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The Heterogeneous Impacts of Job Displacement: Evidence from Canadian Job Separation Records*

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Abstract

This paper examines the anatomy of mass layoffs using Canadian employer–employee data linked to detailed job separation records. We show that mass layoffs are protracted processes characterized by heterogeneous separations that vary in both reason and timing. Layoffs and quits differ sharply in worker characteristics, earnings losses, and employer-premium dynamics, with layoffs leading to larger and more persistent losses. Layoffs prior to mass layoffs disproportionately involve less-productive workers, who fare worse after separation. Our findings underscore the multidimensionality of mass layoff separations, the strategic behavior of employers and workers around mass layoffs, and the implications for quantitative models of job displacement and policy design.

Keywords: Unemployment, earnings losses, layoffs vs quits
JEL Codes: E24, E32, J31, J65

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1 Introduction

Mass layoffs are events of central importance but remain understudied. They displace large numbers of workers from distressed or restructuring employers, many of whom face large and persistent earnings losses. They are often protracted in nature, implying that both employers and workers can make strategic decisions that shape the timing and selection of separations. This gives rise to heterogeneity in separations that is both cross-sectional and temporal.

Studying mass layoffs requires employer–employee matched data to track large employer contractions and worker-level outcomes within the organization.[1] In such data, job separations are inferred from changes in employer identifiers over time. While this allows researchers to detect large contractions, a common limitation is the lack of detailed information on separations. This limits our understanding of mass layoffs, especially the interaction between reasons for separation *and* their sequencing over time. On the worker side, average estimates of displacement outcomes conceal variation in both the characteristics of separators and their subsequent labor market outcomes. Further, there is limited information on workers' behavior, for instance, whether they preempt layoffs by seeking new jobs. On the employer side, evidence is also limited on how systematic they are in deciding whom to lay off or retain.

This paper uses novel Canadian data to shed light on the inner workings of mass layoffs. We address three key questions. First, what is the composition of separations within a mass layoff, and how do post-separation labor market outcomes vary across workers? Second, what role does strategic behavior by employers and workers play during these episodes, such as who exits first and whether the exit occurs through a layoff or a quit? Finally, do separations from a mass layoff systematically lead to worse outcomes than those outside of a mass layoff?

Understanding the composition, sequencing, and outcomes of mass layoff separations is essential for several reasons. First, existing studies often emphasize the size and sources of average earnings losses, yet such estimates mask variation across worker types and separation contexts. If the burden of displacement falls disproportionately on certain groups or depends on the nature and timing of separation, average effects risk obscuring the margins most relevant for fiscal policy evaluation using quantitative models. Second, it sheds light on how employers decide which workers to lay off and in what order. It also illuminates on the corresponding actions workers might take, such as quitting under the threat of a mass layoff. These insights are crucial for models of job loss that treat separations as entirely exogenous events rather than structured choices. The policy implications are also broad, especially for the design of advance notice requirements and fiscal transfers to support workers whose

[1] While survey data may contain some information on job losses from mass layoffs, they typically suffer from small sample sizes and provide little to no detail on the employers workers leave or join.

prospects may be worsened by searching in a congested market created by mass layoffs.

For this study, we use the Canadian Employer-Employee Dynamics Database (CEEDD), an annual administrative employer-employee matched data. Crucially, we merge this data with Record of Employment (ROE) data, forms which employers are legally obligated to issue during separations and are primarily used to determine eligibility for transfers. The ROE contains data on the start and end dates of employment and the reason for separation. As discussed later, we emphasize that Canada's regulatory environment lends itself naturally to accurate reporting on the ROE. Overall, our data allow us to (i) evaluate the efficacy of the existing strategy in the literature for identifying involuntary separations, (ii) offer guidance to researchers working with datasets with limited information on the nature of separations, and (iii) shed light on the composition, outcomes, and sequencing of mass-layoff separations. We present five key findings that deepen our understanding of mass layoffs.

First, existing mass-layoff identification strategies often mix in spurious separations. In our data, around 45% of identified separations are not job losses but employer ID changes due to reorganization (e.g., M&A). While data limitations force users to adopt these strategies, our results highlight the shortcomings of identifying involuntary separations by relying only on employer ID changes. Using the ROE, we examine the distribution of separations by reason across different concentrated-flow thresholds. We show that a 50% threshold—excluding events where at least 50% of workers flow to the same destination or 50% in a destination come from the same origin—strikes an effective balance between removing spurious flows and retaining sufficient observations. This result provides a useful benchmark to researchers who use similar datasets but without the information on the reason for separation.[2]

Second, separations within a mass layoff are highly varied in composition. Among non-spurious separations, roughly half are layoffs, a quarter are quits, while the rest reflect idiosyncratic reasons such as parental leave, illness, or retirement. Focusing on layoffs and quits, we document substantial heterogeneity in worker outcomes. In particular, the average earnings loss for *all* mass-layoff separators in the Canadian data is 28%, well within the range of existing estimates from administrative data in the U.S. and Europe (18%–46%).[3] However, exploiting ROE information on the reason for separation, we show that involuntary separators face much steeper losses of 54%, compared to just 22% for quits. Our estimates therefore provide a clearer picture of the outcomes facing *involuntary* job separators—precisely the group that existing studies have struggled to isolate due to data limitations.

Third, the sources of earnings losses vary significantly depending on the reason for sep-

[2] Focusing on employer closures, Hethey-Maier and Schmieder (2013) propose a similarly restrictive threshold. We generalize this approach to mass layoffs by leveraging observed reasons for separation.

[3] By contrast, survey-based U.S. estimates of earnings losses in the separation year are somewhat lower, ranging from 14% to 30% (see Von Wachter, Handwerker, and Hildreth 2009 for an in-depth comparison).

aration. Specifically, employer-specific pay premium (á la Abowd, Kramarz, and Margolis 1999, henceforth AKM) losses are larger and more persistent for layoffs than for quits. For layoffs, employer-premium loss explains 26% of short-term and 59% of long-term earnings losses. For quits, the premium is unchanged at separation and 4% higher after six years. Overall, we show that the AKM model with additive worker and employer effects better explains earnings changes for layoffs than for quits. Since AKM estimates rely on an exogenous mobility assumption, we also undertake bias-correction measures as in Bonhomme, Lamadon, and Manresa (2019) and still find the same result.

Fourth, leveraging ROE data on exact separation dates, we examine the sequencing of separations within a mass layoff. We first document that employers experience substantial employment losses several months before the mass-layoff month: around half of quits and a quarter of layoffs occur before that month. We also find sizable heterogeneity in short-term earnings losses for separations with different reason and timing, ranging from 10% for quits nine months after the mass-layoff month to 60% for layoffs nine months before. Among layoffs, those separated before the mass-layoff month experience larger earnings and employer premium losses than those separated after. In terms of worker characteristics, workers who are laid off before the mass-layoff month have lower worker fixed effects, pre-separation earnings, and positions within the original employer's earnings distribution. These findings suggest that employers lay off less-productive workers first during a severe contraction.[4] Among quits, earnings losses are also larger for workers who quit before the mass-layoff month than for those who quit after, but the employer premium changes between the two groups are similar. Importantly, we do not find any evidence that more-productive workers quit and "jump ship" in anticipation of the mass layoff. Instead, workers who quit early tend to have lower pre-separation earnings and worker fixed effects. This can be rationalized by the fact that workers who retain their jobs during the mass layoff enjoy sufficient time to sample offers, wait for favorable job opportunities, and bargain better contracts. Overall, a key implication of these results is that the pool of separators is not entirely random but instead is an outcome of strategic decisions made by employers and workers.

Finally, we compare outcomes for equivalent types of separations occurring within and outside of mass-layoff events, providing new insights into the distinct impact of mass layoffs. Earnings losses outside of mass layoffs are smaller and less persistent, and the gap between quits and layoffs is also narrower than in mass layoffs. Overall, separation outcomes are systematically worse and more dispersed in mass layoffs than in non-mass layoffs.

Our findings show that outcomes differ sharply by both the reason and timing of separations. One might argue that distinguishing between layoffs and quits during mass layoffs is

[4] This result helps explain why employers lay off workers instead of lowering wages (Bertheau et al. 2025).

unnecessary, as quits in the context of employer distress—so-called *"quits under duress"*—could resemble involuntary separations. Yet the stark divergence in post-separation outcomes challenges that view. If such quits were truly involuntary, their outcomes would mirror those of layoffs. This underscores that the reason for separation, even within the same adverse event, conveys important information about post-displacement trajectories. Pushed further, *even* within the same mode of separation (e.g., layoffs), worker types and outcomes vary greatly, particularly when employers are systematic in deciding whom to lay off and when.

Beyond their intrinsic empirical importance, our findings are also relevant for quantitative models that take earnings loss estimates as key inputs to discipline parameters, analyze the mechanisms behind these losses, and evaluate policy (Huckfeldt 2022; Braxton, Herkenhoff, and Phillips 2023; Jarosch 2023). For instance, using smaller estimates understates the role of the job ladder in explaining earnings losses and the insurance value of public transfers provided to job losers. Because employers strategically choose to lay off less-productive workers early—and these workers experience more severe earnings losses and downward movements along the job ladder—providing payroll subsidies to such employers could discourage them from laying off long-tenure workers. Similarly, requiring advance notice could allow employees to proactively change jobs and avoid steep earnings losses (Cederlöf et al. 2025). Our results emphasize the importance of modeling distinct types of separations and allowing for the strategic decisions made by both workers and employers during contractions. Models that incorporate these features would be well-suited to evaluate such labor market policies.

Related literature. A large literature analyzes the consequences of job separations during mass layoffs (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010; Davis and von Wachter 2011; Bertheau et al. 2023, among others). More recently, studies have explored the sources of earnings losses by quantifying the roles of employer-specific pay premia and worker sorting (Lachowska, Mas, and Woodbury 2020; Schmieder, von Wachter, and Heining 2023), the job ladder (Krolikowski 2017; Jarosch 2023), non-participation (Huttunen, Møen, and Salvanes 2011), and occupational switching (Huckfeldt 2022). We contribute to this literature in two ways. First, we document heterogeneous worker outcomes that underlie the average earnings losses. Second, we trace the sources of these disparities to how job ladder dynamics vary across workers, depending on the type and timing of separations.

Another strand of research highlights heterogeneity in displaced worker outcomes across various dimensions such as proximity to mass-layoff events (Schwerdt 2011), reason for separation (Flaaen, Shapiro, and Sorkin 2019), exposure to local shocks and mobility (Gathmann, Helm, and Schönberg 2020), employer size (Fackler, Mueller, and Stegmaier 2021), severity of contraction (Cederlöf 2024), and non-employment duration (Fallick et al. 2025). We build on this work by treating mass layoffs as *protracted* processes involving *multiple types* of separations. Our emphasis is on how outcomes depend on the intersection of the reason and timing of separations, highlighting that heterogeneity is multidimensional—spanning both cross-sectional and temporal dimensions—rather than one-dimensional. Our analysis extends Flaaen, Shapiro, and Sorkin (2019), who use administrative and survey data to distinguish layoffs from quits and show that layoffs cause larger earnings losses. We show that the composition of layoffs and quits during a mass layoff evolves over time. Even conditional on layoffs or quits, worker characteristics and outcomes vary systematically with the timing of separation, making the added temporal dimension key for understanding heterogeneity.

Finally, a separate literature examines how employers restructure, emphasizing the "cleansing" effects of recessions whereby labor reallocation and the shedding of less-productive matches occur (e.g., Caballero and Hammour 1991; Caballero and Hammour 1996; Gomes, Greenwood, and Rebelo 2001). More recent work highlights opposing forces that hinder efficient reallocation, such as slowed job ladders (Barlevy 2002), financial constraints (Barlevy 2003), and the exit of young but productive firms (Ouyang 2009). While these studies offer important insights on the reallocation process, they abstract from the micro-level details of individual separations *within* a contracting employer. Our paper fills this gap by showing who exits during downsizing episodes and how. We provide micro-level evidence that helps explain productivity and reallocation patterns that arise not only between employers but also within. In doing so, our work complements studies of within-employer adjustments, which focus on skill composition (Margolis and Montana 2024; Seim 2019), mutual agreements during separation (Carry and Schoefer 2024), or plant closures (Bender et al. 2002).

The rest of the paper is organized as follows. Section 2 explains our data and empirical methodology. Section 3 presents the heterogeneity in earnings losses and the underlying sources behind these losses. Section 4 examines the sequencing of mass layoffs and the characteristics of workers exiting at different stages of mass layoffs. Section 5 compares outcomes between mass-layoff and non-mass-layoff separations, and Section 6 concludes.

2 Data and empirical methodology

In this section, we introduce our data focusing on its novel aspects, provide details about our sample, and present sample descriptive statistics. We then discuss our methodology in estimating the magnitude of earnings losses upon separations and in decomposing the sources behind these losses. Appendix A provides additional details about the data.

2.1 Canadian Employer-Employee Dynamics Database

Data. We utilize the 2001-2016 Canadian Employer-Employee Dynamics Database (CEEDD), an annual employer-employee matched, longitudinal administrative record of the universe of

Canadian individual and employer income tax filings, with a separate form providing detailed job separation information. The CEEDD links *person*-level tax returns, *employer*-level National Accounts Longitudinal Microdata File (NALMF) records from employer tax returns, and *job*-level returns and Records of Employment (ROE).

On the worker side, the CEEDD provides information on demographics (e.g., age, gender, and province), as well as earnings from *all* jobs. We define earnings as total *annual* pre-tax earnings received from employment.[5] Like most administrative data used in the literature, ours do not contain data on hours worked.[6] In the CEEDD, each worker is linked to each employer from which they derive employment income using employer identifiers. Individual and employer identifiers along with the panel nature of the tax records allow us to identify a worker's tenure at an employer. On the employer side, employer characteristics include size, industry, legal status, and a wide range of income statement and balance sheet variables.

The ROE form is central to this study as it provides detailed information on how job separation circumstances influence worker outcomes. By law, employers are required to issue an ROE whenever there is an "interruption in earnings." These include cases when the worker does not receive any payment for at least seven consecutive days or when the worker's salary falls below 60% of regular weekly earnings.[7] Therefore, an ROE must be issued after all job separations. However, an ROE may be issued without an interruption in earnings due to changes in pay period type, payroll account, business ownership, or name.

The ROE contains information on employer and worker identifiers, exact hiring and separation dates, and the reason behind a job separation. Importantly, this information is available only when an ROE is issued. As such, when an individual separates from an employer and finds a new job, we can obtain information on the exact beginning date of the new job only if the new employer also issues an ROE.[8]

In this paper, we focus on two key dimensions of job separations—reason and timing—and relate them to detailed employer and worker attributes. First, using information on the reason for separation, we can separately identify quits and layoffs within and outside of severe employer contractions, i.e., mass layoffs (defined below). These mass-layoff events are widely used to identify unexpected job separations when estimating scarring effects of displacements. ROE codes cover detailed separation reasons, wherein we focus on two primary reasons—

[5]Earnings are converted to constant 2010 Canadian dollars (CAD) using the CPI (all items).

[6]Several studies have explored the implications of earnings definitions: see Lachowska et al. (2020) and Bonhomme et al. (2023) for a comparison of analysis using earnings and hourly wages.

[7]Two additional details are of note. First, the requirement to submit an ROE under these conditions applies to employees on contract and seasonal workers. Second, M&As may or may not trigger an ROE. When earnings are uninterrupted and the new employer has access to payroll records, an ROE is not required.

[8]An implication of this limitation is that we can calculate the duration of non-employment after a separation only for those who find a new job and also separate from the new job.

layoff and quit.[9] Second, information on the exact separation dates allows us to determine the proximity of a separation from the height of a mass layoff. Timing information allows us to treat episodes of employer contraction as protracted events wherein workers separate over time and potentially in a systematic manner that is related to worker characteristics.

Institutional details. In Canada, separating workers need the ROE form to provide authorities with information to assess eligibility for employment insurance (EI), as well as the benefit amount and duration they are entitled to. These workers are ineligible for regular EI benefits and severance payments based on years of service if they quit the job without just cause. As such, a potential concern with the accuracy of information provided by employers in the ROE form is that employers may have incentives to disguise or misreport a layoff as a quit. This would be especially concerning if the EI program in Canada featured experience rating as in the U.S., where an employer's payroll tax rate would rise with the number of previous employees receiving EI benefits. Facing such consequences, employers possess incentives to reduce the likelihood that their ex-employees' EI applications succeed (Lachowska, Sorkin, and Woodbury, 2022). In the context of submitting the ROE, disguising a layoff as a quit may prevent a worker from receiving EI. Reassuringly, employer EI contributions in Canada have not featured experience rating since 2001, unlike the U.S. system.[10] As such, an employer has less of a motive to misreport a layoff as a quit. Misreporting in the other direction (i.e. disguising a quit as a layoff) is also less likely as employers who do so become liable for paying severance packages. A final safeguard against misreporting is that eligibility for other transfers (e.g., disability, parental leave, and retirement) for former employees is contingent on the reason for job separation. Since the ROE has significant implications for various worker outcomes, employees can appeal or challenge incorrect entries and are protected by employment laws. In fact, misrepresentation of information on ROEs filed faces severe financial penalties, both for EI claimants and employers. While some degree of non-compliance and misreporting is inevitable (e.g., under-the-table arrangements between employees and employers), the information provided in the CEEDD, merged with ROE data, is collected within a favorable institutional framework. This provides an ideal setting for studying differences among workers who lose their jobs during mass layoffs.

[9] ROE includes codes for shortage of work (layoff), strike or lockout, return to school, illness or injury, quit, maternity leave, retirement, work-sharing, apprentice training, dismissal or suspension, leave of absence, parental, compassionate care/family caregiver, and "code K". The "code K" category includes technical cases when there is a change in payroll/ownership or company name, or a change in pay period type.

[10] The EI program in Canada has been financed by contributions shared by employees and employers since 1990. EI premiums are deducted from an employee's insurable earnings, and an employer contributes 1.4 times the employee's contributions. The federal government briefly experimented with experience rating in the 1990s; the reforms were implemented in 1996 but repealed in 2001.

Sample selection. Our sample selection criteria are chosen in a manner consistent with previous work (Jacobson, LaLonde, and Sullivan 1993 and Lachowska, Mas, and Woodbury 2020). This allows for a comparison with prior work before incorporating the ROE data.

We focus on employers with at least 50 employees in any year from 2002–2007, positive employment in 2006 and 2007, and a non-missing industry code in 2007.[11] We limit our sample to the working-age population aged 50 or younger in 2010 and long-tenure workers, defined as individuals who report positive earnings and have been continuously employed by the same primary employer for at least six years.[12] For workers with multiple jobs recorded during the year, we define the primary employer as that which accounts for the highest share of earnings. We restrict the sample to individuals with positive earnings from 2002–2014 and for whom an employer identifier is available. Thus, our estimates reflect the effects of separations on highly-attached workers.[13] Importantly, we explore the impact of relaxing these sample restrictions, presenting our main findings when including workers with zero earnings (Figure A2) or removing workers with low earnings (Figure A3); removing the long-tenure requirement (Figure A4); and excluding all multiple job-holders (Figure A5).

Conventional mass-layoff identification. We now describe how mass layoffs are identified. Here, we closely track the literature and use the same set of criteria that rely only on changes in employer size over time. Following Lachowska et al. (2020), we define a *separator* as a long-tenure worker who is separated from her primary employer at any point during 2008–2010.[14] A separator is classified as a *mass-layoff separator* if the separation occurs in a year during which her primary employer experiences a mass layoff.[15] For the years between 2008 and 2010, a mass-layoff event occurs when (i) an employer experiences an employment drop of 30% or more relative to its 2007 employment and (ii) its 2007 employment does not exceed 130% of 2006 employment. The second condition reduces the chances of classifying

[11] Since mass layoffs are based on percentage changes in employment, a small decline can qualify as a mass layoff for small employers. We therefore focus on those with at least 50 employees.

[12] We also restrict our sample to individuals who are at least 21 years old in 2008, implying that they would have been in the working-age population in 2002. Further, we focus on the years 2002 to 2014 due to an oil price shock in 2015 in Canada that induced highly sector-specific job separations.

[13] Our baseline sample excludes individuals with zero earnings. Since tax filers with zero earnings for an entire year are rare, the original positive earnings restriction primarily excludes workers with missing earnings (i.e., taxes not filed) during any of the years considered. This is likely to occur among informal workers or emigrants. In Appendix B.2, we discuss further issues when including such individuals.

[14] We use the job flow exclusion methodology employed by Benedetto et al. (2007) to filter out employer ID changes associated with M&As, legal restructuring, and cross-establishment movements within a parent organization. A separation is excluded if (i) 80% or more of the origin employer's total workforce moves to the same destination (concentrated outflow), or (ii) over 80% of the destination employer's total employees are new hires from the same origin (concentrated inflow).

[15] The group we label *mass-layoff separators* tend to be what the literature labels as *displaced workers*, defined as workers who experience an involuntary job separation. We retain this distinction throughout this paper as the ROE reveals that not all mass-layoff separators are laid-off workers.

temporary employment fluctuations as mass layoffs (Davis and von Wachter, 2011).[16] Finally, a *stayer* is a worker who remains attached with the same primary employer between 2002–2014. In our analysis, the treatment group consists of mass-layoff separators, while the control group incorporates all job stayers as in Lachowska et al. (2020).[17]

Utilizing ROE data. We now utilize information from the ROE data regarding the reason for job separation. Using the ROE forms for all mass-layoff separators, we first calculate the fraction of mass-layoff separators across different reasons for separation. The first column of Table 1 strikingly shows that 44.3% of mass-layoff separations in our sample are in fact employer identifier changes without an ROE, implying that these observations are more likely to be spurious separations (not related to actual job losses). We provide three additional pieces of evidence to support this claim. First, the last two columns of Table 1 show that separations without an ROE have a significantly higher likelihood of being associated with employer merger, acquisition, and reorganization activities. The values in the column "Outflow" are constructed as follows. For a given mass-layoff separator moving from employer A to employer B, we compute the ratio of total number of individuals who move from employer A to employer B to the total number of employees at employer A. The values reported are the average of ratios across all separators of a given reason for job separation. The column "Inflow" presents statistics for when the ratio's denominator is the total number of employees at employer B. To interpret, among mass-layoff separators without an ROE, an average of 53.9% of all employees from the origin employer move to the same destination employer. Further, on average, workers originating from the same employer as the separator represent 49.8% of all employees at the destination employer.[18] Second, there is a negligible change in average earnings for mass-layoff separators with a missing ROE after 2008 (Figure A14 in Appendix B.3). Third, only a small fraction of such separators receives EI (Table A1). These results highlight that the methodology in the literature to exclude highly concentrated flows from the analysis of mass-layoff separators, as in Benedetto et al. (2007) and implemented

[16]Figure A1 in Appendix B.1 explores the implications of altering the 30% employment drop condition in criterion (i). We show that increasing the employment drop threshold results in a higher share of separations with missing ROEs and those associated with concentrated-flow events. We also consider an alternative sample and adopt definitions of long-tenure workers and mass-layoff events following Davis and von Wachter (2011), without restricting separations to the 2008–2010 period (Figure A6). Finally, we present results using a subset of mass layoff events that involve establishment closures (Figure A9).

[17]We also present results separately for two changes to the control group. First, we restrict the control group to include only stayers who remain employed *within* the same employer from which mass-layoff separators are displaced (Figure A10). Second, we expand the control group to include workers who did not separate from their original employer during 2008–2010 but separated in subsequent years (Figure A11).

[18]While the degree of flow concentration is high among separations with missing ROEs, it is not 100% for several reasons. First, M&As may not involve the entire organization. Second, ROEs may be missing for reasons unrelated to flow concentration (e.g., the use of independent contractors or employer non-compliance).

Table 1: Mass-layoff separations by reason for separation: Summary statistics

ROE reasons	Share (%)	Share (%) of ROE	Average fraction (%) Outflow	Average fraction (%) Inflow
Layoff	25.3	45.5	5.8	8.3
Quit	11.9	21.4	2.0	4.7
Other	18.4	33.1	18.1	17.4
Missing	44.3	-	53.9	49.8

Note: This table presents ROE summary statistics for the mass-layoff separator sample. The first column presents a breakdown of separations by reason for separation, while the second column presents the same breakdown but conditional on separations with ROEs. The third and fourth columns present the extent to which different types of separations are associated with concentrated flows, as described in the main text.

in Section 2.1, is unable to capture all highly concentrated flows. In Section 2.2, we provide guidance on addressing spurious separations for users of admin data without supplemental information like that of the ROE and who thus must rely solely on employer ID changes.

Of all mass-layoff separations, 25.3% are recorded as actual layoffs, accounting for 45.5% of separations with an ROE. More than half of mass-layoff separations with ROE are actually due to quits or other reasons. This implies that, even when focusing on separations with an ROE, the method of identifying involuntary job losses using separations that occur during large employer contractions, i.e., "mass layoffs," as in previous studies, actually captures a large number of voluntary separations. Given ROE information in our data, we are able to identify separations that arise from different reasons. Importantly, in Section 3, we show that this distinction is relevant as we find that estimated earnings and employer-pay premium losses differ greatly across separations with different reasons for separation in mass layoffs.

For the remainder of the paper, we focus on analyzing differences between layoffs and quits in mass layoffs for two reasons. First, separations with missing ROE are often not actual job losses, as evidenced by concentrated flows of workers, negligible changes in average earnings, and limited EI receipt around these events. Second, we also find that separations that we group under "Other" in Table 1 are less likely to be actual separations. This is because around 80% of separations in this group are coded under "code K", a category that is used when an ROE is issued without an interruption in earnings.[19] Other major categories in this group cover idiosyncratic reasons (e.g., pregnancy (6%), injury/illness (5%), going back to school (3%), and retirement (2%)), which we do not intend to differentiate in this paper.

Summary statistics. Before discussing our estimation strategy, Table 2 presents descriptive statistics for mass-layoff separators with different reasons for separation and compare them with stayers. We document several differences across groups. First, relative to stayers,

[19] The "code K" encompasses reasons such as a change in payroll/ownership, company name, or pay period type. This also explains why the "Other" category has larger fractions of concentrated flows than the "Layoff" and "Quit" categories as some of the M&A activities may fall under "code K".

Table 2: Sample descriptive statistics

	Mass-layoff separators			Stayers
	Average	Layoff	Quit	
Worker characteristics				
Average earnings 2002–2005 (2010 CAD)	52,500	51,400	54,800	56,500
Female (proportions)	0.326	0.298	0.386	0.522
Age in 2007 (years)	39.14	40.02	37.29	40.74
	(6.78)	(6.58)	(6.83)	(6.17)
Fraction received EI	0.64	0.79	0.32	0.16
Average EI among recipients (2010 CAD)	12,132	13,800	8,600	8,600
Employer characteristics in 2007				
Employer size (number of workers)	3,755	1,805	7,899	9,575
	(10,744)	(4,219)	(17,253)	(22,469)
One-digit NAICS Industry (proportions)				
1 agriculture, forestry, fishing	0.021	0.027	0.009	0.003
2 mining, utilities, construction	0.041	0.041	0.040	0.040
3 manufacturing	0.620	0.712	0.425	0.189
4 trade, transportation	0.126	0.081	0.221	0.159
5 information, finance, professional services	0.128	0.085	0.220	0.126
6 educational and health care services	0.015	0.012	0.021	0.364
7 arts, recreation, hospitality services	0.035	0.036	0.034	0.019
8 other services	0.005	0.004	0.008	0.015
9 public administration and unclassified	0.009	0.002	0.023	0.085
Number of employers (pre- and post-separation)	20,780	15,065	8,775	12,825
Number of workers	19,410	13,185	6,225	774,075

Note: This table presents descriptive statistics for workers who separate from their jobs during a mass layoff between 2008 and 2010 because of a layoff or quit (or the average across the two reasons) and stayers who remain attached to the same primary employer between 2002 and 2014. Standard deviations are provided in parentheses. The fraction that received EI and average EI amount among recipients are calculated from the year of separation and the year following the separation for mass-layoff separators, while they are calculated from between 2008 and 2009 for stayers. Because of confidentiality, dollar values are rounded to the nearest 100 CAD and counts are rounded to the nearest 5.

laid-off workers had around $5,000$ CAD lower average earnings between 2002 and 2005, while those who quit had much similar earnings. Second, laid-off workers are more likely to be male and older than workers who quit. Third, laid-off workers are more likely to receive EI transfers and to receive larger amounts compared with those who quit.[20] Fourth, the primary employers of laid-off workers are smaller in size than those of workers who quit. Finally, layoffs are highly concentrated within manufacturing, while quits are also prevalent in trade and transportation and in information, finance, and professional services.

2.2 Identifying concentrated flows without the ROE

A prerequisite to estimating the effects of job loss is a sound strategy to identify involuntary separations. Several strategies have been developed to identify involuntary separations

[20] While workers who quit their job without just cause are ineligible for EI, those who quit for just cause (e.g., harassment, discrimination) may qualify. In our sample, 32% of mass-layoff separators who quit receive EI. Furthermore, part-time workers can also collect EI. In our sample, 16% of stayers receive EI.

in administrative datasets. For example, Cederlöf (2024) exploit Sweden's "last-in-first-out" regulations to address selection and misclassification during mass layoffs. By contrast, most studies using standard employer–employee matched data lack such institutional variation and must infer separations from employed ID changes. To account for restructuring or mergers, these studies commonly filter out concentrated worker flows between firms. A well-known approach by Benedetto et al. (2007) excludes cases where at least 80% of an origin's workforce joins the same destination (concentrated outflow) or at least 80% of a destination's workforce originates from the same firm (concentrated inflow). Variants of this procedure exist (see Hethey-Maier and Schmieder 2013), but the underlying principle of tracking concentrated flows is the same. These approaches share a common trade-off: lowering the threshold filters out more spurious separations but also reduces sample size and excludes genuine cases.

Our data allow us to evaluate this sample–accuracy trade-off directly, since the ROE indicates whether a separation was involuntary. For brevity, we validate concentrated-flow exclusion rules using the threshold approach of Benedetto et al. (2007) and Hethey-Maier and Schmieder (2013), while noting that our findings extend to related methods.

Panel (a) of Figure 1 shows the distribution of separations (i.e., employer ID changes) after dropping concentrated flows that exceed each threshold. A notable pattern is the decline in separations with missing ROEs (previously shown to be likely spurious) until around the 50% threshold, after which the share stabilizes.[21] Clearly, a lower threshold also implies a smaller sample of separations, as seen in Panel (a). However, this decline is primarily driven by the removal of separations with missing ROEs. Panels (b) and (c) show that fractions of concentrated outflows and inflows—as defined previously—for reported layoffs and quits are small even at the most relaxed threshold of 100%. The concentration also drops significantly up to the 50% threshold, especially for those with missing ROEs.

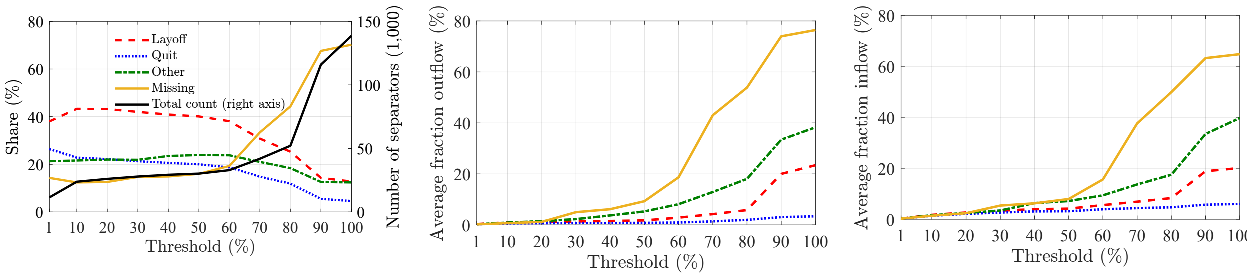
Figure 1 indicates that in the absence of ROE information, adopting a relaxed threshold (e.g., 80% as discussed in Footnote 14) to identify concentrated flows results in the inclusion of a large number of spurious separations. In our data, a more restrictive cutoff of 50% appears to be ideal when the researcher does not have separation information.[22] This finding supports a stricter criterion as in Schmieder et al. (2023), who consider mass layoffs where at most 20% of workers move to the same destination employer. Figure A7 shows that earnings losses become larger under the 50% threshold, but results remain similar when conditioning on the reason for separation. Figure A8 provides further results under different thresholds.

[21] Even at the lowest thresholds, over 10% of mass-layoff separations still have missing ROEs. This may be due to various factors, such as exemptions from ROE issuance for workers classified as part-time, on-call, or casual. This may also reflect a rate of non-compliance among employers.

[22] Hethey-Maier and Schmieder (2013), in the context of establishment closures, explore various cutoffs for the concentrated outflow definition we use and similarly find a restrictive 30% threshold.

Figure 1: The impact of varying threshold for concentrated flows

(a) Separation count and reasons (b) Average outflow (c) Average inflow



Note: For each threshold tested, Panel (a) presents the distribution of separations after concentrated flows that surpass the threshold are dropped (left axis) and the corresponding total number of separations remaining (right axis, solid black line). Panel (b) presents the fraction of the total workforce of an employer who exit and transition into the same employer, averaged over each mass-layoff separator (outflow). Panel (c) presents the ratio of new hires who originate from the same employer to the total workforce of the destination employer, averaged over each mass-layoff separator (inflow).

2.3 Estimating the consequences of job separation

We now discuss our methodology to estimate earnings losses upon separations during mass layoffs. We then explain how we use the AKM model to estimate the importance of employer and match effects in driving these earnings losses.

To estimate the scarring effects of job separation on earnings, we follow Jacobson et al. (1993) and Lachowska et al. (2020) and use a distributed lag regression:

$$y_{i,t} = \alpha_i + \zeta_t + \beta x_{i,t} + \sum_{s \in S} \sum_{k=-4}^{6} d^s_{i,t,k} \times \gamma^s_k + \varepsilon_{i,t}. \tag{1}$$

Here, $y_{i,t}$ denotes the log of annual earnings of individual i reported in year t, while α_i and ζ_t capture individual and time fixed effects, respectively.[23] Further, $x_{i,t}$ is a vector of individual and primary employer characteristics including a quadratic on individual's age, interactions between gender and age, interactions between year dummies and worker's average earnings (over 2005–2007), as well as primary employer size in 2007 and its one-digit employment industry (NAICS) code.[24] The vector of dummy variables $d^s_{i,t,k}$ indicates that the worker at year t is observed k years before, on, or after a separation. For example, $d^s_{i,t,3} = 1$ if year t is three years after a mass-layoff separation for individual i and zero otherwise.

Our main interest lies in the estimates of γ^s_k, which are estimated percent differences in annual earnings between mass-layoff separators and stayers for the four years preceding the separation ($k = -4, -3, -2, -1$), for the year of the separation ($k = 0$), and for every year until six years after the separation ($k = 1, 2, ..., 6$).[25] Under the assumption that, absent

[23] A potential concern with the use of log earnings as our outcome variable is that very low earnings will translate to very large (log) earnings losses, where making a percent interpretation inappropriate. To ameliorate concerns around this, we re-estimate this regression on a sample that exclude low-earners and show that our main findings stand (Figure A3).

[24] Controlling for pre-displacement average earnings was also employed by Davis and von Wachter (2011) and aims to capture differential trends in earnings for separators and stayers.

[25] For each separator during a mass layoff, we assume that separation happens a year prior to an employer

separation, average earnings of mass-layoff separators would be parallel to those of stayers, estimated γ_k^s is interpreted as the causal effect of separations. Figure A12 in Appendix B.2 relaxes the parallel trends assumption by modifying Equation (1) to allow for worker-specific linear time trends. We find that this change does not materially alter our main findings.

In our analysis, the set S can take several forms. In the absence of ROE information, $d_{i,t,k}^s$ would simply refer to the occurrence of a mass-layoff separation. In this case, the estimated coefficients-of-interest γ_k^s would reduce to γ_k and represent regression-implied differences in earnings outcomes between mass-layoff separators and stayers. Such estimates would be comparable to those estimated in the existing literature. However, unlike previous studies, we separately estimate differences in outcomes between separators and stayers depending on reasons or timing of the separation. As such, when coefficients differ by subgroup s, we interact separation dummies with dummies indicating membership in $s \in S$.

Finally, the changes in earnings upon job separation can then be decomposed into three main components as in Lachowska et al. (2020): those that arise from changes in employer-specific pay premium, match effect, and a residual effect.

Employer-specific pay premium. A rich literature analyzes the role of employers in determining earnings differences across workers (Abowd et al., 1999; Card et al., 2013; Card et al., 2016; Sorkin, 2018; Song et al., 2019). Following this literature, we estimate an AKM model using our data and use the estimated employer effects to measure the fraction of earnings losses accounted for by the loss of employer-specific pay premium.

We identify employer-specific time-invariant effects on earnings, termed "employer-specific pay premium," using the following Abowd et al. (1999) (AKM) regression:

$$y_{i,t} = \kappa_i + \psi_{j(i,t)} + \lambda_t + v_{i,t}. \tag{2}$$

We regress the log of annual earnings $y_{i,t}$ on individual fixed effects κ_i, year fixed effects λ_t, and employer fixed effects $\psi_{j(i,t)}$, where $j(i,t)$ denotes individual i's primary employer in year t. For this regression, following the literature, we use a different sample, restricting the full database of earnings between 2001 and 2016 to exclude (i) stayers and all separators, including the mass-layoff separators as defined above, (ii) earnings in the first or last year of an employment spell, (iii) earnings below 400 times the national average minimum hourly wage, and (iv) employers with less than 5 employees in that year.[26]

identifier change for that individual. This assumption is not important for estimates presented throughout the paper; it only shifts estimates one year before and after separations.

[26]These sample restrictions are similar to those made in Card et al. (2013), Sorkin (2018), Song et al. (2019), and Lachowska et al. (2020). The first restriction is imposed to avoid a mechanical relationship between employer effects and earnings losses of mass-layoff separators that would potentially overstate the role of employer effects. A potential drawback of this assumption is that if mobility decisions are not exogenous, AKM estimates derived from a different sample may be unsuitable for application to our main

Estimation of Equation (2) yields a vector of employer-specific premiums $\widehat{\psi}_j$ for log earnings, which represent time-invariant employer characteristics such as compensation policy. Following the interpretation of Card et al. (2013), $\widehat{\psi}_j$ represents a measure of pay advantages associated with being employed by a particular employer j.[27] We assign $\widehat{\psi}_j$ to each worker-year observation whenever possible and use them as outcomes of job separations.[28] We then estimate the effect of separations on employer effects, in a manner similar to Equation (1):

$$\widehat{\psi}_{j(i,t)} = \alpha_i + \zeta_t + \beta x_{i,t} + \sum_{s \in S} \sum_{k=-4}^{6} d^s_{i,t,k} \times \gamma^s_k + \varepsilon_{i,t}, \tag{3}$$

where variables in the right-hand side are identical to those in Equation (1).

Match effects. We estimate match effects as time-invariant worker–employer fixed effects following Woodcock (2015) and Lachowska et al. (2020). These can be interpreted to reflect changes in a worker's productivity across employers due to, say, differing work arrangements. Specifically, we first compute average log earnings $\overline{y_{ij}}$ for each worker–employer pair (i, j) over the match.[29] We then regress these average earnings on worker θ_i and employer $\xi_{j(i,t)}$ fixed effects, weighting by match duration. Formally,

$$\overline{y_{ij}} = \theta_i + \xi_{j(i,t)} + \mu_{ij},$$

where the error term μ_{ij} is assumed orthogonal to worker and employer fixed effects. The residuals $\widehat{\mu}_{ij}$ capture the component of earnings attributable to time-invariant match effects, averaged over the match duration after controlling for worker and employer effects.

We estimate match effects from this equation using our sample to estimate the AKM model described above except that we keep stayers and all separators because the match effects are individual-specific. We then estimate the impact of separations on match effects where we use $\widehat{\mu}_{ij}$ as the dependent variable in Equation (3).

For our baseline results, we follow the approach of Lachowska et al. (2020) in estimating

displaced worker sample, particularly when assessing the role of AKM effects in explaining earnings losses. Figure A13 in Appendix B.2 addresses this concern by re-estimating employer-specific premia using a sample that does not remove stayers and mass-layoff separators. We find that including mass-layoff separators leads to larger employer premium losses, suggesting that workers transitioning out of employers experiencing mass layoffs indeed introduce a downward bias in AKM estimates. The second restriction is imposed to eliminate non-full-year earnings from an employer. Finally, the last two restrictions are imposed to drop workers with very little earnings so that we do not incorporate the logarithm of very small amounts and to drop employers with few workers so that employer effects are estimated for a reasonably-sized employers.

[27] While we do not focus on variance-covariance estimates from the AKM specification, in Appendix B.4, we report results on worker mobility in Canada to mitigate concerns on limited mobility bias.

[28] We cannot assign employer effects when, for example, a separated worker is reemployed by an employer that does not belong to the "connected set" used to estimate employer effects or that has less than 5 employees. However, such cases occur in less than 0.1% of all observations.

[29] As in Lachowska et al. (2020), we net out year and tenure effects from the average of log $\overline{y_{ij}}$: year effects are removed, the adjusted outcome is regressed on tenure and worker-employer match indicators, and the tenure component is subtracted from the outcome variable before averaging within matches.

employer and match effects using the AKM and Woodcock estimators, respectively. This allows for a direct comparison of our results with their findings. However, two comments on our approach are in order. First, we acknowledge that a well-known critique of the AKM procedure is its reliance on the assumption of exogenous mobility (see Card et al. 2013). To address this concern, we provide alternative results in Appendix C.1 and C.2, employing the methodology developed by Bonhomme et al. (2019). Furthermore, we extend the bias-correction process to account for bias arising from time-invariant match effects. Second, as discussed in Section 2.1, our data allow us to analyze annual earnings but not hourly wages. While it is common to run AKM regressions on hourly wages, many papers analyzed the employer component of annual earnings (Card et al. 2013, Sorkin 2018, and Song et al. 2019). In addition, Lachowska et al. (2020) and Bonhomme et al. (2023) made comparisons between employer premia based on both measures and find similar results.

3 Earnings loss heterogeneity in mass layoffs

In this section, we first document earnings and employer effects outcomes for *all* mass-layoff separations, providing a benchmark to prior estimates. We then show that both the size and the sources of earnings losses differ sharply between separation types within mass layoffs.

3.1 Outcomes for all mass-layoff separations

We start by estimating separation outcomes during mass layoffs on log earnings (Equation (1)) and employer-specific pay premiums (Equation (3)). The objective here is to establish a benchmark consistent with the literature by retaining the same approach to mass layoff identification and intentionally excluding ROE information. Accordingly, in Section 3.1, S contains only the mass-layoff separator sample, including those with missing ROEs.

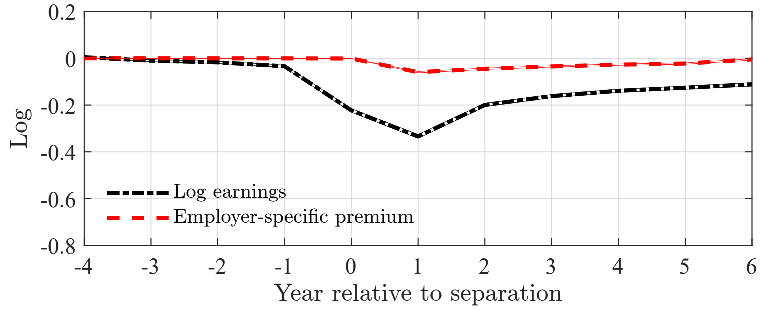
The black line in Figure 2 plots the effects of mass-layoff separations on earnings, i.e., the estimated γ_k values. We find that the average earnings of mass-layoff separators start declining one year before the separation, consistent with the findings in the literature.[30] We also find that the average earnings loss in the year following the separation was 28% ($\exp(-0.33) - 1 = -0.28$ or 33 log points) during the 2008–2010 episode in Canada. The earnings of separators remain 11% lower even after six years past the job separation.[31] These results fall within the range of existing estimates from admin data in the U.S. and Europe.[32]

[30]This is often called the "Ashenfelter's dip," where separators' earnings decline even before separation. In addition, in Section 4, we will show that a sizable fraction of separations occur prior to the year of mass-layoff, leading to a decline in the average earnings of mass-layoff separators even before the mass layoff.

[31]Figure A15 in Appendix B.5 presents estimated earnings losses upon separations in mass layoffs in 2010 CAD. The average earnings of all mass-layoff separators drop by around 7,800 CAD in the year following the separation and remain around 5,000 CAD lower six years after the separation.

[32]This result provides some reassurance that the substantial heterogeneity documented in Section 3.2 does

Figure 2: Effects of job separation among mass-layoff separators



Note: This figure plots estimates for earnings and employer-specific pay premium losses for all job separations during mass layoffs. The dashed-dotted-black line shows estimated γ_k values from Equation (1), while the dashed-red line presents estimated γ_k values from Equation (3). 95% confidence intervals are given by solid lines.

According to existing estimates, earnings losses range between 18% and 46% in the year of separation and between 10% and 30% five years after (Jacobson et al. 1993; Couch and Placzek 2010; Davis and von Wachter 2011; Lachowska et al. 2020; Schmieder et al. 2023).

A growing literature examines employer-specific pay premiums in mass-layoff earnings losses. Lachowska et al. (2020) attribute 6% of short-term losses and between 9% and 17% of long-term losses to employer effects, whereas Schmieder et al. (2023) find a much larger role, with 75% of wage losses explained by declining employer effects. Our results from Canadian data indicate that the change in employer effects are in between these two estimates, as shown by the red line in Figure 2. Lost employer effects account for 18% of the earnings loss in the year after separation: 6 of the 33 log points are due to reemployment with lower-paying employers. However, we also find that employer effects almost fully recover six years after the separation, accounting for only 4.5% of earnings losses by this point.[33]

3.2 Differences in outcomes among layoffs vs quits

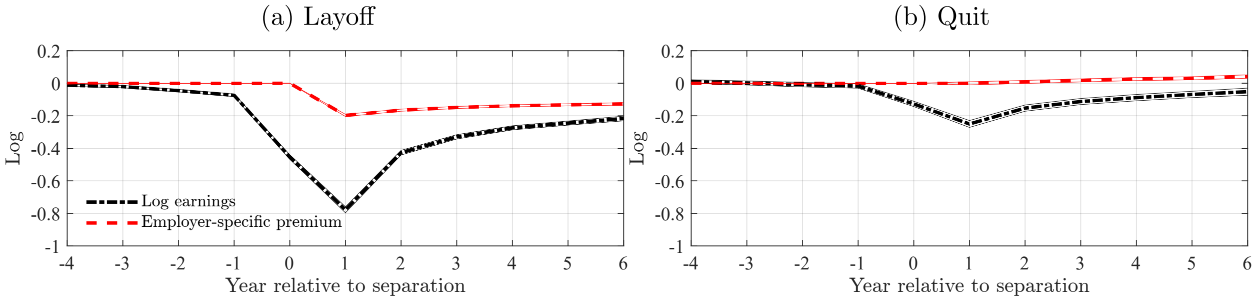
Earnings and employer-effect dynamics. Having presented our benchmark estimates of the consequences of job separation, we now examine the extent to which this average may mask differential scarring effects of displacement between layoffs and quits. Specifically, we estimate the effects of a mass-layoff separation on log earnings (Equation (1)) and on the employer premia (Equation (3)) for separations types $s \in S = \{\text{layoff, quit}\}$.

The black lines in Panels (a) and (b) of Figure 3 plot earnings dynamics for individuals who are laid off or who quit during mass layoffs, respectively. We find significant differences between the two groups. In the year following a separation, laid-off workers experience an average earnings loss of 78 log points, while those who quit experience a loss of only 25 log

not simply arise from differences in the source of data.

[33]In general, employers play an important role in shaping earnings inequality in Canada. Gee et al. (2020) document that 40% of the total earnings variance is explained by between-firm earnings variance. They also show that between-firm earnings variance is constant after 2000 in Canada. Li et al. (2020) report that AKM employer effects explain 11% of log earnings variance in Canada.

Figure 3: Effects of job separation by reason for separation



Note: This figure plots estimates for earnings and employer-specific pay premium dynamics upon job separation by reason of separation during mass layoffs. Panels (a) and (b) present estimates for layoffs and quits, respectively. Dashed-dotted-black lines show estimated γ_k^s from Equation (1), while dashed-red lines represent estimates from Equation (3). 95% confidence intervals are given by solid lines.

points. Six years post-separation, the first group's earnings remain 22 log points lower, while the second group's are only 5 log points lower.[34] Thus, involuntary job separators experience substantially larger and more persistent earnings losses than what is implied by estimates both in the literature and in Section 3.1 that mix a variety of separations stemming from layoffs, quits, or idiosyncratic reasons, as well as non-separation events.[35]

Importantly, Figures A2 to A13 reconstruct our results in Figure 3 under alternative samples, details on identifying mass layoffs, control groups, and model specifications.[36] As we summarize in Section 3.4, the layoff-quit gap remains similar across all these exercises.

We further show that the dynamics of employer-specific pay premium are also drastically different for both types of separations within the mass-layoff separators sample. While laid-off workers face significant and long-lasting losses in employer-specific premium, those who

[34]Figure A15 in Appendix B.5 presents these earnings-loss estimates in levels of 2010 CAD. For quits (layoffs), the average earnings drop by about 7,300 CAD (21,600 CAD) in the year following the separation and remain around 1,800 CAD (9,900 CAD) lower six years after.

[35]Using data from Italy and Spain, both of which include information on the reason for separation, Bertheau et al. (2023) compare earnings losses for two groups: (a) all mass-layoff separations and (b) all involuntary separations. They find similar post-separation earnings dynamics for both, concluding that mass-layoff separations, as a whole, are representative of the effects of displacements. While we find a similar pattern in our data for these two groups, we emphasize two key nuances. First, as shown in Section 5, layoffs occurring outside of mass layoffs are associated with lower earnings losses. While group (a) includes separations that attenuate average losses (e.g., quits or spurious separations), group (b) does so *but* for a different reason: it includes layoffs from non-mass-layoff contexts, where losses tend to be lower. Second, even if average outcomes across the two groups are similar, our findings show large heterogeneity *within* mass layoffs. In particular, outcomes differ markedly between voluntary and involuntary separations. Focusing only on average effects masks this variation, which is relevant for both model calibration and policy design.

[36]We present results for the following cases: (i) including zero-earners, (ii) excluding low-earners, (iii) relaxing the long-tenure requirement, (iv) excluding multiple-job holders, (v) implementing an alternative definition of mass-layoff events as in Davis and von Wachter (2011), (vi) using alternative exclusion thresholds for concentrated flows, (vii) focusing only on employer closures when identifying mass-layoff events, (viii) comparing outcomes of mass-layoff separators to stayers within the same employer, (ix) expanding the control group to include workers who separate after 2010, (x) incorporating heterogeneous (worker-specific) trends in Equation (1), (xi) expanding the AKM sample to include mass-layoff separators and stayers, and (xii) estimating employer effects under a bias-correction procedure following Bonhomme et al. (2019).

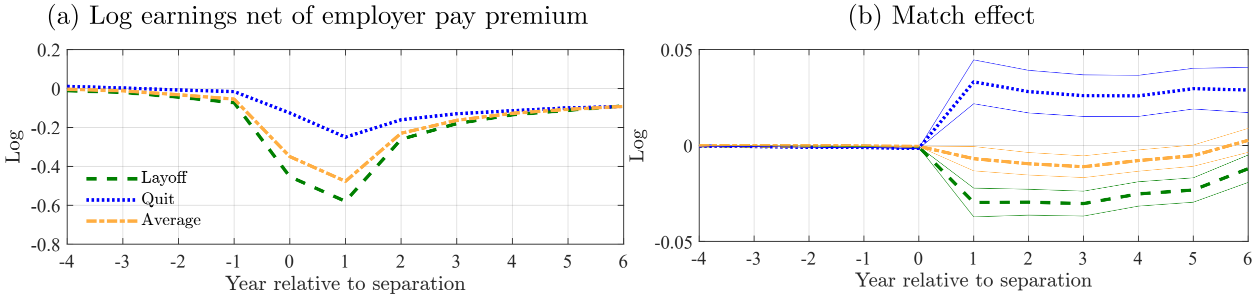
quit see *no* short-term decline and even a long-term *gain*, as shown by the red lines in Figure 3. For workers who are laid off, the average employer-specific premium is 20 log points lower in the year following the layoff and 13 log points lower six years after. These imply that the loss of employer-specific premium accounts for 26% (20/78) of earnings losses in the short term and 59% (13/22) in the long term. In contrast, for those who quit, the employer premium remains unchanged in the first year and rises by 4 log points after six years.

A growing literature (Card et al., 2013; Card et al., 2016; Barth et al., 2016; Sorkin, 2018; and Song et al., 2019, among others) has established that employer effects are important for explaining earnings differences across workers. More recently, Lachowska et al. (2020) estimated a limited role of employer effects in explaining earnings losses of mass layoff separators in Washington state, while Schmieder et al. (2023) conclude that they play a significant role in German data. We contribute to this literature by showing that even *within* a common sample of mass-layoff separators, the role of employer effects can vary substantially depending on the nature of separation. For workers who are laid off, the loss of employer-specific premium accounts for a significant share of earnings losses. For workers who quit, by contrast, employer effects explain little of the observed decline in earnings. This pattern highlights the heterogeneity in post-displacement job ladder dynamics: Laid-off workers are more likely to move to lower-paying employers, resulting in substantial losses in employer-specific pay premia, while quitters tend to transition to employers of comparable or higher pay. Our findings emphasize that the contribution of employer effects to earnings losses is not uniform, even among workers who separate from the same contracting employers.

Sources of earnings losses. To better understand the reasons behind differences in earnings losses from layoffs and quits among mass-layoff separators, we present the dynamics of log earnings *net* of employer-specific premium in Panel (a) of Figure 4. Four years post-separation, the earnings net of employer premia for both groups converge, suggesting that long-term differences in earnings losses are mainly due to employer effects. In terms of the gap in the year following the separation, employer effects account for 20 log points of the total 53 log points gap ($78 - 25 = 53$), given that the gap in log earnings net of employer effects between the two groups is 33 log points ($58 - 25 = 33$). Put differently, 38% (20/53) of the earnings gap one year post-separation is due to differences in employer effects. Meanwhile, Panel (b) shows that match effects result in a 3-log-point increase in average earnings for quits and a 3-log-point decline for layoffs. This implies that individuals who quit gain time-invariant worker-employer match effects, while those who are laid off lose them.[37]

[37]Figure A16 in Appendix B.6 shows a complete decomposition for the sources of earnings losses.

Figure 4: Underlying reasons behind earnings losses for layoffs and quits



Note: This figure plots the dynamics of log earnings net of employer-specific pay premium (Panel (a)) and match effects (Panel (b)) upon a mass layoff separation, by reason of separation. Dashed-green lines show estimates for layoffs, dotted-blue lines for quits, and dashed-dotted-orange lines for the average of both. 95% confidence intervals are given by solid lines in Panel (b).

Taking stock. We find larger and more persistent earnings losses from involuntary job loss compared with existing estimates. The earnings loss gap between layoffs and quits is attributable to the disproportionately large loss of employer effects among layoffs. In contrast, while match effects amplify this gap, their quantitative effects are small.

3.3 Can quits during mass layoffs be treated as involuntary?

We emphasize a key implication of the large heterogeneity in outcomes between workers who are laid off and those who quit. The stark differences in post-separation earnings and employer effects dynamics challenge the common assumption that quits during mass layoffs can be treated as effectively involuntary, or that they can be reasonably pooled with layoffs. In practice, including both groups in a single mass-layoff separations sample masks important variation and produces average earnings and employer effect losses that do not capture the experience of displaced workers. The manner of separation matters: Layoffs are associated with substantially worse outcomes and more accurately capture the notion of involuntary displacement. In contrast, quits experience significantly less severe outcomes.

3.4 Robustness and alternative specifications

The sample selection, mass-layoff identification, and model specification adopted in the main analysis above follow Lachowska et al. (2020) and much of the existing literature to ensure comparability. Before proceeding to further analysis, we explore the implications of adopting alternative approaches and criteria. This section summarizes a range of robustness checks and briefly discusses that our main findings are preserved.

Alternative samples. We first consider relaxing the baseline restriction that requires continuous positive earnings throughout the panel. Including workers with zero earnings in certain years (Figure A2) introduces some compositional changes, but results remain broadly similar to the baseline. Conversely, excluding low earners—defined as those with annual earnings below 400 times the minimum hourly wage—reduces the magnitude of earnings losses by about 20 log points (Figure A3), though the earnings loss gap between layoffs and quits remains robust. Relaxing the long-tenure requirement of six years of attachment to an employer before 2008 (Figure A4) and excluding multiple-job holders (Figure A5) also lead to modest changes in magnitudes but preserve the differences between layoffs and quits.

Alternative mass-layoff identification. We test a broader definition of mass layoffs following Davis and von Wachter (2011), which allows separations to occur beyond 2008–2010 and adopts a multi-period criterion (Figure A6). The results are similar to the baseline, with persistent losses concentrated among laid-off workers. Additionally, we explore the implications of changing other criteria in the mass-layoff identification procedure. Using a stricter 50% exclusion threshold for concentrated flows results in larger observed earnings losses for all mass-layoff separators (Figure A7), confirming that spurious separations linked to missing ROEs may downward-bias loss estimates. However, when conditioning on the reason for separation (e.g., layoff or quit), changing the exclusion threshold has little impact on estimated outcomes (Figure A8). This highlights an important implication: the exclusion threshold is considerably more consequential in datasets that lack information on the reason for separation and therefore rely on procedures to exclude spurious separations. By contrast, when the reason for separation is observed, as in our case, the exclusion threshold plays a more limited role. Finally, restricting the sample to separations due to employer or plant closures (Figure A9) yields slightly smaller losses for layoffs and slightly larger ones for quits, consistent with a stronger negative selection in layoffs where the employer does not close.

Alternative control groups. Next, we examine two changes to the control group: We compare separators to stayers within the same employer (Figure A10) and allow the control group to include workers who did not separate during 2008–2010 but eventually separated (Figure A11). These changes also preserve the core differential between layoffs and quits.

Model specifications. We incorporate heterogeneous individual trends into the regression model (Figure A12) to account for potential bias from differential earnings growth. We find that such bias does not materially affect our estimates. We also re-estimate employer effects using a sample that includes mass-layoff separators (Figure A13). This adjustment leads to slightly larger losses, consistent with greater selection, but does not alter the main findings.

Addressing endogenous mobility bias. Finally, while AKM estimates enable comparisons with prior work, we also present our results in Appendix C.1 and C.2 under a bias-correction procedure as in Bonhomme et al. (2019). Using models where worker and employer effects are either additive or interactive, we find similar results (Figures A17 to A21).

Table 3: Below-, on-, and above-diagonal sums and averages

	Below diagonal	On diagonal	Above diagonal
(a) Layoff			
Share of separators	0.468	0.370	0.164
Average change in log earnings	-0.332	-0.017	0.159
Average change in employer effect	-0.414	-0.002	0.303
Average change in match effect	0.037	-0.050	-0.167
Average residual effect	0.046	0.034	0.023
(b) Quit			
Share of separators	0.288	0.380	0.335
Average change in log earnings	-0.097	0.139	0.277
Average change in employer effect	-0.360	0.017	0.365
Average change in match effect	0.191	0.048	-0.130
Average residual effect	0.073	0.074	0.042
(c) Average			
Share of separators	0.410	0.373	0.219
Average change in log earnings	-0.279	0.034	0.217
Average change in employer effect	-0.402	0.004	0.334
Average change in match effect	0.071	-0.018	-0.149
Average residual effect	0.052	0.047	0.033

Note: This table presents five rows for separations with a different reason (layoff, quit, or an average of both) with below-, on-, and above-diagonal transitions: (i) the fraction of separators, (ii) average change in log earnings, (iii) average change in employer effects, (iv) average change in match effects, and (v) average residual effects of the transition. Below (above)-diagonal transitions are moves to an employer with a lower (higher)-quintile employer effects and on-diagonal transitions are moves to a same-quintile employer. Values are based on a comparison of outcomes between one year before and three years after separation.

3.5 Cross-sectional differences in earnings losses

Overview. In this section, we investigate the reasons behind the gaps in post-separation outcomes between layoffs and quits. Section 3.2 established that employer effects play an important role in explaining the difference in earnings losses between the two groups. The following discussion provides further insights on the divergence of post-separation outcomes when separators are subdivided into different segments of the employer premium ladder. In particular, we assign each employer into employer premium quintiles based on their AKM estimates. Each separation is then assigned to one of 25 quintile-to-quintile transitions based on the separator's origin and destination employer quintiles. In doing so, we compare outcomes between one year before and three years after the separation, as in Card et al. (2013) and Lachowska et al. (2020). This allows us to calculate the average change in earnings and underlying changes in employer, match, and residual effects for each of the 25 transitions for all separations, and separately for individuals who are laid off and who quit.

Table 3 compares outcomes between layoffs and quits by focusing on below-, on-, and above-diagonal transitions. Next, Figures 5 and 6 present results on quintile-to-quintile transitions, including transition shares, earnings losses, and their sources.

Below-, on-, and above-diagonal transitions. In Table 3, below-diagonal transitions represent moves to a destination employer in a lower employer-effects quintile; on-diagonal and above-diagonal transitions represent moves to a same-quintile employer and to a higher-quintile employer, respectively. Five statistics (rows) are presented based on separation reason (layoff, quit, or average of both) and transition type (below-, on-, or above-diagonal): the fraction of separators, the average changes in log earnings, employer effects, and match effects, and the average residual effects. For instance, among individuals laid off in our mass-layoff separators sample, 46.8% found reemployment with an employer in a lower employer-effect quintile than their original employer. These individuals experienced an average of 33.2 log points earnings loss, and this loss was largely driven by a loss of employer effects (41.4 log points) but partially mitigated by gains of match effects and residual effects (3.7 and 4.6 log points, respectively).[38] We highlight three main observations from Table 3.

First, laid-off workers (Panel (a)) are more likely than quitters (Panel (b)) to transition into lower employer-premium quintiles. Almost half of those laid-off move to lower-paying employers, while this fraction is less than 30% for quits. Since laid-off workers dominate below-diagonal transitions, the changes in earnings and its sub components (Panel (c)) for the average below-diagonal separation closely mirror that of layoffs. In contrast, the average earnings changes of above-diagonal transitions are closer to those who quit.

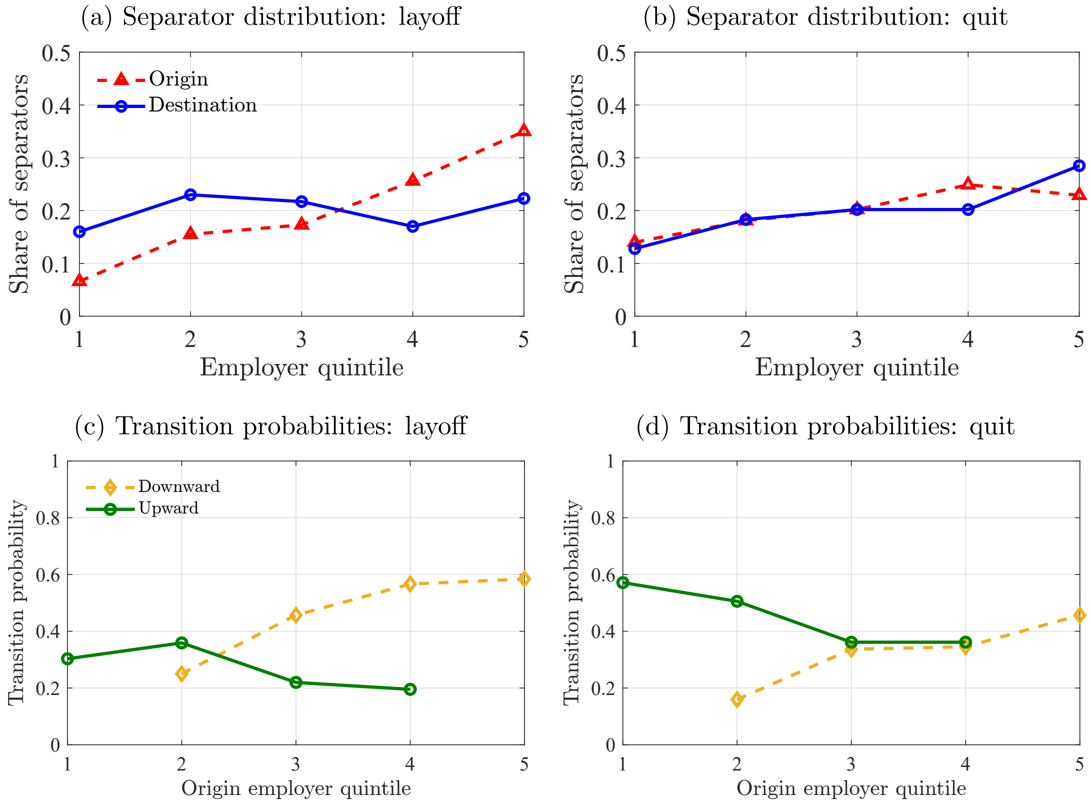
Second, conditional on falling into lower employer-premium quintiles, workers who are laid-off experience much larger declines in earnings than those who quit (33.2 vs. 9.7 log points). Importantly, we find that losses of employer effects are comparable for layoffs and quits with a below-diagonal transition (41.4 vs. 36.0 log points) and that the smaller loss in earnings for quits is mostly driven by a larger average gain in match effects (3.7 vs 19.1 log points), mitigating lost employer effects. This means that when workers quit and are reemployed at a lower-paying employer, they are compensated by better matches at the new employer, which may reflect a closer skill fit or improved contracts that enhance productivity.

Finally, conditional on rising along employer-premium quintiles, quits experience larger increases in earnings (15.9 vs. 27.7 log points) relative to layoffs. However, this gap is smaller for upward movements than for downward ones. Above-diagonal transitions show substantial gains in employer effects and losses in match effects, with employer premium gains slightly larger and match effect losses slightly smaller for quits than layoffs. These results imply that, after a quit or layoff during a mass layoff, moving to a higher-paying employer is associated with an accompanying loss of worker-employer specific match quality.

Analyzing outcomes conditional on transitions across employer-effect quintiles also turns out to be relevant for understanding the average of outcomes for layoffs and quits presented in Figures 3 and 4 previously. We highlight two key results regarding this conclusion. First,

[38] These estimates may not match our results in Figures 3 and 4. This is because the results in this section are simple averages and not obtained from regressions where stayers are the control group.

Figure 5: Transitions across employer effect distribution



Note: Panels (a) and (b) plot the distribution of separations by origin (dashed-red) and destination (solid-blue) employer effect quintiles for layoffs and quits in mass layoffs, respectively. Panels (c) and (d) present upward (solid-green) and downward (dashed-orange) transition probabilities by origin employer effects quintiles for layoffs and quit, respectively. Values are based on a comparison of outcomes between one year before and three years after separation.

while Figure 3 shows that employer effects are *small* on average for those who quit, Table 3 documents that this result masks substantial heterogeneity. Below-diagonal quits experience a large decline in employer effects (36.0 log points), while above-diagonal quits experience a large increase in employer effects (36.5 log points). Second, recall from our results in Figure 4 that match effects on average are positive for quits and negative for layoffs but small in magnitude. Table 3 again reveals substantial heterogeneity in match effects for both layoffs and quits based on transitions across employer-effect quintiles. Workers with below-diagonal transitions gain match effects, while those with above-diagonal transitions lose them.

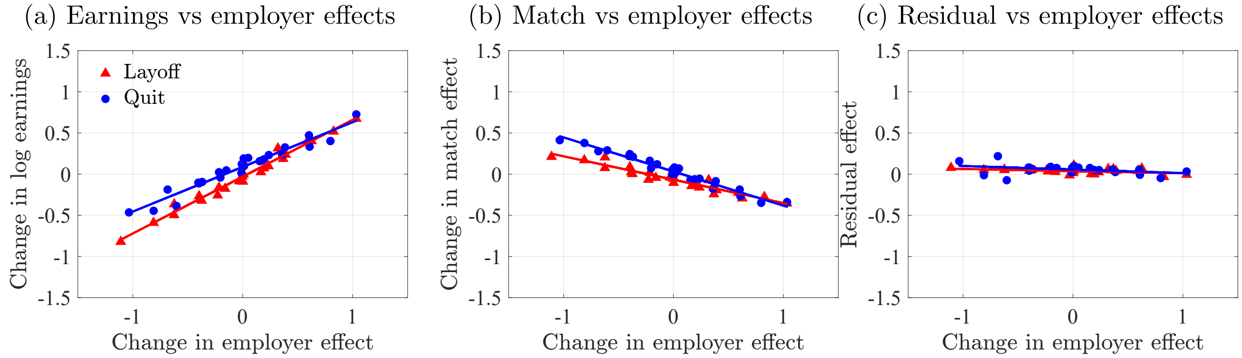
Positional dynamics. We further examine the asymmetries in employer premium dynamics between layoffs and quits in Figure 5. Panels (a) and (b) present the distribution of separations by origin (dashed-red) and destination (solid-blue) quintiles for both types of separations. Comparing origin-quintile distributions, laid-off workers are more likely to originate from high employer-premium quintiles, whereas the same distribution is more even for quits. For example, 35% of layoffs during mass layoffs originate from employers in the top quintile, while only 7% come from those in the bottom quintile. Meanwhile, these shares are 23% and 14% for quits, indicating much less heterogeneity along the origin-quintile distribution. Moreover, Panel (a) documents that while layoffs are more prevalent in higher origin quintiles, the fraction of workers transitioning toward higher destination quintiles is much lower. This implies the employer-effects distribution shifts leftward upon layoffs and workers suffer a substantial *net* loss in their employer premium position. In contrast, for quits, the distribution of employer effects remains largely unchanged (Panel (b)).

Panels (c) and (d) present downward and upward transition probabilities by origin employee quintiles for layoffs and quits in mass layoffs, respectively. Specifically, dashed-orange (solid-green) lines represent the probability that a worker finds reemployment with an employer whose employer effect is in a lower (higher) quintile than their origin employer, conditional on the origin-employer quintile. Comparing layoffs and quits, we note two key takeaways. First, downward transition probabilities are larger for laid-off workers, especially when the origin employer is in the fourth and fifth employer-effect quintiles (dashed-orange lines in Panel (c) and (d)). Upward transition probabilities, on the other hand, are larger for workers who quit, regardless of origin-employer quintile (solid-green lines in Panel (c) and (d)). Second, downward transition probabilities increase more with the origin-employer quintile for layoffs than for quits. These results reveal the underlying reason behind the larger loss of employer effects for laid-off workers: They are more likely to come from employers with high pay premium, and the likelihood of experiencing a transition into a new employer with a lower pay premium is higher when the origin-employer quintile is high.[39]

Earnings outcomes. Thus far, we have focused on the *positional* dynamics of employer premia upon separations. Now, Figure 6 plots interquintile changes in log earnings and match effects against changes in employer effects. Each panel is a scatter plot of 25 points, one for each origin-destination combination of employer-effect quintiles. Panel (a) shows that, for the same change in employer effects, workers who quit in the mass-layoff separator sample enjoy better earnings outcomes than their laid-off counterparts. This gap is more prominent for transitions with a decline in employer premiums. Since the change in employer premium is the same for any given point on the x-axis and changes in residual effects are roughly zero (Panel (c)), the gap in log earnings changes between layoffs and quits is largely explained by differences in match effects. Panel (b) shows that, for the same decline in employer effects, quitters receive a larger offsetting increase in match effects than laid-off workers. This suggests that workers who quit into lower-paying employers may be doing so in pursuit of a better match for their skills or a better contract. In contrast, laid-off workers may have little choice but to accept lower compensation in pursuit of reemployment.

[39]Section 5 presents analogous results for non-mass layoffs (equivalents of Table 3 and Figure 5), showing that both laid-off and quitting workers face lower risks of falling down the employer premium ladder.

Figure 6: Interquintile changes by employer effects



Note: This figure plots interquintile changes in log earnings, match effects, and residual effects against changes in employer effects separately for layoffs and quits in mass layoffs. Each panel contains a scatter plot of 25 points, one for each origin-destination combination of employer-effect quintiles. Results are obtained by comparing outcomes between one year before and three years after separation. The table below summarizes outcomes from linear regressions of changes in log earnings, match effects, and residual effects on changes in employer effects.

	(a) Changes in log earnings		(b) Changes in match effects		(c) Residual effects	
	Layoff	Quit	Layoff	Quit	Layoff	Quit
Constant	-0.030	0.088	-0.067	0.032	0.037	0.056
	(0.011)	(0.013)	(0.009)	(0.008)	(0.006)	(0.011)
Changes in employer effects	0.690	0.543	-0.283	-0.414	-0.026	-0.043
	(0.023)	(0.027)	(0.019)	(0.017)	(0.011)	(0.023)
R-squared	0.975	0.945	0.910	0.964	0.185	0.137
Root mean squared error	0.058	0.067	0.046	0.041	0.029	0.056
Observations	25	25	25	25	25	25

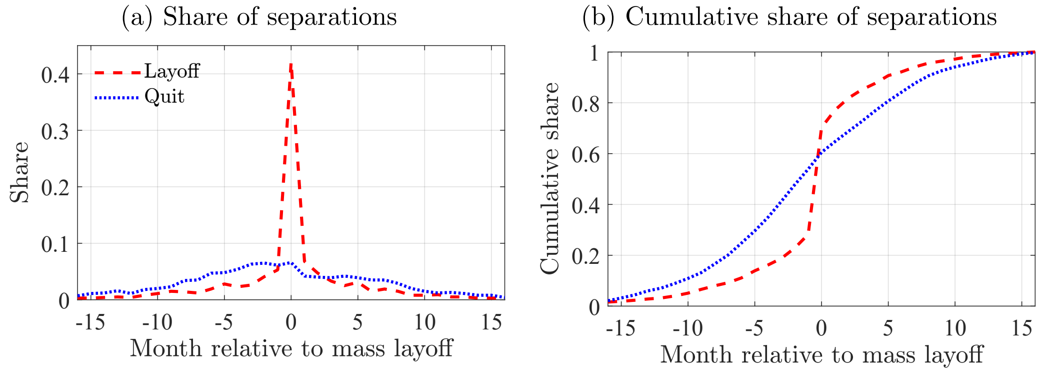
Finally, we find that the AKM model with additive worker and employer effects explains earnings changes better for layoffs than for quits, in line with our results in Section 3.2. As the table below Figure 6 shows, a regression of changes in log earnings on changes in employer effects yields a larger coefficient for layoffs than for quits (0.690 vs 0.543).

Taking stock. Laid-off workers tend to come from high-paying employers and move to lower-paying ones, experiencing larger losses in earnings and employer premiums than those who quit. Quitting workers often offset employer premium declines with better match effects, resulting in milder earnings losses even after a downward transition. Overall, while the average earnings and employer premium dynamics for all mass-layoff separations in our data closely reflect the findings in Lachowska et al. (2020), our results above show that looking at cross-sectional outcomes without conditioning on the reason for job separation masks substantial heterogeneity in the earnings and job-ladder dynamics upon separation.

4 Earnings losses and the role of timing

The previous section highlighted the high degree of cross-sectional heterogeneity in separations during mass layoffs. We now add another dimension by exploiting information in

Figure 7: Distribution of separations around a mass-layoff based on timing



Note: This figure shows the distribution of separations (layoffs and quits) based on their proximity to the mass-layoff month, defined as the month during which the largest number of ROE layoffs are recorded for a mass-layoff employer. For each mass-layoff separator, we use ROE job end-dates to find the separation's distance from the mass-layoff month.

the ROE forms that allows us to interact *cross-sectional* heterogeneity with the *timing* of separations. First, we show that employer contractions are protracted, with a substantial share of separations occurring months before the peak of the mass-layoff event. We then examine how separation timing affects earnings outcomes and relates to worker characteristics. Importantly, our findings reveal how employers restructure their workforce when contracting.

4.1 Identifying the timing of separation during mass layoffs

The date-of-separation information from the ROE serves two purposes. First, it allows us to classify the exact month during which the height of a mass layoff occurred. For each employer experiencing a mass layoff, the mass-layoff month is the month during which the largest number of ROE layoffs are recorded.[40] Second, it allows us to group separators by how close their *own* separation date is to the peak month of the mass-layoff month.

Figure 7 presents the distribution of separations in our mass-layoff separator sample by proximity to the mass-layoff month. Panel (a) shows that quits occur gradually before and after the mass-layoff month, whereas layoffs are more concentrated around that month as expected. For example, 42% of layoffs occur in the mass-layoff month, compared to just 7% of quits. Panel (b) presents the cumulative share of these separations, for both layoffs and quits, and shows that 53% of quits and 27% of layoffs occur *before* the mass-layoff month.[41]

[40] To account for the possibility that distressed employers may suffer from multiple mass layoffs, we allow for the reference month of the mass layoff to be worker specific. Consider an employer that experienced mass layoffs in 2008 and in 2009. If a worker from the employer is observed with a new main employer in 2009 (2010), then we assign the mass-layoff month of the employer to be the month with the largest ROE layoffs recorded between 2008 and 2009 (2009 and 2010). Results are robust to alternative specifications.

[41] Canada requires employers in federally-regulated industries (e.g., utilities, transportation, and financial services) to give a notice at least 16 weeks before laying off 50 or more employees within any four-week period. In other industries, the advance notice period varies by province. For example, in Ontario and Quebec, it is between 8 and 16 weeks depending on the layoff size. Thus, some workers may start searching for jobs when they receive the notice and quit before the mass-layoff month.

These results suggest that separation timing might be associated with worker characteristics and the consequences of separations via strategic decisions of workers and employers. For instance, quits prior to the mass-layoff month may be driven by worker decisions to find a new job. Similarly, employers may sequence layoffs based on worker productivity to reduce labor costs. On the other hand, quits after the mass-layoff month may indicate that workers who survive a mass layoff have incentives to leave a distressed employer once a suitable job is found. Below, Section 4.2 analyzes the effects of separation timing on earnings and employer premium dynamics, while Section 4.3 explores potential selection patterns by studying the characteristics of workers who quit or are laid off before and after the mass-layoff month.

4.2 Earnings and employer effects by timing

We now estimate the dynamics of earnings and employer-specific pay premia for layoffs and quits before and after the mass-layoff month. In what follows, the set of groups within the mass-layoff separator sample is $S = \{\text{layoff, quit}\} \times V$, where V is a set of groups that divide mass-layoff separators by proximity (in months) to the mass-layoff month.[42]

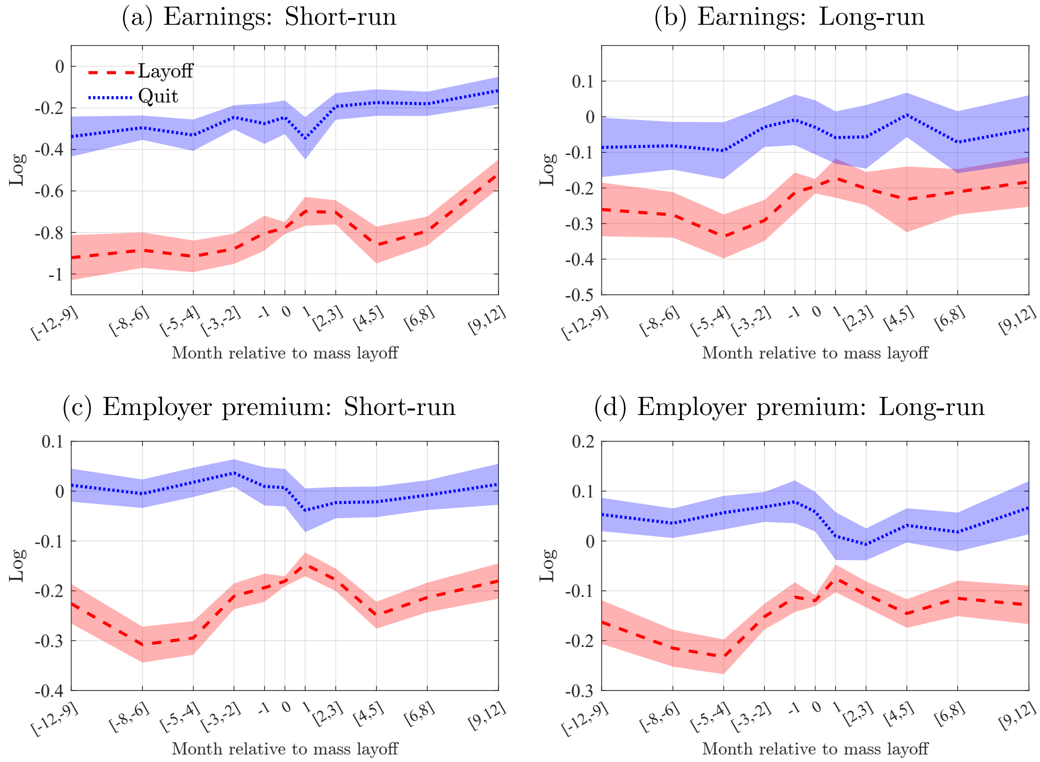
Earnings and employer effect dynamics. Panels (a) and (b) of Figure 8 present estimates for earnings losses upon separations across layoffs and quits when they are also grouped by their proximity to the mass-layoff month. Importantly, we uncover substantial heterogeneity in earnings losses across the timing of separation.

Starting with short-term (one year after the separation) earnings losses (Panel (a)), we find that those who experience a layoff prior to the mass-layoff month incur even larger earnings losses than those who are laid off at that month. For instance, while those who are laid off between 9 to 12 months before the mass-layoff month experience a 92-log-point earnings loss in the year following the separation, those who are laid off in the mass-layoff month incur a 78-log-point earnings loss. On the other hand, short-term earnings losses are typically smaller for workers laid off after the mass-layoff month than for those laid off during it. Albeit to a lesser degree, similar results are also obtained for those who quit during mass layoffs: Short-term earnings losses are smaller, especially for those who quit two months (or later) after the mass-layoff month. In the long run (six years post-separation), earnings losses remain generally worse for workers separating before the mass-layoff month, but timing-related heterogeneity is smaller than in the short run, as seen in Panel (b).

The larger earnings losses among workers laid off before the mass-layoff month suggest that employers may target less-productive workers for earlier layoffs. In contrast, the smaller earnings losses among workers who quit after the mass-layoff month, relative to those who quit before, suggest that the former group likely achieved better reemployment outcomes.

[42] The groups in V are $\{[-12, -9], [-8, -6], [-5, -4], [-3, -2], -1, 0, 1, [2, 3], [4, 5], [6, 8], [9, 12]\}$.

Figure 8: Earnings and employer effects upon separation by timing of separation



Note: Top (bottom) panels present estimates for earnings (employer premium) dynamics upon separations across layoffs and quits when they are also grouped by their proximity to the mass-layoff month. Panels (a) and (c) present estimated outcomes one year after separation (short-run), while Panels (b) and (d) present estimates for six years after (long-run). For each mass-layoff employer, the mass-layoff month is identified as the month during which the largest number of ROE layoffs are recorded. 95% confidence intervals are given by the shaded regions.

This may be due to their job search occurring under less desperate conditions, as they retained their jobs despite the large employer contraction. Further, the smaller earnings loss gap between early and late quits compared with layoffs suggests that workers who quit can optimally time their separations based on their unique labor market opportunities.[43]

Why are earnings losses larger for those separating before the mass-layoff month? Panels (c) and (d) address this by showing employer-premium dynamics for layoffs and quits, grouped by proximity to the mass-layoff month. Among those who are laid off, employer premium losses are much larger for those whose jobs are dissolved relatively early into the mass layoff. The short-term gap in employer-premium losses between early and later layoffs (Panel (c)) is sizable and persists in the long term (Panel (d)). Thus, employer premium losses largely contribute to the gap in earnings losses between those who are laid off prior to the mass-layoff month and those who are laid off after that month. In contrast, for quits, there are similar employer-effects dynamics between the two groups both in the short run and in the long run. As such, employer-effect dynamics are unimportant not only for explaining

[43] As mentioned, advance notices may influence workers' job search behavior and estimates of earnings losses around mass layoffs. If notices enable workers to search before the mass layoff and reduce earnings losses, then the losses for early layoffs would have been even larger without them.

the average earnings loss for workers who quit (Section 3.2), but also for accounting for the cross-sectional difference in earnings losses upon quits based on the timing of separation.[44]

Transitions along the employer premium ladder. We now compare outcomes for below-, on-, and above-diagonal transitions between employer-effect quintiles to uncover the sources of heterogeneity in outcomes by timing. Table A2 in Appendix B.7 repeats Table 3 for separations before (Panel A), around (Panel B), and after (Panel C) the mass layoff.[45]

Starting with layoffs, we highlight three results. First, around half of layoffs result in below-diagonal transitions regardless of separation timing, suggesting that even workers laid off after the mass-layoff month typically find reemployment with employers in lower quintiles of the employer effects distribution. Second, consistent with Figure 8, earnings losses for below-diagonal transitions are smaller for workers laid off after the mass-layoff month compared with those laid off before it. In contrast, for on- and above-diagonal transitions, changes in earnings upon layoffs are mostly similar regardless of separation timing. Finally, changes in earnings mostly track changes in employer effects independent of the type of employer-premium quintile transition and the timing of separation, implying that employer effects are key to understanding earnings changes across the distribution of layoffs.

Moving to results for workers who quit, we also emphasize three main findings. First, the shares of quits across the types of transition between employer-effect quintiles are almost equally distributed, and this is independent of the timing of separation. Thus, the incidence of quits to higher or lower employer-premium quintiles is almost equally likely across quits with different timing of separation. Second, earnings losses among below-diagonal transitions are similar for quits before or after the mass-layoff month, while earnings gains among above-diagonal transitions are larger for quits after that month. Finally, for below-diagonal transitions, independent of timing of separation, changes in employer effects and match effects move in opposite directions and offset each other, suggesting that the trade-off between finding a job at an employer with higher average pay and forming a more valuable match is present even across subpopulations based on the timing of separation. For above-diagonal transitions, those who quit after the mass-layoff month experience not only a larger average increase in employer effects but also a smaller decline in match effects, supporting the possibility that those who are able to remain attached to their employers during large employment contractions are more likely to find a new job at employers with better pay and form more valuable specific worker-employer matches than those who quit before the mass-layoff month.

[44] In Appendix C.1, we present the same results from Panels (c) and (d) but use a procedure based on Bonhomme et al. (2019) to obtain an alternative estimate of employer-specific pay premia. This method addresses the endogenous mobility bias inherent in AKM estimates.

[45] Here, separations before (after) the mass-layoff month occur 8 to 23 months before (after), while separations around the mass-layoff month occur within one month before, during, or after.

4.3 Characteristics of mass-layoff separators: role of timing

We now investigate whether worker characteristics are systematically related to the timing of separation by estimating the following cross-sectional regression:

$$y_i = \alpha_{j(i)} + x_i\beta + \sum_{s\in S} d_i^s \xi^s + \epsilon_i, \tag{4}$$

where the outcome variable y_i can be the log average earnings (over 2002–2005), a dummy variable that indicates being in the bottom quintile of the within-employer earnings distribution in 2007 (i.e., the earnings distribution at the origin employer), the worker's fixed effects component of log earnings from the AKM estimation in Equation (2), or the worker's age.[46] The dummy variable d_i^s is 1 if individual i is a mass-layoff separator with reason and timing subgroup s. The vector x_i consists of worker characteristics. Finally, $\alpha_{j(i)}$ controls for the employer j of worker i in 2007. The coefficient of interest ξ^s measures the difference in the outcome variable between a separator subgroup and stayers in the same origin employer.

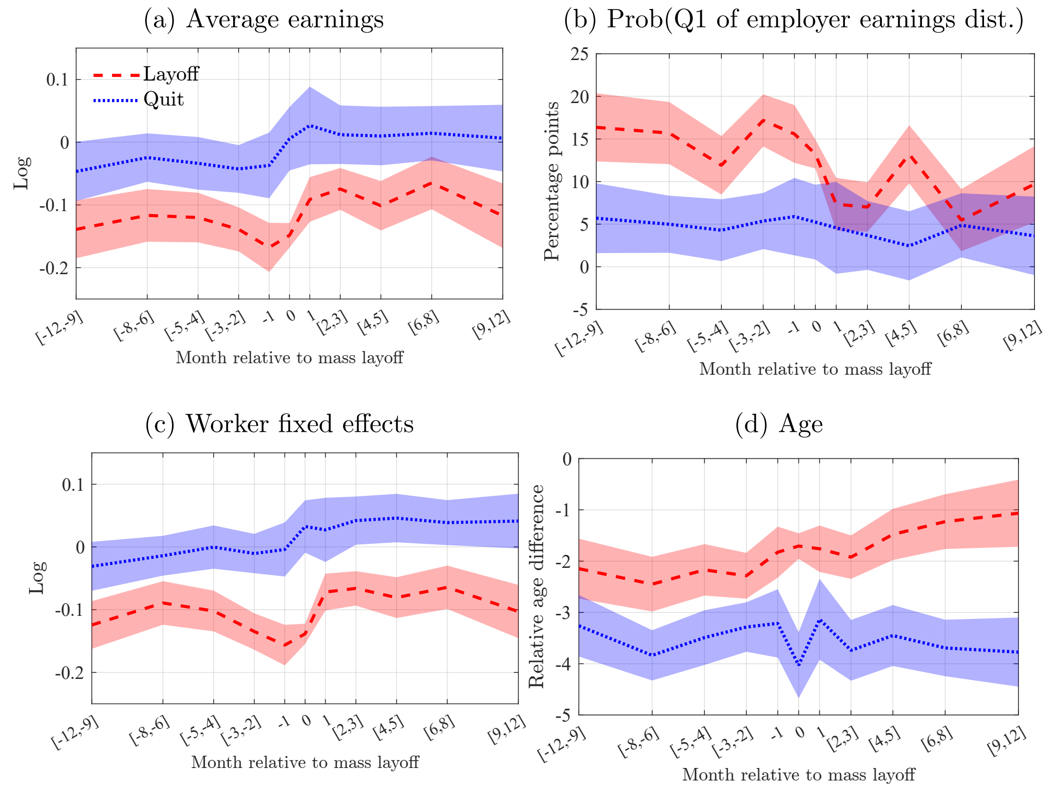
Panel (a) in Figure 9 measures the log points difference in average earnings (over 2002–2005) for workers in the mass-layoff separator sample (prior to separation) relative to stayers by timing of separation, separately for layoffs and quits. Laid-off workers have much lower average pre-separation earnings than stayers, while those who quit have similar earnings to stayers. Focusing on subpopulations by timing of separation, average pre-separation earnings is around 5 log points lower among those who separate before the mass-layoff month than among those who separate after, for both layoffs and quits.[47] Because the composition of separators closer to the mass-layoff month is increasingly dominated by layoffs and less by quits (Figure 7), if we were to mix layoffs and quits in Panel (a) of Figure 9, the average earnings for separators in the mass-layoff month would seem to be much lower than it is for those who separate before that month. As such, this compositional change within the mass-layoff separator sample would lead to a false conclusion that high-paid workers separate from their employer first, highlighting the importance of accounting for the reason for separation.

Panel (b) shows the percentage point difference in the probability of being in the first quintile of the within-employer earnings distribution in 2007 for separators (pre-separation) relative to stayers in 2007. The differences are presented by timing of separation for both layoffs and quits. We highlight two main results. First, the probability of being in the bottom quintile of the distribution is between 5 and 16 percentage points higher (depending on the timing of separation) for those who are laid off relative to stayers, while it is between 2

[46] The sample for estimating Equation (4) includes the AKM sample, but retains stayers and separators to obtain their worker fixed effects. This is the sample used to estimate match effects.

[47] For layoffs (quits), the average of point estimates for separations before the mass-layoff month is -14 (-4) log points, while the average for separations after is -9 (1) log points.

Figure 9: Characteristics of mass-layoff separators by timing of separation



Note: This figure plots differences in worker characteristics for mass-layoff separators grouped by reason and timing of separations relative to stayers, as in Equation (4). Panel (a) measures the log difference in average earnings (over 2002–2005) for workers in the mass-layoff separator sample (prior to separation, i.e., at the origin employer) relative to average earnings of stayers. Panel (b) measures the percentage-points difference in the probability of being in the first quintile of the within-employer earnings distribution in 2007 for separators (prior to separation, i.e., at the origin employer) relative to that of stayers in 2007. Panel (c) shows the log difference in the worker fixed effects between these groups, while Panel (d) provides the age gap between these groups. These estimates are provided for separations with different reason (layoff in dashed-red lines and quit in dotted-blue lines) and different timing (x-axis). 95% confidence intervals are given by the shaded regions.

and 6 percentage points higher for quits. Second, focusing on the timing of separation, while this probability is around 6 percentage points higher on average for those who are laid off before the mass-layoff month than for those who are laid off after, it does not change much for those who quit across timing of separation.[48] These results complement our findings in Section 3.5. While Figure 5 documents that layoffs mostly originate from employers with high-employer effects, Panel (b) in Figure 9 suggests that workers who are laid off from these employers are more likely to be those in the bottom quintile of the within-employer earnings distribution, even more so when they experience the layoff prior to the mass-layoff month.

Next, Panel (c) provides the difference between the average of worker fixed effects among mass layoff separators and of stayers by timing of separation, separately for layoffs and quits. Compared to stayers, laid-off workers have average fixed effects that are 10 log points lower, while workers who quit have fixed effects that are similar or slightly higher. Focusing on the

[48]The 6-percentage-point gap is obtained by comparing the average of point estimates for layoffs after the mass-layoff month and before the mass-layoff month.

effects of the timing of separation, we find that, for both types, those who separate prior to the mass-layoff month have a lower average relative to those who separate after that month.

Finally, Panel (d) presents the age differences between separators and stayers. Laid-off workers are on average older than quitting workers, regardless of the timing. Moreover, workers laid off before the mass-layoff month are slightly younger than those laid off after.

Overall, workers who are laid off early are likely to be less productive than those who are laid off late, considering that the former group has lower pre-separation earnings, lower ranks in the within-employer earnings distribution, and lower worker fixed effects. These patterns suggest that employers make strategic decisions to lay off less-productive workers first during a period of severe contraction. For quits, we do not find strong evidence to support the hypothesis that more-productive workers quit their jobs before the mass layoff. Instead, workers who quit after the mass-layoff month are potentially more productive, as they have higher average pre-separation earnings and worker fixed effects than those who quit before that month. This suggest that separators in the former group are typically valuable to the employer and are thus retained even during employer distress. This provides them sufficient time to search for favorable employment opportunities. Potentially, a job search that is conducted in less desperate conditions helps them to bargain better contracts compared with less-productive workers who quit early for fear of being laid-off eventually.

5 Separations outside mass layoffs

While we focus on separations during mass layoffs, we also examine outcomes for separations outside of these events. Appendix D analyzes the interaction between employer and worker outcomes and whether employer distress worsen earnings losses.

First, we show that separation distributions differ markedly between mass-layoff and non-mass-layoff events. Among non-mass-layoff separations with non-missing ROE information, 55% are quits and only 18% are layoffs. Thus, unlike our mass-layoff sample where layoffs dominate, quits are the most common separation type among non-mass-layoff separations.

Separations outside of mass layoffs are associated with smaller and less persistent earnings losses, as shown in Figure A22. Layoffs in non-mass layoffs result in a 45-log-point drop in earnings in the first year, compared with a 78-log-point drop for layoffs in mass layoffs. Quits in non-mass layoffs also see a faster recovery. The gap between earnings losses upon layoffs and quits in non-mass layoffs is also much smaller than that in mass layoffs.

Employer effects losses are similarly less severe in non-mass layoffs, with laid-off workers experiencing only a 7-log-point decline one year post-separation, and full recovery after six years. In fact, workers who quit in non-mass layoffs often achieve larger pay premium gains.

Larger gaps in outcomes between layoffs and quits during mass-layoff events, *relative* to non-mass-layoff separations, further reinforce the notion that quits during mass layoffs should be viewed as distinct from involuntary separations. If mass-layoff quits were truly similar to layoffs, reflecting comparable worker distress or lack of choice, the outcome gap between the two groups would be smaller, not larger, during these episodes.

Finally, Table A3 and Figure A23 show that laid-off workers in non-mass-layoff events are less likely to move to lower-paying employers and incur smaller earnings losses upon a downward transition, compared with their mass layoff counterparts. Non-mass-layoff quits lead to larger earnings gains, especially for workers moving to higher-paying employers. Differences in outcomes between mass-layoff and non-mass-layoff separations are mainly driven by match effects, with quitters in non-mass layoffs experiencing greater gains.

6 Conclusion

This paper sheds light on the complex anatomy and evolution of mass layoffs using Canadian employer-employee matched administrative data combined with detailed job separation records. We document stark differences in earnings and employer pay premiums between workers who are laid off and those who quit, with layoffs experiencing more severe and persistent losses driven primarily by declines in employer effects. We also show that mass layoffs are protracted events: layoffs and quits begin months before the peak month of job cuts. Workers laid off earlier tend to have lower productivity and experience larger earnings and employer-premium losses. This highlights the importance of treating mass-layoff separations as multidimensional—early and late layoffs are systematically different, and employer distress may trigger early quits while some workers may optimally time their departure.

The stark differences in earnings and employer premium outcomes by separation reason and timing, even within the same mass layoff, show that separations are not uniform. Workers at a distressed employer can experience very different labor market outcomes and respond differently to policy interventions. Our findings underscore the importance of modeling distinct types of separations in quantitative analyses of job dissolution. They also stress the role of decisions made by workers and employers during an employer contraction, as employers lay off less-productive workers first, while more-productive workers time quits based on job search outcomes. Models that account for these mechanisms may be well-suited to evaluate labor market policies. For example, payroll subsidies could discourage employers from laying off long-tenure workers. Advance notice requirements may help workers avoid large earnings losses by giving them time to transition to another employer. Meanwhile, unemployment insurance could encourage less-productive workers to quit and climb the job ladder.

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Online Appendix

A Data

In this section, we provide additional details on our database.

The Canadian Employer-Employee Dynamics Database (CEEDD) is an employer-employee matched database which covers the universe of individual and corporate tax filers in Canada. It is maintained by Statistics Canada and is a linked collection of administrative data from Statistics Canada, Canada Revenue Agency (CRA), Employment and Social Development Canada (ESDC), and Immigration, Refugees, and Citizenship Canada (IRCC). This paper utilizes a subset of forms used to construct the CEEDD. In particular, we use information from the following forms:

- Individual-level tax files: This is obtained from the T1 Income Tax and Benefit Return, the main tax return used by individuals to file annual income taxes. It consolidates information on income earned from all sources, including those derived from employment, businesses, and investments. It also contains detailed demographic and other financial information about the individual.

- Employer-level tax files: The National Accounts Longitudinal Microdata File (NALMF) combines tax and administrative forms submitted by employers, including the T2 Corporation Income Tax Return, T4, Payroll Account Deductions (PD7), and Goods and Services tax/Harmonized Sales tax (GST/HST). Any enterprise that files at least one of these forms is included in the NALMF. Thus, the NALMF includes all corporate tax filers and unincorporated businesses with at least one employee but excludes non-employer businesses.

- Job-level tax files: Employers are required to submit the T4 Statement of Remuneration Paid for all their employees. The T4 contains information on various forms of compensation, among which includes wages and salaries, tips or gratuities, bonuses, taxable benefits, and commissions. Amounts reported on the T4 are based on when the income was paid, and not when the services were rendered. Individuals who received compensation from multiple employers during the year would have multiple T4s as well. As discussed in Section 2.1, the ROE is a form that employers must issue to employees whenever an interruption in earnings occurs. An interruption in earnings occurs when at least one of the following two conditions are met. First, an employee experiences seven consecutive calendar days with no work and no insurable earnings from the employer. This condition covers separations associated with a layoff, quit, or termination. Second, the employee's salary falls below 60% of their regular weekly earnings and the interruption is caused by reasons such as illness, injury, maternity/parental leave, child care among others. The ROE contains information on the worker's employment start and end dates as well as the separation reason, and is primarily used for the determination of EI eligibility.

Each T1 form features an individual identifier, while each NALMF record features an employer identifier. The T4 job-level records contain *both* individual and employer level identifiers and allow for linkages between T1 individual demographics and financials with NALMF employer characteristics.

B Additional results

We provide additional results to supplement our discussions in the main text.

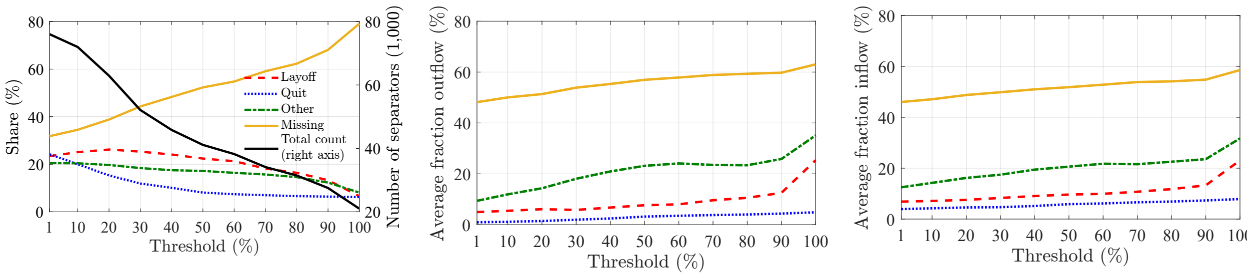
B.1 Implications of mass-layoff definition

In Section 2.1, we outline our approach to identifying mass-layoff events. As a baseline, we follow Lachowska et al. (2020) and define a mass layoff during the 2008–2010 period as requiring, among other criteria, that an employer experiences a reduction in employment of at least 30% relative to its 2007 level. While this threshold is commonly used in the literature, the choice of cutoff for defining an employment contraction may influence the types of mass-layoff events—and the associated separations—captured by the identification strategy. For example, a relatively low threshold may capture employers that are not truly undergoing mass layoffs, thereby introducing noise into the sample, while a high threshold may restrict the sample to cases that more closely resemble plant closures or M&A's, which also reflect as widespread changes to employer identifiers in the data. To examine the implications of this threshold choice, Figure A1 presents summary statistics for the sample of mass-layoff separations obtained under alternative thresholds for employment contraction, holding all other identification criteria the same. We note that the concentrated flow exclusion criterion from Benedetto et al. (2007) is not applied in this exercise, in order to more clearly reveal the effects of varying the mass-layoff threshold on the inclusion of spurious separations.

We highlight three key observations. First, the 30% threshold appears to correspond closely to the point at which the share of layoffs among all ROE-reported separations is maximized (Panel (a)). Second, increasing the mass-layoff threshold results in a higher proportion of missing ROE separations (Panel (a)) due to a greater incidence of concentrated-flow events associated with large employment contractions, as suggested by Panels (c) and (d). Finally, raising this threshold not only leads to a substantial reduction in the number of identified separators (Panel (b)), but also in a remaining sample that is both small and

Figure A1: Effects of varying employed contraction threshold for mass-layoff identification

(a) Separation count and reasons (b) Average outflow (c) Average inflow



Note: This figure presents various statistics associated with varying the employed contraction threshold used for identifying a mass-layoff event. For each threshold tested, Panel (a) presents the distribution of separations after concentrated flows that surpass the threshold are dropped (left axis) and the corresponding total number of separations remaining (right axis, solid black line). Panel (b) presents the fraction of the total workforce of an employer who exit and transition into the same employer, averaged over each mass-layoff separator (outflow). Panel (c) presents the ratio of new hires who originate from the same employer to the total workforce of the destination employer, averaged over each mass-layoff separator (inflow).

increasingly composed of workers with missing ROEs and ties to concentrated-flow events. Taken together, these findings suggest that the 30% threshold strikes a reasonable balance: It effectively captures events primarily driven by layoffs, and—when combined with the concentrated-flow exclusion criterion used by Benedetto et al. (2007)—it also helps reduce the incidence of spurious separations associated with missing ROEs.

B.2 Results under alternative specifications and samples

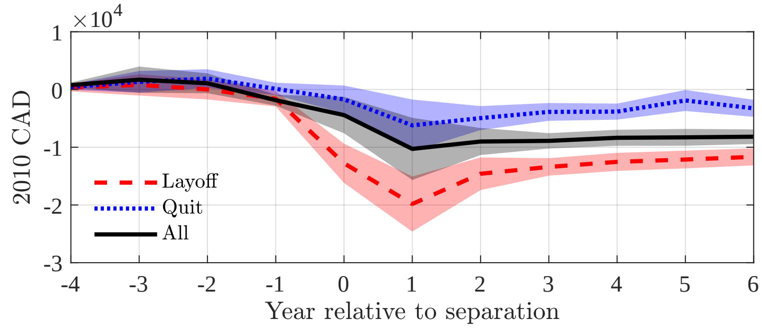
In this section, we repeat our main result in Figure 3 of Section 3.2 under alternative samples, mass-layoff identification, and empirical specification, as summarized in Section 3.4.

Including zero earners. The baseline sample restriction imposes that workers report positive earnings for the entire duration considered (2002–2014). We relax this restriction by allowing for workers with zero earnings in certain years. Figure A2 presents the dynamics of earnings upon job loss for layoffs (blue), quits (red), and mass-layoff separators (black) for this alternative sample.[1] Results in Figure A2 are similar to those in Figure A15 in Appendix B.5, which provides the results from the same analysis for the baseline sample.

We argue that our baseline assumption is a more reasonable approach for a key reason: Since tax filers with zero earnings for an entire year are rare, the original positive earnings restriction primarily excludes workers who do not file taxes. Relaxing this restriction introduces workers with missing data and leads to an unbalanced panel, especially towards the end of the sample. Further, in this unbalanced panel, we observe a declining fraction of workers with reported earnings over time, possibly due to increased non-filing related to emigration. Thus, including individuals with zero or missing earnings introduces composi-

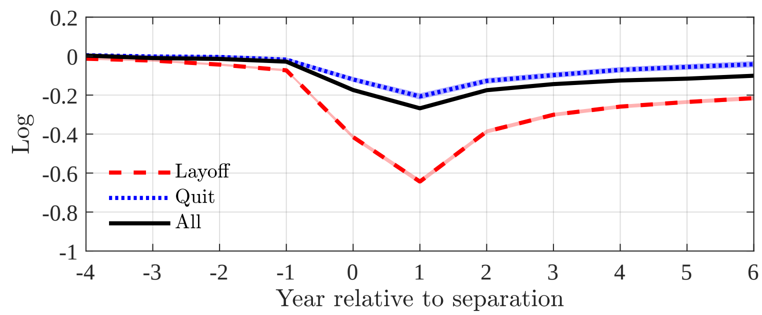
[1] To accommodate zero earnings in our analysis, we do not take logs; as a result, we report results in levels and omit employer premia dynamics.

Figure A2: Job separation outcomes by reason: Including zero earnings



Note: This figure plots estimates for earnings losses in levels (2010 CAD) upon job separation by reason of separation during mass layoffs when the sample is modified to allow for individuals who do not report positive earnings. Dashed-red and dotted-blue lines (along with shaded 95% confidence intervals) show estimated earnings losses for layoffs and quits, respectively, while the solid-black line presents these losses for all mass-layoff separators.

Figure A3: Job separation outcomes by reason: Excluding low earners



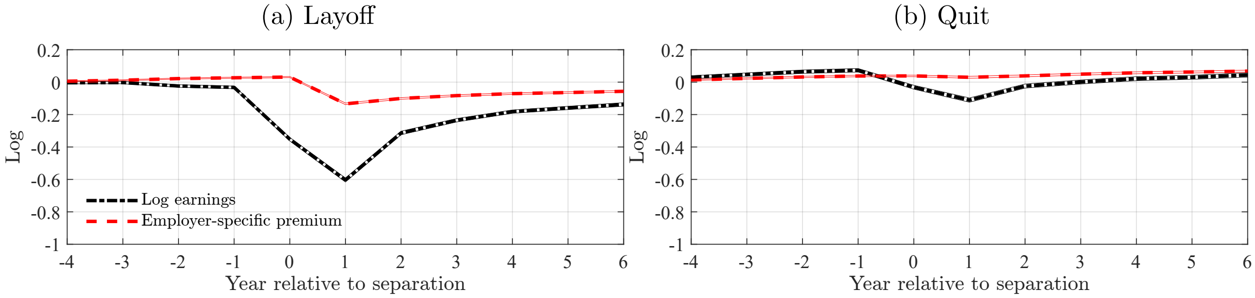
Note: This figure plots estimates for earnings losses upon job separation by reason of separation during mass layoffs when the sample is modified to exclude individuals with very low earnings. Specifically, workers reporting annual earnings below 400 times the national average minimum hourly wage—the same threshold employed in the AKM sample—are excluded. Dashed-red and dotted-blue lines (along with shaded 95% confidence intervals) show estimated earnings losses for layoffs and quits, respectively, while the solid-black line presents these losses for all mass-layoff separators.

tional changes. To address the issue of emigration, we conduct an additional analysis that includes only workers with missing earnings who later returned by the end of the sample. Since there are far fewer individuals added back into the sample, results remain similar.

Excluding low earners. The use of a log earnings as our main outcome variable in Equation (1) may imply that very low earnings will translate to very large (log) earnings losses, where making a percent interpretation is inappropriate. To ameliorate concerns around this, we re-run the earnings loss regression on a sample that exclude low-earners. Specifically, worker observations reporting annual earnings below 400 times the national average minimum hourly wage—the same threshold employed in the AKM sample—are excluded from the sample. As shown in Figure A3, excluding low earners reduces the estimated magnitude of log earnings losses by approximately 20 log points. Nevertheless, the central finding—that substantial heterogeneity in outcomes persists between layoffs and quits—remains robust.

Relaxing tenure requirement. Next, we explore the implications of relaxing the long-tenure requirement imposed under our baseline sample selection. Figure A4 presents our main results for earnings and employer premium dynamics when we *do not* require workers to be attached to the same employer six years prior to separation.

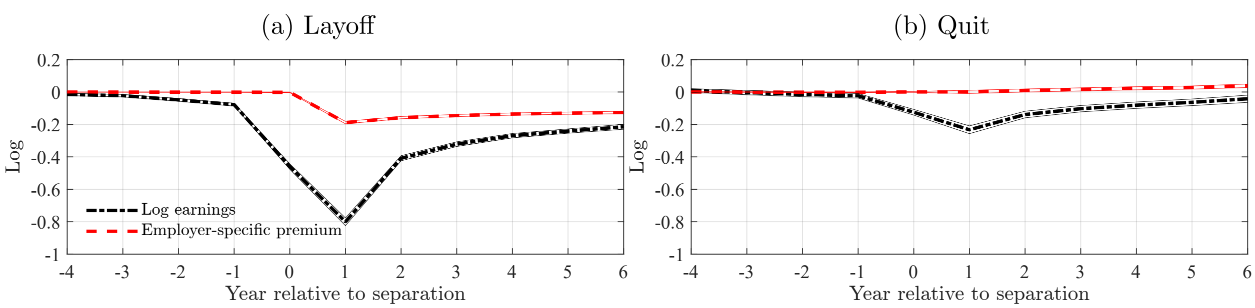
Figure A4: Job separation outcomes by reason: Relaxing long-tenure requirement



Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation during mass layoffs. Panel (a) presents estimates for separations due to layoff and Panel (b) presents estimates for separations due to quit. Dashed-dotted-black lines show estimated γ_k^s values (along with 95% confidence intervals given by solid-black lines) in Equation (1), and dashed-red lines present estimated γ_k^s values (along with 95% confidence intervals given by solid-red lines) in Equation (3). Here, we relax the long-tenure requirement by no longer limiting the sample to workers continuously employed by the same employer for the six years prior to separation.

Compared to the baseline results in Figure 3 (Section 3.2), the earnings drop for both layoffs and quits is somewhat smaller under this specification. A likely explanation is that high-tenure workers experience greater earnings losses due to the loss of employer-specific, non-transferable human capital built over their extended time with one employer. Including short-tenure workers, who have accumulated less employer-specific capital, reduces the magnitude of earnings losses. However, our main conclusions on the gap between earnings losses for layoffs vs quits remain the same: Earnings losses are much larger for layoffs than quits, and employer effects account for earnings losses for layoffs but not for quits.

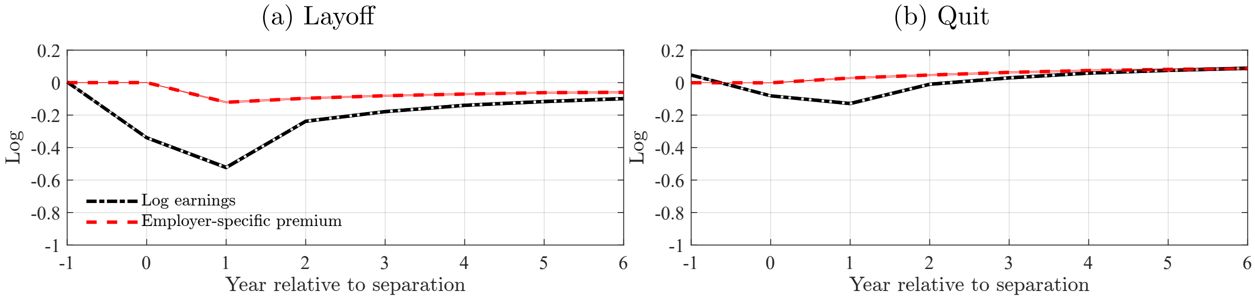
Figure A5: Job separation outcomes by reason: Excluding multiple-job holders



Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation during mass layoffs. Panel (a) presents estimates for separations due to layoff and Panel (b) presents estimates for separations due to quit. Dashed-dotted-black lines show estimated γ_k^s values (along with 95% confidence intervals given by solid-black lines) in Equation (1), and dashed-red lines present estimated γ_k^s values (along with 95% confidence intervals given by solid-red lines) in Equation (3). Here, we drop individuals with multiple job records.

Excluding multiple-job holders. Our main sample includes workers with multiple jobs in a year, with the primary employer defined as the one contributing the largest share of annual earnings. Figure A5 shows that, even after excluding workers with multiple job records, the earnings and employer effect dynamics for layoffs and quits remain similar to the baseline results shown in Figure 3.

Figure A6: Job separation outcomes by reason: Alternative mass-layoff identification



Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation during mass layoffs. Panel (a) presents estimates for separations due to layoff and Panel (b) presents estimates for separations due to quit. Dashed-dotted-black lines show estimated γ_k^s values (along with 95% confidence intervals given by solid-black lines) in Equation (1), and dashed-red lines present estimated γ_k^s values (along with 95% confidence intervals given by solid-red lines) in Equation (3). Here, without restricting separations to be within the 2008–2010 period, we consider an alternative sample and adopt definitions of long-tenure workers and mass-layoff events following Davis and von Wachter (2011).

Implementing an alternative mass-layoff identification. Section 2.1 explains the identification of mass-layoff events between 2008 and 2010. In this section, we remove the restriction that separations fall within that timeframe and explore alternative sample restrictions, along with different definitions of long-tenure workers and mass-layoff events, as in Davis and von Wachter (2011). Consider a reference year t. A mass layoff occurs in year t when: (i) employment drops more than 30% between $t-2$ and t, (ii) employment in $t-2$ is not more than 130% of employment in $t-3$ and (iii) employment in $t+1$ is less than 90% of employment in $t-2$. We focus on individuals who are at most 50 years old and who have been employed with the same primary employer for at least three years, as in Davis and von Wachter (2011). Similar to them, we define a mass-layoff separator in year t to be an individual who separates from their primary employer in year t, while the employer is identified as having experienced a mass layoff in year t or $t+1$.[2] The tenure requirement implies that a separator at t must have been employed by the employer at $t-2$, $t-1$, and t. We also restrict attention to all observations two years before and six years after the separation year t. For any given year t, this implies a panel with at most nine observations per worker, with the earliest observation for all workers being at $t-2$. Importantly, this restriction excludes cases where a worker meets the three-year tenure with their primary employer in t but was employed by a different employer more than two years earlier. To maintain this restriction, we are left with reference years t from 2003 to 2010, given our data. For any given nine-year

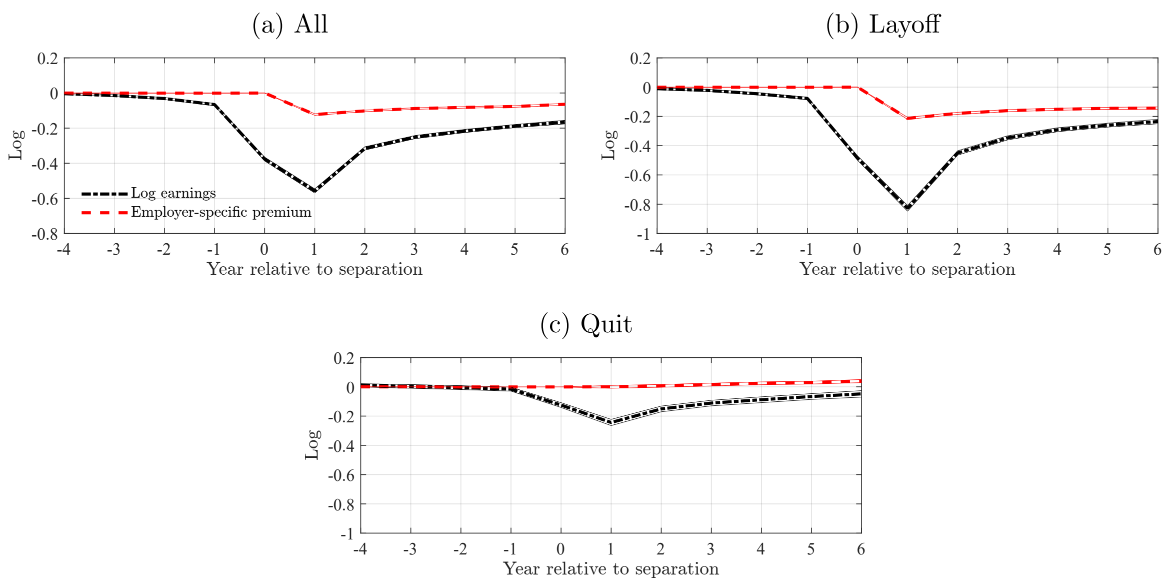
[2]We also drop separations associated with concentrated flows, following the job flow exclusion methodology described in Section 2.1.

window for a reference year t, a stayer is defined as an individual who maintains positive earnings with the same primary employer from $t-2$ to $t+6$. Finally, employer effects are estimated repeatedly for each reference year t as in Section 2.3. Unique to this procedure is that the exclusion of stayers and separators is specific to the sample in each t.

Using the 2001 to 2016 data, we identify mass layoffs, mass-layoff separators, and stayers for each reference year t from 2003 to 2010. We then estimate the regression in Equation (1) using pooled data from each panel constructed using each reference year t. We note that as in Davis and von Wachter (2011), our controls include an interaction of year dummies with a worker's average earnings (over $t-2$ to t).

Figure A6 presents the results in this exercise.[3] In this case, earnings and employer effects losses are only slightly smaller for both layoffs and quits when compared with Figure 3. Importantly, our main conclusions also remain similar: Gaps in earnings and employer effects losses between layoffs and quits are still large and employer effects account for earnings losses for layoffs but not for quits.

Figure A7: Outcomes under stricter exclusion threshold for concentrated flows



Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation during mass layoffs. Panel (a) presents estimates for all mass-layoff separations, Panel (b) presents estimates for those due to layoff, and Panel (c) presents estimates for those due to quits. Dashed-dotted-black lines show estimated γ_k^s values (along with 95% confidence intervals given by solid-black lines) in Equation (1), and dashed-red lines present estimated γ_k^s values (along with 95% confidence intervals given by solid-red lines) in Equation (3). Here, we lower the exclusion threshold associated with concentrated flows from 80% to 50%.

Alternative exclusion threshold. As outlined in Section 2.1, we adopt the job flow exclusion methodology employed by Benedetto et al. (2007) to partially filter out employer identifier changes associated with merger and acquisition, changes in legal structure and

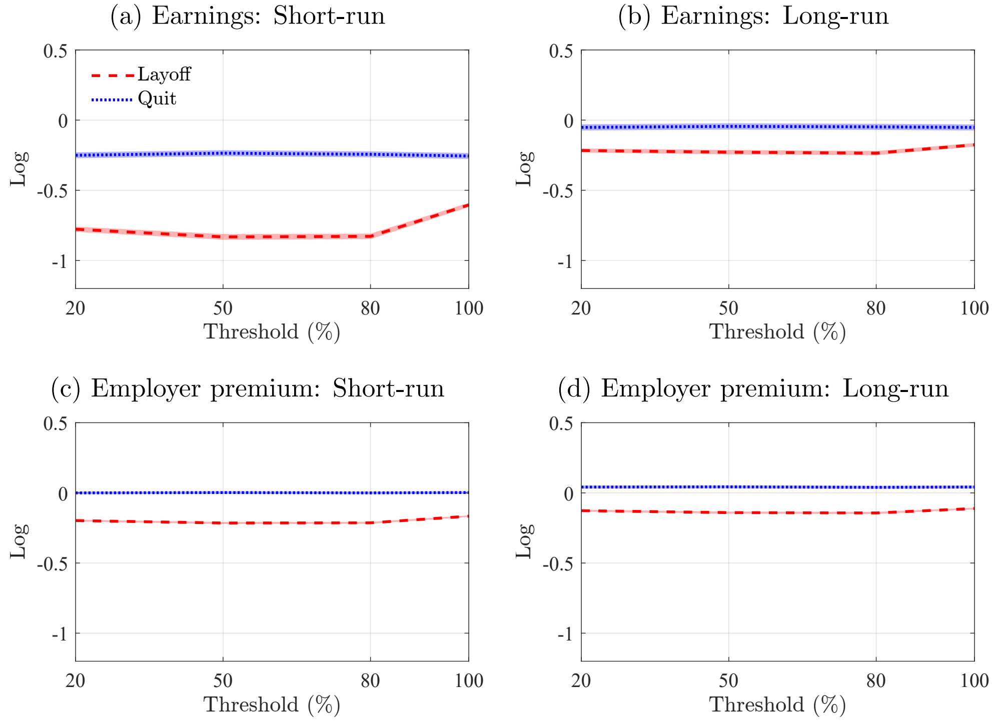
[3] The shorter pre-displacement horizon is due to the lower three-year tenure requirement.

name, as well as other movements across establishments within a large parent organization. In our baseline approach, a separation is excluded from our sample if: (i) 80% or more of the origin employer's workforce exits and transitions to the same destination employer (concentrated outflow), or (ii) over 80% of the destination employer's employees are new hires from the same origin employer as the worker (concentrated inflow). This exclusion method (or some variant of it) is widely adopted by work that uses employer-employee matched data to study the effects of job loss (see Hethey-Maier and Schmieder (2013), Halla et al. (2020), Lachowska et al. (2020), and Schmieder et al. (2023) among several others).

Section 2.2 shows that while the standard exclusion threshold mitigates the inclusion of missing ROEs into our mass-layoff separator sample, significant reductions of employer ID changes of this sort can be achieved by a 50% threshold without reducing the sample of other valid separations much further. Figure A7, Panel (a), shows that for all mass-layoff separators, the earnings decline is significantly larger compared to the baseline in Figure 2, with a drop of 56 log points versus 33 log points. This larger loss is attributed to the stricter threshold, which reduces the presence of missing ROEs. A sample with missing ROEs lowers earnings (and employer premium) losses, so sample restrictions which are more effective in filtering out spurious separations (in the absence of ROE information) results in significantly larger earnings losses. In contrast to Panel (a), Panels (b) and (c) indicate that earnings and employer premium losses for layoffs and quits are very close to those in Figure 3. This provides additional reassurance that the stricter exclusion threshold effectively targets spurious separations without significantly altering the sample of layoffs and quits. These findings are reinforced by Figure A8, which shows that short- and long-run earnings and employer premium losses remain relatively stable across different exclusion thresholds *when* conditioning on layoffs and quits. Of note, however, is that relaxing the threshold to 100% reduces short-term earnings losses. This pattern can be rationalized by the fact that such concentrated flows are highly likely to reflect mergers, acquisitions, or other forms of organizational restructuring. While layoffs may still occur in such contexts, the associated earnings losses are often smaller, likely due to the more orderly and predictable nature of these transitions and the absence of distress at the origin employer.

Focusing on employer closures. As we discussed in Section 2 of the main text, the literature identifies mass-layoff events from large employment contractions experienced by an employer. Such employment contractions can potentially occur for reasons that keep the employer in the business (e.g., reorganization and restructuring) or result in a complete closure of the business. Recall that in Figure 3, we do not take any stance on the underlying reasons behind large employment contractions. As an alternative specification, we consider a subset of the mass-layoff separations that occur between 2008 to 2010 as defined in Section

Figure A8: Earnings and employer effects dynamics: Varying threshold of concentrated flows

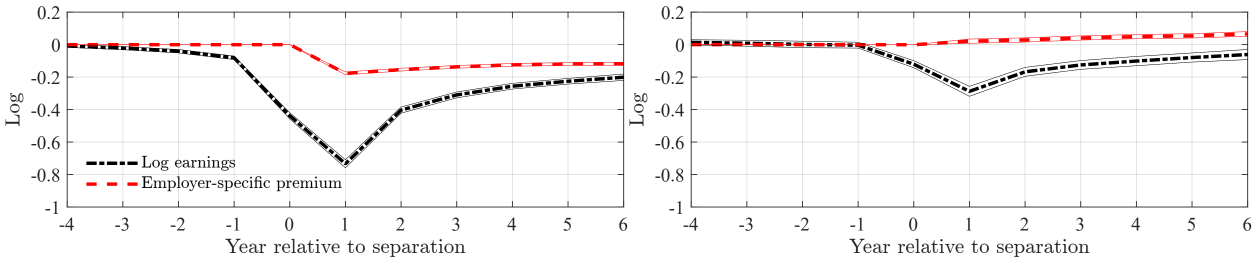


Note: This figure illustrates how short- and long-term earnings losses and employer premium dynamics vary for layoffs and quits when the exclusion threshold for concentrated flows is adjusted from the baseline of 80% to both lower and higher values. Panels (a) and (b) display earnings losses one year and six years after separation, respectively, while Panels (c) and (d) present the corresponding changes in employer-specific pay premia.

2.1 that only includes separations associated with employer closures. Formally, these closures are defined as employers that experience a mass layoff and either register zero employment or drop out from the sample for some given year, without ever returning to positive employment until the end of the sample period. Given our long-tenure sample restriction that workers must be attached to the same primary employer from 2002 through 2007, no plant closures occur before 2008. To illustrate, a mass-layoff separation that occurs in 2009 from an employer that eventually disappears permanently from 2013 onward would be considered a separation associated with an employer closure. The results in this case are presented in Figure A9. Restricting attention to employer closures retains the key message that laid-off workers suffer much larger earnings losses than quitting workers. However, compared with our baseline estimates, we note that earnings losses are slightly lower for the layoffs that coincide with an employer closure (73 vs. 78 log points) and slightly higher for quits (29 vs. 25 log points). This can be rationalized by the fact that mass layoffs may involve some discretion by employers in terms of selecting who to lay off and by workers in terms of deciding when to quit to join a new employer. An employer closure dilutes the negative selection for layoffs and the strategic opportunities for workers to time their quits. These findings are broadly in line with those documented in Gibbons and Katz (1991) and in Section 4.3.

Figure A9: Job separation outcomes by reason: Separators from employer closures

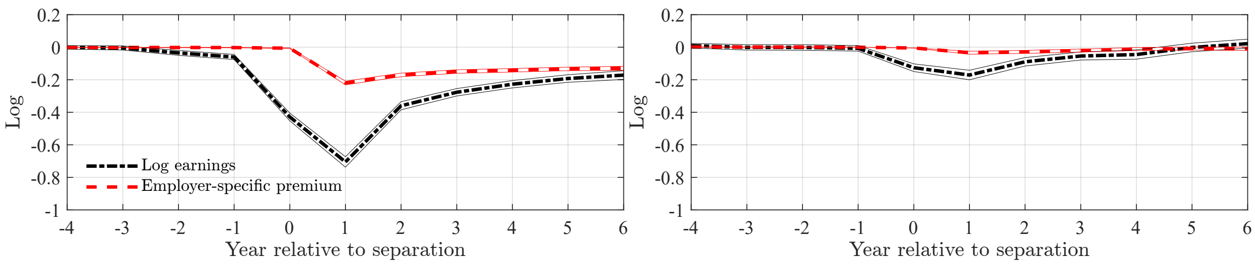
(a) Layoff (b) Quit



Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation during mass layoffs. Panel (a) presents estimates for separations due to layoff and Panel (b) presents estimates for separations due to quit. Dashed-dotted-black lines show estimated γ_k^s values (along with 95% confidence intervals given by solid-black lines) in Equation (1), and dashed-red lines present estimated γ_k^s values (along with 95% confidence intervals given by solid-red lines) in Equation (3). Here, we focus only on employer closures when identifying mass-layoff events in our sample.

Figure A10: Job separation outcomes by reason: Within-employer comparison

(a) Layoff (b) Quit



Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation during mass layoffs. Panel (a) presents estimates for separations due to layoff and Panel (b) presents estimates for separations due to quit. Dashed-dotted-black lines show estimated γ_k^s values (along with 95% confidence intervals given by solid-black lines) in Equation (A1), and dashed-red lines present estimated γ_k^s values (along with 95% confidence intervals given by solid-red lines) in Equation (3). Here, we compare outcomes between mass-layoff separators and stayers from the same employer.

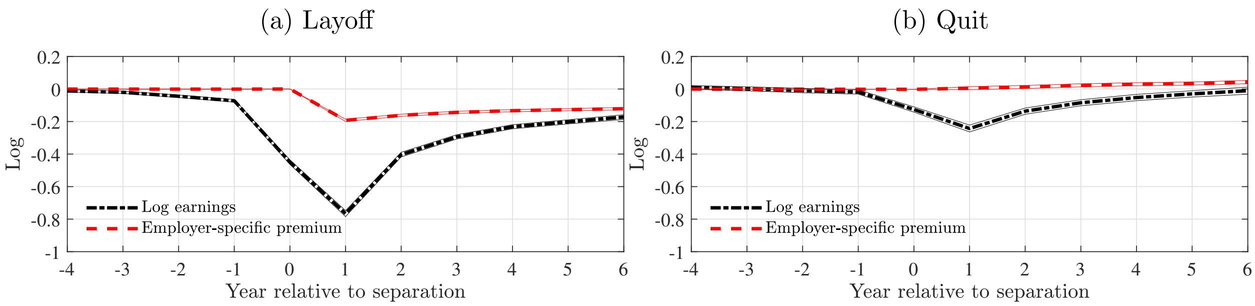
Comparing separators and within-employer stayers. When estimating the effects of mass-layoff separations on worker outcomes, we compare outcomes of long-tenure workers in the mass-layoff separator sample with outcomes of long-tenure workers who retain employment. We now redefine the control group to be the colleagues of mass-layoff separators who remain with their employers. As such, the comparison is now between separators and stayers from the same employer. To do this, we implement the following specification as in Jacobson et al. (1993):

$$y_{i,t} = \alpha_{i,j} + \zeta_t + \beta x_{i,t} + \sum_{s\in S}\sum_{k=-4}^{6} d_{i,t,k}^s \times \gamma_k^s + \varepsilon_{i,t}. \tag{A1}$$

The difference between this specification when compared with Equation (1) is that fixed effects $\alpha_{i,j}$ are conditional upon employer affiliation j, such that the estimated outcomes translate to differences between separators and stayers at the *same* employer. When stayers at an employer experiencing a mass layoff also systematically face earnings losses, the magnitude of estimated coefficients γ_k^s in Equation (A1) will be smaller than those estimated in Equation (1). Moreover, by construction, this specification excludes all separators whose previous employer goes out of business or otherwise disappears from the sample, which is another reason to expect lower earnings losses in this case relative to the baseline estimates.

Figure A10 shows that earnings and employer premium losses are slightly less severe in this case for both layoffs and quits. However, gaps in earnings and employer effect losses between layoffs and quits remain very similar to those in Figure 3.

Figure A11: Job separation outcomes by reason: Allowing for separations after 2010

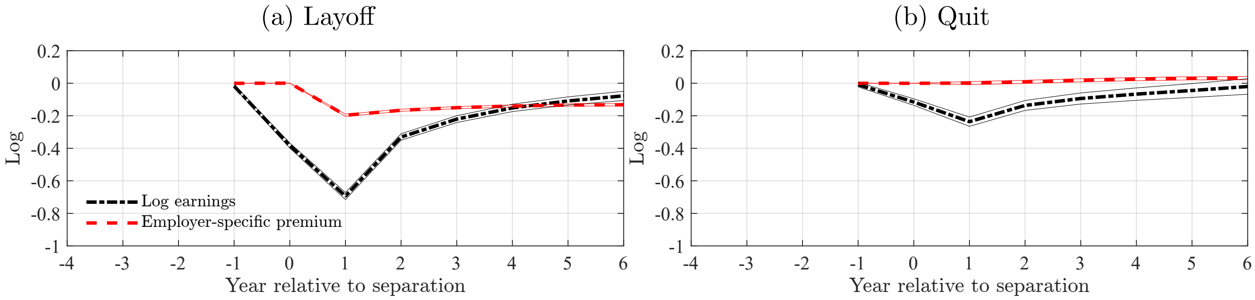


Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation during mass layoffs. Panel (a) presents estimates for those due to layoff and Panel (b) presents estimates for those due to quits. Dashed-dotted-black lines show estimated γ_k^s values (along with 95% confidence intervals given by solid-black lines) in Equation (1), and dashed-red lines present estimated γ_k^s values (along with 95% confidence intervals given by solid-red lines) in Equation (3). Here, we expand the control group to include workers who, while not experiencing an employer separation between 2008 and 2010, did separate from an employer in the period following 2010.

Alternative control group that allows for separations after 2010. In our baseline approach, we follow Lachowska et al. (2020) and set the control group to include only stayers who never separate from their original employer throughout the sample. This approach is motivated by findings from Bertheau et al. (2023), who demonstrate that restricting the control group to workers who remain continuously with the same employer tends to overstate the estimated effects of job displacement. To address this concern, we relax this assumption by expanding the control group to include workers who did not separate from their employer during the 2008–2010 period (the window used to identify mass-layoff events), but who may have separated thereafter. As shown in Figure A11, this broader control group leads to a roughly one log point reduction in earnings losses among laid-off workers, but ultimately produces similar estimates of the earnings impact of job loss, and does not meaningfully alter the observed gap in outcomes between laid-off workers and those who quit.

Incorporating heterogeneous time trends. Estimates of the effects of mass-layoff separations from the main regression specification in Equation (1) may be biased if there are worker fixed effects in earnings growth (in addition to the fixed effects in earnings level). For example, if those with lower lifetime earnings growth are more likely to be laid off, then the

Figure A12: Job separation outcomes by reason: Heterogeneous worker time trends



Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation during mass layoffs. Panel (a) presents estimates for separations due to layoff and Panel (b) presents estimates for separations due to quit. Dashed-dotted-black lines show estimated γ_k^s values (along with 95% confidence intervals given by solid-black lines) in Equation (A2), and dashed-red lines present estimated γ_k^s values (along with 95% confidence intervals given by solid-red lines) in Equation (3). Here, we estimate a version of Equation (1) with heterogeneous (worker-specific) trends, as in Equation (A2).

earnings losses estimated from Equation (1) may be overstated. To address this potential source of bias, we estimate a version of Equation (1) with worker-specific linear time trends. The new specification is:

$$y_{i,t} = \alpha_i + \xi_i t + \zeta_t + \beta x_{i,t} + \sum_{s \in S} \sum_{k=-1}^{6} d_{i,t,k}^s \times \gamma_k^s + \varepsilon_{i,t}, \tag{A2}$$

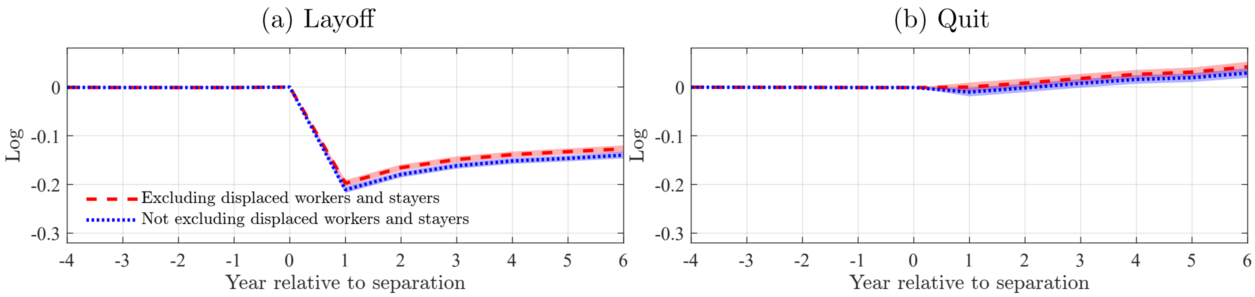
where ξ_i is the worker fixed effects in growth. Notice that we do not estimate the effects of separation for two, three, and four years before the separation. This is to ensure that the worker-specific linear time trends are well identified based on these three additional years of observations. With the inclusion of worker-specific trends, the estimates of γ_k^s now reflect the effect of a mass-layoff separation relative to stayers, controlling for differences in workers' unobserved characteristics that lead to differences in level as well as growth of worker outcomes.

Figure A12 presents results for this case. Again, we find that the magnitudes of earnings and employer effect losses remain similar to those in Figure 3. This provides reassurance that differential trends in earnings growth do *not* significantly bias the estimated effects of mass-layoff separations in our baseline specification.

Including the displaced worker sample for AKM estimation. As outlined in Section 2.3, we estimate employer-specific premia using a sample that excludes stayers and mass-layoff separators. This restriction helps avoid a mechanical relationship between employer effects and the earnings losses of mass-layoff separators, which could otherwise overstate the impact of employer effects. To assess the implications of estimating AKM employer premia with a different sample and then using those estimates to decompose the relative earnings losses of displaced workers excluded from the initial estimation, we recalculate the dynamics of employer-specific premium losses using a full sample that includes both stayers and mass-layoff separators.

Figure A13 shows that reintroducing the displaced worker sample into the analysis results in slightly greater losses and smaller gains in employer-specific premia for layoffs and quits, respectively. This suggests that the decline in employer premia is slightly larger when estimates are drawn from a more selected set of transitions, particularly those associated with mass layoffs. Thus, including displaced workers may exacerbate the endogenous mobility bias in the AKM estimation. Overall, we conclude that this sample change does not largely affect our main conclusions in Figure 3.

Figure A13: Effects of mass-layoff separations on AKM employer-specific premium



Note: This figure presents estimates of employer-specific pay premium losses when the employer premia are calculated using a sample that includes both the stayers and separators. Panels (a) and (b) compare the path of employer premium losses under the baseline AKM sample (red-dashed lines) with the expanded sample (blue-dotted lines), for layoffs and quits, respectively. Shaded regions represent 95% confidence intervals.

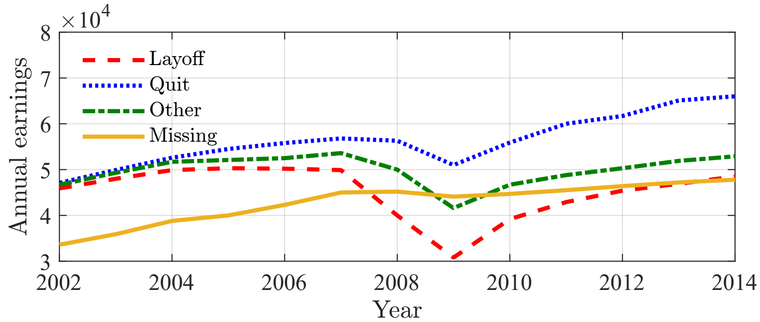
B.3 Outcomes of separators with missing ROE

Here, we further explore the characteristics of mass-layoff separators with missing ROEs, as discussed in Section 2.1.

First, Figure A14 presents average annual earnings over time for workers who separate from their job in 2008 for different reasons when their employer is experiencing a mass layoff in 2008–2009. We find that while the average earnings of workers who are laid off, who quit, and who separated for other reasons declines in 2009, it remains nearly unchanged in 2009 for separators with missing ROE data.

Second, Table A1 shows the fraction of individuals receiving employment insurance (EI) and the average annual amount received over a two-year period. For stayers, the period is 2008–2009, while for mass-layoff separators, it includes the separation year and the year after. Less than 10% of workers with missing ROEs in the mass-layoff separator sample received EI in the year of separation or year the after.

Figure A14: Average annual earnings: Separators by ROE reason



Note: This figure shows average annual earnings over time for workers who separate from their job in 2008 for different reasons when their employer is experiencing a mass layoff in 2008–2009. Earnings are denominated in 2010 CAD and are rounded to the nearest 100 CAD because of confidentiality.

Table A1: Statistics on employment insurance (EI) benefits

	Stayers	Mass-layoff separators			
		Layoff	Quit	Other	Missing
Fraction received EI benefit	0.164	0.789	0.316	0.450	0.093
Average amount of total EI benefit received (among those received positive amount)	8,600	13,800	8,600	10,900	10,600

Note: This table provides the fraction of individuals received employment insurance (EI) benefits and average amount of total annual EI benefits received among those who receive EI during a two-year period. For stayers, the two-year period is 2008–2009. For mass-layoff separators, it is the separation year and the following year. Earnings are denominated in 2010 CAD and are rounded to the nearest 100 CAD because of confidentiality.

B.4 Worker mobility in Canadian data

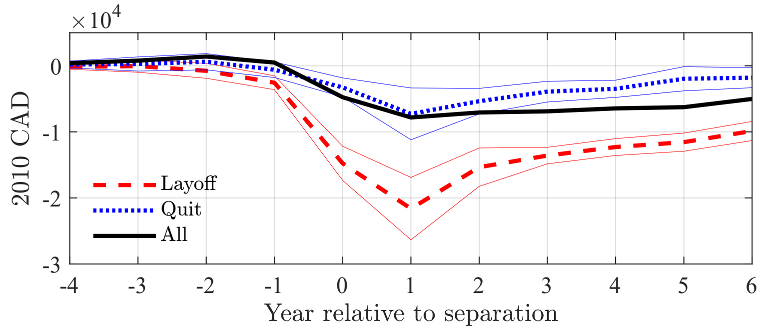
The employer-specific fixed effects in Equation (2) are identified by workers' transitions between employers. As such, limited worker mobility in the data yields to biased AKM estimates for the variance of employer effects, as shown by Abowd et al. (2003) and Andrews et al. (2012), leading to a misleading variance decomposition of $y_{i,t}$ into individual effects, employer effects, and sorting of individuals to employers. Although we do not focus on variance-covariance estimates from the AKM specification in our paper, in this section, we briefly summarize results in relation to worker mobility in Canada to mitigate these concerns on limited worker mobility bias. First, worker mobility in Canada is quite high as in the U.S. Between 2002 and 2014, the average monthly job-separation, job-finding, and job-to-job transition rates in Canada are 1.5%, 24.7%, and 0.73%, respectively.[4] Second, the average number of movers per employer in the AKM sample is 14, which is larger than the value of 10 reported in Lachowska et al. (2020) and also above the value of 6, below which limited mobility poses a problem (Andrews et al., 2012). The number of moves per person in our data is 0.533, while the number of moves per person-year is 0.084. This is in line with values calculated from the Washington data in Lachowska et al. (2020), which are 0.63 and 0.097,

[4]These are obtained from the monthly Labour Force Survey published by Statistics Canada.

respectively. Lachowska et al. (2020) also calculate implied mobility rates from the German admin data used by Card et al. (2013), Fackler et al. (2021), and Schmieder et al. (2023). They find moves per person and moves per person-year to be 0.19 and 0.03, respectively.

B.5 Earnings losses upon separations: Level changes

Figure A15: Job separation outcomes by reason: Level changes in earnings



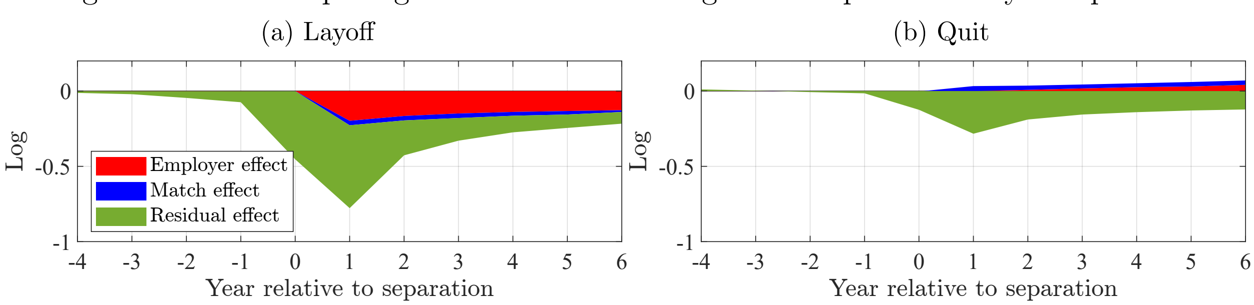
Note: This figure plots estimates for earnings losses in levels (2010 CAD) upon job separation by reason of separation during mass layoffs. Dashed-red and dotted-blue lines (along with 95% confidence intervals in solid lines) show estimated earnings losses for layoffs and quits, respectively, while solid-black line presents these losses for all mass-layoff separators (layoffs, quits, other, and missing ROE).

Figure A15 shows earnings losses (in 2010 CAD) by reason for separation during mass layoffs. Dashed-red and dotted-blue lines represent losses for layoffs and quits, while the solid-black line shows losses for all mass-layoff separators (layoffs, quits, other, and missing ROE). For layoffs (quits), earnings decline by $21,600 ($7,300) in the year following the separation and stay $9,900 ($1,800) lower six years after.

B.6 Decomposition of sources of earnings losses

We also provide results to complement our discussions in Section 3.2. Figure A16 presents a complete decomposition for the sources of earnings losses upon separations during mass layoffs for workers who are laid off (Panel (a)) and who quit (Panel (b)).

Figure A16: Decomposing the sources of earnings losses upon mass-layoff separation



Note: This figure presents a decomposition of earnings losses upon job separation by reason of separation, as indicated in the ROE, from employers experiencing a mass layoff. Earnings losses are decomposed into those attributable to changes in employer-premium, match, and residual effects.

Results in Panels (a) and (b) reveal that how employer effects contribute to explaining earnings losses substantially differ between layoffs and quits. For quits, match effects and employer effects are both positive and thus mitigate the decline in earnings due to a decline in residual effects. For layoffs, match effects are negative but small, while residual effects decline largely, especially in the short run. Quantitatively, for layoffs (quits), we find that employer, match, and residual effects are -20 (0), -3 (3), and -55 (-28) log points lower relative to stayers in the year following the separation, while these effects are -13 (4), -1 (3), and -8 (-12) log points lower relative to stayers, respectively, six years after the separation.

B.7 Employer premium dynamics by timing of separation

To complement our discussions in Section 4.2, Table A2 repeats Table 3. It presents five statistics for separations with a different timing (before the mass-layoff month (Panel A), around that month (Panel B), and after that month (Panel C)) and reason (layoff and quit), categorized by below-, on-, and above-diagonal transitions: (i) the fraction of separators, (ii) the average change in log earnings, (iii) employer effects, (iv) match effects, and (v) residual effects of the transition. A discussion of results from this table is provided in Section 4.2.

C Addressing endogenous mobility bias

Throughout the paper, we have highlighted the role of employer-premium ladder dynamics in shaping disparities in outcomes for workers who separate during mass-layoff events. We now discuss certain limitations of AKM-based estimates of employer-specific premia. We then demonstrate the robustness of our results when applying corrective measures.

Consider the AKM regression in Equation (2):

$$y_{i,t} = \kappa_i + \psi_{j(i,t)} + \lambda_t + \upsilon_{i,t},$$

where $y_{i,t}$ is log earnings of individual i in year $t = 1, 2, \ldots, T$, κ_i, ψ_j, and λ_t are fixed effects for worker, employer, and year, respectively, and $\upsilon_{i,t}$ is an error term that might include a time-varying match-specific component, satisfying $\mathbb{E}[\upsilon_t] = 0$.[5] To simplify our discussion, we abstract from time effects λ_t, which can be achieved by subtracting year-specific averages from log earnings, assuming that year-to-year changes in average log earnings do not reflect changes in worker or employer composition.

Since the employer effects are identified from movers, they are biased when mobility decisions are affected by the error term $\upsilon_{i,t}$. This will be the case, for example, when workers change employers after a negative shock to their match-specific component. To see this, consider workers moving from employer j in year t to employer j' in year t'. Their average

[5]Let x_t be a random variable and $x_{i,t}$ be its realization for individual i. Denote its cross-sectional first- and second moments by $\mathbb{E}[x_t]$, $\mathrm{Var}(x_t)$, and $\mathrm{Cov}(x_t, x_{t'})$.

Table A2: Below-, on-, and above-diagonal sums and averages, by timing of separation

	Below diagonal	On diagonal	Above diagonal
A. Separations before mass-layoff month			
(a) Layoff			
Share of separators	0.499	0.307	0.194
Average change in log earnings	-0.458	-0.100	0.211
Average change in employer effect	-0.489	-0.008	0.320
Average change in match effect	-0.003	-0.052	-0.147
Average residual effect	0.033	-0.040	0.038
(b) Quit			
Share of separators	0.275	0.373	0.353
Average change in log earnings	-0.063	0.116	0.215
Average change in employer effect	-0.345	0.007	0.328
Average change in match effect	0.212	0.055	-0.164
Average residual effect	0.069	0.055	0.051
B. Separations around mass-layoff month			
(a) Layoff			
Share of separators	0.451	0.375	0.174
Average change in log earnings	-0.305	0.005	0.168
Average change in employer effect	-0.390	0.004	0.300
Average change in match effect	0.033	-0.044	-0.167
Average residual effect	0.052	0.045	0.036
(b) Quit			
Share of separators	0.288	0.344	0.368
Average change in log earnings	-0.130	0.152	0.240
Average change in employer effect	-0.361	0.010	0.357
Average change in match effect	0.132	0.058	-0.118
Average residual effect	0.099	0.084	0.001
C. Separations after mass-layoff month			
(a) Layoff			
Share of separators	0.475	0.378	0.147
Average change in log earnings	-0.277	0.027	0.206
Average change in employer effect	-0.341	0.007	0.340
Average change in match effect	0.037	-0.040	-0.165
Average residual effect	0.028	0.059	0.031
(b) Quit			
Share of separators	0.304	0.390	0.306
Average change in log earnings	-0.043	0.160	0.348
Average change in employer effect	-0.324	0.042	0.383
Average change in match effect	0.217	0.025	-0.111
Average residual effect	0.064	0.094	0.076

Note: This table presents five rows for separations with a different timing (separations before the mass-layoff month (Panel A), around that month (Panel B), and after that month (Panel C)) and reason (layoff and quit) with below-diagonal, on-diagonal, and above-diagonal transitions: (i) the fraction of separators, (ii) average change in log earnings, (iii) average change in employer effect, (iv) average change in match effect, and (v) average residual effect of the transition. Below-diagonal transitions represent moves to an employer with a lower-quintile employer effects, on-diagonal (above-diagonal) transitions represent moves to a same-quintile (higher-quintile) employer. Values are based on a comparison of employment one year before and three years after separation.

earnings growth between years t and t' is

$$\underbrace{\mathbb{E}[y_{t'} - y_t | j_{t'} = j', j_t = j]}_{\text{earnings change for movers}} = \underbrace{\psi_{j'} - \psi_j}_{\text{change in employer premium}} + \underbrace{\mathbb{E}[\upsilon_{t'} - \upsilon_t | j_{t'} = j', j_t = j]}_{\text{endogenous mobility bias}}.$$

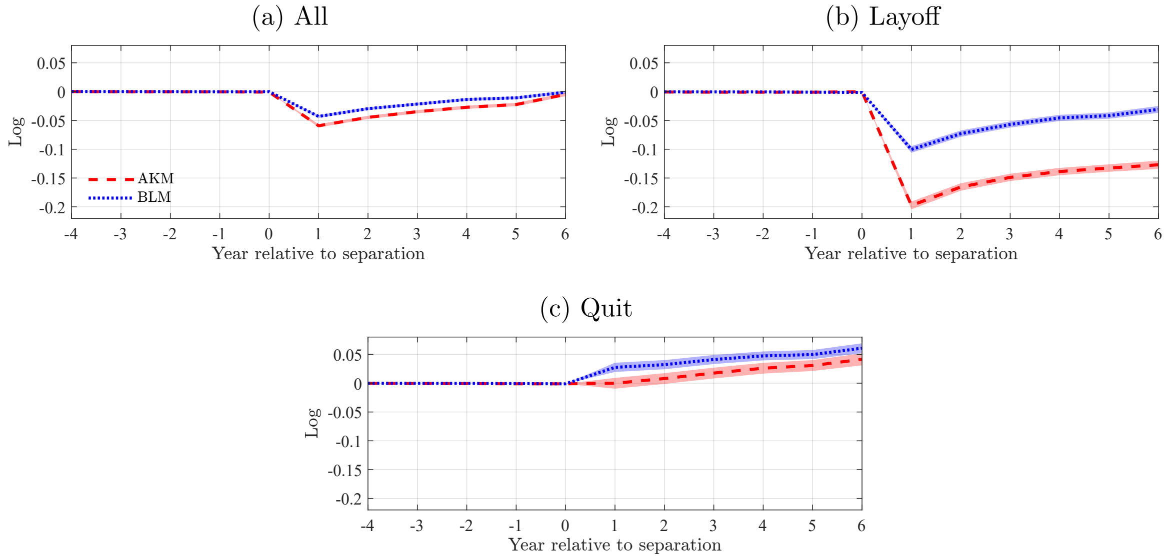
It is easy to see that the above equation identifies employer premium under the exogenous mobility assumption of AKM, which implies $\mathbb{E}[\upsilon_{t'} - \upsilon_t | j_{t'} = j', j_t = j] = 0$.

We outline several reasons endogenous mobility might arise (see Card, Heining, and Kline, 2013 for more details). Consider different interpretations of the error term and how they might give rise to endogenous mobility bias. First, sorting might occur based on idiosyncratic match effects. Especially for voluntary moves, workers are likely to move towards jobs with higher match effects leading to an upward bias in the AKM estimator (i.e., $\mathbb{E}[y_{t'} - y_t | j_{t'} = j', j_t = j] > \psi_{j'} - \psi_j$). In contrast, workers in employers undergoing a mass layoff may be more likely to move to employers with which they have lower match effects if they expect a decline in the match effect with their incumbent employer or if they are laid-off and must find a job quickly in a depressed labor market. Finally, match effects may also reflect job-specific human capital that is destroyed upon job-separation, in which case the AKM estimator is biased downward. Second, time-varying worker effect may also predict transitions from one employer to another. This might reflect changes in workers' general skills or in their bargaining power with their current and potential employers. For example, under positive assortative matching, a worker may move to a better-paying employer after a positive shock to their general skill, in which case the bias is positive for upward moves and negative for downward moves. Meanwhile, mass layoffs are likely to deteriorate workers' bargaining power and downward bias employer premium losses.

Overall, the problem of endogenous mobility bias arises because the AKM framework does not allow for worker mobility to depend on earnings realizations conditional on worker and employer heterogeneity. Bonhomme et al. (2019) (BLM) proposed a method to address the endogenous mobility bias based on the assumption that log earnings and mobility decisions follow a joint first-order Markov process in a general model that features time-varying interaction between worker and employer heterogeneity. In Supplemental Appendix Section 1, we show how the first-order Markov assumption (combined with additional mild assumptions) leads to a simple bias-correction procedure to the AKM estimator (which assumes additive and time-invariant worker and employer effects as in Equation (2)), as long as $\upsilon_{i,t}$ is not a permanent component.[6] This procedure allows us to address the endogenous mobility bias that originates from time-varying match or worker effects that are transitory in nature, but it does not allow the bias to solely reflect time-invariant match effect. To account for time-invariant match effects, we next show in Supplemental Appendix Section 2 that a similar bias-correction method can be applied to the case of time-invariant interaction between worker and employer effects, a special case of the time-varying model of BLM. Using our dynamic model, we find that endogenous mobility, by which earnings shocks affect mobility

[6] A separate Supplemental Appendix for this paper is available in the following link: https://tinyurl.com/bdc9hz9e.

Figure A17: Effects of mass-layoff separation on BLM employer-specific premium



Note: This figure plots estimates for employer-specific pay premium dynamics upon job separation by reason of separation during mass layoffs. Panels (a), (b), and (c) show estimates for all mass-layoff separations, layoffs, and quits, respectively. The red-dashed lines show estimated γ_k^s from Equation (3) using the baseline AKM procedure, while the blue-dotted lines present results using the BLM procedure.

decisions, and state dependence and network effects, by which past employers have an impact on earnings after a job move, are features of our data.

C.1 Additive worker and employer effects

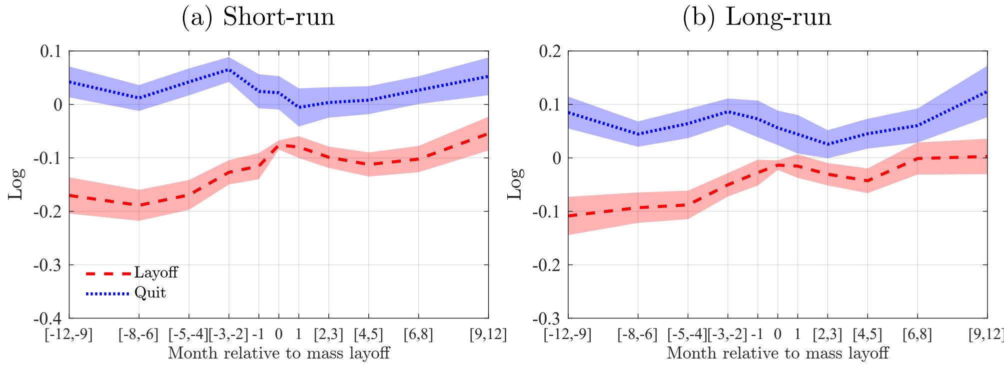
Here, we use the model that assumes additive worker and employer effects and run the same regressions as in Equation (3) to examine how our findings on the dynamics of employer premia upon a layoff or quit change.

Figure A17 compares the dynamics of employer-specific pay premia by separation reason during mass layoffs. It shows AKM premia from Equation (3) (red dashed) and BLM-corrected premia (blue dotted). Panels (a), (b), and (c) present results for all mass layoff separations, layoffs, and quits, respectively. Two key observations follow.

First, BLM estimates result in slightly lower losses upon separation when we consider all separators (4 vs 6 log points). However, Panel (b) shows a wider gap for layoffs. While BLM estimates decrease by 10 log points, AKM estimates drop by 20 log points. This divergence persists even in the long run. For quits, employer premia six years post-separation increase more under BLM (6 vs 4 log points). Overall, while both BLM and AKM employer effects have a large role in explaining earnings losses for layoffs, their roles are smaller under BLM.

Second, these results also shed new light on the differences between employer premia estimates from the widely used AKM model and bias correction approaches like BLM. Our comparison shows that $\mathbb{E}[\Delta\hat{\psi}_{\text{AKM}}] < \mathbb{E}[\Delta\hat{\psi}_{\text{BLM}}]$ for displaced workers, particularly for layoffs. This suggests that, in our context, the AKM model underestimates employer pay premia

Figure A18: BLM employer effects upon separation by timing of separation



Note: This figure plots estimates for employer premium dynamics upon separations for layoffs and quits when they are also grouped by their proximity to the mass-layoff month. Instead of using AKM estimates for employer premia (as in Figure 8), results presented use BLM bias-corrected premia. Panel (a) presents estimated outcomes one year post-separation (short-run), while Panel (b) presents estimates for six years after (long-run). For each employer experiencing a mass layoff, the mass-layoff month is identified as the month during which the largest number of ROE layoffs are recorded. 95% confidence intervals are given by the shaded regions.

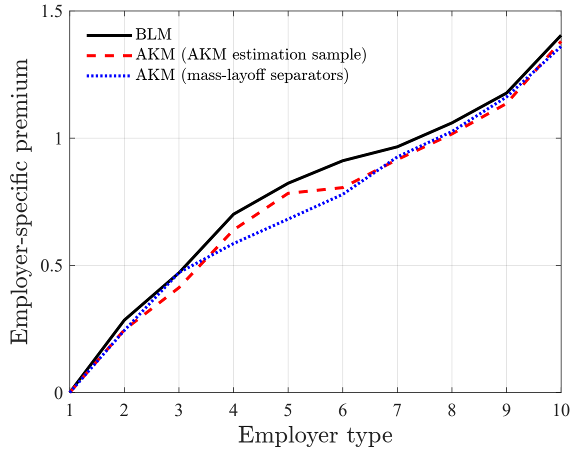
associated with upward mobility (often linked to quits) and overestimates the losses from downward moves (commonly associated with layoffs). What might explain this disparity? Recall that our analysis focuses on origin employers that experienced mass layoffs during the Great Recession. Even when we exclude displaced workers from the AKM estimation, many workers displaced from these employers (e.g., low-tenure workers) remain in the AKM sample. If these workers experience larger earnings losses, potentially due to a decline in time-varying match effects, the AKM employer premium will be biased downward. The fact that the gap, as implied by $\mathbb{E}[\Delta\hat{\psi}_{\text{AKM}}] < \mathbb{E}[\Delta\hat{\psi}_{\text{BLM}}]$, is less pronounced for quits suggests that such workers who left these origin employers experienced a less severe drop in their match-specific component (e.g., possibly due to a stronger local labor market).

We end our discussion with a comparison of employer premium dynamics under AKM and BLM when we consider the proximity of separations to the mass-layoff month. Figure A18 replicates Panels (c) and (d) of Figure 8, showing the dynamics of employer premia by proximity to a mass layoff. Unlike Figure 8, which uses AKM estimates, Figure A18 presents employer premia derived from the BLM bias-correction.

We note two findings. First, the differences in employer effects between separations before and after the mass-layoff month remain large. Second, the BLM estimates imply an even steeper slope of employer premium losses with respect to a laid-off worker's time of separation. Panel (a) shows that workers laid off six months before the mass-layoff month face a nearly 20-log-point drop in employer effects, while those laid off six months after see a drop of 10 log points. Similar conclusions hold for long-run outcomes (Panel (b)).

Additional results. Figure A19 presents the estimated employer-specific premia by employer type. The solid-black line represents the premia estimated using the procedure de-

Figure A19: Employer-specific premium by employer type under AKM and BLM



Note: This figure plots estimated employer-premium effects by employer type. The solid-black line shows the estimated employer-specific premia by employer type estimated with the procedure that largely follows BLM. For the red-dashed and blue-dotted lines, we obtain AKM employer premium averages for each employer type on the x-axis over each worker-year observation in two different samples. The red-dashed line represents averages for the AKM estimation sample (which excludes stayers and mass-layoff separators), while the blue line represents averages for mass-layoff separators (not including stayers).

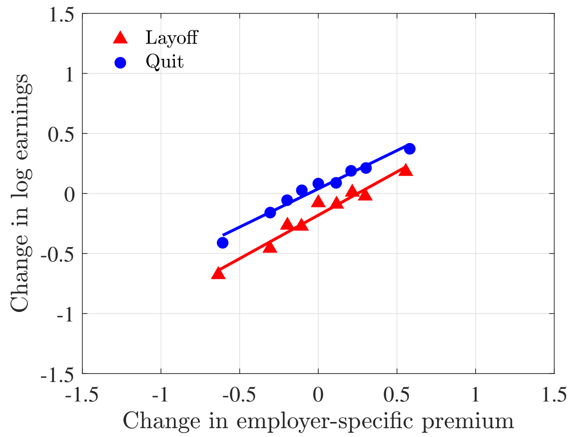
tailed in Supplemental Appendix Sections 1 and 3. For the red-dashed and blue-dotted lines, we calculate the average AKM employer premia for each employer type (x-axis) across worker-year observations from two distinct samples. The red-dashed line shows averages for the AKM estimation sample, which excludes stayers and mass-layoff separators, while the blue-dotted line represents averages for only mass-layoff separators.[7] We observe that the estimated employer premia are quite similar in both the BLM estimates and the AKM estimates from the AKM estimation sample, indicating that endogenous mobility bias is not a significant concern in the AKM sample (which excludes mass-layoff separators). However, a more pronounced difference emerges when comparing the BLM-corrected estimates with the AKM averages for the mass-layoff separator sample. Specifically, mass-layoff separators tend to be associated with much lower employer-specific premia on average.

Finally, Figure A20 plots changes in log earnings for layoffs and quits when we divide each group of mass-layoff separators into nine groups based on the distribution of changes in BLM employer premium estimates $\Delta\hat{\psi}_{\text{BLM}}$: quartiles for negative values (4 groups), zeros (1 group), and quartiles for positive values (4 groups). This results in nine points for each type of separation in the scatter plot. These results are obtained by comparing outcomes between one year before and three after separation. Similar to our baseline result in Figure 6 Panel (a), Figure A20 shows that workers who quit experience better earnings outcomes than those who are laid-off, even after controlling for changes in employer effects. However, one difference is that under the AKM estimates, the slope between two axes was larger for

[7]For the mass-layoff separator sample, we average employer premia at the worker-year level from 2002–2014, consistent with our sample. For the AKM sample, we use 2002–2015, the time period implied by utilizing the full dataset (2001–2016) but excluding the first and last year on the job.

layoffs than for quits. Under the BLM estimates, the slopes are more or less similar.

Figure A20: Changes in earnings vs. BLM employer-specific premium



Note: This figure plots changes in log earnings for layoffs and quits when we divide each group of mass-layoff separators into nine groups based on the distribution of $\Delta\hat{\psi}_{\mathrm{BLM}}$: quartiles for negative values (4 groups), zeros (1 group), and quartiles for positive values (4 groups). This results in nine points for each type of separation in the scatter plot. These results are obtained by comparing outcomes between one year before and three after separation.

C.2 Interactive worker and employer effects

The bias-correction procedure for the model with additively separable worker and employer effects does not allow the bias to be entirely driven by time-invariant match effects, since it requires $\rho_{4|3}(k') \neq 1$ and $\rho_{1|2}(k) \neq 1$, as detailed in Supplemental Appendix Section 1. Now, we explicitly account for time-invariant match-specific component in the log earnings equation. Here, there cannot be distinct worker or employer effects in addition to the match effect, because the match effect can be thought of as interactive worker and employer effects. Thus, we consider a model that only includes interactive worker and employer effects:

$$y_{i,t} = \phi_{l_i,k_{i,t}} + \upsilon_{i,t}, \tag{A3}$$

where $l_i \in \{1, 2, \ldots, L\}$ represents discrete *type* of individual i, which entirely characterizes unobserved heterogeneity of the worker, $\phi_{l,k}$ is the effect of a match between worker type l and employer type k, and the error term satisfies $\mathbb{E}[\upsilon_t | l, k_t] = 0$.

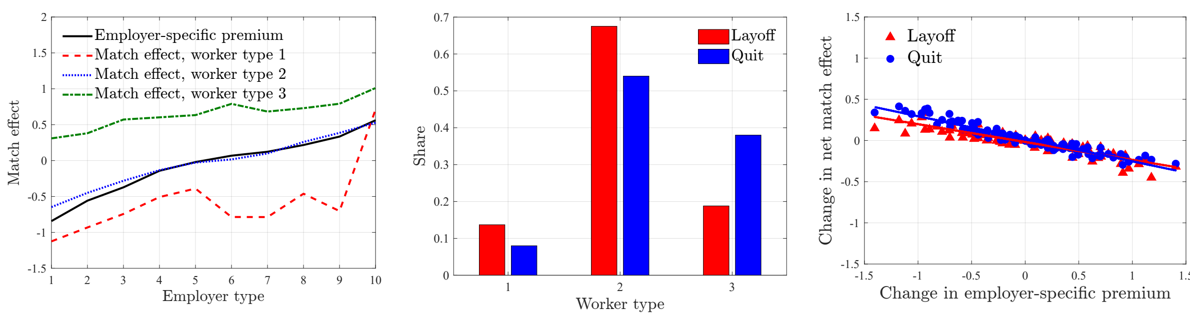
The interactive specification in Equation (A3) is more general and becomes equivalent to the additively specification in Equation (2) when $\phi_{l,k} = \kappa_l + \psi_k$. Note that the match effect $\phi_{l,k}$ is slightly different from that in the specification for the Woodcock estimator (μ_{ij}), which reflects the match effect *net* of worker and employer effects. However, it should be emphasized that the Woodcoock estimator would be based on a mis-specified model if the specification the Equation (A3) is correct, and it does not address the the endogenous mobility bias originating from the error term.

Notice that, with the interactive worker and employer effects, there is no single employer premium that can be applied to all workers because changing employers now have different effects across workers. Therefore, the model with interactive worker and employer effects must be estimated based on our main sample of stayer and mass-layoff separators rather than a separate AKM sample. Supplemental Appendix Sections 2 and 4 provide details on the identification and estimation strategies of this model.

Figure A21 presents the key findings that emerge from the estimation procedure outlined in Supplemental Appendix Sections 2 and 4. Panel (a) shows the estimated match effect $\phi_{l,k}$ for each worker type $l \in \{1, 2, 3\}$ and employer type $k \in \{1, 2, \ldots, 10\}$. The estimated match effects are generally increasing in employer type, suggesting strong complementary between employer and worker types. Panel (b) plots the distribution of separators across each worker type, by reason for separation. It shows that workers who quit are more likely to be higher-type workers than those who are laid-off. Finally, Panel (c) shows the average change in match effects net of employer effects $\phi_{l,k} - \psi_k$, where each dot represents an employer-type transition, for a total of 10×10 possible moves. These results are obtained by comparing outcomes between one year before and three after separation. Similar to our baseline result for the Woodcock estimator in Figure 6 Panel (b), workers that experience larger declines in BLM employer-specific premia also experience larger declines in net match effects. Notably, the steeper slope for quits suggests that declines in employer-specific premia for quits are more likely to coincide with increases in match effects, as in Figure 6 Panel (b).

Figure A21: Key results from estimation of time-invariant match effects

(a) Match effect ($\phi_{l,k}$) vs. emp. premium (ψ_k) | (b) Distribution of unobserved worker types | (c) Δ net match effect ($\phi_{l,k} - \psi_k$) vs. emp. premium (ψ_k)



Note: This figure plots the main results from the estimation of time-invariant match effects. Panel (a) shows the estimated match effect $\phi_{l,k}$ for each worker type $l \in \{1, 2, 3\}$ and for each employer type $k \in \{1, 2, \ldots, 10\}$. Panel (b) plots the distribution of separators across each worker type, by reason for separation. Panel (c) shows the average change in match effects net of employer effects $\phi_{l,k} - \psi_k$, where each dot represents a employer-type transition, for a total of 10×10 possible moves. These results are obtained by comparing outcomes between one year before and three after separation.

D Interaction between worker and employer outcomes

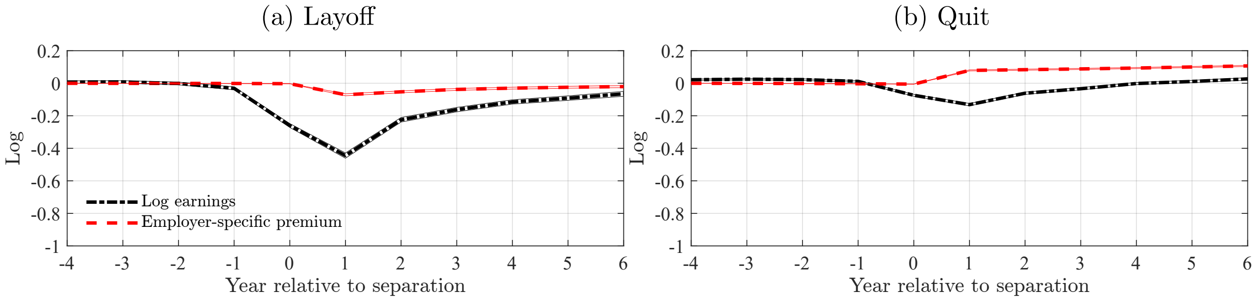
In this section, we present results to supplement our discussions in Section 5. In particular, we document that both the composition and the impact of separations differ greatly between distressed employers undergoing mass layoffs and employers that are *not*. These findings are relevant for two reasons. First, they demonstrate that one cannot treat worker separation outcomes as distinct and isolated from employer outcomes. Second, these results provide further evidence for why the literature has documented worse outcomes for workers who separate from their employers during recessions, during which mass layoffs are much more prevalent, as we discuss below.

Distribution of separations. We first document differences in the composition of separations compared with a mass-layoff event. Based on ROE reasons, a majority of non-mass-layoff separations are because of quits. In particular, among all non-mass-layoff separations with non-missing ROE information, 55% are quits, while only 18% are layoffs. This is very different than the composition of mass-layoff separations where layoffs are much more prevalent than quits, as shown in Table 1.

Earnings outcomes. Next, we provide estimated earnings and employer-specific pay premium losses separately by reason for separations that do not occur during a mass layoff. The results from this exercise are shown in Figure A22. When comparing the estimates for separations originating from mass layoffs presented in Figure 3, we highlight two results. First, earnings losses associated with a separation outside a mass layoff are much smaller and less persistent. Declines in earnings in the year following layoffs and quits are much smaller for non-mass-layoff separations (45 log points and 13 log points) when compared with declines in earnings upon layoffs and quits for mass-layoff separations (78 log points and 25 log points). In addition, for non-mass-layoff separations, earnings upon quits fully recover three years after the separation and earnings upon layoffs remain only 7 log points lower six years following the separation. Overall, the gap between earnings losses upon layoffs and quits for non-mass-layoff separations is much smaller than that for mass-layoff separations both in the short run and in the long run. Second, non-mass-layoff separations are also associated with a smaller and less-persistent decline in employer-specific pay premium for laid-off workers and a much larger increase in employer-specific pay premium for workers who quit. In particular, employer premium declines by only 7 log points in the year following the layoff and almost fully recovers six years after the layoff. In fact, workers who quit experience a long-lasting 11-log-point gain in their employer premium. As such, when a worker quits from an employer not undergoing a mass layoff, the worker experiences a temporary small decline in earnings but typically finds reemployment with an employer that pays more on average.

Cross-sectional earnings loss differences between layoffs and quits. To understand the underlying reasons behind these results, we now turn to cross-sectional differences in the consequences of non-mass-layoff separations, as in Section 3.5.

Figure A22: Effects of job separation by reason for separation: Non-mass layoffs



Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation from employers that are *not* experiencing mass layoffs. Panel (a) presents estimates for separations due to layoff and Panel (b) presents estimates for separations due to quit. Dashed-dotted-black lines show estimated γ_k^s values (along with 95% confidence intervals given by solid-black lines) in Equation (1), and dashed-red lines present estimated γ_k^s values (along with 95% confidence intervals given by solid-red lines) in Equation (3).

Table A3: Below-, on-, and above-diagonal sums and averages: Non-mass layoffs

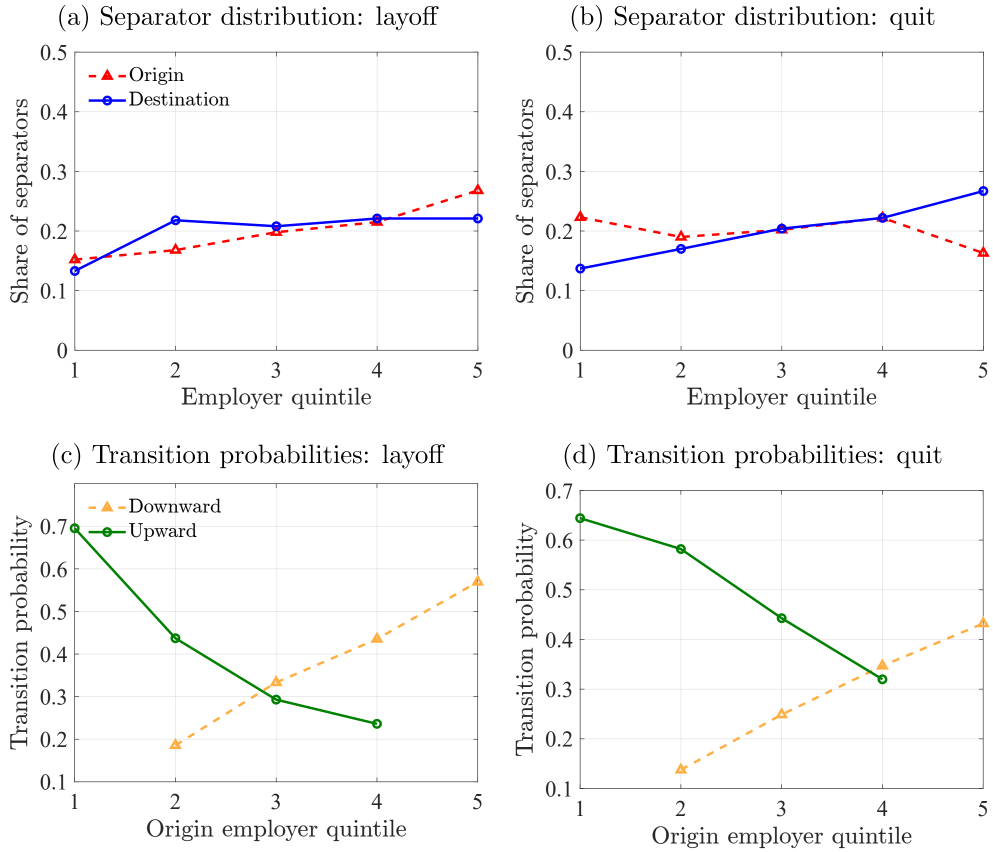
	Below diagonal	On diagonal	Above diagonal
(a) Layoff			
Share of separators	0.343	0.369	0.287
Average change in log earnings	-0.162	0.075	0.308
Average change in employer effect	-0.367	0.003	0.352
Average change in match effect	0.167	0.023	-0.075
Average residual effect	0.038	0.049	0.030
(b) Quit			
Share of separators	0.223	0.360	0.413
Average change in log earnings	0.018	0.185	0.381
Average change in employer effect	-0.335	0.014	0.424
Average change in match effect	0.298	0.115	-0.080
Average residual effect	0.056	0.056	0.037
(c) Average			
Share of separators	0.252	0.362	0.383
Average change in log earnings	-0.041	0.158	0.368
Average change in employer effect	-0.346	0.011	0.411
Average change in match effect	0.255	0.093	-0.079
Average residual effect	0.050	0.054	0.035

Note: This table presents five rows for non-mass-layoff separations with a different reason (layoff, quit, or average across layoffs and quits) with below-diagonal, on-diagonal, and above-diagonal transitions: (i) the fraction of separators, (ii) average change in log earnings, (iii) average change in employer effects, (iv) average change in match effects, and (v) average residual effects of the transition. Below-diagonal transitions represent moves to an employer with a lower-quintile employer effects, on-diagonal and above-diagonal transitions represent moves to a same-quintile employer and to a higher-quintile employer, respectively. Values are based on a comparison of outcomes between one year before and three years after separation.

Table A3 presents fractions of separations, average changes in earnings, underlying reasons behind these changes in earnings (employer, match, and residual effects) based on below-, on-, and above-diagonal transitions between origin and destination employer-specific pay premium quintiles. Starting with the share of separators, workers who are laid-off during non-mass layoffs are less likely to find reemployment with employers in a lower quintile of the employer-specific pay premium distribution relative to workers who are laid-off during mass layoffs, as shown in Table 3 (34.3% vs 46.8%). Workers who quit during non-mass layoffs are also less likely to fall to lower quintiles and more likely to climb to higher quintiles of the distribution relative to workers quit who during mass layoffs (22.3% vs 28.8% for below-diagonal transitions and 41.3% vs 33.5% for above diagonal transitions). Relative to laid-off workers in mass layoffs (Table 3), laid-off workers in non-mass layoffs experience smaller average earnings losses when their destination employer is in a lower quintile of the employer-specific pay premium distribution (-16.2 vs -33.2 log points) and larger average earnings gains when their destination employer is in a higher quintile of the distribution (30.8 vs 15.9 log points). These differences in earnings changes between laid-off workers in non-mass layoffs and mass layoffs are mostly accounted for by differences in average changes in match effects (16.7 vs 3.7 log points for below diagonal transitions and -7.5 vs -16.7 log points for above diagonal transitions). On the other hand, workers who quit during non-mass layoffs do not experience earnings losses when they find reemployment with employers in lower quintiles but experience large earnings gains (38.1 log points) when they are reemployed at employers with higher quintiles. These results are different for workers who quit during mass layoffs: They experience close to a 10-log-points decline in earnings when they fall in the employer-premium quintile upon separation and experience a smaller increase (27.7 log points) in earnings when they climb in the distribution upon separation. These differences in earnings changes between workers who quit in non-mass layoffs vs in mass layoffs are driven by differences in match effects (29.8 vs 19.1 log points for below-diagonal transitions and -8 vs -13 log points for above-diagonal transitions).

Finally, Figure A23 presents more-detailed results on shares and transition probabilities across quintiles of the employer-specific pay premium distribution for non-mass-layoff separators who are laid-off (Panel (a) and Panel (c)) and workers who quit (Panel (b) and Panel (d))). Relative to the same moments documented in Figure 5, we highlight the following differences. First, while the incidence of layoffs increase in employer-effects quintile of the origin employer for both non-mass-layoff and mass-layoff separations, this profile is much flatter for the former group. In particular, the red-dashed line in Panel (a) shows that 15% and 27% of all layoffs originate from the bottom and top quintiles for non-mass-layoff separations, respectively, while these fractions are 7% and 35% for mass-layoff separations. Second, because the distributions of layoffs among non-mass-layoff separations across origin and destination employer effects quintiles closely track each other (red-dashed and solid-blue lines in Panel (a)), the overall distribution of employer effects remains largely unchanged following layoffs during non-mass layoffs. This is very different from layoffs during mass layoffs, where the distribution of employer effects shifts leftward upon separations, leading to a substantial net decline in employer premium position. Third, dashed-orange and solid-green lines in Panel (c) show that downward transition probabilities are smaller and upward transition probabil-

Figure A23: Transitions across the employer effect distribution: Non-mass layoffs



Note: Panels (a) and (b) present the distribution of separations by origin (dashed-red lines) and destination (solid-blue lines) quintiles for both layoffs and quits that do not occur during a mass layoff, respectively. Panels (c) and (d) present upward (solid-green lines) and downward (dashed-orange lines) transition probabilities by origin employer effect quintiles for layoffs and quits that do not occur during a mass layoff, respectively. Values are based on a comparison of outcomes between one year before and three years after separation.

ities are larger for layoffs during non-mass layoffs regardless of the employer effect quintile of the origin employer. Fourth, moving to quits, the dashed-red line in Panel (b) shows that the fraction of all separations that originate from workers separating from employers in the bottom quintile is higher for non-mass-layoff separations than for mass-layoff separations (22% vs 14%). Finally, the upward transition probability is typically higher for workers who quit during non-mass layoffs (solid-green line in Panel (d)) than for workers who quit during mass layoffs especially for lower origin quintiles.