Job Duration, Wages, and the Cleansing and Sullying Effects of Recessions

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Abstract

Models of on-the-job search imply that recessions can cleanse and sully the labor market by hastening the termination of low quality matches and stifling the reallocation of currently employed workers into better matches. This paper evaluates these predictions using data from the National Longitudinal Survey of Youth (NLSY) under the hypothesis that match duration reflects quality. The results provide little evidence of the cleansing effect, but do support a significant sullying effect suggesting that new match quality is pro-cyclical. In line with a sullying view of recessions, wages of new hires are found to be pro-cyclical, even after controlling for cyclical composition in match quality across jobs.

Keywords: Business cycles, search and matching, on-the-job search, match quality, job duration, wages.
JEL classification: E24, E32, J21, J22, J62, J63, J64.

A central question in economics is how business cycles affect the allocation of resources. Focusing on the labor market it is a priori unclear whether recessions lead to above or below average productive arrangements. The textbook Diamond-Mortensen-Pissarides (DMP) model with endogenous separations implies that recessions cleanse the labor market from inferior matches. Following a negative productivity shock, jobs at the bottom of the surplus distribution fail to generate positive surplus and are destroyed. As reservation match quality rises in response to the aggregate productivity shock, only exceptionally high quality new matches are formed.

In contrast, recessions may sully the labor market once the possibility of on-the-job search is considered (e.g. Barlevy, 2002). In recessions, the job-to-job transition rate

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falls as firms post fewer vacancies. This impedes the reallocation of workers into better matches and hence more workers are stuck in poor matches. Thus, average quality of existing matches falls and the average quality of new matches may suffer as well. The reason is that with more workers stuck in mediocre matches workers will agree to enter matches of lower average quality given that they accept any match exceeding the quality of their current match. Whether recessions are more likely to cleanse or sully the labor market is ultimately a quantitative question.

This paper quantifies these effects with data from the National Longitudinal Survey of Youth (NLSY) 1979-2010 using the duration of a job match as a measure of quality. As guided by theory, it distinguishes between matches formed from by previously employed versus nonemployed workers. Evidence from the cyclicality of wages of new hires is also used to gauge the relative importance of these competing forces.

The results provide little evidence for the cleansing effect but do support a significant sullying effect. While matches ending in recessions are of shorter realized duration (and hence lower quality) than matches ending in booms these differences are modest and not statistically significant. For example, the difference in median duration between a match that ends in a recession versus a boom is only four months. Under extreme assumptions about the cleansing effect this difference increases to 13 months. By contrast, the sullying effect appears to be large and significant as matches starting in recessions are of systematically shorter duration than matches formed in booms. The difference in median duration between a new match formed in a recession versus a boom is 11 months. Under extreme assumptions this difference rises to 19 months, which is larger than the most extreme cleansing effect. As predicted by theory, the duration of new matches formed by previously employed workers falls in recessions. This finding holds true even when looking specifically at previously employed workers who voluntarily quit their jobs.

Additional evidence in favor of the sullying is found by analyzing the cyclicality of wages of new hires. If the cleansing effect dominates, then match quality of new hires should be counter-cyclical and their wages should be counter- or a-cyclical as selection into high quality matches helps offset low aggregate productivity in recessions. In contrast, if the sullying effect is dominant, then match quality of new hires should be pro-cyclical and wages of new hires should be pro-cyclical as well since match quality co-moves with aggregate productivity. Regressions reveal that wages of new hires are pro-cyclical and cyclically more sensitive than wages of existing workers. Importantly, this result holds even after controlling for cyclical variation in match quality in a number of ways. These findings are relevant in their own right as work by Gertler and Trigari (2009) and Gertler, Trigari and Huckfeld (2008) argues that the observed cyclical sensitivity of wages of new hires may be capturing uncontrolled cyclical changes in job quality. Additionally, these findings suggest that modeling assumptions like staggered Nash bargaining, which impose a similar cyclicality of wages between new and existing workers, are inconsistent with the
As previously mentioned, this paper draws from the theoretical insights of Barlevy’s (2002) model of on-the-job search. He provides some quantitative evidence that the average quality of new matches formed in recessions falls, however, because his model does not perform well at very high frequencies those conclusions are suggestive. This paper is complementary to his as it directly tests for the cleansing and sullying effects using data on new matches and focuses on the implications of voluntary job-to-job transitions. Related is also the work of Mukoyama (2014). He constructs a model of on-the-job search and finds that the decline in worker reallocation through job-to-job transitions accounts for roughly 0.5% of the decline in aggregate total factor productivity from 2009 to 2011. Relative to his paper, this paper makes the important distinction that not only are job switches less likely in a recession, but they also lead to lower quality matches.

The notion that a good match is represented by a lengthy duration is inspired by Jovanovic (1979). In his model of turnover matches are experienced goods whose quality is revealed over the duration of the match. Because of this learning, his model predicts that worker separation rates fall with tenure as lengthier matches are revealed to be better ones. In his model there is a time invariant distribution of match qualities to be drawn from. Bowlus (1995) extends his analysis by assuming that the distribution of match qualities varies over the business cycle and finds that matches starting in recessions are of shorter duration. She also uses data from the NLSY, but only through 1988. This paper also complements her work by considering a much longer time horizon with greater cyclical variation, match creation and destruction, allowing for worker fixed-effects, and distinguishing between matches formed by previously employed versus nonemployed workers. This last feature is a key component for identifying the sullying effect of recessions on new matches.

Lastly, the results regarding the cyclicality of wages are related to recent work by Hagedorn and Manovskii (2013), Bellou and Kaymak (2011), and Kudlyak (2011). Those papers focus on whether observed wages of existing workers are more consistent with a spot market versus a contractual view of wage determination. Hagedorn and Manovskii (2013) find that once controlling for match quality, using the sum of all labor market tightness through out the duration of a job, evidence that wages are history-dependent vanishes. In contrast, Bellou and Kaymak (2011), and Kudlyak (2011) find that wages appear to be history-dependent and thus more consistent with a contractual view of the labor market. While the tests proposed by Bellou and Kaymak (2011) require no direct measurement of match quality, Kudlyak (2011) finds evidence of history dependence by decomposing Hagedorn and Manovskii’s measure of match quality into the average labor market tightness faced by a match and its duration. This paper does not test for

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1. The first models of on-the-job search are due to Parsons (1973) and Burdett (1978). More recent studies include the work of Burdett and Mortensen (1998), Nagypál (2005a,b, 2006), and Tasci (2005).
contractual agreements, but rather focuses on the cyclicality of wages of new workers and distinguishes between new hires who were previously employed versus nonemployed, an important distinction emphasized by Gertler, Trigari and Huckfeld (2008).

The next section discusses the data used for the empirical analysis. Section 2 presents the procedure used to test for the cleansing and sullying effects of recessions. Section 3 presents the results on match duration. Section 4 presents the implications for the cyclicality of new hires. Section 5 concludes.

1 Data description

The data used in this study come from the National Longitudinal Survey of Youth (NLSY), survey years 1979 through 2010. The NLSY is a nationally representative sample of 12,686 young men and women who were 14-21 years old when first interviewed in 1979. Interviews were conducted annually through 1994 and biennially thereafter.

The NLSY has important advantages over other surveys for studying match duration. Compared to addressed based surveys, such as the Current Population Survey (CPS), individuals do not drop out of the sample following a change in geographical location, which may be highly correlated with job duration. During each survey participants report information for up to five jobs that can be linked across consecutive interviews. Thus, the NLSY’s format allows for more consistent construction of duration variables when compared to surveys such as the Panel Study of Income Dynamics (PSID). Importantly, the NLSY should capture short matches in between interview dates. Lastly, the NLSY has a much longer panel dimension in comparison to other longitudinal surveys such as the Survey of Income and Program Participation (SIPP), which only follows individuals for four years. Given that average match duration in the NLSY is over two years it is likely that using the SIPP would result in a highly right-censored sample.

Following Bowlus (1995) the sample is restricted to males from the cross-sectional samples and only spells that start when the individual is at least 18 years old and not in school are included. Individuals must report valid wages and working at least 15 hours per week. Spells that end prior to 1979 or lasting less than a month are dropped. Unlike Bowlus (1995), all spells of an individual meeting the above requirements are considered, rather than restricting the sample to a single random spell per individual. Hence, the sample not only covers more years but also more information per individual.

To construct the main sample of jobs and their respective durations data from the Employer Roster Survey is used. Using the variables that contain the start and stop

\(^2\)See Brown and Light (1992) for an in depth discussion of the issues when measuring job tenure in the PSID.

\(^3\)The implications of this sampling choice for the estimation procedure are discussed in the next subsection.
dates for each job report, the start of the match is defined as the week when the job is first recorded. The end of the match is defined as the week when the job is last linked. Gaps within the duration of a match are ignored. This distinguishes this paper’s measure of match duration to continuous tenure on the job. Given an ultimate interest in match quality, this broader definition of duration is used as repeated meetings between a worker and firm likely reflect a good match.

All jobs satisfying the previous requirements are used to measure the cyclicality of job duration. The resulting sample consists of 19,130 spells from 2,527 individuals or 7.6 spells per individual, on average. This sample excludes individuals with only one spell as fixed-effects cannot be estimated with such individuals. For comparison, Table 1 includes summary statistics for the main sample and the excluded sample. In the main sample the average match lasts 2.4 years, which underscores the importance of using a dataset with a long panel dimension. The majority of individuals in the main sample are white and have at least a high school degree. The excluded group has lengthier matches, lasting nearly 10 years, and are more educated.

Table 1: Summary statistics

<table>
<thead>
<tr>
<th></th>
<th>Main sample</th>
<th>Excluded sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Average job duration (in months)</td>
<td>28.2</td>
<td>117.6</td>
</tr>
<tr>
<td>Average age when job starts ($a_0$)</td>
<td>27.6</td>
<td>26.3</td>
</tr>
<tr>
<td>Average unemployment rate when job starts ($u_0$)</td>
<td>6.6</td>
<td>6.8</td>
</tr>
<tr>
<td>Average unemployment rate when job ends ($u_T$)</td>
<td>6.6</td>
<td>7.5</td>
</tr>
<tr>
<td>% non-white</td>
<td>21.5</td>
<td>14.3</td>
</tr>
<tr>
<td>% less than high school</td>
<td>21.3</td>
<td>6.9</td>
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</tr>
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<td>% some college</td>
<td>17.1</td>
<td>16.6</td>
</tr>
<tr>
<td>% college or more</td>
<td>13.6</td>
<td>36.3</td>
</tr>
<tr>
<td># individuals</td>
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<td>259</td>
</tr>
<tr>
<td># spells</td>
<td>19,130</td>
<td>259</td>
</tr>
<tr>
<td>Average spells per individual</td>
<td>7.6</td>
<td>1</td>
</tr>
</tbody>
</table>

Note: Source NLSY 1979-2010 men in cross-sectional samples.

Figure 1 shows the cross-sectional distribution of spells per individual for the main sample. As can be seen from the figure the distribution is highly left skewed (skewness coefficient of 1.46 ) and disperse (standard deviation of 5.12). In terms of identifying individual fixed-effects it is important to notice that over 87 percent of individuals in the main sample have more than two spells, while the median number of spells is seven.\footnote{The duration of a job is defined as right-censored whenever the individual is currently working at the job during the time of the interview when the match is last reported.}

\footnote{The Appendix replicates the main results on job duration where instead of using fixed-effects the number of spells per individual (normalized by the number of surveys to which they respond) is used as an explanatory variable.}
2 Theory and estimation strategy

Proportional hazard models with time-varying regressors are estimated to test for the cleansing and sullying effects of recessions. This type of model is chosen as it allows for the inclusion of censored observations in the estimation without imposing additional assumptions on the hazard function.\(^6\)

Recall the cleansing effect suggests that matches ending in recessions should be of systematically lower quality. Following a negative productivity shock, matches that used to provide some positive surplus no longer do and therefore are terminated. To test for this prediction a cleansing hazard \(\lambda^c\) is estimated that relates the realized duration of a match to cyclical conditions when it dissolved. Analysis time begins when the match is last observed and ends when the match starts. This hazard measures the instantaneous probability of observing a job start in period \(\tau\) conditional on not starting prior to \(\tau\). If matches ending in recessions are systematically shorter (i.e. lower quality) than in booms, then including the unemployment rate when the match ends, \(u_{\tau}\), as an explanatory variable should result in a positive estimated coefficient. This is because a higher \(u_{\tau}\) increases the hazard and thus reduces realized duration. Alternatively, if matches ending in recessions are no different than matches ending in booms, then cyclical conditions when the match ends should have no predictive power for duration.

Following Barlevy (2002), the sullying effect suggests that recessions exacerbate allocative inefficiency by moving fewer workers towards jobs where they are more productive. In recessions, labor market tightness falls as firms post fewer vacancies. Hence, workers are less likely to find good matches before exogenous separations leave them unemployed. In steady state fewer workers are employed in higher quality matches. Importantly, this last

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\(^6\)See Bowlus (1995) for a related application.
feature can lead to a fall in the quality of new matches. With more workers employed in lower quality matches, workers will agree to enter matches of lower average quality given that they accept any match exceeding the quality of their current match. To test for these predictions, a sullying hazard function $\lambda^s$ that relates cyclical conditions when the match begins to its expected duration is estimated. Analysis time for this hazard begins when the match is first observed and ends when the match dissolves. If matches starting in recessions are of shorter expected duration (i.e. lower quality), then the coefficient on the unemployment rate when the match begins, $u_0$, should be positive when estimating this hazard. If the sullying effect operates by reducing the quality of new matches formed through on-the-job search, then restricting the estimation of $\lambda^s$ to matches formed from job-to-job transitions should also lead to a positive estimated coefficient on $u_0$.

These hazards take the following forms:

\begin{align*}
\lambda^c(\tau|X(\tau)) &= \lambda^c_0(\tau) \exp(\beta'X(\tau)) \\
\lambda^s(t|X(t)) &= \lambda^s_0(t) \exp(\gamma'X(t))
\end{align*}

Here $\lambda^c_0(\tau)$ represents the baseline hazard of a job starting at time $\tau$; $\beta$ is a coefficient vector to be estimated, and $X(\tau)$ is a vector of individual and aggregate characteristics at time $\tau$. These characteristics are both time-constant (e.g. unemployment rate at the end of the job, race, and education) and time-varying (e.g. age, labor market experience, and current unemployment rate) variables. Likewise, $\lambda^s_0(t)$ represents the baseline hazard of the job ending at time $t$; $\gamma$ is a coefficient vector to be estimated, and $X(t)$ is a vector of individual and aggregate characteristics at time $t$. The vector $X(t)$ differs from $X(\tau)$ by including the unemployment rate at the beginning of the job rather than the unemployment rate at the end of the match.

In general, when using panel data correlation may arise between observations from the same individual but not across individuals if unobserved heterogeneity is present. In particular, the duration of each spell within an individual’s employment history may not be independent from the rest since surveys have fixed time horizons. Hence, unobserved components and explanatory variables capturing duration can be correlated. Thanks to the long length of the NLSY this should not be an issue as all individuals have several spells and the length of each spell is not determined by survey design, but rather individual decisions. Nonetheless, solutions to this potential problem include using only the first two observed spells for each individual, as suggested by Chamberlain (1985), or using one randomly chosen spell as suggested by Bowlus (1995). These sampling schemes, however, make the analysis of job-to-job transitions difficult.

Thus, in an attempt to use as much data as possible this paper uses all valid job spells for an individual. To alleviate the effect of the aforementioned biases each individual’s spells are weighted by the inverse of the number of spells observed for them normalized
by the number of survey waves to which they respond. Normalizing by the number of
surveys helps distinguish between individuals who report few long duration jobs lasting
over several years versus individuals who report few jobs because of attrition. Additionally,
individual fixed-effects are included in the estimation. Controlling for fixed-effects
alleviates biases arising from unobserved fixed heterogeneity.\(^7\)

3 Hazard results

3.1 Cleansing hazard

Table 2 presents the results from estimating the cleansing hazard function \(\lambda^c\). The variable \(u_T\) represents the unemployment rate when the match ends, while the variable \(u_\tau\)
represents the time-varying current unemployment rate. Positive (negative) coefficients
imply increases (decreases) in the hazard, and therefore, decreases (increases) in realized
duration. The first column of the table shows that the final unemployment rate has a
positive but statistically insignificant effect on the cleansing hazard. The size of the co-
efficient suggests only a modest role for the cleansing effect. The results from the second
column confirm this assertion even after controlling for worker fixed-effects.\(^8\)

Table 2: Estimates for cleansing hazard

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>(u_T)</td>
<td>.005877</td>
<td>.01322</td>
<td>-.004309</td>
<td>.01238</td>
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<td></td>
<td>(.00851)</td>
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<td>(.0353)</td>
<td>(.0336)</td>
<td>(.0555)</td>
<td>(.0709)</td>
</tr>
<tr>
<td>(u_\tau)</td>
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<td>.1284</td>
<td>.1005</td>
<td>.1295</td>
<td>.06542</td>
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<td></td>
<td>(.0515)</td>
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<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
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<td>153,754</td>
<td>193,544</td>
<td>58,959</td>
<td>97,088</td>
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</table>

Note: \(u_T\) denotes the unemployment rate at the time of separation. \(u_\tau\) denotes the time-varying
current unemployment rate. Standard errors are clustered by time and appear in parentheses.
Regressors not reported: cubic in experience, year fixed-effects, and indicators for race, less than high
school education, some college, and college (or more) graduate. +, *, **, *** indicate statistical
significance at 10%, 5%, 1%, and 0.1% levels.

To gauge the quantitative importance of the cleansing effect, Figure 2 presents the
survivor function implied by the estimates from Column 2. Recall that survival of a match
in this context means not starting. Consequently, any point on the survivor curve is the

\(^7\)Allison (1996) finds that under conditions of moderate censoring (e.g. the majority of individuals
experience at least two events) the fixed-effects estimator is nearly always better than the conventional
partial likelihood estimator when applied to repeated events with unobserved heterogeneity.

\(^8\)The Appendix replicates the main results on job duration where instead of using fixed-effects the
number of spells per individual (normalized by the number of surveys to which they respond) is used as
an explanatory variable.
cumulative probability that a match has at least that much time before it starts. The solid line represents the survivor function for a white male, with a high school diploma and 10 years of labor market experience, holding a job that ends when the final unemployment rate is at its sample mean and setting the current unemployment rate to its sample mean. The “- -” line represents the survivor function for the same person, only now facing a more favorable final unemployment rate that is four percentage points below its mean (i.e. boom). The “-. .” line is the survivor function when the final unemployment rate is four percentage points above its mean (i.e. recession). Median duration under typical conditions (solid line) is 26 months. Meanwhile, median duration falls to 24 months in a recession and rises to 28 months in a boom. Larger effects are statistically possible because of the significant uncertainty around the coefficient of $u_T$ in Column 2. Assuming a coefficient on $u_T$ at the upper bound of its 95% confidence interval does imply larger effects. In this case, median duration falls to 20 months in a recession and rises to 33 months in a boom. In contrast, assuming a coefficient on $u_T$ at the lower bound of its 95% confidence interval implies effects that go in the wrong direction. In this scenario, median duration of matches ending in recessions actually rises to 29 months, and falls to 23 months in a boom.

Figure 2: Survivor functions for the final unemployment rate at its mean (—) and four percentage point increase/decrease ( - - / -. . ).

Column 3 looks specifically at matches that end with the worker transiting into nonemployment. As typical DMP models with endogenous separations do not model job-to-job transitions, a more focused test is whether matches ending in nonemployment are systematically shorter in recessions. In this case, the coefficient on $u_T$ in Column 3 should be positive. The estimated coefficient on $u_T$ is of the wrong sign, but not statistically
significant.

As a prelude to the results in the following subsection, Column 4 considers matches that end with the worker transiting into another job.\textsuperscript{9} If recessions sully the labor market by reducing the quality of matches formed from job-to-job transitions, then the coefficient on $u_T$ in Column 4 should be positive. The reason is that if workers are willing to accept lower quality matches through on-the-job search then, they must be exiting matches that are, on average, of low quality. The estimated coefficient on $u_T$ is positive in concordance with the sullying effect, but not statistically significant.

Columns 5 and 6 repeat the exercises from Columns 3 and 4 by adding controls for voluntary versus involuntary separations. Column 5 restricts the sample to matches that end with the worker being laid off and becoming nonemployed. Column 6 restricts the sample to matches that end with the worker quitting the current job and becoming reemployed. The results from Column 5 suggest that the duration of matches ending with layoffs increases in recessions, potentially reflecting mass layoff events or plant closures. Alternatively, this finding may be reminiscent of the sullying effect as these workers cling to poor matches until they are laid off. Somewhat contrary to the sullying effect, the results of Column 6 suggest that the duration of matches ending in quits also rises in recessions, though this effect is not statistically significant. On the other hand, contemporaneous conditions measured by the coefficient on $u_T$ appear to have the correct effect on duration. In other words, worse contemporaneous conditions reduce the duration of matches ending in quits.

### 3.2 Sullying hazard

Focusing next on the estimated sullying function, Table 3 presents the results from estimating $\lambda^s$. The variable $u_0$ represents the unemployment rate when the match begins, while the variable $u_t$ represents the current unemployment rate. Column 1 shows that the initial unemployment rate, $u_0$, has a positive and statistically significant effect on the sullying hazard rate. Hence, matches starting in recessions are of shorter expected duration as suggested by the sullying effect of recessions. Using a smaller sample and narrower time frame, Bowlus (1995) estimates a coefficient on the initial unemployment rate of 0.0497, which is essentially identical to the coefficient estimated in this paper. Column 2 confirms the significance of the sullying effect even after controlling for worker fixed-effects.

To illustrate the magnitude of the estimated sullying effect, Figure 3 presents the survivor functions implied by Column 2. The solid line represents the survivor function for a white male, with a high school diploma and 10 years of labor market experience, A match is assumed to end in a job-to-job transition if the spell of nonemployment following match dissolution is at most two weeks. Increasing this threshold to four weeks does not change the results significantly.

\textsuperscript{9}
Table 3: Estimates for sullying hazard

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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<tbody>
<tr>
<td>u₀</td>
<td>.04991***</td>
<td>.04932**</td>
<td>.04532+</td>
<td>.05944*</td>
<td>.04485</td>
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<td>-.007094</td>
<td>.03893</td>
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<td>(.0465)</td>
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<td>yes</td>
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<td>538,539</td>
<td>231,553</td>
<td>252,811</td>
<td>203,901</td>
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</tbody>
</table>

Note: u₀ denotes the unemployment rate at the time when the match begins. uₜ denotes the time-varying current unemployment rate. Standard errors are clustered by time and appear in parentheses. Regressors not reported: cubic in experience, year fixed-effects, and indicators for race, less than high school education, some college, and college (or more) graduate. +, *, **, *** indicate statistical significance at 10%, 5%, 1%, and 0.1% levels.

holding a job that starts when the unemployment rate is at its sample mean and setting the current unemployment rate to its sample mean. The “- -” line represents the survivor function for the same person, only now facing a more favorable initial unemployment rate that is four percentage points below its mean (i.e. boom). The “-..” line is the survivor function when the initial unemployment rate is four percentage points above its mean (i.e. recession). The solid line implies the median new match lasts 18 months under standard conditions. Median expected duration falls to 14 months in a recession and rises to 25 months in a boom. These estimates imply a sullying effect over twice as large as the baseline cleansing effect from Column 2 in Table 2. Assuming a coefficient on u₀ at the upper bound of the estimated 95% confidence interval implies an even larger sullying effect. Median duration falls to 12 months in a recession and rises to 31 months in boom. Assuming a coefficient on u₀ at the lower bound of the estimated 95% confidence interval still implies a non-trivial sullying effect as median duration falls to 16 months in a recession and rises to 20 months in a boom. Based on the shape of the survivor functions in Figure 3 it is apparent that these effects are even larger at lower survival rates. For example, median duration of matches in the 30th percentile falls to 30 months in a recession and rises to 78 months in a boom, a difference of 4 years. Hence, the sullying effect appears to be even more meaningful for longer lasting higher quality matches.

Mirroring the analysis from the previous section, Columns 3 and 4 consider separately matches formed with previously nonemployed workers versus those switching between jobs. The positive and statistically significant coefficient on u₀ in Column 3 suggests that even matches formed with previously nonemployed workers are of lower quality in recessions. The positive and significant coefficient on u₀ in Column 4 confirms that the sullying effect stifles the creation of better matches formed through job-to-job transitions. Recall that models with on-the-job search, like Barlevy (2002), predict that the quality
of new matches formed by previously employed workers will decline in recessions because these workers accept any offer better than their previous mediocre match.

Columns 5 and 6 repeat the estimations of Columns 3 and 4 by looking at workers who were most recently nonemployed and laid off from their previous employment stint and workers who were recently employed and quit their last job. The estimated coefficient on $u_0$ in Column 5 is positive but not statically significant, suggesting the sullying effect does not critically affect matches with workers who were previously nonemployed and forced to leave their last job. By contrast and in line with theory, the results in Column 6 confirm that the sullying effect has a large and statistically significant effect on the duration of new matches from by previously employed workers who voluntarily left their previous jobs.

In sum, the measured sullying effect of recessions appears to be larger than the baseline cleansing effect. Moreover, the measured sullying effect is also robust to controlling for worker fixed-effects. Consistent with theory, recessions sully the labor market by reducing the quality of matches formed through job-to-job transitions and quits.

4 Implications for the cyclicality of wages

As additional support for the sullying effect this section considers evidence gleaned from the cyclicality of wages of new hires under the assumption that wages are flexible, as would arise under typically assumed Nash bargaining between worker and firm. If the
cleansing effect dominates, then match quality of new hires should be counter-cyclical as only exceptionally high quality matches are formed in recessions. Under this scenario, wages of new hires should be counter- or a-cyclical as selective hiring helps offset low aggregate productivity in recessions.

In contrast, if the sullying effect is dominant, then match quality of new hires should be pro-cyclical. In recessions, new hires accept on average lower quality matches given that they are currently stuck in mediocre matches. Under this scenario, wages of new hires should be pro-cyclical since match quality co-moves with aggregate productivity.

To test between these two scenarios, wage regressions of the following form are considered:

\[
\ln w_{ijt} = \alpha + \beta_1 X_{it} + \beta_2 I_{\text{newemp}} + \beta_3 u_t + \beta_4 (I_{\text{newemp}} \times u_t) + v_i + \epsilon_{ijt} \quad (3)
\]

where: \( \ln w_{ijt} \) is the log real hourly wage of individual \( i \) in job \( j \) at time \( t \), \( X_{it} \) is a matrix of time-constant and time-varying individual characteristics, \( I_{\text{newemp}} \) is an indicator equal to one whenever the job is new, \( u_t \) is the current unemployment rate, \( I_{\text{newemp}} \times u_t \) is an interaction term between the new job indicator and current unemployment rate, \( v_i \) is an individual fixed-effect, and \( \epsilon_{ijt} \) is classical measurement error. Note that if wages of new hires are pro-cyclical, then the coefficient \( \beta_4 \) should be negative.

For the results presented below the sample is restricted to primary jobs where the individual works at least 15 hours and earns real wages between $1 and $100 dollars. A job is identified as new if it has not been previously reported by the individual. The monthly unemployment rate used in Equation 3 is reflective of the most recent reference time of the job report. For example, if the job is held at the time of interview, then the unemployment rate is based on the interview month. If the job is not currently held, then the unemployment rate is based on the most recent month the job was held.

The results of estimating Equation 3 appear in Table 4. Note that fixed-effects are controlled for in all columns. Column 1 presents the baseline results. The coefficient on the interaction term of new employer and the current unemployment rate \( I_{\text{newemp}} \times u_t \) suggests that wages of new hires are quite pro-cyclical. The estimated coefficient is highly statistically significant and in line with estimates presented in Bils (1985) and Pissarides (2009). Recall that compared to those studies, the time period considered is this paper is longer and includes the Great Recession and ensuing recovery.

One criticism when interpreting the results of Column 1 is that the observed cyclicality of wages of new hires may be capturing unmeasured cyclical differences in match quality. The second column of the table attempts to mitigate such concerns by using the total duration of the match as a measure of quality. The coefficient on \( \ln \text{duration} \)
in Column 2 suggests that in general longer lasting matches command higher wages. More importantly, the coefficient on the interaction term between new employer and the unemployment rate remains negative and statistically significant.

As an alternative measure of match quality, residual or excess duration can be calculated using the results from the previous section. To do so, the deviance residuals implied by the hazard regression in Column 2 of Table 3 are computed. A large positive deviance residual suggests the match ended “too soon” relative to what the hazard model predicts. These matches can be interpreted as worse than expected given what observables and individual fixed-effects imply. In contrast, a large negative residual implies that the match lasted “too long” relative to what the hazard model predicts. These matches can be interpreted as better than expected given what explanatory variables predict. For expositional purposes, the deviance residuals are multiplied by $-1$ so that a large positive value is suggestive of a better than expected match, whereas a large negative value is a worse than expected match.

Including the deviance residuals as an explanatory variable for match quality delivers the results in Column 3. As seen from the coefficient on the interaction term, the cyclicality of wages of new hires remains largely pro-cyclical and statistically significant. Additionally, the deviance residuals also appear to have important explanatory power for wages as judged by the coefficient on residualduration.\footnote{Admittedly, inference on the coefficient on residualduration is difficult since this explanatory variable is the result of a first-stage regression and the standard errors reported in the Table do not account for this.}

Recent work by Gertler, Trigari and Huckfeld (2008) suggests that it is important to distinguish between previously employed versus previously nonemployed workers when estimating regressions of the form of Equation 3. Table 5 repeats the previous regressions only now distinguishing between new jobs where the individual was previously employed newempEE versus previously nonemployed newempNE.\footnote{As in the previous section, an individual is considered to be previously employed if the time elapsed between jobs is at most two weeks.} The first column of Table 5 confirms that the wages of new hires are pro-cyclical and highly sensitive to cyclical conditions regardless of whether the individual was previously employed or not. Column 2 shows that the basic conclusions remain true even after controlling for match quality using match duration. Lastly, Column 3 further confirms the pro-cyclicality of wages of new hires (either previously employed or nonemployed) when controlling for match quality using the proposed measure of excess duration.

The findings presented in this section are important regardless of whether recessions are cleansing or sulling. Chiefly, they suggest that wages of new hires are more cyclical than wages of existing workers even after controlling for variation in match quality in a number of ways. These empirical findings question theoretical assumptions like staggered Nash bargaining, which impose a similar cyclicality of wages for new and existing
### Table 4: Estimates of the cyclicality of wages of new hires

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>(newemp \times u_t)</td>
<td>-.0243*** (0.00383)</td>
<td>-.01927*** (0.0037)</td>
<td>-.02599*** (0.00414)</td>
</tr>
<tr>
<td>(\ln(duration))</td>
<td>.07638*** (0.00343)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(residualduration)</td>
<td></td>
<td>.09982*** (0.00445)</td>
<td></td>
</tr>
<tr>
<td>Worker fixed-effects</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>No. of obs.</td>
<td>44,020</td>
<td>44,020</td>
<td>38,736</td>
</tr>
</tbody>
</table>

Note: \(u_t\) denotes the current unemployment rate. \(newemp\) is an indicator equal to one whenever the individual reports a new employer. \(newemp \times u_t\) is an interaction between the new employer indicator and the current unemployment rate. \(\ln(duration)\) is the log of the total duration of the job match (in months). \(residualduration\) is a measure of the job’s residual duration using the deviance residuals from the hazard regression in column (2) of Table 3 multiplied by \(-1\). Standard errors are clustered by time and appear in parentheses. Regressors not reported: cubic in experience, year fixed-effects, current unemployment rate and new job indicator. +, *, **, *** indicate statistical significance at 10%, 5%, 1%, and 0.1% levels.

### Table 5: Estimates of the cyclicality of wages of new hires by previous employment status

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>(newempEE \times u_t)</td>
<td>-.02071*** (0.00528)</td>
<td>-.0148** (0.00535)</td>
<td>-.02128*** (0.00595)</td>
</tr>
<tr>
<td>(newempNE \times u_t)</td>
<td>-.01996*** (0.00406)</td>
<td>-.01649*** (0.00399)</td>
<td>-.02276*** (0.00439)</td>
</tr>
<tr>
<td>(\ln(duration))</td>
<td>.0754*** (0.00343)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(residualduration)</td>
<td></td>
<td>.09827*** (0.00445)</td>
<td></td>
</tr>
<tr>
<td>Worker fixed-effects</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
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<tr>
<td>No. of obs.</td>
<td>44,020</td>
<td>44,020</td>
<td>38,736</td>
</tr>
</tbody>
</table>

Note: \(u_t\) denotes the current unemployment rate. \(newempEE\) is an indicator equal to one whenever the individual reports a new employer and was previously employed. \(newempNE\) is an indicator equal to one whenever the individual reports a new employer and was previously not working. \(newempEE \times u_t\), \(newempNE \times u_t\) are interactions between the corresponding new employer indicator and the current unemployment rate. \(\ln(duration)\) is the log of the total duration of the job match (in months). \(residualduration\) is a measure of the job’s residual duration using the deviance residuals from the hazard regression in column (2) of Table 3 multiplied by \(-1\). Standard errors are clustered by time and appear in parentheses. Regressors not reported: cubic in experience, year fixed-effects, current unemployment rate and new job indicators. +, *, **, *** indicate statistical significance at 10%, 5%, 1%, and 0.1% levels.

The work by Gertler and Trigari (2009) and Gertler, Trigari and Huckfeld (2008) finds that once controlling for match quality using worker-job fixed-effects the cyclicality of wages of new hires disappears. The
5 Conclusion

This paper tests for the cleansing and sullyng effects of recessions using job duration as a measure of match quality. Using panel data from the NLSY this paper finds little evidence in favor of the cleansing effect, but does find significant evidence supporting the sullyng effect. While matches ending in recessions are four months shorter than those ending in booms, this finding is not significant. Meanwhile, recessions tend to sully the labor market as matches starting in recessions are nearly 11 months shorter than those starting in booms. This finding is robust to controlling for worker composition. As predicted by theory, this finding is driven by the job-to-job transition channel: the expected duration of matches formed from job-to-job transitions and quits declines in recessions.

Additional evidence for the cleansing effect is found by looking at the cyclicality of wages of new hires. Wages of new hires are found to be highly pro-cyclical even after match quality is taken into account. Using a constructed measure of excess match quality further confirms the results. The results are also robust to distinguishing between new hires who were previously employed versus nonemployed. These latter findings are of interest in their own right as they suggest that modeling assumptions like staggered Nash bargaining of wages are inconsistent with the data.

An important component missing from the present analysis is the firm dimension. Using firm-level data, Kahn (2011) also finds that employment relationships that start in recessions are short-lived. However, once firm heterogeneity is taken into account this effect is reversed. Future research should reconcile the findings of this paper and Kahn (2011) with a matched employer-employee dataset where both worker and firm effects can be isolated.

Online Appendix—not for publication

A Data

The data used in the paper comes from the NLSY survey years 1979-2010. The Employer History Roster is used to compile all variables of interest.

- **Wages.** Wages are calculated from the Employer roster variables EMPLOYERS ALL TIMERATE, EMPLOYERS ALL PAYRATE, and EMPLOYERS ALL HRLY WAGE. All wages are deflated by the CPI-U all urban consumers index. Appendix presents regression results with this alternative identifying assumption and still finds evidence in favor of the cyclicality of wages of new hires.
• **Hours.** Hours are calculated from the Employer roster variables EMPLOYERS ALL HOURSWEEK and EMPLOYERS ALL HOURSDAY.

• **Start and stop dates.** Dates are calculated from the Employer roster variables EMPLOYERS ALL STARTDATE ORIGINAL, EMPLOYERS ALL STOPDATE, and EMPLOYERS ALL STARTWEEK.

• **Layoffs and quits.** Layoffs and quits are identified using the variable EMPLOYERS ALL WHYLEFT.

### B Additional results

Hazard regressions using number of spells per individual instead of fixed-effects

<table>
<thead>
<tr>
<th></th>
<th>(1) all</th>
<th>(2) all</th>
<th>(3) to NE</th>
<th>(4) to E</th>
<th>(5) to NE+layoff</th>
<th>(6) to E+quit</th>
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</thead>
<tbody>
<tr>
<td>$u_T$</td>
<td>.005877</td>
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<td>.0278+</td>
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<td>.02742</td>
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<tr>
<td></td>
<td>(.00851)</td>
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<td>(.0165)</td>
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<tr>
<td>$u_τ$</td>
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<td>.02867</td>
<td>.046</td>
<td>-.01334</td>
<td>.03324</td>
<td>.06614</td>
</tr>
<tr>
<td></td>
<td>(.0515)</td>
<td>(.0515)</td>
<td>(.0584)</td>
<td>(.0614)</td>
<td>(.0597)</td>
<td>(.0732)</td>
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</table>

Normalized spells per individual

<table>
<thead>
<tr>
<th></th>
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<th>yes</th>
<th>yes</th>
<th>yes</th>
<th>yes</th>
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</table>

No. of obs. 538,539 538,539 153,754 193,544 58,959 97,088

Note: For comparison purposes, Column (1) displays the corresponding column from Table 2 in the text. $u_T$ denotes the unemployment rate at the time of separation. $u_τ$ denotes the time-varying current unemployment rate. Standard errors are clustered by time and appear in parentheses. Regressors not reported: number of spells per individual divided by number of survey years observed in, cubic in experience, year fixed-effects, and indicators for race, less than high school education, some college, and college (or more) graduate. +, *, **, *** indicate statistical significance at 10%, 5%, 1%, and 0.1% levels.
Table 7: Estimates for sullying hazard

<table>
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<tbody>
<tr>
<td></td>
<td>all</td>
<td>all</td>
<td>from NE</td>
<td>from E</td>
<td>from NE+layoff</td>
<td>from E+quit</td>
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<tr>
<td>$u_0$</td>
<td>.04991***</td>
<td>.05153***</td>
<td>-.004499</td>
<td>.05973**</td>
<td>-.002218</td>
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<tr>
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<td>(.0145)</td>
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<td>-.05343</td>
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<td>.01717</td>
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<tr>
<td></td>
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<td>(.0361)</td>
<td>(.0459)</td>
<td>(.0568)</td>
<td>(.0463)</td>
<td>(.0555)</td>
</tr>
<tr>
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<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>spells per individual</td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. of obs.</td>
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<td>538,539</td>
<td>231,553</td>
<td>252,811</td>
<td>203,901</td>
<td>228,534</td>
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</table>

Note: For comparison purposes, Column (1) displays the corresponding column from Table 3 in the text. $u_0$ denotes the unemployment rate at the time when the match begins. $u_t$ denotes the time-varying current unemployment rate. Standard errors are clustered by time and appear in parentheses. Regressors not reported: number of spells per individual divided by number of survey years observed in, cubic in experience, year fixed-effects, and indicators for race, less than high school education, some college, and college (or more) graduate. +, *, **, *** indicate statistical significance at 10%, 5%, 1%, and 0.1% levels.

Wage regressions using worker-job fixed-effects

Table 8: Estimates of the cyclicality of wages of new hires

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$newemp \times u_t$</td>
<td>-.0244***</td>
<td>( .00383)</td>
</tr>
<tr>
<td>$newempEE \times u_t$</td>
<td>-.0208***</td>
<td>( .00528)</td>
</tr>
<tr>
<td>$newempNE \times u_t$</td>
<td>-.0201***</td>
<td>( .00407)</td>
</tr>
<tr>
<td>Worker-job</td>
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<td>yes</td>
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<tr>
<td>fixed-effects</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. of obs.</td>
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<td>44,020</td>
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</tbody>
</table>

Note: $u_t$ denotes the current unemployment rate. $newemp$ is an indicator equal to one whenever the individual reports a new employer. $newempEE$ is an indicator equal to one whenever the individual reports a new employer and was previously employed. $newempNE$ is an indicator equal to one whenever the individual reports a new employer and was previously not working. $newempEE \times u_t$, $newempNE \times u_t$ are interactions between the corresponding new employer indicator and the current unemployment rate. Standard errors are clustered by time and appear in parentheses. Regressors not reported: cubic in experience, year fixed-effects, current unemployment rate and new job indicators. +, *, **, *** indicate statistical significance at 10%, 5%, 1%, and 0.1% levels.
References


