LABOR PRODUCTIVITY AND JOB-MARKET FLOWS
Trends, Cycles, and Correlations

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The views expressed in this paper are those of the author(s), not necessarily those of the Federal Reserve Bank of St. Louis or the Federal Reserve System.
ABSTRACT. I derive measures of U.S. job-separation and job-matching rates from aggregate Current Population Survey data. Using an unrestricted unobserved-components approach, I decompose these series into trends and cycles and compare the results with the trend and cyclical behavior of labor-productivity growth. Both transitory and permanent shocks to productivity are strongly positively correlated with fluctuations in the rate of job matching and negatively correlated with cyclical fluctuations in separation rates. Productivity growth thereby accounts for about a third of the overall variation in the unemployment rate. However, it displays only weak correlation with trend separation rates. Because trend movements in unemployment are dominated by permanent changes in separation rates, productivity shocks alone cannot account for most of the movement in the natural rate of unemployment over time.

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1 Introduction

During the late 1990s, the rate of unemployment in the United States fell to levels that had not been seen in nearly thirty years. Despite the worries of policymakers and academics, this decline occurred without a concomitant rise in inflation. The apparent breakdown of the Phillips Curve has led many economists to hypothesize a decrease in the natural rate of unemployment, driven, at least in part, by increases in the growth rate of labor productivity (Gordon, 1997; Staiger et al., 2001; Ball and Mankiw, 2002; Trehan, 2003). This conjecture breathes new life into an older literature that applied the converse story to the productivity slowdown and rising unemployment of the 1970s (e.g., Grubb et al., 1982; Bruno and Sachs, 1985). For both episodes, the explanation is attractive, because existing empirical work largely confirms an inverse relationship between productivity growth and unemployment, at least at low frequencies. (In addition to the above references, see Hoon and Phelps, 1997; Mincer and Danninger, 2000; Muscatelli and Tirelli, 2001; Hatton, 2002; King and Morley, 2003; and Pissarides and Vallanti, 2003).

Theoretically, the impact of productivity-growth shocks on unemployment is ambiguous. On one hand, higher per-worker productivity growth may result in higher discounted per-worker profits for firm owners—the “capitalization effect”—increasing incentives for hiring and retention. On the other hand, rapid technological change may bring a faster pace of skill obsolescence and job churning, as firms continually seek the most productive workers. As stressed by Bean and Pissarides (1993), Aghion and Howitt (1994), and Mortenson and Pissarides (1998), which of these channels dominates depends crucially on the exact structure of one’s model and what one assumes about the values of the model parameters—in other words, it is indeed an empirical question.

Figure 1 exemplifies the sort of time-series evidence often adduced to support an inverse relationship between unemployment and productivity growth in the U.S. data. It shows quarterly unemployment and annualized labor-productivity-growth rates from 1948:1 through 1999:4, together with their Hodrick-Prescott trends, computed using a rather high smoothing parameter of 20,000. The trends move closer together during the early 1960s, then apart during the 1970s, and then come slowly back to near their starting levels. The fall in trend unemployment and the rise in trend productivity growth both accelerate somewhat during the mid- to late 1990s. At this cursory level, it thus seems as if there is a strong negative correspondence between the two trends and that this correspondence might account, in a causal sense, for the unusual events of the so-called New Economy.

Of course, there is no particular reason to view smoothing at 20,000 as correct—the trends could well be reflecting correlations that are truly cyclical and thus not appropriately regarded as changes in the steady state. If, as suggested by certain theoretical models, the relationship between productivity and unemployment differs in the short and long runs, the resulting trend estimates could be misleading. In addition, as Staiger et al. (2001) note, the two trends in Figure 1 are nonstationary series, and the apparent correlation between them could therefore be spurious. Indeed, closer inspection reveals some uncomfortable discrepancies in the timing of the HP-trend movements: all of the big turning points in productivity growth occur at least a couple of years before the supposedly corresponding turning points in unemployment. From a technical standpoint, it does not make sense to attribute these delays to causation with a lag because, if the

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1 Throughout the paper, I measure labor productivity as real output per employee, as reported in the BLS establishment survey, rather than the more common output per hour. Theoretically, in making employment decisions, firms should care about the total profits a worker will generate, whether they accrue from a higher markup over wages or more hours worked. Moreover, simple correlations between the series suggest that unemployment’s association with per-worker productivity is stronger. Nevertheless, using output per hour yields similar results.
trends are viewed as steady-state values, their innovations should exhibit no dynamic correlation. Furthermore, if the post-1999 data are included (not shown), the correspondence seems to break down, as both trends move sharply upward in the final years of the sample.

The ideal way to get at the correlations between the trends of two series—and perhaps their causality—is to estimate those trends jointly in the context of a structural model. In principle, one could perform this estimation for a bivariate unemployment-productivity system. However, this approach would ignore important theoretical insights into the way that productivity is likely to affect unemployment in both the short and long run. The dynamics of the unemployment rate are determined by the underlying flows into and out of unemployment, particularly by the rates at which individuals match with and separate from jobs. Separation and matching rates exhibit much different time-series properties, and there are good reasons, both theoretical and anecdotal, to suspect that productivity shocks affect them differently. Moreover, if we view the natural rate of unemployment as the steady-state rate at each point in time, then its value must be consistent with the steady states of these flows. Yet no previous natural-rate estimation, to my knowledge, has attempted to ensure that this consistency holds.\(^2\)

In this paper, I develop measures of the key labor-market flow rates based on aggregate data from the Current Population Survey. Both separation and matching rates exhibit substantial variation over the 57-year sample and contribute roughly equally to the total variance of unemployment. Still, an open question is whether these fluctuations are primarily permanent or transitory. Using an unrestricted unobserved-components approach, I decompose the flow rates into trends and cycles. The most striking result to emerge is that separation rates are dominated by permanent changes, while matching rates exhibit large cyclical fluctuations around a slowly moving trend. Finally, I examine the relationship between the flow rates and labor-productivity growth. I find that the cyclical movements in both series are highly correlated with productivity shocks. In addition, permanent changes in the rate of job matching are almost perfectly correlated with permanent changes in productivity growth. Most of the cyclical variation—and about a third of the overall variation—in the unemployment rate can be explained through these channels. However, trend separation rates are only weakly correlated with productivity growth. Because trend changes in unemployment result mainly from trend changes in separation rates, productivity cannot, therefore, account for most of the changes in the natural rate of unemployment, including its post-1990 decline.

The paper proceeds as follows. The next section briefly reviews the theoretical and empirical research linking job-market flows and unemployment to labor-productivity growth. Section 3 explains my computation of the separation- and matching-rate time series, which involves the correction of some non-trivial biases that have been largely overlooked by previous studies. Section 4 presents the trend-cycle decomposition of the two flow-rate series and productivity growth and provides an estimate of the time-varying steady-state (i.e., “natural”) unemployment rate. Section 5 assesses the importance of productivity in determining the labor-market variables by deriving a series of orthogonal structural shocks using short-run restrictions. I consider both the impulse-response functions associated with these shocks and counterfactual experiments in which the permanent and cyclical productivity shocks are removed. Section 6 concludes.

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\(^2\) Hall (2003) estimates a steady-state unemployment rate for the period 2000-2002 using flow-rate averages over this period, but he does not allow for trend movements in any of these series.
2 Productivity Growth and Unemployment: Theory and Evidence to Date

2.1 Search and Matching in Theory

The idea that labor productivity and unemployment could be negatively correlated has an intuitive theoretic appeal: for a given real wage, higher productivity translates into higher profits per worker, and thus firms have an incentive to do more hiring and less firing when they expect productivity to grow quickly. This “capitalization effect” is the essential mechanism behind most modern theoretical models of unemployment, including the search and matching models popularized by Mortensen and Pissarides (1994) and surveyed in Pissarides (2000) and Rogerson et al. (2004), as well as alternative but related approaches, such as those presented by Lucas and Prescott (1974), Blanchard and Diamond (1989), and Phelps (1994). Several recent papers, including Merz (1995), Adolfatto (1996), Cooley and Quadrini (1999), Den Hann et al. (2001), and Trigari (2003), have built on such models to incorporate labor-market flows into more comprehensive business-cycle systems. In all of these papers, unemployment fluctuations arise primarily—sometimes entirely—from the propagation of aggregate productivity shocks, inducing a negative correlation between the series.

On the other hand, versions of the labor-market search model also exist in which the effect of increased productivity growth on unemployment is ambiguous or even strictly positive. For example, Aghion and Howitt (1994) study a model in which the capitalization effect competes against a “creative destruction” channel, whereby technological change increases separations, as workers are displaced, and decreases matches, as skills become obsolete. Although they only consider permanent shocks, temporary positive productivity fluctuations may be even more likely to increase unemployment. Caballero and Hammour (1994, 1996) emphasize these “cleaning” effects of recessions and show that the empirical pattern of job creation and destruction is broadly consistent with this type of effect. Similarly, Gali (1999) argues that, in the short run, firms reduce employment in response to positive technology shocks. Carre and Drouot (2004) show that a learning-by-doing channel can amplify this effect in some states of the world. Postel-Vinay (2002) examines a model in which creative destruction causes a positive long-run response of unemployment to productivity growth, but this effect is swamped in the short-run by a large negative association. As mentioned in the previous section, prior empirical work on the unemployment-productivity tradeoff has made little effort to distinguish rigorously between trend and cyclical behavior. It is important to make this distinction, because it is likely that the qualitative relationship between productivity and the labor market depends on whether one is considering permanent or transitory behavior.

Empirically, labor-market search models seem capable of replicating many features of the data. Cole and Rogerson (1999) and Yashiv (2000) have argued that their empirical evidence largely supports the basic search framework, with the latter paper, in particular, finding large effects of productivity growth on unemployment for Israeli data. However, in the context of

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3 Another literature, which produces outcomes similar to search theory, though it differs in spirit, suggests that workers come to demand a certain level of wages, based on recent history, and that when productivity growth falls below the growth of these “wage aspirations,” unemployment results. This theory, apparently due originally to Grubb et al. (1982), has recently been resurrected by Ball and Moffitt (2001) and Ball and Mankiw (2002).

4 There is, of course, an echo here of an earlier literature exploring the extent to which productivity shocks alone could account for fluctuations in employment and output in dynamic-equilibrium models. Although Real Business Cycle theory (e.g., Long and Plosser, 1983) is often pilloried for its inadequacies along labor-market dimensions, these failures should not reflect poorly on search-and-matching models, which rely on a different set of structural assumptions and examine a different set of variables—most notably, unemployment. Indeed, many of the papers cited here demonstrate that amending models that are essentially RBC with search frictions brings them into closer harmony with the labor-market data.
these models, Hall (2003) and Shimer (2005a) argue that productivity shocks by themselves are not sufficient to account for the magnitude of the observed fluctuations in unemployment, and vacancies. The latter paper hypothesizes real-wage rigidity as a contributing factor. In addition, Fujita (2004) points out that, in standard versions of the model, productivity shocks cannot generate the high persistence of vacancies observed in the data. On the other hand, Shimer (2003) and Krause and Lubik (2004) suggest that, once job-to-job transitions are taken into account, the impact of productivity on both unemployment and vacancies may be greater. In summary, despite an extensive literature modeling productivity and unemployment under the search-and-matching paradigm, the relationship between these variables remains theoretically ambiguous.

2.2 Empirical Evidence

Despite the centrality of labor productivity and flow rates in labor-market search theories, very little work directly investigates the empirical relationships between these variables. The evidence that does exist largely emanates from calibration of the theoretical models. Although these calibration exercises have been successful in replicating many salient features of cyclical unemployment rates and other labor-market variables, they retain some potential shortcomings. First, calibration studies almost always rely on univariately detrended data, thus ignoring the determinants of trend fluctuations in the series. Consequently, they are silent about the long-run features of the data, such as those suggested by Figure 1. Second, search models are seldom calibrated to or evaluated in terms of the flow rates themselves. Indeed, many studies (e.g., Andolfatto, 1996; Cole and Rogerson, 1999; Trigari, 2003; Nason and Slotsvne, 2004) simply assume that the separation rate is constant. As shown below, this assumption is highly inconsistent with the data. Den Hann et al. (2001) and Fujita (2004) argue that allowing for endogenous separation rates can strongly influence the empirical performance of such models. In addition, ignoring the properties of the flow rates necessarily means that certain features of the data will go unexplained. For example, Abraham and Shimer (2001) and Mukoyama and Sahin (2004), among others, have noted substantial increases in unemployment duration during the 1990s, at the same time that the natural rate is supposed to have fallen. Similarly, Fedorov (2003) shows that controlling for duration improves the fit of the Phillips Curve and, he argues, estimates of the NAIRU. Because higher unemployment duration is tantamount to a lower rate of job matching, these observations can only be reconciled by an offsetting decrease in separations. Most of the existing calibrated theoretical models have little to say about these phenomena.

Beyond calibration, econometric work has also tended to focus on the relationship between productivity and unemployment, eschewing the analysis of flow rates. On the time-series side, Blanchard and Diamond (1989) estimate a structural VAR in unemployment, vacancies, and labor-force participation. Although they do not look at productivity shocks per se, their closest analogues, “aggregate” shocks, have large effects on unemployment in the short run. However, the direction of this effect is imposed as an identifying assumption, making it untestable. Trehan (2003) also examines unemployment in structural-VAR models but focuses specifically on productivity growth. Although he assumes stationarity for both series, eliminating the possibility of long-run correlations, he finds a strong and persistent negative relationship in the short run. King and Morley (2003) estimate the natural rate of unemployment using a structural VAR and compare its value over time to a vector of labor-market variables. In their primary specification, labor productivity is among the most significant of these explanatory factors, with a negative sign. With emphasis on the experience of the 1990s, Ball and Makiw (2002) estimate a natural rate of unemployment by assuming a Phillips Curve and compare it to the HP trends in productivity growth, similarly to my Figure 1. Also focusing on the 90s, Mincer and Danninger (2000) emphasize the skill acquisition associated with new technologies and conclude that much
of the recent decrease in unemployment can be attributed to this channel, although they also ascribe importance to demographics and international trade. Finally, Hatton (2002) examines over 100 years of productivity, wage, and unemployment data from the United Kingdom, allowing for structural breaks in the dynamics. He finds a negative effect of growth on unemployment but emphasizes that this mechanism can only account for some of unemployment’s long-run movements.

Other papers have employed cross-sectional techniques. Looking at simple scatter plots, Bean and Pissarides (1993) and Hoon and Phelps (1997) find only weak correlation between unemployment and labor-productivity growth across developed countries. In contrast, Basile and Benedictis (2004) find strong negative cross-sectional correlations between productivity growth and unemployment in Europe in the late 1990s, although they do not find this relationship across U.S. states. Pissarides and Vallanti (2003) and Vallanti (2004) estimate a theory-based structural model on OECD panel data and simulate the responses of various types of shocks to total-factor productivity. They conclude that positive TFP shocks have negative effects on unemployment, and that the 1970s increases and 1990s decreases in unemployment are attributable to movements in productivity growth. However, they have less success in accounting for short-run fluctuations, particularly in Europe. A general difficulty with the cross-sectional and panel approaches is that, as Blanchard and Wolfers (2000) argue, cross-country differences in labor market institutions (e.g., unemployment-insurance systems, collective-bargaining arrangements, minimum-wage laws) may affect the productivity-unemployment relationship. If this is the case, cross-sectional and panel models that constrain parameters to be identical across countries could be misspecified.

Three papers that have gone beyond unemployment rates to look at underlying flow series are Blanchard and Diamond (1990), Davis and Haltiwanger (2001), and Collard et al. (2002). All three technically examine job flow rates (i.e., job-creation and -destruction rates), rather than worker flows per se. Blanchard and Diamond (1990) extend their (1989) structural VAR model to incorporate flow data. They find that job-destruction rates turn strongly negative following a positive “aggregate” shock, whereas job-creation rates decline by a smaller magnitude. (The addition of the flow variables to the model does not change their earlier finding of a large short-run effect of aggregate shocks on vacancies and unemployment.) Similarly, in an estimated search model, Collard et al. (2002) find opposite short-run responses of job-creation and job-destruction rates to an aggregate (as opposed to reallocative) shock, with the effect on job creation being somewhat smaller. Davis and Haltiwanger (2001) look at the impact of oil-price shocks on the labor market, and find that they can account for about a quarter of the variation in employment growth between 1972 and 1988, mainly through job destruction. Although all three of these papers find strong responses of job-flow rates to their respective shocks, none of them explicitly considers productivity growth or attempts to distinguish between cyclical and permanent innovations in the series.

Overall, the extant empirical evidence comes down on the side of a negative relationship between productivity growth and unemployment, at least for U.S. data. However, as I have tried to emphasize, little of this previous research has looked specifically at the relationship between productivity and worker flow rates, and it has largely ignored potential differences between short- and long-run correlations. The following subsection discusses some reasons that it might be worthwhile to examine these issues.

2.3 Labor Flows and Steady-State Unemployment

Although there is conflicting thinking about whether unemployment should be treated as stationary for empirical purposes (see Mocan, 1999, and Papell et al., 2000), a time-varying natural rate of unemployment is only meaningful if unemployment is allowed to have some sort of trend component. In estimating this trend, most authors have imposed some type of arbitrary
smoothing restriction. For example, Staiger et al. (1997) assume that the trend is a deterministic third-order polynomial function of time. Gordon (1997) estimates the time-varying natural rate (or, more precisely, the NAIRU) by choosing the value of unemployment that best fits a Phillips Curve, subject to an arbitrarily chosen standard deviation for its innovations. Ball and Mankiw (2002) use a similar procedure, but smooth the natural rate with an HP filter, providing results similar to those in Figure 1. Staiger et al. (2001) use a band-pass filter on unemployment with a chosen cutoff frequency of 15 years. As suggested above, the relationship between productivity and unemployment is likely to be different at low and high frequencies (see, e.g., Postel-Vinay, 2002). Therefore, arbitrary smoothing restrictions, which are likely to mix both horizons inappropriately, are not necessarily innocuous.

In addition, one must take care that estimated trends and cycles in unemployment are consistent with those in the underlying labor-flow series. Indeed, ensuring this consistency is a central theme of this paper. Despite continuing interest in generating better measures of labor-market flow rates, no serious attempt to distinguish the trend and cyclical behavior of these rates has been made. This is somewhat surprising, given the emphasis on time-varying trend rates of unemployment in discussions of the Phillips Curve and other macroeconomic issues.

To see the importance of this point, consider the law of motion for the unemployment rate. In continuous time and with no changes in labor-force participation, unemployment as a percentage of the labor force evolves according to the first-order differential equation

\[ \dot{u}_t = \lambda(1-u_t) - \mu u_t, \]

where \( \lambda \) is the rate of transition from employment to unemployment (i.e., job separation), \( \mu \) is the rate of transition from unemployment to employment (i.e., job matching), and I have used the fact that the ratio of employment to the labor force is simply equal to \( 1 - u_t \). The steady state of this process is given by

\[ u^* = \frac{\lambda}{\lambda + \mu}. \]

Throughout the paper, I will interpret this steady-state value as the natural rate of unemployment, following Phelps (1994), Hall (2003), and others.

For an initial value of the unemployment rate \( u_0 \), the solution to equation (1) at time \( s \) is

\[ u_s = u^* + (u_0 - u^*)\exp[-s(\lambda + \mu)]. \]

Thus, for given separation and matching rates, unemployment decays exponentially toward the natural rate. Note, however, that the rate at which it decays depends on the values of both \( \lambda \) and \( \mu \). To the extent that these flow rates vary over time, the out-of-steady-state dynamics of \( u \) will not be constant. Indeed, a time-varying steady-state rate of unemployment is only possible if one allows for time variation in \( \lambda \) and \( \mu \). This necessarily implies time variation in the parameters governing the short-run dynamics of \( u \). Attempts to estimate a time-varying \( u^* \) that do not allow for time variation in these parameters result in a fundamental inconsistency and misestimate \( u^* \), even if no arbitrary smoothing restrictions are imposed. The only obvious way to get around this

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5 Deterministic linear and polynomial trends are technically impossible for unemployment rates, because they must eventually travel outside the [0,1] range. A similar criticism applies to treating the trend as a random walk. In small samples, however, these specifications may yield good approximations.

6 To be fair, he does estimate this standard deviation in Gordon (1998).
problem is to focus on the flow rates themselves, building up estimates of $u^*$ from the estimates of the trends in the flows.

One reason that both evaluation of theoretical unemployment models and empirical attempts to estimate the natural rate have shied away from the flow-rate perspective may be that reliable aggregate measures of these rates have been difficult to obtain. Indeed, this lack of data has prevented even the accumulation of a set of stylized facts for labor market behavior, so that there remain disagreements over such fundamental issues as whether separation or matching rates primarily drive unemployment fluctuations. A sizeable literature has consequently emerged that attempts to estimate labor-market flow rates through various techniques. Davis and Haltiwanger (1998) provide a survey of many of these methods. The best data currently available for this purpose are probably those provided by the Job Openings and Labor Turnover Survey (JOLTS). However, JOLTS was first administered only in December 2000, making it useless for extensive time-series analysis.

At first glance, the most promising method for deriving longer series of worker flows appears to be to use matched records from the Current Population Survey to track individuals’ labor-market status across months. Unfortunately, flows computed using the CPS micro data suffer from certain well known biases, primarily introduced by misclassification errors, for which only imperfect correction methods exist (Abowd and Zellner, 1985; Poterba and Summers, 1986). The BLS briefly reported these “gross flow” series publicly in the 1950s before concluding that they were too unreliable. Even so, several studies have pressed ahead with these data, often employing complex corrective measures to reduce the biases (e.g., Blanchard and Diamond, 1990; Bleakley et al., 1999). Unfortunately, the records necessary for these corrections do not extend back prior to 1967. Another popular dataset used in the analysis of labor-market dynamics is the Longitudinal Research Datafile (LRD), analyzed extensively by Davis et al. (1996). Although these data contain detailed information on the creation and destruction rates of jobs, they are less informative about the flows of workers. Moreover, they only cover the period 1963 through 1991 and only the manufacturing sector.

In practice, the greatest barrier to the use of both the LRD and CPS micro data may be that the datasets necessary to employ them, which are micro-level and cumbersome, are not available to most researchers and can require substantial institutional expertise to manipulate and interpret. The measures of job matching and job separation I derive in the following section use readily available aggregate data from the Current Population Survey. Although my technique suffers from its own limitations, it is easy to implement and, overall, produces series that are consistent with other methods.

3 Computing Labor-Market Flow Rates

Although a few other researchers have relied on the aggregate CPS data to compute flow rates, the unadjusted data they have used suffer from several biases, in some cases substantial, which render them suspect unless appropriately corrected. By employing to a continuous-time framework and imposing a few mild assumptions, I derive the necessary corrections and produce what I argue are good estimates of separation and matching rates. An additional advantage of the continuous-time formulation is that it permits the computation of the “instantaneous” transition probabilities used in equation (1)—i.e., the density at zero of exponential probability distributions for separation and matching. This allows one to back out several interesting quantities in each period, including cumulative transition rates over arbitrary horizons, expected durations of employment and unemployment spells, and steady-state rates of unemployment and employment.

7 These are also sometimes known as “hazard” or “escape” rates.
These are values that can only be estimated imprecisely using the conventional discrete-time measures.

The basic idea behind the calculation begins with the discrete time analogue of equation (1):

\[
(4) \quad u_t = u_{t-1} + p_t(separation_t | employment_{t-1})(1-u_{t-1}) + p_t(matching_t | unemployment_{t-1})u_{t-1},
\]

where \(p_t\) indicates the probability of an event occurring between times \(t-1\) and \(t\), and I have assumed that no intra-period separation and matching takes place—i.e., each worker can make only one transition per period. Thus, under these assumptions, the job flows net of intra-period transitions are identical to the gross flows. (Ultimately, we will want to relax this restriction, allowing for positive intra-period transition rates.) Given data on unemployment rates and one of the net flow rates, we can back out the series for the other net flow rate from equation (4).

The best available aggregate measure of the net rate of job separation is the short-term unemployment (STU) series reported in the BLS’s Current Population Survey. When the survey is administered, the interviewer first asks a series of questions to determine the subject’s current labor-force status (employed, unemployed, or out of the labor force). If the subject is classified as unemployed, the next series of questions slots him into a cohort based on the duration of his current unemployment spell—less than five weeks, five to thirteen weeks, thirteen to 26 weeks, or over 26 weeks. Ignoring measurement error, the number of individuals slotted in the first category corresponds to the number of workers who lost their jobs and neither found a new job nor exited the labor force during the previous five weeks.

As a first approximation, this STU number may be viewed as representing the number of separations over the previous month. Dividing by the previous month’s employment then provides a measure of the separation rate. Assuming a fixed labor-force size and no intra-period flows then allows us to calculate an approximation of the net matching rate over the period, using equation (4). Merz (1999), Mukoyama and Sahin (2004), and Shimer (2005) have examined the time-series properties of the worker-flow rates computed in this manner. Davis et al. (1995) compute similar measures and show that their correlation with the flows computed from the CPS micro data is over 90%. Unfortunately, there are several possible sources of measurement error that arise when using STU to proxy separations, and in some cases the resulting biases can be nontrivial. Previous work using STU to measure separations has largely ignored these problems.

The first and most prosaic bias arises from the simple fact that months vary in length and none of them is as long as 35 days, which is the length of time covered by the survey question.\(^8\) Thus, STU always overstates monthly net separation, and the amount by which it overstates it depends on which month is being considered. On average over a year, the resulting bias is 14.9% (assuming that there are no systematic differences in separation rates between each pair of monthly and 35-day periods). This problem is easy to correct by simply multiplying STU by \(D/35\), where \(D\) is the number of days between the surveys conducted for month \(t-1\) and month \(t\). However, previous authors do not appear to have carried out this computation.

Second, as noted by Polivka and Miller (1998), the CPS redesign that occurred in January 1994 makes the pre-1994 unemployment-duration data incomparable with the more recent numbers. The break in the series at this point is evident from visual inspection. Most authors have ignored this issue altogether. Shimer (2005b) makes note of its influence on some of his calculations. One possibility for dealing with the problem for trend-cycle decomposition

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\(^8\) Some previous authors have interpreted STU as referring to individuals that have been unemployed zero to four weeks, apparently inferring this from the fact that the number of weeks reported by the survey respondents can take only integer values. However, the survey question asks for the number of full weeks of unemployment that the subject has experienced (see Bureau of Labor Statistics, 2000). Thus, an individual who has been unemployed for 34.9 days at the time of the survey should, at least in principle, report four weeks and be slotted into STU.
purposes is to allow for a structural break in the filter estimation. However, this imposes that the true pre-break and post-break means are equal (up to deterministic drift), which, given the persistence of the separation-rate series, seems unlikely. The alternative approach that I adopt is to assume that the 1993:4 and 1994:1 average percentages of STU in the labor force were equal.\(^9\) This obviously introduces some measurement error as well, but it should not affect the filtered estimates of trend and cycle beyond a few periods after the revision. Because the post-revision data are presumably more accurate, I move the pre-revision data downward to achieve the equality, although this is just a choice of level and has no effect on the time-series properties of the data. This adjustment of the aggregate series is similar in spirit (and in effect) to that used by Polivka and Miller (1998) and Bleakley et al. (1999) on the CPS micro data.

Third, STU may result from either employment-to-unemployment transitions or entry into the labor force. Conversely, job separations may result in either unemployment or exit from the labor force. By equating STU with separations, one ignores the effects of changes in participation rates. Blanchard and Diamond (1989) and Bleakley et al. (1999) estimate that the average flows between participation and non-participation are at least as large as those between unemployment and employment, so that this problem could have a large effect. To some extent, the issue is simply one of labeling—we can accurately call the STU-based measure “unemployment inflows” rather than “separations” without worrying about where these inflows come from. This is the approach taken, for example, by Merz (1999). However, when combining STU with other labor-market data to construct the matching-rate series, the assumption matters. Note that the problem will only affect the time-series properties of the data to the extent that the nonparticipation-to-unemployment flow is correlated with the employment-to-unemployment flow. For example, Shimer (2005b) examines the sensitivity of his results to this assumption, using CPS micro data, and concludes that it is not serious. Still, this problem demands further work.

Finally, even assuming away changes in participation, STU measures net separations, whereas gross separation rates are typically of more interest. Specifically, if we are interested in the rate at which people become unemployed, it is most useful to know the total number of job losers in a given period. STU underestimates this number, because some job losers find new work before being surveyed and thus are never counted as unemployed. This is a common problem with virtually all measures of job-market flows, and it is rarely corrected. The resulting bias increases with the time-aggregation interval and also with the levels of the true flow rates (so that we cannot even view the rates of change in the series as unbiased).

The problem can be corrected by reverting to the continuous-time specification for separation and matching within each month. If we assume that separations follow a Poisson process with constant arrival rate during this interval, then the time until a separation occurs, conditional on a worker being employed, is given by an exponential distribution with mean \(1/\lambda_t\). By the properties of the exponential distribution, \(\lambda_t\) is the probability density at zero. Assuming a similar process for job matching, the instantaneous match rate is \(\mu_t\), and the expected time until a match is \(1/\mu_t\) (provided there are no forecastable movements in \(\mu_t\)).

The total percentage of separations that occur among the initially employed during the month—or, equivalently, the probability of an initially employed worker becoming unemployed—is given by

\[^9\] I equate the quarterly data rather than monthly because, although the revised survey was instituted in January 1994, the large break in the series does not appear until the February numbers. The quarterly data also have the advantage of reducing the effect of month-to-month noise on the adjustment.
where, again, \( D_t \) is the number of days in month \( t \). Similarly,

\[
p_t(separation, \mid employment_{t-1}) = \frac{\int_0^{D_t} \lambda_t \exp[-\tau \lambda_t] d\tau}{\mu_t \exp[-D_t \lambda_t]}
\]

The probability that an unemployed individual will find a job and lose it within a month is equal to the cumulative probability of finding a job at each point in the month, times the cumulative probability of losing a job over the remainder of the month:

\[
p_t(separation, \mid unemployment_{t-1}) = \frac{\int_0^{D_t} \mu_t \exp[-\tau \mu_t] d\tau}{\mu_t (1 - \exp[-D_t \mu_t])}
\]

Similarly,

\[
p_t(matching, \mid unemployment_{t-1}) = \frac{\int_0^{D_t} \lambda_t \exp[-\tau \lambda_t] d\tau}{\mu_t (1 - \exp[-D_t \mu_t])}
\]

which evaluates to the same quantity as equation (7).

If we assume a maximum of two transitions per individual per month (so that it is not possible, for instance, to begin the month employed, lose one’s job, find a new job, and lose the new job by the end of the month), then equations (5) through (8) exhaust the possibilities. The total number of separations over the month, normalized by the initial labor-force size, is

\[
\lambda_t = (1 - u_{t-1}) p_t(separation, \mid employment_{t-1}) + u_{t-1} p_t(separation, \mid unemployment_{t-1})
\]

The total number of matches is

\[
\mu_t = u_{t-1} p_t(matching, \mid unemployment_{t-1}) + (1 - u_{t-1}) p_t(matching, \mid employment_{t-1})
\]

Maintaining the assumption of no labor-force entry or exit, the unemployment rate in period \( t \) is simply equal to the unemployment rate in period \( t-1 \), plus the separation ratio, minus the match ratio.

\[
u_t = u_{t-1} + \lambda_t - \mu_t
\]
The observed STU number (after adjusting for the month length) is equal to the total number of separations, less the number of separated workers who match during the month. Denoting the level of STU in month $t$, as a proportion of the labor force, as $s_t$,

\begin{equation}
 s_t = \tilde{\lambda}_t - (1 - u_{t-1}) p_t \left( \text{matching} \mid \text{employment}_{t-1} \right)
\end{equation}

These relationships allow us to solve for $\lambda_t$ and $\mu_t$ as functions of the observed data. Specifically, the instantaneous matching rate is given by

\begin{equation}
 \mu_t = \frac{\ln u_{t-1} - \ln(u_t - s_t)}{D_t}
\end{equation}

The instantaneous separation rate $\lambda_t$ is not analytically tractable, but it satisfies the equation

\begin{equation}
 s_t = \frac{\lambda_t (1 - 2u_{t-1}) (1 - \exp[-D_t \mu_t]) - (\lambda_t - \lambda_t u_{t-1} - \mu_t u_{t-1}) (1 - \exp[-D_t \lambda_t])}{\mu_t - \lambda_t},
\end{equation}

which has a unique solution that can be found numerically at each point in time, given values for $\mu_t$ and the observed series.\(^\text{10}\) Summary statistics for the instantaneous separation and matching rates are provided in Table 1.

The instantaneous transition probabilities contain all of the necessary information for analyzing labor-market flows. However, it is often more intuitive to work with cumulative rates. Given the instantaneous probabilities, cumulative rates can be calculated over arbitrary horizons. Of course, if cumulative rates are constructed over each month, shorter months will produce smaller rates. Although these numbers are indeed the quantities we want if we are interested in the probability of a transition during a particular month—shorter months should be associated with lower transition rates, because they allow less time for a transition to occur—they contain an element of artificial seasonality and make inter-month comparisons difficult. When reporting cumulative rates in this paper, I therefore use a constant interval for each month, equal to the average month length (30.4375 days). Because productivity numbers are reported only on a quarterly basis, the econometric analysis in the next section requires the aggregate of the flow rates over quarters. To do this, I compute the quarterly cumulative rate resulting from the three underlying instantaneous rates. I then solve for the single instantaneous rate that is consistent with this value. In other words, I find the unique instantaneous rate that, had it applied for the entire quarter, would have resulted in the same three-month cumulative rate as is produced by the accumulation of the three different instantaneous rates computed from the monthly data. In practice, this procedure gives results that are similar to those that result from a simple summation.

\(^{10}\) Shimer (2005b) has also attempted a time-aggregation correction in the spirit of the one I develop. His calculation differs from mine in four key ways: (1) he does not compute instantaneous transition probabilities, instead working entirely in terms of one-month cumulative rates; (2) although he makes no distributional assumptions about intra-month matching rates, he implicitly assumes a uniform distribution, rather than exponential, for separation rates; (3) rather than computing corrected matching rates, he assumes that Hall’s (2004) figures are correct; and (4) he assumes that the quantity expressed in equation (7) is zero—that is, he corrects for newly separated workers finding jobs before being surveyed, but not for newly employed workers losing their jobs, a situation that arises frequently among temporary workers. The net result of these differences (plus month-length correction described above) is that the adjusted separation rates reported in Shimer (2005b) are about one percentage point higher on average than those I compute. Hall’s (2004) matching rates are about fifteen percentage points higher than mine on average. The general time paths of both series are qualitatively similar to but more volatile than the ones I find.
of the cumulative monthly rates. Summary statistics for the quarterly and monthly cumulative rates are included in Table 1.

Figures 2 and 3 show the monthly cumulative separation and matching rates, corrected for month length and time-aggregation bias. To demonstrate the importance of the corrections, the figures also show uncorrected rates, as computed from equation (4), using STU as the measure of separations. Both the corrected and uncorrected numbers are shown with the 1994 break removed, as described above. For separation rates, the month-length and intra-period-transition corrections nearly offset in most months, so that the corrected series is just 0.1 percentage points higher than the uncorrected series on average, although there are periods where this difference is as high as 0.4 percentage points. For the matching rate, the discrepancy is more serious, with the corrected series averaging 5.2 percentage points less than the uncorrected series and in some months reaching nearly 9 percentage points less. For both series, the corrections seem to make more of a difference during expansions than during recessions, so that they are likely to affect the time-series properties of the data to some degree.

The striking thing about these figures—both the corrected and uncorrected series—is the difference in the apparent behavior of the two flow rates over time. Matching rates exhibit large, procyclical oscillations with only a small downward drift. In contrast, the separation rate seems to be dominated by long-run movements—upward during the 1960s and 70s and downward subsequently. Although one can also detect some apparently cyclical fluctuations around this trend, they are relatively small. The following section will examine these patterns, which Shimer (2005) also notes, more precisely.

Figure 4 shows the intra-period transition rates computed from equations (7) and (8). (Again, these two rates are identical under my assumptions.) On average over the sample, about 0.5% of the labor force has experienced two transitions in each month, according to these calculations. This finding further illustrates the importance of correcting for these intra-period flows, since this value amounts to about 20% of total separations on average. Interestingly, the behavior of the intra-period transition rate is similar to that of the job-to-job transition rate reported in Fallick and Fleischman (2004) over the post-1994 period for which the comparison is possible. This similarity is consistent with a view of job-to-job transitions as a sort of extreme version of intra-period separation and matching.

Fallick and Fleischman (2004) also report that monthly employment-to-unemployment transition rates averaged 1.3% between 1994 and 2004 and unemployment-to-employment rates averaged 29%. Based on my computations, these figures were 2.2% and 32%. Similarly, based on CPS micro data, Bleakley et al.’s (1999) data give matching rates that average 25% for 1967 through 1999, and their separation rate averages 1.3%. My estimates average 34% and 2.7% over this period. However, neither of the other papers accounts for intra-period transitions, which, as shown above, should bias their figures downward by about half a percentage point.

On the other hand, the recently available JOLTS data have small discrepancies in the opposite directions. The JOLTS separation rate (quits plus layoffs as a percentage of employment) averaged about 3.2% between 2001 and 2004, whereas my estimate averages just 2.1% during this time. The JOLTS hiring rate (as a percentage of the unemployed) averaged 56.7%, whereas my estimate was just 31.5%. However, the JOLTS data include both job-to-job transitions and entry and exit from the labor force and thus overstate both the separation and matching rates that are relevant for unemployment. Indeed, the Fallick-Fleischman evidence indicates that this overstatement could be quite severe. (Also see Nagypal, 2004). On the whole, the flow-rate estimates presented here seem to be near the middle of the distribution of previous estimates.

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11 The behavior of matching is consistent with the findings of Valletta (1998), who examines the properties of unemployment duration (the inverse of the matching rate) over time.

12 I have combined Bleakley et al.’s data with CPS aggregate data on unemployment and labor-force size to derive these rates.
4 Trend-Cycle Decomposition

This section examines the relative contributions of trend and cyclical fluctuations in the flow-rate series and looks for correlations between these trends and cycles and those that constitute labor-productivity growth. The analysis proceeds from the premise that labor-productivity growth, separation rates, and matching rates can be meaningfully decomposed into permanent and cyclical components. For the job-transition rates, for which augmented Dickey-Fuller tests cannot reject a unit root, this decomposition should be uncontroversial. Productivity growth, on the other hand, is not very persistent, so that the unit-root hypothesis is rejected at conventional levels. Of course, it is possible that the ADF test does not have sufficient power to detect a low-variance trend in a small sample. Moreover, as will be seen below, the maximum-likelihood value for this trend variance is substantially greater than zero. In any case, the presence of permanent shocks to productivity growth (or, rather, their potential presence—I do not impose nonzero variance) is central to the thesis of productivity growth having long-run effects on unemployment. This is similar to the common assumption of a nonstationary natural rate, despite the weak evidence for a unit root in unemployment.

I estimate trend and cycle components for the three series using an unrestricted unobserved-components (UC-UR) model with an error term in the observation equation to account for measurement error and quarter-to-quarter noise in the data. I specify the cycles as VAR(4) processes. The sense in which the model is “unrestricted” is that I allow all six of the trend and cycle innovations to be correlated with one another. This is in contrast to the standard unobserved-components technique, which typically assumes that all of the off-diagonal terms in the state covariance matrix are zero (or, in some cases, one). It is important to leave these correlations unrestricted here, because one of the central questions I want to answer is whether the trends in the series move together. I also allow the noise terms to be contemporaneously correlated with one another, although they are assumed to be uncorrelated with shocks to both the trend and cycle components.

Explicitly, denoting the productivity growth rate in time $t$ as $p_t$, and combining the three series into the observation vector

$$\mathbf{y}_t \equiv \begin{pmatrix} p_t \\ \mu_t \\ \lambda_t \end{pmatrix},$$

the state-space system is

\begin{align}
(15) \quad \mathbf{y}_t &= \mathbf{Gx}_t + \mathbf{e}_t \\
(16) \quad \mathbf{x}_t &= \mathbf{d} + \mathbf{Fx}_{t-1} + \mathbf{u}_t,
\end{align}

where $\mathbf{d}$ is 15x1 vector containing drift terms for the trend components.

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13 See Canova (1998), Harvey and Koopman (2000), and Morley et al. (2003) for details and other applications of the UC-UR framework. As the Morley et al. paper shows, UC-UR estimates of trend and cycle are identical to those produced by the Beveridge-Nelson (1981) decomposition for univariate models, apart from transitory differences introduced by the priors fed to the Kalman filter. However, Sinclair (2004) points out that this equivalence does not hold in a multivariate context if the specified processes for the cycles over-identify the model, which is the case here.
\[ G = \begin{pmatrix}
1 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\
0 & 1 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\
0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0
\end{pmatrix}, \]
\[ F = \begin{pmatrix}
0 & 0 & 0 & \phi_{p,1}^\mu & \phi_{p,2}^\mu & \phi_{p,3}^\mu & \phi_{p,4}^\mu & \phi_{\mu,1}^\mu & \phi_{\mu,2}^\mu & \phi_{\mu,3}^\mu & \phi_{\mu,4}^\mu & \phi_{\lambda,1}^\mu & \phi_{\lambda,2}^\mu & \phi_{\lambda,3}^\mu & \phi_{\lambda,4}^\mu \\
0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\
0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\
0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\
0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\
0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\
0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 \\
0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 \\
0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 & 0 \\
0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 & 0 \\
0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 & 0 \\
0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 \\
0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 1
\end{pmatrix}, \]

and \( \phi_{j,s}^i \) is the VAR coefficient for the cyclical component of variable \( i \) on the \( s \)th lag of the cyclical component of variable \( j \).\(^{14}\) Again, in the UC-UR framework, the state-variable shocks contained in \( \mathbf{u} \) are all allowed to be contemporaneously correlated. The measurement-error terms contained in \( \mathbf{e} \) are assumed to be uncorrelated with the state-variable shocks but may be correlated with each other. The primary object of interest is the state vector \( \mathbf{x} \), which contains the estimates of the trends and cycles for each of the three series.

Table 2 displays the maximum-likelihood estimates of autoregressive and drift parameters involved in the state equation (16). Table 3 shows the estimated standard deviations of and correlations between the stochastic shocks in the system. Figures 5 through 8 present the trends and cycles that result from these parameters, estimated using the Kalman fixed-point smoother.\(^{15}\) (The cycles are not simply equal to the difference between the trends and the data, due to the presence of the noise term.) To get an intuitive sense of what these numbers mean, the information for the flow rates is shown in terms of quarterly cumulative rates (again assuming all quarters are of equal length), computed from the estimated instantaneous rates through equations

\(^{14}\) Merz (1999) has provided evidence of structural instabilities in the HP-detrended job-market flow dynamics, suggesting that the specification of fixed VAR parameters may be inappropriate. I do not investigate this issue, because allowing for a time-varying \( \mathbf{F} \) matrix introduces complex nonlinearities that cannot be attacked with standard analytical methods. However, I note that my specification is more general than Merz’s—in that it estimates the variance of the trend, allows more autoregressive lags, and incorporates a third variable—so that her results would not necessarily hold here.

\(^{15}\) In running the Kalman filter, I use fairly diffuse priors. Specifically, the prior means of the two trend components are equal to the average values of the series over the first eight quarters. The standard deviations of the prior distributions are equal to twice the standard deviations of the series over the first eight quarters, and the off-diagonal terms in the prior covariance matrix are set to zero. The resulting estimates are fairly insensitive to the choice of priors, however.
Note that, because we are accumulating over quarters here, the magnitudes of are considerably larger than those of the monthly rates reported in Figures 1 and 2. Because the cumulative rates are nonlinear functions of the instantaneous rates, their computed cycles are not symmetric around zero, although in this sample the asymmetry is small.

The most striking thing about these figures is the difference in the time-series properties of the matching and separation rates. Fluctuations in matching rates are dominated by a large, persistent cycle around a trend that moves very little. Although the cyclical component of separations displayed in Figure 5 is also pronounced, it is considerably smaller than the permanent component of this series. These results, which Shimer (2005b) also notes using his construction of the series, are heuristically apparent from inspection of the data alone—even the uncorrected data—as displayed in Figures 2 and 3. They are also confirmed by the parameter estimates: although the standard deviations of the cyclical shocks are about 2.5 times larger than those of the trend shocks for both series, the cyclical matching shocks are considerably more persistent—and therefore account for more of the variation in the data—than cyclical separation shocks. Also note that the cycles of the two flow rates are highly negatively correlated. On the other hand, changes in the two trends display little correlation with each other. The matching-rate trend is almost perfectly positively correlated with trend productivity growth. In contrast, trend separation rates and trend productivity have a correlation coefficient of just -10.8%.

4.1 Steady-State Unemployment

Because the trends represent the time-varying steady states of the flow rates and unemployment is a deterministic dynamic function of these flows, we can use the estimated trends to construct the time-varying steady-state rate of unemployment, as given by equation (2). The result of this computation is shown in Figure 9. Although many previous studies have produced estimates of the natural rate, this is the first estimate that is consistent with the trends in the underlying labor flows. Moreover, it does not impose any arbitrary smoothness restrictions on the natural rate or assume any relationships between unemployment and any other variables (such as inflation). It is based only on the identity in equation (1) and the decomposition of the two flow rates.

Another advantage of this method of estimating the natural rate is that, unlike most estimates, it is not restricted to produce a symmetric cycle. Several recent studies have argued for asymmetries in the business cycle, particularly with respect to unemployment. (See Rothman, 1997; Koop and Potter, 1999; Kim and Nelson, 1999; and Hamilton, 2005). Assuming the flow-rate cycles are symmetric, as I have done, does not impose symmetry on the unemployment cycle unless the two types of flows contribute roughly equally to cyclical unemployment. This is because a change in the matching rate causes the level and the persistence of unemployment to move together, whereas a change in the separation rate causes them to move in opposite directions. Because, in the data, the matching rate is responsible for much more of the cyclical variation than the separation rate, positive unemployment shocks thus tend to be more persistent than negative ones, and unemployment thus spends more time above trend than below it. This is consistent with the findings of Koop and Potter (1999). By my estimates, cyclical unemployment over the sample period averages 0.18 percentage points, and its coefficient of skewness is 0.369.

4.2 Discussion

With regard to the labor-flow rates, the formal decomposition here confirms the heuristic observations made in the previous section: job-matching rates are dominated by a large cyclical component, whereas separation-rate fluctuations arise mainly from changes in trend. Several
authors (e.g., Blanchard and Diamond, 1990; Davis et al., 1995) have demonstrated that job-destruction rates tend to be more cyclical than job-creation rates. Because job creation is roughly associated with matching and job destruction is roughly associated with separation, the above results may seem to defy this literature. However, the two observations are not technically inconsistent. To see why, consider the relationship between the rates of job creation and job matching. Ignoring unfilled vacancies, the numerators in these ratios are the same. But the denominators are different—job matching is measured relative to unemployment, whereas job creation is measured relative to the number of existing jobs. Since the latter denominator is always much larger, the fluctuations in job separation are relatively damped. Although it is true that the number of job separations is more cyclically volatile that the number of hires, the pool from which the separations are drawn is considerably larger. Job creation and destruction are useful concepts for some purposes, but when studying unemployment rates it often makes more sense to talk in terms of separation and matching, so that one can use a formulation like equation (1).

In addition, the permanent component of the matching rate is, for all practical purposes, perfectly correlated with the permanent component of labor-productivity growth; in other words, the two series share a common stochastic trend. (Again, however, this conclusion rests on the assumption of nonstationarity for both series, which is technically rejected by the data.) Although there is no theoretical reason to expect this correlation to be exactly equal to one, the result certainly seems to support the capitalization-effect story for hiring. On the other hand, the weak correlation between permanent productivity and separations suggests that this story does not hold as well on the other side of the labor market. Because the trend volatility of separation rates is large, relative to that of trend matching rates, permanent separation-rate movements are responsible for most of the fluctuations in the natural rate of unemployment. Their low correlation with productivity thus does not bode well for theories that associate changes in the natural rate with changes in trend productivity growth.

For the most part, the estimated natural rate follows a path that is broadly consistent with previous estimates—rising steadily in the early part of the sample, peaking in the early 1980s, and then falling. However, it contradicts the conventional wisdom in a couple of interesting ways. First, the largest decreases in the natural rate occur prior to 1992. It is essentially flat over the remainder of the 1990s, during the supposed New Economy period. I find that the low unemployment of that era was primarily cyclical. Second, the behavior of the natural rate since the late 1990s has not received much attention in the literature and indeed, by these estimates, seems somewhat aberrant. Around 1998, the natural rate rises somewhat, just as unemployment itself is reaching thirty-year lows, and subsequently it falls, while actual unemployment increases in response to the recession.

Examining the trend flow rates sheds some light on this second issue. The counterintuitive behavior of the natural rate stems entirely from changes in the estimated permanent component of the separation rate. This component rises in the late 1990s because, during that time, cyclical matching rates were strongly positive. By the estimated correlations, this implies a negative cycle for separation rates. However, the level of separations is essentially flat over this period. Thus, the only way to obtain a negative cycle is for the trend to rise. Exactly the opposite story holds in the post-2000 period—matching rates have a large negative cycle during this time, implying a positive cycle for separations. But, again, the observed separation rate is largely unchanged, so that it must be the case that trend separation rates—and thus trend unemployment—has fallen. Note that both of these movements in the natural rate have little to do with changes in trend productivity growth, which was, respectively, rising and flat during the two periods in question. As I will demonstrate more rigorously below, the fluctuations in trend separations and the natural rate are driven primarily by shocks that are orthogonal to productivity.
5 How Much Do Productivity Shocks Matter?

The estimates in Table 1 indicate strong contemporaneous correlations between certain components of productivity and unemployment, and a superficial glance at Figures 5 through 8 suggests that much of both the permanent and cyclical variation in the flow rates may be related to productivity movements. This section attempts to quantify the importance of these relationships more precisely by conducting experiments using the estimated parameters of the model. I perform two types of analysis. First, I compute the dynamic effects of hypothetical productivity shocks on the job-market flow rates and unemployment (i.e., impulse-response functions), using the VAR coefficients. Second, I ask what the labor-market variables would have looked like had productivity shocks been absent. These counterfactual exercises indicate how much of the observed variance in unemployment was due to productivity growth.

Moving from the estimated simple correlations to a story of causation requires a structural model that maps into the reduced-form state-space system. To generate this structure, I make assumptions about the causal ordering of the contemporaneous structural shocks (i.e., short-run restrictions). In practice, this is accomplished by taking the Choleski factorization of the covariance matrix for the six stochastic state variables. The reduced-form effect of a structural shock is calculated by multiplying the structural shock by this lower triangular matrix. Implicitly, the factorization imposes three relevant assumptions: (1) trend productivity growth is causally prior to the other five series (i.e., it is strictly exogenous); (2) all of the permanent shocks are causally prior to all of the transitory shocks; and (3) transitory productivity-growth shocks are causally prior to the transitory flow-rate shocks. (The relative ordering of the matching and separation rates, for both permanent and transitory shocks, is unimportant here, because I will only be considering the effects of exogenous productivity shocks.)

Of course, it may be that the causality between the flows and productivity growth runs in the opposite direction, with shocks to separation and matching causing productivity changes. For cyclical fluctuations, this is theoretically plausible—if, for example, an exogenous increase in separations siphons off the least-productive workers first—but, fortunately, the contemporaneous correlations between the cyclical components of productivity growth and the flow rates are small, so that their relative causal ordering makes little practical difference. In terms of the permanent components, where the structural assumptions have more bite, reverse causation could occur if sustained periods of unemployment result in loss of skills or decreases in the savings rate that lead to capital decumulation. Both the theoretical and empirical literatures in support of this view are less developed than those supporting the creative-destruction and capitalization effects that motivate my decomposition. Moreover, although these ideas sometimes play a role in discussions of the European hysteresis phenomenon, they are rarely associated with the U.S. experience.

A more likely problem is that productivity is correlated with other, omitted variables. For example, King and Morley (2003) show that, once sectoral shifts and monetary-policy shocks are taken into account, the measured effect of productivity growth on trend unemployment diminishes. However, even in this case, it may be reasonable to think of productivity shocks as being the proximate cause of labor-market fluctuations, even if they are not truly the primitive structural variables. In other words, we can view sectoral shifts and other structural shocks as operating on the labor market through the channel of productivity, so that productivity is effectively a sufficient statistic for the relevant structural shocks (see, e.g., Phelan and Trejos, 2000).

16 However, see Muscatelli and Tirelli (2001) for evidence in favor of unemployment causing slower productivity growth.
5.1 Impulse-Response Functions

Figures 11 and 12 show the response of the instantaneous flow rates and productivity growth to a one-standard-deviation positive permanent productivity-growth shock. The graphs are scaled in terms of the average absolute values of the first differences of the actual series. The separation rate jumps briefly in the positive direction, but quickly reverses sign, achieving its maximum negative response after about seven quarters. The matching rate displays behavior that is qualitatively the opposite. However, the magnitude of the matching rate response (relative to its average fluctuations) is much larger at all horizons. The long-run effect of the permanent productivity shock on the separation rate is just 6% of the separation rate’s average quarterly change. The long-run effect on the matching rate is more than three times as large.

The impulse-response functions for a one-standard-deviation transitory productivity shock are shown in Figures 13 and 14. They have the same scale as Figures 11 and 12. In the short run, the magnitude of the responses is considerably larger for both flow series. Both flow-rate response functions display hump-shaped patterns, with matching rates rising at first and separation rates falling. For both variables, the bulk of the initial effects die off within two to three years, and the series then marginally reverse sign, before returning to zero after about six or seven years. Moreover, the scale of both responses is roughly the same. The matching-rate response is slightly more persistent, but both peak at about 85% of the respective average first-difference level. Thus, in contrast to the permanent shocks, exogenous changes in cyclical productivity growth seem to have roughly equal effects on the cyclical components of the two flow rates.

Using equation (1), we can calculate how the responses of the flow rates would map into unemployment fluctuations. Figures 15 and 16 display the resulting responses of unemployment to the permanent and transitory productivity shocks. Because unemployment results from the cumulative effects of the flows over time, it is a nonlinear function of the instantaneous rates. Therefore, initial values matter. In the figure, I have assumed that the instantaneous flows are initially at their steady states and that these values are equal to the averages over the sample period. The steady-state unemployment rate associated with these values is 5.46%, close to its average.

As we would guess from the flow-rate responses, unemployment initially rises in response to a positive permanent productivity shock but then quickly falls to below its initial value. It subsequently climbs slowly upward to a new, lower steady state. However, the permanent effect of a one-standard-deviation productivity-growth shock on unemployment is less than one tenth of one percentage point. Even the short-run effects are relatively small, lowering unemployment by a maximum of just 0.2% in the eighth quarter after the shock. In contrast, the transitory productivity shock lowers unemployment by over 0.5% after just five quarters. Although by construction temporary, the shock is somewhat persistent, resulting in significantly lower unemployment rates for over three years. Again, this result is the combined effect of an increase in the matching rate and a decrease in the separation rate that are of roughly equal relative magnitude.

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17 Fujita (2004) also presents IRFs for flow rates in response to “aggregate shocks,” although he does not associate these shocks with productivity or distinguish between permanent and transitory innovations. Broadly speaking, however, his results are consistent with the ones presented in this section.

18 The sign and magnitude of the shock also matters. However, for this experiment, the quantitative differences between positive and negative productivity-growth shocks of one standard deviation are negligible.
5.2 Counterfactual Analyses

As a final exercise, I run counterfactual experiments to examine what the permanent and cyclical components of the labor-market variables would have been in the absence of productivity shocks. This is done by recovering the quarterly values of the six reduced-form shocks, computing the corresponding structural shocks using the Choleski-factorized covariance matrix discussed above, and then simulating the system with one or more of the structural shocks set to zero. To assess the relative importance of the trend and cyclical components of productivity growth, I run this exercise shutting down these two series individually and together. I allow the trend series retain its deterministic drifts, even when their stochastic shocks are shut down. To make the comparisons easier, I also assume that the noise vector $\mathbf{e}$ is always zero for the purposes of computing the counterfactual series.

Figures 17 through 19 show the counterfactual flow rates and unemployment rate with the permanent productivity shock set to zero. Note that this does not impose zero stochastic trends on the flow rates, since these trends are also affected by the other permanent shocks, although the high correlation between permanent productivity and matching-rate innovations implies that this will essentially be the outcome for the matching-rate series. The experiment also affects all three cyclical components indirectly, since the permanent shocks are correlated with (and assumed to be causally prior to) the cyclical shocks.

Removing the permanent productivity shocks has very little effect on any of the series. In light of the results presented above, this is not surprising. Trend productivity growth has little correlation with trend separation rates, and, although its correlation with cyclical separation is somewhat stronger, cyclical fluctuations account for relatively little of the over separation-rate series. Thus, shutting down permanent productivity shocks has virtually no effect on separation rates. The effects on the matching rate are slightly more important, particularly during certain periods. As mentioned above, because matching rates and productivity growth share a common stochastic trend, removing permanent productivity shocks effectively removes permanent matching-rate shocks as well. However, matching rates are dominated by cyclical fluctuations, so that this adjustment has little effect on the aggregate series. The most notable exceptions occur during the 1970s, when the counterfactual matching rate falls short the actual by a few percentage points, and in the late 1990s and early 2000s, when it exceeds it.

Because unemployment is simply the accumulation of the matching and separation rates, shutting down permanent productivity shocks has little effect on this series either. To the extent that it does, the main differences occur during periods when the counterfactual matching rate also differed significantly from the actual matching rate. Figure 20 shows the actual and counterfactual steady-state unemployment rates. The removal of the stochastic trend in the matching rate causes the series to deviate in certain periods—again primarily in the 1970s and during the most recent decade—but most of the qualitative properties of the natural rate remain. Overall, the counterfactual unemployment series has a standard deviation of 1.35%, compared to the actual value of 1.52%.

The second counterfactual experiment, summarized in Figures 21 through 23, sets the cyclical productivity shocks to zero. This has more noticeable effects on the labor-market series. Essentially, the removal of the cyclical productivity shock also removes most of the cyclical flow-rate innovations, leaving behind only the trend components, plus some cyclical variation resulting from the other two cyclical structural shocks. The variance of these other two shocks is relatively small, however, and they are not very persistent, so that the resulting series resemble random walks with noise. In other words, eliminating cyclical productivity shocks sweeps out almost all of the cyclical variation in the flow rates and unemployment. The standard deviation of the counterfactual unemployment series in this case is just 1.14%. (The steady-state unemployment series does not change in this experiment, because I have assumed that the cyclical structural shocks do not affect the permanent components.)
Finally, I try shutting down both types of productivity shocks. The results—which basically combine those of the first two experiments—are shown in Figures 24 through 26. The counterfactual separation rate continues to track the actual series to a large extent, although most of the more persistent cyclical movements disappear. The counterfactual matching rates, on the other hand, consist of little more than a deterministic trend plus noise, in stark contrast to the large, persistent cycles exhibited by the actual series. The net effect on unemployment is the removal of most of the interesting cyclical behavior, although the natural rate (which, again, is not affected by the cyclical shocks and is therefore the same here as in Figure 20) remains largely unchanged.

The removal of both productivity shocks reduces the standard deviation of the unemployment rate from 1.52% to 1.05%. In other words, productivity growth accounts for about a third of the movements in unemployment. Almost all of this effect arises from cyclical productivity shocks, and most of it operates through changes in the rate of matching. The natural rate, being largely determined by permanent changes in the rate of separation, undergoes only a modest transformation in the absence of productivity shocks. Thus, productivity growth cannot account for most of the movements in the natural rate. There are, however, two substantive exceptions to this overall finding.

First, during the period running roughly from the early 1970s through the late 1980s, the counterfactual natural rate was less than the actual natural rate by about 0.4 percentage points on average. In other words, the productivity slowdown that occurred during this time did cause a moderate increase in the natural rate of unemployment. However, in comparison to the large unemployment swings—spanning about five percentage points—that occurred over this period, this effect seems small. Moreover, the bulk of the 1970s rise in the natural rate remains even after the productivity effects are removed.

The second exception begins in about 1998 and runs through the end of the sample. During this period, the counterfactual natural rate exceeded the actual by about half a percentage point. As can be seen in Figure 18, this results almost entirely from a lower counterfactual matching rate in the absence of the permanent productivity shock. A closer look reveals that this is the lingering effect of the steep increase in trend matching rates, resulting from trend productivity shocks, that occurred in the actual series during the late 1990s. Had this acceleration in productivity not occurred, the experiment indicates, unemployment following the last recession would have reached as high as 6.7%. Moreover, if not for the subsequent cyclical productivity acceleration, it would have maintained this level through the end of 2004.

What about the much-touted productivity-induced decrease in the natural rate that was supposedly responsible for the New Economy of the 1990s? The evidence I have presented is not kind to this story. There is a decrease of about one percentage point in the natural rate between 1989 and 1993 (see Figure 20), but for the remainder of the decade this series is essentially flat. More importantly, the drop does not result from productivity-growth shocks: it remains—and, in fact, even increases slightly—in the counterfactual analysis. Indeed, Figure 9 demonstrates that there was no sizeable improvement in trend productivity growth around this time. The big increase came later, beginning around 1996, and it was responsible, to some extent, for a further improvement in the natural rate, although the bulk of this improvement was due to unexplained shocks to the separation rate that were orthogonal to productivity growth. In any case, this most recent drop occurred too late to account for the New Economy.

6 Conclusion

In this paper, I have constructed measures of key labor-market flow rates, correcting certain biases that have not been adequately addressed in previous literature. It is apparent from casual inspection of these series that job-matching rates are dominated by large cyclical fluctuations and
job-separation rates are dominated by trend movements, and these observations are confirmed by rigorous unobserved-components decomposition. The natural rate of unemployment—defined as the time-varying steady-state unemployment rate—is constructed from the trend movements in the flow-rate series and is consequently determined primarily by permanent changes in job separation. Although productivity growth accounts for the majority of the permanent and cyclical fluctuations in matching rates—and thereby for about a third of the fluctuations in total unemployment—it is only weakly correlated with separation, so that it cannot explain most of the movements in the natural rate over time.

These findings are at odds with the conventional hypothesis about the causes of long-run fluctuations in the unemployment rate. I do find that steady-state unemployment decreased during the 1990s (although the primary drop was in the early 90s, before the supposed breakdown of the Phillips Curve), but this decline was not the result of productivity-growth shocks. Rather, it occurred because of exogenous changes in the rate of job separation. More recently, a similar episode occurred between 2001 and 2004, with the trend separation rate—and therefore the natural rate of unemployment—dropping by approximately one percentage point. Again, this was not primarily the result of an increase in trend productivity growth.

If productivity growth does not account for the trend fluctuations in separation rates, then what does? Part of the answer may lie in labor-market institutions. Blachard and Wolfers (2000), for example, attribute cross-sectional differences in European unemployment partially to differences in unemployment insurance systems and other institutional arrangements, and Hatton (2002) gives some importance to such effects over the 130-year U.K. time series he considers. In the United States, King and Morley (2003) find a strong correlation between their estimate of the natural rate and the time series of unemployment benefits.

Demographics also surely contribute to some degree. Bleakly and Fuhrer (1997) argue that the decrease in flows both into and out of employment since the early 1980s result in part from the experience of the Baby Boom generation. Shimer (1999) and Mincer and Danninger (2000) also emphasize demographic factors. However, demographics are unlikely to be the whole story, because estimates of demographically adjusted unemployment rates display only modest differences from the unadjusted series. Juhn et al., 1991, for example, demonstrate this lack of importance of demographics among working-age men, although they give some emphasis to the changing role of women in the workforce.

A more intriguing possibility is that it is not productivity growth alone that matters but the relationship between productivity and real wages. Indeed, in terms of the equilibrium unemployment theory, it is typically the gap between these two variables that affects the worker flows, as this gap translates into higher potential profits for firms. Staiger et al. (2001) and Ball and Moffitt (2001) have demonstrated that including measures of the productivity-wage gap improves the fit of Phillips Curve specifications, although neither paper directly examines its impact on job separations or the natural rate. In the U.S., during the episode of rising separation rates—roughly 1968 through 1982—real-wage growth exceeded productivity growth for the only sustained period in the sample. Conversely, in the early 1990s and again in the early 2000s, labor-productivity growth outpaced real wage growth, at roughly the same time that trend separation rates fell. Nickell et al. (2001) also present evidence consistent with this view for a larger sample of OECD countries. It is not possible to consider wages as an additional variable in the sort of VAR state-space system I have estimated here—the number of parameters involved becomes too large—but the evidence indicates that this is potentially fertile ground for future investigation.
### Table 1. Summary Statistics for Instantaneous and Cumulative Monthly Transition Rates

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Table 2. ML Estimates of State-Space Model – State Equation Parameters

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<tr>
<th>Dependent Variable</th>
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<th>Separation Rate</th>
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Table 3. ML Estimates of State-Space Model – Covariance Parameters

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Figure 1. Hodrick-Prescott Trends of Unemployment Rates and Labor-Productivity Growth
Figure 2. Monthly Separation Rates


Figure 3. Monthly Matching Rates

Figure 4. Intra-Month Transition Rate
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Figure 16. Response of Unemployment Rate to a Positive Transitory Productivity-Growth Shock
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Dotted line – actual series; solid line – counterfactual

Figure 18. Counterfactual Quarterly Cumulative Matching Rate with Permanent Productivity-Growth Shocks Set to Zero

Dotted line – actual series; solid line – counterfactual
Figure 19. Counterfactual Unemployment Rate with Permanent Productivity-Growth Shocks Set to Zero

Figure 20. Counterfactual Steady-State Unemployment Rate with Permanent Productivity-Growth Shocks Set to Zero
Figure 21. Counterfactual Quarterly Cumulative Separation Rate with Transitory Productivity-Growth Shocks Set to Zero

Figure 22. Counterfactual Quarterly Cumulative Matching Rate with Transitory Productivity-Growth Shocks Set to Zero

Dotted line – actual series; solid line – counterfactual
Figure 23. Counterfactual Unemployment Rate with Transitory Productivity-Growth Shocks Set to Zero

Dotted line – actual series; solid line – counterfactual
Figure 24. Counterfactual Quarterly Cumulative Separation Rate with Both Productivity-Growth Shocks Set to Zero

Figure 25. Counterfactual Quarterly Cumulative Matching Rate with Both Productivity-Growth Shocks Set to Zero
Figure 26. Counterfactual Unemployment Rate with Both Productivity-Growth Shocks Set to Zero

Dotted line – actual series; solid line – counterfactual
References


Shimer, R., 1999. Why Is the Unemployment Rate So Much Lower?


