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Reconsidering the Cost of Job Loss: Evidence from Redundancies and Mass Layoffs*

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 larger layoffs increase exposure to non-employment, prolong unemployment duration and cause workers to leave the labor force to a greater extent.

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] Since the seminal study by Jacobson, Lalonde and Sullivan (1993) the vast majority of research estimating the consequences of job loss compare displaced *vis-a-vis* non-displaced workers, across firms, using mass layoff and plant closure as an exogenous source of variation. However, such events are quite exceptional 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.[2] Unfortunately, evidence from the remaining 92 percent of smaller layoffs is 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 novel 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.

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, 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.[3] 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.[4] While the empiri-

[1]For example, see Ruhm (1991); Jacobson et al. (1993); Eliason and Storrie (2006); Hijzen et al. (2010); Davis and von Wachter (2011); Lachowska et al. (2020); Song and von Wachter (2014); Burdett et al. (2020); Fallick et al. (2021); Bertheau et al. (2023); Schmieder et al. (2023); Athey et al. (2024). Reviews of the literature can be found in Fallick (1996); Couch and Placzek (2010); Davis and von Wachter (2011). Displacement has also been shown to lead to worse health (see e.g. Browning, Dano and Heinesen, 2006; Kuhn, Lalive and Zweimüller, 2009; Black, Devereux and Salvanes, 2015; Schaller and Stevens, 2015; Jolly and Phelan, 2017), 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 (Bennett and Ouazad, 2019; Britto et al., 2022).

[2]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.

[3]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.

[4]Two prominent exemptions are von Wachter and Bender (2006) and Rose and Shem-Tov (2023), studying young and low wage workers, respectively. Both papers exploit firm-specific labor demand shocks to instrument

cal strategy may solve the selection problem, estimates will by design pertain to a particular population of workers laid off under rather specific circumstances.[5] 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 of 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.

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., 2023). 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's share of the initial difference in earnings, these have subsided after three years. Displaced workers suffer wage cuts at their new jobs but have faster wage growth than their non-displaced former colleagues and after seven years earn

for worker displacement.

[5]Naturally, the same argument can be made to this study as it imposes int own sample restrictions and specific empirical strategy. In section 5, I investigate in detail how differences in sample composition and empirical strategy, relative to the previous literature, affect the interpretation and external validity of my estimates.

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, I probe this result in great detail. I begin by estimating earnings losses using the conventional event study mass layoff estimator, finding large and permanent earnings losses ranging from 14 to 21 percent ten years after displacement. This suggests that the transitory pattern observed in the RD analysis is not specific to the particular country or time period being studied. Furthermore, to ensure that these results are not driven by compositional differences across the two samples, I reweight the estimates to mimic the LIFO sample along multiple dimensions. While rendering a slight decrease in long-run losses, they remain permanent and substantial ten 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.

A key difference between the canonical mass layoff estimator and the RD design 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 an average treatment effect on the treated (ATT), the RD analysis pertains to the marginally displaced worker and when instrumented reflects a local average treatment effect (LATE). I examine to what extent differences in target parameters and complier populations can account for the diverging results by combing the two estimators. Matching workers below the LIFO-threshold to workers in other (non-downsizing) establishments, I emulate the conventional across firm control group used in the previous literature while still maintaining identification of the LATE parameter. While these results show slightly larger earnings losses on average, the transitory earnings pattern remains intact with workers fully recovering within ten years. These estimates, however, conceal a great deal of heterogeneity: splitting the sample by layoff size shows that permanent earnings losses occur only among workers displaced in large layoffs. In fact, in large layoffs the LATE estimates are very similar in magnitude to those of the ATT obtained using the standard mass layoff approach, suggesting that from an empirical point of view, the LATE is similar to the ATT. Workers displaced in smaller layoffs, which make up the majority of displacements, recover much faster, closing the earnings gap six to seven years after displacement. This pattern is also confirmed in the original (non-matched) RD sample, showing a clear negative relationship between long-run earnings losses and the size of layoff, even after reweighting larger layoffs to resemble small layoffs along several dimensions.

Taken together, these findings suggest that mass layoffs are intrinsically 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.[6]

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, Fackler, Mueller and Stegmaier (2021); Schmieder, von Wachter and Heining (2023) have emphasized the loss of firm 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 et al., 2016; Burdett et al., 2020; Fallick et al., 2021; Jarosch, 2023). I show that larger layoffs, relative to smaller ones, lengthen unemployment duration and increase workers' exposure to non-employment. Furthermore, workers displaced in large layoffs have higher probability of leaving the labor force altogether some years after the initial displacement. Finally, among those who do find employment, workers displaced in large layoffs tend to be more likely to find jobs outside their local labor market, compared workers displaced in smaller layoffs.

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 novel evidence on the consequences of job loss not due to mass layoff or plant closure and offer new insights into how the size of layoff shapes the persistence of earnings losses following job loss.[7] The findings also speak to a recent strand of the literature that seeks to explain why earnings losses become permanent. This includes both empirically oriented work (see e.g. Lachowska et al., 2020; Fackler et al., 2021; Schmieder et al., 2023; Athey et al., 2024), as well as theoretical contributions (see e.g. Krolikowski, 2017; Jung and Kuhn, 2018; Burdett, Carrillo-Tudela and Coles, 2020; Huckfeldt, 2022; Jarosch, 2023) as the persistence of earnings losses has frequently eluded standard labor market models (Davis and von Wachter, 2011).

[6] These results contrast one auxiliary finding in Flaaen et al. (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 biased downwards. In addition, for this auxiliary analysis Flaaen et al. (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.

[7] An interesting, although somewhat overlooked, fact is that Jacobson, Lalonde and Sullivan (1993) already in their seminal 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.

My findings closely align with recent evidence indicating that the duration of subsequent joblessness plays a key role in explaining the magnitude and persistence of workers' earnings losses following displacement (Fallick et al., 2021; Schmieder et al., 2023; Rose and Shem-Tov, 2023).[8] They are also consistent with a recent literature showing that displacement leads to longer and serially correlated unemployment spells, during which workers lose human capital (see Burdett, Carrillo-Tudela and Coles, 2020; Jarosch, 2023). Finally, while my paper is the first to document that earnings losses are transitory in small layoffs and permanent only in large layoffs, the importance of event size is also underscored by Athey et al. (2024), who – focusing on plant closures – find that larger events are associated with greater earnings losses. Also, in a related study, Fackler et al. (2021) examine how firm size shapes workers' earnings losses after displacement. They find that workers displaced from smaller firms experience smaller — though still permanent — losses than those from larger firms, attributing this gap to the loss of firm-specific wage premiums. While consistent with my findings, their estimates do not disentangle firm size from layoff size, since displacement is identified through firm closures (bankruptcies). By separating these two dimensions, I show that the earnings gradient with respect to layoff size is roughly an order of magnitude larger in absolute terms, indicating that the size of the layoff, rather than the size of the firm, accounts for most of the persistence in workers' earnings losses.

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 and a discussion on the interpretation of the LATE. 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 examine when and why earnings losses become permanent following displacement, whereas Section 6 explores why permanent earnings losses are found primarily among workers displaced in large layoffs. Section 7 concludes.

[8] When estimating distributed-lag models with a matched control group, Fallick et al. (2021) find that workers separating from non-distressed firms experience similar – perhaps even larger – earnings losses compared to those separating from distressed firms. My results do not corroborate this finding, which also contrasts with Jacobson, Lalonde and Sullivan (1993). Unfortunately, Fallick et al. (2021) cannot assess whether their result is driven by adverse selection among workers in small layoffs, as they lack data on worker characteristics (aside from gender and age). Moreover, unlike Flaaen et al. (2019), who rely on survey data containing individuals' reported reasons for separation, it is not possible to determine to what extent separations from non-distressed firms reflect worker choice or dismissals for cause, which is one of the biases the use of mass layoffs aim to avoid (Couch and Placzek, 2010).

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 during layoffs in many countries (e.g., Germany, the UK, and the Netherlands), although they vary considerably across sectors and national contexts. 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 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) find 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 by establishing discontinuities in seniority ranking.

The Swedish LIFO rule The 22:nd paragraph in the Swedish Employment Protection Act (EPA) stipulates that when a firm downsizes due to "shortage of work", it must follow a last-in-first-out (LIFO) principle, whereby workers are laid off in inverse order of seniority. In case of equal tenure, priority is given to the older worker. The rule applies at the establishment level, and in the event of multiple layoffs, employers must divide workers into groups based on collective bargaining agreement (CBA) affiliation and rank them according to tenure.[9] These groups constitute so-called order of termination circuits (*turordningskretsar*), henceforth referred to as order circuits or circuits. The law also contains a provision that workers who lack *sufficient qualifications* to perform the tasks of the new job, may be exempted from the LIFO rule. This requirement does not, however, confer broad discretion to the employer as case law interprets "sufficient qualifications" very narrowly — typically as the minimum competence required to perform the job, not the relative nor optimal skills. The burden of proof that a given worker lacks the nessecary general skills normally expected for the position (and therefore could be exempted from the LIFO rule) lies with the employer. Moreover, the employer needs to make

[9] 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).

it credible that the worker would not be able to acquire these skills within a reasonable time period (typically 3–6 months). Finally, the law also provides certain exemptions from the LIFO principle: firms with fewer than ten employees may exclude up to two workers who are considered particularly important to the business, and workers in managerial positions or belonging to the employer's family may also be exempted.

Certain provisions of Swedish labor law are semi-optional, meaning they can be modified through collective agreements. The LIFO rule is one such provision: employers may negotiate a different order of priority with local union representatives as long as it does not contravene "good practice in the labor market" or the Discrimination Act. If no agreement is reached, the statutory rule applies, making LIFO the default starting point in negotiations. Little is known about the frequency of such deviations, and it remains unclear whether compliance with LIFO is usually voluntary or results from union enforcement.

Lastly, Swedish labor law also establishes a "last-out-first-in" principle (EPA:26§) governing recall rights. The displaced worker with the highest tenure in the circuit has priority for re-employment if the firm resumes hiring within nine months, provided the worker has at least 12 months of tenure, is sufficiently qualified for the new position, and has notified the employer of their wish to be recalled.

2.1 Data

The main data source used is an individual level administrative register of all layoff notifications reported to the Public Employment Service (PES) during 2005–2015. By law, a firm that intends to displace more than five workers within a three month period must notify the PES. In a first stage, the firm reports to the PES the number of intended layoffs and the reason for downsizing. The initial report is not public and kept confidential by the PES. Of course, the firm may themselves announce an impending layoff and therefore, I use the date of the first report as the date of notification. In a second stage, occurring on average 70 days later, the firm submits a list of workers who are notified of their displacement. These data contain in addition to the individual workers' notification and displacement date, their (anonymized) social security number which enables me to match notified workers to other administrative records. Typically, all workers who are notified are being so on the same date whereas the date of displacement differs due to differences in statutory notification times.[10] 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

[10]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 and at times by age according to local CBA's. Appendix E describes the notification process and the law in more detail and provides some descriptive statistics (see also Cederlöf et al., 2025). The tenure thresholds governing notification times, could occasionally line up with the LIFO-threshold but in practice do so in only one percent of the layoffs in my main sample. Note, however, that this does not affect the estimated jump at the threshold as it is identified from the comparison between displaced and non-displaced workers. Regardless, the jump at the threshold among notified workers just above and below as estimated to 0.296 days ($SE = 2.179$).

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 spell which may in turn generate some measurement error in tenure. To minimize the influence of this error, I divide workers starting in January in the first year of employment into quartiles of annual earnings where lower quartiles are assumed to have started employment later.[11]

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 inaccurate ties in tenure within a firm. Therefore, I exclude order circuits in which more than two-thirds of workers have tenure equal to the circuit's modal tenure. [12]

As described above, the LIFO rule applies at the establishment×CBA level. Ideally, one would have accesses to which workers are covered by which CBA. As these data do not exist, I proxy workers' CBA affiliation using (the Swedish version of) 2-digit level ISCO-88 (International Standard Classification of Occupations 1988) occupational codes provided in the annual wage survey collected by Statistics Sweden. The register also contain information on workers' (full-time equivalent) wages, occupation and hours worked at the individual level. This 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.

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. I include in my sample all employees working at the establishment when the first notification report is sent in to the PES. I then rank individuals according to seniority within their respective circuit, 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

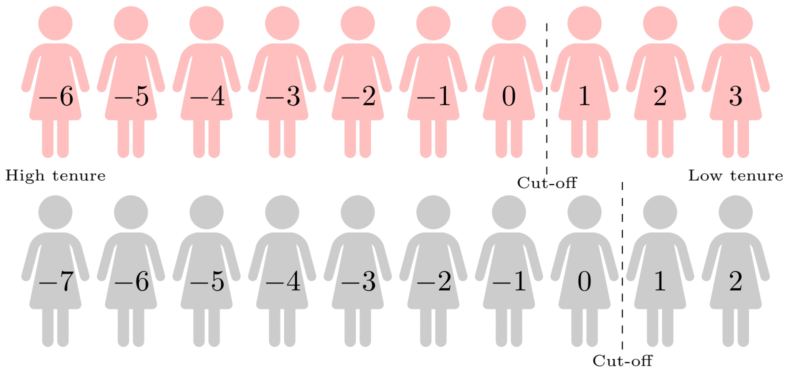
$$RR_{ic} = SR_{ic} - (\max_{i \in c}(SR_i) - N_c) \tag{1}$$

where SR_{ic} is the seniority ranking and N_c is the number of notified workers (within a given occupation) reported in the list submitted by the employer to the PES. RR_{ic} is the running

[11] Appendix B provides more detail on the procedure for calculating tenure and elaborates on the sources of measurement error relevant for the running variable.

[12] An alternative approach, suggested by Benedetto et al. (2007), 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 view placing only one restriction as being more transparent.

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.

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 are ranked according to tenure (and age in case of a tie). In the upper row $N_c = 3$ and in the lower row $N_c = 2$. Thus, workers to the right of the cut-off ($RR > 0$) 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 impose several sample restrictions on the data. First, I exclude layoff notifications attributed to plant closures or bankruptcies, because the threshold within circuits in such events is undefined when 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 apply among blue-collar workers to a greater extent. Finally, I condition on layoff notifications affecting at least ten workers within an order circuit which is restricted to contain at most 100 workers.[13]

2.1.1 Descriptive statistics

Column (1) of Table 1 presents descriptive statistics for the sample used in the main analysis (restricted to a bandwidth of ±16 in relative seniority). Workers in this sample are on average 38 years old and have an average tenure of about 6.5 years. Because the sample is restricted

[13]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 it lowers the probability of including "wrong" workers. Figure A.13b in Appendix A display the distribution of the size of the order circuits.

Table 1: Descriptive statistics

	LIFO sample		Mass layoff sample	
	Mean	SD	Mean	SD
Age	38.35	12.06	41.99	8.12
Female	0.19	0.39	0.35	0.48
Immigrant	0.16	0.36	0.14	0.35
Tenure	6.45	5.83	12.79	6.69
Earnings$_{t-1}$ (1000 SEK)	247.58	95.13	277.65	120.16
Level of education				
Compulsory	0.51	0.50	0.52	0.50
Upper-secondary	0.45	0.50	0.38	0.48
College	0.04	0.20	0.10	0.30
Industry shares				
Manufacturing	0.83	0.38	0.61	0.49
Construction	0.14	0.35	0.01	0.11
Transport	0.00	0.06	0.09	0.29
Non-financial services	0.00	0.00	0.11	0.31
Retail	0.00	0.00	0.08	0.28
Other	0.03	0.16	0.08	0.28
Firm size	472.98	1407.74	877.951	3261.11
Establishment size	169.38	304.79	158.45	163.95
N	15,795		30,112	

Notes: The table reports summary statistics for workers notified of displacement between 2005 and 2015. Columns (1) and (2) present sample means and standard deviations for the workers included in the main analysis. Columns (3) and (4) report corresponding statistics for a comparison sample constructed using the standard restrictions applied in the mass layoff literature (see Section 5.1 for details). All characteristics are measured at the individual level, except firm and establishment size, which represent the average size of the respective layoff event.

to industries dominated by blue-collar workers, employees from manufacturing firms are over-represented. Consequently, the majority of workers are male, and roughly 50 percent have compulsory school as their highest level of education.[14] Note that this sample contains both notified/displaced and non-notified/non-displaced workers.

Column (2) shows descriptives for the sample of (displaced and non-displaced) workers in a typical mass layoff sample (non-overlapping with the LIFO sample) which will also be used in parts of the analysis. To facilitate comparison with previous literature, I follow Schmieder et al. (2023) and Lachowska et al. (2020) and restrict attention to 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) which employ at least 50 workers (section 5.1 provides more detail on how the sample is constructed). Also in this sample, workers are primarily concentrated in the manufacturing industry and as a consequences of the imposed (standard) sample restrictions,

[14] 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 in the mass layoff sample have about twice the tenure of workers in the LIFO sample and therefore about ten percent higher earnings. The share of immigrants and the level of education is roughly the same between the two samples. Finally, firms are on average larger in the mass layoff sample compared to the LIFO sample while establishments are about the same size.

It is worth emphasizing that workers in the LIFO and mass layoff sample are indeed different from each other in terms of some key characteristics, which in turn could influence the cost of job loss. In section 5, I probe this question in detail and investigate how sample composition affect estimated earnings losses.

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. 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 relative seniority ranking is discrete, 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 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.[15] 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 prox-

[15]Typically, the included covariates are: age, age squared, gender, immigrant status, educational attainment FE:s.

ied 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 is thus a local average treatment effect (LATE), whose interpretation I discuss in more depth in Section 3.4. Importantly, the IV takes care of measurement error in the independent variable stemming from not being able to perfectly measure relative seniority (see section 2.1).

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.[16] 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 Manipulation of the LIFO-threshold

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 where in the seniority ranking workers end up and thereby determine who gets notified and eventually displaced. From the perspective of the worker, sorting around the threshold 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).[17] However, for this to generate imbalances around the threshold, it would require that workers simultaneously *i*) have pre-knowledge about the impending notification even before the PES is notified, *ii*) are aware of their *exact* position in the seniority ranking within the order circuit and that *iii*) workers leave the establishment based on characteristics directly affecting the outcome differentially across the threshold.[18]

A more plausible type of manipulation is one where the employer chooses who to displace,

[16]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 C, the results remain virtually unchanged when I allow the optimal bandwidth to differ in each time period.

[17]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.

[18]Figure A.2 in Appendix A provide evidence of workers being unaware of the impending notification. Combing data on layoff notifications with the Swedish labor force survey and unemployment register, containing questions about job search and caseworker meetings, respectively, both outcomes exhibits a sharp increase following the month of notification. Before notification both remain constant or slightly decreasing, at much lower levels.

either by endogenously forming the order circuits in negotiations with the union or by invoking the 'sufficient qualifications' provision (see Section 2. However, this type of manipulation of the does not enter into the RD as the establishment×occupation combination functions as a proxy/instrument for actual order circuits. So if the local union and the employer either agrees upon deviations from LIFO or if the employer is successful in arguing for exemptions due to insufficient qualifications, the relative tenure ranking within the *proxied* order circuit is still intact. Hence, such deviations would merely render relative seniority within proxied order circuits unpredictive of displacement and attenuate the first stage coefficient.[19]

Manipulation of who gets displaced could, however, occur as firms set the cut-off endogenously by choosing how many workers to notify and eventually displace. For example, a 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 required 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.[20] Columns (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. A joint significance test is 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.[21]

Taken together, the fact that observable worker characteristics and previous 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

[19] In theory, employers could just before notification switch occupational titles on the workers they want to keep or displace. However, I find no significant differences at the threshold on within establishment occupational switching before notifications arrives. That is, there is no evidence of employers selectively displacing workers by placing them in a different statutory order circuit, where they would have lower relative seniority.

[20] 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. For completeness, however, Figure A.1 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.

[21] Figure A.3 show the balancing tests corresponding to column (4) and (5) graphically.

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.

laying off $n+1$ workers are also small as the marginal worker being laid off have first priority of recall 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.

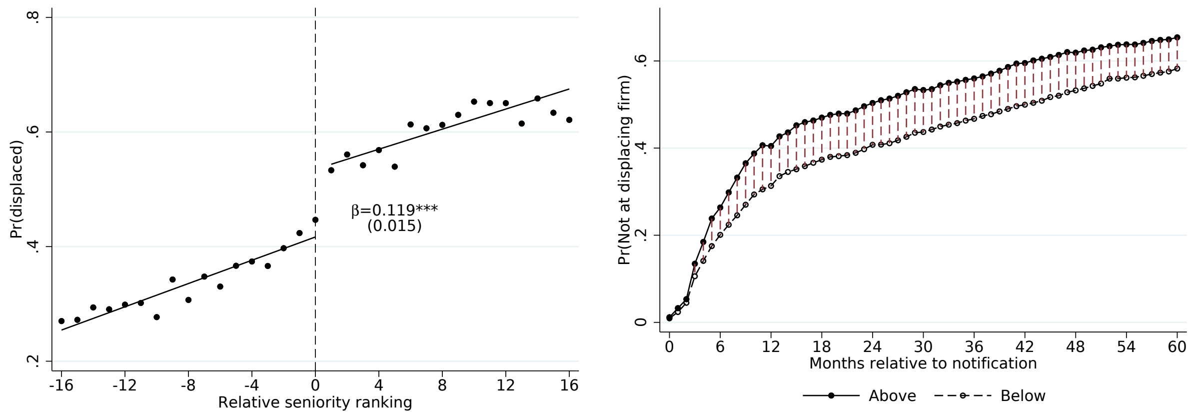
3.3 Layoff and the LIFO-threshold

Figure 2a shows the probability of being displaced as a function of workers relative seniority ranking within an order circuit, where displacement is defined as having left the notifying firm within 15 months after notification.[22] As predicted by the LIFO rule, the probability of

[22]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.

Figure 2: First stage effects

(a) Pr(displacement) (b) Probability not working at displacing firm



Notes: The figure shows in panel a) 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 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. Panel b) plots the probability of having left the notifying firm for a given month relative the month of notification where 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. 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. Standard errors are clustered at the order circuit level.

displacement jumps discontinuously at the threshold with workers just above it having about a 12 percentage point higher risk of displacement (corresponding to a 30 percent increase). The effect is precisely estimated with a F-statistic of 62, indicating adequate relevance of the instrument. Note that even though workers are displaced from the notifying firm, this does not necessitate subsequent non-employment as workers may find new employment without an intermittent spell of unemployment (i.e job-to-job transition).

Had the LIFO rule been fully binding, this would imply a sharp jump in the probability of displacement, going from 0 to 1 at the threshold. However, as seen in Figure 2a, workers just below the threshold have about a 40 percent risk of being displaced. The "fuzziness" of the first stage is partly due to measurement error in the running variable, arising from inaccurate reporting of tenure, but primarily due to approximating workers' CBA affiliation using two-digit occupational codes. Moreover, because the LIFO rule is semi-optional and subject to negotiations between the local union and the employer, this makes the proxied order circuits merely predictive of the *de facto* ones.[23] Importantly, these type of measurement errors do not induce bias but would, if sufficiently severe, merely "smooth out" a true discontinuous first stage (Davezies and Le Barbanchon, 2017). As the IV takes care of measurement error in the independent variable, these data shortcomings are likely less of a concern given that rule is predictive of actual displacement.

Nevertheless, the first stage is not sensitive to changing this to any number ≥ 12 months.

[23] Note that even if *de facto* order circuits were observed, using them could introduce endogeneity because they are outcomes of negotiations between employers and local unions.

In Appendix B, I provide additional results on the robustness of the first stage. These indicate that the instrument remains highly predictive of future displacement both when altering the bandwidth and adding a second-order polynomial, albeit yielding a smaller coefficient. Controlling for baseline covariates barely changes the estimate, again reassuring that worker characteristics are balanced around the threshold. Figure B.1 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. Finally, Figure B.2 confirms that the first stage is driven by differences in the likelihood of displacement (as opposed to voluntary quits) by showing the first stage using individual worker layoff notification as the dependent variable.[24]

As mentioned above, displacement is defined as having left the firm within 15 months of notification. This allows for some time between when the firm reports the notification to the PES and the actual displacement, as the firm must personally notify the worker and provide a 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 2b 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}. 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 vertical lines indicate significance at a five percent level. As three quarters of the notified workers in the 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 displaced and later recalled to any large extent, nor that workers surviving a layoff become displaced in later notifications.[25]

[24] The first-stage estimate is larger when using notification as the outcome. This is not surprising, as a notification need not materialize into a layoff—for instance, if conditions improve for the firm. This is also why notification is not used as the primary independent variable, since being notified may represent a different form of treatment than actual displacement.

[25] Appendix A provides additional evidence of the control group not being displaced at a later point in time to a greater extent. Figure A.6a shows that the earnings losses and the overall pattern is virtually identical to the main specification (c.f. Figure 3b) when restricting the sample to establishments with one single layoff during the sample period (81 percent of the main sample) Moreover, Figure A.5 shows that there is no difference in the probability of recall just above and below the threshold with the estimated jump being is 0.009 percent ($SE = 0.01$).

3.4 Interpreting the LATE

Before turning to the results, it is important to clarify which parameter is identified by the LIFO threshold. The consequences of job loss are typically estimated by comparing displaced and non-displaced workers in a matched event-study design, which recovers an average treatment effect on the treated (ATT). In contrast, the fuzzy RD based on the LIFO threshold identifies a local average treatment effect (LATE) for workers at the margin of displacement. In a standard IV setting, the LATE may differ sharply from the ATT because individuals may select into treatment based on private information about their expected gains. Thus, even though assignment to treatment is orthogonal to workers' potential outcomes (as shown in Section 3.2), this "Heckman-type" selection into compliance with the LIFO rule nonetheless affects the interpretation of the causal effect.

In the LIFO setting, workers cannot self-select out of displacement. Instead, who is laid off is determined by the seniority rule, and any deviations from it are initiated by the employer. Consequently, the LATE may differ from the ATT if, for instance, employers are more prone to comply with the LIFO rule when the marginal worker about to be displaced has low productivity. In that case, if low-productivity workers suffer greater losses from displacement than high-productivity workers, the LATE would capture larger earnings losses than the ATT. Conversely, if employers choose to comply with the LIFO rule only when the worker just above the threshold has high productivity, the LATE would reflect smaller losses. Importantly, in the first — and arguably more likely — scenario, such selection implies that earnings losses, and their persistence, would exceed those typically documented in the existing literature.

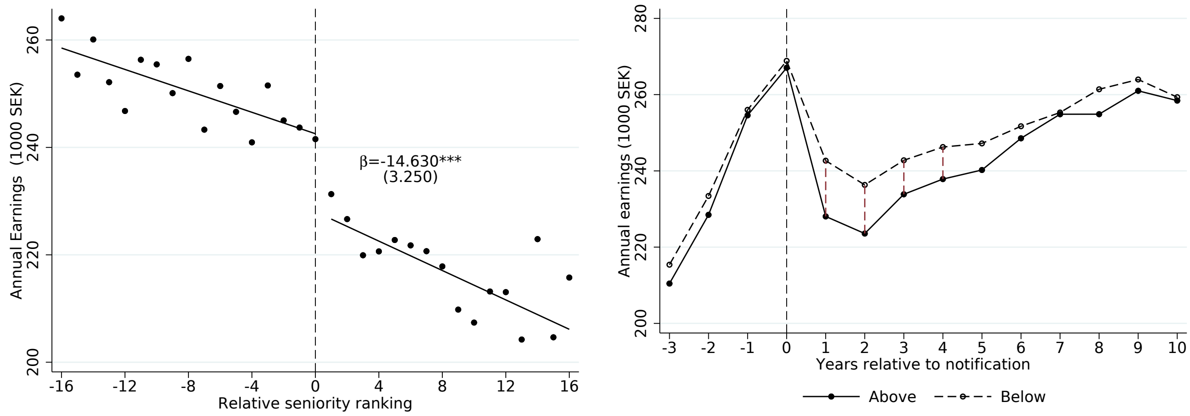
Nevertheless, the primary purpose of the LIFO rule is to prevent employers from exercising discretion in selecting which workers to displace — for instance, on the basis of relative productivity — and, as described in Section 2, the scope for invoking "insufficient qualifications" remains limited. As a result, employers are largely constrained in systematically selecting workers for displacement based on (relative) productivity. Moreover, characterizing the compliers shows that they are very similar to the overall sample (see Table A.2), providing little empirical evidence of selection into displacement. Crucially, if employers' choice to comply with the LIFO rule is unrelated to the potential earnings losses of workers around the cutoff, the LATE coincides with a local ATT identified at the threshold (see Appendix F for proof). Whether a local ATT is larger or smaller than the overall ATT (as identified from mass layoffs) is *a priori* ambiguous. In Section 5, I estimate the overall ATT using the standard mass-layoff approach and compare it to the LATE (local ATT) recovered from the RD design.

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.

Figure 3: Effect on annual earnings

(a) 1 year after notification (b) Dynamic effect on annual earnings



Notes: The figure shows in panel a) annual earnings (in 1,000 SEK) in the year after notification as a function of workers' relative ranking within an order circuit (in discrete bins), normalized to zero at the cut-off. The point estimate of the jump at the threshold is 14.63 with a standard error of 3.25. Panel b) shows the estimated discontinuity at the threshold by time relative to 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 estimate in $t+1$ thus corresponds to the jump at the threshold depicted in panel a). The dashed vertical line indicates significance at the 5-percent 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. Standard errors are clustered at the order circuit level.

4.1 The total earnings effect

Figure 3a gives a snapshot of annual earnings around the threshold in the year following layoff notification.[26] 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 one percent level with a standard error (SE) of 3.25.[27] To trace out the dynamic response of earnings losses and examine its persistence, Figure 3b shows the evolution of annual earnings relative to time of notification where again each time point corresponds to a separate (reduced form) RD regression. 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. As such, the estimate in $t+1$ corresponds to Figure 3a.

Figure 3b reveals that before notification earnings evolve in parallel for workers just above and below the threshold, again suggesting an absence of selective firing among employers. 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

[26] 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.

[27] Figure A.4 shows the uniqueness of this discontinuity in a placebo permutation test by plotting estimates of annual earnings in $t+1$.

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 start to recover as annual earnings start to increase, but in year three and four there are still significant differences in earnings estimated to -8.92 ($SE = 3.53$) -8.41 ($SE = 3.71$), respectively. 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 3b, 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 C.1 shows the estimates from these regressions using first- and second-order polynomials, with and without predetermined covariates. The results are remarkably stable when using the optimal bandwidth selector, even though the optimal bandwidth varies slightly over time. Introducing a second-order polynomial slightly dampens the initial earnings differential, but the absence of long-run earnings losses persists. Figure A.6 further evaluates the sensitivity of the results to recurring layoffs and changes in sample composition over time, and the findings remain essentially unchanged.

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. Panel a) in Table 3 shows IV-estimates on annual 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, while 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. 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). However, these long-run estimates should be interpreted with some caution as confidence intervals contain earnings losses that are indeed quite substantial. However, as will be shown in section 5, this is large occur due to significant treatment heterogeneity across layoff size.

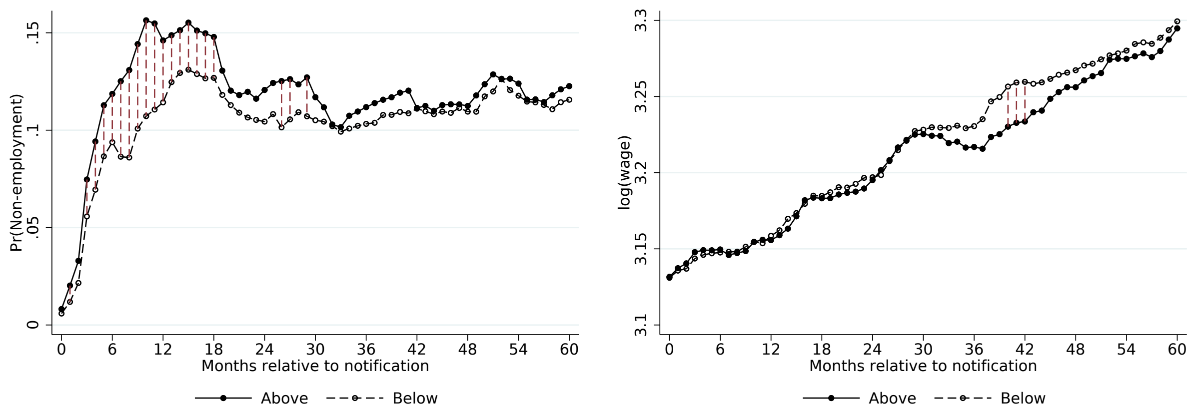
Table 3: IV-estimates on earnings and non-employment

	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.

Figure 4: 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.

Panel b) of Table 3 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. 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.

4.2 Earnings losses by margins of adjustment

In this subsection, I unpack the total earnings effect and estimate the effect of layoff separately on employment, wages and hours worked. Based on these estimates, Appendix D provides a full decomposition.

Extensive margin Separation from the notifying firm does not mechanically induce non-employment, as workers may well find new jobs during their notification period (see Cederlöf et al., 2025). To provide more detail on the effects on employment, I take advantage of the monthly employment markers to trace out the dynamic response. Figure 4a plots the reduced-form probability of non-employment by month relative to notification, where each time point is estimated from a separate RD regression. As with separation from the notifying firm, significant differences in non-employment appear starting three months after notification. These differences remain significant up to 19 months after notification, and additional significant differences appear between months 26 and 29. During this period, workers just above the threshold are about 2–4 percentage points more likely to be non-employed than workers in the same order circuit just below the threshold. Scaling these estimates by the first stage implies that displaced workers are 16–33 percentage points more likely to experience non-employment in a given month, 4 to 19 months after notification. From month 30 onward, however, employment differences appear to have subsided, as no significant differences are detected in subsequent months.

Wage effect Several studies find that lower post-displacement wages are an important driver of long-term earnings losses (see, e.g., Lachowska et al., 2020; Schmieder et al., 2023; Jarosch, 2023). To estimate wage differences between displaced and non-displaced workers, I use data from the Swedish Wage Survey. Because the survey is a random sample of the working population, not all employed workers are observed each year, which substantially reduces the sample size and makes the estimates less precise. Figure 4b shows the evolution of log wages relative to the month of notification for workers just above and below the threshold, where the observed pattern closely mirrors the employment response in Figure 4a. Wage differences only emerge beyond 30 months after notification—that is, once the differences in employment rates have subsided. This reflects the fact that workers who return to employment earlier are positively selected; as such, the wage gap can be interpreted as a pure wage effect of displacement, absent selection, only when employment rates have equalized between the two groups. The reduced-form wage gap is estimated at 2.5–2.6 percent in months 40–42 after notification and is significant at the 5 percent level. When scaled by the first stage, this implies that displaced workers experience, on average, a 15 percent wage loss on their new job. Interestingly, the effects are short-lived: once the wage differential emerges, workers just above the threshold exhibit faster wage growth than their coworkers just below it, closing the gap entirely roughly six years after notification.

Hours response Similar to the analysis on wages, I use the Swedish wage survey 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.

5 Understanding earnings losses upon job loss

The main finding of Section 4 — that earnings losses upon displacement are transitory rather than persistent — appears at first glance to contradict the standard result in the literature. To better understand what drives earnings losses after job loss and the persistence of such losses, I begin by estimating earnings losses for workers displaced in mass layoffs using the standard distributed-lag model, following the conventional definitions and sample restrictions used in prior work. The goal of this exercise is to produce estimates that are comparable to those in the existing literature and to verify that nothing specific to the Swedish institutional context, or to the period I study, mechanically generates transitory earnings losses. The results replicate the well-known finding of large and highly persistent earnings losses, which raises a natural question: why do the long-run earnings effects differ so markedly between the two settings?

To explore this, I first test whether compositional differences across the two samples can account for the divergence in long-run outcomes by reweighting the mass layoff sample to mimic the LIFO sample on observable characteristics. Second, I harmonize the two estimators and combine them into a unified framework, acknowledging that the canonical mass layoff estimator and the RD estimator arise from two potentially different experiments and identify different target parameters. Finally, using the original LIFO sample, I assess the role of layoff size by estimating earnings losses by quartiles of layoff size.

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 the subsequent 4 years.[28] Finally, to facilitate comparisons with the earlier literature, I consider workers between age 25 to 55 with at least 5 years of tenure.

To create a control group for displaced workers, I sample all workers from establishments which did not carry out a mass layoff.[29] 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).

[28] 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.8b shows 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 an upward bias. Long-run losses are virtually unchanged.

[29] 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).

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

$$y_{it} = \gamma_t + \alpha_i + \pi_k + \sum_{k=-5}^{10} \delta_k D_{it}^k + u_{it} \tag{6}$$

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

The black solid line in Figure 5 shows the difference in earnings by pooling workers displaced 2005-2015 along with their matched non-displaced workers. 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, not conditioning on the control group being employed in $t > 0$ (as in e.g. Schmieder, von Wachter and Heining, 2023).[30]

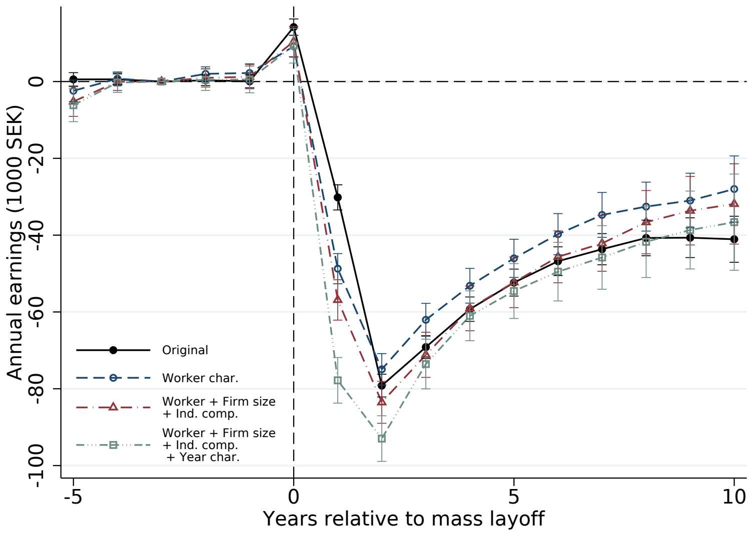
Initial earnings losses of displaced workers amount to a little more than 79 thousand SEK two years after the layoff event which corresponds to about 27 percent of pre-displacement income. As time goes by, displaced workers recover some of their initial losses but even 10 years after displacement the earnings differential between displaced and non-displaced workers are on average about 41,000 SEK (14 percent).[31] 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., 2024; Seim, 2019; Eliason and Storrie, 2006).

In Appendix A, I conduct several robustness checks on the mass layoff analysis. Figure A.8 displays the results. The estimates are virtually unaffected by lowering the worker-tenure restriction to three years or by excluding plant closures. Including non-notified workers as displaced reduces the initial earnings losses, while the long-run losses remain unchanged. I also examine a sample of workers aged 25–50 with at least three years of tenure, employed at firms with at least 30 workers. This restriction slightly reduces the earnings losses during the first two years following displacement, but the longer-run losses are unchanged.

[30] Some studies choose condition on the control group staying employed throughout the entire sample period (see e.g. Lachowska et al., 2020). However, as noted 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. Figure A.7 in Appendix A shows that imposing this restriction amplify earnings losses by about 50% in the long-run.

[31] Figure A.9 shows the earnings losses, defined in percentage terms relative to the average worker in $t = 0$ ($(y_t - \mathbb{E}[y_0])/\mathbb{E}[y_0]$), and provides a side-by-side comparison with the IV estimates from the RD design (Table 4).

Figure 5: 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 (6) along with 95 percent confidence intervals where standard errors are clustered at the individual level. The solid black line show earnings losses of displaced workers compared to non-displaced not conditioning on the control group being employed in $t > 0$. 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.

5.1.2 Reweighting mass layoff estimates

As seen in Table 1, the LIFO-sample used in the RD analysis differ substantially from a standard mass layoff sample. 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 somewhat in industry composition. In order 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.[32] This amounts to estimating a probit regression in data combining the two samples. The dependent variable is an indicator for belonging to the LIFO-sample which is regressed on several sets of covariates such as worker characteristics, industry composition, firm size and year of notification. From this regression, I obtain a propensity score $\hat{p}(x)$ which I use to reweight the mass layoff sample by $\varphi(x) = \hat{p}/(1-\hat{p})$.

Figure 5 plots the estimated earnings gap when applying DFL-reweighting, 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. 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

[32]This analysis is very much inspired by Illing, Schmieder and Trenkle (2024) who use the same procedure to study the post displacement gender earnings gap.

displaced and non-displaced workers 10 years after notification, amounting to 24 thousand SEK. The red line in Figure 5 weights in addition to worker characteristics on the log of firm size and industry composition whereas the green line also reweight on year of layoff to hold constant overall economic conditions. The latter adjustment increases 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 thousand SEK, 10 years after notification.

Importantly, by aligning with the literature and restricting the mass layoff sample to consist only of high-tenure workers, it becomes difficult to achieve balance between the two samples on individual covariates, even after reweighting. In Table A.4, I evaluate the reweighting procedure by reporting summary statistics for the LIFO sample, the mass-layoff sample, and the reweighted mass-layoff sample, along with the standardized mean difference (SMD) for each covariate. Although the SMDs typically become substantially smaller after reweighting, potentially meaningful differences in age and tenure remain. Nevertheless, in Appendix A, I show results from redoing the analysis while including in the mass-layoff sample all workers with at least one year of tenure. While the reweighted sample more closely mimics the LIFO sample (see Table A.5), this adjustment does not materially affect the estimated long-run earnings losses (see Figure A.10).

Overall, the 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 attributed to differences in sample composition. Even after adjusting for these compositional differences, permanent earnings losses remain evident ten years after layoff, suggesting that mass layoffs — unlike smaller redundancies — generate persistent earnings losses for reasons other than sample composition.

5.2 Event study vs. RD estimator

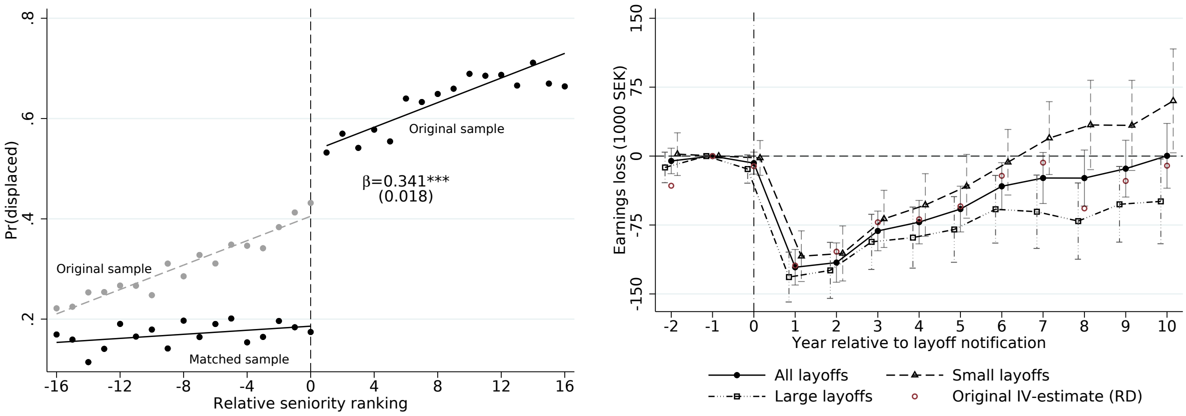
A key difference between the canonical mass layoff estimator and the RD is that the former exploits variation across establishments and the latter within establishment. Thus, the two estimators focuses on two potentially very different experiments. In the LIFO analysis, treatment is defined as being displaced *conditional* on working at a distressed firm. In contrast, the traditional estimator conflates the effects of displacement *and* firm distress, as its control group comprises workers in relatively 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 2b and Figure A.6a), 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 is that the traditional estimator identifies the treatment effect for the average displaced worker (ATT), whereas the RD analysis identifies the effect on the marginally displaced worker (LATE).

In an effort to align the two estimators, I combine the two approaches by matching workers

Figure 6: 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: The figure shows in panel a) 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 3). 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, earnings in year before notification, educational attainment FE:s as well as order circuit FE:s. Standard errors are clustered at the order circuit level.

below the threshold to workers at more stable firms (similar to the typical control group used in the literature) finding their "identical twin" using a nearest-neighbor algorithm.[33] 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.7) 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.8). This combined estimator thus relies on across establishment variation and would then capture any additional effect of working at a distressed firm while also identifying the LATE parameter, pertaining to the marginally displaced worker.

Figure 6a 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 2a). 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 6b where I plot IV-estimates having instrument displacement with the indicator for being above the threshold, again con-

[33]Note that if one were to match workers above the threshold, this would be equivalent to using the standard mass layoff control group and thus would not provide estimates for the marginal worker.

trolling 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 point estimates from Table 3 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 recover over time with the earnings gap turning insignificant 7 years after notification and being more or less closed after 10 years.[34]

A novel feature of this new estimator is that it, in contrast to the previous literature using mass layoffs as an instrument for displacement, relies on the LIFO rule which enables the study of smaller sized layoffs. The dashed lines in Figure 6b splits the sample into small and large layoffs by the median, implying large layoffs have at least 19 percent of its workforce laid off (average 31 percent). 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 very 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). Notably, both in terms of the magnitude and the stagnation of the recovery pattern, the LATE estimates are — from an empirical perspective — strikingly similar to the ATT estimates recovered using the standard approach in section 5.1.[35] This suggests that neither differences in target parameters (LATE vs. ATT), nor the choice of control group (within or across establishment) can 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.

5.3 Earnings losses by size of layoff

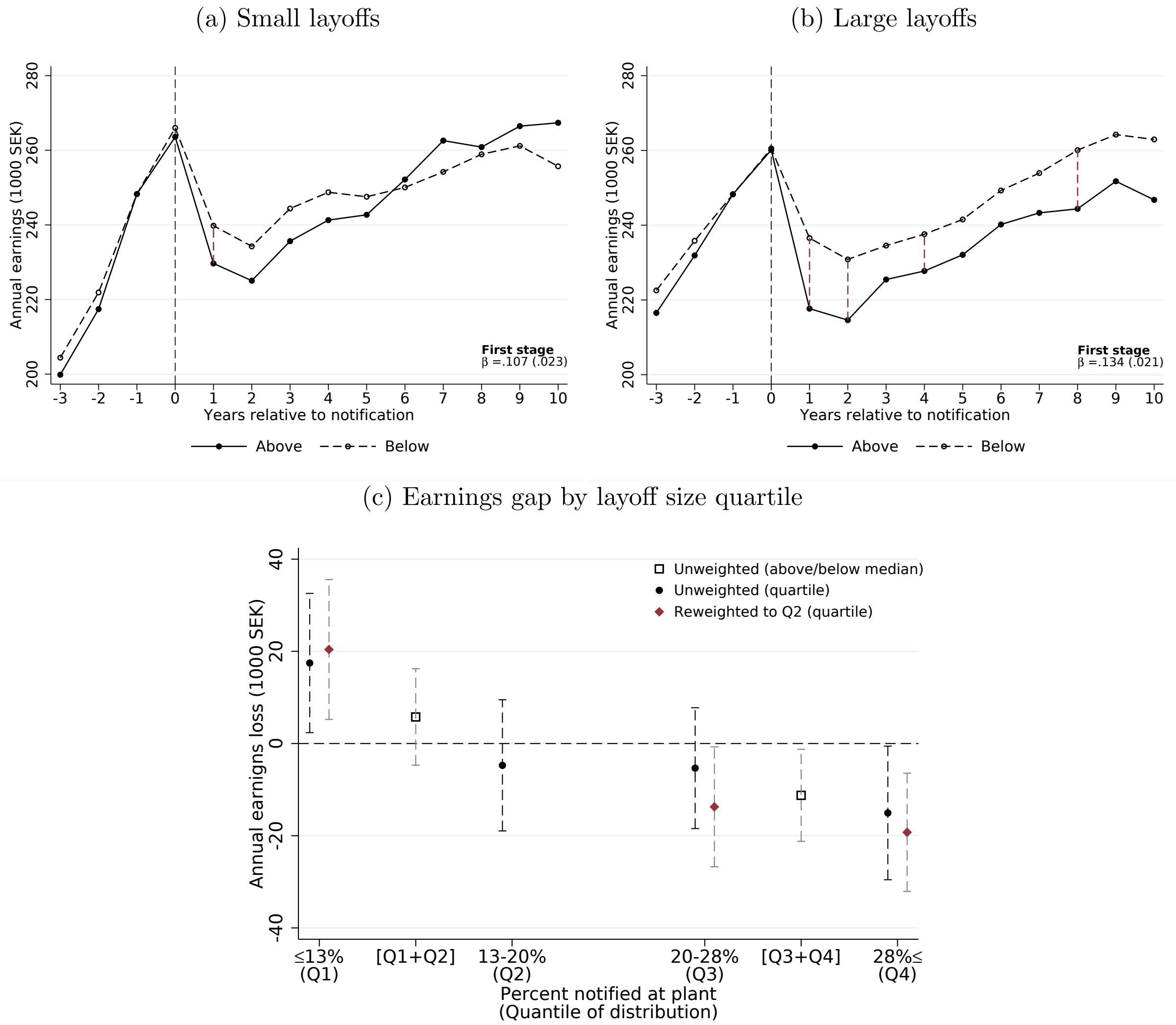
If larger layoffs do render more severe consequences for workers, the pattern should also be visible within the LIFO sample, where both the target parameter and sample restrictions are the same. As such, I use the LIFO thresholds and estimate earnings losses using the RD, splitting layoffs by size.[36]

[34] I have also conducted a robustness analysis, also including firm size in the matching to account for potential differences firm wage trajectories. Figure A.11a in Appendix A displays the results, showing virtually identical results as without matching on firm size. In addition, Figure A.11b plots the evolution of earnings in the matched control group (with and without firm size) and the control group in the original sample.

[35] Figure A.12 plots a comparison between these estimates. 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 5).

[36] 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. Figure A.13a shows the distribution of layoff size in the sample.

Figure 7: 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 age, age squared, gender, immigrant status, earnings in year before notification, educational attainment FE:s as well as order circuit FE:s. Standard errors are clustered at the level of the order circuit.

Figure 7a and 7b 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. 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 have closes the earnings gap after 6 years and thereafter earn more than their former coworkers, workers laid off in large layoffs lack the same recovery and earn on average 13,000 SEK less than their former coworkers 7 to 10 years after notification. Noteworthy, is that the difference seem to occur due to workers above the 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 some precision on these estimates, Figure 7c plots the long-run earnings gap separately for small and large layoffs, pooling years 7–10 after notification (hollow squares). This indicates that workers above the threshold who are displaced in large layoffs earn on average 11,000 SEK less than their former coworkers just below the threshold, significant at the 10 percent level ($p = .065$). The corresponding IV-estimate implies that displaced workers earn about 70,000 SEK less than their non-displaced former coworkers. In small layoffs on the other hand, I find no significant difference in long-run earnings between workers just above and below the threshold. The difference in earnings losses between small and large layoffs is estimated to 16,844 SEK and barley significant at the 5 percent level ($p = .055$).

Probing this pattern further, Figure 7c also show estimated earnings losses splitting the sample into quartiles of size (black circles).[37] 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 ($p = .056$). The difference in estimated earnings gap between Q1 and Q4 is also statistically significant and estimated to 32,320 SEK ($p = .010$). To examine the extent to which this correlation is driven by compositional differences across layoffs, the red circles show the estimated earnings losses after adjusting for differences (relative to workers in Q2) in worker and industry composition, as well as in the timing of the layoff, using the DFL reweighting scheme described in Section 5.1.2. This renders a slight increases of the long-run earnings gap for workers displaced in large layoffs, turning the estimates significant at the 10 and 5 percent level for Q3 and Q4, respectively.

[37]In Appendix A, Figure A.14 show the negative correlation between estimated earnings losses and size of layoff even more granularly and Figure A.15 shows the first stage estimates for each layoff size quartile.

6 Sources of permanent earnings losses in mass layoffs

Why do larger layoffs lead to greater and more persistent earnings losses? On the one hand, at the individual level, larger layoffs may weaken negative signals about worker productivity, thereby dampening subsequent earnings and employment losses (Gibbons and Katz, 1991). Conversely, larger layoffs may destroy more human capital if they occur disproportionately in industries with substantial firm-specific human capital or among workers whose characteristics predict weaker labor market trajectories. At the aggregate level, mass layoffs are more common during periods of macroeconomic distress, and workers displaced in recessions suffer larger earnings declines (Davis and von Wachter, 2011; Schmieder, von Wachter and Heining, 2023). However, as shown above, these channels—while informative about the overall costs of job loss—do not account for the *difference* in long-run earnings losses between mass layoffs and smaller redundancies as the gap persists even after conditioning on worker composition, industry characteristics, and macroeconomic conditions.

A remaining possibility is that differently sized layoffs generate distinct post-displacement adjustments that themselves contribute to long-term earnings divergence. To explore this, Table 4 reports IV estimates in which I separately estimate the discontinuity for small and large layoffs, reweighting the latter to match the worker and industry composition, as well as the timing, of smaller layoffs.[38] The first row of Table 4 confirms the earlier pattern: short-run earnings losses are similar across small and large layoffs, but only workers displaced in large layoffs experience substantial permanent losses. Square brackets report p-values for equality tests across the two estimates.

What stands out is that workers displaced in large layoffs face substantially higher non-employment risk in both the medium and long run. They are also more likely to register as unemployed at the PES, with this difference becoming statistically significant across layoff sizes in the long run. As expected, displaced workers are more likely to switch industry and occupation than their non-displaced former coworkers, but these adjustments do not vary systematically with layoff size. There is also suggestive evidence that workers displaced in large layoffs are more geographically mobile: their probability of working in a different region from where they were displaced is marginally higher ($p = 0.055$), and they are correspondingly less likely to live and work in the same region. In contrast to findings such as those in Schmieder, von Wachter and Heining (2023), I find no significant differences in wages or firm size between displaced and non-displaced workers. However, both variables are highly dispersed to begin with, and partitioning the sample into three groups further reduces precision. These null results should therefore be interpreted with some caution.[39]

[38] Reweighting uses worker age, tenure, immigrant status, educational attainment, and fixed effects for industry and notification year.

[39] I have also examined alternative firm-level outcomes including value added, profits, revenue, and firm and establishment fixed effects estimated using AKM. None of these dimensions exhibit significant differences either between displaced and non-displaced workers or across layoff sizes.

Table 4: Labor market outcomes by size of layoff

	Short-run (1-3 years)		Medium-run (4-6 years)		Long-run (7-10 years)	
	Small layoff (1)	Large layoff (2)	Small layoff (3)	Large layoff (4)	Small layoff (5)	Large layoff (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, separate 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 observed at maximum 6 years post notification due to occupational codes being recorded 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. The estimates for large layoffs have been reweighted on worker age, tenure, immigrant status, level of education as well as industry and notification fixed effects, such that it mimics the composition in small layoffs. 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 coefficients 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.

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 correlating with longer spells of subsequent joblessness and an increase in the probability work in another region. One potential interpretation of these findings, in line with Gathmann et al. (2018), could be that large layoffs have an impact on the entire local labor market, prolonging non-employment and making workers more prone to find work elsewhere.

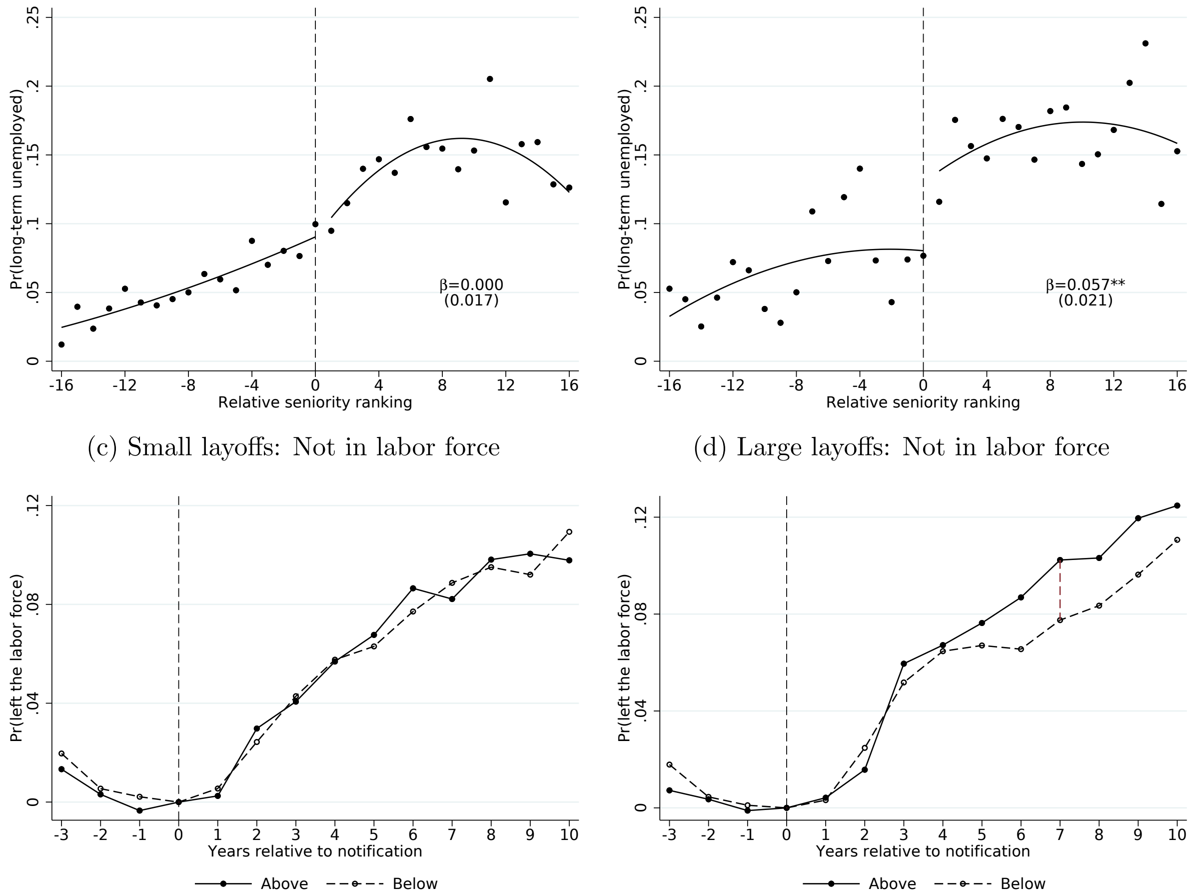
Figure 8: Unemployment and leaving the labor force by size of layoff

(a) Small layoffs: Long-term unemployment

(b) Large layoffs: Long-term unemployment

(c) Small layoffs: Not in labor force

(d) Large layoffs: Not in labor force



Notes: Panel a) and b) show the probability of becoming long-term unemployed following the year of 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 whereas long-term unemployed is defined as being registered as unemployed at least 180 days. Figures in the lower panel comes estimating equation (3) separately for each time point, where 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. 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 age, age squared, gender, immigrant status, earnings in year before notification, educational attainment FE:s as well as order circuit FE:s. Standard errors are clustered at the level of the order circuit.

6.1 The role of non-employment

Recent work by Schmieder, von Wachter and Heining (2023) and Fallick et al. (2021) emphasize the role of non-employment duration in explaining earnings losses following 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, if mass layoffs causes longer spells of joblessness this could potentially explain why earnings losses become permanent.

The top panel of Figure 8 shows the probability of becoming long-term unemployed in the year following notification separately estimated for small *vis-a-vis* large layoffs where again large layoffs have been reweighted to mimic small layoffs. It shows that workers in large layoffs who are just above the threshold have a 5.7 percentage point higher probability of becoming long-term unemployed following displacement, compared to their coworkers just below the threshold. Interestingly, there is no indication of a discrete jump at the threshold in the incidence of long-term unemployment among workers displaced in smaller layoffs.

The bottom panel of Figure 8 shows the evolution of workers having left the labor force, relative to time of notification. Also here, there are no discernible differences between workers above and below the threshold in small layoffs while for large layoffs, workers above the threshold are between 2-2.5 percentage points more likely to have left the labor force 6–10 years after layoff. The estimated difference in Figure 8d is significant at the five percent level in period $t+7$ and at ten percent level in periods $t+6, t+8$ and $t+9$. Again, it is worth emphasizing that these estimates are reweighted to match the worker composition among workers in smaller layoffs, in particular on age and tenure.[40]

These findings suggests that indeed workers displaced in large layoffs are exposed to more joblessness compared to workers displaced in smaller layoffs. This result is very much in line with recent evidence in the literature. First, Schmieder et al. (2023); Fallick et al. (2021) show how subsequent non-employment duration following displacement plays a key role in accounting for the magnitude and persistence in worker earnings losses. Second, Flaaen et al. (2019) find that earnings losses post displacement are only one-third the size when excluding periods with zero. Finally, Rose and Shem-Tov (2023) find that for low wage workers, the majority of long-run earnings following displacement are accounted for by reduced employment. To the best of my knowledge, my findings that large layoffs — as opposed to smaller ones — lead to longer periods of joblessness and causing discouragement among workers, thereby prompting their withdrawal from the labor force, are new to the literature.

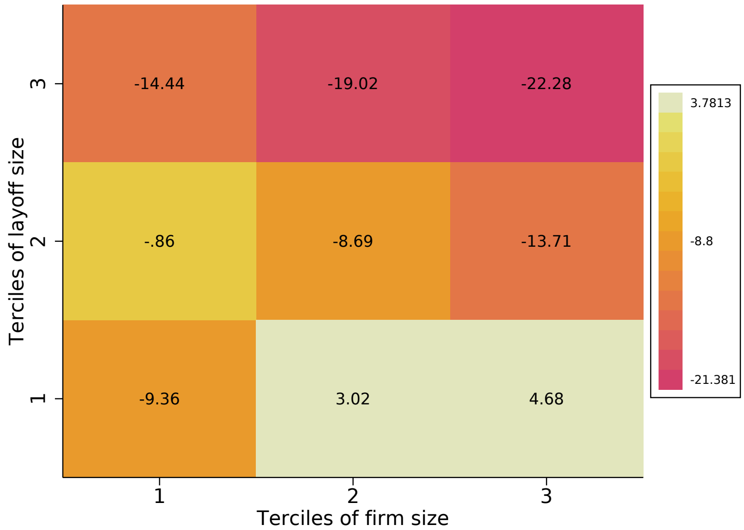
6.2 The role of firm size

The analysis have so far shown that the size of layoff, defined as the ratio between the number of notified workers and number of employees, is highly predictive of the long-run consequences of displacement in terms of earnings losses and non-employment. A natural question that arise is: What is the role of the denominator, i.e firm size? Previous studies (see e.g. Fackler et al., 2021; Schmieder et al., 2023) finds that the loss of firm-specific wage premiums, which are typically strongly positively correlated with employer size, is an important factor in explaining earnings losses following displacement.

Fackler et al. (2021) provides evidence of a firm size gradient in the cost of displacement: studying job loss due to firm bankruptcies, they show that workers incur small losses if displaced from a small firm and vice versa. An important limitation of these results is the inability to separate between the role of firm size and the size of the layoff; which are synonyms in the case

[40] Whereas the upward trend is driven by workers being near retirement age, the difference remain roughly the same when restricting the sample to workers age 18–55 (see Figure A.16 in Appendix A). This is indeed expected as age is balanced across the threshold.

Figure 9: Long-run earnings losses by layoff and firm size



Notes: The figure shows the average earnings difference between workers just below and above the LIFO-threshold by firm and layoff size. The averages come from estimating long-run earnings differences at the threshold within each order circuit and grouping these by firm size × layoff size cells and taking the average.

of bankruptcies (i.e plant closures). This distinction, however, is important as a key prediction of loosing out on a firm wage preima is that a worker displaced from a large firm incur equal earnings and wage losses irrespective of whether (s)he is laid off alone or alongside several of her coworkers. In contrast, other mechanisms may be in play when considering the size of the layoff; such as large layoffs causing congestion in the local labor market with multiple workers displaced from the same plant, sharing the same network and similar set of skills, thus competing for the types of jobs. Moreover, large layoffs have been shown to have spillovers onto other firms (in particular in the same industry), further decreasing labor demand (Gathmann et al., 2018), thereby amplifying the negative effects of displacement.

To differentiate between firm and layoff size, I estimate the difference in long-run earnings for workers just above and below the threshold separately within each order circuit and group the estimates into terciles of the layoff and firm size distribution. Figure 9 shows the average earnings losses within each layoff×firm size cell. Focusing on the second and third tercile in the firm size distribution (columns 2 and 3), there is a clear layoff size gradient with workers displaced from larger layoffs having significantly larger long-run earnings losses. In the lowest firm size tercile (column 1) there is also a negative relationship, albeit non-monotonic.[41] Conversely, focusing on the top two terciles in the layoff size distribution (row 2 and 3), there is an apparent firm size gradient, echoing the findings of Fackler et al. (2021). In the lowest layoff size tercile, however, there is no evidence of a firm size gradient. On the contrary it appears as, if anything, the relationship is positive. Interestingly, the average gradient in layoff size is roughly an order of magnitude larger than the gradient in firm size. The layoff size gradient, averaged over the

[41] It should be noted, however, that the 1:1 cell is imprecisely estimated due to being, by far, the smallest cell as it contains both the smallest firms and layoffs. It is identified off of 616 workers within 22 order circuits which corresponds to about half of the size of the second smallest cell 3:3).

three firm size cells is -9.02 whereas the equivalent firm size gradient, averaged across the layoff size cells, is -1.1. These findings suggest that, although firm size may play some role in shaping post-displacement earnings losses, the size of the layoff appears to be the more decisive factor.

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 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 a 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 vastly 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, Schmieder et al. (2023) and Fallick et al. (2021) who show that workers non-employment duration plays a key role in accounting for the persistence in wage and earnings losses upon displacement. The findings are also congruent with those of Burdett, Carrillo-Tudela and Coles (2020) and Jarosch (2023) on how displacement render longer and serially correlated unemployment spells where workers lose human capital.

This paper sheds new light on how workers are affected by job loss and whether displacement leaves lasting scars or merely temporary blemishes. While the former appears to hold for workers displaced in large layoffs, the vast majority of job losses occur in smaller layoffs, for which workers eventually recover from the initial shock. These findings therefore raise important questions about the generalizability of evidence drawn from 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 type and size of a layoff influence worker outcomes. These results also has important implications for public policy, suggesting that focus and resources should be geared towards workers displaced in large layoffs.

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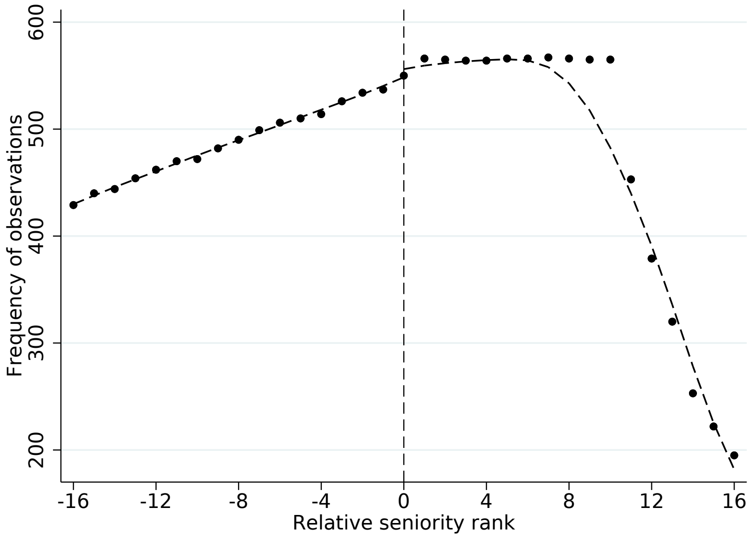
Appendix – For Online Publication

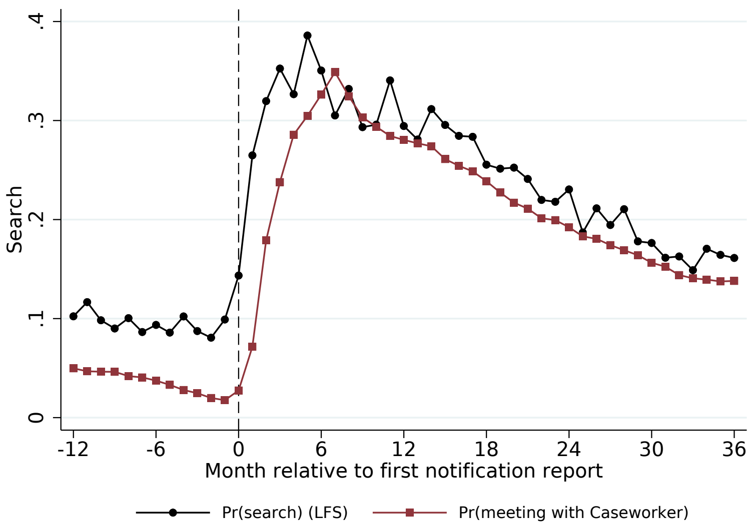
A Additional Figures and Tables

Figure A.1: Frequency of observations around the threshold



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

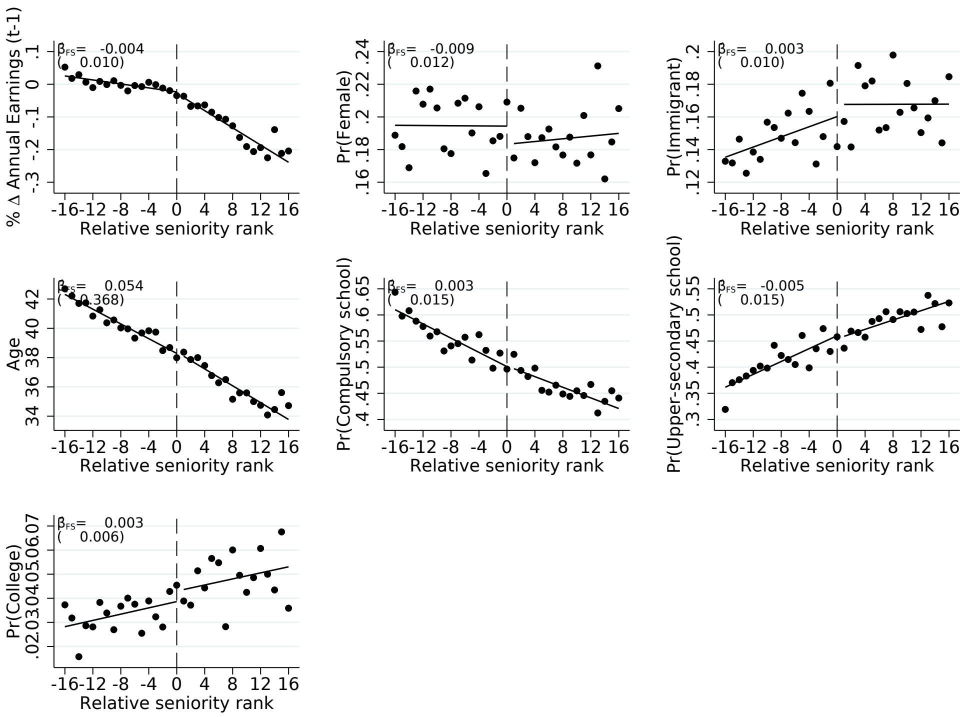
Figure A.2: Evolution of job search relative to time of notification



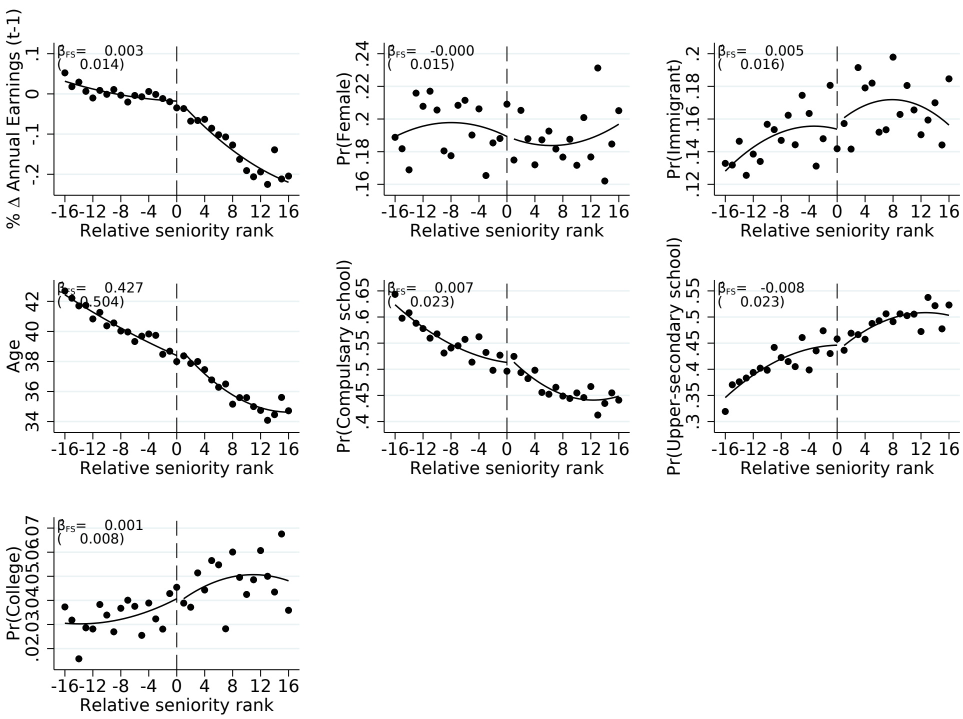
Notes: The figure shows the probability of reporting having looked for a job during the previous four weeks (black line) and the probability of having met with a caseworker at the PES, relative to the month a layoff notification is sent in to the PES. The sample consist 225,235 workers notified of layoff in the PES-registers between 2005–2015, excluding bankruptcies and plant closures. These data are then matched with data on self-reported job search comes from the the Swedish labor force survey which contains information on individuals' labor market status and job search for a 0.4% sample of the population age 15–74. Caseworker meetings are registered in the PES unemployment register which covers the entire unemployed population.

Figure A.3: Balancing of individual covariates

(a) Linear fit

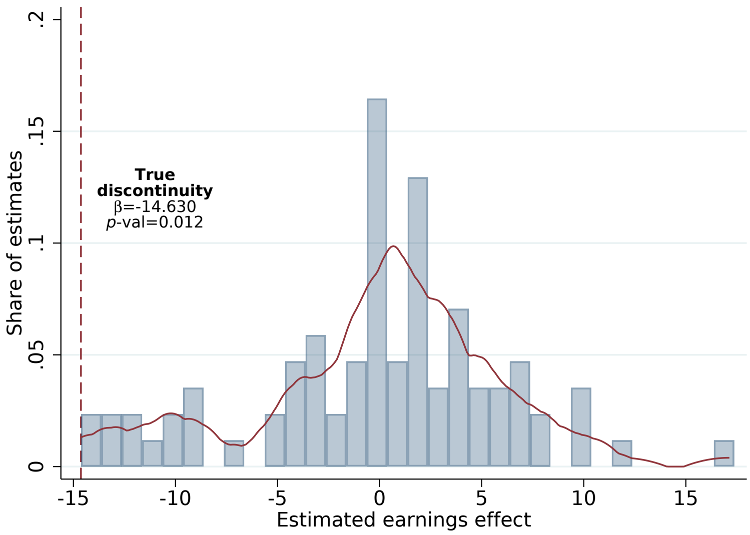


(b) Quadratic fit



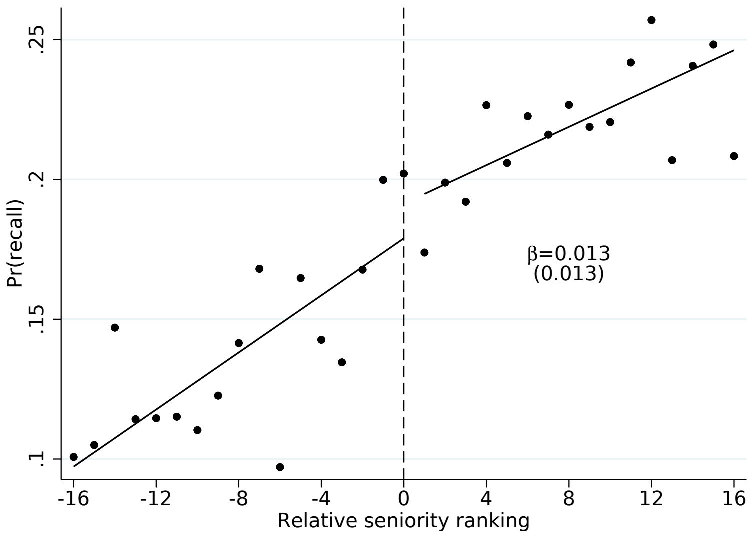
Notes: The figures show balancing at the threshold of pre-determined worker characteristics using a a) first order and b) second order polynomial function interacted with the threshold as well as order circuit fixed effects and is run using a bandwidth of ±16. The point estimate of the jump at the threshold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

Figure A.4: Permutation test of annual earnings



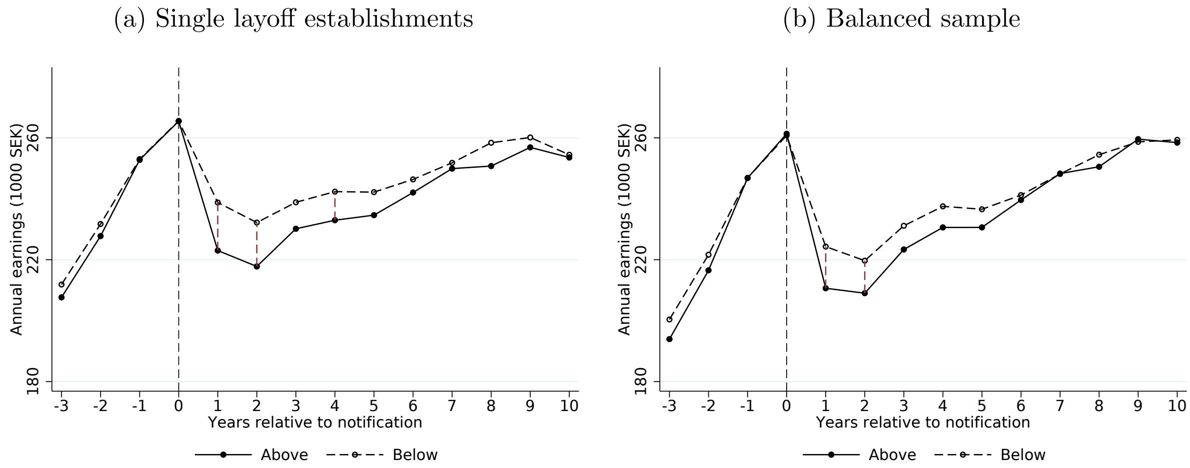
Notes: The figure shows the distribution of estimated effect for the a) first-stage and b) annual earnings in $t+1$ for 85 different permutations of the RD-regression. For each permutation I vary the LIFO cut-off to values of the running variable between -56 and +28. Each regression, comes from estimating 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.5: Probability of recall



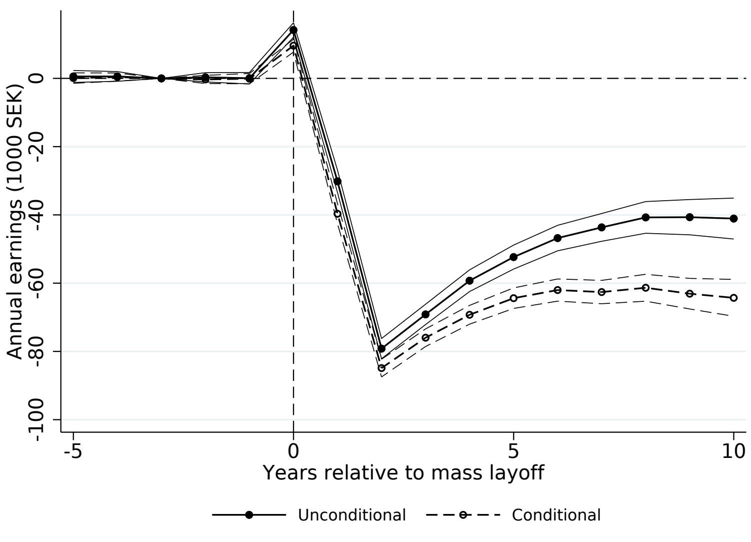
Notes: The figure shows the probability of recall as a function of a workers relative ranking within an order circuit (in discrete bins), normalized to zero at the threshold. Recalled is defined as having left the notifying within within 15 months after layoff notification and after that coming back 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. Regressions control for the baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s. 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.6: Robustness of evolution of annual earnings relative to year of notification



Notes: The Figure shows a) annual earnings relative to the year of notification for establishments with only one layoff notification during the sample period and b) for a balanced sample (notified prior to 2009). 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.

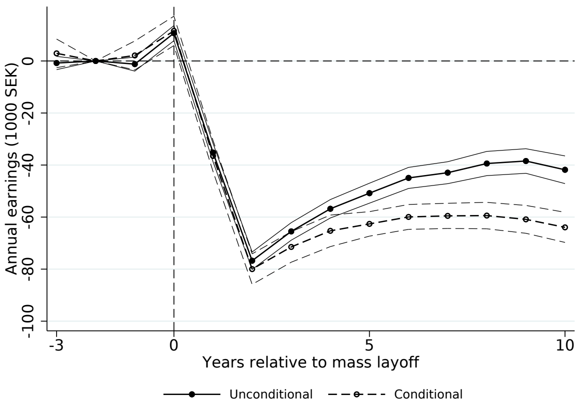
Figure A.7: Earnings losses upon mass layoff



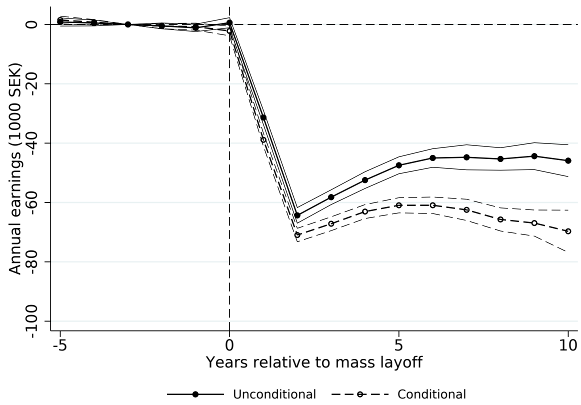
Notes: The figure shows the difference of annual earnings between displaced and non-displaced workers by years relative to a mass layoff event where earnings is normalized to zero at $t-3$. Displacement is defined as a worker being notified and leaving the plant in year $t+1$ or $t+2$ during a mass layoff event. The plotted estimates are δ_k from equation (6) 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 A.8: Robustness of mass layoff earnings estimates

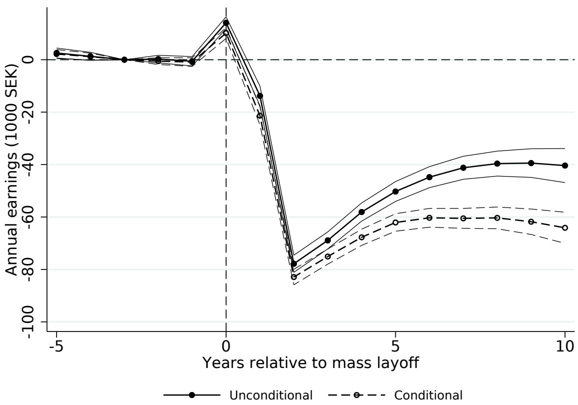
(a) Workers with > 3 years of tenure



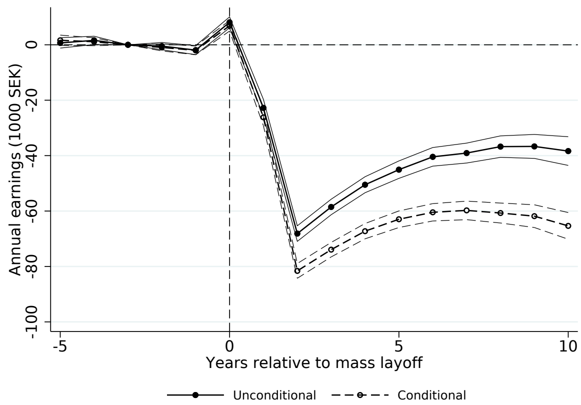
(b) Including non-notified workers



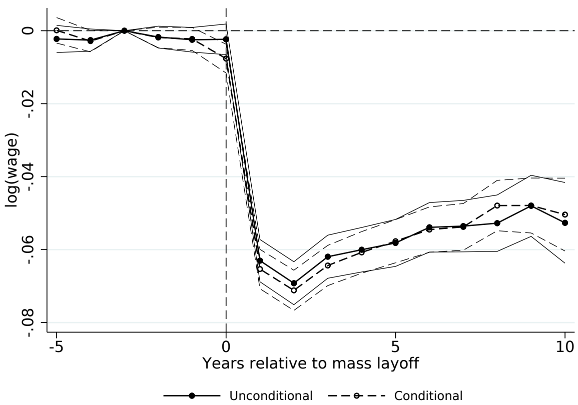
(c) Excluding plant closures



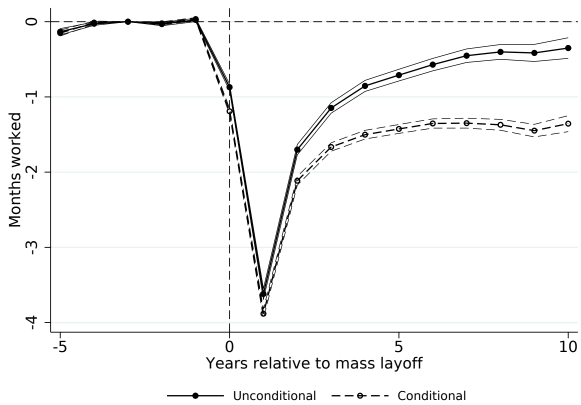
(d) Age 25-50, Tenure> 3 years, firm size > 30



(e) Wage losses

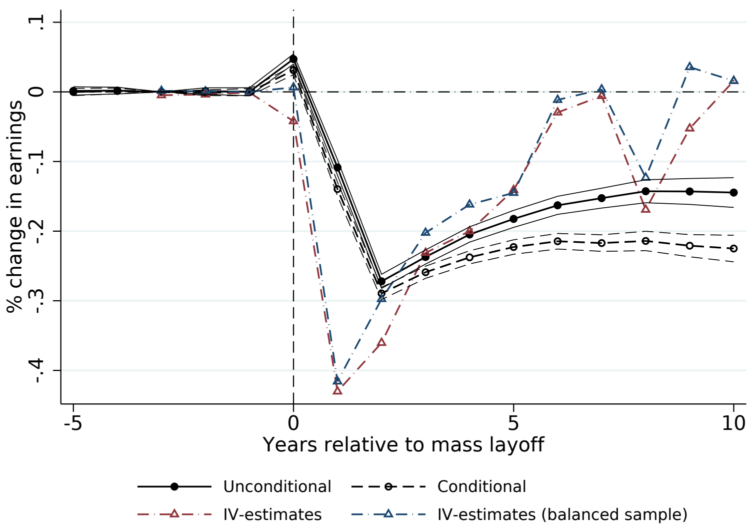


(f) Months worked



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 and panel d) shows estimates from a sample of workers age between 25–50, with a minimum of 3 years of tenure at the firm which employees at least 30 workers. Panel e) plots the differences in wages between displaced and non-displaced workers whereas panel f) plots the difference in months worked during a given year. All plotted estimates are δ_k from equation (6) 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 percentage terms

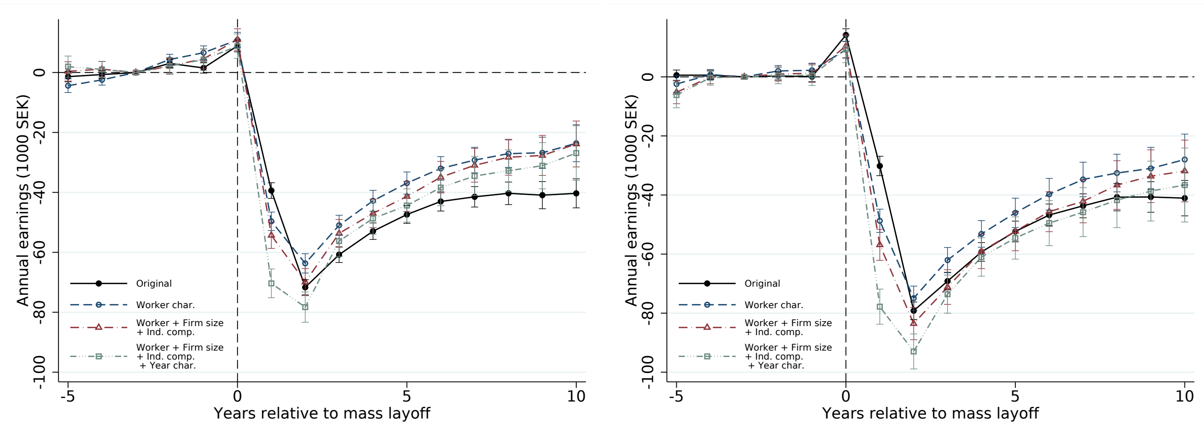


Notes: The figure shows the percentage change in the difference of annual earnings between displaced and non-displaced workers by years relative to a mass layoff event where earnings is normalized to zero at $t-3$. For the mass layoff estimates (black lines), displacement is defined as a worker being 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 (6) 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. The red and blue lines show for comparison the IV-estimates from the RD-analysis. These 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 and previous earnings as well as order circuit FE:s. For more details on estimation of the IV, see section 4.

Figure A.10: Robustness to reweighting mass layoff estimates

(a) Workers with ≥ 1 year of tenure

(b) Workers with ≥ 5 year of tenure

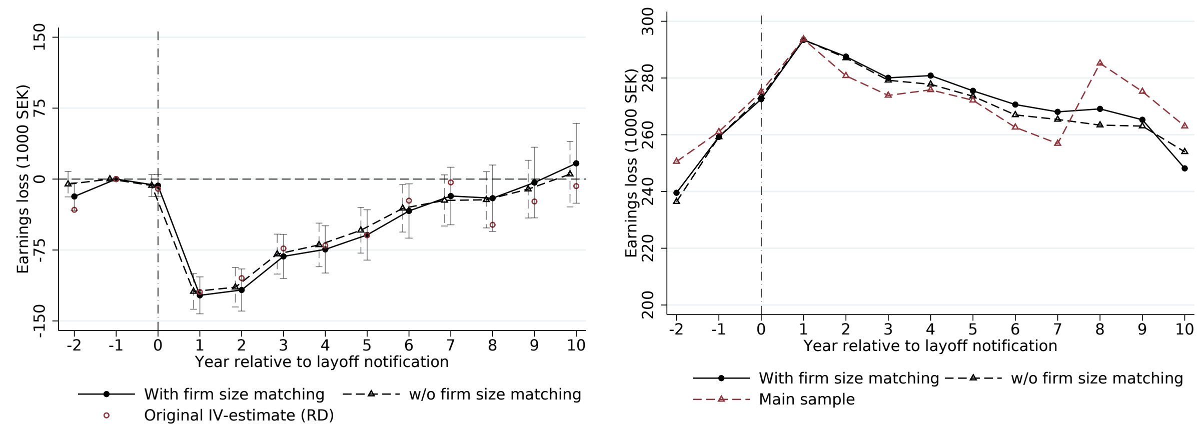


Notes: The figures show the difference of annual earnings between displaced and non-displaced workers by years relative to a mass layoff event where earnings is normalized to zero at $t-3$. In panel a) the sample consists of workers with a minimum of one year of firm tenure, whereas in panel b) the sample is workers with tenure of at least 5 years at the firm. 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 (8) in the paper, along with 95 percent confidence intervals where standard errors are clustered at the individual level. The solid black line presents unweighted estimates. The dashed, blue, red and teal line reweight 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 A.11: Sensitivity to matching on firm size in control group

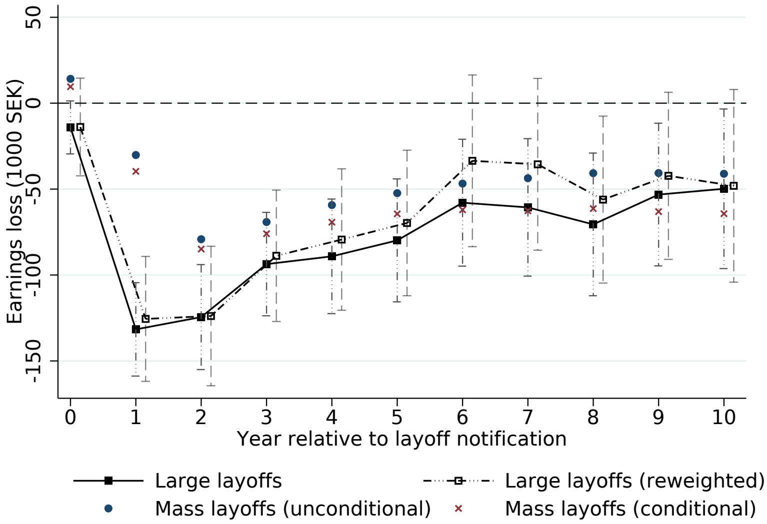
(a) IV-estimates of earnings losses by year relative to notification

(b) Evolution of earnings in control groups



Notes: The figure shows in panel a) 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 while matching the control group also on firm size control group whereas dashed line does not. The red hollow circles show the original IV-estimates for comparison (see Table 3). The dashed lines show the same estimates but for small (hollow triangle) and large layoffs (hollow square) along with 95 percent confidence intervals. Panel b) shows the constant from these regressions, thereby displaying the evolution of earnings in the control group. 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, earnings in year before notification, educational attainment FE:s as well as order circuit FE:s. Standard errors are clustered at the order circuit level.

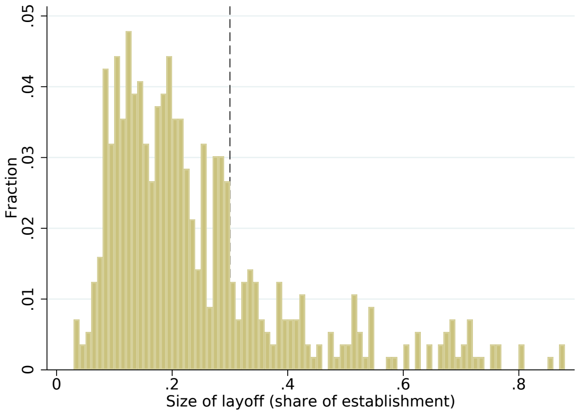
Figure A.12: Earnings losses in using matched sample (reweighted)



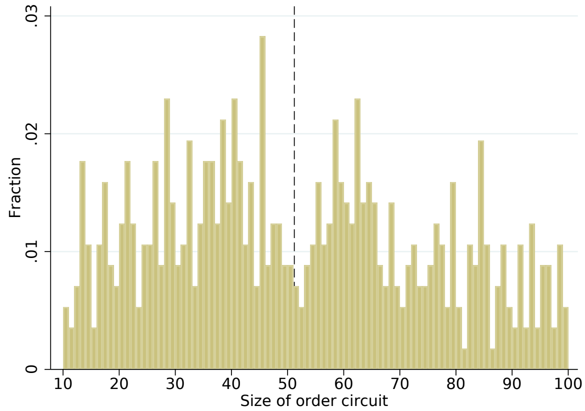
Notes: The figure shows IV-estimates, comparing earnings of displaced and non-displaced workers just above and below the threshold, respectively. The black solid line shows the earnings gap using the matched control group replicating the estimates for large layoffs, replicating the estimates of Figure 6. 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 A.7 showing earnings losses using using the standard mass layoff approach (see section 5.1 for details on estimation).

Figure A.13: Distribution of size of layoff and size of order circuits

(a) Size of layoff

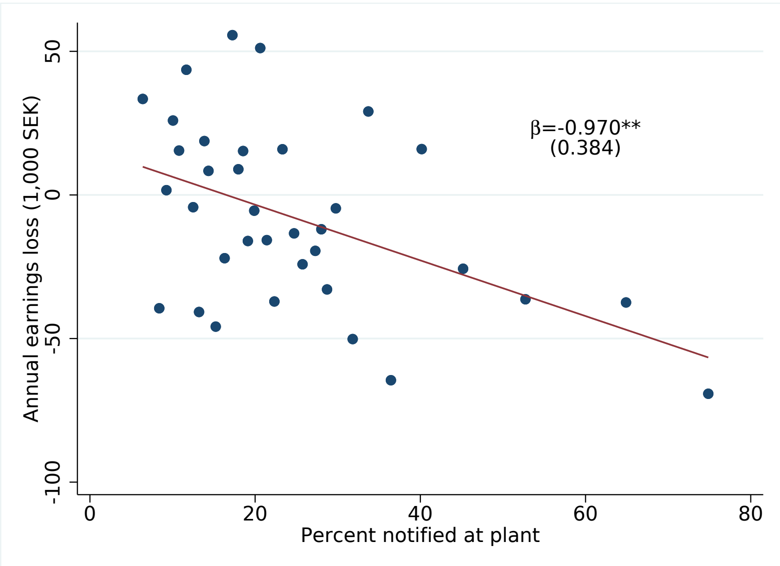


(b) Size of order circuit



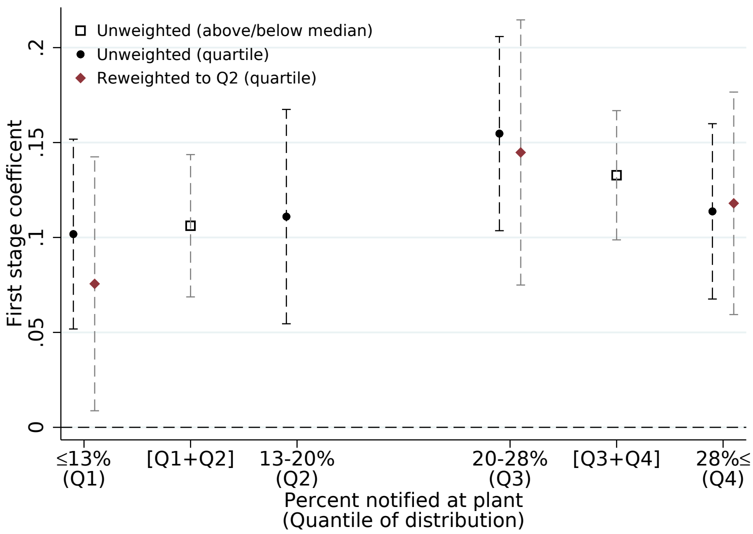
Notes: The figure in panel a) shows the distribution of the size of all layoff notifications in the main estimation sample where size is defined as the number of notified workers divided by establishment size. The dashed vertical line indicates 30 percent of the workforce which corresponds to the standard definition of a mass layoff in the literature. Panel b) shows the distribution of the size of the order circuits used in the main analysis. The vertical line indicates the mean size of 51 workers.

Figure A.14: 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. The 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.15: First stage estimate by size of layoff

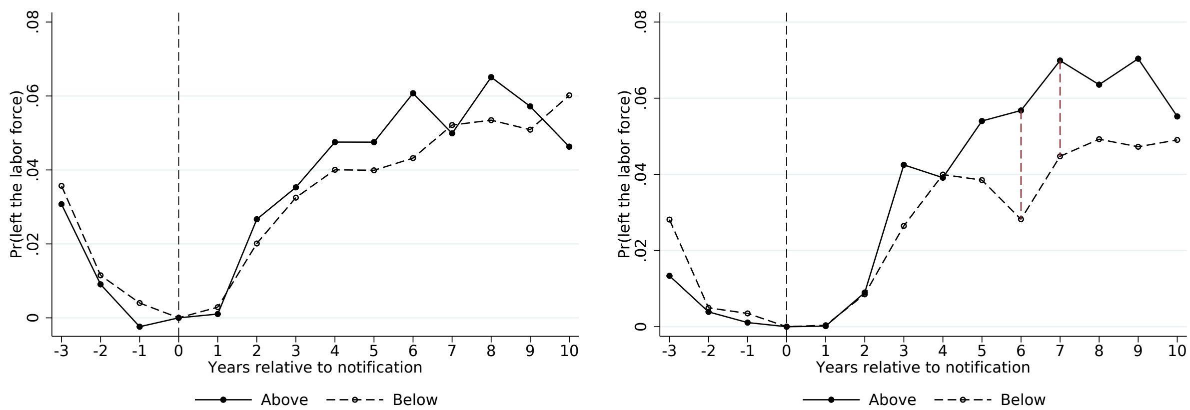


Notes: The Figure shows the first stage effect corresponding to the estimates Figure 7c. All regressions use a bandwidth of ±16 and include a linear polynomial function interacted with the threshold as well as order circuit fixed effects. Included in the regressions are controls for female, immigrant status, age, age squared level of education and earnings prior to notification. Standard errors are clustered at the level of the order circuit.

Figure A.16: Leaving labor force by size of layoff for workers age 18–55

(a) Small layoffs

(b) Large layoffs



Notes: The Figure shows the probability of being not the labor force for workers age 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. 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. All regressions use a bandwidth of ±16 and include a first order 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.

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

Author(s) (year)	Journal	Data: Country (state)	Data: Time	Sample restrictions (main analysis): Tenure (years)	Sample restrictions (main analysis): Gender	Sample restrictions (main analysis): Share laid off	Percent earnings losses: Short-run (years)	Percent earnings losses: 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	$\geq 30\%$	$\cdot$ ($\cdot$)	$\cdot$ ($\cdot$)
Fackler et al. (2021)	JEEA	Germany	2007–2009	≥ 3	Both	100%	26 (1)	9 (5)
Athey et al. (2024)	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. (2024), 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	Compliers	Never-takers	Always-takers
Panel a): *All layoffs*				
Earnings (1,000 SEK)	247.584	246.317	253.457	241.945
	(0.731)	(2.333)	(1.811)	(1.879)
Female	0.191	0.190	0.172	0.213
	(0.003)	(0.010)	(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
	(0.093)	(0.293)	(0.229)	(0.245)
Tenure	6.448	7.509	5.267	6.726
	(0.045)	(0.150)	(0.095)	(0.108)
Compulsory school	0.515	0.543	0.491	0.515
	(0.004)	(0.013)	(0.010)	(0.010)
Upper secondary school	0.445	0.430	0.453	0.450
	(0.004)	(0.012)	(0.009)	(0.010)
College	0.040	0.027	0.056	0.035
	(0.002)	(0.005)	(0.004)	(0.004)
Panel b): *Small layoffs*				
Earnings (1,000 SEK)	239.492	245.035	245.142	226.437
	(1.153)	(3.231)	(3.008)	(2.562)
Female	0.226	0.219	0.202	0.264
	(0.005)	(0.014)	(0.011)	(0.013)
Immigrant	0.179	0.179	0.169	0.192
	(0.004)	(0.013)	(0.011)	(0.011)
Age (years)	36.407	37.319	36.380	35.417
	(0.134)	(0.402)	(0.318)	(0.336)
Tenure	5.298	6.975	3.617	5.455
	(0.059)	(0.191)	(0.100)	(0.138)
Compulsory school	0.454	0.502	0.419	0.442
	(0.005)	(0.017)	(0.013)	(0.013)
Upper secondary school	0.498	0.480	0.499	0.516
	(0.006)	(0.017)	(0.015)	(0.013)
College	0.048	0.018	0.082	0.042
	(0.002)	(0.007)	(0.008)	(0.006)
Panel c): *Large layoffs*				
Earnings (1,000 SEK)	255.742	246.798	260.466	259.167
	(1.034)	(3.304)	(2.328)	(2.587)
Female	0.155	0.161	0.147	0.157
	(0.004)	(0.013)	(0.009)	(0.011)
Immigrant	0.136	0.133	0.141	0.133
	(0.004)	(0.012)	(0.009)	(0.010)
Age (years)	40.304	40.240	39.687	41.083
	(0.135)	(0.442)	(0.304)	(0.364)
Tenure	7.607	8.180	6.659	8.138
	(0.071)	(0.233)	(0.147)	(0.165)
Compulsory school	0.577	0.588	0.551	0.595
	(0.006)	(0.018)	(0.013)	(0.014)
Upper secondary school	0.391	0.379	0.414	0.377
	(0.006)	(0.018)	(0.013)	(0.014)
College	0.032	0.033	0.034	0.028
	(0.002)	(0.007)	(0.005)	(0.005)

Notes: The table shows summary statistics by complier status following Abadie et al. (2002) and using the user-written package `ivdesc` in Stata. Large and small layoffs are split at the median in the distribution. 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 between the two groups. For details on how the two groups are created and matched see section 5.1.

Table A.4: Sample characteristics by size of layoff (high tenured workers only)

	LIFO sample		Mass layoff sample		Mass layoff sample (reweighted)		Standard mean difference	
	Mean	SD	Mean	SD	Mean	SD	Unweighted	Reweighted
Age	38.35	12.06	41.99	8.12	40.59	10.08	-0.355	-0.202
Female	0.19	0.39	0.35	0.48	0.20	0.40	-0.358	-0.033
Tenure	6.00	5.83	12.79	6.69	8.84	5.40	-1.081	-0.505
Immigrant	0.16	0.36	0.14	0.35	0.17	0.38	0.039	-0.040
Compulsory	0.51	0.50	0.52	0.50	0.58	0.49	-0.019	-0.130
Upper-secondary	0.45	0.50	0.38	0.48	0.39	0.49	0.140	0.124
College	0.04	0.20	0.10	0.30	0.04	0.19	-0.234	0.017
log(establishment size)	4.85	0.96	5.03	0.74	4.96	0.73	-0.218	-0.135
Notification year	2009.2	2.20	2010.0	2.97	2009.5	2.35	-0.303	-0.119
Industry shares								
Manufacturing	0.83	0.38	0.61	0.49	0.87	0.34	0.494	-0.114
Construction	0.14	0.35	0.01	0.11	0.09	0.28	0.496	0.171
Transport	0.00	0.06	0.09	0.29	0.01	0.08	-0.425	-0.029
Non-financial services	0.00	0.00	0.11	0.31	0.00	0.00	-0.501	.
Retail	0.00	0.00	0.08	0.28	0.00	0.00	-0.431	.
Other	0.03	0.16	0.08	0.28	0.04	0.19	-0.257	-0.064
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). The last two columns show standard mean differences between the LIFO sample and the mass layoff sample when unweighted and reweighted. The mass layoff sample follows the sample restrictions descibred in section 5.1.

Table A.5: Sample characteristics by size of layoff (including low tenured workers)

	LIFO sample		Mass layoff sample		Mass layoff sample (reweighted)		Standard mean difference	
	Mean	SD	Mean	SD	Mean	SD	Unweighted	Reweighted
Age	38.35	12.06	39.77	8.67	38.76	10.45	-0.136	-0.036
Female	0.19	0.39	0.36	0.48	0.19	0.40	-0.384	-0.011
Tenure	6.00	5.83	8.41	7.07	6.41	5.73	-0.372	-0.070
Immigrant	0.16	0.36	0.17	0.38	0.17	0.38	-0.046	-0.043
Compulsory	0.51	0.50	0.44	0.50	0.54	0.50	0.156	-0.054
Upper-secondary	0.45	0.50	0.40	0.49	0.41	0.49	0.093	0.066
College	0.04	0.20	0.16	0.37	0.05	0.21	-0.416	-0.027
log(establishment size)	4.85	0.96	4.98	0.79	4.89	0.77	-0.152	-0.049
Notification year	2009.23	2.20	2009.97	3.00	2009.33	2.26	-0.282	-0.046
Industry shares								
Manufacturing	0.83	0.38	0.47	0.50	0.83	0.37	0.810	-0.008
Construction	0.14	0.35	0.03	0.16	0.13	0.34	0.420	0.022
Transport	0.00	0.06	0.12	0.33	0.00	0.07	-0.508	-0.010
Non-financial services	0.00	0.00	0.20	0.40	0.00	0.00	-0.708	0.000
Retail	0.00	0.00	0.08	0.27	0.00	0.00	-0.415	0.000
Other	0.03	0.16	0.10	0.30	0.03	0.17	-0.303	-0.023
N	15,795		58,384		41,838			

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). The last two columns show standard mean differences between the LIFO sample and the mass layoff sample when unweighted and reweighted. The mass layoff sample follows the sample restrictions descibred in section 5.1 with the exception of including workers with at least one year of tenure.

Table A.6: 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.7: 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.8: 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.

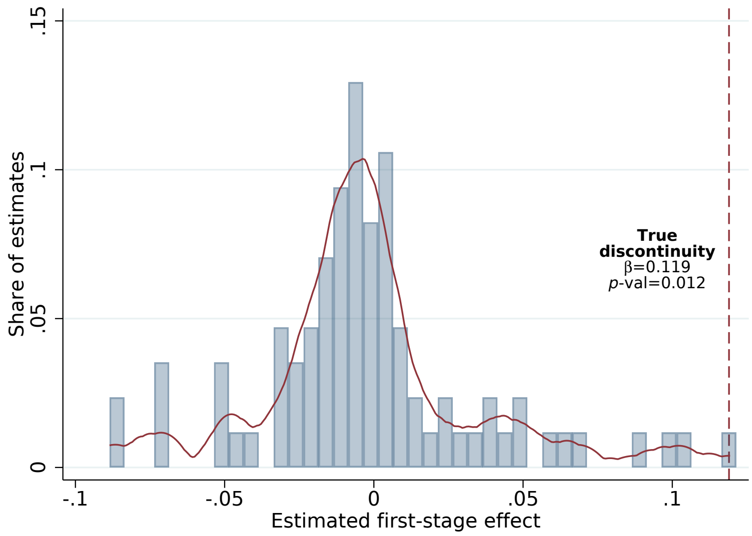
B Robustness of the first stage

Table B.1: 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.

Figure B.1: Permutation test of the first stage

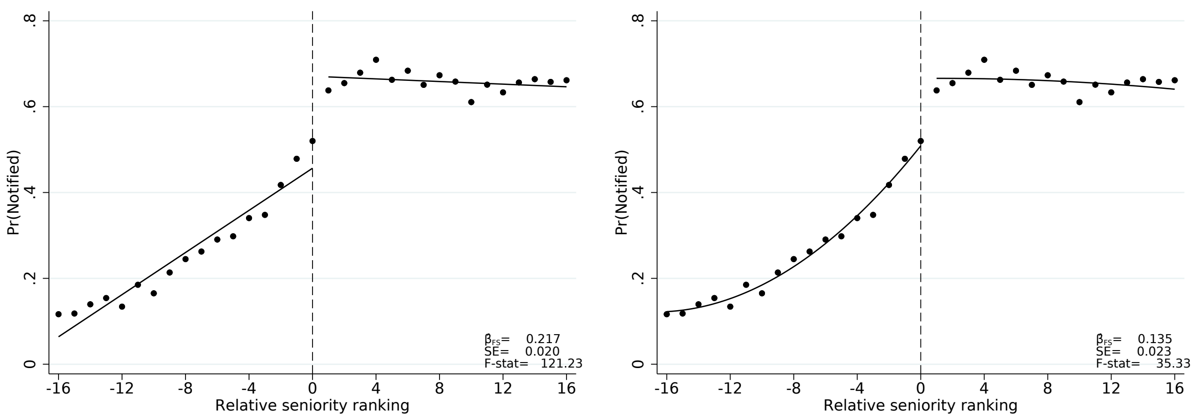


Notes: The figure shows the distribution of estimated effect for the first-stage for 85 different permutations of the RD-regression. For each permutation I vary the LIFO cut-off to values of the running variable between -56 and +28. Each regression, comes from estimating equation (3) with annual earnings 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 B.2: 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.

B.1 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.

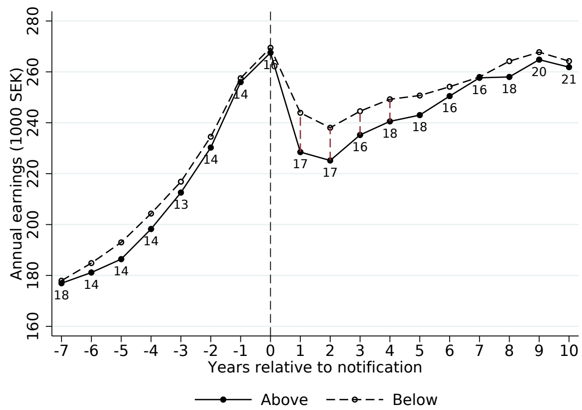
Importantly, as long as there is some information about the true running variable in the proxy that is used, the above listed causes which may generate measurement error does not affect the consistency or causal interpretation of my estimates but only induces noise in the running variable (RR), thereby 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.

C Optimal Bandwidth

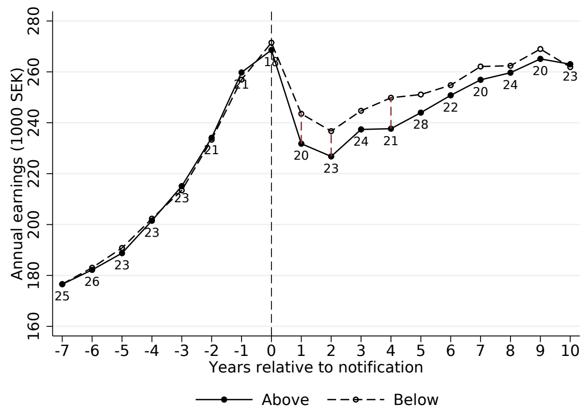
Figure C.1 reproduces Figure 3b using the optimal bandwidth selector suggested by Calonico et al. (2014) for each time point. The upper panel displays estimates without adjusting for pre-determined covariates whereas the bottom panel include the covariates: age, age squared, gender, immigrant status, educational attainment FE:s.

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

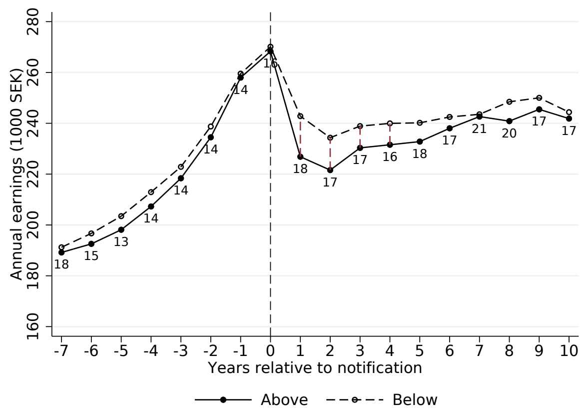
(a) Linear fit without covariates



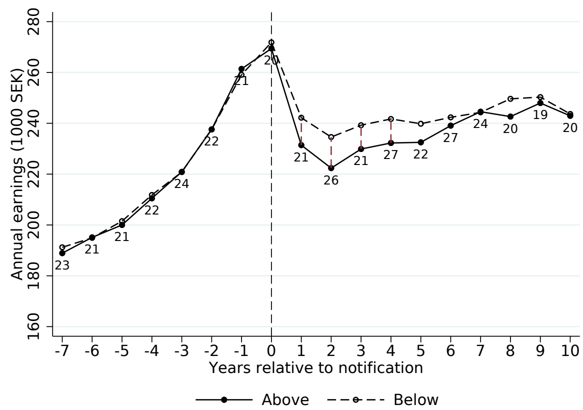
(b) Quadratic fit without covariates



(c) Linear fit with covariates

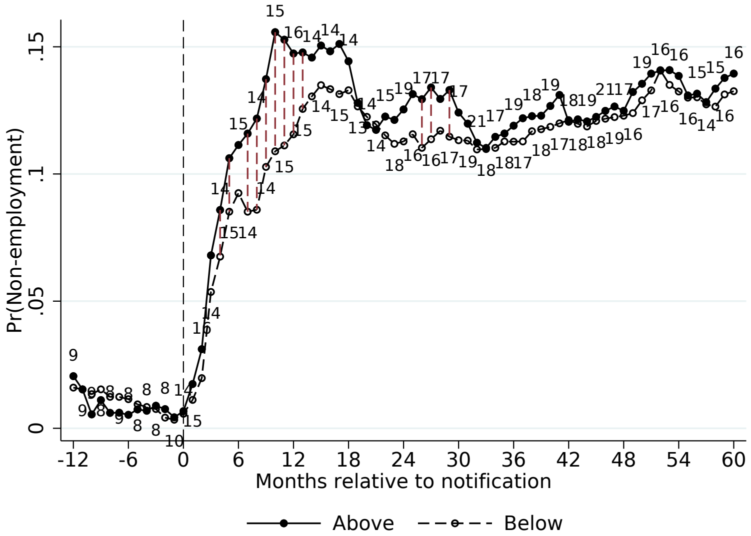


(d) Quadratic fit with covariates



Notes: The figure shows 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.

Figure C.2: Evolution of non-employment by month relative to notification (optimal bandwidth)



Notes: The figure shows the probability of non-employment by month relative to 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 to the left and right of the threshold, respectively. Indicated below each or above 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. All regressions have a linear control function interacted with the threshold and include baseline covariates: age, age squared, gender, immigrant status, educational attainment FE:s as well as order circuit FE:s.

D Decomposition of earnings effect

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,

$$\Delta y = w^D h^D l^D - w^S h^S l^S \tag{D.1}$$

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,

$$\Delta y = w^S h^S \underbrace{(l^D - l^S)}_{\text{Extensive margin}} + l^D [h^s \underbrace{(w^D - w^S)}_{\text{Wage effect}} + w^D \underbrace{(h^D - h^S)}_{\text{Intensive margin}}] \tag{D.2}$$

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.

Table D.1 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 3 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 D.1 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 4). 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.

I now return to the decomposition of the difference in earnings as described in equation (D.2) 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 (D.2) yields

$$-76.8 \approx \underbrace{24.5 \overbrace{(-1.21)}^{\text{Extensive margin}}}_{39\%} + 9.9[\underbrace{134 \overbrace{(-.043 \times 0.01)}^{\text{Wage effect}}}_{14\%} + \underbrace{0.17 \overbrace{(-3.58)}^{\text{Intensive margin}}}_{8\%}] + \underbrace{\varepsilon}_{39\%} \tag{D.3}$$

where Table D.1 provides the estimates plugged into equation (D.3). The total earnings effect

Table D.1: 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.

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.

E 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.

F Relating RD estimates to mass layoff estimates

This appendix clarifies how the treatment parameters identified in the LIFO regression discontinuity (RD) design relate to those identified in the canonical mass-layoff (ML) design. Using the potential outcomes framework, I show that the RD identifies a local average treatment effect (LATE) at the seniority cutoff, while the ML design identifies an average treatment effect on the treated (ATT) for displaced workers in mass-layoff events. I then discuss under which conditions these two parameters coincide.

Let $D_i(z) \in \{0, 1\}$ denote displacement of individual i and $Y_i(d)$ the potential outcome (earnings) if $D_i = d_i$. The observed outcome is $Y_i = D_i Y_i(1) + (1 - D_i)Y_i(0)$. The causal parameter of broad interest is the effect of displacement, $\Delta_i = Y_i(1) - Y_i(0)$. The ATT is defined as:

$$\text{ATT} = \mathbb{E}[Y_i(1) - Y_i(0) \mid D_i = 1],$$

which shows the average effect among those workers actually displaced.

In the LIFO-RD setting, let RR_i denote the relative seniority rank within an order circuit, relative to the cutoff and $Z_i = \mathbf{1}\{RR_i > 0\}$. Under the standard assumptions of the LATE theorem, the fuzzy RD Wald ratio identifies:

$$\text{LATE}^{\text{LIFO}} = \mathbb{E}[Y_i(1) - Y_i(0) \mid D_i(1) > D_i(0), RR_i = 0],$$

that is, the causal effect for compliers who are displaced if and only if they are above the LIFO threshold. Seen this way, both the the mass layoff design and the RD identify causal effects of displacement but at different margins: mass layoff estimates reflects the ATT for all actually displaced workers, while RD estimates pertain to the marginal compliers at the cutoff.

In contrast to the standard IV setting, where the LATE likely differs from ATT because individuals may select into treatment using private information about their own gains, selection into or out of displacement from the individual perspective can almost entirely be ruled out. This as the decision of how many to lay off lies with the employer and potentially exempting workers from layoff by deviating from the LIFO rule are also at the employers discretion (often in negotiations with the union). Note, however, that the primary purpose of the LIFO rule is to prevent and restrict from exercising discretion in selecting which workers to displace – for instance, on the basis of relative productivity – and as described in section 2, the scope for invoking insufficient qualifications remains limited. As a result, there is even less scope for for systematic "selection on gains".

The following lemma which maps the LATE to a locally identified ATT applying to workers just around the threshold:

Lemma (Equivalence of LATE and local ATT)**.** *Let RR_i be the running variable with cutoff at $RR_i = 0$ and $Z_i = \mathbf{1}\{RR_i > 0\}$ and $D_i(z) \in \{0,1\}$ displacement. Assuming: (i)* Continuity*: $\mathbb{E}[Y_i(d) \mid RR_i = r]$ is continuous at $r = 0$ for $d \in \{0,1\}$; (ii)* Exclusion*: Z_i affects Y_i only via D_i in a neighborhood of $RR_i = 0$; (iii)* Relevance (first stage)*: $\lim_{r\downarrow 0} \mathbb{E}[D_i \mid RR_i = r] - \lim_{r\uparrow 0} \mathbb{E}[D_i \mid RR_i = r] > 0$ (iv)* Monotonicity*: $D_i(1) \geq D_i(0)$ $\forall i$ near $RR_i = 0$; (v)* No selection on 'gains' locally (mean independence)*: $Y_i(1) - Y_i(0) \perp (D_i(0), D_i(1)) \mid RR_i = r$ for r in a neighborhood of* 0*;*

$$LATE^{LIFO} = \mathbb{E}\big[Y_i(1) - Y_i(0) \,\big|\, D_i = 1,\, RR_i = 0\big] \equiv ATT^{local}.$$

Proof. By assumption (i)–(iv) (continuity, exclusion, relevance and monotonicity) the fuzzy-RD Wald ratio at $RR = 0$ identifies the LATE for compliers (see Angrist & Pishke, 2009)

$$\frac{\lim_{r\downarrow 0} \mathbb{E}[Y_i \mid RR_i = r] - \lim_{r\uparrow 0} \mathbb{E}[Y_i \mid RR_i = r]}{\lim_{r\downarrow 0} \mathbb{E}[D_i \mid RR_i = r] - \lim_{r\uparrow 0} \mathbb{E}[D_i \mid RR_i = r]} = \lim_{r\to 0} \mathbb{E}[Y_i(1) - Y_i(0) \mid D_i(1) > D_i(0),\, RR_i = r]\,,$$

where $D(1) > D(0)$ are the compliers.

By assumption (v) the individual gain is mean-independent of $D_i(0), D_i(1) \mid RR_i = 0$ at r near 0. So it follows that

$$\lim_{r\to 0} \mathbb{E}[Y_i(1) - Y_i(0) \mid D_i(1) > D_i(0),\, RR_i = r] = \lim_{r\to 0} \mathbb{E}[Y_i(1) - Y_i(0) \mid RR_i = r]\,.$$

which may equally be written as

$$ATT^{\text{local}} = \lim_{r\to 0} \mathbb{E}[Y_i(1) - Y_i(0) \mid D_i = 1, RR_i = r]\,.$$

which is the ATT identified at the limit of the threshold $RR_i = 0$. □