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Surviving a layoff

Search and reallocation after downsizing

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Surviving a layoff:
Search and reallocation after downsizing*

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Abstract

How do workers respond when their colleagues are displaced? Using a unique and detailed register of layoff notifications, I distinguish voluntary and involuntary separations. A matched difference-in-differences design reveals a 3.8% earnings drop among survivors, following workplace downsizing. On-the-job search rises sharply in the month of announcement. When the identities of the retained and displaced workers are revealed, search effort is amplified among survivors, suggesting widespread insecurity across the workforce. This surge is driven primarily by workers with high future layoff risk; slack labor markets, and in large-scale downsizing events. The increase in search effort translates into higher job mobility: survivors become 9 percentage points less likely to remain with their employer than comparable workers in stable firms. Notably, 65% of this increase is driven by voluntary job-to-job transitions. These voluntary movers are positively selected on several productivity-related traits, possibly undermining future firm performance.

Keywords: downsizing, layoffs, job search, worker mobility, job security, difference-in-differences

Jel: J63, J64

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1. Introduction

The individual consequences of layoffs are well-documented, with multiple studies demonstrating that job loss leads to significant and persistent declines in earnings, employment stability, and long-term well-being (Jacobson, LaLonde, and Sullivan, 1993; Davis and von Wachter, 2011; Black, Devereux, and Salvanes, 2015; Eliason and Storrie, 2009). While less is known about how downsizing affects those who remain at the firm, evidence from psychology ranks job insecurity among the most important job-related stressors. This literature suggests that structural change often triggers widespread uncertainty and impacts both worker health and firm productivity through decreased job satisfaction and organizational loyalty (De Witte, Pienaar, and De Cuyper, 2016; Ferrie, 2001; Hartley et al., 1990; Darvishmotevali and Ali, 2020; Sverke, Hellgren, and Näswall, 2002). This indicates that the behavior of the survivors is critical for a firm's post-layoff performance.[1] Despite this, survivors' responses have received comparatively little empirical attention in economics.[2]

In this study, I investigate how downsizing events affect earnings, job search, and mobility among survivors. I exploit a unique dataset compiled by the Public Employment Service (PES), covering the near universe of displaced individuals in Sweden. These data include not only full lists of workers included in each downsizing event, but also dates of announcement, notification, and displacement. They allow me to (1) distinguish displaced workers from survivors, (2) discern voluntary and involuntary separations in the follow-up period, and (3) study worker adjustment on a monthly basis from the initial announcement onward. I match the events with other administrative registers including earnings, demographics, and job and unemployment spells. I then combine exact and propensity score matching to construct a very similar comparison group of workers in non-notifying firms and employ a stacked difference-in-differences design.

[1] I define survivors as workers employed in downsizing workplaces that are initially retained. However, I allow them to separate voluntarily or be displaced at a later point in time.

[2] There is a literature in health economics documenting negative associations between aggregate unemployment rates and mental health (Brenner, 1971; Ruhm, 2000; Laporte, 2004; Breuer, 2015; Tefft, 2011). Other studies have found negative associations between job loss and life satisfaction (Green, 2011; Knabe and Rätzel, 2010; Knabe and Rätzel, 2011; Luechinger, Meier, and Stutzer, 2010; Reichert and Tauchmann, 2017).

Workers have incentives to preemptively safeguard against unemployment risk.[3] Downsizing likely carries new information on firm performance, thereby leading workers to engage in precautionary on-the-job search. Layoffs may further change working conditions by redistributing workloads among those who remain; survivors can face both a higher workload and a different mix of tasks, sometimes because this is the very purpose of downsizing (Margolis and Montana, 2024). Whether such changes in perceived displacement risk and workplace organization trigger responses among survivors remains an open question. Understanding these responses is critical for policy design, since unemployment insurance may induce workers to remain at firms with high layoff risk, thereby imposing a fiscal externality (Baily, 1978; Chetty, 2006; Jarosch, 2023; Simmons, 2024).

I find that downsizing leads to a 3.8% drop in the earnings of surviving workers compared to similar workers in non-downsizing firms one year after notification. The drop is persistent and never fully recovers in my five-year follow-up period. To understand what drives this income drop, I track survivors' on-the-job search behavior, job moves, and separations over time. In the pre-period, survivors are equally likely to search as workers in stable firms. This indicates that they are unaware of the upcoming downsizing. However, in the month when the employer announces its intent to downsize, there is a sharp increase in job search for the surviving workers.

At the announcement date, it is not yet known who will actually be laid off. This indicates that uncertainty is an important driver, as the layoffs have not yet occurred. Around 2–3 months later, it is settled which workers will be displaced, and it is revealed to the workers whether they may remain with the firm or not. When this occurs, on-the-job search continues to *increase* among the survivors who are selected to be retained by the employer. This suggests persistent insecurity across the entire workforce and potential concerns about subsequent downsizing or closure. Even after the layoffs have occurred, on-the-job search effort remains high among the surviving workers. To my knowledge, this is the first study to document this job search pattern among non-displaced workers.

[3]This has been studied in the context of precautionary job search (Jarosch, 2023; Baghai et al., 2021; Simmons, 2024; Cederlöf et al., 2025; Grindaker, Kostøl, and Merkle, 2023), savings (Barceló and Villanueva, 2016; Basten, Fagereng, and Telle, 2016; Campos and Reggio, 2015; Gallen, 2013), and insurance (Landais et al., 2021; Hendren, 2017).

The surge in job search translates into increased job mobility. Three years after the announcement, surviving workers are 9.2 percentage points more likely to have separated relative to the comparison group. Most of these separations are to other firms (7.9 pps) while separations to nonemployment are less common (1.3 pps).

Using the notification data, I then decompose separations by type and find that the observed rise in separations is driven primarily by voluntary moves to new firms, although future layoffs also become more common. Three years after the notification, roughly 65% of the reemployment effect can be attributed to voluntary moves.

Examining the characteristics of those who voluntarily leave, I find that voluntary movers are positively selected relative to stayers along several dimensions: they have higher prior earnings, greater educational attainment, and higher AKM worker effects. This finding has important implications for employers: while downsizing often aims to restructure the workforce, it may inadvertently trigger a 'brain drain' effect. When the most productive employees exit voluntarily in response to negative signals, the firm's residual workforce may be of lower average quality than the employer intended during the initial selection process. Ultimately, retention is shaped by two forces: (1) who the firm chooses to lay off and (2) who chooses to stay.

In addition, I find evidence that survivors are willing to trade off earnings in exchange for greater job security. Voluntary movers match with firms that pay on average 2% less than the notifying firm. I also find that voluntary leavers tend to match with relatively higher-paying firms compared to survivors who are later displaced in a future event, suggesting that they may have better outside options when searching for a new job.

I then break down the observed income decline by each of the survivor groups. The evidence suggests that the drop is driven mainly by a higher likelihood of future nonemployment among survivors relative to the control group. Earnings changes resulting from job switches contribute less to aggregate earnings differences. However, earnings increase among those who remain. This pattern may indicate that the marginal value of the remaining employees rises when the firm downsizes.

Finally, I examine how these effects vary across event, worker, and labor market character-

istics. The findings reveal that the aggregate response is primarily driven by survivors facing the highest perceived risk of future displacement. Specifically, on-the-job search is more pronounced: (1) in large-scale events; (2) among junior workers, who face a higher risk of being displaced in subsequent layoffs; and (3) during slack labor market conditions, where anticipated unemployment duration is longer. Together, these patterns suggest that survivors search proactively to mitigate the risk of future involuntary separation.

This study contributes to several strands of literature. First, I extend the literature on earnings losses from displacement[4] by investigating losses for surviving workers, which to my knowledge has only been done using survey data and not register data. Many previous studies rely on either mass layoffs or surveys to identify displaced workers (Couch and Placzek, 2010). However, mass layoffs represent only a fraction of total displacements;[5] consequently, these studies may suffer from measurement error by misclassifying coinciding voluntary separations as involuntary. Furthermore, survey-based studies are limited by the fact that they are often conducted years after the event; as a result, workers may misremember a layoff or misperceive an involuntary separation as voluntary (von Wachter, Handwerker, and Hildreth, 2009). The PES data do not suffer from these potential issues, as they directly record both layoff notifications and actual separations as they occur.

Second, I expand the literature on search and mobility with respect to unemployment risk (Cederlöf et al., 2025; Jarosch, 2023; Baghai et al., 2021; Grindaker, Kostøl, and Merkle, 2023; Simmons, 2024) while complementing these studies by (1) focusing on surviving workers and (2) exploiting layoff register data to distinguish between voluntary and involuntary separations. I am also able to observe when a layoff is announced, enabling me to identify the formal revelation of the downsizing preceding notification. Third, I add to the literature on spillovers in a layoff setting (Rege, Telle, and Votruba, 2011; Lindo, 2011; Mörk, Sjögren, and Svaleryd, 2020) by showing that these effects extend beyond the immediate family. Finally, I contribute to the literature on the selection process in a layoff context (Gibbons and

[4]See Jacobson, LaLonde, and Sullivan (1993), Lachowska, Mas, and Woodbury (2020), von Wachter, Song, and Manchester (2009), Eliason and Storrie (2006), Burdett, Carrillo-Tudela, and Coles (2020), Athey et al. (2026), Schmieder, von Wachter, and Heining (2023), and Cederlöf (2024).

[5]About 8 percent of all involuntary separations in the U.S. between 2003–2012 occurred during mass layoffs (Cederlöf, 2024).

Katz, 1991; Bender et al., 2002; Seim, 2019; Margolis and Montana, 2024) by investigating a second layer of this process: the voluntary separations soon after notifications have been sent out. I am able to do this by observing layoffs directly in administrative register data.

The rest of the paper is organized as follows. Section 2 describes the institutional framework governing layoffs in Sweden and introduces the administrative registers used in the analysis. It also details the sampling of surviving workers and their matched controls. Section 3 outlines the empirical methodology. Section 4 presents results on traditional labor market outcomes such as income, job search intensity, and job mobility. Section 5 characterizes the movers, while Section 6 examines how different types of survivors respond to shocks of varying type and intensity. Section 7 concludes.

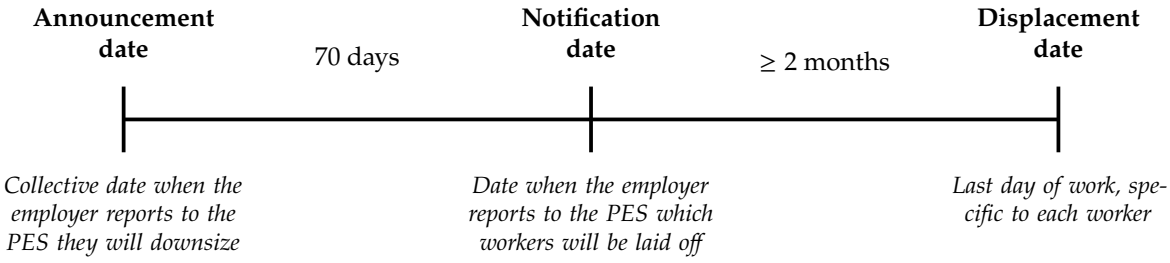
2. Institutional Setting, Data Sources, and Sampling

2.1. Setting

Figure 1 illustrates the sequence of events in the Swedish layoff procedure. Under Swedish law, firms planning to lay off five or more workers must notify the Public Employment Service (PES). The first observable step is when the firm reports to the PES the anticipated number of affected workers and the reason for downsizing. In most cases, this is when it becomes publicly apparent that layoffs are possible, although there is no legal requirement to inform employees at this stage. Prior research finds that voluntary separations often rise before formal notification, suggesting that workers can anticipate downsizing (Lengermann and Vilhuber, 2002; Schwerdt, 2011). Likewise, Cederlöf et al. (2025) show that longer advance notice improves reemployment prospects by allowing job search to begin while still employed. Ideally, the data would contain the exact date when workers are first informed. Nevertheless, as shown in Section 4.2, job search activity tends to increase around this point, therefore, I refer to the initial report to the PES as the announcement date.

Following the announcement, the firm negotiates with relevant labor unions to determine

Figure 1: Timeline of the Displacement Process



which workers will be laid off. Unless otherwise agreed, the LIFO rule applies, prioritizing layoffs of the least tenured workers. Although the LIFO rule serves as the default in negotiations, employers generally exercise significant discretion over which workers are displaced, provided an agreement is reached with the union.

When negotiations conclude, the employer submits the finalized list of displaced workers to the PES. I define the date this list is reported as the notification date, which corresponds to when workers are formally informed of their displacement and their last day of work. On average, about 70 days elapse between announcement and notification. The notice period between notification and displacement averages slightly more than two months and varies with worker tenure. While notification dates are generally the same across workers within an event, displacement dates tend to be later for workers with longer tenure.

2.2. Data and Sampling

My main dataset is the administrative register of all notifications reported to the PES between 2005 and 2019 (Arbetsförmedlingen, 2025b). For each worker, I observe the notification date and the reported displacement date.[6] The data also includes the date when the employer first reports the intent to downsize to the PES.

I link the notification data to several other administrative sources: (1) RAMS (SCB, 2018), containing the universe of employer–employee matches from 1985 onward, with earnings and monthly employment markers; (2) LISA (SCB, 2017), with individual demographics

[6]For almost all workers, the individual notification date coincides with the event level list date. I use the latter to get a consistent date for each event.

such as education, marital status, and migration background; (3) Business Register (SCB, 2025a), with establishment-level industry codes; (4) Wage Statistics (SCB, 2025b), with full-time equivalent wages for all public-sector and a random sample[7] private-sector workers, along with occupation codes and hours worked; (5) Unemployment Register (Arbetsförmedlingen, 2025a), including daily spell information.

The treatment sample consists of individuals employed at a notifying plant in the same month as the notification date but not themselves notified. I mechanically exclude plant closures as there are no survivors and require that at least one notified worker and one survivor appear in the employer–employee register in the notification month. I also drop establishments with more than ten downsizing events during the study period (about 2% of all cases).

The control group is constructed following Schmieder, von Wachter, and Heining (2023). For computational reasons, I first draw a 25% random sample from the universe of workers, then combine exact propensity score matching to select controls for each survivor, as detailed in Section 3.1. Controls are excluded if they work in a *firm* which is notifying in any of its plants in the same month. Both survivors and their matched controls are allowed to voluntary exit or become displaced in a later layoff event in the post period i.e. I do not require that they remain at the notifying firm.

For both survivors and controls, I impose standard restrictions from the layoff literature (see Table A.1). Workers must earn more than 10 000SEK per month,[8] have at least three years of tenure in the notification month, be aged 25–54, and work in plants with 30–1,000 employees one year before notification.[9] I allow workers to have any labor market attachment post event.

After these restrictions, I am left with a final sample consisting of 370,000 workers in 7,900 individual events. The median event involves 11 notified and 88 surviving workers. 90% of the sample of survivors consists of events where not more than 40 workers are notified

[7]The sample includes roughly 50% of all private sector workers. The sampling is by firm and stratified by employer size.

[8]1 USD ≈ 8 SEK during the study period.

[9]The average workplace workforce drops by 15 log points around the event. See Figures B.2 and B.3.

and 98% of events are in the private sector.

3. Methodology

Employers typically choose to downsize due to several factors. For instance, a negative demand shock driven by a decline in product demand or structural change altering the need for specific types of labor may prompt a firm to reduce its workforce. Downsizing can also result from other types of efficiency reasons such as acquisitions and mergers where operational synergies lead to a reduction in necessary positions.[10] Regardless of the underlying cause, workplaces that downsize and those that do not are likely different. Consequently, the treatment studied here encompasses both the downsizing event itself and the underlying economic or organizational shock that initiated it. It is important to stress that the methodology and identifying assumptions are very similar to those of a standard mass layoff study, although I focus on survivors instead of displaced workers.

In this section, I describe the methodology of the study. Section 3.1 first outlines the empirical challenge and design. This is then followed by the empirical specification in Section 3.2.

3.1. Matching Procedure

The main empirical challenge lies in identifying an appropriate counterfactual for workers who survive a layoff event. This is difficult primarily due to two forms of selection: (1) workplace-level heterogeneity and (2) within-workplace selection of notified employees, and by extension those who survive. Establishments where layoffs typically occur differ significantly from other workplaces. For example, cyclical industries, such as manufacturing or construction, downsize more frequently during recessions than industries like education or health. Mechanically, larger plants are also more likely to experience layoff events than smaller ones simply because they employ more workers. Furthermore, notification

[10] There is a separate literature which study these events, see Arnold et al. (2026), Lagaras (2019), and Beuschlein, Sigurdsson, and Wong (2025).

is likely non-random. Selection occurs across several dimensions: employer preferences for productivity (Seim, 2019) and specific skills (Margolis and Montana, 2024), as well as institutional constraints like the LIFO rule (Cederlöf, 2024).

The ideal experiment would compare two identical workers in similarly performing workplaces up until the downsizing event. The treated establishment is then exposed to an unexpected shock that triggers downsizing while the control is not. The treatment effect will then capture both the impact of the negative shock and the consequences of the downsizing itself. The counterfactual to surviving a downsizing event is to work in a non-downsizing establishment in the same industry which is performing equally well in the periods preceding the layoff event. Any differences that emerge at the time of the downsizing and onward can then be interpreted as the combined effect of the negative shock and the downsizing.

Level differences between the survivors and controls can be accounted for using individual fixed effects. More challenging is to account for differences in worker trends over time in the outcomes. To account for differences in trajectories, I combine propensity score matching with exact matching to find a credible control group. Specifically, I divide the sample into cells defined by the industry $\times$ the year/month of the notification date. There are 21 industry codes in the registers. I then regress the likelihood of working in a downsizing plant, DS_i, on a number of variables within each of the cells. These variables are education, tenure, age, log income in the three years before notification, $c-1$, $c-2$ and $c-3$ as well as firm and establishment sizes in year $c-1$.[11] Each survivor is matched to a single comparison worker with the closest propensity score (without replacement). I finally stack all the different cells into my main analysis sample.

The stacking approach is similar to the strategies proposed by Schmieder, von Wachter, and Heining (2023) and Callaway and Sant'Anna (2021). This design selects workers and workplaces which are very similar while it handles issues with some observations receiving negative weights when treatment effects are heterogeneous, as emphasized by Goodman-Bacon (2021). In essence, the concern is that the standard two-way fixed effects

[11]The treatment and controls show parallel trends in a number of non-matched characteristics, such as sales (Figure F.2) and total labor costs at the workplace level (Figure F.3).

estimator assumes all treatment effects are identical regardless of treatment cohort c, in addition to the parallel trends assumption. This is unlikely to hold in my setting, due to business cycle fluctuations.

The unmatched sample reveals that survivors in downsizing plants are selected along a number of dimensions, see Table I. They work in different industries, such as manufacturing, to a larger degree which in turn means that the share of males is higher and education attainment lower. They are also more likely to work at larger workplaces. They are however similar in other dimensions such as earnings, tenure and immigrant background. After matching, the treated survivors are very similar to the selected controls.[12]

3.2. Empirical Design

I use the same specification as Schmieder, von Wachter, and Heining (2023) to estimate the effect of working in a downsizing plant on a number of outcomes. I estimate the following equation:

$$y_{itc} = \sum_{k=-5}^{5} \delta_k \times \mathbf{1}\{t = c + k\} \times \mathrm{DS}_i + \sum_{k=-5}^{5} \gamma_k \times \mathbf{1}\{t = c + k\} + \alpha_i + \pi_t + \epsilon_{itc} \tag{1}$$

Here, y_{itc} denotes the outcome of interest for worker i at calendar time t, with c indicating the treatment time of notification. I observe each individual from five years before notification, up to five years after notification. The treatment indicator variable DS_i equals one if individual i is in the treatment group, i.e. is a survivor at a plant where at least one other worker is notified, and 0 if i is in the matched control group.

The main parameters of interest are δ_k, which capture the relative change in outcomes between survivors and their matched controls for each period k. I also control for event time fixed effects γ_k, individual fixed effects α_i, and notification month fixed effects π_t. Finally, ϵ_{itc} is the error term.[13]

[12] See Figure B.1 for after matching propensity score distribution.

[13] My findings are robust to a range of alternative specifications, notably the one proposed by Sun and Abraham (2021). These results are presented in Appendix C.

Table I: Descriptive Statistics

	Matched Sample		Unmatched Sample	
	Survivors	Controls	Survivors	Controls
Workers				
Log earnings t-1	12.75	12.75	12.75	12.76
	(0.479)	(0.522)	(0.484)	(0.518)
Tenure	131.5	130.8	130.4	117.9
	(77.39)	(76.87)	(77.15)	(72.70)
Experience	244.2	244.2	243.8	241.3
	(79.62)	(79.50)	(79.89)	(86.89)
Age	41.86	41.79	41.84	41.43
	(7.629)	(7.649)	(7.628)	(7.958)
Pr(Woman)	0.294	0.301	0.299	0.361
	(0.456)	(0.459)	(0.458)	(0.480)
Pr(Immigrant)	0.135	0.129	0.137	0.129
	(0.342)	(0.336)	(0.343)	(0.336)
Pr(Married)	0.460	0.462	0.461	0.460
	(0.498)	(0.499)	(0.498)	(0.498)
Pr(College)	0.225	0.231	0.236	0.293
	(0.418)	(0.421)	(0.425)	(0.455)
Managers	0.0793	0.0768	0.0804	0.0858
	(0.270)	(0.266)	(0.272)	(0.280)
Plants				
Plant size t-1	274.5	271.3	277.9	167.6
	(212.4)	(247.5)	(218.3)	(178.5)
Firm size t-1	380.5	378.0	380.4	257.7
	(267.1)	(300.8)	(270.6)	(254.6)
Manufacturing	0.625	0.625	0.595	0.309
	(0.484)	(0.484)	(0.491)	(0.462)
Trade	0.102	0.102	0.0997	0.145
	(0.302)	(0.302)	(0.300)	(0.353)
Information	0.0718	0.0718	0.0705	0.0588
	(0.258)	(0.258)	(0.256)	(0.235)
Observations	371074	371074	390142	4540665

$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Notes: The table reports descriptive statistics for workers in Sweden from 2005 to 2019. The first set of variables describes worker characteristics, while the remainder pertains to their firms. For each group the sample is restricted to individuals aged 25–54 with at least 10,000 SEK in earnings and three years of tenure. See Section 2.2 for details on sample construction. Log earnings, plant size, and firm size are measured in the year before notification, while the remaining variables are measured at the time of notification. "Immigrant" is defined as being born outside of Sweden, and "college" as having ever enrolled in a university program. "Managers" is the share of managers as defined by the occupation codes. "Manufacturing," "Trade," and "Information" represent the share of firm industries based on industry codes.

4. Income, Job Search, and Reallocation

I apply the above mentioned specification to several outcomes, starting with the impact of downsizing on survivors' earnings. I then shift the focus to job search and reallocation. Next, I decompose the mobility into voluntary and involuntary. Finally, I decompose the overall income effect to investigate which job moves drive the income effect.

4.1. Income

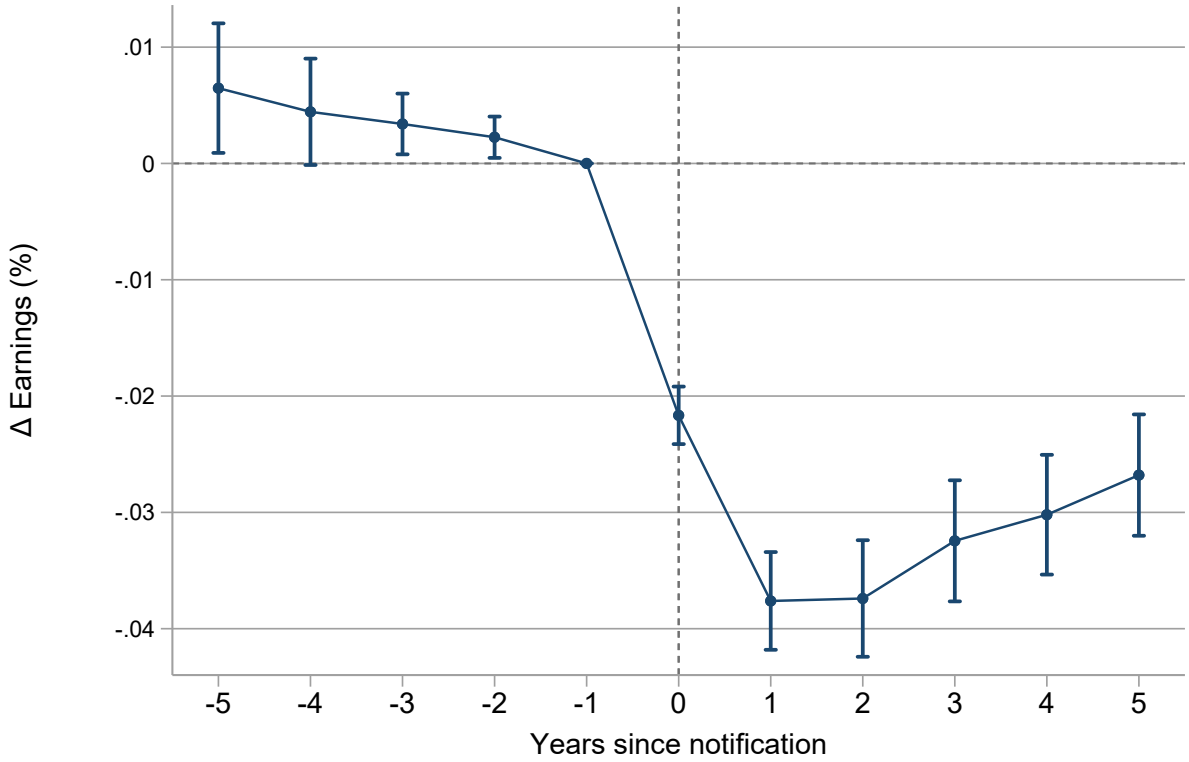
First, I study the evolution of earnings for survivors over time relative to their matched control group, following the event-study design presented in Equation (1), Section 3.2. Figure 2 shows the estimates of δ_k with corresponding confidence intervals around the notification. The counterfactuals are the matched control group detailed in Section 3.1. In the years preceding notification, earnings are stable for survivors compared to the control group. At the year of notification, there is a significant drop in earnings, which reaches its lowest point in the following year. One year after the event, survivors' earnings are about 3.8% lower than the control group, and partially recover to approximately 2.8% below the control five years post-notification.[14]

Taken together, these results indicate that income losses extend beyond those directly displaced and that survivors are also affected by downsizing. This naturally raises the question of how survivors adjust their behavior around the announcement and implementation of the downsizing. Along which margins do their behavior change and which are more important for explaining the income drop?

I will investigate several potential channels contributing to the observed income decline. The first is that surviving workers voluntarily leave the firm for lower-paying jobs elsewhere. One possible motivation is increased job insecurity following the layoff event. A second

[14] I apply the same matching procedure to the group of notified workers and find income drops of approximately 40% (see Appendix F). While this magnitude exceeds typical estimates in the mass layoff literature, it is consistent with the findings of Cederlöf (2024), who uses the same empirical setting and data. One potential explanation is that prior studies include a proportion of workers who are not displaced.

Figure 2: Evolution of Earnings



Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed. The estimates show the evolution of yearly earnings, relative to the year before notification. Presented in table format in Appendix G.

explanation is that survivors are more likely to be laid off in subsequent years, resulting in more involuntary separations. Third, the decline may stem from task reassignments at the notifying firm, leading to reduced working hours and/or less favorable wage trajectories.

In the next section Section 4.2, I turn to evidence on job search to examine whether downsizing and, in turn, concerns about job security lead survivors to actively explore opportunities outside the firm.

4.2. Search

First, I examine if the surviving workers increase their search effort suggesting increased job insecurity. A key indicator of whether workers are actively seeking new employment is

their registration as employed job seekers at the PES. Employed workers can voluntarily register in this category to receive job search support from the PES. It is rare that workers register in this category. Nevertheless, relative changes in the share of workers who do register provide a meaningful indication of whether some employees intend to leave the firm. I will therefore use the share registering in this category as a proxy for on the job search intensity.

Figure 3a plots the likelihood of being registered as an employed job seeker at the PES by month relative to the notification date for survivors and their matched controls. The gray vertical dashed lines denote the mean month of announcement across all events in the sample (left) and the notification month (right). The red dashed lines indicate if the difference between the survivors and the controls is significant at the 5% level.

In the months preceding the announcement the two groups have very similar likelihood of registration, and there are no significant differences. However, around the time of announcement, as indicated by the leftmost vertical line, an excess amount of survivors start registering at the PES as employed job seekers. This suggests that the initial report, which I use as the definition for the announcement (see Section 2.1), accurately captures the timing of the announcement. Simmons (2024) finds a similar timing for increased search activity using survey data.

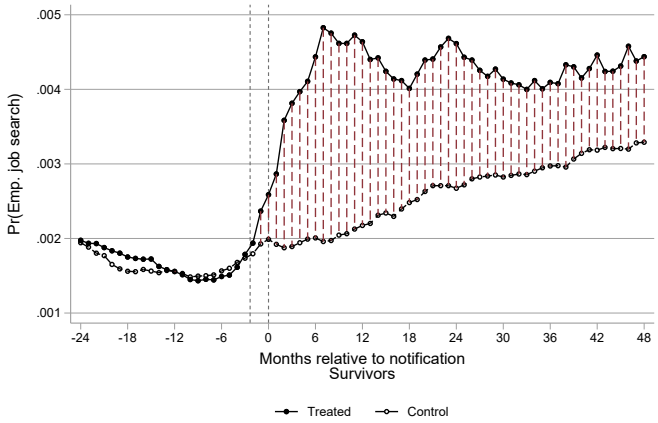
One might hypothesize that survivors would decrease their search effort upon notification, as it is then clear that they will not be displaced. Interestingly, the evidence suggests otherwise. Search intensity continues to increase at the time of notification, indicating that willingness to leave the notifying firm extend beyond those directly affected. This implies that being informed of retention does not alleviate the concerns about the prospects at the notifying firm.

A possible explanation is that downsizing incentivizes many workers to seek new employment elsewhere due to concerns about future layoffs. As I will show later in Section 4.4, these concerns are well-founded: firms that have displaced workers once are more likely to do so again, affecting a substantial number of survivors.

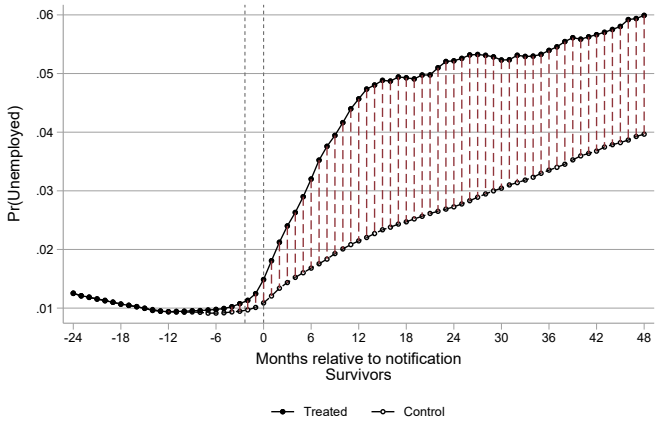
In sum, the surge in registrations at the PES supports the hypothesis that the survivors are

Figure 3: Evolution of Unemployment and Employed Job Seeker

(a) Probability employed job seeker



(b) Probability unemployed



Notes: The solid lines show the levels of the outcome for survivors (filled dots) and their controls (hollow dots) respectively. The red dashed lines indicate months in which the δ_k as defined in Equation (1) are statistically significant at the 5% level. The dashed vertical lines indicate the mean notification ($t = 0$) and announcement dates for the survivor sample. Standard errors are clustered at the event level. Figure 3b shows the evolution of the share of workers classified as unemployed, defined as being registered with the PES in any job-seeking category. Figure 3a presents the subset of these registrations corresponding to employed job seekers, as indicated in the unemployment register. Both measures are constructed from daily spell data and aggregated to monthly observations. Presented in table format in Appendix G.

more concerned about the future of their employment. When other workers are displaced, they are encouraged to start looking for new employment. Thus, registrations as employed job seeker more than doubles within a few months.

I now turn to all PES registrations, including unemployment. It is important to note that,

while survivors are required to be employed in the three years preceding notification, they may still be registered as unemployed at the PES. A worker is coded as unemployed if they experience even a single day of unemployment in a given month.

Figure 3b plots the same groups as in Figure 3a, but includes all types of registrations related to unemployment at the PES. Interestingly, the evolution of unemployment among survivors largely follows the surge in registrations as employed job seeker. As shown in the figure, there is a significant increase in unemployment registrations in the months following notification. This suggests that even if a worker survives the initial layoff, their likelihood of becoming unemployed rises shortly thereafter. I return to this mechanism in Section 4.4.

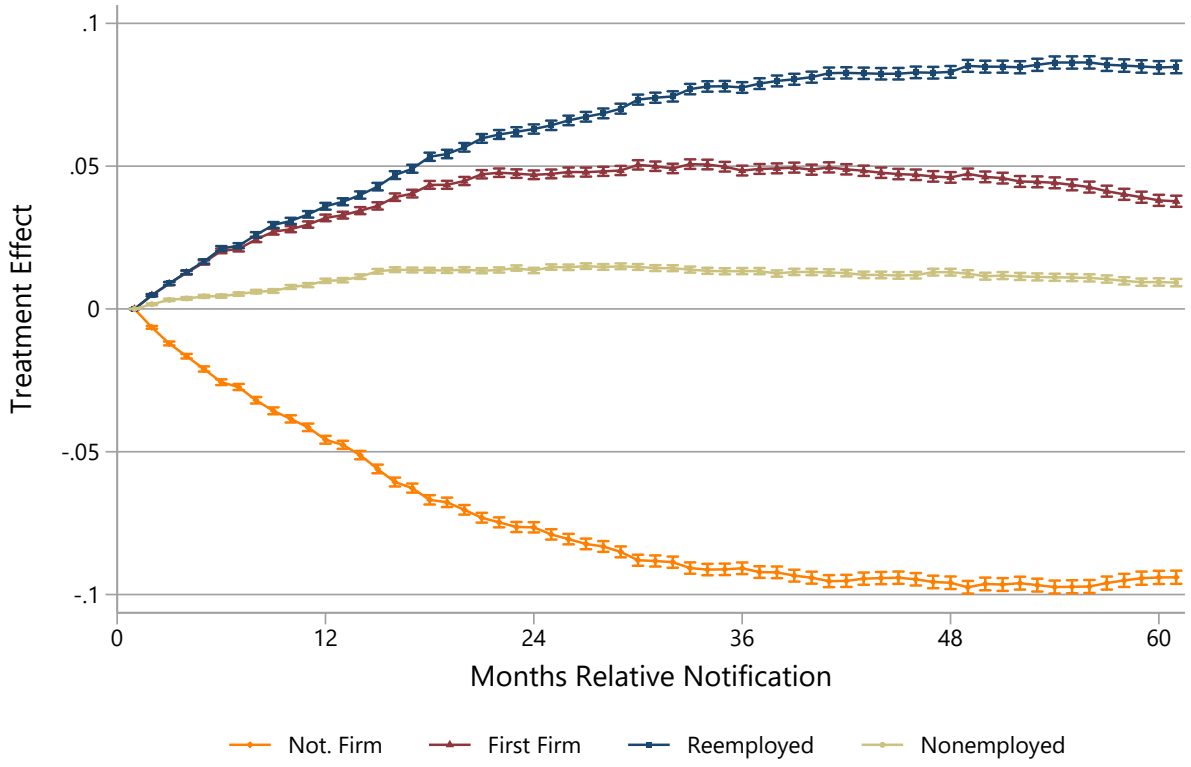
The next step is to investigate: (1) whether the observed increase in search intensity among survivors translates into concrete actions, such as job changes and separations from the firm; and (2) whether the surge in unemployment is driven by additional layoffs in the future. I examine these questions in the next two sections.

4.3. Reallocation

The evidence so far suggests that the effects of downsizing extend beyond those directly displaced, affecting both earnings and search effort. I now examine whether this increased effort translates into job moves. Figure 4 plots the likelihood of being in different labor market states by time relative to notification, using the employer–employee register which provides monthly indicators of each worker's attachment to a given firm and workplace. I classify workers into three mutually exclusive states: notifying firm, defined as being registered at the notifying firm with any earnings; nonemployed, defined as having no employer paying at least 10,000 SEK in a given month; and reemployed, defined as earning more than 10,000 SEK from a new firm.

The results suggest that workers in original notifying plants are more likely to leave the notifying firm compared to the controls. After three years, survivors are about 9.2 percentage points less likely to remain at the notifying firm compared to similar workers.

Figure 4: Labor Market States



Notes: The figure shows the treatment effect on different labor market states. The orange line (dots) represents the likelihood of working for the notifying firm, the red line (triangles) shows the likelihood of working for the first newly occurring firm, the blue line (squares) displays the likelihood of being reemployed in any other firm, and the green line (dots) indicates the likelihood of being nonemployed. The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed.

Around two-thirds of all survivors remain at the notifying firm at that point in time.

Most of the excess workers who leave the notifying firm find new employment in other firms. After three years, about 25% are reemployed which is about 7.9 percentage points more than the control group. Separations into nonemployment are also more common among survivors compared to their controls but this difference is a lot smaller, only 1.3 percentage points. This can be compared to the difference in unemployment, as shown in Figure 3b, which is around 2 percentage points.[15]

[15]One reason for the discrepancy between the number of unemployed and the number of nonemployed is the difference in measurement frequency. Unemployment is recorded at the daily level, whereas nonemployment is measured monthly. As a result, many individuals may be classified as employed in a given month but still appear in the unemployment register if they experienced even a single day of unemployment during that month.

In sum, the evidence suggests that survivors are more likely to leave the notifying firm, consistent with the surge in search effort documented in Section 4.2. In the next section, I use the PES notification data to examine this mechanism in greater detail.

4.4. Voluntary and Involuntary Moves

The evidence in Section 4.2 suggests that the information about the decision to downsize is revealed at the time of announcement. The number of workers who register as employed job seekers dramatically increases, suggesting an increase in search effort among the surviving workers, which in turn translates into actual job moves as shown in Section 4.3. However, there is also a large increase in the number of workers who register as unemployed suggesting that some of the survivors are displaced in future events. The drop in earnings could therefore both be explained by job moves and separations into nonemployment.

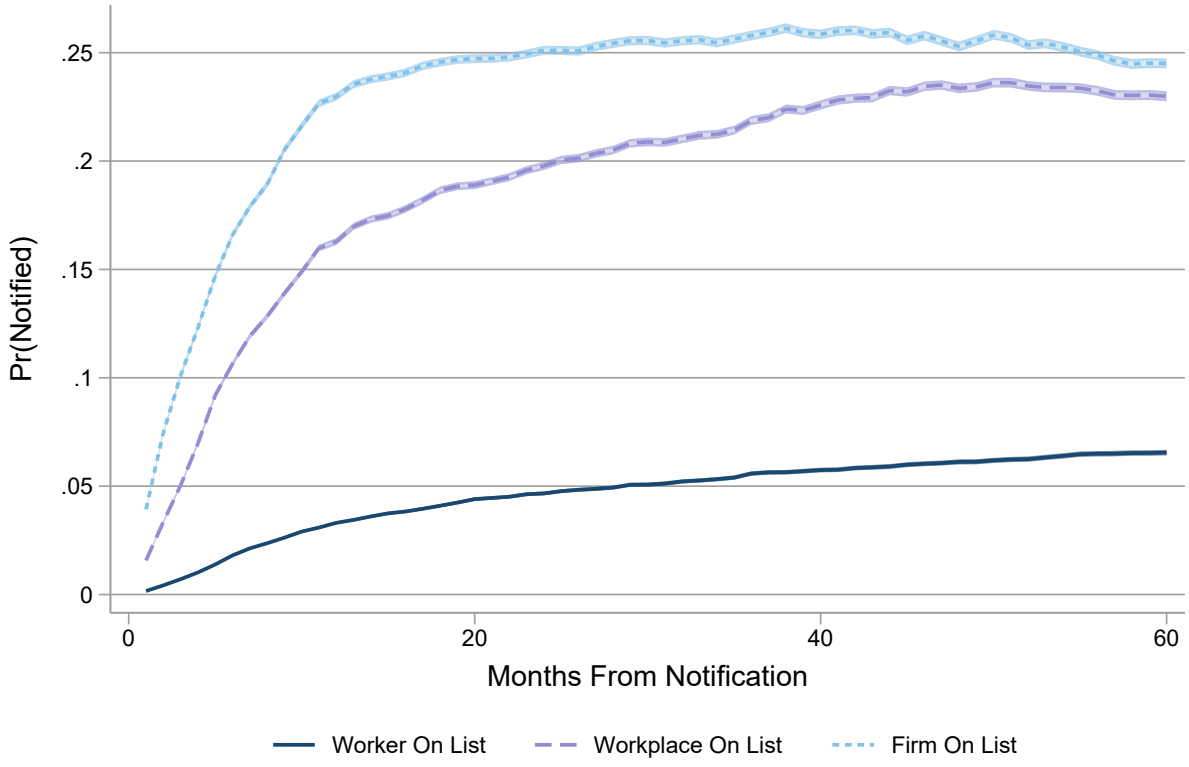
While it is likely that the bulk of the increase in unemployment is due to involuntary separations, disentangling which share of job-to-job moves are voluntary or forced is more challenging. I resolve this issue by using the PES data to study the incidence of notifications across firms, plants and workers in the treated and control groups.

As a motivating exercise, I compare the probability of future downsizing events between survivors and their matched controls. Figure 5 shows the difference in cumulative likelihood of there being a notification involving the firm (dark blue), plant (lavender) or worker (light blue) respectively.

The figure reveals that both downsizing firms and plants have a higher likelihood of downsizing again within a few years compared to other workplaces. As a result, survivors have an increased risk of becoming displaced in a future event. Separations from the notifying firm thus reflect a combination of voluntary moves and additional layoffs in future time periods. I will explicitly show that this is the case in Section 5.

With the help of the notification data, I categorize all moves to new workplaces from Section 4.3 depending on if the move is preceded by a notification of the worker or not. If the worker is notified between the first downsizing event and the date of becoming reemployed

Figure 5: Difference Notifications Survivors vs Controls

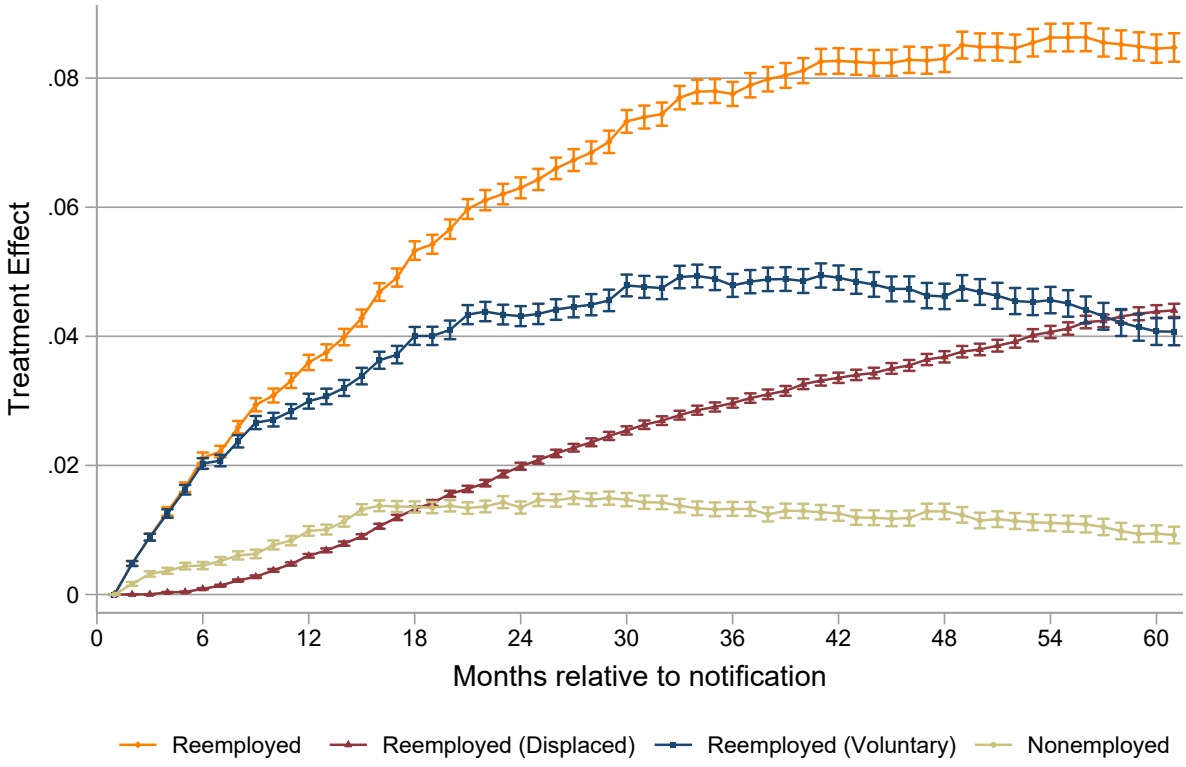


Notes: The figures show the difference between survivors and controls appearing on any PES list in the months following the notification date. The analysis is done separately on the worker, firm and plant level. The x-axis indicates months since notification, and the y-axis shows the difference in likelihood that a notification has occurred in period t or earlier between survivors and the matched controls. By construction, all firms and plants in the survivor group are notified in $t = 0$, as discussed in Section 2.2. Presented in table format in Appendix G.

as recorded in the employer-employee register, I code this as a future displacement. Importantly, I require that the list originate from the same firm as the first notification. I assume all other exits are voluntary. This is important to establish because if workers leave voluntarily, it is presumably negative for the employers and an unforeseen consequence of the downsizing event.

Figure 6 presents the results from this exercise. The orange line shows the effect on the share of reemployed workers relative to the control group, by month relative to the notification date. The brown and blue lines split this effect into subsamples: those reemployed after displacement and those classified as voluntarily reemployed. Finally, the green line depicts the effect on nonemployment.

Figure 6: Separations by Type



Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed. The figure plots the estimates for each labor market state, being reemployed, reemployed and displaced, reemployed voluntarily and nonemployed. Reemployed is the sum of reemployed displaced and reemployed voluntarily.

The effects on reemployment, can now be decomposed into workers reemployed due to displacement and those reemployed following a voluntary separation. I find that although some separations are preceded by notifications, the majority of the reemployment differences are driven by voluntary ones. Three years after the notification, roughly 65% of the reemployment effect can be attributed to voluntary moves, suggesting that the observed increase in search effort translates into actual job transitions.

Interestingly, there is also an excess of workers who are reemployed following a future layoff. This indicates that even though many survivors leave voluntarily in response to increased unemployment risk, the net effect is still an increased likelihood of displacement. This aligns with the evidence from Sections 4.2 and 4.3.

The evidence suggests that survivors face both a higher risk of future displacement and

unemployment compared to similar workers at other firms. This increased risk leads to an increase in voluntary job moves. These channels, together with changes within the workplace, contribute to the negative income effects reported in Section 4.1.

To decompose the earnings drop reported in Section 4.1, I classify survivors into four groups—stayers, nonemployed, voluntary leavers, and involuntary leavers—based on their status exactly 36 months after the notification. I then calculate the income change for each group at that same point in time. Finally, using Equation (2), I compute each group's contribution to the total earnings effect.

$$\Delta Y_{\text{total}} = \sum_{g \in \{\text{stay, nonemp, vol, invol}\}} s_g \cdot \Delta Y_g \tag{2}$$

The results of this exercise are presented in Table II. s_g denotes the share s of each group g. ΔY_g represents the change in income for each group three years after the notification, relative to the year before the event, measured in percentage points. The column $s_g \times Y_g$ shows the group share multiplied by the income change, which sums to the aggregate earnings effect of the downsizing event. The last column reports the relative contribution of each group to the total earnings change.

As shown in Section 4.3, the majority of workers remain at the notifying firm three years after notification. These workers earn 5.75 percentage points more than their matched controls. One possible explanation is that employers compensate certain key workers to persuade them stay, or that their marginal product increases if the firm's production function is concave. Stayers therefore contribute in the opposite direction of the aggregate negative income effect.

Conversely, the excess number of nonemployed survivors exclusively cause the negative income effect. Their earnings by construction drop sharply. The nonemployed account for the largest share of the total negative income effect, despite representing only a small fraction of survivors.

Voluntary movers display earnings trajectories that are similar to those of their matched

Table II: Decomposition of Income Effect

Group	s_g (%)	ΔY_g (pp)	$s_g \times Y_g$ (pp)	Contribution (%)
Stayers	67.13	5.75	3.86	-120
Nonemployed	6.75	-100.00	-6.75	210
Voluntary movers	21.92	0.73	0.16	-5
Involuntary movers	4.20	-11.59	-0.49	15
Total	1		-3.22	100%

controls. While their decision to leave the notifying firm suggests an active search for better opportunities, the data indicate that such moves do not systematically result in either large gains or large losses in earnings relative to comparable workers. Their aggregate contribution to the total income effect is therefore modest. The involuntary movers experience significantly worse earnings trajectories than their matched controls. Their contribution is relatively small compared to the nonemployed, because their earnings loss is moderate relative to the nonemployed.

In sum, while the stayers exhibit relatively higher earnings compared to their counterfactuals, this positive effect is not sufficient to offset the substantial losses generated by the excess number of nonemployed survivors.[16]

5. Characterization of Movers

In Sections 4.3 and 4.4, I establish that voluntary separations constitute the most important channel of worker mobility following downsizing events. I now turn to the question of what types of workers are most likely to leave voluntarily. This is interesting for two reasons.

First, it is interesting to understand how outside options shape worker behavior. Characterizing the selection process provides insight into which workers react to new information about their future at their current employer. Second, from the perspective of employers, it is important to understand which employees decide to leave. These workers are initially

[16] I also investigate the effects on wages and hours worked and find small effects, see Figures D.1 and D.2

retained, indicating they are individuals the firm intends to keep.

To investigate the average skill of workers who leave, I calculate worker fixed effects following Abowd, Kramarz, and Margolis (1999) as a general measure of productivity. I first pull the annual pre-event wage histories of all workers and the firms they matched with after the event. I then estimate the worker and firm fixed effects by regressing wages on dummies for all workers and firms. Finally, I classify workers based on their labor market status three years after the notification as in previous sections, and compare their fixed effects.

The results from this exercise are shown in Figures 7 and 8. The blue bars represent the sample of survivors who voluntarily moved to another firm as defined in Section 4.4. The other bars represent workers who are displaced in a future event and reemployed in another firm (red) and those who remain at the initial notifying firms (orange), respectively. All groups are evaluated three years after notification. The dashed lines are fitted Epanechnikov kernels.

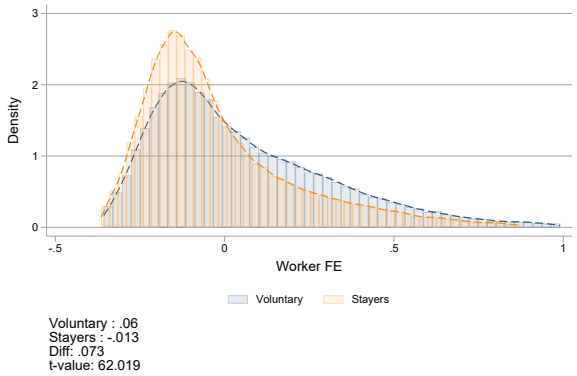
The distributions in Figures 7a and 8a indicate that voluntary movers are positively selected in terms of their individual fixed effects, both relative to workers who are displaced in future events and to workers who remain at the notifying firm. In line with this, I find that those who leave voluntarily are positively selected on previous earnings compared to both future displaced workers and stayers as shown in Figures 7b and 8b. On average, they earn about 18 log points more than those displaced in a future event and are about half as likely to have a college background. Moreover, their pre-event earnings difference is around +7 log points (see Table B.2).

These patterns suggest that, although firms aim to retain their most skilled workers, these efforts often fail, and high-ability individuals instead transition to new jobs after the layoff announcement. This type of selection has not been previously studied and highlights that post-downsizing retention is shaped not only by employer decisions but also by worker self-selection.

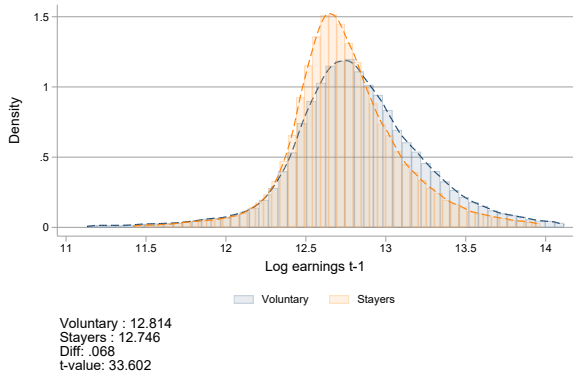
One reason why voluntary moves are more prevalent among high ability workers may be that they have better outside options compared to other workers. Similarly, it may be that

Figure 7: Distributions: Voluntary vs. Stayers

(a) Worker Fixed Effects



(b) Log Earnings, $t-1$



Notes: Figure 7a displays the distribution of AKM worker fixed effects and Figure 7b, log earnings in year $t-1$. The y-axes measure the density of workers, while the x-axes correspond to either the size of the worker fixed effects or previous earnings. Blue bars indicate survivors who are reemployed voluntarily, and orange bars represent survivors who remain employed at the notifying firm. The two groups are defined three years after the initial layoff notification, following the procedure described in Section 4.4. The dashed lines show fitted Epanechnikov kernel densities for each group. Below each figure, the mean for each group and their difference are reported, along with the t-value from a linear OLS regression comparing the two groups.

the high ability workers are more able to find new employment relative low ability workers. To investigate this, I compare the AKM fixed effects for the firms that the voluntary and displaced survivors match with after the first downsizing event. Figure 8d suggests that the voluntary movers are not only positively selected, but are also able to find better paying firms compared to other workers.

Is it also the case that it is the workers with the highest unemployment risk that are leaving the firm? I analyze this channel by defining circuits on the workplace $\times$ occupation level and rank the workers by their tenure compared to their circuit peers.[17] The idea behind this is that survivors with shorter tenure relative to their coworkers in the same job and workplace are more likely to be laid off, in line with LIFO rule. A value of 0 denotes the lowest rank — that is, being the most junior within a circuit — while a value of 1 indicates the highest seniority.

[17] I first split the sample by their notifying workplace and two-digit occupation. There are 26 occupation categories. Workers who are not sampled in the Wage Statistics are omitted from the analysis. Important to note is that the sampling is done in clusters of firm in each year, meaning that circuits are complete by event. Within these cells, I rank the workers by their tenure by summing the monthly indicators in the RAMS register.

Figure 8c presents the distribution of seniority ranks among surviving workers, distinguishing between those who leave voluntarily and those who are displaced in a future layoff event. The distributions reveal that voluntary leavers tend, on average, to hold higher ranks within the firm, whereas workers who are eventually displaced are more often found lower in the rank distribution. This pattern suggests that junior employees face a disproportionately higher risk of displacement in future layoffs, while more senior employees are relatively more likely to exit voluntarily.

The results suggest that it is the highest ability workers who choose to leave voluntarily. I now investigate which types of firms they go to using the difference in AKM fixed effects between the reemploying and notifying firms. The results are shown in Figure 8d.

Interestingly, the change in the firm fixed effect is on average negative relative to the notifying firm, regardless of whether the move is voluntary or involuntary. Workers who become displaced soon after the initial downsizing event match with employers paying on average 5.1% lower wages; workers who voluntarily leave match with employers paying 2.3% less than the notifying firm.

For involuntary leavers, this aligns with findings from the existing layoff literature, which consistently shows that displaced workers are often reemployed at lower-paying firms Davis and von Wachter (2011), Couch and Placzek (2010), and Schmieder, von Wachter, and Heining (2023). This pattern reflects the fact that displaced workers are generally willing to accept a worse-paying job because the outside option of being unemployed is considered even less desirable.

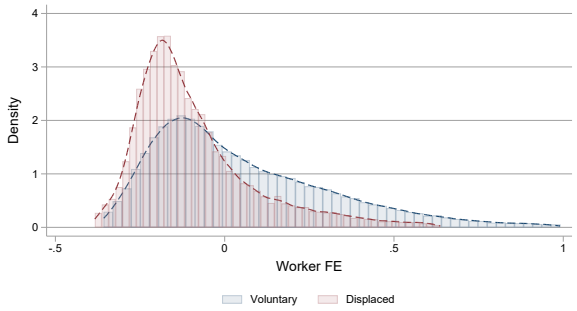
For voluntary leavers, a similar decline in firm fixed effects suggests a different mechanism. These workers may be willing to accept slightly lower-paying positions because the new employers are perceived as more stable or carry a lower risk of future layoffs. In other words, they may trade some wages for improved job security.[18]

In sum, voluntary movers are not only the primary source of excess separations, as established in Section 4.4, but they are also positively selected along several dimensions.

[18] I descriptively investigate if voluntary leavers are less likely to become notified in their new workplaces and find that that this is on average the case (Figure B.4).

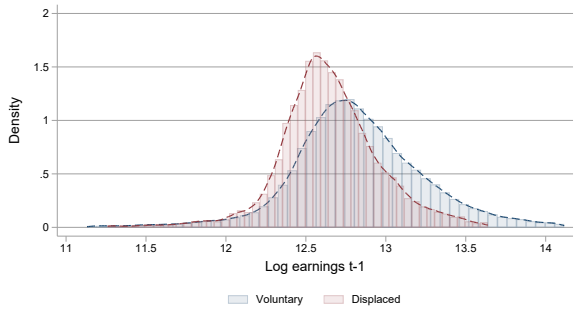
Figure 8: Distributions: Voluntary vs. Involuntary Movers

(a) Worker Fixed Effects



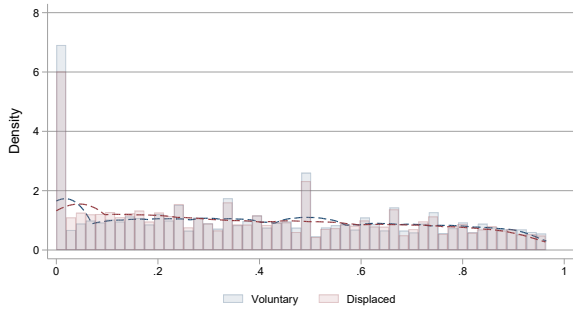
Voluntary : .06
Displaced : -.085
Diff: .145
t-value: 73.969

(b) Log Earnings, $t-1$



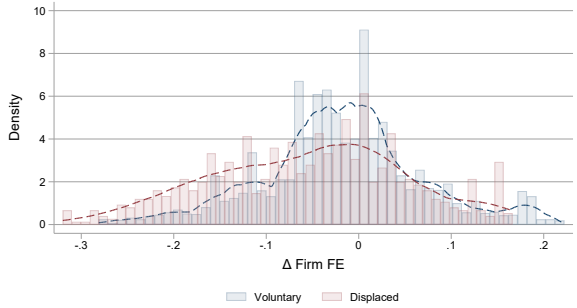
Voluntary : 12.814
Displaced : 12.637
Diff: .177
t-value: 46.836

(c) Seniority Ranking



Voluntary : .408
Displaced : .393
Diff: .015
t-value: 4.165

(d) Δ Firm Fixed Effects



Voluntary : -.019
Displaced : -.055
Diff: .036
t-value: 8.331

Notes: Figure 8a displays the distribution of AKM worker fixed effects; Figure 8b, log earnings in year $t-1$; Figure 8c, relative within workplace × occupation seniority; and Figure 8d, the difference between the firm fixed effects of the reemploying and notifying firms. The y-axes measure the density of workers, while the x-axes correspond to each of the four measures. Blue bars indicate survivors who are reemployed voluntarily, and red bars represent survivors who are displaced in a future layoff and subsequently reemployed. The dashed lines show fitted Epanechnikov kernel densities for each group. The two groups are defined three years after the initial layoff notification, following the procedure described in Section 4.4. Below each figure, the mean for each group and their difference are reported, along with the t-value from a linear OLS regression comparing the two groups.

Next, I examine whether this pattern depends on the type of shock: Does it matter how many workers are laid off? Does seniority influence the likelihood or concern of being displaced in the future? And does the tightness of the current labor market matter?

6. Adjustments to Different Kinds of Shocks

In this section, I examine how survivor adjustment varies across several worker, workplace, and event characteristics. This heterogeneity analysis is motivated by the premise that the specific circumstances of a downsizing event potentially influence worker reactions. I split the main sample along three main dimensions, each selected because they yield testable predictions regarding perceived or factual unemployment risk.[19]

First, I split the sample by event size, measured as the share of employees notified as a share of total employment. Beyond the fact that large events are likely more salient than small ones, large downsizing may lead surviving workers to feel relatively more concerned about the firm's future stability and the likelihood of further layoffs. Following the argument by Gibbons and Katz (1991), small downsizing events tend to reflect individual worker characteristics, while large-scale events are more likely attributable to general market conditions. In addition, previous evidence has shown that large layoff events may be associated with negative spillovers to the surrounding labor market, potentially making the event even more salient and important for the survivors (Gathmann, Helm, and Schönberg, 2020).

On the other hand, it may be that some firms choose to displace many workers as a first stage to avoid recurring downsizing. If this is the case, it is not obvious that the survivors will increase their search behavior more in larger events. If and how survivors' adjustment depends on event size is therefore theoretically ambiguous.

Second, I compare different workers within the same event and examine whether survivor adjustment varies by seniority. The reason why this might be important is due to institutional factors. Swedish law stipulates that employers must displace workers in reverse order of tenure, also known as the Last-In-First-Out (LIFO) rule (SFS, 1982). If concern about future layoffs is present among the survivors, junior workers are potentially

[19] In addition, psychological literature suggests that survivors exhibit more pronounced negative reactions when they identify closely with displaced workers (Brockner et al., 1987). Consequently, Appendix E explores further dimensions of this identification, such as whether the notified employee and the survivor share the same occupation.

more likely to react, as they face higher unemployment risk. Previous evidence has shown that relative seniority matters for who is displaced, indicating that this factor might be important for how workers react to the downsizing event (Cederlöf, 2024). Additionally, younger workers may be more prone to job-to-job transitions, even in the baseline (Topel and Ward, 1992; Michelacci and Ruffo, 2015).

On the other hand, there are certain exceptions embedded in the legal framework. For example, the rule is dispositive, meaning that the employer and the labor union are permitted to sidestep the LIFO rule by mutual agreement (SFS, 1976). There are also partial waivers for small employers which allows them to retain some junior workers even without the labor union's consent. If a large share of the surviving workers are subject to such an exception in the initial downsizing event, it might imply that they are selected by the employer as especially valuable. Consequently, these workers may be less concerned about future events if the initial event revealed their worth to the employer.

Third, I investigate the importance of labor market tightness for my results. The direction of the workers' reaction to tightness is theoretically ambiguous. On the one hand, there is the prediction that the expected payoff from searching is higher in tight labor markets (Pissarides, 2000). The intuition is that when the ratio of vacancies to job searchers is high, the job-finding rate for workers increases. In such an environment, the opportunity cost of remaining in a potentially unstable firm rises. This would then encourage precautionary separations from the downsizing workplace in my setting.

A contrasting argument is that the consequences of becoming unemployed are larger during slack labor markets; if a survivor is notified in a future event, the risk of long-term unemployment is likely higher if there are few vacancies relative to the number of job seekers. This would then potentially amplify the severity of the negative signal of the downsizing announcement. It is thus an open question if and how this affects survivor adjustment.

6.1. Adjustment to Large vs. Small Layoffs

To investigate the importance of the layoff size for the effects in Section 4, I first calculate the share displaced in each event. Next, I let worker i be in a large event if she is surviving an event e where the share notified exceeds the median, $\widetilde{\text{LargeEvent}} = 1$, and the rest as small events. This is described in Equation (3). Finally, I do the same event study analysis as before, estimating δ_k relative to the year before notification. The treated survivors keep the same matched controls as in previous sections.

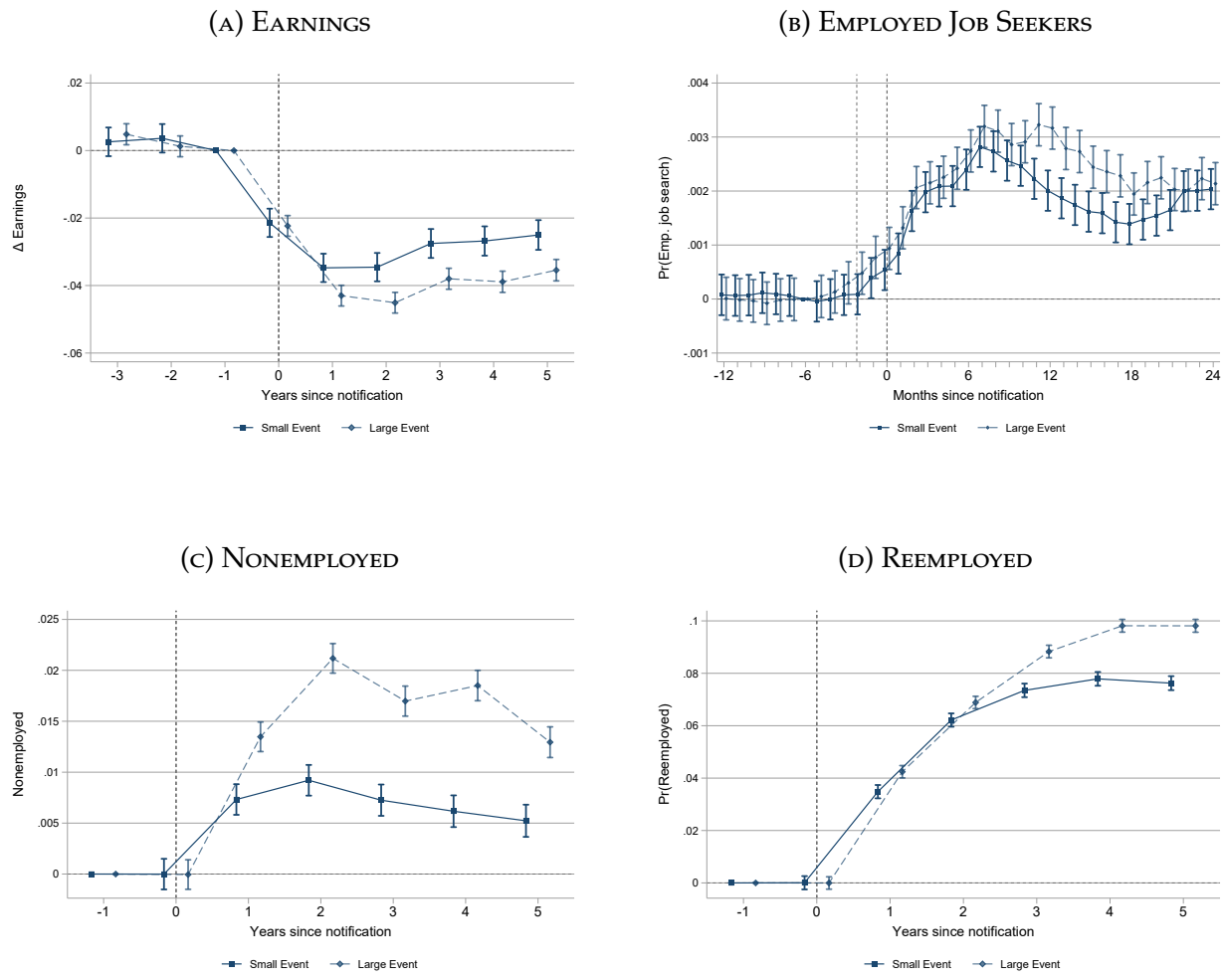
$$\begin{aligned} \text{ShareDisp}_e &= \frac{\text{Notified}_e}{\text{Employment}_e}, \\ \text{LargeEvent}_e &= \begin{cases} 1 & \text{if LargeEvent}_e > \widetilde{\text{LargeEvent}}, \\ 0 & \text{otherwise}, \end{cases} \end{aligned} \tag{3}$$

Figure 9a depicts the changes in earnings for the workers by the two event size groups. The squares (solid lines) represent the estimates for workers which are part of small events while the diamonds (dashed lines) represent those in large events. The results suggest that the effect on earnings varies with event size. Income drops are around 1 percentage point larger for workers who survive large events compared to small events. This difference emerges 1-2 years after notification and persists throughout my study period.

One potential reason is that the information effect is larger for events when many workers are laid off, leading to more voluntary separations. Another hypothesis is that large events are more positively associated with future layoffs and that the income drop is driven by excess nonemployment in the large event group.

Figure 9b depicts the share of employed job seekers among workers in large and small events respectively. Again, squares (solid lines) represent small events and diamonds (dashed lines) large events. The results show that large events trigger more workers to register as employed job seekers. This difference arises in the year after notification. This is an indication that the differences in earnings are to some extent due to workers increasing

Figure 9: Outcomes By Share of Workers Displaced



Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed. The figures show the evolution of the outcomes for survivors and their controls, split by event size. Figure 9a reports yearly earnings. Figure 9b reports the share of employed job seekers by months relative to notification. Small events (squares, solid lines) are defined as greater than or equal to the median layoff size, measured by the number of workers notified, while large events (diamonds, dashed lines) are below the median. The split is based on the value for the *survivors* and workers keep the same one-to-one controls as in the main analysis. The dashed vertical lines indicate the notification date and announcement date in the monthly figure. Figure 9c the share in nonemployment (i.e., monthly earnings $\leq$ 10 000SEK), Figure 9d the share working at a reemploying firm (i.e., no longer attached to the notifying firm and earning more than 10 000SEK in a newly occurring firm). Presented in table format in Appendix G.

their search behavior as a result of the layoff event.

Figure 9c shows the evolution of employment at the notifying firm for small and large events. Three years after notification, workers in large events are about 1 percentage point more likely to be nonemployed. This suggests that large events are more likely to be followed by involuntary separations, driven by subsequent layoffs. This increase can fully

explain the earnings differences between the two groups of survivors. Examining inflow into new firms, Figure 9d reveals that workers exposed to large events are also more likely to enter new firms three years after notification.[20]

In sum, the evidence indicates that the size of a downsizing event shapes worker outcomes. Workers in large events suffer income losses approximately one percentage point greater than those in smaller events. Coupled with a higher probability of entering non-employment, these findings suggest that larger events increase the risk of future displacement. Furthermore, survivors of large events increase their job-search behavior more compared to those in small events, suggesting that larger layoffs are more salient and predict increase in unemployment risk. Crucially, while this behavioral response is temporary, the negative impact on earnings remains persistent. Taken together, these results demonstrate that large layoffs amplify future job-loss risk, leaving lasting scars on workers' earnings and employment trajectories.

6.2. Adjustment of Junior vs. Senior Workers

In the previous subsection, the evidence suggested that larger layoff events are associated with greater earnings losses and more transitions both to new firms and into nonemployment.

I now shift the focus from differences across downsizing events to differences between workers in the same event. In particular, I examine the importance of relative unemployment risk for the results in Section 4. To do this, I use relative seniority at the workplace as a proxy for future unemployment risk. As described in Section 2.1, Swedish law stipulates that workers with short tenure should be displaced before those with long tenure, unless otherwise agreed upon between the employer and the labor union. While not legally binding, previous studies have shown that it has considerable predictive power for determining who is displaced (Landais et al., 2021; Cederlöf, 2024). This institutional feature implies that newly hired workers face a higher risk of displacement in future layoff

[20]When splitting the sample by the size of the sales drop, I get qualitatively similar results. These results are detailed in Figure E.9 in the appendix.

events than their more tenured colleagues.

Because the workforce has mechanically shrunk following the initial round of layoffs, some workers who previously occupied the middle of the tenure distribution find themselves at the bottom of the seniority ladder. By contrast, the most senior workers remain relatively safe in this regard. Comparing junior and senior workers thus allows me to disentangle the general effects, such as increased aggregate layoff risk and organizational changes, from the specific risk borne by individual workers due to their position in the seniority ranking. Consequently, any observed heterogeneity by seniority potentially reflects the specific risk among junior workers.

To analyze this mechanism, I use the same seniority measure as in Section 5. I first group the survivors into circuits based on their two-digit SSYK occupation code and workplace.[21] Within each circuit, I then rank workers by tenure, breaking ties using age in accordance with the LIFO rule. If survivor i is ranked below the median in circuit q, she is classified as junior, while workers above are classified as senior, as described in Equation (4). As in Section 6.1, I keep the same matched controls as before.

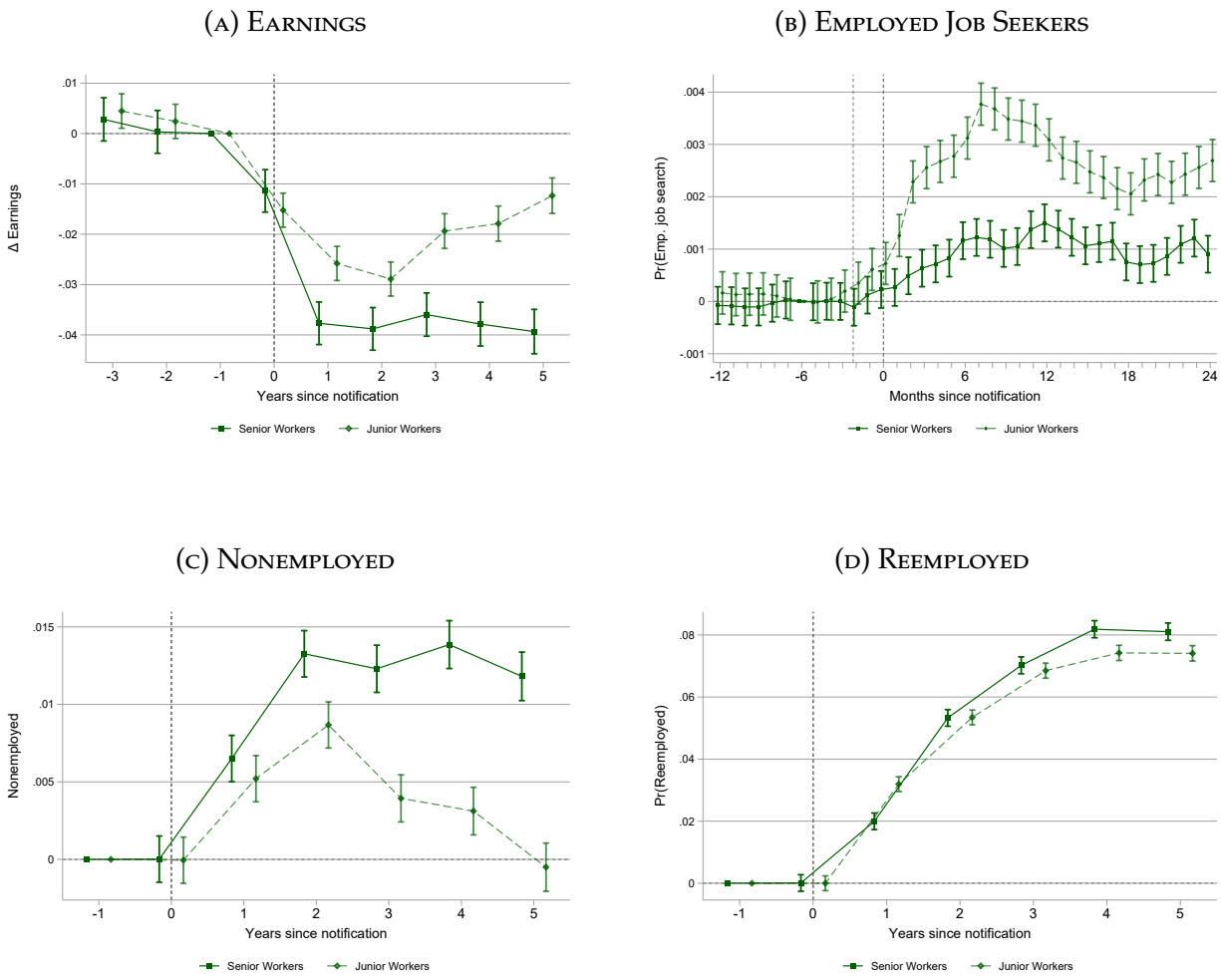
$$\text{Rank}_{i,q} = \text{rank}\Big(\text{Tenure}_{i,q},\ \text{Age}_{i,q}\Big), \qquad \text{Senior}_{i,q} = \begin{cases} 1 & \text{if Rank}_{i,q} > \widetilde{\text{Rank}}_q, \\ 0 & \text{otherwise,} \end{cases} \tag{4}$$

Figure 10a shows the evolution of earnings for the two seniority groups relative to the year before notification. The squares (solid lines) shows the estimates for the senior workers while the diamonds (dashed lines) shows the equivalent for the junior workers. When comparing the two groups, senior workers have lower earnings compared to junior workers 3-5 years after notification.

I then examine how the likelihood of becoming an employed job seeker varies between the two groups. The evidence in Figure 10b indicates that search intensity increases more sharply for junior workers, suggesting that they are more concerned about their future

[21] Swedish law stipulates that the seniority ranking should be done within collective bargaining agreement. However, because CBA information is not recorded in the register data, I follow Cederlöf (2024) and proxy the cells using occupation instead.

Figure 10: Outcomes By Seniority



Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed. The figures show the evolution of the outcomes for survivors and their controls, split by worker seniority. Figure 10a reports yearly earnings. Figure 10b reports the share of employed job seekers by months relative to notification. Junior workers (squares, solid lines) are defined as greater than or equal to the median workplace tenure, measured within occupation, while senior workers (diamonds, dashed lines) are above the median. The split is based on the value for the *survivors* and workers keep the same one-to-one controls as in the main analysis. The dashed vertical lines indicate the notification date and announcement date in the monthly figure. Figure 10c the share in nonemployment (i.e., monthly earnings $\leq$ 10 000SEK), Figure 10d the share working at a reemploying firm (i.e., no longer attached to the notifying firm and earning more than 10 000SEK in a newly occurring firm). Presented in table format in Appendix G.

employment prospects. The effect is visible shortly after the downsizing event is announced as highlighted by the leftmost dashed vertical line. The effect then becomes statistically significant shortly after notification.

This suggests that the behavioral response to layoffs differs across the seniority distribution. An explanation for this pattern could be that the junior workers are more concerned about

their own prospects at the firm and therefore search more intensely for new employment.

Next, I examine whether the increased job search among junior workers translates into actual mobility. When investigating the inflow into nonemployment, the effect is more pronounced for senior workers, as shown in Figure 10c. This indicates that although junior workers are ex ante more likely to be displaced due to LIFO rule, the earnings and employment results suggest this is not the case in practice.

One potential explanation is that a select group of junior workers increases their search effort and moves voluntarily. While the two groups do not differ in the extent to which they find new employment (Figure 10d), junior workers who *leave* are potentially more selected than their senior counterparts. While poorly-matched juniors choose to leave, poorly matched senior workers do not move to the same extent. This may be because senior workers possess more firm-specific human capital or face poorer outside options compared to junior workers. Consequently, they are displaced at higher rates in subsequent events despite having stronger formal employment protection.

Building on the analysis of event size and relative seniority, I next examine how labor market tightness influences the magnitude of worker adjustment.

6.3. Adjustment in Slack vs. Tight Labor Markets

Another possible source of heterogeneity is the tightness of the local labor market at the time of the event, as explained above. The direction of survivors' reaction is ex ante ambiguous.

Theory suggests that search effort should be higher in tight labor markets, reflecting both increased job-finding probability and higher expected quality of the new job (Pissarides, 2000).

In contrast, if workers are risk-averse, they may intensify their job search efforts during slack labor markets since the risk of long-term unemployment following displacement is greater. Conversely, when the labor market is tight, surviving workers might feel less motivated to search preemptively if they believe unemployment is unlikely. Hence, the

degree of labor market tightness may shape how surviving workers react to the layoff signal, although the direction of the effect remains an open question.

To investigate this relationship, I construct a measure of local labor market tightness by combining data on unemployment and vacancies in Sweden. First, I pull job advertisements posted on the PES portal (*Platsbanken*), the largest job board in Sweden. It covers the vast majority of publicly posted vacancies and has been used by Eurostat to estimate the total number of job ads in Sweden (Hensvik, Le Barbanchon, and Rathelot, 2020).

Next, I combine these data with the total number of registered unemployed individuals along with the local labor market where they are registered. I then calculate tightness by dividing the total number of vacancies with the number of unemployed individuals for each local labor market l and notification year c, as expressed in Equation (5).[22]

$$\text{Tightness}_{l,c} = \frac{\text{Vacancies}_{l,c}}{\text{Unemployed}_{l,c}}, \text{Tight}_{l,c} = \begin{cases} 1 & \text{if Tightness}_{l,c} > \widetilde{\text{Tightness}}_{l}, \\ 0 & \text{otherwise} \end{cases} \tag{5}$$

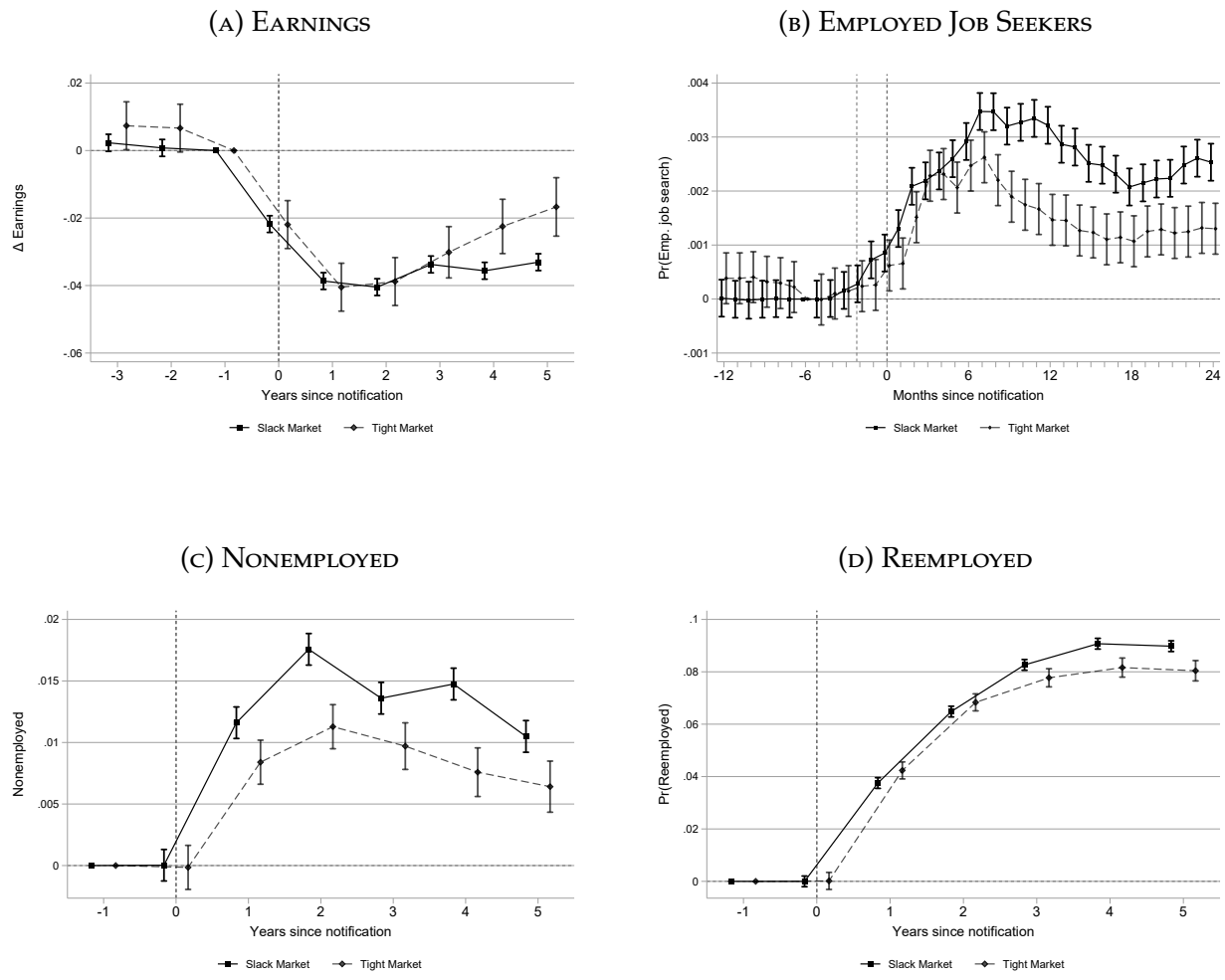
Because I do not want to compare urban to rural labor markets, I define tight and slack markets within local labor market over time. I classify events as tight if the tightness is above the within local labor market median during my time period and slack for below median cases. The comparison is therefore between workers in the same local labor market but meeting different labor market conditions due to business cycle fluctuations.

Figure 11a shows that the income evolution is similar for slack and tight labor markets. The squares (solid lines) indicate tight labor markets while the diamonds (dashed lines) indicate slack labor markets. However, Figure 11b shows that survivors in slack labor markets are more likely to register as employed job seekers compared to workers in tight labor markets.

This suggests that workers in slack labor markets feel more concerned when the workplace starts downsizing. This result supports the hypothesis that the information channel is

[22]The local labor markets are defined by Statistics Sweden based on commuting patterns, which capture economically integrated regions rather than administrative boundaries.

Figure 11: Outcomes By Labor Market Tightness



Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed. The figures show the evolution of the outcomes for survivors and their controls, split by labor market tightness. Figure 11a reports yearly earnings. Figure 11b reports the share of employed job seekers by months relative to notification. Slack events (squares, solid lines) are defined as lower than or equal to the median tightness, measured within local labor market, while tight events (diamonds, dashed lines) are above the median. The split is based on the value for the *survivors* and workers keep the same one-to-one controls as in the main analysis. The dashed vertical lines indicate the notification date and announcement date in the monthly figure. Figure 11c the share in nonemployment (i.e., monthly earnings $\leq$ 10 000SEK), Figure 11d the share working at a reemploying firm (i.e., no longer attached to the notifying firm and earning more than 10 000SEK in a newly occurring firm). Presented in table format in Appendix G.

important, as the perceived cost of job loss is likely to be larger in a slack labor market. The theoretical prediction that the value of job search is higher in tight markets seems to be relatively less important in this setting.

Figure 11c suggests that workers in slack labor markets are however more likely to enter nonemployment, indicating that involuntary separations are more common in slack markets.

There is however little evidence of it impacting moves from the notifying workplace to new employment (Figure 11d).

7. Conclusions

This study contributes to a growing literature on the broader consequences of layoffs, emphasizing that the effects extend beyond those who are directly displaced. While most previous research has focused on the earnings and employment outcomes of laid-off workers, this analysis shifts the focus from the displaced to the non-displaced survivors. By leveraging detailed administrative data on layoffs, I am able to (1) identify which workers are non-displaced, (2) study short-term effects between announcement, notification, and displacement, and (3) distinguish between voluntary and involuntary job-to-job moves.

I first investigate the earnings of the survivors and find that earnings drop by 3.8% in the year following notification and then recover to just under 3% five years after the event. This evidence provides a strong indication that the effects of layoffs extend beyond those directly displaced.

Examining the mechanisms behind the earnings drop, I find that layoff events lead to increased search effort. The effects are visible and significant when the downsizing event is announced, i.e. before individual workers are notified of layoff. After it has been determined who will be notified of layoff, job search continues to rise, suggesting widespread unease about the firm's prospects even among those who are not displaced.

I then turn to job mobility and find that the rise in search intensity translates into more job-to-job transitions. Three years after the event, an additional 9.2 percentage points of survivors have left the notifying firm, with 7.9 percentage points reemployed in new firms and the remainder exiting into nonemployment.

After establishing that moves to new jobs is the most important driver for mobility, I use the notification data from the PES to categorize the moves, and find that most transitions are voluntary. Next, I examine which types of moves drive the income drop and show

that the decline is driven almost entirely by the higher likelihood of eventually becoming nonemployed after surviving the initial layoff.

Turning to the characteristics of those who voluntarily leave, the results show that these workers have higher pre-separation earnings and educational attainment compared both to those who remain and to those later displaced. They are also positively selected, as indicated by their AKM individual fixed effects. This suggests that while employers may attempt to retain their most skilled employees, they are often unsuccessful: many of these workers voluntarily move to other firms.

Thus, the two main findings of the study are: (1) downsizing leads to a large increase in voluntary separations, and (2) the workers who leave are positively selected along several dimensions associated with productivity. The main explanation for the first result seems to be that workers interpret downsizing as a negative signal about the firm's prospects and update their beliefs about their unemployment risk. Specifically, the immediate increase in employed job seekers at the point of announcement supports an information mechanism: workers begin searching for new jobs in response to negative news about the firm's outlook.

Further evidence of the information effect is its concentration among workers facing the highest perceived risk, specifically those affected by: (1) Events involving many workers or large drops in sales, which are arguably more salient and perceived as more severe by the survivors; (2) Lower-seniority workers, who are more vulnerable given Swedish seniority rules; and (3) Events occurring under slack labor market conditions, where the expected difficulty of finding reemployment is higher. Consequently, these workers face the greatest risk of future displacement and unemployment, which incentivizes them to search more actively than their more tenured colleagues or those facing tighter labor markets.

To summarize, this study makes two main contributions. First, I show that downsizing events not only affect displaced workers but also trigger strategic responses among survivors, such as increased job search and mobility. To my knowledge, these responses have not previously been documented in the literature. Second, I document a surge in voluntary mobility among high-performing employees. When downsizing is intended to remove less productive workers or restructure the workforce, firms may instead face

unintended consequences if their most productive employees choose to leave voluntarily.

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A. Literature Review

Table A.1: Overview of Previous Studies

Author(s) (Year)	Country	Time	Tenure	Gender	Share
Jacobson et al. (1993)	United States	1975–1985	≥ 6	Both	≥ 30%
Eliason and Storrie (2006)	Sweden	1983–1999	None	Both	100%
Sullivan and von Wachter (2009)	United States	1974–1991	≥ 6	Males	≥ 30%
von Wachter et al. (2009)	United States	1974–2004	≥ 6	Males	≥ 30%
Couch and Placzek (2010)	United States	2002–2014	≥ 6	Both	≥ 30%
Davis and von Wachter (2011)	United States	1974–2008	≥ 3	Males	≥ 30%
Seim (2019)	Sweden	1999–2009	≥ 1.5	Males	≥ 30%
Burdett et al. (2020)	Germany	2002–2014	≥ 3	Males	≥ 30%
Lachowska et al. (2020)	United States	1981–2005	≥ 6	Both	≥ 30%
Schmieder et al. (2023)	United States	2000–2006	≥ 1	Both	≥ 30%
Flaeen et al. (2019)	United States	2000–2006	≥ 1	Both	≥ 30%
Athey et al. (2026)	Sweden	1997–2014	≥ 3	Both	≥ 30%
Cederlöf (2024)	Sweden	2005–2015	≥ 5	Both	≥ 30%
Häggkvist (2026) [this paper]	Sweden	2005–2019	≥ 3	Both	-

Notes: The table summarizes the sample restrictions in several of the most cited studies on the effects of job displacement. "Country" indicates the country from which the data originate. "Tenure" specifies the minimum tenure required for inclusion in the study, while "Gender" indicates whether both sexes or only males are included. "Share" refers to the minimum share of workers required for inclusion in the study. The order and selection of studies is made by the author.

B. Descriptive Evidence

Table B.1: Descriptive Statistics By Event

	Mean	**Median**	**SD**	**Min**	**Max**
Event Characteristics					
Plant size	52.94	32.00	62.95	1	560
# Notified	7.71	4.00	13.82	1	444
# Survivors	46.98	26.00	60.66	1	555
Share notified	0.17	0.08	0.23	0	1
Notice time (days)	108.21	87.00	80.27	0	690
Arrival time (months)	1.94	1.00	1.87	0	12
Cause					
Streamlining	0.49	0.00	0.50	0	1
Downsizing	0.08	0.00	0.27	0	1
Other	0.43	0.00	0.50	0	1
Sector					
Private White Collar	0.19	0.00	0.40	0	1
Private Blue Collar	0.18	0.00	0.38	0	1
Public	0.01	0.00	0.10	0	1
Missing	0.63	1.00	0.48	0	1
Observations	7899				

Notes: The table reports descriptive statistics for event-level variables. SD stands for the standard deviation, min the minimum observed value, and max the maximum observed value. Plant size denotes the total number workers in each establishment, # Notified the number of workers receiving notice, and # Survivors the number of remaining coworkers. Share notified is the proportion of workers notified among all employees in the workplace. Notice time (days) and Arrival time (months) represent the average notice period and time between announcement and notification, respectively. Streamlining and Downsizing indicate the reported cause of the layoff event, while Other/Missing captures unclassified categories. Private White Collar, Private Blue Collar, and Public/Missing denote the sector of the workplace. Reported statistics include the mean, standard deviation, minimum, and maximum values.

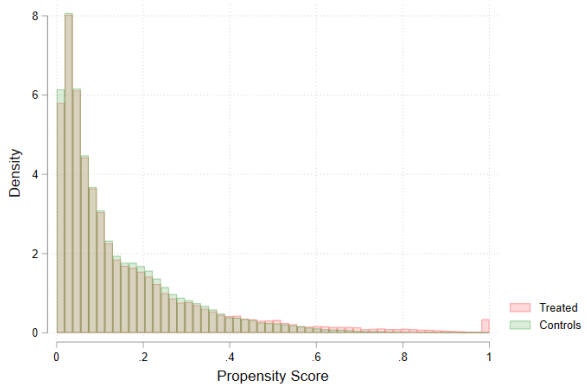
Table B.2: Descriptive Statistics By Status Three Years After Notification

	Not Reemployed		Reemployed	
	Not. Firm	Nonemployed	Displaced	Voluntary
Log earnings t-1	12.75	12.66	12.64	12.81
	(0.447)	(0.637)	(0.414)	(0.519)
Tenure	140.3	113.4	116.4	113.3
	(79.05)	(69.85)	(70.05)	(70.96)
Experience	249.3	221.2	231.3	238.4
	(77.79)	(87.20)	(80.94)	(80.54)
Age	42.37	41.15	40.84	40.74
	(7.519)	(8.202)	(7.797)	(7.589)
Pr(Woman)	0.289	0.336	0.278	0.300
	(0.453)	(0.472)	(0.448)	(0.458)
Pr(Immigrant)	0.133	0.198	0.155	0.119
	(0.340)	(0.399)	(0.362)	(0.323)
Pr(Married)	0.459	0.421	0.429	0.480
	(0.498)	(0.494)	(0.495)	(0.500)
Pr(College)	0.197	0.244	0.168	0.317
	(0.398)	(0.429)	(0.374)	(0.465)
Observations	249120	25034	15573	81347

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

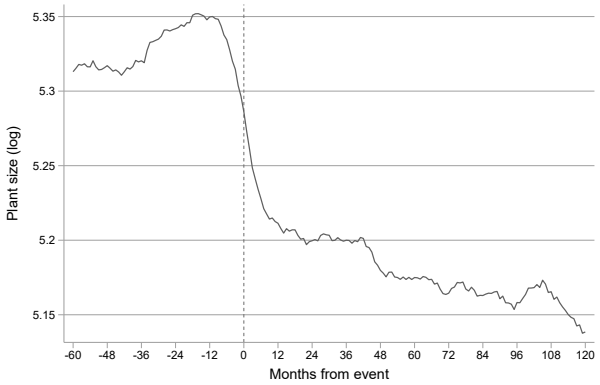
Notes: The table presents descriptive statistics for treated survivors measured three years (36 months) after notification. Values are sample means with standard deviations in parentheses. "Not. Firm" refers to workers who remain at the notifying firm, "Nonemployed" to those with monthly earnings below 10 000SEK, "Displaced" to survivors who are reemployed after being laid off in a future event at the notifying firm, and "Voluntary" to those reemployed without having been displaced. All variables are measured at the time of notification, except log earnings, which are measured in the year prior.

Figure B.1: Propensity Scores



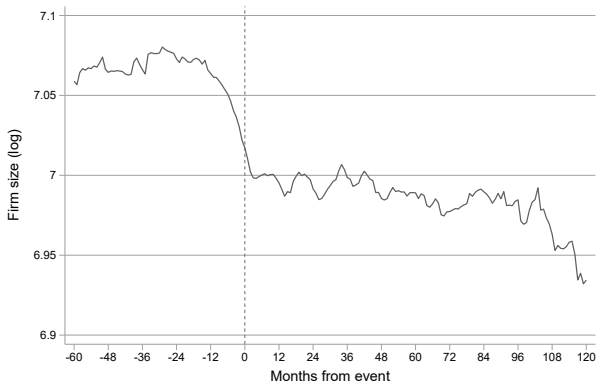
Notes: Figure B.1 shows the density of propensity scores for survivors and their matched controls. Propensity scores are estimated using the matching method detailed in Section 2.2. The red bars are the propensity scores for the treated survivors while the green bars are for the matched controls.

Figure B.2: Evolution of Plant Size



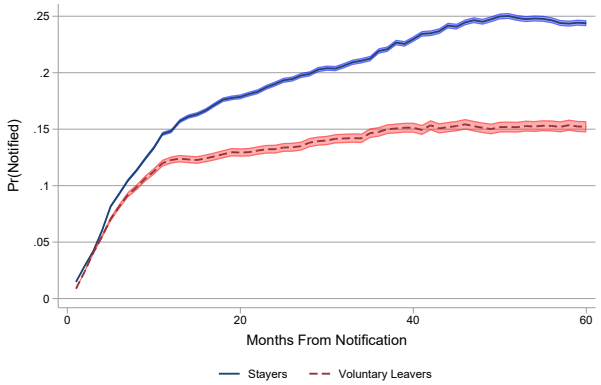
Notes: This figure shows the evolution of log plant size for each workplace in my sample by month relative to notification. The vertical dashed line highlights the month of notification.

Figure B.3: Evolution of Firm Size



Notes: This figure shows the evolution of log firm size for each workplace in my sample by month relative to notification. The vertical dashed line highlights the month of notification.

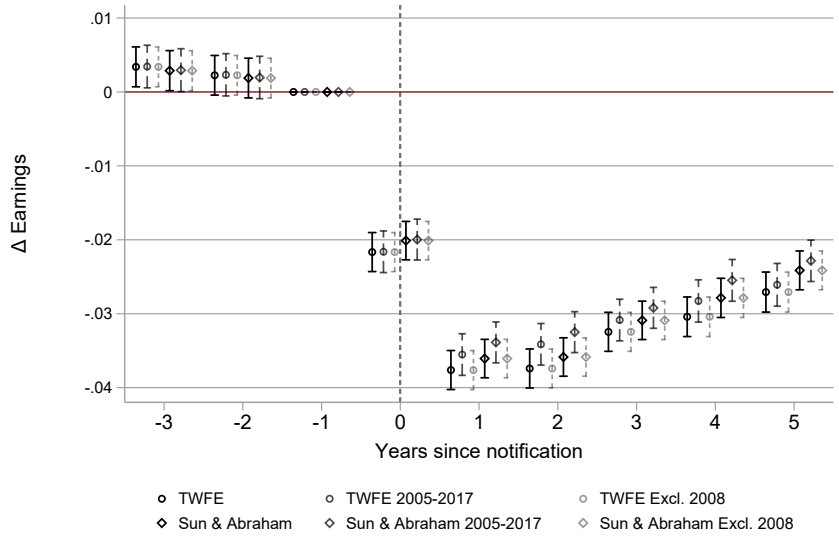
Figure B.4: Notification Incidence By Stayers and Voluntary Leavers



Notes: This figure shows the incidence of the likelihood of being notified by month since the layoff. The sample is divided into workers who remain at the notifying plant 36 months after notification (stayers) and those who have voluntarily left (voluntary leavers). For a worker to have voluntarily left the notifying firm, they must have both been reemployed at a new workplace and not been notified from their initial workplace where they became a survivor. The blue solid line depicts the difference between survivors and their controls for the stayers, while the red dashed line shows the corresponding difference for the voluntary leavers. The shaded areas represent 95% confidence intervals.

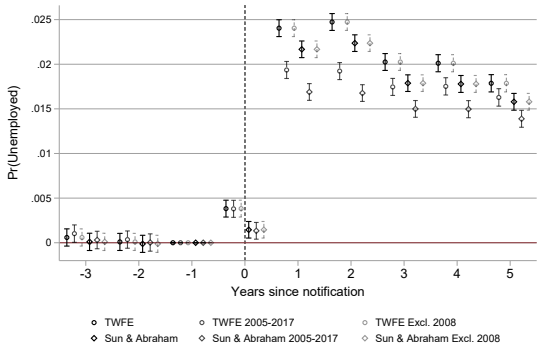
C. Robustness Analysis

Figure C.1: Evolution of Earnings: Different Estimators & Samples



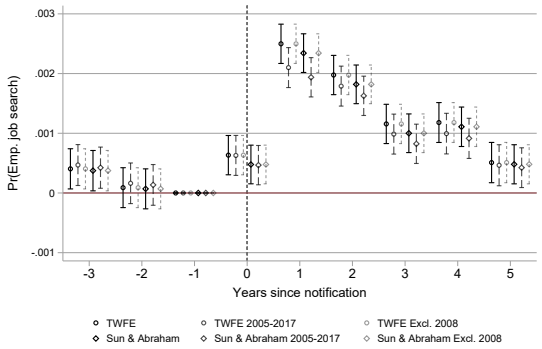
Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples are presented: the Main sample (dark markers); a sample excluding the years 2018 and 2019 (medium dark), as these observations lack the full five-year follow-up period necessary for the analysis; and finally, a sample excluding 2008 observations (light gray).

Figure C.2: Unemployed: Different Estimators & Samples



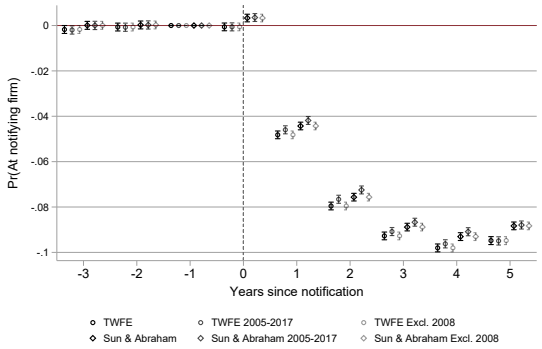
Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples are presented: the Main sample (dark markers); a sample excluding the years 2018 and 2019 (medium dark), as these observations lack the full five-year follow-up period necessary for the analysis; and finally, a sample excluding 2008 observations (light gray).

Figure C.3: Employed Job Seeker: Different Estimators & Samples



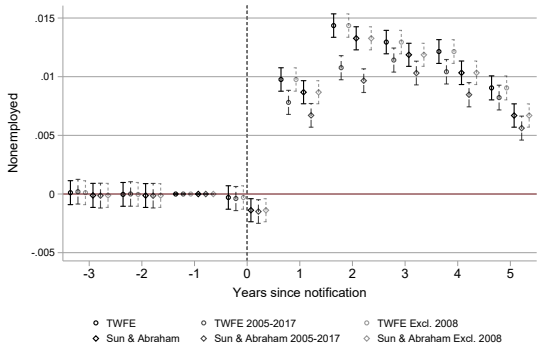
Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples are presented: the Main sample (dark markers); a sample excluding the years 2018 and 2019 (medium dark), as these observations lack the full five-year follow-up period necessary for the analysis; and finally, a sample excluding 2008 observations (light gray).

Figure C.4: Working at Notifying Firm: Different Estimators & Samples



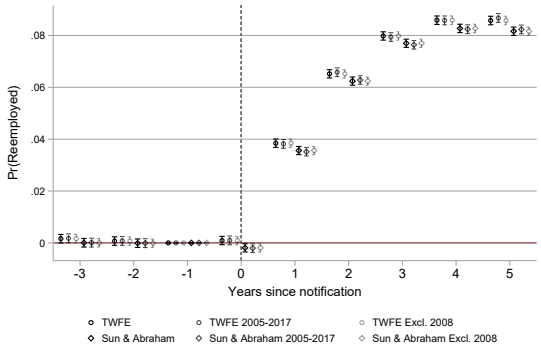
Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples are presented: the Main sample (dark markers); a sample excluding the years 2018 and 2019 (medium dark), as these observations lack the full five-year follow-up period necessary for the analysis; and finally, a sample excluding 2008 observations (light gray).

Figure C.5: Nonemployed: Different Estimators & Samples



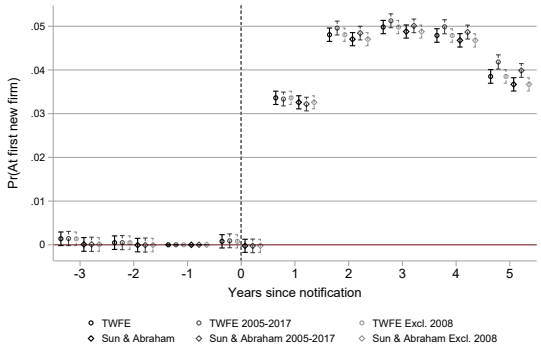
Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples are presented: the Main sample (dark markers); a sample excluding the years 2018 and 2019 (medium dark), as these observations lack the full five-year follow-up period necessary for the analysis; and finally, a sample excluding 2008 observations (light gray).

Figure C.6: Reemployed: Different Estimators & Samples



Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples are presented: the Main sample (dark markers); a sample excluding the years 2018 and 2019 (medium dark), as these observations lack the full five-year follow-up period necessary for the analysis; and finally, a sample excluding 2008 observations (light gray).

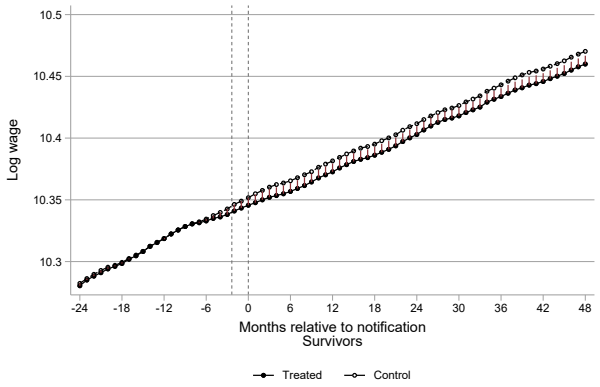
Figure C.7: First New Firm: Different Estimators & Samples



Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples are presented: the Main sample (dark markers); a sample excluding the years 2018 and 2019 (medium dark), as these observations lack the full five-year follow-up period necessary for the analysis; and finally, a sample excluding 2008 observations (light gray).

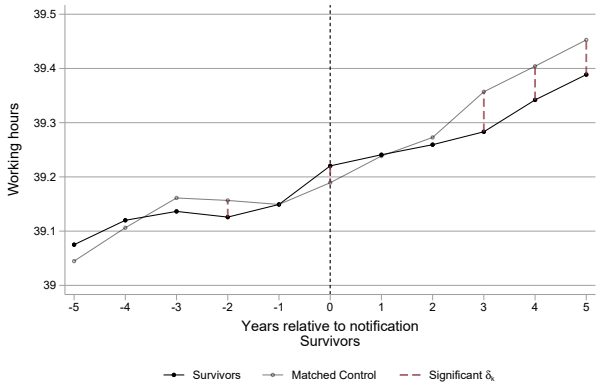
D. Additional Outcomes

Figure D.1: Evolution of Wages



Notes: The solid lines show the levels of the outcome for survivors (filled dots) and their controls (hollow dots) respectively. The red dashed lines indicate months in which the δ_k as defined in Equation (1) are statistically significant at the 5% level. The dashed vertical lines indicate the mean notification ($t = 0$) and announcement dates for the survivor sample. Standard errors are clustered at the event level. This figure shows the evolution of log wages for the survivors and their matched controls.

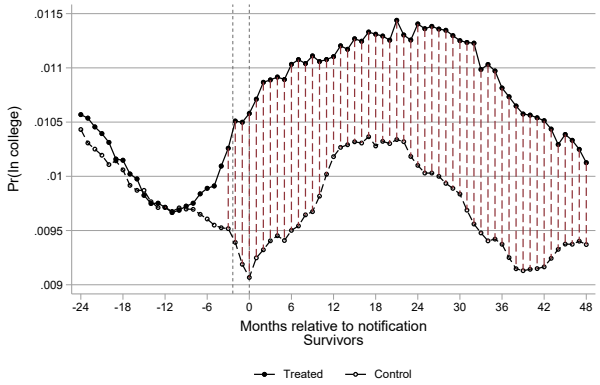
Figure D.2: Evolution of Hours Worked



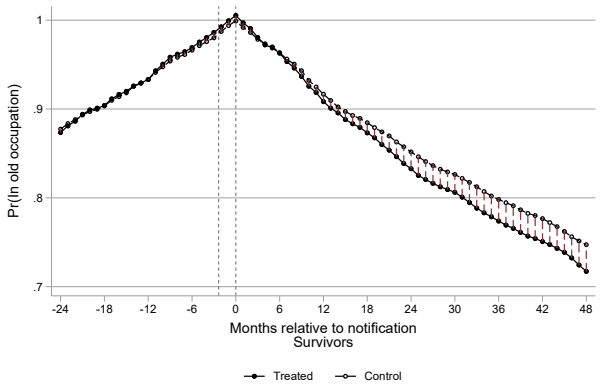
Notes: The solid lines show the levels of the outcome for survivors (filled dots) and their controls (hollow dots) respectively. The red dashed lines indicate months in which the δ_k as defined in Equation (1) are statistically significant at the 5% level. The dashed vertical lines indicate the mean notification ($t = 0$) and announcement dates for the survivor sample. Standard errors are clustered at the event level. The figure shows the evolution of contracted hours worked derived from the Wage Statistics.

Figure D.3: Educational Enrollment and Occupation Reassignment

(a) College Enrollment



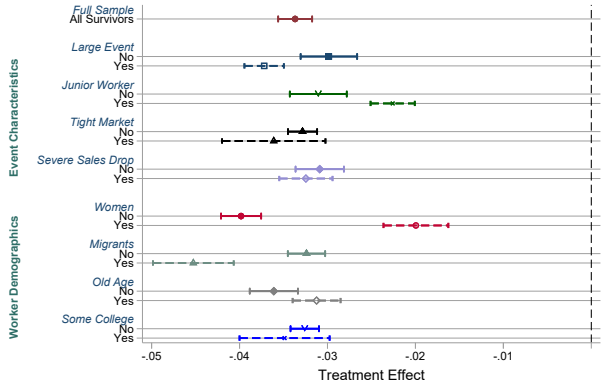
(b) Occupation Retention



Notes: The solid lines show the levels of the outcome for survivors (filled dots) and their controls (hollow dots) respectively. The red dashed lines indicate months in which the δ_k as defined in Equation (1) are statistically significant at the 5% level. The dashed vertical lines indicate the mean notification ($t = 0$) and announcement dates for the survivor sample. Standard errors are clustered at the event level. Figure D.3a shows the share enrolled in any university or university college and Figure D.3b the share of workers retaining the same two-digit SSYK occupation as in the notification month, by months relative to notification.

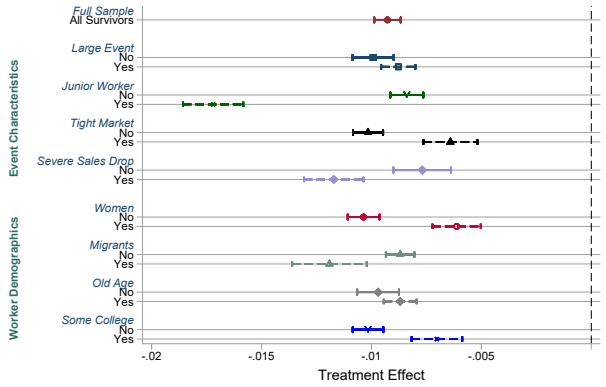
E. Heterogeneity Analysis

Figure E.1: Heterogeneity in Earnings



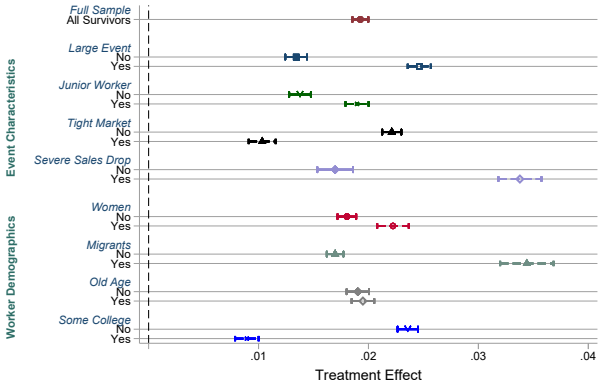
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker characteristics, detailed in Section 6. The dashed vertical line marks a zero treatment effect. Standard errors are clustered at the event level.

Figure E.2: Heterogeneity in log Wage



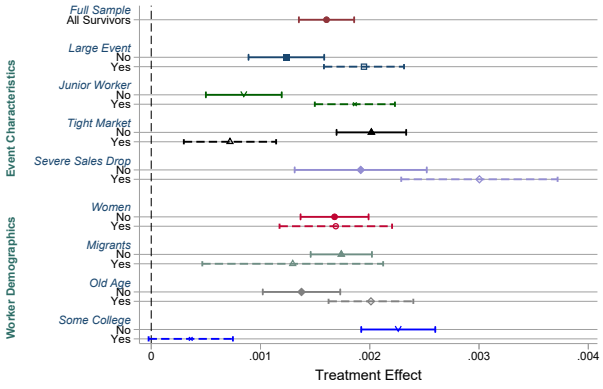
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker characteristics, detailed in Section 6. The dashed vertical line marks a zero treatment effect. Standard errors are clustered at the event level.

Figure E.3: Heterogeneity in Unemployment



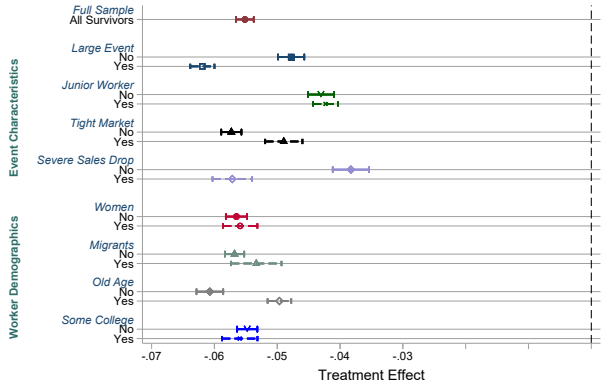
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker characteristics, detailed in Section 6. The dashed vertical line marks a zero treatment effect. Standard errors are clustered at the event level.

Figure E.4: Heterogeneity in Employed Job Seeker



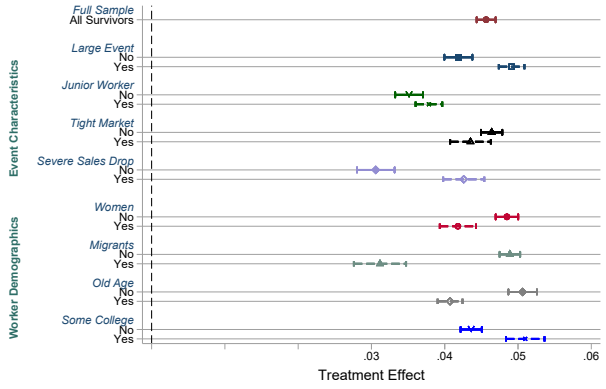
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker characteristics, detailed in Section 6. The dashed vertical line marks a zero treatment effect. Standard errors are clustered at the event level.

Figure E.5: Heterogeneity in Employed at Notifying Firm



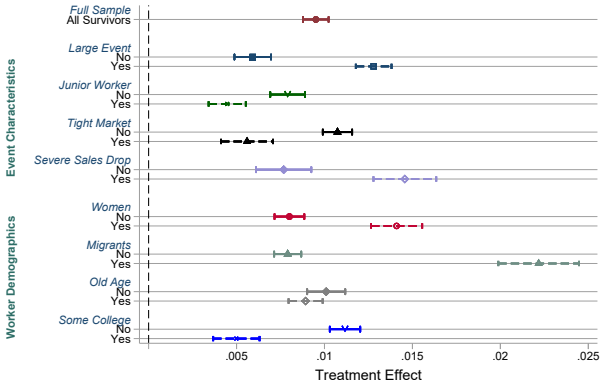
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker characteristics, detailed in Section 6. The dashed vertical line marks a zero treatment effect. Standard errors are clustered at the event level.

Figure E.6: Heterogeneity in Reemployed



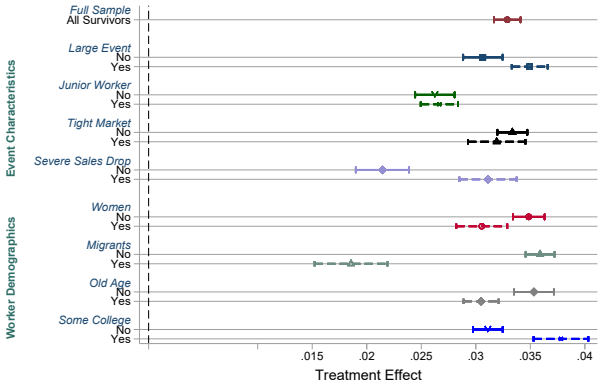
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker characteristics, detailed in Section 6. The dashed vertical line marks a zero treatment effect. Standard errors are clustered at the event level.

Figure E.7: Heterogeneity in Nonemployed



Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker characteristics, detailed in Section 6. The dashed vertical line marks a zero treatment effect. Standard errors are clustered at the event level.

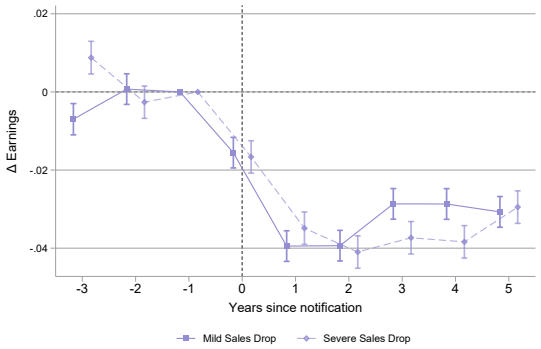
Figure E.8: Heterogeneity in Employed at First New Firm



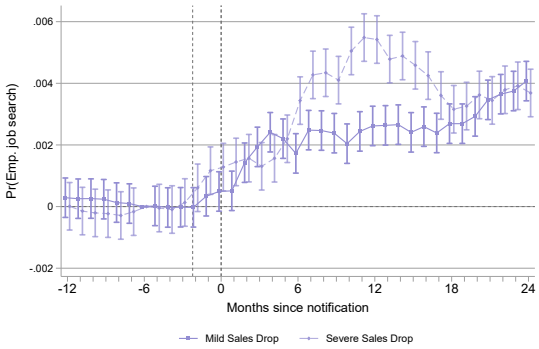
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker characteristics, detailed in Section 6. The dashed vertical line marks a zero treatment effect. Standard errors are clustered at the event level.

Figure E.9: Outcomes By Sales Drop Severity

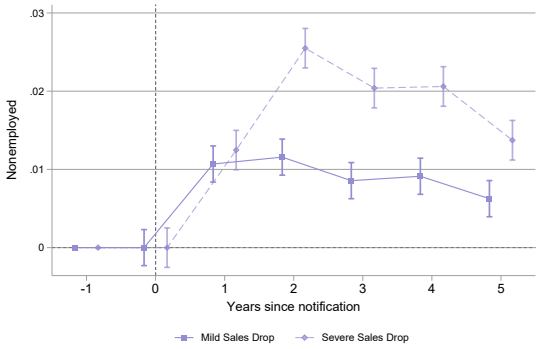
(a) Earnings



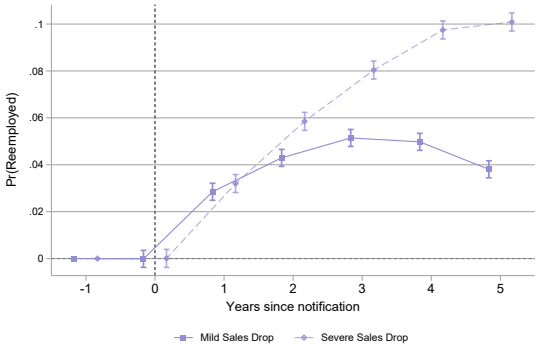
(b) Employed Job Seekers



(c) Nonemployed



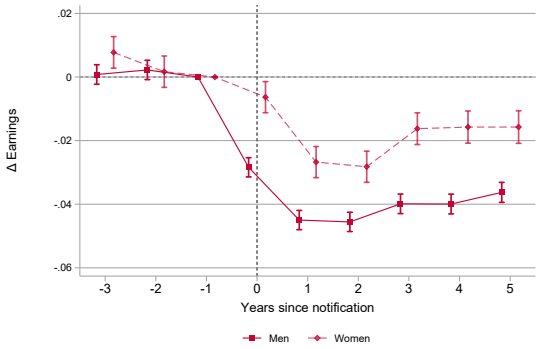
(d) Reemployed



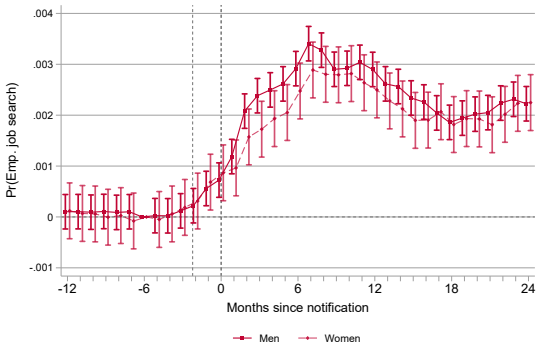
Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed. The figures show the evolution of the outcomes for survivors and their controls, split by sales drop severity. Figure E.9a reports yearly earnings. Figure E.9b reports the share of employed job seekers by months relative to notification. Mild events (squares, solid lines) are defined as greater than or equal to the median sales drop, measured between year $t-1$ and t, while severe events (diamonds, dashed lines) are above the median. The split is based on the value for the *survivors* and workers keep the same one-to-one controls as in the main analysis. The dashed vertical lines indicate the notification date and announcement date in the monthly figure. Figure E.9c the share in nonemployment (i.e., monthly earnings $\leq$ 10 000SEK), Figure E.9d the share working at a reemploying firm (i.e., no longer attached to the notifying firm and earning more than 10 000SEK in a newly occurring firm).

Figure E.10: Outcomes By Gender

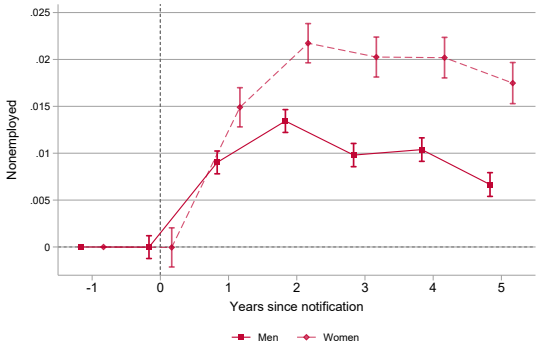
(a) Earnings



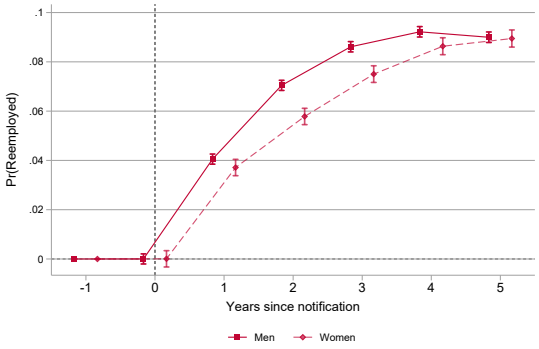
(b) Employed Job Seekers



(c) Nonemployed



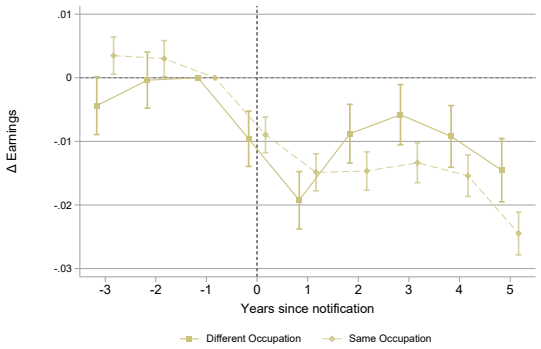
(d) Reemployed



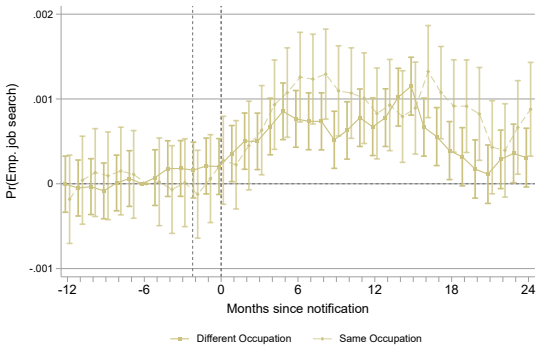
Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed. The figures show the evolution of the outcomes for survivors and their controls, split by gender. Figure E.10a reports yearly earnings. Figure E.10b reports the share of employed job seekers by months relative to notification. Men are depicted using squares and solid lines and women with diamonds and dashed lines. The split is based on the category for the *survivors* and workers keep the same one-to-one controls as in the main analysis. The dashed vertical lines indicate the notification date and announcement date in the monthly figure. Figure E.10c the share in nonemployment (i.e., monthly earnings $\leq$ 10 000SEK), Figure E.10d the share working at a reemploying firm (i.e., no longer attached to the notifying firm and earning more than 10 000SEK in a newly occurring firm).

Figure E.11: Outcomes By Occupation

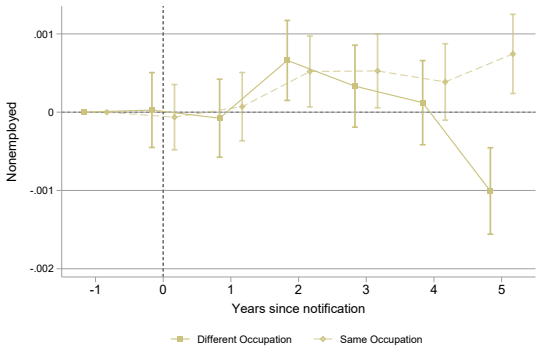
(a) Earnings



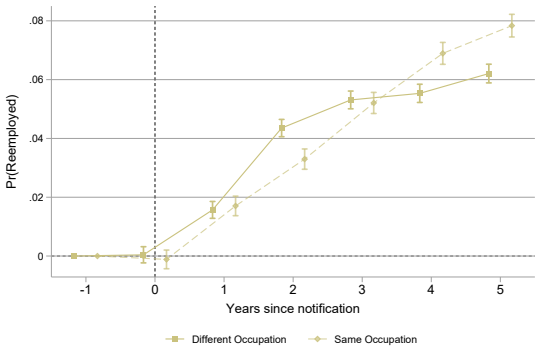
(b) Employed Job Seekers



(c) Nonemployed



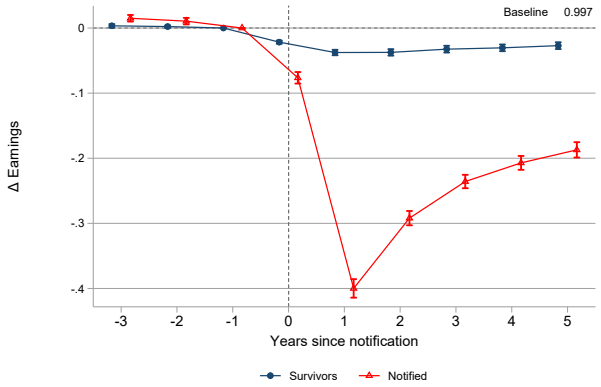
(d) Reemployed



Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed. The figures show the evolution of the outcomes for survivors and their controls, split by same vs. different occupation of the survivor and the most common occupation among notified in the event. Figure E.11a reports yearly earnings. Figure E.11b reports the share of employed job seekers by months relative to notification. Different Occupation (squares, solid lines) is defined as the same two digit occupation, comparing survivors and mode of the notified workers in the event, while same occupation (diamonds, dashed lines) are when the coincide. The split is based on the value for the *survivors* and workers keep the same one-to-one controls as in the main analysis. The dashed vertical lines indicate the notification date and announcement date in the monthly figure. Figure E.11c the share in nonemployment (i.e., monthly earnings $\leq$ 10 000SEK), Figure E.11d the share working at a reemploying firm (i.e., no longer attached to the notifying firm and earning more than 10 000SEK in a newly occurring firm).

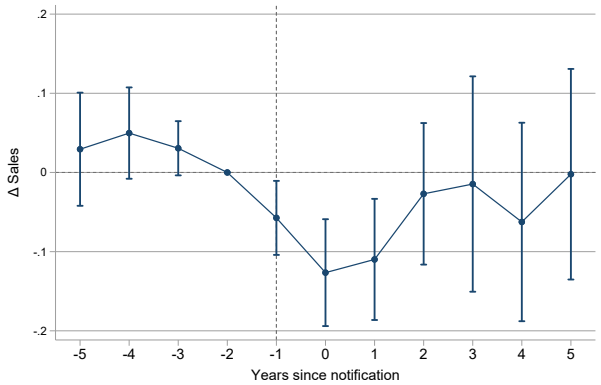
F. Additional Figures

Figure F.1: Evolution of Earnings For Notified and Survivors



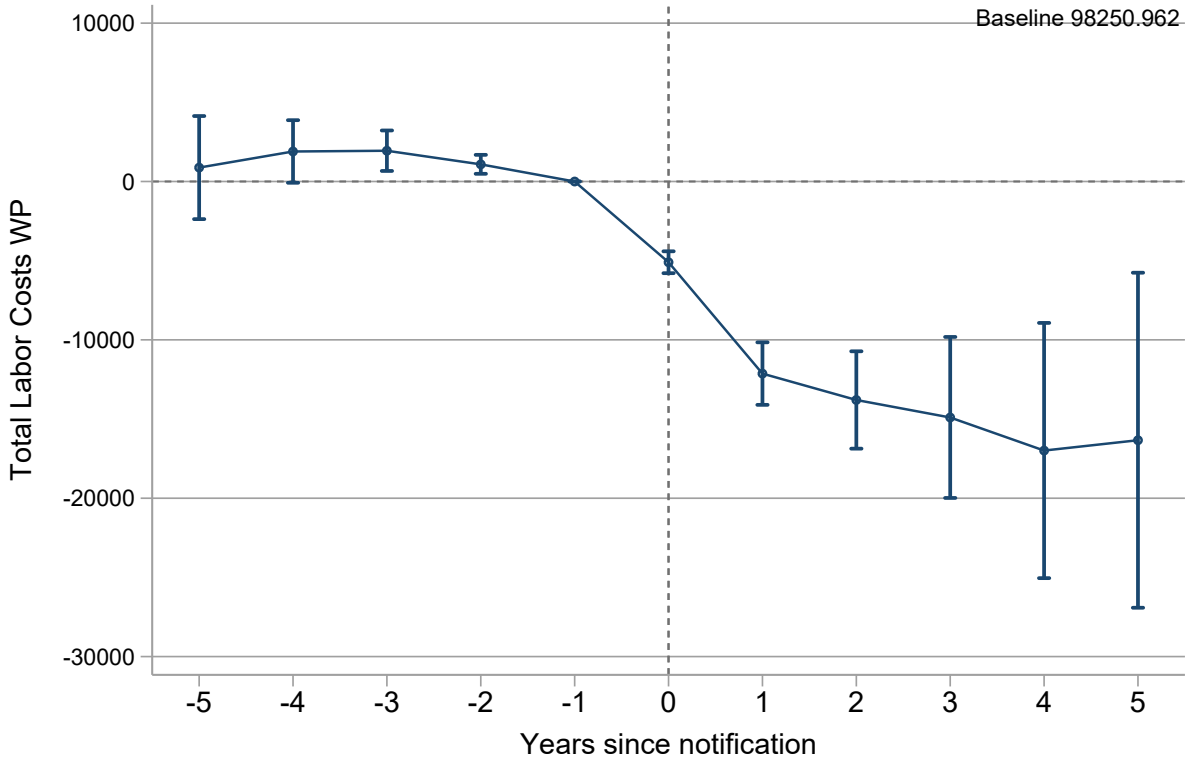
Notes: The outcome is labor earnings. The red line (triangles) shows the notified workers and the blue line (dots) the survivors. The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. The control groups are separately matched for the survivors and notified using the methodology described in Section 3. Standard errors are clustered at the event level, with 95% confidence intervals.

Figure F.2: Evolution of Sales



Notes: The outcome is differences in sales for workplaces in the treatment and control groups relative to two years before notification. The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals.

Figure F.3: Evolution of Total Labor Costs



Notes: The outcome is total labor costs (measured in SEK) for workplaces in the treatment and control groups. The estimates correspond to δ_k as defined in Equation (1) and the reference year is the year before notification. The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals.

G. Additional Tables

Table G.1: Outcomes

	(1) Δ Earnings	(2) log Wage	(3) Pr(Unemployed)	(4) Pr(Emp. job search)
t+1	-0.0376***	-0.00833***	0.0240***	0.00250***
	(0.00214)	(0.00139)	(0.00173)	(0.000363)
t+2	-0.0374***	-0.00800***	0.0247***	0.00198***
	(0.00256)	(0.00146)	(0.00193)	(0.000351)
t+3	-0.0325***	-0.00898***	0.0203***	0.00116***
	(0.00266)	(0.00144)	(0.00162)	(0.000317)
t+4	-0.0304***	-0.0103***	0.0201***	0.00118*
	(0.00263)	(0.00150)	(0.00171)	(0.000485)
t+5	-0.0271***	-0.0114***	0.0179***	0.000509*
	(0.00266)	(0.00166)	(0.00147)	(0.000234)
Time Fixed Effects	✓	✓	✓	✓
Worker Fixed Effects	✓	✓	✓	✓
Control Mean	0.991	10.32	0.00959	0.00156
R-squared	0.732	0.950	0.374	0.233
N	6441912	3429478	6441912	6441912

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual for each group are their matched controls. Estimates are measured in years relative to the notification. Standard errors are clustered at the event level.

Table G.2: Outcomes

	(1) Pr(At notifying firm)	(2) Nonemployed	(3) Pr(Reemployed)	(4) Pr(At first new firm)
t+1	-0.0482***	0.00976***	0.0384***	0.0336***
	(0.00330)	(0.00100)	(0.00291)	(0.00274)
t+2	-0.0796***	0.0144***	0.0652***	0.0481***
	(0.00475)	(0.00153)	(0.00409)	(0.00362)
t+3	-0.0927***	0.0129***	0.0798***	0.0498***
	(0.00567)	(0.00154)	(0.00505)	(0.00440)
t+4	-0.0980***	0.0121***	0.0859***	0.0479***
	(0.00619)	(0.00145)	(0.00555)	(0.00471)
t+5	-0.0948***	0.00905***	0.0858***	0.0385***
	(0.00639)	(0.00130)	(0.00580)	(0.00468)
Time Fixed Effects	✓	✓	✓	✓
Worker Fixed Effects	✓	✓	✓	✓
Control Mean	1.001	-0.0000932	-0.000435	-0.000234
R-squared	0.520	0.327	0.476	0.400
N	6441912	6441912	6441912	6441912

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual for each group are their matched controls. Estimates are measured in years relative to the notification. Standard errors are clustered at the event level.

Table G.3: Notifying Firm

	Event Size		Seniority		Tightness		Sales Drop		Gender	
	(1) Small	(2) Large	(3) Senior	(4) Junior	(5) Slack	(6) Tight	(7) Mild	(8) Severe	(9) Men	(10) Women
t+1	-0.0424***	-0.0536***	-0.0259***	-0.0360***	-0.0487***	-0.0509***	-0.0394***	-0.0443***	-0.0495***	-0.0519***
	(0.00127)	(0.00119)	(0.00134)	(0.00119)	(0.00105)	(0.00161)	(0.00190)	(0.00197)	(0.00105)	(0.00168)
t+2	-0.0704***	-0.0881***	-0.0655***	-0.0606***	-0.0836***	-0.0754***	-0.0548***	-0.0838***	-0.0838***	-0.0795***
	(0.00127)	(0.00119)	(0.00134)	(0.00119)	(0.00105)	(0.00161)	(0.00190)	(0.00197)	(0.00105)	(0.00169)
t+3	-0.0789***	-0.106***	-0.0820***	-0.0715***	-0.0982***	-0.0803***	-0.0601***	-0.101***	-0.0957***	-0.0951***
	(0.00127)	(0.00119)	(0.00134)	(0.00119)	(0.00105)	(0.00171)	(0.00190)	(0.00198)	(0.00107)	(0.00172)
t+4	-0.0817***	-0.113***	-0.0928***	-0.0744***	-0.108***	-0.0795***	-0.0593***	-0.118***	-0.102***	-0.106***
	(0.00129)	(0.00121)	(0.00136)	(0.00121)	(0.00105)	(0.00179)	(0.00190)	(0.00198)	(0.00108)	(0.00174)
t+5	-0.0806***	-0.108***	-0.0919***	-0.0708***	-0.102***	-0.0790***	-0.0446***	-0.115***	-0.0963***	-0.106***
	(0.00130)	(0.00122)	(0.00138)	(0.00122)	(0.00105)	(0.00190)	(0.00191)	(0.00198)	(0.00109)	(0.00176)
Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	1.001	1.000	1.000	1.001	1.000	0.999	1.000	1.000	1.000	1.001
R-squared	0.521	0.519	0.491	0.535	0.514	0.519	0.486	0.517	0.512	0.517
N	3092090	3349822	2614179	3827733	4229076	1641246	1226929	1315903	4107029	1654022

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual for each group are their matched controls. Estimates are measured in years relative to the notification. Standard errors are clustered at the event level.

Table G.4: Reemployed

	Event Size		Seniority		Tightness		Sales Drop		Gender	
	(1) Small	(2) Large	(3) Senior	(4) Junior	(5) Slack	(6) Tight	(7) Mild	(8) Severe	(9) Men	(10) Women
t+1	0.0348***	0.0424***	0.0200***	0.0319***	0.0370***	0.0439***	0.0287***	0.0319***	0.0405***	0.0371***
	(0.00123)	(0.00114)	(0.00128)	(0.00115)	(0.000982)	(0.00158)	(0.00176)	(0.00186)	(0.000994)	(0.00160)
t+2	0.0621***	0.0689***	0.0533***	0.0534***	0.0637***	0.0715***	0.0430***	0.0584***	0.0703***	0.0578***
	(0.00123)	(0.00114)	(0.00128)	(0.00115)	(0.000983)	(0.00158)	(0.00176)	(0.00186)	(0.000995)	(0.00160)
t+3	0.0736***	0.0882***	0.0702***	0.0685***	0.0824***	0.0781***	0.0515***	0.0804***	0.0859***	0.0750***
	(0.00125)	(0.00116)	(0.00130)	(0.00117)	(0.000984)	(0.00168)	(0.00176)	(0.00186)	(0.00101)	(0.00163)
t+4	0.0778***	0.0978***	0.0817***	0.0741***	0.0911***	0.0790***	0.0501***	0.0976***	0.0919***	0.0859***
	(0.00127)	(0.00117)	(0.00132)	(0.00118)	(0.000985)	(0.00177)	(0.00176)	(0.00186)	(0.00102)	(0.00165)
t+5	0.0761***	0.0979***	0.0808***	0.0740***	0.0901***	0.0776***	0.0382***	0.101***	0.0897***	0.0891***
	(0.00129)	(0.00118)	(0.00134)	(0.00120)	(0.000986)	(0.00188)	(0.00176)	(0.00186)	(0.00103)	(0.00167)
Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	-0.000263	-0.000217	-0.000251	-0.000323	0.0000240	0.00136	-0.000159	-0.000225	-0.000247	-0.000258
R-squared	0.474	0.473	0.449	0.485	0.473	0.486	0.447	0.466	0.474	0.472
N	2800086	3071724	2367996	3503814	4331620	1538707	1226929	1315903	4107029	1654022

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual for each group are their matched controls. Estimates are measured in years relative to the notification. Standard errors are clustered at the event level.

Table G.5: Nonemployed

	Event Size		Seniority		Tightness		Sales Drop		Gender	
	(1) Small	(2) Large	(3) Senior	(4) Junior	(5) Slack	(6) Tight	(7) Mild	(8) Severe	(9) Men	(10) Women
t+1	0.00731***	0.0135***	0.00649***	0.00522***	0.0120***	0.00679***	0.0107***	0.0124***	0.00901***	0.0149***
	(0.000704)	(0.000681)	(0.000697)	(0.000698)	(0.000583)	(0.000891)	(0.00109)	(0.00119)	(0.000571)	(0.000976)
t+2	0.00922***	0.0212***	0.0133***	0.00871***	0.0183***	0.00845***	0.0118***	0.0254***	0.0134***	0.0217***
	(0.000705)	(0.000681)	(0.000697)	(0.000699)	(0.000584)	(0.000892)	(0.00109)	(0.00119)	(0.000571)	(0.000976)
t+3	0.00727***	0.0169***	0.0123***	0.00392***	0.0144***	0.00636***	0.00867***	0.0204***	0.00976***	0.0201***
	(0.000718)	(0.000691)	(0.000709)	(0.000711)	(0.000584)	(0.000948)	(0.00109)	(0.00119)	(0.000580)	(0.000993)
t+4	0.00612***	0.0184***	0.0138***	0.00305***	0.0153***	0.00386***	0.00915***	0.0204***	0.0103***	0.0200***
	(0.000727)	(0.000698)	(0.000718)	(0.000719)	(0.000585)	(0.000996)	(0.00109)	(0.00119)	(0.000586)	(0.00101)
t+5	0.00519***	0.0130***	0.0118***	-0.000488	0.0106***	0.00510***	0.00640***	0.0137***	0.00665***	0.0174***
	(0.000737)	(0.000705)	(0.000727)	(0.000727)	(0.000585)	(0.00106)	(0.00109)	(0.00119)	(0.000593)	(0.00102)
Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	-0.000280	-0.000143	-0.000237	-0.000219	-0.0000933	0.0000644	-0.0000707	-0.000113	-0.000174	-0.000274
R-squared	0.309	0.317	0.289	0.323	0.319	0.307	0.321	0.320	0.316	0.307
N	2800086	3071724	2367996	3503814	4299647	1570836	1226929	1315903	4107029	1654022

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual for each group are their matched controls. Estimates are measured in years relative to the notification. Standard errors are clustered at the event level.

Table G.6: First New Firm

	Event Size		Seniority		Tightness		Sales Drop		Gender	
	(1) Small	(2) Large	(3) Senior	(4) Junior	(5) Slack	(6) Tight	(7) Mild	(8) Severe	(9) Men	(10) Women
t+1	0.0306***	0.0373***	0.0183***	0.0273***	0.0338***	0.0352***	0.0256***	0.0295***	0.0354***	0.0333***
	(0.00117)	(0.00108)	(0.00122)	(0.00109)	(0.000928)	(0.00151)	(0.00166)	(0.00174)	(0.000940)	(0.00152)
t+2	0.0472***	0.0490***	0.0422***	0.0376***	0.0465***	0.0528***	0.0303***	0.0436***	0.0516***	0.0427***
	(0.00117)	(0.00108)	(0.00122)	(0.00109)	(0.000929)	(0.00151)	(0.00166)	(0.00175)	(0.000941)	(0.00152)
t+3	0.0465***	0.0552***	0.0463***	0.0433***	0.0486***	0.0598***	0.0303***	0.0516***	0.0540***	0.0477***
	(0.00119)	(0.00109)	(0.00124)	(0.00111)	(0.000930)	(0.00161)	(0.00166)	(0.00175)	(0.000956)	(0.00154)
t+4	0.0417***	0.0572***	0.0512***	0.0415***	0.0488***	0.0546***	0.0294***	0.0571***	0.0520***	0.0485***
	(0.00121)	(0.00110)	(0.00126)	(0.00112)	(0.000931)	(0.00169)	(0.00166)	(0.00175)	(0.000966)	(0.00157)
t+5	0.0339***	0.0443***	0.0391***	0.0343***	0.0375***	0.0477***	0.0134***	0.0472***	0.0405***	0.0405***
	(0.00122)	(0.00111)	(0.00127)	(0.00113)	(0.000932)	(0.00181)	(0.00166)	(0.00175)	(0.000977)	(0.00159)
Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	-0.000184	-0.000217	-0.000200	-0.000258	-0.0000329	0.000760	-0.000146	-0.000179	-0.000193	-0.000259
R-squared	0.396	0.399	0.387	0.403	0.397	0.407	0.383	0.385	0.399	0.394
N	2800086	3071724	2367996	3503814	4345939	1524532	1226929	1315903	4107029	1654022

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual for each group are their matched controls. Estimates are measured in years relative to the notification. Standard errors are clustered at the event level.

Table G.7: Unemployed

	Event Size		Seniority		Tightness		Sales Drop		Gender	
	(1) Small	(2) Large	(3) Senior	(4) Junior	(5) Slack	(6) Tight	(7) Mild	(8) Severe	(9) Men	(10) Women
t+1	0.0182***	0.0321***	0.0161***	0.0259***	0.0286***	0.0168***	0.0252***	0.0418***	0.0249***	0.0267***
	(0.000687)	(0.000713)	(0.000705)	(0.000710)	(0.000603)	(0.000827)	(0.00115)	(0.00129)	(0.000581)	(0.000990)
t+2	0.0170***	0.0341***	0.0196***	0.0258***	0.0302***	0.0141***	0.0239***	0.0483***	0.0245***	0.0296***
	(0.000688)	(0.000713)	(0.000706)	(0.000710)	(0.000603)	(0.000827)	(0.00115)	(0.00129)	(0.000581)	(0.000991)
t+3	0.0159***	0.0275***	0.0183***	0.0209***	0.0247***	0.0132***	0.0173***	0.0395***	0.0195***	0.0283***
	(0.000701)	(0.000724)	(0.000717)	(0.000723)	(0.000604)	(0.000883)	(0.00115)	(0.00129)	(0.000590)	(0.00101)
t+4	0.0137***	0.0261***	0.0174***	0.0194***	0.0227***	0.0113***	0.0195***	0.0307***	0.0167***	0.0288***
	(0.000709)	(0.000732)	(0.000727)	(0.000731)	(0.000605)	(0.000932)	(0.00116)	(0.00129)	(0.000597)	(0.00102)
t+5	0.0129***	0.0217***	0.0162***	0.0169***	0.0194***	0.0106***	0.0167***	0.0276***	0.0137***	0.0264***
	(0.000719)	(0.000739)	(0.000736)	(0.000739)	(0.000605)	(0.001000)	(0.00116)	(0.00130)	(0.000603)	(0.00104)
Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.0111	0.00805	0.00642	0.0116	0.00927	0.0104	0.00986	0.00805	0.00854	0.0117
R-squared	0.395	0.364	0.353	0.387	0.371	0.420	0.367	0.365	0.378	0.376
N	2800086	3071724	2367996	3503814	4415988	1454381	1226929	1315903	4107029	1654022

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual for each group are their matched controls. Estimates are measured in years relative to the notification. Standard errors are clustered at the event level.

Table G.8: Employed Job Seeker

	Event Size		Seniority		Tightness		Sales Drop		Gender	
	(1) Small	(2) Large	(3) Senior	(4) Junior	(5) Slack	(6) Tight	(7) Mild	(8) Severe	(9) Men	(10) Women
t+1	0.00182***	0.00312***	0.00150***	0.00286***	0.00323***	0.00103***	0.00233***	0.00542***	0.00278***	0.00238***
	(0.000233)	(0.000241)	(0.000241)	(0.000239)	(0.000212)	(0.000280)	(0.000410)	(0.000452)	(0.000206)	(0.000344)
t+2	0.00185***	0.00209***	0.000905***	0.00247***	0.00256***	0.000848**	0.00375***	0.00369***	0.00211***	0.00212***
	(0.000233)	(0.000241)	(0.000241)	(0.000239)	(0.000212)	(0.000280)	(0.000411)	(0.000452)	(0.000206)	(0.000344)
t+3	0.000626**	0.00165***	0.000590*	0.00146***	0.00159***	0.000188	0.00135**	0.00166***	0.00115***	0.00149***
	(0.000233)	(0.000241)	(0.000241)	(0.000239)	(0.000213)	(0.000301)	(0.000411)	(0.000452)	(0.000210)	(0.000350)
t+4	0.000541*	0.00177***	0.00123***	0.00124***	0.00140***	0.000859**	0.000489	0.00119**	0.00107***	0.00158***
	(0.000237)	(0.000244)	(0.000244)	(0.000243)	(0.000213)	(0.000319)	(0.000411)	(0.000453)	(0.000212)	(0.000355)
t+5	0.000370	0.000644**	0.000604*	0.000712**	0.000564**	0.0000329	0.000486	0.00135**	0.000344	0.000547
	(0.000240)	(0.000246)	(0.000247)	(0.000245)	(0.000213)	(0.000345)	(0.000412)	(0.000453)	(0.000214)	(0.000360)
Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.00173	0.00140	0.00112	0.00186	0.00152	0.00166	0.00135	0.00153	0.00147	0.00179
R-squared	0.254	0.216	0.228	0.235	0.228	0.282	0.212	0.212	0.230	0.242
N	3092090	3349822	2614179	3827733	4507917	1362508	1226929	1315903	4107029	1654022

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual for each group are their matched controls. Estimates are measured in years relative to the notification. Standard errors are clustered at the event level.

Table G.9: Δ Earnings (%)

	Event Size		Seniority		Tightness		Sales Drop		Gender	
	(1) Small	(2) Large	(3) Senior	(4) Junior	(5) Slack	(6) Tight	(7) Mild	(8) Severe	(9) Men	(10) Women
t+1	-0.0342***	-0.0408***	-0.0383***	-0.0232***	-0.0389***	-0.0398***	-0.0394***	-0.0348***	-0.0450***	-0.0268***
	(0.00218)	(0.00163)	(0.00220)	(0.00179)	(0.00126)	(0.00368)	(0.00200)	(0.00211)	(0.00154)	(0.00250)
t+2	-0.0315***	-0.0429***	-0.0381***	-0.0249***	-0.0402***	-0.0394***	-0.0397***	-0.0409***	-0.0456***	-0.0282***
	(0.00218)	(0.00163)	(0.00220)	(0.00179)	(0.00126)	(0.00368)	(0.00200)	(0.00211)	(0.00154)	(0.00250)
t+3	-0.0261***	-0.0383***	-0.0381***	-0.0171***	-0.0336***	-0.0305***	-0.0291***	-0.0376***	-0.0399***	-0.0162***
	(0.00218)	(0.00163)	(0.00220)	(0.00179)	(0.00126)	(0.00394)	(0.00200)	(0.00212)	(0.00157)	(0.00255)
t+4	-0.0242***	-0.0361***	-0.0360***	-0.0146***	-0.0353***	-0.0236***	-0.0289***	-0.0383***	-0.0399***	-0.0157***
	(0.00222)	(0.00165)	(0.00223)	(0.00181)	(0.00126)	(0.00418)	(0.00200)	(0.00212)	(0.00159)	(0.00258)
t+5	-0.0203***	-0.0333***	-0.0392***	-0.00691***	-0.0330***	-0.0169***	-0.0307***	-0.0297***	-0.0363***	-0.0157***
	(0.00224)	(0.00166)	(0.00226)	(0.00183)	(0.00127)	(0.00451)	(0.00201)	(0.00212)	(0.00160)	(0.00262)
Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	1.027	0.959	1.022	0.971	0.930	1.168	0.872	0.878	1.005	0.932
R-squared	0.717	0.751	0.737	0.728	0.760	0.742	0.782	0.774	0.764	0.729
N	3092090	3349822	2614179	3827733	4485088	1385241	1226929	1315903	4107029	1654022

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual for each group are their matched controls. Estimates are measured in years relative to the notification. Standard errors are clustered at the event level.

Table G.10: log Wage

	Event Size		Seniority		Tightness		Sales Drop		Gender	
	(1) Small	(2) Large	(3) Senior	(4) Junior	(5) Slack	(6) Tight	(7) Mild	(8) Severe	(9) Men	(10) Women
t+1	-0.00814***	-0.00939***	-0.00701***	-0.00717***	-0.0105***	-0.00390***	-0.0105***	-0.00914***	-0.00973***	-0.00608***
	(0.000616)	(0.000514)	(0.000506)	(0.000825)	(0.000455)	(0.000794)	(0.000862)	(0.000870)	(0.000476)	(0.000727)
t+2	-0.0121***	-0.00509***	-0.00634***	-0.00723***	-0.00829***	-0.00777***	-0.00514***	-0.00825***	-0.00883***	-0.00575***
	(0.000629)	(0.000527)	(0.000525)	(0.000812)	(0.000467)	(0.000806)	(0.000882)	(0.000896)	(0.000488)	(0.000741)
t+3	-0.0113***	-0.00727***	-0.00667***	-0.00921***	-0.0106***	-0.00357***	-0.0103***	-0.0119***	-0.0102***	-0.00535***
	(0.000647)	(0.000537)	(0.000545)	(0.000812)	(0.000471)	(0.000865)	(0.000895)	(0.000909)	(0.000499)	(0.000760)
t+4	-0.0113***	-0.00924***	-0.00871***	-0.00945***	-0.0117***	-0.00400***	-0.0126***	-0.0100***	-0.0111***	-0.00662***
	(0.000659)	(0.000550)	(0.000563)	(0.000814)	(0.000476)	(0.000927)	(0.000910)	(0.000917)	(0.000509)	(0.000778)
t+5	-0.0132***	-0.00981***	-0.0106***	-0.0122***	-0.0123***	-0.00708***	-0.0126***	-0.0120***	-0.0120***	-0.00824***
	(0.000676)	(0.000557)	(0.000578)	(0.000820)	(0.000479)	(0.00101)	(0.000914)	(0.000926)	(0.000519)	(0.000793)
Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Worker Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	10.36	10.29	10.34	10.29	10.27	10.47	10.22	10.22	10.33	10.30
R-squared	0.951	0.947	0.952	0.943	0.943	0.955	0.936	0.937	0.947	0.952
N	1378089	1758111	1885545	1250655	2400778	734167	680480	683044	2188979	890885

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual for each group are their matched controls. Estimates are measured in years relative to the notification. Standard errors are clustered at the event level.