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ABSTRACT

This paper studies the cyclical behavior of job separation and the characteristics of matches between workers and jobs. We estimate a proportional hazard model with competing risks, distinguishing between different types of separations. A higher unemployment rate at the start of an employment relationship increases the probability of job-to-job transitions, whereas its effect on employment-to-unemployment transitions is negative. We then build a simple job-ladder model to interpret our empirical results. A model with two-dimensional heterogeneity in match (job) characteristics has the same qualitative features as the data. Once the model is calibrated to include cyclicality in the offered match characteristics, it can also fit the quantitative features of the data. The model reveals matches formed in booms provide more nonwage utility to the workers but are subject to a higher future probability of separation shocks.

1. Introduction

This paper analyzes how the characteristics of the match between jobs and workers vary over the business cycle. In a frictional labor market with heterogeneous workers and jobs, the match between workers and firms can differ from the first best. The degree of frictions in the labor market may vary over different phases of the business cycle. A natural question, therefore, is whether better matches are formed during booms than in recessions. We approach this question by analyzing workers' job durations.

The main idea behind our study is that a better worker-firm match would last longer. If a worker has a better match with a particular firm than with another firm, he is more likely to feel better off working in the former firm, and thus he is less likely to leave that firm. To break such a match would require a large negative shock. Our approach is in the spirit of the revealed-preferences theory in that we infer the characteristics of a match from observed behavior. The advantage of our approach over the alternative approach of directly measuring productivity and wages from matched employer-employee data is that we can consider a broader concept of match quality that does not show up in output or wages, such as the attachment of a worker to a particular firm or the worker's geographical preferences.[1]

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[1] Many recent papers, including Nosal and Rupert (2007), Nunn (2013), Sullivan and To (2014), Jarosch (2015), Taber and Vejlin (2016), Hall and Mueller (2018), and Sorkin (2018) emphasize the importance of considering non-wage characteristics in analyzing workers' job choices.

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In the following, we first empirically study the effects of aggregate labor-market conditions on job duration using data from the National Longitudinal Survey of Youth (NLSY) 1979 cohort. We use the Cox (1972) proportional hazard model for job separations. A novel feature of our estimation is that we estimate the effects of the unemployment rate using a competing-risk structure in which an individual can experience different types of separations. We find the likelihood of a separation that leads the worker into a job-to-job transition (EE transition) and the likelihood of a separation that moves the worker into unemployment (EU transition) have strikingly different relationships to aggregate labor-market conditions at the formation of the match.[2] A high unemployment rate at the start of a match increases the probability that it ends with an EE transition, but it has a negative effect on the probability that it ends with an EU transition. Our results are robust to several alternative specifications.

Our motivation for distinguishing between the types of job separation stems from the fact that different types of job separations are known to exhibit different cyclical properties. For example, the EE transition rate is known to be procyclical, whereas the EU transition rate is countercyclical, and thus the current unemployment rate is expected to have the opposite effect on job duration. These opposing forces can cancel each other out if the estimation does not distinguish between EE and EU transitions. In our setup, we can estimate both effects separately. As expected, an increase in the current unemployment rate (for a given unemployment rate at the start of the job spell) reduces the probability that the job spell ends with an EE transition, but it increases the probability that it ends with an EU transition.

We establish these results regarding the responses of EE and EU transitions to the unemployment rate after controlling for the starting wages. The pioneering work by Bowlus (1995) also studies the job duration over the business cycle but without distinguishing between different separation types. She shows the unemployment rate at the start of the job has a negative effect on job durations, but this effect disappears once the starting wages are controlled for. We obtain similar estimates but smaller in magnitude when we mix together all the separation types in our sample, as in Bowlus (1995).[3] Bowlus's (1995) interpretation of her result is that the cyclical movements in the match quality are internalized by wages. Our findings with different separation types draw a different conclusion. The opposite movements in EE and EU transitions over the business cycle can make overall separation rate close to acyclical. Analogously, our results suggest Bowlus's (1995) result reflects the opposite responses of EE and EU transitions to the unemployment rate at the start of a spell.

Using data from NLSY 1979, Mustre-del-Rio (2019) also studies job duration over the business cycle. Our paper methodologically differs from his paper in that he does not employ a competing risk structure. Instead, he estimates the hazard rates for certain transition types based on pre- and post-employment status and studies job duration for each transition type using their respective hazard-rate estimates. For example, he finds that a match formed during a boom lasts longer for jobs that are created from non-employment and ends with a transition to non-employment. Under a competing-risk structure, we are able to interact the hazard rates for different transition types and study overall job duration over the business cycle. Finally, using a matched employer-employee dataset, Kahn (2008) finds job spells are shorter when a match is formed during a recession. However, this relationship reverses after controlling for firm fixed effects.

The contrasting effect of aggregate conditions on different types of separations calls for a theoretical interpretation. To this end, we build a quantitative job-ladder model that features both endogenous separation and on-the-job search. We focus on the worker side of the problem, because our dataset contains information only on workers. (Thus, we use the terms "matches" and "jobs" interchangeably in the context of the model.) The model contains endogenous and exogenous separations: a match can break up either because the worker chooses to separate from a job (to move to another job or to unemployment) or because an exogenous shock moves a worker into unemployment. An employed worker can receive a job offer from another employer with some probability and decide whether to accept it. She may instead receive an exogenous-separations shock, or may choose to move into unemployment. An unemployed worker can receive a job offer with some probability and decide whether to accept it. Matches (or, equivalently, jobs) are heterogeneous and the characteristics of matches are randomly drawn when job offers are generated.

We consider two-dimensional heterogeneity in match characteristics. The first is the (nonwage) utility from working at the particular firm; this type of heterogeneity is emphasized, for example, by Sorkin (2018). We call this component *match quality*. The second is the frequency of the exogenous job-destruction shocks. We call this component *job stability* (or *match stability*), and we consider two types of jobs for this dimension. This type of heterogeneity in jobs received attention in the recent literature; see Jarosch (2015) and Mukoyama (2019), for example.

We estimate the model-generated data in the same manner that we treat the NLSY data. In our baseline calibration, which assumes the job-offer distribution (in terms of the match quality and job stability) is acyclical, the Cox-estimation coefficients are of the same sign and of similar quantitative magnitude as the empirical counterparts except for one. We provide intuitions for the Cox coefficients by providing counterfactual experiments using the model.

To match the quantitative magnitudes of all coefficients in the Cox estimation, we allow for the cyclicality of job-offer distribution. We are able to match the empirical coefficients by assuming (i) the average of the offered match quality is better during booms and (ii) the matches (jobs) that are offered in recessions are more stable. With this specification, the matches that are formed during the booms are of higher quality but are less stable. In that sense, the answer to the question

[2] Although we also estimate the hazard rate for the transitions out of the labor force (EN transitions), some measurement issues are present in our dataset, and we mainly focus our discussion on comparing EE and EU transitions.

[3] These two papers differ in several ways. For example, our sample includes a longer time period, and more importantly, we are able to include prime-aged workers. The longer panel also allows us to address unobserved heterogeneity more effectively.

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"Are matches formed in booms better?" is quite subtle; they are better in the sense that they provide more nonwage utility to the workers, but they are worse in the sense that their future probability of exogenous separation is higher.

In the next section, we describe the dataset. Section 3 describes the empirical methodology, in particular, cause-specific and subhazard regressions. We present the estimation results in Section 4. Section 5 describes the model and the baseline calibration. The model results are shown in Section 6. Section 7 extends the model calibration to allow for cyclicality in the characteristics of offered jobs. Section 8 concludes.

2. Data description

We use data from the NLSY 1979 cohort in this study. A total of 12,686 individuals born between 1957 and 1964 participated in this survey. These individuals were interviewed annually from 1979 through 1994 and biennially thereafter until 2014. The survey collected detailed information about each job a respondent currently holds or previously held.[4] We use the Employer History Roster for employment histories for all the individuals participating in the survey. This roster includes exact start and stop dates for each job a respondent reported, and alleviates the more involved linking process across different survey years.

Our analysis is based on differentiating across job separations. The panel aspect of the data allows us to keep track of what happens after separating from the current employer, that is, whether the worker becomes non-employed or finds another job immediately. Based on this information, we distinguish employment-to-employment transitions, EE, as follows. When a worker loses his job but is able to find a new job within one month, we call this separation an EE transition.[5] In a number of cases, the individual had another ongoing employment relationship when the job spell ended.[6] We treat these job terminations also as EE transitions because the individual already had another job.

The job spells that are not an EE transition can either be a transition to unemployment, EU, or movement out of the labor force, EN. The distinction between the two types of separation might potentially impact our estimates because transitions to unemployment and out of the labor force exhibit distinct cyclical behavior. Although transitions to unemployment are strongly countercyclical, transitions out of the labor force are weakly procyclical.[7] Merging the two types of transitions would undermine the effect of aggregate labor-market conditions on transitions to unemployment.

To isolate EU transitions, we utilize the weekly array of employment status in NLSY 1979 database. Unfortunately, the survey did not collect the exact dates for job-search activity during the interviews. It only inquired about the total number of weeks spent actively searching for a job. If an individual was not unemployed for the entire spell of non-employment, the total number of weeks reported as unemployed is placed in the middle of the non-employment spell in the weekly array of employment status. Given this measurement error, we are only certain that an individual made a *direct* transition to unemployment if he is recorded as unemployed in the week immediately following the employment stop date. Our goal is to estimate the effect of aggregate labor-market conditions on transitions to unemployment. Therefore, we further distinguish these direct transitions to unemployment from those transitions potentially mixed with movements out of the labor force. More specifically, we label transitions to non-employment EU if the individual is recorded as unemployed in the week immediately following the last employment spell. We stress that the remaining transitions to non-employment, which we label EN, are a mixture of transitions to unemployment and out of the labor force. This procedure roughly equally divides the transitions to non-employment EU and EN.[8]

An exception to EE transitions is the case in which the separation was due to an obvious layoff or firing. We check the reason for separation for each job spell, and if it was one of the "firing" reasons we define below in Appendix C, we label this transition EU. The reason for this exception is that, by EE transition, we want to capture the behavior of a worker voluntarily moving to a new job, which is how we model the EE transition in Section 5. Furthermore, the individual makes a quick transition to employment in such cases and is likely to be attached to the labor force. Therefore, we label them an EU transition rather than an EN transition.

Our sample consists of multiple job spells for each individual. We measure the duration of a job spell in months. Some of the job spells are right censored due to the finite horizon of the survey and loss of follow-up. We include the unemployment rate at the start of the job, u_0, to analyze the effect of aggregate labor-market conditions when the job was created. We also include the current unemployment rate, u_t, as a time-varying regressor to capture on-going labor-market conditions. We obtain data from the Bureau of Labor Statistics (BLS) for the national unemployment rate. We do not seasonally adjust the time series so that it is consistent with the data from NLSY.

[4] Mustre-del-Rio (2019) discusses the advantages of NLSY over other surveys such as Current Population Survey (CPS), Panel Study of Income Dynamics (PSID), and Survey of Income and Program Participation (SIPP).

[5] This criterion is in line with the identification of EE transitions in the literature. Studies that use monthly CPS, such as Fallick and Fleischman (2004), rely on survey responses that are one month apart from each other. Tjaden and Wellschmied (2014), who use the SIPP dataset, define job-to-job transitions as those transitions in which the worker works in two consecutive months without reporting unemployment in between (in addition to the ones identified from the occupational switch).

[6] These job spells correspond to 15% of all the job spells in our final sample.

[7] See Fujita and Ramey (2006), Elsby et al. (2015), and Krusell et al. (2017), among others.

[8] In some cases, the unemployment and out-of-labor-force status cannot be determined, or no information is available for the weeks following the job end dates. We excluded these spells from our final sample.

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We also control the hourly wage rate paid at the start of the job spell. Wages are reported as the usual wage earnings during the interview for all the jobs held by the respondent. This wage information is accompanied by a follow-up question about the frequency of payments, such as hourly, monthly, and annually. The NLSY 1979 database provides a calculated hourly wage rate using these variables along with the usual hours worked. This newly created variable is comparable across all the job spells. For most of the job spells, only one wage rate is recorded. When multiple hourly wages are recorded, for example, the job spell lasts more than a year, we use the first reported wage rate in our regressions. In what follows, we refer to this variable as the starting hourly wage rate.[9]

We also include industry and occupation controls in our regressions. In NLSY 1979, the industry and occupation coding schemes switch from Census 1970 to Census 2000 classification. To create comparable industry and occupation variables, we utilize the classification schemes from IPUMS-CPS that are based on Census 1990 industry and occupation codes. We further aggregate these variables using the broad industry and occupation categories in these classification schemes. We provide a detailed description of these broad categories in Appendix B.

The other explanatory variables include personal and job characteristics at the start of a job, such as age, race, education, and whether the job is protected by a union. Finally, NLSY has three subsamples representing the cross-section of the civilian population, a supplemental sample of disadvantaged groups, and a military sample. We use the sampling weights assigned to each individual in the first round of the survey to have a nationally representative sample.

3. Estimation strategy

The Cox (1972) proportional hazard model is widely applied to duration data when time to a failure event is of interest. In the analysis of job duration, the failure event of interest is job separation. Our analysis involves multiple types (“causes”) of job separations, and only the first of these causes for job separation, if any, is observed. In other words, each cause for job separation is a competing risk for the other causes. In this section, we start with a description of Cox’s proportional hazard model when the failure event has a single cause. Then, we describe two alternative approaches proposed in the literature when competing risks are present: cause-specific hazard regressions and regression on a subhazard function.[10]

3.1. Cox proportional hazard model

Let the hazard function for job separations be:

$$h(t) = \lim_{\Delta t \to 0} \frac{P(t \le T < t + \Delta t | T \ge t)}{\Delta t}.$$

The hazard function is the instantaneous probability that a job separation occurs at time t conditional on surviving up to time t. Cox (1972) further imposes that the hazard function for job separations, conditional on a set of explanatory variables at time t, takes the following proportional form:

$$h(t|X(t)) = h_0(t)\exp(X(t)\beta), \tag{1}$$

where $X(t)$ are time-varying explanatory variables, β is a vector of parameters common across all job spells and all individuals, and $h_0(t)$ is the baseline hazard. The baseline hazard is assumed to be the same across individuals, although it is left unspecified. Cox (1972) describes a semi-parametric approach for obtaining estimates of the model parameters, $\hat{\beta}$, through the maximization of the following partial likelihood function:

$$\mathscr{L} = \prod_{i:\delta_i=1} \frac{h(t_i|X_i(t_i))}{\sum_{j:t_j \ge t_i} h(t_i|X_j(t_i))} = \prod_{i:\delta_i=1} \frac{\exp(X_i(t_i)\beta)}{\sum_{j:t_j \ge t_i} \exp(X_j(t_i)\beta)}, \tag{2}$$

where $\delta_i = 0$ if the job spell is right-censored and $\delta_i = 1$ if it is not. Note that right-censored job spells enter the partial likelihood function only through the denominator. Further, the baseline hazard can be recovered non-parametrically after obtaining $\hat{\beta}$ even though it cancels from the estimating equation. The proportionality assumption implies the (log) hazard functions are strictly parallel and inference is possible solely based on $\hat{\beta}$. Specifically, a positive (negative) value of $\hat{\beta}$ implies the probability of job separation increases (decreases) with an increase in the value of the explanatory variable.

3.2. Cause-specific hazard functions

A standard application of the Cox proportional hazard model can be misleading in the presence of competing events. The proportional hazard model in equation (1) assumes the explanatory variables affect the probability of job separation

[9] Bowlus (1995) also uses the starting wages in her regressions. Her justification for including the starting wage rather than the current wage in the hazard regressions is based on the fact that wages reported in later interviews are less responsive to the business cycles. Moreover, our data come from a longitudinal survey where interview dates are too infrequent to reflect wage earnings accurately for each month during the job spell.

[10] Both methods have their pros and cons; see, for example, Putter et al. (2007) for a general discussion.

in the same manner regardless of its cause. In this study, an EE transition and an EU transition are competing events for job separations. Hence, the starting and current unemployment rate may affect the probability of job separation in different directions for these two causes.

Taking this issue into account, here we define a separate hazard function for each cause-specific job separation. Formally, let k denote one of the K possible types of job separations. The hazard function for a job separation due to type k is:

$$h^k(t|X(t)) = h_0^k(t)\exp(X(t)\beta^k). \tag{3}$$

The specification in equation (3) is similar to the standard specification in equation (1) except that it is now separately defined for K different possible types of job separations. Both the baseline hazard functions and the parameters are allowed to differ across different types of job separations. The β^k's can be estimated separately for each cause-specific hazard function by maximizing the partial likelihood function in equation (2). However, the occurrence of a competing event is treated as right-censored in each of these estimations. Although the estimation procedure with cause-specific hazard functions is the same as with the standard Cox proportional hazard model without competing risks, the interpretations of the parameter estimates are different. Because the distributions of time to a job separation for each cause-specific event are potentially dependent, the sign of the parameter estimates alone cannot determine the effect of a covariate on the duration of job. When the hazard functions are estimated separately for each cause-specific job separation, the effect of a change in the variable of interest on a cause-specific job separation depends nonlinearly on baseline hazard functions and parameter estimates of the other cause-specific hazard functions.

To illustrate this point, let the cumulative cause-specific hazard function for an individual with $X(t)$ be

$$H^k(t|X(t)) = \int_0^t h_0^k(s)\exp(X(s)\beta^k)ds.$$

Then, the probability of surviving from any event at time t is

$$S(t|X(t)) = \exp\left(-\sum_{k=1}^{K} H^k(t|X(t))\right).$$

The survival probability now depends on the baseline and parameter estimates not only from the hazard regression of the event of interest, but also from the hazard regressions of the other competing events. Further, the probability of failing from cause k before time t is:

$$I^k(t|X(t)) = \int_0^t h^k(s|X(s))S(s|X(s))ds. \tag{4}$$

The probability in equation (4) is called the *cumulative incidence function*. The cumulative incidence function represents the probability that failure from cause k occurs before time t. Because this function is an intuitively appealing object, below we measure the effect of a change in the starting and current unemployment rates on different types of separations by constructing cumulative incidence functions.

3.3. Regression on a subhazard function

As an alternative to cause-specific hazard regressions, Fine and Gray (1999) propose a methodology that allows inference on cumulative incidence functions based solely on estimates of β^k. They define a subhazard function for the competing risk k as follows:

$$\bar{h}^k(t) = \lim_{\Delta t \to 0} \frac{P(t \le T < t + \Delta t | T \ge t \cup (T \le t \cap K \ne k))}{\Delta t}. \tag{5}$$

The subhazard function shows the instantaneous probability of a job ending due to reason k conditional on surviving up to time t *or* ending before time t due to a reason other than k. Similarly to Cox's proportional hazard model, Fine and Gray (1999) assume the subhazard function takes the following form:

$$\bar{h}^k(t|X(t)) = \bar{h}_0^k(t)\exp(X(t)\beta^k). \tag{6}$$

The subhazard function in equation (6) can be estimated analogously to equation (2). The only difference in the estimation procedure is in the treatment of the risk set. According to equation (5), job spells that have already ended due to another cause are still considered to be in the risk set for the competing risk k. Because these observations can potentially become right-censored and dropped from the risk set (but the censoring cannot be observed, because job spells have already ended), Fine and Gray (1999) weight them using the Kaplan-Meier estimate of the survivor function for the censoring distribution.

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One of the advantages of the estimation strategy proposed by Fine and Gray (1999) is that inference can now be made based solely on $\hat{\beta}^k$, because the subhazard function is directly linked to the cumulative incidence function. Note the cumulative incidence and subhazard functions for the competing risk k for an individual with $X(t)$ are related as follows:

$$I^k(t|X(t)) = 1 - \exp\left(-\int_0^t \bar{h}_0^k(s)\exp(X(s)\beta^k)ds\right).$$

The estimates of β^k have a similar interpretation to the standard Cox proportional hazard model. A positive (negative) value of $\hat{\beta}^k$ implies increasing the value of the explanatory variable increases (decreases) the probability of job separation due to cause k.

Although the methodology in Fine and Grey (1999) facilitates inference for the covariate effects on a particular event of interest, the estimation procedure for a subhazard function is carried out independently of the subhazard functions for other competing risks. A drawback of this approach is that a joint distribution of competing events is not identified. If a researcher is interested in the effects of a covariate on some combination of the subhazard functions, these estimates can produce implausible results. For example, the survival function is equal to one minus the sum of the cumulative incidence functions over all the competing risks. The estimation procedure in Fine and Grey (1999) does not guarantee that this value is nonnegative over the entire domain of the covariate values. For large t, the predicted survival probability can be negative. Therefore, we use the cause-specific hazard estimates to produce cumulative incidence functions below because survival probabilities as well as cumulative incidence functions are between 0 and 1 by construction from equation (4).

3.4. Controlling for unobserved heterogeneity

If an individual-specific unobserved component exists, the job spells for the same individual are potentially correlated, and the estimates of β^k are biased. To address the concerns about unobserved heterogeneity, we implement the most commonly used methods in the literature.[11] First, we introduce a random *frailty* term to the hazard function, which is the same across all the job spells for that individual but differs from other individuals. We still assume the baseline hazard function and the coefficients are the same for all job spells. More specifically, the hazard rate due to cause k for job j of individual i takes the following form:

$$h^k(t|X_{ij}(t), u_{ik}) = h_0^k(t)\exp\left(X_{ij}(t)\beta^k + u_{ik}\right).$$

We define the subhazard function similarly. For both the cause-specific hazard and subhazard estimations, we assume the frailty terms for each job-termination reason, u_{ik}, are independent draws from their respective normal distributions with mean 0 and variances to be estimated from the data. This method is the counterpart of the random-effect estimation in a linear regression model. Despite the specific distributional assumption, this approach allows us to retain all the job spells in our sample.

The second method is based on *stratification* of the baseline hazard across individuals. This method is the counterpart of the fixed-effect estimation in a linear regression model. Under this specification, we restrict the baseline hazard function for each individual to be the same for all job spells, although the baseline hazards can differ across individuals. The hazard rate due to cause k for job j of individual i takes the following form:

$$h_i^k(t|X_{ij}(t)) = h_{0,i}^k(t)\exp\left(X_{ij}(t)\beta^k\right).$$

The advantage of this method is that no distributional assumption is necessary. The risk sets are separately constructed for each individual and include only the job spells for that individual. The baseline hazard functions, $h_{0,i}^k(t)$, can then be recovered non-parametrically for each individual. However, individuals with only one job spell do not contribute to the likelihood function. Thus, we remove those individuals from our sample.[12] We also drop the individual characteristics that are constant over time, for example, indicators for high school and college degrees. These variables are not informative and simply cancel out from the likelihood under this specification.

4. Empirical results

4.1. Sample restrictions

Our sample covers all the survey years. Following Bowlus (1995), we restrict our sample to include only private-sector employment. Jobs that start before the individual completes all schooling or is younger than 16 years old are dropped from

[11] To address the concerns about unobserved heterogeneity, Bowlus (1995) randomly selects one job spell per individual. This procedure still produces unbiased estimates of β^k. However, we would end up dropping about 80% of the observations from our sample if we used this method.

[12] When we estimate the competing-risk models, some of the individuals may not have experienced a given type of event. Keeping these individuals in the sample does not affect the estimation procedures because their contribution to the corresponding likelihood function is nil.

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Table 1
Cause-specific hazard estimation under EE, EU, and EN classifications: these estimations use data from all the survey years available. SWAGE = natural logarithm of real starting wages; SQAGE = age squared; UNION = 1 if the job is covered under a union contract or collective bargaining agreement; HS = 1 if the respondent is a high-school graduate; COL = 1 if he completed 16 or more years of education; NWHITE = 1 if the respondent is black or Hispanic. Standard errors are given in parentheses. Coefficient estimates for industry and occupation variables are not reported. Significance of the variance of the frailty terms are based on a likelihood-ratio test comparing the model with and without frailty terms. For stratified regressions, standard errors are clustered over individuals. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Variable	Normal frailty			Stratified		
	EE	EU	EN	EE	EU	EN
u_0	.058***	−.057***	−.052***	.059***	−.053***	−.011
	(.010)	(.011)	(.017)	(.017)	(.019)	(.029)
u_t	−.110***	.122***	.038**	−.122***	.108***	−.030
	(.010)	(.010)	(.016)	(.017)	(.019)	(.030)
SWAGE	−.602***	−.434***	−.748***	−.972***	−.271***	−.689***
	(.030)	(.035)	(.055)	(.068)	(.072)	(.114)
AGE	−.000	−.035***	−.080***	−.011	−.074***	−.194***
	(.012)	(.012)	(.019)	(.019)	(.018)	(.033)
SQAGE	−.001***	.000	.001***	−.000	.001**	.002***
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
UNION	−.304***	.107**	−.112	−.133*	.043	−.043
	(.041)	(.040)	(.067)	(.068)	(.069)	(.093)
HS	.027	−.150***	−.331***	-	-	
	(.038)	(.040)	(.057)	-	-	
COL	.356***	−0.420***	−.624***	-	-	
	(.052)	(.065)	(.099)	-	-	
NWHITE	−.139***	.250***	.433***	-	-	
	(.030)	(.032)	(.047)	-	-	
Frailty (variance)	.354***	.383***	.686***	-	-	
Occurrence:	8,063	6,657	3,100	7,913	6,542	2,991
# of job spells:	20,741			19,998		
# of individuals:	4,331			3,588		
# of right-censored:	2,921			2,552		

the sample.[13] Further, the sample does not include jobs with missing information and those lasting less than a month. We focus on full-time work and drop job spells with less than 35 weekly hours worked. We also exclude females from our sample because they are likely to quit for reasons other than poor match quality, such as marriage, pregnancy, and childcare.

We drop job spells that report wage earnings below the federal minimum wage rate at the start of the job spell. We then convert this variable to real starting hourly wage rate using the Consumer Price Index (CPI) series obtained from the BLS website. For a few observations, this procedure yields unrealistically high wage earning potentially due to a measurement error in the original data. Therefore, we dropped the job spells that report hourly real wage rates greater than 120 US dollars. These job spells correspond to less than 0.1% of the total number of job spells in our sample.

Our goal is to measure the effects of aggregate labor-market conditions on the civilian population. Thus, we drop the job spells from the military subsample. We also re-scale the sampling weights so that the average weight over all the jobs is equal to 1.

Our final sample consists of multiple job spells for each individual. We provide summary statistics for the explanatory variables in Appendix A.

4.2. Main results

Tables 1 and 2 present our main estimation results from cause-specific and subhazard regressions, respectively, using data from 1979 to 2014. The first three columns in Table 1 show the estimation results from the cause-specific hazard regressions with normally distributed frailty terms for EE, EU, and EN transitions. The last three columns show the results from the cause-specific regressions with stratification. Table 2 similarly shows our estimation results from subhazard regressions separately for normal frailty and stratified regressions.[14] All the regressions control for the occupation and industry of the job, although their coefficient estimates are not presented. The coefficients of interest are those for the unemployment rate at the start of the job spell, u_0, and the current unemployment rate, u_t.[15]

[13] We use the variable indicating monthly school-enrollment status to determine the final month of school enrollment.

[14] The stratified subhazard estimation is not available in standard statistical packages. We extend our original sample by assigning the censoring probability as sampling weights to the job spells ending by a competing risk, and run a Cox regression to this extended sample. See Fine and Gray (1999) for details. For u_t in the extended part of the data, we inserted the value observed in the last period of the job spell.

[15] An alternative to using the unemployment rate at the start of the job spell is to use the unemployment rate at the start of the employment spell. We use the former because (i) we are interested in matches rather than employment states, and (ii) it is more straightforward for the comparison to the existing literature.

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Table 2
Subhazard estimation under EE, EU, and EN classifications: these estimations use data from all the survey years available. SWAGE = natural logarithm of real starting wages; SQAGE = age squared; UNION = 1 if the job is covered under a union contract or collective bargaining agreement; HS = 1 if the respondent is a high-school graduate; COL = 1 if he completed 16 or more years of education; NWHITE = 1 if the respondent is black or Hispanic. Standard errors are given in parentheses. Coefficient estimates for industry and occupation variables are not reported. Significance of the variance of the frailty terms are based on a likelihood-ratio test comparing the model with and without frailty terms. For stratified regressions, standard errors are clustered over individuals and corrected for KM weighting errors. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Variable	Normal frailty			Stratified		
	EE	EU	EN	EE	EU	EN
u_0	.134***	−.103***	−.064**	.100***	−.075***	−.026
	(.015)	(.018)	(.030)	(.016)	(.018)	(.027)
u_t	−.222***	.169***	.032	−.186***	.150***	.002
	(.018)	(.018)	(.031)	(.017)	(.017)	(.029)
SWAGE	−.251***	−.101**	−.314***	−.653***	.160***	−.080
	(.044)	(.049)	(.071)	(.058)	(.060)	(.083)
AGE	.042***	.007	−.041**	.057***	−.021	−.100***
	(.016)	(.016)	(.024)	(.018)	(.017)	(.029)
SQAGE	−.001***	−.000	.001**	−.001***	.000	.001***
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
UNION	−.274***	.221***	−.043	−.173**	.102**	−.044
	(.055)	(.050)	(.077)	(.060)	(.060)	(.083)
HS	.151**	−.071	−.265***	-	-	
	(.074)	(.068)	(.091)	-	-	
COL	.519***	−0.405***	−.610***	-	-	
	(.102)	(.107)	(.153)	-	-	
NWHITE	−.236***	.225***	.404***	-	-	
	(.039)	(.040)	(.058)	-	-	
Frailty (variance)	.256***	.272***	.517***	-	-	
Occurrence:	8,063	6,657	3,100	7,913	6,542	2,991
# of job spells:	20,741			19,998		
# of individuals:	4,331			3,588		
# of right-censored:	2,921			2,552		

The effects of the explanatory variables can be directly inferred from the estimates of the subhazard regressions. Note, as discussed earlier, that although the coefficient estimates of the cause-specific regressions are not suitable for direct interpretations, the estimates from the subhazard regressions can be used for inferences. The effect of u_0 is positive and statistically significant for EE transitions. The positive sign implies a high unemployment rate at the start of a job spell increases the probability that the worker moves from his current job to another job. By contrast, for the job separations that results in EU transitions, the sign of the coefficient for u_0 is negative, which implies a high unemployment rate at the start of a job reduces the probability that the worker experiences EU transition in the future.

In the aggregate data, the cyclical behavior of the EE and EU flow rates are qualitatively different. Whereas the EE flow rate is strongly procyclical, the EU flow rate is strongly countercyclical. This macro-level observation suggests they also respond to u_t in opposite directions. The negative coefficient for u_t from the subhazard regression for EE transitions indicates the probability that a job spell ends with an EE transition is lower during recessions. By contrast, the coefficient estimate from the subhazard regression for the EU transitions is positive and statistically significant. The positive coefficient implies the probability that a job spell ends with an EU transition is higher during a recession. Both of these estimates are consistent with the cyclical behavior of EE and EU flows. The effect of u_t on EN transitions is similar to EU transitions, although the effects are smaller and insignificant. This result is as expected because transitions to unemployment and out of the labor force are mixed together in this group, and they move in opposite directions over the business cycle.

The results for the effect of u_t are crucial for isolating the effect of u_0, because the duration of a job spell is affected by the current cyclical fluctuations. Bowlus (1995) includes the unemployment rate as an explanatory variable for hazard regressions. However, the effects are ambiguous if u_t has opposite effects on the decisions of workers and firms. In Bowlus (1995), the coefficient estimate for u_t is statistically insignificant when it is added linearly to the model. Bowlus (1995) further adds the squared value of u_t to the right-hand-side variables, and the estimates for the explanatory variables involving u_t are significant in her results. By distinguishing job separations according to their causes, we separately identify the effects of current cyclical fluctuations on the duration of a job spell ending with EE transitions and EU transitions. The opposite signs for different causes support the discussion about the effect of u_t raised in Bowlus (1995).[16]

Although the estimates from the subhazard regressions provide a direct inference on the effects for u_0 and u_t on EE transitions and EU transitions, using these estimates to make inferences about the overall duration of job can be mislead-

[16] Bowlus's (1995) motivation for including the current unemployment rate non-linearly in her analysis is to capture the countercyclicality of firings. Because we make a distinction across job spells according to transition type, we do not need to maintain this non-linearity assumption. Hence, we drop u_t^2 from our analysis.

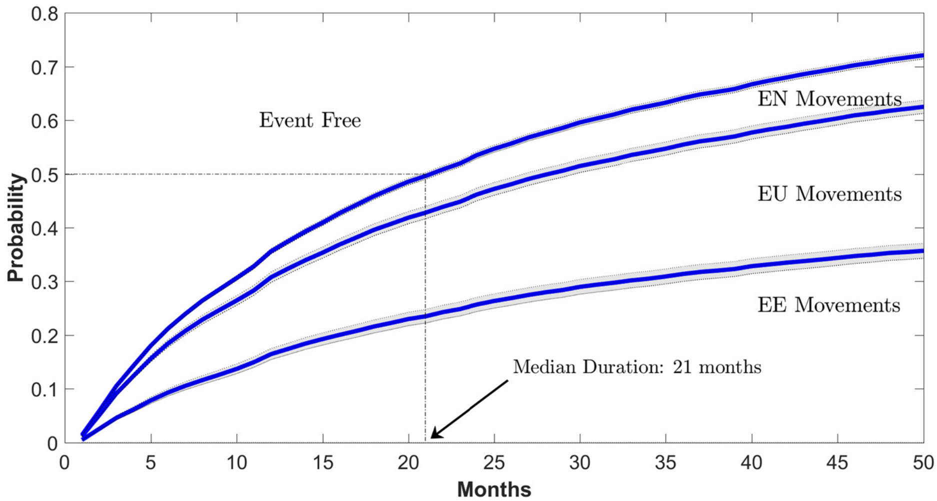

Fig. 1. Cumulative incidence functions for EE, EU and EN movements: the cumulative incidence functions are stacked so that the distance between two curves represents the probabilities of the different events. Shaded areas around each transition type represent 95% confidence intervals.

ing. The subhazard functions for different reasons of job separations are estimated separately, and the probability of job separation can potentially exceed 1 when the value of one of the explanatory variables is changed.

To evaluate the overall behavior of job durations, we use the coefficient estimates from the cause-specific hazard regressions. Note the estimates of the coefficients from the cause-specific regressions alone are not informative about the effects of u_0 and u_t, although the signs agree with the estimates from the subhazard regressions. Note also that, under stratified estimation, we have a separate baseline hazard estimate for each individual in our sample. Therefore, we obtain the cumulative incidence functions for each job-separation category, using the coefficient and baseline hazard estimates from the cause-specific hazard regressions with random frailty. By construction, the probability of job separation is less than 1 at any point in time.

Fig. 1 shows the cumulative incidence functions for each cause-specific job separations. The cumulative incidence functions are drawn for a job in the manufacturing sector that is not protected by a union. This job is held by a 29-year-old high-school graduate white male production worker whose frailty term is equal to 0. His starting hourly wage is 5.68 in 1982 US dollars, which corresponds to the average value for this profile in our sample. The unemployment rate is set equal to the average value of the unemployment rate for the survey years, 6.4%, and is assumed to be equal to this value for all of the time periods from the start of the job. The plots for all transition types are stacked so that the differences show the probability of observing the corresponding cause-specific job separation before time t. The shaded areas around the cumulative incidence functions represent the 95% confidence intervals for each transition type.[17] At any time t, the difference between the sum of cumulative incidence functions and 1 represents the survival probability. From the survival probability, we can calculate that the median duration of a job is around 21 months for this particular job.

Fig. 2 shows the effects of a change in u_0 on the cumulative incidence functions for EE, EU, and EN transitions. In each plot, the solid curve shows the cumulative incidence functions when u_0 is equal to its sample mean for the period under study, 6.4%. The dashed and dotted curves correspond to the cumulative incidence functions when u_0 is one standard deviation, 1.62 percentage points, above or below its sample mean. The current unemployment rate is still kept at its sample mean for all of the remaining time periods.

Fig. 3 shows the change in the cumulative incidence functions for cause-specific job separations after a change in u_t. In each plot, the solid curves show the cumulative incidence functions when u_t is equal to 6.4%. The dashed and dotted curves correspond to the cumulative incidence functions when u_t is permanently one standard deviation above or below its sample mean for all the periods after the job spell has started.

Comparing Figs. 2 and 3, the effect of u_0 on the transition probabilities is quantitatively smaller than the effect of u_t, but of a similar order of magnitude. The effect of aggregate economic conditions (here represented by u_t) on worker flows is well documented and extensively studied. These figures show the effect of u_0 on worker flows is also quantitatively important.

[17] Rosthøj et al. (2004) describe the procedure for calculating standard errors for cumulative incidence functions. We adapted their procedure to account for frailty terms present in our setup. See our online Appendix A for the details.

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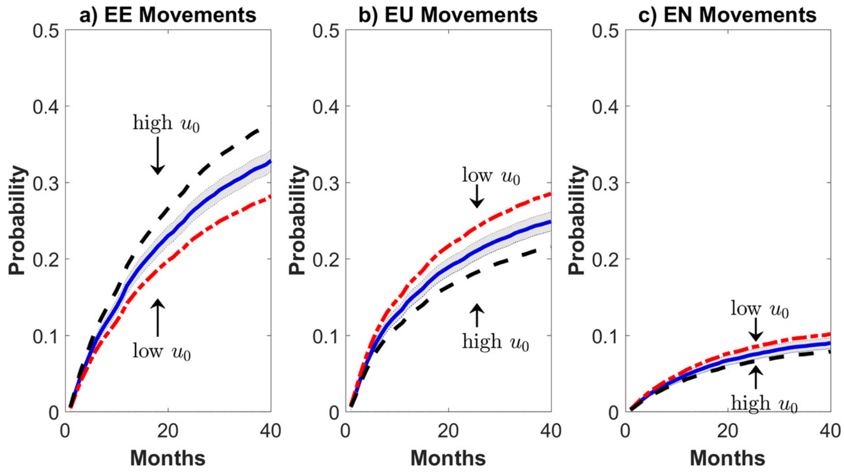

Fig. 2. Changes in cumulative incidence functions in response to a change in u_0. Shaded areas around each transition type represent 95% confidence intervals.

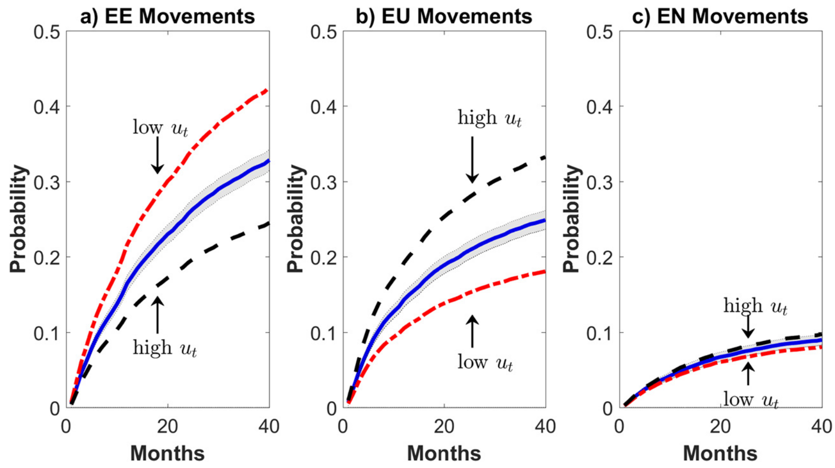

Fig. 3. Changes in cumulative incidence functions in response to a change in u_t. Shaded areas around each transition type represent 95% confidence intervals.

4.3. Cox regression without competing risks

In this section, we pool all the separations together and run a standard Cox regression to highlight the importance of the competing-risks structure. This analysis is akin to Bowlus (1995) with the differences in the right-hand-side variables, the sample restrictions, and the methods used for controlling unobserved heterogeneity.

Our results are in Table 3. The first column shows our estimation results from the Cox regression under normal frailty with the same regressors in the main analysis. The second column adds the squared current unemployment rate, u_t^2, as in Bowlus (1995), and the third column drops the starting wages. Columns IV through VI repeat columns I through III, respectively, under stratification over the individuals. Overall, neither u_0 nor u_t terms have a significant effect on job duration once wages are controlled for. These results have two important implications. First, columns II and III and columns V and VI echo the findings in Bowlus (1995): u_0 has a positive effect on the separation hazard rate (although the magnitude is substantially smaller than hers), but this effect disappears once the starting wages are controlled for. Her interpretation of this result is that any cyclical fluctuation in match quality is internalized by the wage-bargaining process. However, under competing-risk structure, the starting unemployment rate has a significant effect on both EE and EU transitions even after controlling for the starting wage. One interpretation of this result is that non-pecuniary aspects of the jobs, not reflected in the starting wages, exist: this result further highlights the advantage of our approach over directly measuring productivity and wages from matched employer-employee data. Second, we know from macro-labor literature that the opposite cyclical patterns of EE and EU transitions largely cancel each other out. This observation is reflected as a zero coefficient in front of the current unemployment rate in column I of Table 3. We also find the unemployment rate at the start of the job spell has no effect once all separation types are mixed together. These findings suggest that pooling all the regression types to-

Table 3
Cox hazard estimation without competing risk: these estimations use data from all the survey years. SWAGE = natural logarithm of real starting wages; SQAGE = age squared; UNION = 1 if the job is covered under a union contract or collective bargaining agreement; HS = 1 if the respondent is a high-school graduate; COL = 1 if he completed 16 or more years of education; NWHITE = 1 if the respondent is black or Hispanic. Standard errors are given in parentheses. Coefficient estimates for industry and occupation variables are not reported. Significance of the variance of the frailty terms are based on a likelihood-ratio test comparing the model with and without frailty terms. For stratified regressions, standard errors are clustered over individuals. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Variable	Normal frailty			Stratified		
	I	II	III	IV	V	VI
u_0	.001	.001	.015**	.009	.009	.026**
	(.007)	(.007)	(.006)	(.011)	(.011)	(.010)
u_t	.000	.000	.015	−.020*	−.005	.020
	(.006)	(.037)	(.037)	(.011)	(.056)	(.055)
u_t^2	-	−.000	−.001	-	.001	−.003
	-	(.003)	(.003)	-	(.004)	(.004)
SWAGE	−.585***	−.585***	-	−.665***	−.665***	-
	(.022)	(.064)	-	(.047)	(.047)	-
AGE	−.034***	−.034***	−.039***	−.066***	−.066***	−.074***
	(.008)	(.092)	(.008)	(.129)	(.013)	(.013)
SQAGE	.000	.000	.000	.001***	.001***	.001***
	(.000)	(.002)	(.000)	(.000)	(.000)	(.000)
UNION	−.106***	−.106***	−.228***	−.041	−.041	−.157***
	(.027)	(.055)	(.026)	(.044)	(.044)	(.043)
HS	−.102***	−.103***	−.156***	-	-	-
	(.026)	(.055)	(.026)	-	-	-
COL	−.032	−0.032	−.299***	-	-	-
	(.039)	(.124)	(.038)	-	-	-
NWHITE	.076***	.076***	.151***	-	-	-
	(.049)	(.050)	(.020)	-	-	-
Frailty (variance)	.264***	.264***	.255***	-	-	-
Occurrence:	17,820			8,306		
# of individuals:	4,331			2,206		
# of right-censored:	2,921			1,541		

gether masks not only the cyclicality of the different types of separation rates, but also its dependence on the labor-market conditions on the onset of the job spell.

4.4. Robustness analysis

We performed several robustness checks to examine our main results. We discuss each of them briefly in this section. All the details about these regressions are deferred to our online Appendix B.[18]

NLSY data provide detailed information about the reason a job spell ended. This variable allows us to define an alternative classification for competing risks. In particular, we observe whether the job ended due to the worker's quit decision or due to the firm's decision, such as firing or plant closure. Using this piece of information, we define three categories for the reasons for job separations: quits, firings, and other reasons. At the macro level, it is well known that business-cycle properties of quits and firings are similar to EE and EU transitions, respectively. We investigate whether they are also similar with respect to their responses to the aggregate labor-market conditions at the start of the job spell. We find the results are very similar, with quits corresponding to EE transitions and firings corresponding to EU transitions.

The literature has documented that labor market dynamism, represented by various labor-market flow variables, has been declining in the US in recent years.[19] These trends may have an impact on job duration. Hyatt and Spletzer (2016) show the share of long-term jobs has been increasing in the US since around 2000. Although they show most of this trend can be explained by the aging population and other observables, time effects may be present that our analysis above does not capture. More specifically, because our sample comes from a cohort study, the age-related variables implicitly capture the time effects. To evaluate this point, we repeat our main analysis using a shorter panel by dropping the job spells that started before 1988, which also corresponds to the final survey year in Bowlus (1995). Overall, our main results for EE and EU transitions are robust to this restriction. One interesting finding is that the distinction between EU and EN transitions become clearer in our short panel regressions in that they respond to u_t (and also u_0) in opposite directions. Based on this result, we argue that the EN category in our short panel includes relatively more transitions out of labor force compared to our long panel, because the individuals are younger in the short panel and therefore are loosely attached to the labor force. Our results are broadly consistent with the findings in Fujita and Ramey (2006).

[18] The URL address for the online Appendix is: https://sites.google.com/site/toshimukoyama/BM_online_appendix.pdf.
[19] See Decker et al. (2014).

In frailty regressions, we make two strong assumptions about the frailty terms: they are drawn independently from their respective normal distributions. Based on the results in Heckman and Honoré (1989), both the normality and independence assumptions can be relaxed. We do so by estimating the probability mass function defined over a discrete grid for the frailty terms along with other coefficients. Note that each grid point consists of one frailty term for each of the three competing risks. We find the estimated marginal distribution for each frailty is left skewed. We also find a positive correlation between the three frailty terms. In other words, individuals with short job spells tend to have high frailty terms for all types of separations. Nonetheless, the coefficient estimates for u_0 and u_t are similar to our findings in the main analysis.

We repeat our analysis above with the confidential geocode data from NLSY 1979. In this extension, we replace national unemployment rates with state-level unemployment rates and include the state-level unemployment insurance replacement ratio in the regressors. Our results are robust to this extension too, although u_0 effects are somewhat smaller for EE and EU transitions.

In addition to industry fixed effects, we also explore whether the cyclical effects are concentrated in certain industries. To this aim, we broadly divide industries into two groups—goods-producing and service-providing industries—and include their interaction with u_0 and u_t in our regressions. We find the effect of u_0 on EU transitions is negative for both industries, but it is much smaller for the goods-producing industries, and is sometimes insignificant, depending on the specification of the estimating model.

5. Model

In this section, we build a simple model to interpret the empirical results in Section 4. In particular, we consider the u_0 and u_t coefficients for EE and EU transitions in Table 1 as the baseline empirical result against which we compare the model outcome.

5.1. Setup

The model setup is a simple extension of the standard job-ladder model.[20] An infinitely-lived worker is either employed or unemployed. We assume his utility is linear and he consumes what he receives each period.

The flow utility of an unemployed worker is assumed to be a constant b. The flow utility of an employed worker has an aggregate and an idiosyncratic component. The aggregate component, z, is stochastic and common across agents. It follows a Markov process $F(z'|z)$, where $'$ (prime) represents the next-period value. The idiosyncratic component, x, is match-specific. It is determined when the match is formed, and stays constant until separation. In sum, each employed worker receives $z+x$. The motivation for including the idiosyncratic component, x, is to capture the heterogeneity of matches (which affects both EE movement and EU movement) at the individual level. Given that the empirical patterns we focus on are still present even after controlling for wages, the idiosyncratic component x is best interpreted as nonwage job attributes, such as the amenity of the job. In what follows, we abstract from worker-specific (or job-specific) components in wages, such as the ones based on education, and assume that z is the common wage of employed workers. We call the nonwage component x as the *match quality* of a job.

Each match also has another dimension of heterogeneity. Matches can have a different likelihood of exogenous separation.[21] We call the separation likelihood the *type* of match and denote it by $s \in \{1, 2\}$. We assume that a type-2 match is more stable (has a smaller exogenous separation probability) than a type-1 match. Similar to the match quality, the match type is also determined when the match is formed, and stays constant until separation. In addition to the exogenous separation, the worker can separate from a job voluntarily.

For an employed worker, labor-market frictions are formulated as two stochastic events. First, he may receive a job offer from another employer that he can choose whether to accept. The probability of this shock is $\lambda_e(z') \in [0, 1]$. Note this probability is a function of the next-period aggregate state z', reflecting the cyclicality of labor demand. The match quality and the type of this new job is randomly drawn at the time of the job offer. After seeing the match quality and the type, the worker chooses whether to stay in the same job, move to the new job, or move to unemployment. The second shock is an exogenous separation shock with probability $\delta(s, z') \in [0, 1]$ that forces him to move to unemployment. As we described above, we assume $\delta(2, z') \le \delta(1, z')$. We also assume $\lambda_e(z') + \delta(s, z') \le 1$ for all s and z'.

From these assumptions, an employed worker's Bellman equation can be written as

$$\begin{aligned} W(s, x, z) =& z + x + \beta E_{s',x',z'}[\lambda_e(z') \max\{W(s', x', z'), W(s, x, z'), U(z')\} \\ &+ (1 - \lambda_e(z') - \delta(s, z')) \max\{W(s, x, z'), U(z')\} + \delta(s, z') U(z')]. \end{aligned}$$

Here, $W(s, x, z)$ is the value function of an employed worker with type-s match with amenity x, $U(z)$ is the value function of an unemployed worker, and β is the discount factor. The operator $E_{s',x',z'}[\cdot]$ takes the expected value with regard to s', x', and z'. We assume that conditional on a job offer, a type-s job arrives with the probability $\nu(s)$.

[20] Moscarini and Postel-Vinay (2016) use a similar model in their analysis of the Great Recession.

[21] This type of heterogeneity has received special attention in the recent literature. See Jarosch (2015), for example. Mukoyama (2019) analyzes a Diamond-Mortensen-Pissarides-style model with endogenous job creation with such heterogeneity of jobs.

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An unemployed worker receives a job offer with probability $\lambda_u(z') \in [0, 1]$. While unemployed, he receives a flow value of b, which can be interpreted as the combination of his unemployment-insurance benefit, home production, and the value of leisure. After observing the match quality of the offer, he chooses whether to take that job. An unemployed worker's Bellman equation is therefore

$$U(z) = b + \beta E_{s',x',z'} \left[\lambda_u(z') \max\{W(s', x', z'), U(z')\} + (1 - \lambda_u(z'))U(z') \right].$$

We assume that conditional on a job offer, a type-s job arrives with the probability $\nu(s)$.

5.2. Baseline calibration

One period is set as one month. The discount factor is set at $\beta = 0.99^{\frac{1}{3}}$, as in Gertler and Trigari (2009). The stochastic process for z, $F(z'|z)$, approximates an AR(1) process:

$$\log(z_{t+1}) = \rho_z \log(z_t) + \epsilon_t,$$

where $\epsilon_t \sim N(0, \sigma_z^2)$. We set $\rho_z = 0.95^{\frac{1}{3}}$, again following Gertler and Trigari (2009). The standard deviation of z, σ_z, is set so that the quarterly value corresponds to 0.005, which is on the conservative side of the wage-cyclicality estimates in empirical studies. The approximation procedure by Chang and Kim (2006) yields $\sigma_z = 0.005$. Given that we compare the model results with the results in Table 1, where we control for the starting wages, we also repeat the exercises below without the fluctuations in z in Appendix D. The results are essentially the same.

We assume the match-quality process to follow a log-normal distribution with mean 0: $\log(x) \sim N(0, \sigma_x^2)$. Given that we interpret x as a nonwage component of the utility from an individual job, it is not straightforward to tie σ_x^2 to observable statistics. For the wage component, Tjaden and Wellschmied (2014) estimate the average wage gain upon job-to-job transition to be 3.3%. We assume the nonwage gain upon job-to-job transition is of similar magnitude as the wage gain, and target the average 3.3% gain in x upon job switching. This yields $\sigma_x = 0.0395$.

The types are drawn when a new job offer is made. We assume the probability of receiving the type-1 draw, denoted $\nu(1)$, is 0.5. Because we have only two types, type 1 and type 2, $\nu(2)$ is also 0.5. Following Hall and Milgrom (2008), we choose b so that the income the unemployed receives is 71% of the average value of z. Adding the mean value of x, we set $b = 1.71$.

The labor-market frictions, $\lambda_u(z)$, $\lambda_e(z)$, and $\delta(s, z)$, are calibrated using labor-market flows. First, we assume $\lambda_e(z)$ is proportional to $\lambda_u(z)$; $\lambda_e(z) = \alpha\lambda_u(z)$.[22] Krusell et al. (2017) calculate the average job-to-job flow rate as 2.2% per month. We assume half of this movement is driven by the nonwage component, and thus target 1.1% job-to-job transition rate per month. This yields $\alpha = 0.19$.

We assume $\lambda_u(z)$ takes the following form:

$$\lambda_u(z) = \bar{\lambda}_u + \phi_\lambda \log(z).$$

The average value of λ_u, $\bar{\lambda}_u$, is set so that the gross flow from unemployment to employment in the steady-state model matches the corresponding value in the data. Krusell et al. (2017) calculate this flow rate to be 0.228 in monthly frequency.[23] This target results in the parameter value $\bar{\lambda}_u = 0.255$. The parameter governing the fluctuations, ϕ_λ, is set so that the fluctuations of the corresponding flow rate in the model matches the data. The standard deviation of (quarterly averaged, logged, and HP-filtered with parameter value 1600) the flow rate from Krusell et al. (2017) is 0.088, and the model matches this number with $\phi_\lambda = 2.53$.

The separation probability in the model takes the form

$$\delta(s, z) = \bar{\delta}(s) - \phi_\delta \log(z).$$

The average values for each type, $\bar{\delta}(1)$, and $\bar{\delta}(2)$, are set at 0.021 and 0.011. The aggregate gross flow rate from employment to unemployment is 0.014 per month, matching the empirical value from Krusell et al. (2017). In setting ϕ_δ, we target the standard deviation of the EU flow rate (quarterly averaged, logged, and HP-filtered with parameter value 1600), which is 0.089. The resulting ϕ_δ is 0.123. Table 4 summarizes the calibration.

For the model computation, the Bellman equations are computed on discretized grids.[24] The stochastic processes are approximated with Markov chains using Tauchen's (1986) method. The value functions are computed by the standard value-function iteration. Once the solution is found, we simulate the model to generate the data to be used in the next section.

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[22] Shimer (2005), Mukoyama (2014), and Moscarini and Postel-Vinay (2016) employ a similar assumption. This property comes out naturally from a search and matching model in which employed and unemployed workers compete for the same set of vacancies, but they differ in the efficiency of the search.

[23] The time span Krusell et al. (2017) analyze is from 1978 to 2012, which is almost identical to our data period, although they use a different dataset (the Current Population Survey) from us.

[24] We put 11 grid points in $\log(z)$ dimension and 51 grid points on $\log(x)$ dimension. The upper and lower bounds of the $\log(z)$ grid are set at plus and minus two standard deviations of the unconditional distribution of $\log(z)$. The upper and lower bounds of the $\log(x)$ grid are set at plus and minus five standard deviations of the $\log(x)$ draw.

Table 4
Calibration.

Parameter	Description	Value
β	Discount factor	$0.99^{\frac{1}{3}}$
b	Value of unemployment	1.71
ρ_z	Persistence of aggregate shocks	$0.95^{\frac{1}{3}}$
σ_z	Dispersion of aggregate shocks	0.004
σ_x	Dispersion of match quality	0.0395
α	$\lambda_e(z)$ relative to $\lambda_u(z)$	0.19
$\nu(1)$	Probability of type 1 job offer	0.5
$\bar{\lambda}_u$	Average value of λ_u	0.255
$\bar{\delta}(1)$	Average value of δ for type 1	0.021
$\bar{\delta}(2)$	Average value of δ for type 2	0.011
ϕ_λ	Fluctuation parameter of λ_u	2.53
ϕ_δ	Fluctuation parameter of δ	0.123

Table 5
Cause-specific hazard estimation results.

Variable	EE	EU
u_0	.046	−.009
u_t	−.074	.105

6. Results

In this section, we present the results for the baseline calibration. We find the model calibrated above can match the qualitative features of the separation hazards. We then run counterfactual experiments to investigate the mechanism that gives rise to the particular cyclical pattern of hazards.

6.1. Baseline results

The model implies the steady-state value of unemployment at 5.9%. The standard deviations of the employment and unemployment rate are 0.009 and 0.137. The corresponding empirical values are 0.010 and 0.117 in the data (Krusell et al., 2017).[25]

To evaluate the implications of the model for different hazard rates, we run cause-specific Cox regressions to model-generated data with u_0 and u_t as explanatory variables. We note a few practical points. First, we generate a long time series of the aggregate shock, z, and create monthly employment histories for a large number of individuals starting from the steady-state distribution. Owing to the large sample size, the regressions yield model-implied limits of the coefficient estimates with very small standard errors.[26] Therefore, we do not report standard errors in the tables below. Second, the transition rates in the model contain no individual-level heterogeneity. Jobs with identical characteristics, that is, the same x, s, and z, face the same EE and EU hazard rates. Therefore, we run standard Cox regressions without any specification for unobserved heterogeneity. Finally, extension with Kaplan-Meier weighting makes the dataset for subhazard regressions extremely large. Due to its impracticality, we skip them in this section.

Table 5 describes the cause-specific Cox hazard regressions. The results are qualitatively consistent with the empirical results in Section 4.2, in particular, Table 1. The coefficients on u_0 are positive for the jobs that end with EE transition, and negative for the jobs that end with EU. The u_t coefficients are negative for EE and positive for EU. Quantitatively, all coefficients except for the u_0 coefficient for EU sample are comparable to the ones in Table 1. We will revisit the quantitative fit in the next section.

6.2. Inspecting the mechanism

To investigate the background economic mechanism behind the results in Table 5, we run counterfactual experiments by allowing only one variable to fluctuate as in the baseline case. Four parameters fluctuate in the baseline calibration: z, λ_u, λ_e, and δ. Table 6 shows the results. The second to the fifth rows corresponds to the experiment that allows only one of the parameters to fluctuate. The first row repeats the baseline results for comparison. Our main focus is the coefficients on u_0.

The positive coefficient on the jobs that end with EE is the most strongly influenced by the fluctuations of λ_e. The intuition is that, because a high λ_e in booms improves the distribution of x and s among the employed, the average values

[25] For all summary statistics here and below, the variables are quarterly averaged, logged, and HP-filtered with parameter value 1600.

[26] To alleviate the impact of the initial distribution, we dropped the job spells within the first 1,000 periods. The resulting sample has more than half a million job spells.

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Table 6
Hazard estimation results: counterfactuals.

Experiment	u_0 coeff. for EE	u_0 coeff. for EU	u_t coeff. for EE	u_t coeff. for EU
Baseline	.046	−.009	−.074	.105
z only	−.028	−.033	−.015	.068
λ_u only	.027	−.011	.046	.014
λ_e only	.060	.004	−.059	.051
δ only	.033	−.033	.020	.229

of x and s tend to be better in booms. The improved distribution in booms imply the matches formed via job-to-job transition during booms are better in terms of x and s than during recessions, because a new job has to be better than the current job for the worker to switch. Therefore, these jobs that are accepted during booms (with low u) are less likely to separate into even better matches (through EE transition) in the future—finding jobs that are even better is difficult. Through the EE transition, the composition of the current matches has an important influence on the characteristics of new matches.

The negative coefficient on the jobs that end with EU is most strongly affected by the fluctuations in z and δ. To see the intuition, first note that the matches that separates into unemployment tend to have very low x (and unstable s). A high z in booms implies that a worker accepts a job offer even when these jobs are not very attractive in terms of x and s. Thus, the jobs at the bottom tail of the x distribution and of the unstable type are accepted more in booms. In other words, the marginally accepted matches are worse in booms, and these tend to be separated more quickly. A similar logic works with δ. Because δ is low during booms, even jobs with the unstable type (in terms of s) are sufficiently attractive to be accepted. The role of δ is particularly important, because z can be interpreted as the wage factor, and the empirical patterns are robust to controlling for the starting wage.[27]

7. Cyclical job-offer distribution

One shortcoming of the baseline calibration is that the u_0 coefficient for the jobs that separates into EU has a quantitatively small magnitude (in terms of absolute value). This section shows the model can be quantitatively consistent with the empirical results if the job-offer distribution changes over the business cycle.

We make two modifications to the assumptions on the job-offer distribution. First, the distribution of the match quality x is modified to $\log(x) \sim N(\mu_x, \sigma_x^2)$, allowing the average to be nonzero. In addition, we allow μ_x to depend on z in the form

$$\mu_x(z) = \phi_x \log(z).$$

We calibrate z as in the baseline calibration. Our specification of μ_x here implies that the mean of $\log(x)$ is tied to the value of (the aggregate component of) the wage z. Note that here we introduce the fluctuations in μ_x to match the cyclical changes in the workers' mobility decisions, and not the cyclical changes of the wage-offer distribution. The fluctuations in μ_x here capture the variation in the (unobserved) return to job offers that is not explained by the (observed) fluctuations in the aggregate component of the wages. From this viewpoint, although an alternative interpretation of x can be the sum of the idiosyncratic component of wages and idiosyncratic amenities, our identification here is still valid under this alternative interpretation, because here we do not attempt to directly match the cyclical changes in the wage-offer distribution. Second, we allow the fraction of type s, $\nu(s)$, to be cyclical. In particular, we assume the fraction of a new type-2 match takes the form

$$\nu(2, z) = 0.5 + \phi_\nu \log(z).$$

We set the values of ϕ_x and ϕ_ν so that both coefficients are quantitatively similar in magnitude to the empirical values in Table 1. The results are in the final row of Table 7.[28] There, we set $\phi_x = 0.22$ and $\phi_\nu = -7.90$. These values imply that, on average, higher match-quality jobs are offered in booms, whereas more unstable jobs are offered at the same time.[29]

The second and third rows of Table 7 evaluate the effects of $\phi_x = 0.22$ and $\phi_\nu = -7.90$ separately. A procyclical μ_x enhances the magnitude of the u_0 coefficient on the jobs separated as EE. The reason is that the procyclical μ_x improves the distribution of x among existing matches. As we discuss in Section 6.2, this improvement in the distribution makes new job offers that are accepted during booms difficult to improve further. A countercyclical ν reduces the magnitude of the u_0 coefficients on the jobs separated as EE and EU. This result is intuitive—because the matches formed in booms tend to be of the unstable type, they are likely to separate in future.

[27] We repeat the exercises below without the fluctuations in z in Appendix D. There, indeed, δ is the single most important factor.

[28] The rest of the parameters are set at the same value as in the baseline calibration. The results are essentially the same if we recalibrate ϕ_λ and ϕ_δ so that the standard deviations of the UE flow rate and the EU flow rate match the data values.

[29] Note that, these correlations are about the *offered* jobs. As we see below, the correlations are the same for the *accepted* jobs.

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Table 7
Hazard estimation results: extended model.

Experiment	u_0 coeff. for EE	u_0 coeff. for EU	u_t coeff. for EE	u_t coeff. for EU
Baseline	.046	−.009	−.074	.105
Cyclical μ_x	.099	−.007	−.123	.096
Cyclical ν	.004	−.053	−.052	.100
Both μ_x and ν	.059	−.047	−.106	.097

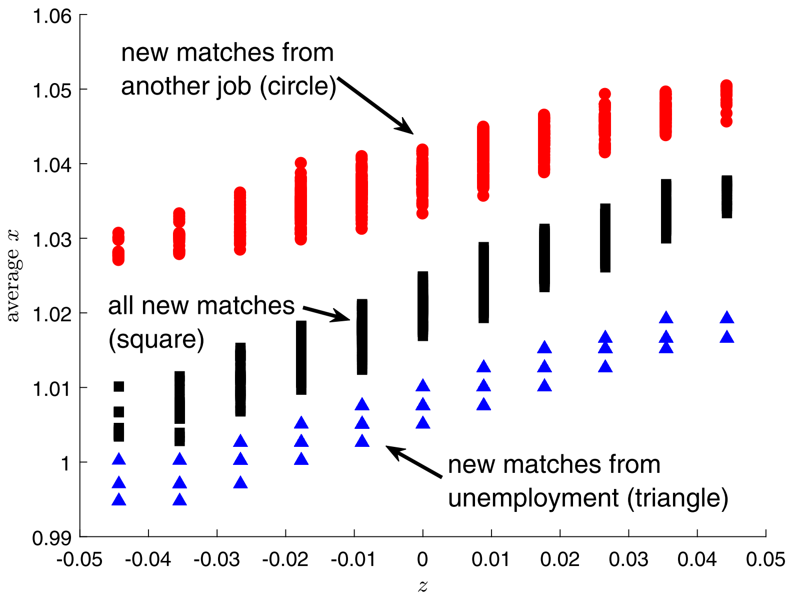

Fig. 4. Scatter plots of average x against z, for all new matches, the matches from another job, and the matches from unemployment.

The result here presents a nuanced answer to the question, “Do matches formed in booms tend to be better?” The answer is yes and no—yes, in the sense that the match quality x tends to be better, and no, in the sense that the matches tend to be less stable in terms of s.[30] The same relationship is true for the jobs that are actually accepted. Fig. 4 plots the values of average x among the new matches, from the model simulation. We also plot the averages for the matches from another job and matches from unemployment. The correlations are positive, showing the jobs that are newly accepted in booms are of higher quality, both for the workers that are from another job and for the workers that come from unemployment. Fig. 5 plots the simulated values of average s among the new matches. In contrast to Fig. 4, the correlations are negative, indicating that the jobs that are newly accepted in booms are less stable. Again, this correlation holds for both for the workers that are from another job and for the workers that come from unemployment.

8. Conclusion

In this paper, we empirically examined the effects of labor-market conditions on job duration. Using data from the NLSY 1979 cohort, we estimated a proportional hazard model under the assumption that different causes of job separations are competing risks. Distinguishing between different types of separations is the main contribution of this paper because it allowed us to test separately for *both* of these opposing forces rather than estimating their *net* effect on the duration of job.

Two functions, the cause-specific hazard function and the subhazard function, have been widely used in the literature to estimate hazard models in the presence of competing risks. We applied both functions in this paper, and they produced results that are consistent with each other. We found that an increase in the unemployment rate at the start of an employment relation increases the probability that a separation with job-to-job transition occurs, but it reduces the probability that it ends with the worker moving into unemployment. Our results are robust to several alternative specifications.

[30] In many macroeconomic contexts, whether the matches formed in booms are better has important aggregate consequences. For example, the models by Moscarini (2001) and Barlevy (2002) feature procyclical match quality, and this result has important implications on the aggregate labor-market outcome. These models consider on-the-job search and share some features with our job-ladder model, although our model is substantially simpler (and our model has an additional dimension of heterogeneity in job stability). In Costain and Reiter (2008), this relationship between match quality and business cycles plays an important role in explaining labor-market volatility.

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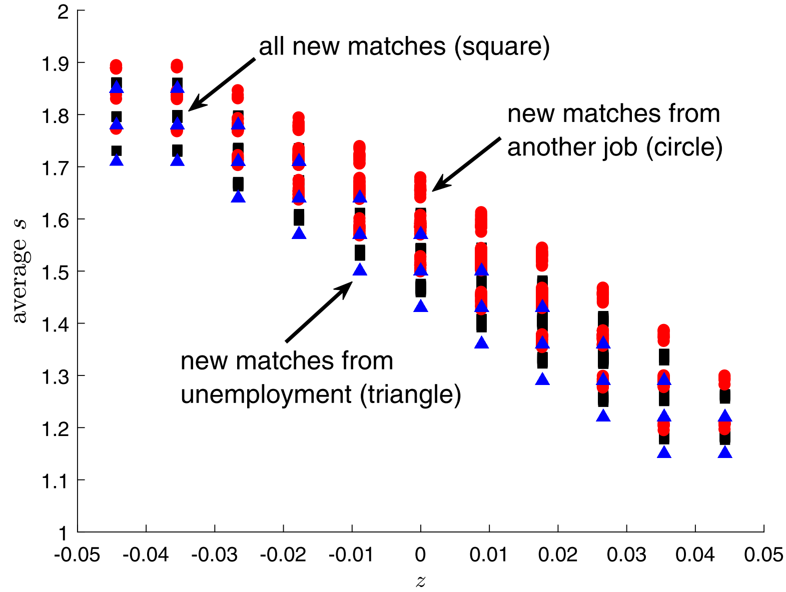

Fig. 5. Scatter plots of average s against z, for all new matches, the matches from another job, and the matches from unemployment.

To interpret the empirical findings, we constructed a simple job-ladder model with two-dimensional job characteristics. We assume matches are different in match quality (i.e., nonwage utility that a job provides to the worker) and stability (i.e., the probability of exogenous separation in the future). Our model is qualitatively in line with the empirical results from the US data. We ran counterfactual experiments to investigate the mechanism behind the correlations between the initial labor-market condition and subsequent separations. We found the job-to-job transition plays an important role in generating the relationship between the initial labor-market condition and the separations due to job-to-job transitions. In booms, the existing jobs are already well matched, and the jobs that are created by job-to-job transitions also have to be good matches. For the jobs that separate into unemployment, the behavior of the marginally accepted matches is important.

By allowing the characteristics of the offered jobs to be cyclical, we were able to fit the model to the data quantitatively. The important properties are that the jobs that are offered (and accepted) in booms tend to have high match quality but low stability. Examining the cause of the cyclicality of the offered job characteristics is an important future research topic.

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Appendix A. Details of data construction and summary statistics

We use the Employer History Roster provided by the NLSY 1979. This roster contains information on up to 65 jobs every individual holds. We retrieved the job start and end dates, the employment status, and the reason for job separation directly from this roster. Class of worker, union status, industry and occupation, usual wage, and hours are spread out across a roster created separately for each survey year. We create these variables by linking the rosters for each survey year. We use the job start and end dates to calculate the duration of the job and the information on the employment status to determine the censored observations.

Starting from 1980, each survey asked respondents about their school enrollment for each month from the date of the last interview until the date of the current interview. We use this information to determine the last month a respondent was enrolled in school. For those respondents who did not attend school after 1980, we use the information in the first survey in 1979 about the last date a respondent attended school. If the year of the last school enrollment is missing, we drop this individual from our sample. If only the month of the last school enrollment is missing, we insert May as the

Table 8
Description and means for our samples: these samples are used in the competing-risk regressions in the main analysis with frailty specification.

Variable	Explanation	Mean
Job spells		
u_0	Unemployment rate at the start of the job	6.497
SWAGE	(log) Real hourly starting wage	1.848
AGE	Age of the respondent at the start of the job	29.889
UNION	= 1 if covered by union	0.120
Individuals		
HS	= 1 if obtained high school diploma or equivalent	0.658
COL	= 1 if completed 16 years of schooling	0.175
NWHITE	= 1 if black or Hispanic	0.432
# of job spells:		20,741
# of right-censored:		2,921
Median duration:		14 months
# of individuals:		4,331

graduation month, which is the modal month in our sample. For our main analysis, we drop job spells that start before all schooling is completed.

To determine EE, EU, and EN transitions for a job spell, we compare the stop date of that job spell with the start date of other job spells. If the difference between the stop and the start dates is less than 30 days and the job did not end with a firing, we label this job EE. In some cases, multiple jobs overlap. We labeled these cases EE if the respondent already had another job at the time the job ended. For the remaining jobs, we check the employment status in the week immediately following the job end date from the weekly status array. We classify the job as EU if the respondent is recorded as unemployed. We labeled all the other job spells EN.

The sample does not include jobs lasting less than a month. We focus on full-time work in the private sector and drop the job spells with less than 35 weekly hours worked or reported as self-employed or in the public sector. We exclude females from our sample because they are likely to quit for reasons other than poor match quality, such as marriage, pregnancy, and childcare. We also focus on the civilian population and drop individuals in the military subsample. For every regression, sampling weights are scaled so that the average weight of the job spells is equal to 1. Finally, we define the first reported hourly wage as the starting hourly wage and drop the job spell if this value is below the federal minimum wage in the month the job spell started. We use the Consumer Price Index (CPI) series to convert this variable to real starting hourly wage. In a few cases, the calculated wage rate was astronomically high, for example, 24,000 US dollars per hour. To address the potential measurement errors, we dropped the job spells that paid wages more than 120 US dollars.

Table 8 shows summary statistics from our sample used in the competing-risk regressions with frailty specification after imposing these restrictions above. We note the median job duration is 14 months in our final sample. Another data source for job duration in the US is the Longitudinal Employer-Household Database (LEHD), which is a matched employer-employee dataset covering 98% of all the jobs in the US. Using this dataset, Hyatt and Spletzer (2017) study the behavior of single-quarter jobs, which they define as jobs lasting less than three months. They find the incidence of the single-quarter jobs was 8.3% in 1998. The corresponding number in our sample is 9.9%. Although the job durations are slightly shorter in our sample, we note several differences between their measure and ours. First, our calculations include right-censored job spells that potentially last longer but are lost due to follow-up. Because LEHD is constructed from administrative data, sample attrition is not an issue. Second, our sample consists primarily of job spells that started when the individuals were relatively young. Hyatt and Spletzer (2017) further report the incidence of single-quarter jobs declines with age except for those around retirement age. Finally, our sample excludes women who tend to have longer job spells according to the same study.

For a better comparison, we include the job spells from women in our sample and calculate the incidence of single-quarter jobs for each age group. To match their age categories, we also add job spells from younger ages. Our results are presented in Table 9. To address the right-censoring issue in our sample, we calculate two measures. The first measure, shown in the first column, counts only the jobs that are completed within three months as single-quarter jobs. This number yields the lower bound for the incidence of single-quarter jobs. The second measure, shown in the second column, adds to this measure the job spells that are right-censored within the first three months. Because some of these job spells potentially lasted more than three months, this measure gives the upper bound for the incidence of single quarter jobs. The last column is copied from Table 1a in Hyatt and Spletzer (2017), which is based on the data from LEHD between 1996 and 2012.[31] With these modifications, our calculations accord with theirs quite well with one exception. From age 35 and onward, our sample has systematically longer job spells. The discrepancy might stem from the fact that the incidence of

[31] We exclude the last age category because our sample does not have any job spell recorded after age 65.

Table 9
Comparison of our sample to LEHD: this table shows the incidence of single-quarter jobs (in percentages) from our sample and Hyatt and Spletzer (2017).

Age groups	Lower bound	Upper bound	LEHD
14-18	13.3	26.0	15.4
19-21	11.1	16.3	13.1
22-24	8.4	12.2	9.8
25-34	7.4	9.1	7.1
35-44	4.0	5.3	5.5
45-54	2.4	3.1	4.4
54-64	0.4	1.2	4.0

Table 10
Occupation and industry classifications: these classifications are based on OCC1990 and IND1990 variables from the IPUMS-CPS database, respectively. The second column indicates the number of observations in each category in our final sample.

Category	Observations
Occupation classes	
Managerial and professional	2,473
Technical, sales, and administrative	2,692
Service	2,397
Farming, forestry, and fishing	787
Precision production, craft, and repairers	4,769
Operatives and laborers	7,623
Industry classes	
Agriculture, forestry, and fisheries	789
Mining	274
Construction	3,822
Manufacturing	4,679
Transportation, communication, and other public utilities	1,852
Wholesale trade	887
Retail trade	3,459
Finance, insurance, and real estate	716
Business and repair services	2,233
Personal services	504
Entertainment and recreation services	292
Professional and related services	1,193
Public administration	41

single-quarter jobs falls steadily starting around 2000. This date roughly corresponds to the job spells when the individuals in our sample are 36 years old and over.

The expected job duration for the median individual is around 26 months. This number is naturally greater than the median job duration, because individuals who change jobs frequently have shorter job spells and contributes disproportionately more observations to our sample.

Appendix B. Occupation and industry classification

We use OCC1990 and IND1990 variables available at the IPUMS-CPS database to have a comparable occupation and industry classifications throughout our analysis. We further group these occupation and industry codes into the broad categories presented in Table 10. Our occupation and industry dummy variables are based on these categories. The second column indicates the number of observations in our final sample.

Appendix C. List of job-separation reasons

Employer History Roster in the NLSY 1979 provides the following reasons for job separations. These job categories are not consistently available in all the survey years. We indicate their availability below. We categorize these reasons as [Q]-quit, [F]-firing, and [O]-other reasons. We re-label the EE transitions as EU if they ended due to one of these reasons in the firing category. We use these categories in the online Appendix, where we repeat our analysis with competing risks defined by reason.[32]

[32] Note that some of the categories are labeled either as quit or other reasons. Our main quit category in the analysis in the online Appendix is quit to take another job, but this category is available only after 1990. For the other quit categories, we require the individual to have had a job lined up to be classified in the quit category. Otherwise, we classify this spell under the other reasons category. We also exclude reasons 27 through 30 because they are related to self-employment.

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Table 11
Hazard estimation results.

Variable	EE	EU
u_0	.057	−.007
u_t	−.083	.092

1. Layoff, plant closed, or end of temporary or seasonal job [F]: 1983 and before.
2. Layoff, job eliminated [F]: 2002 and onwards.
3. Layoff [F]: 1984 and onwards.
4. Plant closed [F]: 1984 and onwards.
5. Company, office or workplace closed [F]: 2002 and onwards.
6. End of temporary or seasonal job [F]: 1984 and onwards.
7. Discharged or fired [F]: All survey years.
8. Program ended [O]: All survey years.
9. Government program ended [O]: 2002 and onwards.
10. Project completed or job ended [O]: only in 2012.
11. Pregnancy [Q/O]: only in 1979.
12. Family reasons (to get married, to care for children, illness of other family members) [Q/O]: only in 1979.
13. Quit for pregnancy or family reasons [Q/O]: from 1980 to 2000.
14. Quit for pregnancy, childbirth or adoption of a child [Q/O]: 2002 and onwards.
15. Quit to spend time with or take care of children, spouse, parents, or other family members [Q/O]: 2002 and onwards.
16. Quit because found a better job [Q]: only in 1979.
17. Quit to take another job [Q]: 1990 and onwards.
18. Quit to look for another job [O]: 1990 and onwards.
19. Quit because interfered with school [Q/O]: only in 1979.
20. Quit to attend school or training [Q/O]: 2002 and onwards.
21. Quit to enter armed forces [Q/O]: only in 1979.
22. Quit because respondent's ill health, disability, or medical problems [Q/O]: 2002 and onwards.
23. Quit because wages too low [Q/O]: only in 1979.
24. Quit because of employment conditions (didn't like work, hours, conditions, or location, etc.) [Q/O]: only in 1979.
25. Quit because didn't like job, boss, coworkers, pay or benefits [Q/O]: 2002 and onwards.
26. Quit for other reasons [Q/O]: 1980 and onwards.
27. Business failed or bankruptcy [-]: 2002 and onwards.
28. Sold business to another person or firm [-]: 2002 and onwards.
29. Closed business down or dissolved partnership [-]: 2002 and onwards.
30. Business temporarily inactive [-]: 2002 and onwards.
31. Moved to another geographic area [Q/O] 2002 and onwards.
32. Job assigned through a temp agency or a contract firm became permanent [Q/O]: 2002 and onwards.
33. Dissatisfied with job matching service [Q/O] 2002 and onwards.
34. No desirable assignments available [Q/O] 2002 and onwards.
35. Husband or wife changed jobs and/or moved [Q/O] only in 1979.
36. Mother or father changed jobs and/or moved [Q/O] only in 1979.
37. Transportation problems [Q/O] 2002 and onwards.
38. Went to jail, prison, had legal problems [O]: 2002 and onwards.
39. Retired [O]: 2002 and onwards.
40. Other (specify) [Q/O]: all the survey years.

Appendix D. No fluctuations in z

Here, we repeat the exercises of Sections 6 and 7 while keeping z constant over time. In this specification, the wage is constant at 1 for everyone.

First, ϕ_λ and ϕ_δ are recalibrated in the baseline model so that the standard deviations of the UE flow rate and EU flow rate match the data. Note that $\log(z)$ that is multiplied to ϕ_λ and ϕ_δ is now a latent variable.

Table 11 repeats Table 5 in the main text. The results are essentially the same; all coefficients are qualitatively in the same sign as in the data, and the u_0 coefficient for the jobs that experience EU transitions is one order of magnitude smaller.

Table 12 conducts the same decomposition (except for the "z only" case) as Table 6. The intuition is the same, except that now z plays no role by construction. The fluctuations in λ_e play an important role in the u_0 coefficient for the jobs that end with EE transition, and δ fluctuations play the most important role in generating the negative u_0 coefficient for the jobs that transition into EU.

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Table 12
Hazard estimation results: counterfactuals.

Experiment	u_0 coeff. for EE	u_0 coeff. for EU	u_t coeff. for EE	u_t coeff. for EU
Baseline	.057	−.007	−.083	.092
λ_u only	.029	−.011	.041	.007
λ_e only	.077	.005	−.060	.061
δ only	.028	−.032	.024	.220

Table 13
Hazard estimation results: extended model.

Experiment	u_0 coeff. for EE	u_0 coeff. for EU	u_t coeff. for EE	u_t coeff. for EU
Baseline	.057	−.007	−.083	.092
Cyclical μ_x	.105	−.004	−.121	.090
Cyclical ν	.011	−.051	−.057	.097
Both μ_x and ν	.066	−.044	−.108	.093

Table 13 evaluates the effects of $\phi_x = 0.22$ and $\phi_\nu = -7.90$, as in Table 7 in the main text. As in the baseline case, we find that when μ_x and ν are cyclical, the model coefficients on u_0 and u_t can quantitatively match their data counterparts.

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