Job-to-Job Flows and the Consequences of Job Separations

Bruce Fallick
Federal Reserve Bank of Cleveland
bruce.fallick@clev.frb.org

John Haltiwanger
University of Maryland
halt@umd.edu

Erika McEntarfer
U.S. Census Bureau
erika.mcentarfer@census.gov

Matthew Staiger
University of Maryland &
U.S. Census Bureau
mstaiger@umd.edu

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Abstract

A substantial empirical literature documents large and persistent average earnings losses following job displacement, defined as a worker separating from a rapidly contracting firm. Our paper extends the literature on displaced workers by providing a comprehensive picture of earnings and employment outcomes for all workers who separate. We show that for workers not recalled to their previous employers, earnings losses follow separations in general, as opposed to displacements in particular. The key predictor of earnings losses is not whether the origin firm is contracting but rather the length of the nonemployment spell following the job separation. Moreover, displaced workers are no more likely to experience a substantial spell of nonemployment than are other non-recalled separators. Our results suggest that future research on the consequences of job loss should work to disentangle the strong association between nonemployment and earnings losses, as opposed to focusing specifically on displaced workers. We also find that workers who experience spells of nonemployment tend to move to less productive, lower-paying firms, suggesting that models of the labor market that produce job ladders may offer a useful framework for studying this issue.

JEL codes: J63, J64

I. Introduction

The US labor market exhibits a high rate of reallocation of workers across firms. While this dynamism is considered to be a key ingredient in aggregate productivity growth (Haltiwanger et al. 2017) and important for the career progression of individual workers (Topel and Ward 1992), there is a general concern that some workers are harmed in the process (Jacobson et al. 1993a). The economics literature has focused on one group of workers in particular: displaced workers, who are often defined as workers who separate from distressed employers (firms undergoing major downsizings through plant closings or large contractions).[1] The many studies on displaced workers in the United States consistently find that these workers suffer large and persistent earnings losses on average. Documenting these losses and understanding their source are critical steps in devising effective policies to mitigate the adverse consequences of the reallocation of workers across firms.[2] Furthermore, given the magnitude of the earnings losses, the explanation is likely to be informative of the more general process through which earnings are determined.

In this paper, we show that the earnings losses of displaced workers are not specific to displacement per se; rather, they are common to all separators and are, instead, specific to individuals who experience prolonged periods of nonemployment. While we are unable to identify with confidence the key mechanism, available evidence suggests that theories of job ladders offer a useful framework through which to interpret the results.

Our main contribution in this paper is to estimate the earnings consequences of separations by both the health of the origin firm (distressed vs. non-distressed) and the length of the nonemployment spell prior to finding a new job. To do this, we use administrative earnings data from the Longitudinal Employer-Household Dynamics (LEHD) program for workers from five large states. We find that all permanent separators—individuals who change employers—do well if they spend little to no time in nonemployment. Separators who find a new job in the same quarter in which they separate tend to experience positive earnings growth;[3] this is true for separators from

[1] In addition to Jacobson, LaLonde, and Sullivan (1993a), see Schoeni and Dardia (2003), Couch and Placzek (2010), and von Wachter, Song, and Manchester (2009), all of whom use administrative data similar in structure to what we use here. On the use of firm distress as an indicator of displacement, see Flaaen, Shapiro, and Sorkin (2019); and von Wachter, Handwerker, and Hildreth (2009).

[2] See Carrington and Fallick (2017) for a recent assessment of the sources of loss.

[3] This finding is consistent with the literature on job mobility and wage growth (see Topel and Ward 1992; Brown, Haltiwanger, and Lane 2006; Haltiwanger, et al. 2018; and Liu 2019).

both distressed and non-distressed employers.[4] However, permanent separators who remain in nonemployment for a substantial period of time—at least one full quarter—experience large and persistent reductions in earnings. Again, we observe this same pattern for separators from both distressed and non-distressed firms, with similar magnitudes across the two groups. Using quantile regression, we continue to find a central role for nonemployment and a negligible role for firm distress throughout the distribution of effects.

Not only are permanent separators from distressed firms no more likely than other separators to suffer large earnings losses conditional on the duration of nonemployment, we also find that they are no more likely to spend significant periods in nonemployment. We do find, not surprisingly, that separators from non-distressed firms are much more likely to be recalled—return to the same employer after a spell of nonemployment—after a separation. As we discuss in more detail below, this increased propensity to be recalled and our focus of permanent separators helps to reconcile some of the differences between our findings and other existing work, which finds that workers who separate (permanent separators and recalls together) from distressed firms are more likely to experience prolonged periods of unemployment.

Given the emphasis in the displaced worker literature on the exogenous nature of job loss in mass layoff events, it may be puzzling that we find similar outcomes for job separators regardless of firm distress. A plausible interpretation of our results is that these job separators share a common mechanism driving earnings losses—a mechanism in which the duration of nonemployment plays a central role. However, this interpretation is complicated by two potential sources of endogeneity. First, separations from non-distressed firms are more likely to be the consequence of choices made by the worker, and these may be fundamentally different than those arising from firm-level decisions. Second, the relationship between the duration of nonemployment and subsequent earnings losses may be in part determined by the worker. In particular, workers who choose to take longer periods of time off from work might also choose to find a new job with lower wage rates or shorter hours. While our main specifications include individual fixed effects, it is possible that the decision to separate is correlated with time-varying factors (other than the separation itself) that determine future earnings. Both points raise the

[4] Although workers who separate from distressed firms are often referred to as "displaced workers" in the literature, we will often refer to them as distressed separators (i.e., separators from distressed firms) for greater precision.

concern that the main findings could be driven by worker heterogeneity (either observed or unobserved).

A number of results cast doubt on the possibility that the relationship between earnings losses and the duration of nonemployment is driven by heterogeneity across workers. First, our main findings (of no difference in earnings losses across firm status and a strong relationship between duration of nonemployment and earnings losses) hold within demographic groups that we would expect to have stronger or weaker degrees of attachment to the labor force, and thus for which we are less or more concerned about the role of worker choice. Second, because the nature of separations is likely to be different at different points in the business cycle, we would expect the relevant heterogeneity to differ as well. However, our main results hold across a large range of macroeconomic conditions in the quarter of separation.[5] Third, our findings are robust to the inclusion of a linear individual-specific time trend, suggesting that workers who spend significant time in nonemployment are not simply on a flatter earnings trajectory.

Additional estimates based on firm-level outcomes suggest that models of the labor market that predict the existence of job ladders—i.e., that some firms offer higher pay than others—offer a useful lens through which to interpret our results. We find that the duration of time spent in nonemployment is strongly associated with movements down the job ladder defined by firm-level measures of average pay and productivity. Furthermore, using estimates of the firm-level pay premium based on the empirical methodology developed by Abowd et al. (1999), we find that the movements down the job ladder explain a quantitatively important portion of the effects on individual earnings.

Our paper contributes to recent research that interprets the earnings losses of displaced workers using a job ladder framework. Empirical work on the topic has found mixed results. In the context of Germany, Schmeider et al. (2020) and Fackler et a. (2017) find that the earnings losses of displaced workers are driven by a loss in firm-specific pay premiums. In contrast, Lachowska et al. (2020) and Moore and Scott-Clayton (2019) find a smaller role for firm-level pay premiums for displaced workers in the United States. The theoretical models developed by Jarosch (2015), Krolikowski (2017), and Jung and Kuhn (2019) interpret the earnings losses of

[5] Couch, Jolly, and Placzek (2011) and Davis and von Wachter (2011) also examine how losses vary over the business cycle.

displaced workers using models of the labor market that produce job ladders. While these models offer a partial explanation of our results, none of the existing models explain our finding that the duration of time spent in nonemployment is strongly related to both individual-level earnings losses and movements down the job ladder. Extending existing models of job ladders to explain the relationship between earnings losses and duration of nonemployment—for separators from both distressed and non-distressed firms—should be a priority of future research.

A small number of previous empirical papers have studied the link between the duration of nonemployment and earnings losses for displaced workers in the US.[6] Leveraging the large sample size and administrative nature of our data, we complement the earlier work by exploring this relationship in a more detailed and nonparametric way.[7] More important, our paper helps to reconcile findings from the literatures on displaced workers and job mobility (Topel and Ward 1992). These literatures would seem to provide conflicting views on whether job mobility leads to earnings losses or gains. By using a unified empirical framework, we are able to reconcile these apparent contradictions by illustrating the central role played by the time spent in nonemployment between jobs: Workers who switch employers tend to experience earnings gains when they spend little to no time in nonemployment but tend to experience substantial and persistent earnings losses if they experience a prolonged period of nonemployment.

This paper proceeds as follows. Section II describes the LEHD data infrastructure and the samples we use in this analysis. Section III presents the measurement methodology for tracking separations, employer-to-employer flows, and nonemployment durations in the administrative data. Section IV provides descriptive statistics for our sample. Section V presents our main estimates of earnings losses for distressed and non-distressed separators. Section VI presents our estimates of the predicted duration of nonemployment for distressed and non-distressed separators. Section VII explores various economic explanations for our main findings. Section VIII concludes.

[6] For example, Addison and Portugal (1989). A few papers have documented a similar relationship between duration of nonemployment and the earnings losses of displaced workers in the context of Europe. Using data for Portugal, Carneiro and Portugal (2006) find that earnings losses are larger for displaced workers who experience a spell of joblessness. The same pattern is documented in the United Kingdom by Hijzen, Upward, and Wright (2010) and in France and Germany by Bender et al. (2002).

[7] See Jacobson, LaLonde, and Sullivan 1993a for a discussion of the advantages of using administrative data to estimate the earnings losses of displaced workers.

II. Data

We analyze the employment and earnings consequences of job separations using data housed at the US Census Bureau's LEHD program. The LEHD program maintains a variety of survey and administrative data from a number of state and federal agencies. For this analysis, we chiefly exploit administrative data that combine a worker's employment and earnings history with information about the firm available from state-level unemployment insurance (UI) wage data and the Quarterly Census of Employment and Wages (QCEW) data. Both UI and QCEW data are available for states that have partnered with the LEHD program, currently all 50 states and the District of Columbia. A thorough discussion of the LEHD data is provided in Abowd, Haltiwanger, and Lane (2004) and in Abowd et al. (2006); a brief description follows.

State-level unemployment insurance (UI) data contain quarterly earnings for employees covered by state unemployment insurance systems, over 96 percent of private-sector employment.[8] A firm, as defined in this analysis, is a collection of workers who share a common unemployment insurance system identifier.[9] Individual wage records are then linked across quarters to create individual work histories. The firm identifier on the UI records is used to link to information on the firm available in the QCEW data, which contain information on the industry and location of the firm. A limited list of worker demographics, namely, sex and date of birth, is available from links to the Census administrative data, providing a virtual universe of information about sex and age.

From this administrative data we construct a panel of linked employer-employee observations, pooling the wage histories from five large states: California, North Carolina, Oregon, Washington, and Wisconsin.[10] From these pooled data we create a sample of workers, namely, workers with at least three years of job tenure in one of four reference quarters–1999:2, 2001:2, 2005:2, and 2009:2–that span a variety of macroeconomic conditions. For simplicity, much of our

[8] Data quality issues produce a small number of large outlier observations in the earnings data. We identify outliers by comparing quarterly earnings records to the median earnings value observed over the sample for each individual and winsorize these outliers at the 95th percentile. This approach is more appealing than winsorizing by earnings levels, since it does not incorrectly adjust the earnings of high-wage workers. See Appendix for a detailed description of the winsorization methodology.

[9] Our data do not identify occupations or job titles. We use the terms "firm" and "employer" interchangeably throughout the paper.

[10] We narrow the sample to these five states in part to reduce the size of the analysis as well as to have the longest possible time series, as the availability of LEHD data for a particular year varies by state. Approximately 10 states have data available in the early 1990s.

analysis focuses on reference quarter 2005:2, but we also show some results for the other reference quarters. We include in our sample both male and female workers, age 25-55, in the reference quarter. We impose an additional restriction and focus on the worker's "main" job, i.e., the worker's primary source of earnings during the year previous to the separation. Previous work estimating employer-to-employer flows for all jobs found that over 95 percent of employer-to-employer flows were main job to main job flows, so this restriction simplifies the analysis while retaining almost all flows of interest (Bjelland et al., 2011). Although our sample comprises workers from five states, we track their earnings outcomes on a national basis. That is, for a worker who separates from one of our five states, we use all available national LEHD data to track earnings and employment outcomes.

We limit the sample to workers who remain employed or become re-employed within eight quarters of separation in the reference quarter. We divide this sample into three categories: stayers, permanent separators, and recalls. We define "stayers" as workers who are continually employed with the same employer for at least the three quarters after the reference quarter. We define "permanent separators" as workers who separate from their employer in the reference quarter and become re-employed with a new employer. We define "recalls" as workers who separate from their employer in the reference quarter but return to this same employer. Note that given the nature of our data, we can only identify recalls if the worker experiences a full quarter of nonemployment before rejoining the firm.[11] Recalls that do not experience a full quarter of nonemployment are, perforce, categorized as stayers. Our analysis excludes workers who do not fall within one of the three categories. Specifically, we exclude workers who do not separate in the reference quarter but separate in one of the subsequent three quarters and workers who separate in the reference quarter but remain in nonemployment for more than eight quarters.

We further categorize workers by whether they are employed at a distressed firm in the reference quarter. We define a "distressed firm" as one that experiences a 30 percent or larger decline in employment in the year ending in the quarter subsequent to the separation.[12] This is similar to the definition of "distressed firm" used in Jacobson, LaLonde, and Sullivan (1993a)

[11] Attempts to use variation in quarter earnings to identify likely short temporary layoffs have proven unsuccessful.
[12] Because this categorization works less well for smaller firms, for all analysis where separations are broken out by the growth rate of the separating employer we restrict our analysis to firms with at least 50 employees.

(hereafter JLS).[13] However, in order to facilitate comparisons between separators and stayers from like firms, we do not include separators in the reference quarter from closed firms in our sample. Sensitivity analysis shows that retaining the separators from closed firms does not substantially change our results, partly because separators from closed firms are a small proportion of separators.[14] Although some of the "distressed separators" may have been quits or firings for cause, the overwhelming majority are likely separations that would not have occurred in the absence of the displacement event (Davis, Faberman, and Haltiwanger 2006, 2012).

III. Tracking Separations and Re-employment in Administrative Data

As discussed above, our goal here is to trace the job and earnings paths of workers following job separations. Our earlier work (Bjelland et al., 2011) on employer-to-employer flows was restricted to job changes that occurred within the quarter of job separation. We found that, on average, 30 percent of main job separations were directly to another job, and that, on average, these job changes were associated with positive earnings growth for the worker. To generalize the implications of employer-to-employer flows for labor market dynamics, here we also study transitions to new jobs that include a spell of nonemployment. As evidence from the displaced worker literature suggests, the ability to retain—as well as find—new employment is important in the adjustment from a job separation.[15]

We categorize worker flows by the duration of the spell of joblessness following a separation in the reference quarter. Since quarterly wage data do not provide exact start and end dates for jobs, the duration of joblessness must be inferred from the pattern of quarterly earnings in the administrative data. An example is illustrative; Example 1 below provides a sample of a fictional wage record for a worker John Doe.

[13] We do not include in the sample apparent employment separations that occur in the administrative data due to firm ID changes or mergers/acquisitions. We use the pattern of worker flows to identify separations and accessions due to such events and suppress the flows that result.

[14] In the quarterly data, for a closing firm the final quarter of activity has less relevance than in annual data because closings occur in stages. We also think that potential linkage issues may underlie any residual large last quarter apparent closings even with our use of worker flows to abstract from such issues. In any event, our results are robust to the inclusion of such closings.

[15] Both this paper and our earlier paper were instrumental in the development of the Census Bureau's Job-to-Job Flows statistics. This paper uses an early prototype of the Job-to-Job Flows microdata to identify moves across firms. Similar to our findings here, job-to-job moves with a spell of nonemployment between jobs have lower earnings growth in the public use data, available here: https://lehd.ces.census.gov/data/#j2j.

Example 1: UI Wage Record for John Doe

	Firm	Y1:Q1	Y1:Q2	Y1:Q3	Y1:Q4	Y2:Q1	Y2:Q2	Y2:Q3
John Doe	A	$6700	$5900	$3100				
John Doe	B			$4500	$5200			
John Doe	C					$2900		
John Doe	D							$3700

Employer-to-employer flows that occur within the same quarter are the shortest transitions to new employment from a job separation we can identify in the data. In Example 1 above, John Doe experiences such a flow from A to B in the third quarter of the first year. There may be a short nonemployment spell associated with such a flow. For example, if separations and accessions were uniformly distributed throughout the quarter, the implied average nonemployment spell is five to six weeks long. However, the average spell may, in fact, be shorter: The wage patterns during these transition quarters suggest a period of overlapping paychecks associated with these flows, with the sum of quarterly earnings across all employers higher during the quarter of transition than in surrounding quarters. This suggests relatively short or nonexistent spells of joblessness between jobs. When the accession to a new job occurs in the next adjacent quarter after the job separation, the worker is much more likely to experience a short spell of joblessness that we do not observe– about three months, on average, again assuming a uniform distribution of separations and accessions in each quarter. In the example above, John Doe experiences this type of job flow from employer B to job C in the fourth quarter of year one.[16] We categorize the remaining flows according to the number of full quarters of joblessness.[17] It is only for these workers that we can state with confidence that they experienced a spell of joblessness.

IV. Descriptive Statistics

The main findings of the paper are immediately apparent from inspection of Figure 1, which plots the average quarterly earnings for permanent separators by firm distress and duration of nonemployment. Panels (a) and (b) present the results separately for workers from distressed

[16] For job flows that occur across several quarters, we choose to identify the timing of the flow as occurring in the quarter of separation from a job.

[17] Again, it is important to note that a worker for whom we observe a full quarter of nonemployment most likely also did not work the entire quarter of his job separation or job accession. If we again assume uniform distributions of separations and accessions, the average worker experiencing a job flow with one full quarter of nonemployment observed experienced a six-month nonemployment spell.

and non-distressed employers, respectively. Each line represents the average earnings for a group of workers defined by the duration of nonemployment. We have divided durations into six categories, being those re-employed at a new job:

1. In the same quarter as separation ("within");
2. In the quarter adjacent to the quarter of separation ("adjacent");
3. After one full quarter of nonemployment ("one");
4. After two quarters of nonemployment ("two");
5. After three quarters of nonemployment ("three");
6. After four to eight quarters of nonemployment ("≥four").

The post-separation earnings dynamics look strikingly similar for separators from non-distressed firms, which is the first indication that firm health is not predictive of earnings losses for separators. In contrast, post-separation earnings losses are strongly related to the duration of the nonemployment spell, with longer periods of nonemployment being associated with greater and more persistent losses.

Tables 1 and 2 describe the workers in the sample in more detail. Table 1 presents descriptive statistics for reference quarter 2005:2. The results show that relative to stayers, permanent separators are younger and less likely to be employed at a large firm, and that distressed separators are older than non-distressed separators but are younger than stayers. The industry that contributed the largest share of distressed separators is manufacturing, with 21 percent of distressed separators coming from that sector. Table 2 presents the sample size, with 680,000 and 18,000 workers separating from non-distressed and distressed employers, respectively. The table also shows that separators from distressed employers are less likely to be recalled but no more likely than other separators to fail to find employment within eight quarters of separation. The last five columns illustrate that, conditional on a permanent separation, separators from distressed and non-distressed employers spend similar amounts of time in nonemployment. Importantly, among permanent separators, distressed separators are no more likely to experience substantial periods of nonemployment than are non-distressed separators.

V. Earnings Outcomes Following Separation

The summary statistics in Figure 1 suggest a strong relationship between nonemployment and subsequent earnings losses but a weak relationship between these losses and the health of the firm. This section formalizes these findings and explores them in more detail. We start by estimating fairly standard models of earnings losses, which demonstrate that permanent separators tend to experience large and persistent earnings losses regardless of whether they were employed at a distressed or a non-distressed firm. We then show that periods of nonemployment after separation are strongly predictive of earnings outcomes.

A. "Standard" Estimation in the JLS Tradition

As a starting point, we estimate the effect of separations using the distributed lag model that has become the standard in the literature since the publication of JLS (1993a). A standard representation of that model is:

$$(1)\ y_{it} = \propto_i + \gamma_t + X_{it}\beta + \sum_{k\geq -12} S_{it}^{k}\delta_k + u_{it}$$

where y is the quarterly earnings of worker i in quarter t, α is an individual fixed effect, γ is a quarter fixed effect, X are time-varying individual characteristics, S_{it}^{k} is an indicator equal to one if individual i separated k quarters ago as of quarter t, and u is a regression residual, which is clustered at the level of the employer in the reference quarter. As in JLS (1993a), the vector X contains the interactions between sex, age, and age squared. We estimate this equation with ordinary least squares (OLS).

In contrast to most of the literature, we estimate this specification on a sample defined by a single reference quarter, initially 2005:2. That is, we estimate the specification for a sample that defines separation or nonseparation in 2005:2. We do this in order to allow us to investigate how our main results differ over time and, in particular, at different points in the business cycle (as noted above, the four reference quarters are 1999:Q2, 2001:Q2, 2005:Q2, and 2009:Q2). Thus, for a given reference quarter, calendar time t and time-since-reference-quarter k move in lock step. To make this clear and to facilitate expanded specifications below, we rewrite equation 1 as:

$$(2)\ y_{it} = \propto_i + X_{it}\beta + \sum_{k\geq -23} A_{it}^{k}\gamma^{k} + \sum_{k\geq -12} S_{it}^{k}\delta^{k} + u_{it}$$

where A_{it}^{k} is an indicator for the reference quarter being k quarters ago as of quarter t.[18] Note that with a single reference quarter, Aγ in (2) serves the function served by the time fixed effects γ in (1), and we have been explicit about the date range of the estimation before separators are distinguished from stayers.[19]

As in the main regressions in JLS, we estimate this equation separately for the sample of distressed separators (what JLS called the "mass layoff sample") and the sample of non-distressed separators ("non-mass-layoff sample"). In each case the comparison group of stayers includes stayers from all types of firms, both distressed and non-distressed, while excluding the other type of permanent separator. In both cases, recalls are not included in the sample. The sample includes all earnings records from 24 quarters before and after the reference quarters.

Figure 2 depicts the estimates from equation (2) and plots the estimates of δ^{k} relative to the reference quarter. Panels (a) and (b) present earnings losses (relative to stayers) for distressed and non-distressed separators, respectively. Panel (a) replicates the standard finding that separators from distressed firms experience large reductions in earnings that persist for years. We find an initial drop of $5,150 in quarterly earnings, and even six years after the separation, these workers earn around $2,000 less per quarter. A comparison of the results in panels (a) and (b) reveals that the earnings patterns for separators from non-distressed firms are both qualitatively and quantitatively similar. We find no indication that earnings losses are larger for distressed separators (i.e., displaced workers, as defined by firm contraction) than for other separators.

We recognize that separations from non-distressed employers are more likely to result from decisions made by the worker, although the restriction to workers re-employed within 8 quarters may weaken this distinction.[20] Thus, the post-separation earnings patterns may represent consequences of the decisions as opposed to a causal effect of the separation. Nonetheless, we find

[18] The summation of A begins earlier than the summation for S to allow identification of individual fixed effects in estimation with a single reference quarter.

[19] While the data include observations up to 24 quarters prior to the reference quarter, we follow JLS and estimate the effect of a separation starting 12 quarters before the reference quarter. Thus, the estimated effect of the separation is relative to the earnings differences (conditional on the other covariates in the model) between separators and stayers prior to 12 quarters before the reference quarter.

[20] For example, Flaaen, Shapiro, and Sorkin (2019) find that a much larger proportion of separators from non-distressed than from distressed firms give a reason for separation when surveyed that may reflect workers' choices.

the similarity in earnings consequences between distressed and non-distressed separators to be striking and to suggest that a common explanation may be behind these patterns.

This similarity between distressed and non-distressed separators stands in contrast with JLS (1993a, 1993b), who found that non-distressed separators suffer insignificant losses in earnings after several (e.g., five or six) years. We explored a number of differences between our sample design and specification and those of JLS and found them to be unable to explain the difference in results. The possible explanations we explored (estimates not reported) include:

a. JLS included in their comparison group workers who were observed to separate and later returned to the same employer (recalls), while we omit these individuals.

b. JLS included in their sample separators from firms that closed, while we omit these individuals.

c. JLS restricted their sample to workers with at least six years of tenure, while our tenure restriction is three years.

d. In pooling the sample across dates of separation, JLS hold coefficients constant over time, and therefore across macroeconomic conditions, whereas our separate samples allow those coefficients to vary.

e. JLS's data do not allow them to follow workers who become re-employed in another state, while our data infrastructure allows us to track individuals who move to any state participating in the LEHD program.[21]

This sensitivity analysis suggests that the differences between our results for non-distressed separators and those in JLS are due to the differences in time and place of our data.[22] In particular, JLS's sample involves separations that take place in Pennsylvania in the period 1980-1986 (with pre-separation data that begin in 1974). Our sample (including results for the additional reference quarters reported below) involves separations that take place in California, North Carolina,

[21] In addition, JLS restricted their sample to workers with positive earnings in every calendar year, whereas we require positive earnings within eight quarters of separation. Von Wachter, Song, and Manchester (2009) show that the earnings losses for non-distressed separators are larger and more persistent when separators with zero annual earnings are included in the sample. JLS also appear to limit their sample of stayers to stayers at firms that experienced some separations. We have not replicated these sample restrictions, but we expect that the differences between them and our restrictions are too small to account for the large difference in estimated outcomes.

[22] JLS also did not have the benefit of the extensive data quality controls currently used by the Census Bureau in the LEHD program. The resulting measurement error could also contribute to the difference in findings.

Oregon, Washington, and Wisconsin in the period 1999-2009 (with pre-separation data that begin in 1993). A comparison to two other studies similarly using administrative data on earnings suggests that time is the key factor. Von Wachter, Song, and Manchester (2009) use national data for separations in 1980-1986, a period intentionally similar to that in JLS. Although they do not estimate the formal model on non-distressed separators, in simple averages, they, like JLS, find non-distressed separators faring better than distressed separators. In contrast, Couch and Placzek (2010, figures 1 and 2) use data from Connecticut for separations in 1999-2004, a period that overlaps ours. Through two years following separation (the period we cover), they find little difference between distressed and non-distressed separators.[23]

B. Distressed and Non-Distressed Stayers

The regression in equation (2) implicitly compares both distressed and non-distressed separators to all stayers, regardless of whether those stayers work for distressed or non-distressed firms. Figures 4 and 5 of JLS (1993a) and section 6.1 of JLS (1993b) instead distinguish stayers by type of firm. As noted in JLS (1993b, p.163), this can be interpreted as estimating the effects of separation itself as opposed to the effects of the firm-side conditions that contributed to the separation.

In a further step toward our main specification, we, too, distinguish between stayers at distressed and non-distressed firms. Specifically, we estimate the following equation:

$$(3)\ y_{it} = \propto_i + X_{it}\beta + \sum_{d=0}^{1}\sum_{k\geq -23} A_{it}^{k,d}\theta^{k,d} + \sum_{d=0}^{1}\sum_{k\geq -12} S_{it}^{k,d}\,\delta^{k,d} + u_{it}$$

where d=1 for distressed firms and d=0 for non-distressed firms. $A_{it}^{k,0}$ ($A_{it}^{k,1}$) is an indicator equal to one if the reference quarter is k quarters after t and the individual is employed at a non-distressed (distressed) employer in the reference quarter and $S_{it}^{k,0}$ ($S_{it}^{k,1}$) is an indicator equal to one if $A_{it}^{k,0}$ ($A_{it}^{k,1}$) is equal to one and the individual i is a separator. In contrast to our previous specification, we estimate this equation on a pooled sample that contains both separators and stayers from distressed and non-distressed employers. As before, recalls are not included in the sample, and the sample includes all earnings records from 24 quarters before and after the reference quarter.

[23] They find more of a recovery in years three to six following separation for non-distressed than for distressed separators, although even at the end of six years, average losses for non-distressed separators remain substantial.

The estimated earnings losses for the 2005:2 sample are depicted in Figure 3. The results lead to a similar conclusion: There do not appear to be large differences in the earnings consequences of separations from distressed versus non-distressed firms. If anything, we find that separators from distressed firms experience smaller losses. Compared to the estimates from equation 2, the long-term earnings losses are slightly smaller for separators from distressed firms. This is likely because stayers at distressed firms tend to experience slower earnings growth. Results for non-distressed separators are similar to the estimates from equation (2), which is to be expected given that workers at distressed employers make up a relatively small share of total stayers.

C. Earnings Consequences and Nonemployment Spells

A main contribution of our study is to investigate the role of nonemployment in explaining the post-separation earnings patterns. To do this, we expand upon equation (3) and estimate the following equation:

$$(4)\ y_{it} = \propto_i + X_{it}\beta + \sum_{d=0}^{1}\sum_{k\geq -23} A_{it}^{k,d}\theta^{k,d} + \sum_{d=0}^{1}\sum_{0>k\geq -12} S_{it}^{k,d}\delta^{k,d} + \sum_{N=0}^{5}\sum_{d=0}^{1}\sum_{k\geq \max\{N-1,0\}} S_{it}^{k,d,N}\delta^{k,d,N} + u_{it}$$

where $S_{it}^{k,d,N}$ is an indicator equal to one if $A_{it}^{k,d}$ is equal to one and i is a separator that had a duration of nonemployment equal to N, and where N is defined (as in section IV) as re-employed at a new job:

N=0: In the same quarter as separation ("within");

N=1: In the quarter adjacent to the quarter of separation ("adjacent");

N=2: After one full quarter of nonemployment ("one");

N=3: After two quarters of nonemployment ("two");

N=4: After three quarters of nonemployment ("three");

N=5: After four to eight quarters of nonemployment ("≥four").

As in equation (3), we estimate this equation on a sample that includes all permanent separators and all stayers (we still exclude recalls from the sample). We do not allow for the pre-separation

effects to differ by the subsequent duration of nonemployment.[24] The sample includes earnings records from 24 quarters before and after the reference quarters. However, we drop observations for separators between the quarter of separation and the quarter of re-employment, since these earnings are zero by construction.

The estimates from equation 4 are presented in Figure 4, where panels (a) and (b) present results for distressed ($\delta^{k,1,N}$) and non-distressed ($\delta^{k,0,N}$) separators, respectively. A very clear pattern emerges from the figure: The duration of time spent in nonemployment prior to finding a new job is strongly related to the magnitude and persistence of earnings losses, while these losses are not strongly related to the health of the firm (distressed versus non-distressed).[25] We find small earnings losses for individuals who find re-employment within the quarter of separation, with an average quarterly loss six years after separation of only $298 and $537 for separators from distressed and non-distressed firms, respectively. For individuals who find re-employment in the adjacent quarter, these numbers are $1,120 and $1,600, respectively. For individuals who experience four or more quarters of nonemployment, the reduction in quarterly earnings exceeds $3,000. The figure clearly illustrates the monotonic relationship in which longer periods of nonemployment are associated with larger, more persistent earnings losses.

The standard division of firms into only two categories, distressed and non-distressed, was intended to isolate separations that are likely to be exogenous from the worker's perspective. For our purposes, however, it is possible that the coarseness of that division could conceal differences within the non-distressed category that would indicate that firm growth is, in fact, related to earnings losses. To test this proposition, we also estimated variants of equations (2), (3) and (4) that use finer categories of firms' employment growth (see section F for details). Our conclusions are unaffected: With the finer categories as well, earnings losses and nonemployment durations among permanent separators are similar across firm growth categories and vary greatly by

[24]As above, pre-separation effects can differ between separators and stayers. However, the duration of nonemployment conditional on separating is a function of the worker, not the firm of separation, so the same logic does not apply.

[25] In interpreting our results, it is important to highlight that our findings are about spells of nonemployment, not only unemployment. Using the matched monthly CPS data for the years 2004 and 2006 (bracketing one of our reference quarters in 2005), we find, as others have, that the rate of workers moving from employment to out of the labor force at high frequencies is quite high even for sub-groups with seemingly strong labor force attachment. In particular, even among employed men ages 35-44, a highly attached group, about 15 percent of separators each month leave the labor force. Of these, 38 percent said that they want a job. This suggests that movements out of the labor force may be an important element of the nonemployment we observe even among prime-age males.

nonemployment duration. Because this specification greatly increases the number of parameters, we do not show these results, but we return to using these finer growth categories in later analysis.

D. Quantifying the Importance of Nonemployment

As noted above, the results presented in Figures 3 and 4 indicate that the duration of time spent in nonemployment is predictive of post-separation earnings outcomes, whereas the health of the employer is not. In order to quantify this statement in more precise terms, we estimate two restricted versions of equation 4 and compare their explanatory power to that of the unrestricted model. In the most restrictive model, we do not allow for differential effects of separations by either employer type or duration of nonemployment. Formally, we require that $\delta^{k,d,N} = \delta^{k}$. In the intermediate model, we allow the effect of separation to differ by the health of the firm but not by duration of nonemployment. Formally, we require that $\delta^{k,d,N} = \delta^{k,d}$. To quantify the explanatory power of each model, we implement the fixed effects estimation using a within estimator, which allows us to interpret the resulting R-squared as the proportion of within individual variation explained by the model.[26] Both restricted models are estimated on the same sample described for the estimation of equation 4.

The results indicate that the most restrictive model, in which the effects of separations do not vary by employer type or nonemployment duration, explains 3.7 percent of the within individual variation in earnings (that is, the R-square is 0.037). As expected, we find that allowing the effect of separation to vary by employer type, but not by nonemployment duration, adds virtually no explanatory power to the model, increasing the R-squared by only 0.005 percent. In contrast, the unrestricted version (equation 4), which allows the effects of separation to vary by nonemployment duration, explains about 6.9 percent more of the within individual variation than the most restrictive model.[27] While the overall increase in explanatory power may be considered modest, clearly the differential effects of separation by nonemployment duration are far more important than the differential effects by firm health.

E. Earnings Changes Throughout the Distribution

[26] To implement the within estimator we de-mean all variables in the model by individual level means (averages are calculated across time and within individuals) and run OLS on transformed variables.
[27] Note that both increases (0.005 and 6.9 percent) are percentage increases (not percentage point), calculated off of the R-squared of the most restrictive model, which is 0.037.

In addition to the JLS-type equations estimated above, another method used to estimate earnings losses in the displaced worker literature is to estimate directly the change in log earnings before and after the job separation. In this section, we estimate the change from four quarters before the reference quarter (e.g., 2004:Q2 for reference quarter 2005:Q2) to the first full quarter of earnings after re-employment. We estimate this separately for each duration of nonemployment N (as defined above).[28] In addition to providing an alternative view, this method will facilitate our examination below of the distribution of earnings changes.

For each reference quarter and each subsample of permanent separators based on the duration of nonemployment, we estimate

$$(5)\ \Delta y_i = \alpha + \beta X_i + \gamma Z_{j(i)} + \sum_{g=1}^{4} A_i^g \lambda^g + \sum_{g=1}^{4} S_i^g \delta^g$$

where Δy_{it} is the change in log real earnings; X_i is a vector of worker characteristics that include age, sex, and tenure as of the reference quarter; $Z_{j(i)}$ is a vector of characteristics of the firm of employment as of the reference quarter that include size, state, and the growth rate of the industry within the state; A_i^g is an indicator equal to one if person i worked at a firm in growth category g; and S_i^g is a dummy variable equal to 1 if A_i^g equals one and the worker separated in the reference quarter. Standard errors are clustered at the level of the employer in the reference quarter.

Because we estimate the earnings change for a single interval instead of across the range of quarters following separation, the number of parameters is greatly reduced relative to equation (4). We take advantage of this to expand the number of categories of firm growth, in keeping with the discussion at the end of section C. The categories are:

g=1: distressed or fast-shrinking: -100% < change in employment < -30%;

g=2: slow-shrinking: -30% ≤ change in employment < 0;

g=3: slow-growing: 0 ≤ change in employment < +30%;

g=4: fast-growing: change in employment ≥ +30%.

[28] While we use the first quarter of re-employment to define the length of the nonemployment spell, we use the first full quarter of earnings in that job to define the change in earnings.

This first difference specification implies that we are abstracting from fixed unobserved heterogeneity that affects the level of earnings. The vectors X and Z control for differences in earnings trajectories along the dimensions that we can measure in our data. In each case we restrict the sample to individuals who had changes in log earnings between -1.2 and 0.8, to eliminate outliers. Furthermore, to ease the computational burden, we select a subsample of stayers who are observably similar to the separators using propensity score matching. In the interests of space, we show the results of these regressions for only the 2005:Q2 reference quarter.

Panel (a) of Figure 5 plots the predicted earnings changes for separators from each growth category of firm relative to stayers, evaluated at the means of the other covariates. Consistent with the previous results, the average earnings losses are similar between distressed and non-distressed separators, while the losses vary substantially by duration of nonemployment. Average losses for separators who become re-employed within the same quarter are fairly small and rise dramatically for those with a substantial spell of nonemployment. This is true for the other reference years as well (not shown).

Previous work has documented a large dispersion in earnings outcomes for displaced workers. Therefore, we estimate quantile regressions of the same form as equation 5 for the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} quantiles to investigate whether the key patterns hold up throughout the distribution of earnings changes. The predicted earnings changes at each quantile for the 2005 sample (again, evaluated at the means of the other covariates) are shown in Panels (b) through (f) of Figure 5. At the 10^{th}, 25^{th}, 50^{th}, and 75^{th} percentiles, there is no clear pattern of distressed separators faring worse than other separators, while earnings changes fall markedly as the observed nonemployment duration increases. At the 90^{th} percentile we continue to find no role for the firm's growth rate in predicting earnings changes. But while longer periods of nonemployment are generally associated with worse outcomes, the relationship is weaker and noisier relative to the other results.

VI. Nonemployment Following Job Separation

Our estimates so far indicate that permanent separators from distressed firms suffer no larger earnings losses, on average, than do permanent separators from other firms, both overall and conditional on the length of nonemployment between separation and accession to a new job. While these results would seem to imply that distressed separators are no more likely to experience a

substantial period of nonemployment following a separation, this section directly examines this relationship. In addition, we explore the role of recalls, which up until this point have been excluded from our analysis.

We estimate a competing-risks hazard model in which the two risks are becoming re-employed at a new employer and becoming re-employed at the same employer from which one separated (recall). We assume that recalls dominate new jobs, in the sense that a worker recalled in a particular quarter is not in the risk set for taking a new job in that quarter, while a worker taking a new job in a particular quarter is in the risk set for being recalled in that quarter. We use the same categories of nonemployment duration as we have throughout, and the finer categories of firm employment growth as in section V. We model the probability of becoming re-employed at a new job at each duration of nonemployment, conditional on not already being re-employed, as

$$(6)\ logit(new\ job\ in\ t \mid not\ reemployed\ before\ t\ and\ not\ recalled\ in\ t)_i = \alpha_t + \beta_t X_i + \gamma_t Z_{j(i)} + \lambda_t g_{j(i)} + \mu_{it}$$

and the probability of recall analogously as

$$(7)\ logit(recalled\ in\ t \mid not\ reemployed\ before\ t)_i = \alpha'_t + \beta'_t X_i + \gamma'_t Z_{j(i)} + \lambda_t g_{j(i)} + \mu'_{it}$$

As above, X_i is a vector of worker characteristics that include age, sex, and tenure at the separating firm and Z_i is a vector of characteristics of the separating firm, namely, size, state, growth rate in the year prior to separation, and the growth rate of the industry within the state.

From these two models we then obtain predicted probabilities for each of the four growth rate categories evaluated at the mean of all other covariates. We use these predicted probabilities to construct the cumulative distribution function (CDF) of time until re-employment, that is, the probability of exiting nonemployment by a given quarter after separation (the complement of the survivor function implied by equation (6)). The results are displayed in Figure 6. Panel (a) displays the CDF for new jobs (conditional on no recall) and illustrates that the duration of time spent in nonemployment is unrelated to the growth of the firm from which the worker separated. Panel (b) shows analogous results for recalls. Here we see markedly different patterns for distressed

separators and other separators. As would be expected, individuals who separate from fast-shrinking firms are far less likely to be recalled.

It is somewhat surprising that distressed separators experience similar durations of nonemployment conditional on never being recalled. If individuals choose to separate from their employer, we might think that many would have another job already lined up and thus be less likely to experience nonemployment. However, there are several possible reasons for why distressed separators might find employment at a new job at as fast a rate as non-distressed separators. One is that workers from distressed firms may anticipate the separation and begin searching for another job ahead of time.[29] Another is that the workers separating from distressed firms know that recall to their former employer is unlikely, and so they search more intensely for new jobs rather than wait to be recalled. This explanation would be broadly consistent with Katz and Meyer (1990) and Fallick and Ryu (2007). A third possibility is that the patterns in Figure 6 may reflect a smaller "lemon's effect" for separators from distressed firms: Potential employers might have greater confidence in the quality of the pool of workers separating from a distressed firm, increasing the rate and quality of job offers relative to those of non-distressed separators.[30]

To close this section, it is useful to discuss our findings in light of the empirical literature regarding distressed separators, layoffs, and unemployment. Two key findings from that literature are important in this context. First, the proportion of separations that are job losses–layoffs rather than quits–increases sharply with the rate at which a firm contracts (Davis, Faberman, and Haltiwanger 2006, 2012). Second, job losers are more likely to become unemployed and to experience more unemployment than job leavers (see, for example, Elsby, Hobijn, and Sahin 2010). These two observations imply that separators from distressed firms should experience more unemployment, both in incidence and duration, than do separators from non-distressed firms.

Our findings are not inconsistent with these observations, for two main reasons. First, our findings about distressed separators being more likely to exit nonemployment by moving to a new

[29] The Worker Adjustment and Retraining Notification (WARN) Act requires most employers in the US with more than 100 employees to give 60-day advance notice of a plant closure or mass layoff. Research into the impact of advance notice on post-displacement earnings and employment has generally found that notice reduces the number of displaced individuals who experienced a jobless spell during the event (e.g., Addison and Blackburn, 1997).

[30] For recent discussions, see Carrington and Fallick (2017, p.697); and Flaaen, Shapiro, and Sorkin (2019, p.212). Recent work by Abraham et al. (2019) suggests that unobserved heterogeneity does not account for observed duration dependence, but it still might be important in distinguishing between separators.

firm are conditional on not being recalled. Not only are non-distressed separators more likely to be recalled conditional on at least one quarter of nonemployment, but, as discussed above, in our data we cannot identify separations that end in recall within the quarter of separation or in the adjacent quarter. Thus, recalls associated with short durations of nonemployment are not captured in our sample. Such recalls after short durations of nonemployment are likely higher at growing firms.[31] Second, as noted previously, we measure nonemployment rather than unemployment, and periods of labor force withdrawal appear to be common even among demographic groups with high average attachment to the labor market.

VII. Why the Relationship between Earnings Losses and Nonemployment?

Why is the duration of time spent in nonemployment so strongly related to post-separation earnings losses? A number of possible economic explanations have important—and potentially conflicting—implications for how we understand the experiences of displaced workers and of the more general process through which earnings are determined. One may divide explanations into three rough and potentially overlapping classes. First, differences in time spent in nonemployment may reflect heterogeneity across workers that is correlated with earnings, and in particular heterogeneity in degree of labor market attachment. Second, spending an extended period of time in nonemployment might produce earnings losses. This could happen because of a depreciation of human capital (or lack of human capital gained), because spending time in nonemployment sends a bad signal to potential employers, or because separation with nonemployment causes a worker to fall to a lower rung of the job ladder. Third, periods of nonemployment prior to finding a new job may be a symptom of other factors that lead to earnings losses. For example, workers whose skills are becoming obsolete for technological reasons or whose local economies have suffered decline might have a harder time finding a new job and might have to settle for lower wages upon re-employment. In this section we discuss several possible explanations about which we can offer some evidence.

A. Worker Heterogeneity

[31] Another possibility is that the finding in the literature that job losers experience more unemployment is an artifact of respondents to household surveys like the Current Population Survey failing to report short or casual jobs (Abraham, et al., 2013). We investigated this possibility by deleting new jobs with particularly low quarterly earnings or those that lasted only one quarter. We also tried deleting jobs with temporary help firms or professional employer organizations (NAICS 5623), and adding separations that occurred in the other three quarters of the year (in case one quarter is prone to more short seasonal jobs than another). None of these alterations significantly changed our results.

As in previous research in the JLS tradition, heterogeneity across workers in our sample in their degree of labor force attachment is limited by the restriction that every separated worker in our sample is observed to be re-employed within 8 quarters of separation. However, it is possible that significant heterogeneity along this dimension remains. Less strongly attached workers may be both slower to become re-employed and more likely to choose to accept jobs with reduced hours or lower wages.

Arguing against this possibility is the quantitative as well as qualitative similarity in post-separation labor market outcomes between separators from distressed and non-distressed firms in the results already presented, both conditional and not conditional on the duration of time spent in nonemployment, combined with the finding that firm distress is not predictive of the amount of time separators spend in nonemployment (absent recall). If heterogeneity in labor market attachment were a major factor, we would not expect to find these similarities, since separations from non-distressed employers are relatively more likely to result from decisions made by the worker.[32]

In addition, we find similar patterns in reference periods that span a range of macroeconomic conditions. In particular, in addition to the reference quarter 2005:2 which we feature, we estimate equation (4) on three additional samples defined by the reference quarters 1999:2 (a tight labor market), 2001:2 (a relatively mild recession) and 2009:2 (a severe recession). The results are presented in Figure 7 and illustrate the robustness of our main results across these various macroeconomic conditions, even comparing a boom year like 1999 to the Global Financial Crisis year of 2009. Because one would expect the mix of labor force attachment among permanent separators to vary with the cyclical state of the labor market, the similarity in results across the reference years also argues against worker heterogeneity in attachment as an explanation for our results.

To further examine the possibility that heterogeneity plays a role in our results, we re-estimate equation (4) within a number of subgroups of workers that are likely to be more homogeneous in labor market attachment than is the full sample. Specifically, we re-estimate equation (4) on the following subsamples:

[32] It is important in this connection that we measure *non*employment, not *un*employment.

a. Workers re-employed within four quarters of separation;
b. Workers with at least five years of tenure before separation;
c. Omitting jobs with particularly low quarterly earnings (average annual earnings in three years prior to reference quarter do not exceed $10,000);
d. Omitting jobs in the temporary help and related industries (NAICS 5623).
e. Men ages 35-44;
f. Women ages 25-34;[33]
g. Women who gave birth during the 2005 reference quarter or the adjacent quarters;[34]

These results are summarized in Table 3 which presents the average values of $\delta^{k,d,N}$, for k=[max{N-1,0},max{N-1,0}+19] which represent the average earnings losses for each group in the 20 quarters after re-entry into the labor market. Our main result is robust within every group: Earnings of separators are similar across the distressed and non-distressed firms, while nonemployment duration is a key factor associated with earnings losses.

The analyses in Table 3 and Figure 7 focus on earnings, but our main results related to the duration of nonemployment are similarly robust. In unreported results, we estimate the duration specifications presented in Figures 6 within each of the samples defined by the other reference periods and the characteristics of the workers. Within each sample, we find that distressed and non-distressed separators have similar durations to re-employment at a new job, but distressed separators are much less likely to be recalled.

Finally, we follow JLS in adding linear individual-specific time trends to our main specifications. The results are presented in Figure 8.[35] Relative to our main results, (i) the earnings losses of distressed separators are slightly smaller and (ii) the strength of the association between duration of nonemployment and earnings losses is somewhat weaker. However, qualitatively the relationship between duration of nonemployment and earnings losses is robust to the inclusion of the individual-level trend, as we continue to find that average earnings losses are monotonically increasing in the duration of nonemployment. These results suggest that workers who spend more

[33] That is, women in the post-schooling age groups with the highest fertility rates.
[34] We use the ages of own-children in the 2010 decennial census to identify these women. To increase power, we retain all stayers in the sample, but limit the sample of separators to new mothers.
[35] We omit confidence intervals from the figure since computational constraints prevent us from clustering standard errors.

time in nonemployment were not simply on flatter earnings trajectories prior to separating. It is worth noting that individual-specific time trend is partially identified from post-separation earnings observations, and thus this represents a fairly extreme way of controlling for heterogeneity in earnings trajectories.

Taken together, the similarity in the pattern of outcomes between distressed and non-distressed separators, across different macroeconomic conditions, across groups with likely more homogeneous labor market attachment, and with the inclusion of individual-level time trends lead us to conclude that is unlikely that heterogeneity in labor force attachment accounts for our results. The similarity across distressed and non-distressed firms and across macroeconomic conditions also argues against a "lemons" explanation, in which prospective employers infer worker quality from the length of nonemployment.

B. Depreciation of Human Capital

Many models of earnings suggest that human capital depreciates in an absolute sense from nonuse during periods of nonemployment. While depreciation of human capital would predict larger earnings losses for workers who spend more time in nonemployment, two features of our results argue against this explanation. The first is the steep increase in earnings losses between groups of workers that experience a within-quarter versus an adjacent-quarter transition, and again between groups that experience an adjacent-quarter transition and one-quarter of nonemployment. It seems unlikely that human capital would depreciate so quickly with lack of use. The second is the persistence of the losses 16 or more quarters following re-employment (recalling that everyone in our sample was re-employment within 8 quarters of separation). It would seem that human capital that depreciated within a quarter of separation would be regained long before that point. Thus, it seems unlikely that depreciation in human capital explains our results, although we do caution that we do not have direct evidence that would rule out this mechanism.

C. Job Ladders

A large body of empirical evidence suggests that a substantial component of earnings inequality is attributable to dispersion in firm-level pay policies. An important piece of this evidence is based on the empirical model developed by Abowd et al. (1999), hereafter AKM, in which log earnings are regressed against worked and firm fixed effects. Under a set of plausible but controversial assumptions, the estimated firm fixed effects can be interpreted as firm-level pay

premia. Motivated by this literature, we estimate an AKM model using the LEHD data and use the earnings change specification described in equation (5) to estimate the relationship between firm distress, duration of nonemployment, and job mobility to firms with higher (or lower) firm-fixed effects.[36] Specifically, we estimate a version of equation (5) where we replace the change in earnings on the left-hand side with the difference between firm fixed effect of the destination and origin firm. Given the nature of this outcome variable, these regressions include only separators.

The results presented in panel (a) of Figure 9 indicate that transitions to firms with a lower firm fixed effect are strongly related to the duration of time spent in nonemployment but unrelated to the health of the firm. In other words, the qualitative patterns in changes in individual-level earnings are mirrored in changes in the firm fixed effect. Moreover, a comparison of panel (a) of Figure 5 and panel (a) of Figure 9 reveals that the point estimates are of the same sign and order of magnitude. On average, workers who make a within-quarter transition move to firms with a fixed effect that is approximately 2 log points higher, whereas workers who spend four or more quarters in nonemployment move to firms with fixed effects that are at least 9 log points lower. These estimates are roughly half the size of the estimated effects on the change in individual log earnings. Thus, these estimates suggest that workers who spend significant time in nonemployment may suffer earnings losses, in large part, because they move to lower-paying firms upon re-employment.

A wide class of models explain how imperfect competition in the labor market can produce job ladders. Differences in pay across firms arise out of models with market imperfections attributable to search frictions (e.g., Postel Vinay and Robin 2002) or heterogeneous preferences across firms (e.g., Card et al. 2018) but a common prediction of these models is that firms that are higher up on the job ladder tend to pay their workers more and are more productive. Motivated by this theoretical work, we rank firms based on their average earnings and productivity and estimate a version of equation (5) in which the outcome variable is the difference between the percentile rank of the destination and origin firm. The estimates in panels (b) and (c) of Figure 9 indicate the duration of time spent in nonemployment is also strongly related to movements down the job

[36] The firm fixed effects are estimated on an annual sample that contain average quarterly earnings of the main employer between 2002 and 2009. In addition to individual and firm fixed effects, the empirical model also controls for year fixed effects and the interaction between education, sex, and a third-order polynomial in age. Following Card et al. (2018) age is normalized to 40.

ladder defined by average earnings and productivity, respectively.[37] The patterns for the productivity ladder are more noisy, which may be due to the fact that our measure of productivity is only a valid measure of productivity within industries.

Jarosch (2015), Krolikowski (2017), and Jung and Kuhn (2019) develop models in which job ladders play an important role in generating persistent earnings losses following job losses and which offer a partial, but incomplete, explanation of our results. In these models, firms that occupy higher rungs of the job ladder offer jobs that pay more and are more stable (i.e., the jobs have a lower probability of exogenously ending with an involuntary separation). An involuntary separation is costly because the subsequent job is likely to be lower on the ladder (since it takes time to find a high-quality job), yielding lower pay immediately, and also persistently by increasing the frequency of subsequent separations. In Jarosch (2015) and Jung and Kuhn (2019), the periods of nonemployment also reduce human capital relative to employed workers, which further persistently reduces relative earnings. Thus, these models are consistent with our findings in the sense that they predict that earnings losses will be associated with movements down the job ladder.

However, none of the existing models offer a clear explanation for why the duration of nonemployment is strongly related to earnings losses and movements down the job ladder. Are these patterns driven by the choices of the firm or the workers? Are firms that occupy higher rungs of the job ladder more likely to search for workers that are currently employed at other firms? Alternatively, does the reservation wage of a worker decline with time spent in nonemployment? Given the strength of this relationship, a priority of future work should be to extend models of job ladders to explain why time spent in nonemployment is related to earnings losses and movements down the job ladder.

Figure 10 presents estimates based on whether the original employer had a high or low firm fixed effect (defined by above or below the sample average). Within each of these samples, we continue to see that longer periods of nonemployment are associated with moves further down the ladder. However, we also observe some mean reversion, as individual who initially start on a low

[37] The firm-level measures of average earnings and productivity are measured between 2002 and 2009. Productivity data are measured as the log revenue per worker deviated from the four-digit industry average (see Haltiwanger et al. 2017). Percentile ranks are calculated across the national distribution and are employment weighted.

rung of the job ladder tend to move to higher paying firms, while those that start on a high rung of the job ladder tend to move to lower paying firms.

D. Local Labor Demand

Lastly, workers who separate into labor markets with little demand for their skills may struggle to find a new job quickly and may command lower earnings, generating the observed correlation between nonemployment and earnings losses. The variation across workers in our sample thus may reflect declines in demand for labor in local areas or in some workers' industries or occupations (and thus declines in the value of those workers' industry- or occupation-specific human capital). To explore this possibility, we measure the strength of the relevant local labor market by the growth rate of employment in each 3-digit NAICS industry in each state, and the growth rate of employment in each 3-digit Census occupation in each state, and divide them into weak and strong categories.[38] We then estimate equation (4) as well as the duration results from equations (6) and (7) separately by the strength of the local labor market defined by industry and occupation classifications.

Table 4 presents the estimates of the earnings consequences and shows that within all subsamples, we continue to find a strong associated between earnings losses and duration of time spent in nonemployment.[39] As before, we also see little difference between the earnings losses by firm distress. Figures 11 and 12 present the duration estimates for the subsamples defined by industry and occupation growth, respectively. For the results by industry, we find some evidence that individuals who separate from rapidly shrinking firms in shrinking industries take longer to find a new job. In addition, within both of the subsamples defined by occupation, workers who separate from rapidly shrinking firms take slightly longer to find a job. However, these differences are relatively modest. More importantly, there do not appear to be large differences in the duration of time spent in nonemployment across the different samples that characterize the strength of the

[38] Industry/state employment growth is measured within the LEHD and assigned to each worker by her industry in 2005:2. We categorize observations into three groups: weak, average, and strong. Occupation/state employment growth is measured between 2001 and 2002 using data from the BLS' Occupational Employment Studies program and is assigned to each worker using his occupation in the 2000 Decennial Census. Thus, for the results by occupation, we use the sample based on the 2001 reference quarter. Given that the sample for which we can measure occupation is much smaller, we defined only weak and strong labor markets based on above and below the median employment growth rate for the occupation class within the state.

[39] As in Table 3, we Table 4 presents the average values of $\delta^{k,d,N}$, for k=[max{N-1,0},max{N-1,0}+19] which represent the average earnings losses for each group in the 20 quarters after re-entry into the labor market.

local labor market (as opposed to firm growth). This latter result casts some doubt on the possibility that that association between earnings losses and nonemployment is driven by local labor market demand. While state may be too crude a measure of geography and the industry and occupation measures may be too broad to fully capture changes in "local" labor demand, these results suggest that declining local demand is not the primary explanation of our main findings.

VIII. Conclusion

A large literature has studied the consequences of job displacement, or separation from a distressed firm. Using a methodology developed for tracking employer-to-employer transitions (both direct transitions and transitions involving spells of nonemployment), we investigate the consequences of separations from both distressed and non-distressed firms for both earnings and nonemployment outcomes for workers who are not recalled to their previous employers.

We find that, on average, separators from distressed firms and separators from non-distressed firms experience similar earnings losses. Instead, the major distinction is that separators of either type who make rapid transitions between employers tend to experience little if any loss; rather, substantial losses in earnings are concentrated among separators who spend a substantial period in nonemployment. Relatedly, we find that separators from distressed firms experience no more nonemployment that do separators from non-distressed firms if they are not recalled.

We examine several possible economic explanations for these results. While an exhaustive investigation of possible mechanisms is beyond the scope of this paper, we present several analyses that we believe weigh in favor of models as job ladders as the most likely explanation. Developing a more complete explanation of the relationship between time spent in nonemployment and earnings losses is an important task for future research.

It is worth emphasizing that we do not argue that the focus on distressed separators is entirely misplaced. This group of workers is of interest for at least two reasons. First, they may be more likely to experience the separation as an unanticipated shock, and thus while the earnings consequences are similar to those of other separators, the welfare consequences may be different. Second, separations are more likely to be exogenous, which makes for a more straightforward interpretation of the empirical results. However, we do argue that future attempts to uncover the mechanisms driving earnings dynamics surrounding job separations should focus on the role of nonemployment as opposed to something particularly about separations from distressed firms.

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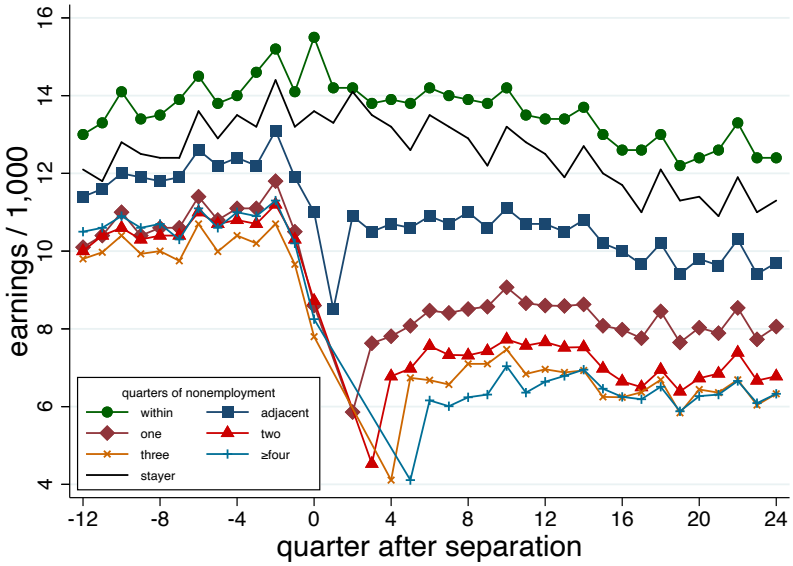
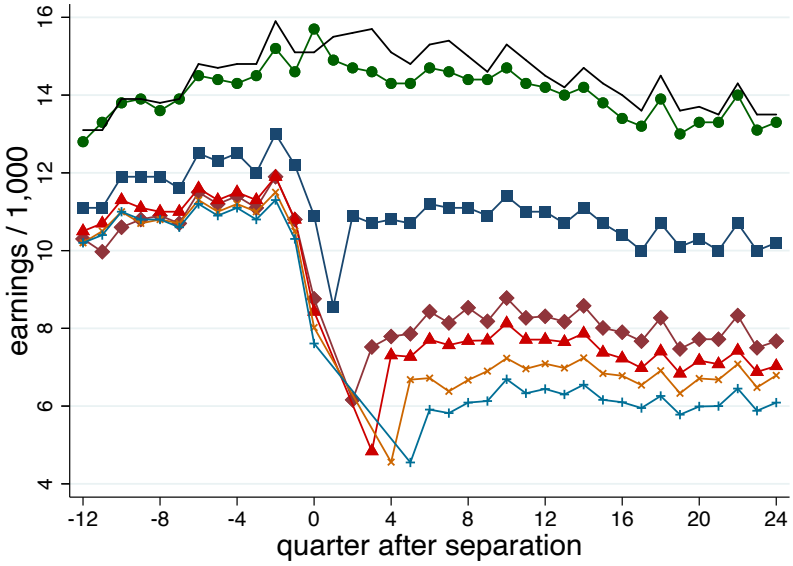
IX. Figures

Figure 1: Average Earnings

(a) Distressed



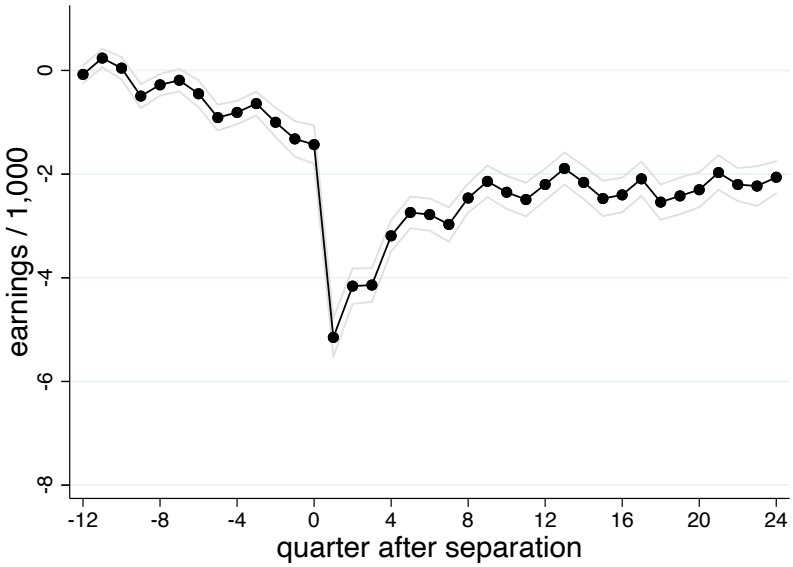
(b) Non-Distressed



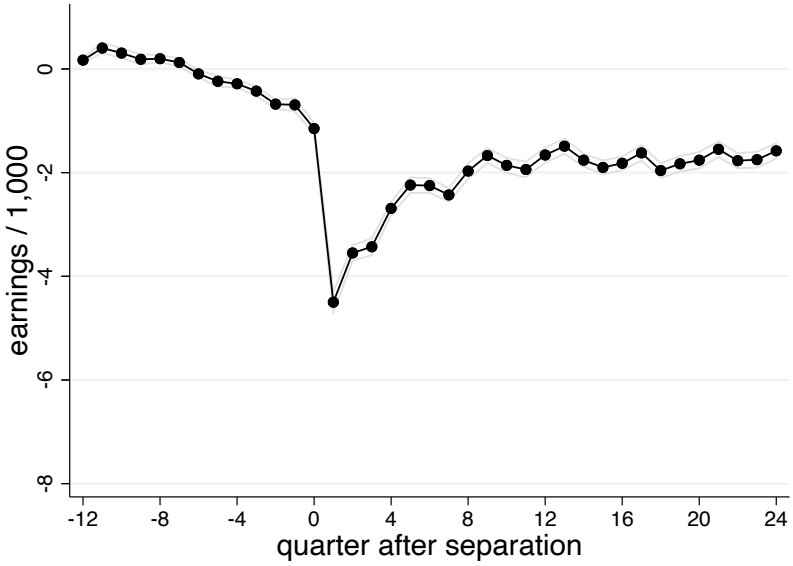
Note: The figure presents the average earnings of workers in the three years before and six years after 2005:2. Panels (a) and (b) present results for distressed and non-distressed employers, respectively. The lines within each panel present averages for different groups of workers, including stayers and separators who spend various amounts of time in nonemployment.

Figure 2: Effect of Separation Relative to all Stayers

(a) Distressed



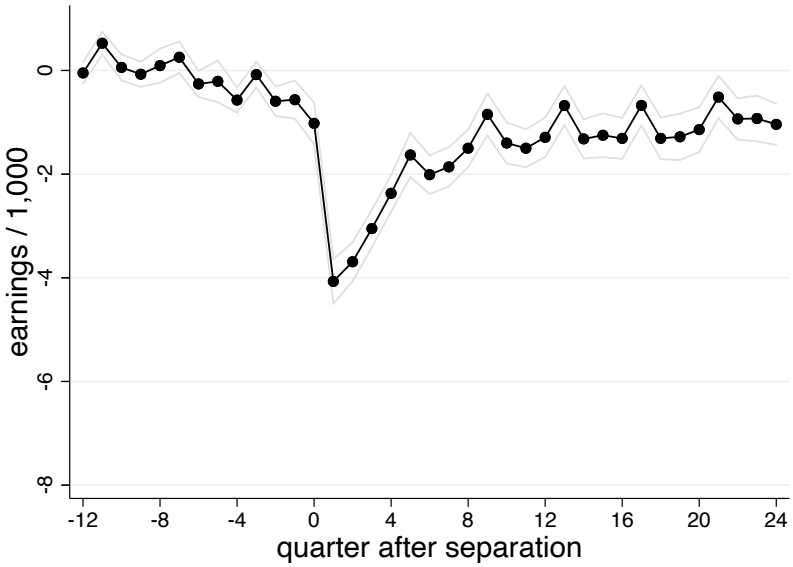
(b) Non-Distressed



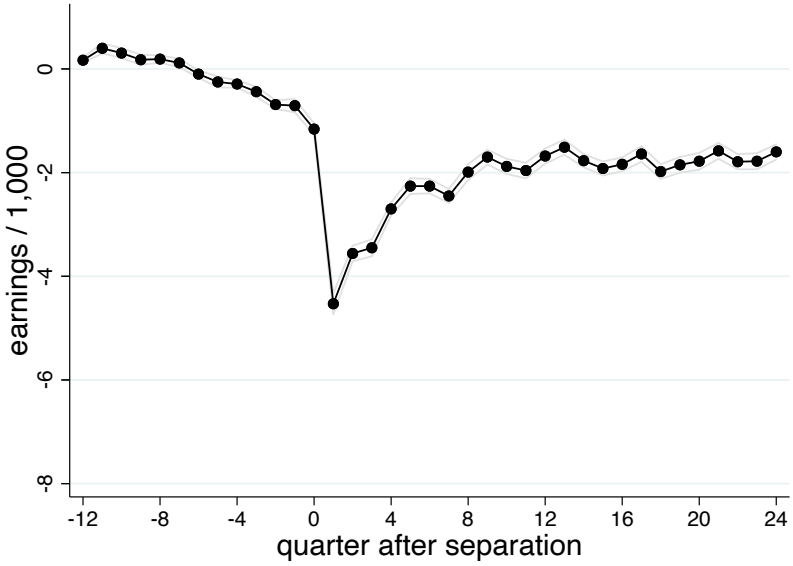
Note: The figure presents the estimated earnings consequences of separating from a non-distressed and distressed firm in 2005:2. Results presented in panels (a) and (b) are derived from a sample in which separators are from distressed and non-distressed firms, respectively. Stayers from all employers are included in both samples and neither sample includes recalls. The figure displays estimates obtained from the empirical model described in equation 2. Specifically, the estimates of δ^k are plotted against the quarter relative to displacement. Standard errors are clustered at the level of the employer in the reference quarter and the solid gray lines depict the 95 percent confidence interval around the estimates.

Figure 3: Effect of Separation Relative to Stayers in Same Firm Category

(a) Distressed



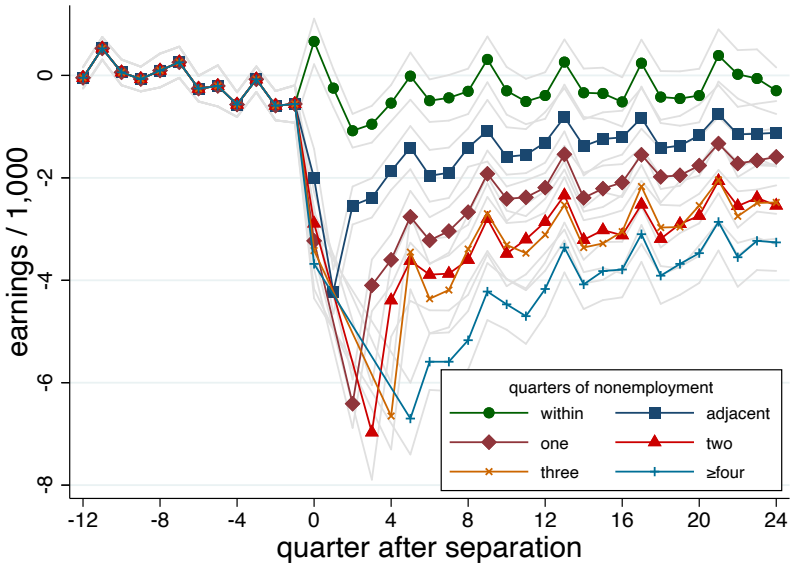
(b) Non-Distressed



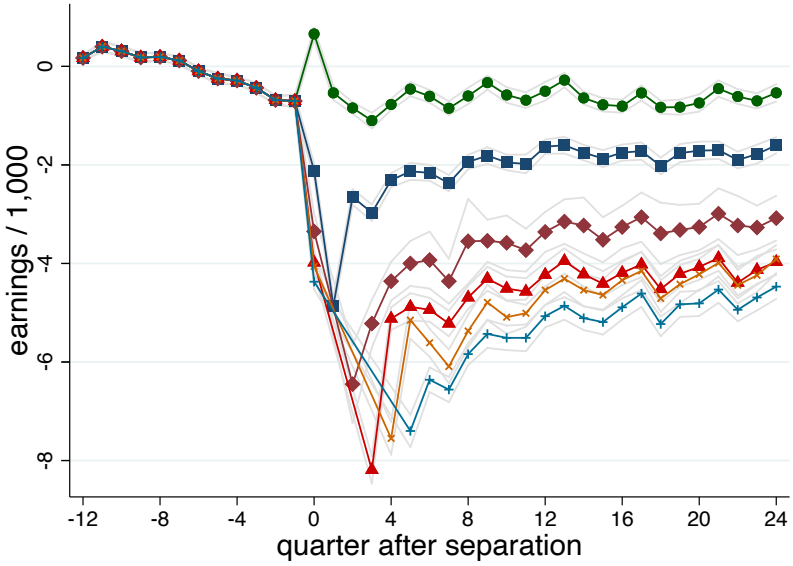
Note: The figure presents the estimated earnings consequences of separating from a distressed and non-distressed firm in 2005:2. The results are derived from a sample that excludes recalls but includes all other stayers and separators. The figure displays estimates obtained from equation 3. Specifically, panels (a) and (b) plot the estimates of $\delta^{k,1}$ and $\delta^{k,0}$ against the quarter relative to displacement, respectively. Standard errors are clustered at the level of the employer in the reference quarter and the solid gray lines depict the 95 percent confidence interval around the estimates.

Figure 4: Effect of Separation by Duration of Nonemployment

(a) Distressed

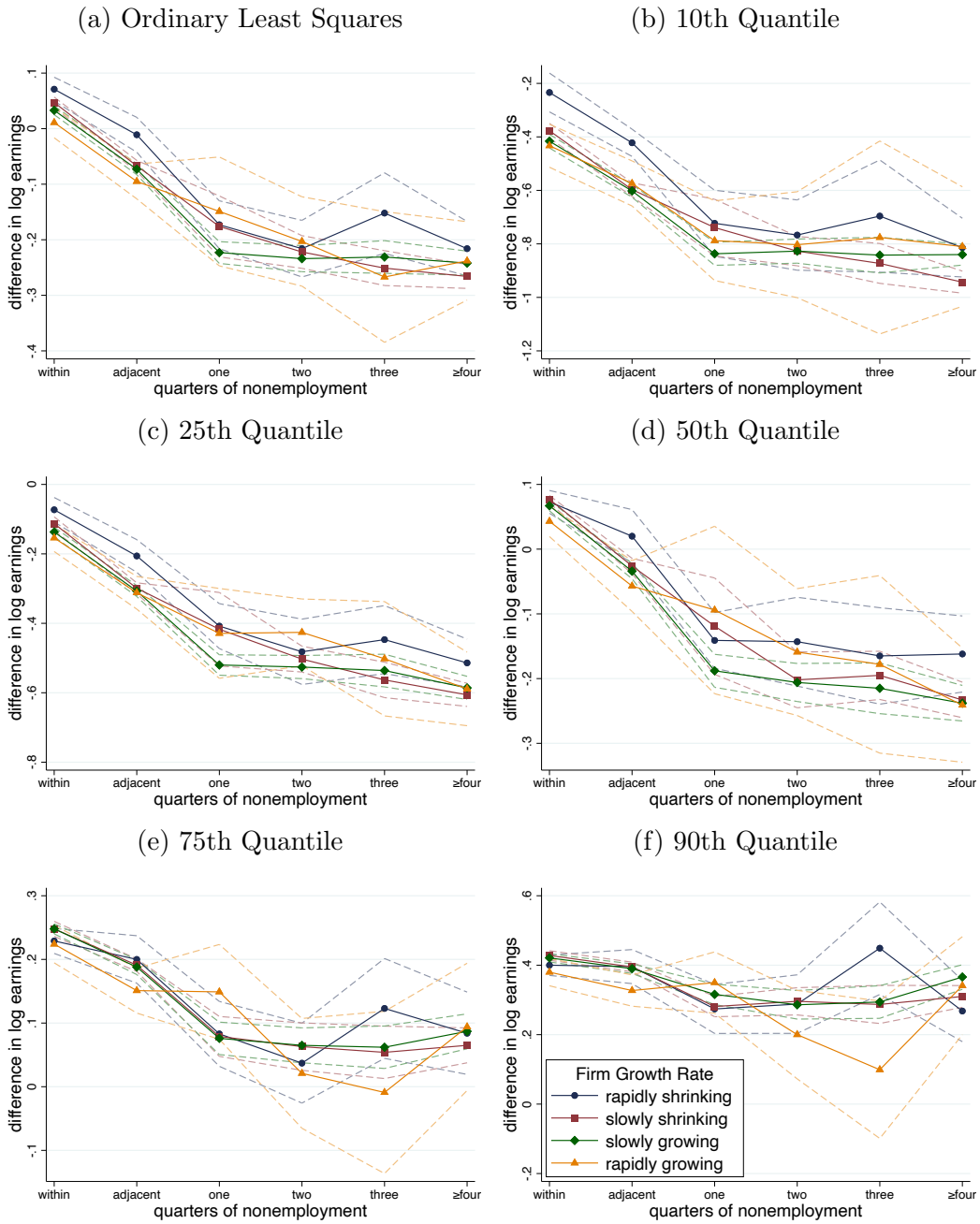


(b) Non-Distressed



Note: The figure presents the estimated earnings consequences of separating from a distressed and non-distressed firm in 2005:2 broken out by the duration of nonemployment. The results in panels (a) and (b) are derived from a sample in which separators are from non-distressed and distressed firms, respectively. All stayers are included in both samples and neither sample includes recalls. The figure displays estimates obtained from equation 4. Specifically, panel (a) and (b) plot $\delta^{k,1,N}$ and $\delta^{k,0,N}$ against the quarter relative to displacement, respectively. Standard errors are clustered at the level of the employer in the reference quarter and the solid gray lines depict the 95 percent confidence interval around the estimates.

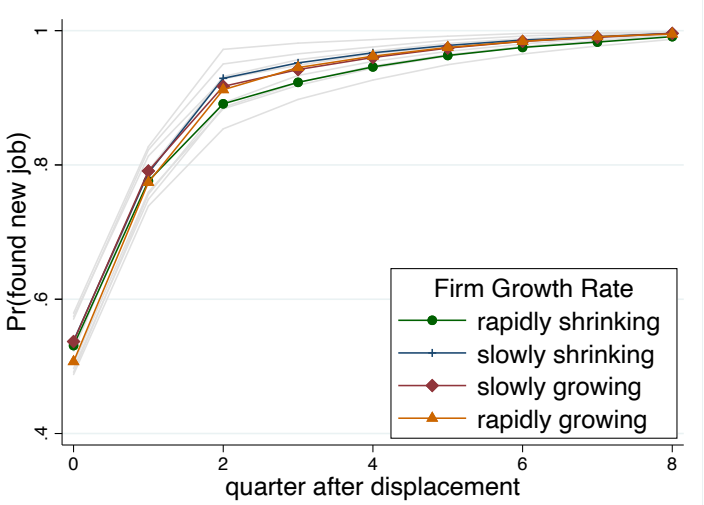
Figure 5: Predicted Change in Earnings



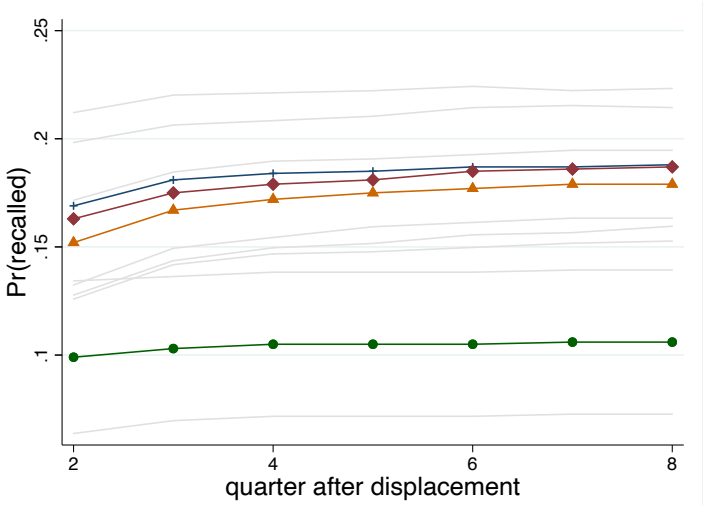
Notes: The figures present the predicted earnings penalties for separators from each growth category of firm relative to stayers, evaluated at the means of the other covariates. The estimates are obtained from equation 5. Earnings prior to separation are measured four quarters prior to separation and earnings post-separation are measured one quarter after re-employment. Panel (a) presents results from ordinary least squares (OLS) and panels (b)-(f) present results from quantile regressions, where the 10th, 25th, 50th, 75th, and 90th percentiles are reported. Within each figure, each line represents the firm growth category and the horizontal axis denotes the duration of time spent in nonemployment prior to re-employment. Standard errors are clustered at the level of the employer in the reference quarter and the dotted lines represent the 95% confidence interval.

Figure 6: Duration of Nonemployment for Permanent Separators and Recalls

(a) Permanent Separators



(b) Recalls



Notes: Panels (a) and (b) present the probability of exiting nonemployment by a given quarter after separation for permanent separators and recalls, respectively. The probability of a separator finding a new job in a given quarter after separation and the probability of a separator being recalled in a given quarter after separation are estimated by logistic regression. We then use these estimated probabilities to calculate the problem of finding a new job by a given quarter after separation conditional on never being recalled as well the probability of being recalled by a given quarter after separation. Note that the sample excludes separators who do not return within eight quarters of the separation; thus, the probability of a permanent separator finding a new job within eight quarters is one. Standard errors are clustered at the level of the employer in the reference quarter and the dotted lines represent the 95% confidence interval.

Figure 7: Effects of Separations in Other Reference Periods

(a) Distressed Separators in 1999

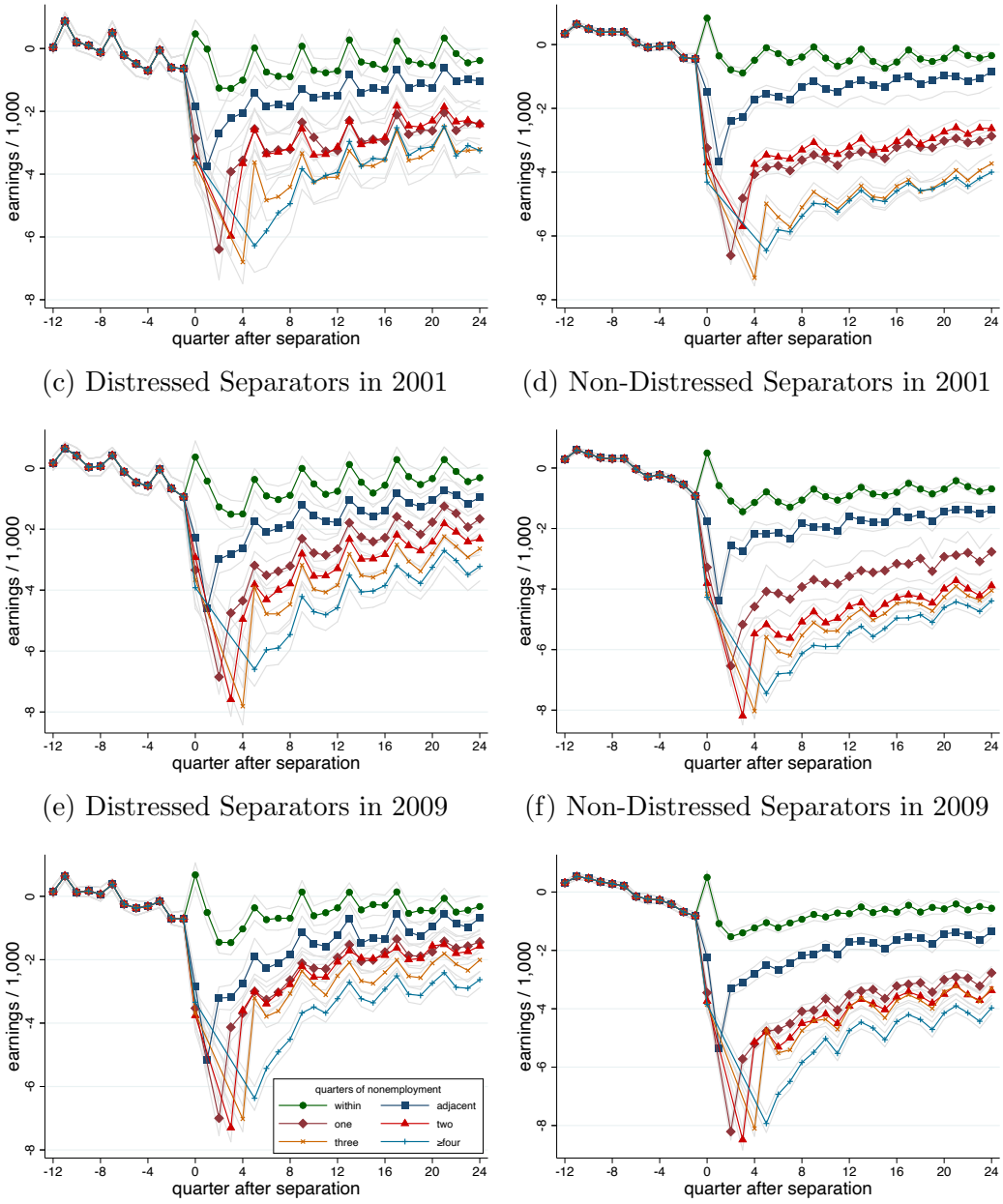
(b) Non-Distressed Separators in 1999

(c) Distressed Separators in 2001

(d) Non-Distressed Separators in 2001

(e) Distressed Separators in 2009

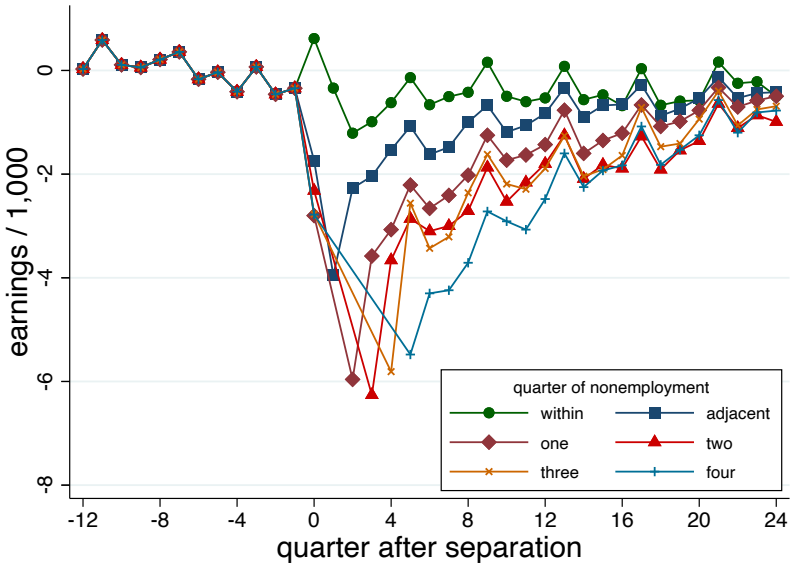
(f) Non-Distressed Separators in 2009



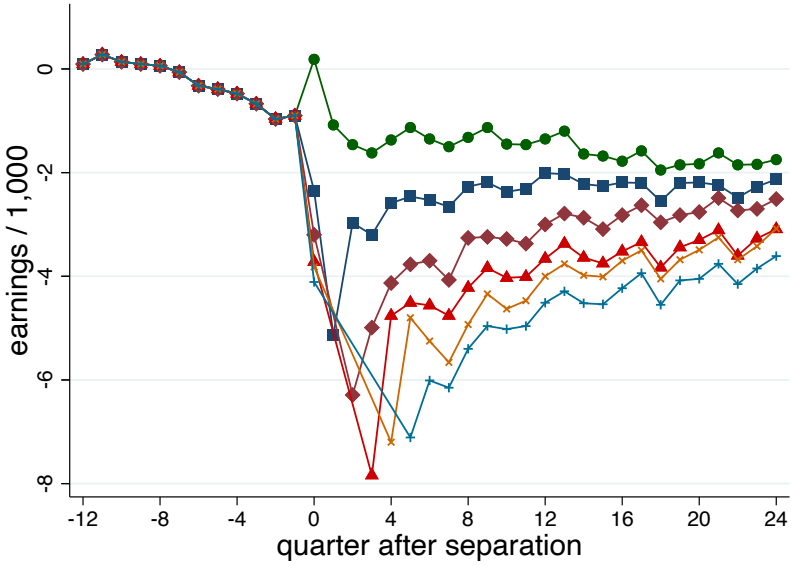
Notes: The figure presents the estimated earnings consequences of separating from a non-distressed and distressed firm for reference quarters 1999:2, 2001:2, and 2009:2. Within each figure the effects are broken out by the duration of nonemployment. Results presented in the left ((a), (c) and (e)) and right ((b), (d), and (f)) panels are derived from a sample in which separators are from distressed and non-distressed firms, respectively. All stayers are included in both samples and neither sample includes recalls. The figures display estimates obtained from equation 3. Specifically, the estimates of $\delta^{k,d,N}$ are plotted against the quarter relative to displacement. Standard errors are clustered at the level of the employer in the reference quarter and the dotted lines represent the 95% confidence interval.

Figure 8: Inclusion of Individual-Specific Time Trend

(a) Distressed



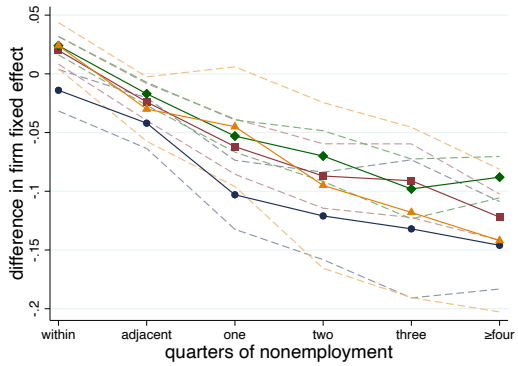
(b) Non-Distressed



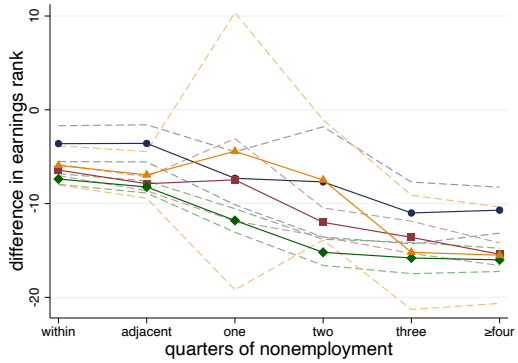
Notes: The figure presents the estimated earnings consequences of separating from a distressed and non-distressed firm in 2005:2 broken out by the duration of nonemployment. The results in panels (a) and (b) are derived from a sample in which separators are from non-distressed and distressed firms, respectively. All stayers are included in both samples and neither sample includes recalls. The figure displays estimates obtained from equation 4. Specifically, panel (a) and (b) plot $\delta^{k,1,N}$ and $\delta^{k,0,N}$ against the quarter relative to displacement, respectively. Regressions include a linear individual-specific time trend.

Figure 9: Change in Firm-Level Characteristics

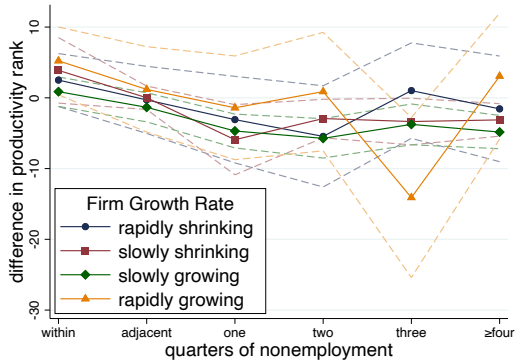
(a) Firm Fixed Effect



(b) Average Earnings



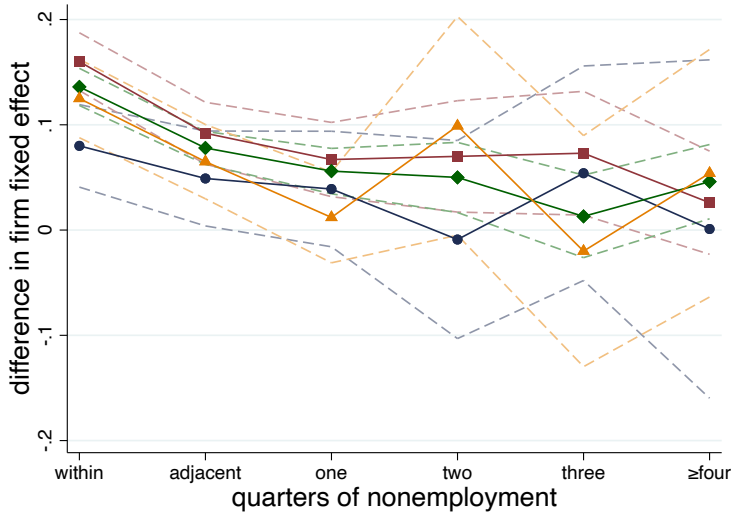
(c) Productivity



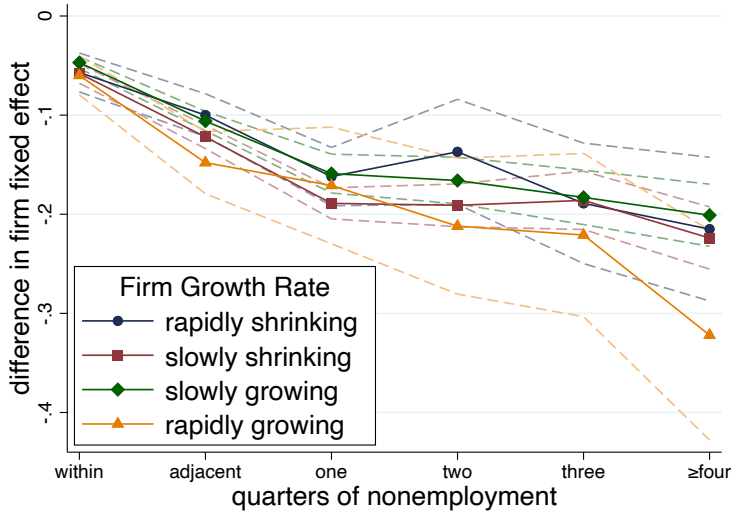
Notes: Each panel presents the predicted change in a different firm-level variable for separators from each growth category, evaluated at the means of the other covariates. The estimates are obtained from equation 5 estimated via Ordinary Least Squares. The outcome variable in panels (a), (b), and (c) are the difference between the destination and origin based on the fixed fixed effect, average earnings rank, and productivity rank, respectively. Within each figure, each line represents the firm growth category and the horizontal axis denotes the duration of time spent in nonemployment prior to re-employment. Standard errors are clustered at the level of the employer in the reference quarter and the dotted lines represent the 95% confidence interval.

Figure 10: Heterogeneity in Change in Firm-Level Characteristics

(a) Low Firm Fixed Effect



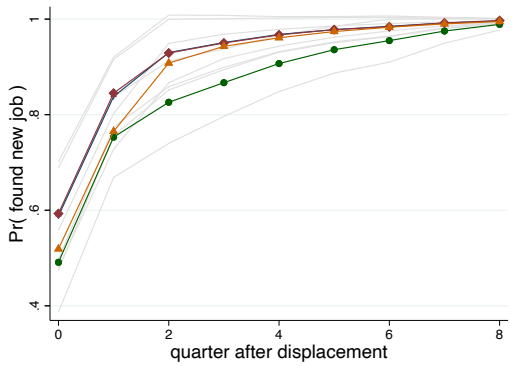
(b) High Firm Fixed Effect



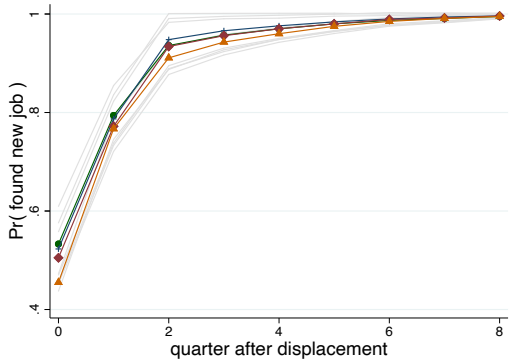
Notes: Panels (a) and (b) present estimates based on a sample of workers whose initial employer has a firm fixed effect that is below and above the sample median, respectively. Each panel presents the predicted difference between the destination and origin based on the fixed fixed effect, evaluated at the means of the other covariates. The estimates are obtained from equation 5 estimated via Ordinary Least Squares. Within each figure, each line represents the firm growth category and the horizontal axis denotes the duration of time spent in nonemployment prior to re-employment. Standard errors are clustered at the level of the employer in the reference quarter and the dotted lines represent the 95% confidence interval.

Figure 11: Duration of Nonemployment by Industry Growth

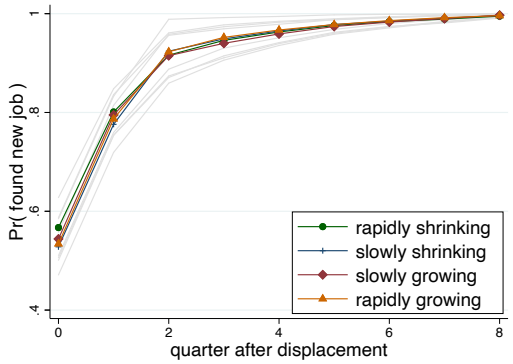
(a) Weak



(b) Average



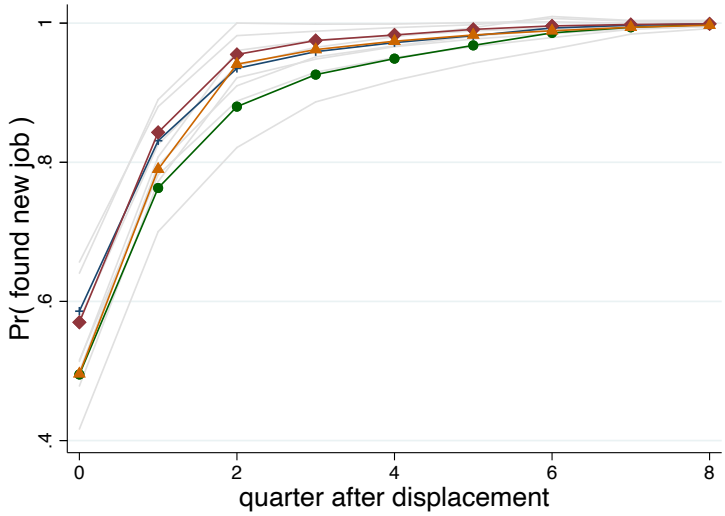
(c) Strong



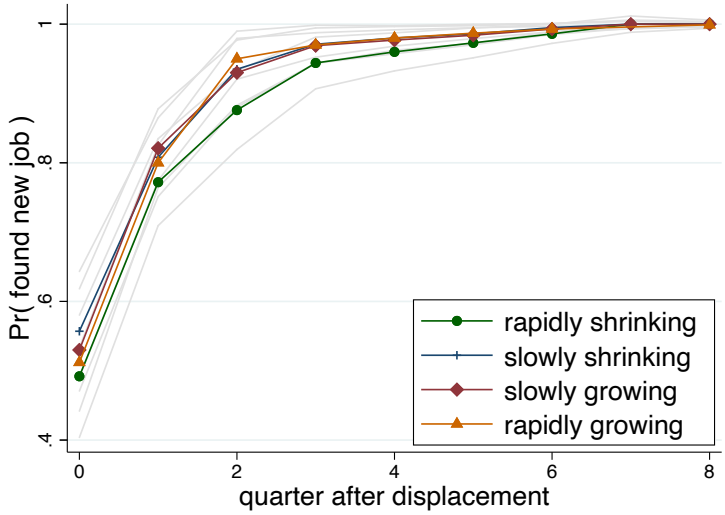
Notes: The figure presents the probability of exiting nonemployment by a given quarter after separation for permanent separators. The probability of a separator finding a new job in a given quarter after separation and the probability of a separator being recalled in a given quarter after separation are estimated by logistic regression. We then use these estimated probabilities to calculate the problem of finding a new job by a given quarter after separation conditional on never being recalled. Panels (a), (b), and (c) present estimates based on subsamples define by the employment growth rate in the state and industry of the origin employer. Standard errors are clustered at the level of the employer in the reference quarter and the dotted lines represent the 95% confidence interval.

Figure 12: Duration of Nonemployment by Occupation Growth

(a) Weak



(b) Strong



Notes: The figure presents the probability of exiting nonemployment by a given quarter after separation for permanent separators. The probability of a separator finding a new job in a given quarter after separation and the probability of a separator being recalled in a given quarter after separation are estimated by logistic regression. We then use these estimated probabilities to calculate the problem of finding a new job by a given quarter after separation conditional on never being recalled. Panels (a) and (b) present estimates based on subsamples define by the employment growth rate in the state and occupation of the origin job. Standard errors are clustered at the level of the employer in the reference quarter and the dotted lines represent the 95% confidence interval.

X. Tables

Table 1: Descriptive Statistics

	Non-Distressed		Distressed	
	stayers (1)	permanent separators (2)	stayers (3)	permanent separators (4)
Age at time of separation				
25≤age≤34	24.9	37.9	22.6	28.7
35≤age≤44	34.8	34.7	34.8	36.4
45≤age≤55	40.3	27.4	42.5	34.9
Sex				
Male	50.5	52.1	53.5	59.6
Industry				
Finance, Insurance, and Real Estate Rental and Leasing	7.3	8.6	3.8	5.3
Administrative and Support	3.0	5.7	4.4	8.4
Agriculture, Forestry, Fishing and Hunting	0.8	0.8	2.1	1.6
Arts, Entertainment, and Recreation	1.4	1.6	1.3	1.0
Construction	3.2	4.2	11.3	10.6
Manufacturing (Durable)	16.4	13.5	16.8	21.0
Educational Services	14.8	9.0	21.4	5.3
Accommodation and Food Services	3.6	6.4	3.6	3.6
Health Care and Social Assistance	13.9	11.5	5.6	5.4
Information	4.6	5.2	2.4	3.4
Management of Companies and Enterprises	1.8	1.8	0.4	0.8
Mining, Quarrying and Oil and Gas Extraction	0.2	0.2		0.0
Manufacturing (Nondurable)	3.3	3.0	7.1	7.4
Other Services	1.3	1.6	2.2	2.0
Professional, Scientific and Technical Services	4.6	5.6	5.8	8.6
Retail Trade	10.7	13.6	4.0	5.5
Transportation and Warehousing	3.7	3.3	4.0	5.2
Utilities	1.2	0.3	0.1	0.0
Wholesale Trade	4.3	4.3	3.7	4.6
Firm Size				
50≤firm size<100	10.6	12.6	22.5	23.8
100≤firm size<500	27.6	31.6	45.0	49.7
500≤firm size	61.8	55.8	32.6	26.5
N	680,000	178,000	13,000	14,000

Notes: Separators are workers from CA, NC, OR, WA, or WI with at least three years of tenure at the firm at time of separation. Distressed separators are those separating from a firm with at least 50 workers that lost 30 percent or more of its employment in the year ending in the quarter subsequent to the time of separation. Firm closures are not included in the sample. Stayers are those attached workers with at least three years of tenure at their main job in the reference quarter and are employed for the entire quarter subsequent to the reference quarter. Data include observations that are missing industry information.

Table 2: Sample Size

	Sample Size		Distribution of Separators			Qrtrs. of Nonemployment Before Finding New Employer					
	stayers	separators	new employer	recall	not employed within 8 qrtrs	within	adjacent	one	two	three	≥four
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
non-distressed	680,000	250,000	0.71	0.18	0.11	0.50	0.26	0.09	0.05	0.03	0.07
distressed	13,000	18,000	0.80	0.11	0.09	0.50	0.25	0.09	0.05	0.03	0.07

Notes: Columns 1 and 2 present the total number of stayers and separators, respectively, in non-distressed and distressed firms by year. Columns 3-5 describe the composition of separators by presenting the share of separators who fall into one of three mutually exclusive categories: found new employer, recalled, and did not return to the labor market within eight quarters after the separation. Columns 6-11 present the share of permanent separators who make a transition within quarters, in the adjacent quarter or spend one to four (or more) quarters in nonemployment.

Table 3: Post-Separation Earnings within Alternative Subsamples

	within qrtr. (1)	adjacent qrtr. (2)	one (3)	two (4)	three (5)	≥four (6)
A. Re-employed within four quarters						
distressed	-356	-1520	-2490	-3240	-3160	-3990
	(174)	(205)	(196)	(307)	(301)	(268)
non-distressed	-640	-2030	-3690	-4490	-4800	-5020
	(49)	(71)	(252)	(111)	(157)	(121)
B. At least five years tenure						
distressed	-1110	-2400	-3330	-4110	-4040	-4790
	(202)	(243)	(225)	(376)	(383)	(259)
non-distressed	-1500	-3070	-4690	-5590	-6140	-6710
	(65)	(74)	(418)	(138)	(225)	(152)
C. Exclude temporary help industries						
distressed	-328	-1520	-2500	-3270	-3200	-4110
	(176)	(207)	(198)	(311)	(306)	(226)
non-distressed	-642	-2050	-3730	-4540	-4830	-5320
	(50)	(72)	(258)	(113)	(159)	(118)
D. Pre-separation earnings>10,000						
distressed	-538	-2240	-3670	-5320	-4910	-6290
	(258)	(327)	(323)	(456)	(593)	(389)
non-distressed	-789	-2640	-5410	-6640	-7260	-8080
	(72)	(116)	(473)	(177)	(309)	(201)
E. Men ages 35-44						
distressed	-551	-1970	-2900	-3840	-3520	-4650
	(232)	(235)	(264)	(397)	(388)	(302)
non-distressed	-803	-2440	-4380	-5080	-5540	-6070
	(68)	(94)	(309)	(152)	(252)	(187)
F. Women ages 25-34						
distressed	-157	-952	-2060	-2550	-2780	-3570
	(197)	(231)	(237)	(337)	(449)	(285)
non-distressed	-503	-1620	-3110	-3960	-4130	-4620
	(46)	(66)	(207)	(120)	(129)	(103)
G. New mothers						
distressed	-2320	-1710	-1800	-2810	-5700	-4390
	(1230)	(932)	(1550)	(1250)	(1460)	(1280)
non-distressed	-2160	-2530	-3890	-4980	-5950	-5910
	(296)	(306)	(288)	(398)	(451)	(247)

Notes: This table presents post-separation earnings changes for separators by growth of the employer (distressed vs. non-distressed) and quarters spent in nonemployemnt. Each panel presents results estimated from a different subsamples that include individuals that: (A) spent less than five quarters in nonemployment after separation, (B) had at least five years of tenure in 2005:2, (C) do not work for firms in the temporary help industry, (D) had average annual earnings that exceed 10,000 in the three years leading up to 2005:2, (E) are male ages 35-44, (F) are female ages 25-34, (G) are women that give birth in the first three quarters of 2005 or are a stayer. The table summarizes estimates obtained from estimating equation 4 and presents the average post-separation earnings in the four years following re-employment. Standard errors are clustered at the level of the origin firm.

Table 4: Earnings Consequences by Strength of Local Labor Market

	within qrtr. (1)	adjacent qrtr. (2)	one (3)	two (4)	three (5)	≥four (6)
A. Industry						
A.1. Weak						
distressed	-164	-1150	-1660	-2970	-2580	-2670
	(257)	(311)	(308)	(433)	(474)	(343)
non-distressed	-431	-2160	-3800	-4400	-3990	-4900
	(111)	(112)	(220)	(247)	(176)	(160)
A.2. Average						
distressed	-686	-1850	-3180	-3100	-4320	-4230
	(476)	(579)	(345)	(385)	(756)	(393)
non-distressed	-602	-1670	-3750	-4230	-5020	-5160
	(73)	(105)	(121)	(143)	(376)	(188)
A.3. Strong						
distressed	-254	-1420	-2450	-3150	-2620	-5000
	(247)	(243)	(338)	(552)	(418)	(438)
non-distressed	-828	-2300	-3630	-4720	-5120	-5560
	(85)	(126)	(492)	(178)	(222)	(211)
B. Occupation						
B.1. Weak						
distressed	-778	-1430	-2180	-3580	-3310	-4190
	(337)	(300)	(476)	(590)	(692)	(419)
non-distressed	-1120	-2070	-4070	-4860	-4900	-5560
	(123)	(186)	(226)	(286)	(348)	(238)
B.2. Strong						
distressed	-175	-1510	-1930	-2250	-3020	-4280
	(373)	(516)	(498)	(549)	(780)	(674)
non-distressed	-857	-1740	-3620	-4860	-5260	-5280
	(145)	(227)	(497)	(339)	(453)	(279)

Note: This table presents post-separation earnings changes for separators by growth of the employer (distressed vs. non-distressed) and quarters spent in nonemployemnt. Each panel presents results estimated from a different subsamples. Panel A defines the subsample based on the employment growth rate of the industry within the state. Panel B defines the subsmaples based on the employment growth rate within the occuption and state. The table summarizes estimates obtained from estimating equation 4 and presents the average post-separation earnings in the four years following re-employment. Standard errors are clustered at the level of the origin firm.

Online Appendix not for Publication

Winsorization:

Let z_i be the greater of the median of earnings observed for individual i 24 quarters before or after the reference quarter and 10,000 ($z_i = \max\{\text{median}\{y_{it}\},10000\}$). Then define the earnings growth rate for each individual and quarter as:

$$\Delta_{it} = (y_{it} - z_i)/[\frac{1}{2} * (y_{it} + z_i)]$$

The growth rate, Δ_{it}, captures the extent to which the current earnings exceed the typical earnings of that individual in a given quarter. This growth rate, made popular by Davis et al. (1996) and commonly referred to as the DHS growth rate, is bounded between -2 and 2. We use this growth rate to identify large increases in quarterly earnings that are likely driven by data errors. The choice of the minimum value of z as 10,000 is made such that we do not accidentally winsorize earnings for low earners. We chose to edit the earnings values if they exceed the 95th percentile of earnings growth rates such that if we were to recalculate Δ_{it} using the edited earnings then Δ_{it} would be equal to the 95th percentile of earnings growth rates. Specifically, let $\Delta(p95)$ be the 95th percentile of Δ_{it}. The earnings data used in the analysis are equal to:

$$y_{it} = \begin{cases} y_{it} & if\ \Delta_{it} < \Delta(p95) \\ z_i * \dfrac{1 + .5 * \Delta(p95)}{1 - .5 * \Delta(p95)} & else \end{cases}$$

Relative to standard winsorization methods that identify outliers in levels, this method has the advantage of correctly retaining the earnings records of high wage individuals.

Inverse Survival Functions:

We start by estimating the logistic regressions presented in equations 7 and 8. For notational simplicity, let M=[1;X;Z;g] be a matrix of the concatenation of all the right-hand-side variables and let $\emptyset_t = [\alpha_t; \beta_t; \gamma_t; \lambda_t]$ be the corresponding vector of coefficients. For each t, we use the output from the logistic regression from equation 7 to calculate the baseline probability of finding a new job in that period conditional on not being re-employed prior to t and having a firm of growth rate g=[1,2,3,4]. We denote this conditional probability as, h_t^n, and it is calculated as follows:

$$h_t^{n,g} = \frac{\exp\left(\bar{M}_t\hat{\emptyset}_t + \frac{\delta_{gt}}{2}\right)}{\left[1 + \exp\left(\bar{M}_t\hat{\emptyset}_t + \frac{\delta_{gt}}{2}\right)\right]} - \frac{\exp\left(\bar{M}_t\hat{\emptyset}_t - \frac{\delta_{gt}}{2}\right)}{\left[1 + \exp\left(\bar{M}_t\hat{\emptyset}_t - \frac{\delta_{gt}}{2}\right)\right]}$$

Where $\bar{M}$ denotes a vector of the mean value of all covariates and $\widehat{\emptyset}_t$ is the vector of coefficient estimates from the logistic regression. [1] Using this same methodology we use the estimates from equation 8 to calculate the condition probabilities for recalls, denoted h_t^r. To summarize, the estimates from the logistic regression allow us to calculate two types of conditional probabilities:

$$h_t^{n,g} = Pr(new\ job\ in\ t \mid not\ reemployed\ before\ t\ \&\ firm\ growth\ rate\ g)$$

$$h_t^{r,g} = Pr(recall\ in\ t \mid not\ reemployed\ before\ t\ \&\ firm\ growth\ rate\ g)$$

Note that $h_t^r = 0$ for t<2 by construction.

Using these probabilities we then calculate the following:

$$h_t^g = \Pr(reemployment\ in\ t\ \&firm\ growth\ rate\ g) = h_t^{n,g} + h_t^{r,g}$$

$$P_0^{n,g} = \Pr(new\ job\ by\ t = 0\ \&firm\ growth\ rate\ g) = h_0^{n,g}$$

$$P_t^{n,g} = \Pr(new\ job\ by\ t > 0\ \&\ firm\ growth\ rate\ g) = \sum_{\tau=0}^{t}\left(\prod_{s=0}^{\tau-1}(1-h_s^g)\right)h_\tau^{n,g}$$

$$P_t^{r,g} = \Pr(recall\ by\ time\ t\ \&\ firm\ growth\ rate\ g) = \sum_{\tau=2}^{t}\left(\prod_{s=0}^{\tau-1}(1-h_s^g)\right)h_\tau^{r,g}$$

Lastly, we calculate the probability of finding a new job by time t, conditional on never being recalled as:

$$\Pr(\text{new job by t} \mid \text{never recalled \&}\ firm\ growth\ rate\ g) = {P_t^{n,g}}\Big/{(1-P_8^{r,g})}$$

We present the results as an inverse survival plot in which we create a plot in which the x-axis is t and the y-axis is the probability of re-employment by t and we plot four separate lines for the four estimates of Pr(new job by t | never recalled & firm growth rate g).

[1] Note that when we estimate the logistic regression g=4 is the reference firm growth rate category. The above notation is consistent with this if you simply assume that $\delta_{4t} = 0$.