor losses, or even gains, after displacement. Beyond that, we add to Jung and Kuhn (2017) by showing that what they call the 'job ladder' might as well be characterized as a 'premium ladder' in Germany, since the mild convergence in wages is driven by the premium gains of displaced workers.

The impact of the 2008/2009 global crisis

Because our main sample comprises the years of the global financial crisis 2008/2009 and because we have additional data on the first post-crisis year 2010,[21] we are able to test

[21] The year 2010 is not included in our main sample, as our data allow us to follow these workers for only four years post-displacement.

whether wage and premium losses are different in crises. Confirming stylized results for the US (e.g., Davis and von Wachter 2011), we find that workers displaced in 2008/2009 face larger losses than workers displaced in 2010. The difference in the *t+1* (*t+4*) wage loss is 3.9 (3.1) percentage points (Appendix Table A3).[22] Premium losses for workers displaced in 2010 are also smaller than for those who lost their jobs during the crisis, but for both periods, we find that wage losses occur because workers get re-employed in lower-paying firms.[23]

One potential explanation for business cycle effects in displacement costs is that having to look for a new job is difficult during recessions and that workers displaced during recessions therefore accept lower entry wages. However, differential losses may also occur due to a different composition of firms going bankrupt during recessions, which may lead to different premium losses compared to firms failing during booms. In normal times, it is mostly small firms that go out of business (Fackler et al. 2013, Mueller and Stegmaier 2015), while poorly performing larger firms may downsize and survive. During a severe recession, however, larger firms may be forced to go out of business entirely as well.

We show in Appendix Figure A6 that the share of workers losing their jobs due to bankruptcies of *larger* firms during the 2008/2009 crisis is indeed larger than after the crisis. Correspondingly, we find that pre-displacement premiums are higher by 3.8 percentage points in the 2008/2009 cohort (Appendix Figure A7). Because we also find that the 2008/2009 displacement cohort actually re-enters the labor market in higher-paying firms compared to workers displaced in 2010 (the difference in post-displacement premiums in the year after job loss is still 1.2 percentage points), workers displaced in 2008/2009 lose more premiums because of their higher pre-displacement premiums and not because of the limited availability

[22] We could compare the 2008/2009 recession with 2007 as well. However, the problem would be that workers displaced in 2007 would probably search for a job in the crisis and are therefore affected by it.

[23] We also find for the 2010 cohort that wage and premium losses strongly increase with pre-displacement plant size and that the size differential in wage losses can be explained by higher premium losses of workers displaced from larger plants. This suggests that our main insights are not driven by the observation period covering the Great Recession.

of high-paying employers post-displacement. This implies that higher pre-displacement premiums during the 2008/2009 recession are an additional explanation for higher wage losses in recessions. Of course, we do not know whether this holds during other recessions as well.

3.4 Comparison with other studies

Most job displacement studies report earnings losses instead of wage losses. Notable exceptions are Krashinsky (2002) and Stevens (1997) for the US, as well as Burda and Mertens (2001), Bender et al. (2002), and Schmieder et al. (2010) for Germany.[24] Burda and Mertens (2001) estimate displacement probabilities using German survey data and use these estimates to impute displacements in social security data. They report rather small losses in wage growth of approximately four percent. However, due to the unavoidable precision problems associated with their imputation method, their estimates are likely biased toward zero. Also using social security data, Bender et al. (2002) find wage losses between one and two percent, on average. But because they use only disappearing plant IDs to identify job displacements, wage losses are likely to be understated in their study, since plant IDs vanishing from the data do not necessarily reflect true closures (e.g., Hethey-Maier and Schmieder 2010). Schmieder et al. (2010) analyze mass layoffs from large employers during the 1982 crisis in Germany using the same data source as we do and report five-year wage losses of eight Euros, which corresponds to roughly ten percent of the average wage level in the sample and thus perfectly matches wage losses for workers displaced from large plants in our study. Since Schmieder et al. (2010) used the same mass layoff definition as classic job displacement studies, such as Jacobson et al. (1993), the similarity with our results suggests

[24] Hijzen et al. (2010) do not report average wage losses for their sample but show that five-year losses are higher for workers displaced during a mass layoff compared to a plant closure and for those re-entering the labor market later. However, the number of displaced workers is small in their wage sample and the coefficients are often imprecisely measured.

that bankruptcy as an event per se does not affect the magnitude of wage losses compared to the classic approach.

Pooling short- and long-run effects and without controlling for worker fixed effects, Krashinsky (2002:92) reports wage losses of 11 percent for young low-tenure workers. This figure is substantially larger than the losses we find. Stevens (1997) reports a ten percent wage reduction for male workers averaged over one to five years after displacement via plant closure. Comparison is difficult, as neither Krashinsky (2002) nor Stevens (1997) report information on the pre-displacement sector and as Stevens (1997) does not show the pre-displacement employer size. The earnings losses reported by the two very prominent US studies of Jacobsen et al. (1993) and Couch and Placzek (2010) are larger than the wage losses in Krashinsky (2002) and Stevens (1997). This difference may be caused by long-run (percentage) wage losses being smaller than earnings losses (due to reduced employment), by firms being larger in the former two studies, or by both studies looking at time periods with a strong contraction of manufacturing industries in the federal states under observation (steel industry in Jacobsen et al. 1993 and shipbuilding in Couch and Placzek 2010). The latter can be expected to result in higher wage losses, as the transferability of skills across sectors is limited (e.g., Neal 1995 or Yi et al. 2017). We conclude that our results fit well with previous studies using German data and that wage losses seem to be smaller in Germany compared to the US. Among other factors, the latter is likely driven by the vast majority of German men holding an occupational degree certifying human capital that is easily transferable across employers (Acemoglu and Pischke 1998) and by the generally lower wage dispersion in Germany.[25] Moreover, the fact that collective agreements at the sectoral level are more

[25] In 2012, for example, the ratio of the 90^{th} and 10^{th} percentiles of the wage distribution was 4.2 in Germany and 4.8 in the US (OECD 2015).

prevalent in Germany might also serve as an explanation for overall lower wage losses than in the US.[26]

4. Conclusions

We analyzed to what extent wage losses after job displacement can be explained by losses in firm-specific wage premiums, the latter being measured as firm-fixed effects from an AKM model. By construction of the firm fixed effects, losses in firm wage premiums do not reflect, for instance, losses in individual worker productivity induced by firm-specific human capital becoming worthless. Whether workers lose premiums or productivity has important implications for adequate policy responses to job loss. Our second contribution is to analyze job displacements from small plants. Newly available data on employer bankruptcy enabled us to fill this important gap in the literature using high-quality administrative data. Strong evidence on firm-size wage differentials in previous studies makes pre-displacement employer size a potentially very important determinant in wage and premiums losses, and we therefore investigated whether workers face losses in wages and firm wage premiums because they get re-employed by smaller and lower-paying employers.

We showed that the wage losses of full-time male workers are moderate on average, amounting to six percent in the short run and three percent in the long run. Importantly, the magnitude of losses in firm rents is comparable to overall wage losses. Our first major insight is therefore that wage losses after job displacement in Germany reflect losses in firm rents rather than productivity losses of workers. Arguably, two features of the German labor market can be expected to be responsible for this pattern. First, the vast majority of men hold an occupational degree certifying human capital, thereby making it easily transferable across employers (Acemoglu and Pischke 1998). Second, there is firm-specific variation in the

[26] As a case in point, Janssen (2017) shows that the decentralization of wage bargaining in the Danish manufacturing sector lead to higher income losses of displaced workers.

incidence of bargaining institutions, i.e., works councils and collective wage bargaining (Dustmann et al. 2014), that are directly related to differences in wage premiums across firms (Hirsch and Mueller 2017).

The observed dominance of employer-related over worker-related explanations for wage losses is not in line with theories highlighting firm-specific human capital or match effects, and it 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 being relocated in the course of economic restructuring. Hence, if low-productivity firms shrink or exit, worker relocation is likely to increase aggregate productivity.

Our second major finding is that workers displaced from larger plants have much higher wage losses *because* they move to smaller plants after job loss, and they lose firm wage premiums that are strongly associated with the gap in plant size between their pre-displacement and post-displacement employers. Because the vast majority of previous job displacement studies use mass layoffs from large employers, our findings first and foremost imply that much of the previous literature has overestimated wage losses after job displacement by focusing on a subgroup of displaced workers facing particularly high wage losses. In Germany, the firm size differential in wage losses is enormous: short-run wage losses for workers displaced from plants having 100 or more employees are approximately three times larger than losses after displacement from plants with less than ten employees. Five years after displacement, workers displaced from large plants still suffer substantial losses, whereas workers affected by a small plant's bankruptcy face no significant wage losses compared to a non-displaced control group. Our finding that wage loss differentials by employer size mostly mimic premium loss differentials is in line with the literature relating firm size wage differentials to efficiency wages, monopsony, or rent-sharing practices.

In many studies, wage losses are found to be permanent, and this is a longstanding puzzle in the job displacement literature. One very intuitive candidate explanation for the non-catch-up is the loss in firm pay premiums following job displacement, since the premium loss is permanent unless displaced workers are able to climb up the pay premium ladder again by switching employers. We find that displaced workers indeed show a higher probability of switching employers conditional on re-employment after displacement but that gains in premiums are small. This leads to long-lasting wage losses because, at the same time, non-displaced workers rarely lose wage premiums over time. Our findings support the results of structural search and matching models, as in Jung and Kuhn (2017), but add to them by showing that what they call the 'job ladder' might be characterized as a 'premium ladder' in Germany.

Wage and premium losses are highest for workers displaced from large plants, particularly from large manufacturing plants. This has implications for the recent literature on the distributional consequences of important competition (Autor et al. 2013, Dauth et al. 2014) and labor-saving technological change (Acemoglu and Restrepo 2017, Dauth et al. 2017), both focusing on manufacturing workers. First, import competition and labor-saving technological change cause enormous wage losses, because manufacturing jobs *ex ante* enjoy substantial wage premiums the average displaced worker cannot recoup in the next-best job. Second, wage losses in local labor markets with initially small employers are much smaller because premiums have already been low prior to the shock.

Policy makers may take away from this study that wage losses attributable to lost wage premiums are purely private and that – other than in the case of depreciations of specific human capital – there is no reason to expect aggregate efficiency gains from compensating workers for their private losses (Acemoglu and Shimer 2000). Because wage losses are not rooted in lower worker productivity, retraining measures will have a very limited reach in helping displaced workers. If policy makers aim to buffer losses after displacement, e.g., due to fairness considerations or because job displacement induces welfare costs due to incomplete insurance against labor income risk (Rogerson and Schindler 2002), they should focus on workers displaced from larger employers (and the manufacturing sector), as workers displaced from small plants in many cases experience wage gains if they are able to find employment in larger and better-paying firms. Because most workers lose wage premiums because they move to a smaller employer after displacement, policy makers aiming to reduce losses may, for instance, consider changing the bargaining positions of workers in small plants, e.g., by increasing the incidence or the power of worker representations (Hirsch and Mueller 2017). Moreover, workers displaced during a crisis seem to lose more firm wage premiums because the plants from which they are displaced are larger. This implies that generally conditioning aid on minimum pre-displacement firm size might also supplement particular political action in recession periods. However, the proponents of such a policy should be aware that the workers losing most have *ex ante* been the lucky ones, having been employed by high-paying firms, and after displacement, they typically still work at better-paying firms than workers displaced from low-paying employers.

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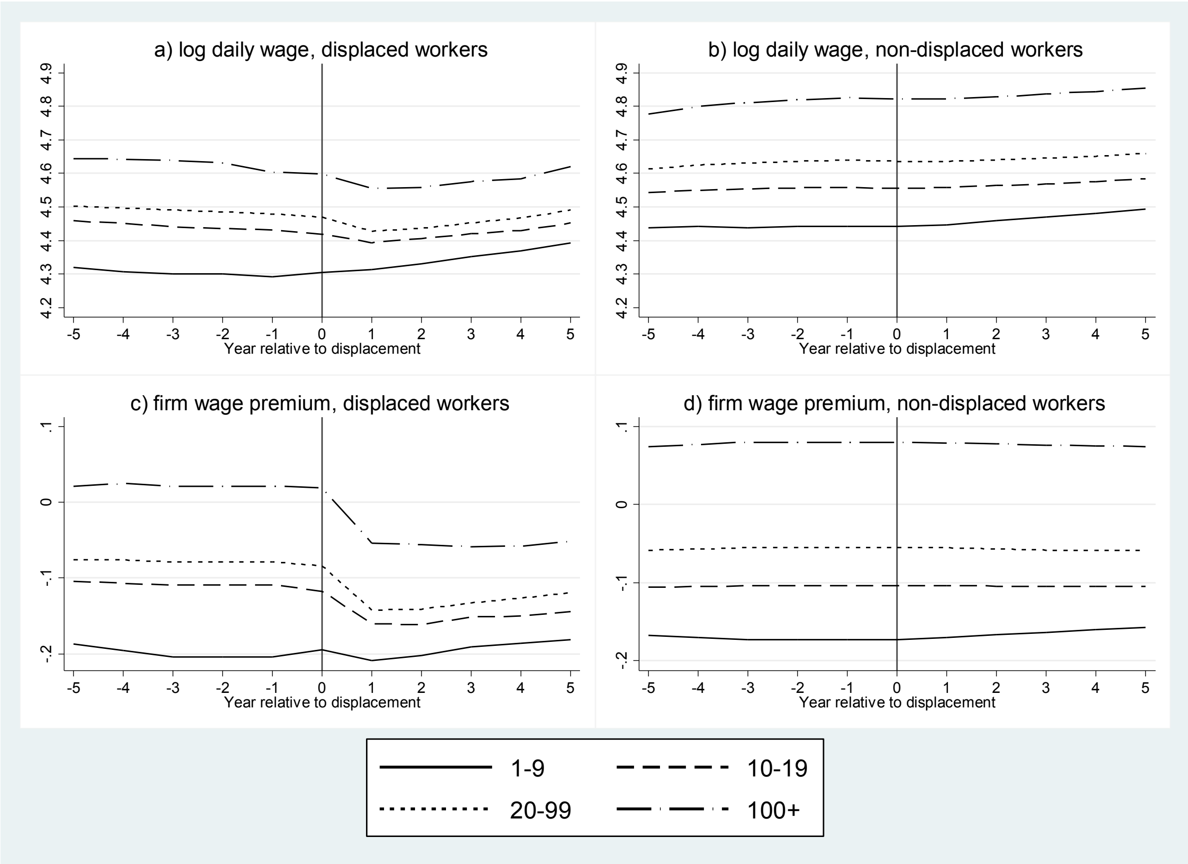
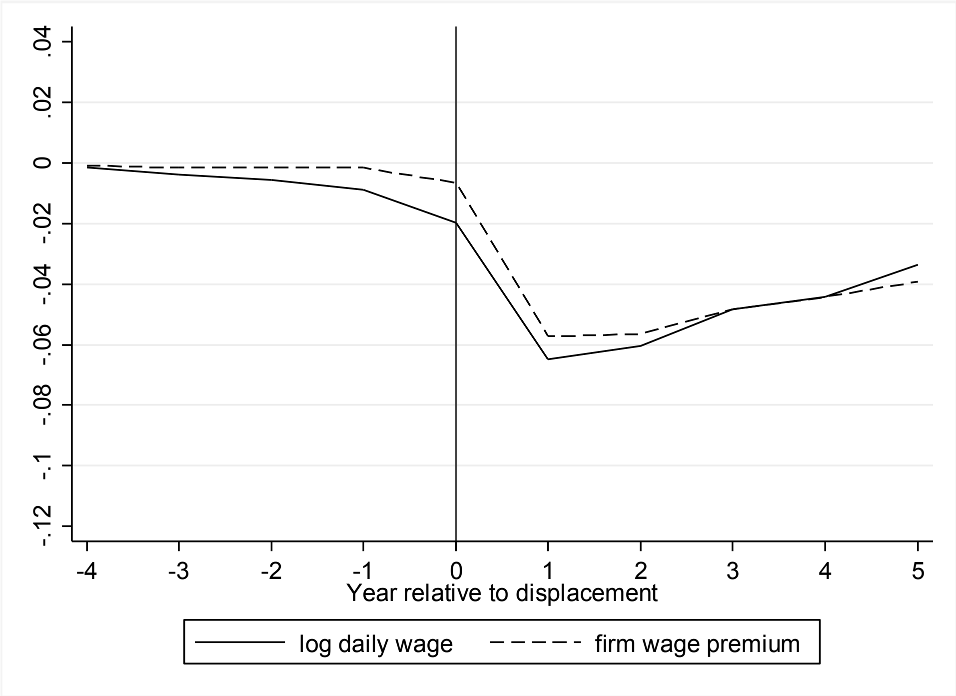
Figures and Tables

Figure 1: Log daily wages and firm wage premiums by pre-displacement plant size (in t-3), unmatched sample



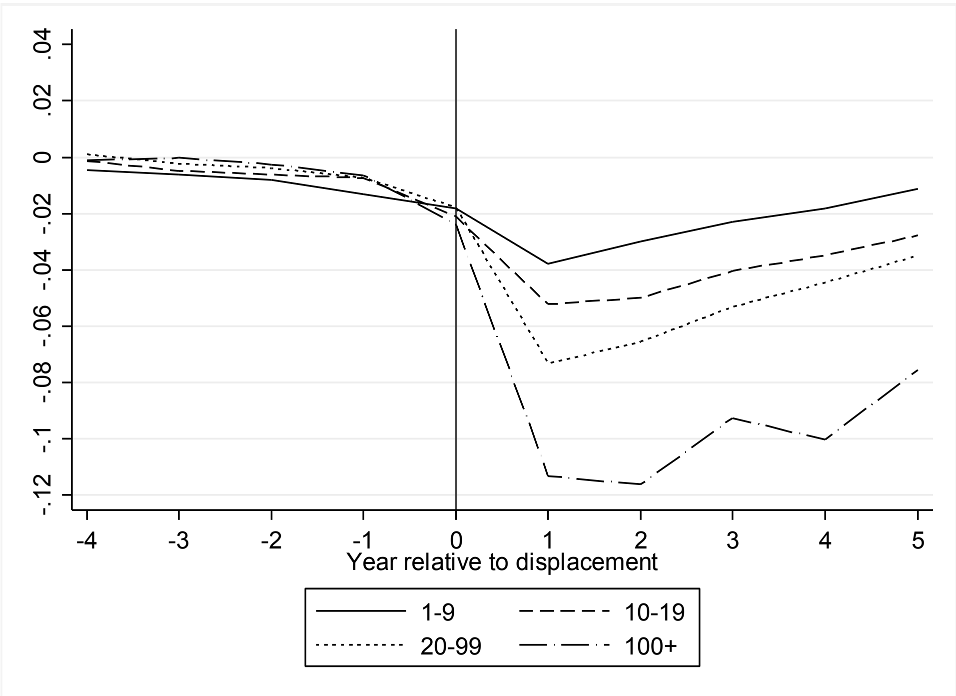
Notes: Firm wage premiums estimated based on the code by Card et al. (2013) and demeaned by sample average.

Figure 2: Changes in log daily wages and firm wage premiums



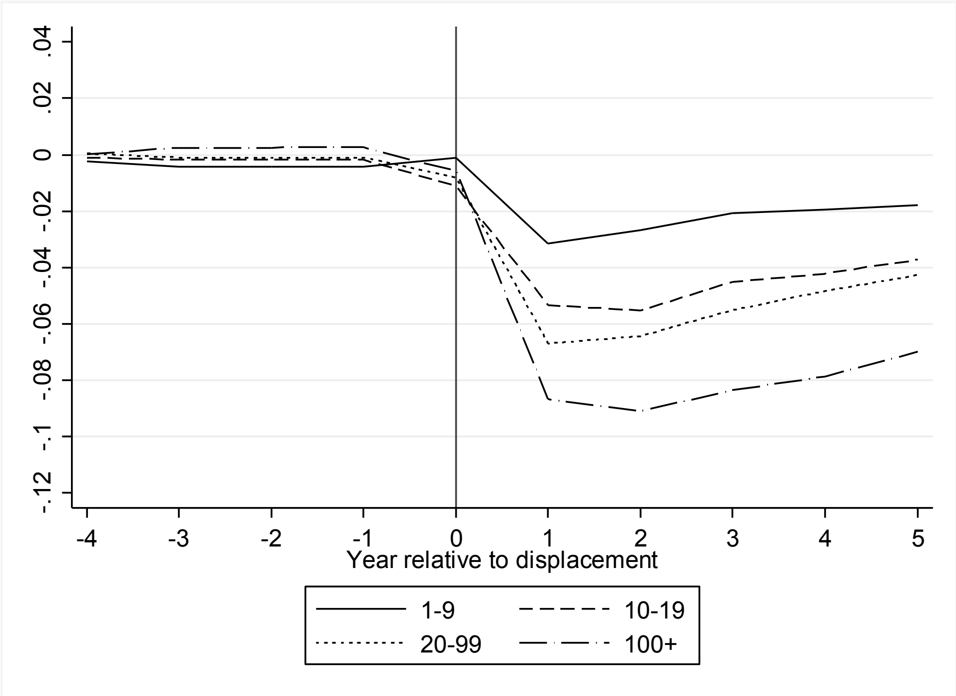
Notes: Graph shows differences between treatment and control group; coefficients from fixed effects regressions for the matched sample; outcome difference between treatment and control group is normalized to zero in t-5 (via worker fixed effects); firm wage premiums estimated based on the code by Card et al. (2013).

Figure 3: Changes in log daily wages by pre-displacement plant size (in t-3)



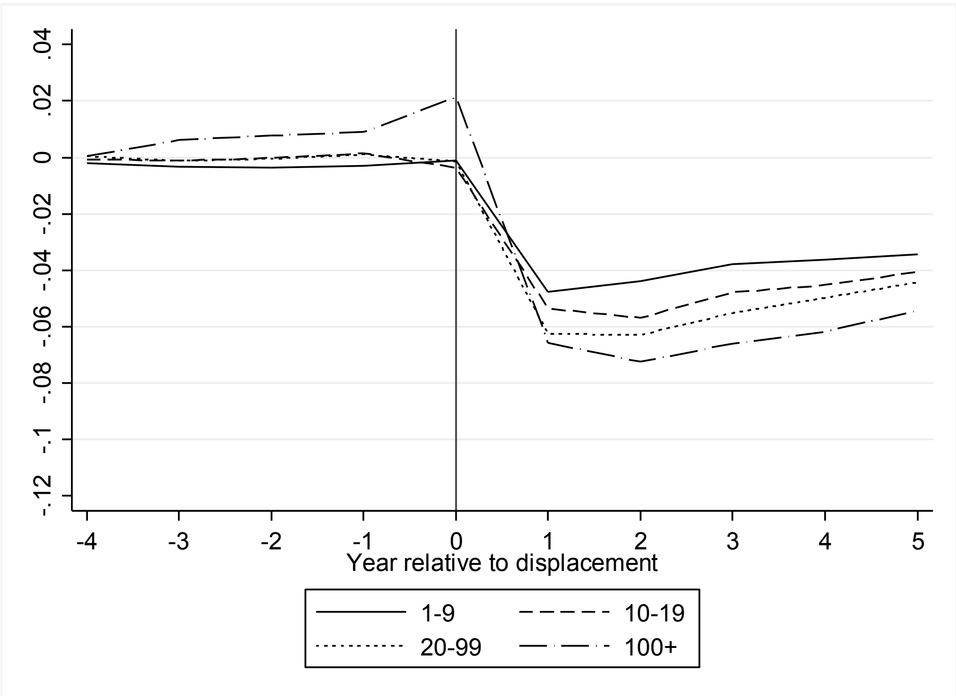
Notes: Same notes as in Figure 2 apply.

Figure 4: Changes in firm wage premiums by pre-displacement plant size (in t-3)



Notes: Same notes as in Figure 2 apply.

Figure 5: Changes in firm wage premiums by pre-displacement plant size (in t-3), conditional on actual plant size



Notes: Same notes as in Figure 2 apply; actual plant size is controlled for by including dummy variables for nine plant size classes.

Table 1: Descriptive worker-level statistics, unmatched sample (period t-1, standard deviations in parentheses)

	Displaced workers					Non-displaced workers				
Plant size (no. of employees in t-3)	Full Sample	1-9	10-19	20-99	100+	Full Sample	1-9	10-19	20-99	100+
Age (years)	41.41	40.39	41.16	41.65	42.75	41.29	39.77	40.59	41.27	41.65
	(7.484)	(7.743)	(7.610)	(7.469)	(6.743)	(7.316)	(7.646)	(7.451)	(7.357)	(7.181)
Experience (years)	17.01	15.43	16.82	17.32	18.90	17.57	15.82	16.74	17.22	18.13
	(7.734)	(7.619)	(7.778)	(7.774)	(7.305)	(7.593)	(7.517)	(7.541)	(7.574)	(7.558)
Tenure (years)	9.027	7.720	9.050	9.832	9.556	11.23	9.125	9.913	10.14	12.23
	(6.578)	(6.119)	(6.687)	(7.117)	(5.914)	(7.327)	(6.354)	(6.639)	(6.791)	(7.614)
Low skilled occupation (dummy)	0.4038	0.3662	0.3494	0.4052	0.5195	0.3750	0.2770	0.3154	0.3864	0.3954
	(0.4907)	(0.4818)	(0.4768)	(0.4910)	(0.4997)	(0.4841)	(0.4475)	(0.4647)	(0.4869)	(0.4889)
Medium skilled occupation (dummy)	0.4702	0.5149	0.5350	0.4855	0.3080	0.4367	0.5814	0.5401	0.4607	0.3876
	(0.4991)	(0.4998)	(0.4988)	(0.4998)	(0.4617)	(0.4960)	(0.4933)	(0.4984)	(0.4985)	(0.4872)
Highly skilled occupation (dummy)	0.1214	0.1119	0.1073	0.1075	0.1716	0.1858	0.1354	0.1401	0.1507	0.2155
	(0.3266)	(0.3152)	(0.3095)	(0.3098)	(0.3771)	(0.3889)	(0.3421)	(0.3471)	(0.3577)	(0.4111)
No. of employees per plant	105.4	6.233	13.98	42.95	441.3	2009	6.161	15.22	54.09	3447
	(243.6)	(5.583)	(5.525)	(25.95)	(393.9)	(6461)	(13.06)	(8.120)	(29.24)	(8201)
Manufacturing (dummy)	0.3384	0.2008	0.3072	0.3811	0.4972	0.5397	0.2114	0.2726	0.3825	0.6987
	(0.4732)	(0.4006)	(0.4614)	(0.4857)	(0.5000)	(0.4984)	(0.4084)	(0.4453)	(0.4860)	(0.4588)
Construction (dummy)	0.2559	0.2997	0.2770	0.2023	0.2498	0.0955	0.2821	0.2423	0.1316	0.0280
	(0.4364)	(0.4581)	(0.4476)	(0.4017)	(0.4329)	(0.2939)	(0.4500)	(0.4285)	(0.3381)	(0.1651)
Trade, logistics (dummy)	0.2865	0.3612	0.3184	0.2838	0.1515	0.2117	0.3252	0.3213	0.3198	0.1318
	(0.4521)	(0.4804)	(0.4659)	(0.4509)	(0.3585)	(0.4085)	(0.4685)	(0.4670)	(0.4664)	(0.3382)
Services (dummy)	0.1192	0.1383	0.0973	0.1328	0.1015	0.1531	0.1812	0.1638	0.1660	0.1415
	(0.3241)	(0.3453)	(0.2964)	(0.3394)	(0.3020)	(0.3601)	(0.3852)	(0.3701)	(0.3721)	(0.3485)
No. of workers	30,965	8,162	7,678	8,959	6,166	481,481	46,853	40,023	116,061	278,544

Table 2: Changes in log wages and firm wage premiums

Period (relative to displacement)	Log daily wage	Firm wage premium
t-4	-0.0014 (0.0016)	-0.0007 (0.0007)
t-3	-0.0037 (0.0018)*	-0.0016 (0.0008)
t-2	-0.0054 (0.0019)**	-0.0016 (0.0008)
t-1	-0.0088 (0.0020)**	-0.0016 (0.0008)
t	-0.0197 (0.0023)**	-0.0065 (0.0010)**
t+1	-0.0648 (0.0026)**	-0.0573 (0.0015)**
t+2	-0.0603 (0.0027)**	-0.0565 (0.0015)**
t+3	-0.0484 (0.0028)**	-0.0482 (0.0016)**
t+4	-0.0442 (0.0028)**	-0.0442 (0.0016)**
t+5	-0.0335 (0.0028)**	-0.0393 (0.0016)**
No. of observations	581,013	581,013
Within-R-squared	0.0442	0.0394

Notes: Fixed effects regressions for the matched sample; robust standard errors in parentheses; **, * denote significance at the 1 or 5 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 wage premiums estimated based on the code by Card et al. (2013).

Table 3: Changes in log wages and firm wage premiums by pre-displacement plant size

Period (relative to displacement)	Log daily wage	Firm wage premium
t-4	-0.0044 (0.0043)	-0.0024 (0.0018)
t-3	-0.0063 (0.0046)	-0.0044 (0.0022)*
t-2	-0.0082 (0.0049)	-0.0044 (0.0022)*
t-1	-0.0132 (0.0052)*	-0.0044 (0.0022)*
t	-0.0182 (0.0056)**	-0.0010 (0.0025)
t+1	-0.0377 (0.0063)**	-0.0316 (0.0035)**
t+2	-0.0299 (0.0064)**	-0.0268 (0.0036)**
t+3	-0.0231 (0.0066)**	-0.0207 (0.0038)**
t+4	-0.0183 (0.0065)**	-0.0194 (0.0039)**
t+5	-0.0111 (0.0066)	-0.0177 (0.0039)**
t-4 * size 10-19	0.0031 (0.0052)	0.0013 (0.0021)
t-3 * size 10-19	0.0015 (0.0056)	0.0026 (0.0026)
t-2 * size 10-19	0.0021 (0.0060)	0.0026 (0.0026)
t-1 * size 10-19	0.0058 (0.0064)	0.0026 (0.0026)
t * size 10-19	-0.0030 (0.0070)	-0.0101 (0.0030)**
t+1 * size 10-19	-0.0145 (0.0079)	-0.0218 (0.0044)**
t+2 * size 10-19	-0.0200 (0.0082)*	-0.0284 (0.0046)**
t+3 * size 10-19	-0.0173 (0.0083)*	-0.0244 (0.0047)**
t+4 * size 10-19	-0.0165 (0.0085)	-0.0227 (0.0049)**
t+5 * size 10-19	-0.0167 (0.0084)*	-0.0195 (0.0049)**
t-4 * size 20-99	0.0055 (0.0049)	0.0028 (0.0020)
t-3 * size 20-99	0.0040 (0.0053)	0.0034 (0.0025)
t-2 * size 20-99	0.0043 (0.0057)	0.0034 (0.0025)
t-1 * size 20-99	0.0059 (0.0060)	0.0034 (0.0025)
t * size 20-99	0.0003 (0.0067)	-0.0071 (0.0029)*
t+1 * size 20-99	-0.0356 (0.0076)**	-0.0354 (0.0043)**
t+2 * size 20-99	-0.0357 (0.0078)**	-0.0376 (0.0044)**
t+3 * size 20-99	-0.0300 (0.0079)**	-0.0344 (0.0046)**
t+4 * size 20-99	-0.0262 (0.0080)**	-0.0290 (0.0047)**
t+5 * size 20-99	-0.0238 (0.0080)**	-0.0250 (0.0047)**
t-4 * size 100+	0.0035 (0.0050)	0.0026 (0.0022)
t-3 * size 100+	0.0060 (0.0055)	0.0069 (0.0027)**
t-2 * size 100+	0.0057 (0.0060)	0.0069 (0.0027)**
t-1 * size 100+	0.0068 (0.0064)	0.0070 (0.0027)**
t * size 100+	-0.0057 (0.0070)	-0.0048 (0.0031)
t+1 * size 100+	-0.0755 (0.0084)**	-0.0551 (0.0047)**
t+2 * size 100+	-0.0864 (0.0086)**	-0.0642 (0.0049)**
t+3 * size 100+	-0.0695 (0.0088)**	-0.0628 (0.0051)**
t+4 * size 100+	-0.0820 (0.0088)**	-0.0593 (0.0052)**
t+5 * size 100+	-0.0645 (0.0090)**	-0.0521 (0.0053)**
No. of observations	581,013	581,013
Within-R-squared	0.0470	0.0464

Notes: Fixed effects regressions for the matched sample; reference: plant size 1-9 workers; robust standard errors in parentheses; **, * denote significance at the 1 or 5 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 wage premiums estimated based on the code by Card et al. (2013).

Appendix

New bankruptcy data

The bankruptcy data that are used to identify displacements are mainly based on information on *Insolvenzgeld*, a compensation scheme that each employee who has not received wages due to employer bankruptcy is eligible for. 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 exempts 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 have not yet closed. For vanishing plant IDs with more than 20 employees, we additionally make use of worker flow information, as described in Hethey-Maier and Schmieder (2013), to ensure that employees of these plants do not end up in subsidized employment ('*Auffanggesellschaften*'), which is very unlikely for small plants. 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 premiums

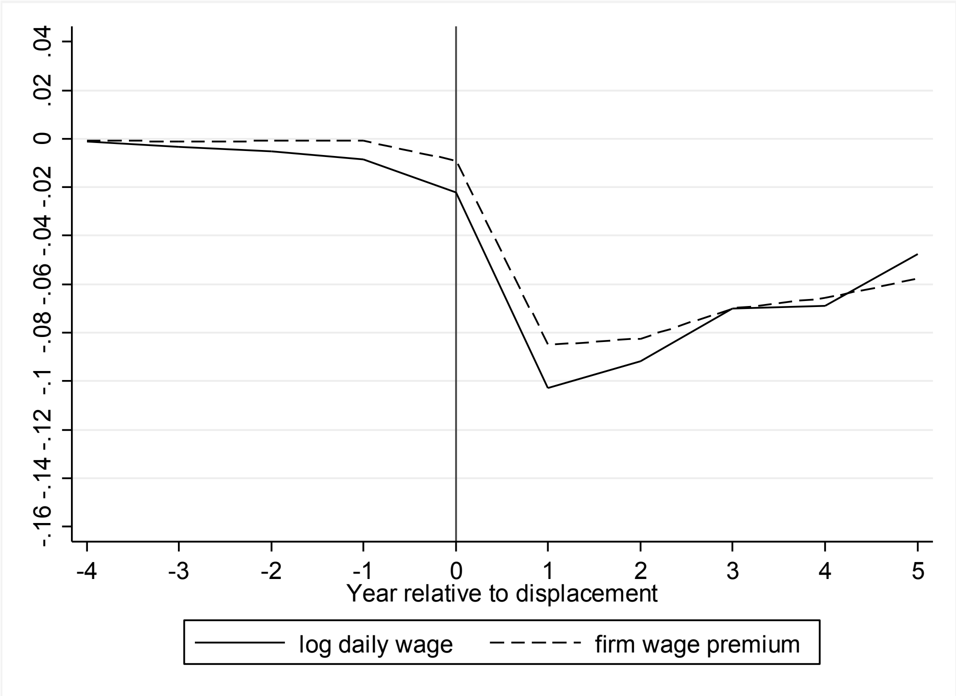
For the measurement of firm-specific wage premiums, we use firm fixed effects from a two-way fixed-effects model (as in AKM) of log wages estimated by CHK (see Card et al. 2015 for the description of the version of the estimates that is available at the Research Data Centre of the IAB). In their two-step 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. More detailed information on the data and how these can be was merged to standard IAB data sets is provided by Card et al. (2015). The most recent CHK time period publicly available is 2002-2009, and hence, there are no estimates available for plants established after 2009, which would lead to the exclusion of observations of workers moving to new firms. We therefore modified CHK's code and estimated the CHK model for the period 2002-2014 to obtain CHK firm effects for 2002-2014. Merging the extended CHK firm effects to our sample was possible for 97 percent of observations in the treatment group and for 99 percent in the control group. Comparing our estimates to the original CHK effects for 2002-2009 yields a correlation coefficient of 0.91 and a rank correlation of 0.90.

The core assumption for disentangling worker and firm effects in an AKM decomposition is that the error term is uncorrelated to the sequence of worker i's employers, which rules out mobility based on the match-specific component of pay. CHK support this assumption by showing that match-specific log wage components are quantitatively unimportant and unrelated to the direction of worker flows across employers. The AKM model implies that firm effects are additive and homogenous across all workers employed in the same plant. If firm effects differ within plants, our analysis may yield biased results. Since CHK also demonstrate that workers' (log) wage premiums overall do not depend on their skill level, the additive separability assumptions of the AKM model are supported. Similar conclusions are drawn by Card, Cardoso, and Kline (2016) for Portugal. Macis and Schivardi (2016) and Bonhomme et al. (2017) also report no or negligible deviations from the simple additive AKM model using samples of Brazilian and Swedish data, respectively.

Appendix Table A1: Descriptive worker-level statistics, matched sample (period t-1, standard deviations in parentheses)

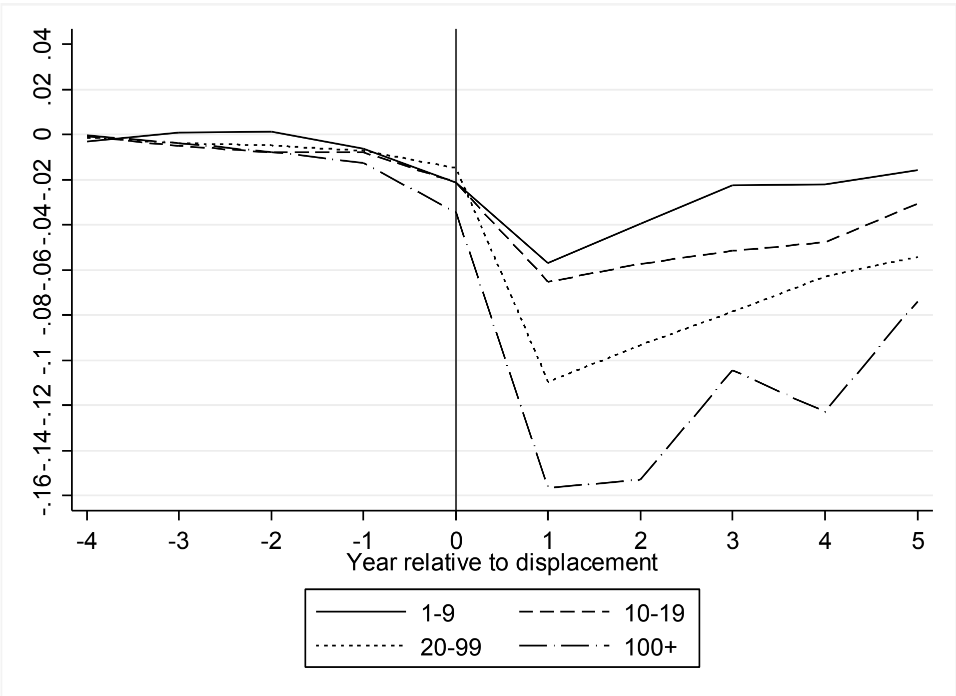
	Displaced workers					Non-displaced workers				
Plant size (no. of employees in t-3)	Full Sample	1-9	10-19	20-99	100+	Full Sample	1-9	10-19	20-99	100+
Age (years)	41.29	40.43	41.19	41.65	42.27	41.28	40.50	41.14	41.66	42.16
	(7.530)	(7.735)	(7.592)	(7.464)	(6.994)	(7.532)	(7.743)	(7.743)	(7.479)	(7.041)
Experience (years)	16.91	15.56	16.90	17.37	18.43	16.79	15.55	16.70	17.31	18.12
	(7.772)	(7.640)	(7.764)	(7.777)	(7.614)	(7.746)	(7.624)	(7.624)	(7.801)	(7.624)
Tenure (years)	9.051	7.899	9.199	9.998	8.964	9.008	7.908	8.988	9.947	9.134
	(6.627)	(6.161)	(6.698)	(7.120)	(5.937)	(6.621)	(6.152)	(6.152)	(7.137)	(6.091)
Low skilled occupation (dummy)	0.3886	0.3635	0.3468	0.4044	0.4738	0.3892	0.3647	0.3508	0.3971	0.4830
	(0.4874)	(0.4810)	(0.4760)	(0.4908)	(0.4994)	(0.4876)	(0.4814)	(0.4814)	(0.4893)	(0.4998)
Medium skilled occupation (dummy)	0.4901	0.5182	0.5366	0.4866	0.3670	0.4900	0.5197	0.5336	0.4919	0.3583
	(0.4999)	(0.4997)	(0.4987)	(0.4998)	(0.4820)	(0.4999)	(0.4996)	(0.4996)	(0.5000)	(0.4795)
Highly skilled occupation (dummy)	0.1165	0.1113	0.1085	0.1074	0.1579	0.1167	0.1095	0.1085	0.1095	0.1583
	(0.3208)	(0.3145)	(0.3110)	(0.3096)	(0.3647)	(0.3211)	(0.3123)	(0.3123)	(0.3123)	(0.3651)
No. of employees per plant	57.03	6.236	13.96	42.60	250.8	56.58	6.706	15.49	46.16	237.3
	(105.2)	(5.445)	(5.526)	(25.60)	(157.4)	(98.58)	(21.95)	(21.95)	(26.33)	(142.9)
Manufacturing (dummy)	0.3413	0.2015	0.3070	0.3806	0.5720	0.3349	0.1970	0.3025	0.3753	0.5566
	(0.4741)	(0.4012)	(0.4613)	(0.4856)	(0.4949)	(0.4720)	(0.3978)	(0.3978)	(0.4842)	(0.4968)
Construction (dummy)	0.2406	0.2962	0.2778	0.2053	0.1480	0.2401	0.2962	0.2763	0.2098	0.1380
	(0.4275)	(0.4566)	(0.4479)	(0.4040)	(0.3552)	(0.4271)	(0.4566)	(0.4566)	(0.4072)	(0.3449)
Trade, logistics (dummy)	0.2941	0.3647	0.3182	0.2814	0.1519	0.2967	0.3693	0.3213	0.2801	0.1576
	(0.4557)	(0.4814)	(0.4658)	(0.4497)	(0.3590)	(0.4568)	(0.4827)	(0.4827)	(0.4491)	(0.3644)
Services (dummy)	0.1239	0.1376	0.0970	0.1327	0.1281	0.1283	0.1374	0.0999	0.1348	0.1478
	(0.3295)	(0.3445)	(0.2960)	(0.3393)	(0.3343)	(0.3345)	(0.3443)	(0.3443)	(0.3415)	(0.3549)
No. of workers	28,449	7,836	7,495	8,747	4,371	28,449	7,836	7,495	8,747	4,371

Appendix Figure A1: Changes in log daily wages and firm wage premiums, workers displaced from manufacturing plants



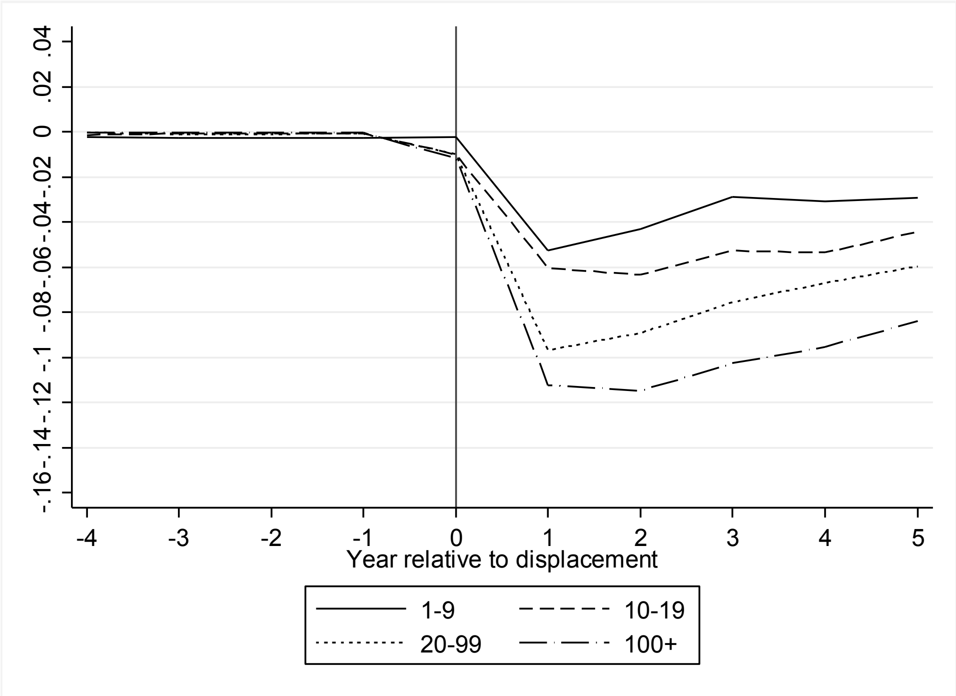
Notes: Same notes as in Figure 2 apply.

Appendix Figure A2: Changes in log daily wages by pre-displacement plant size (in t-3), workers displaced from manufacturing plants



Notes: Same notes as in Figure 2 apply.

Appendix Figure A3: Changes in firm wage premiums by pre-displacement plant size (in t-3), workers displaced from manufacturing plants

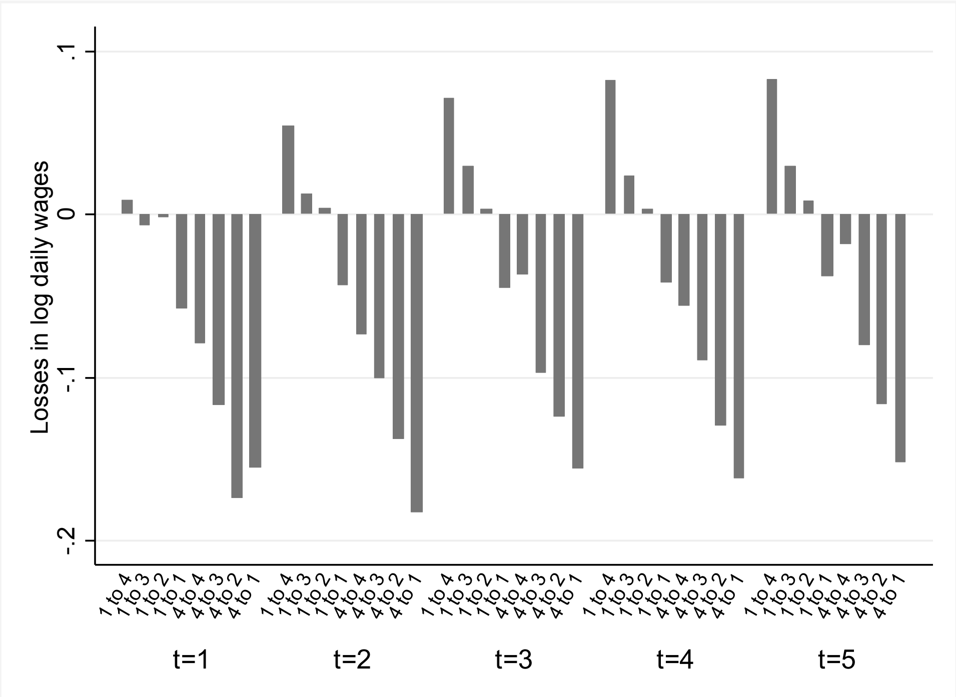


Notes: Same notes as in Figure 2 apply.

Appendix Table A2: Transition matrix for displaced workers' pre- and post-displacement plant size, matched sample

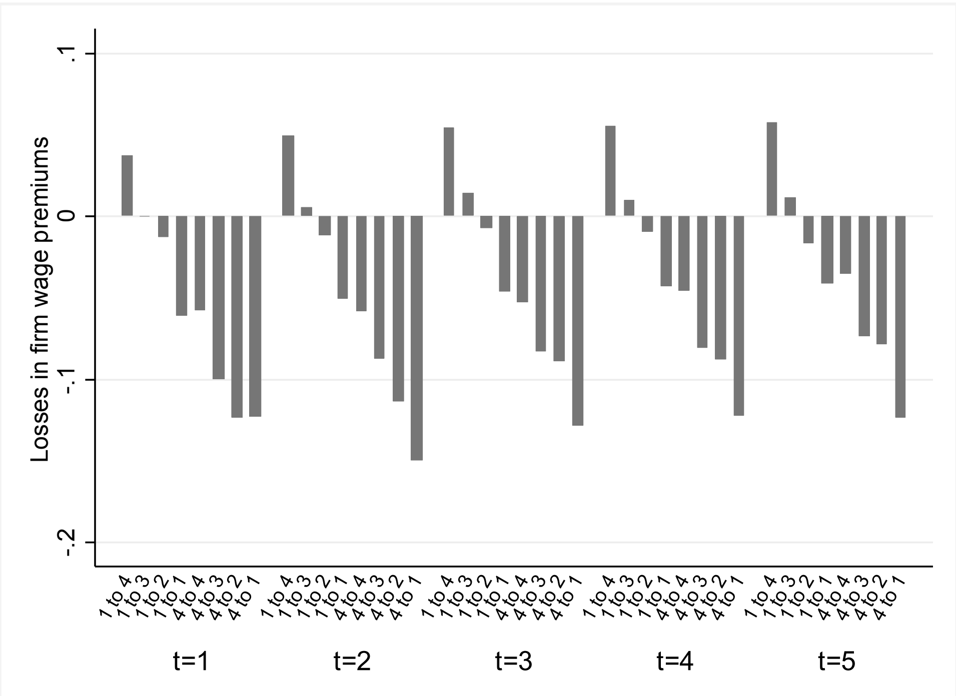
Pre-displacement plant size (in t-3)	Post-displacement plant size (in t+1)				
	1-9	10-19	20-99	100+	Total
1-9	2,955	891	1,343	715	5,904
10-19	2,103	1,387	1,660	885	6,035
20-99	1,529	1,255	2,639	1,659	7,082
100+	361	270	1,212	1,871	3,714
Total	6,948	3,803	6,854	5,130	22,735

Appendix Figure A4: Changes in log daily wages by pre- and post-displacement plant size



Notes: Same notes as in Figure 2 apply; pre-displacement plant size refers to t-3, post-displacement plant size to t+1; only post-displacement years are reported; size classes 1, 2, 3, 4 have 1-9, 10-19, 20-99, 100+ workers, respectively.

Appendix Figure A5: Changes in firm wage premiums by pre- and post-displacement plant size



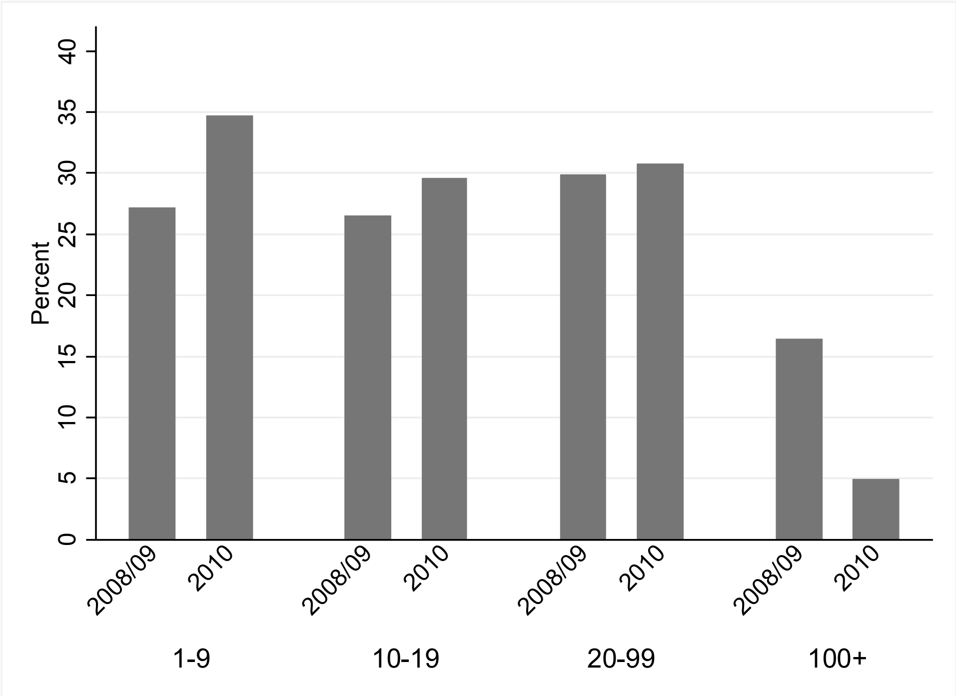
Notes: Same notes as in Appendix Figure A4 apply.

Appendix Table A3: Changes in log wages and firm wage premiums, workers displaced in 2008/09 versus 2010

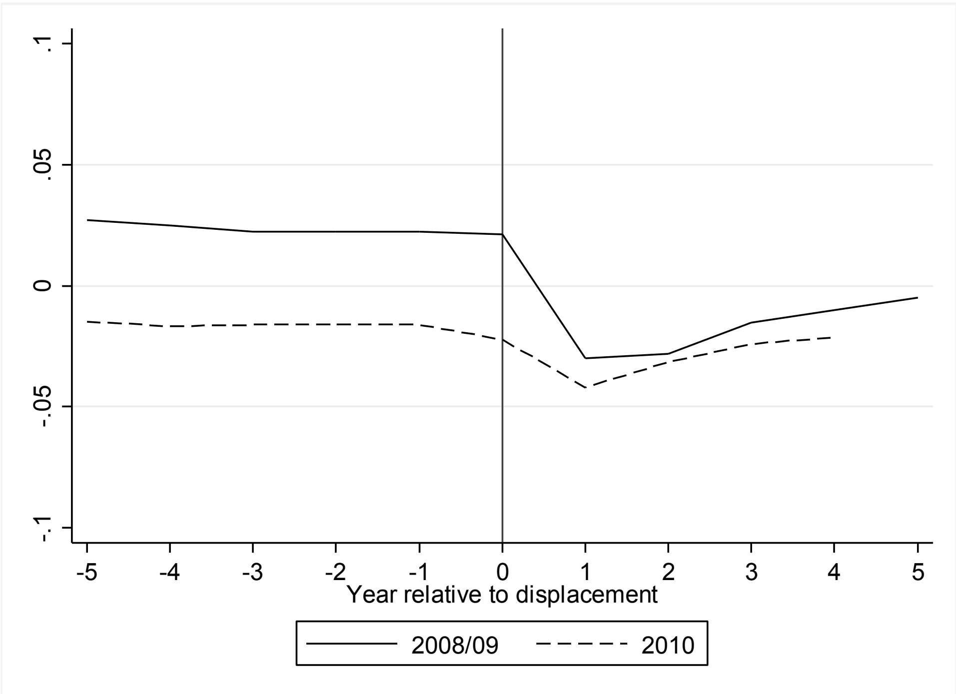
Period (relative to displacement)	2008/09		2010	
	Log daily wage	Firm wage premium	Log daily wage	Firm wage premium
t-4	-0.0019 (0.0018)	-0.0005 (0.0008)	-0.0007 (0.0030)	0.0009 (0.0014)
t-3	-0.0039 (0.0020)*	-0.0012 (0.0010)	-0.0006 (0.0036)	0.0014 (0.0017)
t-2	-0.0055 (0.0021)**	-0.0012 (0.0010)	-0.0006 (0.0038)	0.0014 (0.0017)
t-1	-0.0091 (0.0022)**	-0.0012 (0.0010)	-0.0068 (0.0041)	0.0014 (0.0017)
t	-0.0207 (0.0026)**	-0.0074 (0.0011)**	-0.0143 (0.0045)**	-0.0083 (0.0021)**
t+1	-0.0713 (0.0030)**	-0.0617 (0.0017)**	-0.0325 (0.0050)**	-0.0389 (0.0029)**
t+2	-0.0653 (0.0030)**	-0.0599 (0.0017)**	-0.0215 (0.0050)**	-0.0322 (0.0029)**
t+3	-0.0495 (0.0031)**	-0.0500 (0.0018)**	-0.0173 (0.0052)**	-0.0266 (0.0030)**
t+4	-0.0466 (0.0031)**	-0.0454 (0.0018)**	-0.0160 (0.0054)**	-0.0259 (0.0031)**
t+5	-0.0355 (0.0031)**	-0.0408 (0.0018)**	---	---
No. of observations	464,198	464,198	152,744	152,744
Within-R-squared	0.0475	0.0439	0.0507	0.0187

Notes: Fixed effects regressions for the matched sample; robust standard errors in parentheses; **, * denote significance at the 1 or 5 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 wage premiums estimated based on the code by Card et al. (2013); number of post-displacement years is lower for displacements in 2010, as data are available up to 2014 only.

Appendix Figure A6: Shares of displaced workers by pre-displacement plants size (in t-3), workers displaced in 2008/09 versus 2010, matched sample



Appendix Figure A7: Firm wage premiums (descriptive) for workers displaced in 2008/09 versus 2010, matched sample



Notes: Firm wage premiums estimated based on the code by Card et al. (2013) and demeaned by sample average.

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