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Reconsidering the cost of job loss: Evidence from redundancies and mass layoffs

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Jonas Cederlöf†

Abstract

This paper studies the consequences of job loss. While previous literature has relied on mass layoffs and plant closures for identification, I exploit discontinuities in the likelihood of displacement generated by a last-in-first-out rule used at layoffs in Sweden. Matching data on individual layoff notifications to administrative records, I find that permanent earnings losses are only found among workers losing their job in mass layoffs, whereas workers displaced in smaller layoffs fully recover. Auxiliary analysis suggests that large layoffs increase exposure to non-employment, prolong unemployment and cause workers to leave the labor force, conceivably by affecting the local labor market.

JEL: J63, J64, J65
Keywords: last-in-first-out, job loss, displaced worker, mass layoff, earnings loss

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1 Introduction

A large body of literature documents that displaced workers suffer significant and even permanent losses in terms of their future earnings, wages and employment.[1] What drives these earnings losses is, however, vividly debated as standard models of the labor market have trouble generating both the magnitude and the persistence of empirically observed losses (Davis and von Wachter, 2011) or disagree upon its sources (Carrington and Fallick, 2017). Meanwhile, understanding why and under what circumstances the costs of job loss are most persistent is of great importance not only for our theoretical understanding of the labor market but also for public policy.

Since the seminal study by Jacobson, Lalonde and Sullivan (1993) the vast majority of research estimating the consequences of job loss compare displaced *vis-à-vis* non-displaced workers, across firms, using mass layoff and plant closure as an exogenous source of variation.[2] However, such events are quite extraordinary as most job loss occur due to less drastic and more marginal adjustments to employment. In fact, only about 8 percent of all involuntary separations in the US between 2003–2012 occurred during mass layoffs.[3] Unfortunately, evidence from the remaining 92 percent of smaller layoffs are conspicuously absent, and little is known how previous findings generalize beyond mass layoffs or to what extent such extraordinary events render extraordinary consequences. This paper aims to fill this gap by providing the first empirical evidence showing how the size of layoff influence earnings losses upon job loss and how it is imperative for our understanding of the standard result of permanent losses among displaced workers.[4]

The key challenge in obtaining credible estimates of earnings losses following job loss is that displacement is a non-random event. For instance, it is widely recognized that displaced workers may be adversely selected (see e.g. Gibbons and Katz, 1991; Pfann and Hamermesh,

[1]For example, see Davis and von Wachter (2011); Eliason and Storrie (2006); Hijzen, Upward and Wright (2010); Jacobson, Lalonde and Sullivan (1993); Lachowska, Mas and Woodbury (2020); Ruhm (1991); Song and von Wachter (2014); Schmieder, von Wachter and Heining (2023); Burdett, Carrillo-Tudela and Coles (2020); Athey, Simon, Skans, Vikstrom and Yakymovych (2023). Reviews of the literature can be found in Couch and Placzek (2010); Davis and von Wachter (2011); Fallick (1996). Displacement has also been shown to lead to worse health (see e.g. Black, Devereux and Salvanes, 2015; Browning, Dano and Heinesen, 2006; Jolly and Phelan, 2017; Kuhn, Lalive and Zweimüller, 2009; Schaller and Stevens, 2015), increased mortality (Eliason and Storrie, 2009; Sullivan and von Wachter, 2009), lower fertility (Huttunen and Kellokumpu, 2016), negatively affecting children of displaced workers (Rege, Telle and Votruba, 2011; Stevens and Schaller, 2011) and increasing the probability of committing crime (Britto et al., 2022; Bennett and Ouazad, 2019).

[2]Two exceptions are Stevens (1997) and Kletzer and Fairlie (2003) who, by relying on survey data, also include smaller layoffs in their analysis. However, neither study performs a separate analysis for small *vis-a-vis* large layoffs.

[3]Calculations are based upon data from the Bureau of Labor Statistics by combining data from the Mass Layoff Statistics program (which ended in March 2013) with the Job Openings and Labor Turnover Survey (JOLTS) reporting the total number of layoffs and discharges which is made up of all involuntary separations initiated by the employer. Both these data sources can be accessed at http://www.bls.gov.

[4]An interesting, although somewhat overlooked, fact is that Jacobson, Lalonde and Sullivan (1993) already in their study, in one specification, studied earnings of workers laid off in non-mass layoffs (i.e smaller layoffs) and found that "[...] following separation they drop by only one-half as much as workers in the mass-layoff sample" (p. 699). Moreover, these losses were transitory as workers fully recovered within 3-5 years. However, the finding was primarily attributed to the non-mass layoff sample including "[...] larger fractions of workers who quit their jobs [...]" (p. 699), and thus considered to be a non-representative estimate for involuntarily displaced workers.

2001; Lengerman and Vilhuber, 2002; von Wachter and Bender, 2006; Couch and Placzek, 2010; Davis and von Wachter, 2011; Schwerdt, 2011; Jung and Kuhn, 2018). That is, if employers are able to select which workers to lay off, whereas others may leave the firm early in anticipation of future downsizing, displaced workers may be of lower quality and have *ex ante* lower earnings trajectories. One additional difficulty lies in distinguishing between voluntary and involuntary separations in data, where mistaking the former for the latter may understate workers' true earnings losses.[5] To overcome these challenges, the literature has typically relied on estimating distributed lag models with individual fixed effects; comparing earnings trajectories of high tenured (often male) workers displaced during mass layoffs or plant closures to those of non-displaced observationally equivalent workers at non-downsizing firms. While the empirical strategy may solve the selection problem, estimates will by design pertain to a particular population of workers laid off under rather specific circumstances.[6] Also, to the extent that low productivity firms attract low productivity workers (Abowd, Kramarz and Margolis, 1999) and inference is based on high tenured workers, estimates may reflect the effect for displaced workers with less favorable characteristics or a particularly good match (Lachowska, Mas and Woodbury, 2020).

This paper takes a novel approach to estimating the consequences of job loss on subsequent labor market outcomes by exploiting the use of a seniority rule used at layoffs in Sweden, namely the last-in-first-out (LIFO) rule. The LIFO rule is written into Swedish labor law and mandates that workers within the same establishment, performing similar tasks and being part of the same collective bargain agreement (CBA), should be laid off in inverse order of seniority, whereby more recent hires ought to be let go before workers with higher tenure. Using detailed matched employer-employee data, containing information on job start and end dates, I rank workers according to their relative seniority (tenure) within an establishment which, by the LIFO rule, renders variation in the probability of displacement. Combining these data with wage registers and a unique individual register dataset containing all layoff notifications involving at least 5 workers during 2005–2015, I identify occupation specific cut-offs in downsizing establishments where the probability of displacement jumps discontinuously. This generates quasi-experimental variation which lends itself to a (fuzzy) regression discontinuity (RD) design. The key threat to a causal interpretation of these estimates is that firms selectively displace workers by choosing not who, but rather, how many workers to lay off. Although such manipulation is unlikely due to priority of recall for the last displaced worker, I carefully address this concern through a series of tests and find no evidence of selective firing based on observable pre-determined characteristics or earnings prior to the displacement event.[7]

[5]As pointed out by Couch and Placzek (2010), one additional justification for looking only at workers separating during a mass layoff is to avoid including people fired for cause, which would likely overstate true earnings losses. Likewise, earnings losses may also be exaggerated by including quits if these are predominantly induced by workers' choosing non-employment due to e.g. discouragement or poor health.

[6]Previous research has shown that firms executing mass layoffs tend to be concentrated to particular industries with overall higher turnover rates (Fallick, 1996; Krashinsky, 2002; von Wachter and Bender, 2006; Sullivan and von Wachter, 2009)

[7]To the extent that there are imbalances in unobserved worker productivity due to employers being able to

Leveraging the discontinuities implied by the LIFO rule, I find that displaced workers have about 40 percent lower earnings compared to their non-displaced coworkers, one to two years after layoff notification. The size of these initial losses are similar in magnitude to what has been observed in the US (cf. Jacobson, Lalonde and Sullivan, 1993; Sullivan and von Wachter, 2009; Lachowska, Mas and Woodbury, 2020) as well as in Europe (cf. Schmieder et al., 2023; Burdett et al., 2020; Bertheau et al., 2022). However, as time goes by, the earnings gap between displaced and non-displaced workers shrink and is fully closed 7 years after displacement. Importantly, the closing of the gap is not driven by recalls or initially non-displaced workers getting laid off at a later point in time, but from displaced workers climbing back up the job ladder. I decompose the earnings losses into different margins of adjustment; wages, employment and hours worked. While employment differences account for the lion share of the initial difference in earnings, these have subsided after 3 years. Displaced workers suffer wage cuts at their new jobs but have faster wage growth than their non-displaced former colleagues and after 7 years earn the same wage. Hours responses are small and insignificant throughout the recovery.

As the finding of earnings losses being transitory, rather than persistent, stands in stark contrast to the standard result in the literature (see Table A.1 for a summary of estimates), I probe this result in great detail. I begin by estimating earnings losses using the conventional event study mass layoff estimator. Here I find large and permanent earnings losses ranging between 14 to 21 percent 10 years after displacement, thus ruling out that the transitory pattern observed in the RD analysis is context or time specific. Further, to ensure that these results are not driven by compositional differences across the two samples, I reweight the estimates to mimic the LIFO sample in multiple dimensions. While rendering a slight decrease in long-run losses, they remain permanent and substantial 10 years after layoff, suggesting that differences in e.g. worker composition, industry composition, firm size or time of layoff cannot explain the transitory pattern found in the RD analysis.

One key difference between the canonical mass layoff estimator and the RD is that the former exploits variation across firms and the latter within firms, thus rendering results from two potentially different experiments with different contra factual states. Moreover, whereas the traditional estimator identifies the effect for the average displaced worker, the RD analysis pertains to the marginally displaced worker. To account for these differences, I combine the two estimators by matching workers below the LIFO-threshold to workers in other (non-downsizing) establishments, emulating the conventional control group used in the literature. Estimating earnings losses at the threshold using variation across establishments renders slightly larger earnings losses on average, but nevertheless continue to be transitory with workers fully recovering within 10 years. These results, however, conceal a great deal of heterogeneity, as splitting the sample by size of layoff reveals that permanent earnings losses are present, but only among workers displaced in large layoffs while workers displaced in smaller layoffs recover much faster,

selectively displace workers, estimates should be downward biased rendering earnings losses to be exaggerated. Nevertheless, in light of the finding that earnings losses are transitory rather then persistent, this would suggest that in absence of selection, displaced workers recover even faster.

closing the earnings gap 6–7 years after displacement. The pattern is also confirmed when splitting layoffs by size using the original (non-matched) sample where a clear negative correlation between long-run earnings losses and the size of layoff is visible, even after reweighting larger layoffs to resemble small layoffs in multiple dimensions.

Altogether, these findings suggest that mass layoffs are very different from less drastic and more common redundancies in that they generate not only larger but also permanent earnings losses; a fact which cannot be accounted for by estimation technique nor differences in industry or worker composition, or economic conditions at the time of layoff. This result is, to the best of my knowledge, new to the literature on worker displacement and suggests that, although being a serious concern, permanent worker scarring is not as ubiquitous a phenomenon as previous research may have lead one to believe.[8] This result also has important implications for public policy, suggesting that focus and resources should be geared towards workers displaced in large layoffs.

In the last part of the paper, I explore why large layoffs – in contrast to smaller redundancies – have permanent effects on workers' earnings. Several recent studies have set out provide explanations for why displacement during mass layoff render permanent losses. For example, Schmieder, von Wachter and Heining (2023) have emphasized the loss of establishment specific wage premiums, while Krolikowski (2017) and Lachowska, Mas and Woodbury (2020) highlight the loss of a match specific component. Others point to the role of aggregate labor market conditions as an important determinant for earnings losses (Davis and von Wachter, 2011; Schmieder, von Wachter and Heining, 2023; Huckfeldt, 2022) and that longer unemployment duration may lead to the loss of human capital and lower reemployment wages (Schmieder, von Wachter and Bender, 2016; Burdett, Carrillo-Tudela and Coles, 2020; Jarosch, 2023).

I show that workers displaced in large layoffs (in comparison to smaller ones) have more exposure to non-employment, higher probability of (long-term) unemployment, as well as leaving the labor force altogether. Moreover, having been part of a large layoff, workers are more likely to find work outside their local labor market, compared to if the layoff was small. As these effects are again robust to accounting for differences in worker and industry compositions across layoff size, as well as overall economic conditions at layoff, I consider the possibility of large layoffs affecting the entire local labor market through e.g. labor congestion. To this end, I first note that large layoff are typically large also in relation to the local industry labor market. As such, I study hetrogenity in the impact of layoff size by the local importance of a particular

[8] These results contrast one auxiliary finding in Flaaen, Shapiro and Sorkin (2019) who combine administrative data with individual level data from the Survey of Income and Program Participation (SIPP). They find that "[...] conditional on the survey reason for separation, the differences in earnings loss estimates between firms that are contracting and firms that are stable are small" (p.215). A potential concern, however, is that their identification strategy cannot – in contrast to the LIFO-rule – account for adverse selection of workers in smaller layoffs, which may render their estimates downward biased. In addition, for this auxiliary analysis Flaaen, Shapiro and Sorkin (2019) exclude observations with zero earnings, but show for the main analysis that the inclusion of zero earnings generates almost three times larger long-run losses. As I show in section 6, a key difference between small and large layoffs is that the latter renders longer unemployment and induce more workers to leave the labor force, making the inclusion of zero earnings observations vital for studying the differential impact of small *vis-à-vis* large layoffs.

industry, finding that earnings losses are only permanent among workers displaced in large layoffs where the downsizing firms' industry also make up a large share of total employment in the local labor market.

Relative to the previous literature estimating earnings losses upon displacement, this is the first paper to exploit a seniority rule as an exogenous source of variation to involuntary job loss. In doing so, I am able to provide first time, and until now strikingly absent, evidence on the consequences of job loss not due to mass layoff or plant closure, absent of adverse selection of workers. The paper adds new and important insights to the literature on displaced workers by showing that the size of layoff is an imperative determinant to understanding persistent earnings losses.[9] The findings also speak to a recent part of the literature seeking to understand what drives earnings losses to become permanent. Both the empirically oriented part (see e.g. Lachowska et al., 2020; Schmieder et al., 2023; Athey et al., 2023), as well as the more theoretically oriented one (see e.g. Krolikowski, 2017; Jung and Kuhn, 2018; Burdett, Carrillo-Tudela and Coles, 2020; Huckfeldt, 2022; Jarosch, 2023) where the persistence in earnings losses often have eluded standard models of the labor market (Davis and von Wachter, 2011). I see my findings to be much in line with Gathmann et al. (2018) showing how firms in the same industry as a mass layoff plant suffer from negative spillovers, and Gulyas and Pytka (2020) documenting that displacement firm wage premia and the availability of jobs in a local labor market are key factors in explaining post displacement outcomes. Finally, my findings are also congruent with the recent literature showing how displacement renders longer and serially correlated unemployment spells where workers lose human capital (see Burdett, Carrillo-Tudela and Coles, 2020; Jarosch, 2023).

The rest of the paper unfolds as follows. Section 2 provides a brief description of the overall usage of seniority rules at layoff and gives a more detailed description of the Swedish LIFO principle that is used for identification. I also describe the data and define the relevant variables used to identify workers' relative seniority within an establishment. The empirical strategy is laid out in Section 3, together with a discussion and multiple tests of the identifying assumptions needed for causal inference. The section ends with examining the empirical relationship between workers' relative seniority and layoff, i.e. the first stage. Section 4 presents the results on workers subsequent labor market outcomes and decomposes the overall earnings effect into various margins of adjustment. In Section 5, I seek to understand when and why, following displacement, permanent earnings losses are present. Section 6 examines why permanent earnings losses are primarily found among workers displaced in large layoffs. Section 7 concludes.

[9] In a recent paper, Fackler et al. (2021) exploit bankruptcies (firm closures) in Germany during the Great Recession, to study the importance of firm size on workers' earnings and wages after job loss. They show that workers displaced from small firms (< 10 employees) tend to have smaller earnings losses compared to workers displaced in large firms (≥ 100 employees) (roughly 7 percent vis-a-vis 11 percent, 5 years after dispalcement; see Online Appendix Figure G.1 of Fackler et al., 2021). Importantly, however, one should not conflate firm size and layoff size. While these are synonymous in the particular case of bankruptcies, large layoffs are generally, and almost mechanically, more frequent among small firms and vice versa, as layoff size is defined as the number of displaced workers divided by the number of employed. The empirical correlation between firm and layoff size in my main sample is $\rho = -0.383$ and the average establishment in my main sample has 169 employees (see Table 1).

2 Institutional setting & data

The LIFO rule is a type of seniority rule which mandates that more recent hires should be displaced before workers with longer tenure. Thus a workers' relative tenure ranking is predictive, albeit not perfectly, of displacement in the event of an establishment downsizing. Seniority rules are part of the broader concept of employment protection as it provides insurance and protects tenured workers against unjust termination (Pissarides, 2001). While being largely beneficial for the incumbent worker, high employment protection is generally thought to increase firms firing costs which in turn may hamper job creation and generate inefficiently low labor turnover (see e.g. Lazear, 1990; Mortensen and Pissarides, 1994).

Seniority rules are used at layoff in many countries (e.g., Germany, UK, Netherlands, and others) although with considerable differences across sectors and countries. Buhai et al. (2014) empirically documents the use of seniority rankings in layoff decisions in Denmark and Portugal, although it is unclear whether any formal rules are the cause of these findings. Lee (2004) documents the use of seniority rules in the United States and notes that in e.g. 1995 about 88 percent of all union contracts contained at least some seniority provision for layoff decisions. For Sweden, Böckerman, Skedinger and Uusitalo (2018) and Landais et al. (2021) documents empirical patterns consistent with the use of a seniority rule, which together with the Netherlands, is one of few countries who explicitly refer to a seniority rule in the Employment Protection Act as the main criteria for prioritizing among workers in the event of downsizing. However, none of the aforementioned papers have been able to pin down the use of a strict seniority rule (e.g., a LIFO rule) by establishing discontinuities in seniority ranking.

The Swedish LIFO rule The Swedish Employment Protection Act (EPA:22§) stipulates that when a firm needs to downsize due to "shortage of work" it should follow a LIFO principle which mandates that workers should be laid off in inverse order of seniority.[10] In the event of a tie in tenure, priority should be given to the older worker. Formally, the LIFO rule applies at the establishment level. In the event of multiple layoffs, employers should divide workers into groups based on workers CBA affiliation and list workers according to the length of employment.[11] These groups form so called order of termination circuits (*turordningskrets*) (henceforth refereed to as an order circuit or circuit, for short). Importantly, labor law also stipulates a "last-out-first-in" principle (EPA:26§) where the displaced worker with the highest tenure within the circuit has priority of recall if the firm needs to start hiring within 9 months of the displacement. Priority of recall applies to workers with at least 12 months of tenure, who is deemed sufficiently qualified for the new job and had expressed a wish for recall to the employer prior to layoff.

[10]The term "shortage of work" can be somewhat misleading as legal practice has come to interpret this as all layoffs not related to personal behavior of an individual worker.

[11]Whereas the LIFO rule applies at the establishment level, a workers' tenure – on which he is ranked upon – is based on total time at the firm, irrespective of whether the worker has worked sporadically, part-time or full-time. During e.g. firm acquisitions or mergers tenure is not reset but the start date of employment is that of the initial employer.

Some parts of Swedish labor law consists of semi optional paragraphs, meaning that these could be bypassed by employee and employer organizations through CBA's or local agreements.[12] One such paragraph is the LIFO rule. An employer may deviate from the LIFO principle by agreeing on a different order of priority with local union representatives in a negotiation. However, if the employer and the union are unable to strike a deal, the LIFO rule as written in law should be applied. As such, the Swedish LIFO rule functions as a default or starting point for negotiations between the local union and the employer. Unfortunately, little is known about how frequently agreements of deviations from the LIFO principle are made in practice.[13] Hence, it is ambiguous whether employer compliance with the LIFO principle at layoff is voluntary or invoked by the local union. Finally, firms with less than 10 employees are allowed to exempt two workers that are of particular importance for the firm. Also, workers in managerial positions or part of the employers' family may be exempted from the LIFO rule.

2.1 Data

I have data on layoff notifications from 2005 to 2015. By law, any firm that intends to displace more than 5 workers within a 90 day period must notify the Public Employment Service (PES). In a first stage, the firm reports to the PES the number of intended layoffs and the reason for downsizing. In a second stage, occurring on average 70 days after the first, the firm submits a list of names of the workers affected by the displacement. This list should be sent in at least one month before the first worker being laid off. Typically, all workers are notified on the same date whereas the date of displacement differs due to differences in statutory notification times.[14] This implies that all workers, irrespective of their seniority, are made aware of the impending layoff at the same time.

These data are then matched with a data set containing the universe of employer-employee matches between 1985 to 2018, which contains information on both firm and individual characteristics such as age, level of education and annual earnings. The data is annual, and along with the annual income statement, the employer reports the first and last month worked for each employee. These monthly markers make it possible to calculate firm specific tenure as well as determine the current workforce within an establishment in any given month. One issue with the monthly markers is that employers sometimes routinely report workers as having worked the entire year so that January is too often reported as being the start of the employment

[12] CBA's are industry or occupation specific and covers all employees (also non union members) at firms who has signed such an agreement. There are separate CBA's for white- and blue-collar workers and about 90 percent of the Swedish workforce is covered by a CBA whereas the union membership rate has declined from 81 to 69 percent between 2000–2020 (Kjellberg, 2019).

[13] Deviations from the LIFO principle should, however, not contravene "good practice in the labor market" or violate the Discrimination Act (EPA:22§).

[14] Workers that are laid off due to no-fault individual dismissals are entitled to advance notice where the length of the notice period varies (discontinuously) with tenure by law and at times by age according to local CBA's. Appendix C describes the notification process and law in more detail and provides some descriptive statistics (see also Cederlöf et al., 2021). The tenure thresholds that govern notification times, may occasionally line up with the LIFO-threshold. Note, however, that this does not affect identification at the LIFO threshold as it is based on the comparison between displaced and non-displaced workers. Nevertheless, empirically the jump at the threshold among notified workers just above and below as estimated to 0.296 days ($SE = 2.179$).

spell which may in turn generate measurement error in tenure. Moreover, a common feature of matched employer-employee data are so called false firm deaths where firms for other reasons than shut-down change identification number. Such occurrences would lead to erroneously resetting workers tenure, thereby creating large amount of inaccurate ties in tenure within a firm. As these data shortcomings in measuring tenure will map directly onto the running variable I try to minimize its influence by dividing workers starting in January into quartiles of annual earnings in the first year of employment where lower quartiles are assumed to have started employment later. I also exclude circuits where more than 2/3 of workers have tenure equal to the mode of tenure within the circuit.[15] In Appendix D, I explain in detail the procedure for calculating tenure and elaborate on the sources of measurement error relevant for the running variable.

As described in Section 2, the LIFO rule applies at the establishment×CBA level. Ideally, one would like to have accesses to which workers are covered by which CBA. As these data do not exist, I proxy CBA affiliation using (the Swedish version of) 2-digit level ISCO-88 (International Standard Classification of Occupations 1988) occupational codes provided in the wage register collected by Statistics Sweden each year. The register also contain information on (full-time equivalent) wages and is available for a large sample of establishments, covering almost 50 percent of all private sector workers and all public sector workers from 2000 to 2018. The sampling of private sector workers is done by firm (stratified by size) which again enables me to classify occupation for the entire workforce at each (sampled) establishment.[16]

Through these data, I determine the order of termination implied by the LIFO rule, for all establishments having sent a layoff notification to the PES and for which I have data on workers' occupation. Within circuits, I rank individuals according to seniority, adapting the convention of 1 being the highest tenured worker. Individual i's relative ranking within an order circuit c could then be written as

$$RR_{ic} = SR_{ic} - (\max_{i \in c}(SR_i) - N_c) \tag{1}$$

where SR_{ic} is the seniority ranking and N_c is the number of notified workers reported in the list submitted by the employer to the PES.[17] RR_{ic} is the running variable defining the relative tenure ranking normalized to zero for the worker who, by the LIFO rule, should be the last worker to remain employed. Figure 1 illustrates RR for two occupations (pink and gray) within a downsizing establishment in a given year. These form two separate circuits where workers

[15] An alternative approach, suggested by Hethey-Maier and Schmieder (2013), is to correct false firm/establishment deaths using worker flows. This involves categorizing last appearances of establishment identifiers as closures, mergers, spin-offs, etcetera, by placing restrictions on observed worker flows. As this approach requires more than one, possibly arbitrary, restrictions I find that placing only one restriction is more transparent.

[16] The survey is carried out in September and November and thus, strictly speaking, information on the composition of the workforce, occupation and wages corresponds to these months. This implies that order circuits should be better approximated for notifications that occur around September and November. Indeed, the precision of the first stage is much better for notifications made in the month of September to January than other months.

[17] I use the number of notified workers provided in the second stage of the reporting process. The reason is that firms have an incentive to over report the initial number of intended layoffs as they are prohibited from going beyond this number when finalizing the list of workers getting displaced.

are ranked according to tenure (and age in case of a tie). In the upper row $N_c = 3$ and the lower $N_c = 2$. Thus, workers to the right of the cut-off (with $RR > 0$) would get displaced if the establishment fully applied the LIFO rule. Note that the number of notified workers (N_c) is set endogenously by the firm. This may be problematic if firms select N_c based on worker characteristics as it would cause selective firing. I address this concern thoroughly in Section 3.2.

I have imposed some further restrictions on the data. First, I exclude layoff notifications where plant closures and bankruptcies are reported as the cause of displacement, as the threshold within circuits in such establishments are undefined since everyone is laid off. I also discard notifications due to an establishment moving as it may be endogenous whether the worker chooses to reallocate with the establishment. Second, I restrict the analysis to industries dominated by blue-collar workers as the LIFO rule to a greater extent applies among blue-collar workers. From this restriction it also follows that almost all establishments operate in the private sector. Finally, I condition on layoff notifications affecting at least 10 workers within an order circuit which is restricted to contain at most 100 workers.[18]

2.1.1 Descriptive statistics

Table 1 presents descriptive statistics for the main analysis (LIFO) sample along with two relevant comparison groups. For reference, column (1) shows average characteristics for all workers reported to the PES as being notified of their displacement. These workers are on average 40 years old and have about four and a half year of tenure at the displacing firm. Little more than one third of the sample are female and only 12 percent have a college degree likely reflecting the fact that one third of all notified workers come from the manufacturing industry.

Compared to the average notified worker, workers in the LIFO sample (column 2) are about 2 years younger, have about 2 years longer tenure, and are somewhat less educated. Note that column (2), as opposed to column (1), have both notified/displaced and non-notified/non-displaced workers. Due to restricting the analysis to blue-collar dominated sectors, workers from manufacturing firms are heavily overrepresented.[19]

To facilitate comparison with previous literature, I define a typical mass layoff sample whose characteristics can be seen in column 3 of Table 1. How this sample is constructed is detailed in Section 5.1, but, broadly I follow Schmieder et al. (2023) and Lachowska et al. (2020) and focus on workers with at least 5 years of tenure who are displaced along with at least 30 percent of the workforce at an establishment (i.e in a mass layoff) with at least 50 workers. Again,

[18] The lack of a one-to-one mapping between CBA's and occupation codes makes it difficult to precisely define the relevant workforce subject to the notification. Thus, the full tenure distribution within the order circuit may be obscured by erroneously including workers in an order circuit which they do not belong to. Misallocating just one worker will render the circuit too large or too small, thereby leading me to place the discontinuity in the wrong place in the tenure distribution. Thus placing restrictions on the maximum size of the order circuit increases precision of the first stage as the probability of including the "wrong" workers decreases.

[19] As the identifying variation comes from the compliers just at the threshold Table A.2 characterizes the complier population following Abadie et al. (2002). The overall estimation sample is very similar to the complier population.

workers are primarily concentrated in the manufacturing industry and as a consequences of the imposed standard restrictions workers in the mass layoff sample have about twice the tenure as the average notified worker but only about 4 percent higher earnings. Firms are on average larger in the mass layoff sample, however, establishments are about the same size.

3 The LIFO rule and layoff

3.1 Empirical strategy

As seniority within an establishment will be positively correlated with worker ability and productivity, correlating workers relative ranking with future earnings will inevitably be biased due to omitted variables. Similarly, a mere comparison of displaced *vis-à-vis* non-displaced workers will render biased estimates as firms could selectively displace workers with an ex ante lower earnings trajectory (due to e.g. low productivity). The LIFO rule, however, imposes restrictions on the employer in choosing between two workers working at the same establishment who performs similar tasks.

Following the definition of relative ranking (RR) in equation (1), I define the instrument as $Z_{ic} = \mathbf{1}[RR_{ic} > 0]$ where $\mathbf{1}[\cdot]$ is the indicator function. Further, I define a control function for relative ranking

$$h(RR_{ic}) = [h_0(RR_{ic}) + h_1(\mathbf{1}[RR_{ic} > 0] \times RR_{ic})] \tag{2}$$

which allows for different slopes on each side of the threshold. Since tenure is discrete and measured in months, I rely on a parametric control function varying the functional form in contrast to more non-parametric estimation techniques suggested by Calonico et al. (2014). The first stage equation can then be written as

$$D_{ic} = \alpha + \gamma Z_{ic} + h(RR_{ic}) + \phi_c + \rho X_i' + \varepsilon_{ic} \tag{3}$$

where γ is the first stage effect on the probability of being displaced (D_{ic}). X_i' is a vector of pre-determined baseline covariates included to increase efficiency and ε_{ic} an error-term.[20] ϕ_c is an order circuit fixed effect which consists of unique combinations of a firm, establishment, occupation and notification year fixed effects. The corresponding outcome equation is

$$y_{ict} = \pi + \beta D_{ic} + h(RR_{ic}) + \phi_c + \delta X_i' + u_{ict}. \tag{4}$$

Substituting equation (3) into (4) yields the reduced form equation. As order circuits are proxied and the LIFO rule semi optional, assignment to displacement will not be a fully deterministic function of a workers relative ranking (i.e., $\gamma < 1$). Hence, in order to estimate the cost of displacement, I instrument D_{ic} with Z_{ic}, rendering a fuzzy RD-design. The resulting instrumental variable (IV) estimate may then be interpreted as the local average treatment effect (LATE)

[20]The included covariates are: age, age squared, gender, immigrant status, educational attainment FE:s.

for workers just at the margin of lay off within establishments complying with the LIFO rule.

Excludability of the instrument hinges upon the assumption that being just above the (proxied) threshold only affects subsequent labor market outcomes through displacement. While exclusion is an assumption, it is useful to note that there are no other formal rules pertaining to the LIFO threshold. Also, the reduced form coefficient is interpretable as the average effect of being exposed to a higher risk of displacement in the event of downsizing.

In the main specification I use a bandwidth of ± 16 which is the preferred bandwidth using Calonico et al. (2014) optimal bandwidth selector.[21] I also confirm the robustness of the results by varying both the bandwidth and the functional form of $h(\cdot)$ as suggested by Lee and Lemieux (2010). In all regressions, I use a uniform kernel and cluster the standard errors at the order circuit level.

3.2 Selection around the discontinuity

The empirical strategy relies on the assumption of non-manipulation of the running variable. Specifically, this implies that neither workers nor firms should be able to perfectly manipulate who gets notified and eventually displaced, as this could create non-random selection into displacement which would invalidate the RD research design.

From the perspective of the worker, selective sorting around the threshold is unlikely but could arise if some workers are more prone than others to leave the establishment before the notification occurs (cf. Lengerman and Vilhuber, 2002; Schwerdt, 2011).[22] However, in order for this to amount into imbalances around the threshold, it would require that *i*) workers have pre-knowledge about the impending notification, *ii*) are aware of their *exact* position in the seniority ranking and that *iii*) workers just above the threshold, to a greater extent than workers just below, sort out of the establishment based on characteristics directly affecting the outcome.

A more plausible type of manipulation is one in which the firm chooses whom to displace by either endogenously forming, i.e manipulating, the order circuits, or by choosing the number of workers to lay off. The first issue is resolved by proxying order circuits with combinations of establishment and occupation. This avoids potential manipulation of order circuits as the proxy functions as an instrument, only picking up establishment/occupation combinations that adhere to the LIFO rule. If the relative tenure ranking within the establishment/occupation combination were not predictive of actual order circuits, due to, e.g., deviations agreed upon between local union representatives and the employer, the first stage coefficient would be zero. The second potential source of firm manipulation could however be of concern, as firms do set the cut-off endogenously by choosing how many workers to notify and eventually displace. A

[21]The optimal bandwidth is based on the main outcome of annual earnings at time of notification. In order to avoid using different samples for each single point estimate, I maintain the same bandwidth for all time horizons. However, as can be seen in Appendix B, the results remain virtually unchanged when using the proposed optimal bandwidth for each time period, separately.

[22]Note that I exploit variation within order circuits (establishment$\times$occupation$\times$year combinations) and that all workers employed within the circuit at the time of notification are included in the sample. Thus, any selection around the threshold have to occur prior to notification. Consequently, any differential mobility across the threshold e.g. in between notification and actual displacement is a result of the treatment.

firm that intends to lay off n workers, but realizes that worker $n+1$ in the seniority ranking has lower productivity, can instead decide to notify and lay off $n+1$ workers.

Formally, the identifying assumption that is needed for causal inference can be stated as

$$\lim_{\Delta \to 0^+} \mathbb{E}[\varepsilon_{ic} \mid RR_{ic} = \Delta] - \lim_{\Delta \to 0^-} \mathbb{E}[\varepsilon_{ic} \mid RR_{ic} = \Delta] = 0 \tag{5}$$

meaning that the distribution of unobserved worker characteristics be continuous at the threshold. Although the continuity assumption cannot be fully tested, its validity is usually be assessed by checking balance of average worker characteristics just around the threshold.[23]

First, Figure A.1 plots predicted annual earnings estimated by taking the fitted values from a regression of annual earnings on age, tenure and dummies for female, immigrant and level of education. Whereas the overall downward trend in predicted earnings stems from workers with higher relative ranking having lower tenure, there is no indication of selection around the discontinuity as the estimated jump at the threshold is less than 104 SEK (approximately 10.5 USD) and statistically insignificant. Thus predicted annual earnings evolves smoothly around the threshold.

Next, in an additional test of the continuity assumption, column (1)–(3) in Table 2 show estimates from regressing the instrument Z_i on a set of pre-determined covariates and the control function $h(RR_i)$. Irrespective of the choice of functional form, or the exclusion of circuit FE's, none of the individual variables are predictive of treatment status as coefficients are typically small as well as statistically indistinguishable from zero. Testing for joint significance using a F-test, I am also unable to reject all coefficients being jointly zero, as can be seen in the bottom of Table 2. Columns (4) and (5) show results from separate regressions for each baseline covariate, regressed on the instrument and a first and second order polynomial function, respectively. The point estimate in column (4) suggest that the difference in annual earnings between workers just to the right and left of the threshold is less than -0.04% and insignificant. Figure A.4 show graphically the bivariate balancing tests corresponding to column (4) and (5) as well as for monthly wages which I have access to for a smaller sample of workers.

Taken together, the fact that observable characteristics and earnings are neither jointly nor individually predictive of treatment speaks strongly in favor of the continuity assumption. It suggests that workers just around the threshold are not differentially leaving before notification, and that employers are unable or unwilling to adjust the number of workers being notified/displaced such that selective displacement occurs. Arguably, the incentives for laying off $n+1$ workers are also small as the marginal worker being laid off have first priority of recall

[23] It is also customary to examine the density around the threshold to see whether people have selected into treatment. However, as the threshold here is defined by where in the seniority distribution the last worker is notified, standard density tests as suggested by McCrary (2008) are no longer valid as the density around the threshold is balanced almost by construction (I say *almost* due to the fact that I allow for ties in relative ranking if both tenure and age at notification are the same for workers within the same order circuit). For completeness, Figure A.2 in the Appendix shows the density around the threshold. Due to having restricted the sample to at least 10 workers getting notified within a circuit the frequency of observations are about the same up until $RR_{ic} > 10$ where it starts to drop.

up to 9 months after displacement. In sum, I find no evidence of worker sorting or firms setting the cut-off endogenously such that it would invalidate the RD-design.

The one caveat is that as the number of notified workers is set endogenously by the employer, imbalances may still exist on *unobservable* worker characteristics. While this, per definition, cannot be tested, selective displacement of low productivity workers would imply that my estimates of earnings losses and its persistence are exaggerated. However, in light of the finding that earnings losses are transitory rather then persistent, this would imply that, in absence of any bias, displaced workers have in fact smaller earnings losses and recover even faster.

3.3 Layoff and the LIFO-threshold

Figure 2 shows the probability of being displaced as a function of workers relative tenure ranking within an order circuit where displacement is defined as having left the notifying firm within 15 months after notification.[24] As predicted by the LIFO rule, there is a discontinuous jump at the threshold where the probability of displacement increases by 12 percentage points which translates to a 29 percent increase in marginal likelihood of getting displaced when surpassing the threshold. Figure A.3 in Appendix A, shows the corresponding first stage regression using individual worker layoff notification as the dependent variable where again the probability of notification jumps discontinuously at the threshold 13–22 percentage points depending on the functional form of the control function. These figures reaffirm that the first stage is driven by differences in the likelihood of displacements as opposed to voluntary quits. In other words, workers just to the right of the threshold are not more likely to voluntary leave the firm compared to workers just below the threshold.

If the LIFO rule was fully binding, it would imply a sharp jump in the probability of displacement going from 0 to 1 at the threshold. However, as seen in Figure 2, workers just below the threshold have about a 41 percent risk of being displaced. This “fuzzyness” primarily arise from three sources. First, the 2-digit occupational codes is only a proxy for the workers’ CBA afaffiliation, as the latter unobserved in data. This renders the establishment$\times$occupation combination only approximate of statutory order circuits. Second, tenure is not perfectly measured in the data due to January being reported too often as the first month of employment (see Appendix D). Finally, actual circuits may deviate from the statutory circuits if agreed upon by the employer and local union representatives (as described in section 2). The first two reasons are consequences of to data imperfections (i.e measurement error) which, if sufficiently severe, would attenuate and “smooth out” a true discontinuous first stage (Davezies and Le Barbanchon, 2017).[25] The final reason is an outcome of negotiations and thus even if actual order

[24] The maximum notification time is 12 months and the average difference between workers’ individual notification dates and the date the firm sends in the notification to the PES is 70 days. Hence, not working at the notifying firm 15 months after notification is a fairly good proxy of displacement due to the downsizing. Nevertheless, the first stage is not sensitive to changing this to any number ≥ 12 months.

[25] Davezies and Le Barbanchon (2017) propose an alternative estimator to identify the LATE, made possible when the running variable is observed without measurement error in an auxiliary sample of treated individuals. However, as CBA-affiliation is unobserved for all individuals in my data, no such auxiliary sample exists.

circuits where observed one would be reluctant to use them. Note that the first stage ranks workers on tenure within the (proxied) statutory circuits. Therefore, although actual circuits may deviate from the statutory circuits, this do not induce bias but only attenuates the first stage coefficient towards zero. As seen in Figure 2, using (proxied) statutory circuits captures some compliance to the LIFO rule as the jump at the threshold is both precisely estimated as well as stable across various specifications.

Table 3 show first stage estimates varying the bandwidth and functional form of the control function. Column (1) shows the estimate corresponding to Figure 2 where the estimated jump in the probability of displacement is 12 percentage points. The instrument is highly predictive of displacement with an F-statistic of 62 which is well above conventional levels for evaluating instrument relevance. It is also reassuring that adding covariates (column 2) does not change the estimated first stage coefficient by much, which again confirms balancedness around the threshold. Column (3) and (4) show the first stage estimate narrowing and widening the bandwidth by one fourth, changing the coefficients slightly while still remaining highly predictive of displacement. To further investigate the robustness of the first stage, column (5) and (6) fits a second order polynomial to the control function. Fitting a higher order polynomial to the (optimal) bandwidth of 17 reduces the first stage estimate compared to, e.g., column (2). However, it remains a strong predictor despite the narrow bandwidth where one worry may be that the model over-fits the data. Using a second order polynomial and again increasing the optimal bandwidth by one fourth renders a small change in the first stage estimate by 1 percentage point, as can be seen in column (6). Finally, Figure A.5 a) in the Appendix show the uniqueness of the first stage by displaying results from a placebo permutation test where the cut off is intentionally set at wrong values of the running variable.

Displacement was defined above as having left the firm within 15 months of notification. This allowed for a some time between the firm first reporting the notification to the PES and actual displacement, as the firm is required to personally notify the worker and provide her with some notification period. Nevertheless, there is a dynamic dimension to this first stage as some workers may separate from the establishment later and some workers may be recalled. Moreover, if the firm was doing poorly, future layoffs might be expected which should affect workers who just managed to keep their employment during the first downsizing event. To investigate whether the difference in employment at the notifying firm persists over time, I take advantage of the monthly markers provided by employers along with the annual income statement to trace the dynamic pattern of when workers separate from the notifying firm.

Figure 3 plots the results from 48 separate RD regressions for each month relative to the month of notification where I regress the monthly indicator of having separated from the notifying firm on the instrument Z_{ic}.[26] For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimates of equation (3) which corresponds to the predicted value of separation for workers just below and above of the threshold, respectively. The red dashed

[26] Pooling data from all time periods and staurating the model causes no, or very little, change to the standard errors.

vertical line indicates significance at the 5-percent level, clustering the standard errors at the level of the order circuit. As three quarters of the notified workers in my main sample have at least three months of notification, the likelihood of leaving the notifying firm starts diverging after this notification period has run out. The gap then widens up until about 12 months (99th percentile in notification times) where it stabilizes around a 10 percentage point difference. Importantly, the difference in probability of separation (i.e., not working at the notifying firm in a given month) remains stable throughout. Hence, it does not seem to be the case that workers are getting displaced and later recalled to any large extent, nor that workers surviving a first layoff notification later become displaced in later notifications.[27] The latter statement is also corroborated by Figure A.7 in Appendix, A showing that earnings losses are virtually identical to the main specification (cf. Figure 5) when restricting the sample to establishments with one single layoff during the sample period.[28]

4 Consequences of layoff for workers

This section investigates earnings losses upon job loss induced by the LIFO rule. I start by estimating the effect on annual earnings and proceed by breaking-down the total earnings effect into separate estimates for each margin of adjustment: employment, wages and hours worked. The section ends by decomposing the total earnings losses by margins of adjustment and evaluating their relative importance.

4.1 The total earnings effect

Figure 4 a) gives a snapshot of annual earnings around the threshold in the year following layoff notification.[29] There is a clear downward jump at the discontinuity, where workers just surpassing the threshold earn on average -14.62 thousand SEK less then their coworkers just below the threshold. The effect is precisely estimated and significant at the 1 percent level with a standard error (SE) of 3.25.[30] Adding a second order polynomial decreases the estimated earnings differential to -10.76 ($SE = 4.64$). Figure 4 b) plots annual earnings four years after notification by relative seniority. While the variance in earnings has increased compared to the first year after notification, the earnings differential at the threshold remain significant, but, have become somewhat smaller with workers just above the threshold earning -8.41 thousand SEK (SE = 3.71.) less than their (former) coworkers. The slope of the control function has also changed from a steep downward slope to being almost completely flat. This reflects that

[27] Figure A.6 show the probability of recall by relative tenure ranking where there is no visible nor statistically significant difference in recall at the threshold and the estimated jump at the threshold is 0.009 percent ($SE = 0.01$).

[28] About 81 percent of all estblishments have only one reported layoff notification, 15 percent have 2 notifications and about 4 percent have 3 or more layoff notifications (max 5).

[29] All earnings and wages have been deflated to 2005 values in thousands of Swedish krona (SEK). One thousand SEK roughly corresponds to 100 US dollar or 90 Euros.

[30] Figure A.5 of in the Appendix show that this estimate is indeed unique outcome varying the cut off in a placebo permutation test on annual earnings in $t + 1$.

displaced workers' current earnings no longer correlates with their previous relative ranking at the notifying firm.

To trace out the dynamic response of earnings losses and examine its persistence, Figure 5 shows the evolution of annual earnings relative to time of notification where again each time point corresponds to a separate (reduced form) RD regression. For example, estimates in $t+1$ and $t+4$ corresponds to Figure 4 a) and b), respectively. As before, hollow circles correspond to workers just below the threshold, solid circles to workers just above and the red dash vertical line indicates a significant difference at the 5 percent level.

Figure 5 reveals that before notification earnings evolve in parallel for workers just above and below the threshold which again is indicative of the absence of manipulation and selective firing among employers.[31] Following the layoff notification in $t = 0$, earnings drop and due to the "fuzzy" nature of the LIFO rule, it does so for workers both just to the left and to the right of the threshold. The drop is, however, significantly larger for workers just surpassing the threshold where the difference stems from workers/circuits complying with the instrument. The largest losses occur within the two subsequent years following notification where workers just surpassing the threshold earn 13–15 thousand SEK less than their coworkers just below the threshold. After two years, both groups recover and annual earnings start to increase. There is still, however, a significant earnings differential between the two groups, estimated to -8.92 ($SE = 3.53$) and -8.41 ($SE = 3.71$) in year 3 and 4, respectively. Thus, having a relative ranking just above the threshold, thereby having a 12 percentage point higher probability of layoff, have significant negative impact on a workers' annual earnings even 4 years after notification. Thereafter, workers start to recover and those just above the threshold do so at a faster rate, closing the earnings gap in year 7 after notification where the estimated difference is -0.45 ($SE = 4.35$).

I investigate the robustness of the results by replicating Figure 5, allowing the optimal bandwidth suggested by Calonico et al. (2014) to vary over time and by changing the functional form of the control function. Figure B.1 a) and b) show the estimates from these regressions using both a first and second order polynomial, respectively. The results are remarkably stable when using the optimal bandwidth selector even though optimal bandwidth varies slightly over time. The inclusion of a second order polynomial slightly decreases the initial earnings differential but the pattern of no long-term earnings losses remains consistent and qualitatively the same.

The above results showed the reduced form response, i.e the effect of being just above the threshold within an order circuit, having a 12 percentage point higher likelihood of displacement. To quantify the effect of actual displacement, I instrument displacement D_{ic} with the indicator for being above the threshold Z_{ic} while controlling linearly for workers relative rank (RR_{ic}) within a circuit. These estimates should be interpreted as a LATE for those order circuits and workers complying with the LIFO rule. Panel a) in Table 4 shows IV-estimates on annual

[31] Figure B.1 provide additional evidence of non-selective firing showing balance in earnings for all 7 years prior to notification.

earnings each year relative to notification. During the first year after notification, displaced workers lose on average 122 thousand SEK compared to their non-displaced coworkers. This corresponds to a 42 percent earnings loss which is similar to what has been observed in other studies (cf. Jacobson, Lalonde and Sullivan, 1993; Sullivan and von Wachter, 2009; Lachowska, Mas and Woodbury, 2020; Schmieder, von Wachter and Heining, 2023). The earnings gap shrinks over time, still being significant 4-5 years after notification the gap is closed 7 years after notification with an estimate suggesting that displaced workers earn -3.6 thousand SEK less than their former non-displaced coworkers (who may or may not still be working at the notifying firm). This result stands in stark contrast to the standard result in literature where displaced workers tend to have 10–20 percent lower earnings, well beyond 10 years post displacement (see Table A.1 for a summary). I investigate these seemingly incongruent results more in detail in Section 5 and find that the type of layoff studied, specifically the size of layoff, can to a large extent account for the discrepancy.

Panel b) of Table 4 shows how non-employment differs between displaced and non-displaced workers. This is defined as being registered as not working at least one month during the year. Note that displacement need not imply non-employment as workers may very well find a new job during e.g. their notification period (see Cederlöf et al., 2021). Nevertheless, as expected displaced workers are much more likely to experience non-employment during the first and second year following notification. Among the compliers in the control group, only about 2 percent experience non-employment in $t + 1$ (an estimate which is not significantly different from zero).

The findings suggest that the lions share of initial earnings losses are driven by the loss of employment, but beyond that, displaced workers may also incur lower wages and/or face more volatile employment in terms of e.g. fewer hours. To formalize what drives the earnings differential the difference in earnings between displaced (D) and non-displaced (S) workers at the firm can be written as,

$$\Delta y = w^D h^D l^D - w^S h^S l^S \tag{6}$$

where w and h is the hourly wage rate and hours worked during a month, respectively, whereas l is the number of months worked during a year. This expression can be rewritten as,

$$\Delta y = w^S h^S \underbrace{(l^D - l^S)}_{\text{Extensive margin}} + l^D [h^s \underbrace{(w^D - w^S)}_{\text{Wage effect}} + w^D \underbrace{(h^D - h^S)}_{\text{Intensive margin}}] \tag{7}$$

where the first component reflects the part of the earnings differential stemming from differences in employment. The second and third component reflects the possibility that displaced workers may end up in new jobs paying lower wages or providing fewer hours. This decomposition may be applied either separately for each year or averaged over some fixed time interval T. In the remainder of Section 4, I unpack the total earnings effect and estimate the effect of layoff separately on employment, wages and hours worked, following its dynamics and evaluating its impact over a period of six year period. I then put these pieces of evidence together and decompose the earnings losses using equation (7) to evaluate the relative importance of each adjustment margin.

4.2 Earnings losses by margins of adjustment

Extensive margin Separation from the notifying firm does not mechanically induce non-employment as workers may well find work, e.g., within their notification period. To provide more detail on the effects on employment, I take advantage of the monthly employment markers to trace out the dynamic response. The monthly markers are noisy measures of labor supply due to some employers (both incumbent and new ones) routinely reporting workers having started work in January while the actual employment began in, e.g., March. As this type of measurement error may be more common among displaced workers, estimates should be interpreted as a lower bound.

Figure 6 a) plots the reduced form probability of non-employment by month relative to notification where again each time point is a separate RD regression. Just as with separation from the notifying firm, significant differences in non-employment starts 3 months after notification (as indicated by the vertical dashed red line). These differences remain significant up to 19 months after notification and there are also some significant differences in 26–29 months after notification. During this time, workers just surpassing the threshold are about 2–4 percentage points more likely of being non-employed compared to workers within the same order circuit just below the threshold. Scaling these estimates with the first stage implies that workers that are displaced are about 16-33 percentage points more likely to experience non-employment in a given month, 4 to 19 months after notification. From month 30 and onwards, however, employment differences seems to have subsided as no significant differences can be found within each respective month.

Wage effect Several studies find lower post displacement wages to be an important driver of long-term earnings losses (see e.g. Lachowska et al., 2020; Schmieder et al., 2023; Jarosch, 2023). To estimate differences in wages between displaced and non-displaced workers, I use the Swedish wage register which is an annual survey covering about 50 percent of the private sector workers and all public sector workers. These data contain information on hours worked and full-time equivalent wages conditional on the worker working at least one hour during a sampling week between September-November when the data is collected. Due to the wage register being a random sample (stratified by firm size) of the working population not all employed workers are observed each year which reduces the sample size substantially, making the estimates more imprecise.

Figure 6 b) shows the evolution of log wages relative to month of notification for workers just above and below the threshold. The observed pattern has an interesting connection to the employment response in Figure 6 a). Wage differences are only visible beyond 30 months after notification, that is, when the differences in employment rates have subsided. This implies that the wage gap reflects a pure wage effect induced by displacement and absent of selection. The (reduced form) wage gap is estimated to 2.5–2.6 percent in month 40–42 after notification and significant at the 5 percent level. When scaled by the first stage this would imply that displaced workers incur on average a 20 percent wage loss on their new job. Interestingly, the effects are short lasting; after the wage differential starts to appear, workers having just surpassed the threshold have faster wage growth than their coworkers just below, fully closing the gap about 6 years after notification.

Hours response Similar to the analysis on wages, I use the Swedish wage register to estimate differences in hours worked for workers just above and below the threshold. Similar to Schmieder et al. (2023), I find little evidence of any effect on hours worked following displacement with not significant differences between the two groups at the threshold (not reported). Unfortunately, data on hours worked are somewhat imprecise and highly variable making it difficult to draw any solid conclusions about its variation.

Summary Table 5 summarize the effect of job loss averaged over a period of 6 years post notification. Column (1) show estimates from regressing average total earnings loss on displacement D_{ic} which has been instrumented with Z_{ic}. This corresponds to the weighted average of columns (1)-(6) in Table 4 showing that displaced workers having forgone about 77 thousand SEK on average each year, 6 years after job loss. Similarly, columns (2)-(4) in Table 5 show IV-estimates for each margin of adjustment over the 6 years following displacement. Column (2) indicates that displaced workers work an average of 1.21 months less during the 6 year period and earn about 4 percent lower wages during this time (column 3),. As expected, however, the wage effect is not statistically significant as the wage differential only is present during a short period of time (see Figure 6). The hours response is very imprecisely estimated. If anything displaced workers work somewhat fewer hours, but, I interpret this effect to be of minor importance in explaining the total earnings loss.

4.3 Decomposing the earnings effect

I now return to the decomposition of the difference in earnings as described in equation (7) considering a period of 6 years. I use of the estimates from Section 4.2 to determine the relative contribution of each adjustment margin. Plugging in the estimates into equation (7) yields

$$-76.8 \approx \underbrace{24.5 \overbrace{(-1.21)}^{\text{Extensive margin}}}_{39\%} + 9.9[\underbrace{134 \overbrace{(-.043 \times 0.01)}^{\text{Wage effect}}}_{14\%} + \underbrace{0.17 \overbrace{(-3.58)}^{\text{Intensive margin}}}_{8\%}] + \underbrace{\varepsilon}_{39\%} \tag{8}$$

where Table 5 provides the estimates plugged into equation (8). The total earnings effect on the left-hand-side its taken from column (1) whereas estimates to the first, second and third component on the right-hand-side are taken from column (2), (3) and (4), respectively.

Over 6 years, about 39 percent of the average losses incurred by a displaced worker can be attributed to non-employment. The second term multiplies the wage difference of 4.3 percent with the difference in hourly wage (in thousand SEK) and when multiplied with the months and hours worked by stayers and leavers, respectively, the wage effect make up only 14 percent of the average losses over time. Plugging in the point estimate for hours worked suggest that about 8 percent of the earnings loss comes from reductions in hours, but is measured with a large error. As I take the left-hand-side of the equation as given and try to predict it by separate estimates from each margin of adjustment I end up with a residual, being the difference between the estimated total earnings loss and that predicted jointly by the three adjustment margins. The share of earnings loss is left unexplained when joining the separate predictions is about 39 percent.

5 Understanding earnings losses upon job loss

The main finding of Section 4, that earnings losses upon displacement are transitory rather than persistent, appears to be at odds with the standard result in literature. As noted above, permanent earnings losses following displacement have been found in several different countries and over multiple time periods.[32] For example, Davis and von Wachter (2011); Lachowska, Mas and Woodbury (2020) both find that displaced workers in the US have earnings losses ranging between 15-20 percent up to 20 years after displacement. Although somewhat smaller, Schmieder, von Wachter and Heining (2023) and Eliason and Storrie (2006); Seim (2019); Athey et al. (2023) also find substantial long-term earnings losses in Germany and Sweden, respectively. Finally, Bertheau et al. (2022) document similar earnings pattern after displacement in several European countries.

To better understand what drives earnings losses upon job loss and its persistence, I start by estimating earnings losses for workers displaced during mass layoff using the standard distributed lag model, following the conventional definitions and sample restrictions of the literature. The aim of this exercise is to provide estimates that are comparable to previous studies and to verify that there is nothing special in the Swedish context or the particular time period I study which render earnings losses to be transitory. The result from this exercise replicates the standard finding of large and highly persistent earnings losses which begs the question; why are the long-run earnings losses so different between the two settings?

First, I explore if compositional differences across the two samples could account for the difference in long-run outcomes. To this end, I reweight the conventional mass layoff estimates to mimic the LIFO sample on observable characteristics. Second, I address the fact that the identifying variation in the canonical mass layoff estimator and the RD-estimator differes in that the former exploits variation across firms and the latter within firms, thus making it two potentially very different experiments. To account for this, I combine the two estimators into one, by matching workers in the LIFO sample who are below the threshold to workers in non-

[32] Table A.1 lists some of the most recent and influential studies estimating earnings losses upon displacement.

downsizing firms, creating a control group similar to that of the previous literature. Finally, to probe the impact of the size of layoff, I estimate earnings losses using the original LIFO sample, pooling layoffs by size into quartiles.

5.1 Estimating earnings losses using mass layoff

Using the matched employer-employee data, I follow Jacobson, Lalonde and Sullivan (1993) and define a mass layoff to be an event where at least 30 percent of the workforce leaves a plant within in a year t. To be sure that 30 percent is indeed a significant event, I consider only plants with 50 or more employees in a given year as smaller firms are subject to larger percentage fluctuations in employment. I define a workers' main employer as being the one which gives him the highest earnings in a year and the worker is displaced if he is notified in year t and leaves the establishment between year t and $t+1$ or $t+2$ and do not reappear at the displacing establishment within subsequent 4 years.[33] Finally, to facilitate comparisons with the earlier literature I consider only male workers between age 25 to 55 with at least 5 years of (consecutive) tenure.

To create a control group for displaced workers, I sample all workers from plants which did not carry out a mass layoff.[34] I then restrict attention to workers satisfying the baseline restrictions made on the displaced workers and use propensity score matching and match workers on 2-digit industry, tenure, age, earnings in $t-2, t-3$ and $t-4$, separately by each year of displacement. Using a nearest-neighbor algorithm each displaced worker is then assigned a comparison worker (without replacement) in a non-displacing firm. This yields a group of non-displaced workers for whom I can observe their entire work record and who are almost identical to workers who later become displaced (see Table A.3). A common, although debated, restriction made on the control group is conditioning them staying employed throughout the entire sample period. As highlighted by Krolikowski (2018), this renders one to attribute all future job instability of the treated workers to the initial displacement thus exaggerating the impact of displacement on earnings losses. I therefore compare displaced workers with and without this restriction in order to align with the previous literature.

Following standard procedure, I estimate earnings losses upon displacement using a distributed lag model of the form

$$y_{it} = \gamma_t + \alpha_i + \pi_k + \sum_{k=-5}^{10} \delta_k D_{it}^k + u_{it} \tag{9}$$

[33] One limitation in the literature on the consequences of displacement is the inability to separate between involuntary and voluntary separations in matched employer-employee data. This is also one of the main reasons for focusing on high tenured workers. In my main analysis, I make use of the notification data to identify involuntary separations. Figure A.8 b) show results when defining a worker as being displaced as the plant in $t+1$ or $t+2$ without conditioning on being notified. Here, short-run earnings losses are somewhat smaller indicating that a non-negligible share of separations are voluntary creating and upward bias. Long-run losses are virtually unchanged.

[34] I have also experimented with restricting the growth in the non-displacing establishments as in e.g. Flaaen et al. (2019). The results remain unchanged (results available upon request).

where y_{it} is annual earnings for worker i in year t. γ_t and α_i are calendar-year and worker fixed effects, respectively. The D_{it} are dummy variables equal to 1 in the k^{th} year relative to displacement, where $k = -3$ is the baseline year. Following Schmieder, von Wachter and Heining (2023), I also include π_k which are fixed effects for year relative to baseline year. The coefficients of interest are the δ_k which reflect differences in annual earnings between displaced and non-displaced workers by each year relative to the baseline year.

5.1.1 Results

Figure 7 shows the difference in earnings by pooling workers displaced 2005-2015 along with their matched non-displaced workers. Due to propensity score matching, both groups have almost identical trends in annual earnings in the pre-displacement period, suggesting that the matching procedure along with individual fixed effects has created a comparable control group. The solid black line shows the earnings differential between displaced and non-displaced workers when not conditioning on the control group being employed in $t > 0$ (as in e.g. Schmieder, von Wachter and Heining, 2023). The dashed line show the same differential, but, conditional on non-displaced workers remaining employed with same employer throughout the sample period (as in e.g. Lachowska, Mas and Woodbury, 2020).

Initial earnings losses of displaced workers amount to a little more than 73 thousand SEK two years after the layoff event which corresponds to about 25 percent of pre-displacement income. As time goes by, displaced workers recover some of their initial losses but even after 10 years the earnings differential between displaced and non-displaced workers are on average about 41,000 SEK (14.2 percent). The wage losses 10 years after notification amount to about 5 percent (see Figure A.8). These results are very similar to what has previously been found for mass layoffs in Sweden (c.f. Athey et al., 2023; Seim, 2019; Eliason and Storrie, 2006). In line with Krolikowski (2018), I also find that a key factor in explaining the high persistence of earnings losses lies in the handling of the control group. The dashed line in Figure 7 shows estimates conditioning on future employment of the non-displaced workers. This increases earnings losses after 10 years by almost 50 percent implying a loss of about 72,000 SEK which corresponds to 20.8 percent of pre-displacement earnings.

In the Appendix, Figure A.8 show results from several robustness checks on the mass layoff analysis. Estimates are unaffected by lowering the worker tenure restriction to 3 years. Treating also non-notified workers as displaced lowers initial earnings losses while long-run losses are virtually unchanged. As the LIFO analysis in Section 4 exclude plant closures, I exclude these from the mass layoff analysis to make the estimates more comparable. This only affects losses directly in the year after displacement while again long-run losses remain unaffected.

In summary, the results suggests that there is nothing particular about the Swedish context or time period which could explain the absence of long-term earnings losses in the main analysis.

5.1.2 Reweighting mass layoff estimates

As seen in Table 1, the LIFO-sample used in the RD analysis differ from the mass layoff sample having applied the typical restrictions in the literature. In particular, workers in the latter sample are on average almost 4 years older, work in larger firms and have almost twice as long tenure. Furthermore, the two samples differ in industry composition. To determine to what extent differences along these dimensions contribute to the difference in the earnings gap, I use a weighting produce, following DiNardo, Fortin and Lemieux (1996) (henceforth DFL), to reweight the mass layoff sample to match the LIFO sample.[35] This amounts to estimating a probit regression in data combining the two samples where the dependent variable is an indicator for belonging to the LIFO-sample. From this regression, I obtain a propensity score $\hat{p}(x)$ which I use to reweight the mass layoff sample by $\phi(x) = \hat{p}/(1 - \hat{p})$.[36]

Figure 8 plots the estimated earnings gap applying the weighting scheme where the blue dashed line depicts estimates reweighted on the worker characteristics; age, age squared, level of education, immigrant status, gender, tenure and tenure squared. The original (unconditional) estimates (same as in Figure 7) are shown in black for comparison. Reweighting the sample on worker characteristics increase earnings losses one year after notification by about 46 percent while long-run losses decrease by about 30 percent (year 7–10). Nevertheless, despite this decrease there is still an economically as well as statistically significant earnings gap between displaced and non-displaced workers 10 years after notification, amounting to 24 thousand SEK. The red line in Figure 8 weights in addition to worker characteristics on the log of establishment size and industry composition whereas the green line also reweight on year of layoff to hold constant overall economic conditions. The latter adjustment increases the short-run losses substantially due to putting more weight on years of the Great Recession, very much consistent with Schmieder, von Wachter and Heining (2023) finding that earnings losses are larger in recessions. Long-run earnings losses also increase to about 31 SEK, 10 years after notification.

These results suggest that only a small part of the difference in the earnings gap between the canonical event-study approach using mass layoffs and the RD analysis can be explained by differences in worker characteristics. After adjusting for compositional differences, permanent earnings losses are still present even 10 years after layoff suggesting that mass layoffs, in contrast to smaller redundancies, render permanent earnings losses primarily for other reasons.

5.2 Event study vs. RD estimator

A key difference between the canonical mass layoff estimator and the RD is that the former exploits variation across establishments and the latter within establishment. Thus, the two estimators focuses on two potentially very different experiments. In the LIFO analysis, treatment consists of being displaced *contingent* on working at a distressed establishment whereas the traditional estimator captures the effect of displacement *and* working at a distressed establishment.

[35] This is very much inspired by and follows Illing, Schmieder and Trenkle (2021) who use the same procedure to study the post displacement gender earnings gap.

[36] Table A.4 present average characteristics of the mass layoff sample reweighted to mimic the LIFO-sample.

This as the matched control group in the latter consists of workers in relatively more stable firms. Although there is little indication of the earnings gap in the RD analysis closing due to workers below the threshold getting laid off at a later point in time (see Figure 3 and Figure A.7), remaining at a distressed firm may imply smaller wage growth compared to working at a more stable firm, which could potentially explain the discrepancy in results. An additional difference between the two estimators, although more subtle, is that the traditional estimator identifies the effect for the average displaced worker, whereas the RD analysis pertains to the marginally displaced worker.

In an effort to align the two estimators, I combine the two approaches by matching workers below the threshold to workers at more stable firms (similar to the typical control group used in the literature). Again, using a nearest-neighbor algorithm, I match each worker to an "identical twin" working at a relatively stable firm which I define as establishment growth in the interval ± 5 percent, as in Flaaen, Shapiro and Sorkin (2019). Each twin then replaces the original observation and is assigned the same relative ranking in the order circuit. I match workers on age, age squared, earnings in t to $t-3$, all within year of notification, occupation, tenure and industry cells. This renders a matched sample almost identical to the original workers below the threshold (see Table A.6) and as a consequence the "twins" just below and the (original) workers just above the threshold are also balanced in terms of observables (see Table A.7). This combined estimator thus relies on across establishment variation and would then capture any additional effect of working at a distressed establishment while also estimating earnings losses for the marginally displaced worker.

Figure 9 a) illustrates the first stage of this combined estimator where the black dots left of the threshold are the identical twins of the original workers below the threshold (shown I gray) and workers to the right of the threshold are from the original sample (same as in Figure 2). Naturally, using workers at more stable firms as the contra factual renders a larger first stage where the probability displacement jumps by 34.7 percentage points just at the threshold.

The effect of displacement on future earnings is shown in Figure 9 b) where I plot IV-estimates having instrument displacement with the indicator for being above the threshold, again controlling linearly for workers relative rank. The black solid line shows the earnings gap using the matched control group whereas the red hollow circles show the original IV-estimates (without confidence intervals) from Table 4 for comparison. Displaced workers when compared to workers in non-downsizing firms (the matched sample) do suffer about as large earnings losses initially as when using within establishment variation. However, the following recovery is somewhat slower rendering a larger earnings gap beyond $t+1$. This may be due to non-displaced workers in downsizing firms having on average lower wage growth than non-displaced workers in more stable firms. Nevertheless, displaced workers do on average seem to recover with the earnings gap turning insignificant 7 years after notification and is more or less closed after 10 years.

A novel feature of this new estimator is that in contrast to the previous literature who has exploited mass layoffs as an instrument for displacement, it relies in the LIFO rules which enables the study of smaller sized layoffs. The dashed lines in Figure 9 b) splits the sample into small and large layoffs by the median, rendering large layoffs to have on average 19 percent of its workers displaced. An interesting pattern emerges where workers in small and large layoffs have about the same earnings dynamics during the first 4 years, but their recovery pattern is quite different thereafter. While workers laid off in smaller layoffs recover and close the earnings gap around 7 years after notification, workers displaced in large layoffs earn at that time about 50 thousand SEK less compared to similar non-displaced workers (in stable firms). In terms of magnitude, these long-run losses are also very similar to those found using the standard approach in section 5.1.[37] Moreover, the recovery for these workers appear to stagnate beyond 7 years after layoff.

These results suggests that although the choice of control group (across or within establishment) can matter for the average size of the earnings losses, it cannot account for the transitory pattern seen in section 4. Instead, the size of layoff appear to be a key determinant in replicating the earnings gap as permanent earnings losses can only be found among workers displaced in large layoffs. Thus, mass layoffs appear to be something fundamentally different than displacements due to more regular adjustments to employment, generating more severe consequences for displaced workers.

5.3 Earnings losses by size of layoff

If large layoffs do render more severe consequences for workers, the pattern should also be visible within the LIFO sample. Therefore, I use the LIFO thresholds and estimate earnings losses, pooling layoffs by size.[38] This analysis has the advantage of holding constant any specific sample restrictions or caveats imposed in the RD analysis (see section 2.1), thus making sure that an absence of a long-run earnings effect cannot be attributed to these factors.

Figure 10 a) and b) plots the earnings dynamics for workers above (solid line) and below (dashed line) the threshold for small and large layoffs, respectively, where again each time point correspond to a separate (reduced form) regression with the vertical dashed line indicates significance at the 5 percent level. Although somewhat imprecisely estimated due to splitting the sample in two (below/above the median), the figures reveal striking heterogeneity across layoffs. Whereas workers displaced in smaller redundancies close the earnings gap 6 years and thereafter earn more than their former coworkers, workers laid off in large layoffs lack the same recovery and have an average estimated (reduced form) earnings gap of 13,000 SEK, 7 to 10 years after notification. Noteworthy, is that the difference seem to occur due to workers above the

[37]Figure A.9 plots for comparison these estimates along with the ones attained in Figure 7 the using the standard approach. For the short run earnings gap the standard approach renders smaller losses which could to a large extent be explained differences in the timing of layoff (see Figure 8). Figure A.9 also show estimates when reweighting the large layoffs to mimic the smaller ones in terms of the composition of worker, industry and year of notification. This changes the estimates only marginally.

[38]Size of layoff is defined as the number of notified workers divided by the total number of workers at an establishment. The number of notified workers is thus summed over potentially several different order circuits within the same establishment.

threshold having different recovery patterns in small *vis-à-vis* large layoffs while their respective control groups to a large extent are at similar earnings levels 6 years after notification. This again suggests the effect is not driven by the control group getting displaced at a later point in time but rather that workers displaced in smaller layoffs recover faster in contrast workers loosing their job in large layoffs.

To gain precision, Figure 10 shows estimates of differences in long-run earnings, pooling years 7–10 after notification for small and large layoffs, respectively. This indicates that workers above the threshold who are displaced in large layoffs earn on average 10,000 SEK less than their former coworkers just below the threshold, significant at the 10 percent level. The corresponding IV-estimate implies that displaced workers earn 66,000 SEK less than their non-displaced former coworkers, which is roughly the same magnitude of long-run losses seen in section 5.1 and 5.2 using alternative empirical strategies.

To probe the pattern further, Figure 10 also show estimated earnings losses splitting the sample into quartiles of size (solid circles).[39] Strikingly, there is a clear (monotonic) negative correlation between the size of layoff and long-run earnings losses. Among the largest layoffs (Q4), workers above the threshold earn on average 14,800 less than their former coworkers below the threshold. ($p = .089$). In contrast, workers displaced in the smallest layoffs (Q1) earn 17,500 more then there former colleagues ($p = .056$).[40] To what extent is this correlation driven by compositional differences across layoffs? To answer the red circles show estimated earnings losses when adjusting for difference (relative to workers in $Q2$) in worker and industry composition as well as time of layoff, using the DFL-reweighting scheme as in section 5.1.2.[41] Doing so does render a slight increases of the long-run earnings gap for workers displaced in large layoffs, turning significant at the 10 and 5 percent level for Q3 and Q4, respectively, but does not alter the overall pattern.

6 Sources of permanent earnings losses in mass layoffs

Why do larger layoffs bring about greater and more permanent earnings losses? On the one hand, at the individual level, larger layoffs may reduce negative signals to employers which in turn could reduce workers earnings and employment losses (Gibbons and Katz, 1991). On the other hand, large layoffs may cause workers to lose a larger share of their human capital if larger layoffs are e.g. more frequent in industries where the degree of firm specific human capital is high or if workers in these layoffs have certain characteristics which negatively influence future earnings. On the aggregate, mass layoffs tend to be more frequent during periods of economic distress and earnings losses tend larger for workers laid off during economic recessions (Davis and von Wachter, 2011; Schmieder, von Wachter and Heining, 2023). However, as shown above, while these mechanisms help explain the overall cost of jobs loss they seem unable to account

[39]In the Appendix, Figure A.10 show the negative correlation between estimated earnings losses and size of layoff even more granularly.

[40]The difference in estimated earnings gap between the unweighted Q1 and Q4 is 32,320 SEK with $p = .010$.

[41]Table A.5 in Appendix A show summary statistics for the unweighted and weighted samples.

for the *difference* in long-run earnings losses between mass layoffs and smaller redundancies as these remain after adjusting for worker and industry composition as well as the time of layoff.

One possibility is that differently sized layoffs generate differences in other post-displacement outcomes which are correlated with lower earnings. Table 6 provides IV-estimates from the discontinuity separately for small and large layoffs, where the latter have again been reweighted to mimic smaller layoffs. The first row of Table 6 reiterates the earnings pattern seen above with short-run losses being more or less equal for displaced workers in small and large layoffs, while in the long-run permanent losses are only present among workers displaced in large layoffs. In square brackets below each set of estimates is the p-value from testing equality between the two estimates.

Driving the difference between large and small layoffs appear to be exposure to non-employment. Both in the medium and long-run, workers displaced in large layoffs are exposed to more non-employment and are more likely to be registered as unemployed than their non-displaced coworkers. The latter effect is also significantly different across layoff size in the long-run. As expected, displaced workers have a higher probability of having switched industry and occupation compared to their non-displaced former coworkers. Interestingly, workers displaced in large layoffs have a significantly higher probability of working in another region compared to where they were displaced and are less likely to live and work in the same region. The latter effect is significantly different across small an large layoffs at the 10 percent level ($p = .055$). In contrasts to e.g. Schmieder, von Wachter and Heining (2023), I detect no significant differences in wages or firm size between previously displaced and non-displaced workers. However, both outcomes are already to begin with highly variable and thus partitioning the data into three groups further exacerbates the problem. Thus the results should be interpreted with caution.[42]

Overall, there are few detectable differences in post employment outcomes between workers displaced in small *vis-a-vis* large layoffs. What stands out is large layoffs rendering more exposure to non-employment and increasing the probability to find employment in another region. One interpretation of these findings, in line with Gathmann et al. (2018), is that large layoffs may have an impact on the entire local labor market, prolonging non-employment and making workers more prone to find work elsewhere. I examine the role of non-employment and the local labor market in the following sections.

6.1 The role of non-employment

Recent work by Schmieder, von Wachter and Heining (2023) and Fallick et al. (2021) show that non-employment duration is a key factor in explaining earnings losses after displacement. Also, Burdett et al. (2020) and Jarosch (2023) suggest that the loss (or forgoing) of human capital while unemployed is a key contributor to permanent earnings losses. Thus, for a given

[42] I have examined several other dimensions of firms such as firm value-added, profits, revenue as well as both firm and establishment FE's estimated using AKM. None of these outcomes show any significant difference in outcomes, neither between displaced an non-displaced workers, nor across layoff size.

worker, getting displaced in a mass layoff may render worse outcomes if it causes longer periods of unemployment, rendering depreciation of human capital and/or potentially discouraging the worker from participating in the labor market all together. Figure 11 a) and b) show the probability of becoming long-term unemployed after layoff while c) and d) show the likelihood of workers having left the labor force.[43] The outcomes are shown separately by size of layoff where large layoffs are again reweighted to match the worker and industry composition among the smaller layoffs as well as the time of layoff to account for differences in overall economic conditions. The top panel of Figure 11 reveals that workers just above the threshold in large layoffs are more likely to become long-term unemployed in the year following notification compared to their coworkers just below the threshold. Interestingly, no such difference is observed at the discontinuity among the smaller layoffs. The bottom panel show the evolution of workers having left the labor force, relative to time of notification. Also here, there is no discernible difference among workers in small layoffs while for large layoffs, displaced workers are about a 3 percentage point more likely to have left the labor force 7–10 years after layoff. Again, it is worth reemphasizing that these estimates are reweighted to match the worker composition among workers in smaller layoffs, in particular on age and tenure.[44]

The finding that non-employment and zero earnings contribute a lot to the persistence of earnings losses is not new. Again, both Schmieder et al. (2023) and Fallick et al. (2021) who show that workers non-employment duration plays key role in explaining for the magnitude and persistence in earnings losses after displacement. Furthermore, Flaaen et al. (2019) shows that excluding zero earnings observations render long-run earnings losses of only about a third of those observed when accounting for non-employment. However, the fact that large layoffs, in contrast to smaller ones, render longer unemployment duration and cause substantial discouragement among workers is to the best of my knowledge new to the literature.[45]

6.2 The role of local labor markets

Trivially, the key difference between small and large layoffs is the sheer number of workers getting displaced. As such, in a mass layoff, more workers with the same type of skills become unemployed and in general compete for the same type of jobs. This type of labor congestion

[43] Having left the labor force is defined as, for a given year, having zero earnings and not being registered as unemployed at the PES.

[44] Whereas the upward trend is driven by workers being near retirement age, the difference remain roughly the same when restricting the sample to workers aged 18–55 (see Figure A.11 in Appendix A). This is indeed expected as aged is balanced across the threshold.

[45] When estimating distributed lags models with a matched control group, Fallick et al. (2021) finds that workers separating from non-distressed firms have similar, and quite surprisingly, perhaps even larger earnings losses compared to workers separating from distressed firms. My results do not corroborate this finding, which in turn also stands in contrast to those of Jacobson, Lalonde and Sullivan (1993). Unfortunately, Fallick et al. (2021) are unable to evaluate to what extent their result could be accounted for by workers in small layoffs being more adversely selected as they lack data on worker characteristics (aside form gender and age). Moreover, unlike in Flaaen et al. (2019) who through survey data have the individuals' reported reason for separation, it is impossible to asses to what extent the result is driven by separations in non-distressed firms being more likely to be due to worker choice and firings for cause; which is one of the biases aimed to be avoided by focusing on mass layoffs (Couch and Placzek, 2010).

would not only prolong unemployment but also make workers more prone to find work outside the local labor market. Furthermore, as shown by Gathmann et al. (2018), mass layoffs may have negative spillovers, bringing about additional employment losses as other firms, in particular in the same industry, are either forced to downsize or abstain from hiring. This would further increase competition and limit the amount of jobs available which, as shown by Gulyas and Pytka (2020), is a key determinant of post displacement earnings losses.

Layoffs that are large relative to size of the establishment are also typically large in relation to the local labor market and industry.[46] As such, mass layoffs may have an impact the entire local labor market. Figure 12 shows worker earnings relative to displacement splitting layoffs not only by size relative to the establishment (as in Figure 10), but also by the size of the downsizing firms' industry in the local labor market.[47] One may view the latter partition as the importance of a particular industry to employment in a local labor market. As seen in panel a) and b), splitting small layoffs by the size of the industry matters little for long run earnings losses. Nevertheless, there is some indication that getting displaced in a small layoff from a firm operating in one of the (locally) larger industries may even be beneficial for the worker in the long run. One potential explanation could be that workers who are being displaced from firms doing poorly could instead quite easily transfer their skills to new firms with better prospects.

Panel c) and d) in Figure 12 partitions large layoffs into small and large industries, respectively. Interestingly, workers laid off in large layoffs but in relativity smaller industries have overall small earnings losses and no sign of permanent scarring. In contrast, earnings losses are both large and permanent among workers displaced in large layoffs where the industry of the downsizing firm account for a large share of total employment in the local labor market. I interpret these results as even though a layoff may be large in terms of a the share of the workforce getting displaced, there are other alternative employers and industries able to absorb the unemployment shock. In contrast, once a layoff is both large and occurs in one of the (locally) dominating industries, overall demand drops and workers get exposed to more non-employment causing discouragement and loss of human capital.

7 Conclusions

This paper examines the question of how workers are affected by job loss in terms of their future earnings, wages and employment. The empirical approach builds on exploiting discontinuities in the probability of displacement generated by a last-in-first-out (LIFO) rule used at layoffs in Sweden. Whereas current evidence almost exclusively pertain to workers displaced in mass layoffs and plant closures, the new research design employed in this paper allows me to study

[46] Figure A.12 in the Appendix plots the correlation between size of layoff relative to the establishment against the size relative to the local labor market (in logs) as defined by 1-digit industry and municipality. The slope coefficient is $\beta = 0.398$ (0.011).

[47] The split between small and large is done at the median in each distribution such that a large layoff corresponds to downsizing by more than 20 percent of the workforce whereas a large industry (one digit level) employs at least 18.2 percent of all employed in the local labor market.

the effects of job loss for both smaller redundancies and large (mass) layoffs.

I find that displaced workers suffer substantial earnings losses during the first two years after displacement compared to their non-displaced coworkers. However, on average, workers recover fully within 7 years after being laid off. Importantly, this not driven by initially non-displaced workers loosing their job at later point in time but rather that displaced workers climb back up the job ladder, experiencing faster wage growth.

As these findings stand in contrast to the standard result in the literature of permanent earnings losses after displacement, I probe this result in great detail. First, I replicate the canonical mass layoff approach (Jacobson, Lalonde and Sullivan, 1993) finding large and persistent earnings losses, also when accounting for differneces in sample composition. Second, I construct a new estimator, combining the traditional mass layoff estimator and the RD-design, to account for differences in estimation strategies and contra factual states. I find that workers on average recover from displacement, but workers displaced in large (mass) layoffs suffer long run earnings losses. Third, estimating worker earnings losses separately by size of layoff again reveal that permanent losses are only found among workers displaced in large layoffs whereas workers displaced in smaller redundancies could potentially even benefit from job loss in the long-run. I show that the pattern remains, even after reweighting large layoffs to mimic the worker and industry composition of larger layoffs, as well as the economic conditions in which they occurred. Finally, I find that large layoffs render workers more exposed to non-employment, (long-term) unemployment and have and higher probability of leaving the labor force and switching local labor market.

Taken together, these results suggests that displacement due to mass layoff is different from job loss due to smaller and far more common redundancies and that permanent worker scarring may not be as ubiquitous a phenomenon as previous research may have lead one to believe. Still, I see my findings as being consistent with much of the recent literature exploring the sources of long-term earnings losses after a mass layoff. In particular, Gathmann et al. (2018) showing how firms in the same industry as a mass layoff plant suffer from negative spillovers and Burdett, Carrillo-Tudela and Coles (2020) and Jarosch (2023) showing how displacement render longer and serially correlated unemployment spells where workers lose human capital.

This paper sheds new light on the question on how workers are affected by job loss and whether displacement create lasting scars or merely temporary blemishes. While the former shows every sign of being true for workers displaced in large layoffs, for the vast majority of job loss occurring in smaller sized layoffs, workers eventually recover from the adverse shock. These findings raise questions about the generalizability of results based on mass layoffs. Given its widespread use as an instrument for job loss in identifying the consequences on various outcomes such as health, mortality, crime, etcetera, more research is needed to understand how the size of layoff influence worker outcomes and what role local labor market conditions play in determining its severity.

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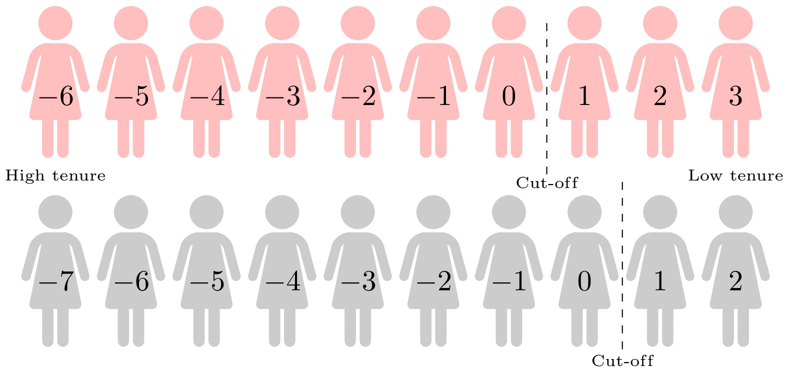
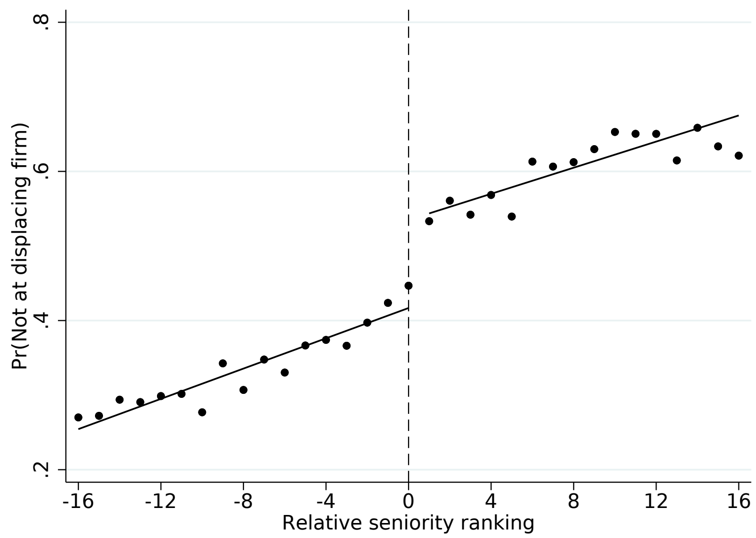
Figures and Tables

Figure 1: Graphical illustration of two order circuits



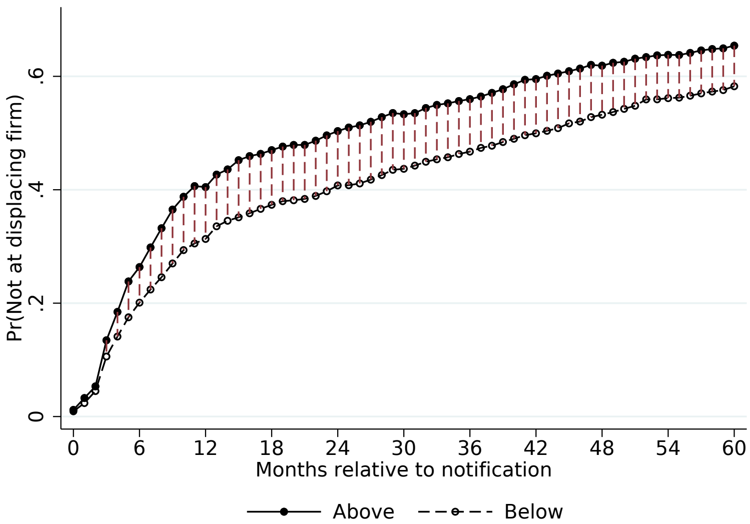
Notes: The figure illustrates workers relative tenure/seniority ranking, normalized to zero at cut-off, for two different occupations (pink and gray) within an establishment in a given year which together forms two order circuits. Workers right of the cut-off, with positive relative ranking are those who, according to the LIFO rule, ought to be displaced when a firm downsizes due to shortage of work.

Figure 2: Probability of having left notifying firm



Notes: The figure shows the probability of displacement as a function of a workers relative ranking within an order circuit (in discrete bins), normalized to zero at the threshold. The regression comes from estimating equation (3) with a linear control function interacted with the threshold, using a bandwidth of ±16. The regression also includes baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s. The point estimate of the jump at the threshold is 0.119 with a standard error of 0.015 which corresponds to an F-statistic of 61.98. Standard errors are clustered at the order circuit level.

Figure 3: Probability of having left notifying firm relative month of notification

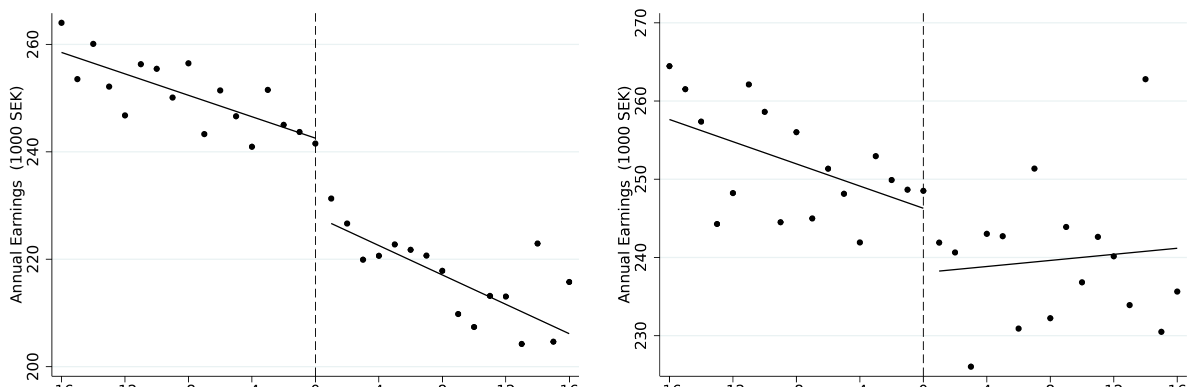


Notes: The figure shows the probability of having left the notifying firm for a given month relative the month of notification. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (3) which corresponds to the average predicted value of each outcome for workers just to the left and right of the threshold, respectively. The dashed vertical line indicates significance at the 5-percent level where standard errors are clustered at the order circuit level. The regressions use a bandwidth of ±16 and include a linear control function interacted with the threshold as well as baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s and order circuit FE:s.

Figure 4: Annual earnings by relative seniority ranking

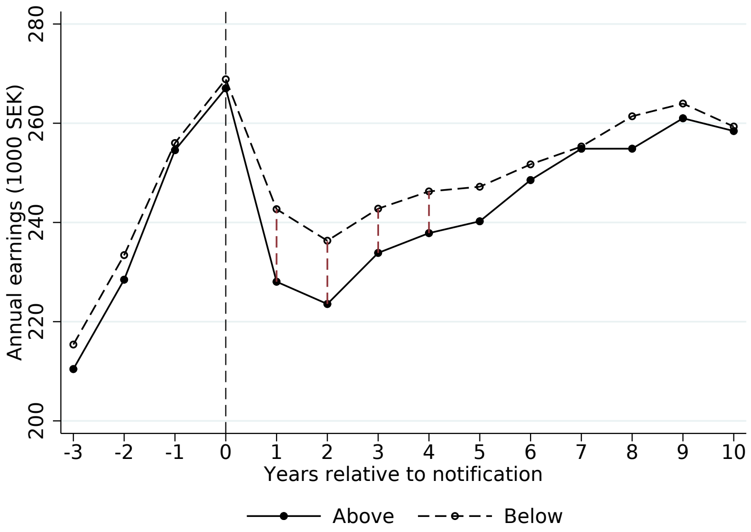
(a) 1 year after notification

(b) 4 years after notification



Notes: The figure shows annual earnings in 1,000 SEK in (a) the first year and (b) four years after notification as a function of workers' relative ranking within an order circuit (in discrete bins), normalized to zero at the cut-off. The regressions comes from estimating equation (3) with annual earnings as the dependent variable, using a bandwidth of ±16 and including a linear control function interacted with the threshold. The regressions also includes the baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s.

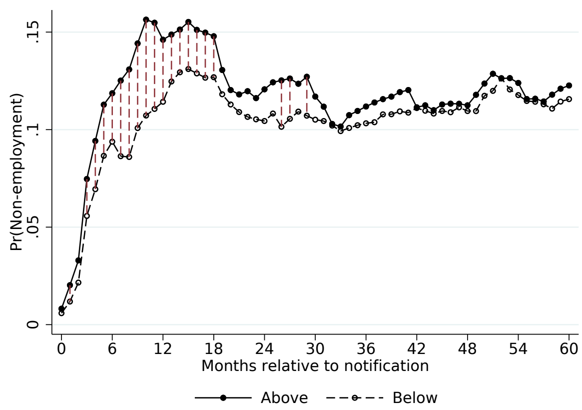
Figure 5: Evolution of annual earnings relative to year of notification



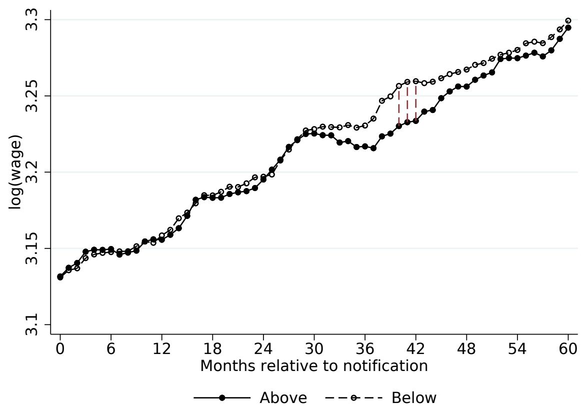
Notes: The figure shows annual earnings relative to the year of notification. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (3) with annual earnings as the dependent variable which corresponds to the average predicted value of each outcome for workers to the left and right of the threshold, respectively. The dashed vertical line indicates significance at the 5-percent level where standard errors are clustered at the order circuit level. All regressions use a bandwidth of ±16 and include a linear control function interacted with the threshold and baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s.

Figure 6: Evolution of non-employment and wages by month relative to notification

(a) Non-employment

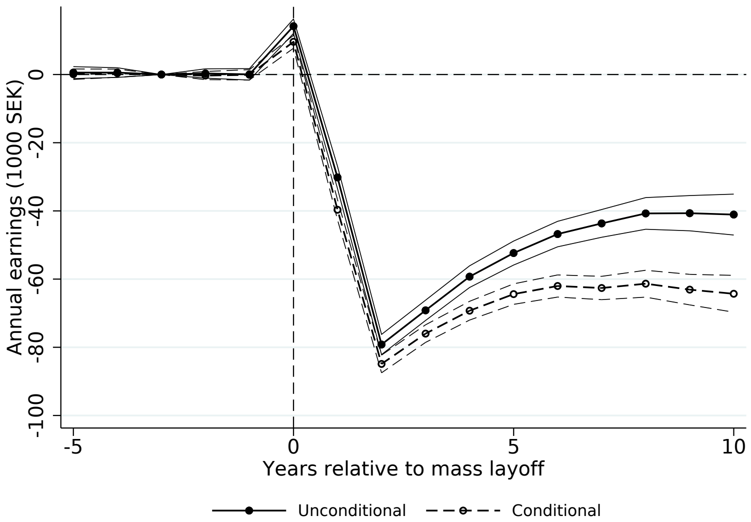


(b) log(monthly wage)



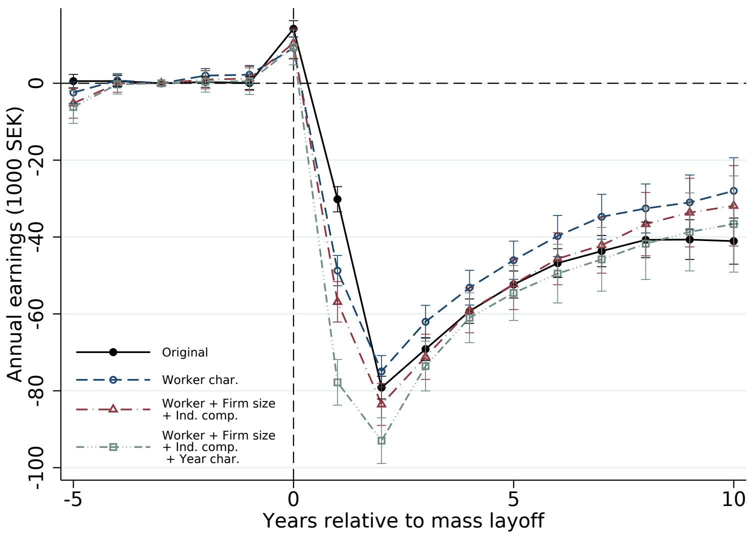
Notes: The figure show the (a) probability of non-employment and (b) log monthly full-time equivalent wage for a given month relative the month of notification. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (3) which corresponds to the average predicted value of each outcome for workers just to the left and right of the threshold, respectively. The dashed vertical line indicates significance at the 5-percent level where standard errors are clustered at the order circuit level. All regressions use a bandwidth of ±16 and include a linear control function interacted with the threshold and baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s.

Figure 7: Earnings losses upon mass layoff



Notes: The figure shows the difference of annual earnings between displaced and non-displaced workers by years relative to a mass layoff event where earnings is normalized to zero at $t-3$. Displacement is defined as a worker being notified and leaving the plant in year $t+1$ or $t+2$ during a mass layoff event. The plotted estimates are δ_k from equation (9) along with 95 percent confidence intervals where standard errors are clustered at the individual level. The solid black line show losses of displaced workers when not conditioning on the control group being employed in $t > 0$. The dashed black line conditions on the control group being employed throughout the entire sample period. Standard errors are clustered at the individual worker level.

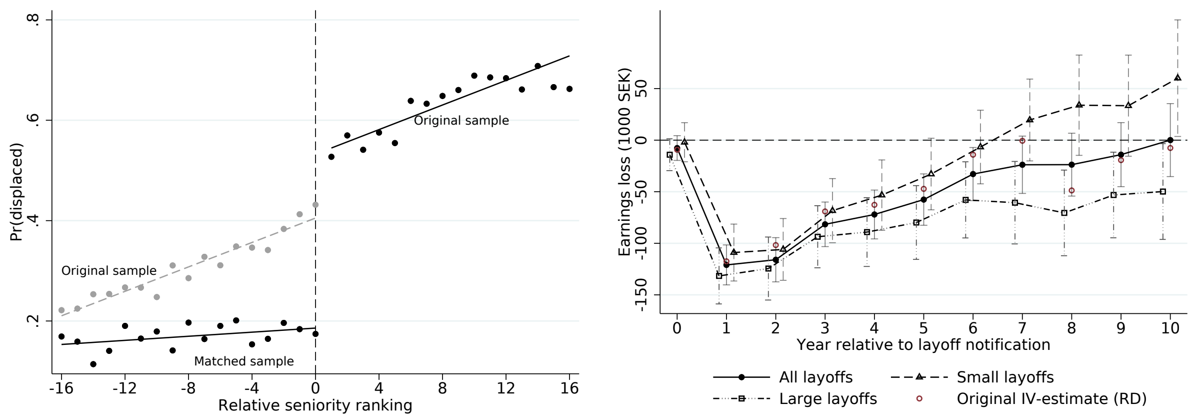
Figure 8: Reweighted earnings losses upon mass layoff



Notes: The figure shows the difference of annual earnings between displaced and non-displaced workers by years relative to a mass layoff event where earnings is normalized to zero at $t-3$. Displacement is defined as a worker being notified and leaving the plant in year $t+1$ or $t+2$ during a mass layoff event. The plotted estimates are δ_k from equation (9) along with 95 percent confidence intervals where standard errors are clustered at the individual level. The solid black line replicate the (unconditional) estimates of Figure 7. The dashed, blue, red and teal line reweights the original estimates to mimic LIFO-sample in various dimensions to account for compositional differences between the mass layoff and LIFO-sample. Standard errors are clustered at the individual worker level.

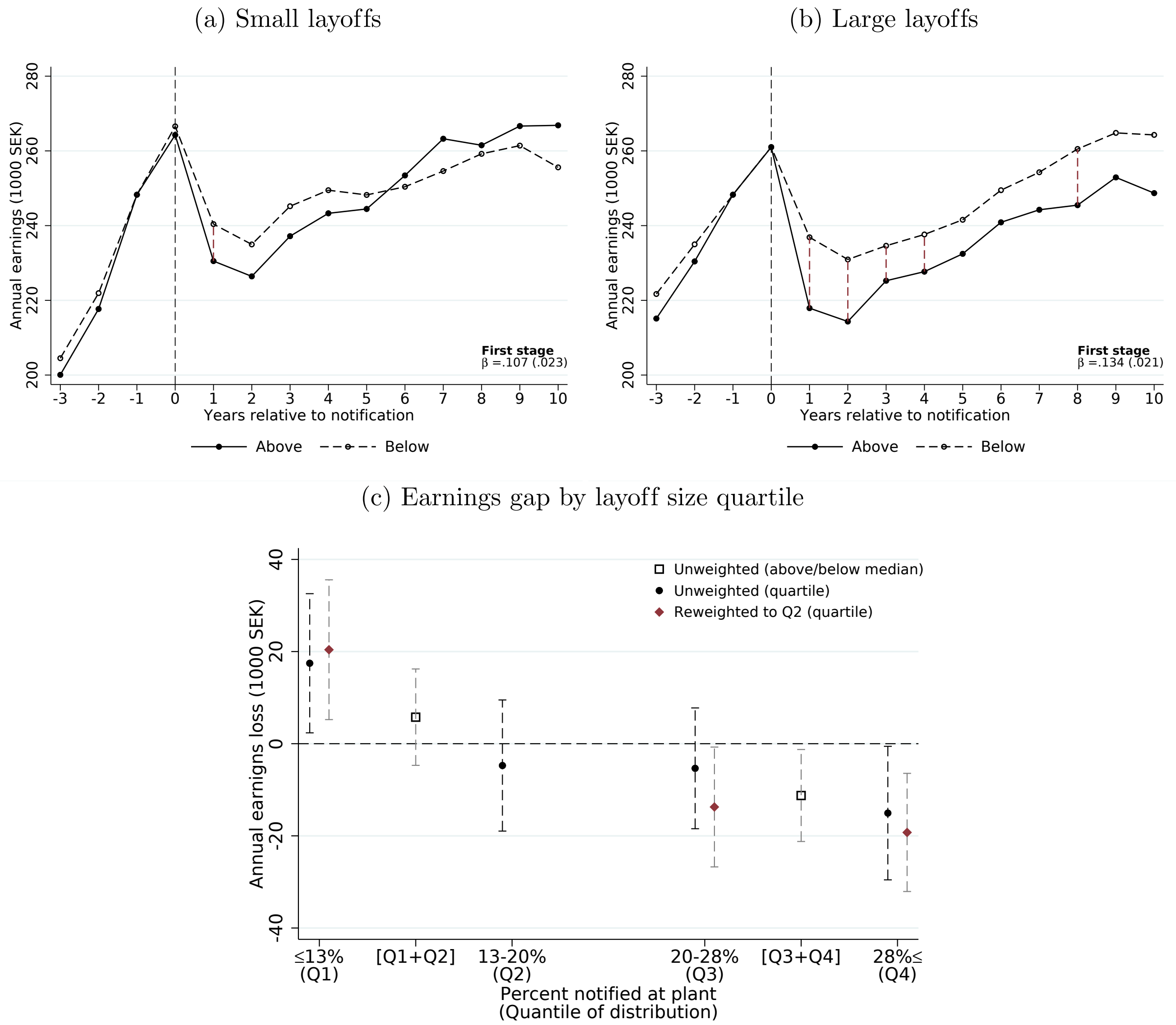
Figure 9: Matched worker sample

(a) First stage with matched sample for workers below threshold (b) IV-estimates of earnings losses by year relative to notification



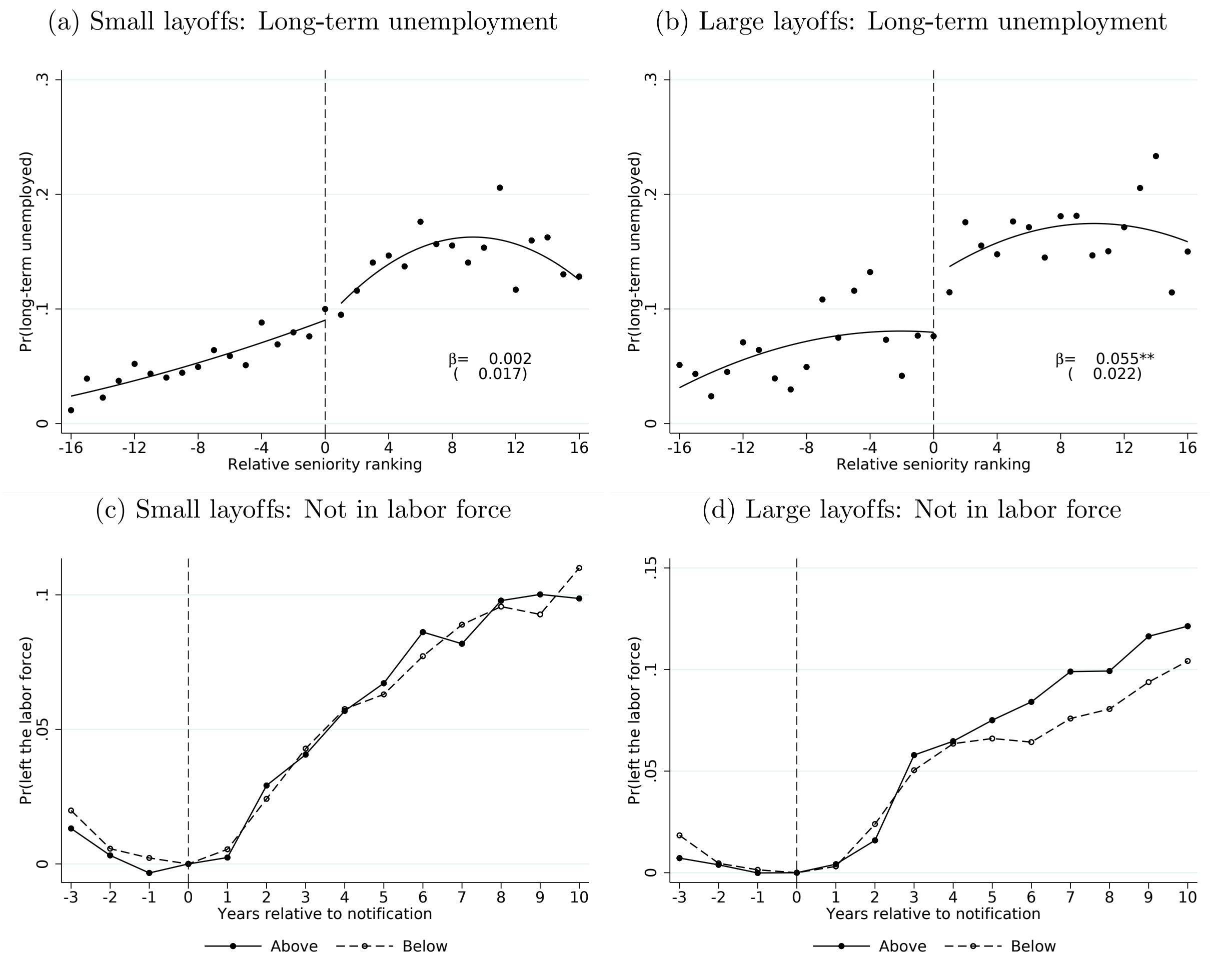
Notes: Panel a) of the Figure shows the probability of displacement as a function of workers relative ranking within an order circuit (in discrete bins), normalized to zero at the threshold. Workers below the threshold are matched on observables to mimic the actual workers (gray circles) below the threshold within each order circuit (see section 5.2 for a detailed description). The point estimate of the jump at the threshold is 0.34 with a standard error of 0.018 which corresponds to an F-statistic of 375. Panel b) shows IV-estimates, comparing earnings of displaced and non-displaced workers just above and below the threshold, respectively. The black solid line show the earnings gap using the matched control group whereas the red hollow circles show the original IV-estimates for comparison (see Table 4). The dashed lines show the same estimates but for small (hollow triangle) and large layoffs (hollow square) along with 95 percent confidence intervals. All regressions have a linear control function interacted with the threshold, using a bandwidth of ±16 and include baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s. Standard errors are clustered at the order circuit level.

Figure 10: Earnings losses by size of layoff



Notes: The Figure shows heterogeneity in the evolution of earnings after displacement by size of layoff for workers above and below the LIFO threshold. Figure a) and b) plots for each point in time the constant (hollow circles) and constant+γ (solid circles) estimate of equation (3) with annual earnings as the dependent variable which corresponds to the average predicted value of each outcome for workers to the left and right of the threshold, respectively. The dashed vertical line indicates significance at the 5-percent level. Figure c) shows estimated earnings differentials beyond seven years after notification along with 90 percent confidence intervals. The hollow squares are estimates for small and large layoff as represented in figure a) and b), respectively. The black dots are estimated earnings gap for quartiles of the layoff size distribution and the red dots are the equivalent estimates but reweighted to match the age, tenure, female, immigrant, level of education and notification year/quarter distribution as well as the industry composition of the 10-20% layoff sample. In each figure estimates are obtained by pooling order circuits into groups by size of layoff measured by the share of workers notified within an establishment and separately regressing annual earnings on displacement. All regressions use a bandwidth of ± 16 and include a linear polynomial function interacted with the threshold as well as order circuit fixed effects. Included in the regressions are controls for female, immigrant status, age, age squared level of education and earnings prior to notification. Standard errors are clustered at the level of the order circuit.

Figure 11: Unemployment and non-employment by size of layoff



Notes: Panel a) and b) show the probability of becoming long-term unemployed in the year after layoff for small and large layoffs, respectively. Panel c) and d) show the evolution of the probability of being not the labor force relative to time of notification for small and large layoffs, respectively. Not the labor force is defined as not working and while also not being registered at the PES. All regressions use a bandwidth of ± 16 and include a second order (top panel) or a first order (lower panel) polynomial function interacted with the threshold as well as order circuit fixed effects. Included in the regressions are controls for female, immigrant status, age, age squared level of education. Standard errors are clustered at the level of the order circuit.

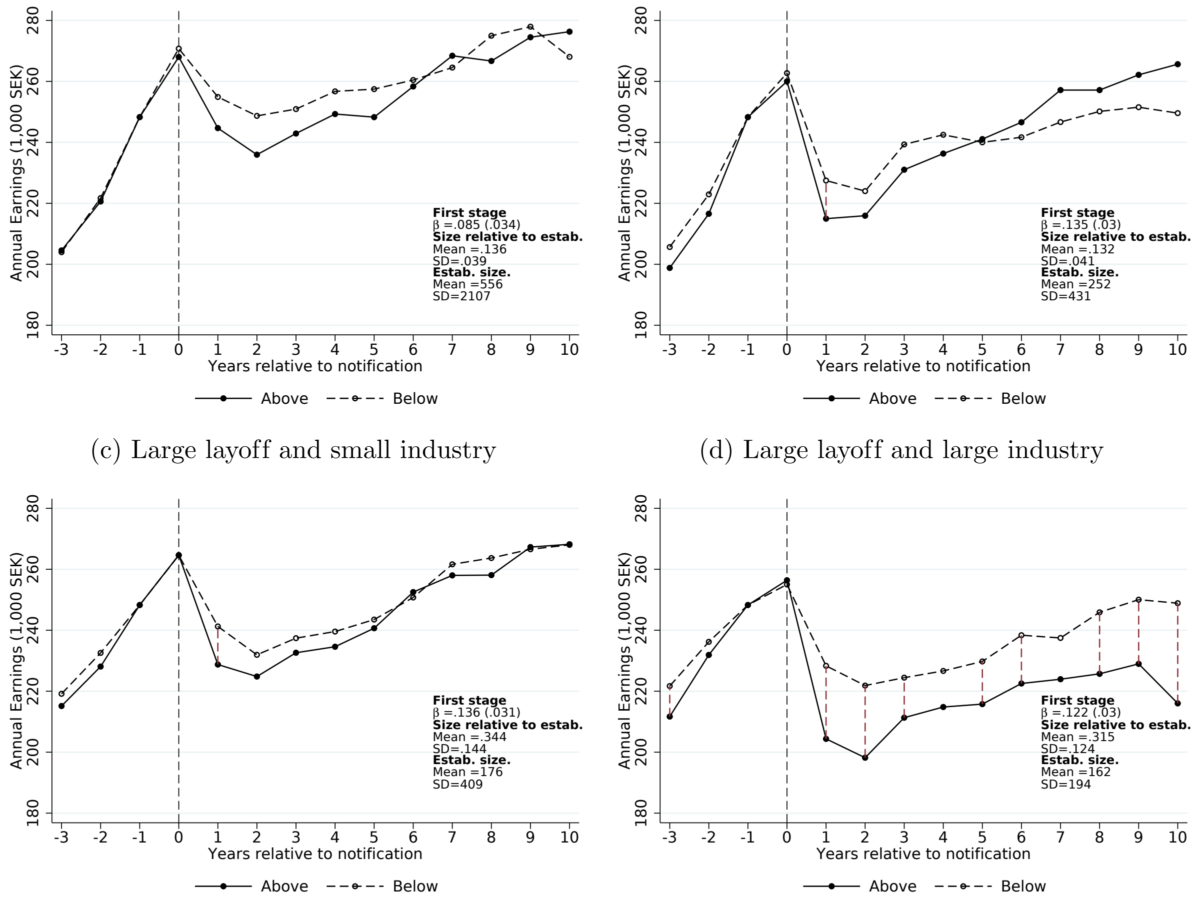
Figure 12: Earnings losses by size of layoff and size of industry relative to the local labor market

(a) Small layoff in small industry

(b) Small layoff and large industry

(c) Large layoff and small industry

(d) Large layoff and large industry



Notes: The figure shows annual earnings relative to the year of notification by size of layoff and size of the industry of the downsizing firm. The grouping of large and small are defined by the median in the sample for each category. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (3) with annual earnings as the dependent variable which corresponds to the average predicted value of each outcome for workers to the left and right of the threshold, respectively. The dashed vertical line indicates significance at the 5-percent level where standard errors are clustered at the order circuit level. All regressions use a bandwidth of ± 16 and include a linear control function interacted with the threshold and baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s, earnings prior to notification as well as order circuit FE:s.

Table 1: Sample characteristics

	All notified workers		LIFO sample		Mass layoff sample	
	Mean	SD	Mean	SD	Mean	SD
Age	40.66	12.74	38.35	12.06	41.99	8.12
Female	0.35	0.48	0.19	0.39	0.35	0.48
Immigrant	0.17	0.38	0.16	0.36	0.14	0.35
Tenure	5.89	5.70	6.45	5.83	12.79	6.69
Earnings$_{t-1}$ (1000 SEK)	266.46	137.50	247.58	95.13	277.65	120.16
Level of education						
Compulsary	0.44	0.50	0.51	0.50	0.52	0.50
Upper-secondary	0.43	0.50	0.45	0.50	0.38	0.48
College	0.12	0.33	0.04	0.20	0.10	0.30
Industry shares						
Manufacturing	0.33	0.47	0.83	0.38	0.61	0.49
Construction	0.08	0.27	0.14	0.35	0.01	0.11
Transport	0.11	0.31	0.00	0.06	0.09	0.29
Non-financial services	0.15	0.36	0.00	0.00	0.11	0.31
Retail	0.10	0.30	0.00	0.00	0.08	0.28
Other	0.22	0.42	0.03	0.16	0.08	0.28
Firm size	415.34	1724.28	472.98	1407.74	877.951	3261.11
Establishment size	98.63	385.37	169.38	304.79	158.45	163.95
N	425,939		15,795		30,112	

Notes: The table shows summary statistics for workers notified of their displacement between 2005-2015. The first column includes all workers notified in layoffs where more than 5 workers are involved and hence reported to the PES. The second column shows sample characteristics for workers used in the main analysis of this paper. The third column shows worker characteristics when following the standard restrictions imposed in the literature using mass layoffs (for details see section 5.1). All characteristics are computed at the individual level, expect firm and establishment size which reflects the average size per layoff.

Table 2: Balancing of baseline covariates

	(1)	(2)	(3)	(4)	(5)
Earnings$_{t-1}$	-0.0046	-0.0046	0.0004	-0.0041	0.0030
	(0.0062)	(0.0082)	(0.0049)	(0.0102)	(0.0137)
Female	-0.0060	-0.0062	-0.0002	-0.0092	-0.0003
	(0.0051)	(0.0069)	(0.0039)	(0.0115)	(0.0149)
Immigrant	0.0034	0.0015	0.0008	0.0029	-0049
	(0.0053)	(0.0067)	(0.0045)	(0.0104)	(0.0156)
Age	0.0001	0.0000	0.0001	0.0534	0.4268
	(0.0002)	(0.0003)	(0.0002)	(0.0368)	(0.5039)
Compulsory school	ref.	ref.	ref.	0.0025	0.0072
				(0.0149)	(0.0228)
Upper-Secondary school	-0.0002	-0.0010	0.0001	-0.0055	-0.0082
	(0.0047)	(0.0051)	(0.0036)	(0.0151)	(0.0228)
College	0.0084	0.0059	0.0010	0.0029	0.0010
	(0.0108)	(0.0139)	(0.0089)	(0.0059)	(0.0084)
Order of polynomial					
1st degree	✓	✓		✓	
2nd degree			✓		✓
Circuit FE		✓	✓	✓	✓
F-statistic	0.397	0.218	0.126	.	.
p-value	0.881	0.971	0.993	.	.
R^2	0.736	0.730	0.880	.	.
# clusters	564	564	564	564	564
N	15,795	15,795	15,795	15,795	15,795

Notes: The table show balance tests of baseline covariates at the LIFO threshold. Columns (1)-(3) show results from regressing the instrument (being above the threshold) on a set of baseline covariates and a polynomial control function in relative ranking interacted with the threshold. Annual earnings in measured in the year prior to notification and is normalized to reflect percentage point deviations from the mean of the workers below the threshold. The bottom of the table displays the F-statistic and the corresponding p-value from testing the hypothesis that all coefficients being jointly equal to zero. Columns (4)-(5) report results from balancing tests where each covariate has been regressed separately on the instrument and a polynomial control function in relative ranking interacted with the threshold. All regressions use a bandwidth ± 16. Standard errors clustered at the level of the order circuit and shown in parentheses.

Table 3: First stage estimates

	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbf{1}[RR > 0]$	0.120***	0.119***	0.092***	0.137***	0.068***	0.081***
	(0.015)	(0.015)	(0.017)	(0.015)	(0.019)	(0.018)
Polynomial order						
1st degree	✓	✓	✓	✓		
2nd degree					✓	✓
Covarites		✓	✓	✓	✓	✓
Circuit FE	✓	✓	✓	✓	✓	✓
Bandwidth ±	16	16	11	20	18	22
F-statistic	61.08	61.98	30.40	88.42	12.20	19.45
p-value	0.000	0.000	0.000	0.000	0.001	0.000
R^2	0.381	0.391	0.376	0.401	0.395	0.404
# clusters	564	564	564	564	564	564
N	15,795	15,795	12,197	17,954	16,925	18,894

Notes: The table shows the estimated first stage coefficient γ of equation (3) which is the difference in probability of displacement for worker just below and above the LIFO threshold. The bottom of the table displays the first stage F-statistic and its corresponding p-value used to evaluate instrument relevance. All regressions, expect column (4), use the optimal bandwidth selector suggested by Calonico et al. (2014) where column (1), (2) and (5) are based off the dependent variable annual earnings whereas column (3) and (6) use the probability of leaving the notifying firm. All regressions include order circuit FE:s and a control function interacted with the threshold where the polynomial order is indicated at the bottom of the table. Where indicated regressions control for the baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, $p < 0.01$ level.

Table 4: IV-estimates on earnings and non-employment

	Annual Earnings (1,000 SEK)									
Panel a)	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$	$t+7$	$t+8$	$t+9$	$t+10$
Displaced	-122.56***	-106.85***	-74.71***	-71.15**	-58.91*	-25.81	-3.63	-54.03	-25.47	-7.49
	(26.00)	(28.30)	(28.82)	(31.18)	(32.56)	(33.13)	(35.20)	(39.69)	(44.88)	(54.13)
Control mean	294	281	274	276	272	263	257	285	275	263
% of Control	-41.73	-38.06	-27.28	-25.80	-21.64	-9.83	-1.41	-18.95	-9.25	-2.85
First stage F-statistic	62	62	62	60	56	57	48	41	37	26
N	15,795	15,795	15,795	15,604	14,987	14,313	12,612	11,442	11,007	7,674
	Pr(Non-employment)									
Panel b)	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$	$t+7$	$t+8$	$t+9$	$t+10$
Displaced	0.48***	0.30***	0.09	0.07	0.07	-0.06	-0.02	0.11	0.18*	0.12
	(0.09)	(0.10)	(0.09)	(0.09)	(0.10)	(0.09)	(0.10)	(0.10)	(0.11)	(0.13)
Control mean	0.02	0.07	0.13	0.14	0.14	0.19	0.18	0.11	0.08	0.11
% of Control	1951.17	405.13	70.24	53.97	45.75	-30.16	-11.46	101.80	229.88	107.87
First stage F-statistic	62	62	62	60	56	57	48	41	37	26
N	15,795	15,795	15,795	15,604	14,987	14,313	12,612	11,442	11,007	7,674

Notes: The table shows IV estimates on annual earnings (top panel) and non-employment (bottom panel) by year relative to notification. Earnings are in thousands of Swedish krona in 2005 values and employment is defined being registered as non-employed at least one month during a year. Displacement has been instrumented with being just above the threshold. All regressions use a bandwidth of ±16 and include a linear control function interacted with the threshold and baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s. The bottom of the table show the first stage F-statistic. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, $p < 0.01$ level.

Table 5: Cumulated average earnings losses after 6 years by margins of adjustment

		Adjustment margins		
	Average total earnings loss	Months worked	log(Monthly wage)	Monthly hours worked
	(1)	(2)	(3)	(4)
Displaced	-76.84***	-1.21*	-0.04	-3.59
	(24.74)	(0.62)	(0.06)	(12.13)
Control mean	274.8	11.1	3.2	134.3
% of Control	-0.28	-0.11	.	-0.03
F-statistic	57	57	32	32
# clusters	510	510	497	497
N	14,313	14,313	9,831	9,821

Notes: The table shows IV estimates on worker outcomes cumulated and averaged over 3 years post notification. Earnings and wages are in thousands of Swedish krona in 2005 values. Displacement has been instrumented with being just above the threshold. The bottom of the table show the first stage F-statistic. All regressions use a bandwidth of ±16 and include a linear control function interacted with the threshold and baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level, *** $p < 0.01$ level.

Table 6: Labor market outcomes by size of layoff

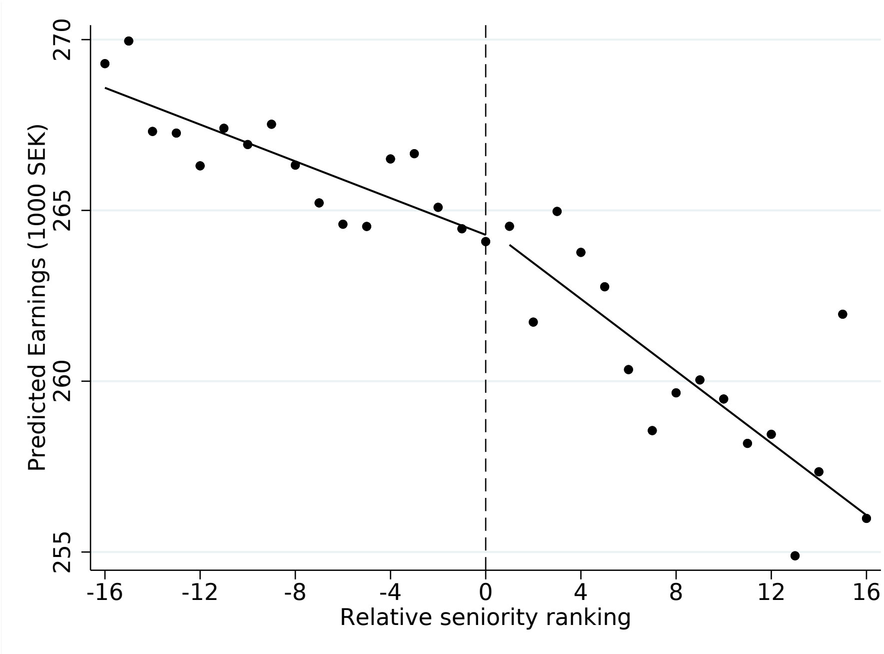
	Short-run (1-3 years)		Medium-run (4-6 years)		Long-run (7-10 years)	
	Small (1)	Large (2)	Small (3)	Large (4)	Small (5)	Large (6)
Earnings (1,000 SEK)	-84.464**	-76.782**	-24.195	-38.683	66.757	-69.301*
	(33.415)	(35.113)	(42.184)	(36.154)	(75.548)	(41.882)
	[0.874]		[0.794]		[0.115]	
Non-employment (months)	2.344**	2.402***	0.716	1.871*	-0.345	2.493**
	(1.013)	(0.899)	(1.211)	(1.001)	(1.803)	(1.107)
	[0.966]		[0.462]		[0.180]	
Pr(unemployed)	0.396***	0.566***	-0.089	0.139	-0.273	0.172**
	(0.118)	(0.124)	(0.126)	(0.110)	(0.179)	(0.080)
	[0.321]		[0.172]		[0.024]	
log(wage)	0.023	-0.002	0.052	0.075	0.119	0.070
	(0.083)	(0.073)	(0.095)	(0.087)	(0.124)	(0.096)
	[0.816]		[0.857]		[0.756]	
Pr(change industry)	0.499***	0.590***	0.601***	0.405***	0.423**	0.263*
	(0.126)	(0.116)	(0.192)	(0.131)	(0.202)	(0.135)
	[0.593]		[0.400]		[0.511]	
Pr(change occupation)	0.436***	0.481***	0.268	0.297		
	(0.148)	(0.101)	(0.351)	(0.237)		
	[0.804]		[0.946]			
Pr(live and work in same region)	0.115	-0.156	0.262*	-0.217**	0.217	-0.238**
	(0.116)	(0.097)	(0.143)	(0.104)	(0.205)	(0.118)
	[0.075]		[0.007]		[0.055]	
Pr(change work region)	0.142	0.153*	0.029	0.263**	0.077	0.277**
	(0.107)	(0.085)	(0.155)	(0.108)	(0.209)	(0.118)
	[0.939]		[0.214]		[0.405]	
log(firm size)	-0.429	0.183	-0.183	-0.102	0.276	0.326
	(0.543)	(0.414)	(0.775)	(0.671)	(1.061)	(0.739)
	[0.370]		[0.937]		[0.969]	

Notes: The table shows IV-estimates on post employment outcomes, seperate for small and large layoffs by 3 time periods relative to layoff notification. Earnings are in thousands of Swedish krona in 2005 values. The probability of changing occupation can only be observred at maximum 6 years post notification due to occupational codes being recoded in 2012. All regressions use a bandwidth of ±16 and include a linear control function interacted with the threshold and baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s. Standard errors clustered at the level of the order circuit and shown in parentheses. Below each set of estimates in hard brackets are p-values from testing equality between the coefficents on small and large layoffs. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Appendix

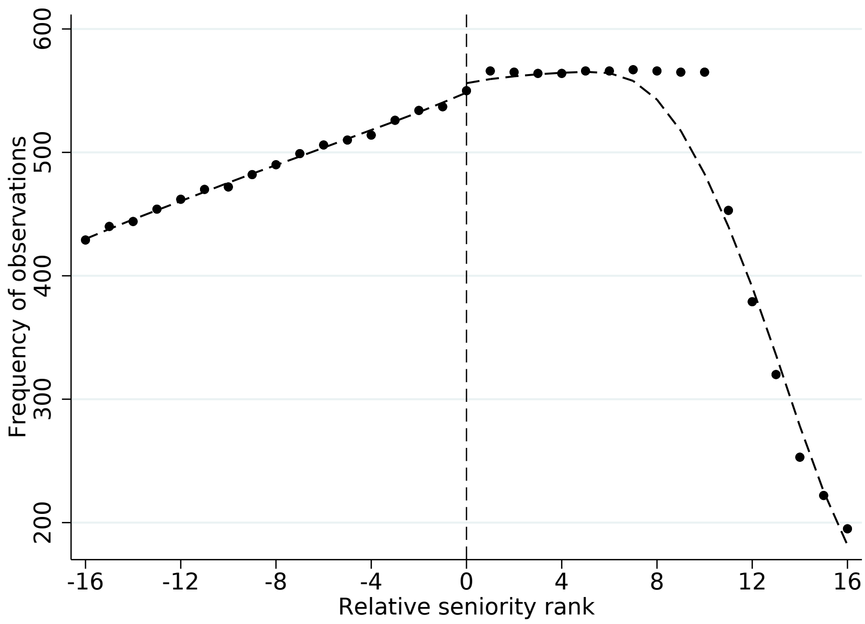
A Additional figures and tables

Figure A.1: Selection on observables



Notes: The figure shows predicted annual earnings as a function of a workers relative ranking within an order circuit (in discrete bins), normalized to zero at the threshold. The dependent variable is generated by taking the fitted values from a regression of annual earnings on age, age squared and dummies for gender, immigrant status, level of education FE's and year FE's. The regression comes from estimating equation (3) with a linear control function interacted with the threshold including order circuit FE:s, using a bandwidth of ±16. The point estimate of the jump at the threshold is 0.064 with a standard error of 0.921. Standard errors are clustered at the order circuit level.

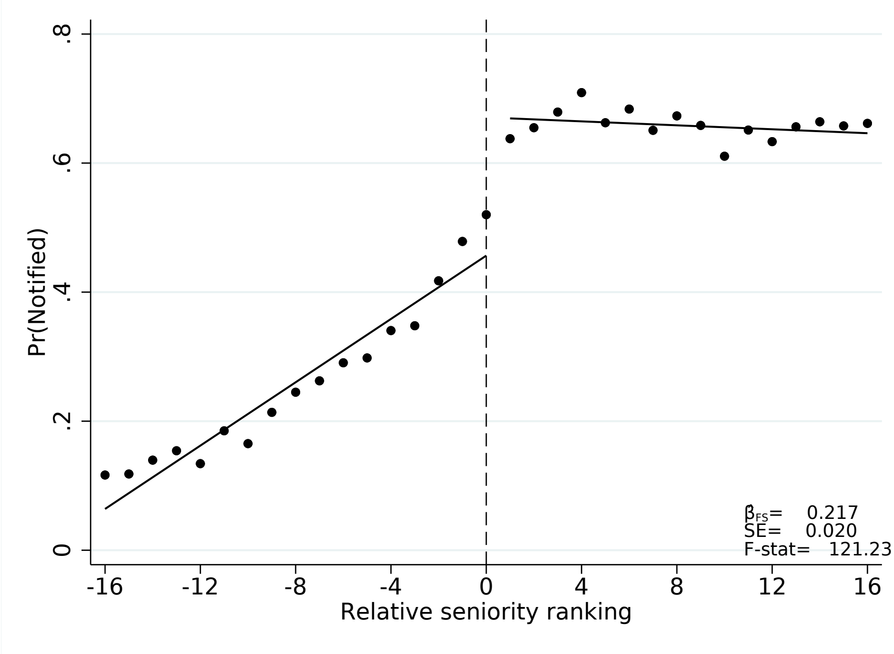
Figure A.2: Frequency of observations around the threshold



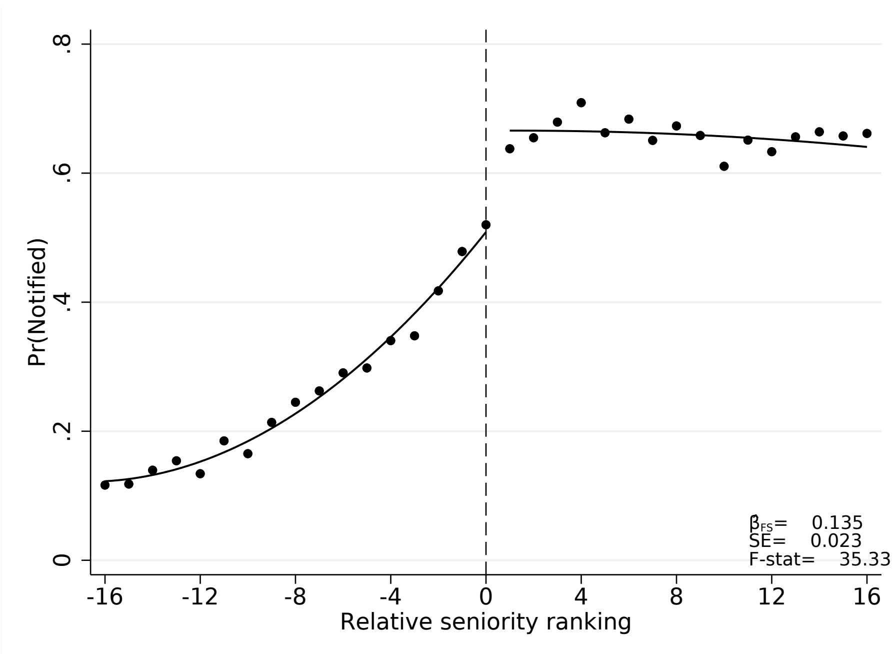
Notes: The figure show the frequency of observations around the LIFO threshold.

Figure A.3: Probability of individual layoff notification

(a) Linear fit



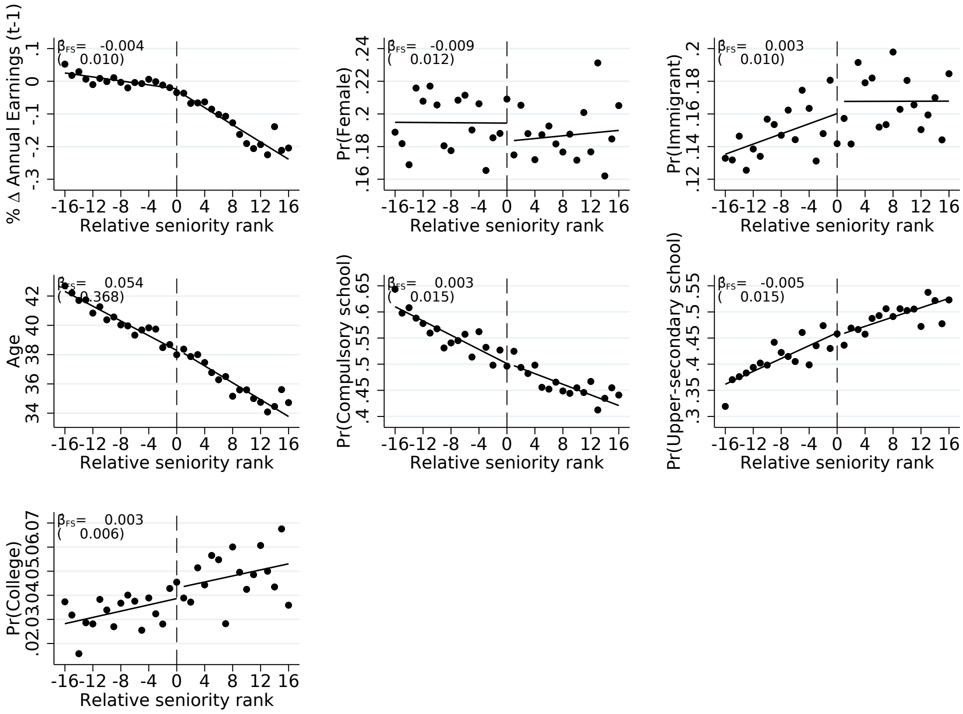
(b) Quadratic fit



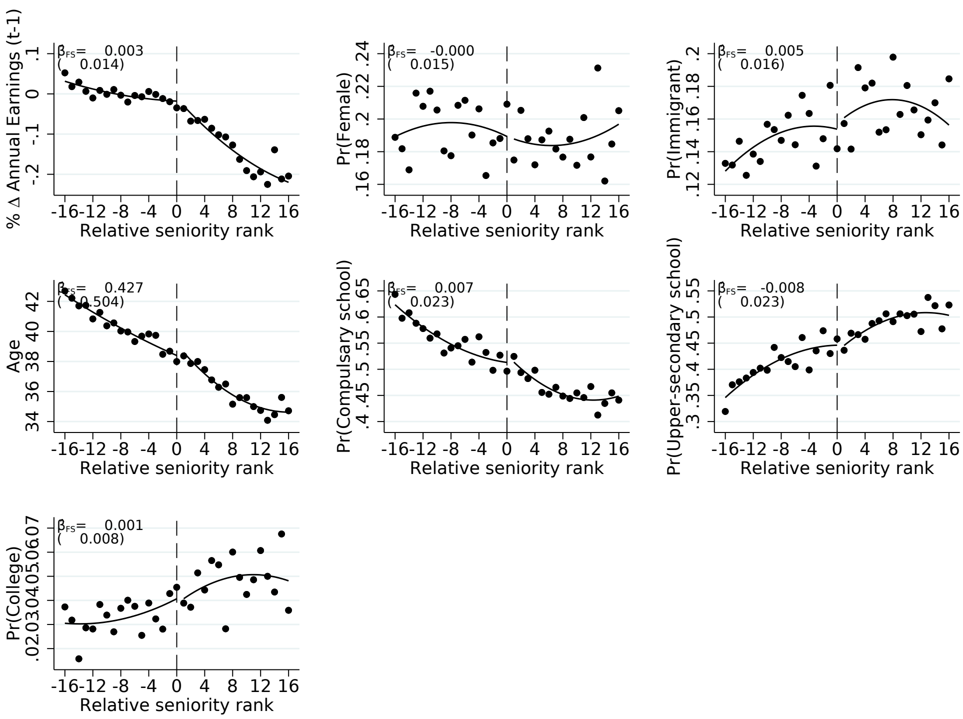
Notes: The figures show the probability of receiving a layoff notification as a function of a workers relative ranking within an order circuit (in discrete bins), normalized to zero at the threshold. The regression include in a) a first order and b) second order polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ± 16. The point estimate of the jump at the threshold are supplied in the graph along with standard errors clustered at the order circuit level and F-statistic.

Figure A.4: Balancing of individual covariates

(a) Linear fit



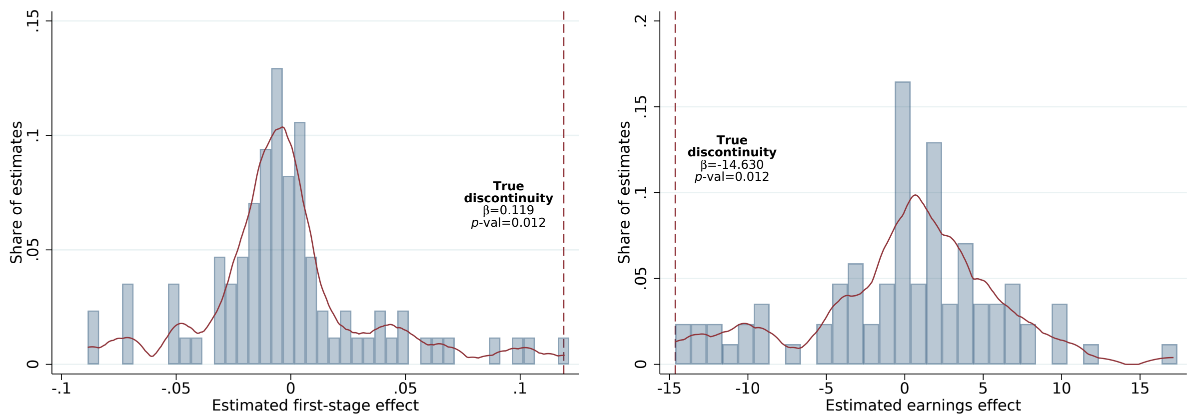
(b) Quadratic fit



Notes: The figures show balancing at the threshold of pre-determined worker characteristics using a a) first order and b) second order polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ±16. The point estimate of the jump at the threshold are supplied in the graph along with standard errors clustered at the order circuit level.

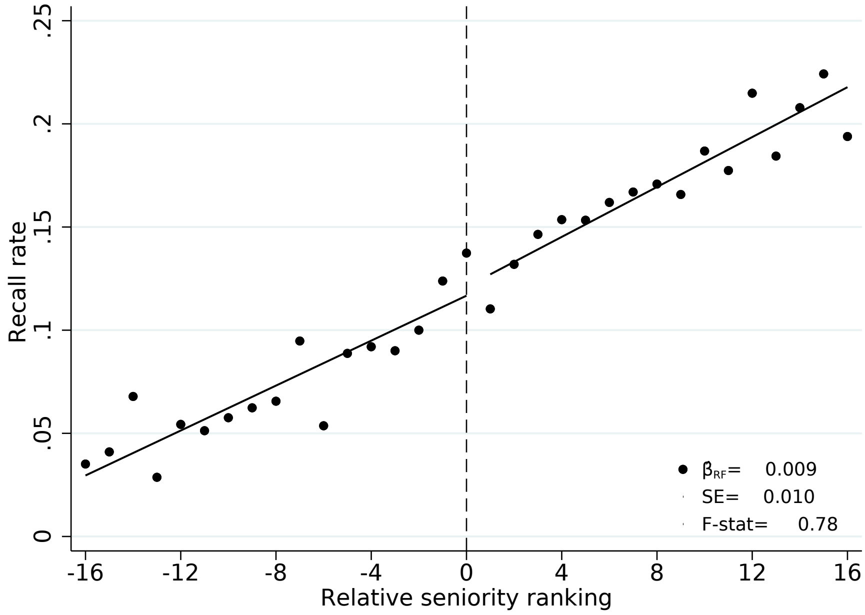
Figure A.5: Permutation test

(a) First stage (b) Annual earnings in $t+1$



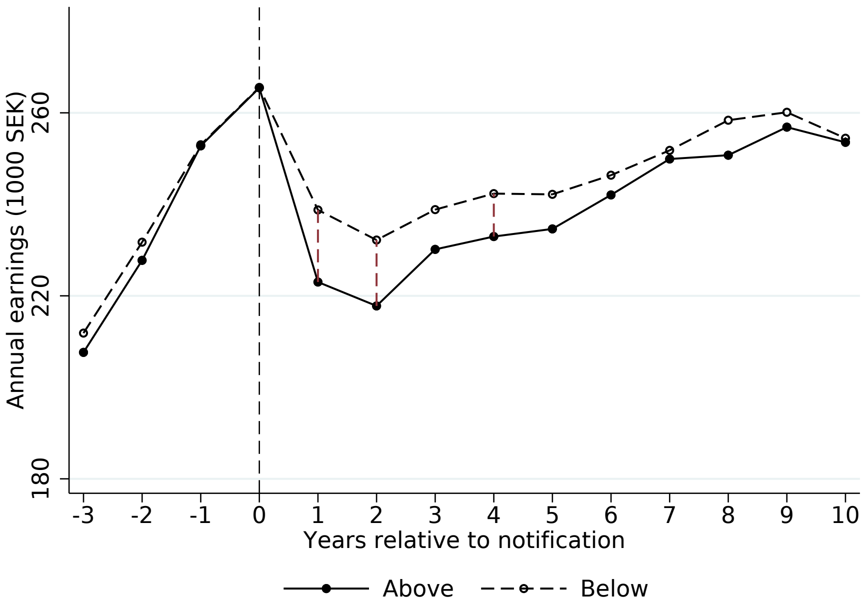
Notes: The figure shows the distribution of estimated effect for the a) first-stage and b) annual earnings in $t+1$ for 85 different permutations of the RD-regression. For each permutation I vary the LIFO cut-off to values of the running variable between -56 and +28. Each regression, comes from estimating equation (3) with annual earnings as the dependent variable, using a bandwidth of ± 16 (around the placebo threshold) and includes a linear control function interacted with the threshold. The regressions also includes the baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s. The true estimate is indicated by the vertical red dashed line and displayed in the figure along with the p-value showing the share of placebo estimates greater or equal to the true estimates (minimum possible p-value is 1/85=0.012). The solid red line is an Epanechnikov kernel showing the distribution among the estimates.

Figure A.6: Probability of recall



Notes: The figure shows the probability of recall as a function of a workers relative ranking within an order circuit (in discrete bins), normalized to zero at the threshold. Recalled is defined as being registered as not working at the notifying firm for two consecutive months after layoff notification and after that coming back working at least three consecutive months at the firm. The regression include a first order polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ± 16. The point estimate of the jump at the threshold are supplied in the graph along with standard errors clustered at the order circuit level and F-statistic.

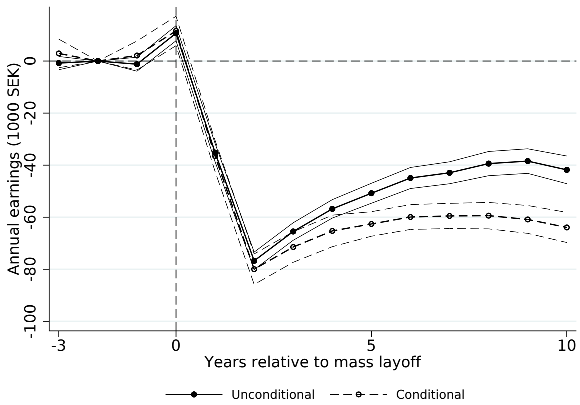
Figure A.7: Evolution of annual earnings relative to year of notification for single layoff establishments



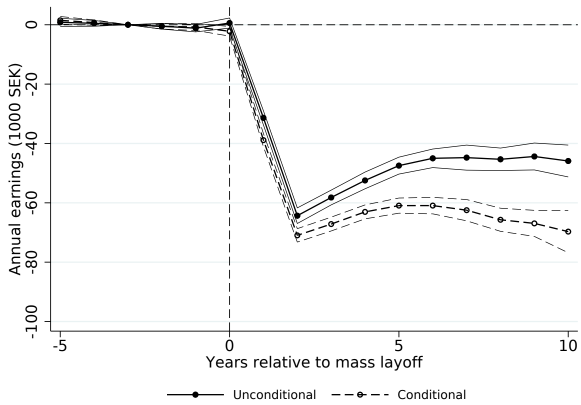
Notes: The figure shows annual earnings relative to the year of notification for establishments with only one layoff notification during the sample period. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (3) with annual earnings as the dependent variable which corresponds to the average predicted value of each outcome for workers to the left and right of the threshold, respectively. The dashed vertical line indicates significance at the 5-percent level where standard errors are clustered at the order circuit level. The regressions use a bandwidth of ± 16 and include a linear polynomial function interacted with the threshold as well as order circuit fixed effects. Regressions control for the baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s.

Figure A.8: Robustness of mass layoff earnings estimates

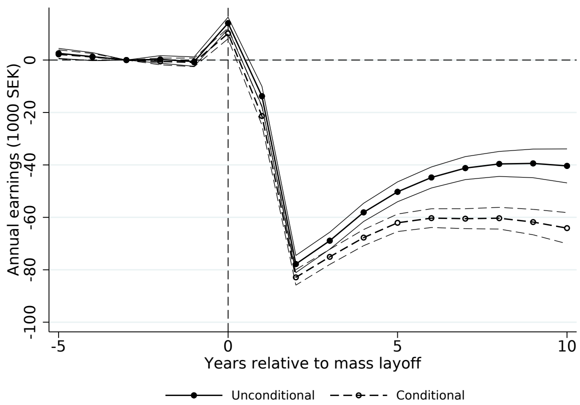
(a) Workers with > 3 years of tenure



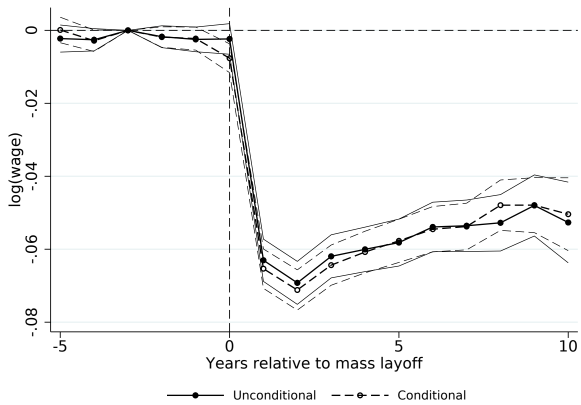
(b) Including non-notified workers



(c) Excluding plant closures

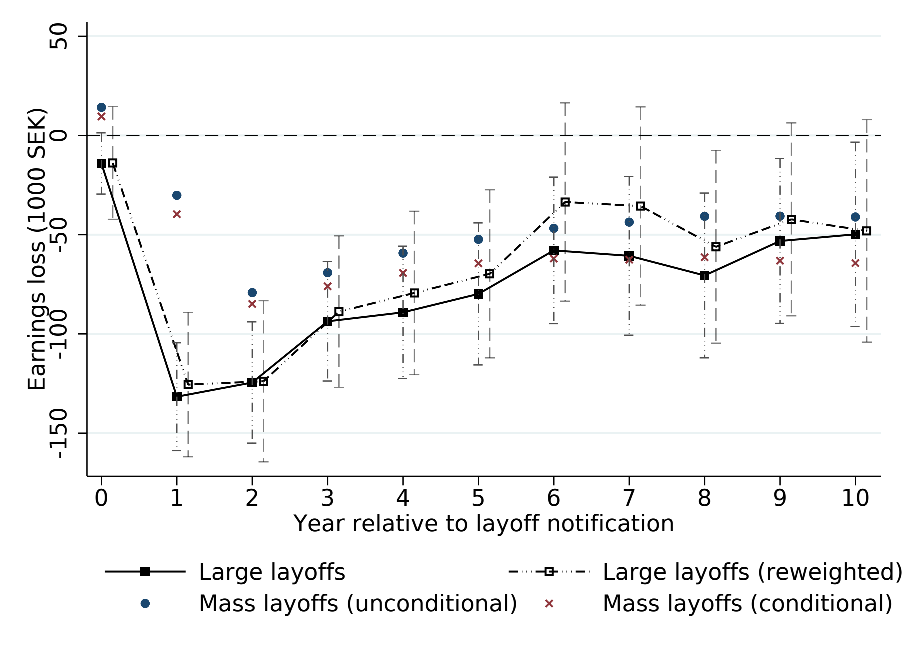


(d) Wage losses



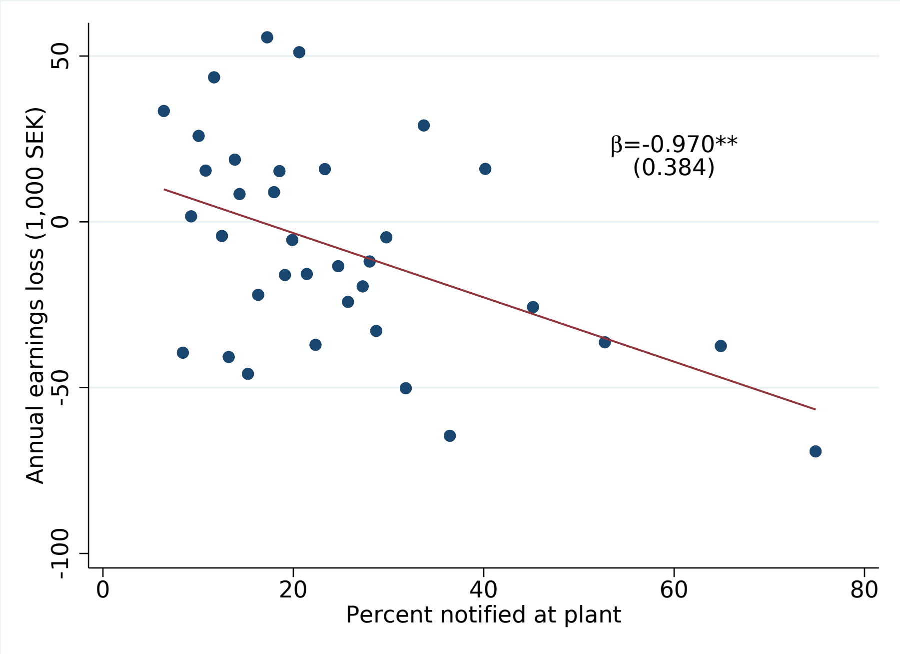
Notes: The figure shows the difference of annual earnings between displaced and non-displaced workers by years relative to a mass layoff event normalized to zero at $t-3$. Panel a) alter the tenure restriction and focuses on workers with at least 3 years of tenure. In panel b) displacement is defined as a worker leaving the plant in year $t+1$ or $t+2$ during a mass layoff event and thus include also non-notified workers. Panel c) exclude plant closures whereas panel d) plots the differences in wages between displaced and non-displaced workers. All plotted estimates are δ_k from equation (9) along with 95 percent confidence intervals where standard errors are clustered at the individual level. The solid black line show losses of displaced workers when not conditioning on the control group being employed in $t > 0$. The dashed black line conditions on the control group being employed throughout the entire sample period.

Figure A.9: Earnings losses in using matched sample (reweighted)



Notes: The Figure shows IV-estimates, comparing earnings of displaced and non-displaced workers just above and below the threshold, respectively. The black solid line shows the earnings gap using the matched control group replicating the estimates for large layoffs, replicating the estimates of Figure 9. The dashed line reweights the estimates for large layoffs to mimic the worker and industry composition of small layoffs as well as the time of notice. All regressions have a linear control function interacted with the threshold, using a bandwidth of ±16 and include baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s. Standard errors are clustered at the order circuit level. The blue (red) dots (cross) plots for comparison the unconditional (conditional) estimates from Figure 7 showing earnings losses using using the standard mass layoff approach (see section 5.1 for details on estimation).

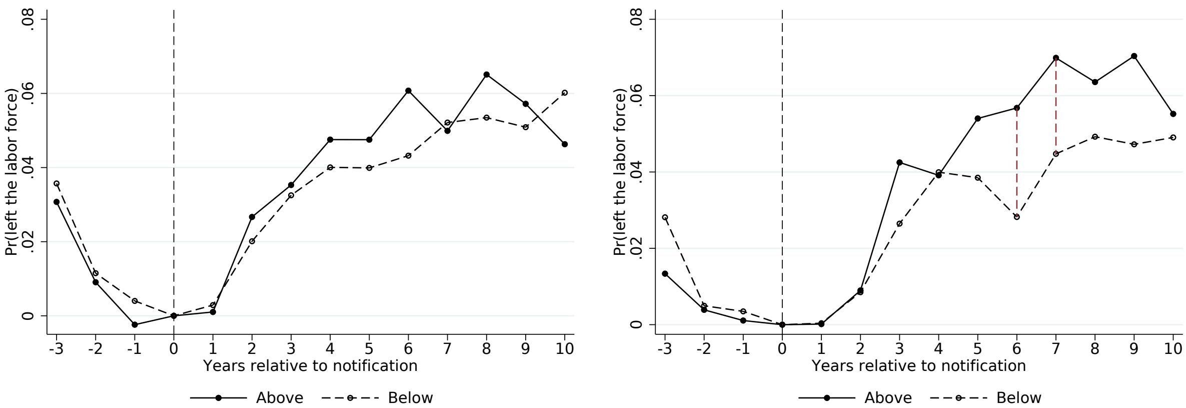
Figure A.10: Earnings losses by size of layoff



Notes: The Figure shows differences in earnings between workers just above and below the threshold within an order circuit, seven to ten years after notification. The Figure is a bin-scatter plot where separate estimates from each order circuit has been pooled into 35 equally sized bins and plotted against the size of the layoff. Each earnings differential estimate is estimated using a bandwidth of ±16 and including a linear polynomial function interacted with the threshold as well as order circuit fixed effects. Included in the regressions are also controls for female, immigrant status, age, age squared level of education and earnings prior to notification. In estimate reported in the figure comes from a regression of the estimated earnings differential and size of layoff at the order circuit level using robust standard errors.

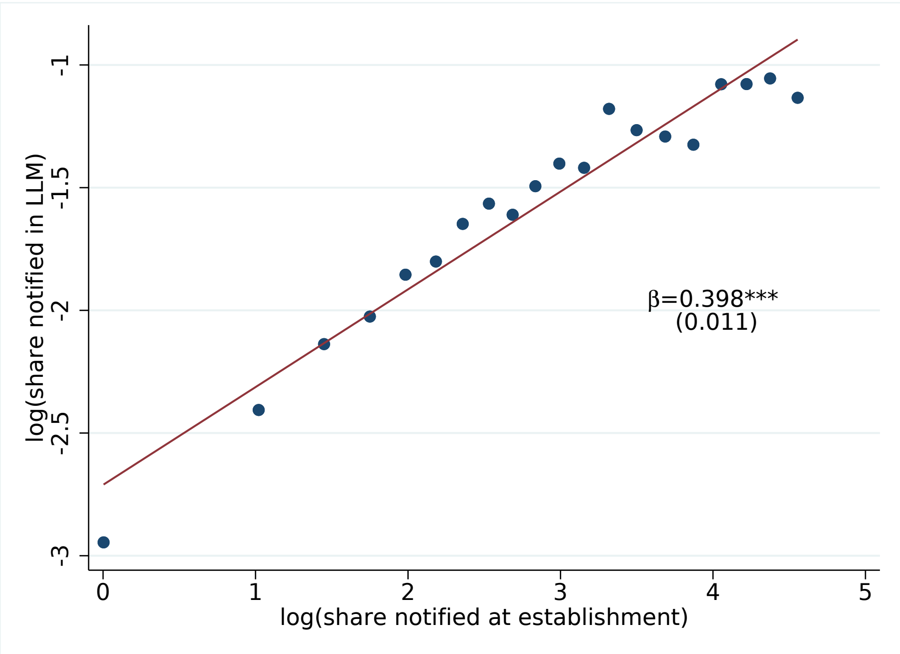
Figure A.11: Leaving labor force by size of layoff for workers aged 18–55

(a) Small layoffs (b) Large layoffs



Notes: The Figure shows the probability of being not the labor force for workers aged 18-55 at the time of layoff. This is done separately for a) small and b) large layoffs. Not the labor force is defined as not working and while also not being registered at the PES. All regressions use a bandwidth of ± 16 and include a second order (top panel) or a first order (lower panel) polynomial function interacted with the threshold as well as order circuit fixed effects. Included in the regressions are controls for female, immigrant status, age, age squared level of education. Standard errors are clustered at the level of the order circuit.

Figure A.12: Correlation between size of layoff relative to establishment and local labor market



Notes: The figure plots the log of share of notified in a local labor market against the log of share notified at the establishment in 20 equally sized bins. A local labor market is defined by municipality and 1-digit industry.

Table A.1: Overview of studies of effects of job displacement on earnings

		Data		Sample restrictions (main analysis)			Percent earnings losses	
Author(s) (year)	Journal	Country (state)	Time	Tenure (years)	Gender	Share laid off	Short-run (years)	Long-run (years)
Jacobson et al. (1993)	AER	United States (Pennsylvania)	1975–1985	≥ 6	Both	$\geq 30\%$	40 (1)	25 (6)
Stevens (1997)	JOLE	United States	1968–1988	None	89% male	$> 0\%$	25 (1)	6-9 (10+)
Eliason and Storrie (2006)[a)]	JOLE	Sweden	1983–1999	None	Both	100%	11.4 (0)	9.4 (12)
Sullivan and von Wachter (2009)	QJE	United States (Pennsylvania)	1974–1991	≥ 6	Males only	$\geq 30\%$	40–50 (1)	15–20 (9)
von Wachter et al. (2009)	IZA WP	United States	1974–2004	≥ 6	Male focus	$\geq 30\%$	30 (1)	20 (15–20)
Couch and Placzek (2010)	AER	United States (Connecticut)	2002–2014	≥ 6	Both	$\geq 30\%$	32–33 (1)	12–15 (6)
Davis and von Wachter (2011)	Brookings papers	United States	1974–2008	≥ 3	Males only	$\geq 30\%$	25–39 (1)	15–20 (10-20)
Seim (2019)	Labour Economics	Sweden	1999–2009	≥ 1.5	Males only	$\geq 80\%$	23.5 (1)	16.4 (7)
Burdett et al. (2020)	ReStad	Germany	2002–2014	≥ 3	Males only	$\geq 30\%$	40 (0)	~ 8 (15)
Lachowska et al. (2020)	AER	United States Washington	1981–2005	≥ 6	Both	$\geq 30\%$	48 (.25)	16 (5)
Schmieder et al. (2023)	Mimeo	Germany	1980–2008	≥ 3	Males only	$\geq 30\%$	30 (0)	15 (10)
Flaaen et al. (2019)[b)]	AEJ Macro	United States	2000–2006	≥ 1	Both	$\geq 30\%$	. (·)	. (·)
Fackler et al. (2021)	JEEA	Germany	2007–2009	≥ 3	Both	100%	26 (1)	9 (5)
Athey et al. (2023)	ArXiv (WP)	Sweden	1997–2014	≥ 3	Both	$\geq 90\%$	24 (1)	8 (10)

Notes: The table summarizes results and sample restrictions made in some of the most cited studies using of earnings losses upon job displacement. All papers listed above, expect Eliason and Storrie (2006) and Athey et al. (2023), include the additional sample restrictions of establishments/firms having no less than 50 employees at the year of mass layoff.

[a)] The paper only reports nominal losses in Swedish krona so estimates are calculated based upon summary statistics provided in the paper.

[b)] No exact point estimates are provided in the paper but merely graphical evidence. This due to confidentialty reasons according to the authors.

Table A.2: Characterizing compliers in LIFO sample

	Whole sample	Complier	Never-taker	Always-taker
Earnings (1,000 SEK)	247.584	246.317	253.457	241.945
	(0.729)	(2.337)	(1.878)	(1.803)
Female	0.191	0.190	0.172	0.213
	(0.003)	(0.009)	(0.007)	(0.008)
Immigrant	0.158	0.156	0.154	0.164
	(0.003)	(0.009)	(0.007)	(0.007)
Age (years)	38.347	38.772	38.175	38.102
	(0.098)	(0.293)	(0.231)	(0.254)
Compulsory school	0.511	0.543	0.482	0.513
	(0.004)	(0.012)	(0.010)	(0.010)
Upper secondary school	0.448	0.430	0.462	0.452
	(0.004)	(0.013)	(0.010)	(0.010)
College	0.040	0.027	0.056	0.035
	(0.001)	(0.005)	(0.004)	(0.003)

Notes: The table shows summary statistics by complier status following Abadie et al. (2002) and using the user-written package `ivdesc` in Stata. Bootstraped standrad errors shown in parentheses and based on 1000 replications.

Table A.3: Characteristics of displaced vs. non-displaced workers in mass layoff sample

	(1) Displaced workers	(2) Non-displaced workers	(3) Difference col. (1)-(2)
Worker characteristics			
Age	42.47	42.48	-0.02
Tenure	10.94	11.10	-0.16
Primary school	0.57	0.59	-0.02
High school	0.35	0.34	0.01
College	0.08	0.07	0.00
Earnings (t-1)	305.46	296.59	8.86
Earnings (t-2)	297.36	293.10	4.26
Earnings (t-3)	291.72	288.05	3.67
Industry shares			
Agricultural	0.00	0.00	0.00
Mining	0.00	0.00	0.00
Manufacturing	0.61	0.61	0.00
Construction	0.06	0.06	-0.00
Retail	0.10	0.10	-0.00
Transport	0.09	0.10	-0.01
Financial	0.00	0.00	0.00
Non-financial	0.06	0.06	-0.00
N	11,440	11,440	22,880

Notes: The table show in column (1) and (2) average characteristics of displaced and non-displaced workers, respectively, used in the mass layoff analysis in section 5.1.1. Column (3) show differences in means for the two groups and column (4) the p-value for a test of equality of means. For details on how the two groups are created and matched see section 5.1.

Table A.4: Sample characteristics by size of layoff

	LIFO sample		Mass layoff sample		Mass layoff sample (reweighted)	
	Mean	SD	Mean	SD	Mean	SD
Age	38.35	12.06	42.04	8.09	40.57	10.10
Female	0.19	0.39	0.35	0.48	0.24	0.43
Tenure	6.00	5.83	12.80	6.70	8.84	5.40
Immigrant	0.16	0.36	0.15	0.35	0.17	0.38
Level of education						
Compulsary school	0.51	0.50	0.52	0.50	0.58	0.49
Upper-secondary school	0.45	0.50	0.38	0.48	0.39	0.49
College	0.04	0.20	0.10	0.30	0.04	0.19
log(establishment size)	4.84	0.94	4.96	0.71	4.88	0.72
Notification year	2009.23	2.20	2010.02	2.97	2009.49	2.34
Industry shares						
Manufacturing	0.83	0.38	0.61	0.49	0.87	0.34
Construction	0.14	0.35	0.01	0.12	0.09	0.28
Transport	0.00	0.06	0.09	0.29	0.01	0.08
Non-financial services	0.00	0.00	0.11	0.32	0.00	0.00
Retail	0.00	0.00	0.08	0.28	0.00	0.00
Other	0.03	0.16	0.08	0.28	0.04	0.19
N	15,795		30,112		24,145	

Notes: The table shows average charateristics and standrad deviations of the LIFO sample (column 1) used for analysis in section 4 an the mass layoff sample used in section 5.1. Column 3 show the sample in column 2 reweighted to mimic the sample composition of column (1).

Table A.5: Sample characteristics by size of layoff

	Size of layoff as share of workforce (quantile of distribtuion)							
Panel A. Unweighted sample	≤ 13% (Q1)		13 – 19% (Q2)		19 – 28% (Q3)		28% ≤ (Q4)	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	36.39	11.83	36.46	11.92	39.04	11.91	41.64	11.78
Female	0.24	0.43	0.22	0.41	0.18	0.38	0.13	0.34
Tenure (months)	61.78	60.24	66.02	62.20	83.13	68.27	99.85	81.50
Immigrant	0.18	0.38	0.18	0.38	0.16	0.36	0.11	0.32
Level of education								
Compulsary	0.44	0.50	0.47	0.50	0.55	0.50	0.60	0.49
Upper-secondary	0.50	0.50	0.49	0.50	0.42	0.49	0.37	0.48
College	0.06	0.23	0.04	0.20	0.03	0.17	0.03	0.18
Industry shares								
Manufacturing	0.88	0.33	0.86	0.34	0.82	0.39	0.77	0.42
Construction	0.11	0.31	0.12	0.33	0.15	0.36	0.17	0.38
Transport	0.01	0.09	0.00	0.00	0.00	0.00	0.01	0.08
Non-financial services	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Retail	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Other	0.01	0.09	0.02	0.12	0.03	0.17	0.05	0.22
N	3,951		3,919		3,917		3,924	

	Size of layoff as share of workforce (quantile of distribtuion)							
Panel B. Reweighted sample	≤ 13% (Q1)		13 – 19% (Q2)		19 – 28% (Q3)		28% ≤ (Q4)	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	36.13	11.84	36.46	11.92	36.06	11.56	36.49	11.19
Female	0.19	0.39	0.22	0.41	0.23	0.42	0.21	0.41
Tenure (months)	66.51	64.78	66.02	62.20	65.27	55.62	63.55	59.39
Immigrant	0.17	0.38	0.18	0.38	0.17	0.37	0.17	0.38
Level of education								
Compulsary	0.47	0.50	0.47	0.50	0.47	0.50	0.46	0.50
Upper-secondary	0.50	0.50	0.49	0.50	0.49	0.50	0.49	0.50
College	0.03	0.18	0.04	0.20	0.04	0.19	0.04	0.21
Industry shares								
Manufacturing	0.88	0.32	0.86	0.34	0.85	0.35	0.86	0.34
Construction	0.11	0.31	0.12	0.33	0.12	0.33	0.13	0.34
Transport	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Non-financial services	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Retail	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Other	0.01	0.09	0.02	0.12	0.03	0.16	0.01	0.08
N	3,523		3,919		3,584		3,446	

Notes: The table shows summary statistics seperatly for each quartile of layoff size relative to the establishment. Panel A) presents unweighted estimates whereas Panel B) weights Q1, Q3 and Q4 to mimic the sample composition of Q2.

Table A.6: Balancing of covariates between orginal and matched sample

	(1)	(2)	(3)
Earnings in $t-1$ (1000 SEK)	0.0523	0.0840*	0.0778
	(0.0394)	(0.0488)	(0.1199)
Female	-0.0075	-0.0119	-0.0228
	(0.0149)	(0.0183)	(0.0348)
Immigrant	0.0016	-0.0046	0.0009
	(0.0172)	(0.0195)	(0.0368)
Age	-0.0000	0.0002	-0.0000
	(0.0003)	(0.0004)	(0.0011)
Primary School	ref.	ref.	ref.
High school	-0.0017	-0.0002	-0.0022
	(0.0081)	(0.0091)	(0.0218)
College	-0.0342**	-0.0271	-0.0365
	(0.0170)	(0.0225)	(0.0570)
Circuit FE		✓	
Circuit×RR FE			✓
F-statistic	0.905	0.880	0.216
p-value	0.491	0.509	0.972
# clusters	513	513	513
N	15,072	15,072	14,168

Notes: The table show balancing of pre determined covariets between workers below the threshold and matched workers using propensity score matching. Columns (1)-(3) show results from regressing the instrument (being above the threshold) on a set of baseline covariates and a polynomial control function in relative ranking interacted with the threshold. Annual earnings in measured in the year prior to notification in 1000 SEK. The bottom of the table displays the F-statistic and the corresponding p-value from testing the hypothesis that all coefficients being jointly equal to zero. All regressions use a bandwidth ± 16. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table A.7: Balancing of covariates at threshold with matched workers

	(1)	(2)	(3)	(4)	(5)
Earnings in $t-1$ (1000 SEK)	-0.0134	-0.0113	-0.0064	-0.0024	-0.0022
	(0.0235)	(0.0281)	(0.0163)	(0.0034)	(0.0045)
Female	-0.0030	-0.0018	-0.0018	-0.0032	-0.0068
	(0.0060)	(0.0070)	(0.0041)	(0.0136)	(0.0179)
Immigrant	0.0064	0.0073	-0.0027	0.0113	-0.0072
	(0.0068)	(0.0074)	(0.0047)	(0.0131)	(0.0186)
Age	-0.0003	-0.0004	-0.0001	-0.7525*	-0.4330
	(0.0002)	(0.0003)	(0.0002)	(0.4181)	(0.5583)
Primary school	ref.	ref.	ref.	-0.0150	0.0068
				(0.0180)	(0.0248)
High school	0.0014	-0.0000	-0.0029	0.0114	-0.0132
	(0.0056)	(0.0061)	(0.0039)	(0.0182)	(0.0248)
College	0.0143	0.0055	0.0050	0.0036	0.0064
	(0.0119)	(0.0151)	(0.0100)	(0.0060)	(0.0089)
Order of polynomial					
1st degree	✓	✓		✓	
2nd degree			✓		✓
Circuit FE		✓	✓	✓	✓
F-statistic	0.984	0.686	0.379	·	·
p-value	0.435	0.661	0.892	·	·
R^2	0.737	0.731	0.880	·	·
# clusters	513	513	513	513	513
N	13,818	13,818	13,818	13,848–13,887	

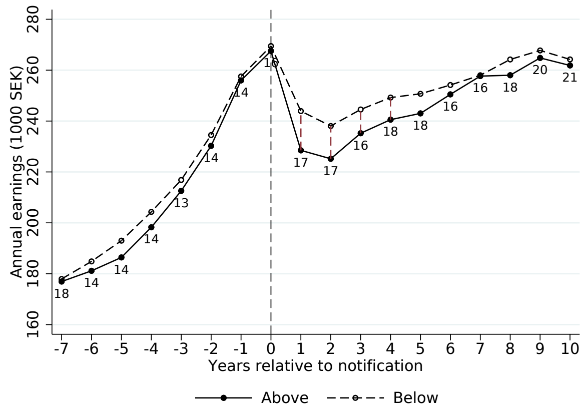
Notes: The table show balance tests of baseline covariates at the LIFO threshold between the original sample (above threshold) and the matched sample (below threshold) which consists of workers matched to original workers below the threshold. Columns (1)-(3) show results from regressing the instrument (being above the threshold) on a set of baseline covariates and a polynomial control function in relative ranking interacted with the threshold. Annual earnings in measured in the year prior to notification in 1000 SEK. The bottom of the table displays the F-statistic and the corresponding p-value from testing the hypothesis that all coefficients being jointly equal to zero. Columns (4)-(5) report results from balancing tests where each covariate has been regressed separately on the instrument and a polynomial control function in relative ranking interacted with the threshold. All regressions use a bandwidth ±16. Standard errors clustered at the level of the order circuit and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

B Optimal bandwidth

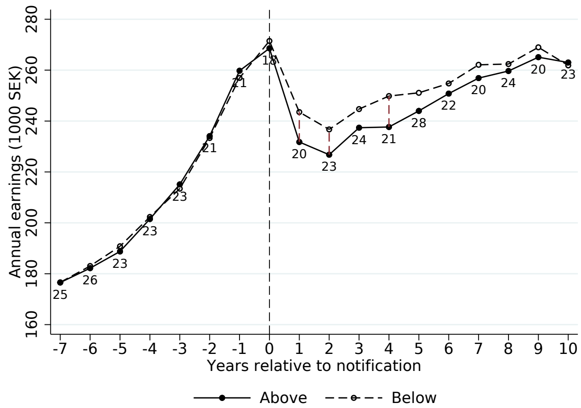
Figure B.1 reproduces Figure 5 using the optimal bandwidth selector suggested by Calonico et al. (2014) with and without adjusting for pre-determined covariates in the bottom and top panel, respectively.

Figure B.1: Evolution of annual earnings relative to year of notification (optimal bandwidth)

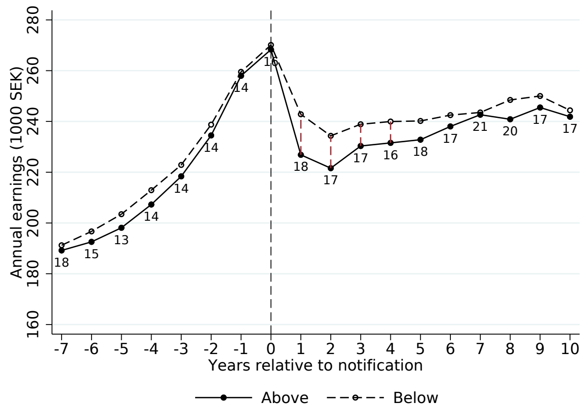
(a) Linear fit without covariates



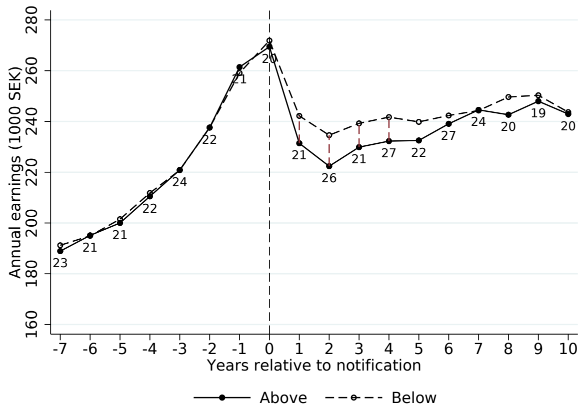
(b) Quadratic fit without covariates



(c) Linear fit with covariates



(d) Quadratic fit with covariates



Notes: The figure show annual earnings relative to the year of notification. The bottom panel show estimates while controlling for female, immigrant status, age, age squared and level of education FE:s, whereas the top panel excludes these covariates. For each time point, I plot the constant (hollow circles) and constant+γ (solid circles) estimate of equation (3) which corresponds to the average predicted value of each outcome for workers to the left and right of the threshold, respectively. Indicated below each point is the optimal bandwidth suggested by Calonico et al. (2014). The dashed vertical line indicates significance at the 5-percent level where standard errors are clustered at the order circuit level. The regressions include a linear or second order polynomial function interacted with the threshold as well as order circuit fixed effects.

C Notifications - institutions, law and data

Mandated notice policies are far from a unique Swedish phenomenon, but, exists in all OECD countries. Swedish labor law stipulates that an employer who wants to lay off a worker must give written notice to him or her in advance. The length of the notification period in Sweden is a step-wise function determined by a workers' tenure (at the time of notice) where the minimum notice of 1 month is given to employees with less than 2 years of tenure. Workers with at least 4 years of tenure are entitled to 2 months of notice, 6 years of tenure implies 3 months of notice and so on until the maximum legislated notice of 6 month is acquired at 10 years of tenure. The law functions as a minimum requirement and can be side-stepped by collective agreements that are in favor of the worker. For instance, many white-collar agreements stipulate that workers above age 55 with 10 years of tenure get an additional 6 months of notice. Workers and firms are also free to agree on severance packages that deviate from the default rules as long as they are perceived as more generous from the worker's point of view.

A firm intending to notify at least 5 workers must report this to the Public Employment Services (PES). In a first stage, the employer reports to the PES how many workers it intends to displace. How early the firm needs notify the PES is regulated in law and depends on the number of intended displacements with: 5–25 workers requiring 2 months before the first displacement, 26–100 workers requiring 4 months and more than 100 workers requiring 6 months before the first worker is laid off. If a firm fails to oblige by the law, it is subject to a fine of 100–500 Swedish krona per worker and week the notification is late. Note that, while these rules require firms to report earlier to the PES, this does not necessarily imply that workers get their individual notice earlier. Whether the workers are made aware of the notification being sent in to the PES is up to the employer. The PES treats all information received by the employer as confidential.

Upon the initial report to the PES, the firm enter negotiations with the labor unions on who to lay off, respecting the last-in-fist-out principle. In a second stage, a list of all individuals who are notified of their displacement is later sent in to the PES. The list contains the workers' name, social security and date of their last day of employment and individual notification. Employers are free to recall entire notification or update the list, subtracting or adding workers, as long as the number of notified workers on the list does not exceed the number of intended displacements stated in the first stage.

In less than one percent of all instances, the entire notification is recalled by the employer. In notifications that lead to at least one worker being displaced, about 15 percent of notified workers are still working at the notifying firm 2 years after notification. Adding workers at a later point in time to the list in uncommon. Most likely due to negotiations with he union having already been settled when the original list is sent in. Only about 1.2 percent of all notifications have workers added to or taken off the list after the initial submission.

No systematic review of to what extent firms comply with the above mentioned time restrictions imposed by the notification law have not been done. However, in email correspondence with the PES, they reveal that their impression is that a great majority of all displacements exceeding 5 workers are reported to them. When it comes the larger layoffs, probably all. With respect to individual notification times, essentially all employers comply with the law and/or the collective agreement regulating the minimum amount of notice. This as there are almost always labor unions present to supervise the process.

D Calculation of tenure

A worker's relative seniority is directly related to tenure since it is defined as his tenure relative to the tenure distribution of the rest of the workforce within an order circuit. Hence, any measurement error in workers' individual tenure or the full tenure distribution within the circuit induces measurement error in the running variable as defined in equation (1).

I construct tenure using the indicators of first and last month worked at a firm which are reported by the employer along with the annual income statement in the matched employer-employee data. Employment spells may be interrupted by months of non-employment if the employer has reported two or more spells of employment where e.g. the first spell may last January to March and the second spell e.g. August to December. As such I can create monthly markers indicating whether the worker is employed in any given month at a particular firm or an establishment. Using these monthly indicators I rank workers, who are employed at the time of notification, by their date of first employment at the firm. As noted in Section 2.1 there may be false ties in tenure due to employers too often reporting January as the month where the worker started employment. To avoid such ties which are due to measurement error, I divide workers with the same start date into quartiles of annual earnings in the first year of employment, where workers in lower quartile are assumed to have started employment later than workers in higher quartiles. I drop entire circuits where more than 2/3 of workers have a tenure equal to the mode of the circuit as these ties are most likely due to so called false firm deaths where firms for other reason than bankruptcy change identification number. Finally, when constructing the running variable, relative ranking, I break ties in tenure by age at notification (following the LIFO rule).

Even if tenure was perfectly measured, there are still potential sources of measurement error in the running variable. The LIFO rule which applies at the CBA $\times$ establishment level is proxied by 2-digit occupational codes. The lack of a one-to-one mapping between CBA's and occupation codes makes it difficult to precisely define the relevant workforce subject to the notification. Thus the full tenure distribution within the order circuit may be obscured by including workers in an order circuit which they do not belong to. Misallocating just one worker will render the circuit too large or too small and thereby leading me to place the discontinuity in the wrong place in the tenure distribution when normalizing the running variable with the number of notified workers (N_c). To minimize the risk of missmeasuring order circuits I restrict the circuits to be no larger than 100 workers.

Importantly, as long as there is some information about the true running variable in the proxy that is used, the above listed causes which may generate measurement error does not affect the consistency or causal interpretation of my estimates but only induces noise in the running variable (RR), thereby attenuating the first stage. However, had there been zero mass of individuals with the correct values of their running variable, the RD-estimator would have been inconsistent as any first stage discontinuity would have been smoothed out (see Davezies and Le Barbanchon, 2017). By analogy, this is similar to what happens in two-stage least squares with weak instruments where the denominator in the Wald-estimator is zero.

Appendix References

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik, "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs," *Econometrica*, 2014, 82 (6), 2295–2326

Davezies, Laurent and Thomas Le Barbanchon, "Regression discontinuity design with continuous measurement error in the running variable," *Journal of Econometrics*, 2017, 200 (2), 260–281. Measurement Error Models.