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

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Abstract

When estimating earnings losses upon job separations, existing strategies focus on separations in mass layoffs to distinguish involuntary separations from voluntary separations. We revisit the measurement of the sources and consequences of involuntary separations using Canadian job separation records. We refine existing strategies and find that only a quarter of mass-layoff separations are indeed layoffs. Isolating mass-layoff separations that reflect involuntary displacement, we find twice the earnings losses relative to existing estimates. We also uncover significant heterogeneity in losses for separations with different reasons and timing, ranging from 10% for quits after a mass layoff to 60% for layoffs before it. Our findings are relevant for quantitative models that take earnings loss estimates as key inputs to discipline parameters, analyze mechanisms behind these losses, and evaluate policy.

Keywords: Job displacement, Earnings losses, Layoffs vs quits, Employer effects
JEL Codes: E24, E32, J31, J63, J65

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1 Introduction

A large and active literature pursues estimates of the magnitude and sources of earnings losses following a job displacement. Estimates of displacement outcomes are of foremost empirical interest, as they shed light on outcomes upon involuntary job separation; they are also pivotal inputs to quantitative models used for macroeconomic and policy analysis. To arrive at these estimates, the literature has often relied on employer-employee-matched administrative data, where a job separation is detected by a change in a worker's employer identifier from one period to another. Thus far, a common strategy adopted to address selection and capture involuntary separations is to focus on separations of long-tenure workers during large employment contractions within a particular employer, events labeled as "mass layoff" (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010; Davis and von Wachter 2011; Lachowska, Mas, and Woodbury 2020; Schmieder, von Wachter, and Heining 2023, among others).[1] While this strategy is plausible, relying on employer identifier changes during mass layoffs alone to capture involuntary separations introduces new challenges. For instance, it is not clear that all separations during employer contractions are involuntary, as some workers may quit during such episodes. Further, the protracted nature of employer distress implies that estimates may depend heavily on how systematic decisions by employers and workers shape the composition of separations.

This paper leverages Canadian administrative data, which uniquely includes detailed information on the reason and exact timing of separations. In particular, we use the Canadian Employer-Employee Dynamics Database (CEEDD), annual employer-employee-matched data that link various individual-, employer-, and job-level tax forms, covering the universe of individual and corporate tax filers. Crucially, we merge this database with Record of Employment (ROE) data, forms which employers are legally obligated to issue during separations and are primarily used to determine eligibility for transfer programs, such as employment insurance (EI). The ROE contains information on the beginning and end dates of employment and the reason for separation. We emphasize that Canada's regulatory environment lends itself naturally to accurate reporting on the ROE. First, unlike the U.S., employers' payroll taxes for funding EI are not subject to experience rating, limiting an employer's incentives to

[1] Survey data, while useful for capturing reasons for job separation at high frequency, often suffer from small sample sizes, measurement errors, and the inability to link workers to their employers.

misreport a layoff as a quit. Second, disguising a quit as a layoff is also less likely as employers who do so become liable for paying severance packages. Finally, verification mechanisms through labor laws exist for rare cases when an employee and employer disagree with ROE information. Overall, our data allow us to (i) evaluate the efficacy of the existing strategy in identifying involuntary separations, (ii) distinguish separations that conform to the notion of involuntarily displaced workers, and (iii) shed light on outcomes of different types of mass-layoff separations. We provide four main findings that advance our understanding of outcomes upon job separations.

First, existing mass-layoff identification strategies used to detect involuntary job losses mix separations that arise from a variety of reasons. We find that only a *quarter* of mass-layoff separations are actual layoffs, while 12% are quits. Surprisingly, 45% are *not* even actual job losses but are employer ID changes due to reorganization (e.g., business ownership changes, mergers, acquisitions), and the rest are separations for idiosyncratic reasons (e.g., parental leave, illness, retirement). While data limitations force the hands of researchers to implement this identification strategy, our results demonstrate the shortcomings of identifying involuntary separations without utilizing the exact reason for separation. Importantly, we take advantage of the ROE information to offer guidance to researchers without ROE information to ameliorate this problem by filtering out spurious separations from genuine layoffs or quits.

Second, the average earnings loss in the year following the separation for *all* mass-layoff separators is 28% in the Canadian data, well within the range of existing estimates obtained using administrative data from the U.S. and Europe (18%–46%).[2] However, utilizing ROE information on the reason for separation in our data, we find that involuntary mass-layoff separators experience a much larger loss of 54%. Thus, we offer estimates that better reflect the outcomes of involuntary job separators, which existing studies could not fully capture due to data limitations. Beyond its intrinsic empirical value, this finding is relevant for quantitative models that take earnings loss estimates as key inputs to discipline parameters, analyze mechanisms behind these losses, and evaluate policy (Huckfeldt 2022; Jarosch 2023; Braxton, Herkenhoff, and Phillips 2023). For instance, using the smaller estimates understates the role of the job ladder in explaining earnings losses and the insurance value of public transfers.

Third, we show that the magnitude and sources of earnings losses differ greatly

[2] On the other hand, available U.S. survey-based estimates on average earnings losses in the year following the separation are even lower. They fall between 14% and 30%.

across separations with different reasons. We find that earnings and employer-specific premium (á la Abowd, Kramarz, and Margolis 1999, henceforth AKM) losses are larger and more persistent for layoffs than for quits. In the year following a mass-layoff separation during the Great Recession, the average earnings loss is 54% for layoffs but only 22% for quits. Six years after the separation, earnings remain 20% lower for layoffs but only 5% lower for quits when compared with job stayers. We also show that employer effects are important in explaining earnings losses for layoffs but not for quits. For layoffs, the loss of employer premium accounts for 26% (59)% of short-term (long-term) earnings losses. For quits, the average employer-specific premium does not change upon separation and becomes 4% higher six years after. Overall, we show that the AKM model with additive worker and employer effects explains earnings changes better for layoffs than for quits. We conjecture that this finding may help explain recent divergent results on the role of lost employer effects in explaining earnings losses obtained using data from Washington state (Lachowska, Mas, and Woodbury, 2020) vs Germany (Schmieder, von Wachter, and Heining, 2023). Since employer effects are important only for layoffs, country-specific compositional differences between layoffs and quits influence the overall role of employer effects. Finally, since AKM estimates rely on an exogenous mobility assumption, we also undertake bias-correction measures to obtain alternative estimates of employer-premia as in Bonhomme, Lamadon, and Manresa (2019). With these alternative estimates, employer effects still play a larger role in explaining the earnings losses of those who are laid-off.

Finally, using ROE information on the exact separation date, we analyze the second key dimension of heterogeneity in separations that has been understudied in the literature: timing. 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. Next, to understand whether the timing of separation matters for its consequences, we estimate the dynamics of earnings and employer effects for separations before and after the mass-layoff month, separately for layoffs and quits. Overall, we uncover 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 the mass-layoff month. Among layoffs, earnings and employer premium losses are larger for those who are laid off before the mass-layoff month than for those who are laid off after. In terms of worker characteristics, we find that 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. Taken as a whole, these findings suggest that employers lay off less-productive workers first during a severe contraction. 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 can retain their jobs throughout the employer contraction enjoy sufficient time to sample job 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 random but instead is an outcome of strategic decisions made by employers and workers.

Related literature. This paper contributes to the extensive literature analyzing the consequences of job displacement, drawing on administrative data from various countries (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010; Davis and von Wachter 2011; Bertheau et al. 2023, among others) and U.S. survey data (Ruhm 1991; Stevens 1997; Stephens 2002; Birinci 2021, among others). The substantial and long-lasting losses associated with job displacement have spurred investigations into the causes of these losses and how outcomes differ across workers. In particular, 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), and occupational switching (Huckfeldt 2022), among others. Another strand of research has focused on various dimensions of heterogeneity in terms of displaced worker outcomes: by reason for separation (Flaaen, Shapiro, and Sorkin 2019), proximity to mass-layoff events (Schwerdt 2011), tenure (Cederlöf 2024), employer size (Fackler, Mueller, and Stegmaier 2021), and exposure to local economic shocks and mobility (Gathmann, Helm, and Schönberg 2020), among others.

We build on previous work that either focused on specific dimensions of heterogeneity or analyzed the sources of average losses. At the core of our approach is treating mass-layoff events as *protracted* processes during which a *variety* of separations occur. Using administrative employer-employee-matched data from Canada merged with rich job separation records, we advance the literature along two dimensions.[3] First, we document and analyze the intricate anatomy and progression of mass-layoff events. On the worker side, we examine the composition of workers separating from distressed employer, both in the cross-section and over the protracted duration of these mass layoffs. We not only uncover sizable heterogeneity in worker outcomes, but also analyze the underlying sources of this variation. On the employer side, we explore how employer-level outcomes influence worker outcomes, comparing different separation scenarios: mass layoffs vs. non-mass layoffs, and employment downsizing vs. plant closures. Second, we reveal the limitations of the existing methods used in the literature to identify involuntary separations and offer corrective measures.

Our paper is most closely related to Flaaen, Shapiro, and Sorkin (2019), Lachowska, Mas, and Woodbury (2020), and Schmieder, von Wachter, and Heining (2023). The novel approach of Flaaen, Shapiro, and Sorkin (2019) combines administrative and survey data to distinguish between layoffs and quits during mass layoffs in the U.S. Similar to our results, they show that earnings losses are larger and more persistent following separations due to distress (covering layoffs, employer distress, and slack work conditions) than quits. Relative to this paper, our data allow us to link employees to their employers for *all* separations, not just those that appear in the survey data and provide a response to the survey question on the reason for separation. We not only document the differential magnitudes of earnings losses between layoffs and quits but also demonstrate how (i) the composition of layoffs and quits change substantially during the course of a mass layoff and (ii) the underlying sources behind earnings losses of both types of separations differ. We uncover drastically different roles of employer effects in explaining earnings losses for layoffs and quits, and show that the AKM model with additive worker and employer effects explain earnings changes better for layoffs than for quits. Finally, we provide novel findings on how worker characteristics and outcomes differ by timing of separation and argue that the pool of mass-layoff separators is a result of strategic decisions made by employers and workers. On the other hand, Lachowska et al. (2020) and Schmieder et al. (2023) focus on revealing the sources of average earnings losses upon job displacement, focusing on the role of employer premia and sorting. Compared to both, we distinguish the

[3] The information contained in Canadian job separation records has been used for other applications. Nakamura et al. (2019) and Nakamura et al. (2020) use the ROE to measure gross worker flows in Canada without a time-aggregation bias. Bowlus et al. (2022) use the ROE to compare short-run earnings growth distribution of stayers, laid-off separators, and non-laid-off separators.

relative importance of each component based on the reason and timing of separations. Importantly, we conjecture that our findings may help reconcile divergent results in these two studies on the role of employer effects in explaining earnings losses.

The rest of the paper is organized as follows. Section 2 explains our data and empirical methodology. Section 3 provides results when we differentiate mass-layoff separations based on the reason for separation, and Section 4 documents results when we group them based on timing of separation around mass layoffs. Section 5 presents results under our AKM bias-correction procedure and results in the context of non-mass layoffs. Finally, Section 6 provides concluding remarks.

2 Data and empirical methodology

In this section, we provide details about our database, focusing on its novel aspects and how it allows us to differentiate separations during mass layoffs based on their reason and timing; provide details about our sample; and present sample descriptive statistics. We then discuss our empirical methodology in estimating the magnitude of earnings losses upon job separations during mass layoffs 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 income tax filings for both individuals and employers, with a separate form that provides detailed job separation information. The CEEDD links *individual*-level information from T1 returns, *employer*-level information from the National Accounts Longitudinal Microdata File (NALMF) using employer tax returns, and *job*-level information from T4 slips and Record of Employment (ROE).[4]

On the worker side, the CEEDD provides information on demographics recorded from the T1 (e.g., age, gender, and province), as well as earnings from *all* jobs reported from all T4s. We define earnings as total *annual* pre-tax earnings received from employment.[5] Like most administrative data used in this area of study, ours do not

[4]T1 returns and T4 slips are similar to the individual-level 1040 form and job-level W2 form in the U.S., respectively. The NALMF is a longitudinal database of Canadian enterprises built from Statistics Canada's Business Register (BR), corporate tax returns (T2), payroll forms (PD7), and statements of renumeration (T4). The NALMF includes all incorporated businesses (which submit T2) and unincorporated businesses with at least one employee (which submit T4 and PD7).

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

contain information on hours worked. Thus we are not able to obtain hourly wages or higher frequency earnings measures. Several studies have explored the implications of earnings definitions: see Lachowska et al. (2020) and Bonhomme et al. (2023) for a thorough comparison of analysis using earnings and hourly wages.

In the CEEDD, each worker is linked to each employer from which they derive employment income using T4 employer identifiers. Individual and employer identifiers along with the panel nature of the administrative records allow us to identify a worker's tenure at an employer. On the employer side, employer characteristics contained in the CEEDD include employer 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. Therefore, an ROE must be issued after all job separations. However, an ROE may be issued, even without an interruption in earnings, because of changes in pay period type, payroll account number, business ownership, or business name.

The ROE contains information on a worker's employment history with an employer. Specifically, it provides 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.[6]

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—layoff and quit.[7] Second, in-

[6]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.

[7]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,

formation 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.[8] As such, an employer has less of a motive to misreport a layoff as a quit in the ROE form to undermine the EI claim of its previous employee. 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, which increase with tenure. 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 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 col-

leave of absence, parental, compassionate care/family caregiver, and other.

[8]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.

lected within a favorable institutional framework. This provides an ideal setting for studying differences among workers who lose their jobs during mass layoffs.

Sample selection. We now describe the criteria used to construct our worker sample and to identify mass layoff separations. The criteria are chosen in a way that is consistent with previous work (Jacobson, LaLonde, and Sullivan 1993 and Lachowska, Mas, and Woodbury 2020, among others). This key step allows for a baseline comparison with prior work before incorporating new ROE data on separations.

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.[9] For workers, we limit our sample to the working-age population aged 50 or younger in 2010.[10] As in Jacobson et al. (1993), we concentrate on 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. For workers with multiple jobs recorded during the tax year, we define the primary employer as that which accounts for the highest share of earnings during the year. Similar to Jacobson et al. (1993), we restrict the sample to individuals with positive earnings from 2002–2014 and for whom an employer identifier is always available. Thus, our estimates should be interpreted as the effects of separations on highly-attached workers.[11] Importantly, in Appendix B.1, we explore the impact of relaxing these sample restrictions, presenting our main findings when including workers with zero earnings (Figure A1), removing the long-tenure requirement (Figure A2), and excluding all multiple job-holders (Figure A3).

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

[9] As discussed below, because mass layoffs are defined by percentage changes in employment, a small absolute change in employment may be considered a mass layoff for employers with few employees. As such, we focus on employers with at least 50 employees, as in the literature.

[10] 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.

[11] Our baseline sample excludes individuals with zero earnings because it is difficult to distinguish them from those simply missing a tax form. Since tax filers with zero earnings for an entire year are rare, the original non-zero 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.1, we discuss further issues when including such individuals.

at any point during 2008–2010.[12] 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.[13] 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 temporary employment fluctuations as mass layoffs. (Davis and von Wachter, 2011).[14] 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).[15]

Utilizing ROE data. We now proceed to utilize additional information from the ROE data regarding the reason for job separation. Using the ROE forms for all mass-layoff separators in our sample, 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. We provide three pieces of evidence to support the idea that mass-layoff separations with no ROE are more likely to be spurious separations (not related to actual job losses). 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

[12] 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).

[13] 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.

[14] We consider an alternative sample and definitions of long-tenure workers and mass-layoff events, as in Davis and von Wachter (2011), without limiting separations to 2008–2010 (Figure A4).

[15] We also present results when the control group focuses on stayers *within* the same employer from which mass-layoff separators separate (Figure A5).

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.

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. Second, there is a negligible change in the trend of average earnings for mass-layoff separators with a missing ROE after 2008 (Figure A10 in Appendix B.2). 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 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 administrative data without supplemental information like that of the ROE and who thus must rely solely on changes in employer ID.

Having shown that separations with missing ROEs are unlikely to be relevant separations for this study, we narrow our attention to mass-layoff separations associated with an ROE. 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, and negligible changes in average earnings and 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 a category that is primarily used when an ROE is issued without an interruption in earnings.[16] Other major categories in this group cover many 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 approach to estimating the consequences of mass-layoff separations, Table 2 presents descriptive statistics for mass-layoff separators with different reasons for separation (layoff, quit, or the average of both) and compare them with stayers. We document several differences across groups. First, relative to stayers, laid-off workers had around $5,000$ CAD lower average earnings between 2002 and 2005, while those who quit had much similar earnings. Second, in terms of demographics, laid-off workers are more likely to be male and younger 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.[17] 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

We note that the presence of ROE information in our database enables us to offer guidance to researchers who utilize databases that constrain them to rely only on employer ID changes to identify separations. As discussed in the previous section, a plausible approach to weed out spurious separations would be to eliminate from the sample employer ID changes associated with concentrated flows. However, this approach comes with a crucial trade-off: While lowering the threshold for concentrated

[16] Employers often use "code K" in the ROE form when there is no interruption in earnings.

[17] While workers who quit their job without just cause are ineligible for EI transfers, those who quit for just cause (e.g., harassment, discrimination, significant change in work duties) may qualify. In our sample, around one-third of mass-layoff separators who quit receive EI transfers. Furthermore, part-time workers can also collect EI. In our sample, 16% of stayers receive EI.

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, prof. 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

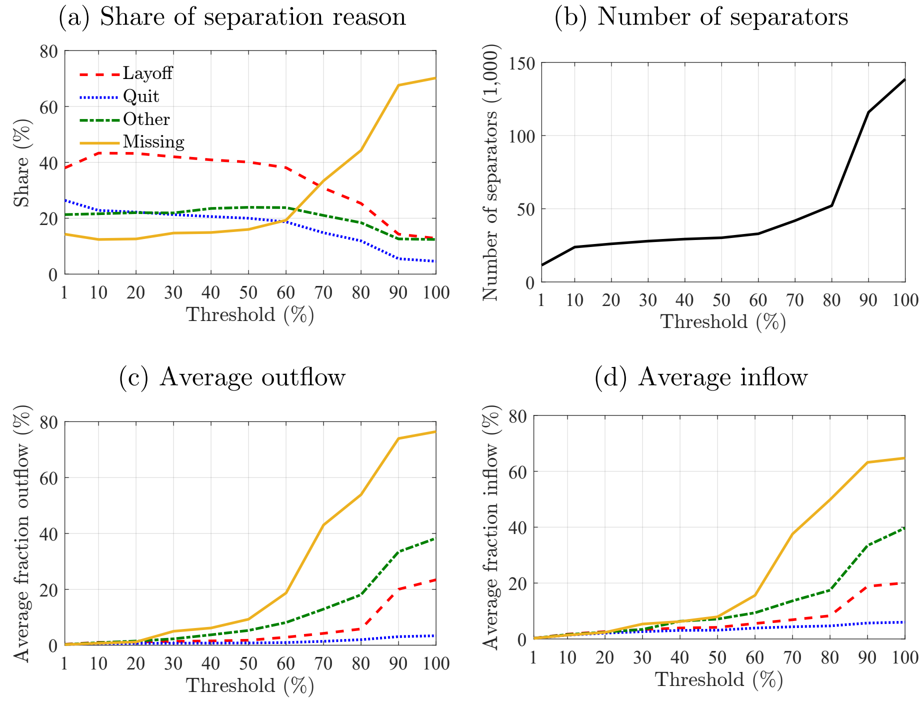
Note: This table presents descriptive statistics for workers who separate from their jobs during a mass layoff between 2008 and 2010 because of a layoff or quit (or the average across the two reasons) and stayers who remain attached to the same primary employer between 2002 and 2014. Standard deviations 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.

flows might filter out more spurious separations, it might also substantially reduce the number of separators and increasingly result in the omission of valid separations.

To meaningfully explore this trade-off and assess the impact of defining an excessively concentrated flow, we examine outcomes across a range of alternative thresholds. To fix ideas, the lower the threshold, the more restrictive the sample selection becomes. Panel (a) of Figure 1 shows the distribution of separations (i.e., employer ID changes) after concentrated flows that surpass the threshold hold are dropped. 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.[18]

[18] 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

Figure 1: The impact of varying threshold for concentrated flows



Note: For each threshold tested, Panel (a) presents the distribution of separations after concentrated flows that surpass the threshold are dropped. Panel (b) presents the corresponding number of separations remaining. Finally, Panel (c) 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 (d) 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).

Clearly, a lower threshold also implies a smaller sample of separations, as seen in Panel (b). However, Panel (a) would suggest that this decline is primarily driven by the removal of separations with missing ROEs. Finally, Panels (c) and (d) present the fractions of concentrated outflows and inflows as defined previously, respectively. The fraction of concentrated flows 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 separations with missing ROEs.

Overall, Figure 1 indicates that in the absence of ROE information, adopting a relaxed threshold (e.g., 80% as discussed in Footnote 12) to identify concentrated flows may result in the inclusion of a large number of spurious separations. For our database, a more restrictive cutoff of around 50% appears to be ideal when the researcher does not have the information about the separation as provided by the ROE.[19] This finding also supports a more restrictive criteria as in Schmieder et al.

part-time, on-call, or casual. This may also reflect a rate of non-compliance among employers.

[19] Hethey-Maier and Schmieder (2013), in the context of establishment closures, explore various

(2023), who consider only mass layoffs where no more than 20% of workers transition to a single destination employer. We demonstrate in Figure A6 in Appendix B.1 that earnings losses become larger when we apply the stricter 50% exclusion threshold.[20]

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 logarithm of annual earnings of individual i reported in year t, while α_i and ζ_t capture individual and time fixed effects, respectively. Further, $x_{i,t}$ represents 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.[21] 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 ($k = 1, 2, ..., 6$).[22] Under the assumption that, absent separation, average earnings of mass-layoff separators would be parallel to those of stayers, estimated γ^s_k is interpreted as the causal effect of separations. Figure A7 in Appendix B.1 relaxes the parallel trends assumption by

cutoffs for the concentrated outflow definition we use and similarly find a restrictive 30% threshold.

[20] In Figure 2, we reproduce the main findings of the existing literature without using the ROE information. In that figure, for comparability, we choose our baseline exclusion threshold as 80%.

[21] 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.

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

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, $d^s_{i,t,k}$ would simply refer to the occurrence of a mass-layoff separation. In this case, the estimated coefficients-of-interest γ^s_k 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, such as Jacobson et al. (1993) and Lachowska et al. (2020). However, unlike previous studies, we separately estimate differences in outcomes between separators and stayers depending on reasons or timing of the separation. In particular, ROE information allows us to determine differences within mass-layoff separations characterized by a layoff and quit. Moreover, we also divide mass-layoff separations into groupings based on the proximity of the separation date to the mass layoff. As such, when coefficients differ by subgroup s, we interact separation dummies, along with their leads and lags, 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 individuals (Abowd, Kramarz, and Margolis, 1999; Card, Heining, and Kline, 2013; Card, Cardoso, and Kline, 2016; Sorkin, 2018; Song et al., 2019). Following this literature, we estimate an AKM model using our data. We then use the estimated employer fixed 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 logarithm of annual earnings $y_{i,t}$ of individual i in year t on individual fixed effects κ_i, time fixed effects λ_t, and employer fixed effects $\psi_j(i,t)$, where the function $j(i,t)$ indexes the primary employer j of individual i 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.[23]

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. Put differently, 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.[24] We assign $\widehat{\psi}_j$ to each worker-year observation whenever possible and use them as outcomes of job separations.[25] 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 also estimate match effects as a time-invariant worker-employer fixed effect as in Woodcock (2015) and Lachowska et al. (2020). These match effects can be interpreted as changes in a worker's productivity when the worker is employed by different employers due to differences in work arrangements that affect the worker's productivity. To do so, the following steps are taken. First, we calculate the average log of earnings $\overline{y_{ij}}$ for each worker-employer pair (i, j) in the sample over the duration of the match.[26] Second, average earnings are regressed on worker θ_i and employer

[23]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 displaced worker sample, particularly when assessing the role of AKM effects in explaining earnings losses. Figure A8 in Appendix B.1 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.

[24]While we do not focus on variance-covariance estimates from the AKM specification, in Appendix B.3, we report results on worker mobility in Canada to mitigate concerns on limited mobility bias.

[25]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.

[26]As in Lachowska et al. (2020), we net out year and tenure effects from the average of log earnings $\overline{y_{ij}}$. First, we remove year effects from the outcome variable and then regress this adjusted variable

$\xi_{j(i,t)}$ fixed effects, where the regression is weighted by match duration. Formally,

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

where the error μ_{ij} is assumed to be orthogonal to the worker and employer fixed effects. The residuals $\widehat{\mu}_{ij}$ represent the component of earnings accounted for by time-invariant worker-employer match effects, averaged over the period the match is observed, after accounting for worker and employer effects.[27]

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 employer and match effects using the AKM and Woodcock estimators, respectively. This allows for a direct comparison with their findings on the magnitude and sources of displaced workers' earnings losses. 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 for cases where this assumption may not hold.) To address this concern, we provide alternative results in Section 5.1 and Appendix C, 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 (see Card et al. 2013, Sorkin 2018, and Song et al. 2019, among others).[28] In addition, Lachowska et al. (2020) and Bonhomme et al. (2023) made comparisons between employer premia based on both annual earnings and hourly wages, with similar findings for both measures.

on job tenure and worker-employer match indicators. Next, we subtract the contribution of job tenure from the outcome variable and calculate within-match averages of the outcome variable.

[27]Since worker and employer effects are constant and $\widehat{\mu}_{ij}$ is constant for each match, some individual-level earnings variation remains after accounting for worker, employer, and match effects.

[28]Our sample selection also follows these papers closely. For example, we exclude earnings in the first and last year of the employment spell to eliminate non-full-year earnings from an employer. We also impose a minimum earnings threshold to reduce the presence of marginally attached workers.

3 Earnings losses by reason for separation

In this section, we first document how earnings and employer effects evolve upon *all* mass-layoff separations. This step allows us to benchmark with previous estimates in the literature. Next, we show how the magnitude and sources of earnings losses due to employer and match effects differ between layoffs and quits during mass layoffs.

3.1 Earnings losses for all separation

We start by estimating the effects of a separation during mass layoffs on log earnings from Equation (1) and on employer-specific pay premium from Equation (3). The objective of this exercise is to establish a consistent benchmark with the literature by retaining the same approach used in identifying mass layoffs, without utilizing new information (reason or timing) from the ROE. As a result, in Section 3.1, S contains only one element: the mass-layoff separator sample.[29]

The black line in Figure 2 plots the estimated effects of mass-layoff separations on earnings, i.e., the estimated γ_k values, along with 95% confidence intervals, which are narrow because of the large sample size. 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 a mass-layoff separation was 28% ($\exp(-0.33) - 1 = -0.28$ or 33 log points) during the 2008–2010 episode in Canada. This loss was also persistent: The earnings of mass-layoff separators remain 11% lower even after six years past the job separation when compared with the earnings of job stayers.[31]

These results are comparable to previous estimates in the literature using U.S. national- and state-level administrative data and European administrative data.[32] In particular, according to existing estimates, the decline in earnings ranges between 18% and 46% in the year of separation and between 10% and 30% five years after the

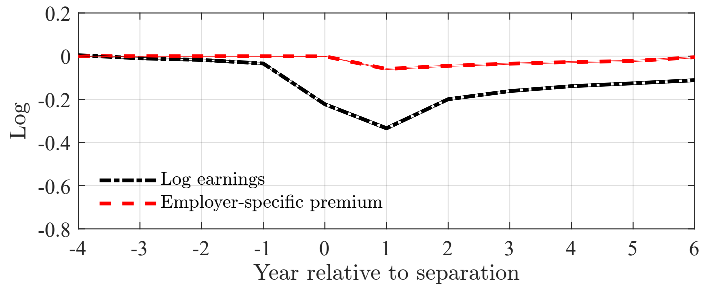
[29]Recall that this sample covers all mass-layoff separations, including those with missing ROEs.

[30]This result is often called the "Ashenfelter's dip," which refers to the finding that the earnings of separators decline even before the 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 A11 in Appendix B.4 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]The similarity of estimates found in the Canadian data to those found in U.S. national- and state-level data and European data provides some reassurance that the substantial heterogeneity documented in Section 3.2 does not simply arise from differences in the source of data.

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.

separation (see, among others, Jacobson et al. 1993, Couch and Placzek 2010, Davis and von Wachter 2011, Lachowska et al. 2020, and Schmieder et al. 2023).

There is a growing literature that studies the role of employer-specific pay premiums in explaining the magnitude and persistence of earnings losses following separations upon a mass layoff. Lachowska et al. (2020) find that 6% of all earnings losses in the quarter following separations in mass layoffs is due to reemployment with an employer that pays less on average. In terms of long-term effects, they show that employer effects account for 9% and 17% of all earnings and wage rate losses, respectively, five years after separations in mass layoffs. On the other hand, Schmieder et al. (2023) find a much larger role of employer effects in explaining wage losses. They find that 75% of wage losses are explained by the decline in 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, which presents the estimated dynamics of employer-specific pay premium. We find that lost employer effects explain 18% of the earnings loss in the year following separations: 6 log points of the average earnings loss of 33 log points are due to reemployment with a lower-paying employer. 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 investigate the extent to which

[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.

this average estimate might mask differential scarring effects of job displacement for those who are laid off vs those who quit. 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 $s \in S = \{\text{layoff, quit}\}$.

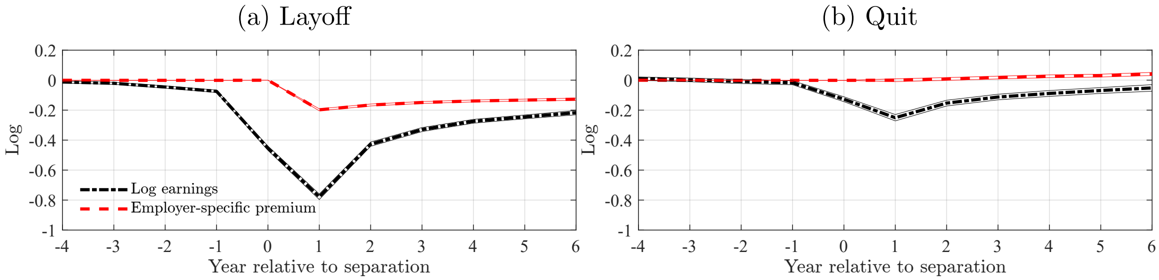
The black lines in Panels (a) and (b) of Figure 3 plot earnings dynamics for individuals who are laid off or who quit during mass layoffs, respectively. We find significant differences between the two groups. In the year following a separation, laid-off workers experience an average earnings loss of 78 log points, while those who quit experience a loss of only 25 log 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. Importantly, Figures A1 to A9 reconstruct our results in Figure 3 under alternative specifications and samples.[35] Overall, our main results remain intact across all these exercises, with two exceptions. Including short-tenure workers ameliorates earnings losses, while imposing stricter exclusion thresholds for concentrated flows exacerbates them. However, the gap in earnings losses between layoffs and quits remains similar even in these two cases.

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 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%

[34] Figure A11 in Appendix B.4 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] Specifically, we present results for the following cases: (i) including zero-earners, (ii) relaxing the long-tenure requirement, (iii) excluding multiple-job holders, (iv) implementing an alternative definition of mass-layoff events as in Davis and von Wachter (2011), (v) comparing outcomes of mass-layoff separators to stayers within the same employer, (vi) introducing a stricter threshold for removing concentrated flows, (vii) incorporating heterogeneous (worker-specific) trends in Equation (1), (viii) expanding the AKM sample to include mass-layoff separators and stayers, and (ix) focusing only on employer closures when identifying mass-layoff events.

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.

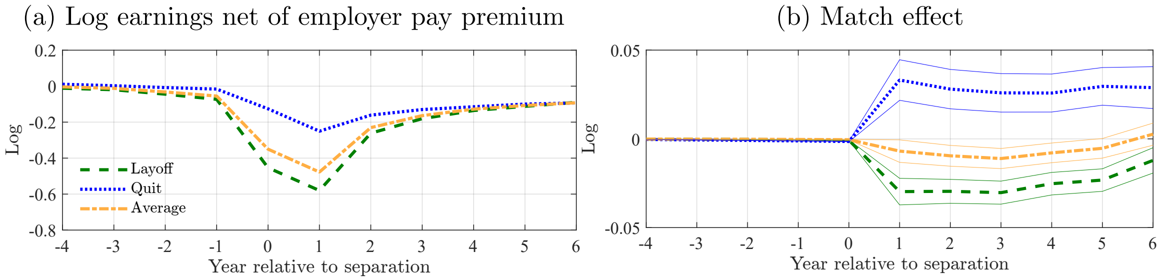
(13/22) in the long term. In contrast, for those who quit, the employer premium remains unchanged in the first year and rises by 4 log points after six years.

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, focusing on mass-layoff separators, Lachowska et al. (2020) estimated a limited role of employer effects in explaining earnings losses of separators in Washington state, while Schmieder et al. (2023) conclude that they play a significant role in German data. We document novel evidence that the role of employer premia in explaining earnings losses is drastically different between those who are laid-off and those who quit. The loss of employer-specific premium accounts for a quarter of short-term earnings losses and two-thirds of long-term earnings losses for the former group, but it has a negligible role for the earnings dynamics of the latter group. This suggests that the role of employer effects may vary based on whether the separation is voluntary. This difference could help explain the contrasting findings between Washington state and Germany, as the mix of layoffs and quits may differ across the two datasets.

While our sample selection procedure and use of AKM estimates allow us to make direct comparisons with previous work, we relax the exogenous mobility assumption to address the possibility of endogenous mobility bias. Section 5.1 and Appendix C outline a bias-correction procedure following Bonhomme et al. (2019) and present our main findings under an alternative estimate of the employer premium.

Finally, in Section 5.2, we repeat our analysis in this section for separations that occur outside a mass-layoff event. We find key interactions between worker and employer outcomes. For both layoffs and quits, average earnings and employer premium

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



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

outcomes arising from a non-mass layoff separation are less severe when compared with those arising from mass-layoffs separations. In addition, the gap in outcomes documented between workers who are laid off and those who quit also narrows when restricting the analysis to non-mass layoff separations.

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.[36]

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 both the short and long term. In contrast, while match effects decline for layoffs and rise for quits, their quantitative effects are small.

[36]Figure A12 in Appendix B.5 shows a complete decomposition for the sources of earnings losses.

3.3 Cross-sectional differences in earnings losses

Overview. In this section, we investigate the reasons behind the cross-sectional 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, both upon impact and especially over longer horizons. 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 mass-layoff 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, similar to 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, as well as 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).[37] We highlight three main observations from Table 3.

[37] 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.

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-diagonal transitions are moves to an employer with a lower-quintile employer effects, on-diagonal and above-diagonal transitions are 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.

First, workers who are laid-off (Panel (a)) are more likely to transition into lower employer-premium quintiles compared with those who quit (Panel (b)). 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, given that the share of upward movements is much larger for quits (33%) than for layoffs (16%).

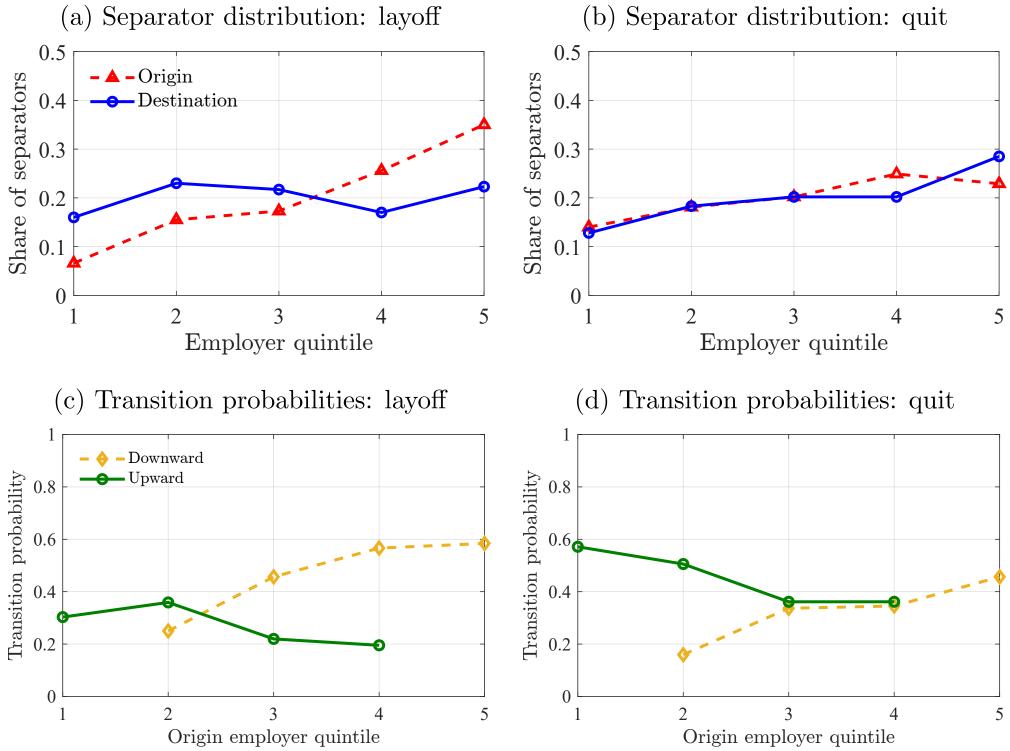
Second, conditional on falling into lower employer-premium quintiles, workers who are laid-off experience much larger declines in earnings than those who quit (33.2 vs. 9.7 log points). Importantly, we find that losses of employer effects are comparable for layoffs and quits with a below-diagonal transition (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 individuals who quit and find reemployment at a lower-paying employer, they are compensated by higher worker-employer matches at their destination employer, which may be due a better fit for their skill sets or better contracts that raise 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 movements. For above-diagonal transitions, workers experience a substantial gain in employer effects and a sizable loss in match effects, where gains of employer effects are slightly larger and losses of match effects are slightly smaller for quits than for layoffs. These results imply that, after a quit or layoff during a mass layoff, moving to a higher-paying employer is often associated with a loss of valuable specific worker-employer matches.

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 important results regarding this conclusion. First, 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 investigate 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 lines) and destination (solid-blue lines) 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 those who quit. For example, while 35% of all layoffs in mass layoffs originate from employers in the fifth quintile, only 7% of them originate from employers in the bottom quintile. On the other hand, these shares are 23% and 14% for quits, indicating much less heterogeneity along the origin-quintile distribution. Moreover, Panel (a) documents strong asymmetry between the origin-quintile and destination-quintile distributions among laid-off workers. 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-

Figure 5: Transitions across employer effect distribution



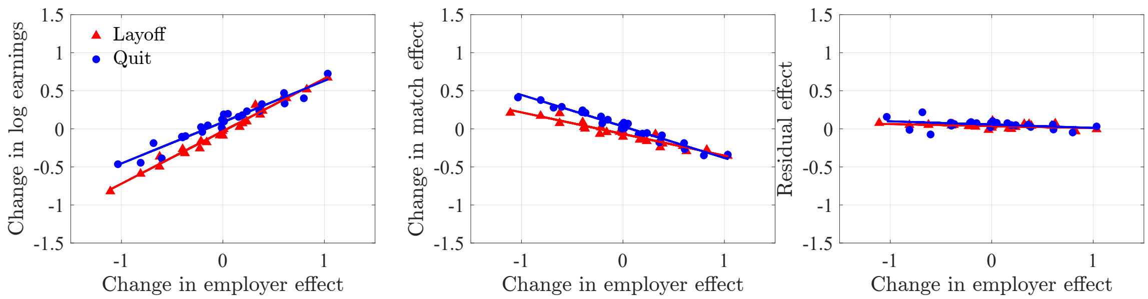
Note: Panels (a) and (b) plot the distribution of separations by origin (dashed-red) and destination (solid-blue) employer effect quintiles for layoffs and quits in mass layoffs, respectively. Panels (c) and (d) present upward (solid-green) and downward (dashed-orange) transition probabilities by origin 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.

effects distribution shifts leftward upon layoffs and workers suffer a substantial *net* loss in their employer premium position. In contrast, workers who quit face similar origin and destination distributions (Panel (b)). This implies that for quits, the distribution of employer effects remains largely unchanged.

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

Figure 6: Interquintile changes by employer effects

(a) Earnings vs employer effects (b) Match vs employer effects (c) Residual vs 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

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.[38]

Earnings outcomes. Thus far, we have focused on the *positional* dynamics of employer premia among workers who are laid off and who quit during mass layoffs. Now, Figure 6 plots interquintile changes in log earnings and match effects against changes in employer effects. Each panel contains 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 especially prominent in transitions that involve a decline in employer premiums. Note

[38] In Section 5.2, we present similar results for separations associated with non-mass layoffs. The equivalent results for Table 3 and Figure 5 show that workers who are laid-off or quit during non-mass layoffs face lower chances of falling the employer premium ladder.

that since the change in employer premium is the same for any given point in the x-axis and changes in residual effects are roughly zero (Panel (c)), the gap in log earnings changes between those laid-off and those who quit will be largely explained by differences in match effects between the two groups. Indeed, this is documented in Panel (b): Conditional on experiencing the same decline in employer effects, workers who quit are compensated by a larger increase in match effects relative to those who are laid-off. This result suggests that workers who quit into lower-paying employers may be doing so strategically in pursuit of a better match for their skills or a better contract that increases their productivity. In contrast, laid-off workers may have little choice but to accept lower compensation in pursuit of reemployment.

Finally, the differential slopes in Panel (a) also reveal that the AKM model with additive worker and employer effects explains earnings changes better for layoffs than for quits, a result that is in line with our previous findings 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 offset employer premium declines with better match effects, resulting in milder earnings losses even after a downward transition.

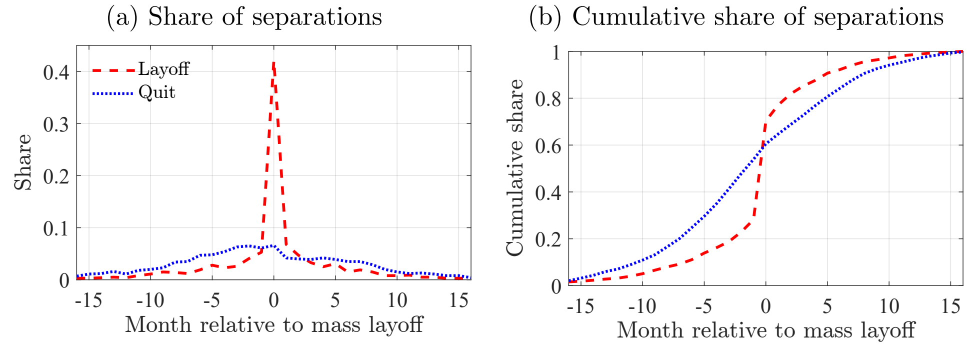
4 Earnings losses and the role of timing

The previous section focused on the high degree in cross-sectional heterogeneity present in separations during mass layoffs. Now, we utilize another important piece of information provided by the ROE forms that allows us to analyze how *cross-sectional* heterogeneity interacts with the *timing* of separations during such events. We first provide novel evidence that employer contractions are protracted in nature in that a large share of separations occur several months before the height of the mass-layoff event. We then investigate how the timing of separation affects earnings outcomes and whether it is systematically linked to observable worker characteristics.

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

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.

identified as the month during which the largest number of ROE layoffs are recorded.[39] Second, it allows us to group mass-layoff separators based on the proximity of their *own* separation to the mass-layoff month. Specifically, for each mass-layoff separator, because we know the exact date of the separation, we can determine how far this date was from the peak of their origin employer's mass-layoff event. This step allows us to study sources and consequences of separations based on the timing of separation.

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 within that month. For example, 42% of layoffs occur at the exact month of the mass layoff, while only 7% of quits are observed at that month. 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.[40]

These results suggest that separation timing might be associated with worker characteristics and the consequences of separations via strategic decisions of workers

[39]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 a mass layoff in 2008 and another one in 2009. If a worker from the employer is observed to be employed 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.

[40]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.

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 mechanisms 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

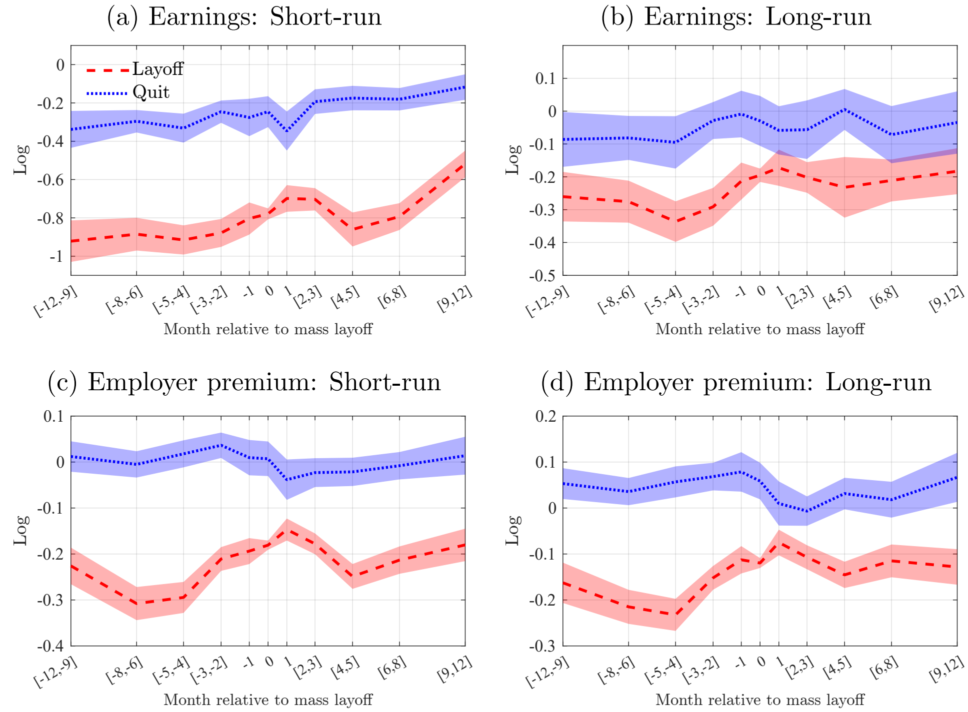
To assess whether separation timing affects its consequences, we estimate the dynamics of earnings and employer-specific pay premia for layoffs and quits before and after the mass-layoff month. In what follows, the set of groups within the mass-layoff separator sample is $S = \{\text{layoff, quit}\} \times V$, where V is a set of groups that divide mass-layoff separators by proximity (in months) to the mass-layoff month.[41]

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, the short-term earnings losses are typically smaller for those who experience a layoff after the mass-layoff month when compared with the earnings losses of those who are laid off in the mass-layoff month. 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. Moving to long-term (six years after the separation) earnings losses (Panel (b)), we find that while the resulting point estimates on earnings losses are typically larger for those who experience a separation (both layoffs and quits) prior to the

[41] 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.

mass-layoff month relative to earnings losses for those who experience a separation after the mass-layoff month, the degree of heterogeneity in the magnitudes of earnings losses by timing of separation is smaller in the long-run than in the short-run.

The finding that workers laid off before the mass-layoff month experience larger earnings losses than those laid off after suggests that employers may be prioritizing less-productive workers for layoffs who tend to have poorer prospects of reemployment with a comparable employer. In contrast, the smaller earnings losses among workers who quit after the mass-layoff month compared with those who quit before suggest that the former group may have secured better reemployment outcomes. 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.[42]

[42] 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

Why are earnings losses larger for separators prior to the mass-layoff month? To answer this question, Panels (c) and (d) of Figure 8 present estimates for employer-specific pay premium dynamics upon separations across layoffs and quits when they are also grouped by their proximity to the mass-layoff month. We find that, among those who are laid off, employer premium losses are much larger for separators whose jobs are dissolved relatively early into the mass-layoff event. The short-term gap (Panel (c)) in employer premium losses between those who are laid off prior to the mass-layoff month and those who are laid off after that month is sizable and sustained over the long-term (Panel (d)). Thus, employer pay premium losses largely contribute to the gap in earnings losses observed 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 the average earnings loss for workers who quit in mass layoffs (as discussed in Section 3.2), but also for accounting for the cross-sectional difference in earnings losses upon quits based on the timing of separation.[43]

Transitions along the employer premium ladder. As in Section 3.3, we now compare outcomes for below-, on-, and above-diagonal transitions between employer-effect quintiles to understand the sources of heterogeneity in outcomes upon job loss by separation timing. Table A2 in Appendix B.6 repeats Table 3 for separations before (Panel A), around (Panel B), and after (Panel C) the mass-layoff month.[44]

Beginning with laid-off workers, 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 much smaller for workers laid off after the mass-layoff month compared with those laid off before it. On the other hand, for on- and above-diagonal transitions, changes in earnings upon layoffs are mostly similar regardless of separation timing. Finally, changes in earnings

reduce earnings losses, then the losses for early layoffs would have been even larger without them.

[43]In Section 5.1 and Appendix C, 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.

[44]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.

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 laid-off workers.

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 very similar for quits before or after the mass-layoff month, while earnings gains among above-diagonal transitions are larger for quits after the mass-layoff month. Finally, for below-diagonal transitions, independent of timing of separation, changes in employer effects and match effects move in opposite directions and thus 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. On the other hand, 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 average pay and form more valuable specific worker-employer matches than those who quit before the mass-layoff month.

4.3 Characteristics of mass-layoff separators: role of timing

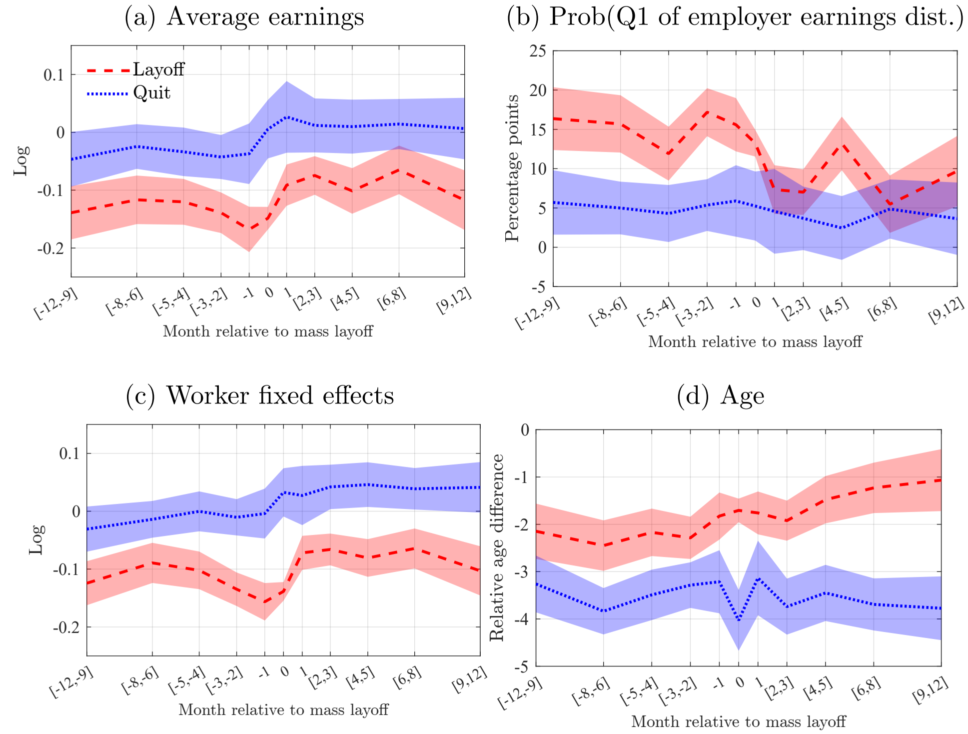
We now investigate whether worker characteristics are systematically related to the timing of separation by estimating the following cross-sectional regression:

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

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.[45] The dummy variable d_i^s is 1 if individual i is a

[45] 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.

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 workers in the mass-layoff separator sample (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 (as measured by the x-axis). 95% confidence intervals are given by the shaded regions.

mass-layoff separator with reason and timing subgroup s. The vector x_i consists of observable 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 associated with 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 in the mass-layoff sample 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.[46] Because the composition of separators closer to the mass-layoff month is increasingly dominated by layoffs and less by quits, as shown in Figure 7, if we were to mix layoffs and quits in Panel (a) of Figure 9, the average pre-separation earnings for separators in the mass-layoff month would seem to be substantially 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) presents the percentage points difference in the probability of being in the first quintile of the within-employer earnings distribution in 2007 for mass-layoff separators (prior to separation) relative to that of stayers in 2007 by timing of separation, separately for 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 and 6 percentage points higher for quits relative to stayers. Second, focusing on how the timing of separation impacts the probability of being in the bottom quintile of the distribution, we find that 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 that month, it does not change much for those who quit across timing of separation.[47] Taken together, these results complement our findings in Section 3.3. 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 effects of the timing of separation, we find that, for both layoffs and quits, those who separate prior to the mass-layoff month have a

[46]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.

[47]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.

lower average relative to those who separate after that month.

Finally, Panel (d) presents the relative age differences between mass-layoff separators and stayers, by timing and reason for separation. Laid-off workers are on average older than quitting workers, regardless of the timing of separation. 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 Results under alternative specifications

5.1 Addressing the endogenous mobility bias

Additive worker and employer effects. The AKM approach to estimating employer-specific premia through a two-way fixed effects model is subject to the endogenous mobility bias, details of which are provided in Appendix C and D. Bonhomme et al. (2019) (BLM) introduced a method to correct for this bias, based on the assumption that log earnings and mobility decisions are jointly governed by a first-order Markov process. In Appendix C.1, we outline how we adopt BLM's approach to arrive at an alternative estimate for employer-specific pay premia. Using these biased-corrected estimates, we run the same regressions as in Equation (3) to explore how our main findings on the dynamics of employer premia upon a layoff or quit change.[48]

[48]The correction procedure for the model with additively separable worker and employer effects in Appendix C.1 prevents the bias from being entirely driven by time-invariant match effects. In Appendix C.3, we account for time-invariant match-specific component in the log earnings equation.

Figure 10 compares the dynamics of employer-specific pay premia by reason of separation during mass layoffs. We compare AKM employer premia as specified in Equation (3) (red-dashed lines) and the BLM bias-corrected premia as in Appendix C.1 (blue-dotted lines). Panels (a), (b), and (c) show estimates for all mass-layoff separations, layoffs, and quits, respectively. We note two key observations.

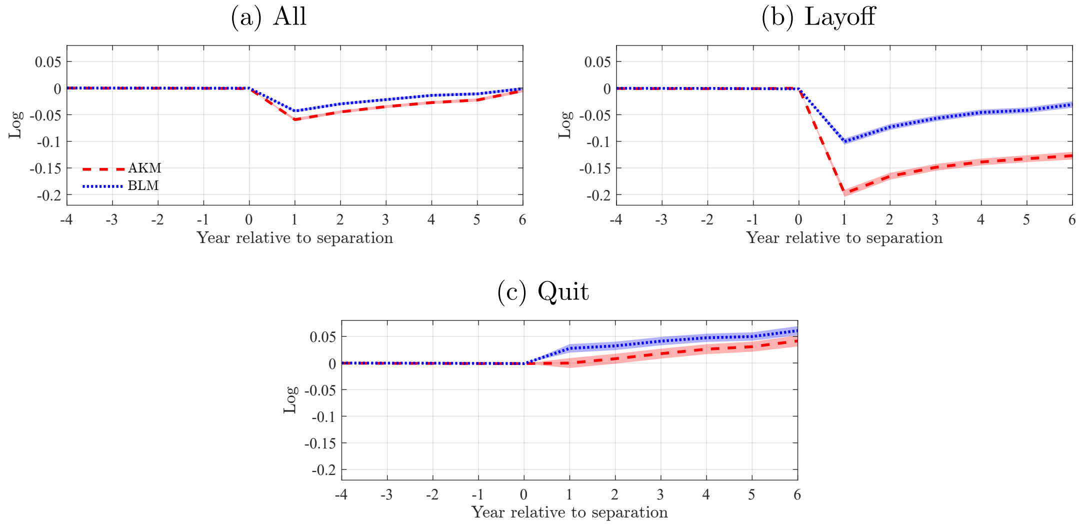
First, BLM estimates result in slightly lower losses upon separation when we consider all separators (4 vs 6 log points). However, Panel (b) shows a wider gap for layoffs. While BLM employer premia decrease by 10 log points, AKM estimates drop by 20 log points. This divergence persists even in the long run. For quits, employer premia six years post-separation increase more under BLM estimates (6 vs 4 log points). Overall, while both BLM and AKM employer effects have a large role in explaining earnings losses especially 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 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 a mass layoff. Figure 11 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, this figure presents employer premia derived from the BLM bias-correction as in Appendix C.1.

We note two findings from Figure 11. First, the differences in employer premium outcomes between separations before and after the mass-layoff month remain large.

Figure 10: 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 from Appendix C.1.

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 pay premia, while those laid off more than six months after see a drop of 10 log points. Similar conclusions hold for long-run outcomes (Panel (b)).[49]

Interactive worker and employer effects. Finally, in Appendix C.3, we depart from the assumption of additive worker and employer effects (as in the BLM approach Appendix C.1) and consider a model of log earnings that only includes *interactive* worker and employer effects. Specifically:

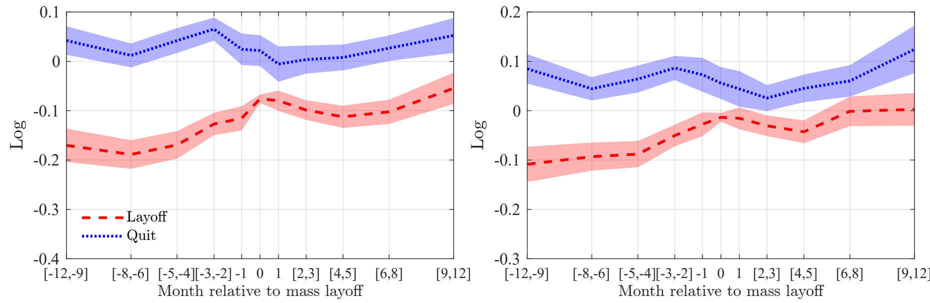
$$y_{i,t} = \phi_{l_i,k_{i,t}} + \upsilon_{i,t},$$

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 (as identified from the procedure in Appendix C.1), and the error term satisfies $\mathbb{E}[\upsilon_t | l, k_t] = 0$. We have two motivations for this extension. First, this a generalization of the previous bias-correction procedure which now allows endogenous mobility bias to be driven by time-invariant match effects.

[49]Figures A13 and A14 in Appendix C.2 provide further comparisons between results obtained under AKM and BLM specifications.

Figure 11: BLM employer effects upon separation by timing of separation

(a) Short-run (b) Long-run



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 as outlined in Appendix C.1. 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.

Second, the approach also allows us to compare an alternative measure of match effects versus the Woodcock estimator described in Section 2.3. However, we note the match effect $\phi_{l,k}$ is distinct from that in the specification for the Woodcock estimator (μ_{ij}), which reflects the match effect *net* of worker and employer effects.

We note three findings from Figure A15 in Appendix C.4. First, match effects $\phi_{l,k}$ are generally increasing with BLM employer effects. This indicates that, for a given worker type, matching with a high BLM employer premium employer raises match effects. Second, quitters are more likely to be high-type workers relative to those who are laid-off. Finally, changes in match effects net of BLM employer effects are decreasing in the change in BLM employer effects. Thus, transitions with large declines in employer premia are accompanied by increases in net match effects, indicating a trade-off between the two. This pattern is consistent with Panel (b) of Figure 6.

5.2 Outcomes for separations outside mass layoffs

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

First, we show that separation distributions differ significantly 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.

Turning to earnings outcomes, separations outside of mass layoffs are associated with smaller and less persistent earnings losses, as shown in Figure A16 in Appendix E. 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, with earnings fully rebounding after three years. The gap between earnings losses upon layoffs and quits in non-mass layoffs is also much smaller than that in mass layoffs both in the short run and in the long run.

Employer pay premium changes 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, with long-term increases of 11 log points.

Finally, Table A3 and Figure A17 show that laid-off workers in non-mass-layoff events are less likely to move to lower-paying employers and experience 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. The differences in outcomes between mass-layoff and non-mass-layoff separations are mostly driven by the differences in match effects, with workers who quit during non-mass layoffs experiencing larger gains in match effects.

6 Conclusion

This paper revisits the measurement of earnings losses after job displacement using Canadian employer-employee matched data merged with detailed job separation records. It advances the literature in four ways. First, it reveals the shortcomings of conventional mass-layoff identification methods, as only a quarter of separations in mass layoffs are actual layoffs, while nearly half result from reorganization activities without job loss. Second, it shows that earnings losses from involuntary separations are larger and more persistent than previous estimates, which averaged outcomes from different types of separations. Third, it documents significant differences in earnings and employer pay premiums between workers who are laid off and those who quit, with layoffs experiencing more severe earnings losses driven by the loss of employer effects. Finally, it demonstrates that mass layoffs are protracted, with layoffs and quits starting months before the most severe month of job cuts. Workers laid off earlier are more likely to be lower earners and less productive and, at the same time, suffer more significant earnings and employer premium losses.

Our findings emphasize the need for quantitative models of job dissolution to distinguish between types of separations, e.g., layoffs vs. quits. They also stress the role of decisions made by workers and employers during an employment 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 prevent employers from laying off long-tenure workers, while unemployment insurance could encourage less-productive workers to quit and climb the job ladder.

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Supplemental 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 Results under alternative specifications and samples

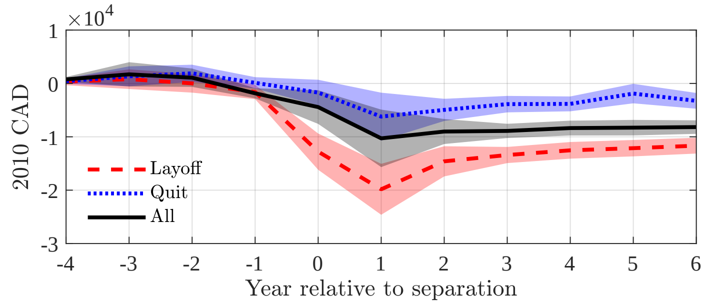
In this section, we repeat our main result in Figure 3 of Section 3.2 under alternative specifications and samples.

Including zero earners. The baseline sample restriction imposes that workers report positive earnings for the entire duration considered (2002–2014). We now relax this restriction by allowing for workers with zero earnings in certain years back into the sample. Figure A1 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 A1 are similar to those in Figure A11 in Appendix B.4, 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: It is difficult to differentiate between individuals with zero earnings and those simply missing a tax form. Since tax filers with zero earnings for an entire year are rare, the original non-zero earnings restriction primarily excludes workers who do not file taxes. Relaxing this restriction introduces workers with missing data and

[1]Note that results are presented in levels due to the inclusion of zero earnings. The absence of employment information for individuals with no tax forms also implies that we cannot present results for employer premia dynamics.

Figure A1: Job separation outcomes by reason: Including zero earnings

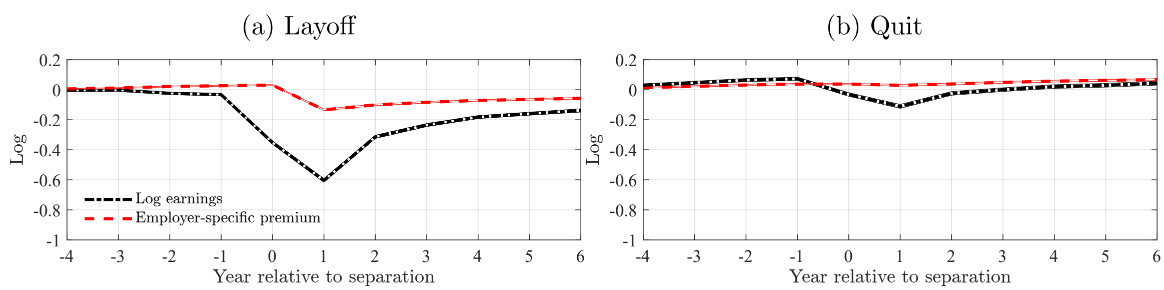


Note: This figure plots estimates for earnings losses in levels (2010 CAD) upon job separation by reason of separation during mass layoffs when the sample is modified to allow for individuals who do not report positive earnings. Dashed-red and dotted-blue lines (along with shaded 95% confidence intervals)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

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 compositional 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.

Relaxing tenure requirement. Next, we explore the implications of relaxing the long-tenure requirement imposed under our baseline sample selection. Figure A2 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 A2: Job separation outcomes by reason: Relaxing long-tenure requirement

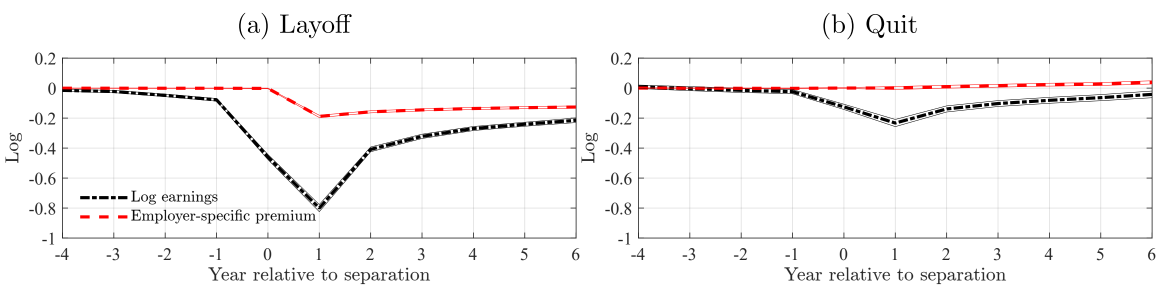


Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation during mass layoffs. Panel (a) presents estimates for separations due to layoff and Panel (b) presents estimates for separations due to quit. Dashed-dotted-black lines show 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 of this type of human capital, reduces the overall magnitude of earnings losses. However, our main conclusion on the gap between earnings losses for layoffs vs quits remains the same: We still find that earnings losses are much larger for layoffs than quits and that employer effects account for earnings losses for layoffs but not for quits.

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 A3 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 A3: Job separation outcomes by reason: Single-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.

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. In our analysis, 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 an individual meets the three-year tenure with their primary employer in year 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. Here, for any given nine-year 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 mass-layoff separators is specific to the sample in each reference year 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).

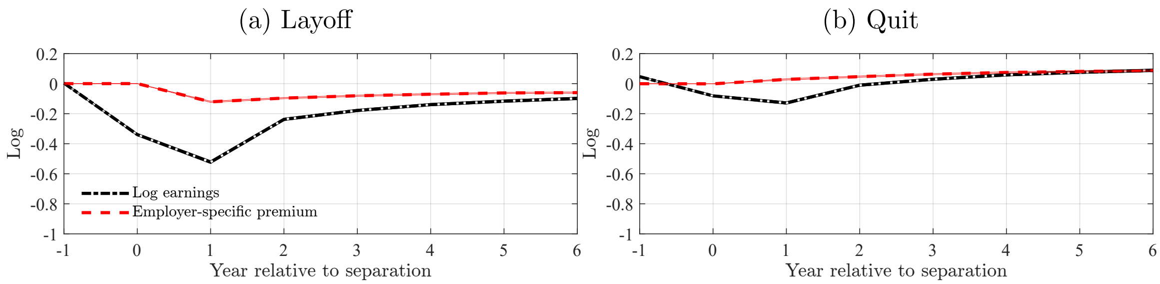
Figure A4 presents the results in this exercise.[3] In this case, earnings and employer effects losses are only slightly smaller for both layoffs and quits when compared with Figure 3. Importantly, our main conclusions also remain similar: Gaps in earnings and employer effects losses between layoffs and quits are still large and employer effects account for earnings losses for layoffs but not for quits.

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

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

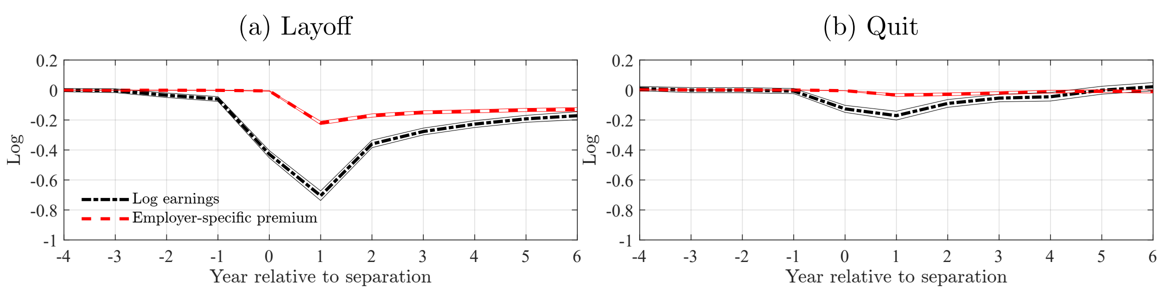
[3] The shorter pre-displacement horizon is due to the lower three-year tenure requirement.

Figure A4: Job separation outcomes by reason: Alternative 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 restriction and definitions of long-tenure workers and mass layoffs as in Davis and von Wachter (2011).

Figure A5: Job separation outcomes by reason: Within-employer comparison



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.

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):

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

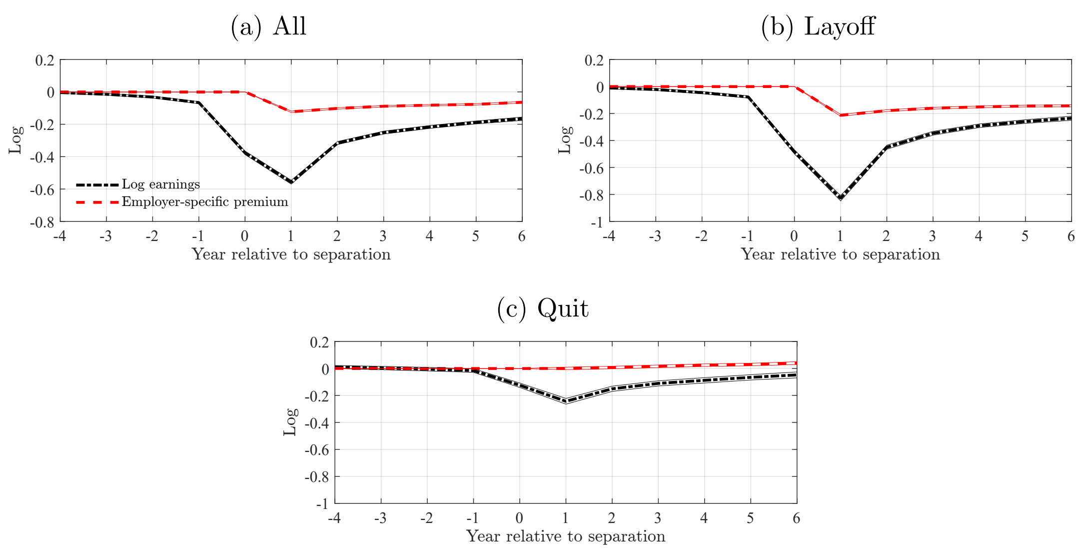
The noticeable difference between this specification when compared with Equation (1) is that fixed effects $\alpha_{i,j}$ are conditional upon employer affiliation j, such that the estimated outcomes translate to differences between separators and stayers at the *same* employer. When stayers at an employer experiencing a mass layoff also systematically face earnings losses, the magnitude of estimated coefficients γ^s_k 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.

As expected, Figure A5 shows that earnings and employer premium losses are slightly less severe in this case for both layoffs and quits relative to those in Figure 3. However, gaps in earnings and employer effect losses between layoffs and quits remain very similar to those in Figure 3.

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 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 A6, 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 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.

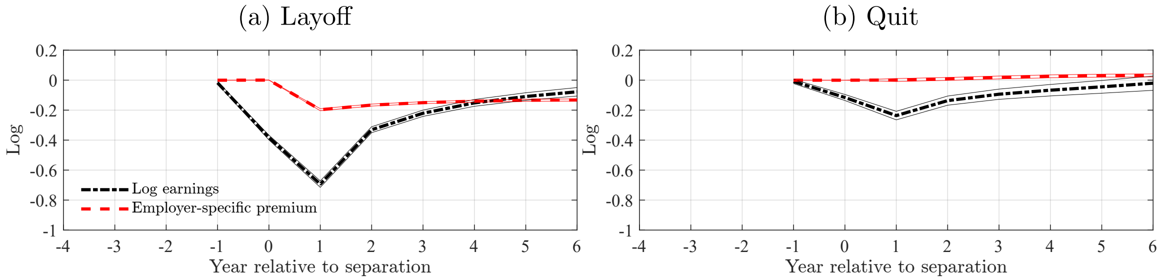
Figure A6: Stricter exclusion threshold for concentrated flows



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%.

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 earnings losses estimated from Equation (1)

Figure A7: 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).

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:

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

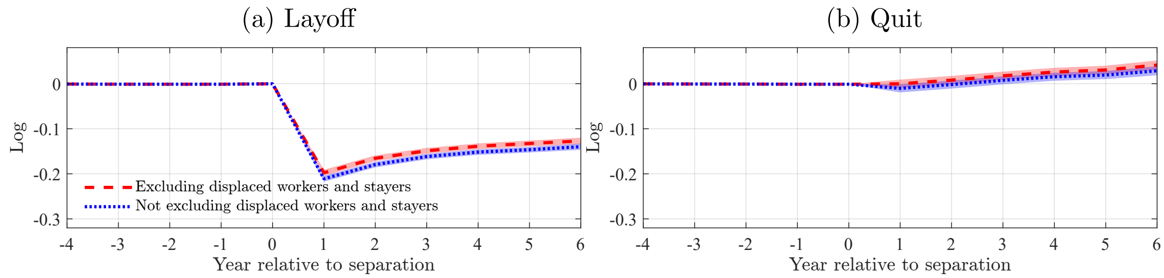
where ξ_i is the worker fixed effects in growth. Notice that we do not estimate the effects of separation for two, three, and four years before the separation. This is to ensure that the worker-specific linear time trends are well identified based on these three additional years of observations. With 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 A7 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 A8 shows that reintroducing the displaced worker sample into the analysis results in only 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-layoff events. Consequently, including displaced workers may exacerbate the endogenous mobility bias for which AKM estimates are often criticized. Overall, we conclude that this sample change does not largely affect our main conclusions in Figure 3.

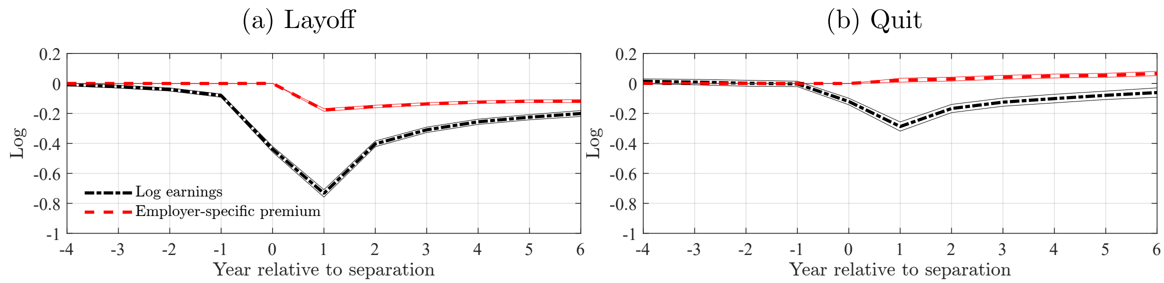
Figure A8: Effects of mass-layoff separations on AKM employer-specific premium



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.

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 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



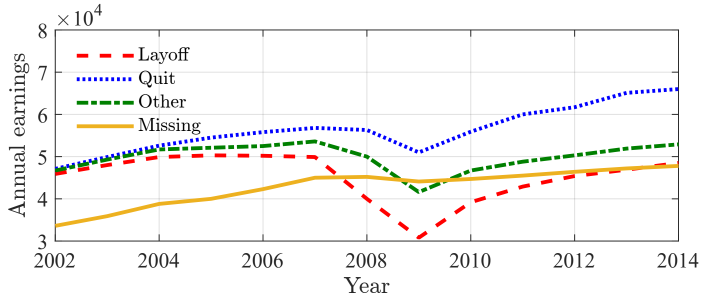
Note: This figure plots estimates for earnings and employer-specific pay premium losses upon job separation by reason of separation during mass layoffs. Panel (a) presents estimates for separations due to layoff and Panel (b) presents estimates for separations due to quit. Dashed-dotted-black lines show 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.

B.2 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 A10 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.

Figure A10: 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.

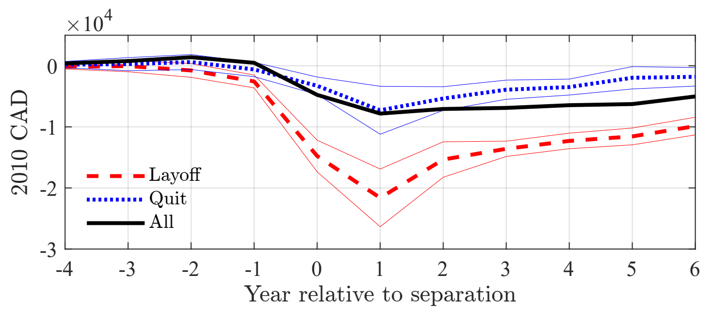
Second, Table A1 shows the fraction of individuals receiving employment insurance (EI) and the average annual amount received over a two-year period. For stayers, the period is 2008–2009, while for mass-layoff separators, it includes the separation year and the year after. Less than 10% of workers with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

B.3 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 around 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 according to 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, respectively. Lachowska et al. (2020) also calculate implied mobility rates from the German administrative 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.4 Earnings losses upon separations: level changes

Figure A11: 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).

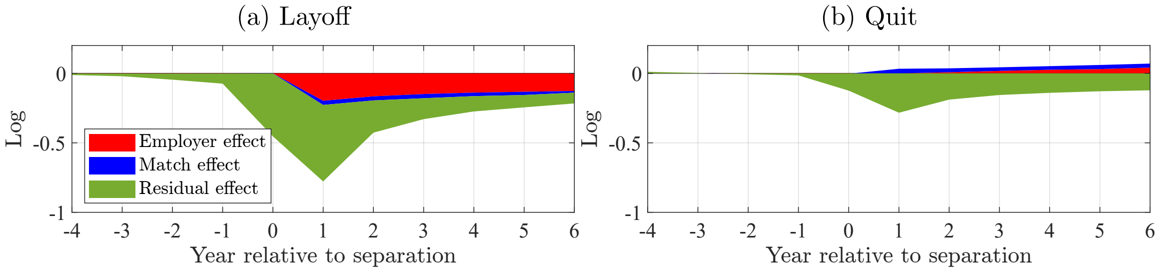
Figure A11 shows earnings losses (in 2010 CAD) by reason for separation during mass layoffs. Dashed-red and dotted-blue lines represent losses for layoffs and quits, while the solid-black line shows losses for all mass-layoff separators (layoffs, quits, other, and missing ROE). For layoffs (quits), earnings decline by $\$21,600$ ($\$7,300$) in the year following the separation and stay $\$9,900$ ($\$1,800$) lower six years after.

B.5 Decomposition of sources of earnings losses

We also provide results to complement our discussions in Section 3.2. Figure A12 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)).

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

Figure A12: 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.6 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. Below-, on-, and above-diagonal transitions represent moves to an employer with a pay premium in a lower, similar, and higher quintile, respectively. A discussion of results from this table is provided in Section 4.2.

C Addressing endogenous mobility bias

In Section 5.1, we provided an overview of the approach we use to address endogenous mobility bias inherent in AKM estimates of employer premia, as well as our main results from this exercise. Here, we provide more details on our methodology and

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.

discuss additional results.

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 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, many of which have been explored extensively in Card, Heining, and Kline (2013). 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 between workers and employers. 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

[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 $\mathrm{Cov}(x_t, x_{t'})$.

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 Section C.1, we first 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 the error term $\upsilon_{i,t}$ is not a permanent component. 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 Appendix C.3 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 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.

Throughout this section, we follow BLM in assuming that employer heterogeneity can be reduced to a broad category of employer *type*, denoted as $k_{i,t} \in \{1, 2, \ldots, K\}$. In particular, we assume that there exists a known mapping from the employer identifier to the employer type index, and that all employers in the same type are identical. Thus, we write the employer premium of the Equation (2) as $\psi_{k_{i,t}}$ rather than $\psi_{j_{i,t}}$.

Although we abstract from employer heterogeneity within employer type, job changes are still defined based on changes in employer ID. Let $m_{i,t}$ be a variable about job mobility between t and $t+1$ for worker i. BLM simply define $m_{i,t}$ as an indicator for job separation, that is, $m_{i,t} = 1$ when $j_{i,t} \neq j_{i,t+1}$ and $m_{i,t} = 0$ otherwise. Since we know the separation reason, we define $m_{i,t} = 0$ when $j_{i,t} = j_{i,t+1}$, $m_{i,t} = 1$ when $j_{i,t} \neq j_{i,t+1}$ and the worker is laid off, $m_{i,t} = 2$ when $j_{i,t} \neq j_{i,t+1}$ and the worker quits. The job separation reason is not needed to address the endogenous mobility bias, but we incorporate it formally to explore how it relates with worker outcomes.

Following BLM, we assume that $(\upsilon_t, k_t, m_{t-1})$ jointly follows a first-order Markov process. This implies that υ_t conditional on $(\upsilon_{t-1}, k_{t-1}, k_t, m_{t-1})$ is independent of past variables, and υ_{t-1} conditional on $(\upsilon_t, k_{t-1}, k_t, m_{t-1})$ is independent of future variables. Further assuming normality of $(\upsilon_{t-1}, \upsilon_t)$ conditional on (k_{t-1}, k_t, m_{t-1}), the means of these conditional distributions can be written as follows:

$$\mathbb{E}[\upsilon_t|\upsilon_{t-1}, k_{t-1}, k_t, m_{t-1}] = \rho_{t|t-1}(k_{t-1}, k_t, m_{t-1})\upsilon_{t-1}, \tag{A3}$$

$$\mathbb{E}[\upsilon_{t-1}|\upsilon_t, k_{t-1}, k_t, m_{t-1}] = \rho_{t-1|t}(k_{t-1}, k_t, m_{t-1})\upsilon_t, \tag{A4}$$

where

$$\rho_{t|t-1}(k_{t-1}, k_t, m_{t-1}) := \frac{\mathrm{Cov}(\upsilon_t, \upsilon_{t-1}|k_{t-1}, k_t, m_{t-1})}{\mathrm{Var}(\upsilon_{t-1}|k_{t-1}, k_t, m_{t-1})},$$

$$\rho_{t-1|t}(k_{t-1}, k_t, m_{t-1}) := \frac{\mathrm{Cov}(\upsilon_t, \upsilon_{t-1}|k_{t-1}, k_t, m_{t-1})}{\mathrm{Var}(\upsilon_t|k_{t-1}, k_t, m_{t-1})}.$$

These forward and backward autoregressive coefficients play a crucial role in controlling for endogenous mobility bias.

Consider the "dynamic model" of BLM that consists of $T = 4$ periods of observations, where workers stay in employer k in the first two periods and work for another employer k' in the last two periods. To keep notations simple, we denote the conditioning on $(k_1 = k_2 = k, k_3 = k_4 = k', m_1 = m_3 = 0, m_2 = m)$ by (k, k', m). Among these workers, those who move employers between periods 2 and 3 have $m \neq 0$ while those who stay have $m = 0$ and $k = k'$.

C.1 Additively separable worker and employer effects

C.1.1 Identification

We begin with the additively separable model of Equation (2). Here, we only briefly illustrate the identification strategy and refer to Appendix D.1 for detailed assumptions and derivations for this subsection.

Based on the first-order Markov property, conditional means (A3)–(A4), and law of iterated expectations, conditional average of log earnings growth while everyone stays with their employer is expressed as follows:

$$\mathbb{E}[y_4 - y_3|k, k', m] = \mathbb{E}[\upsilon_4 - \upsilon_3|k, k', m] = \big(\rho_{4|3}(k') - 1\big)\mathbb{E}[\upsilon_3|k, k', m], \tag{A5}$$

$$\mathbb{E}[y_2 - y_1|k, k', m] = \mathbb{E}[\upsilon_2 - \upsilon_1|k, k', m] = \big(1 - \rho_{1|2}(k)\big)\mathbb{E}[\upsilon_2|k, k', m], \tag{A6}$$

where we denote $\rho_{4|3}(k_3, k_4, m_3)|_{k_3=k_4=k',m_3=0}$ by $\rho_{4|3}(k')$ and $\rho_{1|2}(k_1, k_2, m_1)|_{k_1=k_2=k,m_1=0}$ by $\rho_{1|2}(k)$.

Within-job earnings growth reflects a change in the error component of earnings (rather than changes in worker or employer heterogeneity), which is correlated with the level of the error term as long as the autoregressive coefficient is not 1. Therefore, as Equation (A5) shows, post-separation earnings growth is informative about the value of the error term right after the job move (υ_3), while Equation (A6) indicates that pre-separation earnings growth reflects the value of the error term right before the job move (υ_2). Note that the error terms could be correlated with worker's mobility status between periods 2 and 3 ($m_2 = m$) and the identity of current and past/future employer, while the autoregressive coefficients do not depend on m and depend only on the current employer's identity due to the first-order Markov property.[6]

Next, consider the average log earnings growth between periods 2 and 3 for those who move from employer k to k':

$$\begin{aligned}\mathbb{E}[y_3 - y_2|k, k', m] =& \psi_{k'} - \psi_k + \mathbb{E}[\upsilon_3 - \upsilon_2|k, k', m],\\ =& \psi_{k'} - \psi_k + \frac{\mathbb{E}[y_4 - y_3|k, k', m]}{\rho_{4|3}(k') - 1} - \frac{\mathbb{E}[y_2 - y_1|k, k', m]}{1 - \rho_{1|2}(k)},\end{aligned} \tag{A7}$$

where we used Equations (A5) and (A6) for the second equality.

Earnings growth around a job change reflects a change in employer premium in addition to the change in the error term, which could bias the employer premium estimates. Equations (A5) and (A6) suggest that this bias can be eliminated because the change in the error term can be inferred from post- and pre-separation earnings growth. Therefore, from Equation (A7), $\psi_k - \psi_{k'}$ is identified when the persistence parameters $\rho_{4|3}(k')$ and $\rho_{1|2}(k)$ are identified and neither of them is equal to 1.

Next, we turn to identification of these parameters. Based on Equation (A5), we can estimate $\rho_{4|3}(k')$ by regressing y_4 on y_3 for those currently employed at employer k'. However, the OLS estimator may be biased even when κ is not correlated with υ_t. To see this, consider the covariance between y_4 and y_3:

$$\mathrm{Cov}(y_4, y_3|k, k', m) = \big(1 - \rho_{4|3}(k')\big)\mathrm{Cov}(\kappa, y_3|k, k', m) + \rho_{4|3}(k')\mathrm{Var}(y_3|k, k', m).$$

This shows that y_3 is endogenous because of the worker heterogeneity κ. By differencing out κ, $y_3 - y_2 = \upsilon_3 - \upsilon_2$ (or $y_2 - y_1$) could serve as a valid instrumental variable

[6]The term $\mathrm{E}[\upsilon_3|k, k', m]$ implies that the previous employer may have a direct effect on earnings after a move (called "state dependence" by BLM), and the term $\mathbb{E}[\upsilon_2|k, k', m]$ reflects that earnings before a move can depend on the employer of destination (called "endogenous mobility" by BLM).

(IV) for y_3 if $\mathrm{Cov}(\kappa, \upsilon_t | k, k', m) = 0$ holds. Therefore, $\rho_{4|3}(k')$ is identified from this within-employer IV regression. Similarly, $\rho_{1|2}(k)$ is identified by regressing y_1 on y_2 for those at employer k using $y_3 - y_2$ (or $y_4 - y_3$) as an instrument.

C.1.2 Estimation

Consider the following two-step estimation procedure directly based on the identification argument. In the first step, we estimate $\rho_{4|3}(k')$ and $\rho_{1|2}(k)$ using stayers ($k = k'$ and $m = 0$) based on IV regressions mentioned above. In the second step, using these estimates, we estimate ψ_k based on Equation (A7), which can be rearranged to

$$\mathbb{E}\left[\frac{y_4 - \rho_{4|3}(k')y_3}{1 - \rho_{4|3}(k')} - \frac{y_1 - \rho_{1|2}(k)y_2}{1 - \rho_{1|2}(k)}\middle| k, k', m\right] = \psi_{k'} - \psi_k. \tag{A8}$$

In other words, the second step estimation amounts to running an AKM regression using log earnings that are adjusted to eliminate the endogenous mobility bias. Since the employer premium does not depend on worker's mobility status, we use all separators ($m \neq 0$) for the second stage and do not condition on their separation reasons.

This estimation strategy requires four periods of data, where workers only move employers between the second and the third period. We implement this by constructing multiple subsamples of the AKM regression sample and pooling observations across subsamples. The first subsample consists of observations from years 2002, 2003, 2006, and 2007 and workers who did not change jobs between 2002 and 2003 and between 2006 and 2007.[7] The second subsample consists of observations from years 2003, 2004, 2007, and 2008 and workers who did not change jobs between 2003 and 2004 and between 2007 and 2008. In this way, we create 9 subsamples, with the final subsample taken from years 2010, 2011, 2014, and 2015.

Following BLM, we categorize employers into 10 types using employment-weighted k-means clustering based on log earnings cdf of each employer, evaluated at a grid of 20 percentiles (5%, 10%, ..., 95%) of the entire log earnings distribution. Because the labeling of the employer type is arbitrary, we define higher employer types as those with higher average log earnings. We pool all worker and employer observations from the AKM regression sample and subtract year-specific averages from log earnings before conducting the clustering as well as the two-step estimation. In the first step, the autoregressive coefficients are estimated by the two-stage least squares (2SLS) estimator using both of the available instruments (past or future earnings

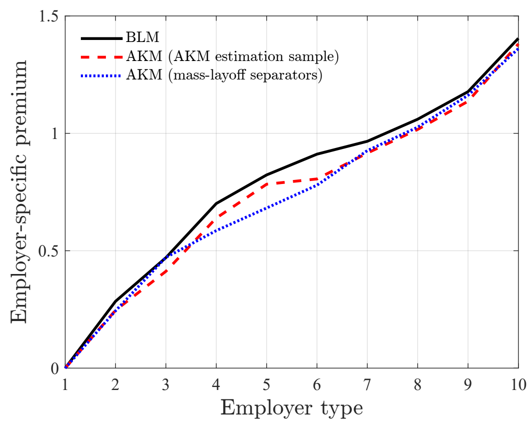
[7]Notice that we do not use data for years 2004–2005 in order to maintain the restriction (used for the AKM regression) that drops the first or last year's earnings with any employers.

growth). The 2SLS regressions are run separately for each subsample and employer type, allowing for the autoregressive coefficients to differ across subsamples as well as employer type. In the second step, we pool observations across all subsamples, and run a OLS regression of the adjusted log earnings on the employer type fixed effects and the interaction between worker and subsample fixed effects.

C.2 Estimation results for additive model

In this section, we present additional results that arise from the procedure outlined above that complement the main findings presented in Section 5.1.

Figure A13: Employer-specific premium by employer type



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 outlined in Appendix C.1, which 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).

Figure A13 presents the estimated employer-specific premia by employer type. The solid-black line represents the premia estimated using the procedure detailed in Appendix C.1, which closely follows the BLM approach. 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.[8] We observe that the estimated employer premia are quite similar in

[8]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.

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.

In Section 5.1 of the main text, we show that replacing our AKM employer premium estimates with BLM employer premium estimates results in small employer premium losses for both layoffs and quits, but the sizeable gap between both types of separations remains (Figure 10). The differences in short- and long-term employer premium outcomes when separators are categorized by their proximity to the mass-layoff are also similar under BLM and AKM estimates (Figure 11).

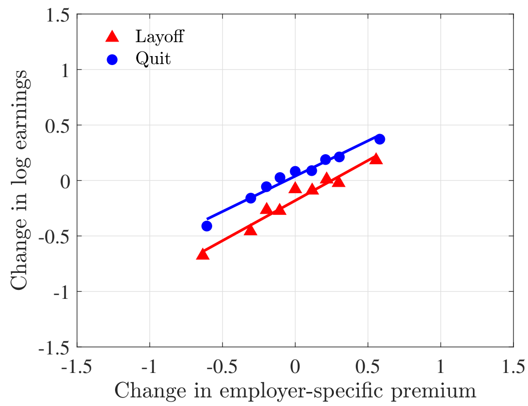
We make one additional comparison to our main results. Figure A14 plots changes in log earnings for layoffs and quits when we divide each group of mass-layoff separators into nine groups based on the distribution of $\Delta\hat{\psi}_{\text{BLM}}$: quartiles for negative values (4 groups), zeros (1 group), 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 A14 shows that workers who quit experience better earnings outcomes than those who are laid-off, even after controlling for changes in employer effects upon a transition. However, one difference is that under the AKM estimates, the slope between two axes was larger for layoffs than for quits. Under the BLM estimates, the slopes are more or less similar.

C.3 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, as it requires $\rho_{4|3}(k') \neq 1$ and $\rho_{1|2}(k) \neq 1$. 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:

$$y_{i,t} = \phi_{l_i,k_{i,t}} + \upsilon_{i,t}, \tag{A9}$$

Figure A14: 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 $\Delta\hat{\psi}_{\text{BLM}}$: 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.

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 (A9) 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 (A9) 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.

C.3.1 Identification

We begin by modifying the prior assumptions slightly so that they hold *conditional on* worker type. We assume that $(\upsilon_t, k_t, m_{t-1})$ jointly follows a first-order Markov process *conditional on* l, and $(\upsilon_{t-1}, \upsilon_t)$ is normally distributed and *independent of* l conditional on (k_{t-1}, k_t, m_{t-1}). These assumptions imply that the autoregressive coefficients still depend only on (k_{t-1}, k_t) even when the expectations in Equations (A3) and (A4) are conditional on l.[9] We relegate details on derivations and assumptions for this subsection to Appendix D.2.

As before, consider 4 periods of data, where some workers change employers k to k' only between the second and the third period. First, notice that the autoregressive coefficients $\rho_{4|3}(k')$ and $\rho_{1|2}(k)$ can be identified from the within-employer IV regressions described in Appendix C.1, because they do not depend on worker types.

Next, consider the joint density of (y_1, y_4) conditional on (y_2, y_3, k, k', m):

$$\sum_{l=1}^{L} f(y_1, y_4|l, y_2, y_3, k, k', m)p(l|y_2, y_3, k, k', m) = \sum_{l=1}^{L} f(y_1|l, y_2, k)f(y_4|l, y_3, k')p(l|k, k', m), \tag{A10}$$

where we define $f(a|b)$ and $p(a|b)$ as the probability density and mass functions of a conditional on b, respectively, and unobserved worker type is integrated out. The conditional independence result of Equation (A10) follows from the two assumptions made above. The first-order Markov property implies that υ_4 is independent of $(\upsilon_1, \upsilon_2, k, m)$ conditional on (l, υ_3, k'). Due to the log earnings model in Equation (A9), this is equivalent to y_4 being independent of (y_1, y_2, k, m) conditional on (l, y_3, k'). Similarly, y_1 is independent of (y_3, y_4, k', m) conditional on (l, y_2, k). Finally, the distribution of l conditional on (k, k', m) does not depend on (y_2, y_3), since (υ_2, υ_3) is assumed to be independent of l conditional on (k, k', m).[10]

As shown by Carroll et al. (2010) and BLM, the conditional marginal densities of y_1 and y_4 and the conditional distribution of l are nonparametrically identified from the conditional joint density of (y_1, y_4) that takes the form of Equation (A10). Then, the match effects $\phi_{l,k}$ and $\phi_{l,k'}$ are identified from the following conditional means that can be calculated from the conditional marginal densities identified earlier:

$$\mathbb{E}[y_1|l, y_2, k] = \big(1 - \rho_{1|2}(k)\big)\phi_{l,k} + \rho_{1|2}(k)y_2, \tag{A11}$$

$$\mathbb{E}[y_4|l, y_3, k'] = \big(1 - \rho_{4|3}(k')\big)\phi_{l,k'} + \rho_{4|3}(k')y_3. \tag{A12}$$

Notice that the identification still requires the autoregressive coefficients to be different from 1, but this requirement is weaker compared to the additive specification as any permanent effects of job change on earnings is absorbed by the match effect $\phi_{l,k}$.

[9] The restriction that the autoregressive coefficients do not depend on worker type is also imposed in the empirical specification of BLM.

[10] BLM also impose the restriction that the distribution of worker type does not depend on (y_2, y_3) in their empirical specification.

C.3.2 Estimation

Consider the following two-step estimation procedure. In the first step, we estimate $\rho_{4|3}(k')$ and $\rho_{1|2}(k)$ using stayers ($k = k'$ and $m = 0$) based on within-employer IV regressions, as described in Section C.1.2. In the second step, we estimate the match effect $\phi_{l,k}$ as well as the conditional distribution of worker types for separators ($m \neq 0$) using maximum likelihood estimator (MLE) based on the conditional joint density formula in Equation (A10). Due to our normality assumption, the conditional marginal densities $f(y_1|l, y_2, k)$ and $f(y_4|l, y_3, k')$ are normal with means given by Equations (A11) and (A12), and with variances $\text{Var}(y_1|l, y_2, k) = \sigma_1^2(k)$ and $\text{Var}(y_4|l, y_3, k') = \sigma_4^2(k')$. The conditional distribution of worker type $p(l|k, k', m)$ could be estimated nonparametrically, but it may not be practical to do so as it requires estimating a large number of parameters and there may be a few workers in some cell (k, k', m). Thus, we parameterize the conditional distribution as follows:

$$p(l|k, k', m) = \frac{\exp(\zeta_{l,k,m} + \xi_{l,k',m})}{\sum_{l'=1}^{L} \exp(\zeta_{l',k,m} + \xi_{l',k',m})}, \tag{A13}$$

with normalizations $\zeta_{L,k,m} = \xi_{L,k',m} = 0$ for all (k, k', m) and $\zeta_{l,K,m} = 0$ for all (l, m). With this parametrization, there are only $(L-1) \times (2K-1)$ parameters for each m instead of $(L-1) \times (K \times K)$ parameters.[11] To summarize, the parameters estimated by MLE are $\phi_{l,k}$, $\sigma_1^2(k)$, $\sigma_4^2(k')$, $\zeta_{l,k,m}$, and $\xi_{l,k',m}$.

We implement this strategy by constructing subsamples of stayers and mass-layoff separators from our main sample (instead of AKM sample). To be consistent with our analysis in Section 3.3, we focus on comparison of log earnings between one year before and three years after separation. The first subsample consists of observations from years 2005, 2006, 2010, and 2011 and (i) stayers and (ii) mass-layoff separators who are separated from the initial employer in 2007 and did not change jobs between 2010 and 2011. Similarly, the third and final subsample consists of observations from years 2007, 2008, 2012, and 2013, and stayers along with 2009 mass-layoff separators who did not change jobs between 2012 and 2013.

We use the same employer classification with $K = 10$ used for estimation of the additive model. We assume $L = 3$ worker types, and define higher worker types as those with higher match effect when matched with the middle employer type. In the first step, we estimate $\rho_{4|3}(k')$ and $\rho_{1|2}(k)$ by running 2SLS with two instruments,

[11] Appendix D.2.3 shows that $p(l|k, k', m)$ with this parametrization can still satisfy the condition required for identification.

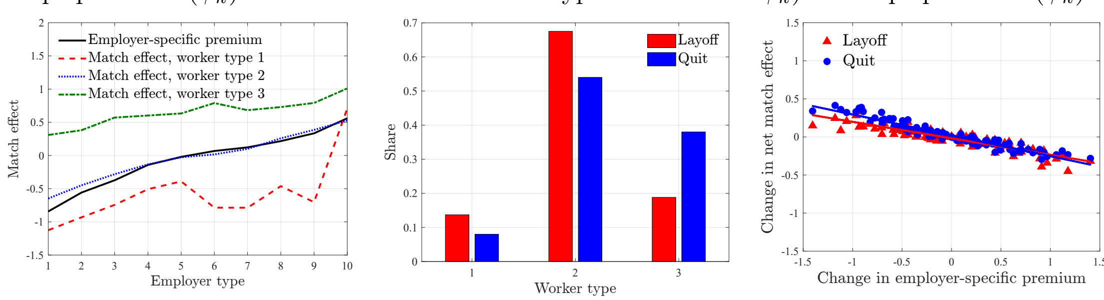
separately for stayers in each employer type and subsample. In the second step, we pool separators who are laid off or quit across subsamples for MLE.

C.4 Estimation results for interactive model

Figure A15 presents the key findings that emerge from the estimation procedure outlined above. Panel (a) shows the estimated match effect $\phi_{l,k}$ for each worker type $l \in \{1, 2, 3\}$ and employer type $k \in \{1, 2, \ldots, 10\}$ (obtained from Appendix C.1). 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 A15: 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 outlined in Appendix C.3.2. Panel (a) shows the estimated match effect $\phi_{l,k}$ for each worker type $l \in \{1, 2, 3\}$ and for each employer type $k \in \{1, 2, \ldots, 10\}$ (obtained from Appendix C.1). 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 Proofs of identification results in Appendix C

Below, we provide proofs and additional details that support the identification and estimation procedure used to correct for endogenous mobility bias in Appendix C.

D.1 Additively separable worker and employer effects

D.1.1 Assumptions

Define $x_t^{t'} \coloneqq (x_t, \ldots, x_{t'})$ to be the history of the random variable x_t between periods t and t'.

Assumption 1.

1. *$(\upsilon_t, k_t, m_{t-1})$ is independent of $(\upsilon_1^{t-2}, k_1^{t-2}, m_1^{t-2})$ conditional on $(\upsilon_{t-1}, k_{t-1})$.*
2. *$(\upsilon_{t-1}, \upsilon_t)$ is normally distributed conditional on (k_{t-1}, k_t, m_{t-1}).*
3. *υ_t is mean independent of κ conditional on (k_1^T, m_1^T).*

Assumption 1(i) is a first-order Markov condition of BLM, and it implies the followings.

Lemma 1. *Assumption 1(i) holds if and only if (a) (k_t, m_{t-1}) is independent of $(\upsilon_1^{t-2}, k_1^{t-2}, m_1^{t-2})$ conditional on $(\upsilon_{t-1}, k_{t-1})$, (b) υ_t is independent of $(\upsilon_1^{t-2}, k_1^{t-2}, m_1^{t-2})$ conditional on $(\upsilon_{t-1}, k_{t-1}, k_t, m_{t-1})$.*

Proof. To simplify notations, we re-state the lemma as follows: $y \perp\!\!\!\perp z|x$ and $w \perp\!\!\!\perp z|(x, y)$ if and only if $(y, w) \perp\!\!\!\perp z|x$. Let $f(y|x)$ be the density of y conditional on x.

($\Rightarrow$) Observe that

$$f(w|x, y, z) = \frac{f(w, x, y, z)}{f(x, y, z)} = \frac{f(y, w|x, z)}{f(y|x, z)} = \frac{f(y, w|x, z)}{f(y|x)},$$

where the last equality follows from $y \perp\!\!\!\perp z|x$.

On the other hand, we have $f(w|x, y, z) = f(w|x, y)$ due to $w \perp\!\!\!\perp z|(x, y)$. Combining these to gives

$$f(y, w|x, z) = f(w|x, y)f(y|x) = f(y, w|x).$$

($\Leftarrow$) First, it is trivial that $(y, w) \perp\!\!\!\perp z|x$ implies $y \perp\!\!\!\perp z|x$. Next,

$$f(w|x, y, z) = \frac{f(w, x, y, z)}{f(x, y, z)} = \frac{f(y, w|x, z)}{f(y|x, z)} = \frac{f(y, w|x)}{f(y|x)}.$$

$\square$

Lemma 2. *Assumption 1(i) implies* $(\upsilon_{t-1}, k_{t-1}, m_{t-1})$ *is independent of* $(\upsilon_{t+1}^T, k_{t+1}^T, m_t^T)$ *conditional on* (υ_t, k_t).

Proof.

$$\begin{aligned} f(\upsilon_{t-1}, k_{t-1}, m_{t-1}|\upsilon_t^T, k_t^T, m_t^T) =& \frac{f(\upsilon_{t-1}^T, k_{t-1}^T, m_{t-1}^T)}{f(\upsilon_t^T, k_t^T, m_t^T)} \\ =& \frac{f(\upsilon_{t+1}^T, k_{t+1}^T, m_t^T|\upsilon_{t-1}, \upsilon_t, k_{t-1}, k_t, m_{t-1})}{f(\upsilon_{t+1}^T, k_{t+1}^T, m_t^T|\upsilon_t, k_t)} \frac{f(\upsilon_{t-1}, \upsilon_t, k_{t-1}, k_t, m_{t-1})}{f(\upsilon_t, k_t)} \\ =& f(\upsilon_{t-1}, k_{t-1}, m_{t-1}|\upsilon_t, k_t), \end{aligned}$$

where we used the fact that $f(\upsilon_{t+1}^T, k_{t+1}^T, m_t^T|\upsilon_{t-1}, \upsilon_t, k_{t-1}, k_t, m_{t-1}) = f(\upsilon_{t+1}^T, k_{t+1}^T, m_t^T|\upsilon_t, k_t)$ because $(\upsilon_{t+1}, k_{t+1}, m_t)$ is independent of $(\upsilon^{t-1}, k^{t-1}, m^{t-1})$ conditional on (υ_t, k_t). To see this,

$$\begin{aligned} f(\upsilon_{t+1}^T, k_{t+1}^T, m_t^T|\upsilon_{t-1}, \upsilon_t, k_{t-1}, k_t, m_{t-1}) &= \prod_{t'=t}^{T-1} f(\upsilon_{t'+1}, k_{t'+1}, m_{t'}|\upsilon_{t-1}^{t'}, k_{t-1}^{t'}, m_{t-1}^{t'-1}) \\ &= \prod_{t'=t}^{T-1} f(\upsilon_{t'+1}, k_{t'+1}, m_{t'}|\upsilon_{t'}, k_{t'}). \end{aligned}$$

Similarly,

$$\begin{aligned} f(\upsilon_{t+1}^T, k_{t+1}^T, m_t^T|\upsilon_t, k_t) &= \left[\prod_{t'=t+1}^{T-1} f(\upsilon_{t'+1}, k_{t'+1}, m_{t'}|\upsilon_{t-1}^{t'}, k_{t-1}^{t'}, m_{t-1}^{t'-1})\right] f(\upsilon_{t+1}, k_{t+1}, m_t|\upsilon_t, k_t) \\ &= \left[\prod_{t'=t+1}^{T-1} f(\upsilon_{t'+1}, k_{t'+1}, m_{t'}|\upsilon_{t'}, k_{t'})\right] f(\upsilon_{t+1}, k_{t+1}, m_t|\upsilon_t, k_t) \\ &= \prod_{t'=t}^{T-1} f(\upsilon_{t'+1}, k_{t'+1}, m_{t'}|\upsilon_{t'}, k_{t'}). \end{aligned}$$

Again, this implies υ_{t-1} is independent of $(\upsilon_{t+1}^T, k_{t+1}^T, m_t^T)$ conditional on $(\upsilon_t, k_{t-1}, k_t, m_{t-1})$ due to Lemma 1. □

Lemmas 1 and 2 imply the following mean independence conditions:

$$\mathrm{E}[\upsilon_t|\upsilon_1^{t-1}, k_1^t, m_1^{t-1}] = \mathrm{E}[\upsilon_t|\upsilon_{t-1}, k_{t-1}, k_t, m_{t-1}], \tag{A14}$$

$$\mathrm{E}[\upsilon_{t-1}|\upsilon_t^T, k_{t-1}^T, m_{t-1}^T] = \mathrm{E}[\upsilon_{t-1}|\upsilon_t, k_{t-1}, k_t, m_{t-1}]. \tag{A15}$$

Assumption 1(ii) further implies the linearity of these conditional means, as shown by Equations (A3) and (A4). Assumption 1(iii), $\mathrm{E}[\upsilon_t|\kappa, k_1^T, m_1^T] = \mathrm{E}[\upsilon_t|k_1^T, m_1^T]$, states that the error term is orthogonal to the worker effect conditional on the entire history of job mobility. Assumptions 1 (ii) and (iii) are additional assumptions (compared to

BLM) that ensure identification of our additive model based on moment conditions.

D.1.2 Details for Section C.1

Derivation of Equation (A5) and (A6). Equation (A5) follows from the following

$$\mathrm{E}[\upsilon_4|k,k',m] = \mathrm{E}\big[\mathrm{E}[\upsilon_4|\upsilon_3,k,k',m]\big|k,k',m\big] = \mathrm{E}\big[\mathrm{E}[\upsilon_4|\upsilon_3,k']\big|k,k',m\big] = \rho_{4|3}(k')\mathrm{E}[\upsilon_3|k,k',m],$$

where the first equality holds due to the law of iterated expectations, the second equality reflects the first-order Markov property in Equation (A14), and the last equality follows from Equation (A3). Similarly, Equation (A6) can be derived as:

$$\mathrm{E}[\upsilon_1|k,k',m] = \mathrm{E}\big[\mathrm{E}[\upsilon_1|\upsilon_2,k,k',m]\big|k,k',m\big] = \mathrm{E}\big[\mathrm{E}[\upsilon_1|\upsilon_2,k]\big|k,k',m\big] = \rho_{1|2}(k)\mathrm{E}[\upsilon_2|k,k',m].$$

Identification of $\rho_{4|3}(k')$ and $\rho_{1|2}(k)$. Consider a within-employer IV regression of y_4 on y_3, using $y_t - y_{t-1}$ for $t \in \{2,3\}$ as an instrument. The probability limit of the IV estimator is

$$\begin{aligned}\frac{\mathrm{Cov}(y_4, y_t - y_{t-1}|k,k',m)}{\mathrm{Cov}(y_3, y_t - y_{t-1}|k,k',m)} &= \frac{\mathrm{Cov}(\kappa + \psi_{k'} + \upsilon_4, \upsilon_t - \upsilon_{t-1}|k,k',m)}{\mathrm{Cov}(\kappa + \psi_{k'} + \upsilon_3, \upsilon_t - \upsilon_{t-1}|k,k',m)} \\ &= \frac{\mathrm{Cov}(\upsilon_4, \upsilon_t - \upsilon_{t-1}|k,k',m)}{\mathrm{Cov}(\upsilon_3, \upsilon_t - \upsilon_{t-1}|k,k',m)} = \rho_{4|3}(k'). \end{aligned} \tag{A16}$$

The second equality of Equation (A16) follows from Assumption 1(iii), which implies $\mathrm{Cov}(\kappa, \upsilon_t|k,k',m) = 0$ for all t because

$$\mathrm{E}[\kappa\upsilon_t|k,k',m] = \mathrm{E}\big[\kappa\mathrm{E}[\upsilon_t|\kappa,k,k',m]|k,k',m\big] = \mathrm{E}[\kappa\mathrm{E}[\upsilon_t|k,k',m]|k,k',m] = \mathrm{E}[\kappa|k,k',m]\mathrm{E}[\upsilon_t|k,k',m].$$

The third equality of Equation (A16) follows from Assumptions 1(i) and (ii), which together imply

$$\mathrm{E}\big[\upsilon_4|\upsilon_3,\upsilon_2,\upsilon_1,k,k'\big] = \mathrm{E}[\upsilon_4|\upsilon_3,k'] = \rho_{4|3}(k')\upsilon_3.$$

Therefore, by the law of iterated expectations, for $t = 1,2,3$,

$$\mathrm{E}\big[\upsilon_4 - \rho_{4|3}(k')\upsilon_3|\upsilon_t,k,k'\big] = 0 \Rightarrow \mathrm{Cov}\left(\upsilon_4 - \rho_{4|3}(k')\upsilon_3, \upsilon_t|k,k'\right) = 0.$$

Similarly, a within-employer IV regression of y_1 on y_2 using $y_t - y_{t-1}$ for $t \in \{3,4\}$ as an IV gives $\rho_{1|2}(k)$:

$$\begin{aligned}\frac{\mathrm{Cov}(y_1, y_t - y_{t-1}|k,k',m)}{\mathrm{Cov}(y_2, y_t - y_{t-1}|k,k',m)} = \frac{\mathrm{Cov}(\kappa + \psi_k + \upsilon_1, \upsilon_t - \upsilon_{t-1}|k,k',m)}{\mathrm{Cov}(\kappa + \psi_k + \upsilon_2, \upsilon_t - \upsilon_{t-1}|k,k',m)} &= \\ \frac{\mathrm{Cov}(\upsilon_1, \upsilon_t - \upsilon_{t-1}|k,k',m)}{\mathrm{Cov}(\upsilon_2, \upsilon_t - \upsilon_{t-1}|k,k',m)}& \\ = \rho_{1|2}(k).& \end{aligned} \tag{A17}$$

D.2 Interactive worker and employer effects

D.2.1 Assumptions

For identification and estimation of the model with worker-employer fixed effects in Equation (A9), we modify Assumption 1 as follows.

Assumption 2.

1. *$(\upsilon_t, k_t, m_{t-1})$ is independent of $(\upsilon_1^{t-2}, k_1^{t-2}, m_1^{t-2})$ conditional on $(l, \upsilon_{t-1}, k_{t-1})$.*

2. *$(\upsilon_{t-1}, \upsilon_t)$ is normally distributed and independent of l conditional on (k_{t-1}, k_t, m_{t-1}).*

3. *υ_t is mean independent of l conditional on (k_1^T, m_1^T).*

Assumption 2(i) corresponds to Assumption 2 of BLM. Assumptions 2(ii)–(iii) are additional assumptions to identify the autoregressive coefficients that do not depend on worker heterogeneity l. Assumption 2 implies that the autoregressive coefficients do not depend on worker heterogeneity and can be identified based on the within-employer IV regressions used for the additive model, as described in Section C.1.

We add assumptions corresponding Assumption 3 of BLM. To simplify exposition, we consider the case of $K = 2$ and define $F(a|b)$ as the cdf of a conditional on b.

Assumption 3.

1. *There exist $(m^{1,1}, m^{2,1}, m^{2,2}, m^{1,2})$ such that $\lambda_1, \ldots, \lambda_L$ are all distinct, where*
$$\lambda_l := \frac{p(l|1,1,m^{1,1})p(l|2,2,m^{2,2})}{p(l|2,1,m^{2,1})p(l|1,2,m^{1,2})}.$$

2. *There exist (y_2^1, y_3^1) and $\{y_1^h, y_4^h\}_{h=1}^H$ such that $A(1,1,m^{1,1})$, $A(2,1,m^{2,1})$, $A(2,2,m^{2,2})$, and $A(1,2,m^{1,2})$ have rank L, where $A(k,k',m)$ is a $H \times H$ matrix with (h,h') element $F(y_1^h, y_4^{h'}|y_2^1, y_3^1, k, k', m)$.*

3. *There exists k such that $\phi_{l',k} > \phi_{l,k}$ for $l' > l$.*

Assumptions 3(i)–(ii) are Assumption 3 of BLM, which is for their "static model." The fact that the distribution of worker type does not depend on (y_2, y_3) enables us to use the assumption for their static model rather than their dynamic model (Assumption 4 of BLM). Here, we modified Assumption 3 of BLM to allow for worker type to depend on the job separation reason.

Assumption 3(iii) defines the labeling of worker type such that higher worker types are those with higher match effect at an employer type. This normalization is commonly made in the literature (e.g., Carroll et al., 2010). Without Assumption 3(iii), identification is up to arbitrary labeling of worker types, as noted by BLM.

D.2.2 Identification of the conditional marginal distributions

We provide the proof for the simple case, where $H = L$, and $y_1^H = y_4^H = \infty$, and $\phi_{l',1} > \phi_{l,1}$ for $l' > l$. See BLM for the general case $H > L$, which enables nonparametric identification of $f(y_1|l, y_2, k)$ and $f(y_4|l, y_3, k')$.

Based on Equation (A10), the matrix $A(k, k', m)$ can be written as follows:

$$A(k, k', m) = B(k)D(k, k', m)C(k')^\top, \tag{A18}$$

where $B(k)$ is a $H \times L$ matrix with (h, l) element $F(y_1^h|l, y_2^1, k)$, $C(k')$ is a $H \times L$ matrix with (h', l) element $F(y_4^{h'}|l, y_3^1, k')$, and $D(k, k', m)$ is a $L \times L$ diagonal matrix with (l, l) element $p(l|k, k', m)$. Since $A(2, 1, m^{2,1})$ is invertible,

$$\begin{aligned} A(1, 1, m^{1,1})A(2, 1, m^{2,1})^{-1} =& \left\{B(1)D(1, 1, m^{1,1})C(1)^\top\right\} \left\{B(2)D(2, 1, m^{2,1})C(1)^\top\right\}^{-1} \\ =& B(1) \left\{D(1, 1, m^{1,1})D(2, 1, m^{2,1})^{-1}\right\} B(2)^{-1}. \end{aligned}$$

Similarly, $A(1, 2, m^{1,2})$ is invertible and

$$A(2, 2, m^{2,2})A(1, 2, m^{1,2})^{-1} = B(2) \left\{D(2, 2, m^{2,2})D(1, 2, m^{1,2})^{-1}\right\} B(1)^{-1}.$$

Therefore,

$$\begin{aligned} &\left\{A(1, 1, m^{1,1})A(2, 1, m^{2,1})^{-1}\right\} \left\{A(2, 2, m^{2,2})A(1, 2, m^{1,2})^{-1}\right\} \\ =& B(1) \underbrace{\left\{D(1, 1, m^{1,1})D(2, 1, m^{2,1})^{-1}D(2, 2, m^{2,2})D(1, 2, m^{1,2})^{-1}\right\}}_{:=\Lambda} B(1)^{-1}, \end{aligned} \tag{A19}$$

where Λ is a $L \times L$ diagonal matrix with (l, l) element λ_l defined above. Equation (A19) is the eigendecomposition, where columns of $B(1)$ are eigenvectors of $A(1, 1, m^{1,1})A(2, 1, m^{2,1})^{-1}A(2, 2, m^{2,2})A(1, 2, m^{1,2})^{-1}$ and λ_l are associated eigenvalues. This decomposition is unique because Assumption 3(i) implies that the eigenvalues (λ_l) are distinct and Assumption 3(iii) implies that the eigenvectors (columns of $B(1)$) are ordered. Moreover, the scale of eigenvectors is determined by the fact that $F(y_1^H|l, y_2^1, 1) = F(\infty|l, y_2^1, 1) = 1$ for all l. Therefore, $B(1)$ is identified.

Next, consider Equation (A18) for $(1, 1, m^{1,1})$. The invertibility of $A(1, 1, m^{1,1})$ implies that $B(1)$ is also invertible. Therefore,

$$B(1)^{-1}A(1, 1, m^{1,1}) = D(1, 1, m^{1,1})C(1)^\top.$$

Since $F(y_4^H|l, y_3^1, k') = F(\infty|l, y_3^1, k') = 1$ for all l, each element in the last row of $C(1)$ is one. So, the elements of $D(1, 1, m^{1,1})$ are identified from the last column of $D(1, 1, m^{1,1})C(1)^\top$, which in turn implies that $C(1)$ is identified. Similarly, we can identify $B(2)$ and $C(2)$ using $A(2, 1, m^{2,1})$ and $A(1, 2, m^{1,2})$. Given $B(1)$, $B(2)$, $C(1)$, and $C(2)$, $D(k, k', m)$ is identified from $A(k, k', m)$ for all (k, k', m).

D.2.3 Parametric assumption on worker type distribution

We show that Assumption 3(i) can be satisfied with the parametric assumption in Equation (A13) as long as there is some variation in m.

First, suppose that there is no heterogeneity in m. Then,

$$\lambda_l = \frac{p(l|1,1,m)p(l|2,2,m)}{p(l|2,1,m)p(l|1,2,m)} = \frac{\left(\sum_{l'=1}^{L} \exp(\zeta_{l',2,m} + \xi_{l',1,m})\right)\left(\sum_{l'=1}^{L} \exp(\zeta_{l',1,m} + \xi_{l',2,m})\right)}{\left(\sum_{l'=1}^{L} \exp(\zeta_{l',1,m} + \xi_{l',1,m})\right)\left(\sum_{l'=1}^{L} \exp(\zeta_{l',2,m} + \xi_{l',2,m})\right)}.$$

Since this does not depend on l, Assumption 3(i) is not satisfied.

Now, consider the two distinct values $m^1 \neq m^2$:

$$\lambda_l = \frac{p(l|1,1,m^1)p(l|2,2,m^1)}{p(l|2,1,m^2)p(l|1,2,m^2)} \propto \exp\left(\sum_{k=1}^{2}(\zeta_{l,k,m^1} - \zeta_{l,k,m^2}) + \sum_{k'=1}^{2}(\xi_{l,k',m^1} - \xi_{l,k',m^2})\right),$$

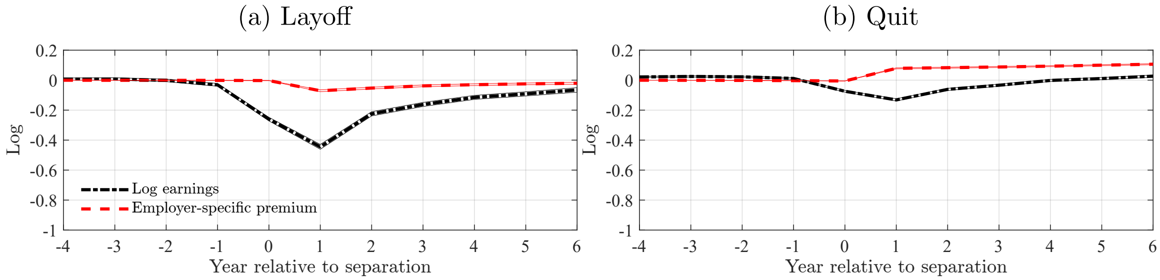
which can vary with l if $\zeta_{l,k,m}$ and $\xi_{l,k',m}$ vary with l and m.

E Interaction between worker and firm outcomes

In this section, we present results to supplement our discussions in Section 5.2. 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 (all non-mass-layoff separations with non-missing ROE information), 45% (55%) are quits, while only 14% (18%) are layoffs. This is very different than the composition of mass-layoff separations where layoffs are much more

Figure A16: 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).

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 A16. 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.

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

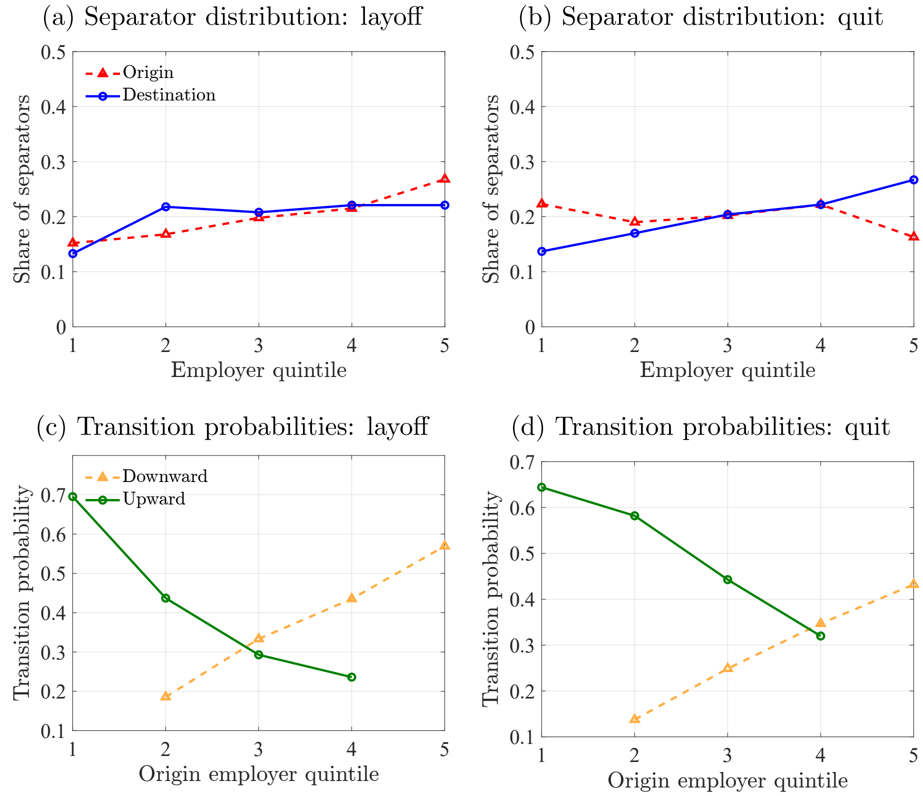
Note: This table presents five rows for non-mass-layoff separations with a different reason (layoff, quit, or average across layoffs and quits) with below-diagonal, on-diagonal, and above-diagonal transitions: (i) the fraction of separators, (ii) average change in log earnings, (iii) average change in 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.

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 3.3.

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). Moving to average changes in earnings, 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). We find that 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 on average do not experience losses in earnings when they find reemployment with employers in lower quintiles but experience large gains (38.1 log points) in earnings 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 average earnings changes between workers who quit in non-mass layoffs and those in mass layoffs are mostly 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 A17 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 prob-

Figure A17: 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.

abilities are smaller and upward transition probabilities are larger for layoffs during non-mass layoffs regardless of the employer effect quintile of the origin employer when compared with those probabilities for layoffs during mass layoffs. 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, we find that 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.