Measuring Jobfinding Rates and Matching Efficiency with Heterogeneous Jobseekers *

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Abstract

Matching efficiency is the productivity of the process for matching jobseekers to available jobs. Job-finding is the output; vacant jobs and active jobseekers 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 jobseekers is heterogeneous. The efficiency index for each type of jobseeker is the monthly jobfinding rate for the type adjusted for the overall tightness of the labor market. We find that overall matching efficiency declined smoothly over the period from 2001 through 2013. Measures of matching efficiency that neglect heterogeneity among the unemployed and also neglect jobseekers other than the unemployed suggest a large 38 percent decline in efficiency between 2007 and 2009. We demonstrate that essentially all of this apparent decline results from changes in the composition of jobseekers rather than any true movement in efficiency. We also develop a new approach to measuring matching rates that avoids counting short-duration jobs as job-seeking successes.

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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 financial crisis. We show that disaggregated measures of matching efficiency did not have large declines after the crisis. Rather, the composition of unemployment shifted dramatically toward groups with chronically lower matching efficiency.

The idea has proven useful that matching is a productive process that combines the efforts of jobseekers 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 jobseekers and the number of positions open for recruiting taken as inputs and the flow of newly matched worker-employer pairs as the output. In our framework, matching efficiency is a set of multiplicative shifters of the job-seeking population, analogous to factor-augmenting productivity indexes in production theory. We measure matching efficiency using standard ideas from production theory. In the DMP model, a decline in matching efficiency changes labor-market equilibrium in the direction of higher unemployment. Proper measurement of matching efficiency is a crucial starting point for understanding the sources of episodes of high unemployment.

One of the key points of this paper is that the majority of jobseekers are not counted as unemployed, but rather as out of the labor force or employed. Despite Blanchard and Diamond's (1990) emphasis on this point, most analysis of the U.S. labor market in the matching-function framework has taken unemployment as the measure of job-seeking in the population. In the Current Population Survey (CPS) in 2007, the distribution of hires into new jobs was 16 percent from unemployment, 33 percent from people who were out of the labor force in the previous month, 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.

Barnichon and Figura (2015b) preceded us in measuring matching efficiency with heterogeneous jobseekers. They were the pioneers in studying matching with heterogeneity among the unemployed. Our main contributions building on their paper are: (1) We consider the remaining groups responsible for the great majority of jobfinding success—we distinguish two categories of people recorded as out of the labor force, one with low jobfinding propensities and another with higher propensities, following Barnichon and Figura (2015a). Most importantly, we include currently employed jobseekers, who are hoping for a job-to-job transition. (2) We focus on measuring job-seeking success over longer spans of time, up to the maximum possible in the CPS of 15 months. (3) We measure labor-market tightness as the ratio of vacancies to hires, rather than as the ratio of vacancies to unemployment. Thus we avoid using data on the right-hand side of our basic estimating equation that also appear on the left-hand side. (4) We introduce a new class of matching functions, suited to dealing with multiple categories of jobseekers, that generalizes the standard Cobb-Douglas matching function in allowing different elasticities of job-seeking success with respect to labor-market tightness for different categories of jobseekers. This new matching function satisfies all of the normal properties of matching functions. Our results show unambiguous heterogeneity in those elasticities..

Barnichon and Figura incorporate an explicit term in their equation for mismatch, resulting from the curvature of the matching function. Because the term makes little contribution, and for the reasons laid out in Appendix E, we do not include that term, though we agree that mismatch is a phenomenon of the labor market that is quantitatively important in other contexts.

Our main finding is that matching efficiency measured consistently with our theory fell smoothly at low rates over our sample period starting in 2001. The crisis starting in 2008 did not result in a sudden drop in matching efficiency. Proper treatment of heterogeneity to include jobseekers who are not counted as unemployed, and to distinguish unemployed jobseekers by reason for unemployment and duration of unemployment to date, reverses the finding of a collapse of matching efficiency during and after the crisis.

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 jobfinding rate. For example, the monthly jobfinding 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 jobfinding 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). The trend we measure is over the 15-year period from 2001.

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 (2015a), builds models of search intensity. Variation 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. We focus entirely on job-seeking success. To explain movements of unemployment, our results on exit from unemployment into jobs would need to be combined with results on exit rates from unemployment to non-market activities and results on entry rates to unemployment from employment and non-market activities. Our emphasis on changes in labor-market status over spans of time greater than a single month would apply to those areas of research as well as to job-seeking success.

We take a close look at the jobfinding productivity of different types of jobseekers, but treat vacancies as homogeneous in our base specification. In principle, vacancies should be disaggregated to recognize their heterogeneity and likely variations in recruiting intensity. Davis, Faberman and Haltiwanger (2013) and Gavazza, Mongey and Violante (2016) are important recent studies of that heterogeneity. In Appendix D, we show that incorporation of recruiting intensity as modeled in those papers has essentially no effect on our conclusions, though it has some effect on the trend rate of decline in efficiency.

Section 7 describes some of the many earlier papers on the topic of this paper.

1 Matching Functions with Heterogeneous Job-seekers

For the purposes of this paper, a matching function is a function $m(P, V)$, increasing and weakly concave in a vector of types of jobseekers P and the number of vacancies V. $H = m(P, V)$ is the flow of new hires emerging from the matching process. H is analogous to output and P and $/v$ are analogous to factor inputs, so m is analogous to a production function. Job-seekers of type i have an increasing job-seeking success hazard $\phi_i(T)$. Here $T = V/H$, the ratio of vacancies to hires, which is the average duration of a vacancy. T measures the tightness of the labor market.

> **Assumption. Common pools of vacancies and competing jobseekers:** All types of jobseekers have success rates that depend on the same scalar measure of tightness, T.

The flow of new hires is

$$H = \sum_i \phi_i(V/H) P_i. \tag{1}$$

The unique solution to this equation defines the matching function $H = m(P, V)$. The assumption that the success hazard depends on T rather than separately on P and V implies that the matching function has constant returns to scale. We impose this property by assumption because it has received support in the earlier literature on the matching function.

In the standard setup, the functions $\phi_i(T)$ are the same for all types. In that case, the matching function solves

$$H = \phi(V/H)\sum_i P_i, \tag{2}$$

which implies a standard matching function $H = m\left(V, \sum_i P_i\right)$. With heterogeneous types, each with different ϕ_i functions, the magnitude of ϕ_i conveys the level of matching efficiency for type i and the elasticity conveys the response of the matching rate to market tightness. One source of differences in the elasticity would be differences in the responsiveness of search effort to tightness. Our empirical work finds unambiguous differences in the elasticities.

Appendix A provides a detailed comparison of the approach we use, based on measuring tightness by the duration of a vacancy, with the standard DMP approach, based on measuring tightness by the vacancy/unemployment ratio. The two approaches are fully isomorphic with a single type of jobseeker. We demonstrate that a our setup delivers jobfinding rates that are driven by a single index, and that the index is the duration of a vacancy, which is the same for all types.

The matching efficiency of type i jobseekers at a reference level of tightness $\bar{T}$ is

$$\mu_i = \phi_i(\bar{T}). \tag{3}$$

Notice that we do not break down matching efficiency into components of search effort and jobfinding success per unit of search effort, because we do not measure search effort directly. See Hornstein and Kudlyak (2015) for a setup where search effort is inferred indirectly. Our approach to estimation does make adjustments for differences in search effort associated with observed personal characteristics, as we will explain shortly. We do not consider the distinction between a contact of a jobseeker 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 jobfinding rates that we measure. We refer to μ_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.

An index of weighted average matching efficiency at a reference set of population shares s_i is

$$\mu = \sum_i s_i \phi_i(\bar{T}). \tag{4}$$

We assume that

$$\phi_i(T) = \gamma_i T^{\eta_i}, \tag{5}$$

so

$$\mu_i = \gamma_i \bar{T}^{\eta_i}. \tag{6}$$

For comparison with other estimates of labor-market matching functions, we note that the elasticity of the matching flow rate with respect to V is

$$\frac{\sum_i \gamma_i \eta_i T^{\eta_i} s_i}{\sum_i \gamma_i (1+\eta_i) T^{\eta_i} s_i}. \tag{7}$$

Here s_i is the share of the population in status i. In the standard case of only one kind of jobseeker, the elasticity is η, a constant across i, and the matching function is Cobb-Douglas with elasticity $\frac{\eta}{1+\eta}$.

2 Job-Finding Rates

2.1 Time span for measuring jobfinding success

The standard concept of a jobfinding rate is the probability that a jobseeker 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 in March 2009 on account of the earlier loss of a permanent job 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.

A spell of unemployment may well be contained within a span. We observe people unemployed when they enter the CPS, employed briefly, then unemployed, and then employed late in the span. Turnover within spans has a central role in our empirical analysis.

Over the 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 jobfinding success because jobseekers may find brief jobs, lasting only a few weeks or a month or two—see Hyatt and Spletzer (2013a). 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.

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, an Unemployment Spell Beginning in November 2008, and a Span Beginning in March 2009

To see this, consider a simple model of labor-market turnover. There are two kinds of jobs, short and long. Job-seekers 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.

Our choice to examine short and long spans is an attempt to extract useful information about job-finding success from the limited data in the CPS. The ideal data would contain accurate month-by-month records of labor-market status over five or more years. Such data would support a model that showed how individuals who start from a given status find jobs, lose or leave them, search more, take subsequent better jobs, and ultimately settle into durable job matches or non-work activities. The CPS reports in only eight separate months. Our short-span measure uses just the initial four months, whereas the long-span measure considers what has happened about a year after the conditioning month. Both measures describe experience that is influenced by both jobfinding success and separations. See Krueger et al. (2014) for further discussion of the benefits of including longer spans in the study of jobfinding success.

2.2 Specification of the jobfinding function

In the formalization of our setup, the jobfinding 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

$$f_{i,t,\tau,x} = \frac{\exp\left(\kappa_{i,t,\tau} + x'\beta_{i,\tau}\right)}{1 + \exp\left(\kappa_{i,t,\tau} + x'\beta_{i,\tau}\right)}, \tag{8}$$

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 jobfinding 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

3.1 The Current Population Survey

We use data from the monthly CPS for November 1999 through March 2015. These data permit the calculation of jobfinding 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 (2015a) found large differences in jobfinding 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 study jobfinding success conditional on standard observable demographic characteristics and on the initial status. We find large differences in jobfinding rates by initial status.

Our results would have the cleanest interpretation if the demographic characteristics accounted for all the heterogeneity in each group defined by initial status. We do not make that claim—people laid off during a recession, for example, differ in some respects not captured by their observable demographic characteristics from people laid off during a boom. We find that overall labor-market tightness accounts for most of the large movements in jobfinding rates around trend within each initial-status group. This finding supports the view that our breakdown by 16 groups captures most of the heterogeneity among jobseekers.

We match respondents across months using the method of Nekarda (2009). His 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 jobfinding 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 jobfinding rates across long time spans because errors in recording race, sex, and age during intervening months are less likely to break the match. Table 7 in Appendix B shows the success rates for the matching process.

We remove high-frequency, likely spurious transitions between unemployment and nonparticipation following Elsby, Hobijn and Şahin (2015b). 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 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 as 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 jobfinding 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

The CPS uses a stratified sampling design and has a rotating panel structure that can induce correlations between observations on different individuals at different dates. These correlations are difficult to account for analytically. Therefore, 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 jobfinding 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 (8) for that initial status, date, and time span. Instead, we impute the 15-month jobfinding rates for new entrants in February 2008 based on the jobfinding rates in adjacent months and years. Specifically, we impute

$$f_{i,\text{Feb 2008},15} =$$

$$\frac{1}{2}\left(\frac{f_{i,\text{Feb 2007},15}}{f_{i,\text{Jan 2007},15}+f_{i,\text{Mar 2007},15}}+\frac{f_{i,\text{Feb 2009},15}}{f_{i,\text{Jan 2009},15}+f_{i,\text{Mar 2009},15}}\right)(f_{i,\text{Jan 2008},15}+f_{i,\text{Mar 2008},15}), \quad (9)$$

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

3.2 Attrition in the CPS sample

Some respondents drop out of the CPS survey during the 16 months they are assigned to the survey. Following standard principles of attrition adjustment, we offset the potential bias caused by higher weighting of the respondents who are less likely to drop out. For each date t and span τ, we estimate a fractional-logit model for the probability that an individual observed at t is also observed at $t+\tau$, as a function of the same variables that are on the right-hand side of our logit for jobfinding rates. Let $\hat{p}_{i,t,\tau}$ be the predicted probabilities of remaining in the sample from this model for individual i observed at t, over a span of τ months. To estimate the jobfinding rates over a span of τ months from the logit equation, we weight each observation by $1/\hat{p}_{i,t,\tau}$ times the product of Nekarda's linking weight and the survey weight. Thus observations with a lower probability of remaining in the sample are given higher weight. We re-estimate the weights for each bootstrap sample. We use a fractional logit model (Papke and Wooldridge (1996)) because *remaining in the sample* is not a binary event with Nekarda's weights and so cannot be the dependent variable in a conventional logit model.

Reweighting to account for attrition did not change the estimated jobfinding rates appreciably. This finding is unsurprising because the variables in the attrition model are also controls in the model for jobfinding rates. In essence, the attrition weights account only for potential misspecification of the functional form of the jobfinding rate equation.

4 Estimated Job-Finding Rates

4.1 Job-finding rates implied by the logit specification

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 jobfinding rates standardized for the changing composition of the labor force. We standardize 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

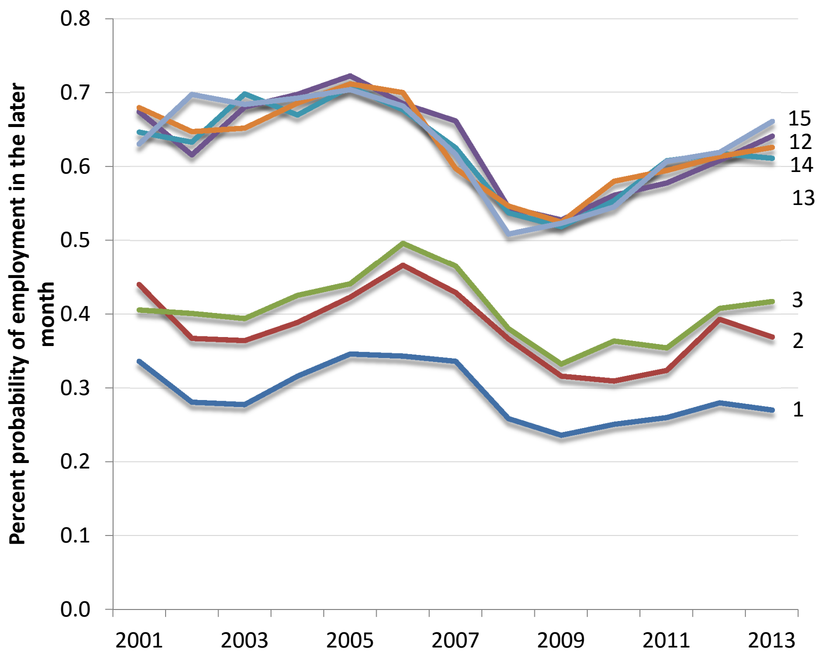


Figure 1: Estimated Job-Finding Probabilities for Losers of Permanent Jobs

from 2001 through 2013, we calculate the fitted jobfinding 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 jobfinding probabilities for one important initial status, *recently lost permanent job*. The data are annual averages of monthly calculations. The horizontal axis gives the date of search, corresponding to the calendar month of the CPS. Each curve is labeled on the right by the number of months in the future of the employment status of the respondent—for example, in 2005, the curve labeled 12 refers to survey responses recorded during 2005 referring to employment in 2006. 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 over the period since 2001.

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-month curve. In 2007, the one-month probability was 34 percent and the two-month probability was 43 percent. If the monthly jobfinding 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 jobseekers 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 jobfinding 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.

Another 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. Hyatt and Spletzer (2013a) provide evidence from a variety of sources on the incidence of short-duration jobs.

Alternatively, the decrease in the jobfinding hazard as the span lengthens may reflect duration dependence—either spurious duration dependence that results from unobserved changes in heterogeneity in jobfinding rates by duration among workers of the same observed type, or structural duration dependence in which a longer search causes each jobseeker's jobfinding rate to fall. We do not distinguish between these sources of duration dependence; for recent efforts at measuring duration dependence, see, for example, Hornstein (2012) and Alvarez, Borovičková and Shimer (2016).

Table 2 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 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 recent 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 jobs quickly have declined over time, while success rates for finding and keeping jobs over longer periods have been roughly stable. As we noted earlier, longer-span employment probabilities are better at capturing success in finding longer-duration jobs.

The employment probabilities in Table 2 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 jobfinding 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).

A striking feature of Table 2 is that the long-span jobfinding rates for laid-off workers are barely higher than the short-span jobfinding rates for these workers. This result implies either that laid-off workers have a very high separation rate upon reemployment or that, after a few months of unemployment, laid-off workers' reemployment hazard is very low. The latter implication is consistent with the findings of Katz (1986) and Fujita and Moscarini (2013) of strong negative duration dependence in recall probabilities for laid-off workers.

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. Our research deals with only the outflow rate from long-term unemployment. An understanding of the high levels of long-term unemployment following the crisis of 2008 would require a study of inflow rates to unemployment, a subject complementary to the subject of this paper—see Ahn and Hamilton (2016) and Hall (2017b).

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	60.5	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	39.5	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 2: Standardized 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 jobseekers.

Table 2 shows that there is generally a downward trend in jobfinding rates since 2001. Only three of the initial status categories had short-span finding rates that were higher in 2013 compared to 2003. Labor-market tightness, as measured by T, the duration of vacancies, was slightly higher in 2013 than in 2003. The trend in long-span finding rates is less frequently downward. Only two of the initial status categories had 2013 rates below 90 percent of their 2003 rates. An important factor in the decline in jobfinding rates appears to be the decline in the incidence of short jobs. Earlier we noted that the long-span finding rate gives less weight to short jobs, so the finding of larger declines in short-span finding rates points in the direction of a role for the decline in short jobs. This paper does not consider the incidence directly, because we do not study job separations.

Davis and Haltiwanger (2014) called attention to the decline in labor turnover in recent years. Because short jobs account for the great majority of separations, a decline in overall separations necessarily involves a diminished incidence of short jobs. Hyatt and Spletzer (2013b) and Hyatt and Spletzer (2015) study jobs that last only one quarter, using data from the Census Bureau's Longitudinal Employer-Household Dynamics program. That program provides data from Quarterly Census of Employment and Wages, obtained from unemployment insurance reports. They find that the overall separation rate fell from 21 percent per quarter in 1996 to 16 percent in 2012. The separation rate from jobs that had lasted no more than one quarter fell from 8 percent in 1996 to 5 percent in 2012. Well over half of the decline in separations came from the decline in separations from the shortest jobs.

Table 3 and Table 4 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 jobfinding rate for month 1 applies in all the later months, along with 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 jobfinding 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.

		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	61.7
	(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	61.2	66.2	56.9
	(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	43.7
	(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	44.1	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 3: 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	56.2
	(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	49.9	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	53.0	79.1	79.4	79.6	79.8
Long-term unemployed	Actual	16.0	22.3	25.9	35.8	37.2	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 4: Subsequent Employment Probabilities by Initial Status, Actual and Benchmark, 2007: Unemployed for Months and Long-Term

	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	41.1	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 ago	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 Short-Span Job-Finding Rates between 2007 and 2010

4.2 Changes in jobfinding rates between 2007 and 2010

Table 5 compares our findings for demographically adjusted jobfinding 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 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 months. 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 recently quit fraction fell, and the lost permanent job fractions 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 the same index of labor-market tightness influences jobfinding rates for all types of jobseekers.

The column headed *Contribution to total rate* is the product of the population fraction in the first column and the jobfinding 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 jobfinding rate was 5.4 percent. But this group, despite its low jobfinding rate, contributed 1.7 percentage points to the total volume of jobfinding, 6.3 percent of the working-age population each month. Workers, in the third line of the table, had the lowest jobfinding rate, 5.0 percent, but account for almost half of all jobfinding. 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.

From the peak year of 2007 to the severely depressed year of 2010, the average jobfinding 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 jobfinding 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 jobfinding 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 jobfinding flow almost unchanged, because the population shifted into unemployment, with high jobfinding rates, enough to offset the general decline of jobfinding 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 jobfinding rate among the unemployed. Of this, 7.4 percentage points arise 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 jobfinding 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 jobfinding 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 jobfinding 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 Combining data from different sources

We use data from the Job Openings and Labor Turnover Survey (JOLTS) to measure labor-market tightness, T. JOLTS is a survey of employers and is independent of the CPS. We view the two surveys 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 a single factor, T, indexes tightness throughout the overall labor market.

From JOLTS, we measure vacancies V and hires H^J, where the superscript distinguishes the JOLTS measure of hires from the measure H that can be computed from the CPS. The ratio, $T = V/H^J$, is market tightness (we use the ratio of the averages of V and H^J, not the average of the underlying vacancy durations). No information about jobfinding rates or matching efficiency is present in JOLTS. 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/jobseeker ratio, usually called θ. Our procedure uses the variable T from JOLTS as a measure that describes the CPS labor market as well as the JOLTS labor market. Under that assumption, CPS data on jobfinding rates identify matching efficiency and the elasticities of the jobfinding functions η_i.

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 this maintained assumption.

5.2 Basic equation for estimation of the elasticity of the jobfinding rate with respect to tightness

Equation (6) leads to the following model of the measured log jobfinding rate for initial status i, over a τ-month span, in month t :

$$\log f_{i,\tau,t} = \gamma_{i,\tau,t} + \eta_i \log T_t + \epsilon_{i,\tau,t}, \tag{10}$$

where $f_{i,\tau,t}$ is the observed jobfinding rate, $\gamma_{i,\tau,t}$ contains constants and trends, and $\epsilon_{i,\tau,t}$ is an index of matching efficiency, the residual in the equation. The parameter η_i is the elasticity of jobfinding with respect to tightness, $T_t = V_t/H_t^J$, which is the duration of vacancies in JOLTS, the ratio of the stock of vacancies to the flow of hires.

We assume that

$$\gamma_{i,\tau,t} = \alpha_{i,\tau} + \delta_{i,\tau} t + \omega_{i,\tau}\mathbb{I}(t \geq \text{ January 2008})t + \psi_{i,\tau,s}, \tag{11}$$

where s is the month of the year, $\delta_{i,\tau} t$ is a linear trend that operates over the whole sample, $\omega_{i,\tau}\mathbb{I}(t \geq \text{ January 2008})t$ is an additional trend starting in 2008, and $\psi_{i,\tau,s}$ is a seasonal effect for each month. The model we estimate is thus

$$\log f_{i,\tau,t} = \alpha_{i,\tau} + \delta_{i,\tau} t + \omega_{i,\tau} \mathbb{I}(t \geq \text{ January 2008}) t + \psi_{i,\tau,s} + \eta_i \log T_t + \epsilon_{i,\tau,t}, \tag{12}$$

Note that this model has a separate equation for each $\{i, \tau\}$ pair—there are no cross-equation restrictions.

5.3 Simultaneity and identification

The jobfinding rate f and labor-market tightness T are jointly determined endogenous variables (for simplicity we consider the case of a single type of jobseeker). Within the broad class of labor-market models associated with Diamond, Mortensen, and Pissarides, the two variables are determined in a two-equation system,

$$\log f = \alpha + \eta \log T + \epsilon \tag{13}$$

and

$$\chi T = J. \tag{14}$$

The second equation expresses the zero-profit condition—χ is the monthly cost of maintaining a vacancy, so χT is the expected cost of hiring one new worker, and that amount equals the payoff J to making a new hire.

A key issue of identification is what happens to tightness T if matching efficiency ϵ changes. Identification generally involves plausible assumptions about the orthogonality of measured variables to the disturbance, in this case, ϵ. From equation (14), tightness can only change when ϵ changes to the extent J changes. J is the present value over the duration of the job of the difference between the worker's marginal product and wage. An increase in ϵ will presumably raise the jobfinding rate, which will lower the unemployment rate, raise employment, and lower the marginal product of labor. Chodorow-Reich and Karabarbounis (2016) show that productivity changes carry over to opportunity cost changes, so there is no effect on the difference between productivity and the wage as productivity varies. Thus we take the effect on T operating through the marginal product of labor to be zero. The increase in the jobfinding rate will raise the value of the worker's outside option in the wage bargain—to the extent that the outside option is influential in the bargain, tightness will fall. But Hall (2017a) presents evidence that the outside option has almost no effect on the wage bargain, in a realistic alternating-offer setting. The influence of the outside option under the conventional assumption of a Nash wage bargain arises from the unrealistic influence of the irrelevant option to discontinue bargaining that is implicit in the Nash bargain setup.

In principle, changes in ϵ could induce changes in the discount rate that would influence T, but we do not believe that these could be important. Hall (2017a) shows that it takes quite large changes in discounts to change tightness materially.

Our overall conclusion is that spontaneous movements in ϵ have essentially full direct effects on $\log f$ and small effects on T. Having excluded fluctuations in T from certain channels, we need to explain the sources of the high observed volatility of T. Hall (2017a) shows that large measured fluctuations in financial discounts results in volatility of J, a financial present value, and thus of T, via equation (14). Another source could be spontaneous volatility of the opportunity cost of work, which would move the wage relative to the marginal product of labor and generate movements of J and thus of T. In the presence of product market power, J is the present value of the difference between the marginal revenue product of labor and the wage. Thus a third source of volatility in T could be fluctuations in markups, which create changes in the marginal revenue product—see Rotemberg and Woodford (1999).

Our assumption of a single economy-wide index of market tightness, measured by the vacancy duration calculated from JOLTS, may be an oversimplification. In Appendix E, we discuss this issue more fully. One source of departure from the assumption is the presence of random errors in measuring duration. The other is what we call *mismatch* error, resulting from the use of the log of aggregate data on vacancy duration which are calculated as averages of the levels, not the logs, of heterogeneous sectors.

Tobin (1972) introduced the concept of mismatch, which has become an important part of the analysis of aggregate unemployment and other labor-market measures, including tightness. Mismatch arises from the aggregation of nonlinear relationships. Tobin observed that, with a convex Phillips curve, the aggregate level of unemployment corresponding to a given inflation rate will be higher when there is more dispersion in unemployment across markets. The effect of mismatch depends on the extent of heterogeneity and the amount of curvature. Şahin, Song, Topa and Violante (2014) describe modern thinking along this line. Their measure compares actual unemployment to the planner's allocation of searchers to markets. They find that mismatch across industries and occupations accounts for somewhat under 20 percent of the increase in unemployment during the Great Recession, while geographic mismatch is insignificant. Their findings support the hypothesis that the dominant source of movements in tightness is an aggregate influence acting on all sectors, though not necessarily with the same elasticity.

Appendix E presents evidence from industry data in JOLTS that there is a an important component of the monthly aggregate vacancy duration, T_t,that does not resemble the properties expected of an industry-level component of true tightness, but rather is essentially entirely a higher-frequency component that is likely a combination of sampling error and other measurement problems. In other words, despite our beliefs that there is no fundamental feedback from the disturbance ϵ to the level of true tightness and little problem with mismatch, there is probably an errors-in-variables problem in estimation. This finding supports the use of an instrumental-variable estimator. As the instrument, we use data from the BLS's CES payroll survey, which has a large number of responding firms and correspondingly low sampling errors. Our identifying hypothesis is that the payroll survey is free of the short-term noise arising from the sampling error in JOLTS. We form forward moving averages of the JOLTS measure of T as the endogenous variable and similar forward moving averages of the employment count from the CES as the instrument—these are equally weighted over 3 and 15 months. We also include a time trend and monthly dummies as instruments, because they are exogenous variables included in the equation. We include data for January 2001 through December 2013 plus the additional later values of T and the payroll count.

5.4 Further aspects of estimation

We average the three short spans (one, two, and three months after the conditioning status) to form the jobfinding rate for the first span category, called *short*, and the four longer spans (12 through 15 months) to form the second jobfinding rate category, called *long*. For the short jobfinding rate, we can include in our data the job-changing rate for those starting in the *employed* status. For the long jobfinding 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 (12) for each initial status by instrumental variables, using monthly data on jobfinding rates. 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 0.10 for short spans both with and without job-to-job, and 0.07 for long spans. We do not believe that a generalized 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, a time trend coefficient, and an elasticity with respect to tightness. Over the full sample, we have $12 \times 13 = 156$ observations for each status.

The residuals from equation (12) form an index of detrended matching efficiency:

$$\epsilon_{i,t,\tau} = \log f_{i,\tau,t} - [\alpha_{i,\tau} + \delta_{i,\tau} t + \omega_{i,\tau} \mathbb{I}(t \geq \text{ January 2008}) t + \psi_{i,\tau,s} + \eta_i \log T_t], \tag{15}$$

as the observed jobfinding 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 jobfinding 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.

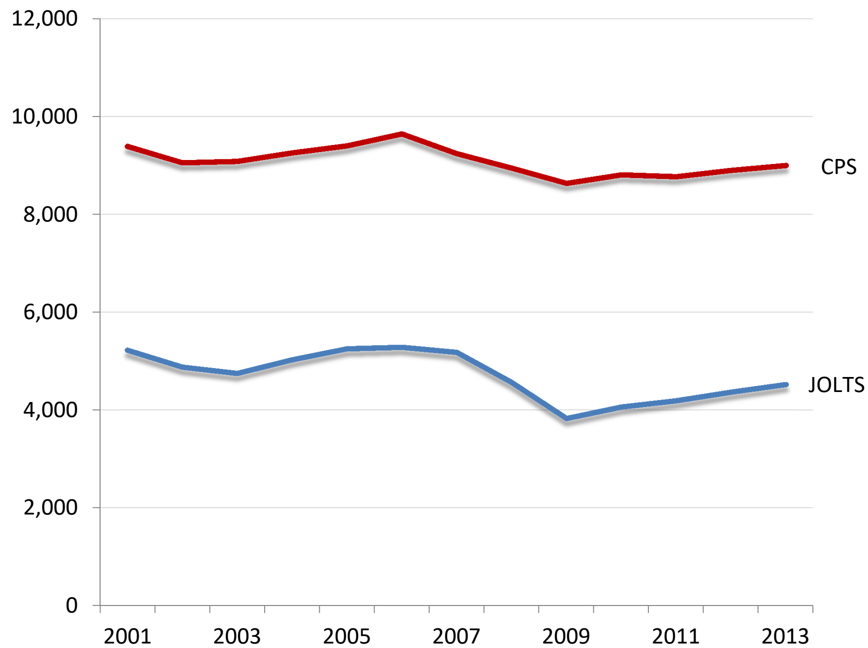


Figure 2: Number of Monthly Hires, in Thousands, from JOLTS and the CPS

We use the estimates of jobfinding rates adjusted for the changing characteristics of the population, as discussed earlier, as the left-hand variable of equation (12). 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.

5.5 Measuring tightness, T

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, T, using the JOLTS measures of hires and vacancies. The measures of hires and vacancies are separately smoothed with annual

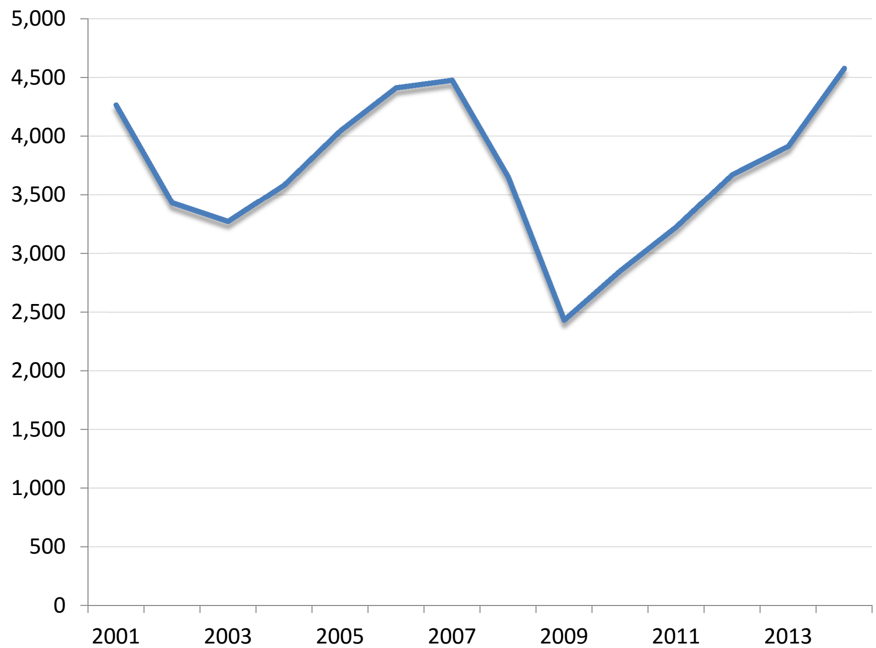


Figure 3: Number of Job Openings, in Thousands, from JOLTS

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

Appendix D considers adjustments of the average duration of vacancies for recruiting intensity.

5.6 Estimates

Our objective is to estimate the parameters η_i, the elasticities of the jobfinding rates of jobseekers with respect to tightness T, across status i. To help interpret the values, recall that, in the case of a single type of jobseeker, the elasticity of the matching function is $\eta/(1+\eta)$. For example, an estimate of $\eta = 1$ corresponds, in this sense, to a matching elasticity of 0.5.

Table 6 shows estimates of the based on equation (12), using data for the JOLTS period, 2001 through 2013. The left panel refers to short spans and the right panel to long spans. In each panel, the left column is the estimated elasticity, with standard error below. The middle column shows the trend for the entire period since 2001, in percent per year. The right column shows the extra trend starting in 2008.

Initial status	*Short span*			*Long span*		
	Elasticity with respect to vacancy duration	*Trend in efficiency, 2001-2013, percent per year*	*Additional trend in efficiency, 2008-2013, percent per year*	*Elasticity with respect to vacancy duration*	*Trend in efficiency, 2001-2013, percent per year*	*Additional trend in efficiency, 2008-2013, percent per year*
Out of labor force	0.50	-2.54	-1.98	0.60	-2.65	-1.06
(Standard error)	(0.04)	(0.20)	(0.37)	(0.05)	(0.22)	(0.42)
Want job	0.79	-2.46	-2.06	0.76	-2.90	0.70
(Standard error)	(0.07)	(0.33)	(0.61)	(0.08)	(0.38)	(0.70)
Employed	0.35	-2.78	0.19			
(Standard error)	(0.03)	(0.14)	(0.28)			
Recently laid off	0.21	0.11	-1.22	0.34	-2.09	3.58
(Standard error)	(0.07)	(0.33)	(0.71)	(0.11)	(0.48)	(0.91)
Recently lost permanent job	0.96	-1.05	-2.44	0.77	-3.06	1.50
(Standard error)	(0.16)	(0.62)	(1.32)	(0.15)	(0.68)	(1.25)
Temp job recently ended	0.47	-0.87	-1.84	0.70	-3.56	2.78
(Standard error)	(0.17)	(0.71)	(1.54)	(0.26)	(1.07)	(2.00)
Recently quit a job	0.48	-0.51	-3.58	0.47	-2.24	2.18
(Standard error)	(0.19)	(0.67)	(1.41)	(0.25)	(0.95)	(1.82)
Recently entered LF	0.77	-3.06	-4.86	0.13	-0.25	-0.54
(Standard error)	(0.40)	(1.76)	(3.38)	(0.58)	(2.39)	(4.25)
Recently re-entered LF	0.76	-2.46	-1.97	0.65	-1.59	-0.69
(Standard error)	(0.14)	(0.61)	(1.22)	(0.18)	(0.70)	(1.36)
On layoff for months	0.50	-0.54	-1.20	0.38	-2.29	2.82
(Standard error)	(0.10)	(0.55)	(1.14)	(0.15)	(0.55)	(1.23)
Lost permanent job months ago	1.21	-2.16	-2.12	0.93	-4.57	3.50
(Standard error)	(0.11)	(0.50)	(1.15)	(0.12)	(0.44)	(0.84)
Temp job ended months ago	0.75	-1.64	-1.48	0.46	-3.06	2.89
(Standard error)	(0.16)	(0.71)	(1.43)	(0.19)	(0.85)	(1.68)
Quit a job months ago	0.83	-2.56	-0.42	0.98	-2.24	-1.35
(Standard error)	(0.17)	(0.68)	(1.45)	(0.20)	(0.83)	(1.51)
Entered LF months ago	1.24	-3.37	-3.24	0.78	-2.30	2.45
(Standard error)	(0.24)	(1.41)	(2.53)	(0.28)	(1.68)	(2.84)
Re-entered LF months ago	0.83	-2.45	-1.83	0.69	-2.43	-0.11
(Standard error)	(0.11)	(0.47)	(1.01)	(0.12)	(0.45)	(0.94)
Long-term unemployed	1.23	-3.89	-2.19	0.51	-3.75	3.55
(Standard error)	(0.11)	(0.64)	(1.12)	(0.12)	(0.65)	(1.25)

Table 6: Elasticity and Trend Estimates

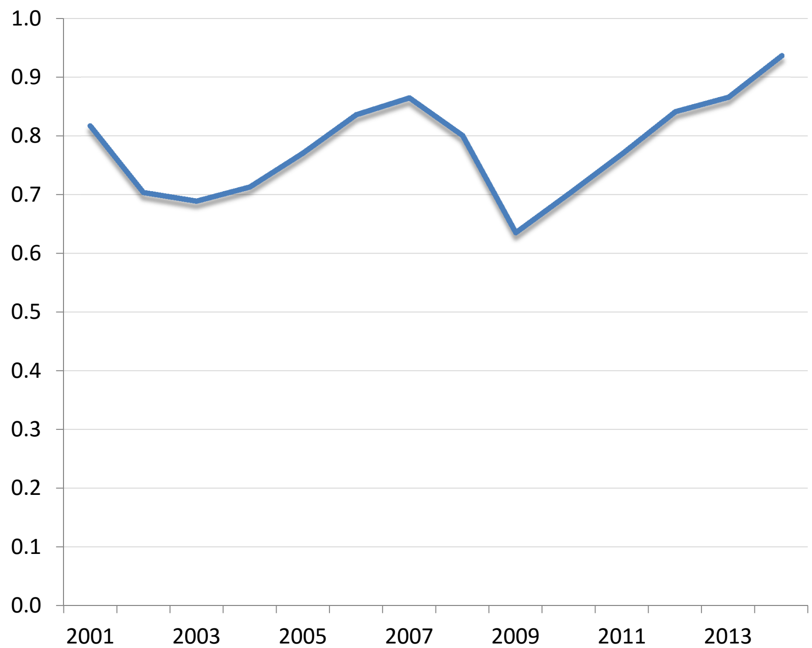


Figure 4: Average Duration of Vacancies, Calculated from JOLTS

All of the elasticities of the jobfinding rate with respect to tightness are positive and most have small bootstrap standard errors. The elasticities have substantial heterogeneity—the evidence against equal elasticities (the case of a Cobb-Douglas overall matching function) is quite strong. For both short and long spans, the *recently lost permanent job* and *lost permanent job months ago* initial statuses have high elasticities of jobfinding with respect to tightness.

The composition of jobseekers changes in recessions for two reasons. First, the mix of reasons why people leave jobs can change. Second, our method reveals a dynamic effect that occurs because recessions do not affect all jobseekers equally. We find that jobfinding rates are much more responsive to labor market tightness for some categories of jobseekers, such as losers of permanent jobs, than for other categories, such as people who were recently laid off. When a recession hits and tightness falls, the categories with higher elasticities experience larger reductions in jobfinding rates. As a result, these categories grow to make up a larger share of the pool of jobseekers. We show that accounting for these compositional changes is fundamental to proper measurement of matching efficiency.

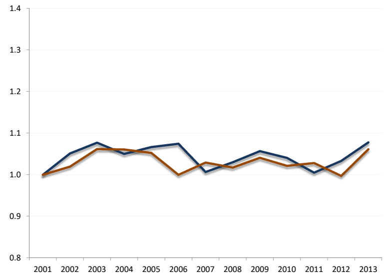
Without the sole exception of the short-span rate for the *recently laid off* category, jobfinding rates adjusted for changes in labor-market tightness trended downward over the period from 2001 through 2007. The downward trend is particularly steep for the long-term unemployed (more than six months). The downward trend in short-span matching efficiencies generally declined faster in 2008 and later. On the other hand, in the majority of the initial-status categories, the earlier downward trend in efficiency for long spans reversed partially or even fully in the period starting in 2008. There is little support for the hypothesis that matching efficiency fell after the financial crisis and the following long period of turmoil.

The average across the short-span elasticities is 0.527, with bootstrap standard error 0.060. The corresponding elasticity of the matching function with respect to vacancies, if all the elasticities had this value, from equation (7), is 0.345, and the elasticity with respect to equal proportional increases in all statuses, is one minus this amount, 0.655. Both have bootstrap standard errors of 0.026. For the short-span equations, the matching elasticity estimate is in line with the estimates surveyed in Petrongolo and Pissarides (2001). We are not aware of any previous research on the longer-span matching-function elasticity.

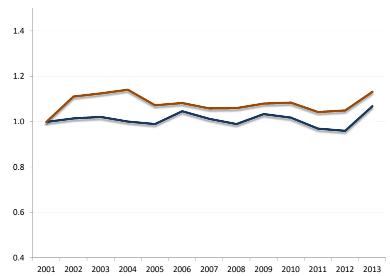
6 Matching Efficiency

6.1 Indexes of matching efficiency calculated from our estimates

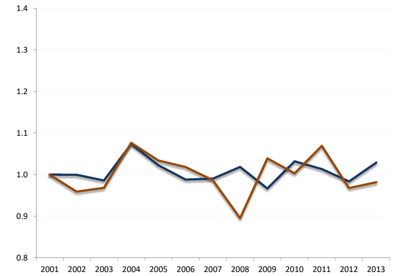
We calculate indexes of matching efficiency for each of the 16 labor-market statuses. Because we hold the distribution of individuals' characteristics constant in calculating the jobfinding rates on the left-hand side of equation (12), the movements in these indexes are insulated from changes in the 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 (15) and are indexes normalized to one in 2001. The trends are shown in Table 6 and allow for different trends, generally downward, in matching efficiency in the pre-crisis period 2001 through 2007 and in the following period, 2008 through 2013.



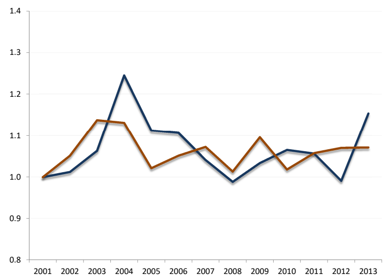
(a) Not in LF



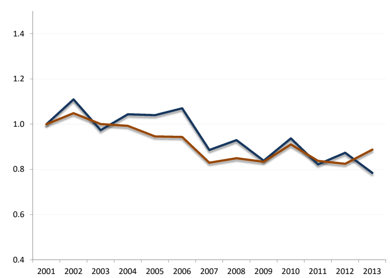
(b) Want job



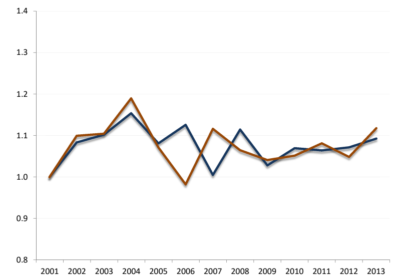
(c) On layoff for months



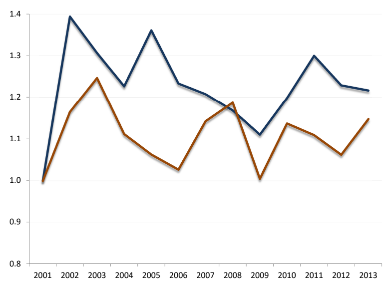
(d) Lost permanent job months ago



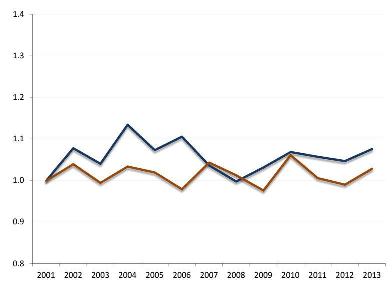
(e) Temp job ended months ago



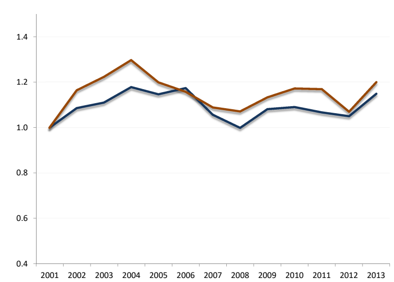
(f) Quit months ago



(g) Entered months ago



(h) Re-entered months ago



(i) Long-term unemployed

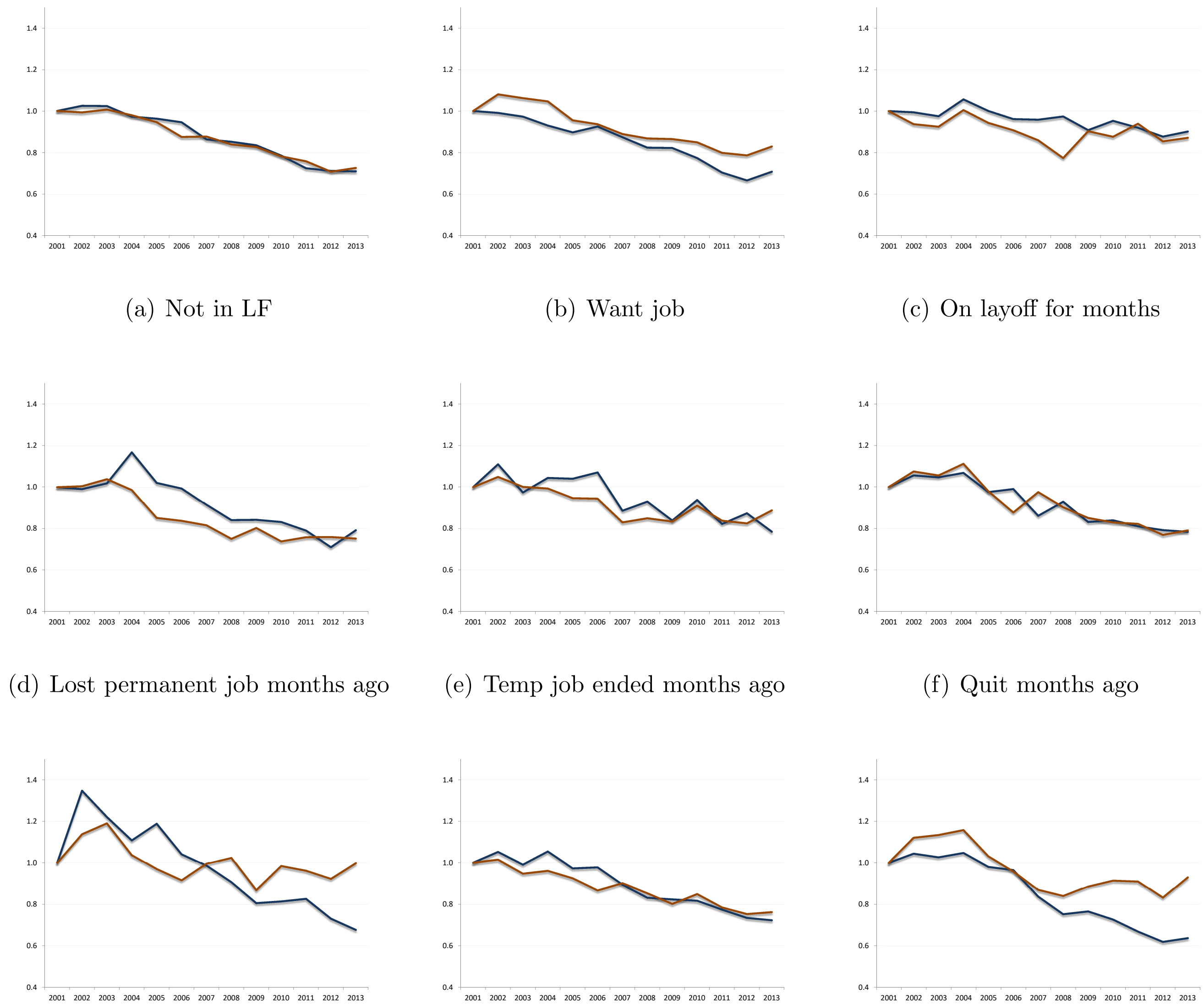
Figure 5: Detrended Matching Efficiency for Nine Statuses (Short Spans in Blue and Long Spans in Orange)

The overall impression from the 9 categories shown in Figure 5, and confirmed for the remaining 7 categories not shown, is that movements in matching efficiency around its downward trend since 2001 are generally small and unsystematic.To put it differently, the tightness measure is able to take account of changes in the labor market when estimation occurs over the relatively mild recession of 2001 and the deep recession that started at the end of 2007, after adjustment for the two-part trend in our setup. We noted earlier that the correlation across the 16 categories of the residual—the measure of matching efficiency—is essentially zero. The only respect in which our estimation procedure lowers the correlations is the common set of right-hand variables in the equations: log tightness, seasonal effects, and the two-part trend. The absence of a non-trend, non-seasonal, non-tightness common element in the residuals is evidence in favor of our specification.

The pattern of annual matching efficiency for the initial status *lost permanent job months ago* is representative in terms of its movement over time, and more precisely estimated because large numbers of jobseekers fell into this category. For both short and long spans, Table 6 shows downward trends in 2001 through 2007, and almost no trend in 2008 through 2013. Both measures of detrended efficiency rose during the recovery from the 2001 recession, and fell as the economy reached its peak in 2007. After 2008, long-span efficiency remain constant, while short-span efficiency rose at the end of our sample period.

In the closely watched category *long-term unemployed*, where the estimated trend over 2008 through 2013 was also close to zero, the short- and long-span indexes move generally together, rising in the recovery following the 2001 recession, falling from 2004 to the peak in 2007, flattening during the recession, then growing at the very end.

Figure 6 shows the indexes without subtraction of the trend terms in equation (12). The scale is compressed relative to the detrended indexs in Figure 5 to accommodate the trends. Notice that the trends are downward over time for all of the initial statuses shown, corresponding to the ratios of 2012 jobfinding rates to 2001 rates in Table 2 that are almost all below one. In most of the categories, the downward trend in efficiency after 2008 is less than in the earlier period, for the long-span measure. In particular, despite the huge increase in unemployment after 2007, there is little sign of any corresponding movement of matching efficiency.



(a) Not in LF

(b) Want job

(c) On layoff for months

(d) Lost permanent job months ago

(e) Temp job ended months ago

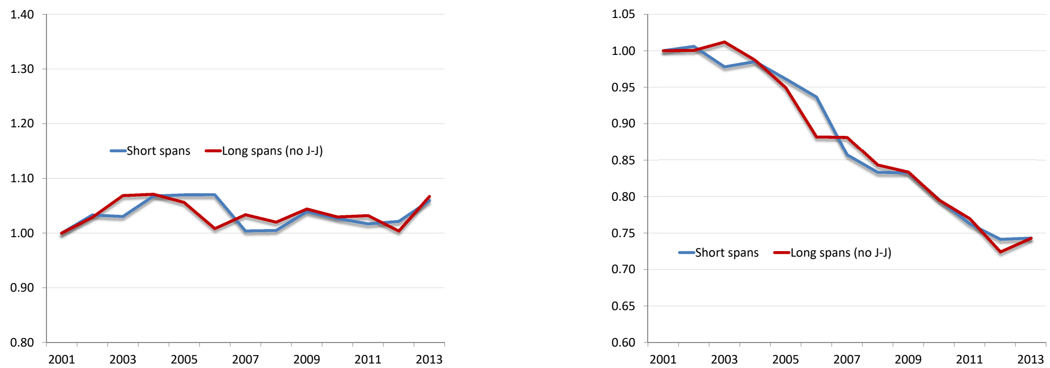
(f) Quit months ago

(g) Entered months ago

(h) Re-entered months ago

(i) Long-term unemployed

Figure 6: Matching Efficiency for Nine Statuses, Including Trend (Short Spans in Blue and Long Spans in Orange)



(a) Detrended

(b) With trend

Figure 7: Weighted Average Matching Efficiency Trend

The left side of Figure 7 shows indexes of weighted average matching efficiency including removal of trends. The indexes use weights calculated as the shares of the components in the population in the three years preceding the crisis, 2005 through 2007. Because the jobfinding rates underlying the indexes hold constant the distribution of worker characteristics conditional on labor-market status, the aggregate indexes hold 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 jobseekers among the initial statuses. The index for short spans includes job-to-job movers while the one for long spans includes only the unemployed and people not in the labor force. Long-span efficiency moves much the same way as short-span. Both are quite smooth—adding over the 16 categories smooths away most of the volatility shown in Figure 5 and Figure 6. The right side of Figure 7 shows the same data without adjustment for trend.

We also constructed Divisia-style indexes with time-varying weights. The difference between these indexes and our fixed-weight indexes was tiny.

6.2 Measuring matching efficiency when there is only one type of jobseeker

Suppose that there is only one type of jobseeker, an unemployed person, without regard to the type of unemployment. We explore this approach because much of the literature on the matching function takes the count of unemployed jobseekers as the single job-seeking input to the function.

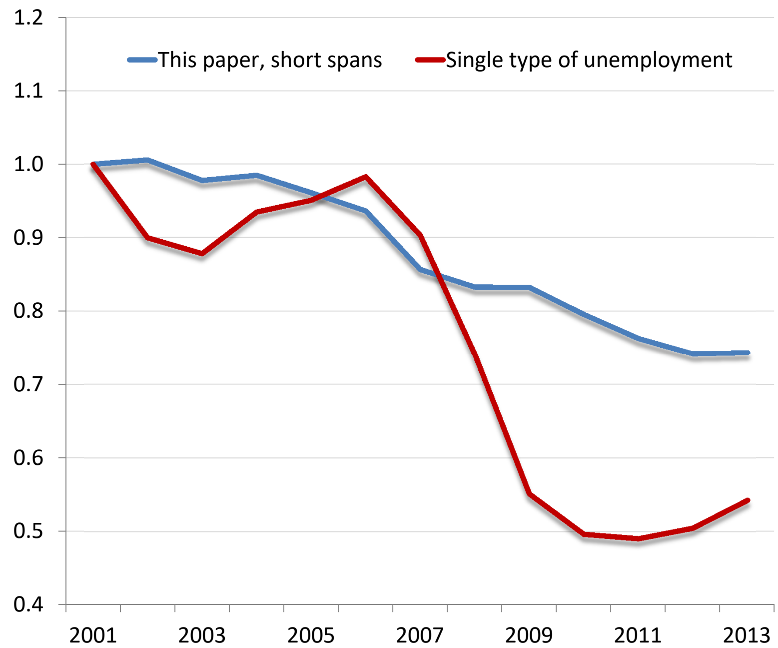


Figure 8: Comparison of this Paper's Measure of Matching Efficiency to a Naive Measure

The relation among the jobfinding rate f_t, matching efficiency, γ_t, and tightness, T_t, is

$$\log f_t = \log \gamma_t + \eta \log T_t. \tag{16}$$

The jobfinding rate is the ratio of hires in the CPS, H_t^C, to the number of unemployed people, U_t. Thus single-type matching efficiency is

$$\gamma_t = \frac{H_t^C/U_t}{T^\eta}. \tag{17}$$

This equation fits quite well with an elasticity of $\eta = 0.5$—the overall jobfinding rate for unemployed people moves with the business cycle, as measured by T_t.

But this approach is fundamentally misleading relative to one that includes all types of jobseekers and that recognizes heterogeneity among the types. The single-type equation finds large movements in γ_t that arise from changes in the composition of the unemployed and not from shifts in matching efficiency for individual types. Figure 8 compares our measure of matching efficiency to the measure that uses unemployment as the sole measure of job-seeking volume.

The single-type measure considerably overstates the decline in matching efficiency between 2007 and 2010, the period when unemployment doubled. It infers a collapse of efficiency from its measure of the jobfinding 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 less than 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 downward trend amount. Notice that the same distortion operated in the recession of 2001 and its aftermath, though not as dramatically.

7 Related Research

Elsby et al. (2015a) 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.

Veracierto 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 jobseekers in the N category. The total number of jobseekers 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 matching efficiency when all people in the economy of working age are potentially jobseekers.

Herz and van Rens (2016) 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.

As we noted earlier, mismatch arises in this paper as a potential source of errors-in-variables bias in our estimates of the elasticity of the job-finding rate.

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 jobfinding hazards while excluding those with low hazards. Such a definition would fit the matching function framework nicely.

Ahn and Hamilton (2016) is an ambitious study of unemployment dynamics with heterogeneous unemployment. 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 jobfinding rates are important for understanding the persistence of high unemployment.

Kroft, Lange, Notowidigdo and Katz (2016) overlaps with this paper in certain respects. It measures jobfinding 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 jobfinding 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.

In addition to Krueger et al. (2014), Cajner and Ratner (2014) study jobfinding among the long-term unemployed over spans of more than a year. Abraham, Haltiwanger, Sandusky and Spletzer (2016) show that the decline in jobfinding rates with increasing duration of unemployment remains after conditioning on earlier administrative data on earnings. They interpret this finding as pointing the the direction of a true duration effect and away from heterogeneity in jobfinding rates.

Carrillo-Tudela, Hobijn, Perkowski 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 jobseekers 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 (2016) 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. Sedlaçek:ME documents procyclical matching efficiency in a standard model where all jobseekers are unemployed and attributes movements in efficiency to large changes in hiring standards.

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 (2014) discusses their paper at greater length.

Davis et al. (2013) provide 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 higher in firms that are growing than in firms with constant employment, so forces beyond unemployment and vacancies influence hiring rates.Their results fit nicely with ours, in the sense that one source of the variations in matching efficiency that we measure is variations in recruiting intensity. Appendix D1 shows the effect of using their adjusted vacancy duration in place of the measure from JOLTS. The effect is small, but does suggest that the adjustment for recruiting intensity accounts for a modest part of the downward trend we find in matching efficiency.

Gavazza et al. (2016) study vacancy yields in a framework related to that of Davis and co-authors. Appendix D.2 shows that their specification establishes a constant-elastic relation between vacancy duration and recruiting intensity, so some part of the overall elasticities we find for each category of unemployment can be attributed to variations in recruiting intensity that are mediated by the same variable as in our setup, T, with constant elasticity. Thus no additional estimation needs to be performed—one can simply subtract the estimates of the elasticity of intensity of Gavazza and co-authors from our elasticity estimates to find the part of the variation in jobfinding rates attributable to market tightness through the matching function, separated from the effect of variations in recruiting intensity.

8 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 generally 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 jobseekers, with a separate measure of matching efficiency for each category and a related measure of weighted average 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 jobfinding 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 jobseekers 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 jobseekers 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 jobfinding among groups not counted as unemployed.

Many discussions of the matching process in the labor market are organized around the Beveridge curve, which portrays movements of unemployment and job vacancies. Shifts in matching efficiency are one source of instability in the Beveridge curve. Changes in the inflow rate to unemployment are another. The large changes in the composition of unemployment over the business cycle are major sources of shifts. This paper focuses only on matching efficiency and not on other shifters of the curve, so we do not try to express our findings in terms of the Beveridge curve. Our finding of stability of matching efficiency at the level of different types of jobseekers is consistent with large shifts in the curve arising from those other sources. Because the Beveridge curve concerns unemployment and not the other important sources of jobfinding, the Beveridge-curve framework does not provide a comprehensive view of flows into employment.

We find that aggregate tightness is influential for jobfinding rates of all types of jobseekers. Hall (2017a) shows that tightness is highly correlated across industries in the JOLTS data. Thus we believe that a framework based on a single measure of tightness, though parsimonious, is a good starting point for understanding fluctuations in jobfinding rates. Barnichon, Elsby, Hobijn and Sahin (2012) discuss industry heterogeneity in a Beveridge-curve framework.

Appendix C discusses three alternative specifications in terms of their implications for the indexes of overall matching efficiency. The first is the same as our baseline specification except that no demographic effects are swept out. The second is similar to the base except that the elasticity of the jobfinding rate with respect to tightness is constrained to be the same for every initial status group. The third is the same as the base specification, but uses only data for 2001 through 2007, the years prior to the crisis. Our basic conclusion holds in all three alternatives that proper accounting for heterogeneity among jobseekers results in an index of matching efficiency that follows a smooth trend with no special movement in the years after the crisis in 2008.

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Appendixes for Online Publication

A Relation between the Standard DMP Matching Setup and the one in this Paper

A.1 The standard DMP setup

Start from the matching function:

$$H = \mu P^{\nu} V^{1-\nu}. \tag{18}$$

Tightness:

$$\theta = \frac{V}{P}. \tag{19}$$

Job-finding rate:

$$f = \frac{H}{P} = \mu \theta^{1-\nu}. \tag{20}$$

Job-filling rate:

$$q = \frac{H}{V} = \mu \theta^{-\nu}. \tag{21}$$

Zero-profit condition:

$$\frac{\kappa}{q} = J. \tag{22}$$

Vacancies:

$$V = \theta P. \tag{23}$$

A.2 The paper's setup with one type of jobseeker

Tightness (vacancy duration):

$$T = \frac{V}{H}. \tag{24}$$

Start from the job-finding rate:

$$f = \gamma T^{\eta}. \tag{25}$$

Matching function:

$$H = \gamma T^{\eta} P; H = \gamma^{1/(1+\eta)} V^{\eta/(1+\eta)} P^{1/(1+\eta)}. \tag{26}$$

Job-filling rate:

$$\frac{H}{V} = \frac{1}{T}. \tag{27}$$

Zero-profit condition:

$$\kappa T = J \tag{28}$$

Vacancies:

$$V = T \cdot H. \tag{29}$$

A.3 The paper's setup with multiple types of jobseekers

Tightness (vacancy duration):

$$T = \frac{V}{H}. \tag{30}$$

Start from the job-finding rates:

$$f_i = \gamma_i T^{\eta_i}. \tag{31}$$

Matching function satisfies:

$$H = \sum_i P_i \gamma_i \left(\frac{V}{H}\right)^{\eta_i}. \tag{32}$$

Job-filling rate:

$$\frac{H}{V} = \frac{1}{T}. \tag{33}$$

Zero-profit condition:

$$\kappa T = J. \tag{34}$$

Vacancies:

$$V = T \cdot H. \tag{35}$$

The property that the zero-profit condition involves only T is important for our approach to identification.

B Attrition in the CPS

Table 7 describes our success in matching respondents in different months in the CPS. It shows the weighted percent of observations that were successfully matched to an observation on the same person some month later, conditional on the initial observation being early enough in the CPS sample rotation that a match was theoretically possible. (For example, a match one month later is theoretically possible if the initial observation is not in the outgoing rotation group; a match 15 months later is theoretically possible only if the initial observation is in the incoming rotation group.) The intervals correspond to the spans that we use for estimation. The short-span match rates are quite high; the long-span match rates less so. We calculated the success rates by year. The bottom line of the upper panel shows the standard deviations of the rates across years. They are uniformly small; the success rates were stable over the period from 2001 through 2013.

The table also shows the matching rates that we would obtain if we used the method of Madrian and Lefgren (2000). That method produces a slightly higher match rate than Nekarda's method at a horizon of 1 month because Madrian and Lefgren 1-month match does not condition on what happens in subsequent months, while Nekarda's 1-month match does. However, the Madrian and Lefgren match rates are lower than the Nekarda match at all horizons longer than 1 month, because at longer horizons Nekarda's method allows some matches that Madrian and Lefgren's method rejects.

Number of months separating observations	1	2	3	12	13	14	15
Percent matched, Nekarda method	93.6	91.3	89.3	75.3	74.5	73.5	72.5
Standard deviation across years	0.2	0.3	0.3	1.9	1.9	2.0	2.1
Percent matched, Lefgren-Madrian method	94.7	90.3	86.3	68.8	66.4	64.2	62.1
Standard deviation across years	0.2	0.3	0.5	1.9	1.9	1.9	1.9

Table 7: Percent of Observations Matched between Months in the Current Population Survey

	Short spans				*Long spans*			
Year	*Base*	*No demo-graphics*	*Common elasticity*	*Pre-crisis*	*Base*	*No demo-graphics*	*Common elasticity*	*Pre-crisis*
2001	1.000	1.000	0.040	1.000	1.000	1.000	1.000	1.000
2002	1.033	1.035	1.076	0.923	1.028	1.034	1.029	0.972
2003	1.030	1.032	1.083	0.875	1.068	1.089	1.068	0.989
2008	1.005	1.005	1.022		1.020	1.015	1.020	
2009	1.039	1.037	1.133		1.044	1.046	1.043	
2010	1.026	1.029	1.086		1.029	1.051	1.029	
Standard deviation	0.024	0.024	0.040	0.029	0.023	0.027	0.023	0.020

Note: The standard deviation includes the omitted years 2004-2007 and 2011-2013

Table 8: Detrended Indexes of Matching Efficiency for Alternative Specifications

C Estimates for Alternative Specifications

Table 8 shows our basic results for three alternative specifications. The left panel shows the detrended index of matching efficiency measured over short spans and the right panel the index for long spans, excluding job-to-job, as in Figure 7. It includes the years 2001 through 2003, years affected by the 2001 tech crash, and 2008, years affected by the financial crisis. The left column in each panel repeats the index from the body of this paper. The next column is similar in all respects except that no demographic effects are swept out. The third column is similar to the base except that the elasticity of the jobfinding rate with respect to tightness is constrained to be the same for every initial status group. This corresponds to the assumption that the matching function is Cobb-Douglas in a weighted sum of jobseekers. The right-most column is based on estimates of the base specification, but uses only data for 2001 through 2007, the years prior to the crisis.

In all cases, the results conform to the overall conclusion of the paper, that a fixed-weight index shows that matching efficiency departed from its trend only slightly. The standard

	Short spans, with trend			Long spans, with trend		
Year	*Base*	*With time aggre-gation*	*With time aggregation and intensity*	*Base*	*With time aggre-gation*	*With time aggregation and intensity*
2001	1.000	1.000	1.000	1.000	1.000	1.000
2002	1.006	1.011	1.004	1.000	1.004	0.999
2003	0.978	0.984	0.980	1.012	1.020	1.007
2008	0.833	0.835	0.869	0.843	0.845	0.893
2009	0.833	0.837	0.869	0.833	0.842	0.882
2010	0.796	0.804	0.832	0.795	0.803	0.849
2013	0.743	0.751	0.786	0.743	0.753	0.807

Table 9: Indexes of Matching Efficiency, with Trends, Including Adjustments from Davis and Co-Authors

deviations of the alternative indexes of matching efficiency, shown at the foot of the table, are greater than the preferred base specification, shown at the left of each panel, but are still quite small. By contrast, the standard deviation of the detrended version of the matching efficiency index in Figure 8, based on a single type of unemployment, is vastly higher, at 0.155.

D Recruiting Intensity

D.1 Davis-Faberman-Haltiwanger's Estimates of Vacancy Duration and Recruiting Intensity

Davis et al. (2013) derive an adjustment to the JOLTS measure of vacancy duration to account for time aggregation, and a second adjustment for recruiting intensity. Table 9 shows, in a format similar to Table 8, our measure of matching efficiency with trend, using T with the time-aggregation adjustment alone, and T with the product of the time-aggregation adjustment and the recruiting-efficiency adjustment. Adding the time-aggregation adjustment by itself has almost no effect on the index of matching efficiency. The adjustment for recruiting intensity eliminates some of the downward trend in both indexes, but does not affect the overall conclusion of the paper.

D.2 Implications of the Findings about Recruiting Intensity of Gavazza and Co-Authors

Gavazza et al. (2016) conclude that, to a fair approximation, recruiting intensity satisfies their equation (29):

$$\log \Phi = -\alpha \frac{\gamma_2}{\gamma_1 + \gamma_2} \log q = \pi \log T, \tag{36}$$

and the value of the elasticity is $\pi = 0.40$, from the values of the parameters reported in their Table 2. If the matching function is written with endogenous recruiting intensity that enters the matching function in vacancy-augmenting form, as in their equation (2), it is

$$H = \sum_i \phi_i \left(\frac{\Phi V}{H} \right) P_i = \sum_i \gamma_i T^{\pi} T^{\tilde{\eta}_i}. \tag{37}$$

Thus we can partition our estimated elasticities η_i into lower values $\tilde{\eta}_i = \eta_i - \pi$ that isolate the effect from tightness itself and the common effect of endogenous recruiting intensity, π. The addition of endogenous recruiting effort requires no modification in our estimation. It only contributes this decomposition of our findings.

E Mismatch Effects in the Duration of Vacancies, T

Our estimation equation (12) involves a concave log transform of T, so there is an issue of using the aggregate value when there is dispersion across units—T is potentially subject to mismatch bias. To understand this issue, we studied the industry-level data on hires and job openings published for JOLTS, across 4 geographic units and 25 industries. We hypothesize that the ratio of openings to hires—the vacancy duration T—has three components: (1) an aggregate tightness measure, as used in our work, (2) a unit-specific component, reflecting the deviation of tightness in the unit from the aggregate measure, and (3) measurement error, occurring because JOLTS is a fairly small survey and from other random sources unrelated to tightness.

We believe that that the unit-specific component is moderately persistent, mainly because both aggregate vacancy duration and its counterpart in the geographic and industry units are persistent. On the other hand, the measurement errors are likely to be transitory. We begin our investigation by studying the autocovariance functions of the disaggregated data stated as deviations from the aggregate series for T_t. If the measurement errors were white noise and the unit-specific tightness process quite persistent, the functions would spike at zero—the only lag value where the measurement error would contribute—then drop immediately to a gradually declining value starting at a log of one month. In fact, the autocovariance functions resemble those of a fairly non-persistent autoregressive process, with no special spike at zero lag. There is no highly persistent component.

To capture this finding more rigorously, we estimate the following equation:

$$\log T_{i,t} = \lambda_i + \bar{T}_t + \rho_\ell \log T_{i,t-\ell} + \iota_{i,t}. \tag{38}$$

Here λ_i is a level effect for industry i, $\bar{T}_t$ is a time effect, ρ_ℓ measures the predictive power of the observation ℓ months earlier, and $\iota_{i,t}$ is the residual. The coefficient ρ_ℓ declines with the length of the lag, ℓ—it is analogous to the autocovariance in this panel setting.

Table 10 shows estimates of the prediction coefficient.

Lag, months	12	18	24
Coefficient	0.226	0.087	0.036
Standard error	(0.015)	(0.015)	(0.015)

Table 10: Estimates for the Forecasting Power of Lagged Vacancy Duration at Selected Lags