Do Job-to-Job Transitions Drive Wage Fluctuations Over the Business Cycle?

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Thirty five years ago, James Tobin (1972) called attention to the topic of job search (among others) in his presidential address to the AEA. He was skeptical that workers who wished to switch employers would first quit into unemployment in order to search. Rather, he suggested that a far more attractive route would be to search while on the job. With the benefit of data unavailable to Tobin, we know now that Tobin had a strong point: though nearly 2 percent of workers quit per month, just 0.2 percent quit to unemployment.[1]

The prevalence of job-to-job (JJ) transitions has shaped our view of labor market dynamics. In this paper, we look specifically at the implications of JJ transitions for theories of wage dynamics. Moscarini and Postel-Vinay (2016a) have stressed that, in the canonical Burdett-Mortensen (1998) model of on-the-job search, real wages are intimately bound with the pace of JJ transitions. Intuitively, competition among firms for *employed* workers, manifested through the pace of JJ transitions, is a key driver of real wages. Indeed, they show that, under certain restrictions that we discuss below, the rate of JJ transitions is, adjusting for productivity, a sufficient statistic for the average wage.

This prediction contrasts starkly with the results in search models that abstract from JJ transitions. In these theories, a common approach to determine wages is to have firms and workers bargain after they meet. In the case of a Nash bargain specifically, the wage is a weighted average of a worker's productivity and her reservation wage, and the latter depends on the speed with which a worker would find a new match if she were to quit to unemployment. In other words, the mechanism that Tobin questioned—the threat to quit in order to undertake job search—plays the crucial role in wage dynamics in this setup. As a result, the pace at which the *unemployed* find jobs is the key driver of wage fluctuations.[2]

We investigate the comparative explanatory power of these two views of wage determination. In brief, we ask if, controlling for productivity, the rate of JJ transitions is a sufficient statistic for average wages. Equivalently, we ask if the unemployed's job-finding rate has any explanatory power for wages conditional on the JJ transition rate. These are the same questions posed by Moscarini and Postel-Vinay in their initial, provocative analysis (see also Faberman and Justiniano, 2015). However, they relied only on aggregate time series variation in their econometric analysis. Since it is generally difficult to establish robust results with so few observations we exploit cross-state variation in worker flows and wages.[3]

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[1] The Job Openings and Labor Turnover survey measures quits from U.S employers. The average monthly quit rate over the sample (December 2000-November 2016) is 1.9 percent. The inflow of quits into unemployment is estimated from Current Population Survey data, which measures the number of workers who report becoming unemployed each month because they quit their job.

[2] Pissarides (2000) merges on the job search and bargaining. He shows that workers who search on the job do bargain a wage that is independent of the job finding rate. However, the bargain is *also* independent of the arrival rate of job offers to the *employed*, which is the critical feature of Moscarini and Postel-Vinay.

[3] Using different measures of earnings and JJ transitions, Moscarini and Postel-Vinay (2016a) find that JJ transitions dominate other labor market flows as a predictor of wages. However, we find that, upon aggregating our data, no conclusive results emerge.

The remainder of this paper proceeds as follows. In section 1, we report our main finding: wage growth is tightly linked to variation in the JJ transition probability, but conditional on the pace of JJ transitions, the job-finding probability of the unemployed has *no* explanatory power. In section 2, we subject our result to a battery of sensitivity tests. Our main finding concerning the importance of JJ transitions comes through relatively unscathed, though one specification does uncover more of a role for other labor market flows. Section 3 concludes by highlighting a few aspects of our results that are *not* so easily reconciled to standard on-the-job search theory and suggests avenues for future research.

1. Baseline results

Our point of departure in this paper is Moscarini and Postel-Vinay's (MPV, 2016a) observation that the Burdett-Mortensen (BM) implies testable restrictions on the map from labor market flows to real wages. MPV highlight that, in BM, real wages are likely to be substantially more sensitive to changes in the arrival rate λ^{e} of job offers to the employed than to changes in the arrival rate λ^{u} of job opportunities to the unemployed.

The key insight is that the average wage depends on λ^{u} only via the influence of the latter on the *reservation wage*, and, crucially, the reservation wage in BM is unlikely to be strongly procyclical, for a few reasons. MPV suggest that it may be anchored by a mandated minimum wage. More generally, changes in labor market flows have offsetting effects on the reservation wage. A higher λ^{u} raises it, since it implies a higher return on job search. On the other hand, a concomitant increase in λ^{e} lowers it; a worker will accept a lower wage now in exchange for more opportunities to advance to higher-wage jobs later.

Accordingly, the BM model isolates λ^{e}, the arrival rate of job offers to the *employed*, as the more significant factor behind movements in real wages. More exactly, it suggests that, modulo aggregate productivity, λ^{e} is the only labor market flow that should influence real wages. A corollary of this is that λ^{u} should have no predictive power, conditional on λ^{e}. This claim stands in contrast to the implications of models with no on the job search, where wage setting is typically pinned down by a Nash bargain (as in the canonical Mortensen-Pissarides model). In these settings, changes in λ^{u} drive wage fluctuations precisely by influencing reservation wages.

Our aim is to test the comparative power of the two flows, λ^{e} and λ^{u}. We do this by simple least squares regression. Our data come from several sources. All of the data is available at a quarterly frequency at the state level. The sample spans from 2000q3 through 2015q2.

First, as our measure of λ^{e}, we use the share of the employed that transition from one employer to another, with no observed intervening spell of non-employment. This data is from the public use series of the Longitudinal Employer-Household Dynamics (LEHD) data. As a measurement of λ^{e}, this does have a few shortcomings—for instance, our data measure the *realized* transition probability rather than the arrival rate of *offers*. However, we shall see in the next section that our results are robust to this and other concerns.

Second, as our measure of λ^{u}, we use the unemployment to employment transition probability, as recovered from the public-use files of the Current Population Survey (CPS). Three quarters of the CPS sample can be observed across two adjacent months. We track the share of the unemployed in one month that transition to employment in the next month. Again, the CPS has its shortcomings, namely, the relatively small sample sizes in several states. We return to this as well as in the subsequent section.

Third, and finally, we use data on earnings from the Quarterly Workforce Indicators (QWI). A key benefit of the QWI is that it measures earnings of all workers and new hires. The prediction of BM relates to average earnings, but this reflects in part an equal treatment constraint: all workers within a firm must be paid the same wage. More generally, we suspect that incumbent workers' wages are updated infrequently relative to the rate at which wage offers to new hires are adjusted (see Haefke, Sonntag, and van Rens 2013). In this case, an increase in λ^{e} may be manifested most clearly in new hires' earnings. The QWI enables a closer look at the latter.[4]

Proceeding, the regression specification is reported below. In the following, we use capital letters to denote measurements and lower-case letters to denote their theoretical counterparts. The estimating equation can then be written as,

$$\ln W_{it} = \alpha_0 + \alpha_{\mathrm{e}}\Lambda^{\mathrm{e}}_{it} + \alpha_{\mathrm{u}}\Lambda^{\mathrm{u}}_{it} + \alpha_i + \alpha_t + \varepsilon_{it}, \tag{1}$$

where i denotes a state and t a calendar quarter; W is average monthly earnings; Λ^{e} is the LEHD measurement of the job-to-job transition probability; Λ^{u} is the CPS measurement of the average monthly transition probability between unemployment and employment; α_i is a state fixed effect; and α_t is a calendar quarter fixed effect that flexibly picks up variation in aggregate inflation and productivity as well as general seasonal factors.[5] For our first set of results, Λ^{e} refers to the "headline" measure of the job-to-job transition probability published by the LEHD program, which includes two types of events: (i) one in which a worker is observed to be employed by two firms within the *same* quarter, and (ii) another in which a worker is employed by only one firm in quarter *t* but by a different firm in quarter *t*-1. We report results based on both measures.[6]

Estimates of (1) are reported in Table 1. Column (1) omits $\Lambda^{\mathrm{e}}_{it}$ from the regression; column (2) adds $\Lambda^{\mathrm{e}}_{it}$ to the regression using the headline measure of job-to-job transitions from the LEHD

[4] The rotating panel structure of the CPS also enables us to identify new hires but offers a much smaller sample than the QWI. We have verified that our results in Table 1 using all workers are robust to measuring earnings using the outgoing rotation groups in the CPS.

[5] Since α_t controls for aggregate variation in prices, we measure W_{it} as nominal monthly earnings

[6] The headline measure is known as J2JHire, in the parlance of the LEHD program. The measurement associated with case (i) is known as EEHire.

(cases (i) and (ii)); and column (3) repeats column (2) but uses the measure of job-to-job transitions that includes only within-quarter transitions (case (i)).Panel A of the table reports results where W_{it} is measured as average earnings of all workers, and Panel B focuses in on new hires.[7]

Table 1. Baseline results

	Panel A] Dependent variable: Log of average earnings		
Regressor	[1] No $\Lambda_{it}^{\mathrm{e}}$	[2] Headline $\Lambda_{it}^{\mathrm{e}}$	[3] Within-qt. $\Lambda_{it}^{\mathrm{e}}$
$\Lambda_{it}^{\mathrm{u}}$	0.099 [0.032]	0.011 [0.022]	0.011 [0.023]
$\Lambda_{it}^{\mathrm{e}}$		3.948 [1.567]	5.436 [2.164]
N	2592	2568	2569
	Panel B] Dependent variable: Log of new hires' earnings		
$\Lambda_{it}^{\mathrm{u}}$	0.179 [0.079]	-0.011 [0.041]	-0.009 [0.039]
$\Lambda_{it}^{\mathrm{e}}$		8.501 [3.806]	11.61 [5.201]
N	2559	2535	2536

NOTE: All regressions include state and quarter fixed effects. Standard errors are clustered at the state level.

The key takeaways from Table 1 are as follows. First, in the absence of $\Lambda_{it}^{\mathrm{e}}$, a higher $\Lambda_{it}^{\mathrm{u}}$ does map to higher wages, as anticipated. The quantitative influence of $\Lambda_{it}^{\mathrm{u}}$ is slight, though, and its statistical significance is not robust across the panels. Second, when $\Lambda_{it}^{\mathrm{e}}$ is introduced, any significance of $\Lambda_{it}^{\mathrm{u}}$ disappears. Further, the estimated semi-elasticity of wages with respect to $\Lambda_{it}^{\mathrm{e}}$ is remarkably large: a one percentage point increase in $\Lambda_{it}^{\mathrm{e}}$ implies between 4 (panel A) and 8.5 percent (panel B) higher monthly earnings.[8] Third, the association between $\Lambda_{it}^{\mathrm{e}}$ and earnings is stronger among new hires, as anticipated.

As a specification check, we have repeated the estimation of (1) using the log *change* in earnings as the outcome variable. This is the specification employed by MPV (2016a). The results confirm the importance of job-to-job transitions, as measured by $\Lambda_{it}^{\mathrm{e}}$. We again find that $\Lambda_{it}^{\mathrm{e}}$ is statistically

[7] "All workers" in quarter *t* refers, specifically, to the "stable" subset of employees who are associated with a single employer in quarter *t*, and who receive positive earnings from that employer in quarters *t*-1 and *t*+1. Stable new hires are workers who accede to a new employer (which excludes recalls) and whose first full quarter of employment with their new employer is quarter *t*. To satisfy a full quarter of employment, these new hires must also receive earnings from their quarter-*t* employer in *t*-1 and *t*+1. The restriction to the stable subset removes very short-duration matches, the causes of which are arguably outside the scope of canonical on-the-job search models. This being said, our findings in Table 1 are robust to dropping this restriction.
[8] It is tempting to use the estimates in Table 1 as estimates of elasticity of aggregate earnings with respect to changes in labor market conditions. However, as Beraja, Hurst, and Ospina (2016) and Karahan, Pugsley and Şahin (2016) argue, local and aggregate elasticities to the same type of shock might vary substantially due to various reasons.

and economically significant; dominates $\Lambda_{it}^{\mathrm{u}}$ as a predictor of wages; and is particularly strongly associated with new hires' earnings.[9]

Taken together, the results in Table 1 suggest strong support for the claim that on-the-job search is critical to cyclical wage dynamics.

2. Sensitivity analysis

In this section, we probe the robustness of the results in Table 1. In particular, we take up a number of concerns regarding how well our measurements, Λ^{e} and Λ^{u}, capture the theoretical objects of interest, λ^{e} and λ^{u}, respectively. As we shall see, our choice of measurements is arguably favorable to our null hypothesis that λ^{e} is the more significant determinant of wage dynamics, and could thus lead us to mistakenly accept the null when it is in fact incorrect.

Arrival v. Acceptance. The first concern is that we use the *realized* transition probabilities, Λ^{e} and Λ^{u}, as proxies for the arrival rates of job *offers*, λ^{e} and λ^{u}. In fact, the realized transition probabilities are the product of the arrival rate of offers, and the probability of acceptance. For the unemployed, there is now evidence that a substantial fraction of unemployed who transition to employment have, in fact, only one offer (Faberman et al 2016). Thus, it is perhaps reasonable to use Λ^{u} as a proxy for λ^{u}. However, this strategy is less suitable with respect to λ^{e}.

The concern here runs deeper than classical measurement error. The acceptance probability is shaped by the distribution of wages (among the employed). Thus, insofar as the acceptance probability is impounded in Λ^{e}, we gave Λ^{e} a "headstart" in its horserace with Λ^{u}: by construction, there is information about wages—our *outcome* variable—embedded in Λ^{e}.

How do we extract the arrival rate from the data? We begin with the observation that, under the assumption (consistent with BM) that matches are made randomly, Λ^{e} is given by $\Lambda^{\mathrm{e}} = \lambda^{\mathrm{e}} \int G(w)\mathrm{d}F(w)$, where G is the c.d.f. of wages among the employed and F is the c.d.f. of wage offers. This says that the measured job-to-job transition probability is the product of the arrival and acceptance probabilities, where the latter is obtained by integrating $G(w)$, the probability that a worker's wage is less than her offer w, against the density function of offers. Since G is available from the data, we could solve this expression for λ^{e} *if* we knew F.

The final step, then, is to infer the offer distribution. We proceed under the null that the BM model is correct. In that case, BM provides, as we detail in the Appendix, a parsimonious map from G to F. We use this map to infer F and solve for λ^{e}, given Λ^{e}. Note that we apply this procedure to the

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[9] A one percentage point increase in $\Lambda_{it}^{\mathrm{e}}$ implies an acceleration in monthly earnings growth of between one-quarter (all workers) and one-third (new hires) of a percent.

“headline” estimate of the job-to-job transition probability, since Table 1 indicates that results are robust to alternatives.

Table 2 reports results using this “purified” measure of the arrival rate, denoted by $\hat{\lambda}^{\mathrm{e}}$. The outcome variable is log average earnings among all employed in column (1) and log new hires’ earnings in column (2). Once again, the arrival rate of offers to the employed dominates Λ^{u} as a predictor of earnings. However, the semi-elasticity of earnings with respect to $\hat{\lambda}^{\mathrm{e}}$ is now between 1 (column (1)) and 2 and a quarter (column (2))–substantially smaller than we obtained using Λ^{e} in Table 1. This suggests that our procedure does indeed remove variation in the acceptance probability from the realized transitions (e.g., Λ^{e}).

Time aggregation. A second concern is that the LEHD measures transitions at a quarterly frequency. As a result, the published estimates suffer from a time aggregation bias. Specifically, consider the case of within-quarter transitions. If the Census observes that a worker is employed by two firms in a quarter, a job-to-job transition is recorded. However, this worker could have instead separated into non-employment, but found a new employer before the end of the quarter. If the latter occurred, it would be spuriously recorded as a job-to-job move. Note that this form of measurement error again tips the scales in favor of our null hypothesis that λ^{e} drives wage dynamics, since it impounds variation *due to* λ^{u} into our measurement of job-to-job transitions.

Our approach to adjust for time aggregation is most easily illustrated for the case where the measurement interval is bi-monthly but the primitive unit of time at which labor market activity takes place is one month. In what follows, then, let $\Lambda_t^{\mathrm{e}}(m)$ denote the probability of a job-to-job transition in month $m = \{1,2\}$ of bi-monthly interval t; $\sigma_t(m)$ the probability of transition from employment to non-employment; and $f_t(m)$ the probability of transition from non-employment to employment. Note that both $\sigma_t(m)$ and $f_t(m)$ can be measured from CPS data.

A number of scenarios can lead to a measured job-to-job transition. First, a worker can make a job-to-job transition in the first month of the interval, which happens with probability $\Lambda_t^{\mathrm{e}}(1)$. (We shall treat the probability of a subsequent job-to-job transition within the interval as negligible.)[10] Second, a worker can remain with her employer in the first month but move to a new employer in the second. This happens with probability $\left(1-\sigma_t(1)\right)\left(1-\Lambda_t^{\mathrm{e}}(1)\right)\Lambda_t^{\mathrm{e}}(2)$. Third, a worker can exit employment in the first month but match with a new employer in the second. This event occurs with probability $\sigma_t(1)f_t(2)$. In the bi-monthly records, this is recorded, spuriously, as a job-to-job transition. Thus, of the three scenarios, only the first two represent genuine job-to-job transitions. To measure the “true” bimonthly job-to-job transition probability, we want to net off $\sigma_t(1)f_t(2)$ from the published estimate. With this corrected estimate in hand, we can then extract the arrival rate in the same manner discussed above.

[10] The worker could also transit out of employment in the second month, but this makes no difference to the measurement of job-to-job transitions: the bimonthly record will still show such a transition.

The Appendix extends (2) to the case where the measurement interval is a quarter. For the U.S. as a whole, we find that the adjusted measurement of the job-to-job transition probability preserves virtually all of the variance of the published estimate and is almost perfectly positively correlated with it. The reason for this is that *products* of transition probabilities, such as $\sigma_t(1)f_t(2)$, are simply far too small in magnitude to drive variation in Λ_t^{e}.

Small samples. A final concern is that our measurement of Λ^{u} relies on CPS state-level data, which can be quite noisy. In contrast, the LEHD measurements of job-to-job transitions are derived from very nearly a census of earnings records. Therefore, the measurement error in Λ^{u} could, once again, lead us to confirm our null, even if it is incorrect.

To address this source of measurement error, we turn to the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS). This dataset does not offer a measurement of the transition probability from unemployment to employment. However, we know that the latter is the primary driver of unemployment variation over any substantial horizon, e.g., over a period more than 4 quarters (Shimer 2012). What the LAUS data do offer is a careful estimate of state-level unemployment rates, which integrates CPS data with auxiliary sources such as unemployment insurance claims and establishment surveys of headcounts. Thus, cyclical variation in the LAUS estimate of the unemployment rate reflects, to a considerable degree, variation in λ^{u} but is less subject to measurement error.

Columns (3) and (4) of Table 2 report estimates of equation (1) when we replace Λ^{u} with the LAUS estimate of the unemployment rate. In each column, we continue to use the "purified" arrival rate, $\hat{\lambda}^{\mathrm{e}}$. Once again, we find that the measured job-to-job transition probability dominates as a predictor of wages for new hires. However, the results for all workers represent a departure from the general theme of Tables 1 and 2, since they show that *both* labor market flows help account for wage dynamics. The coefficient on the LAUS unemployment remains substantial and significant. Moreover, the coefficient $\hat{\lambda}^{\mathrm{e}}$ is more modest in magnitude and marginally statistically significant. Though a single result does not outweigh the preponderance of the evidence presented thus far, it does confirm the importance of our attention to proper measurement of labor market conditions.

Table 2. Robustness

Regressor	[1] Dept. variable: Log average earnings	[2] Dept. variable: Log new hires' earnings	[3] Dept. variable: Log average earnings	[4] Dept. variable: Log new hires' earnings
$\Lambda_{it}^{\mathrm{u}}$	0.016 [0.020]	-0.001 [0.036]		
$\hat{\lambda}_{it}^{\mathrm{e}}$	1.041 [0.406]	2.271 [1.005]	0.817 [0.472]	2.100 [1.109]
LAUS			-0.743 [0.310]	-0.531 [0.514]
N	2592	2559	2592	2559

NOTE: All regressions include state and quarter fixed effects. Standard errors are clustered at the state level.

3. Further questions, final thoughts and policy implications

We have attempted to discriminate between two views of wage setting. On balance, our findings indicate significant support for the importance of on-the-job search to cyclical wage dynamics. There are, however, aspects of our results that are less easily reconciled to canonical theories of on-the-job search. We hope to take up these issues in future research.

First, recall that regression (1) is identified off differences in the cyclical behavior of λ^{u} and λ^{e}. The *source* of these differences is not immediate, however. In standard specifications of the matching technology, a unit of search effort meets a vacancy at the same rate, *regardless* of whether the search is done on or off the job.[11] This means that, if the employed and unemployed vary their effort identically over the business cycle, fluctuations in λ^{u} would mimic those in λ^{e}; the regression (1) would be unidentified. Thus, under standard assumptions on matching, any differences in the behavior of these arrival rates must reflect differences in the cyclical properties of search intensities of the employed and unemployed. However, we are not aware of models that predict such substantially different variation. This missing piece of theory precludes us from offering a more complete interpretation of the variation underlying (1).

Second, though we anticipated that λ^{e} would bear more on wages than λ^{u}, the absence of *any* effect of the latter is puzzling. As shown in Moscarini and Postel-Vinay (2016a), an increase in λ^{u} implies, all else equal, a higher average wage because it raises the reservation wage. This effect can, in principle, be canceled by a concomitant increase in λ^{e}, which lowers the reservation wage; this leaves only the influence of λ^{e} on wage offers *above* the reservation level. However, it seems unlikely that the two would just offset. Though our null hypothesis was that λ^{e} would "win" the "horserace" in (1), we are surprised that λ^{u} plays *no* role at all (with the exception of column (4)

[11] The rate at which a searcher (on or off the job) meets a vacancy depends on market tightness, the ratio of aggregate vacancies to aggregate search effort (see Mortensen 2000; Pissarides 2000; and Moscarini and Postel-Vinay 2016b).

in Table 2, where λ^{u} plays an indirect role). This finding is not easily reconciled to existing theory and merits further research.

Notwithstanding these qualifications, we believe our findings can inform ongoing policy debates. Uncovering the mechanism that links labor market conditions to wage fluctuations is of first order importance to monetary policy. For nearly half a century, policymakers have looked to the unemployment rate specifically as an indicator of pending wage pressures, with the latter presumed to then pass through to higher prices. Recent U.S. experience with low unemployment and relatively quiescent wage growth has called into question whether the unemployment rate is a satisfactory summary of labor market conditions (Blanchard 2016). Our findings indicate that the rate of job-to-job transitions should receive equal attention. The pace of these flows is tightly, and robustly, associated with wage dynamics. The link between job-to-job transitions and wages may offer a more durable foundation for policy analysis.

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