Economic Studies 240

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Job Loss and Labor Market Policy

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ECONOMICS AT UPPSALA UNIVERSITY

The Department of Economics at Uppsala University has a long history. The first chair in Economics in the Nordic countries was instituted at Uppsala University in 1741.

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* Labour economics
* Public economics
* Macroeconomics
* Microeconometrics
* Environmental economics
* Housing and urban economics

Martin Häggkvist

Job Loss and Labor Market Policy

Unemployment, Worker Adjustment, and Employment Protection

UPPSALA
UNIVERSITET

Uppsala 2026

Dissertation presented at Uppsala University to be publicly examined in Hörsal 2, Kyrkogårdsgatan 10, Uppsala, Friday, 6 November 2026 at 13:15 for the degree of Doctor of Philosophy. The examination will be conducted in English. Faculty examiner: Associate Professor Andreas Kostøl (BI Norwegian Business School, Department of Economics).

Abstract
Häggkvist, M. 2026. Job Loss and Labor Market Policy. Unemployment, Worker Adjustment, and Employment Protection. *Economic studies* 240. 197 pp. Uppsala: Department of Economics, Uppsala University. ISBN 978-91-506-3198-2.

Essay I: 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. I find no evidence of worker anticipation prior to the announcement; however, 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. Taken together, my findings demonstrate that the impacts of downsizing extend beyond those directly displaced.

Essay II (with Jonas Cederlöf and Peter Fredriksson): We study how anticipated unemployment insurance (UI) affects unemployment entry, unemployment duration, and the composition of the unemployed. We use a unique and detailed dataset of notified workers linked to administrative records, enabling us to track individuals from the initial revelation of job loss and onward. By exploiting an eligibility threshold – yielding a 21-percentage-point increase in the UI replacement rate in the event of unemployment – we find that eligible workers are less likely to find a new job during the notice period and are more likely to enter unemployment. The effect on unemployment incidence is substantial and accounts for three quarters of the unemployment increase due to higher UI generosity. Indeed, since the behavioral adjustment mainly occurs prior to displacement, the unemployment pool is selected on the treatment: workers with less favorable employment prospects are overrepresented among the unemployed. Our findings thus have implications for optimal UI-design since the negative fiscal externality mainly arises through search behavior prior to job loss.

Essay III: How much are employers willing to pay to sidestep employment protection? I study this in the context of seniority rules, aiming to shield tenured workers from job loss. Using detailed and unique Swedish layoff notification data matched with administrative registers, I identify the set of workers at risk of dismissal and construct counterfactual displacement lists based on the mandated layoff order. I find that workplaces with relatively more productive junior workers are significantly less likely to follow the seniority order. A one-standard-deviation increase in the relative productivity of recent hires reduces compliance by 7.7 percentage points. To circumvent the default order, employers provide higher severance pay and longer advance notice to senior workers, indicating that firms pay for the right to retain more productive workers. These results indicate that seniority rules impose efficiency costs on employers as employment protection is traded for financial compensation.

Keywords: job loss, unemployment insurance, employment protection, downsizing, worker selection

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ISSN 0283-7668
ISBN 978-91-506-3198-2
URN urn:nbn:se:uu:diva-588833 (http://urn.kb.se/resolve?urn=urn:nbn:se:uu:diva-588833)

Acknowledgements

I am deeply thankful for the opportunity to write this thesis. This has been by far the largest and most challenging project I have ever encountered, and it feels both abrupt and slightly unreal that it is coming to an end. This thesis marks the end of ten years at Ekonomikum—literally a third of my life spent as a student here. There are many people I would like to thank who have both directly and indirectly helped create this thesis.

First and foremost, this thesis would not have been possible without the support of my advisors, Peter and Jonas. Thank you for patiently helping me both academically and mentally during these years. Peter, for trusting me as far back as the master's level with your research idea, which turned into the second essay of this thesis, thereby inspiring me to want to pursue a PhD. Jonas, for hands-on help with everything from coding to register data encounters. You have been great complements as advisors, delivered so many insightful and fun comments throughout my time at the department, and it has truly been a privilege to receive such brilliant guidance for such a long time. I really appreciate that you have been so generous with your time, even during times when you had many other things on your plates.

I also want to thank all my colleagues at the Department of Economics for their company and support throughout these years. Whether it was comments during seminars, chatting in the coffee room, helping me to calculate prolongations properly, arranging practicalities related to conferences, or playing blindfolded football, you have all been brilliant. I especially want to thank the faculty of the labor group, who have continuously encouraged and challenged me throughout my time at the department. It is truly astonishing to think about the sheer amount of time, guidance, and attention I have received from such brilliant scholars throughout my time at the department, and you all deserve my deepest thanks.

Furthermore, I want to thank my opponents, Martin Olsson and Pawel Krolikowski, for carefully reading my drafts and their valuable feedback at my licentiate and final seminars, respectively. I am also grateful to my internal discussants, Lena Hensvik and Johan Vikström, whose insightful comments further helped to improve this thesis. Thank you to FORTE for funding the majority of my time at the department, to IFAU for providing the data used in all three essays, and to Jörgen Moen for heroically saving my mistakenly deleted code folder from the void of cyberspace during my third year.

I also want to give special thanks to my PhD cohort: Alex, Christian, Gilson, Greta, Henrik, Jakob, Olle, Ricard, Suna, Svante, and Xuemeng. Pursuing a

PhD can sometimes be a lonely endeavor, but you have all made it feel much less lonely. I will long cherish the memory of us celebrating in the sun by the river after completing our intense first-year courses. A special thank you to Henrik for being the ideal office mate and for patiently putting up with my coding questions over these years. On top of these, many colleagues from other cohorts have contributed to the friendly atmosphere and have enriched my time both in and outside the department. I could not have asked for a luckier draw of colleagues, many of whom have also become close friends.

I am also grateful to have enjoyed the company of friends outside of the department. I especially want to thank my undergraduate friend group which accompanied me throughout my entire time at Ekonomikum, and hopefully will continue to do so beyond. I also want to thank my musician friends from my previous bands, Attra, Phontrattarne, Hornboskapen, and Östgöta Nationskapell, as well as the numerous student theater groups, for the friendship, music, and much-needed distractions you have all provided.

By far the best thing that happened to me during the PhD years was meeting my partner, Lydia. I cannot stress enough how important your support has been throughout the end of my dissertation work. You have been so generous in sharing your friends, hobbies, and family with me. Thank you for improving my life in so many ways.

Finally, I want to thank my beloved family: my parents, Patrik and Cilla, and my siblings, Linus and Sara, for all the support you have given me, both before and during my doctoral studies. You are truly irreplaceable. I also want to thank my grandfather, Love, who passed away this week as I am writing this. I am deeply grateful for his genuine curiosity in me and everyone around him, and for the comfort of knowing he was invited to my thesis defense, which he was so looking forward to attending.

Uppsala, September 2026
Martin Häggkvist
TTT

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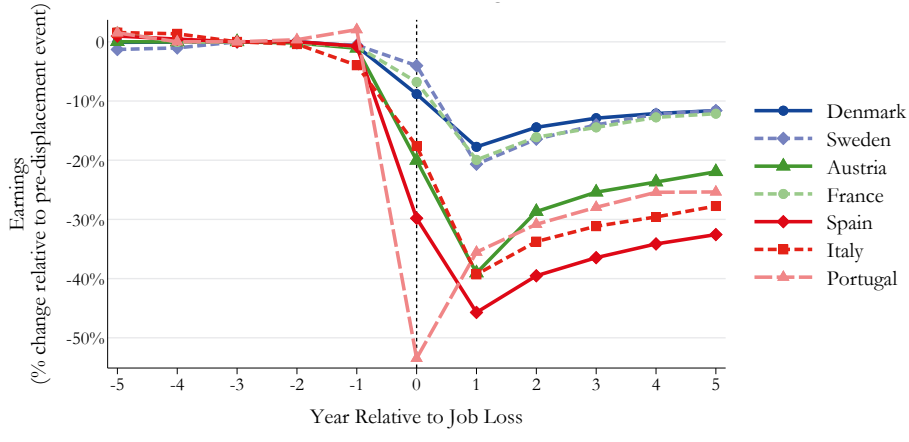
Abbreviations

AKM	Abowd, Kramarz, Margolis
AN	Advance Notice
CBA	Collective Bargaining Agreement
DiD	Difference-in-Differences
EPL	Employment Protection Legislation
FE	Fixed Effects
LIFO	Last-In-First-Out
OECD	Organisation for Economic Co-operation and Development
PES	Public Employment Service (Arbetsförmedlingen)
PSM	Propensity Score Matching
RD(D)	Regression Discontinuity (Design)
UI	Unemployment Insurance
TWFE	Two-Way-Fixed-Effects
WFE	Worker Fixed Effects

Introduction

Job loss is a disruptive life event. Beyond the immediate impact on earnings, it affects vital aspects of life, including career trajectories, mental health, and children's outcomes.[1] Figure 1 illustrates large and persistent earnings losses for displaced workers across several countries. While the magnitude of these losses varies significantly across settings, the evidence shows that income losses are substantial and persistent.

Figure 1. Earnings Losses From Job Loss By Country



Source: Bertheau et al. (2023).

While these strikingly large and persistent losses recur across many contexts, they present a puzzle for baseline economic theory. In a frictionless, perfectly competitive labor market, workers are paid exactly their marginal product, meaning a displaced worker should transition to a new firm at an identical wage. In contrast, earnings declines seem to persist even after re-employment. In their seminal paper, Jacobson, LaLonde, and Sullivan (1993) examine this phenomenon using a sample of displaced workers with long tenure in Pennsylvania. Not only do they document large earnings declines in the short run, they also find meaningful negative effects for workers who successfully secure new

[1] See, for example, Jacobson, LaLonde, and Sullivan (1993), Couch and Placzek (2010), and Schmieder, von Wachter, and Heining (2023) for evidence on earnings losses; Eliason and Storrie (2009), Sullivan and von Wachter (2009), and Reichert and Tauchmann (2017) for health and mortality; and Rege, Telle, and Votruba (2011), Lindo (2011), and Mörk, Sjögren, and Svaleryd (2020) for children's health and educational outcomes.

employment in similar roles at similar firms, relative to a comparison group of non-displaced workers. To reconcile these findings, the authors pose three possible mechanisms for this pattern.

First, contrary to the baseline model assuming general labor skills, workers often accumulate firm-specific skills, which are not fully transferable when moving to a different firm. As formalized by Becker (1962), this specific human capital is not retained when moving, leaving the worker less productive—and thus lower paid—at a new firm. This holds true even in instances where the occupation and work tasks remain the same. This is especially relevant to workers with long firm tenure when displaced, as their firm-specific human capital has accumulated for a longer time compared to junior workers. Another possible mechanism leading to a relative decline in productivity is that unemployment halts skill formation or even depreciates existing skills (Mincer and Polachek, 1974; Blanchard and Summers, 1986; Pissarides, 1992; Edin and Gustavsson, 2008).

Second, some workers are able to capture labor market rents that may disappear upon displacement. Under perfect competition, firms cannot pay workers above their actual productivity level without going bankrupt, nor below, as workers would simply accept jobs elsewhere. Modern labor economics explains these premia through institutional features like unionization and firm-level rent-sharing (Blanchflower, Oswald, and Sanfey, 1996), search frictions (Pissarides, 1985), or efficiency wages designed to lower turnover, improve morale, and deter shirking (Shapiro and Stiglitz, 1986). When displaced, workers may lose access to some of these rents if they are forced to take whatever job is available.

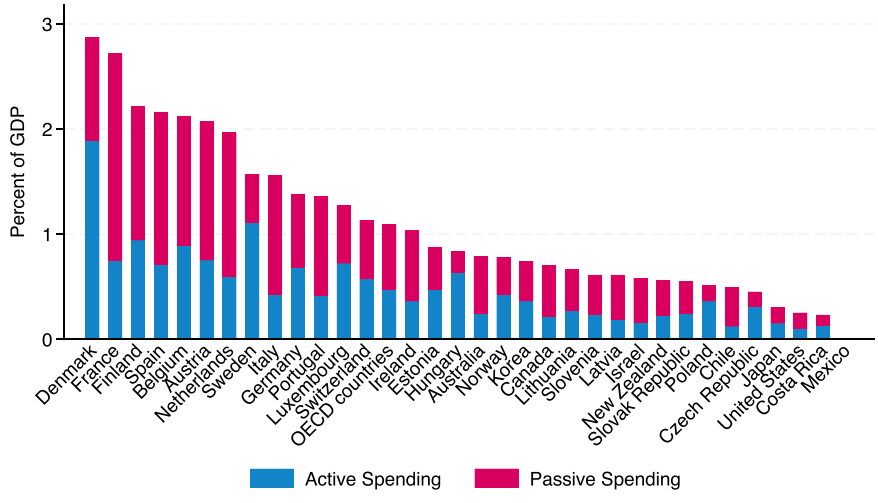
Third, displacement disrupts implicit, long-term contracts and firm-specific career ladders. As argued by Lazear (1981), workers may accept lower initial wages early on their careers in exchange for steeper wage growth later in their tenure. When a worker is displaced, this promise of a future wage premium is dissolved and cannot be brought to the new job, meaning that workers will have lower long-run earnings relative to non-displaced workers.

In recent decades, economists have increasingly moved beyond the frictionless baseline to incorporate structural features such as search costs (Mortensen and Pissarides, 1994), employer market power (Manning, 2003), and the loss of job ladder position built up through on-the-job search (Burdett and Mortensen, 1998; Postel–Vinay and Robin, 2002). There has also been an increasing interest in how workers' long-run adjustment to job loss varies with labor market conditions (Davis and von Wachter, 2011; Schmieder, von Wachter, and Heining, 2023), firm or industry wage differences (Abowd, Kramarz, and Margolis, 1999; Krueger and Summers, 1988; Lachowska, Mas, and Woodbury, 2020), and firm-, industry-, and occupation-specific skills (Neal, 1995; Huckfeldt, 2022).

A different but related literature studies how to mitigate the consequences of job loss (Le Barbanchon, Schmieder, and Weber, 2024). Most countries

have developed labor market policies aimed at protecting workers against consumption drops and promoting reemployment. These include passive policies, such as unemployment insurance (UI), and active policies, such as job search assistance. These policies often constitute a major fiscal cost; Figure 2 shows total labor market policy spending across countries, highlighting that about half of the countries invest amounts exceeding 1% of GDP. Thus, understanding how to optimally allocate these resources is an important issue from a societal perspective.

Figure 2. Labor Market Policy Spending By Country



Source: Le Barbanchon, Schmieder, and Weber (2024).

Consequently, there are several reasons to study the effects of job loss. First, there is an intrinsic interest in documenting the difficulties workers face during involuntary displacement in terms of income, health, and family outcomes. Second, from a policy perspective, it is crucial to understand how adjustment to job loss is influenced by measures intended to insure workers while maintaining incentives for reemployment. Extending our knowledge in these areas is important from a public policy perspective.

In the perfectly competitive model there is no need for government-provided provisions such as unemployment insurance. Even in more sophisticated models, featuring involuntary unemployment, it is not obvious that such provisions are necessary because forward-looking individuals would perfectly insure themselves against unemployment risk through private insurance, precautionary savings, or access to credit. Despite this, consumption drops during job loss are significant, suggesting that workers under-insure; moreover, private job-loss insurance is rarely provided by the market.

One reconciliation for the absence of a private insurance market is adverse selection, which arises because workers hold private knowledge about their own job stability (Hendren, 2017; Landais et al., 2021). High-risk individuals disproportionately seek coverage, forcing private firms to raise premia, which drives low-risk individuals out. Another obstacle is moral hazard, as insurers cannot easily monitor whether insured workers are actively trying to keep their jobs or searching for new ones (Baily, 1978; Chetty, 2008). Furthermore, because unemployment is a systemic macroeconomic risk that spikes during recessions, private insurers cannot easily diversify in the event of many simultaneous claims. A mandatory public UI system solves these failures by requiring universal participation. The state thus eliminates adverse selection by pooling high-risk and low-risk workers, while mitigating moral hazard by enforcing job-search monitoring (Akerlof, 1970; Chetty and Finkelstein, 2013). While our understanding of how to optimally set up government provisions has improved during the last decades, there are still aspects that remain unexplored.

To estimate the consequences of job loss, the dominant approach treats displacement as a sudden, exogenous shock – typically by focusing on mass layoffs or plant closures to ensure that displacements are unrelated to individual worker characteristics (Jacobson, LaLonde, and Sullivan, 1993; Gibbons and Katz, 1991). These designs rely on the assumptions that workers (1) are not systematically selected based on unobserved characteristics and (2) do not foresee or adjust to job loss before it occurs. While these strategies provide a clean framework for identifying causal effects, the underlying assumptions may be too stylized to accurately depict how job loss operates in many settings. For example, recent findings suggest that workers are forward-looking and act upon the risk of job loss well in advance. There is evidence that workers increase on-the-job search effort and switch employers upon learning about unemployment risk (Jarosch, 2023; Baghai et al., 2021; Simmons, 2024) or their outside options (Caldwell and Danieli, 2024; Jäger et al., 2024; Skandalis, 2019), or when receiving advance notice (Cederlöf et al., 2025). Additionally, workers may adjust their behavior along other margins, such as shifting consumption patterns (Basten, Fagereng, and Telle, 2016) or increasing insurance coverage (Landais et al., 2021).

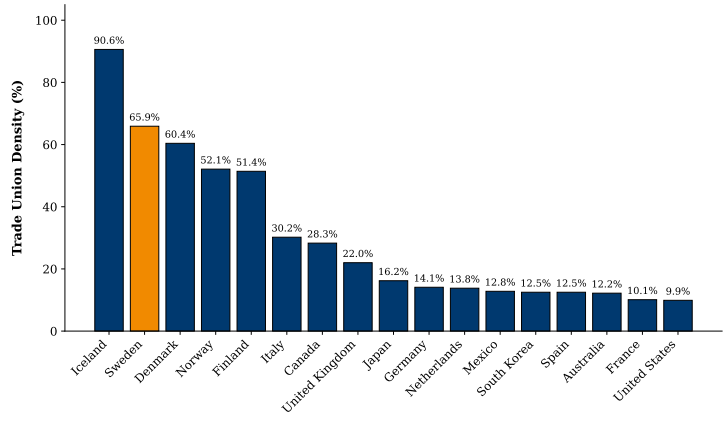
This thesis contains three self-contained essays that collectively advance the view that the impacts of workplace downsizing extend beyond the displaced. Essay I examines the colleagues of displaced workers, showing how information about impending downsizing prompts widespread on-the-job search and precautionary moves to other firms. Essay II focuses on workers who have received notice of future job loss, analyzing how their pre-displacement adjustment interacts with their anticipated level of unemployment insurance (UI). The findings indicate that higher anticipated UI benefits reduce the likelihood of a worker securing a new job while still on notice. Essay III shifts to employment protection legislation (EPL), documenting how compliance with the Last-In-First-Out (LIFO) rule—as well as the provision of severance pay

and extended notice—varies with the workforce characteristics of downsizing firms. Taken together, these three essays consider the entire workforce during a downsizing event, regardless of whether an individual is ultimately displaced.

All three essays focus on workplace downsizing in Sweden between 2005 and 2019. By leveraging unique, detailed data on layoff notifications collected by the Swedish Public Employment Service (PES), the essays identify both notification dates and de facto displacement dates for a large share of downsizing events over this fifteen-year period. They are also linked to a number of other administrative registers, allowing me to identify colleagues of the displaced, occupation information, earnings, and a wide range of demographic information. These data also allow me, first, to study how workers adjust when they foresee displacement while on notice. Second, they enable me to precisely identify which workers are displaced, thereby lowering the risk of conflating displaced workers with voluntary leavers (Flaaen, Shapiro, and Sorkin, 2019). Third, they allow me to capture small layoff events that would otherwise be missed in a traditional mass layoff design, which helps generalize the results.[2]

Beyond these data advantages, studying downsizing in Sweden is interesting in its own right. When comparing Sweden to countries such as the U.S., employment protection is relatively strict, while comparable to other Nordic countries (OECD, 2019). One of the more striking differences between Sweden and many other countries is its high unionization rate (66%, see Figure 3) and collective bargaining coverage (88%, see Ljunglöf, 2026). The prevalence of collective bargaining influences many aspects of the Swedish labor market, such as wage setting, working hours, amenities, and—importantly for this thesis—which workers are displaced during downsizing, as well as their provisions, such as severance pay and the duration of their advance notice.

Figure 3. Unionization Rates By Country



Source: OECD (2025).

[2]In fact, only about 8 percent of all job loss in the United States between 2003 and 2012 occurred during mass layoffs (Cederlöf, 2024).

When an employer wants to downsize, the law mandates that they enter negotiations with the relevant labor unions (SFS, 1976). These negotiations yield a list of workers who are notified of their layoff and informed of their last day of work and, in certain cases, awarded severance pay. Thus, the employer does not have full discretion over which workers are displaced. In the event that the employer and labor union fail to reach an agreement, the law mandates that layoffs follow a reverse order of seniority, LIFO, meaning that workers with shorter tenure are let go first.[3] Because this outcome is generally not in the best interest of the employer, an agreement between the parties is reached in most cases (See p. 408 in SOU, 2020). I return to this discussion in Essay III.

Another interesting feature of the Swedish case is the long, mandated period of advance notice (SFS, 1982).[4] Importantly, this legislative feature resembles the seniority rule in being *semi-dispositive*, meaning that it serves as the fallback option whenever an alternative is not agreed upon during employer-union negotiations. Advance notice implies that workers know about an impending layoff before displacement. Therefore, we expect workers to adjust between notification and displacement, a topic I return to in Essays I and II.

A third institutional feature is the UI system. As shown previously in Figure 2, Sweden spends roughly 1.5% of GDP on labor market programs, a figure that is high by international standards. Sweden is an example of the Ghent system, meaning that while UI is heavily regulated, supervised, and subsidized by the state, it is administered by private UI funds, with historical ties to the labor unions.[5] Unlike in many other settings, membership in these funds is voluntary and, due to state oversight, fees are kept low and independent of individual unemployment risk.[6] Furthermore, a wide range of complementary arrangements exist within collective bargaining agreements (Cronert et al., 2025), one of which provides the source of variation in UI coverage analyzed in Essay II.

Below, I situate each essay within the broader literature and provide a brief description of the main questions, empirical strategies, and results.

Surviving a Layoff: Search and Reallocation after Downsizing

Essay I is motivated by the fact that while the individual consequences of job loss are well-documented (Jacobson, LaLonde, and Sullivan, 1993; Couch

[3]This procedure is described in detail in Cederlöf (2024).

[4]The law stipulates that advance notice must increase with worker tenure. The mandated notice ranges from one to six months; see Cederlöf et al. (2025) for an extensive description.

[5]Similar arrangements exist in Belgium, Denmark, and Finland (Lindellee, 2018).

[6]Membership, combined with fulfillment of a work requirement, provides insurance against unemployment at a replacement rate of up to 80%, with the effective replacement rate decreasing for higher earnings due to a cap on benefits. Currently, 72% of workers are members (IFA, 2026). See Landais et al. (2021) for an extensive description.

and Placzek, 2010), the impact of downsizing on non-displaced survivors has received comparatively little empirical attention. Evidence from psychology suggests that job insecurity is among the most critical job-related stressors, triggering widespread uncertainty and impacting organizational loyalty (Sverke, Hellgren, and Näswall, 2002). Given that the behavior of survivors is likely an important determinant of a firm's post-layoff performance, understanding their labor market responses is essential. In addition, understanding how workers move to other firms is relevant for policy design, as it may interact with government provisions, such as UI (Jarosch, 2023).

Exploiting a unique and detailed register dataset on layoff notifications, I identify the near-universe of downsizing events in Sweden, including the identities of notified workers as well as the specific announcement, notification, and displacement dates. A matched difference-in-differences design reveals that downsizing leads to a persistent 3.8% drop in earnings for surviving workers compared to otherwise similar workers in non-downsizing firms, an effect that does not fully subside within a five-year follow-up period. To understand the causes of this income drop, I study on-the-job search and job mobility among the survivors. At the point of announcement, I find a sharp and immediate increase in precautionary on-the-job search among survivors. At this point in time, it is not yet known to the workers which specific individuals will eventually be notified and displaced. On average, two months later, when workers learn they are not on the layoff list, search effort continues to increase, suggesting widespread concern about future displacement risk.

This surge in search activity translates into excess job mobility among the survivors; three years after the announcement, surviving workers are 9.2 percentage points more likely to have separated from their employer. Exploiting the register data on layoffs, I find that this is mainly driven by voluntary quits to other firms. Notably, these voluntary separations are driven by workers who are positively selected in terms of previous income, educational attainment, and predicted skill measured using the so-called AKM model (Abowd, Kramarz, and Margolis, 1999). I also find that junior workers, those experiencing larger downsizing events, and those looking for work during slack labor markets react more intensely. These patterns suggest that workers update their future unemployment risk, and therefore choose to leave the notifying firm.

Unemployment Insurance and Worker Selection into Unemployment *with Jonas Cederlöf & Peter Fredriksson*

While most previous research on UI focuses on the intensive margin – how benefits affect the duration of unemployment spells – Essay II investigates the extensive margin of how UI affects the inflow into unemployment (see also Citino, Russ, and Scrutinio, 2025; Grindaker and Simmons, 2024; Hartung, Jung, and Kuhn, 2026; Khoury, 2023). Within the setting considered in Essay II, workers are notified on average five to six months before displacements. At

notification, workers in principle know their level of UI benefits post displacement, which influences their on-the-job search. If more generous UI leads to lower search effort while still on the payroll, it may increase the likelihood that workers enter unemployment rather than transitioning directly to a new firm, a mechanism that has important implications for the optimal level of UI (Baily, 1978; Chetty, 2006).

Using a regression-discontinuity design based on age-related eligibility for a UI top-up for white-collar workers in Sweden, we find that more generous benefits reduce the probability of finding a new job during the notice period by 11%. Consequently, UI eligibility causes a 13% increase in the inflow into unemployment. However, when we focus on the subsample of workers entering unemployment, we find no effect on unemployment duration, a result that contrasts with previous evidence. We argue that this is the case because of a relatively long advance notice, which allows for adjustment during the pre-displacement period, and because we study white-collar workers who have, on average, lower long-term unemployment risk than workers in many other settings.

We also find that the workers around the median of the income distribution are the main drivers of the unemployment entry result. We rationalize this with a model showing that while high-income workers receive larger absolute gains from the UI top-up, they do not respond as strongly, because they have on average lower baseline unemployment risk – decreasing the value of the insurance. These two counteracting forces yield a hump-shaped pattern with respect to income. Overall, our results imply that the moral hazard cost of UI is underestimated if pre-separation search responses are ignored.

Interestingly, while we find substantial selection into job loss, the hump-shaped pattern yields very similar average characteristics between the notified and the unemployed. This suggests that, in a design focusing solely on intensive margin responses among the unemployed, one would have erroneously assumed an absence of selection into job loss.

Seniority Rules And Selection Into Job Loss

While a vast literature documents the macro-level consequences of employment protection legislation (EPL) on worker reallocation and firm efficiency (Lazear, 1990; Hopenhayn and Rogerson, 1993; Bentolila, Dolado, and Jimeno, 2019), Essay III investigates how firms may or may not comply with EPL. Specifically, Sweden's legally mandated LIFO rule establishes a default displacement order, but the law is semi-dispositive (SFS, 1982). This institutional feature allows employers to displace more senior workers if they can negotiate an alternative list with the labor union. Because deviating from LIFO may require costly concessions—such as severance packages or extended advance notice—employers face a trade-off between paying negotiation costs and

discretion over which workers to retain, a mechanism that potentially shapes the skill composition of displaced workers.

Using unique and detailed register data from the Swedish Public Employment Service, I distinguish between notified and non-notified workers to identify which workers are notified and retained. By proxying the workers' productivity following Abowd, Kramarz, and Margolis (1999), I investigate whether the alignment between workers' seniority and their productivity determines the degree of compliance with LIFO. I find that workplaces with relatively more productive recent hires are significantly less likely to comply with the LIFO ordering. Specifically, a one-standard-deviation increase in the relative productivity of junior workers reduces compliance by 7.7 percentage points. This suggests that employers are willing to follow mandated seniority rules only when they do not conflict with the retention of high-ability staff; when LIFO is binding, firms are much more likely to negotiate deviations.

Furthermore, I explore the costs to firms associated with these deviations. I find that firms with relatively more productive junior workers pay significantly higher severance and provide longer notice periods to the senior workers they displace. This evidence suggests that employers effectively "buy" flexibility from unions to retain productive workers. These results imply that the efficiency costs of employment protection are partially mitigated by the ability to negotiate, but at the expense of higher displacement costs for the firm. The results indicate that exempting one extra worker incurs a cost of 45,700 SEK, split equally between extended notice and more generous severance. By documenting how firms and unions navigate the trade-off between mandated compliance and retention, this study provides the first empirical evidence on how the productivity structure of a workplace dictates the impact of seniority-based employment protection.

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I. Surviving a Layoff: Search and Reallocation after Downsizing

Acknowledgements: This study has benefited greatly by comments from my advisors Peter Fredriksson and Jonas Cederlöf. I also want to thank Douglas Almond, Dogan Gülümser, Henrik Hällerfors, Lena Hensvik, Martin Olsson, Alexander Willén, and the participants at the IFAU Internal Seminar, Laborocto Final, EALE Job Market Tour, Wages, Employment and Inequality IV Workshop, Uppsala Labor Group and the Uppsala PhD Workshop and Brown Bag seminars for feedback. Funding from FORTE is gratefully acknowledged.

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.

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

[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).

[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).

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.

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 em-

ployees 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 characteristics. 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.

[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).

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 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.

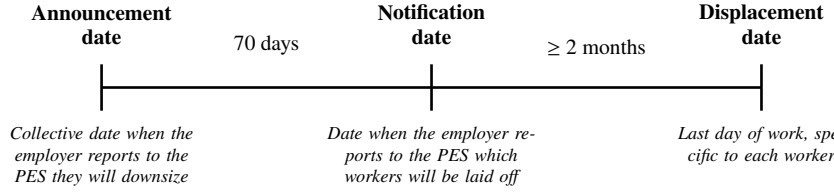
2 Institutional Setting, Data Sources, and Sampling

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 which workers will be laid off. Unless otherwise agreed, the

Figure 1. Timeline of the Displacement Process



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 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

[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.

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

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 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.

[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.3 and B.4.

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

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 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

[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).

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 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 1. 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:

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

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 Section C.

Table 1. *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.

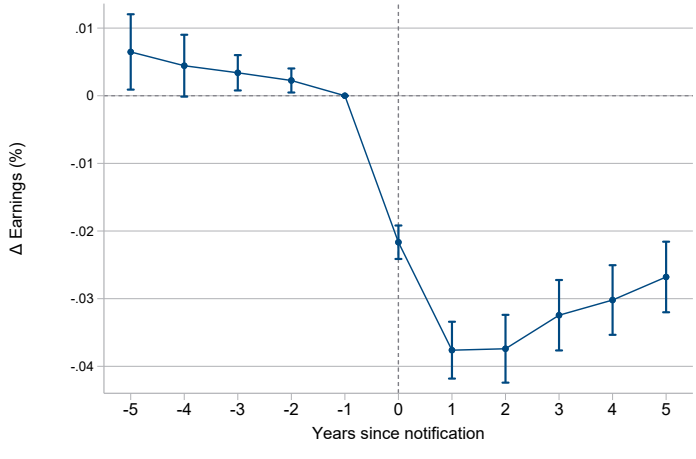
4 Income, Job Search, and Reallocation

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]

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 Section G.

[14] I apply the same matching procedure to the group of notified workers and find income drops of approximately 40% (see Section 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.

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 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

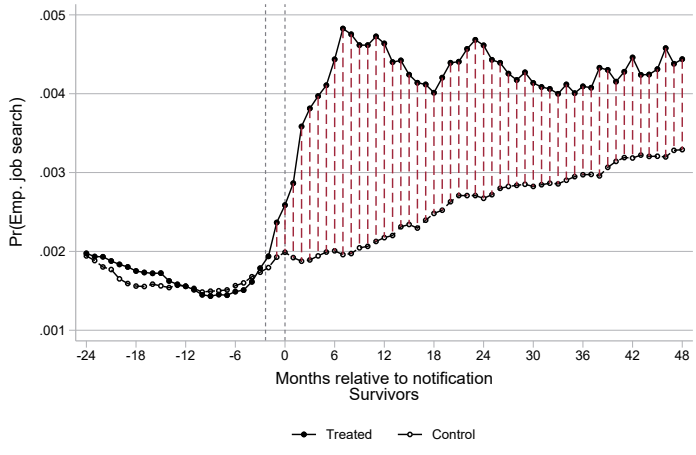
First, I examine if the surviving workers increase their search effort, suggesting increased job insecurity. A key indicator of whether workers are actively seeking new employment is their registration as employed job seekers at the PES. Employed workers can voluntarily register in this category to receive 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.

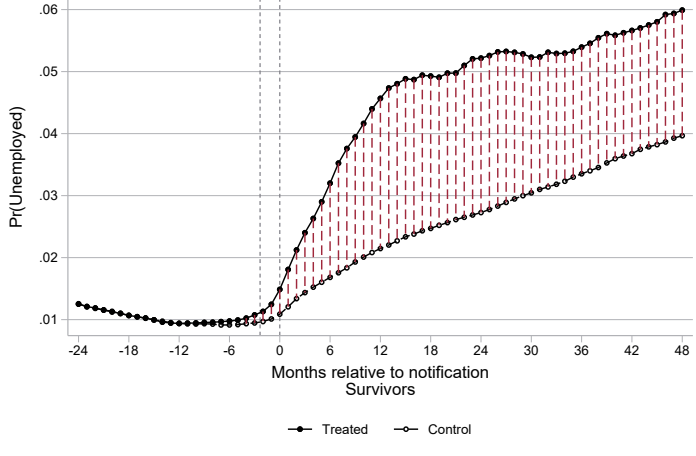
In the months preceding the announcement the two groups have very similar likelihood of registration, and there are no significant differences. However, around the time of announcement, as indicated by the leftmost vertical line, an excess amount of survivors start registering at the PES as employed 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.

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 Section G.

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 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.

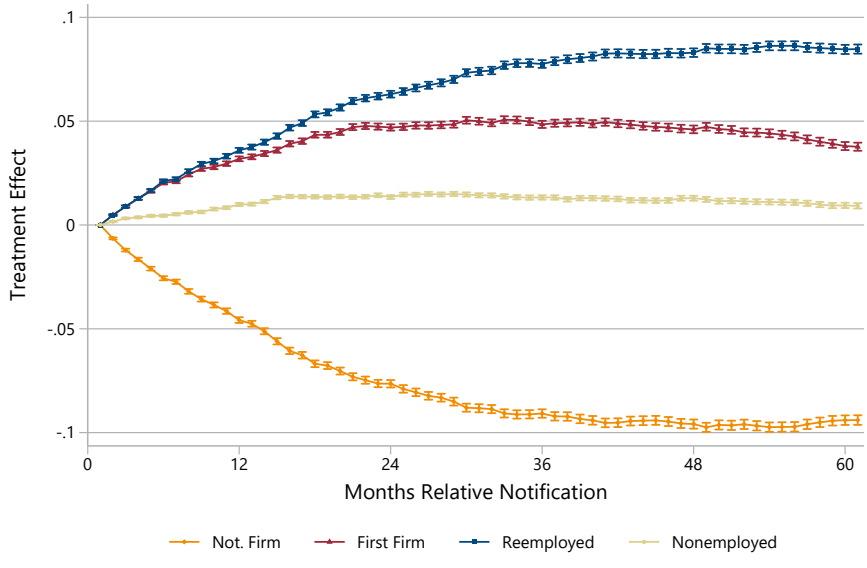
4.3 Reallocation

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

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.

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

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 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.

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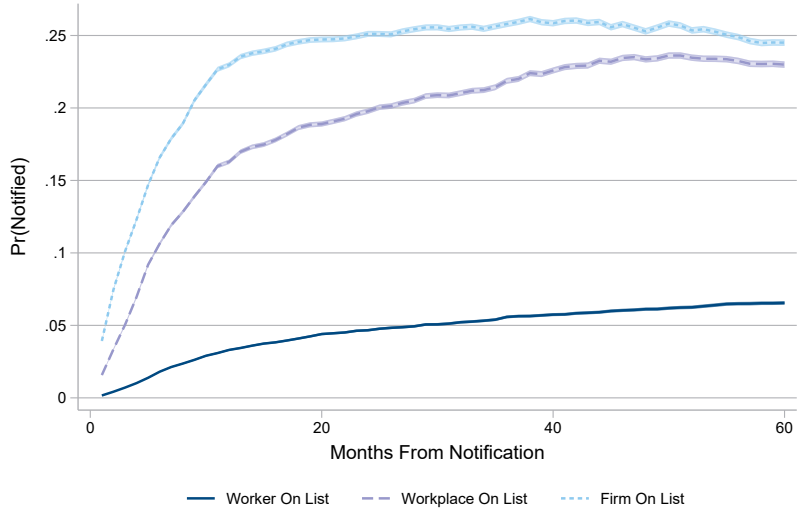
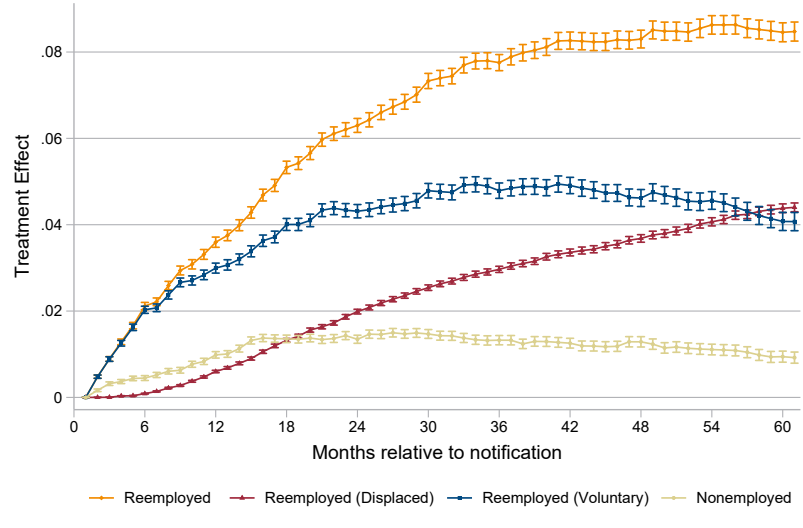
as employed in a given month but still appear in the unemployment register if they experienced even a single day of unemployment during that month.

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 Section G.

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 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.

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 2. 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

Table 2. *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%

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

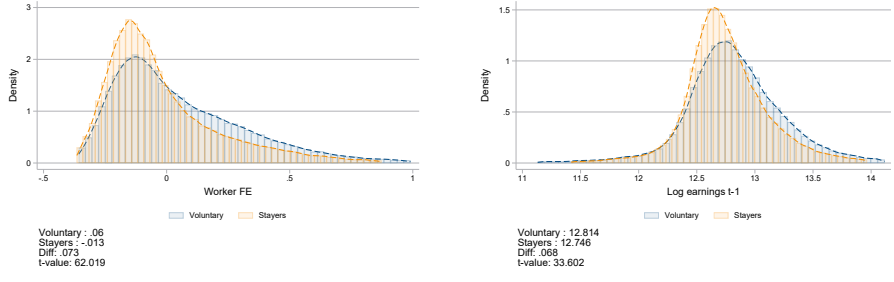
[16] I also investigate the effects on wages and hours worked and find small effects, see Figures D.1 and D.2

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.

Figure 7. Distributions: Voluntary vs. Stayers

(a) Worker Fixed Effects *(b)* Log Earnings, $t-1$

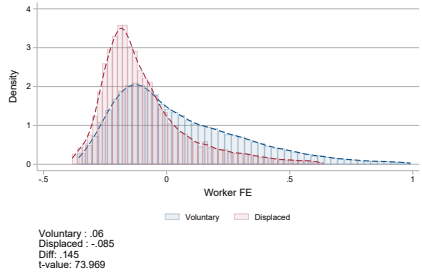


Notes: Figure 7a displays the distribution of AKM worker fixed effects and Figure 7b, log earnings in year $t-1$. The y-axes measure the density of workers, while the x-axes correspond to either the size of the worker fixed effects or previous earnings. Blue bars indicate survivors who are reemployed 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.

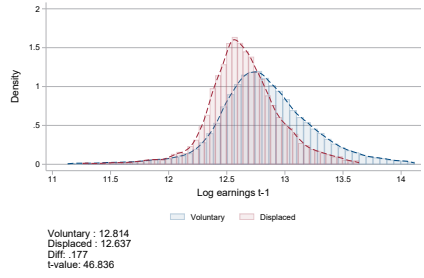
The distributions in Figures 7a and 8a indicate that voluntary movers are positively selected in terms of their individual fixed effects, both relative to workers who are displaced in future events and to workers who remain at the notifying firm. In line with this, I find that those who leave voluntarily 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).

Figure 8. Distributions: Voluntary vs. Involuntary Movers

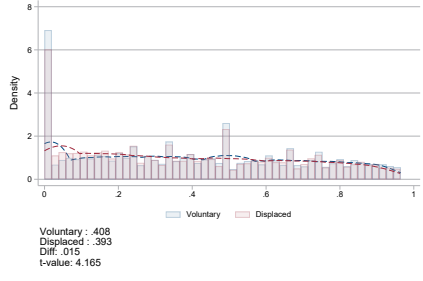
(a) Worker Fixed Effects



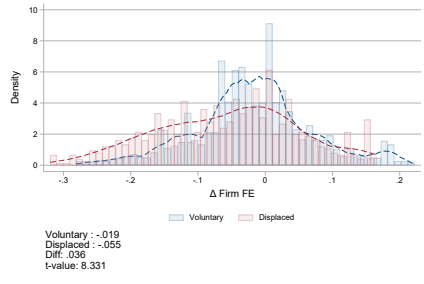
(b) Log Earnings, $t-1$



(c) Seniority Ranking



(d) Δ Firm Fixed Effects



Notes: Figure 8a displays the distribution of AKM worker fixed effects; Figure 8b, log earnings in year $t-1$; Figure 8c, relative within workplace $\times$ 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.

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 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.

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.

[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.

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. 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

[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.2).

[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, Section E explores further dimensions of this identification, such as whether the notified employee and the survivor share the same occupation.

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 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.

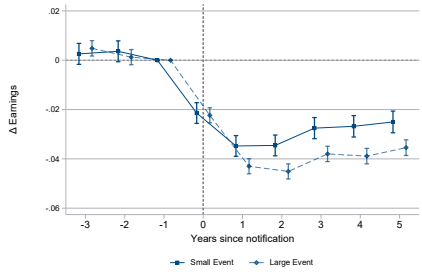
Figure 9b depicts the share of employed job seekers among workers in large and small events respectively. Again, squares (solid lines) represent small events and diamonds (dashed lines) large events. The results show that large events trigger more workers to register as employed job seekers. This difference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

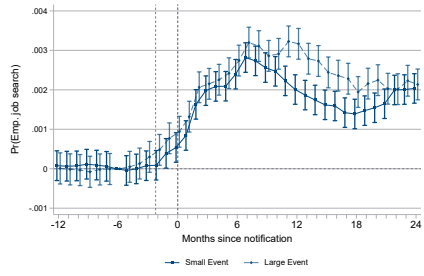
[20] When splitting the sample by the size of the sales drop, I get qualitatively similar results. These results are detailed in Figure E.9 in the appendix.

Figure 9. Outcomes By Share of Workers Displaced

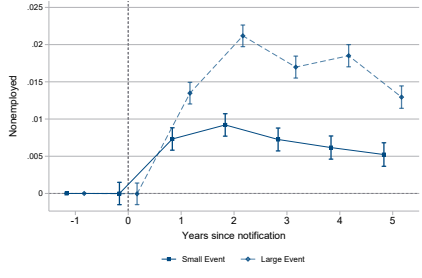
(a) Earnings



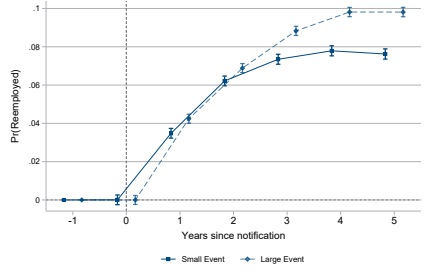
(b) Employed Job Seekers



(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 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 Section G.

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 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.

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

[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 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 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.

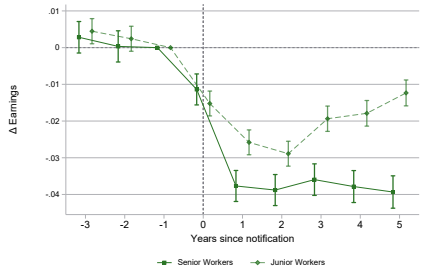
6.3 Adjustment in Slack vs. Tight Labor Markets

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.

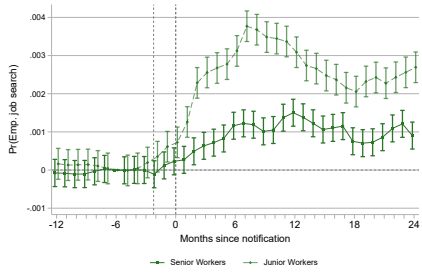
Theory suggests that search effort should be higher in tight labor markets, reflecting both increased job-finding probability and higher expected quality of the new job (Pissarides, 2000).

Figure 10. Outcomes By Seniority

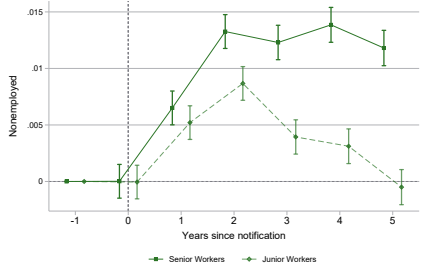
(a) Earnings



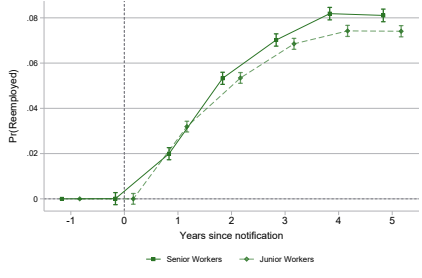
(b) Employed Job Seekers



(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 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 Section G.

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.

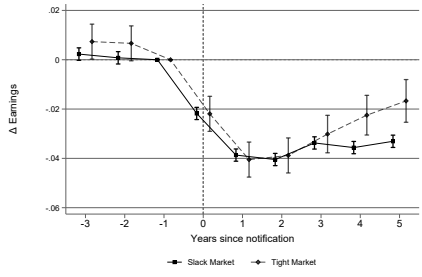
This suggests that workers in slack labor markets feel more concerned when the workplace starts downsizing. This result supports the hypothesis that the information channel is important, as the perceived cost of job loss is likely to be larger in a slack labor market. The theoretical prediction that 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).

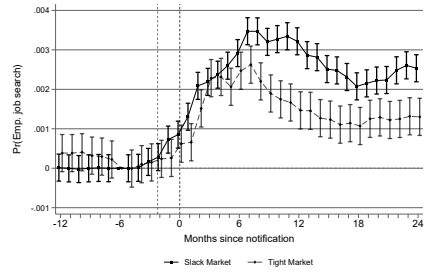
[22]The local labor markets are defined by Statistics Sweden based on commuting patterns, which capture economically integrated regions rather than administrative boundaries.

Figure 11. Outcomes By Labor Market Tightness

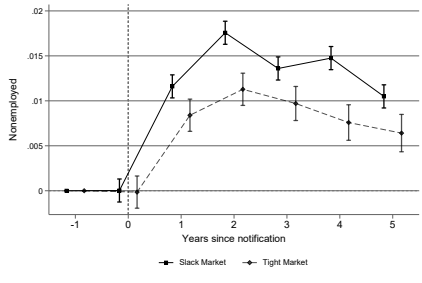
(a) Earnings



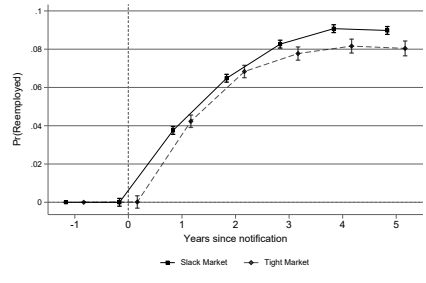
(b) Employed Job Seekers



(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 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 Section G.

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.

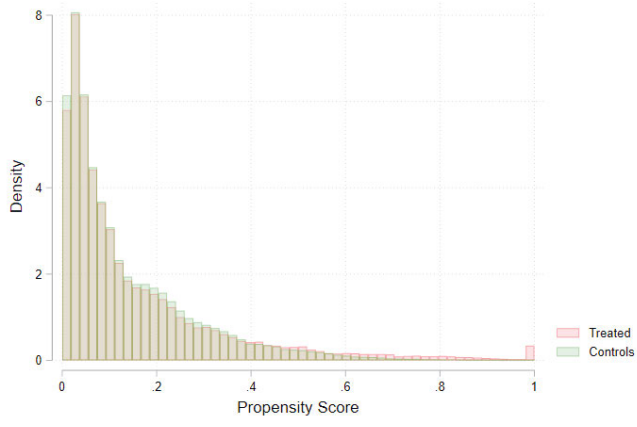
Table B.2. *Descriptive Statistics By Status Three Years After Notification*

	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

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

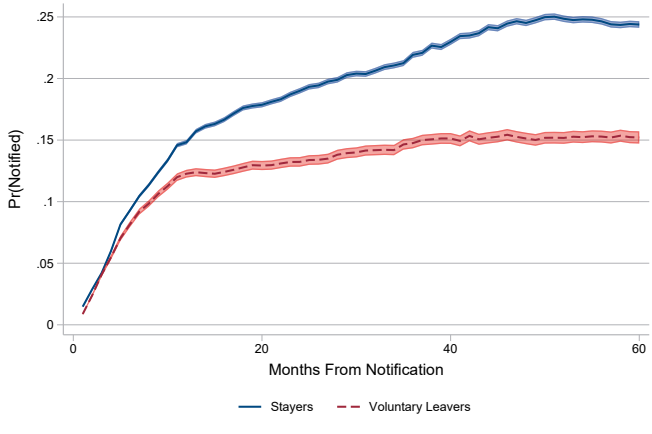
Notes: The table presents descriptive statistics for treated survivors measured three years (36 months) after notification. Values are sample means with standard deviations in parentheses. "Not. Firm" refers to workers who remain at the notifying firm, "Nonemployed" to those with monthly earnings below 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



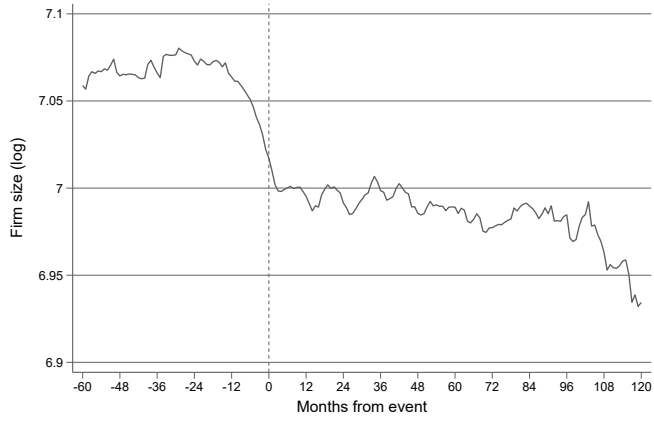
Notes: Figure B.1 shows the density of propensity scores for survivors and their matched controls. Propensity scores are estimated using the matching method detailed in Section 2.2. The red bars are the propensity scores for the treated survivors while the green bars are for the matched controls.

Figure B.2. Notification Incidence By Stayers and Voluntary Leavers



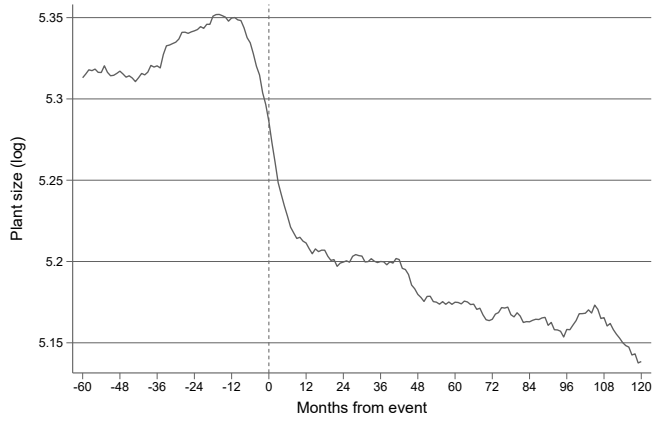
Notes: This figure shows the incidence of the likelihood of being notified by month since the layoff. The sample is divided into workers who remain at the notifying plant 36 months after notification (stayers) and those who have voluntarily left (voluntary leavers). For a worker to have voluntarily left 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.

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.

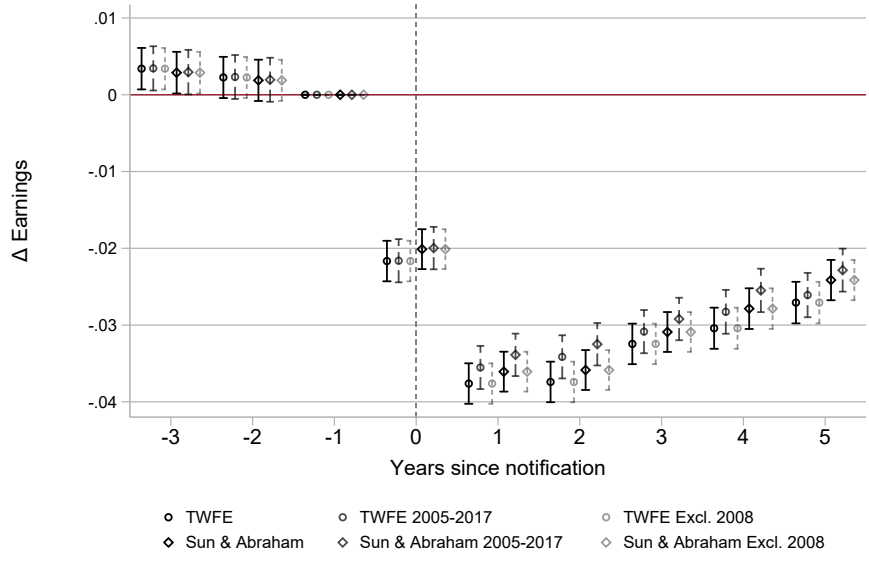
Figure B.4. Evolution of Plant Size



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.

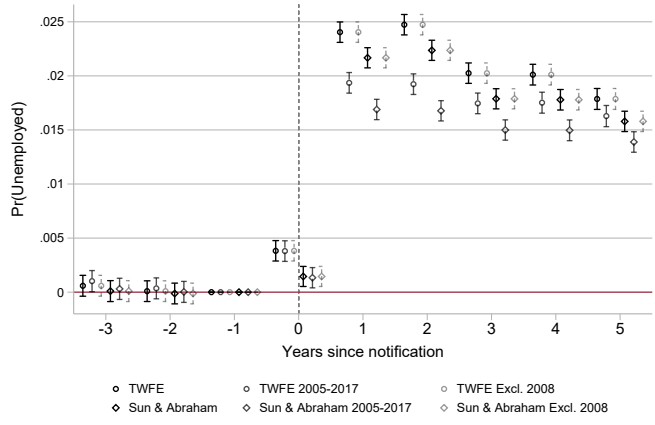
C Robustness Analysis

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



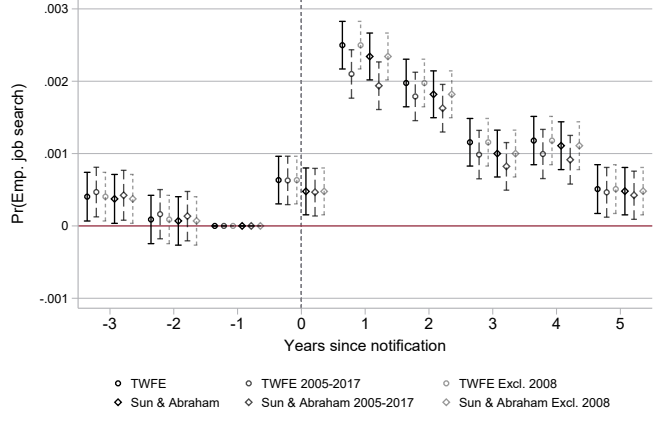
Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples 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



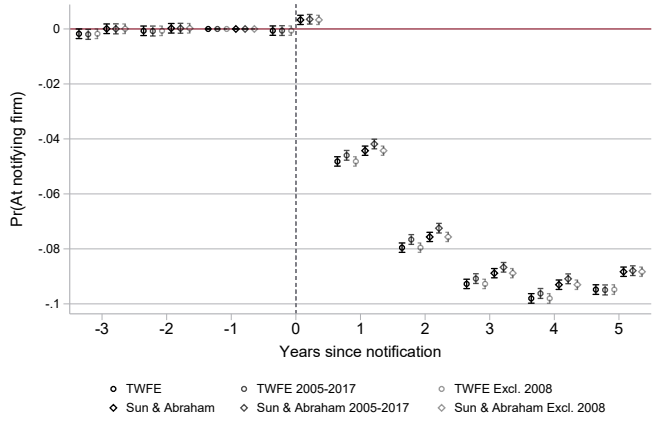
Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples 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



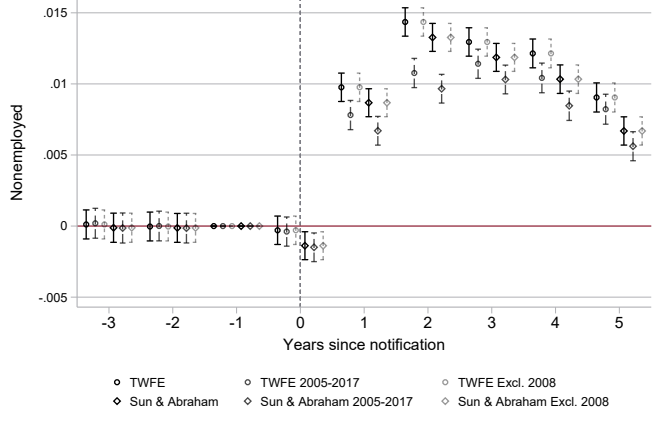
Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples 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



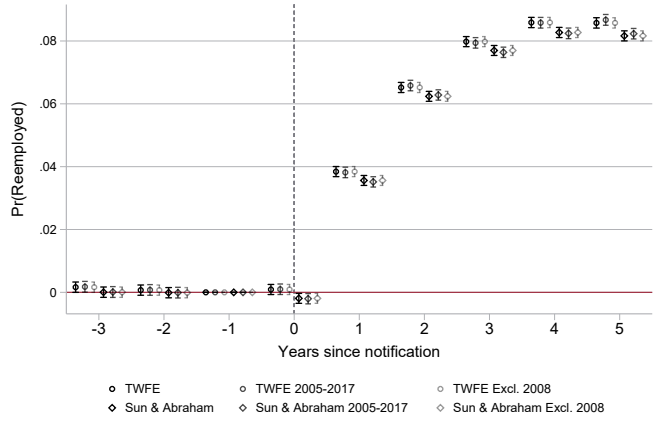
Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples 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



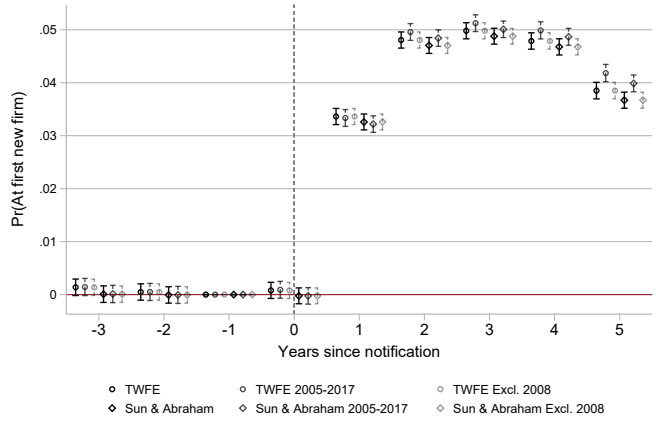
Notes: The figure displays the event study estimates, δ_k, defined in Equation Equation (1). Estimates are shown using both the main staggered difference-in-difference approach (dots) and the alternative estimator proposed by Sun and Abraham (2021) (diamonds). For both estimators, three distinct samples 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).

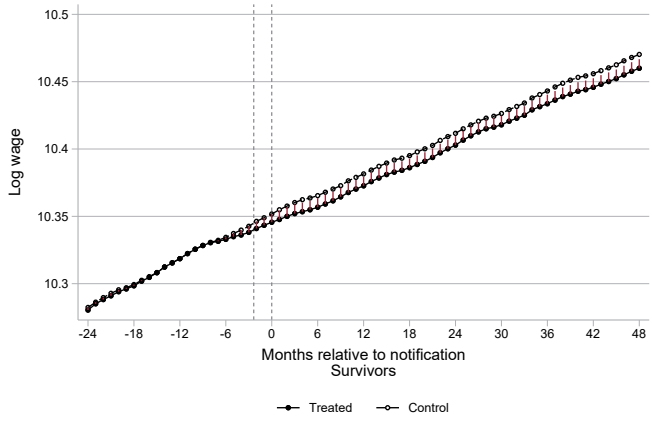
Figure C.7. First New Firm: Different Estimators & Samples



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).

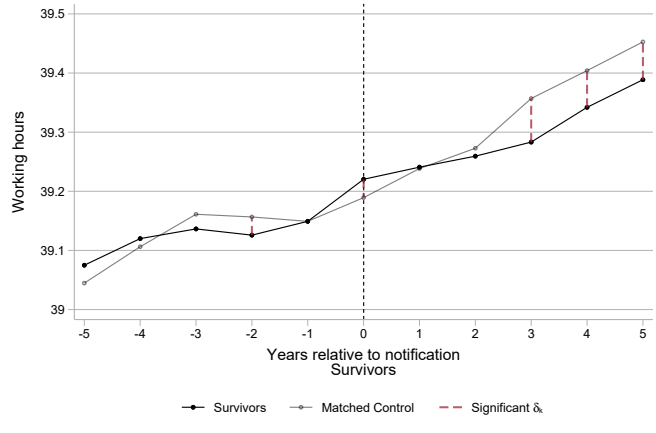
D Additional Outcomes

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.

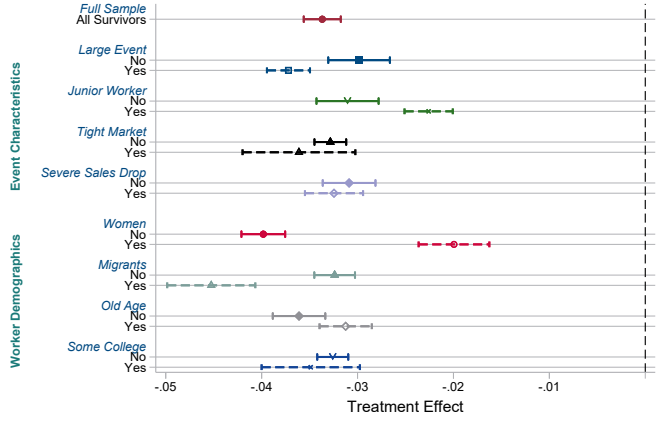
Figure D.2. Evolution of Hours Worked



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.

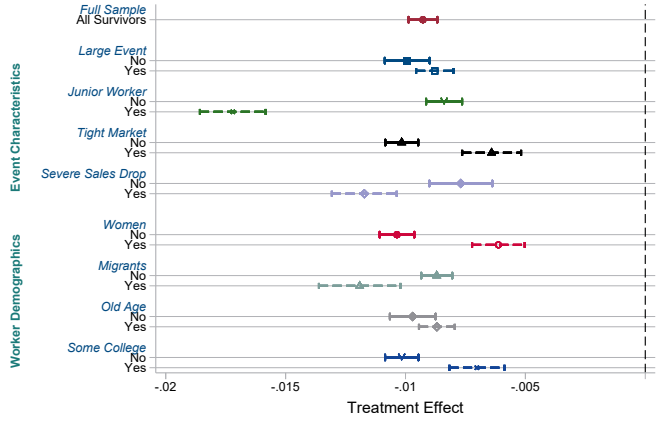
E Heterogeneity Analysis

Figure E.1. Heterogeneity in Earnings



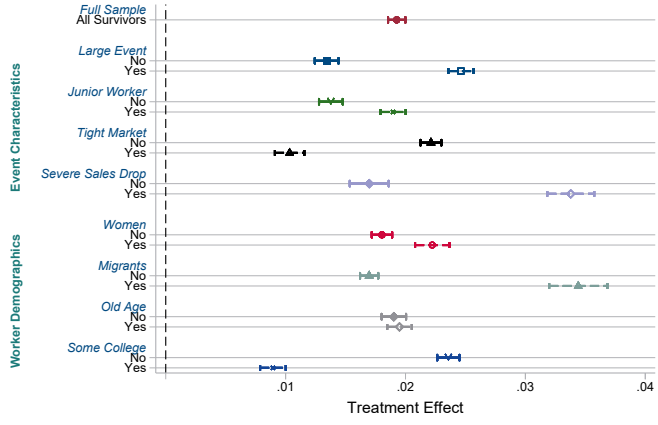
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker 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



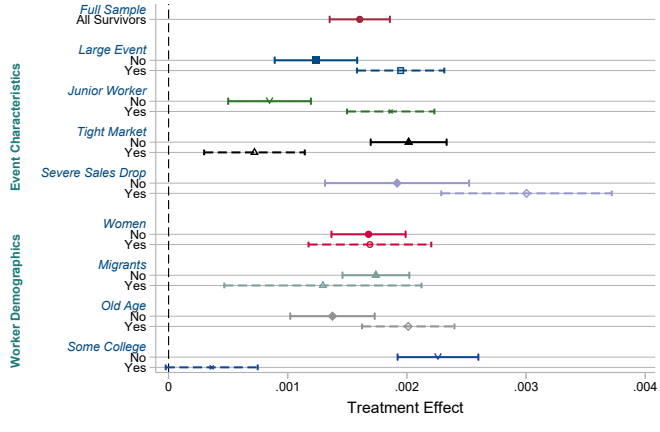
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker 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



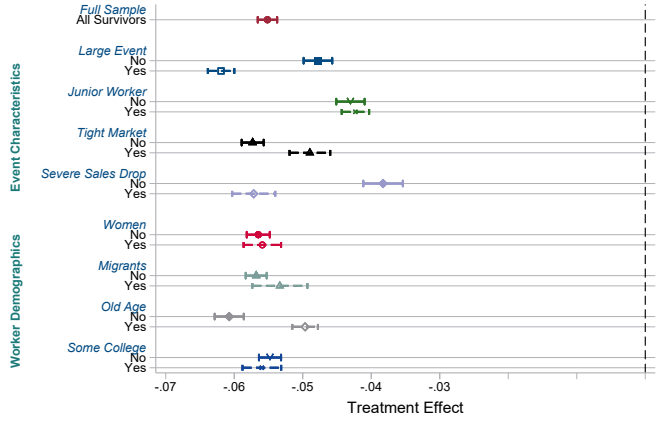
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker 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



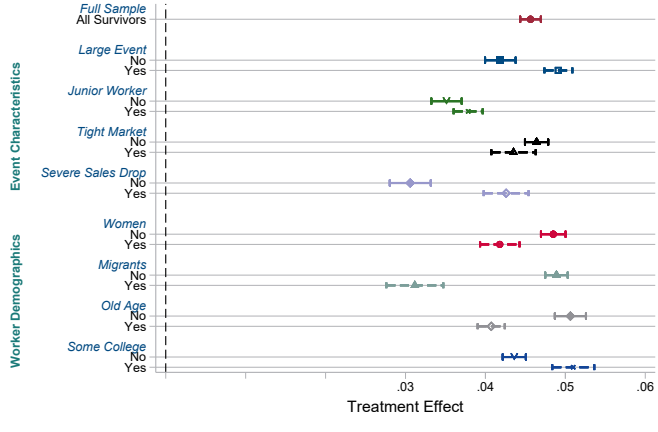
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker 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



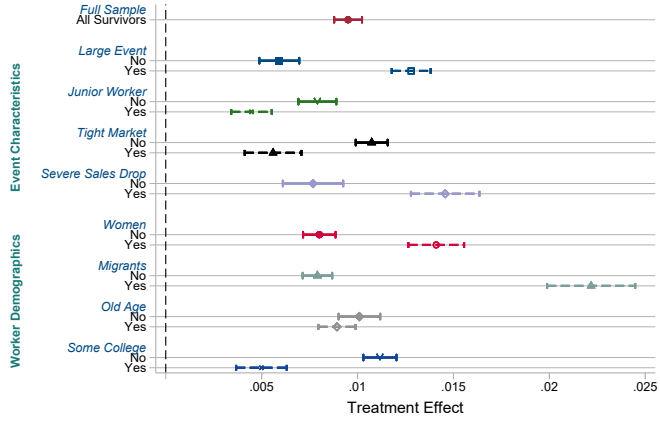
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker 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



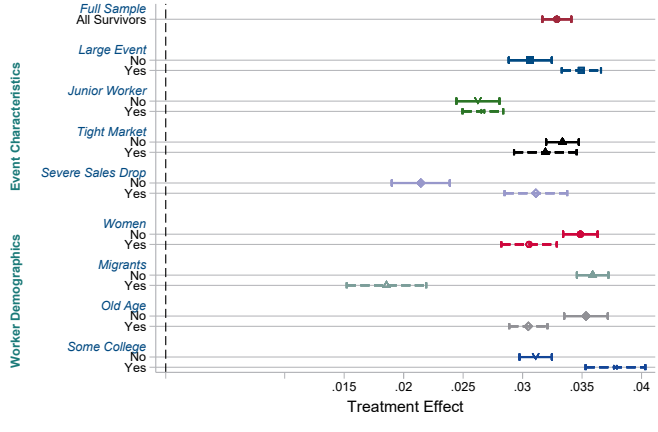
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker 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.

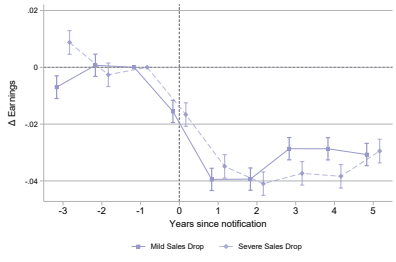
Figure E.8. Heterogeneity in Employed at First New Firm



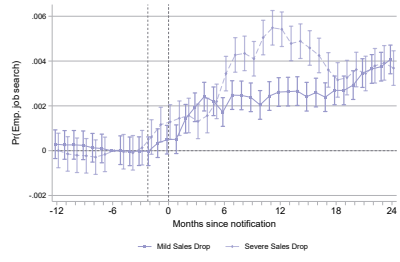
Notes: The figure shows the difference-in-differences estimates for survivors compared to their matched controls. The specification uses the event study model detailed in Section 3, Equation (1), where all time periods ≥ 0 and ≤ 3 are pooled. The sample is divided by various event, workplace, and worker 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

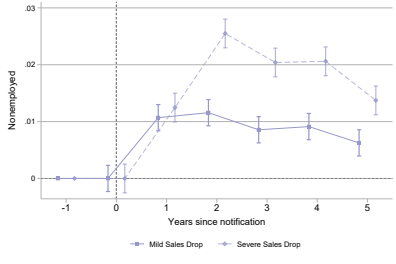
(a) Earnings



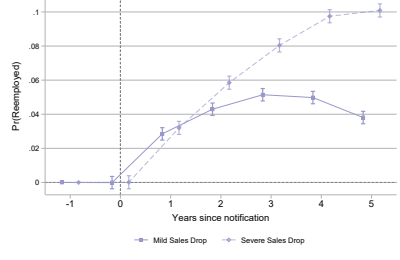
(b) Employed Job Seekers



(c) Nonemployed



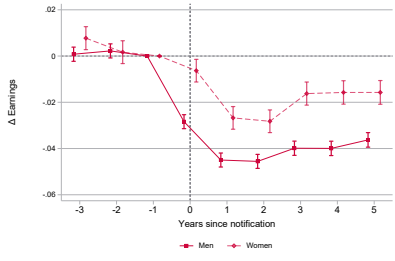
(d) Reemployed



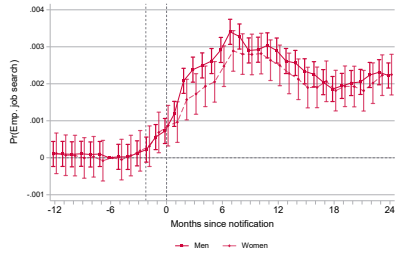
Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed. The figures show the evolution of the outcomes for survivors and their controls, split by sales drop severity. 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

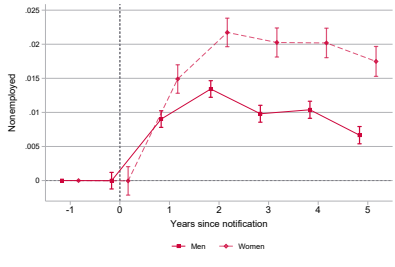
(a) Earnings



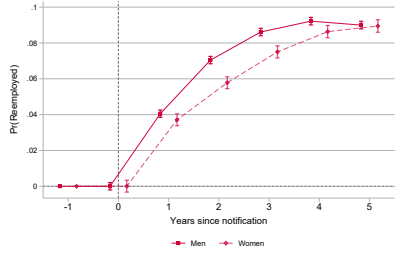
(b) Employed Job Seekers



(c) Nonemployed



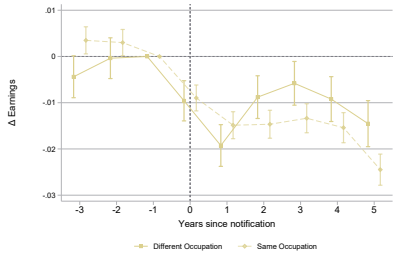
(d) Reemployed



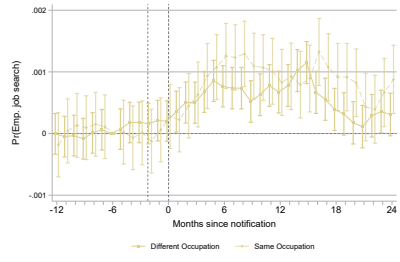
Notes: The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. Standard errors clustered at the event level, with 95% confidence intervals displayed. The figures show the evolution of the outcomes for survivors and their controls, split by gender. Figure 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

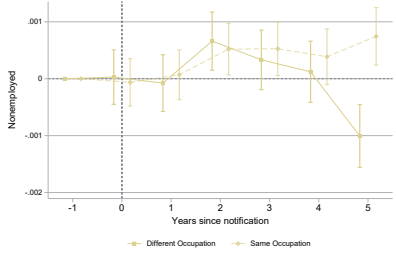
(a) Earnings



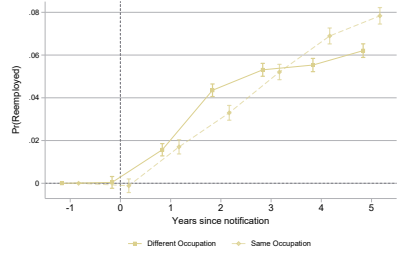
(b) Employed Job Seekers



(c) Nonemployed



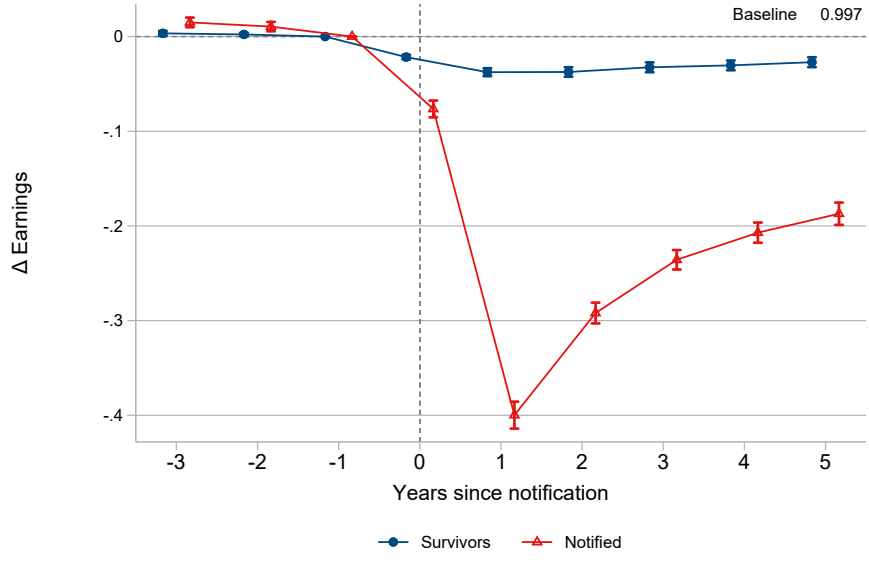
(d) Reemployed



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).

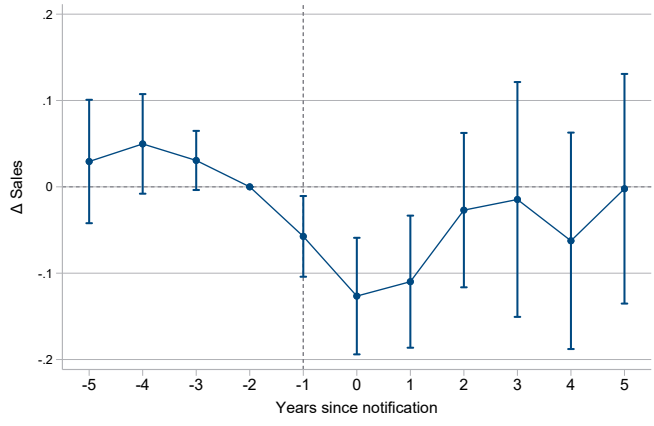
F Additional Figures

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



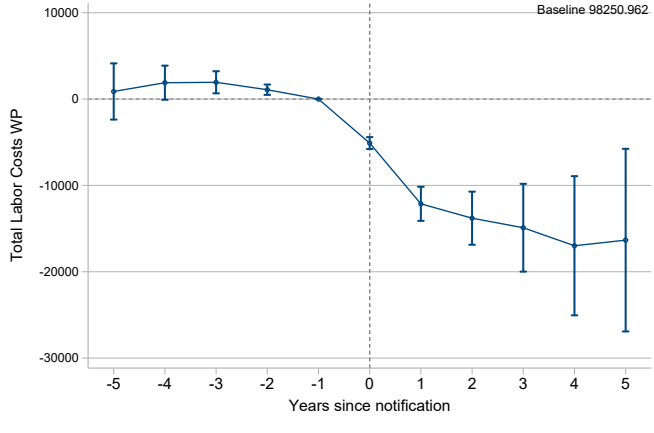
Notes: The outcome is labor earnings. The red line (triangles) shows the notified workers and the blue line (dots) the survivors. The estimates correspond to δ_k as defined in Equation (1). The counterfactual are the matched controls. The control groups are separately matched for the survivors and notified 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.

II. Unemployment Insurance and Worker Selection into Unemployment

Co-Authored With Jonas Cederlöf & Peter Fredriksson

Acknowledgements: We thank Dogan Gülümser, Henrik Hällerfors, Pawel Krolikowski, Mike Simmons, Johan Vikström, Alexander Willén, and participants at the Firms, Technology, AI, and the Labor Market Uppsala Workshop, the Uppsala Labor Group seminars, and the Uppsala PhD workshop for valuable comments. Funding from FORTE is gratefully acknowledged.

1 Introduction

The impact of unemployment insurance (UI) on the duration of unemployment is well-established in the literature.[1] Most studies focus on the *intensive* margin—how UI affects the length of unemployment spells, conditional on becoming unemployed. Less is known about how UI affects the *extensive* margin—inflow into unemployment.

In many settings, it is reasonable to expect extensive margin responses. For example, firms are required to notify workers in advance of impending job loss in almost all countries.[2] Since search behavior responds to the risk of unemployment, we would expect workers with greater UI entitlements to supply lower search effort and target jobs paying higher wages. Consequently, UI generosity likely increases the inflow to unemployment.

We thus examine how unemployment insurance affects unemployment, paying particular attention to the extensive margin. Figure 1 describes the main features of our setting. Workers are notified in advance of impending lay-off. Notified workers search for alternative jobs, while still on the pay-roll. In the event of unemployment, some workers are eligible for more generous UI. The policy variation comes from a top-up of UI available for white-collar workers through their collective bargaining agreement who are above age 40 at the time of layoff. The top-up—which we refer to as *supplemental UI*—is substantial: eligible workers receive a replacement rate of 70%, while non-eligible workers, on average, receive 49%.[3] Using a regression-discontinuity (RD) design for identification, we ask whether supplemental UI eligibility affects the re-employment hazard during the notice period, unemployment incidence, and the selection of workers into unemployment.

To answer these questions we use unique and detailed register data on downsizing events linked to other administrative records, allowing us to identify workers at the time of layoff notification rather than at displacement. We thus capture adjustments already from the point when workers learn about job loss.

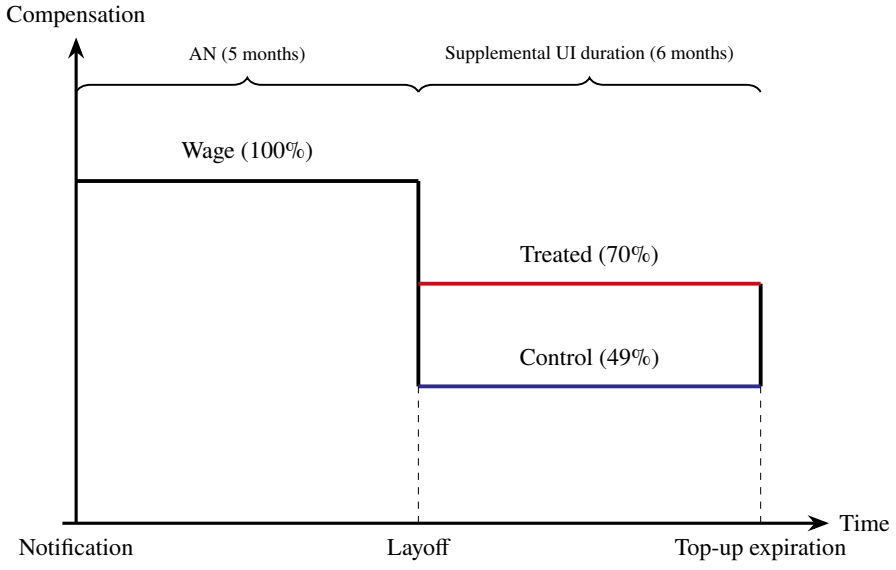
We find that UI eligibility reduces the probability of finding a new job during the notice period by 11%. Consequently, UI eligibility causes a rather substantial increase in the inflow to unemployment, amounting to 13% relative to the baseline. These results are in line with recent research documenting that search behavior is affected by the risk of displacement (e.g., Cederlöf et al., 2025; Häggkvist, 2026; Simmons, 2024) and with Grindaker and Simmons

[1] Le Barbanchon, Schmieder, and Weber (2024) and Figure A.1 provide a comprehensive overview.

[2] According to OECD (2020), firms must notify a white-collar worker with 4 years of tenure 0.5–4 months in advance in European countries; see Figure B.3 for an overview. In the US, firms subject to the WARN-act must notify 60 days in advance. In Sweden, the advance notice requirement is 3 months for white-collar worker with 4 years of tenure.

[3] For non-eligible workers, the system resembles an unemployment assistance system with payments of a fixed *benefit level*.

Figure 1. Treated vs. Control Compensation From Notification



Notes: This figure illustrates the compensation path from notification to post-exhaustion. The initial period represents the full wage (100%). Upon layoff, the treated group receives a supplemental top-up (70% replacement rate), whereas the control group receives regular UI (49% replacement rate, on average). Following top-up expiration, both groups receive the same compensation.

(2024) who show that job-to-job mobility among the employed is affected by UI generosity.

As a result of higher UI generosity, unemployment increases by 17%. This effect is almost entirely driven by the impact on incidence (13%) rather than duration (4%). The UI benefit elasticities implied by our estimates are 0.65 for incidence and 0.20 for duration.[4] Because of selection into unemployment, it is highly unlikely that the estimate on duration has a causal interpretation. The fact that generous UI policies generate large effects on unemployment inflows resembles the results in Zweimüller (2018), who finds that a massive extension of benefit duration in Austria generated large effects on the inflow to unemployment, and Jessen et al. (2025), who find that increases in the benefit level has a larger effect on unemployment inflow than unemployment duration.

We also characterize the selection into unemployment by previous earnings. In the event of unemployment, UI eligibility provides greater direct gains to workers towards the upper-end of the income distribution, and little or no gains towards the lower-end of the distribution. Yet, we find no impact on incidence for those with high previous earnings—all of the impact occurs around the median of distribution. We rationalize this result using a partial equilibrium search model where high-income workers also have better employment

[4]We find no impact of UI on subsequent job quality (see, e.g., Nekoei and Weber, 2017; Schmieder, von Wachter, and Bender, 2016). The mean impacts are small. Since we lack precision, we cannot rule out rather substantial effects, however.

prospects. Since the high-income group expects to find a job faster, they value UI eligibility less, and hence do not respond to the policy variation to the same extent as the median-income group. The effect of UI eligibility on selection into unemployment also rationalize why the observed relationship between unemployment duration and UI is weak; in fact, our numerical calibration implies that the estimate on duration is severely downward biased relative to the causal impact of UI.

We mainly contribute to the literature that has examined the extensive margin responses to UI generosity. Recent examples include Citino, Russ, and Scrutinio (2025), Grindaker and Simmons (2024), Hartung, Jung, and Kuhn (2026), and Khoury (2023); Le Barbanchon, Schmieder, and Weber (2024) survey the earlier literature.[5] We contribute to this literature by identifying and documenting a precise mechanism for the increased inflow into unemployment in response to UI generosity—the reduction in search activity during the notice spell. We also show that being notified of layoff and the timing of layoff is unaffected by UI eligibility; these results are important as they imply that our effects are not driven by reductions in match-specific surplus—as in Jessen et al. (2025)—or by employers and employees agreeing to strategically time the layoff to obtain eligibility as in Khoury (2023) and Citino, Russ, and Scrutinio (2025).

Our estimates are relevant for the optimal level of unemployment insurance (see Baily, 1978; Chetty, 2006). The fiscal externality associated with UI—i.e., the behavioral cost relative to the mechanical cost (see Schmieder and von Wachter, 2017)—is substantially different if we would only focus on the duration impact compared with fully accounting for the impact on unemployment. Since the moral hazard effects mainly occur prior to unemployment entry, the fiscal externality is an order of magnitude larger—it increases by a factor of 4—when appropriately taking the incidence effects into account.

Our analysis is also relevant for evaluations of UI policies, in particular when the policy implies large changes in UI generosity. In our case, it would obviously be misleading to study the behavior of the unemployed to estimate the unemployment impact. Interestingly, it is difficult to detect the selection into unemployment by previous earnings. The selection pattern is non-linear in earnings; as a result, there is no statistical difference in mean earnings for unemployed workers around the threshold. Standard validation exercises would thus not refute a research design based only on the unemployed.

[5]Baily (1977) and Feldstein (1978) are the seminal contributions. Earlier work has used variation coming from changes in UI eligibility at work-history thresholds—Albanese, Picchio, and Ghirelli (2020) and Leung and O'Leary (2020) are recent examples—changes in benefit duration—Baguelin and Remillon (2014) and Jäger, Schoefer, and Zweimüller (2023) are recent examples—and changes in replacement rates (Jäger et al., 2020).

We have emphasized the importance of the notification period in our setting.[6] Nevertheless, search behavior responds to the risk of job loss (Baghai et al., 2021; Jarosch, 2023; Simmons, 2024; Häggkvist, 2026). Thus, it is likely that we would see similar responses to UI generosity also among employed workers who are not formally notified of job-loss; see Grindaker and Simmons (2024).

The paper proceeds as follows. Section 2 describes the institutional setting, while Section 3 details the data sources. Section 4 outlines the empirical methodology and validates our design. Section 5 contains our main results. In Section 6, we characterize the selection of workers into unemployment, and rationalize our findings with the help of a partial equilibrium search model. Finally, Section 8 discusses the findings and concludes.

2 Unemployment Benefits in Sweden

Employers in Sweden wishing to downsize are legally obligated to notify workers in writing (SFS, 1982). In the event of unemployment, there are three types of compensation schemes: mandated unemployment assistance, regular UI, and supplemental UI. While these components interact with each other, they vary in their financing and eligibility requirements.[7] Regular and supplemental UI are the most relevant components for our purposes.

Regular UI is voluntary and provided through membership in UI funds (A-kassor), covering roughly three-quarters of the workforce.[8] Due to state subsidies, membership fees are generally low and unrelated to unemployment risk. All UI funds have uniform eligibility requirements and benefit levels, determined by the government. During our study period, eligibility required being registered as unemployed, actively searching for work, one year of consecutive UI fund membership coinciding with employment, and involuntary job loss (Cronert et al., 2025). Benefits cover 80% of the previous wage up to nominal ceiling, for a maximum of 300 days (which converts to 14 months in calendar time).[9]

Because of the nominal benefit ceiling, effective replacement rates are well below 80% for many workers—in particular for white-collar workers, which

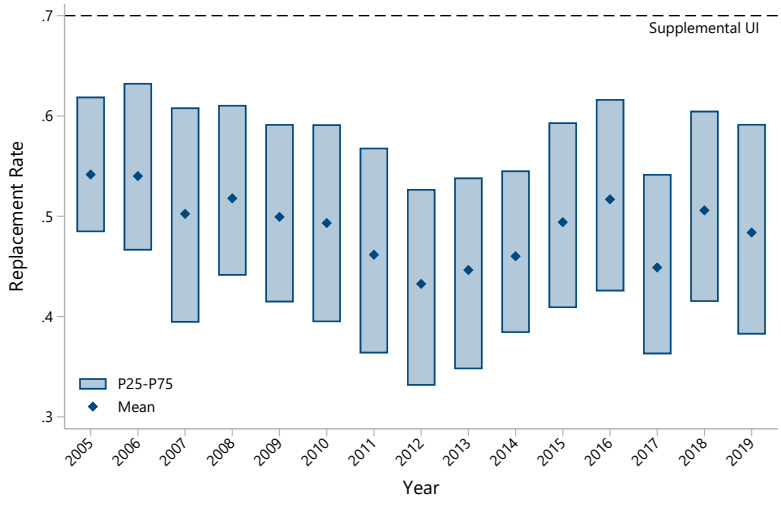
[6] We have also examined whether the impact on incidence varies by the length of the notice period. While workers with longer notice have more time to secure new employment before displacement, they also discount the value of future UI benefits more compared to those with shorter notice. Consequently, the estimates are not precise, although the impact on incidence comes predominantly from workers in the middle of the notice distribution.

[7] See Table B.1 for an overview.

[8] Along with Denmark, Finland, and Iceland, Sweden is one of the few countries with a voluntary UI scheme—a so-called Ghent System (Carroll, 2005).

[9] The generosity of regular UI has varied over the unemployment spell. Part of the period has featured a slightly lower UI ceiling after 100 days, and a replacement rate of 70% after 200 days. After UI exhaustion, workers have the option of entering a workfare type of program, featuring a replacement rate of 65% and a work requirement.

Figure 2. Replacement Rates on Regular and Supplemental UI



Notes: Figure 2 shows the replacement rates for workers eligible for regular UI from 2005 to 2019. The diamonds represent the mean of the wage distribution, while spikes correspond to the 25th and 75 percentiles, respectively. The dashed horizontal line highlights the UI level if eligible for supplemental UI (70%).

we focus on here. Figure 2 shows the replacement rates implied by regular UI for displaced white-collar workers in our sample (described in detail in Section 3). The average replacement rate for workers in our sample varies between 45% and 55% over our sample period (2005–19).

Because of the low effective replacement rates, employer and employee organizations have agreed on a supplemental UI scheme, available through the CBAs.[10] For eligible white-collar workers, supplemental UI implies a total replacement rates of 70%, subject to a high cap, when combined with regular UI.[11] Figure 2 illustrates that there are substantial gains associated with being eligible for supplemental UI, and that these gains are particularly large towards the upper end of the earnings distribution.

Eligibility requires that the worker is above age 40 at the time of displacement, has more than 5 years of tenure at this point in time, and that the workplace is connected to a CBA. 76% of white-collar workers in the private sector (Ljunglöf, 2026) are covered by a CBA, and coverage increases with firm size. Notice also, that eligible workers who are not members of a UI fund still receive supplemental UI. Supplemental UI is available for 130 days (or

[10] In Swedish, they system is known as *Avgångsersättning*. Blue-collar workers have access to a severance pay system (subject to eligibility requirements).

[11] As of 2021, the cap applies only for wages above 100,000 SEK per month, which is binding for less than 1% of the workforce.

6 months in calendar time).[12] The age eligibility requirement is particularly useful for identification of the effects of more generous insurance coverage. It is this age discontinuity that we will focus on in the subsequent analysis.

3 Data and Sampling

Our main dataset is the administrative register of all notifications reported to the PES between 2005 and 2019 (Arbetsförmedlingen, 2025b). These data include the identities of the near universe of workers who are notified of layoff in Sweden, along with event, firm and plant identifiers.[13] Moreover, these data include the exact notification and displacement dates for each event allowing us to calculate advance notice.[14]

We link these data to six other sources: (1) the Employer-Employee register (RAMS) (SCB, 2018), which contains the universe of employer-employee matches along with earnings information; (2) LISA (SCB, 2017), which includes demographic information such as age and education; (3) the Business Register (SCB, 2025a), containing industry codes; (4) the Unemployment register (Arbetsförmedlingen, 2025a), which includes unemployment spells; (5) the Income and Tax register (SCB, 2025b), for information about benefits received by the workers; and (6) the Wage Statistics (SCB, 2025c), which provides full-time equivalent wages and occupation codes.[15]

We begin by matching all notified workers to the employer–employee register and include those who appear in both datasets, have at least 10,000 in monthly earnings 12 months before notification and five years of tenure at the displacement date. As discussed in Section 2, supplemental UI is available for white-collar workers in workplaces covered by a CBA. We include all workers identified as white-collar in either the PES register or the Wage Statistics.[16]

Beyond the formal eligibility requirements for supplemental UI, we impose a few additional restrictions. The purpose of these is to reduce measurement error while more accurately identifying individuals in CBA-covered workplaces.

[12] Employers finance the scheme indirectly through membership fees to the Confederation of Swedish Enterprise (Svenskt Näringsliv). During our study period, these fees were small, at approximately 0.3% of total labor costs (Cronert et al., 2025).

[13] Employers wishing to lay off five or more workers within a period of six months are obliged to report to the PES. However, we see in the data that notifications involving fewer workers are prevalent, indicating that most employers formally report even when they are not obliged.

[14] Figure B.4 a presents the distribution of advance notice in the data.

[15] While we have the universe of workers and firms for all years in the registers, the Wage Statistics is an annual survey that includes occupation codes for all public-sector workers but only a sample of approximately half of all private-sector workers.

[16] Employers report whether workers are white- or blue-collar to the PES; however, because reporting this information is optional, it is available for only a fraction of our sample. To address this, we complement our data with blue/white-collar status from the Wage Statistics, using the most recent worker entry prior to notification. We define a worker as white-collar if they are labeled as such in at least one register.

First, we observe that supplemental UI take-up is very rare among workers who were displaced from small firms, suggesting these workplaces are either not covered by CBAs or fail to inform workers of their benefits. We also find that take-up is significantly lower in small events, suggesting that employers do not inform the workers about supplemental UI also in these events. To address this, we exclude workplaces with fewer than 50 employees one year prior to notification, as well as those laying off fewer than 10 employees during the event.[17]

Second, we exclude instances where workplaces likely misreport the final day of employment, resulting in negative or implausibly long notification periods. Because our running variable is age at the last day of work, such errors lead to the misclassification of eligibility status. Consequently, we exclude bankruptcies because misreporting is particularly prevalent in such events. Moreover, we exclude workers with negative notification periods or periods exceeding one year to minimize the impact of obvious reporting errors.

Our sample does include workers whom we estimate have little to gain from supplemental UI on the basis of their pre-notification earnings. The reason for keeping these workers is that we observe benefit take-up in this group as well, suggesting we have some measurement error in the earnings that go into the benefit calculation formulas. Notice that all our results are qualitatively robust to alternative ways of defining the analysis sample.[18]

Table 1 presents descriptive statistics for displaced workers and our sample. Compared to the broader population, our sample has higher earnings, longer tenure, and greater educational attainment. Women are overrepresented and migrants underrepresented, mainly due to white-collar and tenure requirements.

4 Methodology and Validity

The empirical challenge in estimating the causal effect of UI eligibility on outcomes stems from the fact that benefit levels depend on previous work history, wage levels, age, and sector—factors that may independently influence a worker's outcomes. To estimate the causal effect of eligibility for more generous UI, we employ an RD design where age at the last day of work is the running variable and the age-40 threshold determines eligibility. This builds on the idea that UI generosity is quasi-randomly assigned in a close neighborhood

[17] See Figure B.1 for descriptive evidence on take-up over plant and event size. We also perform the analysis including the small workplaces and imposing regression weighting by the likelihood of having a CBA (see Section E.4).

[18] Section E contain all validity tests and outcomes without (one or all) the exclusion restrictions. Figures E.4.3 and E.4.4) show results when excluding workers with estimated replacement rates exceeding 70% from regular UI.

Table 1. *Descriptive statistics of notified workers*

	(1)		(2)		(3)		(4)	
	Population		**Sample 1**		**Sample 2**		**Main Sample**	
Workers								
log Earnings, t-1	9.971	(0.682)	10.07	(0.730)	10.09	(0.711)	10.29	(0.417)
Tenure at layoff	80.85	(72.69)	81.14	(72.79)	73.65	(57.42)	123.4	(50.67)
Notice time	118.4	(162.2)	135.4	(188.8)	122.3	(170.8)	159.1	(79.29)
Pr(College)	0.195	(0.396)	0.305	(0.461)	0.383	(0.486)	0.431	(0.495)
Pr(Woman)	0.362	(0.481)	0.417	(0.493)	0.447	(0.497)	0.462	(0.499)
Pr(Married)	0.118	(0.322)	0.120	(0.325)	0.0925	(0.290)	0.0798	(0.271)
Age	41.83	(12.69)	43.24	(12.26)	39.62	(2.895)	39.98	(2.889)
Pr(Immigrant)	0.180	(0.384)	0.148	(0.355)	0.168	(0.374)	0.136	(0.343)
Managers	0.0108	(0.104)	0.0224	(0.148)	0.0200	(0.140)	0.0325	(0.177)
Plants								
Plant size, t-1	462.1	(1317.8)	404.3	(1276.6)	408.5	(1224.7)	1029.4	(2129.6)
Firm size, t-1	2049.8	(4449.2)	1859.3	(4364.0)	1865.4	(4381.6)	3624.4	(5782.7)
Number of notified	56.66	(112.2)	51.80	(117.5)	55.08	(120.5)	107.9	(182.6)
Manufacturing	0.388	(0.487)	0.273	(0.445)	0.279	(0.448)	0.421	(0.494)
Trade	0.107	(0.310)	0.138	(0.345)	0.142	(0.349)	0.114	(0.318)
Transport	0.0716	(0.258)	0.0715	(0.258)	0.0674	(0.251)	0.0608	(0.239)
Cause for layoff								
Other	0.219	(0.414)	0.191	(0.393)	0.196	(0.397)	0.234	(0.423)
Bankruptcy	0.113	(0.316)	0.171	(0.377)	0.155	(0.362)	0	(0)
Closure	0.193	(0.395)	0.192	(0.394)	0.204	(0.403)	0.258	(0.437)
Low Demand	0.309	(0.462)	0.220	(0.414)	0.210	(0.407)	0.185	(0.388)
Streamlining	0.166	(0.372)	0.226	(0.418)	0.235	(0.424)	0.324	(0.468)
Observations	387004		183422		48689		9240	
White-Collar			✓		✓		✓	
Age	20-60		20-60		35-45		35-45	
Tenure	≥ 12		≥ 12		≥ 12		≥ 60	

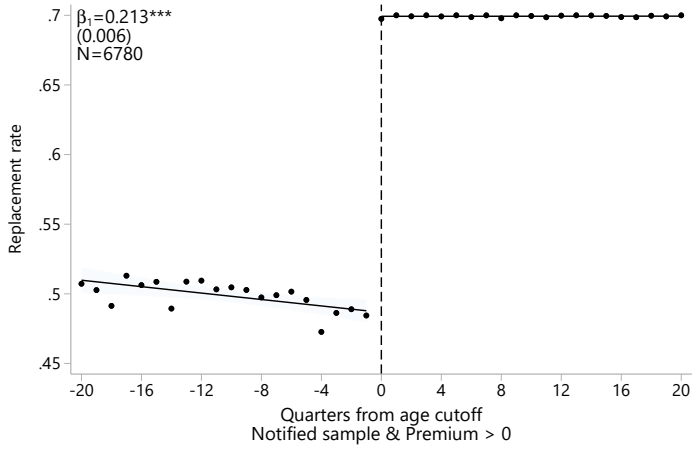
Notes: Table 1 presents summary statistics for four different samples. The first column shows the population of all displaced workers between 2005 and 2019 with at least 12 months of tenure, at least 20 years old and no more than 60 years old at the day of displacement in the PES register. The remaining columns report: Sample 1: Private-sector white-collar workers. Sample 2: White-collar workers aged 35–45 on their last day of work. Main sample: Further restricted to workers with at least 60 months of tenure. Tenure is measured in months and notice time in days. The variable Pr(College) indicates whether the worker has attended university or college, and Pr(Immigrant) indicates whether the worker is foreign-born.

of the age-40 threshold. We thus estimate the following model:

$$Y_i = \beta_1 \cdot \mathbf{1}[\text{Age}_i \geq 40] + \gamma_0 \cdot \text{Age}_i + \gamma_1 \cdot \mathbf{1}[\text{Age}_i \geq 40] \cdot \text{Age}_i + x_i'\phi + v_i \quad (1)$$

The main coefficient of interest is β_1, which is the causal effect of eligibility for supplemental UI on different outcomes, Y_i. This effect is captured by comparing individuals just above and below the 40-year age threshold using the dummy $\mathbf{1}[\text{Age}_i \geq 40]$, which takes the value 1 for individuals above age 40. We also control for age at last day of work separately on each side of the cutoff, which is captured by the coefficients γ_0 and γ_1. Provided that the assignment

Figure 3. Predicted Total Replacement Rate



Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects and covariates for higher education (dummy), number of children, and work experience. The outcome is the predicted replacement rate, inferred from the displacement date and earnings in the year before notification. The sample is all notified in our main sample which have high enough earnings to be eligible for supplemental UI.

variable is not manipulated, and that there are no other discontinuities at age 40 affecting outcomes, the estimate of β_1 is unbiased by design. We control for a number of baseline covariates (x_i')—labor market experience, educational attainment, number of kids, and fixed effects for cells defined by the interaction between event month and industry—to improve precision.[19] v_i denotes the error term.[20]

Our empirical strategy relies on the age-40 threshold rather than the tenure threshold because age is measured more precisely in our data than tenure, and the RD-design is sensitive to measurement errors in the assignment variable.[21]

[19] See Section E.3 for validity tests and results excluding the covariates and fixed effects.

[20] We use a uniform kernel and a bandwidth of five years on each side of the cutoff, which resembles the optimal bandwidth suggested by Calonico, Cattaneo, and Titiunik (2014). See Section E.2 for validity and robustness checks using alternative bandwidths and kernels, including MSE-optimal bandwidths that vary by outcome. We choose to use a constant bandwidth across all outcomes in our main analysis to ensure the sample remains consistent throughout.

[21] There is still a measurement error issue because notification dates and displacement dates are sometimes reported erroneously. This potentially creates a measurement error in age at reported displacement—an issue that is discussed at length in Cederlöf et al. (2025).

We validate our empirical design through three tests.[22] First, we investigate whether the continuity assumption holds by testing for potential bunching at the cutoff; this confirms that workers are not strategically postponing displacement to qualify for higher benefits (McCrary, 2008; Cattaneo, Jansson, and Ma, 2018). Second, we examine the pre-determined characteristics of workers near the threshold, ensuring these individuals are similar along observable dimensions other than their UI treatment status (Lee and Lemieux, 2010). Third, we confirm that the 40-year rule indeed results in a discontinuous increase in paid out UI benefits upon unemployment entry, ensuring the scheme is relevant.

The first identifying assumption we check is continuity. A worry is that workers strategically manipulate the timing of their layoff so that their last day of work occurs on or after the cutoff. For this to be the case, workers would need to convince employers to postpone the termination date until they cross the age threshold.[23] Figure 4a examines whether there is evidence of such manipulation by showing the density of displaced workers by age at the displacement date. The dots represent quarter-level bins, and the lines are fitted separately on each side of the cutoff, following Equation (1). Reassuringly, the density of layoffs is smooth around the cutoff, with no excess mass on the right hand side of age 40.

In a similar spirit, we also test if baseline characteristics vary discontinuously around the age-40 threshold. Figures 4b and 4c demonstrate there is no increment in log earnings or firm tenure when comparing workers on the two sides of the threshold.[24]

We also investigate to what extent the age threshold affects receipt of supplemental UI. Figure 4d shows the probability of receiving the benefit among workers who enter unemployment. There is a clear increase in take-up by 47% at the age threshold, and the age threshold appears to be more or less binding since very few workers below age 40 receive the benefit. Take-up is less than complete because eligibility also requires that the workplace is covered by a CBA; moreover, the individual worker has to apply for the benefit, and all workers may not be aware of its existence.[25]

In sum, we conclude that our empirical design is not refuted, as there is no evidence for manipulation or sorting around the age cutoff and a strong first stage.

[22] Additional robustness and placebo tests are presented in Section E.

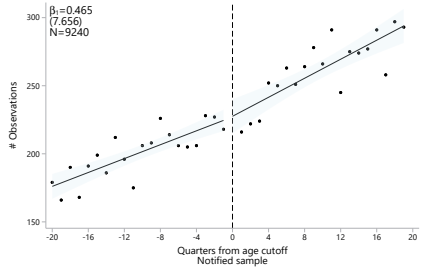
[23] Khoury (2023) and Citino, Russ, and Scrutinio (2025) document examples of this type of manipulation.

[24] In Appendix Section E.1, we do a comprehensive analysis on a number of additional predetermined characteristics, and find no evidence of manipulation.

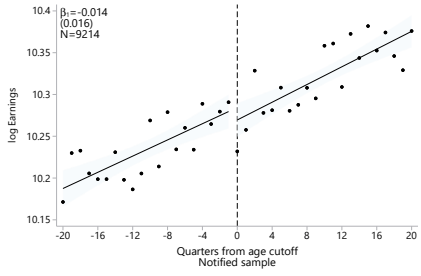
[25] Evidence from CBA sick leave insurances suggests that some workers are not aware they are entitled to extra benefits through their CBA (IFS, 2018).

Figure 4. Validity Tests and Take-up of Supplemental UI

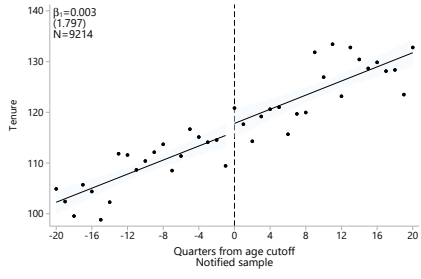
(a) Density of Observations



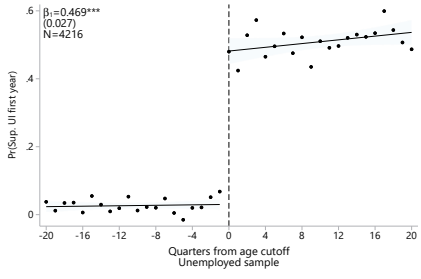
(b) log Earnings in year $t-1$



(c) Tenure



(d) Supplemental UI



Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects and covariates for higher education (dummy), number of children, and work experience. Figures 4b and 4c show validity tests for log monthly earnings one year before notification and firm tenure at notification in the spirit of Lee and Lemieux (2010). Figure 4a shows the density of observations following McCrary (2008) and Cattaneo, Jansson, and Ma (2018). Figure 4d shows the probability of receiving supplemental UI within 12 months following notification conditional on becoming unemployed in the first year.

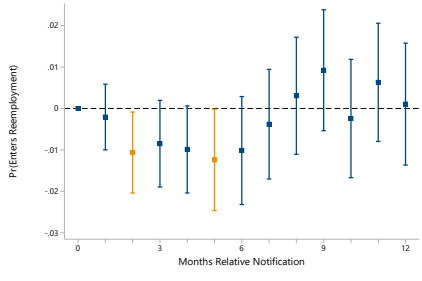
5 Effects of UI Eligibility on Unemployment

Search behavior among workers on notice is likely affected by differential eligibility for UI upon unemployment; UI generosity may reduce search effort and workers may target jobs paying higher wages. If so, we would expect workers who are eligible for higher UI to have a prolonged duration until they find a new job and that they would enter unemployment to a greater extent.

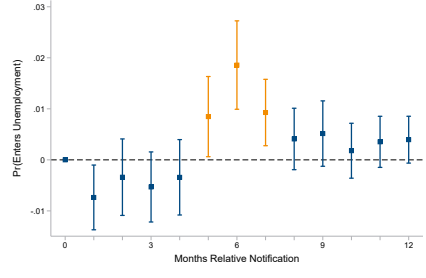
Figure 5 sheds light on these hypotheses by plotting RD-estimates of the hazards to reemployment and unemployment, respectively.[26] Figure 5a shows that the reemployment hazard is lower for workers who are eligible for higher UI during months 2-6 after notification. Treated workers thus find alternative employment to a lesser extent during the notice period, arguably because their search behavior is affected by UI eligibility. The change in search behavior spills over to the unemployment hazard displayed in Figure 5a. There is a significant increase in the hazard to unemployment 5-7 months after notification; this increase coincides with the time when notice periods typically expire (average notice times equal 5.3 months; see Table 1). As a result of being eligible for higher UI, workers have lower search effort—or higher target wages—during the notice period. They thus find alternative employment to a lesser extent and enter unemployment to a greater extent.

Figure 5. Hazards to Reemployment and Unemployment

(a) Hazard to Reemployment



(b) Hazard to Unemployment



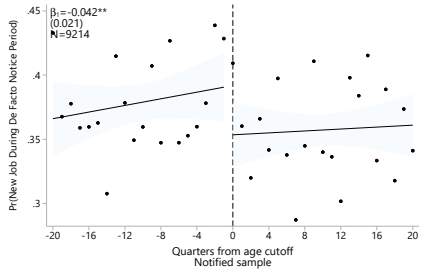
Notes: The figure displays RD estimates—along with 95% confidence intervals— based on Equation (1) detailed in Section 4. The regression specification includes event month × industry fixed effects, an indicator for any higher education, number of kids, and experience. Standard errors are clustered at the event level. Figure 5a and Figure 5b show the hazard into reemployment and unemployment, respectively. In Figure 5a, the risk set for each event month consists of all workers remaining at the notifying firm who have not yet become reemployed. Entry into reemployment is defined as appearing in a new firm a given month, provided the individual was not registered in that firm in the previous month. In Figure 5b the risk set consists of all workers remaining at the notifying firm who have not yet entered unemployment. Entry into unemployment is defined as appearing in the register during a given month, provided the individual was not registered the previous month.

The upshot of Figure 5 is that unemployment incidence is affected by eligibility for supplemental UI. Figure 6a examines the impact on the probability of finding alternative employment during the notice period; it shows that the probability falls by 4.2 percentage points, or 11 percent relative to the baseline. Figure 6b presents direct evidence of the magnitude of the impact of UI eligibility on unemployment incidence. The outcome is defined as being unemployed at any point in time during the first 12 months after notification, for all workers

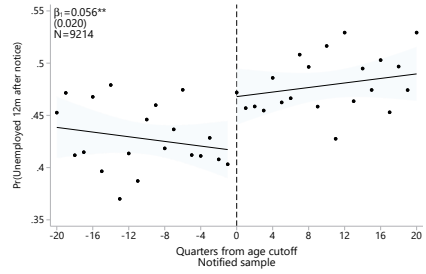
[26] In Figure C.2 , we present the same exercise using the reported displacement date instead of the notification date.

Figure 6. New Job Notice Period & Unemployment Incidence

(a) New Job During the Notice Period



(b) Unemployment Incidence



Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects and covariates for higher education (dummy), number of children, and work experience. The outcome in Panel (a) is the likelihood of entering a new job during the notification period. The outcome in Panel (b) is the likelihood of ever becoming unemployed during the first 12 months following notification.

who received notice. We find that workers eligible for supplemental UI are 5.6 percentage points more likely to enter unemployment at some point during this time period. Relative to the baseline, the increase in unemployment incidence corresponds to 13%.

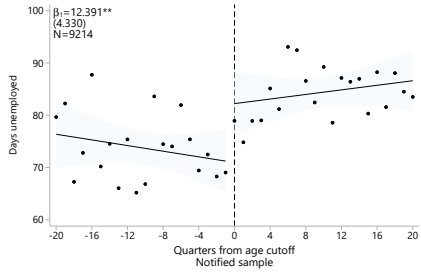
Next, we turn to the impact on the number of days in unemployment; see Figure 7. As unemployment incidence is affected by the treatment of interest, workers who experience unemployment are likely a selected sub-sample of the overall treated population. Figure 7a thus begins by examining how the total number of days in unemployment for all notified workers, where workers who do not experience unemployment are assigned $D = 0$, relates to supplemental UI eligibility. Figure 7b proceeds to duration outcomes for workers who experience unemployment, while recognizing that the estimates for this sub-population likely do not have a causal interpretation.

Figure 7a shows that supplemental UI eligibility increases unemployment by 12.4 days—or 17% relative to the baseline of 70 days. The impact on unemployment is thus mostly driven by the effect on unemployment incidence (13%) rather than unemployment duration. Figure 7b also shows explicitly a fairly marginal, and statistically insignificant, association between duration and UI eligibility, which corresponds to 4% relative to the baseline.

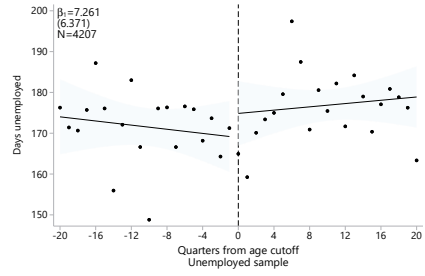
How does our estimate of the impact of UI on unemployment compare to the rest of the literature? Figure 3 shows that supplemental UI generates

Figure 7. Duration Outcomes

(a) Notified



(b) Unemployed Sample



Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects and covariates for higher education (dummy), number of children, and work experience. Figure 7a and Figure 7b show the total days in unemployment for the full and unemployed-only samples, respectively.

a large relative change in benefit replacement rates corresponding to 44% percent relative to the baseline (0.213/0.487).[27] When adjusted for less than complete take-up—0.469 according to Figure 4d—this suggests an elasticity of 0.85.[28] Comparing this elasticity to other studies examining the impact of the UI replacement rate—see Figure A.1 —this is in line with many other studies. With that said, most previous studies only investigate the duration effect among the unemployed. Krueger and Meyer (2002) survey the evidence on the impact of UI while taking the effect on incidence as well as duration of unemployment into account. They conclude that UI benefit elasticity is around 1, which is remarkably close to our estimate.[29]

Our estimates thus suggest that most of the behavioral adjustment to UI occurs prior to unemployment entry. Notified workers respond to UI by altering their search strategies such that they obtain alternative employment to a lesser extent. Therefore, UI causes a larger inflow into unemployment and, in our context, it would be seriously misleading to only focus on the duration impacts

[27] Estimates are also displayed in Table C.1.

[28] The estimate on take-up is arguably an upper-bound for the overall population of notified workers. Compared to unemployment entrants, workers who find alternative employment during the notice period, either value the increase in UI to a lesser extent, are not eligible via their CBA to the same extent, or lack information about the existence of the benefit.

[29] Fredriksson and Söderström (2020) is one of the few studies that directly estimate the total impact of UI on unemployment. Their micro estimates suggest an elasticity of 1.4.

of more generous UI.[30] Our results are in line with analyses showing that search behavior responds to increasing risk of displacement (e.g., Cederlöf et al. (2025), Häggkvist (2026), and Simmons (2024)), in general, and Grindaker and Simmons (2024) who show that the reallocation behavior among the employed is affected by UI generosity, in particular.

6 Selection into Unemployment and UI Take-up

The previous section established that supplemental UI leads more unemployment, primarily by generating a larger inflow into unemployment. Our purpose in this section is to characterize the unemployment inflow and the workers who decide to take-up supplemental UI—issues that have implications for the design of optimal UI.

We begin by splitting the sample into quintiles by their earnings in the year before displacement. We then run regressions for each income group within displacement year. Figures 8a and 8b illustrate the relationship between unemployment incidence and UI take-up, respectively, and previous income. A slightly unusual feature of our setting is that the gain from supplemental UI eligibility increases along the income distribution. To emphasize this feature, Figure 8 reports the increase in the replacement rate associated with supplemental UI. In the bottom quintile of the distribution, there is little to be gained from eligibility; accordingly, we do not find an impact on incidence in this part of the distribution. The gains associated with supplemental UI eligibility are substantial at the top end of the distribution—they are in the order of 30 percentage points in the top quintile—yet, we do not observe an impact at the top end of the distribution. In fact, the effect of UI eligibility on incidence is concentrated at the 3rd-4th quintile of the distribution.

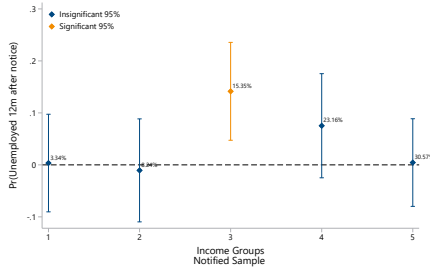
We see a similar pattern for the likelihood of obtaining a new job during the notice period. Figure 8c shows that workers in the middle quintiles of the income distribution are less likely to find a new job during the notice period compared to high and low income workers. Figure 8d illustrates that differential impacts on incidence also converts into higher unemployment, in particular for the middle quintile.

The differential impacts across the distribution may be a bit surprising at first sight, as the gain from supplemental UI eligibility increases monotonically with prior earnings. To investigate the driving forces of this pattern, Appendix Section F sets up a partial equilibrium search model which is consistent with the institutional context that we are analyzing. The key feature of the model is that there is heterogeneity in re-employment probabilities which is positively correlated with prior income. Figure 9 illustrates that there is such an associ-

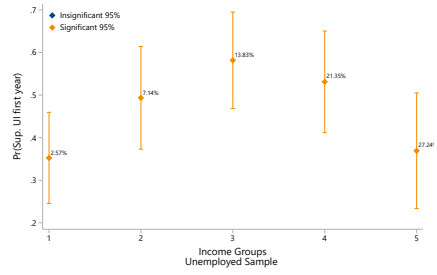
[30] Appendix Section D presents an analysis of how job quality is affected by UI generosity. We find that the mean impacts are small. However, we do not have sufficient precision to rule out substantial effects.

Figure 8. Unemployment and Reemployment Outcomes by Income Quintile

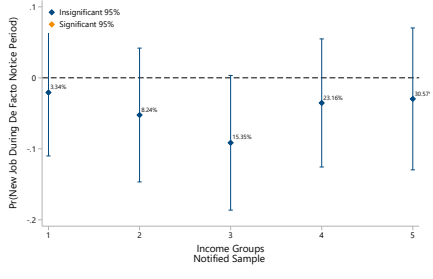
(a) Pr(Unemployed)



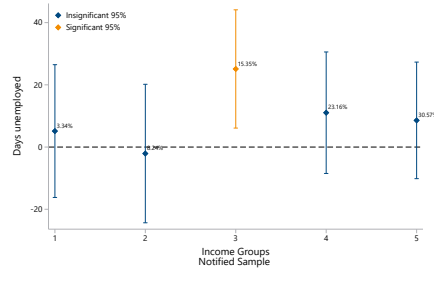
(b) UI Take-up



(c) Pr(New Job During Notice Period)



(d) Days In Unemployment



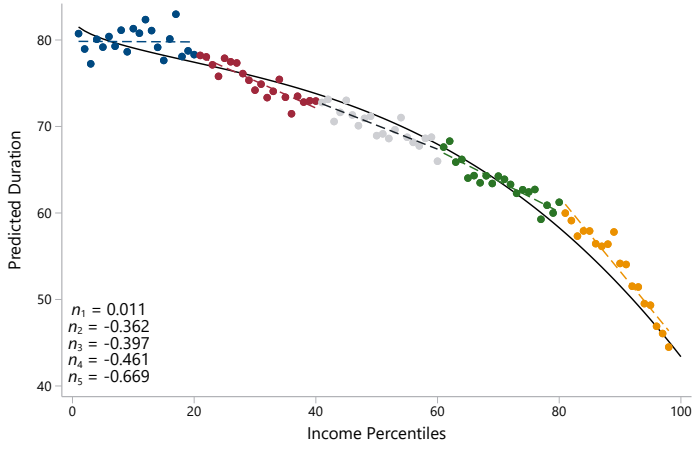
Notes: The sample is divided into income quintiles based on earnings in the year prior to notification. Figure 8a plots the likelihood of entering unemployment within the first year. Figure 8b shows the likelihood of receiving supplemental UI conditional on unemployment. Figure 8c shows the probability of finding a new job during the de facto notice period, and Figure 8d displays the total days in unemployment during the first year. In all panels, the numbers above the markers represent the average percentage point increase in the replacement rate provided by supplemental UI eligibility.

ation in the data by showing how predicted unemployment relates to previous earnings among notified workers.[31] The negative relationship implies that higher-earnings individuals find jobs faster. In fact, the elasticity of unemployment with respect to income increases along the income distribution as shown in the bottom left corner in the figure.

Since the re-employment probability is higher among high-income individuals than among low-income individuals, the expected present value of a given

[31] To construct Figure 9 we have first regressed total unemployment days on a set of pre-determined characteristics (age, tenure, gender, immigrant background, experience, industry, prior earnings, and firm/plant size) for all notified white-collar workers *not* included in our main sample. These out-of-sample regression coefficients are then used to predict unemployment days for the workers in our sample. Figure 9 thus shows how unemployment (incidence as well as duration) relates to previous earnings holding the other characteristics constant.

Figure 9. Unemployment By Income



Notes: The figure shows the association between predicted total unemployment duration and previous earnings in our main sample. Workers are divided into percentiles based on their previous earnings. The solid black line represents a flexible polynomial fit for all observations, while the dashed colored lines represent linear fits for each income quintile. The values in the bottom-left corner are the estimated income elasticities for the five segments of the sample, calculated from a regression of the log of predicted total unemployment days on log previous earnings.

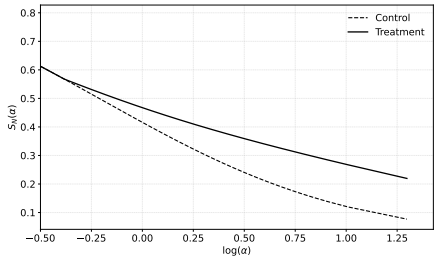
increase in UI replacement rates is lower for high-income individuals simply because they expect to stay unemployed for a shorter period of time. If this effect is sufficiently strong, we obtain the inverted U-shape pattern of take-up illustrated in Figure 8b, despite the fact that high-earnings individuals have more to gain from UI-eligibility.

Moral hazard in our framework comes from workers choosing search effort optimally. In the model, search effort is increasing in employment prospects because of higher marginal returns to given effort. The moral hazard effect will thus be concentrated among workers who have relatively good employment prospects, with short expected durations. The selected sub-set of workers who experience unemployment because of the treatment may thus consist of workers with worse employment prospects—and consequently lower moral hazard—to a greater extent compared to the overall notified population.

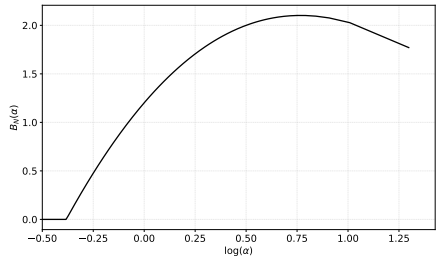
Figure 10 presents a number of key outcomes from a calibrated version of the partial equilibrium search model. We set the key institutional parameters such that they fit the main characteristics of the UI system as well as advance notice. For the purposes of the calibration, advance notice equals 5 months, supplemental UI replaces 70% of previous earnings during 6 months of duration, while regular UI has an average replacement rate of 50%. Notice that the search cost parameters—which are key in a model of this kind—are set to reflect the average moments in the data, *not* the impacts across the distribution (see Appendix F).

Figure 10. Outcomes of a calibrated search model

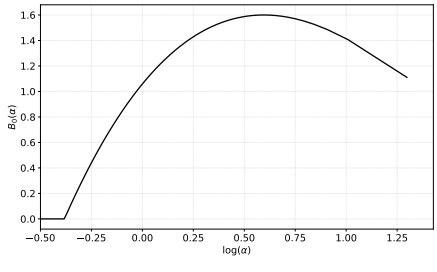
(a) Survival until unemployment entry



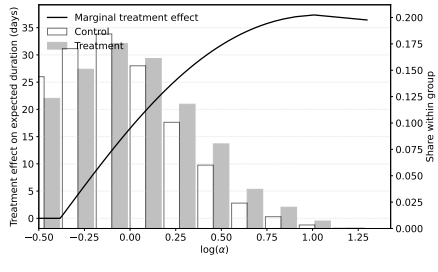
(b) Expected PV of UI at Unemployment Entry



(c) Expected PV of UI at Notification



(d) Selection into Unempl. and Treatment



Notes: This figure illustrates the outcomes of a partial equilibrium search model calibrated to a 5-month notice period and a 70%/50% UI replacement structure. Each outcome is displayed for levels of income and employment prospects, α. Figure 10a shows the survival function until unemployment entry. Figure 10b and Figure 10c show the expected present value of UI at unemployment and notification, respectively. Figure 10d shows the treatment effect on expected duration (solid line) and the density distribution of unemployed workers of different types (bars).

Figure 10a shows the survival function until unemployment by treatment group for workers with different employment and income prospects as indexed by $\log \alpha$ (assumed to be normally distributed with a standard deviation of 0.42); for obvious reasons, it is downward sloping in α. Figure 10b shows the expected present value of UI eligibility at the point of unemployment entry; if take-up costs are unrelated to employment prospects, the shape of this function will govern how take-up varies across the previous income distribution. As illustrated by the figure, we get some of the inverted U-shape that we observe in the data. Figure 10c illustrates the expected present value of UI at notification, i.e., Figure 10b discounted to present value using the survival function shown by the solid line shown in Figure 10a. This value is important since it governs how search behavior at notification responds to the treatment. Workers with really low α (at the lower end of the income distribution) are unaffected by treatment as regular UI already replaces at least 70% of the previous income. Figure 10d, finally, illustrates the marginal treatment effects on the duration given entry (the solid line). Without selection into unemployment, we would observe the population weighted average of the marginal treatment effects. The bars show the distribution of workers who enter unemployment after the notification period. Relative to the original normal distribution, it is skewed to the left—but less so for the treated population. Since the marginal treatment effect is rising for most of the range of α, the left-skew will imply that the true treatment effect is lower among the unemployed than in the notified population, due to selection. However, the treatment effect among the unemployed is not observed. Rather we observe the average duration difference between the treated and control which is rather severely downward biased relative to the true treatment effect because the treated have better employment prospects. Relative to the data, the model is does not quite reproduce the declining effect on incidence among high α individuals. We conjecture that a model with savings, that are increasing in α, would better reproduce this pattern as the value of supplemental UI would decline even faster.

In sum, we show that the impact of supplemental UI eligibility is highest among workers in the middle of the income distribution, despite the fact that the direct monetary gain from eligibility rises with previous earnings. We rationalize this result through the lens of a partial equilibrium search model where higher-income individuals also have greater chances of becoming re-employed. If so, the expected present value of eligibility may be lower for higher-income individuals. The selection pattern into unemployment also causes a downward bias in the estimates of the effect of UI generosity on unemployment duration.

7 Normative Implications

Suppose the government determines the UI replacement rate (b) by maximizing aggregate welfare among all workers subject to a UI budget constraint, $\tau(1-u) = bu^b + au$, where u denotes overall unemployment and u^b the share receiving UI.[32] The budget constraint takes into consideration that there is alternative income support (a) available for workers who are not eligible for UI.

Standard analysis yields that the optimal UI replacement rate is determined by:

$$\frac{v'(c^b) - v'(c^e)}{v'(c^e)} = \frac{BC}{MC} \tag{2}$$

where c^b and c^e denotes consumption among the insured (super-indexed b) and the employed (super-indexed e), respectively. The left-hand-side is thus the marginal social value of transferring an additional \$ from the employed to the insured. The right-hand-side is the fiscal externality, the behavioral cost of transferring an additional \$ from the employed to the insured—the BC/MC-ratio in the terminology of Schmieder and von Wachter (2016).

In our context, the fiscal externality is given by

$$\frac{BC}{MC} = (\eta^I + \eta^{D^b})(1 + \frac{\tau + a}{b}) + \frac{\tau + a}{b}\frac{D - D^b}{D^b}\eta^I \tag{3}$$

where η^I and η^{D^b} denotes the elasticity of incidence and the duration of benefit receipt with respect to b, respectively, while D^b and D denotes the duration of benefit receipt and overall duration, respectively. Equation (3) generalizes the fiscal externality of a standard Bailly-Chetty condition (Baily, 1977; Chetty, 2006) to our setting. The standard setting has $\eta^I = 0$ and $a = 0$, in which case the right-hand-side of Equation (3) is reduced to $\eta^{D^b}(1 + \frac{\tau}{b})$.

We have estimated $\eta^I = 0.65$ and the elasticity of overall unemployment duration with respect to b, $\epsilon^D = 0.20$. In the data, $D = 170$, $D^b = 90$, $b = 0.21$, and $a = 0.49$. Moreover, $\eta^{D^b} = \frac{D}{D^b}\epsilon^D = 0.38$. With an overall unemployment rate of 8%, the tax rate required to fund the UI budget is $\tau = 0.052$. We thus have:

$$\frac{BC}{MC} = (0.65{+}0.38)\left[1 + \frac{0.052 + 0.49}{0.21}\right] + \left[\frac{0.052 + 0.49}{0.21}\right]\left[\frac{170 - 90}{90}\right]0.65 = 5.17$$

If we shut down the incidence margin, on the other hand, we get

$$\left[\frac{BC}{MC}\right]_{I=\bar{I}} = 0.38\left[1 + \frac{0.052 + 0.49}{0.21}\right] = 1.29$$

Adjusting for the impact on incidence thus matters substantially. Doing so makes the fiscal externality four times larger ($5.17/1.29 \approx 4$).

[32] Appendix section Section F.3 presents additional details on this analysis.

So far we have used supplemental UI as a way of identifying the impact of UI policies. Supplemental UI is, however, determined by the bargaining parties involved in the CBA, and the (direct) additional cost of supplemental UI is funded by the bargaining parties. Their behavior, however, generates fiscal externalities onto the public budget which are not accounted for when setting the generosity of supplemental UI. In Appendix Section F.4 we show that when the bargaining parties ignore the cost associated with providing regular UI (a in the notation above), there are large externalities imposed on the central government. The central government has to raise an additional \$4 of tax revenue to cover for the fact that the CBA provides an additional \$ of supplemental UI.

8 Conclusion

We contribute to the literature on the effect of Unemployment Insurance (UI) on unemployment entry. Unlike previous studies we investigate how workers react starting from the point of notification and onward. This is possible due to our access to detailed administrative data on layoffs, which enables us to identify both which workers are notified and the effects between notification and displacement.

We exploit exogenous variation in UI coverage stemming from a Swedish Collective Bargaining Agreement (CBA) granting white-collar workers above age 40 a substantial increase in their UI. We find that eligibility reduces the probability of finding alternative employment during the notice period by 4.2 percentage points and increases the likelihood of entering unemployment within one year after notification by 5.6 percentage points. Given a baseline of 42 percentage points, the latter number implies a relative increase of about 13%. The impact on incidence accounts for the lion's share of the overall unemployment impact (17%). The elasticity of unemployment with respect to the UI replacement rate is 0.85 when scaled with benefit take-up.

The increased unemployment inflow is driven primarily by workers in the middle of the income distribution. We rationalize this finding through the lens of a partial equilibrium search model where the potential gain from supplemental UI eligibility increases with earnings, but the baseline unemployment risk decreases with earnings. The interplay of these two factors creates a hump-shaped relationship: at lower income levels, the potential gains dominate, while at higher income the low baseline unemployment risk is more important. Consequently, the behavioral effects are primarily observed among middle-income workers, while the effects at the lower and higher end of the distribution are small. The selection into unemployment also implies that the duration impact of higher UI is substantially downward biased.

We also examine the normative implications of ignoring the incidence margin. Doing so, in our setting, would imply that we severely underestimate the

fiscal externality associated with raising the UI replacement rate. The fiscal externality increases by a factor 4 when the incidence margin is appropriately into taken account.

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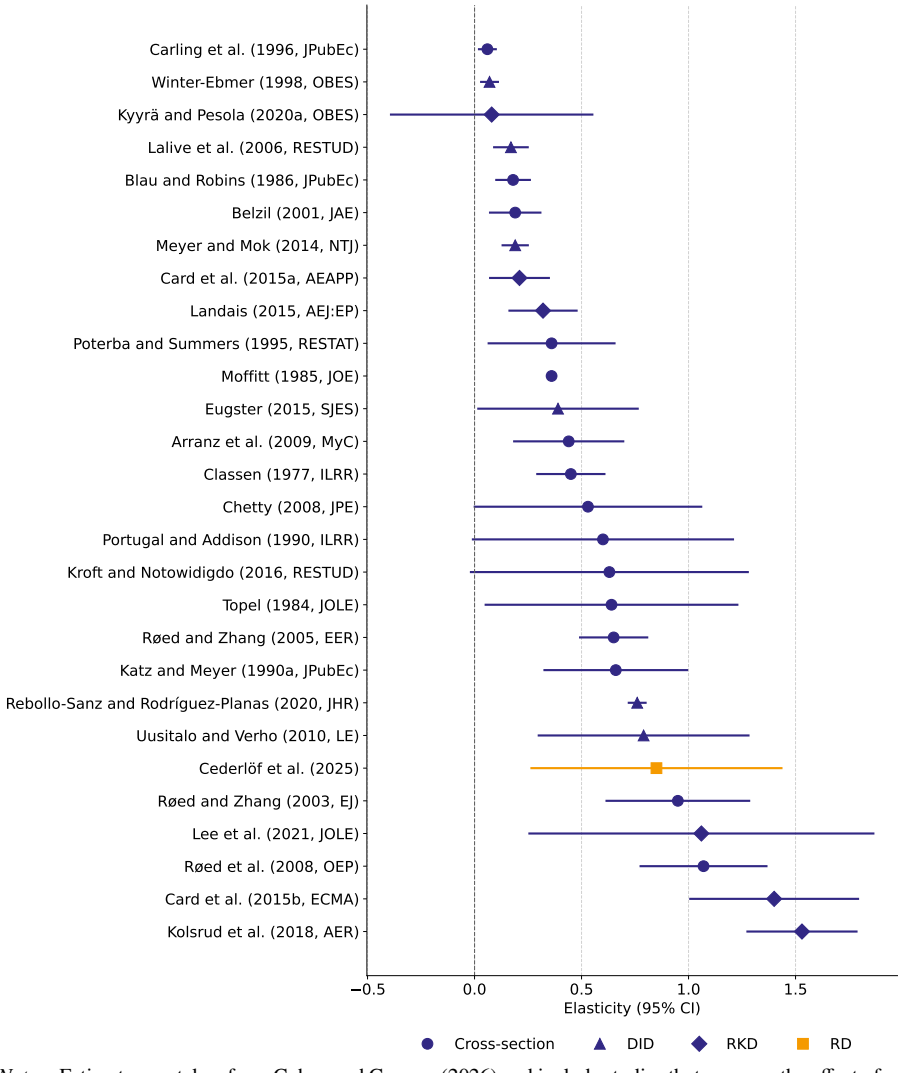
A Literature Review

Table A.1. *Overview of Previous Studies*

Paper	Journal	Design	Measure	Elast.	SE
Arranz et al. (2009)	*MyC*	CS	Claimed	0.44	0.13
Belzil (2001)	*JAE*	CS	Non-emp	0.19	0.06
Bennmarker et al. (2007)	*Labour*	DID	Non-emp	2.01	0.68
Card et al. (2015a)	*AEAPP*	RKD	Claimed	0.21	0.07
Card et al. (2015b)	*ECMA*	RKD	Non-emp	1.40	0.20
Carling et al. (2001)	*EJ*	DID	Non-emp	1.97	0.97
Carling et al. (1996)	*JPubEc*	CS	Non-emp	0.06	0.02
Chetty (2008)	*JPE*	CS	Non-emp	0.53	0.27
Classen (1977)	*ILRR*	CS	Claimed	0.45	0.08
Eugster (2015)	*SJES*	DID	Non-emp	0.39	0.19
Katz and Meyer (1990a)	*JPubEc*	CS	Claimed	0.66	0.17
Katz and Meyer (1990b)	*QJE*	CS	Claimed	0.41	0.35
Kolsrud et al. (2018)	*AER*	RKD	Claimed	1.53	0.13
Kroft and Notowidigdo (2016)	*RESTUD*	CS	Non-emp	0.63	0.33
Kyyrä and Pesola (2020a)	*OBES*	RKD	Non-emp	0.08	0.24
Lalive et al. (2006)	*RESTUD*	DID	Non-emp	0.17	0.04
Landais (2015)	*AEJ:EP*	RKD	Non-emp	0.32	0.08
Landais (2015)	*AEJ:EP*	RKD	Claimed	0.73	0.11
Lee et al. (2021)	*JOLE*	RKD	Claimed	1.06	0.41
Meyer and Mok (2014)	*NTJ*	DID	Non-emp	0.19	0.03
Moffitt (1985)	*JOE*	CS	Non-emp	0.36	0.01
Portugal and Addison (1990)	*ILRR*	CS	Non-emp	0.60	0.31
Poterba and Summers (1995)	*RESTAT*	CS	Non-emp	0.36	0.15
Rebollo-Sanz et al. (2020)	*JHR*	DID	Non-emp	0.76	0.02
Røed and Zhang (2003)	*EJ*	CS	Non-emp	0.95	0.17
Røed and Zhang (2005)	*EER*	CS	Non-emp	0.65	0.08
Røed et al. (2008)	*OEP*	CS	Non-emp	1.07	0.15
Topel (1984)	*JOLE*	CS	Non-emp	0.64	0.30
Uusitalo and Verho (2010)	*LE*	DID	Non-emp	0.79	0.25
Winter-Ebmer (1998)	*OBES*	DID	Non-emp	0.07	0.02
Cederlöf et al. (2026)	–	RD	Total	0.85	0.30

Notes: Estimates are taken from Cohen and Ganong (2026). The table summarizes elasticity estimates of unemployment duration with respect to UI generosity, measured in replacement rates. Design refers to the identification approach: CS is cross-sectional, DID is difference-in-differences, RKD is regression kink design, and RD is regression discontinuity. Measure indicates whether the outcome is unemployment claims or non-employment durations. The elasticities are interpreted as the percentage change in unemployment or non-employment duration resulting from a one percent increase in the replacement rate.

Figure A.1 . Unemployment Duration Elasticities By Study

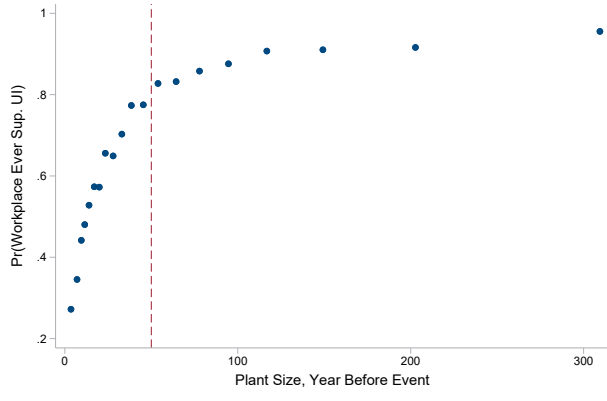


Notes: Estimates are taken from Cohen and Ganong (2026) and include studies that measure the effect of an increase in the UI replacement rate on unemployment duration. Each point represents the estimated elasticity from a single study, with horizontal lines indicating 95% confidence intervals. Marker shapes indicate the empirical design: circles for cross-sectional studies, triangles for difference-in-differences (DID), diamonds for regression kink designs (RKD), and squares for regression discontinuity designs (RDD). Studies are labeled with author(s), year, and journal. The highlighted marker (orange square) represents the estimate from this paper.

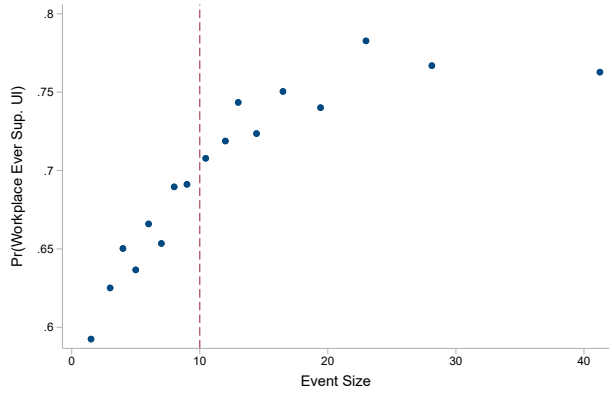
B Descriptive Statistics

Figure B.1 . Share of Workplaces Ever Paid Supplemental UI

(a) Workplace Sizes



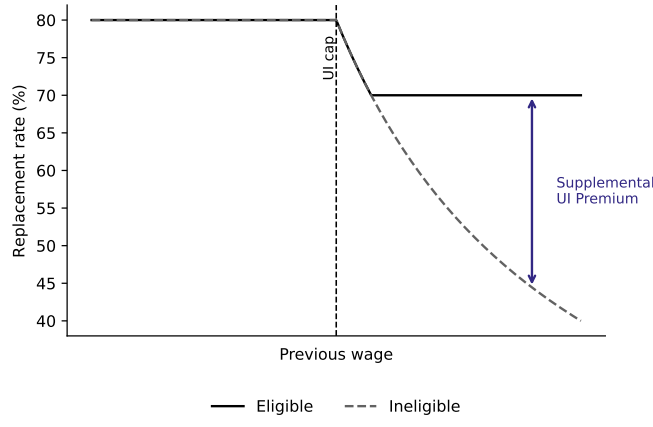
(b) Event Sizes



Notes: Figure B.1 a illustrates the weighted share of firms in the main sample where workers have received supplemental UI at any point in time. We display this measure over plant size in the year before the event. Figure B.1 b shows the equivalent by event size. The dashed red vertical lines show the required minimum level for the observation to be included in our analysis.

Figure B.2 . Evolution of UI Cap

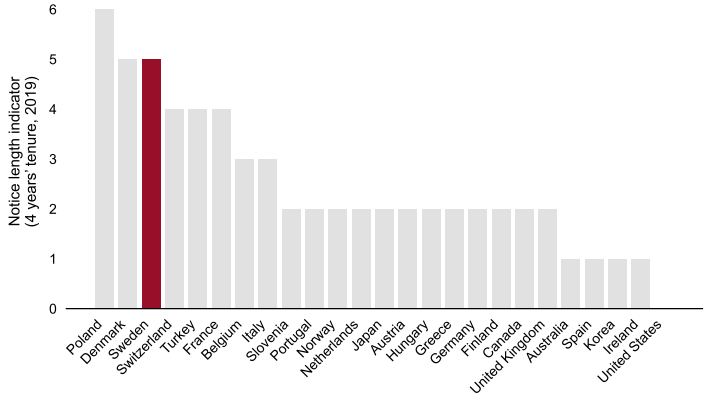
(a) Total UI By Previous Wage



(b) Median Wage and Regular UI Cap

Notes: Figure B.2 a shows replacement rates during the first 130 days by previous wage levels. The solid line represents workers ineligible for supplemental UI (AGE) while eligible for regular UI, while the dashed line represents those eligible for both. The vertical dashed line marks the UI cap, the positioning of which varies by year as shown in Figure 2. Figure B.2 b illustrates the median wage and regular UI cap over time.

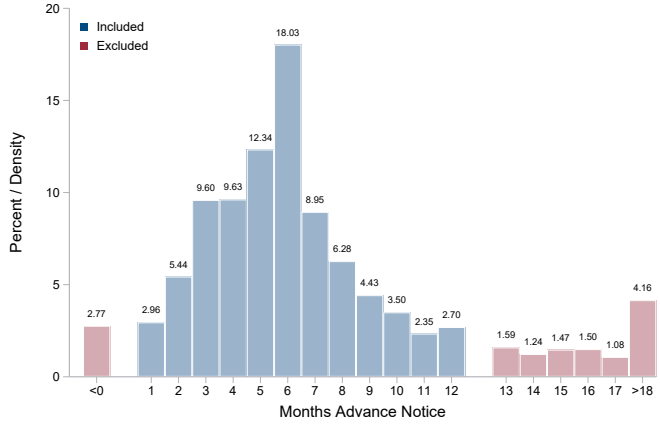
Figure B.3 . Advance Notice Generosity By OECD Countries (2019)



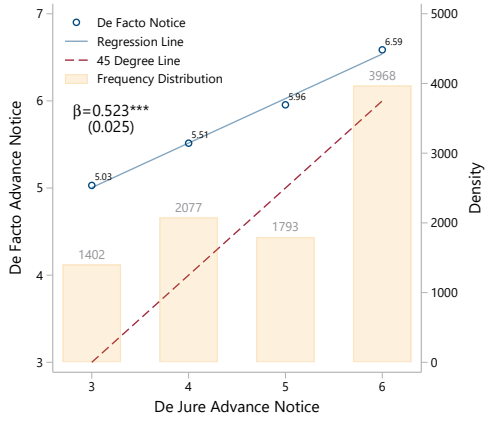
Notes: This figure presents indicators of advance-notice generosity across countries. The values are drawn from OECD (2020). The underlying scale runs from 0 to 6, where a higher number corresponds to more generous regulation – a longer required advance-notice.

Figure B.4 . Advance Notice Descriptive Statistics

(a) Distribution of Advance Notice



(b) De Facto vs De Jure Advance Notice



Notes: Figure B.4 a presents the distribution of advance notice for the workers in our sample (blue) and those excluded (red). Above each bar, we have highlighted the sample share in percent. Figure B.4 b displays average de facto advance notice (from PES data) plotted against de jure advance notice groups, which are calculated based on the statutory rules in SFS (1982). Hollow dots represent the mean de facto notice for each group, while the blue line provides a fit based on the raw data. The red dashed line represents the 45-degree identity line, and the background histogram indicates the frequency distribution across de jure categories.

Table B.1. *Comparison of the UI System Components*

UI Source	Unemployment Assistance	Regular	Supplemental
Provider	Government	UI Funds	CBAs
Financing	Payroll taxes	Gov. subsidies + membership fees[a]	Employer-financed[b]
Eligibility	Universal	12m membership + work req.[c]	Age 40+, 5 years tenure, forced quit
Replacement	$\approx$ 20%	$\leq$ 80%[d]	70%
Duration	300 Days	300 Days	130 Days

[a] $\approx$ 150 SEK $\approx$ 16 USD per month.
[b] $\approx$ 0.3% of labor costs.
[c] Requires at least 80 hours/month (or 480 hours over 6 months) during the last year.
[d] Decreasing in earnings.

Table B.2. *Descriptive statistics, Unemployed Sample*

	(1)		(2)		(3)		(4)	
	Population		**Sample 1**		**Sample 2**		**Main Sample**	
Workers								
log Earnings, t-1	9.947	(0.617)	10.02	(0.673)	10.03	(0.676)	10.21	(0.395)
Tenure at layoff	76.85	(71.07)	75.22	(69.29)	69.98	(57.02)	124.1	(52.16)
Notice time	103.6	(112.7)	114.4	(128.0)	105.1	(110.4)	151.3	(75.37)
Pr(College)	0.161	(0.367)	0.261	(0.439)	0.331	(0.471)	0.368	(0.482)
Pr(Woman)	0.358	(0.479)	0.421	(0.494)	0.458	(0.498)	0.500	(0.500)
Pr(Married)	0.121	(0.326)	0.127	(0.333)	0.103	(0.304)	0.0929	(0.290)
Age	41.49	(12.41)	42.84	(11.89)	39.68	(2.897)	40.13	(2.886)
Pr(Immigrant)	0.197	(0.398)	0.166	(0.372)	0.190	(0.392)	0.148	(0.355)
Managers	0.00685	(0.0825)	0.0152	(0.123)	0.0136	(0.116)	0.0249	(0.156)
Plants								
Plant size, t-1	427.7	(1249.5)	328.7	(1159.9)	345.5	(1151.9)	1013.0	(2207.0)
Firm size, t-1	1720.9	(3899.4)	1427.2	(3692.9)	1478.0	(3776.5)	3394.0	(5564.5)
Number of notified	55.13	(109.3)	44.53	(105.4)	46.32	(105.3)	98.80	(171.1)
Manufacturing	0.437	(0.496)	0.281	(0.450)	0.283	(0.451)	0.443	(0.497)
Trade	0.105	(0.306)	0.140	(0.347)	0.147	(0.355)	0.107	(0.309)
Transport	0.0619	(0.241)	0.0736	(0.261)	0.0711	(0.257)	0.0659	(0.248)
Cause for layoff								
Other	0.186	(0.389)	0.165	(0.371)	0.169	(0.375)	0.226	(0.418)
Bankruptcy	0.152	(0.359)	0.249	(0.432)	0.230	(0.421)	0	(0)
Closure	0.177	(0.381)	0.166	(0.372)	0.172	(0.377)	0.239	(0.427)
Low Demand	0.350	(0.477)	0.237	(0.425)	0.232	(0.422)	0.233	(0.423)
Streamlining	0.135	(0.342)	0.183	(0.386)	0.196	(0.397)	0.302	(0.459)
Observations	219556		97390		25802		4219	
White-Collar			✓		✓		✓	
Age	20-60		20-60		35-45		35-45	
Tenure	≥ 12		≥ 12		≥ 12		≥ 60	

Notes: The table presents summary statistics for four different samples. The first column shows the population of all displaced workers between 2005 and 2019 with at least 12 months of tenure. The remaining columns report: Sample 1: Private-sector white-collar workers. Sample 2: White-collar workers aged 35–45 on their last day of work. Main sample: Further restricted to workers with at least 60 months of tenure who are unemployed at some point during the 12 months following notification. The variable Pr(College) indicates whether the worker has attended university, and Pr(Immigrant) indicates whether the worker is foreign-born.

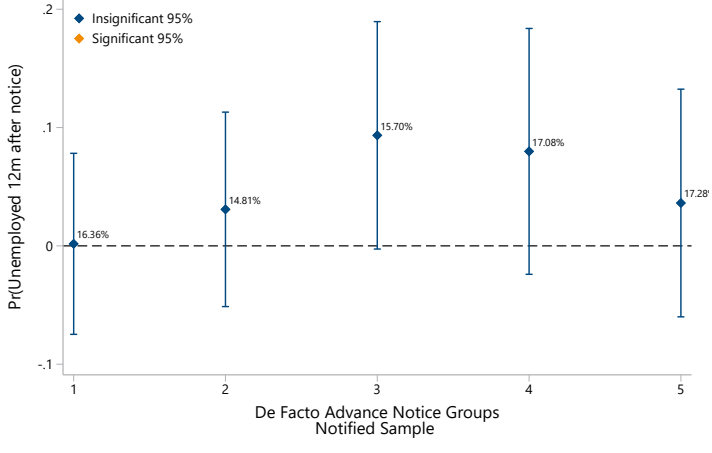
C Additional Outcomes

Table C.1. *Main RD Estimates*

	Notified		Unemployed	
	Constant	RD Estimate	Constant	RD Estimate
Pre-Determined				
Pred. Replacement Rate	0.561***	0.163***	0.600***	0.132***
	(0.006)	(0.007)	(0.008)	(0.008)
log Earnings t-1	10.282***	-0.014	10.192***	-0.031
	(0.011)	(0.016)	(0.017)	(0.023)
Tenure	116.889***	0.003	115.285***	2.356
	(1.530)	(1.797)	(2.087)	(2.650)
Mobility Outcomes				
Unemp. Incidence	0.417***	0.056***	1.000	0.000
	(0.015)	(0.020)	(.)	(.)
New Job During Notice	0.391***	-0.042**	0.241***	-0.061**
	(0.017)	(0.021)	(0.022)	(0.026)
Unemp. Duration	71.174***	12.391***	168.474***	7.261
	(3.447)	(4.330)	(5.229)	(6.371)
Job Quality				
Firm Effects New Job	0.046***	0.003	0.026**	0.003
	(0.007)	(0.008)	(0.011)	(0.013)
Tenure New Job	26.702***	0.418	22.769***	0.172
	(0.706)	(0.911)	(0.935)	(1.276)
Log Earnings New Job	10.180***	0.022	10.073***	0.001
	(0.016)	(0.018)	(0.023)	(0.028)
Covariates	✓		✓	
FEs	✓		✓	
Clusters	2301		1596	
N	9214		4207	

Notes: This table reports RD estimates of the effect of supplemental UI eligibility, corresponding to β_1 in Equation (1). The running variable is age at the last day of work, normalized to 40 years. Estimates are derived from a linear specification estimated separately on each side of the age cutoff. All models include covariates for higher education, number of children, and work experience, as well as event month × industry fixed effects. Standard errors, clustered at the event level, are reported in brackets. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

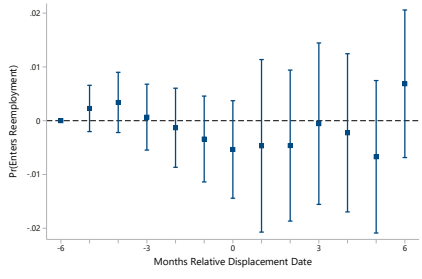
Figure C.1 . Unemployment Incidence By Advance Notice



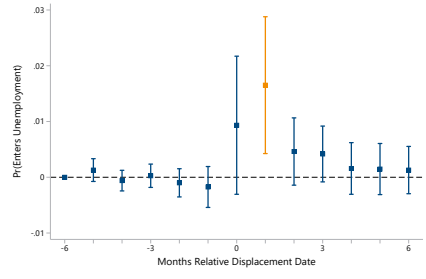
Notes: The figure displays RD estimates—along with 95% confidence intervals— based on Equation (1) detailed in Section 4. The regression specification includes event month × industry fixed effects, an indicator for any higher education, number of kids, and experience. Standard errors are clustered at the event level. The outcome is entering a new job during the de facto notice period. The sample is divided into income quintiles based notification time. The numbers above each circle are the average percentage point increase in replacement rate from becoming eligible for supplemental UI.

Figure C.2 . Hazards to Reemployment and Unemployment, Relative Displacement Date

(a) Hazard to Reemployment



(b) Hazard to Unemployment



Notes: The figure displays RD estimates—along with 95% confidence intervals— based on Equation (1) detailed in Section 4. The regression specification includes event month × industry fixed effects, an indicator for any higher education, number of kids, and experience. Standard errors are clustered at the event level. Figure C.2 a and Figure C.2 b show the hazard into reemployment and unemployment, respectively. In Figure C.2 a, the risk set for each event month consists of all workers remaining at the notifying firm who have not yet become reemployed. Entry into reemployment is defined as appearing in a new firm a given month, provided the individual was not registered in that firm in the previous month. In Figure C.2 b the risk set consists of all workers remaining at the notifying firm who have not yet entered unemployment. Entry into unemployment is defined as appearing in the register during a given month, provided the individual was not registered the previous month.

D Job Quality

Understanding whether higher benefits lead to better employment outcomes is central to evaluating the overall impact of UI. While the negative moral hazard effect of UI is well-documented, it may also enable workers find more suitable job matches, higher earnings, and welfare improvements. On the other hand, prolonged unemployment may lead to skill depreciation or stigmatization as workers remain out of the labor force longer, thereby counteracting the potential benefits.

Here we examine whether the characteristics of the new job—defined as the first new job spell in the employer-employee register after notification—is affected by UI eligibility. The characteristics we consider are: the "quality" of the employer (as measured by AKM firm fixed effects), how long the job match lasts, and monthly earnings.

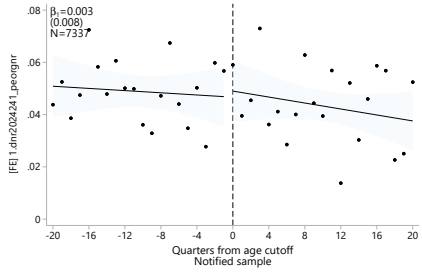
Assessing employer quality is challenging because a new wage may reflect several distinct factors, such as the firm's general pay scale, its ability to attract high-ability individuals, or the degree of match quality between the worker and the firm (Fredriksson, Hensvik, and Skans, 2018). To infer the productivity of the employer, we follow Abowd, Kramarz, and Margolis (1999) by decomposing individual wages into a component attributable to worker and firm specific characteristics respectively. The AKM-decomposition is based on the full wage histories of all workers in our sample up until the year prior to displacement. By using only pre-displacement data, we ensure that the estimated firm effects are not contaminated by the displacement events we study. We then estimate the following fixed effects model:

$$\ln w_{it} = \alpha_{j(i)} + \psi_{J(i,t)} + x'_{it}\beta + \varepsilon_{it} \tag{1}$$

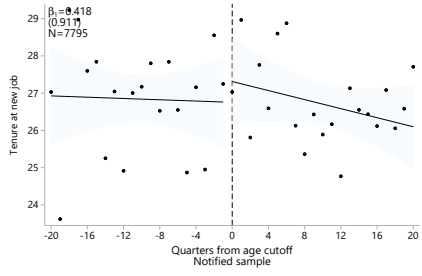
where $\ln w_{it}$ is the log wage of individual i at time t, $\alpha_{j(i)}$ is the worker fixed effect representing time-invariant unobserved human capital, and $\psi_{J(i,t)}$ is the firm fixed effect for the employer J. x_{it} controls for calendar time effects.

Figure D.1 . Quality of New Jobs

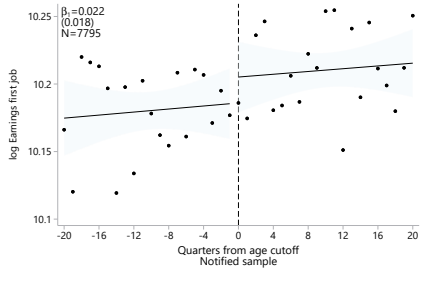
(a) AKM Firm Effects, First New Job



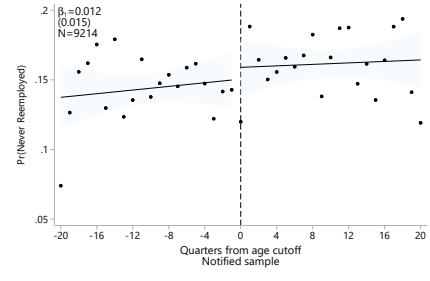
(b) Tenure at the New Job



(c) Log Earnings, First New Job



(d) Pr(No New Job)



Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects and covariates for higher education (dummy), number of children, and work experience. This figure examines quality of the first new job match following notification. Figure D.1 a reports the estimated firm fixed effects from an AKM Abowd, Kramarz, and Margolis (1999) decomposition for the new employer, Figure D.1 b measures tenure at the new job in months, Figure D.1 c shows log monthly earnings in the first new job, and Figure D.1 d shows the probability of not obtaining a new job.

The results from this exercise, along with several other outcomes, are presented in Figure D.1 . Figure D.1 a shows the firm effects associated with the new employer, Figure D.1 b worker tenure at the new job, and Figure D.1 c depicts log earnings at the new job.[33] Finally, Figure D.1 d shows that the probability of obtaining a new job is not affected by eligibility for supplemental UI; thus the results in Figure D.1 a-Figure D.1 c are not contaminated by differential employment rates.

Regardless of which measure of job quality we use, there is no evidence of an impact of increased UI. In particular, Figure D.1 a and Figure D.1 b show that the estimates are literally zero. The earnings impact is substantively larger, but not statistically significant. Our results thus align with previous studies finding that the effects of UI on job quality are small or insignificant (Nekoei and Weber, 2017).

E Validity & Placebo Checks

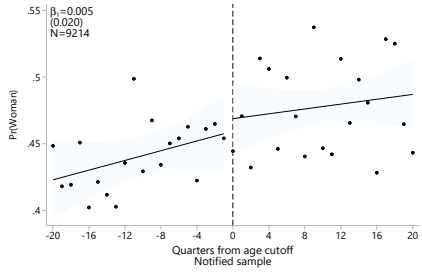
In this section, we present several standard checks to establish the robustness and validity of our design. Section E.1 includes RD figures for additional predetermined characteristics. Section E.2 presents results and validity tests using different kernels and optimal bandwidths following the approach of Calonico, Cattaneo, and Titiunik (2014). Section E.3 provides results excluding controls and fixed effects, while Section E.4 includes events and workplaces omitted from our main analysis. Finally, Section E.5 presents a placebo analysis conducted by varying the RD cutoff.

[33] We obtain very similar results if we use the difference in log earnings between the new and old jobs as the outcome.

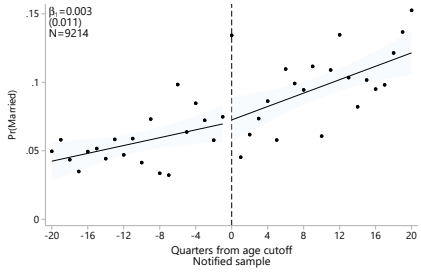
E.1 Validity & Placebo Checks

Figure E.1.1 . Validity RD Plots: Worker Characteristics

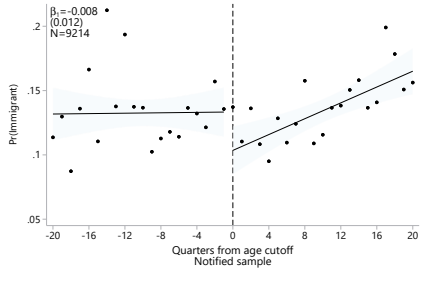
(a) Gender



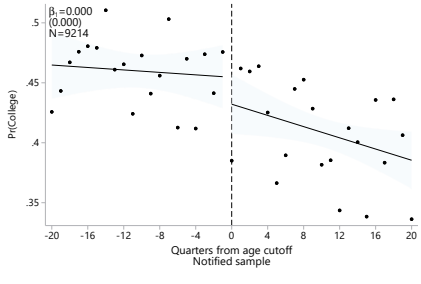
(b) Married



(c) Immigrant Status



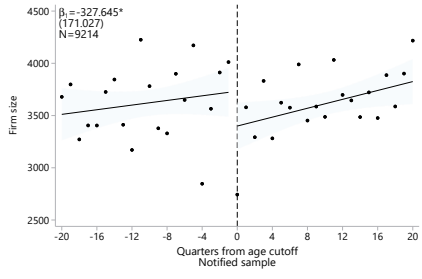
(d) Any College



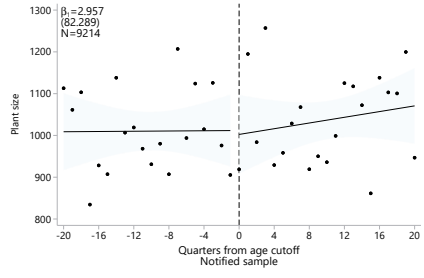
Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects. Figure E.1.1 a and Figure E.1.1 b show the RD plots for gender and married, respectively, while Figure E.1.1 c and Figure E.1.1 d presents the validity check for being foreign born and likelihood of college background, respectively.

Figure E.1.2 . Validity RD Plots: Event Characteristics

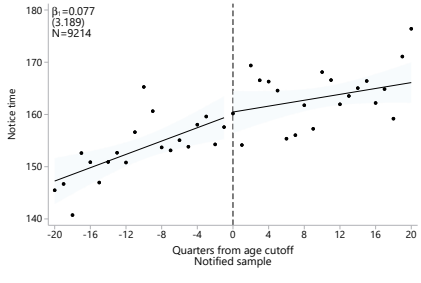
(a) Firm Size



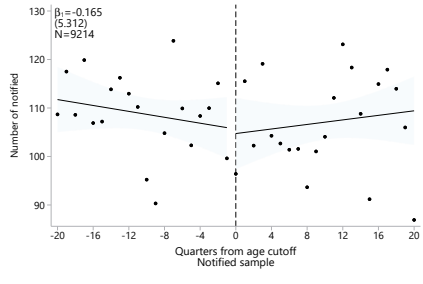
(b) Workplace Size



(c) Advance Notice



(d) Number of Notified



Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects. Figure E.1.2 a and Figure E.1.2 b show the RD plots for firm and workplace size, respectively, while Figure E.1.2 c and Figure E.1.2 d presents the validity check for advance notice and number of notified in the event, respectively.

E.2 Optimal Bandwidths & Different Kernels

Table E.2.1. *Notified Sample*

	Uniform			Triangular			Epanechnikov			Main Spec. (Uniform)		
	BW	Coeff	SE	BW	Coeff	SE	BW	Coeff	SE	BW	Coeff	SE
Pre-Determined												
Replacement rate	15	0.168***	(0.007)	18	0.164***	(0.007)	16	0.167***	(0.007)	20	0.162***	(0.007)
Tenure	14	-0.848	(2.232)	21	1.473	(1.771)	19	-0.305	(1.851)	20	0.897	(1.799)
Earnings t-1	18	-21.529	(605.121)	24	115.622	(499.176)	22	119.693	(521.362)	20	235.587	(577.419)
Notice time	16	2.281	(3.626)	19	0.729	(3.314)	18	1.724	(3.414)	20	0.476	(3.158)
Outcomes												
Pr(Unemployed 12m)	14	0.052**	(0.024)	20	0.056***	(0.019)	18	0.052**	(0.021)	20	0.056***	(0.019)
Days unemployed	14	9.284*	(5.379)	21	10.733**	(4.278)	19	12.002***	(4.480)	20	12.349***	(4.310)
Earnings first job	20	507.543	(899.867)	25	210.055	(726.195)	22	541.969	(792.019)	20	507.543	(899.867)
Δ log Earnings	17	0.050**	(0.021)	22	0.027	(0.018)	20	0.028	(0.019)	20	0.028	(0.019)
Controls	✓			✓			✓			✓		
FEs	✓			✓			✓			✓		

Notes: This table presents MSE-optimal bandwidths in quarters, RD estimates, and standard errors based on the specification in Equation (1). Bandwidths (h_{MSERD}) are calculated following Calonico, Cattaneo, and Titiunik, 2014 using the `rdbwselect` package in Stata. We vary the kernel type (Triangular, Epanechnikov, or Uniform), with bandwidths expressed in quarters relative to the 40-year age cutoff. All specifications include event-month × industry fixed effects and control for higher education, number of children, and work experience. Standard errors are clustered at the event level.

Table E.2.2. *Optimal Bandwidths & Different Kernels, Unemployed*

	Uniform			Triangular			Epanechnikov			Main Spec. (Uniform)		
	BW	Coeff	SE	BW	Coeff	SE	BW	Coeff	SE	BW	Coeff	SE
Pre-Determined												
Pr(Sup. UI first year)	20	0.474***	(0.026)	21	0.477***	(0.025)	20	0.474***	(0.026)	20	0.474***	(0.026)
Replacement rate	19	0.132***	(0.009)	22	0.130***	(0.008)	20	0.132***	(0.008)	20	0.132***	(0.008)
Tenure	16	2.234	(2.977)	22	4.162*	(2.523)	20	3.056	(2.617)	20	3.056	(2.617)
Earnings t-1	16	-1322.106*	(803.315)	23	-258.538	(649.318)	20	-810.876	(701.914)	20	-810.876	(701.914)
Outcomes												
Notice time	15	-0.308	(5.079)	20	-0.991	(4.287)	19	0.635	(4.441)	20	-0.991	(4.287)
Days unemployed	22	5.212	(6.030)	23	5.283	(5.888)	21	5.644	(6.247)	20	7.189	(6.333)
Earnings first job	19	-1367.417	(1562.645)	23	-511.524	(1403.130)	20	-740.807	(1611.747)	20	-740.807	(1611.747)
Δ log Earnings	18	0.049*	(0.030)	23	-0.001	(0.026)	20	0.016	(0.029)	20	0.016	(0.029)
Controls	✓			✓			✓			✓		
FEs	✓			✓			✓			✓		

Notes: This table presents MSE-optimal bandwidths in quarters, RD estimates, and standard errors based on the specification in Equation (1). Bandwidths (h_{MSERD}) are calculated following Calonico, Cattaneo, and Titiunik, 2014 using the `rdbwselect` package in Stata. We vary the kernel type (Triangular, Epanechnikov, or Uniform), with bandwidths expressed in quarters relative to the 40-year age cutoff. All specifications include event-month × industry fixed effects and control for higher education, number of children, and work experience. Standard errors are clustered at the event level.

E.3 Different Controls & Fixed Effects

Table E.3.3. *Balancing RD-Estimates*

	(1)		(2)		(3)		(4)	
Previous Job								
log Earnings	-0.007	(0.020)	-0.015	(0.017)	-0.009	(0.019)	-0.014	(0.016)
log Wage	-0.006	(0.019)	-0.005	(0.014)	0.003	(0.018)	0.000	(0.014)
Hours	0.105	(0.170)	0.003	(0.156)	0.138	(0.167)	0.049	(0.153)
Tenure	3.043	(2.012)	1.581	(1.861)	1.304	(1.931)	0.003	(1.797)
Firm size	-323.445	(267.567)	-333.221*	(170.401)	-303.256	(261.121)	-327.645*	(171.027)
Plant size	28.340	(124.594)	-9.427	(83.807)	45.421	(119.726)	2.957	(82.289)
Number of notified	8.268	(7.817)	-1.400	(5.310)	9.992	(7.800)	-0.165	(5.312)
Notice time	0.769	(3.492)	0.487	(3.183)	0.139	(3.510)	0.077	(3.189)
Worker Fixed Effects	0.008	(0.010)	0.007	(0.009)	0.009	(0.009)	0.009	(0.009)
Demographics								
Pr(Woman)	0.000	(0.021)	0.008	(0.020)	-0.002	(0.021)	0.005	(0.020)
Pr(College)	-0.023	(0.022)	-0.023	(0.019)	0.000	(0.000)	0.000	(0.000)
Pr(Immigrant)	-0.024	(0.016)	-0.028*	(0.016)	-0.003	(0.012)	-0.008	(0.012)
Pr(Married)	0.000	(0.011)	0.000	(0.011)	0.003	(0.011)	0.003	(0.011)
Number of kids	0.031	(0.058)	0.060	(0.060)	-0.000	(0.000)	-0.000	(0.000)
Pol. 1st Degree	✓		✓		✓		✓	
Covariates			✓				✓	
FEs					✓		✓	
Bandwidth ±	5		5		5		5	
Clusters	2302		2302		2301		2301	
N	9240		9240		9214		9214	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table shows RD estimates for each variable separately as detailed in Section 4 and specifically Equation (1). Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects. The tests follow Lee and Lemieux (2010).

Table E.3.4. *RD-Estimates On UI Variables*

	(1)	(2)	(3)	(4)
Pr(Sup. UI first year)	0.249***	0.252***	0.252***	0.255***
	(0.017)	(0.016)	(0.017)	(0.016)
Sup. UI first year	7963.421***	8095.224***	8113.782***	8248.304***
	(785.650)	(771.429)	(796.443)	(776.050)
Pr(General UI first year)	0.022	0.028	0.027	0.034*
	(0.020)	(0.019)	(0.020)	(0.019)
General UI first year	1675.951	2020.781	2379.166	2717.621
	(1951.366)	(1845.565)	(1911.626)	(1823.812)
Pr(Any UI first year)	0.070***	0.078***	0.075***	0.083***
	(0.021)	(0.019)	(0.020)	(0.019)
Total UI first year	9639.372***	10116.006***	10492.949***	10965.925***
	(2210.942)	(2089.645)	(2190.799)	(2085.623)
Pol. 1st Degree	✓	✓	✓	✓
Covariates		✓		✓
FEs			✓	✓
Bandwidth ±	5	5	5	5
Clusters	2302	2302	2301	2301
N	9240	9240	9214	9214

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table shows RD estimates for each variable separately as detailed in Section 4 and specifically Equation (1). Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects.

Table E.3.5. *RD-Estimates On UI Variables, Unemployed Sample*

	(1)	(2)	(3)	(4)
Pr(Sup. UI first year)	0.471***	0.469***	0.472***	0.471***
	(0.026)	(0.027)	(0.027)	(0.027)
Sup. UI first year	13846.159***	13624.259***	13960.156***	13855.548***
	(1161.603)	(1211.237)	(1165.742)	(1211.938)
Pr(General UI first year)	-0.020	-0.015	-0.011	-0.006
	(0.032)	(0.030)	(0.031)	(0.029)
General UI first year	-1340.037	-1609.568	18.984	-298.079
	(3694.762)	(3497.733)	(3584.339)	(3404.947)
Pr(Any UI first year)	0.029	0.037	0.035	0.044
	(0.032)	(0.029)	(0.031)	(0.029)
Total UI first year	12506.122***	12014.691***	13979.140***	13557.469***
	(4102.877)	(3917.548)	(4029.877)	(3856.150)
Pol. 1st Degree	✓	✓	✓	✓
Covariates		✓		✓
FEs			✓	✓
Bandwidth ±	5	5	5	5
Clusters	1600	1597	1599	1596
N	4219	4216	4210	4207

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table shows RD estimates for each variable separately as detailed in Section 4 and specifically Equation (1). Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects.

Table E.3.6. *RD-Estimates On Outcomes*

	(1)	(2)	(3)	(4)
Unemployment Outcomes				
Pr(New Job De Facto Notice)	-0.032	-0.039*	-0.036*	-0.042**
	(0.021)	(0.021)	(0.021)	(0.021)
Pr(Unemployed 12m after notice)	0.047**	0.051***	0.052**	0.056***
	(0.020)	(0.020)	(0.020)	(0.020)
Days unemployed	10.869**	11.239***	12.129***	12.391***
	(4.600)	(4.340)	(4.580)	(4.330)
Job Quality				
Earnings first job	624.092	420.945	859.119	559.632
	(905.456)	(922.714)	(855.542)	(911.560)
log Earnings first job	0.031	0.020	0.035*	0.022
	(0.021)	(0.018)	(0.019)	(0.018)
Δ log Earnings	0.029	0.028	0.034*	0.032
	(0.019)	(0.020)	(0.019)	(0.020)
New Earnings $\geq$ Previous	0.000	0.001	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Tenure at new job	0.669	0.634	0.470	0.418
	(0.906)	(0.901)	(0.909)	(0.911)
Pr(Changes industry)	-0.028	-0.023	-0.029	-0.023
	(0.021)	(0.020)	(0.021)	(0.020)
Pol. 1st Degree	✓	✓	✓	✓
Covariates		✓		✓
FEs			✓	✓
Bandwidth ±	5	5	5	5
Clusters	2302	2302	2301	2301
N	9240	9240	9214	9214

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table shows RD estimates for each variable separately as detailed in Section 4 and specifically Equation (1). Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects.

Table E.3.7. *RD-Estimates On Outcomes, Unemployed Sample*

	(1)	(2)	(3)	(4)
Unemployment Outcomes				
Pr(New Job De Facto Notice)	-0.050*	-0.057**	-0.054**	-0.061**
	(0.026)	(0.026)	(0.026)	(0.026)
Days unemployed	6.081	6.076	7.451	7.261
	(6.505)	(6.381)	(6.491)	(6.371)
Job Quality				
Earnings first job	-480.330	-904.469	-358.679	-818.796
	(1575.448)	(1663.462)	(1509.867)	(1637.416)
log Earnings first job	0.022	0.000	0.023	0.001
	(0.031)	(0.029)	(0.029)	(0.028)
Δ log Earnings	0.018	0.016	0.024	0.022
	(0.028)	(0.029)	(0.028)	(0.029)
New Earnings ≥ Previous	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Tenure at new job	0.051	0.296	-0.123	0.172
	(1.270)	(1.276)	(1.268)	(1.276)
Pr(Changes industry)	-0.014	-0.008	-0.017	-0.009
	(0.030)	(0.030)	(0.030)	(0.030)
Pol. 1st Degree	✓	✓	✓	✓
Covariates		✓		✓
FEs			✓	✓
Bandwidth ±	5	5	5	5
Clusters	1600	1597	1599	1596
N	4219	4216	4210	4207

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table shows RD estimates for each variable separately as detailed in Section 4 and specifically Equation (1). Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects.

E.4 Different Samples

Table E.4.8. *Balancing, Different Samples*

	(1)		(2)		(3)		(4)		(5)	
Previous Job										
log Earnings	-0.006	(0.011)	-0.005	(0.012)	-0.004	(0.012)	-0.007	(0.015)	-0.014	(0.016)
log Wage	0.004	(0.011)	0.004	(0.011)	0.007	(0.011)	0.000	(0.013)	0.000	(0.014)
Hours	0.127	(0.129)	0.130	(0.132)	0.183	(0.142)	0.020	(0.147)	0.049	(0.153)
Tenure	0.702	(1.311)	-0.032	(1.375)	-0.220	(1.429)	0.982	(1.662)	0.003	(1.797)
Firm size	-15.454	(113.932)	-66.381	(127.897)	-112.275	(126.205)	-261.349	(159.323)	-327.645*	(171.027)
Plant size	34.139	(45.239)	30.125	(50.726)	0.033	(51.451)	-0.244	(70.124)	2.957	(82.289)
Number of notified	3.317	(3.175)	3.770	(3.587)	2.529	(3.485)	-0.053	(4.768)	-0.165	(5.312)
Notice time	-1.709	(3.085)	-2.983	(3.499)	1.499	(2.458)	-1.004	(2.942)	0.077	(3.189)
Worker Fixed Effects	0.008	(0.006)	0.013**	(0.006)	0.012*	(0.007)	0.012	(0.008)	0.009	(0.009)
Demographics										
Pr(Woman)	-0.005	(0.014)	-0.014	(0.015)	-0.011	(0.016)	0.005	(0.018)	0.005	(0.020)
Pr(College)	0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)	-0.000	(0.000)
Pr(Immigrant)	0.005	(0.008)	0.003	(0.009)	-0.001	(0.010)	-0.005	(0.011)	-0.008	(0.012)
Pr(Married)	0.009	(0.008)	0.010	(0.008)	0.008	(0.009)	0.008	(0.010)	0.003	(0.011)
Number of kids	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)
Non-Bankruptcies			✓		✓		✓		✓	
Noticetime ≤ 1yr					✓		✓		✓	
Plant Size ≥ 50							✓		✓	
Event Size ≥ 10									✓	
Clusters	7101		6010		5872		3498		2301	
N	19209		16645		15326		11013		9214	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table shows RD estimates for each variable separately as detailed in Section 4 and specifically Equation (1). Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects. These tests follow Lee and Lemieux (2010).

Table E.4.9. *RD-Estimates On UI Variables, Different Samples*

	(1)	(2)	(3)	(4)	(5)
Pr(Sup. UI first year)	0.262***	0.245***	0.262***	0.264***	0.255***
	(0.011)	(0.012)	(0.012)	(0.015)	(0.016)
Sup. UI first year	8261.896***	7780.945***	8233.617***	8558.544***	8248.304***
	(502.736)	(551.796)	(555.483)	(696.558)	(776.050)
Pr(General UI first year)	0.006	0.016	0.016	0.018	0.034*
	(0.013)	(0.014)	(0.015)	(0.018)	(0.019)
General UI first year	652.016	1871.020	2070.471	1380.964	2717.621
	(1282.131)	(1364.711)	(1474.864)	(1705.150)	(1823.812)
Pr(Any UI first year)	0.079***	0.067***	0.069***	0.067***	0.083***
	(0.014)	(0.014)	(0.015)	(0.017)	(0.019)
Total UI first year	8913.912***	9651.965***	10304.088***	9939.508***	10965.925***
	(1462.715)	(1571.216)	(1680.172)	(1956.114)	(2085.623)
Non-Bankruptcies		✓	✓	✓	✓
Noticetime ≤ 1yr			✓	✓	✓
Plant Size ≥ 50				✓	✓
Event Size ≥ 10					✓
Clusters	7101	6010	5872	3498	2301
N	19209	16645	15326	11013	9214

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table shows RD estimates for each variable separately as detailed in Section 4 and specifically Equation (1). Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects.

Table E.4.10. *RD-Estimates On UI Variables, Different Samples (Unemployed)*

	(1)	(2)	(3)	(4)	(5)
Pr(Sup. UI first year)	0.447***	0.460***	0.471***	0.477***	0.471***
	(0.017)	(0.020)	(0.019)	(0.023)	(0.027)
Sup. UI first year	13050.406***	13277.860***	13681.535***	14193.168***	13855.548***
	(749.852)	(900.647)	(893.887)	(1107.618)	(1211.938)
Pr(General UI first year)	-0.011	0.001	-0.004	-0.009	-0.006
	(0.020)	(0.022)	(0.022)	(0.026)	(0.029)
General UI first year	-760.337	1334.813	1225.277	-1233.791	-298.079
	(2227.397)	(2576.732)	(2651.095)	(3093.900)	(3404.947)
Pr(Any UI first year)	0.077***	0.046**	0.043**	0.037	0.044
	(0.020)	(0.021)	(0.021)	(0.026)	(0.029)
Total UI first year	12290.069***	14612.673***	14906.812***	12959.377***	13557.469***
	(2496.692)	(2909.066)	(2982.947)	(3524.574)	(3856.150)
Non-Bankruptcies		✓	✓	✓	✓
Noticetime ≤ 1yr			✓	✓	✓
Plant Size ≥ 50				✓	✓
Event Size ≥ 10					✓
Clusters	4707	3740	3706	2316	1596
N	9512	7401	7173	5118	4207

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table shows RD estimates for each variable separately as detailed in Section 4 and specifically Equation (1). Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects.

Table E.4.11. *RD-Estimates On Outcomes, Different Samples*

	(1)	(2)	(3)	(4)	(5)
Unemployment Outcomes					
Pr(New Job De Facto Notice Period)	-0.018	-0.019	-0.014	-0.027	-0.042**
	(0.014)	(0.015)	(0.016)	(0.019)	(0.021)
Pr(Unemployed 12m after notice)	0.022	0.031**	0.033**	0.035*	0.056***
	(0.014)	(0.015)	(0.016)	(0.018)	(0.020)
Days unemployed	4.904	8.043**	9.058**	8.621**	12.391***
	(3.267)	(3.323)	(3.537)	(4.070)	(4.330)
Job Quality					
Earnings first job	-489.792	-465.677	-472.231	160.826	559.632
	(556.609)	(594.548)	(631.424)	(795.666)	(911.560)
log Earnings first job	0.003	0.001	0.001	0.017	0.022
	(0.013)	(0.014)	(0.014)	(0.017)	(0.018)
Δ log Earnings	0.010	0.009	0.005	0.020	0.032
	(0.013)	(0.014)	(0.015)	(0.018)	(0.020)
New Earnings ≥ Previous	0.000	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Tenure at new job	0.990	0.476	0.527	0.506	0.101
	(0.607)	(0.650)	(0.675)	(0.788)	(0.866)
Pr(Changes industry)	0.001	-0.015	-0.022	-0.034*	-0.023
	(0.016)	(0.017)	(0.017)	(0.019)	(0.020)
Non-Bankruptcies		✓	✓	✓	✓
Noticetime ≤ 1yr			✓	✓	✓
Plant Size ≥ 50				✓	✓
Event Size ≥ 10					✓
Clusters	7101	6010	5872	3498	2301
N	19209	16645	15326	11013	9214

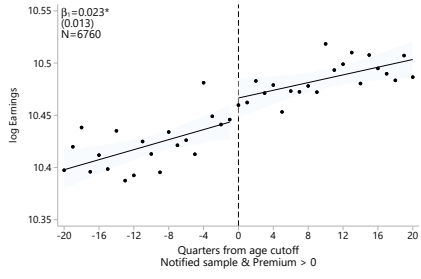
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

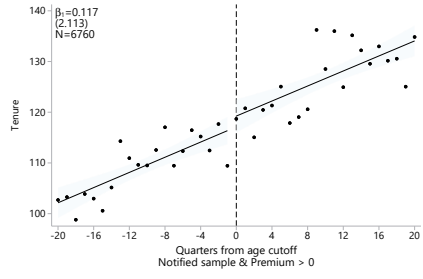
Notes: The table shows RD estimates for each variable separately as detailed in Section 4 and specifically Equation (1). Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects.

Figure E.4.3 . Validity Tests: Excluding Low Earners

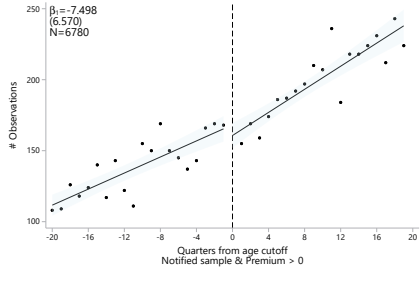
(a) log Earnings in year $t-1$



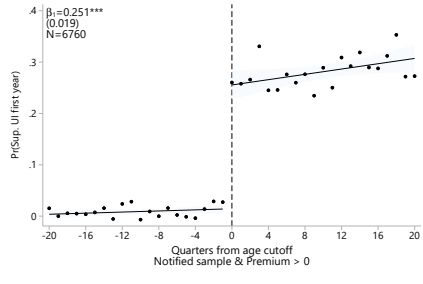
(b) Tenure at Notification



(c) Density of Observations



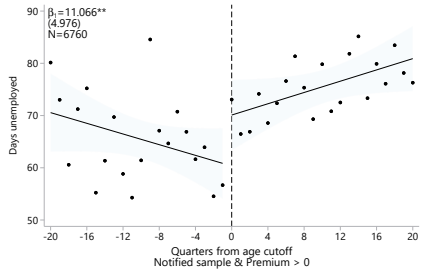
(d) Pr(Any Supplemental Benefits)



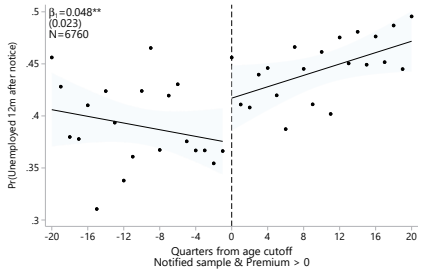
Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects and covariates for higher education (dummy), number of children, and work experience. Figures E.4.3 a to E.4.3 d show balance tests and treatment intensity for the unemployed sample excluding workers with no perceived benefit of supplemental benefits.

Figure E.4.4 . Outcomes: Excluding Low Earners

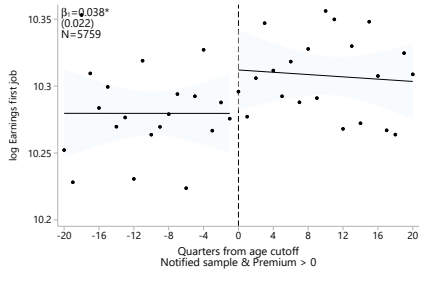
(a) Unemployment Duration (12m)



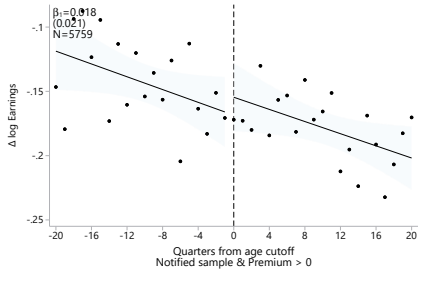
(b) Pr(Unemployed) (12m)



(c) log New Earnings



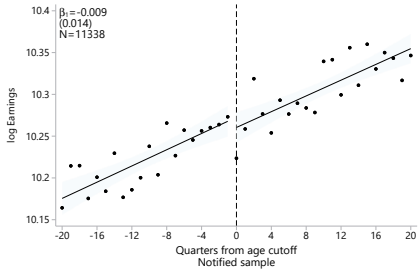
(d) Change in log Earnings



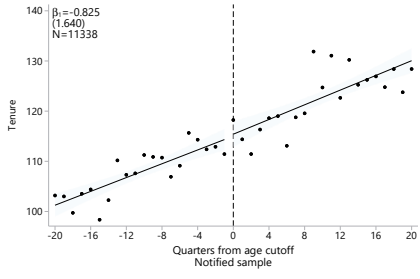
Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects and covariates for higher education (dummy), number of children, and work experience. Figures E.4.4 a to E.4.4 d show the outcomes for the notified sample excluding workers with without estimated benefit of supplemental benefits

Figure E.4.5 . Validity Tests: Weighting Using CBA Probability

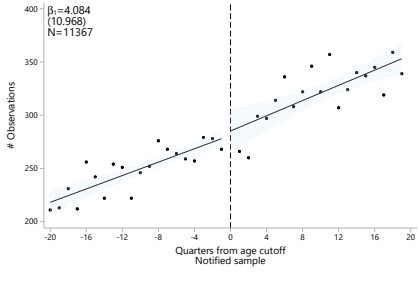
(a) log Earnings in year $t-1$



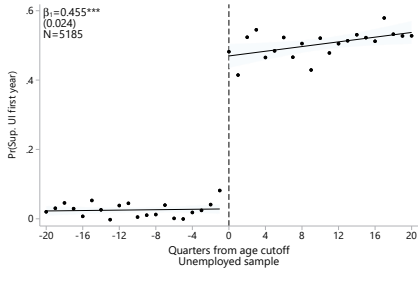
(b) Tenure at Notification



(c) Density of Observations



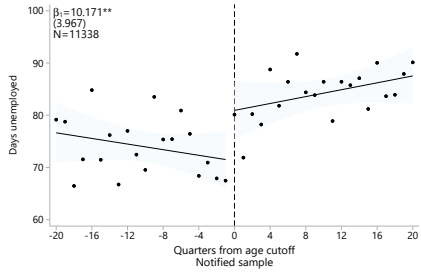
(d) Pr(Any Supplemental Benefits)



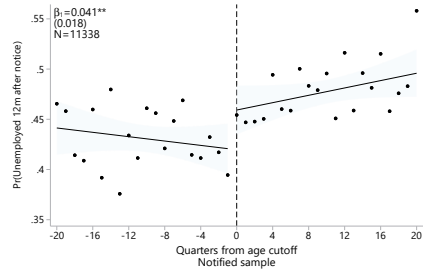
Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects and covariates for higher education (dummy), number of children, and work experience. Figures E.4.5 a to E.4.5 c show balance tests for the notified sample and Figure E.4.5 d treatment intensity for the unemployed sample weighting observations using the probability of the establishment being covered by a CBA using Figure B.1 a.

Figure E.4.6 . Outcomes: Weighting Using CBA Probability

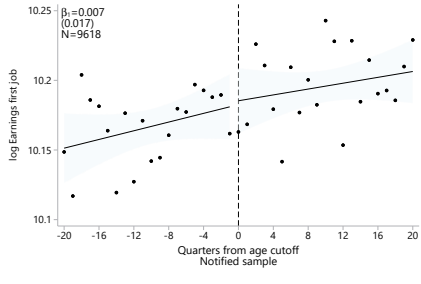
(a) Unemployment Duration (12m)



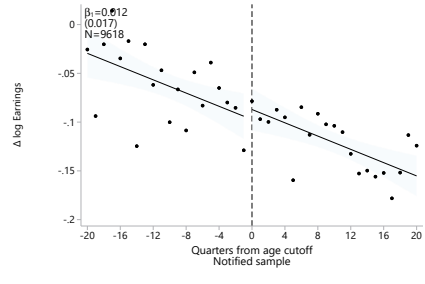
(b) Pr(Unemployed) (12m)



(c) log New Earnings



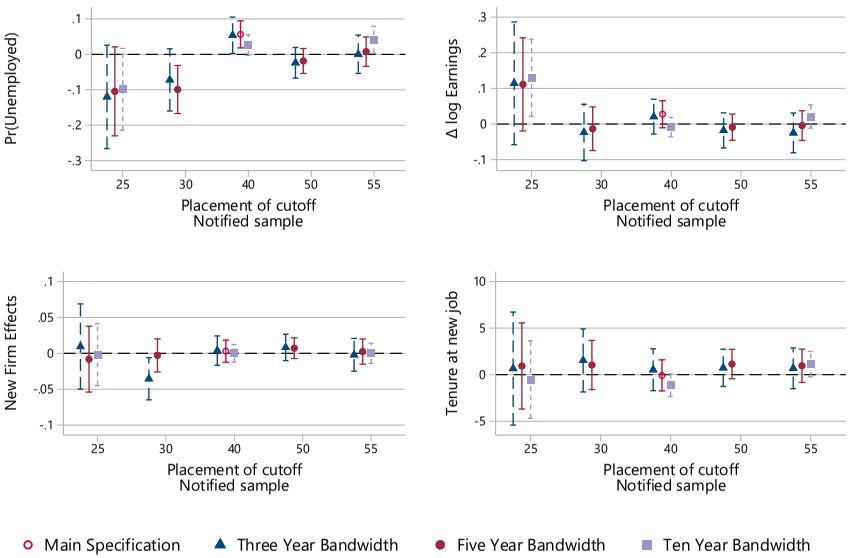
(d) Change in log Earnings



Notes: The figure shows the RD estimates using the specification detailed in Section 4, with age at the last day of work as the running variable on the x-axis. Age is normalized relative to 40 years, highlighted by the dashed vertical line. Data are binned at the quarterly level. The lines are estimated using a linear specification, separately on each side of the age cutoff. The light blue area represents 95% confidence intervals. β_1 refers to the coefficient from Equation (1) and captures the causal effect of being eligible for supplemental UI. The stars represent statistical significance at the 10%, 5%, and 1% levels, respectively. The number in brackets is the standard error, and N is the number of observations. Standard errors are clustered at the event level. All specifications include event month × industry fixed effects and covariates for higher education (dummy), number of children, and work experience. Figures E.4.6 a to E.4.6 d show outcomes for the notified sample weighting observations using the probability of the establishment being covered by a CBA using Figure B.1 a.

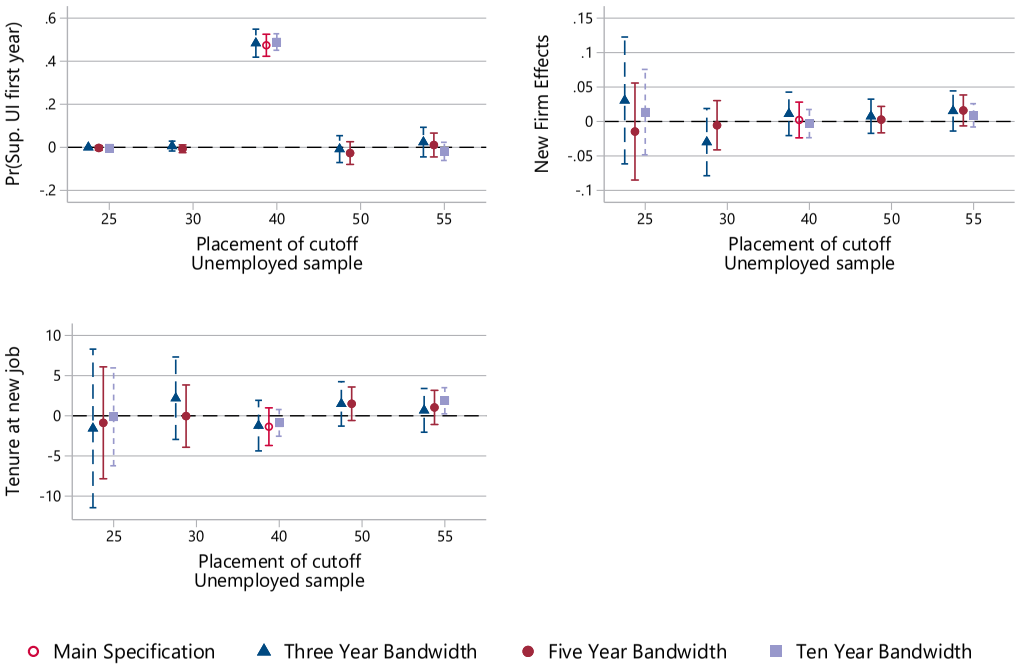
E.5 Placebo Tests

Figure E.5.7 . Placebo Tests (I)



Notes: The figure shows multiple RD estimates obtained using the specification described in Section 4, specifically Equation (1). For each cutoff, we use three different bandwidths, 3 (blue triangles), 5 (red dots) and 10 years (lavender squares). The estimate represented by a hollow dot is the main specification. Confidence intervals are at the 95% level. Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects.

Figure E.5.8 . Placebo Tests (II)



Notes: The figure shows multiple RD estimates obtained using the specification described in Section 4, specifically Equation (1). For each cutoff, we use three different bandwidths, 3 (blue triangles), 5 (red dots) and 10 years (lavender squares). The estimate represented by a hollow dot is the main specification. Confidence intervals are at the 95% level. Standard errors are clustered on the event level. The covariates are a dummy for any higher education, number of kids, and experience. Event month × industry fixed effects.

F Conceptual Framework

Following Schmieder and von Wachter (2016), we set up a stylized partial equilibrium search model having the main institutional features in our context. We consider a worker who becomes notified at time $t = 0$ that will be laid off in N periods. If the workers becomes unemployed, (s)he is eligible for UI and receives total compensation of $bw + a$ for P periods, where b denotes the UI replacement rate and a alternative income sources. Upon UI expiration, the worker receives a indefinitely. Receiving UI requires claiming the benefit; a claimant incurs a fixed cost C. Time is continuous; to simplify we ignore discounting and the acceptance decision.

F.1 Selection into Unemployment

Workers are heterogeneous, as indexed by α. They find alternative employment paying $w(\alpha)$ at rate $s_t\alpha$, where s_t denotes endogenous search effort. We assume that there is a constant exit rate (δ), which is independent of state. The survival probability is then given by

$$S_t(\alpha) = \exp[-\delta t - \alpha \int_0^t s_\tau(\alpha) d\tau] \tag{i}$$

Search effort commands the cost $\psi(s_t)$, which we assume is differentiable, increasing, and convex in s_t. We denote the utility flow while employed by $v(c_e)$ and the utility flow while on notice or unemployed by $v(c_t)$ (which varies over time, only to the extent that the compensation flows vary). Workers are assumed to have infinite horizons. The asset values associated with each state are then given by

$$\delta V_t^n - \dot{V^n} = \max_s [v(c^n) - \psi(s_t^n) + s_t^n \alpha (V^e - V_t^n)] \tag{ii}$$

$$\delta V_t^b - \dot{V^b} = \max_s [v(c^b) - \psi(s_t^b) + s_t^b \alpha (V^e - V_t^b)] \tag{iii}$$

$$\delta V_t^a = \max_s [v(c^a) - \psi(s^a) + s^a \alpha (V^e - V^a)] \tag{iv}$$

where $V^e = v(c^e)/\delta$. The asset values on notification and UI decline over time because of finite duration. The asset value after UI has expired does not vary over time because of infinite horizons. Given that we abstract from assets, the only choice variable for workers is search effort. Optimal search effort satisfies

$$\psi'(s_t^j) = \alpha (V^e - V_t^j) \tag{v}$$

where $j = n, b, a$. Given our assumptions, it follows that $s_t^a = s^a$ (a fact already embodied in Equation (iv)). Moreover

$$\dot{s_t^n} > 0\,, \quad \dot{s_t^b} > 0\,, \quad \lim_{t \to N} s_t^n \to s_N^b\,, \quad \lim_{t \to (N+P)} s_t^b \to s^a \tag{vi}$$

Search effort thus increases over time, and $s_t^n \leq s_t^b \leq s^a$.

An unemployed worker with better prospects on the market (as indexed by α) will search harder. This is easiest to see for s^a. From Equation (v) it follows that

$$\frac{\partial s^a}{\partial \alpha} = \frac{1}{\psi''} \frac{\delta}{\delta + s^a \alpha} [V^e - V^a] > 0$$

Equation (vi) then implies that the entire search path shift upwards, and a worker with better prospects searches harder at every point in time (for given b).

Now, consider an increase in UI benefits. This has the standard implication that search effort is reduced while on UI and on notice. s^a is unaffected, however, because we have assumed that there is no "entitlement effect". Close to UI expiration, a change in UI benefits will have limited effects, while the impact will be maximal around the time when the notification period expires.

Of particular interest to us is how eligibility for UI affects selection into unemployment and UI take-up. The expected value of being eligible at the point of unemployment entry is given by

$$B_N(\alpha) = \int_N^{N+P} (V_t^b(\alpha) - V^a(\alpha))dt = \int_N^{N+P} S_t(\alpha)[\Delta\nu - \Delta\psi_t]dt \quad \text{(vii)}$$

where $S_t(\alpha)$ is defined in Equation (i), $\Delta\nu \equiv [\nu(c^b(\alpha)) - (\nu(c^a)]$, $\Delta\psi_t \equiv [\psi(s_t^b) - \psi(s^a)]$, and so $(\Delta\nu - \Delta\psi_t) > 0$ is the direct utility gain of being insured; we write $c^b(\alpha)$ since supplemental UI provides a benefit which is proportional to prior income. Since $\dot{V}^b < 0$, an eligible worker will claim benefits if and only if $B_N(\alpha) > C$.

We are interested in the skill gradient in claimant behavior, i.e., $\partial B_N / \partial \alpha$. Notice that we do not have to take the indirect effects via search effort into account because of the Envelope theorem. We have

$$\frac{\partial B_N}{\partial \alpha} = \underbrace{D^b \nu'(c^b) b w'(\alpha)}_{(+)} + \underbrace{\int_N^{N+P} \frac{\partial S_t(\alpha)}{\partial \alpha} [\Delta\nu - \Delta\psi_t]dt}_{(-)} \quad \text{(viii)}$$

where $D^b(\alpha) = \int_N^{N+P} S_t(\alpha)dt$ is the expected duration of benefit receipt. In general, the skill gradient is ambiguous in sign. The insurance component of the system implies that high-skilled workers receive marginally higher payments throughout the expected duration of benefit receipt—this is the first component—but value the direct utility gain of receiving UI less because they expect to stay unemployed for a shorter period of time—this is the second component.

If workers are paid their marginal productivity ($w = \alpha$): $b w'(\alpha)\alpha = c^b - c^a \equiv \Delta c$. We can then rewrite Equation (viii) slightly

$$\frac{\partial B_N}{\partial \alpha}\alpha = D^b(\alpha) b w \left\{ \nu'(c^b) + \frac{\Delta\nu}{\Delta c} \frac{\partial D^b}{\partial \alpha} \frac{\alpha}{D^b} \right\} - \int_N^{N+P} \frac{\partial S_t(\alpha)}{\partial \alpha} \alpha \Delta\psi_t dt \quad \text{(ix)}$$

The third component in Equation (ix) is negative since $\partial S_t/\partial\alpha < 0$ and $\Delta\psi_t < 0$. Because $\Delta\nu/\Delta c = 0.5[\nu'(c^b) + \nu'(c^a)]$, we have

$$\frac{\partial D^b}{\partial\alpha}\frac{\alpha}{D^b} \leq -\frac{\nu'(c^b)}{0.5[\nu'(c^b) + \nu'(c^a)]} \Rightarrow \frac{\partial B_N(\alpha)}{\partial\alpha} < 0 \tag{x}$$

and then take-up will be lower among the high-skilled, even though their monetary gain from supplemental UI is higher than among the low-skilled.

In general, however, the skill gradient might well have different slopes along the skill distribution. As shown in Figure 9 the duration elasticity grows in absolute value along the prior income distribution, and in a slight extension of the model—also including assets—the marginal utilities of consumption across eligibility/non-eligibility would as well. In numerical simulation, we examine whether this framework can produce the inverted u-shape illustrated in Figure 8b.

For a worker who has just been notified, the expected present value of future eligibility is given by

$$B_0(\alpha) = S_N(\alpha)B_N(\alpha) \tag{xi}$$

which is simply the probability of entering unemployment—incidence—times the expected gain associated with eligibility. Essentially, individuals are discounting the value of future eligibility by $S_N(\alpha)$.

We are interested in how $B_0(\alpha)$ varies along the skill distribution; again, endogenous search responses are irrelevant for $B_0(\alpha)$ because search effort is optimally chosen. We have

$$\frac{\partial B_0}{\partial\alpha} = \frac{S_N}{\alpha}\left\{\frac{\partial S_N}{\partial\alpha}\frac{\alpha}{S_N}B_N + \frac{\partial B_N}{\partial\alpha}\alpha\right\} \tag{xii}$$

where the second term within brackets is given in Equation (ix) and

$$\frac{\partial S_N}{\partial\alpha}\frac{\alpha}{S_N} = -h^e(\alpha) < 0 \tag{xiii}$$

where $h^e(\alpha)$ the re-employment hazard cumulated over the notice period. While Equation (xii) is in principle ambiguous in sign, it is clear that more-skilled workers value any given future gain associated with UI eligibility less because they expect to find a job during the notice period to a greater extent. This implies that it is unlikely that UI eligibility will affect unemployment incidence among high-skilled individuals. This reasoning ties in with the evidence reported in Figure 8a. At the bottom of the distribution, we find no impact on unemployment incidence, which we attribute to $B_N(\alpha)$ being small. At the top-end of the distribution, there is again no impact of being entitled to UI, but the reason here is primarily that they expect to find a job during the notice period (i.e., S_N is low)

Finally, let us turn to the impact of the benefit entitlement on search behavior and unemployment incidence. Optimal search behavior implies

$S_N = S_N(B_N)$. We have:

$$\eta^B \equiv \frac{\partial S_N}{\partial B_N}\frac{B_N}{S_N} = \alpha \int_0^N \epsilon^B s_\tau d\tau > 0 \tag{xiv}$$

where we have defined $\epsilon^B \equiv -(\partial s_t/\partial B_N)(B_N/s_t) > 0$ as the elasticity of search effort with respect to the value of the benefit entitlement; the sign follows from the standard result that search effort decreases with UI generosity. Workers who are eligible for supplemental UI thus become unemployed to a greater extent.

We are interested in how an extension in the notification period affects the incidence margin; this relates to the evidence shown in Figure C.1 . To simplify the analysis we will assume that ϵ^B is constant over the notification spell. We have

$$\frac{\partial \eta^B}{\partial N} = \epsilon^B \left\{ h_N^e - \epsilon^N \frac{h^e}{N} \right\} \tag{xv}$$

where $\epsilon^N \equiv -(\partial s_t/\partial N)(N/s_t) > 0$ denotes the absolute value of the elasticity of search with respect to N (assumed to be constant over the notice period); the sign of this elasticity comes from the fact that search effort falls with extensions in N. $h_N^e(\alpha)$ is the re-employment hazard at the point where the notice period expires. Equation (xv) is ambiguous in sign. If $\epsilon^N < 1$, then Equation (xv), is positive, since $Nh_N/h^e > 1$ (because search effort increases over the notice period).

F.2 Numerical Analysis

Here we present the results of a numerical analysis of the model presented above. We take $w = \alpha$ and thus assume that α is log-normally distributed with mean 10.42 and standard deviation 0.42, which corresponds to the wage distribution we observe in our analysis sample. We begin by taking a month to be the basic time unit and set $\delta = 0.0025$ (which corresponds to annual "discount rate" of 3 %). Then, we assign the policy parameters $N = 5$ (notification horizon), $b = 0.2$ (supplemental UI), $P = 6$ (supplemental UI duration). We model non-labor income in the absence of the supplemental benefit according to the regular UI system such that $E[a] = 0.5$. Finally, we let 50 % of the population be treated and the remaining workers are controls.

We assume that the utility function is logarithmic in consumption (in all states) and that the search cost function is

$$\psi(s) = \kappa \frac{s^{1+\gamma}}{1+\gamma} \tag{xvi}$$

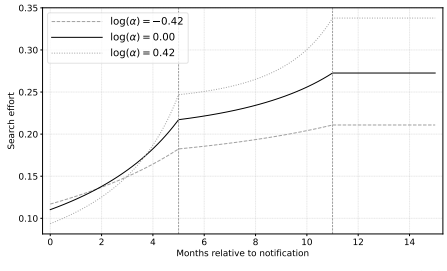
and calibrate the parameters κ, γ such that the model is able to replicate unemployment incidence in the control group $S_N^C = 0.42$ and unemployment

incidence in the treated group $S_N^T = 0.42 + 0.056 = 0.476$; notice that we do not target any distributional moment directly.[34]

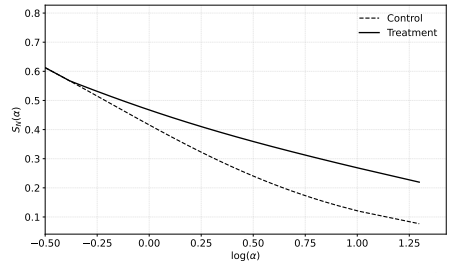
Assuming that take-up is complete among the treated, the model yields the implications illustrated by Figure F.2.1 . The top-left figure shows that search effort is increasing from notification and onward. Workers with better employment prospects, search harder when notification is close to expire, and continue to do so throughout the unemployment spell. The bottom left figure shows that the expected present value of UI is hump-shaped over the distribution of employment prospects because "high α" workers expect to find a job faster; and the bottom-right shows the expected present value discounted back to the point of notification using the survival function. This value is relevant for search behavior at notification, and, relative to the bottom-left figure, the value of future UI becomes even more muted towards the top-end of the distribution.

Figure F.2.1 . Results from Simulation

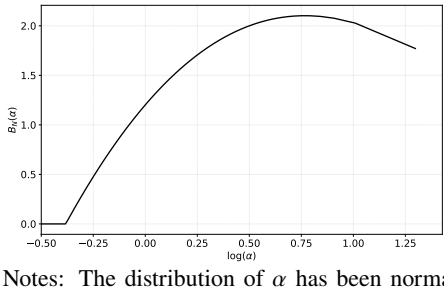
(a) Search effort paths



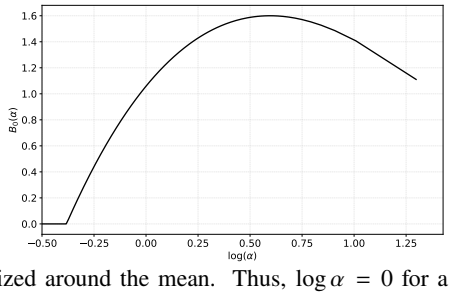
(b) Survival in notification



(c) Expected PV of UI at unemp. entry, $B_N(\alpha)$



(d) Expected PV of UI at notification, $B_0(\alpha)$



Notes: The distribution of α has been normalized around the mean. Thus, $\log \alpha = 0$ for a mean/median worker and, e.g., $\log \alpha = 0.42$ for a worker who is 1 SD above the mean.

We, finally, introduce take-up costs in the simplest possible manner. We begin by assuming that take-up costs become known at the point where workers enter unemployment, and that they are idiosyncratic (and uniform) across the distribution with mean 0. The support of the claiming cost distribution—$\varepsilon \in [-C, C]$ is set such that we target average take-up (46%) in our data. This yields $C = 1.01$. With this set-up, take-up will have the same shape as the bottom-left graph in Figure F.2.1 .

[34]The calibrated parameter values are $\kappa = 29.08$ and $\gamma = 0.54$.

Figure F.2.2 . Treatment effects: take-up costs known at $t = N$

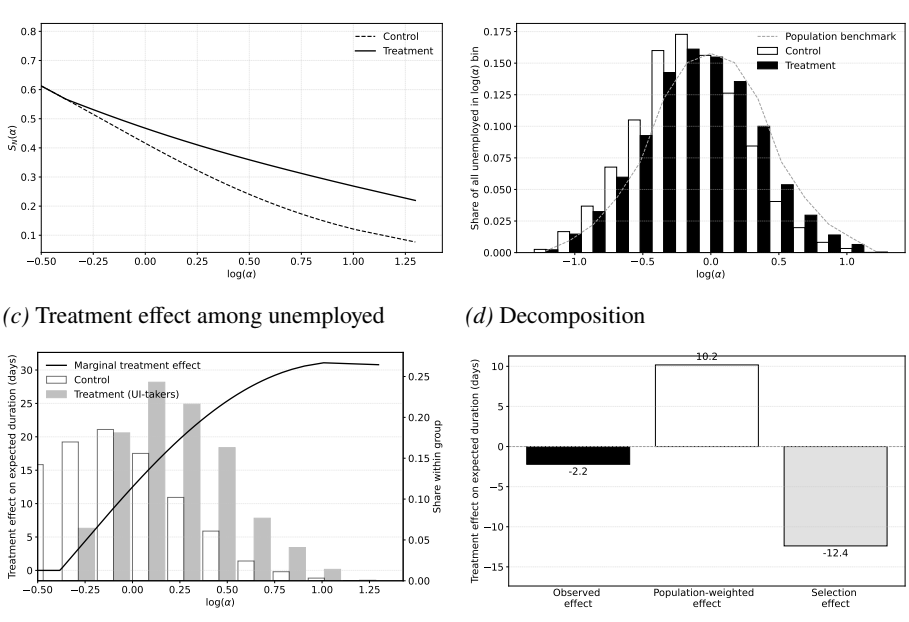


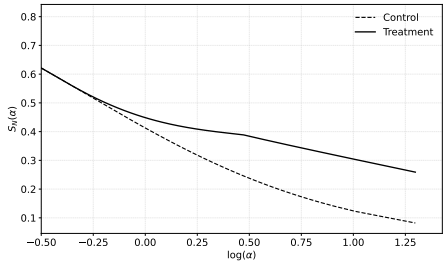
Figure F.2.2 shows a selection of results which are relevant for determining the treatment effects on the unemployed. In particular, the bottom right figure pertains to the bias of the estimate on unemployment duration. The population-weighted average effect (white bar) corresponds to the reduced-form effect of being eligible for supplemental UI among the population that become unemployed. The estimate we would get by just comparing average duration among the treated with the average duration among controls—the "observed effect" (black bar)—is actually negative suggesting a severe downward bias relative to the target parameter for the unemployed.

Figure F.2.3 shows the results from a similar exercise, but in this case we assume that take-up costs are known at the time of notification. In this version of the model, which we recalibrate to fit the targeted moments, we obtain results that are similar to the baseline scenario.[35] The reduced form duration estimate is still downward biased relative to the target, however, as illustrated by in panel d).

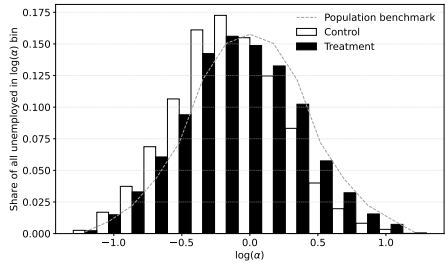
[35]The re-calibrated parameter values are $\kappa = 31.12$, $\gamma = 0.21$ and $C = 2.03$.

Figure F.2.3 . Treatment effects: take-up costs known at $t = 0$

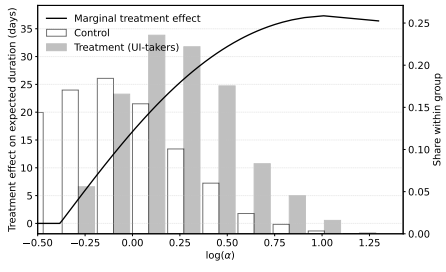
(a) Survival



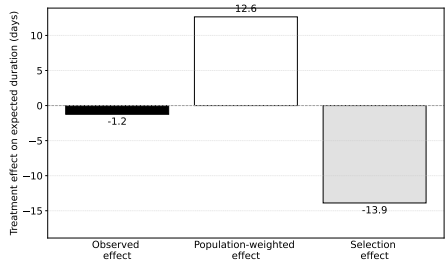
(b) Worker α-composition



(c) Treatment effect among unemployed



(d) Decomposition



F.3 Normative Analysis

Suppose the government determines the UI replacement rate by maximizing aggregate welfare V among all workers subject to the UI budget constraint

$$\tau(1-u) = bu^b + au \tag{xvii}$$

where u denotes overall unemployment and u^b the share receiving UI. In this specification, we have ignored other government expenses. While recognizing that this is not an innocuous assumption—since the size of the fiscal externality depends on the tax rate (τ)—our major point here is to illustrate what happens to the fiscal externality when we do not include the incidence effect. Note, however, that the budget constraint does take into consideration that there is alternative income support (a) available for workers who are not eligible for UI.

Standard analysis yields:

$$\frac{dV}{db}\frac{1}{u^b v'(c^e)} = \frac{v'(c^b) - v'(c^e)}{v'(c^e)} - \left\{ \frac{\partial u^b}{\partial b}\frac{b}{u^b} + \frac{\tau + a}{b}\frac{u}{u^b}\frac{\partial u}{\partial b}\frac{b}{u} \right\} \tag{xviii}$$

The first term is the marginal social value of transferring an additional \$ to the insured. The second term within curly brackets is the fiscal externality, the behavioral cost of transferring an additional \$ to the insured—or the BC/MC-ratio in the terminology of Schmieder and von Wachter (2016). Equation (xviii) is a standard Bailly-Chetty condition generalized to our setting.

Since, $u^b = S_N \times D^b$ and $u = S_N \times D$, where D denotes expected duration of unemployment, we have

$$\frac{BC}{MC} = (\eta^I + \eta^{D^b})(1 + \frac{\tau + a}{b}) + \frac{\tau + a}{b}\frac{D - D^b}{D^b}\eta^I \tag{xix}$$

where η^I and η^{D^b} denote the elasticity of incidence and the duration of benefit receipt with respect to b, respectively.

We have estimated $\eta^I = 0.65$ and the elasticity of overall unemployment duration with respect to b, $\epsilon^D = 0.20$. In the data, $D = 170$, $D^b = 90$, $b = 0.21$, and $a = 0.49$. Moreover, $\eta^{D^b} = \frac{D}{D^b}\epsilon^D = 0.38$. With an overall unemployment rate of 8%, the tax rate required to fund the UI budget is $\tau = 0.052$.

$$\frac{BC}{MC} = (0.65{+}0.38)\left[1 + \frac{0.052 + 0.49}{0.21}\right] + \left[\frac{0.052 + 0.49}{0.21}\right]\left[\frac{170 - 90}{90}\right]0.65 = 5.17$$

If we shut down the incidence margin, on the other hand, we get

$$\left[\frac{BC}{MC}\right]_{I=\bar{I}} = 0.38\left[1 + \frac{0.052 + 0.49}{0.21}\right] = 1.29$$

Adjusting for the impact on incidence thus matter substantially. Doing so, makes the fiscal externality four times larger ($5.17/1.29 \approx 4$).

F.4 Externalities due to the CBA

So far we have used supplemental UI as a way of identifying the impact of UI policies. Supplemental UI is, however, determined by the bargaining parties involved in the CBA. And the (direct) additional cost of supplemental UI is funded by the bargaining parties. Their behavior, however, generates fiscal externalities onto the public budget which are not accounted for when setting the generosity of supplemental UI. Here we shed light on the magnitude of this distortion.

To focus on the main issues, suppose that supplemental UI is determined in a large number of (symmetric) bargaining agreements. The bargaining parties act atomistically and set b subject to the CBA budget constraint

$$\tau_{CBA} e_{CBA} = b u^b_{CBA} \tag{xx}$$

A tax rate (τ_{CBA}) is thus levied on workers employed in the CBA (e_{CBA}) to fund supplemental UI. Optimal UI for the CBA satisfies

$$\frac{v'(c^b) - v'(c^e)}{v'(c^e)} = \left[1 + \frac{\tau_{CBA}}{b}\right]\left[\eta^I + \eta^{D^b}\right] \tag{xxi}$$

The right-hand side is the BC/MC-ratio relevant for the CBA. The bargaining parties thus internalize part of the fiscal externality. But they ignore the fact that the government pays for regular UI (a) and that the government also funds unemployed workers beyond supplemental UI expiration. The CBA tax rate required to fund supplemental UI is $\tau_{CBA} = 0.01$. Evaluated at the same parameters as above, we thus have

$$\left[\frac{BC}{MC}\right]_{CBA} = (0.65 + 0.38)\left[1 + \frac{0.01}{0.21}\right] = 1.08$$

We can also derive a formal expression for the fiscal externality that the CBA imposes on the central government:

$$\underbrace{\left[\frac{BC}{MC}\right] - \left[\frac{BC}{MC}\right]_{CBA}}_{\text{(5.17-1.08=4.09)}} = \underbrace{\frac{\tau - \tau_{CBA}}{b}(\eta^I + \eta^{D^b})}_{\text{=0.206 (5\%)}} + \underbrace{\frac{a}{b}\frac{D}{D^b}(\eta^I + \epsilon^D)}_{\text{=3.746 (91.5\%)}} + \underbrace{\frac{\tau}{b}\frac{D - D^b}{D^b}\eta^I}_{\text{=0.143 (3.5\%)}} \tag{xxii}$$

Evaluated at the same parameters as above, we find that 91.5% of the externality imposed on the central government comes from the second component above, i.e., that the bargaining parties ignore the fact that the government pays for regular UI.

III. Seniority Rules and Selection into Job Loss

Acknowledgements: I want to thank my advisors Peter Fredriksson and Jonas Cederlöf for valuable comments. In addition, Dogan Gülümser, Henrik Hällerfors, Pawel Krolikowski, Oskar Nordström Skans, Jan Sauermann, Johan Vikström, and the participants at the Uppsala Labor Group seminar have provided feedback. Funding from FORTE is gratefully acknowledged.

1 Introduction

A vast literature documents a large variety of labor market consequences of employment protection legislation (EPL), both on worker reallocation and effects on incumbents (Lazear, 1990; Hopenhayn and Rogerson, 1993; Pissarides, 2001; Bentolila, Dolado, and Jimeno, 2019). A prevalent example of EPL is seniority rules, which mandate the order of layoffs.[1] While these rules are typically justified as protecting older workers—who face higher re-skilling costs (Belot, Boone, and Van Ours, 2007) or potential age-related productivity declines (Lindbeck, 1994)—they also restrict an employer's discretion over displacement and retention. Consequently, seniority rules introduce a potential trade-off between distributional equity and firm efficiency.

Despite these implications, there is very limited evidence if and how firms navigate around such regulatory constraints. This is important both from an efficiency point of view and because it has implications for which workers are most at risk of job loss. There is a body of literature suggesting that layoff risk is systematically related to productive characteristics.[2] Figure 1 provides an example by illustrating a strong negative association between within-occupation wage levels and the likelihood of becoming displaced. Yet, how these patterns are associated with EPL remains poorly understood.

I address this gap studying Sweden, where there is a legally mandated Last-In-First-Out (LIFO) rule stipulating that less-tenured workers should be displaced before senior workers (SFS, 1982).[3] Sweden serves as an ideal setting for this analysis because, while firms are mandated to comply with the seniority order, the law is *semi-dispositive*. This means that employers may sidestep the default ordering of workers if they reach an agreement on an alternative list of workers with the labor union; in the absence of such an agreement, the LIFO rule applies as a default.

Thus, the law provides labor unions with leverage in negotiations, particularly in situations when the most junior workers are not those the employer wishes to dismiss. However, because deviations from the mandated order often occur in exchange for concessions—-such as more generous severance pay or extended notification periods—-it is potentially costly for employers to persuade the union to sidestep the default list of displaced workers.[4] This

[1] Seniority rules exist in many countries in both age- and tenure-based forms; see Skedinger (2011) and von Below and Thoursie (2010) for a comprehensive overview.

[2] Gibbons and Katz (1991) discusses potential selection into job loss. Seim (2019) documents an association between cognitive ability and the likelihood of displacement, while evidence from Margolis and Montana (2024) suggests that employers use downsizing as a means to reshape a firm's skill structure.

[3] Various forms of LIFO rules exist in many countries, most commonly within collective bargaining agreements (CBAs) in many European countries; in the United States, most layoffs in school districts are similarly governed by seniority rules (Boyd et al., 2011).

[4] Thees two types of concessions are specifically mentioned by employers themselves when the seniority rules have been evaluated (see p. 305 and p. 408 in SOU, 2020).

Figure 1. Probability of Being Notified Over Wage Rank

Notes: The figure plots the likelihood of notification against the within-year and occupation wage rank for downsizing events in Sweden 2005–2019. The diamond bins show the mean likelihood of being displaced for each of the 50 wage ranks. The triangle bins show the likelihood of being displaced controlling for age. The lines display flexible fits based on a fractional polynomial, while the shaded areas represent the 95% confidence intervals.

institutional feature enables me to examine when employers and unions are most likely to comply with or sidestep the mandated ordering, as well as how much employers are willing to pay to retain newly hired workers through either severance pay or increased advance notice.

The seniority rule is arguably one of the most controversial features of EPL in Sweden. While unions generally view it as a cornerstone of job security that prevents arbitrary dismissals (Lund, 2013), many employers argue it hinders business growth by making it harder to retain key staff, reducing labor mobility, and adding bureaucratic costs (Svenskt Näringsliv, 2009). Meanwhile, some critics argue the debate is performative, noting that in practice, senior employees are rarely protected because employers have numerous ways to bypass the rules (Calleman, 2011, p. 190). This view has been challenged by several studies showing that it indeed affects a variety of labor market outcomes (Bjuggren, Olsson, and Skedinger, 2018) Given these conflicting perspectives, documenting where and when firms actually deviate from the default ordering is central to understanding how this policy operates in practice.

This study is, to my knowledge, the first to document which and when employers *comply* with the seniority ordering when downsizing. Specifically, I investigate the association between the within-workplace productivity ordering

of workers and which individuals are selected for displacement. In Sweden, employers and labor unions negotiate which workers are to be displaced. These negotiations yield a list of workers, which receive advance notice of layoff. First, I identify the list of notified workers for each downsizing event, alongside those who were employed at the same time and place, but not notified. By leveraging information on individual occupations and tenure, I construct the counterfactual lists of workers who *would* have been notified under strict compliance with the LIFO ordering. I then calculate LIFO compliance as the degree of overlap between those would have been and actually were notified. Additionally, I categorize downsizing workplaces by the relative productivity of their most junior workers, proxied using AKM worker fixed effects, to infer how binding the LIFO ordering is for each workplace. Finally, I examine systematic differences in compliance along this productivity dimension.

An important reason for the limited evidence on employer compliance with seniority ordering is the difficulty of distinguishing voluntary from involuntary exits. In many studies of job displacement, researchers cannot definitively determine whether a separation resulted from a layoff or a voluntary quit. The existing literature typically addresses this in two ways: by relying on survey data, which may suffer from recall bias if conducted years after the event (von Wachter, Handwerker, and Hildreth, 2009), or by using mass layoff designs to isolate exogenous job loss (Jacobson, LaLonde, and Sullivan, 1993).[5]

I address this issue by leveraging unique and detailed register data on layoffs in Sweden, collected by the Public Employment Service (PES). These data include the near universe of layoff notifications in Sweden, allowing me to distinguish notified and non-notified workers through linkage with employer–employee registers.[6] This strategy enables me to study the selection of workers for displacement versus retention with significantly less error than in designs lacking register data on notifications.

I find that workplaces with relatively more productive recent hires are less likely to comply with the LIFO ordering when downsizing. A one–standard deviation increase in the relative productivity of junior workers is associated with a reduction in compliance by 7.7 percentage points.[7] This result indicates that employers may be willing to comply with the seniority ranking whenever it is not binding, i.e. when the productivity ranking of the workers aligns with the seniority ranking of their workers. Conversely, whenever the recent hires

[5]Even large-scale events risk conflating involuntary displacement with coinciding voluntary quits (Flaaen, Shapiro, and Sorkin, 2019; Häggkvist, 2026). By design, mass layoffs likely involve less worker selection, which is the very phenomenon I aim to investigate (Gibbons and Katz, 1991).

[6]These data are available for years 2005–2019 and include all parts of the country, see Figures A.1 and A.2.

[7]I define the relative productivity as the normalized difference in mean worker fixed effects between the workers who should have been displaced vs. should not have been displaced, see Section 4.

are relatively productive compared to other workers, employers are much more likely to sidestep LIFO and instead displace more tenured workers.

This study is also, to my knowledge, first to estimate firms willingness to pay to sidestep EPL. Using a measure of severance pay introduced by Cederlöf et al. (2025), I determine if deviations from LIFO are associated with increased severance packages or longer advance notice. I find that workplaces with relatively more productive junior workers pay higher severance to notified workers, consistent with employers incurring costs to deviate from LIFO by buying out more senior workers. I also find that these firms provide longer advance notice. The results indicate that exempting one extra worker incurs a cost of 45,700 SEK, split equally between extended notice and higher severance.

This study contributes to several strands of the literature. First, the literature on how seniority rules affect the labor market. Existing research suggests that such mandates can suppress hiring (von Below and Thoursie, 2010; Bornhäll, Daunfeldt, and Rudholm, 2015), increase firm selectivity during recruitment (Butschek and Sauermann, 2024; Bjuggren and Skedinger, 2018), lead to worker absence (Olsson, 2009), and diminish labor productivity (Bjuggren, 2018). This study is to my knowledge first to study how compliance with the seniority ordering interact with the productivity ranking of the workplace. By analyzing who is displaced, I document the first empirical evidence on how firms navigate the trade-off between mandated compliance and the retention of workers.

Second, I extend the broader literature on the consequences of Employment Protection Legislation (EPL) (Addison and Teixeira, 2003; Autor, Kerr, and Kugler, 2007; Pissarides, 2001; Saez, Schoefer, and Seim, 2023; Skedinger, 2011) by employing register data on displacements. By combining these data with a setting where employers face restricted discretion in whom they are allowed to displace, I am able to investigate selection into job loss without the risk of conflating voluntary and involuntary separations (Gibbons and Katz, 1991).

Third, I contribute to the literature on job security provisions and advance notice for displaced workers (Lazear, 1990; Cederlöf et al., 2025). I document a previously unexplored channel through which employers buy flexibility in who they displace using severance packages and extended advance notice. This allows me to quantify employers' willingness to pay to sidestep the mandated order of layoffs.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting while Section 3 presents the data and sampling procedure. Section 4 presents the empirical specification. Section 5 examines compliance with the LIFO ordering as well as worker compensation across workplaces with different skill structure. Section 6 studies heterogeneity by event characteristics. Section 7 concludes.

2 Institutional Setting

In this section, I first describe the Swedish last-in-first-out rule in Section 2.1. Second, I describe how I define compliance with the rule in Section 2.2.

2.1 The Last-In-First-Out Rule

The strictness of EPL in Sweden is similar to many other OECD countries, for instance Germany, Italy, France, and Spain, while it is more strict than in countries such as the United States or Canada (Figure 2). Regarding the LIFO rule specifically, it exists in more than 80 other countries (World Bank Group, 2016). In most cases, seniority rules are established through CBAs, whereas Sweden and the Netherlands stand out by explicitly anchoring them in law as the primary criterion for prioritizing among workers (Cederlöf, 2024).[8]

The Swedish LIFO rule is regulated by the Employment Protection Act (SFS, 1982).[9] The law stipulates that newly hired workers must be dismissed before those with longer firm tenure. In cases where tenure is equal, older workers are given priority and retained. The legislation applies at the workplace × CBA level.[10] These groups are called order of termination circuits (*turordningskrets*, henceforth referred to as circuits). Another important feature is the priority of recall for workers with at least 12 months of tenure who are deemed sufficiently qualified for the new job and expressed a desire for recall prior to their layoff.[11]

Importantly, the law is semi-dispositive. This means that employers and labor unions may agree to deviate from the mandated ordering and establish an alternative list of workers. When an employer intends to downsize, they are required to enter negotiations with the labor union to determine the layoff order.[12] These negotiations result in a list of workers to be notified.

The employer and union have full discretion over who is selected for displacement, under the condition that they reach an agreement, while the individual worker has no influence over who is dismissed or retained.[13] The alternative list is sometimes constructed using more fine-grained cells within which the ranking occurs. For example, if the cells are narrower than the entire group covered by the CBA, the employer gains greater discretion in deciding

[8] See Section A for a comprehensive overview of previous EPL studies.

[9] *Lagen om anställningsskydd* (LAS).

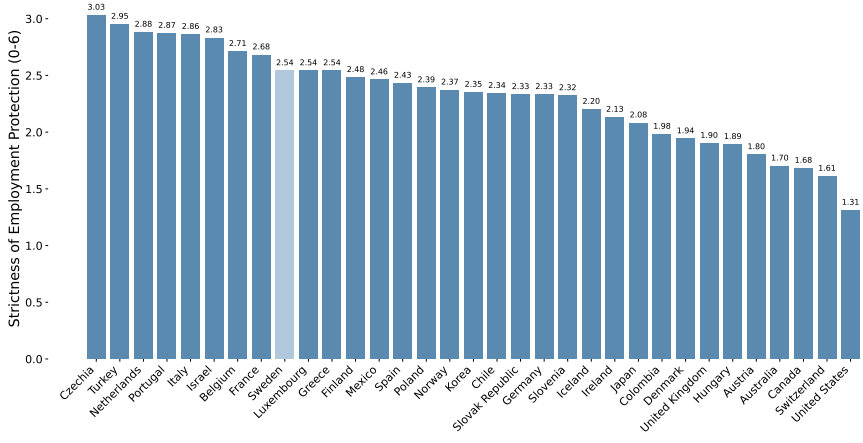
[10] If an employer has several workplaces at the same locality, the labor union may request that all workplaces be treated as a single unit.

[11] During the study period, these rules remained in effect until the worker's 67th birthday. In addition, exemptions included the employer's family members and managerial staff. Small businesses with up to 10 employees were also allowed to exempt two employees if they were considered essential to the continued operation of the firm.

[12] Labor unions have the right to participate in all major decisions affecting the workforce, including downsizing, as regulated by the Co-Determination Act (SFS, 1976).

[13] Important to note is that worker membership in a labor union has no formal importance for implementation of the LIFO ordering.

Figure 2. OECD Employment Protection Index (2019)



Notes: The figure shows the strictness of EPL for individual and collective dismissals of regular contracts across selected OECD countries. The measure is based on the OECD Employment Protection Legislation indicator (OECD, 2019), which ranges from 0 (least strict) to 6 (most strict). Countries are ranked from most to least strict. Sweden is highlighted in light blue. Values above each bar indicate the strictness score for the respective country.

who is dismissed.[14] The definition of these cells is therefore part of the negotiation process, as it directly affects which workers are notified and which are retained (Calleman, 1999).

However, if the employer and union fail to reach an agreement, the LIFO ordering applies as a default, giving the labor union substantial bargaining power.[15] Without an agreement, the fallback option is full compliance with LIFO, meaning that displacement is governed strictly by seniority.[16] In such cases, the employer must retain the most senior workers – which are not necessarily the same workers that would have been retained in absence of the law. This creates a threat point that the union can use in negotiations.

While employers are likely to prioritize the future efficiency of the firm and therefore seek to retain the most productive workers, labor unions are likely to advocate for generous severance packages or advance notice periods for displaced workers.[17] Survey evidence suggests that in 90% of cases where

[14] Some CBAs include all white-collar or all blue-collar workers at the workplace while other are occupation specific.

[15] In negotiation theory, a superior threat point yields a stronger bargaining position and better outcomes – see Nickell and Andrews (1983) for an application to union-employer wage disputes.

[16] There are several additional ways for employers to deviate from strict compliance. First, during the study period, firms with ten or fewer employees could exempt two workers from the seniority ranking. Second, workers who cannot be retrained for a new job within six months after downsizing may be exempted.

[17] A union's preferred ordering is not ex ante obvious. For example, unions may in some instances favor retaining high-productivity workers to ensure the long-term viability of the firm.

the default order was sidestepped, the agreement also included some form of extra-legal benefits, most commonly extended notice periods or severance pay (see p. 306 in SOU, 2020).

While quantitative evidence on the motives of the employers and labor union are scarce, Saez, Schoefer, and Seim (2023) finds that low skilled elderly employees are more likely to be displaced, upon reaching retirement age. There is some qualitative evidence suggesting that labor unions prefer to follow LIFO, as the cost of job loss is potentially larger for more senior workers (Calleman, 1999). This suggests that employers often have incentives to sidestep strict LIFO compliance, while labor unions typically require concessions, such as more generous severance pay, in exchange.

Survey evidence indicates that many employers perceive the mandated ordering as an obstacle. In a 2009 survey, 50% of employers reported that they would likely exempt certain workers from the ranking in the event of downsizing, and 32% had already done so in the previous year (Svenskt Näringsliv, 2009). Among those who had used the exemption, nearly all (93%) stated that the ability to exempt workers from LIFO was vital for the firm's future.[18]

2.2 Examples of Differential Incentives and Compliance

Figure 3 illustrates two examples circuits, with varying degrees of LIFO compliance. The upper row depicts an example of a circuit where there is high compliance with LIFO and the bottom row low less compliance. Workers are ranked from left to right by seniority within circuit following the law. In both examples, the circuit contains ten workers, and the employer wants to lay off four.[19] The four rightmost workers are the most junior and should be notified under perfect LIFO compliance. I refer to these workers as the "de jure notified".

Once negotiations are finalized, the workers who are notified de facto and de jure are not necessarily the same, owing to varying degrees of compliance. The red colored workers are de facto notified, while the rest are retained. LIFO-exempted workers are highlighted, meaning they are de jure notified but not de facto notified.[20]

Under perfect compliance, all four junior workers would be de facto notified. In the upper circuit, compliance is high, as three out of four de jure notified

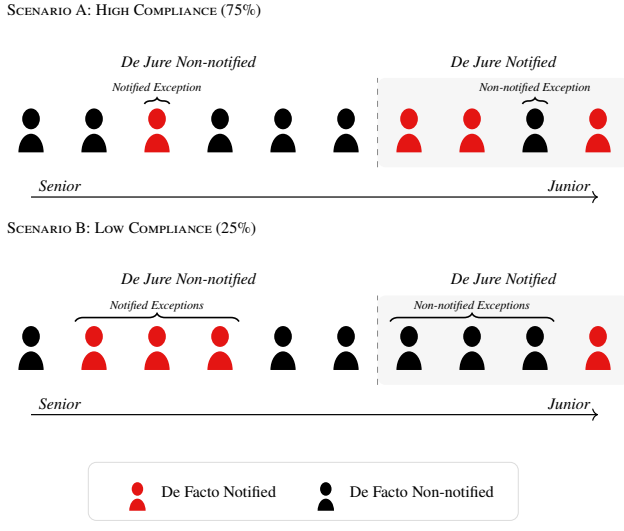
The unions have, however, strongly opposed legislation that would give the employers more discretion over which workers are displaced (Gideonsson and Rehnström, 2020).

[18]The employers' organization has also argued for more relaxed rules allowing employers to prioritize competence and qualification over seniority (Dahl, 2020).

[19]It is possible that also the number of displaced workers is part of the negotiation. I return to this in Section 4.

[20]Cederlöf (2024) exploits the cutoff, highlighted with the vertical dashed line, using a regression discontinuity design to estimate the cost of job loss.

Figure 3. Illustration of Two Circuits with Different LIFO Compliance



Notes: This figure illustrates varying degrees of LIFO compliance. Red figures represent workers who are *de facto* notified; black figures are *de facto* non-notified.

workers are actually notified. In the second example, compliance is low, as only one out of four de jure notified workers is actually notified.

I hypothesize that because workplaces vary in the *relative within-circuit* productivity of their recent hires, they will also vary in their compliance with seniority ordering. The intuition is that because employers want to retain their most productive employees, they are willing to make concessions to the labor union if the perceived gain of sidestepping the ordering is large enough.

This is exemplified in Figure 4. The green workers have high productivity while the blue workers have low productivity. The setup is the same as in Figure 3; there are thus two example circuits where workers are ranked by their tenure. In the upper circuit, the de jure notified workers are all blue meaning that the employer would have a lot to lose to follow LIFO and is therefore incentivized to sidestep. Conversely, in the bottom example, the junior workers are of low productivity meaning that the incentives to comply are larger.

In the next section, I discuss the data sources I use to estimate the relationship between the relative productivity of the most recent hires and LIFO compliance.

3 Data Sources and Sampling

I use detailed register data on layoff notifications collected by the Public Employment Service (PES) (Arbetsförmedlingen, 2025), which provide near-

Figure 4. Illustration of Two Circuits with Different Incentives To Comply

Notes: Blue figures represent highly productive workers; green figures represent less productive workers.

universe coverage of notified individuals in Sweden between 2005 and 2019.[21] Under Swedish law, employers planning to lay off five or more workers within a 90-day window must file an official notification with the PES.

Notification data are rare in the previous literature. Most studies instead rely on employer–employee data to infer downsizing events, typically requiring that the event affects at least 30% of the workforce (Couch and Placzek, 2010). These approaches risk erroneously coding some voluntary quits as layoffs. The PES data resolve this by directly observing the employer's reported list of notified workers.

I link the PES notifications with several administrative registers: (i) RAMS (SCB, 2018), covering all employer–employee matches with earnings and employment status since 1985; (ii) LISA (SCB, 2017), providing demographic characteristics including education, marital status, and migration background; (iii) the Business Register (SCB, 2025a), supplying establishment-level industry codes; and (iv) Wage Statistics (SCB, 2025b), containing full-time equivalent wages, sector information, and detailed occupation codes (SSYK).[22]

I first identify all workers who appear in both the PES and employer–employee data in the notification month. I then define their colleagues as non-notified workers employed in the same establishment–month. The sample is restricted to workers with at least 12 months of tenure and monthly earnings of at least 10,000 SEK.

[21] See Figure A.2 for the number of downsizing events over time.

[22] The Wage Statistics sample covers all public-sector workers and roughly 50% of private-sector workers, with stratified sampling at the firm level.

Ideally, I would construct LIFO circuits based on CBA boundaries, as discussed in Section 2.1. However, because Swedish registers do not contain CBA information, I approximate boundaries following Cederlöf (2024) using workplace × two-digit-occupation cells (SSYK).[23] The Wage Statistics register allows me to assign occupations to all workers in sampled establishments. My main sample therefore includes only workplaces covered by this register. Although this proxy does not perfectly capture true LIFO boundaries, I assume that any resulting measurement error is non-systematic across circuits. That is, I examine the relationship between relative productivity and LIFO compliance, while I cannot capture the true levels of compliance.

I limit the analysis to non-closure downsizing events with circuit sizes between 10 and 50 workers. This ensures that each event contains both notified and non-notified workers and that employers plausibly observe relative worker performance. Unlike the cost-of-job-loss literature, I do not impose a minimum-displacement threshold (such as 30%), since selection into job loss is the focus rather than a confounder.

I define the size of the de jure notified group based on the number of de facto layoffs reported in the PES data. Within each circuit, workers are then ranked by tenure, with the lowest-tenure individuals classified as de jure notified. For instance, in a circuit of ten where five workers were actually laid off, those five with the shortest tenure are categorized as the de jure group.

The final dataset includes approximately 1,900 event–circuit combinations and about 50,000 workers, summarized in Table 1. Mean circuit size is just above 26 workers. Columns 1–2 compare notified and non-notified workers, while Columns 3–4 present the corresponding comparison under perfect LIFO compliance. As shown in the two left-most columns, notified workers are negatively selected along several dimensions: they have 17 log points lower earnings, less labor market experience, and lower educational attainment. By construction, tenure differences are even larger under the de jure assignment than in the actual notified vs. non-notified comparison.

In the next section, I describe the empirical strategy.

4 Methodology

In Sections 2 and 3, I discussed the setting and specifically how I construct the main sample. In particular, I divide the sample by whether or not a certain worker was *de facto* notified by the employer or if the worker *should have been* notified under the mandated order (de jure notified). In this section, I will define two measures, one capturing compliance with LIFO and the other capturing the relative productivity of the most recent hires.

[23] An alternative approach is to use sector-wide cells instead of occupations, in accordance with the coverage of many CBAs. As a robustness check, I also replicate the analysis using workplace × sector definitions and find qualitatively similar results. See Table A.4.

Table 1. *Descriptive statistics*

	(1)		(2)		(3)		(4)	
Panel A: Workers								
	De Facto				**De Jure**			
	Non-Notified		Notified		Non-Notified		Notified	
Job Information								
Log earnings t-1	12.65	(0.513)	12.48	(0.575)	12.68	(0.476)	12.39	(0.647)
Tenure	115.6	(84.16)	86.21	(76.78)	126.2	(83.97)	51.33	(48.59)
Experience	237.3	(94.18)	203.6	(106.1)	245.2	(91.14)	177.8	(102.5)
Pr(Blue Collar)	0.552	(0.497)	0.623	(0.485)	0.554	(0.497)	0.618	(0.486)
Pr(White Collar)	0.448	(0.497)	0.377	(0.485)	0.446	(0.497)	0.382	(0.486)
Rank in circuit	0.549	(0.283)	0.420	(0.286)	0.615	(0.244)	0.204	(0.177)
Demographics								
Age (years)	43.57	(11.13)	41.36	(12.65)	44.69	(11.07)	37.70	(11.46)
Pr(Woman)	0.307	(0.461)	0.322	(0.467)	0.313	(0.464)	0.303	(0.460)
Pr(Immigrant)	0.139	(0.345)	0.182	(0.386)	0.140	(0.347)	0.177	(0.382)
Pr(College)	0.268	(0.443)	0.232	(0.422)	0.256	(0.436)	0.272	(0.445)
Pr(Married)	0.444	(0.497)	0.401	(0.490)	0.453	(0.498)	0.374	(0.484)
Pr(Parent)	0.507	(0.500)	0.472	(0.499)	0.498	(0.500)	0.501	(0.500)
Observations	38098		11633		38098		11633	
Panel B: Circuits								
Event Characteristics								
Circuit Size	26.48	(11.50)						
Event Size	6.194	(4.697)						
Plant Characteristics								
Firm size t-1	1472.0	(2691.5)						
Plant size t-1	218.6	(481.5)						
Layoff Cause								
Pr(Streamlining)	0.490	(0.500)						
Pr(Low Demand)	0.258	(0.437)						
Pr(Other)	0.252	(0.434)						
Observations	1878							

Notes: Table 1 presents summary statistics for workers (Panel A) by de facto and de jure treatment status and for the circuits (Panel B). The first two columns split the sample based on whether the worker was notified in the PES data. The two rightmost columns present the same sample but instead split it by counterfactual treatment status under perfect LIFO compliance.

I first define the main outcome, compliance with the LIFO ordering, denoted as θ_c, captured at the circuit level c. Let $D^{\text{de jure}}$ denote the set of workers who would have been dismissed under perfect LIFO compliance, and $D^{\text{de facto}}$ denote the set of workers who actually received a layoff notice. I then calculate compliance as the share of the de jure notified workers who are de facto notified:

$$\theta_c = \frac{\sum_i \mathbf{1}\{i \in D^{\text{de facto}} \cap D^{\text{de jure}}\}}{\sum_i \mathbf{1}\{i \in D^{\text{de jure}}\}}. \tag{1}$$

If Equation (1) yields a value of 1, it means that circuit c has perfect compliance, i.e. all workers who were notified were the most junior ones in the circuit.[24] Conversely, a value of 0 means that no worker in the lower end of the seniority distribution was notified.

Next, I estimate the relative productivity of the de jure notified. Ideally, I would want to capture the marginal product of labor (MPL) to infer the degree of profitability of the match from the employer's perspective.[25] Because MPL is not directly observed, there are several ways to proxy individual-level productivity. Butschek and Sauermann (2024) suggest three potential ways to estimate worker productivity at the individual level in a similar setting: (i) GPA measured at age 16; (ii) military draft test scores at age 18, which capture cognitive ability and social aptitude; and (iii) AKM worker effects following Abowd, Kramarz, and Margolis (1999).

All three measures have been shown to be positively associated with labor market success.[26] GPA has the advantage of being observable by employers, whereas military test scores cannot be validated by employers. Military test scores also suffer from very low coverage among women, as conscription was only mandatory for men when they were collected. AKM worker effects are limited by their reliance on an individual's employment history, meaning they may be inaccurately measured or impossible to identify for individuals with short working histories in Sweden.[27]

From these alternatives, I use AKM worker effects to evaluate the relative skill of the workers as my main measure. In my setting, this measure has the by far highest coverage rate. To validate that the worker effects capture

[24] This is, as previously noted, conditional upon accurately capturing the true CBA boundaries, which remain unobserved.

[25] One body of literature discusses why employers and workers choose displacement over wage cuts. A factor is the adverse impact of lower wages on worker morale, which in turn reduces productivity (Davis and Krolikowski, 2025).

[26] Farber and Gibbons (1996) finds that school grades strongly predict future career success. Lindqvist and Vestman (2011) and Edin et al. (2022) find a strong association between labor earnings and both cognitive and non-cognitive skill measures. Butschek and Sauermann (2019) finds that AKM worker effects positively correlate with military test scores.

[27] In addition, it may be the case that wage-setting institutions may cause individual wages to depart from workers' MPL. Another potential concern is that there are complementarities between firm and worker productivity or frictions causing wages to be non-monotone in MPL (Eeckhout and Kircher, 2011).

productivity, I compare them to 9th grade GPA and find a strong association between the two measures (see Figure A.3).[28] One major limitation of using school information is that the data only include grades from 1988 onward. Consequently, for events occurring in 2005, I can only observe the productivity of workers aged 34 and younger, whereas the worker fixed effects cover around 90% of workers. Still, using grades conditional on cohort has the advantage of not relying on labor market experience. I therefore present the results using this measure in Table A.5 as a robustness check and find qualitatively similar results.

I estimate worker effects following Abowd, Kramarz, and Margolis (1999):

$$w_{ijt} = \alpha_i + \psi_j + \tau_t + \varepsilon_{ijt}. \tag{2}$$

Equation (2) decomposes wages into worker, firm, and year fixed effects from all employment histories preceding the notifying firm.[29] The dependent variable w_{ijt} denotes the log earnings of worker i employed at firm j in year t. The term α_i captures time-invariant worker-specific heterogeneity, such as skill or ability, while ψ_j reflects firm-specific wage premiums associated with employer characteristics. The year fixed effect τ_t absorbs common changes over time. Finally, ε_{ijt} is an idiosyncratic error term capturing unobserved shocks to earnings.

For each circuit c, I compute the difference in mean worker fixed effects between two groups, $g \in \{\text{de jure}, \text{non-de jure}\}$. Let $\overline{\alpha}_c^g$ denote the mean AKM worker fixed effect for group g in circuit c. I then define the following standardized metric:

$$\phi_c = \frac{\overline{\alpha}_c^{\text{de jure}} - \overline{\alpha}_c^{\text{non-de jure}}}{\text{SD}(\alpha_c)}. \tag{3}$$

A positive value of ϕ_c indicates that the de jure notified workers have higher fixed effects than the de jure non-notified workers. I divide by the standard deviation of the fixed effects to ease interpretation. For a small share of the sample, I cannot observe the worker effects due to short employment histories. I therefore exclude these workers from the analysis.[30]

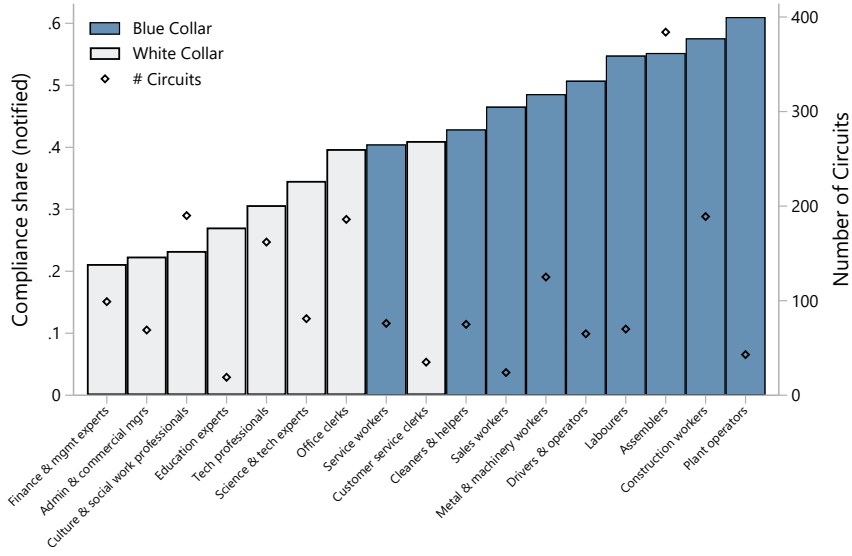
Figure 5 presents mean LIFO compliance across the two-digit occupations in the sample. The average compliance across all circuits is 42.5%, corresponding to 3.56 junior workers exempted from the ranking. Grey bars highlight white-collar occupations, while blue bars represent blue-collar occupations. As expected, mean compliance is higher in blue-collar worker circuits. One

[28] GPA might also have an indirect effect on labor market outcomes by constraining the choice of upper secondary school programs in the admission process (Dahl, Rooth, and Stenberg, 2023).

[29] Implicitly, the measure is only defined for workers with previous jobs.

[30] This restriction is applied before determining event size and selecting the de jure notified sample.

Figure 5. Compliance Share Over Occupations



Notes: Figure 5 presents the mean compliance at the circuit level across occupations. White bars indicate white-collar occupation circuits, while blue bars represent blue-collar circuits. The black diamonds denote the total number of circuits within each occupational category.

potential explanation for this pattern is that blue-collar occupations tend to be more well-defined than white-collar occupations.[31]

Figure 6a shows the density of worker effects for the de jure notified (red) and de jure non-notified (blue) workers. In general, newly hired workers exhibit lower worker effects. When examining the differences between the two groups in Figure 6b, I find that most circuits have, on average, more productive non-notified workers, although a non-negligible share of circuits feature newly hired workers who are more productive.

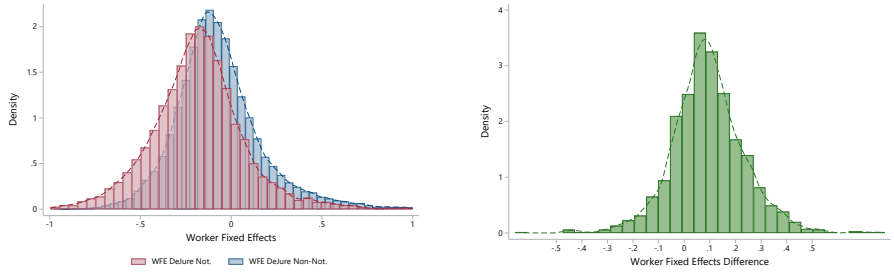
The ideal experiment would involve comparing two workplaces characterized by (1) the random assignment of workers' productivity rankings along the seniority margin and (2) exposure to similar exogenous shocks that prompt identical downsizing requirements. Because a realistic scenario fulfilling both conditions is extremely rare, I cannot claim that my estimates are fully causal. Nevertheless, documenting the association between the relative productivity ordering of the workforce and compliance remains interesting in its own right. To closer approximate a causal interpretation and, I control for event and circuit size, both of which may influence compliance and the relative productivity measure.[32]

[31] See Figures A.4a and A.4b for the distributions of notice time and severance pay across occupations.

[32] Table A.2 describes my main sample by different levels of the variable of interest.

Figure 6. Descriptives: α^g Distributions and Differences

(a) Distribution of α^g *(b)* Difference in α^g distributions



Notes: Figure 6a shows the histogram distributions of α^g (as defined in Section 4) for de jure notified vs. de jure non-notified workers. Figure 6b illustrates the difference between these two distributions. The density is measured as the number of circuit observations.

One potential channel through which workplaces with different recent hires might differ is if employers explicitly incorporate existing employees and their seniority into the hiring process, either by changing the number of hires or the selectivity for a given number of hires (Pries and Rogerson, 2005; Butschek and Sauermann, 2024). For instance, firms with less productive workers might abstain from hiring until they have displaced a worker lower down in the seniority distribution, meaning that low-ϕ workplaces would experience excess separations. In my setting, these workplaces have if any slightly *larger* events, suggesting this is not an important mechanism. While I cannot fully rule out unobserved differences along this margin, I find no systematic differences in pre-trends when comparing establishment size, total labor earnings, and sales prior to the downsizing events.[33]

Correlating the two measures, LIFO compliance θ_c, and relative productivity of the de jure notified workers, ϕ_c, I then estimate the relationship using the following specification:

$$\theta_c = \alpha + \beta\phi_c + \gamma CS_c + \gamma TS_c + \varepsilon_c. \tag{4}$$

The coefficient of interest, β, capture the effect of the relative productivity of recent hires on LIFO compliance. A negative β indicates that as the productivity of recent hires increases relative to senior workers at the same workplace, non-compliance with LIFO increases. CS_c and TS_c denote circuit and event size respectively.[34]

[33] These validation exercises are presented in Figures A.5 and A.6.

[34] High- and low productivity circuits have very similar circuit sizes, while high productivity events are slightly larger, as detailed in Table A.2. See Table 2 for the main results using different sets of controls and fixed effects. I also present the main results splitting the sample into equal sized quintiles, based on the level of ϕ. See Table A.3.

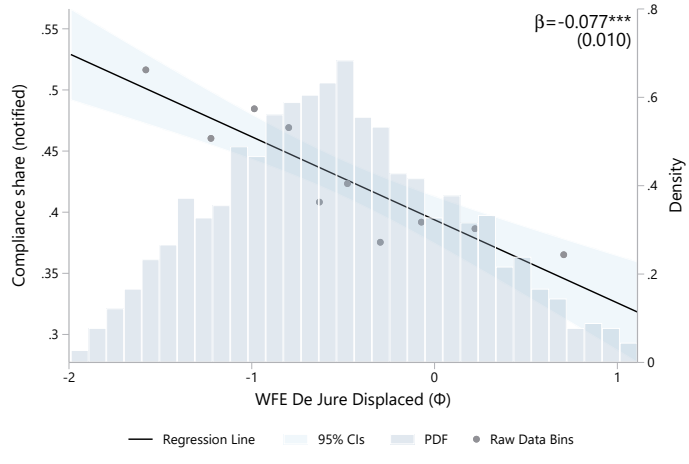
5 Compliance With LIFO & Worker Compensation

In this section, I examine the relationship between compliance with the LIFO ordering and the distribution of worker fixed effects at the circuit level.

Figure 7 plots the estimates from Equation (4) detailed in Section 4. On the x-axis, I have the circuit level worker AKM fixed effects calculated as the difference between the de jure notified and the non-de jure notified. The value 0 indicates that the worker effects of the junior workers are on average equal to the senior workers. On the y-axis, I measure compliance defined as the share of de jure notified workers who are notified, see Equation (1). The data is binned into deciles as indicated by the hollow circles while the solid line indicates the predicted regression line. The shaded blue area shows 95% confidence intervals.

In circuits where the worker effects of the junior workers are one standard deviation lower compared to that of the senior workers, compliance share is 7.7 percentage points higher compared to circuits where the worker effects are similar across the seniority ladder.

Figure 7. Compliance share



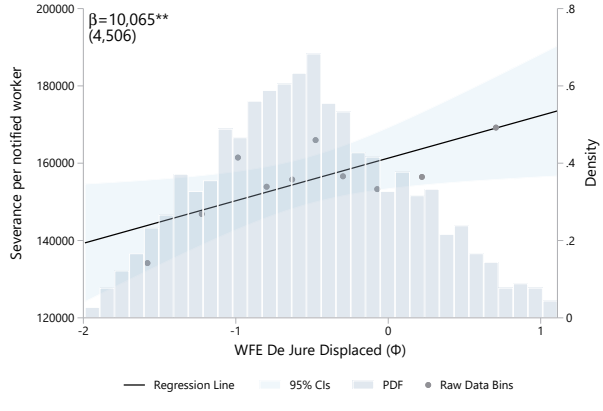
Notes: The solid line corresponds to the predicted estimates, as defined in Equation (4) in Section 4. The light blue area represents the 95% confidence intervals. The dots show the levels of the raw data. The histogram depicts the PDF of Φ – the relative worker fixed effects of the de jure notified workers relative to the non-notified, normalized by the circuit standard deviation of the WFEs. The y-axis shows the compliance share, i.e., the proportion of de jure notified workers who are de facto notified.

This indicates that employers are more likely to comply with LIFO when the productivity of the relatively junior workers is lower, as measured by the AKM fixed effects. This finding is in line with the hypothesis, that employers are more willing to comply with LIFO whenever the seniority ranking and the productivity ranking align. However, in situations when these two rankings do not align, employers are more likely to negotiate with the labor union to have more senior workers should be let go first.

I have now established that in circuits where the relative productivity of the most junior workers is lower, compliance with LIFO is generally higher. This raises the question of whether deviations from LIFO impose costs on employers. There are two main provisions displaced workers could receive in my setting, severance pay and prolonged advance notice.[35] I investigate this by examining the severance pay received by displaced workers.

A key challenge in estimating severance payments is that they are not reported separately in the Swedish registers, but instead lumped together with other labor earnings. This makes it difficult to separate severance payments from regular wage payments. To resolve this issue, I follow Cederlöf et al. (2025) and predict the portion of total earnings in the displacement year that stems from wage payments, using earnings from the year before.[36] For example, if a worker separated from an employer in March, I construct severance by subtracting three months of predicted earnings from total labor earnings. While the absolute levels of this measure may not be precise, the identifying assumption is that any differences across circuits, along the worker-effects dimension, are unbiased. Figure 8 shows the result from this exercise, having severance payments as the outcome while keeping the same specification as in Section 5. The x-axis is still the relative worker effects of the de jure notified.

Figure 8. Severance pay



Notes: The solid line corresponds to the predicted estimates, as defined in Equation (4) in Section 4. The light blue area represents the 95% confidence intervals. The dots show the levels of the raw data. The histogram depicts the PDF of Φ – the relative worker fixed effects of the de jure notified workers relative to the non-notified, normalized by the circuit standard deviation of the WFEs. The y-axis shows the amount of severance received in the displacement year defined as the difference between actual labor earnings in the displacement year and predicted labor earnings, inferred from the year before notification and months worked in the displacement year.

[35] I also investigate whether wage trajectories differ by the productivity level of the circuits in Figure A.5. One concern may be that some workers accept pay cuts to remain at a downsizing firm (Davis and Krolikowski, 2025). However, I find no evidence supporting this mechanism in my setting.

[36] Cederlöf et al. (2025) validates this measure using external data sources on severance payments.

In contrast to Figure 7, there is now a strong positive relationship between the relative ability of the most junior workers and the amount of severance paid out per worker. This indicates that while sidestepping LIFO is more attractive for employers in cases when the new hires are relatively more productive, this may come at a cost of higher severance payments.

One mechanism that could generate this result is that whenever the employer wishes to make deviations from LIFO, the labor union has leverage as they always have the option of demanding full compliance. Because compliance is relatively more costly for employers in cases when there is little alignment between the seniority and productivity rankings, their willingness to pay is larger in these scenarios. The results suggest that the union understands this and are able to extract more generous severance.

While severance is the most obvious way to compensate workers for deviating from LIFO it is also possible for employers to compensate workers through increased advance notice. The idea is that if notified workers receive longer advance notice, they receive (1) mechanically longer employment spell meaning they will prolong time to potential unemployment (2) more time for on-the-job search for a new job which is likely to lead to a higher reemployment wage (Cederlöf et al., 2025). However, providing longer notice time is costly for the employer as they bear the wage cost.

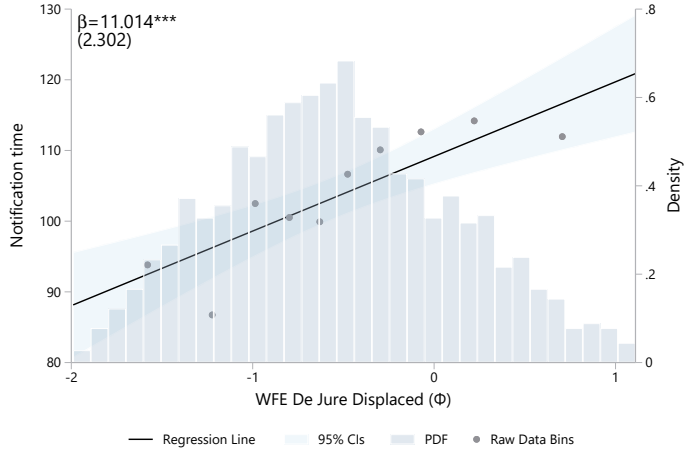
Figure 9 presents the regression estimates using the notice time as the outcome. I find that not only do severance increase in circuits with high compliance, there is also an increase in the notice time. This suggests that only looking at the severance payments underestimates the level of provisions provided by the employer and that more generous advance notice is a complementary channel of provisions.

As a next step, I investigate if the result vary by adding various controls and fixed effects. The results from this exercise are presented in Table 2. I find that the compliance result is robust to all specifications, suggesting that the result hold also when performing the analysis within industry and occupation. However, the result are smaller when analyzing compliance within sector, suggesting that this variation is important for understanding the size of the estimates.

When investigating the effect on advance notice and severance pay, I find that notice time follows a similar pattern as LIFO compliance, where sector is most important for explaining the magnitude. However, the effect on severance pay is entirely explained by across sector variation. Taken together, regardless of which across variation I use, the results suggest that employers trade flexibility for notice time and/or severance whenever the relative productivity of their most recent hires is high. In the next section, I investigate how these patterns vary with event characteristics.

Combining the severance cost and the extended notice, I find that firms pay 21,800 SEK in the main specification to reduce LIFO compliance by 7.7%

Figure 9. Notice time



Notes: The solid line corresponds to the predicted estimates, as defined in Equation (4) in Section 4. The light blue area represents the 95% confidence intervals. The dots show the levels of the raw data. The histogram depicts the PDF of Φ – the relative worker fixed effects of the de jure notified workers relative to the non-notified, normalized by the circuit standard deviation of the WFEs. The y-axis shows the length of notice period received as reported in the PES data.

Table 2. *Regression Outcomes with different specifications*

	(1)	(2)	(3)	(4)	(5)	(6)
Compliance	-0.068***	-0.072***	-0.077***	-0.053***	-0.052***	-0.050***
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
Notice Time	10.939***	10.955***	11.014***	6.448***	5.505**	5.163**
	(2.309)	(2.313)	(2.315)	(2.248)	(2.178)	(2.182)
Severance	9655.288**	9637.042**	10064.937**	222.555	-250.314	-3353.039
	(4700.373)	(4705.902)	(4692.696)	(4532.456)	(4540.513)	(4289.986)
Total Provisions	21024.946***	21129.440***	21804.993***	5842.006	5728.634	14.452
	(6345.231)	(6352.193)	(6323.940)	(5959.180)	(5929.724)	(5419.073)
Circuit Size		✓	✓	✓	✓	✓
Event Size			✓	✓	✓	✓
White Collar				✓	✓	
Industry FEs					✓	✓
Occupation FEs						✓
N	1878	1878	1878	1878	1878	1878

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table 2 presents the main results across seven different specifications, with each subsequent model adding a covariate control. Total provisions is calculated by adding severance pay with advance notice × the monthly wage. Circuit size denotes the total number of workers in the circuit, while event size represents the number of notified workers. White-collar is a dummy variable indicating whether the circuit occupation is white-collar or blue-collar. The industry and occupation fixed effects are included at the one-digit level. All models report robust standard errors.

when laying off 6.2 workers. This implies a cost of around 45,700 SEK per person exempted from LIFO.

6 Compliance & Compensation By Event Characteristics

In Section 5, I documented large differences in both LIFO compliance and worker compensation between workplaces where the de jure notified workers have low worker effects relative to those where they have high worker effects. In this section, I investigate which types of downsizing events drive this result using three dimensions of heterogeneity: circuit size, plant size, and sector.

There are several reasons why circuit and plant size might influence the degree of compliance and the provisions offered. First, the total number of employees may affect the saliency of individual worker productivity; specifically, smaller events may exhibit a lower degree of compliance if individual productivity is more visible to the employer. Second, the capacity to provide severance packages may vary by employer size.

I also investigate whether the results vary by sector. The motivation is that occupations vary in how clearly they are defined. Blue-collar occupations typically have higher baseline compliance than white-collar occupations, as illustrated in Figure 5. This indicates that the mandated ordering is more binding in blue-collar occupations, where tasks are often more standardized and concrete. In such circuits, workers are more interchangeable, making it harder for employers to justify sidestepping seniority without explicit union negotiations.

Table 3 illustrates the heterogeneity along these dimensions. Regarding size, I split the sample at the median. I find that large circuits and plants are more likely to pay severance, while small plants are more likely to provide more generous advance notice. This indicates that advance notice may be used when an employer is small and liquidity-constrained, preventing the payment of lump-sums to displaced workers. Finally, I find that nearly the entire effect is driven by blue-collar circuits. This supports the hypothesis that the results are concentrated among laborers, for whom the mandated ordering is more strictly binding than for white-collar occupations.

Table 3. *Heterogeneity Analysis*

	Circuit Size		Plant Size		Sector
	Small	Large	Small	Large	White-collar
Compliance	-0.071***	-0.074***	-0.075***	-0.049***	-0.057***
	(0.015)	(0.017)	(0.016)	(0.016)	(0.015)
Notice Time	8.454***	13.166***	12.518***	5.256	12.515***
	(3.106)	(3.409)	(2.961)	(3.541)	(2.674)
Severance	5788.078	14493.471**	4082.166	11428.464*	1994.693
	(6334.190)	(6392.511)	(5820.663)	(6930.152)	(4392.906)
Total Provisions	16141.414*	27327.509***	14978.152**	16517.832*	11655.868**
	(8502.300)	(8812.972)	(7503.200)	(9646.487)	(4979.624)
Circuit Size	✓	✓	✓	✓	✓
N	959	919	934	944	1047

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table 3 presents the main results categorized by circuit size, plant size, and sector. Total provisions is calculated by adding severance pay with advance notice × the monthly wage. Circuit and plant sizes are determined by the median value within the sample. The sector variable is partitioned based on whether the circuit occupation is classified as white-collar or blue-collar.

7 Conclusions

This study is to my knowledge first to estimate firms willingness to pay to sidestep EPL. Using Swedish register data on notified layoffs, linked to employer–employee records, I investigate firms' compliance with the LIFO ordering. Sweden serves as an ideal setting for this analysis, as employers are mandated a default ordering, but may negotiate with the labor union for an alternative ordering, in exchange for concessions such as severance pay and more generous advance notice.

I find that firms with relatively more productive recent hires are substantially less likely to comply with the LIFO ordering. A one–standard deviation increase in the relative productivity of junior workers reduces compliance by 7.7 percentage points. This suggests that employers are generally willing to follow seniority ordering when productivity and tenure rankings align, but deviate from them when retaining newly hired workers is more valuable.

Deviations from LIFO are also associated with compensatory measures toward notified senior workers. Firms that retain productive junior employees tend to provide higher severance payments and longer advance notice, indicating that they internalize the cost of non-compliance through negotiated settlements. These findings reveal an efficiency–equity trade-off: while seniority rules protect older or longer-tenured workers, they may constrain firms from retaining their most productive employees. I find that exempting one extra worker incurs a cost of 45,700 SEK, split between extended notice and more generous severance.

The results contribute to the broader literature on EPL by highlighting heterogeneity in firms' compliance behavior and their willingness to pay to sidestep the ordering. Overall, the findings underscore that the distributive and efficiency effects of EPL depend not only on the existence of mandated orderings, such as LIFO, but also on how firms navigate and negotiate around them.

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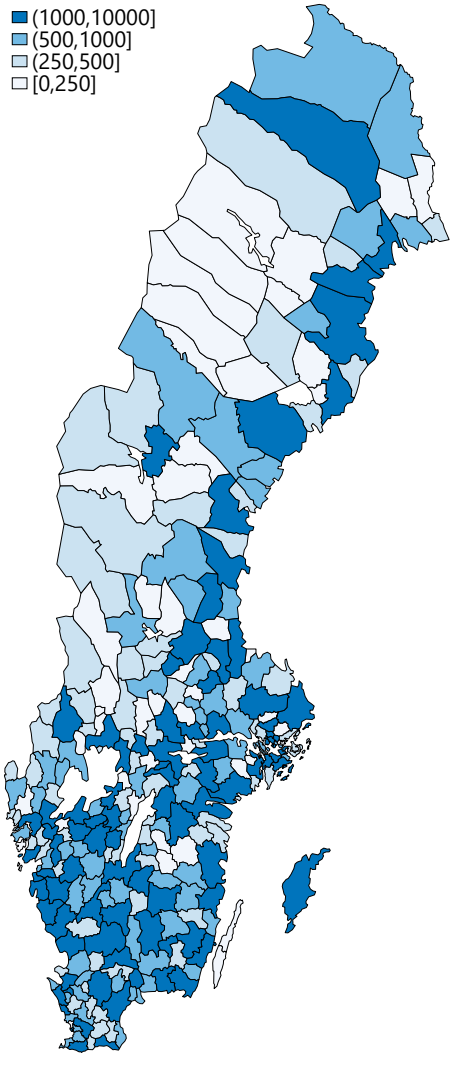
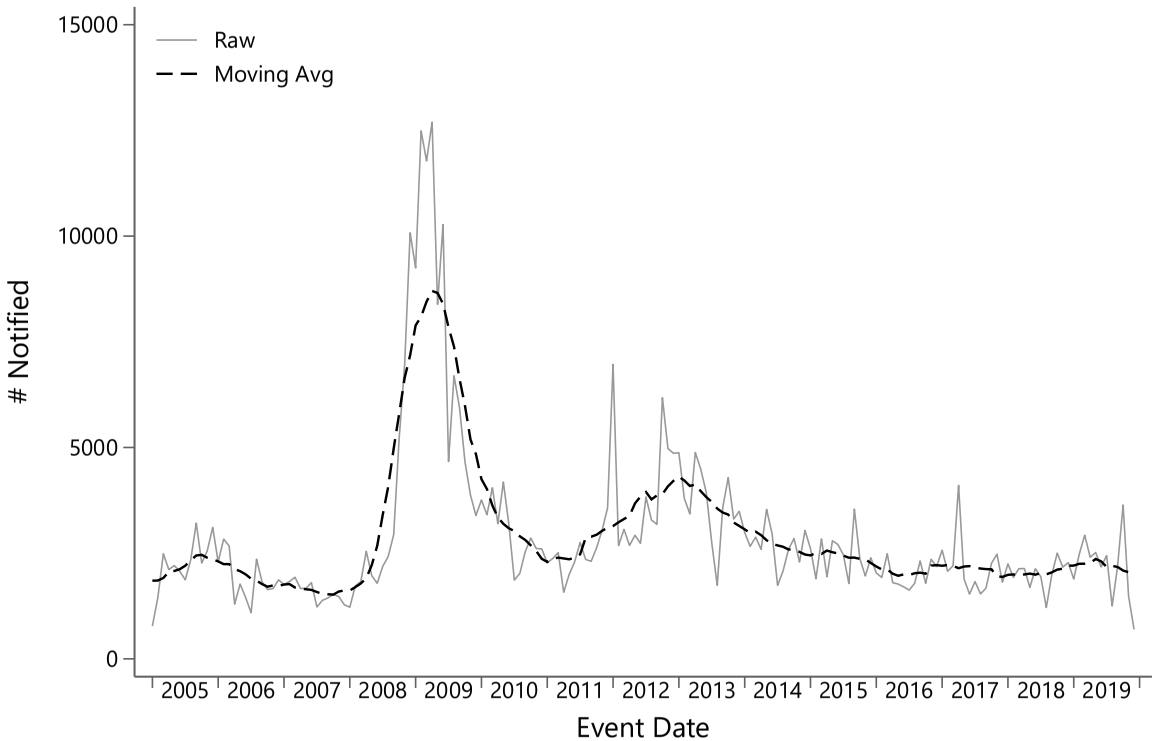
A Additional Figures and Tables

Figure A.1. Number of Notified Workers By Municipality



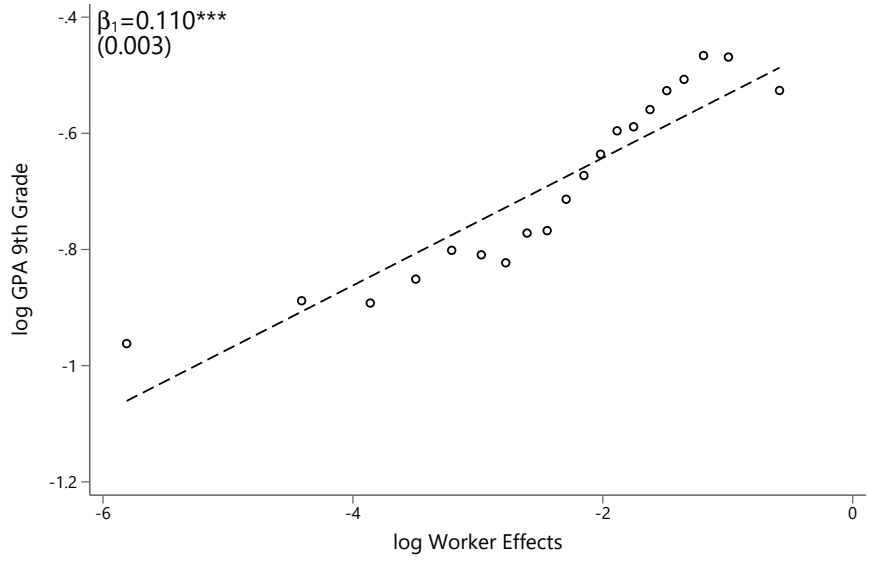
Notes: Figure A.1 plots the number of notified workers during the study period by municipality in Sweden.

Figure A.2. Number of Notified Workers Over Time



Notes: This figure plots the number of notified workers during the study period. The light gray line represents the raw monthly counts, while the solid black line displays the 12-month centered moving average.

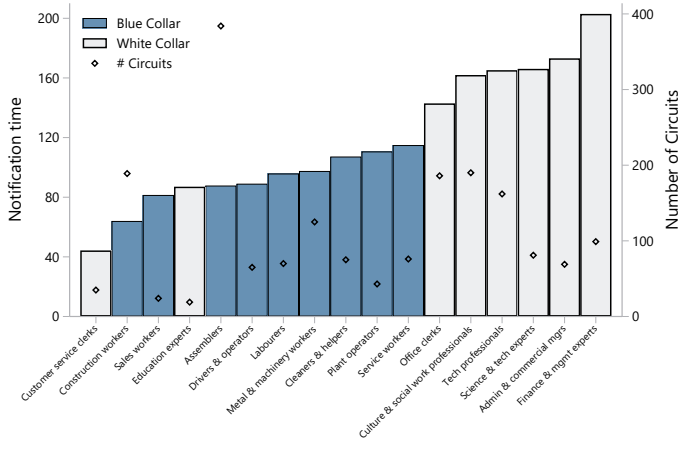
Figure A.3. Correlation WFE and GPA



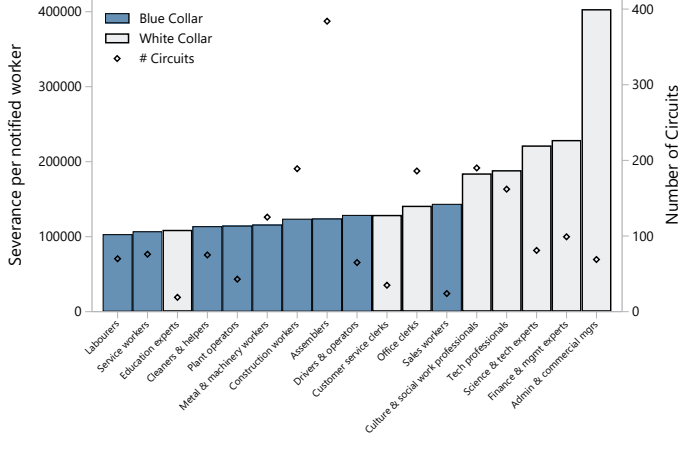
Notes: This figure shows the association between log GPA in 9th grade and log worker effects following Abowd, Kramarz, and Margolis (1999), controlling for school and graduation year effects.

Figure A.4. Descriptives Over Occupations

(a) Advance Notice Time



(b) Severance Pay



Notes: Figure A.4a displays advance notice time while Figure A.4b displays severance pay across occupations. White bars indicate white-collar occupation circuits, while blue bars represent blue-collar circuits. The black diamonds denote the total number of circuits within each occupational category.

Table A.1. *Literature Review on Employment Protection Legislation (EPL)*

Study	Country	EPL of Interest	Outcome
Autor et al. (2007)	USA	Wrongful discharge	Firm productivity
Bauer et al. (2007)	Germany	Wrongful discharge	Hiring & dismissals
Bjuggren (2018)	Sweden	LIFO	Firm productivity
Bjuggren and Skedinger (2018)	Sweden	LIFO	Hiring selectiveness
Bornhäll et al. (2017)	Sweden	LIFO	Firm growth
Boeri and Jimeno (2005)	Italy	Dismissal provisions	Hiring & dismissals
Buhai et al. (2014)	Denmark	LIFO	Wage return to seniority
Butschek and Sauermann (2024)	Sweden	LIFO	Hiring selectiveness
Kugler and Gilles (2004)	USA	Wrongful discharge	Hiring & dismissals
Lazear (1990)	USA	Dismissal provisions	Hiring & dismissals
Marinescu (2009)	UK	Wrongful discharge	Hiring & dismissals
Martins (2009)	Portugal	Just cause	Hiring & dismissals
von Below and Thoursie (2010)	Sweden	LIFO	Hiring & dismissals
Häggkvist (2026). [this paper]	Sweden	LIFO	Employer compliance

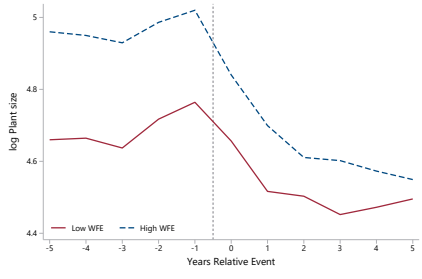
Table A.2. *Descriptive Statistics By Quartiles of Variable of Interest*

	Q1	Q2	Q3	Q4
Earnings	29235.5	30408.0	31318.1	31879.8
	(8952.8)	(11958.4)	(12342.9)	(14095.3)
Wage	28818.3	29852.3	30581.6	31237.2
	(8085.0)	(10405.4)	(10969.6)	(12367.4)
Tenure	107.3	107.9	104.4	113.3
	(51.14)	(59.69)	(61.66)	(60.10)
Age	42.58	42.60	43.10	44.23
	(5.104)	(5.939)	(6.132)	(6.009)
Event size	5.691	6.599	6.557	5.930
	(4.046)	(4.839)	(5.007)	(4.786)
Circuit size	26.44	27.06	27.32	25.11
	(11.21)	(11.56)	(11.49)	(11.64)
Plant size	163.0	184.3	203.0	243.0
	(291.2)	(298.8)	(241.7)	(680.1)
Firm size	1220.7	1456.1	1524.5	1439.3
	(2692.1)	(2522.0)	(2448.9)	(2736.7)
White-collar	0.321	0.397	0.457	0.490
	(0.467)	(0.490)	(0.499)	(0.500)
Private sector	0.970	0.951	0.966	0.959
	(0.170)	(0.216)	(0.182)	(0.197)
Observations	470	469	470	469

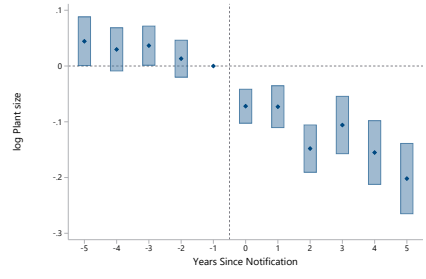
Notes: Table A.2 presents descriptive statistics across quartile levels of ϕ, as defined in Section 4. Q1 denotes the level of worker fixed effects for the junior workers, whereas Q4 is the highest. The values denote the means, standard errors in parenthesis.

Figure A.5. Trends in Plant Size and Labor Earnings

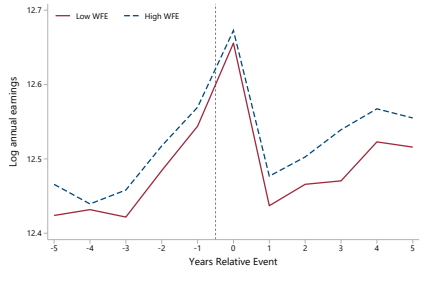
(a) log Plant Size (Raw Levels)



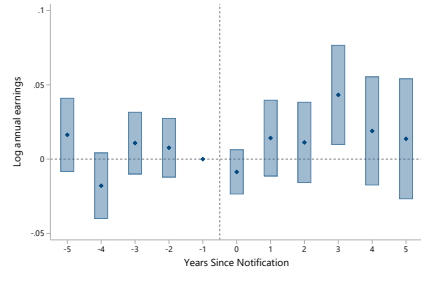
(b) log Plant Size (Regressed Differences)



(c) log Labor Earnings (Raw Levels)



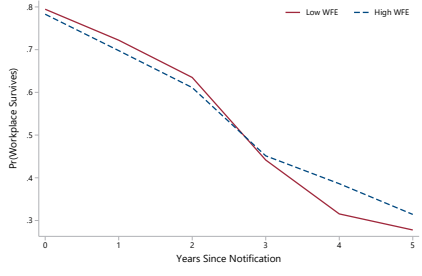
(d) log Labor Earnings (Regressed Differences)



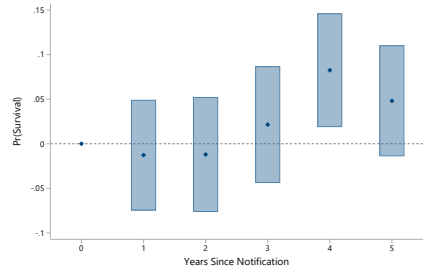
Notes: This figure displays trends in log plant size (top row) and log labor earnings (bottom row) over time in circuits belonging to above-median (solid red line) and below-median ϕ circuits (dashed blue line), as defined in Section 4. Panels (a) and (c) show the raw levels, whereas panels (b) and (d) show the regressed differences. The dashed vertical lines highlight the downsizing year.

Figure A.6. Trends in Survival Rates and Sales

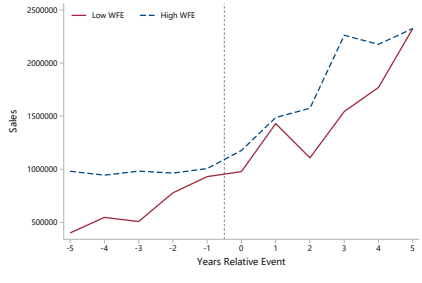
(a) Survival Rate (Raw Levels)



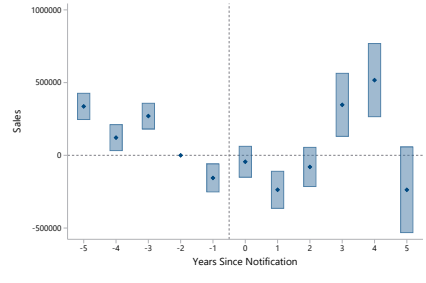
(b) Survival Rate (Regressed Differences)



(c) Sales (Raw Levels)



(d) Sales (Regressed Differences)



Notes: This figure displays trends in firm survival rates (top row) and sales (bottom row) over time in circuits belonging to above-median (solid red line) and below-median ϕ circuits (dashed blue line), as defined in Section 4. Panels (a) and (c) show the raw levels, whereas panels (b) and (d) show the regressed differences. The dashed vertical lines highlight the downsizing year.

Table A.3. *Main Results Quintile Division*

	(1)	(2)	(3)
	Compliance share (notified)	Notification time	Severance per notified worker
Quintile 2	-0.0403	11.40*	19486.3*
	(-1.77)	(2.53)	(2.16)
Quintile 3	-0.108***	13.31**	23915.6*
	(-4.81)	(2.74)	(2.50)
Quintile 4	-0.133***	21.36***	16950.6
	(-6.09)	(4.46)	(1.81)
Quintile 5	-0.135***	22.92***	23531.0*
	(-6.20)	(4.84)	(2.45)
Circuit Size	✓	✓	✓
Event Size	✓	✓	✓
N	1878	1644	1878

t statistics in parentheses

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

Notes: Table A.3 presents the main results using an interacted regression model, where the sample is split by ϕ level quintiles. Quintile 5 represents the highest-productivity junior workers, while Quintile 1 serves as the lowest and omitted category. Robust standard errors are reported.

Table A.4. *Main Results, sector level circuits*

	(1)	(2)	(3)	(4)	(5)	(6)
Compliance	-0.105***	-0.102***	-0.102***	-0.073***	-0.069***	-0.069***
	(0.013)	(0.013)	(0.012)	(0.011)	(0.011)	(0.011)
Notice Time	15.536***	15.527***	15.506***	11.068***	11.086***	10.604***
	(2.558)	(2.558)	(2.555)	(2.575)	(2.512)	(2.529)
Severance	10973.778**	10870.712**	10870.473**	945.036	650.637	256.369
	(4632.861)	(4633.815)	(4635.503)	(4607.743)	(4660.416)	(4669.763)
Circuit Size		✓	✓	✓	✓	✓
Event Size			✓	✓	✓	✓
White Collar				✓	✓	
Industry FEs					✓	✓
Occupation FEs						✓
N	1403	1403	1403	1403	1403	1402

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table A.4 presents the main results across seven different specifications, with each subsequent model adding a covariate control, using sector instead of two-digit occupation as the unit of observation. Circuit size denotes the total number of workers in the circuit, while event size represents the number of notified workers. White-collar is a dummy variable indicating whether the circuit occupation is white-collar or blue-collar. The industry and occupation fixed effects are included at the one-digit level. All models report robust standard errors.

Table A.5. *Outcomes with different controls, GPA*

	(1)	(2)	(3)	(4)	(5)	(6)
Compliance	-0.009	-0.007	-0.007	-0.010**	-0.013**	-0.013***
	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Notice Time	2.915	2.477	2.506	3.336	2.777	2.859
	(2.714)	(2.708)	(2.699)	(2.651)	(2.626)	(2.609)
Severance	-1855.052	-1684.116	-1687.317	-1539.776	-1189.452	-1193.018
	(1413.872)	(1412.924)	(1413.180)	(1411.830)	(1421.301)	(1425.424)
Circuit Size		✓	✓	✓	✓	✓
Event Size			✓	✓	✓	✓
White Collar				✓	✓	
Industry FEs					✓	✓
Occupation FEs						✓
N	1490	1490	1490	1490	1490	1490

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table A.5 presents the main results across seven different specifications, with each subsequent model adding a covariate control, proxying productivity using 9th grade GPA instead of AKM worker effects. Circuit size denotes the total number of workers in the circuit, while event size represents the number of notified workers. White-collar is a dummy variable indicating whether the circuit occupation is white-collar or blue-collar. The industry and occupation fixed effects are included at the one-digit level. All models report robust standard errors.

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