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Publication Date
2017

Peer reviewed|Thesis/dissertation
Essays in Labor Economics

by

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A dissertation submitted in partial satisfaction of the requirements for the degree of

Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor David Card, Chair
Professor Patrick Kline
Professor Jesse Rothstein
Professor Christopher Walters

Spring 2017
Essays in Labor Economics

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Jeffrey Todd Sorensen
Abstract

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Doctor of Philosophy in Economics

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Professor David Card, Chair

Using matched employer-employee data on the universe of mass layoffs in West Germany from 1980 to 2009, I characterize each of the 4,400 mass layoffs by the relative probabilities of displacement for different types of workers. I find substantial heterogeneity in layoff rules across establishments, with the most common types being based on relative tenure, relative wages, or occupation. Laying off workers with low tenure—which I interpret as a less selective layoff rule—has become less common, while laying off workers with low relative wages—which I interpret as a more selective layoff rule—has become more common. I also find an increase in wage-based layoffs and a decrease in tenure-based layoffs in recessions.

In the second chapter, I test for asymmetric information in the labor market by studying how earnings losses of displaced workers vary with the degree of selection in employers’ layoff decisions. I develop an asymmetric employer learning model with heterogeneous firing costs that implies that earnings losses are smaller for workers involved in tenure-based layoffs, since these layoffs do not serve as a negative signal of workers’ productivity. I find strong support for this prediction: earnings losses are 20% smaller for workers displaced in tenure-based layoffs than observationally equivalent workers displaced in other layoffs. Selective layoffs are particularly costly for less-educated workers and workers in high-skilled service occupations, suggesting that asymmetric learning by incumbent employers is more prevalent for these groups.
To Melissa and Quinn
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Acknowledgments

I am grateful for the guidance and support of my dissertation committee: David Card, Patrick Kline, Jesse Rothstein, and Christopher Walters. My advisor, David Card, provided especially generous support throughout my time at Berkeley and introduced me to the German administrative data that I used in my dissertation.

I also appreciate helpful feedback from Yuriy Gorodnichenko, Hilary Hoynes, Lars Lefgren, Justin McCrary, Conrad Miller, Enrico Moretti, Benjamin Schoefer, and seminar participants at the BYU Graduate Student Conference, IAB PhD Workshop, Berkeley Labor Lunch, Center for Labor Economics Symposium, International FDZ Data User Workshop, IRLE Seminar, and Berkeley Labor Seminar. I thank the Institute for Employment Research (IAB) and Jörg Heining for providing the data, and I acknowledge generous financial support from the NBER Pre-Doctoral Fellowship on the Economics of an Aging Workforce and the IRLE Dissertation-Year Fellowship.

This dissertation would not have been possible without the constant support of my wife, Melissa. From encouraging me to work hard throughout my graduate career to paying the bills and taking care of our daughter (and everything else) during the busy job market season, she truly does it all.
Chapter 1

Firms’ layoff rules

1.1 Introduction

In this chapter, I estimate establishment-level layoff rules (i.e., the relative probabilities of job displacement for different types of workers at a given downsizing establishment) for the universe of mass layoffs in West Germany from 1980 to 2009. This allows me to provide the first comprehensive evidence on firms’ layoff and retention decisions and establish a number of new facts about firm layoff behavior—including increases in layoff rule selectivity over time and in recessions.

I begin by using unique administrative longitudinal earnings records from the German social security system to assemble data on all workers employed at large establishments experiencing a 30-90% quarterly drop in employment, comprising over 4,400 mass layoffs. I fit layoff-event-specific models of workers’ probability of being laid off and find substantial heterogeneity in the layoff rules (i.e., the estimated layoff probability parameters) across establishments.\(^1\) At the average downsizing establishment, the probability of being laid off is decreasing in tenure and wage, with occupation also playing an important role. Few mass layoffs are performed solely based on tenure, occupation, or wage, indicating that most layoffs involve discretion in choosing who to lay off. Layoffs have become more selective (in terms of workers’ productivity) and less rule-based since 1980: I find an increasing effect of workers’ wages (relative to observationally equivalent workers at the same establishment) on the probability of being laid off and a decreasing effect of tenure and most other observable characteristics.\(^2\) I also find significant business cycle effects: during recessions establishments

\(^1\)This builds on recent empirical research that investigates the heterogeneity and importance of firms in workers’ labor market outcomes instead of treating all firms as identical “black boxes” (e.g., Abowd et al., 1999; Bloom and Van Reenen, 2011).

\(^2\)This is consistent with Germany’s declining collective bargaining coverage and decreasing firing costs. While there is no direct evidence on secular changes in firms’ layoff rules, Farber and Hallock (2009) show that the reaction of downsizing firms’ stock prices to mass layoff announcements became less negative from 1970 to 1999 in the U.S. They also find that the most common announced reasons for performing a mass layoff shifted from “demand slump” to “reorganization” and “cost issues.”
are relatively less likely to lay off workers with low tenure and more likely to lay off workers with low relative wages.

To summarize heterogeneity in establishment layoff behavior, I partition the 4,414 mass layoff events into groups characterized by different layoff rules. An application of $k$-means clustering suggests four distinct types of layoff rules: tenure rules (lay off workers with low relative tenure), wage rules (lay off workers with low relative wages), occupation rules (lay off workers in certain occupations), and other rules. I find that establishments that use a wage layoff rule (which I interpret as the most selective of the four layoff rule groups) have significantly higher survival rates after the mass layoff.\(^3\) I also show that establishments in industries with strong collective bargaining are more likely to use a tenure layoff rule, a finding that corroborates a key assumption in studies of job displacement.

This chapter provides the first comprehensive evidence on firms’ layoff decisions. While the effects of job displacement on workers are well documented, little is known about how firms choose which workers to lay off.\(^4\) Existing studies either compare the characteristics of displaced and employed workers, with little or no information on the layoff events that led to displacement (e.g., Kletzer, 1998), or present case studies of a single layoff event (e.g., Elvira and Zatzick, 2002; Pfann, 2006). My unique data contain detailed characteristics and complete employment histories of every worker at the universe of downsizing establishments—essential elements for a thorough analysis of firm layoff behavior.

My study of this fundamental question also sheds light on related questions in the literature, such as whether firms’ layoff decisions are more rule-based or selective, and therefore whether mass layoffs can be thought of as an exogenous source of job loss.\(^5\) I find that some firms make their layoff decisions based primarily on workers’ observable characteristics, but the majority of firms appear to put significant weight on unobservable characteristics as well. It appears, however, that selection bias is not of first-order importance in past estimates of the cost of job loss: Even though I find that workers laid off using the least selective tenure layoff rules have smaller earnings losses than those laid off using more selective rules (see Chapter 2), they still experience substantial earnings losses of 40% one year after the layoff (compared to 50% for workers displaced using other rules).

My finding that layoff rules are more selective and less tenure-based in recessions contributes to a line of research on a possible “cleansing effect” of recessions: the idea that firms

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\(^3\)This finding is in the same vein as the empirical industrial organization literature on the links between management practices and survival (e.g., Bloom and Van Reenen, 2007) and firm productivity and survival (e.g., Bartelsman and Doms, 2000; Foster et al., 2008).

\(^4\)See Jacobson et al. (1993), Sullivan and von Wachter (2009), and Rege et al. (2011) for effects of displacement on earnings, mortality, and children’s school performance. The lack of research on firm layoff behavior is largely due to data limitations, as noted in Oyer and Schaefer’s (2011) Handbook of Labor Economics chapter.

\(^5\)The conventional wisdom is that workers’ tenure plays the largest role in layoff decisions, especially for unionized firms, as found by Abraham and Medoff’s (1984) survey of 200 firms. In Gibbons and Katz’s (1991) model, layoff decisions are based entirely on worker productivity, although they state that this is less likely to hold for unionized firms. Jacobson et al. (1993) and Von Wachter et al. (2011) develop methodology to try to mitigate selection bias in estimates of the cost of job loss.
increase their productivity-enhancing activities in recessions because of relatively low opportunity costs (e.g., Davis and Haltiwanger, 1990; Caballero and Hammour, 1994; Aghion and Saint-Paul, 1998; Foster et al., 2016). And my results on increasing selectivity in layoff decisions over time may have implications for other countries that have experienced labor-market liberalization since 1980.

Section 1.2 describes the data, and Section 1.3 outlines relevant institutional features of the German labor market. I present results on establishments’ layoff rules in Section 1.4. The establishment-level layoff rule estimates I obtain in this chapter—combined with the finding of substantial heterogeneity in the degree of selection used in choosing who to lay off—allow me to perform a new test of asymmetric employer learning in Chapter 2.

1.2 Data

I use administrative matched employer-employee data for Germany from 1975 to 2010, made available by Germany’s Institute for Employment Research (IAB). The data are drawn from social security earnings records that employers submit for their employees each year and cover the universe of private-sector employees (the major excluded groups are civil servants and self-employed workers). They include a record for each job held by a worker in each calendar year, with information on earnings, job start and end dates, and detailed worker characteristics, including age, sex, education, occupation (with 340 occupation codes), nationality, trainee status, part-time status, region, and industry.\(^6\)

Table B.1 shows how I construct my sample of mass layoffs in West Germany from 1980 to 2009. Since I estimate layoff rules at the establishment level, I require that there be at least 100 workers at the establishment in the quarter it downsizes. Most of the displacement literature considers a 30-90% drop in annual employment to be a mass layoff. I use the same definition but at the quarterly level to more precisely identify which workers are present during the downsizing and which workers are likely to have been laid off.

To ensure that my sample contains only true mass layoffs, I exclude temp agencies and vocational training establishments, as well as establishments in the mining and agriculture industries (establishment survival in these industries is often subject to political intervention (Fackler et al., 2013)). Next I exclude establishments that are less than one year old and those that do not have stable pre-layoff employment (no more than a 40% increase in quarterly employment over the past year) and post-layoff employment (employment stays below the pre-layoff level for each of the next four quarters) to avoid issues with seasonal or temporary employment. One of the more important sample corrections eliminates spin-offs, establishment ID changes in the data (due to a change in ownership or industry), and cases where a large number of workers are transferred to another establishment owned by the same firm. This sample correction requires that no more than 30% of the separating workers end up at the same establishment the next quarter, and it also requires that the largest cluster

\(^6\)Earnings are censored at the maximum taxable earnings level, which affects no more than 10% of the workers in any of the years.
of separating workers that end up at the same establishment (for sufficiently large clusters) makes up less than 90% of the successor’s employment (see Hethey-Maier and Schmieder (2013) for more on this approach). My final sample corrections require that the number of permanently laid-off workers exceeds the number of temporarily laid-off workers (the mean recall rate for the establishments in my final sample is 2.3%) and that at least 5% of the separating workers receive unemployment insurance benefits within 12 weeks (the waiting period for receiving unemployment insurance benefits for job quitters).

Using my final sample of 4,414 mass layoffs, I create a quarterly data set (as of the first day of each quarter) that contains the complete employment histories of all workers present at downsizing establishments in a four-year window around the mass layoff. The main sample I use includes workers present during the quarter of the mass layoff and contains 926,057 workers, with 438,172 workers laid off. The data’s complete coverage of all workers at this set of establishments and detailed worker characteristics are crucial elements for studying establishment-level layoff rules, and the ability to follow workers before and after the mass layoff is critical for studying the effects of different layoff rules on the cost of job loss (the sample contains 54,836,667 worker-quarter observations). I also create quarterly data sets for the downsizing establishments and worker and establishment control groups.

1.3 Institutional features

West Germany is an appealing setting for studying firm layoff decisions because it is representative of broad trends in labor market institutions taking place in many countries. While Germany’s labor laws are relatively strict overall, Deakin et al. (2007) create numeric indexes for five categories of labor laws and show that Germany’s dismissal laws from 1970 to 2006 were similar to those in the U.K., significantly weaker than those in France and India, and somewhat stronger than those in the U.S. Also, Germany had the fourth largest decline in employment protection strictness of 28 OECD countries from 1985 to 2009 (Venn, 2009). This decline is connected to liberalizing labor market reforms that took place between 1996 and 2005 (with a slight reversal during 1998-2001) and a decline in collective bargaining.

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7The average number of mass layoffs an establishment in my sample performs is 1.16, with 11% performing two, and 2% performing three to five. Although I only consider mass layoffs in West Germany, I include all jobs that workers have in either West or East Germany between 1975 and 2010 in their employment histories.

8These displaced workers make up 3% of all layoffs and 1% of all employment-to-unemployment transitions in Germany during this time period (Gangl, 2003; Bachmann, 2007). This coverage would more than double if I included smaller establishments in my sample, and it would also greatly increase if I did not use as restrictive sample corrections to ensure that I am only analyzing true mass layoffs.

9Other important benefits of this data set compared to the few other available employer-employee data sets for other countries include the time range (1975-2010), coverage of the entire country, precise timing of job start and end dates, unemployment insurance information, and the ability to create detailed establishment variables. The high-quality administrative nature of the data also minimizes common problems with survey data, such as non-response, recall bias (found by Song (2007) to be especially prevalent when studying job loss), and measurement error.
CHAPTER 1. FIRMS’ LAYOFF RULES

(both shown in Figure B.1): the fraction of workers whose wages are set outside the sectoral agreement system increased from 27% to 44% between 1995 and 2007 (Ellguth et al., 2012). After increasing slightly in the 1970s, Germany’s union density decreased from 35% in 1980 to 31% in 1990, 25% in 2000, and 19% in 2010 (compared to 11% in 2010 for the U.S., which experienced its large decline in unionization 10-20 years earlier) (OECD and Visser, 2011).

The weakening of Germany’s employment protection legislation and collective bargaining have contributed to a decline in firing costs (I include severance pay, administrative costs, and legal fees in “firing costs”). Grund (2006) shows that the fraction of dismissed workers who receive severance pay decreased from 40% to 15% between the early 1990s and the early 2000s, and it is also lower in recessions. Industries with higher union density have higher rates of severance pay, and the amount of severance pay workers receive is increasing in tenure and wage. In addition, he finds that collective dismissals are more likely to receive severance pay than individual dismissals, although conditional on severance pay receipt they receive smaller amounts of severance pay (controlling for tenure and wages).

Germany’s laws regarding the dismissal of employees differ by the type of dismissal and the type of job contract. Employees on fixed-term contracts have less job protection than employees on permanent contracts, and the laws regulating their protection have grown significantly weaker since the mid-1990s (Jahn et al., 2012). The share of fixed-term contracts in Germany’s full-time labor force increased from 4% in 1984 to 11% in 1996 and 15% in 2008; 31% of new contracts in 1994 were fixed-term (Hunt, 2000; Jahn et al., 2012). The maximum length of these contracts increased from 6 months to 18 months in 1985 and to 24 months in 1996, and they can last even longer with a valid “objective reason” (Hunt, 2000).

In general, firms wishing to dismiss employees must provide a minimum of four weeks’ notice, and this requirement increases to seven months’ notice for employees with over twenty years of tenure. Dismissal laws apply only to workers with over six months of tenure. If the firm has a works council (a firm-level organization that represents employees and facilitates communication between the employer and employees), it must be given the reason and criteria for dismissals, and it has the right to comment on (but not prevent) them. For dismissals caused by business reasons, firms are supposed to use the proper “social selection” to lay off employees who will be the least harmed, taking into consideration tenure, age, maintenance obligations, and disability status (Baker and McKenzie, 2009). The employer may be able to exclude some employees from the social selection based on legitimate operating reasons, such as knowledge, skills, performance, or the preservation of a balanced staff structure.

A firm planning to perform a mass layoff must inform the local employment office at least one month before the layoff and the firm’s works council two weeks before this.

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10 The economic situation of firms often influences court decisions in wrongful dismissal lawsuits, making firms less likely to offer severance pay in recessions (Ichino et al., 2003).

11 This is an especially difficult and complex process at large firms, making it hard to legally enforce (Jahn, 2009). Also, the social selection criteria have a limited effect empirically on which displaced workers receive severance pay (Grund, 2006).

12 The law defines a “mass layoff” as a dismissal of more than 5 employees at a firm with 20-59 employees, 10% of the workforce or more than 25 employees at a firm with 60-499 employees, or more than 30 employees
Although there are no legal requirements for severance pay, workers often receive 50% of their monthly wages for each year of tenure—another factor that motivates the dismissal of low-tenure workers (OECD, 2003, 2013). Overall, little is known about how Germany’s employment protection laws affect firms’ layoff decisions in practice or how easy it is for firms to circumvent them when they need to lay off a large fraction of their workforce.

1.4 Results: Firms’ layoff rules

Figure A.1 shows the number of mass layoffs (according to my definition of mass layoff, described in Table B.1) and the unemployment rate for West Germany in each year. More mass layoffs occurred in the 1990s than the other two decades, and mass layoff events are countercyclical: the correlation coefficient between the unemployment rate and number of mass layoffs is $r = 0.27$ ($p = 0.15$). The mean layoff size (i.e., fraction of workers laid off) remains quite constant at approximately 50% over time and does not vary significantly over the business cycle (see Figure B.2).

Comparing establishments that perform a mass layoff to those that do not reveals a number of differences (shown in Table B.2 and Figure B.3). Downsizing establishments are smaller, younger, and pay less. Their workers have less tenure and are more likely to be male, non-German, and less educated. Downsizing establishments are also significantly more likely to be in the food and beverage production, consumer goods manufacturing, construction, and business services industries and less likely to be in the finance, public services, education, and health industries. Their workers are more likely to be in low-skilled manual occupations and less likely to be in high-skilled sales or administrative occupations. These differences point to the importance of controlling for the workforce composition of downsizing establishments when trying to explain which workers are displaced instead of simply comparing the mean characteristics of displaced and employed workers.

Figure A.2 depicts the tenure distribution and layoff rates by tenure for workers at downsizing establishments, pooling all workers from the 4,414 mass layoffs (except for a small number of workers whose tenure is censored because they had been working at the establishment since 1975, the first year of the data). The tenure distribution is highly skewed, as it is at most establishments. The probability of being laid off is a decreasing function of tenure, with a stronger relationship for workers with fewer than three years of tenure. Workers at

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13 In 2004, the government simplified the dismissal process and standardized severance pay to 50% of a worker’s monthly wages for each year of tenure in cases where the employer mentions the possibility of severance pay in the layoff notification and the worker does not go to court over the dismissal within three weeks (Jahn, 2009; OECD, 2013). The intent of this legislation was to lower firing costs by reducing the number of wrongful dismissal lawsuits and thus increase labor market flexibility. It is also worth noting that unions and works councils have occasionally agreed on concessions regarding working time and wages to prevent layoffs (Ebbinghaus and Eichhorst, 2006). The increased use of working time accounts, which allow firms to avoid overtime pay if a worker’s hours average to standard hours over a period of time, has also reduced the number of layoffs in recent recessions (Burda and Hunt, 2011).
non-downsizing establishments have slightly higher tenure on average and separation rates of 11%, 6%, and 5% after one, two, and three years.

### 1.4.1 Layoff rules by decade

I first estimate how establishments choose which workers to lay off by pooling the mass layoffs in each decade. Using the sample of all workers present at downsizing establishments during the quarter of the mass layoff, I assign \( D_{ij} = 1 \) if worker \( i \) at establishment \( j \) permanently separates and \( D_{ij} = 0 \) if the worker stays at the establishment (or is only temporarily laid off).\(^{14}\) Then I estimate three linear probability models, one for each decade (results from logit models are almost identical):

\[
D_{ij} = x_{ij}\beta + \psi_j + \varepsilon_{ij},
\]

where \( x_{ij} \) includes the worker characteristics I observe (e.g., tenure, wage, age, education, detailed occupation codes) and \( \psi_j \) are establishment fixed effects. This provides a general look at the types of workers that establishments lay off, given the composition of their workforce, and how layoff rules have changed over time.

Table A.1 provides estimates for the probability of being laid off, with Figures A.3 and A.4 illustrating the coefficients on the tenure dummy variables and wage decile dummy variables (relative to Germany’s wage distribution for the year of the mass layoff) from the same regressions. The probability of being laid off is decreasing in tenure, wage, age, and (slightly in) education, with occupation also playing an important role (the age effect also includes an experience effect). I find an increasing effect of wages over time and a decreasing effect of most other observable characteristics. There appears to have been a decline in “last in, first out” tenure layoff rules, although this is somewhat obscured by the increased use of fixed-term contracts, which have a maximum length of two years and are renewable every six months. While I cannot distinguish fixed-term from permanent contracts in the data, the increasingly large spikes at two, four, six, and eight quarters of tenure in Figure A.3 are likely connected to the drastic increase in fixed-term contracts since 1980 and a positive relationship between having a fixed-term contract that is close to ending and the probability of being laid off.\(^{15}\)

The effect of wages on layoff probability in Figure A.4 can be thought of as the effect of within-establishment wage residuals since the regressions control for establishment fixed effects and worker characteristics that are commonly included in wage equations.\(^{16}\) Al-

\(^{14}\) \( j \) is technically a mass layoff (i.e., establishment-quarter) index, since 13% of the establishments perform more than one mass layoff. Similarly, the “establishment-level” results in the next section are technically “mass-layoff-level” results.

\(^{15}\) There are no spikes in the number of workers at these levels of tenure. I also find a similar decline in the effect of tenure on layoff probability using years of tenure or within-establishment relative tenure instead of the tenure dummy variables.

\(^{16}\) Results using wage residual deciles, where the residuals are obtained from common wage models, are similar to the results using wage deciles. I also find the same pattern using log wages or within-establishment relative wage instead of the wage decile dummy variables.
though the unexplained portion of wages within an establishment could reflect either workers’ productivity or rents, its strong negative effect on layoff probability and the fact that establishment-wide wage premiums are differenced out suggest that the productivity interpretation is more likely to be correct in this setting.\textsuperscript{17} This interpretation also fits with two other findings in this dissertation: establishments that lay off their low-wage workers survive longer after the mass layoff (see Section 1.4.3), and workers displaced by establishments that lay off their low-wage workers have the largest earnings losses (see Section 2.3.1).

Laws that protect current trainees or apprentices from being laid off appear to work: these workers are very unlikely to be displaced. Establishments are also more likely to retain their part-time and German workers. Occupation plays an important role in layoff decisions, while education provides surprisingly little protection from being laid off, and its effect is becoming even smaller over time. Comparing the estimated coefficients across the three decades shows that layoff decisions are becoming more selective (with an increasing effect of workers’ relative wages) and less rule-based (with a decreasing effect of most other observable characteristics), consistent with Germany’s labor market reforms and decline in firing costs and collective bargaining.

Although some workers who separate from downsizing establishments may do so voluntarily even with my more precise quarterly timing of mass layoffs, Germany’s relatively low labor mobility makes this not likely to be a major problem (the mean separation rates are 11%, 6%, and 5% after one, two, and three years for workers at establishments with at least 100 workers). Also, downsizing establishments may be more likely to try to compel workers to quit, making the distinction between involuntary and voluntary separations less important. As a robustness check, I fit the same models using workers with more than three years of tenure (who are even less likely to separate voluntarily) and find that these results hold.

1.4.2 Establishment-level layoff rules

I estimate similar linear probability models using slightly fewer regressors (e.g., less detailed occupation variables) separately for each mass layoff to obtain establishment-level layoff rule estimates (i.e., the relative probabilities of displacement for different types of workers). This results in a vector of coefficients for each of the 4,414 mass layoffs that provides the effect of different worker characteristics on the probability of being laid off. Since some of these estimates have low precision (especially for establishments with only 100 workers), I use an empirical Bayes shrinkage approach to shrink the estimated coefficients toward the mean coefficients across all the mass layoffs based on the estimated coefficients’ precision (Morris, 1983):

\textsuperscript{17}Under asymmetric employer learning, firms earn higher rents on more productive workers, giving them a preference for such workers. My interpretation of within-establishment wage residuals as a proxy for productivity is consistent with asymmetric employer learning where outside firms receive a noisy signal of the productivity of the incumbent firm’s workers, as discussed in Section 2.2. Hagedorn et al. (2016) employ a similar approach by using within-firm wage rankings to infer worker ability.
\[ \tilde{\beta}_j = \lambda_j \hat{\beta}_j + (1 - \lambda_j) \bar{\beta}, \text{ where } \lambda_j = \frac{\sigma^2_{\beta}}{\sigma^2_{\beta} + se^2_j} \]  

(1.2)

Here \( j \) indexes one of the 4,414 mass layoffs, \( \hat{\beta}_j \) is the estimated coefficients from \( j \)’s linear probability model, \( \bar{\beta} \) is the mean coefficients across all the mass layoffs, \( \sigma^2_{\beta} \) is the variance of the coefficients across all the mass layoffs, \( se^2_j \) is the standard errors of \( j \)’s estimated coefficients, and \( \tilde{\beta}_j \) is \( j \)’s shrunk coefficients. This shrinkage approach has a negligible effect on the means of the coefficients across mass layoffs and reduces the standard deviations by 34% on average, mitigating the effect of imprecisely estimated outliers in the following results.

Table A.2 presents summary statistics for the shrunk coefficients. “Low tenure” is a dummy variable that equals 1 if a worker is in the bottom \( x\% \) of the establishment’s tenure distribution, where \( x\% \) is the fraction of workers who are laid off (an establishment that uses a strict “last in, first out” seniority layoff rule, therefore, would have a coefficient estimate close to 1). The mean coefficients across mass layoffs are consistent with the pooled layoff regressions by decade in Section 1.4.1: the average establishment is more likely to lay off its low-tenure, low-wage, and young workers. I also find that workers in low-skilled manual occupations are especially more likely to be laid off, while those in high-skilled service occupations are especially less likely. Figure A.5 shows kernel density estimates for the low-tenure and log-wage coefficients. Even after shrinking imprecisely estimated coefficients toward the mean, there is substantial heterogeneity in how establishments make their layoff decisions.\(^{18}\)

I compare these layoff rule estimates with those from simpler models in Figure A.6, which presents histograms of adjusted \( R^2 \) values from different establishment-level layoff linear probability models that include as regressors tenure dummies only, wage decile dummies only, and occupation dummies only. Most establishments’ layoff decisions are not explained by tenure, occupation, or wage alone, indicating that most mass layoffs involve discretion in choosing who to lay off. Also, all three simple models are clearly outperformed by the complete layoff model used in the analyses below that includes all the observable worker characteristics as regressors.

I now use these establishment-level layoff rule estimates to explore how layoff rules vary over time and the business cycle.\(^{19}\) Calculating means of the establishment-level layoff coefficients by year (using three-year moving averages) and plotting them over time shows similar trends as the previous decade-level estimates (i.e., a decreasing effect of most observable characteristics except for wage). Figure A.7 depicts six of these trends as well as business cycle effects, which are more clearly seen in Figure B.4. Even with the confounding effect

\(^{18}\)These distributions actually understate the heterogeneity in layoff rules, since the true variance is between the unshrunk and shrunk variances.

\(^{19}\)My main use of the layoff rule estimates is testing for asymmetric employer learning in Section 2.3. Another use is in Sorensen (2016b), where I utilize the establishments that use strict tenure layoff rules to provide quasi-experimental estimates of the cost of job loss to workers in a regression discontinuity framework.
of an increased use of fixed-term contracts seen in the decade-level results, it appears that establishments have become less likely to lay off their low-tenure workers.

Although the mean log-wage coefficient does not display a clear trend over time, estimating two similar establishment-level layoff models with different regressors shows that layoff rules have become more wage-residual-selective. To more directly evaluate the effect of the unexplained portion of workers’ wages within an establishment (a proxy for workers’ productivity, as argued in Section 1.4.1) on the probability of being laid off, I obtain within-establishment wage residuals from a regression of workers’ log wages on establishment fixed effects, workers’ tenure, tenure-squared, tenure censored, age, age-squared, sex, nationality, part-time status, trainee status, education group, and occupation. The second-to-last graph in Figure A.7 plots the mean wage-residual coefficient from establishment-level layoff regressions that include workers’ wage residuals as the only regressor. Establishments have clearly become more likely to lay off their workers with low relative wages since the 1980s. Since these wage residuals do not necessarily reflect productivity, and since their correct interpretation may vary across establishments or over time, I also plot the mean adjusted $R^2$ value from establishment-level layoff regressions that include wage residual deciles as the only regressors. This graph also demonstrates an increased role of wage residuals in establishments’ layoff decisions over time, consistent with the corresponding decline in the effect of most observable worker characteristics (especially tenure) and Germany’s falling firing costs and collective bargaining coverage.

The types of establishments that perform a mass layoff remained quite stable from 1980 to 1995, but some changes in downsizing establishments’ mean characteristics (relative to changes in non-downsizing establishments) began to be seen around 1995—the same time that Germany’s labor market reforms began. Plotting mean characteristics for downsizing and non-downsizing establishments over time reveals that since the mid-1990s, downsizing establishments have become more likely to be young, pay less, have workers with less tenure, have more part-time and “marginal” workers (part-time workers who earn less than €400 per month), have more workers in low-skilled service occupations, and have fewer workers in manual occupations. They also have become more likely to be in the business services, finance, and public organizations industries and less likely to be in the consumer goods manufacturing, chemicals and metals manufacturing, and car and machinery manufacturing industries. Since most of these differences appear to be relatively minor and all of the layoff rule trends in Figure A.7 begin before 1995, it is improbable that the changing composition of downsizing establishments over time is responsible for the layoff rule trends I find.

The business cycle effects seen in Figure A.7 are made more clear in Figure B.4, where I apply a Hodrick–Prescott (HP) filter with smoothing parameter 6.25 to remove the trend component from both series and focus on the cyclical component. Figure B.4 shows the

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20. These wage residuals control for more detailed variables than the main establishment-level layoff regressions. The standard deviation of the wage residuals is 0.27.

21. The only changes that did not begin in the mid-1990s are the public organization and consumer goods manufacturing changes, which began in the early 1980s, and the marginal worker change, which began around 2005 (marginal workers are only included in the data after 1999).
detrended mean layoff coefficients and unemployment rate and reports the correlation coefficients between the two series. During recessions, establishments become less likely to lay off their low-tenure, college-educated, male, and high-wage-residual workers. It appears that establishments move away from their traditional tenure layoff rules in recessions and selectively lay off unproductive workers instead. This increase in layoff rule selectivity in recessions may be caused by establishments taking advantage of the opportunity to selectively fire because of lower firing costs, establishments obtaining exemptions from collective bargaining layoff requirements because of the increased risk of going out of business, or establishments increasing their productivity-enhancing activities because of relatively low opportunity costs (as in research on a “cleansing effect” of recessions).²²

All of these business cycle effects are stronger in the post-1995 period, perhaps because the liberalizing labor market reforms between 1996 and 2005 gave establishments more flexibility in their layoff decisions.²³ Downsizing establishments look very similar in recessions and expansions. The major differences—all of which are small—are that establishments that perform a mass layoff during a recession are younger (13.7 vs. 14.3 years), have workers with less tenure (23.9 vs. 25.1 quarters), shrink more over the next year (64% vs. 61%), are slightly less likely to be in the food and beverage production industry and slightly more likely to be in the construction industry, and are slightly less likely to have workers in engineering or sales and administrative occupations and slightly more likely to have workers in manual occupations.²⁴ These business cycle effects, therefore, are likely the result of actual changes in establishments’ layoff rules instead of differences in the types of establishments that downsize or the composition of their workforce over the business cycle.

Overall, the differences in layoff rules that I find over time and the business cycle are consistent with the model in Section 2.2: layoff rule selectivity is increasing over time and in recessions, consistent with Germany’s decreasing firing costs over time and in recessions causing establishments to put less weight on firing costs and more weight on productivity in their layoff decisions. The increased layoff rule selectivity over time is also consistent with the rising inequality, falling relative demand for low-skilled workers, and labor market liberalization seen in Germany. In Sorensen (2016a), I further explore the cyclicality of layoff rules and its implications for a number of macroeconomic questions. For example, I find that establishments’ changing layoff rules over the business cycle can partially explain the increased cost of job loss and increased duration of unemployment for workers displaced in recessions (Baker, 1992; Davis and von Wachter, 2011). I also study the relationship

²²Firing costs are lower in recessions because severance pay is less common and wrongful dismissal lawsuits are less likely to be ruled in favor of the worker, as discussed in Section 1.3. For work on possible “cleansing effects” of recessions, see Davis and Haltiwanger (1990), Caballero and Hammour (1994), Aghion and Saint-Paul (1998), and Foster et al. (2016). These business cycle effects also pertain to research on how unemployment inflows and the composition of the unemployment pool change over the business cycle (Baker, 1992; Elsby et al., 2009; Shimer, 2012).
²³This is especially true for the effects in the last three graphs in Figure B.4, possibly due to changes in the wage setting process and interpretation of wage residuals over this period.
²⁴Plotting downsizing establishments’ mean characteristics over the business cycle also reveals no major differences.
between changes in the cyclicality of layoff rules and changes in the cyclicality of labor productivity.\textsuperscript{25}

1.4.3 Layoff rule groups

To summarize heterogeneity in layoff behavior across establishments, I partition the 4,414 mass layoff events into groups characterized by different layoff rules using the machine-learning method of $k$-means clustering. $k$-means clustering partitions multi-dimensional observations into $k$ clusters, with each observation belonging to the cluster with the nearest mean (minimizing the within-cluster sum of squares).\textsuperscript{26} I choose $k = 4$ and run the algorithm on the estimated establishment-level coefficients on low-tenure, the coefficients on log-wage, and the $p$-values on the $F$-statistics testing the joint significance of the occupational groups—chosen because the previous findings show that tenure, wage, and occupation are the most important factors in layoff decisions. These choices result in a manageable number of layoff rule groups and an intuitive classification of layoff rules in light of the previous findings, although different choices of $k$ and the addition of other layoff coefficients provide similar results.\textsuperscript{27} Table A.3 reports the means of the three variables and number of mass layoffs in each of the four clusters, showing that the four groups that $k$-means clustering divides the mass layoffs into can be thought of as tenure layoff rules (lay off workers with low relative tenure), wage layoff rules (lay off workers with low relative wages), occupation layoff rules (lay off workers in certain occupations), and other layoff rules (rules where tenure, wage, and occupation do not play an especially large role).

Although the main use of these layoff rule groups is in the next section where I explore how workers’ earnings losses from job displacement vary by the layoff rule choice, I also use them here to better understand what types of establishments use different layoff rules. In Table B.3, I estimate a multinomial logit model of the relationship between layoff rule groups and establishment characteristics. Older establishments are more likely to use a tenure layoff rule and less likely to use a wage layoff rule compared to the “other layoff rule” group (although the effect on tenure rule choice is only significant at the 10% level).\textsuperscript{28} Higher-paying establishments are more likely to use a tenure layoff rule and less likely to use a wage layoff rule compared to the “other layoff rule” group (although the effect on tenure rule choice is only significant at the 10% level).\textsuperscript{28} I further explore this at the end of this section.

\textsuperscript{25}If these general results hold for the U.S., then the increased layoff rule selectivity in recessions and over time may explain why average labor productivity has become acyclical or even countercyclical in recent recessions and why jobless recoveries have become more common, as in Berger’s (2015) model.

\textsuperscript{26}See MacQueen (1967). The standard iterative algorithm randomly chooses $k$ of the observations as the initial cluster means/centers, assigns each observation to the nearest cluster mean, chooses new cluster means by calculating the mean of each cluster, and then iterates this process until no observations change clusters.

\textsuperscript{27}I also standardize each of the three variables to have mean 0 and standard deviation 1 so that differences in the variables’ scales do not affect their importance in the clustering algorithm, and I censor the occupation $p$-values at 0.1.

\textsuperscript{28}This is consistent with establishments covered by collective bargaining agreements, which are older on average, being more likely to use a tenure layoff rule and less likely to use a more selective wage layoff rule.
CHAPTER 1. FIRMS’ LAYOFF RULES

use the within-establishment wage residuals from Section 1.4.2 and find that their standard deviation at the establishment level predicts the layoff rule: wages are more closely tied to observable worker characteristics at establishments that use a wage layoff rule than at establishments that use a tenure or occupation rule. Establishments that lay off a larger fraction of their workforce are more likely to use an occupation layoff rule and less likely to use a tenure rule. Industry has the largest effect on tenure layoff rule use, with establishments in manufacturing, wholesale, and retail industries more likely to use a tenure rule.

I estimate the relationship between layoff rule choice and establishment exit hazard rates by fitting Cox proportional hazards models to study whether certain layoff rules help downsizing establishments survive longer. Column (1) of Table A.4 shows that establishments that use a tenure or wage layoff rule have unconditionally lower exit hazard rates. When I control for a rich set of establishment characteristics in column (2), I find that using a wage rule is the most beneficial for establishments’ survival after the mass layoff. Although the initial layoff size may be confounded with the outcome variable for a small number of mass layoffs (i.e., ones where the establishment performs its mass layoff in the beginning of the quarter but then has to lay off more workers later in the quarter), it presumably should be included as a control variable for most layoffs. When I compare downsizing establishments that have the same observable characteristics and lay off the same fraction of their workers in column (3), I find that using a wage rule results in a substantial 18% reduction in the hazard rate of exiting, followed by a 12% reduction for occupation rules. I obtain similar results when estimating ordered logit models of change in establishment size after three or five years (dividing the mass layoffs into five groups, with one of the groups consisting of exits).

It appears that establishments that use a more selective layoff rule to weed out less productive workers survive significantly longer. While I have not shown that this effect is causal, one potentially major source of endogeneity—that establishments that know they have a high chance of going out of business soon may use a more selective layoff rule in an attempt to survive or because they are more able to obtain exemptions from collective bargaining layoff requirements—actually suggests that the causal effect may be even larger. These findings raise the question of why all establishments do not use selective layoff rules. Many establishments that use tenure layoff rules are required to by collective bargaining agreements, while others may choose to because of concerns regarding employment protection legislation, morale or fairness considerations, or possible benefits of having a seniority-based layoff policy (in attracting or retaining workers, for example). It is also probable that some establishments are simply better at performing mass layoffs that allow them to survive than others, since research shows substantial heterogeneity across firms in both management

29The fraction of establishments that have exited one, three, and five years after the mass layoff is 22%, 47%, and 56%. Also, I take into account censoring for establishments that have not exited by the last quarter of the data (the fourth quarter of 2010).

30I also obtain similar results when estimating a Cox hazard model using the establishment-level layoff coefficients themselves instead of the layoff rule groups from k-means clustering and when considering establishments that permanently shrink to less than five workers as exits.
practices and productivity and strong links between management practices and survival (e.g., Bloom and Van Reenen, 2007) and productivity and survival (e.g., Bartelsman and Doms, 2000; Foster et al., 2008).

To conclude this section, I provide evidence that establishments covered by collective bargaining agreements are more likely to use tenure layoff rules and less likely to use selective layoff rules—a key assumption in many studies of job displacement. Although I do not observe union or collective bargaining status for the establishments in my data, I have access to industry-year collective bargaining rates for a number of years between 1995 and 2009 derived from the IAB Establishment Panel data set. I divide the mass layoffs into collective bargaining coverage quartiles based on the industry and year (imputing the 1995 rates back to 1990) and present the mean establishment-level layoff coefficients and $R^2$ values in Table B.4.

Establishments with a greater chance of being covered by a collective bargaining agreement appear to be more likely to use tenure layoff rules. They are also more likely to retain their part-time workers, possibly because fewer of their part-time workers are on fixed-term contracts (Figure A.3 suggests that having a fixed-term contract increases the probability of being laid off). If the effect of having a fixed-term contract on layoff probability were able to be controlled for, there would likely be an even greater difference in the use of tenure layoff rules by collective bargaining rates. It appears that establishments in the lowest collective bargaining coverage quartile are less likely to retain their high-wage workers, but the results using the wage residual coefficients and $R^2$ values show no clear pattern.

Since these findings may be caused more by differences in industries than differences in collective bargaining status, I also investigate how layoff rules changed in industries that had larger declines in collective bargaining coverage between 1995 and 2009. Figure B.5 plots the (negative) log change in collective bargaining coverage by industry against the log change in the fraction of establishments that use tenure, wage, occupation, or other layoff rules by industry (the circle sizes are proportional to the number of workers in the industry). Industries that had larger declines in collective bargaining between 1985 and 2009 also had larger declines in tenure layoff rule use and larger increases in wage rule use. This provides further support for the idea that collective bargaining leads to more seniority-based and fewer selective layoff rules.

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31 The rates are based on the fraction of establishments that are covered by an industry-level collective bargaining agreement (the most common form of agreement in Germany).

32 Establishments in industries with higher collective bargaining coverage also are older, pay more, have workers with more tenure, have fewer part-time and marginal workers, have more workers in manual occupations (especially low-skilled manual), and have fewer workers in professional occupations.

33 The difference in low-tenure coefficients by collective bargaining coverage is significant at the 5% level when controlling for other establishment characteristics, while the difference in log-wage coefficients is not.

34 Industries with low collective bargaining coverage in 1995 had larger decreases in collective bargaining between 1995 and 2009, although this relationship is less clear if I exclude two outlier industries.
Chapter 2
The cost of job loss and asymmetric employer learning

2.1 Introduction
Recent work demonstrates that employers learn about workers’ productivity over time. There are conflicting results, however, on whether employer learning is symmetric or asymmetric across firms—that is, whether a worker’s current employer has significantly more information about his or her productivity than outside employers.\(^1\) The distinction between symmetric and asymmetric employer learning has proven difficult to empirically test and has important labor market implications: asymmetric employer learning creates monopsony power and can result in an inefficient allocation of workers to jobs, statistical discrimination, and underinvestment in general skills by workers (Waldman, 1984, 1990).

In this chapter, I study whether firms have private information about their workers by testing whether different types of mass layoffs—distinguished by the degree of selection used in choosing who to lay off—provide different signals to the outside market. I use estimated establishment-level layoff rules (i.e., the relative probabilities of displacement for different types of workers at a given downsizing establishment) for the universe of mass layoffs in West Germany from 1980 to 2009, obtained in Chapter 1. This allows me to test a model of asymmetric employer learning with heterogeneous firing costs. I find support for asymmetric employer learning: earnings losses are 20% smaller for workers displaced in tenure-based mass layoffs, who are revealed to be relatively more productive, than for observationally equivalent workers displaced in other mass layoffs. I also use this framework to show that employers have more private information about their less-educated workers and their workers in high-skilled service occupations.

I begin this chapter by developing an asymmetric employer learning model of layoffs with

heterogeneous firing costs that explains differences in layoff rules across establishments and, more importantly, provides a framework for testing whether firms’ private information about their workers’ productivity is signaled to outside firms through the layoff rule choice. Key features of the model are that firms decide who to lay off based on a combination of workers’ productivity and firing costs, firms vary in how costly it is to lay off their high-firing-cost workers, and outside firms’ inference of downsizing firms’ private information depends on the layoff rule. The key implication of asymmetric employer learning is that wage losses are smaller for workers laid off using layoff rules that put more weight on firing costs and thus less weight on productivity, since these layoffs signal less negative information to the outside market. The model also predicts that pre-displacement wages do not vary by the layoff rule choice (unlike similar symmetric employer learning models), making the layoff event the only signal of firms’ private information.

The establishment-level layoff rule estimates obtained in Chapter 1—combined with the finding of substantial heterogeneity in the degree of selection used in choosing who to lay off—allow me to perform a new test of asymmetric employer learning. I use event study methodology to test my model’s prediction that the cost of job loss to workers is increasing in the weight put on productivity in the layoff rule. I find strong support for this prediction: workers laid off using tenure layoff rules experience significantly smaller earnings losses than other laid-off workers (especially those laid off using wage layoff rules), since layoffs based on workers’ firing costs (proxied by tenure) reveal less negative information about their quality than layoffs based on productivity. I find similar effects on wages, days worked per quarter, and the duration of the next job (a measure of match quality), and these results are robust to controls for a rich set of worker and establishment characteristics. I also find no difference in pre-displacement earnings trends or levels by layoff rule for otherwise observationally equivalent workers, suggesting that it is the layoff event that signals the firm’s private information.

My final results explore differences in the signal value of layoff rules by worker characteristics and by the state of the business cycle. I show that the layoff rule appears to signal more information to outside employers in expansions than recessions: workers’ earnings losses vary significantly more by the layoff rule choice in expansions. As predicted by the model, I also find that tenure layoff rules are especially beneficial for low-tenure workers (who are the most likely to be laid off according to the rule) and especially harmful for high-tenure workers (who are the least likely to be laid off according to the rule). Selective layoffs are particularly costly for less-educated workers and workers in high-skilled service occupations, suggesting that asymmetric learning by incumbent employers is more prevalent for these groups.

A major contribution of this chapter is that it helps resolve the debate in the literature on whether employer learning is symmetric or asymmetric by providing evidence that downsizing firms’ layoff rules signal their private information to outside firms. My asymmetric employer learning model builds on the framework developed by Gibbons and Katz (1991), who present the first empirical evidence for asymmetric learning by showing that workers’ wage losses are smaller for plant closings than for layoffs. Subsequent studies call their main
result into question, focusing on important differences between plant closings and layoffs that contribute to their result—critiques I am able to directly address with my data and methodology.\(^2\) My test of asymmetric employer learning also improves on past work by using a more direct measure of how selective different job displacements are and a more comparable set of displacements: mass layoffs at large establishments where the main difference is the layoff rule.

This chapter also contributes to an emerging strand of literature by exploring whether asymmetric employer learning occurs more for certain types of workers. Arcidiacono et al. (2010) find that ability is observed nearly perfectly for recent college graduates but revealed to the labor market more gradually for high school graduates, and Mansour (2012) presents evidence that symmetric employer learning varies by occupation. Allowing for differences by both education and occupation in my test of asymmetric employer learning, I find that incumbent employers have more private information about less-educated workers (who likely find it harder to signal their productivity to outside employers) and workers in high-skilled service occupations (where it is likely harder for outside employers to observe their output). I also show the importance of testing for differences in asymmetric learning by education and occupation simultaneously, since the occupations where I find employer learning is most asymmetric have higher average levels of education, potentially affecting the interpretation of past work that allows for differences in only one dimension. My finding that employer learning is highly asymmetric for some groups of workers while potentially symmetric for others helps reconcile the conflicting results in the literature on whether employer learning is symmetric or asymmetric: the answer may depend on the context, varying for different groups of workers.

Section 2.2 introduces the asymmetric employer learning model with heterogeneous firing costs. I present results on asymmetric employer learning in Section 2.3. I then offer concluding remarks in Section 2.4.

### 2.2 Model

I develop a simple asymmetric employer learning model of layoffs with heterogeneous firing costs that yields a set of testable predictions about whether incumbent firms have private information about their workers’ productivity. In the model, downsizing firms decide who to lay off based on the workers’ productivity—which the incumbent firm has learned but outside firms have not—and the workers’ firing costs, and firms differ in how costly it is to lay off high-firing-cost workers. (Firing costs typically include severance pay, administrative costs, and legal fees and can be thought of as an increasing function of workers’ tenure, and differences in firing costs across firms can be thought of as a result of collective bargaining differences, as discussed in Section 2.2.2.) Consequently, firms that find it more costly to lay off certain workers put more weight on firing costs and less on productivity in their layoff

\(^2\)Differences in average firm size, pre-displacement wage changes, recall bias, and local labor market conditions (Stevens, 1997; Krashinsky, 2002; Song, 2007; Hu and Taber, 2011).
rule. Outside firms’ inference of downsizing firms’ private information depends on the layoff rule used. This yields the model’s main testable prediction: workers laid off using layoff rules based more on firing costs have smaller wage losses than workers laid off using rules based more on productivity, since less selective “firing-cost layoff rules” signal less negative information about worker productivity.

An important feature of the model is that pre-displacement wages do not reveal the incumbent firm’s private information about its workers’ productivity. In most asymmetric employer learning models, incumbent firms earn rents on their workers and pay wages that incorporate little, if any, of their private information. Nevertheless, pre-displacement wages would be expected to fully reflect any public signals of worker productivity. For simplicity, I ignore such signals in the model and assume that prior to the layoff all workers have the same wages conditional on the available public information. It is straightforward to extend the model, however, to incorporate publicly observable information that is symmetrically learned by the incumbent firm and outsiders.

A second key distinction between private and public information is that only private information is revealed by selective layoffs. Assuming that pre-displacement wages reflect expected productivity conditional on any public information, there is no additional information about the publicly observable component of productivity that is revealed by the fact that a worker is laid off in a particular kind of layoff (either selective or non-selective). Thus, if employer learning were symmetric, workers laid off using selective layoff rules would experience wage losses leading up to the layoff event (as the market learned their productivity) instead of at the time of the layoff. The idea behind my test of asymmetric employer learning, then, is that the layoff event forces the firm to reveal some or all of its private information through its decision of which workers to lay off, with the amount of information revealed corresponding to the weight put on productivity in its layoff decision.

2.2.1 Model setup and results

The model has two periods. At the beginning of the first period, information about workers’ productivity $\eta_i$ is symmetric across firms and imperfect: all firms believe that workers’ productivity (given observable characteristics) has the distribution function $F$. Productivity is equal to output, which is produced at the end of each period and has the price of 1. Workers have either low or high firing costs (measured in the same units as output), with the low firing cost normalized to 0 and the difference in firing costs between low- and high-firing-cost workers.

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3Wages incorporate no private information in Waldman’s (1984) model and only some of the private information in Schönberg’s (2007) and Pinkston’s (2009) models. If outside firms can observe the wage set by the incumbent firm, then the only way the incumbent can retain its informational advantage is by paying the same wage to more and less productive workers: any departure from this “pooling equilibrium” allows outside firms to invert the wage-setting rule and infer productivity. Gibbons and Katz (1991) make a similar point: if the firm could retain less productive workers at a low wage, then it could also retain more productive workers at this low wage (since the two groups of workers look the same to the outside market), which would destroy the market’s willingness to allow any workers to be retained at the low wage.
workers varying by firm: \(c_{ij} \in \{c_{Lj}, c_{Hj}\}\), where \(c_{Lj} = 0, c_{Hj} = \alpha_j\), \(\alpha_j \sim Uniform(0, \beta)\), and \(i\) and \(j\) index workers and firms. This allows for some firms to find it more costly to lay off their high-firing-cost workers and for firing costs to change over time or the business cycle (through \(\beta\)). Firing cost \(c_{ij}\) is revealed to the market after the worker is hired, so it does not affect hiring decisions.

Competition among firms and the initial symmetric information result in a single first-period wage for all workers equal to \(w_1\). The fraction of firm \(j\)’s workers that have \(c_{ij} = c_{Lj}\) is \(\theta_j\), and all firms observe \(c_{ij}\), \(\alpha_j\), and \(\theta_j\). The key asymmetric information feature of the model is that firms learn the productivity of their own workers after observing their output at the end of the first period, but they do not receive new information about the productivity of other firms’ workers.

In the second period, some firms (the “downsizing firms”) are hit by a demand shock, which can be thought of as a shock to the firms’ output price or their workers’ productivity, and must lay off a fraction \(\gamma_j\) of their workers (observable to all firms), which depends on the severity of the shock. The other firms (the “outside firms”) observe which workers are laid off and make them wage offers, using the layoff rule choice and workers’ firing costs to determine their expected productivity.

Consider downsizing firm \(j\). The value to the firm of retaining worker \(i\) relative to laying him or her off is \(\eta_i + c_{ij}\), as it avoids paying the firing cost by retaining the worker. Profit maximization causes the firm to lay off workers whose wages are higher than \(\eta_i + c_{ij}\). Since wages are still equal to \(w_1\) for all workers, the firm strictly prefers retaining workers with higher productivity (given firing costs) and higher firing costs (given productivity). The firm’s optimal layoff rule is to lay off worker \(i\) if and only if the value of retaining the worker is below some cutoff \(\rho_j\):

\[
\eta_i + c_{ij} < \rho_j
\]  
(2.1)

This results in separate layoff rule productivity cutoffs for low- and high-firing-cost workers:

\[
\eta_i + c_{ij} < \rho_j \iff \begin{cases} 
\eta_i < \rho_j, & \text{if } c_{ij} = c_{Lj} \\
\eta_i < \rho_j - \alpha_j, & \text{if } c_{ij} = c_{Hj}
\end{cases}
\]  
(2.2)

Since \(\rho_j > \rho_j - \alpha_j\), the firm lays off more \(c_{Lj}\) workers than \(c_{Hj}\) workers and is less selective (in terms of productivity) when laying off \(c_{Lj}\) workers. Intuitively, high-firing-cost workers must have especially low productivity for the firm to be willing to lay them off and pay the higher firing cost.

The firm chooses the cutoff \(\rho_j\) subject to the constraint that it lays off a fraction \(\gamma_j\) of its workers:

\[
\theta_j F(\rho_j) + (1 - \theta_j) F(\rho_j - \alpha_j) = \gamma_j,
\]  
(2.3)

where \(F(\rho_j)\) and \(F(\rho_j - \alpha_j)\) are the fraction of \(c_{Lj}\) and \(c_{Hj}\) workers laid off. Equation (2.3) implicitly defines \(\rho_j\) as a function of \(\alpha_j\), \(\theta_j\), and \(\gamma_j\).
Competition among outside firms leads to a second-period wage for laid-off workers equal to their expected productivity given the downsizing firm’s layoff rule:

$$w_{ij} = E(\eta_i|\eta_i + c_{ij} < \rho_j) = \begin{cases} E(\eta_i|\eta_i < \rho_j), & \text{if } c_{ij} = c_{Lj} \\ E(\eta_i|\eta_i < \rho_j - \alpha_j), & \text{if } c_{ij} = c_{Hj} \end{cases}$$ (2.4)

Outside firms observe the workers’ firing costs and the layoff rule productivity cutoffs (by observing $\alpha_j$, $\theta_j$, and $\gamma_j$) and use this information to determine the expected productivity of the displaced workers. The workers are paid their expected productivity because their individual productivity is not observed by outside firms. This results in higher wages for $c_{Lj}$ workers, who are revealed to be relatively more productive, than for $c_{Hj}$ workers.

The average wage for workers laid off by firm $j$ is then the weighted average of the wage of the displaced $c_{Lj}$ and $c_{Hj}$ workers:

$$w_j = \frac{\theta_j F(\rho_j)}{\gamma_j} E(\eta_i|\eta_i < \rho_j) + \frac{(1 - \theta_j) F(\rho_j - \alpha_j)}{\gamma_j} E(\eta_i|\eta_i < \rho_j - \alpha_j)$$ (2.5)

I now consider how differences in layoff rule selectivity across firms—driven by differences in $\alpha_j$—affect the types of workers laid off and the workers’ wage losses from job displacement.

Implicit differentiation of Equation (2.3) yields the following for the effect of $\alpha_j$ on the layoff rule productivity cutoffs for low- and high-firing-cost workers: $\frac{d\rho_j}{d\alpha_j} = 1 - \theta_j > 0$ and $\frac{d(\rho_j - \alpha_j)}{d\alpha_j} = -\theta_j < 0$. Therefore, $\frac{dF(\rho_j)}{d\alpha_j} = (1 - \theta_j) f(\rho_j) > 0$ and $\frac{dF(\rho_j - \alpha_j)}{d\alpha_j} = -\theta_j f(\rho_j - \alpha_j) < 0$, where $f$ is the density function of $\eta_i$. As expected, firms with higher firing costs (i.e., higher $\alpha_j$) lay off more $c_{Lj}$ workers and fewer $c_{Hj}$ workers than firms with lower firing costs and are therefore more selective in laying off their $c_{Hj}$ workers.

As $\alpha_j$ increases and the layoff rule becomes based more on firing costs and less on productivity overall, the post-displacement wage of $c_{Lj}$ workers increases because their layoff rule productivity cutoff increases, while the post-displacement wage of $c_{Hj}$ workers decreases because their layoff rule productivity cutoff decreases:

$$\frac{dw_{ij}}{d\alpha_j} = \begin{cases} (1 - \theta_j) \frac{f(\rho_j)}{F(\rho_j)} [\rho_j - E(\eta_i|\eta_i < \rho_j)] > 0, & \text{if } c_{ij} = c_{Lj} \\ -\theta_j \frac{f(\rho_j - \alpha_j)}{F(\rho_j - \alpha_j)} [\rho_j - \alpha_j - E(\eta_i|\eta_i < \rho_j - \alpha_j)] < 0, & \text{if } c_{ij} = c_{Hj} \end{cases}$$ (2.6)

Thus, layoff rules where firing costs play a larger role are especially beneficial for low-firing-cost displaced workers but especially harmful for high-firing-cost displaced workers (i.e., workers who have a low chance of being laid off according to the layoff rule), as the information signaled to outside firms is different for the two types of workers.

These results show that downsizing firms that find it especially costly to lay off their high-firing-cost workers lay off relatively more low-firing-cost workers and are less selective when laying off these workers. Also, their low-firing-cost workers receive higher post-displacement wages, and their high-firing-cost workers receive lower post-displacement wages. While this may seem to indicate that the average post-displacement wage of workers displaced by such
firms would be higher than the average post-displacement wage of workers displaced using more selective layoff rules, this actually does not hold unconditionally. The effect of layoff rule selectivity on the average wage of workers laid off by the firm is

\[
\frac{dw_j}{d\alpha_j} = \frac{\theta_j(1 - \theta_j)}{\gamma_j}[(\rho_j f(\rho_j) - (\rho_j - \alpha_j) f(\rho_j - \alpha_j)]
\]

(2.7)

This derivative cannot be definitively signed: it depends on the sign of the bracketed term, which is positive if \( F \) has a Uniform distribution or (for most values of \( \rho_j \) and \( \alpha_j \)) a Normal distribution, for example, but it can be negative. Intuitively, average wage losses could be larger for a layoff rule based more on firing costs (i.e., higher \( \alpha_j \)) than for a rule based more on productivity if the relatively few high-firing-cost workers laid off using the “firing-cost layoff rule” have especially large wage losses that outweigh the small wage losses of the many low-firing-cost workers laid off. Although this does not seem to occur for most choices of \( F \), this possibility could be eliminated for all choices of \( F \) by assuming that the layoff rule is imperfectly observable to outside firms for \( c_{Hj} \) workers but perfectly observable for \( c_{Lj} \) workers (since they have a greater incentive to communicate the layoff rule to outside firms in job interviews, for example). Therefore, under reasonable assumptions, the average post-displacement wage is higher for worker displaced using a “firing-cost layoff rule” than a “productivity layoff rule.” Since all workers receive the same first-period wage, the model also predicts that the average wage loss will be smaller for workers displaced using a “firing-cost layoff rule.”

2.2.2 Discussion of model

When I empirically test the predictions of this model, I use workers’ tenure as a proxy for firing costs. This is reasonable because of the strong positive relationship between tenure and severance pay, advance notice requirements, and employment protection legislation, as discussed in Section 1.3. Firing costs for low-tenure workers are low at all firms because employment protection legislation applies only to workers with at least six months of tenure. Firing costs for higher-tenure workers, however, likely vary across firms due to differences in collective bargaining agreements (which often include “last in, first out” (LIFO) seniority layoff requirements), firms’ own seniority-based layoff policies, or the application of dismissal laws. This justifies the model’s assumptions that firing costs vary across workers and firms. The assumption that firms’ layoff rules are observable to outside firms, which I discuss in more detail in Section 2.3.1.1, is also reasonable because these are large mass layoffs at large establishments, making it likely that details of the layoff would be observable to outside firms through various channels. Also, a model where layoff rules are imperfectly observed yields similar predictions.

The model’s results for changes in \( \alpha_j \) (how costly it is for the firm to fire its high-firing-cost workers) also hold for changes in \( \beta \), the upper bound of \( \alpha_j \)’s distribution. Since firing costs in Germany have decreased over time and are lower in recessions (as explained in Section 1.3), the model predicts fewer firing-cost layoff rules and more productivity layoff rules both over
time and in recessions. It also predicts heterogeneity in layoff rules across firms, as opposed to models that assume layoff decisions are entirely seniority-based or entirely productivity-based. I evaluate these results of the model in Section 1.4.

In Section 2.3.1, I test the model’s main asymmetric employer learning prediction by studying whether layoff rules that are based more on workers’ firing costs and thus less on productivity signal that the displaced workers are relatively more productive, resulting in smaller earnings and wage losses. I also test the prediction that pre-displacement wages of observationally equivalent workers do not vary by the layoff rule choice to further distinguish the model from symmetric employer learning models. In Section 2.3.2, I test the prediction that tenure layoff rules are especially beneficial for low-tenure workers (who are the most likely to be laid off according to the layoff rule) and especially harmful for high-tenure workers (who are the least likely to be laid off according to the layoff rule). I also explore whether the layoff rule provides a stronger signal for certain groups of workers based on their education or occupation, which could be allowed for in my model by assuming that firms obtain private information about only certain types of workers.

2.3 Results: Asymmetric employer learning

2.3.1 Testing for asymmetric employer learning

I use the estimated layoff rules from the previous section and the finding that substantial heterogeneity exists in establishments’ layoff decisions—with some more seniority-based and others more selective—to investigate how workers’ earnings losses from job displacement vary by the layoff rule choice. I do so in the context of my asymmetric employer learning model to test whether earnings losses are smaller for workers laid off using less selective layoff rules (i.e., tenure rules) because of the relatively positive signal that these layoffs send to the outside market about the workers’ productivity.

I follow Jacobson et al.’s (1993) worker-level event study approach for estimating the earnings losses of displaced workers. This methodology is essentially a generalization of the difference-in-differences design where different workers are treated at different times, and it allows for the inclusion of a control group of non-displaced workers and the estimation of pre-displacement effects and short- and long-run displacement effects.

The model I estimate is:

\[ y_{it} = \alpha_i + \gamma_t + \beta X_{it} + \sum_{k=-12}^{20} \delta_k D_{it}^k + \varepsilon_{it} \] (2.8)

\( y_{it} \) is quarterly earnings for worker \( i \) in quarter \( t \), \( \alpha_i \) is a worker fixed effect, \( \gamma_t \) is a time effect, \( X_{it} \) includes time-varying worker characteristics (age, education dummies, and their interaction), and \( D_{it}^k = 1 \) if the worker is displaced \( k \) quarters ago (the \( D_{it}^k \) variables are essentially lag and lead variables of the displacement dummy). The \( \delta_k \) coefficients are the coefficients of interest and provide the earnings losses from job displacement for each quarter,
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starting 12 quarters before the layoff event and ending 20 quarters after it. In the following figures, I express earnings losses relative to mean counterfactual earnings, which are calculated by setting $D_{ik}^{k} = 0$ for workers in the treatment group for all $k$, obtaining the predicted worker-level counterfactual earnings, and then averaging over workers for each quarter.\footnote{This mitigates the impact of differences in pre-displacement earnings levels across the layoff rule groups (seen in Figure A.8). When I use absolute earnings losses (i.e., in Euros) instead of relative earnings losses, the difference in earnings losses across layoff rules is even larger than the difference seen in Figure A.9. Also, the following results are similar when calculating relative earnings losses using worker’s pre-displacement mean earnings instead.}

I estimate these models for the 1990s, the most recent decade with a sufficiently long post-displacement period, which pools all of the worker-level layoffs in the decade to estimate the average cost of displacement. The treatment group comprises full-time, non-trainee displaced workers ages 25-50 who do not return to the establishment in the next five years. I include workers with zero earnings as long as they do not completely drop out of the data in the five years after the layoff. Since the data cover all private-sector employees in all of Germany, most workers with zero earnings should be unemployed.\footnote{Including workers with zero earnings instead of only using workers with positive earnings each year doubles the sample size (which is especially important when I take finer cuts of the data for my final results) and allows me to include workers who have an extended spell of unemployment after job displacement, which is quite common in Germany. It also allows me to include workers who experience additional job loss (and subsequent unemployment) after the initial layoff, which is a major component of the cost of job displacement (Stevens, 1997). My main results hold when only using workers who have positive earnings or unemployment insurance receipt each year, although the estimates are less precise due to the smaller sample size.} I divide the displaced workers into groups based on the type of layoff rule their establishment used and estimate Equation (2.8) separately for each group to test my model of asymmetric employer learning. Table A.5 reports summary statistics for the workers in the four layoff rule groups.

I use the same control group for each event study to facilitate comparisons of the treatment groups. The control group comprises full-time, non-trainee workers ages 25-50 who are at non-downsizing establishments that have at least 100 employees and are not in the industries I exclude in my mass layoff definition in Table B.1. I also require that they work at the establishment for at least eight years, since I do not observe these workers before or after they leave their current establishment. The estimated earnings losses in the following figures, therefore, are relative to workers who stay at their (non-downsizing) establishment for the entire eight-year period. Since my focus is on differences in earnings losses across different treatment groups and I use the same control group for each event study, this choice of control group should not affect my main findings.

Although my model’s main predictions are in terms of wage losses, an extension of the model predicts that workers who are revealed to be less productive experience not only larger wage losses but also lower employment rates after the layoff.\footnote{For example, outside employers’ beliefs about a displaced worker’s productivity may be so pessimistic that no one is willing to hire the worker or it takes an extended period of time for the worker to find another job, as discussed by Gibbons and Katz (1991).} I focus on the effect of layoff rules on workers’ earnings losses to study both the wage and employment effects.
simultaneously and because unemployment duration is a much larger part of the cost of job loss in Germany than are wage losses (partially due to wage rigidity) (Burda and Mertens, 2001). Also, the earnings variable is more informative than the wage variable, which measures daily wage (spell earnings divided by spell length) instead of hourly wage. I show, however, that there are differences in wage losses by layoff rule for reemployed workers as well, although the differences are not as large as they are for earnings losses.

In Figure A.8, I plot the raw mean earnings for the treatment group of displaced workers divided into the four layoff rule groups from $k$-means clustering (described in Section 1.4.3) and for the control group. Without using the event study methodology in Equation (2.8), it appears that workers in the four layoff rule groups have similar earnings pre-trends before the displacement event, with workers laid off using a tenure layoff rule (laying off workers with low tenure) hurt less than workers in the other three layoff rule groups, especially the wage rule group (laying off workers with low relative wages). Workers in the tenure rule and other rule groups also have lower pre-displacement earnings levels.

Figure A.9 presents the main test of asymmetric employer learning. I plot the $\delta_k$ coefficients from Equation (2.8) in event time, which shows the effect of job displacement leading up to and after the layoff event. Figure A.9(a) shows the earnings losses from displacement for all four layoff rules, while Figure A.9(b) shows only the tenure and wage layoff rules and their confidence intervals (confidence intervals for the other event studies in this chapter are approximately the same size). A model such as the one in Section 2.2 with asymmetric employer learning and heterogeneous firing costs predicts that earnings losses are smaller for layoff rules that are based more on workers’ firing costs (i.e., tenure) and less on productivity, since these layoff rules signal less negative information. This is indeed what I find: workers laid off using tenure layoff rules have significantly smaller earnings losses than workers laid off using the other layoff rules, especially rules where the establishment lays off its workers with low relative wages (a proxy for workers’ productivity, as argued in Section 1.4.1), which is the most selective layoff rule group. Occupation rules likely lie between the tenure and wage rules in terms of selectivity, since many of these workers are laid off simply because they happen to be in the wrong occupation or department at the establishment, while other establishments may target less productive occupations to lay off.\footnote{The establishments that use an occupation rule do not appear to lay off occupations with lower future employment growth, so the concern that the occupations being selected are “dying” ones is likely not a major issue.}

The event studies show that workers laid off using an occupation rule fare better than workers laid off using a wage rule and worse than workers laid off using a tenure rule. The average worker laid off using a tenure layoff rule has earnings losses that are 13 percentage points less than the average worker laid off using a wage rule one quarter after the layoff, a 25% difference.\footnote{When I only include workers who have positive earnings or unemployment insurance receipt each year, this difference is even larger (16 percentage points, 38%). Also, this additional sample restriction makes my estimate of mean earnings losses in the year after the layoff closer to Schmieder et al.’s (2010) estimate (who use this same sample restriction and German data): my estimate drops from 52% to 30%, which is}
to be persistent throughout the five years after the layoff, although they diminish somewhat.\footnote{The difference in earnings losses between tenure and wage rules 20 quarters after the layoff is 7 percentage points (20%). The signal sent by the layoff rule is likely stronger closer to the layoff event.}

In addition, the model predicts that workers’ pre-displacement earnings do not vary by the layoff rule used, which appears to be true based on the similar earnings pre-trends.

I conduct similar event studies for the effect of layoff rules on displaced workers’ wage losses and reductions in days worked per quarter (the two components of earnings losses). The wage event study (Figure A.10) only includes workers with positive earnings each year, which reduces the sample size by 45%, while the days worked per quarter event study (Figure A.11) continues to include workers with zero earnings. I find the same patterns for wages and days worked as I do for earnings, although the days worked results match more closely.\footnote{As mentioned above, one shortcoming of the wage variable is that it measures daily wage (spell earnings divided by spell length), making it less meaningful than an hourly wage measure. I am also not able to include workers with zero earnings when using the wage variable.} Reemployed workers displaced using a wage layoff rule have especially large wage losses. While differences in recall expectations across the layoff rule groups could affect the results for days worked (and consequently for earnings) through an effect on workers’ search effort, this should not be a problem in this setting for the following reasons: my mass layoff definition excludes layoffs where most of the displaced workers are eventually rehired, my event study treatment group excludes rehired workers, and the mean recall rate for these establishments is only 2.3%. These two figures also show that reductions in days worked plays a much larger role in displaced workers’ earnings losses than reductions in wages, while wage losses are more persistent.

Since workers revealed to be less productive are also likely to have lower match quality at their next job, I present coefficient estimates from Cox proportional hazards models of the duration of workers’ next job after displacement in Table A.6, controlling for the layoff rule and a rich set of worker and establishment characteristics.\footnote{If wages are not perfectly flexible, then less productive workers have fewer potential employers that are interested in hiring them. I use job duration as a measure of job match quality, as in Card et al. (2007). Also, I take into account the censored nature of this variable for workers who are still at their next job in the last quarter of the data (the fourth quarter of 2010).} Workers laid off using a tenure rule have a 3.5% reduction in the hazard rate of leaving their next job relative to workers laid off using one of the other three rules. In column (2) I show the effect of the three main establishment-level layoff coefficients themselves instead of the layoff rule groups from k-means clustering, and I find similar results: workers laid off using a tenure-based rule now have an 8% reduction in the hazard rate of leaving their next job. Tenure-based layoffs, therefore, lead to not only smaller earnings losses but also better match quality at the next job. As most of the earnings benefit of being displaced in a tenure-based layoff comes from higher post-displacement employment rates, it is noteworthy that my main findings are not caused by these workers finding jobs faster because they settle for lower match quality.

essentially the same as Schmieder et al.’s (2010) estimate.
2.3.1.1 Discussion of results

Even though workers laid off using the least selective tenure layoff rules have smaller earnings losses than those laid off using more selective rules, they still experience substantial earnings losses of approximately 40% one year after the layoff, compared to 50% for workers displaced using other rules. This suggests that selection bias is not of first-order importance in past estimates of the cost of job loss to workers, although it does appear to play some role (consistent with the findings of Von Wachter et al. (2011)). This difference in earnings losses also provides an estimate of the contribution of asymmetric information to the cost of job loss: the negative signal of being laid off appears to be a component of the cost of job loss, but it is clearly not the entire explanation.\(^{12}\)

Although I cannot empirically verify the extent to which outside employers observe a downsizing establishment’s layoff rule, the fact that these are large mass layoffs at large establishments makes it likely that at least some details of the layoff would be reported in the news or observable to outside employers through other channels. For example, potential employers could verify the layoff rule through their business contacts, job applicants’ references, or employment agencies where unemployed workers register (these agencies are also provided details about mass layoffs by downsizing firms). Workers themselves could communicate the layoff rule to potential employers, and it would be in their self-interest to do so if they were laid off according to the layoff rule (e.g., if a low-tenure worker was laid off using a tenure rule). Additionally, layoff rules often depend on collective bargaining agreements, which should be observable to outside employers, and I also find that they are correlated with observable establishment characteristics in Table B.3, such as industry, size, age, and mean wage. Finally, a model where layoff rules are imperfectly observed yields similar predictions, as long as they are at least partially observable for at least some of the downsizing establishments. And assuming I am controlling for enough important differences across the four layoff rule groups (I include additional worker and establishment controls in the following results), if the layoff rules were completely unobservable to outside employers, then I would not expect to find any differences in earnings losses by layoff rule.

While my main findings may be consistent with a few specialized symmetric employer learning models, most fail to explain why wages do not fall for less productive workers before the layoff, as discussed in Section 2.2. In my model, wages do not reflect firms’ private information about their workers’ productivity and the layoff rule provides the only signal of this information, so pre-displacement wages of observationally equivalent workers do not vary by the layoff rule choice. If employer learning were symmetric, workers laid off using selective layoff rules would experience wage losses leading up to the layoff event (as the market learned their productivity) instead of at the time of the layoff, since the layoff rule would not provide any additional information to the outside market. The event studies show minimal differences in pre-displacement earnings trends by layoff rule and large

\(^{12}\)A number of other explanations have been proposed for the large and persistent cost of job displacement, including the loss of specific human capital (e.g., Neal, 1995), loss of rents (e.g., Blanchflower et al., 1996), and loss of job security and its interaction with the evolution of human capital (Jarosch, 2015).
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differences in earnings losses at the time of the layoff. Furthermore, there are no symmetric employer learning models (to the best of my knowledge) that are consistent with my finding below that selective layoffs are particularly costly for less-educated workers and workers in high-skilled service occupations. Therefore, while my findings may not offer conclusive proof of asymmetric employer learning, they are more consistent with asymmetric learning than symmetric learning and improve on the few previous tests of this difficult-to-study question.\textsuperscript{13}

One other potential concern is that outside employers may infer displaced workers’ productivity directly (e.g., in job interviews) instead of through the layoff rule. If this were the case, though, outside employers would not be obtaining new information about the workers because anything that can be observed prior to hiring a worker is already part of the public information. Essentially, the outside market does not learn anything by direct inspection \textit{after} the layoff that it did not learn by direct inspection \textit{before} the layoff. Earnings losses, consequently, would not differ by the layoff rule choice in this case, since the outside market’s information about the displaced workers would not have changed. Also, if layoffs were performed based on productivity and outside employers inferred displaced workers’ productivity directly, this would imply that downsizing employers had information about the workers’ productivity that outside employers did not.

2.3.1.2 Robustness checks

As seen in Table A.5, there are differences in mean worker characteristics across the four layoff rule groups that my event study models do not control for. To make sure that the differences in earnings losses by layoff rule are not due to other differences across the four groups, I estimate the same models using propensity score weighting to control for additional worker and establishment characteristics.\textsuperscript{14} I do so by estimating a multinomial logit model of the layoff rule group that workers are in as a function of establishment characteristics (including mean characteristics of the workers displaced by the establishment), obtaining the predicted probabilities, and then weighting workers by the inverse probability that they are in the layoff rule group that they are actually in.\textsuperscript{15} Weighting workers this way when estimating the event study models gives more weight to workers who look less like the other workers in their layoff rule group (and thus more like the workers in the other groups), essentially balancing the establishment and worker characteristics across the four groups.

Figure A.12 shows that propensity score weighting reduces the differences in earnings losses across the layoff rule groups slightly, but workers laid off using a tenure rule are

\begin{itemize}
  \item \textsuperscript{13}The impossibility of directly measuring employers’ information about their workers’ productivity makes indirect and imperfect tests like this the best we can do.
  \item \textsuperscript{14}For example, workers laid off using a tenure layoff rule have 5.7 years of tenure on average compared to 6.9 for the other layoff rules, and workers with less tenure are hurt less when displaced, so this may be contributing to the differences in earnings losses by layoff rule.
  \item \textsuperscript{15}I use establishment-level variables in this estimation because that is the level that layoff rules vary at. I include establishment age, size, industry, mean wage, mean tenure, layoff size, year, and state. I also include the following mean characteristics of the workers displaced by the establishment: tenure, age, education, sex, and occupation.
\end{itemize}
still hurt significantly less than workers laid off using another rule. There is now an 11 percentage point (19%) difference in earnings losses between the tenure and wage layoff rules one quarter after the layoff. To further investigate whether differences in workers’ tenure across the groups may be contributing to the reduced earnings losses for the tenure rule group, I use propensity score weighting with the displaced workers’ mean tenure as the only regressor to ensure that the four groups are perfectly balanced in this dimension, and the results barely change. Propensity score weighting also has a minimal effect on the event studies for wage losses and reductions in days worked per quarter in Figures A.10 and A.11. My ability to control for factors that have been found to bias Gibbons and Katz’s (1991) main finding—combined with my more direct measure of how selective different job displacements are and my more comparable set of displacements—makes this test an improvement on past studies of asymmetric employer learning.

Excluding the 40% of the mass layoffs where the establishment exits or drops to fewer than 20 employees in the next year—mass layoffs where the layoff rule would not be expected to signal as much information because they are more similar to plant closings where all workers are laid off—shows even stronger evidence of asymmetric employer learning. Tenure layoff rules are now associated with drastically smaller earnings losses than the other three groups, as seen in Figure A.13. The average worker laid off using a tenure layoff rule has earnings losses that are 18 percentage points less than the average worker laid off using a wage rule one quarter after the layoff—a substantial 43% difference.

I conduct two final event studies. First, I use an alternative way of putting the mass layoffs into layoff rule groups by dividing them into quartiles based on the low-tenure coefficients from the establishment-level layoff models (as in the previous Cox hazard model). Focusing only on differences in the extent to which establishments lay off their low-tenure workers, I find that establishments with low-tenure coefficients in the fourth quartile (i.e., ones most likely to be using a “last in, first out” (LIFO) layoff rule) have the smallest earnings losses for their workers (see Figure B.6). Earnings losses are monotonically related to the weight that establishments put on seniority in their layoff rules, and these differences in earnings losses grow slightly when using propensity score weighting to control for additional differences across the four groups (see Figure B.7). Similar event studies for the log-wage layoff coefficients and the occupational group layoff p-values show that displaced workers’ earnings losses are increasing in the extent to which establishments lay off their low-wage workers and decreasing in the weight establishments put on occupation in the layoff rule.

A more continuous version of Gibbons and Katz’s (1991) model predicts that workers displaced in larger mass layoffs (ones that are closer to a plant closing) are hurt less than workers displaced in smaller mass layoffs (where more discretion in selecting workers to

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16 The earnings loss differences also hold when restricting the sample to workers with less than one, three, or five years of tenure, which reduces the difference in mean tenure across the four groups. Restricting the sample to workers with more than three years of tenure, who are less likely to be voluntarily leaving the downsizing establishment, also confirms my main findings.

17 I use propensity score weighting in a similar way as above but also control for the log-wage layoff coefficients and the p-values on the occupational group F-statistics.
lay off is possible). I do not find evidence for this prediction (see Figure B.8). Workers displaced in mass layoffs where a larger fraction of the workforce is laid off actually appear to be hurt slightly more. Since subsequent studies show the importance of controlling for differences across different types of displacements and Gibbons and Katz point out a possible counteracting local labor market effect for plant closings, I use propensity score weighting to control for observable differences across the four groups (including differences in the layoff rule coefficients). The results in Figure B.9 are more in line with Gibbons and Katz’s main prediction (especially for the smallest mass layoffs) but fail to provide conclusive evidence that larger mass layoffs are associated with smaller earnings losses. This suggests that testing for asymmetric employer learning using layoff size or the plant closing vs. layoff distinction may not be as effective as the approach I take that uses a more comparable set of displacements and a more direct measure of layoff selectivity.

As further robustness checks, I perform similar event studies using quartiles of the $R^2$ values from the tenure-only establishment-level layoff regressions shown in Figure A.6, which confirm my main findings. I also conduct the previous event studies without the control group, in which case identification of the effect of displacement comes from the differential timing of displacement among the treatment group, and I find similar results (although the estimates are less precise due to the smaller sample size). Analyses for the 1980s give similar results, with the exception of occupation layoff rules being more harmful to workers than in the 1990s. Since my data only go through 2010, I do not have enough post-displacement years to estimate the same event studies for the 2000s. Event studies for 2000-2005 and ones that include a shorter post-period show smaller differences in earnings losses by layoff rule. This may be due to the decline over time in layoff rules based on observable characteristics (see Section 1.4), a decline in asymmetric employer learning over time (due to advances in information technology that decrease the degree of asymmetry in the learning process, for example), or a lack of power because of the smaller sample size.

### 2.3.1.3 Results over the business cycle

I conclude this section by exploring how the information signaled by the layoff rule varies by the state of the business cycle. Figure A.14 shows my main event study model estimated separately for workers displaced in recessions and expansions. There does not appear to be much difference in earnings losses by layoff rule for workers displaced in recessions, except for those laid off using a selective wage rule. Earnings losses for workers laid off in expansions, on the other hand, differ significantly by the layoff rule used, with workers laid off using a tenure rule faring especially well.\footnote{Earnings losses are only slightly more dispersed overall for workers laid off in expansions than recessions.}

While this finding may indicate that downsizing firms have more private information about their workers in expansions, it seems at least as likely that it is due to differences in how outside firms use the layoff rule signal. One possible explanation is that hiring firms may believe that the unemployment pool is of higher quality and more homogeneous in re-
cessions (because more workers are laid off due to bad luck) and thus are less likely to use the layoff rule to try to infer workers’ productivity (because there is a cost to determining the layoff rule used, for example). This finding may also be a result of differences in the types of firms that hire in recessions (there appears to be a slight increase in the relative hiring rates of low-paying establishments in recessions) or by increased homogeneity in downsizing firms in recessions (although I do not find any significant differences in the mean or variance of downsizing establishments’ characteristics over the business cycle, as mentioned in Section 1.4.2). Regardless of the correct explanation, this finding appears to be robust: it holds when using propensity score weighting to control for other differences across the four layoff rule groups and also when using the regression framework in the next section.

2.3.2 Asymmetric employer learning and worker characteristics

As a final contribution, I add to recent work on variation in the extent of employer learning across groups of workers. Arcidiacono et al. (2010) test a symmetric employer learning model and find that ability is observed nearly perfectly for recent college graduates but revealed to the labor market more gradually for high school graduates, possibly because resumes contain more information for college graduates (e.g., grades, major, standardized test scores, the college attended). Schönberg (2007) finds that employer learning appears to be symmetric for high school graduates but possibly asymmetric for college graduates (I discuss her findings in more detail below). Mansour (2012) presents evidence that symmetric employer learning varies by occupation, although he does not study which specific occupations it occurs more for. My analysis is also motivated by the conflicting findings in the literature on whether employer learning is symmetric or asymmetric, which suggest that the answer may vary for different groups of workers.

If incumbent employers learned more about the productivity of certain types of workers than outside employers, then the layoff rule would signal more information about these workers to the outside market, resulting in greater earnings loss differences between selective and non-selective mass layoffs for these workers. In other words, being laid off using a tenure layoff rule instead of a more selective rule may be a positive signal for some groups of workers (workers who the incumbent firm has private information about) but signal no information for other groups of workers (workers who outside employers and the incumbent employer have the same information about). I test for this in Table A.7 by regressing the percentage change of displaced workers’ earnings in the year after the layoff relative to earnings in

\footnote{As in Nakamura’s (2008) model and Kosovich’s (2010) empirical work. This is not necessarily inconsistent with my finding that layoff rules are more selective in recessions because workers laid off in mass layoffs compose only part of the unemployment pool, and displacements due to plant closings (where presumably more high-productivity workers are laid off) are more common in recessions (Davis et al., 1998; Mueller, 2015). In addition, the event study still shows a difference in earnings losses in recessions between workers laid off using a wage rule and other rules. This effect may also lead to a reinforcing effect of workers who are laid off in expansions exerting more effort to communicate the layoff rule to outside firms so that they are not seen as lemons (unless they are laid off using a selective rule).}
the year before the layoff (censored at 1, which affects 1% of the workers) on dummies for the layoff rule used and a rich set of worker and establishment controls (described in the table’s notes), allowing for interactions between the “Tenure rule” dummy and worker characteristics.

Using the same sample of displaced workers as the event studies in the previous section, I first evaluate whether workers’ pre-displacement earnings (log earnings in the year before the layoff) vary by layoff rule in column (1). The finding of no significant differences across the layoff rule groups provides further evidence that my results are more consistent with asymmetric employer learning than symmetric employer learning, as discussed in Section 2.3.1.1. Column (2) performs a similar test as the previous event studies by estimating the effect of layoff rules on earnings losses from job displacement. I once again find that workers laid off using a tenure rule are hurt significantly less than workers laid off using any of the other rules (especially wage rules).

In column (3), I test for differences in the signal value of tenure layoff rules by the state of the business cycle, tenure, education, and occupation. I first confirm the result in the last event studies that the layoff rule signals more information to outside employers in expansions than recessions. Workers laid off using a tenure rule in expansions have significantly smaller earnings losses than workers laid off using other rules, while this effect disappears in recessions. I then test my model’s prediction that being laid off in a tenure-based layoff is especially beneficial for low-tenure displaced workers but especially harmful for high-tenure displaced workers (i.e., workers who have a low chance of being laid off according to the layoff rule). The “Tenure rule x log tenure” interaction term suggests that this is true: the benefit of being laid off using a tenure rule is especially large for low-tenure workers, while workers with the highest tenure are actually worse off from being laid off using a tenure rule. This also provides evidence that tenure layoff rules result in more favorable outcomes for displaced workers because of the information they signal and not for some other reason, because instead of benefiting all workers, they only help those that the asymmetric employer learning model predicts should be benefited.

Being displaced in a more selective mass layoff (i.e., not a tenure-based layoff) is especially costly for workers with less education—specifically, those whose education ended before an apprenticeship or is not reported in the data.\textsuperscript{20} For a worker in the reference group (a low-skilled manual worker with one quarter of tenure who has not completed an apprenticeship and who is laid off in an expansion), the 18 percentage point earnings loss difference between being laid off using a tenure layoff rule or another layoff rule drops to a 13 percentage point difference for workers who have completed an apprenticeship and only a 6 percentage point difference for college-educated workers. It appears that incumbent employers have more private information about their less-educated workers, resulting in outside employers using the layoff rule to infer the productivity of less-educated workers but not highly-educated

\textsuperscript{20}The “missing education” group is similar to the “no apprenticeship” group in terms of observable characteristics. 14% of the workers at downsizing establishments do not have an education level reported, 28% have completed less than an apprenticeship, 50% have completed an apprenticeship, and 8% have completed college.
CHAPTER 2. ASYMMETRIC EMPLOYER LEARNING

workers. A potential explanation for this finding is that less-educated workers are less able to signal their productivity to outside employers (e.g., through an informative resume), as in Arcidiacono et al.’s (2010) study of how symmetric employer learning varies by education.

Asymmetric employer learning also occurs more for workers in certain occupations. The layoff rule choice appears to signal more information for workers in high-skilled sales and engineering occupations than for workers in manual and low-skilled service occupations (controlling for differences in asymmetric employer learning by education). There is also suggestive evidence that the layoff rule signals more information for workers in managerial and professional occupations, although the (large) point estimates are only significant at the 18% and 25% levels. When I estimate the same regression using the layoff rule groups based on the low-tenure layoff coefficient quartiles (as in Figure B.6) instead of the groups from k-means clustering, I find that the “High LIFO x managerial” interaction is the largest occupation interaction and is significant at the 1% level. This also results in an insignificant “High LIFO x engineering” interaction, while the tenure and education interactions remain the same.

One possible explanation for these effects is that outside employers may find it more difficult to assess the output of workers in these occupations due to the nature of their work.21 I explore differences across occupations that may contribute to these results by using new data on the type of tasks that are commonly performed in each occupation (Dengler et al., 2014). These data are based on experts’ assessment and are similar to U.S. task data used in a number of studies (e.g., Autor et al., 2003). When I replace the “Tenure rule x occupation” interactions in column (3) with “Tenure rule x main task type” interactions, I find that employer learning is most asymmetric for occupations where the main tasks are “analytic non-routine,” followed by occupations where the main tasks are “interactive non-routine.” The interactions for “cognitive routine,” “manual non-routine,” and (especially) “manual routine” tasks are close to 0 and not statistically significant.

In this framework, a task is “routine” if it can be performed by a machine. “Analytic” refers to the need to think or analyze, and “interactive” denotes the need to communicate with others orally or through writing. “Manual” refers to activities that are performed with one’s hands, and “cognitive routine” tasks require minimal thinking and no physical work (e.g., data entry). If a machine can perform a task, a machine could also measure the output produced by the task, and potential employers would presumably find it easier to evaluate workers who perform such tasks prior to hiring. Therefore, employers appear to have more private information about their workers who perform tasks where the output is difficult to measure.22

To provide further evidence that these occupation effects are due to differences in asymmetric employer learning, I follow Mansour (2012) by utilizing a common prediction of

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21For example, the boss of a high-skilled sales manager is likely to have a much better idea of how productive the worker is and what his or her job actually entails than outside employers, while this may not be true for manual or low-skilled occupations.

22These results remain the same when using a more continuous measure of the importance of different types of tasks in occupations instead of classifying each occupation by its one largest task type.
employer learning models—namely, that the wage distribution becomes more dispersed as a cohort of workers accumulates experience. I calculate growth in the variance of log wage residuals in a similar way as Mansour, except I calculate it using panel data (following the same workers over time) instead of having to resort to a cohort-based analysis. I create a random sample of workers ages 18-25 who start their first full-time job (not including apprenticeships) between 1990 and 1995 at a non-downsizing establishment, and I obtain wage residuals by regressing log wages on age dummies (one for each year), education dummies, interactions of the education dummies with a quartic in age, year dummies, and occupation dummies. Then for each of nine initial occupational groups, I calculate the change in mean squared residuals between one and ten years of experience, which provides the growth in residual wage variance by initial occupational assignment.

In column (4) of Table A.7, I replace the interactions between the layoff rule dummy and occupations with the interaction between the layoff rule dummy and occupations’ mean growth in residual wage variance. The result confirms my finding that asymmetric employer learning varies by occupation: occupations that have higher growth in residual wage variance are ones for which the layoff rule provides more of a signal. While this approach does not reveal which occupations employers have more private information about, it provides support for the occupation-specific results in column (3) and for the more general idea that asymmetric employer learning varies by occupation. Directly examining how growth in residual wage variance varies by occupation shows that it is highest for the occupations with significant and marginally significant interactions in column (3) and close to zero for the two manual occupations and the low-skilled service occupation (as expected from the previous results).

As a robustness check, I confirm that the results in Table A.7 hold when calculating displaced workers’ earnings losses two or three years after the layoff instead of one. Conducting event studies as in the previous section by education and occupation confirms these findings as well. Most of these results hold for the 1980s, but there is not a large enough difference in earnings losses between tenure layoff rules and other rules in the 2000s to be able to study differences in asymmetric employer learning across groups of workers.

To summarize, it appears that employer learning is highly asymmetric for less-educated workers (who likely find it harder to signal their productivity to outside employers) and workers in high-skilled service occupations (where it is likely harder for outside employers to observe their output). I do not find evidence that incumbent employers have private information about their workers with high education or their workers in manual or low-skilled service occupations, either because employer learning is symmetric for these workers or because their productivity is perfectly observed by all employers at labor market entry. A critical component of this analysis is the ability to test for differences in asymmetric employer learning by both education and occupation simultaneously. Since the occupations where I find employer learning is most asymmetric have higher average levels of education, most of the significant education and occupation interactions shrink in size and lose their significance if I allow the tenure rule effect to vary only by education or only by occupation (the three occupations with the highest levels of education are managerial, engineering,
CHAPTER 2. ASYMMETRIC EMPLOYER LEARNING

and professional occupations). This may have implications for past work that allows for differences in employer learning in only one dimension. Schönberg’s (2007) finding that employer learning appears to be more asymmetric for college graduates than high school graduates, for example, may actually be capturing an occupation effect since she does not allow for employer learning to also vary by occupation (i.e., asymmetric learning may occur more for high-skilled service occupations, which have higher levels of education, and not vary by education). My results, therefore, provide new evidence on the types of workers that asymmetric employer learning occurs most for and also point out methodological concerns relevant to these types of questions.

2.4 Conclusion

Chapter 1 provides the first comprehensive evidence on how firms choose which workers to lay off, establishing a number of new facts about layoff decisions across establishments, over time, and over the business cycle. Using a unique employer-employee data set that contains detailed characteristics and complete employment histories of every worker at downsizing establishments, I find that most mass layoffs involve a significant level of discretion, with the most common layoff rules being based on workers’ relative tenure, relative wages, or occupation. I also shed light on questions in the macroeconomic literature by showing that layoffs have become less tenure-based and more selective since 1980 and that layoff rules are more selective in recessions.

An interesting question is how informative these findings may be for other countries. Significant changes in Germany’s labor market between 1980 and 2009 make it an instructive example that may provide insight into how layoff decisions are made in both more rigid and more flexible labor markets. For example, firms’ layoff rules in the U.S. are more likely to be similar to Germany’s layoff rules in the most recent decade (i.e., more selective, less seniority-based), since Germany’s recent liberalizing labor market reforms and decline in collective bargaining have decreased the differences between the two countries’ labor markets. Likewise, the amplifying business cycle effects over time indicate that countries with more flexible labor markets may experience more cyclicality in their firms’ layoff decisions (with more selective layoff rules in recessions). An intriguing area of future research would be to perform similar analyses for other countries, although this may not be possible for some time due to data limitations.

The model of asymmetric employer learning with heterogeneous firing costs I develop in Chapter 2 is consistent with my first set of results and allows me to test the open question of whether learning about workers’ productivity is symmetric or asymmetric across employers. I test the model’s predictions by combining the establishment-level layoff rule estimates with worker-level event studies to estimate how earnings losses of displaced workers vary by the layoff rule used. I find that the level of selection in the layoff matters: the market appears to use the layoff rule as a signal of workers’ productivity and rewards them accordingly. Workers laid off using tenure layoff rules are revealed to be more productive than workers
laid off using more selective rules and fare better in terms of post-displacement earnings, wages, days worked per quarter, and the duration of the next job (a measure of match quality)—all of which are robust to controls for a rich set of worker and establishment characteristics. I also find no difference in pre-displacement earnings trends or levels by layoff rule for otherwise observationally equivalent workers, suggesting that it is the layoff event that signals the firm’s private information to outside firms. My final results show that employer learning is especially asymmetric for less-educated workers and workers in high-skilled service occupations.

The distinction between asymmetric and symmetric employer learning has important labor market implications but has proven difficult to test. I improve on recent empirical work by estimating a direct measure of how discretionary different job displacements are, controlling for important differences across different types of displacements, and studying a more comparable set of displacements: mass layoffs at large establishments where the main difference is the layoff rule. The methodology I employ—combined with the size and detailed nature of the data—also allows me to explore differences in asymmetric employer learning by worker characteristics. I present some of the first evidence on what types of workers asymmetric employer learning occurs more for and show the importance of allowing for differences in employer learning by both education and occupation. My finding that employer learning is highly asymmetric for some groups of workers while potentially symmetric for others helps reconcile the conflicting results in the literature on whether employer learning is symmetric or asymmetric: the answer may depend on the context, varying for different groups of workers or firms.
Bibliography


OECD and Jelle Visser. ICTWSS database on institutional characteristics of trade unions, wage setting, state intervention and social pacts. *Institute for Advanced Labour Studies*, 2011.


Appendix A

Tables and figures

Figure A.1: Number of mass layoffs over time and the business cycle

Notes: Shaded areas indicate recessions. Mass layoffs are a 30-90% drop in quarterly employment at an establishment with at least 100 workers (plus the sample corrections described in Table B.1). The total number of mass layoffs is 4,414.
Figure A.2: Number of workers and fraction laid off by tenure

Note: Sample is workers at downsizing establishments in the quarter of the mass layoff (926,057 workers).
## Table A.1: Determinants of worker layoff

<table>
<thead>
<tr>
<th></th>
<th>1980-89</th>
<th>1990-99</th>
<th>2000-09</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age under 30</td>
<td>0.047**</td>
<td>0.023**</td>
<td>0.030**</td>
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<tr>
<td></td>
<td>(0.005)</td>
<td>(0.005)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Age over 50</td>
<td>-0.048**</td>
<td>-0.002</td>
<td>-0.037**</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.010)</td>
<td>(0.006)</td>
</tr>
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<td>Female</td>
<td>-0.010</td>
<td>-0.009</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.006)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Non-German</td>
<td>0.053**</td>
<td>0.026**</td>
<td>0.027**</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.005)</td>
<td>(0.006)</td>
</tr>
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<td>Part-time</td>
<td>-0.031*</td>
<td>-0.024*</td>
<td>-0.042**</td>
</tr>
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<td>(0.013)</td>
<td>(0.011)</td>
<td>(0.010)</td>
</tr>
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<td>-0.259**</td>
<td>-0.225**</td>
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<td>(0.026)</td>
<td>(0.024)</td>
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<td>-0.007</td>
<td>0.000</td>
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<td>(0.005)</td>
<td>(0.005)</td>
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<td>-0.024*</td>
<td>-0.019*</td>
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<td>(0.018)</td>
<td>(0.010)</td>
<td>(0.009)</td>
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<td>Yes</td>
<td>Yes</td>
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<td>Tenure F-statistic</td>
<td>44.5</td>
<td>28.6</td>
<td>26.2</td>
</tr>
<tr>
<td>Wage F-statistic</td>
<td>11.5</td>
<td>20.6</td>
<td>28.3</td>
</tr>
<tr>
<td>Education F-statistic</td>
<td>9.5</td>
<td>2.0</td>
<td>1.9</td>
</tr>
<tr>
<td>Occupation F-statistic</td>
<td>79.3</td>
<td>23.8</td>
<td>51.9</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.22</td>
<td>0.18</td>
<td>0.18</td>
</tr>
<tr>
<td>Dependent variable mean</td>
<td>0.49</td>
<td>0.47</td>
<td>0.47</td>
</tr>
<tr>
<td>Number of workers</td>
<td>253,158</td>
<td>390,508</td>
<td>257,994</td>
</tr>
</tbody>
</table>

Notes: Coefficient estimates from decade-level linear probability models of workers’ probability of being laid off. See Figures A.3 and A.4 for the tenure and wage coefficients from the same regressions. Sample is workers at downsizing establishments in the quarter of the mass layoff. Additional controls: missing education, tenure censored. Standard errors clustered at establishment level. * $p < 0.05$, ** $p < 0.01$. 
Figure A.3: Determinants of worker layoff: Tenure coefficients

Notes: Most standard errors are less than 0.015. See Table A.1 and Figure A.4 for the other coefficients.

Figure A.4: Determinants of worker layoff: Wage decile coefficients

Notes: Most standard errors are less than 0.01. See Table A.1 and Figure A.3 for the other coefficients.
Table A.2: Coefficients from establishment-level layoff regressions

<table>
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<th>Mean</th>
<th>Median</th>
<th>s.d.</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low tenure</td>
<td>0.092</td>
<td>0.083</td>
<td>0.171</td>
<td>4,352</td>
</tr>
<tr>
<td>Log daily wage</td>
<td>-0.176</td>
<td>-0.156</td>
<td>0.309</td>
<td>4,414</td>
</tr>
<tr>
<td>Age under 30</td>
<td>0.044</td>
<td>0.041</td>
<td>0.116</td>
<td>4,382</td>
</tr>
<tr>
<td>Age over 50</td>
<td>-0.040</td>
<td>-0.036</td>
<td>0.109</td>
<td>4,376</td>
</tr>
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<td>Female</td>
<td>-0.025</td>
<td>-0.023</td>
<td>0.107</td>
<td>4,366</td>
</tr>
<tr>
<td>Non-German</td>
<td>0.025</td>
<td>0.026</td>
<td>0.115</td>
<td>4,215</td>
</tr>
<tr>
<td>Part-time</td>
<td>-0.059</td>
<td>-0.057</td>
<td>0.234</td>
<td>3,435</td>
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<tr>
<td>Trainee</td>
<td>-0.312</td>
<td>-0.292</td>
<td>0.425</td>
<td>2,855</td>
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<td>Education (Omitted: No apprenticeship)</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>- Apprenticeship</td>
<td>-0.009</td>
<td>-0.009</td>
<td>0.089</td>
<td>4,360</td>
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<td>- College</td>
<td>-0.007</td>
<td>-0.007</td>
<td>0.214</td>
<td>3,292</td>
</tr>
<tr>
<td>Occupation (Omitted: Low-skilled service)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Low-skilled manual</td>
<td>0.062</td>
<td>0.057</td>
<td>0.215</td>
<td>3,376</td>
</tr>
<tr>
<td>- High-skilled manual</td>
<td>0.027</td>
<td>0.027</td>
<td>0.212</td>
<td>3,624</td>
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<tr>
<td>- Engineering</td>
<td>-0.007</td>
<td>-0.003</td>
<td>0.267</td>
<td>3,297</td>
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<td>- Professional</td>
<td>-0.013</td>
<td>-0.000</td>
<td>0.347</td>
<td>1,364</td>
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<tr>
<td>- Low-skilled sales or administrative</td>
<td>-0.055</td>
<td>-0.053</td>
<td>0.298</td>
<td>3,151</td>
</tr>
<tr>
<td>- High-skilled sales or administrative</td>
<td>-0.071</td>
<td>-0.066</td>
<td>0.241</td>
<td>4,116</td>
</tr>
<tr>
<td>- Managerial</td>
<td>-0.060</td>
<td>-0.055</td>
<td>0.317</td>
<td>3,000</td>
</tr>
</tbody>
</table>

Notes: Other variables included in regressions: wage censored, missing education, missing occupation. Coefficients shrunk according to Equation (1.2).

Figure A.5: Coefficients from establishment-level layoff regressions: Kernel density estimates
Figure A.6: $R^2$'s from different establishment-level layoff models

Note: Adjusted $R^2$ values from establishment-level layoff models that include as regressors tenure dummies only, wage decile dummies only, occupation dummies only, and the complete set of regressors listed in Table A.2.
Figure A.7: Layoff rules over time and the business cycle

Mean low-tenure layoff coefficient

Note: Correlations: $r = -0.65$ ($p = 0.01$), Post-1995 $r = -0.68$ ($p = 0.01$)

Mean female layoff coefficient

Note: Correlations: $r = 0.65$ ($p = 0.01$), Post-1995 $r = 0.44$ ($p = 0.11$)
Figure A.7 (cont.): Layoff rules over time and the business cycle

Mean college layoff coefficient

Note: Correlations: $r = 0.02$ (p = 0.91), Post–1995 $r = -0.65$ (p = 0.01)

Mean log–wage layoff coefficient

Note: Correlations: $r = 0.13$ (p = 0.49), Post–1995 $r = -0.73$ (p = 0.01)
Figure A.7 (cont.): Layoff rules over time and the business cycle

Mean wage–residual layoff coefficient (separate model)

![Graph showing layoff rules over time and the business cycle](image)

Note: Correlations: $r = -0.65$ (p = 0.01), Post–1995 $r = -0.41$ (p = 0.13)

Mean $R^2$ from wage–residual layoff model (separate model)

![Graph showing $R^2$ from layoff model](image)

Note: Correlations: $r = 0.54$ (p = 0.01), Post–1995 $r = 0.42$ (p = 0.12)
### Table A.3: Layoff rule grouping from $k$-means clustering

<table>
<thead>
<tr>
<th>Layoff rule</th>
<th>Mean low-tenure coefficient</th>
<th>Mean log-wage coefficient</th>
<th>Mean occupation $p$-value</th>
<th>Number of mass layoffs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tenure rule</td>
<td>0.276</td>
<td>-0.106</td>
<td>0.007</td>
<td>894</td>
</tr>
<tr>
<td>Wage rule</td>
<td>0.001</td>
<td>-0.525</td>
<td>0.008</td>
<td>904</td>
</tr>
<tr>
<td>Occupation rule</td>
<td>0.013</td>
<td>0.027</td>
<td>0.004</td>
<td>1,402</td>
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<tr>
<td>Other rule</td>
<td>0.118</td>
<td>-0.198</td>
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<td>1,111</td>
</tr>
<tr>
<td>All rules</td>
<td>0.092</td>
<td>-0.175</td>
<td>0.029</td>
<td>4,313</td>
</tr>
</tbody>
</table>

Note: Mass layoffs divided into four groups using $k$-means clustering based on establishment-level layoff coefficients on low-tenure, coefficients on log-wage, and $p$-values on occupation $F$-statistics (all three standardized).

### Table A.4: Establishment exit hazard rates and layoff rules

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<tr>
<th>Layoff rule (Omitted: Other rule)</th>
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<th>(2)</th>
<th>(3)</th>
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</thead>
<tbody>
<tr>
<td>- Tenure rule</td>
<td>$-0.107^*$</td>
<td>$-0.052$</td>
<td>$-0.001$</td>
</tr>
<tr>
<td></td>
<td>(0.053)</td>
<td>(0.055)</td>
<td>(0.056)</td>
</tr>
<tr>
<td>- Wage rule</td>
<td>$-0.110^*$</td>
<td>$-0.170^{**}$</td>
<td>$-0.190^{**}$</td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.054)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>- Occupation rule</td>
<td>$-0.064$</td>
<td>$-0.024$</td>
<td>$-0.114^*$</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.049)</td>
<td>(0.050)</td>
</tr>
</tbody>
</table>

Controls: No, Yes
Fraction laid off control: No, No, Yes
Number of establishments: 4,313, 4,313, 4,313

Notes: Coefficient estimates from Cox hazard models of establishments’ survival after the mass layoff. Controls: establishment mean wage, standard deviation of wages, standard deviation of residual wages, size, age, industry, mean tenure, mean age, fraction female, fraction part-time, fraction trainee, fraction non-German, education distribution, occupation distribution, state, and year. Robust standard errors in parentheses. $^* p < 0.05$, $^{**} p < 0.01$. 
## Table A.5: Mean characteristics of displaced workers by layoff rule

<table>
<thead>
<tr>
<th></th>
<th>Tenure rule</th>
<th>Wage rule</th>
<th>Occupation rule</th>
<th>Other rule</th>
</tr>
</thead>
<tbody>
<tr>
<td>Daily wage (1995 Euros)</td>
<td>74.0</td>
<td>77.1</td>
<td>79.9</td>
<td>74.7</td>
</tr>
<tr>
<td>Job duration (days)</td>
<td>70.9</td>
<td>72.3</td>
<td>73.9</td>
<td>72.5</td>
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<tr>
<td>Quarterly earnings</td>
<td>5,320</td>
<td>5,657</td>
<td>5,944</td>
<td>5,497</td>
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<tr>
<td>Earnings next quarter</td>
<td>2,549</td>
<td>1,977</td>
<td>2,753</td>
<td>2,018</td>
</tr>
<tr>
<td>Quarters until next job</td>
<td>6.69</td>
<td>8.03</td>
<td>6.69</td>
<td>7.21</td>
</tr>
<tr>
<td>Tenure (quarters)</td>
<td>22.7</td>
<td>30.8</td>
<td>27.0</td>
<td>25.2</td>
</tr>
<tr>
<td>Age</td>
<td>35.7</td>
<td>36.9</td>
<td>36.9</td>
<td>36.4</td>
</tr>
<tr>
<td>Education group</td>
<td>1.78</td>
<td>1.80</td>
<td>1.87</td>
<td>1.75</td>
</tr>
<tr>
<td>Non-German</td>
<td>0.172</td>
<td>0.185</td>
<td>0.124</td>
<td>0.170</td>
</tr>
<tr>
<td>Female</td>
<td>0.330</td>
<td>0.283</td>
<td>0.316</td>
<td>0.282</td>
</tr>
<tr>
<td>Occupation</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Low-skilled manual</td>
<td>0.359</td>
<td>0.366</td>
<td>0.299</td>
<td>0.354</td>
</tr>
<tr>
<td>- High-skilled manual</td>
<td>0.170</td>
<td>0.236</td>
<td>0.168</td>
<td>0.220</td>
</tr>
<tr>
<td>- Engineering</td>
<td>0.069</td>
<td>0.107</td>
<td>0.105</td>
<td>0.095</td>
</tr>
<tr>
<td>- Low-skilled service</td>
<td>0.148</td>
<td>0.097</td>
<td>0.143</td>
<td>0.126</td>
</tr>
<tr>
<td>- Professional</td>
<td>0.010</td>
<td>0.009</td>
<td>0.027</td>
<td>0.016</td>
</tr>
<tr>
<td>- Low-skilled sales or administrative</td>
<td>0.088</td>
<td>0.034</td>
<td>0.062</td>
<td>0.033</td>
</tr>
<tr>
<td>- High-skilled sales or administrative</td>
<td>0.138</td>
<td>0.129</td>
<td>0.169</td>
<td>0.133</td>
</tr>
<tr>
<td>- Managerial</td>
<td>0.017</td>
<td>0.016</td>
<td>0.021</td>
<td>0.016</td>
</tr>
<tr>
<td>Number of workers</td>
<td>12,006</td>
<td>15,341</td>
<td>23,270</td>
<td>13,498</td>
</tr>
<tr>
<td>Number of worker-quarters</td>
<td>396,198</td>
<td>506,253</td>
<td>767,910</td>
<td>445,434</td>
</tr>
</tbody>
</table>

Notes: Sample is full-time, non-trainee, age 25-50 workers displaced in 1990-99. The number of workers in the control group is 22,853, with 754,149 worker-quarter observations.
Figure A.8: Mean earnings by layoff rule

Notes: This figure shows workers’ mean earnings in event time without estimating the event study model in Equation (2.8). See notes to Table A.5 for sample description for this figure and following figures and tables.
Figure A.9: Earnings losses by layoff rule

(a) All four layoff rules

(b) Tenure and wage layoff rules with confidence intervals
Figure A.10: Wage losses by layoff rule: Workers with positive earnings each year

Figure A.11: Days worked losses by layoff rule
Table A.6: Job-leaving hazard rates for displaced workers’ next job and layoff rules

<table>
<thead>
<tr>
<th>Layoff rule (Omitted: Other rule)</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>- Tenure rule</td>
<td>−0.035*</td>
<td>(0.015)</td>
</tr>
<tr>
<td>- Wage rule</td>
<td>0.003</td>
<td>(0.014)</td>
</tr>
<tr>
<td>- Occupation rule</td>
<td>−0.005</td>
<td>(0.013)</td>
</tr>
</tbody>
</table>

Low-tenure layoff coefficients

| - Quartile 2                      | −0.051**  | (0.013)   |
| - Quartile 3                      | −0.083**  | (0.014)   |
| - Quartile 4                      | −0.075**  | (0.015)   |

Log-wage layoff coefficients

| - Quartile 2                      | −0.003    | (0.014)   |
| - Quartile 3                      | 0.005     | (0.014)   |
| - Quartile 4                      | −0.003    | (0.014)   |

Occupation layoff p-values

| - Quartile 2                      | −0.041**  | (0.013)   |
| - Quartile 3                      | 0.033*    | (0.014)   |
| - Quartile 4                      | 0.021     | (0.014)   |

Number of workers: 58,120

Notes: Coefficient estimates from Cox hazard models of duration of next job (a measure of match quality). Controls: age, age-squared, tenure dummies (2 years, 3 years, 3-5 years, 6-9 years, 10+ years), tenure censored, sex, education dummies, non-German, occupation, log-wage, quarters until next job, year-quarter, state, and the establishment’s age, size, mean wage, mean tenure, industry, and layoff size. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$. 
Figure A.12: Earnings losses by layoff rule: Propensity score weighting to control for differences across layoff rules

Figure A.13: Earnings losses by layoff rule: Establishments with at least 20 employees one year after layoff
Figure A.14: Earnings losses by layoff rule and state of business cycle

(a) Workers displaced in expansion

(b) Workers displaced in recession
Table A.7: Asymmetric employer learning and worker characteristics

<table>
<thead>
<tr>
<th>Layoff rule (Omitted: Other rule)</th>
<th>Pre-layoff log earnings (1)</th>
<th>Earnings percentage change (2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>- Tenure rule</td>
<td>0.005</td>
<td>0.052**</td>
<td>0.185**</td>
<td>0.158**</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.020)</td>
<td>(0.047)</td>
<td>(0.048)</td>
</tr>
<tr>
<td>- Wage rule</td>
<td>-0.017</td>
<td>-0.022</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.019)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Occupation rule</td>
<td>0.010</td>
<td>0.026</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.020)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tenure rule x recession</td>
<td>-0.072*</td>
<td>-0.073*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.029)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tenure rule x log tenure</td>
<td>-0.031*</td>
<td>-0.030*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.012)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tenure rule x education (Omitted: No apprenticeship)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Tenure rule x missing education</td>
<td>0.008</td>
<td>0.018</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.056)</td>
<td>(0.056)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Tenure rule x apprenticeship</td>
<td>-0.054*</td>
<td>-0.043</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.023)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Tenure rule x college</td>
<td>-0.127**</td>
<td>-0.088*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.039)</td>
<td>(0.039)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tenure rule x occupation (Omitted: Low-skilled manual)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Tenure rule x high-skilled manual</td>
<td>-0.002</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Tenure rule x engineering</td>
<td>0.100*</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Tenure rule x low-skilled service</td>
<td>-0.004</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Tenure rule x professional</td>
<td>0.082</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.071)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Tenure rule x low-skilled sales or administrative</td>
<td>0.038</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Tenure rule x high-skilled sales or administrative</td>
<td>0.091*</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.037)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Tenure rule x managerial</td>
<td>0.073</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.054)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tenure rule x occupation residual wage variance growth</td>
<td></td>
<td>0.363*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.152)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| $R^2$                             | 0.49                        | 0.14                          | 0.14 | 0.14 |
| Number of workers                 | 63,123                      | 63,123                        | 63,123 | 63,123 |

Notes: Coefficient estimates from regressions of pre-layoff log earnings (earnings in year before layoff) and earnings percentage change (earnings in year after layoff relative to earnings in year before layoff, with a mean of -0.56). Controls: tenure dummies (2 years, 3 years, 3-5 years, 6-9 years, 10+ years), tenure censored, age, age-squared, female, non-German, education dummies, occupation, industry, year, state, layoff size, establishment size, establishment mean wage, establishment mean tenure, and establishment age. Standard errors clustered at establishment level. * $p < 0.05$, ** $p < 0.01$. 
### Appendix B

### Additional tables and figures

Table B.1: Number of mass layoffs in West Germany after sample corrections, 1980-2009

<table>
<thead>
<tr>
<th>Sample correction</th>
<th>Number of mass layoffs</th>
</tr>
</thead>
<tbody>
<tr>
<td>100+ workers, 30-90% drop in quarterly employment</td>
<td>30,597</td>
</tr>
<tr>
<td>Not temp agency, vocational training, mining, or agriculture</td>
<td>26,173</td>
</tr>
<tr>
<td>&gt; 1 year old</td>
<td>24,933</td>
</tr>
<tr>
<td>Stable pre and post employment</td>
<td>18,350</td>
</tr>
<tr>
<td>Max clustered outflow / Total outflow &lt; 30%</td>
<td>8,862</td>
</tr>
<tr>
<td>Max clustered outflow / Successor’s employment &lt; 90%</td>
<td>7,690</td>
</tr>
<tr>
<td>Minimal rehires</td>
<td>6,184</td>
</tr>
<tr>
<td>&gt; 5% of separators on UI within 12 weeks</td>
<td>4,414</td>
</tr>
</tbody>
</table>

Notes: Final sample includes 3,836 unique establishments and 926,057 workers (54,836,667 worker-quarter observations), with 438,172 workers laid off. See Section 1.2 for details on each sample correction.
Figure B.1: Macroeconomic trends and policy changes in Germany

Source: Card et al. (2013).
Figure B.2: Mean mass layoff size and unemployment rate
Table B.2: Mean establishment characteristics by mass layoff status

<table>
<thead>
<tr>
<th></th>
<th>No mass layoff</th>
<th>Mass layoff</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of workers</td>
<td>340</td>
<td>204</td>
</tr>
<tr>
<td>Daily wage (1995 Euros)</td>
<td>71.8</td>
<td>67.8</td>
</tr>
<tr>
<td>Annual raise over past year</td>
<td>0.030</td>
<td>0.018</td>
</tr>
<tr>
<td>Establishment age (years)</td>
<td>16.6</td>
<td>14.0</td>
</tr>
<tr>
<td>Tenure of workers (quarters)</td>
<td>28.0</td>
<td>24.5</td>
</tr>
<tr>
<td>Join rate</td>
<td>0.047</td>
<td>0.058</td>
</tr>
<tr>
<td>Exit rate</td>
<td>0.049</td>
<td>0.486</td>
</tr>
<tr>
<td>Female</td>
<td>0.411</td>
<td>0.367</td>
</tr>
<tr>
<td>Non-German</td>
<td>0.087</td>
<td>0.148</td>
</tr>
<tr>
<td>Age of workers</td>
<td>39.4</td>
<td>40.0</td>
</tr>
<tr>
<td>Marginal workers (part-time, low-pay)</td>
<td>0.051</td>
<td>0.058</td>
</tr>
<tr>
<td>Part-time low (up to 1/2 of FT)</td>
<td>0.021</td>
<td>0.014</td>
</tr>
<tr>
<td>Part-time high (more than 1/2 of FT)</td>
<td>0.122</td>
<td>0.094</td>
</tr>
<tr>
<td>Trainee</td>
<td>0.056</td>
<td>0.047</td>
</tr>
<tr>
<td>Education</td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Missing education</td>
<td>0.070</td>
<td>0.136</td>
</tr>
<tr>
<td>- Less than apprenticeship</td>
<td>0.230</td>
<td>0.280</td>
</tr>
<tr>
<td>- Apprenticeship</td>
<td>0.578</td>
<td>0.502</td>
</tr>
<tr>
<td>- College</td>
<td>0.122</td>
<td>0.083</td>
</tr>
<tr>
<td>Observations</td>
<td>694,596</td>
<td>4,414</td>
</tr>
</tbody>
</table>

Note: “No mass layoff” establishments have at least 100 workers and are not in the industries I exclude in my mass layoff definition in Table B.1.
Figure B.3: Industry and occupation distributions by mass layoff status

**Industry distribution**

- Food production
- Consumer goods
- Chemicals and metals
- Cars and machinery
- Construction
- Wholesale
- Retail
- Hospitality
- Transportation
- Finance
- Business services
- Public services
- Education
- Health
- Public organizations

**Occupation distribution**

- Low-skilled manual
- High-skilled manual
- Engineering
- Low-skilled service
- Professional
- Low-skilled sales or admin.
- High-skilled sales or admin.
- Managerial
Figure B.4: Layoff rules over the business cycle: HP filtered

Mean low–tenure layoff coefficient

Note: Correlations: $r = -0.50$ (p = 0.01), Post–1995 $r = -0.61$ (p = 0.02)

Mean female layoff coefficient

Note: Correlations: $r = 0.51$ (p = 0.01), Post–1995 $r = 0.67$ (p = 0.01)
Figure B.4 (cont.): Layoff rules over the business cycle: HP filtered

Mean college layoff coefficient

![Plot](image)

Note: Correlations: $r = -0.43$ ($p = 0.02$), Post–1995 $r = -0.48$ ($p = 0.07$)

Mean log–wage layoff coefficient

![Plot](image)

Note: Correlations: $r = -0.27$ ($p = 0.14$), Post–1995 $r = -0.71$ ($p = 0.01$)
Figure B.4 (cont.): Layoff rules over the business cycle: HP filtered

**Mean wage–residual layoff coefficient (separate model)**

Note: Correlations: $r = -0.45$ (p = 0.01), Post-1995 $r = -0.69$ (p = 0.01)

**Mean $R^2$ from wage–residual layoff model (separate model)**

Note: Correlations: $r = 0.62$ (p = 0.01), Post-1995 $r = 0.80$ (p = 0.01)
Table B.3: Layoff rules and establishment characteristics

<table>
<thead>
<tr>
<th>Industry (Omitted: Chemicals and metals)</th>
<th>Tenure rule</th>
<th>Wage rule</th>
<th>Occupation rule</th>
</tr>
</thead>
<tbody>
<tr>
<td>Food and beverage production</td>
<td>0.516*</td>
<td>-0.190</td>
<td>-0.403</td>
</tr>
<tr>
<td>Consumer goods</td>
<td>0.478**</td>
<td>0.080</td>
<td>0.000</td>
</tr>
<tr>
<td>Cars and machinery</td>
<td>-0.083</td>
<td>0.025</td>
<td>-0.405*</td>
</tr>
<tr>
<td>Construction</td>
<td>0.081</td>
<td>0.181</td>
<td>-0.001</td>
</tr>
<tr>
<td>Wholesale</td>
<td>0.592*</td>
<td>0.079</td>
<td>-0.180</td>
</tr>
<tr>
<td>Retail</td>
<td>0.839**</td>
<td>-0.966*</td>
<td>-0.076</td>
</tr>
<tr>
<td>Hospitality</td>
<td>0.437</td>
<td>-0.061</td>
<td>0.144</td>
</tr>
<tr>
<td>Transportation</td>
<td>0.442</td>
<td>-0.394</td>
<td>-0.346</td>
</tr>
<tr>
<td>Finance</td>
<td>-0.535</td>
<td>0.014</td>
<td>-0.548</td>
</tr>
<tr>
<td>Business services</td>
<td>0.379</td>
<td>-0.062</td>
<td>0.003</td>
</tr>
<tr>
<td>Public services</td>
<td>0.420</td>
<td>0.442</td>
<td>0.032</td>
</tr>
<tr>
<td>Education</td>
<td>1.187</td>
<td>0.289</td>
<td>0.403</td>
</tr>
<tr>
<td>Healthcare</td>
<td>0.360</td>
<td>0.548</td>
<td>-0.251</td>
</tr>
<tr>
<td>Public organizations</td>
<td>0.621</td>
<td>0.384</td>
<td>0.065</td>
</tr>
</tbody>
</table>

Number of mass layoffs: 4,313

Notes: Coefficient estimates from multinomial logit model of layoff rule choice. Omitted layoff rule: other rule. Additional controls: year, state, fraction non-German, fraction part-time, fraction trainee, education distribution, occupation distribution. Robust standard errors in parentheses. * p < 0.05, ** p < 0.01.
Table B.4: Mean establishment-level layoff coefficients and $R^2$s by industry collective bargaining coverage quartile, 1990-2009

<table>
<thead>
<tr>
<th></th>
<th>Quartile 1</th>
<th>Quartile 2</th>
<th>Quartile 3</th>
<th>Quartile 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low-tenure coefficient</td>
<td>0.081</td>
<td>0.078</td>
<td>0.080</td>
<td>0.100</td>
</tr>
<tr>
<td>Part-time coefficient</td>
<td>-0.036</td>
<td>-0.047</td>
<td>-0.058</td>
<td>-0.072</td>
</tr>
<tr>
<td>Log-daily-wage coefficient</td>
<td>-0.131</td>
<td>-0.188</td>
<td>-0.194</td>
<td>-0.191</td>
</tr>
<tr>
<td>Occupation $p$-value</td>
<td>0.102</td>
<td>0.096</td>
<td>0.099</td>
<td>0.086</td>
</tr>
<tr>
<td>Separate regressions</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>- Wage residual coefficient</td>
<td>-0.103</td>
<td>-0.142</td>
<td>-0.140</td>
<td>-0.126</td>
</tr>
<tr>
<td>- $R^2$-tenure</td>
<td>0.106</td>
<td>0.117</td>
<td>0.112</td>
<td>0.127</td>
</tr>
<tr>
<td>- $R^2$-occupation</td>
<td>0.123</td>
<td>0.148</td>
<td>0.160</td>
<td>0.144</td>
</tr>
<tr>
<td>- $R^2$-wage</td>
<td>0.099</td>
<td>0.108</td>
<td>0.117</td>
<td>0.113</td>
</tr>
<tr>
<td>- $R^2$-wage-residual</td>
<td>0.069</td>
<td>0.065</td>
<td>0.064</td>
<td>0.056</td>
</tr>
<tr>
<td>- $R^2$-complete</td>
<td>0.206</td>
<td>0.238</td>
<td>0.246</td>
<td>0.239</td>
</tr>
<tr>
<td>Number of mass layoffs</td>
<td>622</td>
<td>667</td>
<td>780</td>
<td>1,103</td>
</tr>
</tbody>
</table>

Note: Establishments divided into quartiles based on industry-year collective bargaining rates (the fraction of establishments that are covered by an industry-level collective bargaining agreement in a given industry and year).
Figure B.5: Changes in industry-level layoff rule use and collective bargaining coverage between 1995 and 2009

**Tenure rule**

- Log change in fraction tenure rule vs. log change (decrease) in collective bargaining coverage
- Note: $\beta = -1.57$ (p = 0.25)

**Wage rule**

- Log change in fraction wage rule vs. log change (decrease) in collective bargaining coverage
- Note: $\beta = 1.98$ (p = 0.09)
Figure B.5 (cont.): Changes in industry-level layoff rule use and collective bargaining coverage between 1995 and 2009

**Occupation rule**

- Log change in fraction occupation rule
- Log change (decrease) in collective bargaining coverage

Note: $\beta = 0.74 \ (p = 0.48)$

**Other rule**

- Log change in fraction other rule
- Log change (decrease) in collective bargaining coverage

Note: $\beta = -0.15 \ (p = 0.94)$
Figure B.6: Earnings losses by role of tenure in layoff rule

Note: “LIFO”: last in, first out.

Figure B.7: Earnings losses by role of tenure in layoff rule: Propensity score weighting to control for differences across layoff rules

Note: “LIFO”: last in, first out.
Figure B.8: Earnings losses by fraction of workers laid off

![Graph showing earnings losses by fraction of workers laid off.](image)

Figure B.9: Earnings losses by fraction of workers laid off: Propensity score weighting to control for differences across layoff rules

![Graph showing earnings losses by fraction of workers laid off with propensity score weighting.](image)