Measuring Job-Finding Rates and Matching Efficiency with Heterogeneous Jobseekers*

Interim Version Pending Resolution of Estimation Issue in Matching Elasticity

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Abstract

Matching efficiency is the productivity of the process for matching job-seekers to available jobs. Job-finding is the output; vacant jobs and active job-seekers are the inputs. Measurement of matching efficiency follows the same principles as measuring an index of productivity of production. We develop a framework for measuring matching productivity when the population of job-seekers is heterogeneous. The efficiency index for each type of job-seeker is the monthly job-finding rate for the type adjusted for the overall tightness of the labor market. We find that overall matching efficiency declined over the period following the crisis in 2008, at just below its earlier downward trend. We develop a new approach to measuring matching rates that avoids counting short-duration jobs as successes. Measures of matching efficiency that neglect heterogeneity among the unemployed and also neglect job-seekers other than the unemployed suggest a large 28 percent decline in efficiency between 2007 and 2009. We demonstrate that most of this apparent decline results from changes in the composition of job-seekers rather than any true movement in efficiency. We show that the Beveridge curve is not a useful way to study the matching process once matching efficiency and hiring flows are measured.

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Matching efficiency is a key concept in understanding turnover in the labor market. In particular, turnover models imply that a decline in matching efficiency causes a rise in unemployment. High unemployment from late 2008 until 2013 generated concern that the U.S. economy's normal unemployment rate rose from the turmoil of the collapse of the housing market and the subsequent financial crisis. Similar concerns developed in previous recessions.

The idea has proven useful that matching is a productive process that combines the efforts of job-seekers and of recruiting employers. The matching function—a central feature of the Diamond-Mortensen-Pissarides model of unemployment—is a production function with the number of job-seekers and the number of positions open for recruiting taken as inputs and the flow of newly matched worker-employer pairs as the output. Matching efficiency is a multiplicative shifter of the production function, analogous to the Hicks-neutral productivity index in production theory.

The term *mismatch* often appears in discussions of high unemployment. Shocks that cause widespread job loss and leave many workers unmatched with employers will generate mismatch. The role of the matching function is to cure mismatch by using resources— job-seekers' time and employers' recruiting expenditures. Thus mismatch is organic to labor-market models built on matching functions. The presence of high levels of unemployment is not necessarily a sign of a decline in matching efficiency. The appropriate way to proceed is to measure matching efficiency using standard ideas from production theory. If measured efficiency declines, a rising incidence of mismatch is one of a number of potential sources. Proper measurement of matching efficiency is a crucial starting point for understanding the sources of high unemployment.

Most analysis of the U.S. labor market in the matching-function framework has taken unemployment to be the appropriate measure of job-seeking in the population. But it is well known that this view is incomplete. In the Current Population Survey (CPS) in 2007, the distribution of hires into new jobs was 16 percent from unemployment, 33 percent from people not previously in the labor force, and 50 percent from workers in previous jobs who took new jobs without intervening unemployment or time out of the labor force. Job-to-job hiring has long been an important part of DMP modeling, but not in the measurement of matching efficiency. The remarkably large flow into jobs of people who were not previously counted as active searchers in the CPS has received less attention. An important exception is Veracierto (2011), a paper that we build on.

We develop the theory of aggregation of matching functions across diverse groups. The condition for aggregation is a natural one: changes in the success rates for job-seekers should move in proportion to one another. Our main finding is that matching efficiency measured consistently with our aggregation theory fell only slightly in recent years, and by no more than would have been expected from the earlier modest downward trend in efficiency. Earlier mis-measurement of matching efficiency was the result of treating job-seekers as homogeneous. Proper treatment of heterogeneity by reason for unemployment and duration of unemployment to date reverses the finding of a collapse of matching efficiency.

This paper measures matching efficiency. It does not attempt to explain why matching efficiency changes over time, in response to its economic determinants. A large literature, surveyed recently in Elsby, Michaels and Ratner (2015), builds models of search intensity. Variations in intensity is potentially an important determinant of what we measure. Hornstein and Kudlyak (2015) study matching efficiency with an explicit treatment of endogenous search intensity.

With the exception of Krueger, Cramer and Cho (2014), research on labor turnover has tended to focus on month-to-month changes in labor-market status—Blanchard and Diamond (1990) is a leading example. Because the separation rate from brand-new jobs is extremely high, the probability of employment a few months later conditional on unemployment in a given month is not as high as one might expect from the monthly job-finding rate. For example, the monthly job-finding rate for workers who recently suffered the loss of a permanent job was 34 percent in 2007. But measured over a three-month span, only 47 percent of those workers held jobs at the end of the span. With average separation rates, 66 percent would have been holding jobs after two more chances of landing jobs with a probability of 34 percent. And 15 months later, with 12 additional chances at a 34 percent success rate, only 62 percent were holding jobs, against 85 percent with normal rates of losing or leaving jobs. Accordingly, we study job-finding rates over the full 15-month history of each worker in the CPS. We find that there has been an upward trend in matching efficiency measured by the longer-span measures of matching success (12 through 15 months after the conditioning date) compared with the shorter-span measures (one to three months after that date).

This paper takes a close look at the job-finding productivity of different types of job-seekers, but treats vacancies as homogeneous. In principle, vacancies should be disaggregated to recognize their heterogeneity and likely variations in worker-finding productivity. Davis, Faberman and Haltiwanger (2013) is an important recent study of that heterogeneity. Research along this line is complementary to our work on job-seekers' heterogeneity.

The appendix describes some of the many earlier papers on the topic of this paper.

1 Aggregating Matching Functions

A matching function is a function $m(X, V)$, increasing and weakly concave in the number of job-seekers X and the number of vacancies V. $H = m(X, V)$ is the flow of new hires emerging from the matching process. Most investigators take the function to have constant returns to scale. The job-seeking success hazard associated with m is

$$f = \phi\left(\frac{V}{X}\right) = \frac{m(X,V)}{X} = m\left(1, \frac{V}{X}\right). \qquad (1)$$

f is the flow rate into new jobs of members of the homogeneous population measured by X.

Now we consider a heterogeneous set of job-seekers of various types. Type i has a matching efficiency parameter μ_i and a parameter ψ_i that indicates what fraction of the population P_i of type i are job-seekers. We define the effective number of job-seekers:

$$X = \sum_i \mu_i \psi_i P_i. \qquad (2)$$

We assume that all the job-seekers search in the same market and have the same matching rate except for the efficiency parameter μ_i:

Assumption. Scaled matching hazard function and common pools of vacancies and competing job-seekers:

$$H_i = \mu_i \psi_i \phi\left(\frac{V}{X}\right) P_i. \qquad (3)$$

Total hires are $H = \sum_i H_i$. Our basic result is:

Aggregation Theorem: Let m be the matching function corresponding to the job-seeking success hazard function ϕ. Then $H = m(X, V)$.

proof:

$$H = \sum_i H_i = \sum_i \mu_i \psi_i \phi \left(\frac{V}{X} \right) P_i = \phi \left(\frac{V}{X} \right) X = m(X, V). \tag{4}$$

We do not consider the distinction between a contact of a job-seeker and employer and the creation of a job match. The probability that a contact results in a hire is one of the factors determining the job-finding rates that we measure .

Only the product of μ_i and ψ_i appears in these equations, not the two measures separately. There is no prospect of distinguishing changes in matching efficiency from changes in search propensities. From this point forward, we define γ_i as the product $\mu_i \psi_i$. We refer to γ_i as efficiency, but it should be kept in mind that a decline in our measure of efficiency may arise from a decline in the search propensity of a type rather than a decline in the efficiency of the search of those choosing to search.

Notice that, in the vocabulary of productivity measures in production functions, we assume that matching efficiency enters in factor-augmenting form rather than Hicks-neutral form.

1.1 Applying the aggregation principle

Petrongolo and Pissarides (2001) discuss the evidence that the matching function has the Cobb-Douglas form, where the elasticities with respect to X and V are η and $1 - \eta$:

$$H = X^{\eta} V^{1-\eta}. \tag{5}$$

The aggregate matching function has no efficiency parameter in our setup—efficiency shows up in the job-finding rates by type and is buried inside the aggregate effective count of job-seeking volume, X. We solve out X to get

$$\phi \left(\frac{V}{X} \right) = \left(\frac{V}{H} \right)^{\frac{1-\eta}{\eta}}, \tag{6}$$

which leads to

$$f_{i,t} = \gamma_{i,t} \left(\frac{V_t}{H_t} \right)^{\frac{1-\eta}{\eta}} = \gamma_{i,t} T_t, \tag{7}$$

where

$$T_t = \left(\frac{V_t}{H_t} \right)^{\frac{1-\eta}{\eta}}, \tag{8}$$

our measure of tightness. Finally,

$$\gamma_{i,t} = \frac{f_{i,t}}{T_t}. \tag{9}$$

We discuss the estimation of the elasticity η in a later section.

1.2 Combining data from different sources

We use data from two independent surveys of the U.S. labor market, the CPS and Job Opening and Labor Turnover Survey (JOLTS). We view them as covering labor markets that are mostly overlapping but not entirely the same. We assume that they both draw from a single U.S. labor market, in the sense that there is a single factor, T, that indexes tightness throughout the overall labor market.

From JOLTS, we measure vacancies V and hires H^J. The ratio, $d = V/H^J$, is market tightness raised to the power $\eta/(1-\eta)$. No information about job-finding rates or matching efficiency is present in JOLTS, so the matching elasticity η is not identified. If the elasticity is known from other sources, the tightness measure T is known from JOLTS, and so is the volume of effective job-seeking, by solving the matching function,

$$X = \frac{(H^J)^{1/\eta}}{V^{\frac{1-\eta}{\eta}}} = \frac{H^J}{T}. \tag{10}$$

The CPS has no information about vacancies in the CPS labor market, so it cannot identify tightness. This fact would remain true if we used the more standard measure of tightness as the vacancy/job-seeker ratio, usually called θ. Our procedure uses the variable $d = V/H$ from JOLTS as a measure that describes the CPS labor market as well as the JOLTS labor market. Under that assumption, CPS data on job-finding rates identify matching efficiency and the elasticity of the matching function. The volume of effective job-seeking in the CPS labor market is H^C/T, where H^C is hiring as measured in the CPS and T is tightness of the overall market, measured from JOLTS, using the elasticity from the CPS. It is straightforward to show that this measure is identically equal to adding together the contributions of the various categories of job-seekers weighted by their matching efficiencies:

$$X = \sum_i \gamma_i P_i. \tag{11}$$

Under our maintained assumption that tightness is the same in the JOLTS and CPS markets, we can measure all of the objects of interest in this paper. We do not have data to test our maintained assumption.

1.3 Measuring matching efficiency when there is only one type of job-seeker

Suppose that there is only one type of job-seeker, and the number of job-seekers of that type is U_t. We use this notation because much of the literature on the matching function takes the count of unemployed job-seekers as the input to the function. Then our measure of job-seeking volume is

$$X_t = \gamma_t U_t. \tag{12}$$

Matching efficiency is

$$\gamma_t = \frac{f_t}{T_t} = \frac{H_t^C}{U_t}\frac{1}{T_t}. \tag{13}$$

Recall that H_t^C is the flow of hires in the CPS and T_t is tightness measured in JOLTS. In a later section, we show that the use of this approach is fundamentally misleading relative to one that includes all types of job-seekers and that recognizes heterogeneity among the types.

2 Job-Finding Rates

The standard concept of a job-finding rate is the probability that a job-seeker will find a job in a given month. We include rates based on that definition, but we also generalize it to study longer time spans, up to the longest found in the CPS. That span is 15 months, comparing the month the person entered the survey to the last month the person was in the survey.

We use the term *span* to mean the number of months between one observation on a person's labor-market status and a subsequent observation. For example, the CPS might determine that a person was unemployed on account of the loss of a permanent job in March 2009 and unemployed as well in April 2010. The span in our sense would then be 13 months. It is important to understand that span is different from, for example, the duration of unemployment. In this example, the person might have been unemployed since November 2008 and thus had a duration of unemployment of four months as of March 2009 and 17 months as of April 2010. The beginning of a span is not necessarily in the month the person entered the CPS. In the example, the person could have entered the CPS in February 2009, so that the span began in the second month of the person's period in the CPS and ended in the 15th month in the CPS. Table 1 shows the relation between the span, the CPS months, and the months of the spell of unemployment, in this example.

Calendar month	*CPS month*	*Span, months*	*Unemployment duration, months*
November 2008			0
December 2008			1
January 2009			2
February 2009	1		3
March 2009	2	0	4
April 2009	3	1	5
May 2009	4	2	6
June 2009		3	7
July 2009		4	8
August 2009		5	9
September 2009		6	10
October 2009		7	11
November 2009		8	12
December 2009		9	13
January 2010		10	14
February 2010	13	11	15
March 2010	14	12	16
April 2010	15	13	17
May 2010	16		18
June 2010			19

Table 1: Example of CPS Survey Months, a Span, and an Unemployment Spell

Over these spans, we focus on the experiences of people who were in a given labor-market status, such as looking for work after having recently quit a job. We define these statuses precisely in the next section. We then examine the probability that such a person would be employed, say, 12 months later. Longer spans matter for measuring job-finding success because many job-seekers find brief jobs, lasting only a few weeks or a month or two. A job lasting a month counts as much as a job lasting years if the measure of success uses a one-month span. Longer spans give higher weight to longer-lasting jobs.

To see this, consider a simple model of labor-market turnover. There are two kinds of jobs, short and long. Jobseekers have a 30 percent monthly probability of taking a short job and a 10 percent probability of taking a long job. The monthly probability that a short job will end is 40 percent, and the probability that a long job will end is two percent. The mix of jobs held by workers one month after a time when they are looking for work but not working is three-fourths short and one-fourth long (the distribution across workers conditional on not working in the previous month and working this month). That fraction switches to one-third short and two-thirds long with a 12-month span, as can be calculated from the 12th power of the transition matrix of the Markov process defined by the transition probabilities.

In the formalization of our setup, the job-finding rate $f_{i,t,\tau,x}$ is the probability that a worker in status i in month t with personal characteristics x is employed in month $t + \tau$.

We let this probability depend on a large vector of observed worker characteristics. The CPS sample is too small to estimate the probabilities nonparametrically, conditional on each possible combination of characteristics. Instead, we specify the probabilities as logit functions of the vector x, with time effects captured by time dummies. We allow different coefficients on the time dummies and worker characteristics for each origin status i and each time span τ. Thus, we assume

$$f_{i,t,\tau,x} = \frac{\exp\left(\kappa_{i,t,\tau} + x'\beta_{i,\tau}\right)}{1 + \exp\left(\kappa_{i,t,\tau} + x'\beta_{i,\tau}\right)}, \tag{14}$$

where $\kappa_{i,t,\tau}$ is the time effect at date t for workers in status i and a span of τ months. For job-to-job transitions, we define job-seeking success as being in a different job at the end of the span from the job at the beginning. With a one-month span, this definition is the same as the standard job-to-job rate. We can measure job-seeking success in the job-to-job case only over spans up to three months because the CPS does not keep track of respondents' employers during the eight-month gap between waves of interviews.

In a small number of cases where all respondents who started in status i in month t were employed at $t + \tau$ or where none of them were, we take the predicted job-finding rate to be 1 or 0.

A substantial literature describes reporting errors in the CPS and similar longitudinal surveys. Random errors in assigning workers to labor-market statuses result in overstatements of month-to-month transition rates. Correction of some of these errors is possible because of redundancies in the data, but most escape detection except through re-interviews. A number of proposals have appeared in the literature to make corrections in population fractions based on heuristics, such as Abowd and Zellner (1985) and Poterba and Summers (1986). More recently, formal models of identified classification errors have appeared in the econometrics literature, such as Feng and Hu (2013). We do not find either of these approaches compelling. We do not think that any realistic model with classification errors is identified by longitudinal data alone. We believe that our approach based on studying longer-span conditional probabilities of employment solves at least part of the problem, in that transitory misclassification in the destination status will be unimportant for our longer-span measures. We do retain conditioning on a single-month measure of the origin status, which results in some blurring of our results.

3 Data

We use data from the monthly CPS for November 1999 through March 2015. These data permit the calculation of job-finding rates for individuals who started their searches in the years 2001 through 2013.

Because the CPS interviews households for 4 consecutive months, skips the next 8 months, then interviews again for 4 months, each person covered for every scheduled interview contributes 6 observations spanning single months, 4 spanning 2 months, 4 spanning 12 months, and one spanning 15 months, to give a few examples. In principle, we can study job-seeking spans of 1, 2, 3, 9, 10, 11, 12, 13, 14, and 15 months. For simplicity, we omit the 9-, 10- and 11-month spans and focus on the short spans from 1 through 3 months and the long spans from 12 through 15 months.

The CPS divides the civilian noninstitutional population, ages 16 and older, into people who are employed, unemployed, and not in the labor force. Employed people are those who worked for pay or profit during the reference week, were temporarily absent from work for reasons such as vacation, illness, weather, or industrial dispute, or did at least 15 hours of unpaid work in a family-owned business. People who are not employed are classified as unemployed if they are currently available for work and either have actively looked for work during the previous four weeks or expect to be recalled from a temporary layoff. All other people who are not employed are classified as not in the labor force. We further divide the unemployed people according to the reasons they became unemployed and the length of time since that happened. We also divide those out of the labor force into two categories. One is those who answer "no" to the question, "Do you want a job now, either full or part-time?" or who answer "yes" but then indicate they are not currently available. The other category is those who want a job and are available. Barnichon and Figura (2015) found large differences in job-finding rates of people classified as out of the labor force between those wanting work and those not wanting work.

We derive a total of 16 labor-market statuses. The first three are:

- *Out of labor force*: people who did not satisfy the CPS definition of either employed or unemployed and who did not want work or were not available to work

- *Want work*: people who did not satisfy the CPS definition of either employed or unemployed and who wanted work and were available to work

- *Working*: employed people.

The next set of statuses is for people who have been unemployed for three weeks or less:

- *Recently laid off*: unemployed people who have been on furlough for three weeks or less from an earlier job, with the possibility of recall.
- *Recently lost permanent job*: people who lost jobs within the previous three weeks, not on layoff or separated from a temporary job, who were working or left military service immediately before they began looking for work.
- *Temp job recently ended*: unemployed people, not on layoff, whose last jobs were explicitly temporary and ended within the past three weeks or less.
- *Recently quit*: unemployed people who quit their last jobs within the past three weeks.
- *Recently entered*: unemployed people who have never worked and who started looking for work within the past three weeks.
- *Recently re-entered*: unemployed people, who started looking for work within the past three weeks, who were not working or in military service immediately before they began looking for work, but who have worked at some time in the past.

The following categories parallel those above, with duration of unemployment to date of 4 to 26 weeks:

- *On layoff for months*
- *Lost permanent job months ago*
- *Temp job ended months ago*
- *Quit months ago*
- *Entered months ago*
- *Re-entered months ago*

The last category is

- *Long-term unemployed*: those unemployed to date more than 26 weeks.

We do not separate the long-term unemployed by reason for unemployment because, at most times, the number of long-term-unemployed respondents in the CPS is too small to estimate probabilities reliably if we further disaggregate those respondents by reason for unemployment.

We match respondents across months using the method of Nekarda (2009). Nekarda's approach considers the full set of eight monthly observations that potentially come from the same person and assigns to each observation a probability of actually coming from the same person, based on the recorded information on the person's race, sex, and age. This probability, combined with the survey weights, is used to weight the observed transitions when we compute job-finding rates. Relative to methods such as that of Madrian and Lefgren (2000), which label respondents as matched or not across each consecutive pair of months, Nekarda's method is more suitable for measuring job-finding rates across long time spans because errors in recording race, sex, and age during intervening months are less likely to break the match.

We remove high-frequency, likely spurious transitions between unemployment and non-participation following Elsby, Hobijn and Şahin (2013). Specifically, if a respondent is out of the labor force, unemployed, and out of the labor force again in three consecutive months, we recode the middle month to *want work*, if the respondent wanted to work in either the first or third month; if not, we recode to *out of the labor force.* If the respondent is unemployed in the first and third months and out of the labor force in the middle month, we recode the middle month to unemployed with the same reason for unemployment as the first month. Among respondents who remain unemployed, we remove spurious changes in the reason for unemployment by requiring that the reason must remain the same as that given in the first interview of the unemployment spell, except that we allow transitions between temporary layoff status and permanent job loss after one month of unemployment because a worker could be temporarily laid off and later learn that the job loss had become permanent. We do not allow transitions between temporary layoff and permanent job loss once unemployment duration exceeds one month because too few such transitions are in the raw data to allow us to estimate the logit model if we allow them.

The CPS allows workers who enter unemployment to report a positive initial duration. Elsby, Hobijn, Şahin and Valletta (2011) show that inflows to high-duration unemployment are essential to understanding labor market flows during the Great Recession. We therefore accept those observations. This procedure implies that unemployment duration should not be interpreted literally as duration of the current spell, but rather an indicator of the time that has elapsed since the individual has held a job more durable than an interim job.

The variables describing personal characteristics, denoted $x_{k,t}$, are dummy variables for

- female
- married
- six age groups—16–24, 25–34, 35–44, 45–54, 55–64, and 65-plus
- four education groups—less than high school, high school graduate, some college but less than a bachelor's degree, and bachelor's or higher degree
- five unemployment duration groups, for the equations describing job-finding conditioned on unemployment of 4 to 26 weeks—categories are 4–8 weeks, 9–13 weeks, 14–17 weeks, 18–21 weeks, and 22–26 weeks

We compute approximate bootstrap standard errors for our estimates. We recompute all of the estimates in 100 bootstrap samples, which we construct as follows: Define a state-month as the set of all households in a given state of the U.S. whose first interview fell in a given month. We create the bootstrap samples by resampling households with replacement within each state-month. Each resampling follows the individual through all subsequent appearances in the CPS. This procedure accounts for the stratification of the CPS sample by state. It amounts to a block-bootstrap design and thus accounts for the correlations across members and over time within each household. It also accounts for our use of overlapping transitions—for example, our estimates of the two-month job-finding rate uses transitions from the first to third month and from the second to the fourth month for the same person. Following Rao, Wu and Yue (1992), we resample $n_h - 1$ households from a state-month with n_h households in the original sample so that the bootstrap is unbiased. We use Kolenikov's (2010) Stata program to construct the bootstrap samples. Because we do not have access to some of the underlying data that the Census Bureau uses to construct poststratified survey weights in the CPS, our bootstrap samples cannot account for the impact of the poststratification procedure. This omission is likely to inflate our bootstrap standard errors because the poststratification procedure reduces variance by holding constant the distributions of some demographic variables.

The rare event of a sample size of zero within a status-month-span cell occurred once in the CPS data. No individuals who are new entrants to the labor force in February 2008 were present for a full 15-month time span. As a result, we cannot estimate the time effect in $\kappa_{i,t,\tau}$ in equation (14) for that initial status, date, and time span. Instead, we impute the 15-month job-finding rates for new entrants in February 2008 based on the job-finding rates in adjacent months and years. Specifically, we impute

$$f_{i,\text{Feb 2008},15} = \frac{1}{2}\left(\frac{f_{i,\text{Feb 2007},15}}{f_{i,\text{Jan 2007},15}+f_{i,\text{Mar 2007},15}}+\frac{f_{i,\text{Feb 2009},15}}{f_{i,\text{Jan 2009},15}+f_{i,\text{Mar 2009},15}}\right)(f_{i,\text{Jan 2008},15}+f_{i,\text{Mar 2008},15}), \tag{15}$$

where i = *recently entered labor force*. We apply a similar procedure in the bootstrapped job-finding rates when a particular bootstrap sample has no observations for a given initial status, date, and time span.

4 Estimated Job-Finding Rates

Our estimation yields a great mass of logit coefficients, available from the online backup for the paper. In this section, we display and interpret the results in terms of calculated job-finding rates adjusted for changing composition of the labor force. We make the adjustment by choosing a base period, January 2005 to December 2007. We calculate the distribution of personal characteristics x across all respondents in the base period. Then, for each month from 2001 through 2013, we calculate the fitted job-finding probabilities from the logits separately for each possible vector of personal characteristics. Finally, we compute the average probabilities across the distribution of personal characteristics measured in the base period.

Figure 1 shows the mix-adjusted estimated job-finding probabilities for one important initial status, *recently lost permanent job*. The lowest curve is the probability that a person who lost a permanent job in the past three weeks and has been searching since then will be employed one month later. The probability runs around 30 percent. It fell in the recession of 2001, rose to a peak in 2005, fell again in the Great Recession, and rose only a bit in the recovery through 2013. The probability has a noticeable downward trend.

The next curve up is the probability that a person will be re-employed after two months. The curve is close to parallel with the one-month curve, and only slightly above the one-

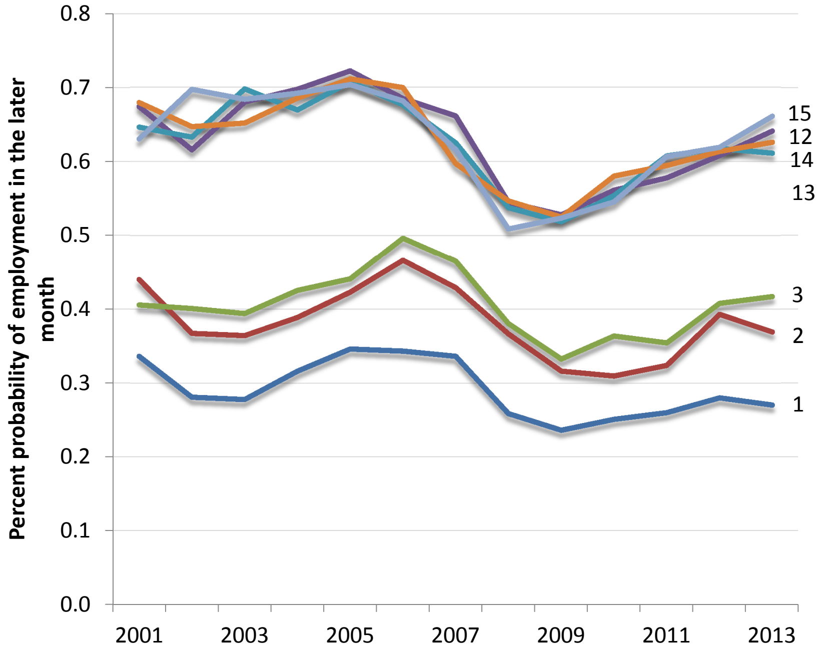

Figure 1: Estimated Job-Finding Probabilities for Losers of Permanent Jobs

month curve. In 2007, the one-month probability was 34 percent and the two-month probability was 43 percent. If the monthly job-finding rate was truly 34 percent and if there was no chance of losing a job in the second month that had been found in the first month, the probability of being employed in the second month would be $0.34 + (1 - 0.34) \times 0.34 = 0.56$, far above the actual value.

As far as we know, Krueger et al. (2014) were the first investigators to note this anomaly. They studied long-term unemployment. They concluded, "...the long-term unemployed face difficulty regaining full-time, steady work over the longest period we can observe in CPS data. It appears that reemployment does not fully reset the clock for the long-term unemployed." Our results show that the same proposition applies to every type of unemployment.

The remaining curves in Figure 1 lie even closer to each other, so the anomaly is even more acute for longer spans. One reason that the multi-month probabilities are so far below their hypothetical levels may be misclassification in the CPS. Errors could take two forms. One is classifying people as unemployed when they are actually employed. Though this type of error would exaggerate one-month employment probabilities, on the assumption that the error would have a probability of correction in the next month, the exaggeration would apply to longer spans as well. For example, suppose that these misclassifications are corrected in the succeeding month and suppose that the jobs have close to zero separation rates. Then, following a misclassification, a long series of observations of employment would occur. There would be an equal upward bias for all of the employment probabilities. So misclassification of the initial status of respondents is not a likely explanation for the anomaly.

The second type of error misclassifies job-seekers as employed when they are actually still unemployed, in months after the initial conditioning month. If such errors are prevalent and transitory, the anomaly would be explained. High measured job-finding rates based on month-to-month changes would be an illusion of phantasmal jobs, so brief that they would not show up in the longer-span probabilities.

The other explanation is that the brief jobs recorded in the CPS are true jobs, but truly brief. Hall (1995) proposed that brief interim jobs were part of the experience of the unemployed. Hall (2014a) shows that the incidence of very short jobs among newly filled jobs is remarkably high, based on the number of respondents in the CPS who report short job tenure. Hyatt and Spletzer (2013) provide evidence from a variety of sources on the incidence of short-duration jobs.

Table 2 and Table 3 show the estimated employment success rates for the year 2007 by initial status. The probabilities are computed separately for each month of the year and averaged over the 12 months. For each status, the row labeled Actual gives the percent of a random sample of people in that status in a given month who are employed in the later months of the CPS schedule. For example, 4.1 percent of those out of the labor force in a given month are employed in the following month and 11.9 percent 15 months later. The row labeled “Benchmark” is the projected percentage if the job-finding rate for month 1 applies in all the later months, but there is a monthly probability of 6 percent that any job found ends in a subsequent month and the worker cycles back to the status named at the left. Six percent per month is the typical job separation rate found in the CPS. For all initial cases and all spans of 2 months or more, the actual employment rate falls short of the benchmark, often by large amounts. For example, for workers starting in the *recently laid off* status, which has a high one-month job-finding rate of 56.0 percent, the benchmark would have 90.3 percent back at work 15 months later, but in fact, only 61.6 percent are back. The separation rates needed to explain the observed employment probabilities are in the range of 50 or even 70 percent per month.

Table 4 summarizes our findings for employment probabilities conditional on originating in each of the job-seeking statuses. The left panel shows the probabilities averaged over the

		Percent employed as of a later month						
		Months later						
Initial status		1	2	3	12	13	14	15
Out of labor force	Actual	4.1	5.6	6.5	10.9	11.2	11.6	11.9
	(Standard error)	(0.0)	(0.1)	(0.1)	(0.1)	(0.1)	(0.2)	(0.2)
	Benchmark	4.1	7.9	11.2	29.5	30.6	31.7	32.6
Want work	Actual	14.7	18.9	21.0	30.9	31.3	31.8	30.4
	(Standard error)	(0.3)	(0.4)	(0.6)	(0.7)	(0.7)	(0.9)	(1.1)
	Benchmark	14.7	26.3	35.6	66.6	67.5	68.2	68.8
Recently laid off	Actual	56.0	64.9	64.9	62.2	60.2	58.2	61.6
	(Standard error)	(1.4)	(1.6)	(2.3)	(1.6)	(2.3)	(2.2)	(2.8)
	Benchmark	56.0	77.3	85.4	90.3	90.3	90.3	90.3
Recently lost permanent job	Actual	33.7	42.9	46.5	66.2	62.5	59.7	61.6
	(Standard error)	(1.7)	(2.1)	(2.7)	(2.2)	(2.5)	(3.0)	(3.8)
	Benchmark	33.7	54.0	66.2	84.7	84.8	84.8	84.8
Temp job recently ended	Actual	42.1	54.1	49.1	59.9	61.3	66.2	57.0
	(Standard error)	(2.0)	(3.2)	(4.5)	(3.4)	(4.1)	(4.9)	(6.6)
	Benchmark	42.1	64.0	75.3	87.5	87.5	87.5	87.5
Recently quit a job	Actual	40.3	51.7	58.1	69.1	64.1	67.5	58.8
	(Standard error)	(1.9)	(2.4)	(3.6)	(2.5)	(2.8)	(3.9)	(4.2)
	Benchmark	40.3	62.0	73.6	87.0	87.0	87.0	87.0
Recently entered LF	Actual	29.3	28.8	25.4	37.4	41.9	37.8	43.6
	(Standard error)	(2.5)	(3.2)	(3.1)	(4.0)	(4.1)	(5.3)	(7.6)
	Benchmark	29.3	48.2	60.5	82.5	82.7	82.8	82.9
Recently re-entered LF	Actual	35.5	44.0	43.7	52.4	56.0	56.5	57.1
	(Standard error)	(1.3)	(1.7)	(2.3)	(2.3)	(2.3)	(3.1)	(3.6)
	Benchmark	35.5	56.2	68.4	85.4	85.5	85.5	85.5

Table 2: Subsequent Employment Probabilities by Initial Status, Actual and Benchmark, 2007: Out of Labor Force and Recently Unemployed

		Percent employed as of a later month						
		Months later						
Initial status		1	2	3	12	13	14	15
On layoff for months	Actual	42.7	51.3	59.1	49.9	54.5	63.1	63.7
	(Standard error)	(1.6)	(2.1)	(3.2)	(2.5)	(3.0)	(3.5)	(4.2)
	Benchmark	42.7	64.6	75.9	87.7	87.7	87.7	87.7
Lost permanent job months ago	Actual	22.9	31.6	37.8	58.8	58.9	59.0	56.3
	(Standard error)	(0.7)	(1.1)	(1.5)	(1.8)	(2.0)	(2.1)	(2.6)
	Benchmark	22.9	39.2	50.8	77.9	78.3	78.6	78.8
Temp job ended months ago	Actual	27.2	33.7	37.4	49.8	50.7	51.1	44.8
	(Standard error)	(1.4)	(2.0)	(2.7)	(2.6)	(2.7)	(3.1)	(4.2)
	Benchmark	27.2	45.3	57.4	81.3	81.5	81.6	81.7
Quit a job months ago	Actual	27.4	35.6	42.6	65.4	65.1	63.0	65.8
	(Standard error)	(1.2)	(1.7)	(2.4)	(2.6)	(2.8)	(3.0)	(3.7)
	Benchmark	27.4	45.6	57.8	81.4	81.6	81.7	81.8
Entered LF months ago	Actual	17.1	21.5	28.0	41.1	44.9	41.5	38.8
	(Standard error)	(1.4)	(2.1)	(2.6)	(3.0)	(3.5)	(3.8)	(4.7)
	Benchmark	17.1	30.3	40.4	70.9	71.6	72.2	72.6
Re-entered LF months ago	Actual	24.2	31.8	35.8	50.0	51.0	51.0	48.9
	(Standard error)	(0.8)	(1.1)	(1.6)	(1.6)	(1.9)	(2.1)	(2.6)
	Benchmark	24.2	41.2	52.9	79.1	79.4	79.6	79.8
Long-term unemployed	Actual	16.0	22.3	25.9	35.8	37.1	37.6	34.7
	(Standard error)	(0.6)	(0.9)	(1.3)	(1.7)	(1.8)	(1.9)	(2.2)
	Benchmark	16.0	28.4	38.2	69.0	69.8	70.4	70.9

Table 3: Subsequent Employment Probabilities by Initial Status, Actual and Benchmark, 2007: Unemployed for Months and Long-Term

early three months following the conditioning month and the right panel over the later four months. The third column in each panel shows the ratio of the employment probability in 2013 to the probability in 2003—these ratios are good measures of the trend because the business cycle was in a similar phase in the two years. In almost all originating statuses, the trend is downward in the probabilities measured up to 3 months after the conditioning month; the one exception is the originating status *recently laid off*, for which the trend is flat. By contrast, the probabilities measured 12 to 15 months after the conditioning month, in the right-hand panel, generally have smaller downward trends and in some cases upward trends. Success rates in finding first jobs following spells of job search have declined over time, while success rates for finding jobs over longer periods of search have risen. As we noted earlier, longer-span employment probabilities are better at capturing success in finding longer-duration jobs.

The employment probabilities in Table 4 vary over a wide range across the conditioning statuses. Not including the employed, for whom we look at the probability of changing jobs, the lowest job-finding rate is for people starting in the status *out of the labor force*. In 2013, their short-span subsequent employment probability was 4.5 percent and their long-span rate was 9.9 percent. Most people classified as out of the labor force remain in non-market activities from one year to the next. The CPS inquires about job-seeking interest among these people, and subsequent employment probabilities are higher among those indicating interest, but we do not pursue that topic in this paper. It would be important for any attempt to place the measurement of unemployment on the footing proposed in Flinn and Heckman (1983).

The long-term unemployed had short-span re-employment success rates of only 16 percent in 2013. Over the longer span of 12 to 15 months after the conditioning month (which is itself at least 6 months after the job loss), 40 percent of this group was employed. Though these figures make it clear that workers who fail to find jobs after six months of unemployment are not very likely to find jobs after another year of search, that proposition was true in all earlier years as well, including 2003, a year of somewhat lower overall unemployment than 2013.

Entrants and re-entrants tend to have lower employment probabilities than other categories of unemployment apart from long-term unemployment. Those who lost permanent

	Average employment probability, months 1 to 3			*Average employment probability, months 12 to 15*		
Initial status	2003	2013	*Ratio*	2003	2013	*Ratio*
Out of labor force	5.7	4.5	0.78	11.8	9.9	0.84
(Standard error)	(0.1)	(0.0)	(0.01)	(0.2)	(0.2)	(0.02)
Want job	16.9	14.9	0.88	32.3	30.8	0.95
(Standard error)	(0.4)	(0.3)	(0.03)	(0.8)	(0.7)	(0.03)
Employed	5.2	4.5	0.87			
(Standard error)	(0.1)	(0.0)	(0.01)			
Recently laid off	59.8	59.2	0.99	64.7	68.7	1.06
(Standard error)	(1.3)	(1.4)	(0.03)	(2.0)	(1.7)	(0.04)
Recently lost permanent job	34.6	35.3	1.02	67.9	63.5	0.94
(Standard error)	(1.4)	(2.0)	(0.07)	(2.2)	(2.4)	(0.04)
Temp job recently ended	44.2	40.3	0.91	62.5	60.6	0.97
(Standard error)	(2.4)	(2.4)	(0.07)	(3.5)	(3.4)	(0.08)
Recently quit a job	42.9	42.6	0.99	64.5	65.9	1.02
(Standard error)	(2.2)	(2.3)	(0.08)	(3.6)	(3.7)	(0.08)
Recently entered LF	30.1	20.8	0.69	51.0	39.4	0.77
(Standard error)	(2.7)	(1.8)	(0.09)	(4.4)	(3.6)	(0.09)
Recently re-entered LF	35.0	31.3	0.89	50.4	48.7	0.97
(Standard error)	(1.3)	(1.3)	(0.05)	(2.3)	(2.1)	(0.06)
On layoff for months	46.6	48.9	1.05	57.9	60.2	1.04
(Standard error)	(1.5)	(1.5)	(0.05)	(2.3)	(2.4)	(0.06)
Lost permanent job months ago	26.0	26.7	1.03	62.7	57.8	0.92
(Standard error)	(0.8)	(1.0)	(0.05)	(1.4)	(1.6)	(0.03)
Temp job ended months ago	30.2	28.9	0.96	54.3	54.3	1.00
(Standard error)	(1.5)	(1.5)	(0.07)	(2.7)	(2.5)	(0.07)
Quit a job months ago	34.8	31.5	0.91	58.7	57.2	0.97
(Standard error)	(1.4)	(1.6)	(0.06)	(2.7)	(3.0)	(0.06)
Entered LF months ago	21.6	15.6	0.72	44.3	44.6	1.01
(Standard error)	(1.7)	(1.0)	(0.07)	(3.1)	(2.7)	(0.09)
Re-entered LF months ago	28.1	24.9	0.88	46.8	45.2	0.97
(Standard error)	(0.9)	(0.9)	(0.04)	(1.6)	(1.6)	(0.05)
Long-term unemployed	19.8	16.4	0.83	43.2	40.4	0.93
(Standard error)	(0.7)	(0.5)	(0.04)	(1.4)	(1.0)	(0.04)

Table 4: Subsequent Employment Probabilities for Short and Long Spans, 2003 and 2013, with Growth Ratio

jobs, either recently or months ago, have quite low short-span success rates but longer-span rates comparable to other categories of unemployed job-seekers.

4.1 Changes in job-finding rates between 2007 and 2010

Table 5 compares our findings for demographically adjusted job-finding rates from 2007, the last normal year before the crisis, and 2010, the year of maximal adverse effects of the crisis in the labor market. We focus on the shorter-span rates, because we are forced to omit the large flow of job-to-job flows into employment over longer spans because of the structure of the CPS, as we discussed earlier. Recall that the short-span rates are averages over spans of one, two, and three month. Notable changes occurred in the distribution of the population among the 16 statuses: the fraction of the working-age population who were out of the labor force, wanted a job, and were available for work rose from 1.9 percent to 2.4 percent. The fraction working fell from 63.0 percent to 58.5 percent. Among the unemployment statuses, the layoff fractions rose, the quit fractions fell, and the lost permanent job fraction rose substantially. By far the largest growth was in the long-term group, which was half a percent of the population in 2007 and 2.7 percent in 2010.

Job-finding rates, stated as percents of the corresponding population group who found a job, declined more or less in proportion in all statuses, in accord with the property of our model that an index of labor-market tightness has the same proportional effect everywhere in the labor market.

The column headed "Contribution to total rate" is the product of the population fraction in the first column and the job-finding rate in the second column. It gives the part of the total rate, shown at the foot of the column, contributed by the people in the status corresponding to the line in the table. For example, in 2007, 32 percent of the population was out of the labor force and not wanting work. The job-finding rate was 5.4 percent. But this group, despite its low job-finding rate, contributed 1.7 percentage points to the total volume of job-finding, 6.3 percent of the working-age population each month. Workers, in the third line of the table, had the lowest job-finding rate, 5.0 percent, but account for half of all job-finding. The subtotals at the bottom of the table show that only 1.0 percentage points of the total of 6.3 percent of the population who found jobs came from the ranks of the unemployed in 2007.

	2007			2010		
	Percent of population	*Job-finding rate*	*Contribu-tion to total rate*	*Percent of population*	*Job-finding rate*	*Contribu-tion to total rate*
Out of labor force	32.2	5.4	1.74	33.0	4.4	1.47
Want job	1.87	18.2	0.34	2.41	13.8	0.33
Working	63.0	5.0	3.17	58.5	4.4	2.58
Recently laid off	0.20	61.9	0.12	0.23	56.6	0.13
Recently lost permanent job	0.14	41.0	0.06	0.19	30.8	0.06
Temp job recently ended	0.08	48.4	0.04	0.08	38.8	0.03
Recently quit	0.09	50.0	0.05	0.06	40.5	0.02
Recently entered	0.06	27.8	0.02	0.06	18.5	0.01
Recently re-entered	0.19	41.1	0.08	0.15	29.0	0.04
On layoff for months	0.22	51.0	0.11	0.32	46.2	0.15
Lost permanent job months ag	0.46	30.8	0.14	0.99	22.0	0.22
Temp job ended months ago	0.19	32.8	0.06	0.30	29.7	0.09
Quit months ago	0.20	35.2	0.07	0.19	29.0	0.05
Entered months ago	0.13	22.2	0.03	0.25	14.0	0.03
Re-entered months ago	0.49	30.6	0.15	0.65	23.9	0.15
Long-term unemployed	0.52	21.4	0.11	2.67	14.5	0.39
Total			6.29			5.76
Not unemployed			5.25			4.38
Unemployed			1.03			1.38

Table 5: Comparison of Job-Finding Rates between 2007 and 2010

From the peak year of 2007 to the severely depressed year of 2010, the average job-finding rate across the 16 statuses declined from 6.3 percent to 5.8 percent. This decline of 0.5 percentage points decomposes into a component that decreased the average by 1.0 percentage points arising from lower job-finding rates in general, and a component that increased the average by 0.7 percentage points arising from a shift of the population shares toward those with higher normal job-finding rates. The high normal rates occur among the unemployed. The residual, a decline of 0.2 percentage points, arises from interaction effects. The tremendous change in the labor market between 2007 and 2010 left the total job-finding flow almost unchanged, because the population shifted into unemployment, with high job-finding rates, enough to offset the general decline of job-finding rates across all the statuses.

A similar analysis within the unemployment statuses starts from the overall decline of 12.3 percentage points in the monthly job-finding rate among the unemployed. Of this, 7.4 percentage points arises from declines in the rate within each status and 5.1 percentage points from a shift of the composition of unemployment toward statuses—notably loss of permanent job and long-term unemployment—with low job-finding rates. There is also a residual of 0.2 percentage points offsetting these declines, arising from interaction effects. Within the unemployed, the shifting composition lowered job-finding success and added to the effects of lower rates for each status.

A good part of the doubling of the unemployment rate that occurred between 2007 and 2010 is associated with the decline in the job-finding rate; the rest is associated with higher flows into unemployment. In this paper, we do not measure flows into unemployment, so we do not quantify our findings in terms of unemployment rates.

5 Job-Finding Rates and Tightness

5.1 Basic equation for estimation of the elasticity of the job-finding rate with respect to tightness

Equation (7) leads to the following model of the measured log job-finding rate over a τ-month span:

$$\log f_{i,t,\tau} = \log \gamma_{i,t,\tau} + \nu_\tau \log d_t + \epsilon^m_{i,t,\tau}, \tag{16}$$

where $\epsilon^m_{i,t,\tau}$ is a measurement error. Here ν_τ is the elasticity of job-finding with respect to the measure of tightness from the employer's perspective, $d_t = V_t/H^J_t$, which is the duration of vacancies in JOLTS, measured as the ratio of the stock of vacancies to the flow of hires. The elasticity is related to the elasticity of the matching function as $\nu = (1 - \eta)/\eta$.

We assume that matching efficiency satisfies

$$\log \gamma_{i,t,\tau} = \alpha_{i,\tau} + \delta_{i,\tau} t + \psi_{i,s} + \xi_{i,t,\tau}, \tag{17}$$

where s is the month of the year, to capture seasonal effects, $\psi_{i,s}$, and t is time measured in months, to capture a trend, $\delta_{i,\tau} t$. The model we estimate is thus

$$\log f_{i,t,\tau} = \alpha_{i,\tau} + \delta_{i,\tau} t + \psi_{i,s} + \nu_\tau \log d_t + \epsilon_{i,t,\tau}, \tag{18}$$

where

$$\epsilon_{i,t,\tau} = \epsilon^m_{i,t,\tau} + \xi_{i,t,\tau}. \tag{19}$$

5.2 Identification

Our first identifying assumption is

$$\mathbb{E}\ (\epsilon_{i,t,\tau} | t) = 0, \tag{20}$$

so the month, t, is eligible as an instrumental variable and seasonal dummies are also eligible as instruments.

The job-finding rate and labor-market tightness are obviously jointly determined, so a further assumption about the disturbance $\epsilon_{i,t,\tau}$ is required for identification—the disturbance is not plausibly orthogonal to either variable. Our second identifying assumption is that $\epsilon_{i,t,\tau}$ is orthogonal to the log of real GDP. This assumption is likely to hold at least for one major source of correlation between the disturbance and the variables, namely measurement error. We use the monthly estimate of real GDP from Macroeconomic Advisers released in July 2015.

5.3 Further aspects of estimation

We average the three short spans (one, two, and three months after the conditioning status) to form the job-finding rate for the first span category, called *short*, and the four longer spans (12 through 15 months) to form the second job-finding rate category, called *long*. For the short job-finding rate, we can include in our data the job-changing rate for those starting in the *employed* status. For the long job-finding rate, we cannot calculate the job-changing rate; thus, for comparability between the short and long equations, we also estimate the short equation without including the job-changing rate. We estimate equation (18) with the instrumental variables noted above, using monthly data on job-finding rates.

As equation (18) indicates, we pool the data for initial statuses in estimation, to enforce the implication of the model that the elasticity of the job-finding rate with respect to vacancy duration, ν_τ, is the same across those statuses, though different between the short and long spans. We do not take into account any correlation of the disturbances across the statuses. Thus our estimates are unbiased but not minimum variance, if correlation is present. Because we use a bootstrap strategy to calculate standard errors that preserves the correlation, those standard errors take account of the correlation. The correlation is positive in almost all cases, but relatively mild—over the full sample, the average absolute values of the off-diagonal elements of the correlation matrices are 0.29 for short spans both with and without job-to-job, and 0.20 for long spans. We do not believe that a three-stage least squares estimation procedure would be appropriate, given the large number of estimated coefficients relative to the number of data points. For each status, we have $12 \times 7 = 84$ observations when we use only pre-crisis data, and we estimate a constant, 11 values of the seasonal effects, and a time trend coefficient. Over the full sample, we have $12 \times 13 = 156$ observations for each status.

The residuals from equation (18) form an index of detrended matching efficiency:

$$\epsilon_{i,t,\tau} = \log f_{i,t,\tau} - \alpha_{i,\tau} - \delta_{i,\tau} t - \psi_{i,s} - \nu_\tau \log d_t, \tag{21}$$

as the observed job-finding rate measured around its its status- and span-specific constant level and trend, and adjusted for changes in labor-market tightness. These residuals also include measurement error in job-finding rates, but such measurement errors should average to zero over time. In particular, our presentation of the results focuses mainly on annual averages, so much of the measurement error should average out over the course of each year.

We use the estimates of job-finding rates adjusted for the changing characteristics of the population, as discussed earlier, as the left-hand variable of equation (18). Although, in principle, it would be possible to combine the two estimation stages, we doubt its practicality and have no reason to believe it would affect our conclusions. Our bootstrap standard errors take both stages into account.

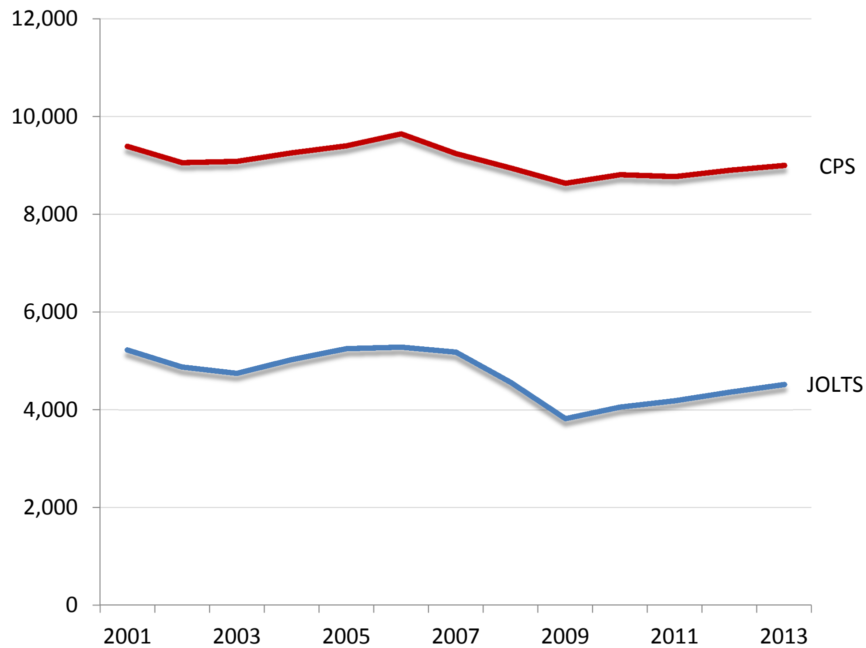

Figure 2: Number of Monthly Hires, in Thousands, from JOLTS and the CPS

5.4 Measuring tightness, d

Figure 2 shows the number of new hires from the CPS and from JOLTS. The CPS and JOLTS figures vary similarly over time, but the level of hires is substantially higher in the CPS. The reasons for the discrepancy may include: (1) JOLTS does not include hires at new establishments or self-employment, as Davis, Faberman, Haltiwanger and Rucker (2010) discuss, and (2) the CPS may capture more of the hiring into jobs that last only days or a few weeks. Hires track the business cycle, but with fairly low amplitude. The decline in hiring reported in JOLTS from 2008 to 2009 was about twice as large in percentage terms as the decline in the CPS.

Figure 3 shows the number of job openings (vacancies) from JOLTS. This series traces the business cycle with high amplitude—vacancies are high in tight market around peaks and low in slack markets around business-cycle troughs.

Figure 4 shows the average duration of vacancies, d, using the JOLTS measures of hires and vacancies. Because vacancies vary more in proportional terms than do hires, the vacancy/hires ratio is quite procyclical. Earlier we discussed the relationship between the JOLTS and CPS measures and why we construct tightness from JOLTS—the CPS survey

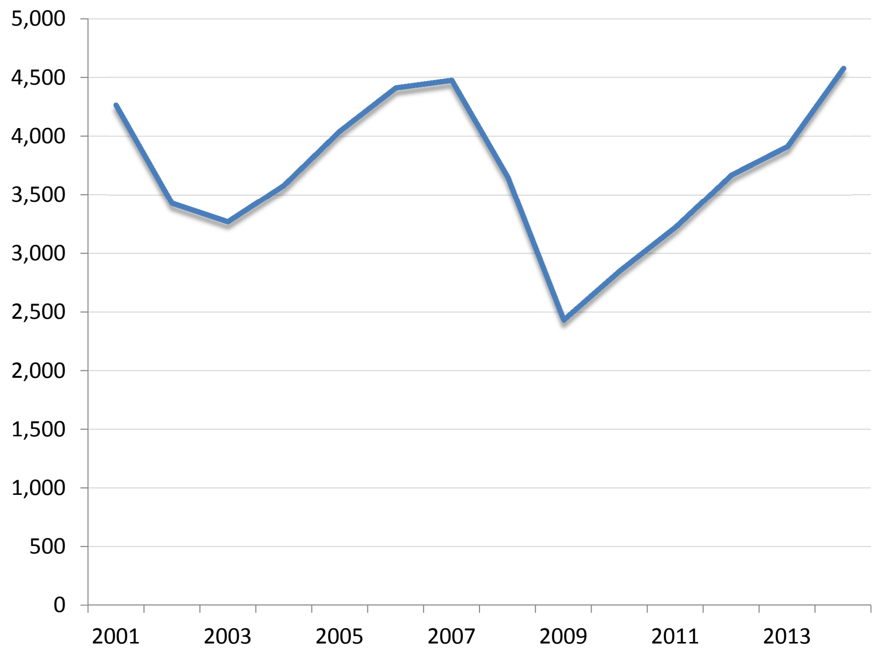

Figure 3: Number of Job Openings, in Thousands, from JOLTS

covers a larger and somewhat different universe of jobs than does JOLTS and we lack vacancy data corresponding to the CPS.

5.5 Estimates

Table 6 shows estimates of the elasticity of the matching function based on equation (18). The top panel uses data for the entire JOLTS period, 2001 through 2013. The middle panel uses data from 2001 through 2007, the last year not affected by the crisis. The bottom panel provides $t-$statistics for the hypothesis of no change in the coefficients between the two periods. The first column of estimates shows the coefficients in equation (18), which estimate $(1-\eta)/\eta$, and the second column the implied matching elasticity η. The right-most column shows the mean of the estimates of the trend in equation (18) across the 15 or 16 statuses. Each panel has three sets of estimates. The top line uses all 16 statuses and the left-hand variable is the log of the average composition-adjusted job-finding rate measured across spans of one, two, and three months. The middle line does the same but omits the finding rate for job-to-job transitions. The bottom line uses the log of the average across the longer spans and necessarily omits the job-to-job rate.

For the short-span equations, the elasticity estimates range from 1.1 to 1.2, corresponding to matching elasticities of 0.45 to 0.47, in line with the estimates surveyed in Petrongolo and

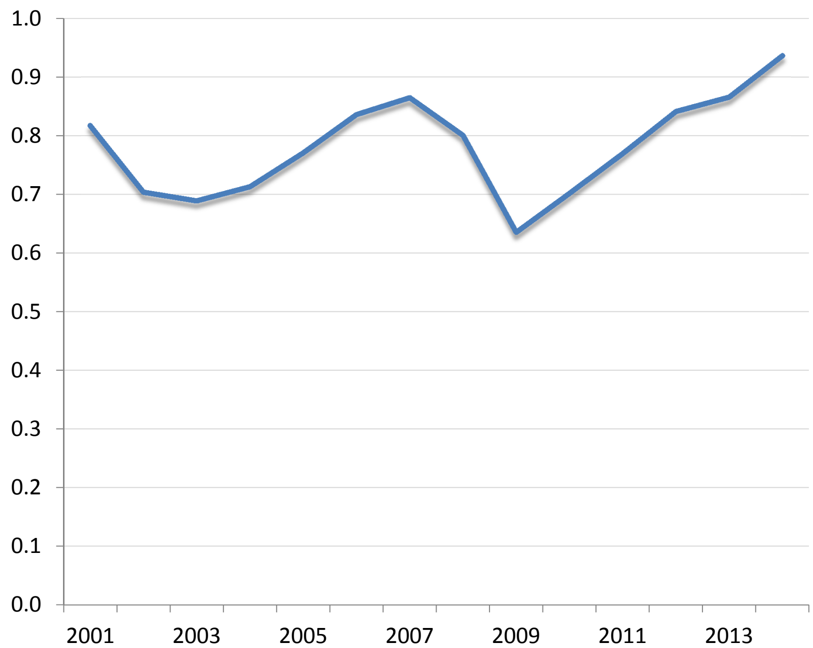

Figure 4: Average Duration of Vacancies, Calculated from JOLTS

	Monthly span of job-finding rate	*Include job-to-job movers?*	*Elasticity with respect to vacancy duration*	*Implied elasticity of the matching function*	*Mean trend in efficiency*
2001-2013	Short	Yes	1.164 (0.067)	0.462 (0.014)	-0.0026 -0.0001
	Short	No	1.208 (0.071)	0.453 (0.015)	-0.0027 (0.0001)
	Long	No	0.419 (0.083)	0.704 (0.040)	-0.0012 (0.0001)
2001-2007	Short	Yes	1.109 (0.219)	0.474 (0.055)	-0.0018 (0.0005)
	Short	No	1.112 (0.232)	0.473 (0.059)	-0.0017 (0.0005)
	Long	No	0.407 (0.240)	0.711 (0.159)	-0.0009 (0.0006)
t-stats for no change	Short	Yes	0.24	-0.21	-1.57
	Short	No	0.41	-0.35	-1.70
	Long	No	0.05	-0.04	-0.58

Table 6: Elasticity Estimates

Pissarides (2001). We are not aware of any previous research on the longer-span matching-function elasticity, which we estimate to be about 0.7. The matching elasticities are robust to the choice of sample period—the $t-$statistics give essentially no evidence of change when adding the data from the crisis and post-crisis years.

Table 6 shows strong evidence that matching efficiency declined during the pre-crisis period and in the entire period. The evidence is weaker but still respectable that the decline was faster in the post-crisis period—the $t-$statistic is 1.6 in absolute value for the hypothesis of no change when extending the sample to the post-crisis years.

A second approach to quantifying the post-crisis decline in matching efficiency and to test the hypothesis of no such effects is to estimate equation (18) on data from the full range of years, 2001 to 2013, and to include year effects for 2008 through 2013 Table 7 shows the results of this approach. For both short and long spans, there were statistically unambiguous shortfalls of matching efficiency relative to the pre-crisis trends in 2008. This decrease in matching efficiency was about six percent for short-span job-finding rates and 17 percent for longer spans. For short spans, matching efficiency also fell about 11 percent below pre-crisis trends in 2012. In 2009 through 2011, the divergence from pre-crisis trends was statistically ambiguous.

5.6 Implied matching efficiency

We calculate indexes of matching efficiency for each of the 16 labor-market statuses. Because we hold the distribution of individuals' characteristics constant in calculating the job-finding rates on the left-hand side of equation (18), the movements in these indexes are insulated from changes in the distribution of characteristics. Figure 5 shows the resulting detrended indexes for 9 of the more important statuses. These are the exponentials of the values described in equation (21) and are indexes normalized to one in 2007. The trends are estimated through 2007 so they exclude the effects of the crisis and its aftermath. The short-span results are derived from estimates of equation (18) that include data on job-to-job transitions.

The pattern of annual matching efficiency for the initial status *recently lost permanent job* is representative in terms of its movement over time and more precisely estimated because large numbers of job-seekers fell into this category. In that category, both measures of detrended efficiency rose during the recovery from the 2001 recession, fell as the economy reached its peak in 2007 (where the index is one by construction). After the recession,

		Short spans, with E-E flows	*Long spans, without E-E flow*
Elasticity of job-finding rate with respect to vacancy duration		1.252	0.588
		(0.1997)	(0.2224)
Implied elasticity of the matching function		0.444	0.630
		(0.0412)	(0.1155)
Year effects	2008	-0.060	-0.166
		(0.0191)	(0.0252)
	2009	0.076	-0.025
		(0.0632)	(0.0723)
	2010	0.019	0.017
		(0.0536)	(0.0584)
	2011	-0.037	0.001
		(0.0404)	(0.0481)
	2012	-0.115	-0.029
		(0.0320)	(0.0384)
	2013	-0.109	0.021
		(0.0283)	(0.0383)

Table 7: Estimates of the Matching Efficiency Equation with Year Effects in and after the Crisis

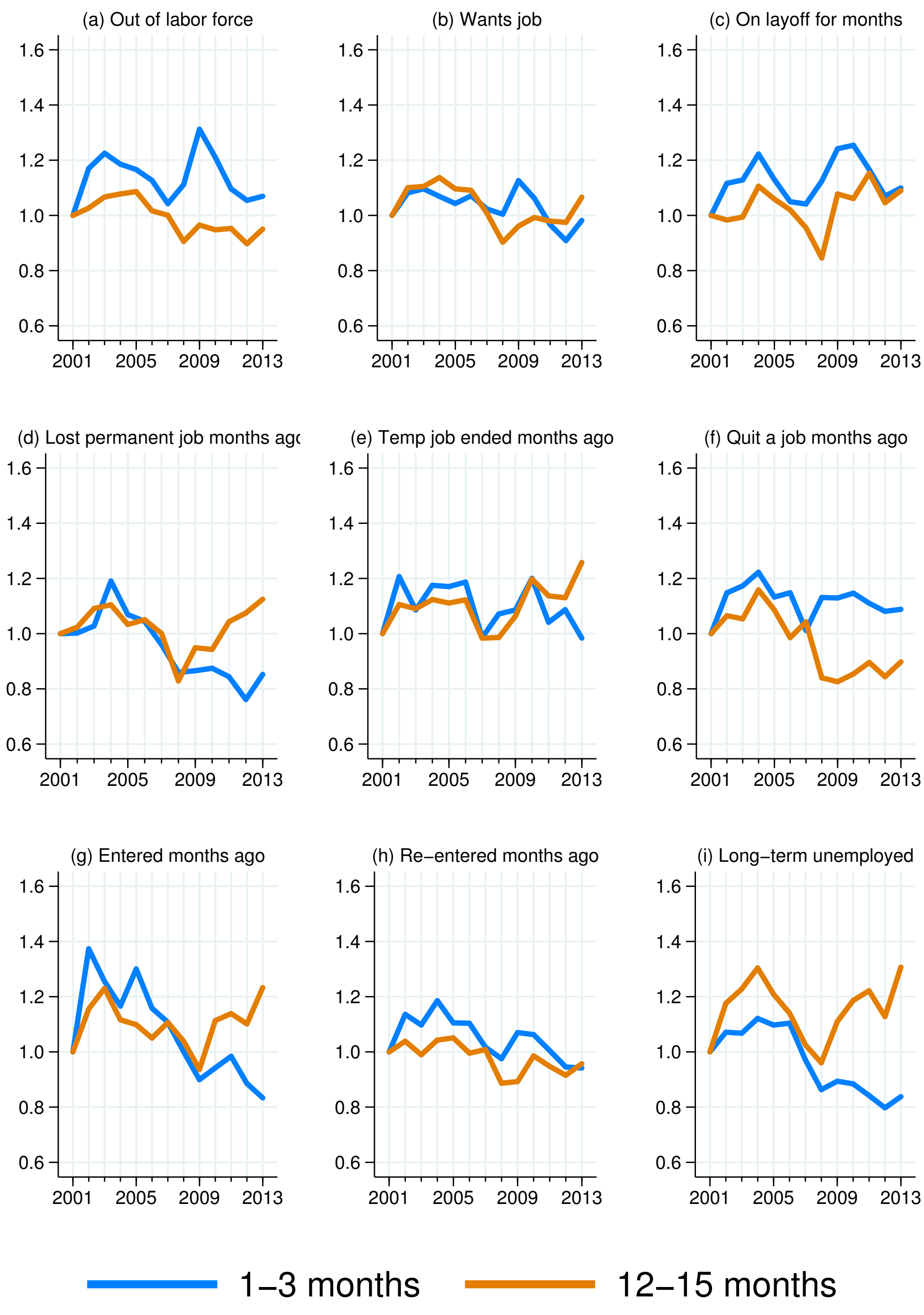

Figure 5: Detrended Matching Efficiency for Nine Statuses

matching efficiency as measured over short spans fell, while efficiency over long spans rose, though neither change was very large. We noted earlier that the measure over long spans gives more weights to longer-lasting new jobs, so the finding of improved efficiency for that measure suggests an improvement in labor-market performance that is not apparent in the conventional approach based on one-month spans. In the category *lost permanent job months ago*, the same pattern of declining efficiency over short spans and improving efficiency over long spans is present but larger. And in the closely watched category *long-term unemployed*, the same pattern is even stronger. By contrast, in the category *quit a job months ago*, the pattern is reversed—matching efficiency plunged by the long-span measure but rose a little by the short-span method.

Figure 6 shows the indexes without subtraction of the trend terms in equation (18). Notice that the trends are downward over time for essentially all of the initial statuses, corresponding to the ratios of 2012 job-finding rates to 2001 rates in Table 4 that are almost all below one.

Sam: My results show much more year-to-year volatility, but I think your results still use all three instruments so I'm not sure whether I have a coding error or whether the different instruments are the cause. See aggefficiency_2007.pdf and aggefficiency_2013.pdf.Bob: Will you be running with 3 IVs to check this?

Figure 7 shows indexes of matching efficiency across the 16 initial statuses, with detrended indexes on the left and indexes with trends on the right. The overall detrended index is a weighted average of the 16 detrended components in Figure 5, using weights representing the relative shares of the components in the population in the three years preceding the crisis, 2005 through 2007. Because the job-finding rates hold constant the distribution of worker characteristics conditional on labor market status, this aggregate index holds constant the joint distribution of worker characteristics and labor market status. The movements in matching efficiency measured by the aggregate index result from changes in the efficiency of particular types of workers, not in the distribution of workers. We construct two versions of the aggregate detrended matching efficiency index: an index for short spans that includes job-to-job movers, an index for short spans that includes only the unemployed and people not in the labor force, and an index for long spans that includes only the unemployed and people not in the labor force. Including job-to-job movers in the overall measure has little effect on the overall measure of matching efficiency because job-to-job movers' matching

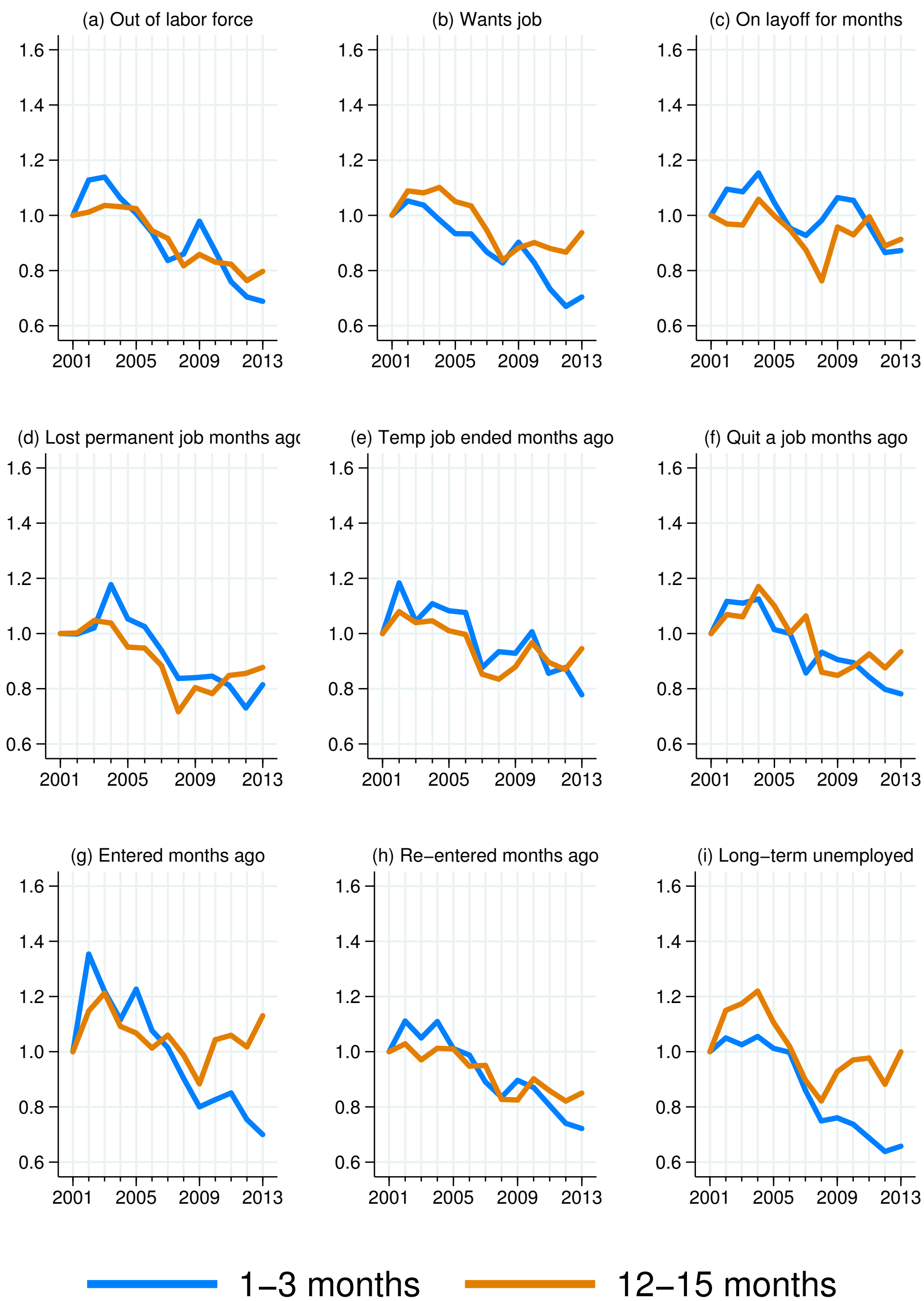

Figure 6: Matching Efficiency Including Trends for Nine Statuses

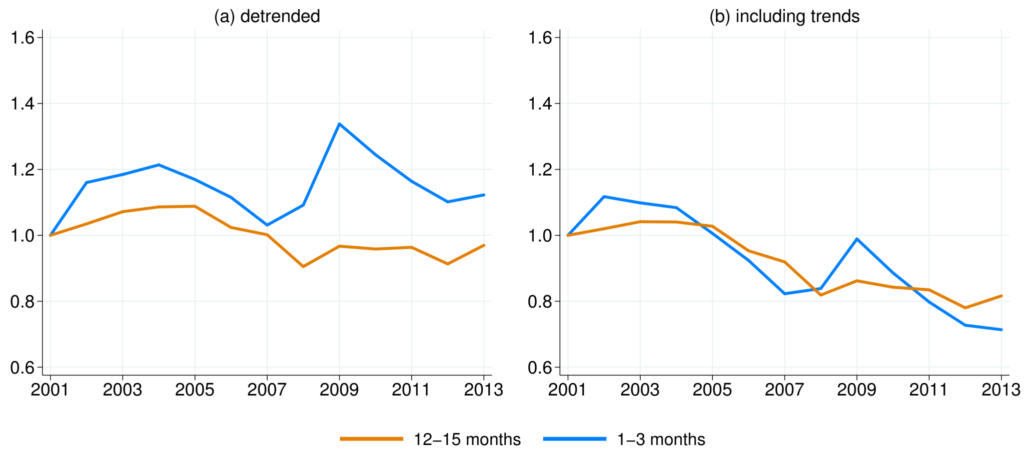

Figure 7: Aggregate Matching EfficiencySam: My results again show much more year-to-year volatility.

efficiency moved similarly to that of other people, as shown in Figure 5. The estimated indexes show that detrended matching efficiency for both measures based on short spans is quite cyclical, rising soon after the onset of recessions and then falling during recoveries. With adjustment for trend, short-span efficiency was essentially the same in 2013 as in 2007 and 2001. Long-span efficiency is less volatile, but was somewhat below its 2007 level in 2013, adjusted for pre-crisis trends.

The right side of Figure 7 shows the same data without adjustment for trend. Matching efficiency at both short and long spans has trended downward since 2001. This trend is more pronounced in the short-span measure.

We also constructed Divisia-style indexes with time-varying weights. The difference between these indexes and our fixed-weight indexes was tiny—at a monthly frequency, the largest difference between the two types of indexes, in the case of the short-span index that included job-to-job flows, was 1.2 percent.

6 Job-seeking Volume

Figure 8 compares the volume of job-seeking, X, in the framework of this paper to a measure restricted to job-seekers counted as unemployed. Recall that X is the sum across the 16 statuses of their current matching efficiencies (ratios of job-finding rate to tightness, T) multiplied by the number of people current in the status. For comparability to unemployment, we divide the volume by the labor force. The comparison would be very similar if

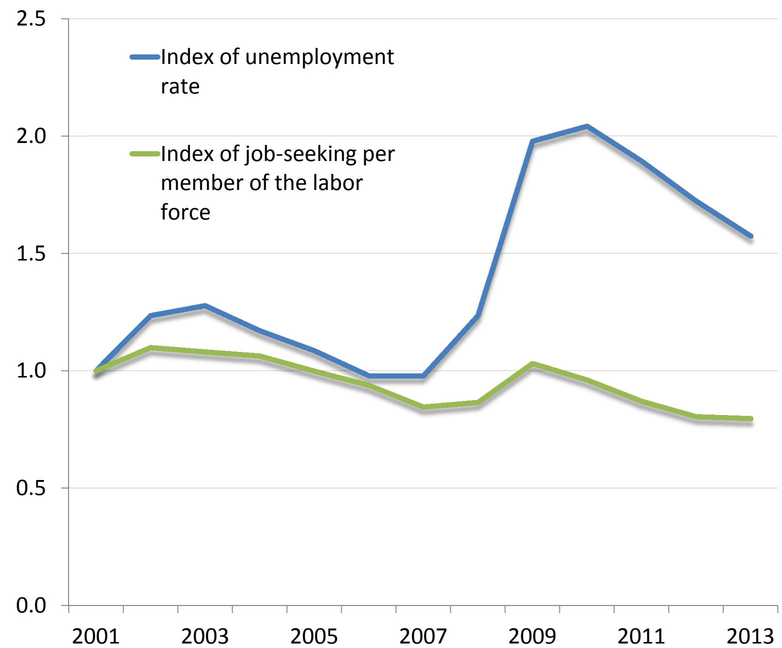

Figure 8: Volume of Overall Job-seeking Compared to Unemployment

we measured both X and unemployment relative to the working-age population. In forming $T_t = d_t^{(-1/\eta)/\eta}$, from the JOLTs data on vacancy duration, d_t, we used $\eta = 0.55$, a reasonable synthesis of the various estimates in Table 6.

Figure 8 shows the dramatic difference between our measure of job-seeking volume and the measure often used earlier in matching-function research, the number of unemployed people. Our measure trends downward over the period from 2001 to 2013, reflecting the downward trend we find in matching efficiency. Unemployment rose vastly more in the crisis and ensuing slump than did our measure. While unemployment did play a more important role than usual during that period, as Table 5 shows, declines in job-seeking among people in other statuses offset most of the increase.

7 Is Tightness a Common Influence in All Markets?

According to the model in this paper, tightness is a common factor for the job-finding rates of all job-seekers in the U.S. We can evaluate this proposition by estimating separate elasticities of the job-finding rate for each of the 16 originating statuses. Table 8 shows the results for this relaxed specification. The table gives strong support to the hypothesis that the same tightness variable influences all job-finding rates positively—there is not a single negative

	Short spans		Long spans	
	Estimated elasticity	*t-stat for deviation from common elasticity*	*Estimated elasticity*	*t-stat for deviation from common elasticity*
Out of labor force	0.84	-3.8	0.68	2.9
Want job	1.06	-0.9	0.67	1.9
Working	0.50	-8.8	.	
Recently laid off	0.51	-5.1	-0.10	-2.8
Recently lost permanent job	1.61	2.0	0.50	0.4
Temp job recently ended	0.70	-1.6	0.22	-0.6
Recently quit	0.87	-1.3	0.26	-0.6
Recently entered	1.39	0.4	0.51	0.1
Recently re-entered	1.07	-0.5	0.71	1.4
On layoff for months	0.80	-2.1	0.03	-1.7
Lost permanent job months ago	1.91	4.1	0.40	-0.1
Temp job ended months ago	1.19	0.1	0.10	-1.1
Quit months ago	1.09	-0.3	0.97	2.3
Entered months ago	1.90	1.8	0.48	0.1
Re-entered months ago	1.29	0.8	0.66	1.4
Long-term unemployed	1.89	3.6	0.20	-1.0
Constrained estimate of common elasticity	1.16		0.42	

Table 8: Unconstrained Elasticities of Job-Finding Rates with Respect to Vacancy Duration

elasticity in the table. In particular, the three categories outside of unemployment—*out of the labor force*, *want job*, and *employed*—all have distinctly positive elasticities, two of which are close to the value when the elasticities are constrained to be equal, from Table 6, shown at the bottom of the table. The column headed *t-stat for deviation from common elasticity* is the difference between the estimated elasticity for the status at the left, divided by its standard error.

It is clear that the elasticities are not the same across all of the originating statuses. Higher elasticities for the *lost permanent job* and *long-term unemployed* statuses disprove the proposition that the elasticities are identical. Our setup and data are capable of detecting relatively small deviations from equality, so we interpret the evidence as generally favorable to the common-factor hypothesis, but we recognize that there are interesting deviations that may merit further investigation.

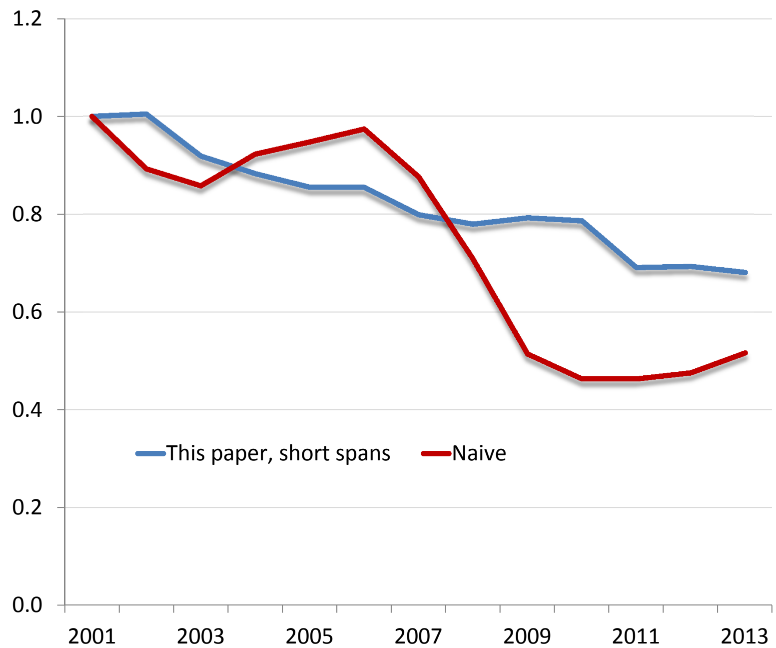

Figure 9: Comparison of this Paper's Measure of Matching Efficiency to a Naive Measure

8 Matching Efficiency

Figure 9 compares our measure of matching efficiency to a naive measure that uses unemployment as the sole measure of job-seeking volume. From equation (13), the naive measure is

$$\frac{H_t^C}{U_t}\frac{1}{T_t}, \tag{22}$$

the ratio of the CPS hiring flow to the number of unemployed people, divided by the JOLTS tightness measure. Figure 9 compares the naive measure to the one in Figure 7, using the same elasticity η=0.5.

The naive measure considerably overstates the decline in matching efficiency between 2007 and 2010, the period when unemployment doubled. The naive measure infers a collapse of efficiency from its measure of the job-finding rate, H_t^C/U_t. But this measure overstates the decline in the rate because its numerator is the flow from all types of job-seeking, whereas the denominator is only unemployment, which accounts for only a quarter of job-seeking success. Naturally, the bulge of unemployment after the crisis drove the ratio down and created the illusion of collapse, when in fact matching efficiency declined by a small amount, a bit less than its normal long-run downward trend amount. Notice that the same distortion operated in the recession of 2001 and its aftermath, though not as dramatically.

9 Concluding Remarks

Many authors have demonstrated a decline in labor-market matching efficiency during the Great Recession and ensuing slump. With the exception of Veracierto's pioneering work, research has made the assumption that the measure of job-seeking volume is the stock of unemployed workers. But the Current Population Survey shows that less than a quarter of newly filled jobs involves hires of the unemployed. The remaining three-quarters have been out of the labor market or are making job-to-job transitions. We develop a consistent approach to aggregation over heterogeneous categories of job-seekers, with a separate measure of matching efficiency for each category and a related measure of aggregate matching efficiency.

A second novel element in our work is to study the effectiveness of job search over spans greater than a month. Longer spans have two advantages: First, they lower the bias from misclassification, which tends to overstate job-finding rates measured as monthly transition rates from job-seeking to employment. Second, they give less weight to transitory interim jobs, which appear to be an important part of the job-seeking process.

Our concept of matching efficiency combines the propensity of the members of a category of potential job-seekers to engage in active search with the per-period effectiveness of those active searchers. Absent direct measures of search effort, as in Krueger and Mueller (2011), we cannot break the two factors apart.

We confirm that matching efficiency has declined in some categories of unemployment, including permanent job loss, a category that rose substantially as a fraction of total unemployment in the Great Recession. Most of the decline is the continuation of a trend that has existed since 2001 and possibly earlier. Because such a large fraction of hiring occurs out of pools of job-seekers other than the unemployed, one important implication is that the decline in matching efficiency among the unemployed drove up the unemployment rate, but the labor market still generated large volumes of job-finding among groups not counted as unemployed.

Many observers use the Beveridge curve, with the unemployment rate on the horizontal axis and the vacancy rate on the vertical axis, to study the matching process. This paper studies one of the determinants of the Beveridge cure, matching efficiency. We do not delve into others, notably fluctuations in the inflows to unemployment.

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A Related Research

Elsby et al. (2015) survey many topics relevant for this paper, though in a Beveridge-curve framework.

Veracierto (2011) introduced the basic idea of including people other than the unemployed in the calculation of matching efficiency. He makes a compelling case that the movements of aggregate unemployment cannot be understood in the DMP framework—especially with respect to the matching function—without considering the role of individuals who are classified as out of the labor market. These people are neither working nor engaging in the specific job-seeking activities in the four weeks prior to the CPS interview that would place them in the category of unemployment. The striking fact is that, after correcting in the standard way for erroneous transitions, the CPS reveals that the number of people classified as out of the labor force in one month who are employed in the next month is always greater than the number moving from unemployment to employment. In normal times, using the obvious notation, the NE flow is almost double the UE flow.

Flinn and Heckman (1983) observe that the natural definition of unemployment is that a non-working individual's transition hazard into employment exceeds a threshold value. By that criterion, it seems likely that a non-trivial fraction of those the CPS classifies as out of the labor force (N) are actually unemployed. But the overall NE hazard in normal times is far lower than the UE hazard—5 percent per month compared to 27 percent, so it is clear that the U category in general satisfies the Flinn-Heckman criterion.

The BLS publishes data on broader definitions of unemployment. It is an interesting question but outside the scope of this paper whether a systematic application of the Flinn-Heckman principle might result in a definition of unemployment that captured the great majority of non-workers with high job-finding hazards while excluding those with low hazards. Such a definition would fit the matching function framework nicely.

Veracierto (2011) proposes a simple way around this issue that incorporates those classified as out of the labor force without identifying the individuals with high NE hazards. A brief discussion in Petrongolo and Pissarides (2001), p. 403, anticipates Veracierto's approach. He uses the ratio of the NE hazard to the UE hazard to weight those classified in N. The resulting figure is interpreted as the effective number of job-seekers in the N category. The total number of job-seekers is the number in U plus the weighted number in N. This figure—interpreted as comprehensive unemployment—is the input to the matching function in a DMP model that takes account of the high incidence of job-seeking in the N category. Veracierto finds (see his figure 36) that matching efficiency was flat before the Great Recession, then declined about 15 percent during the recession.

Our analysis differs from Veracierto's both in the definition of matching efficiency and in the level of disaggregation. Veracierto assumes that unemployed workers and nonparticipants have equal matching efficiency conditional on a given level of search intensity but that nonparticipants have lower search intensity. By contrast, we do not distinguish between matching efficiency and search intensity for a given type of worker and instead estimate an efficiency parameter for each type that combines matching efficiency and search intensity. In addition, our analysis includes job-to-job transitions and further disaggregates workers by their reason for unemployment and by observable characteristics. Our model thus provides a unified treatment of the calculation of aggregate matching efficiency when all people in the economy of working age are potentially job seekers.

Ahn and Hamilton (2015) is an ambitious study of unemployment dynamics with heterogeneous unemployment. It uses the same six-way breakdown of the unemployed by originating event from the CPS that we use, but it does not consider job-seeking by those other than the unemployed. Its framework is entry and exit rates from unemployment. It finds, as we do, that losers of permanent jobs became a larger fraction of entrants to unemployment as a result of the crisis and that their low job-finding rates are important for understanding the persistence of high unemployment.

Kroft, Lange, Notowidigdo and Katz (2014) overlaps with this paper in certain respects. It measures job-finding rates for duration categories among the unemployed and for people who are employed and out of the labor force. It does not break down the unemployed by originating event as we and Ahn-Hamilton do. It does not focus explicitly on matching efficiency. Its scope is broader than ours in its concern for unemployment rates and the corresponding need to study entry rates to unemployment as well as exit rates, including the job-finding rate. Its main focus is on dissecting the huge expansion in long-term unemployment in the immediate post-crisis years.

Ghayad and Dickens (2012) study shifts in the Beveridge curve with a detailed decomposition of unemployment, concentrating on the comparison of the post-crisis period to the 1970s.

Barnichon and Figura (2012) also estimate matching efficiency while allowing heterogeneity across workers in demographics, distinguishing between reasons for unemployment, and including nonparticipants in the analysis. However, they assume that the matching function applies only to unemployed workers and do not consider job-to-job transitions.

In addition to Krueger et al. (2014), Cajner and Ratner1 (2014) study job-finding among the long-term unemployed over spans of more than a year.

Carrillo-Tudela, Bart Hobijn and Visschers (2015) demonstrate that workers who report active search while on the job have substantially higher job-to-job transition rates than those who are inactive, so a breakdown of the employed by search activity would be desirable in hour framework. But the question about job-seeking among the employed is only asked in an occasional supplement to the CPS and is not part of the regular monthly CPS that we use.

Fujita and Moscarini (2013) study the effect of recalls by unemployed workers' former employers on transition rates and the matching function. They show that if the matching function describes only matches between job-seekers and new employers—not recalls—then matching efficiency is estimated to have declined much more during the Great Recession. Key to their result is that workers on temporary layoffs are not the only ones who experience recalls; about 20 percent of workers who report that they permanently lost their jobs are nonetheless eventually recalled. In our work, we disaggregate workers by their reason for unemployment but do not attempt to distinguish between matches with new employers and recall by the previous employer. Thus, in our specification, a group that is more likely to be recalled will have a higher matching efficiency.

Barlevy (2011) calculates the decline in matching efficiency from the shift in the Beveridge curve, on the assumptions that the separation rate remains unchanged and that unemployment is at its stochastic equilibrium. This analysis depends only on the unemployment rate, not on the number of nonparticipants, job-to-job transitions, or changes in the composition of the unemployed.

Bachmann and Sinning (2012) measure the effects of compositional changes on labor force transition rates without relating these findings to matching efficiency. They find that changes in composition reduce the cyclicality of inflows to unemployment and raise outflows from unemployment early in recessions but reduce outflows later in recessions.

Some papers discuss the decline in matching efficiency, or, equivalently, the outward shift of the Beveridge curve, as the result of a variety of forces. Some, such as Daly, Hobijn, Şahin and Valletta (2012), frame the subject within the more general issue of a possible increase in the natural rate of unemployment. Only part of their discussion relates to changes in matching efficiency. The paper identifies two factors that may have reduced match efficiency since the Great Recession: mismatch and more generous unemployment benefits.

Şahin, Song, Topa and Violante (2012) find that mismatch across industries and occupations accounts for at most one-third of the increase in unemployment during the Great Recession, while geographic mismatch is insignificant. Herz and van Rens (2011) likewise find modest effects of mismatch across industries and very small effects of mismatch across states, while Estevão and Tsounta (2011) find substantial skill mismatches but argue that changes in migration rates and dispersion in unemployment across states are evidence of geographic mismatch as well. These studies all measure mismatches by the distribution of unemployed workers and jobs across distinct markets defined by locations, industries, or occupations. Estevão and Smith (2013) measure skill mismatches in a different way, by imputing wages for labor force participants based on their observed characteristics; if mismatch is low and unemployment is mainly due to low quality of unemployed workers, unemployed workers will have relatively low imputed wages, while if mismatch is high, unemployed workers will have relatively high imputed wages. Consistent with the papers that look at mismatch across distinct markets, Estevão and Smith (2013) find evidence of an increase in mismatch during the recession.

A number of papers, including Daly, Hobijn and Valletta (2011), Fujita (2011), Nakajima (2012), and Valletta and Kuang (2010), culminating in Farber and Valletta (2013), find that extended unemployment benefits raised the unemployment rate by an amount ranging from a few tenths of a percentage point to one point. However, Hagedorn, Karahan, Manovskii and Mitman (2013) argue that many of these analyses do not account for the effect of unemployment benefits on firms' incentive to create jobs and that a research design that accounts for such effects finds a much larger impact from unemployment benefits. Hall (2014b) discusses their paper at greater length.

Davis et al. (2013) provide convincing evidence that vacancies are heterogeneous in their rates of finding workers. In the micro data from JOLTS, they show that the job-filling rate for vacancies is dramatically higher in firms that are growing than in firms with constant employment, a contradiction to the hypothesis that only unemployment and vacancies determine hiring rates. They lack any direct measures of the other inputs, but construct an indirect measure from the JOLTS data that eliminates most of the apparent decline in matching efficiency. They do not consider the topic of this paper, the importance of job-seekers who are not counted as unemployed. Their results fit nicely with ours, in the sense that one reasonable interpretation of the variations in matching efficiency that we measure is exactly the combined effect of the omitted inputs to the matching process that they consider.