Recall and Unemployment[*]

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Abstract

Using data from the Survey of Income and Program Participation (SIPP) covering $1990-2011$, we document that recalls of former employees are surprisingly common and associated with dramatically different unemployment and post-unemployment outcomes, compared to those who change employer after a jobless spell. Over 40% of all workers separating into unemployment return to their previous employer after a jobless spell. One quarter of them are permanently separated workers who, unlike temporary layoffs, did not have an expectation of recall. Recalls are associated with much shorter unemployment duration and smaller wage changes after the jobless spell. Negative duration dependence of unemployment disappears once recalls are excluded: Those who change their employer after a jobless spell leave unemployment at lower but roughly constant hazard. We also show that the probability of finding a new job is much more procyclical than the probability of being recalled. Taking this fact into account significantly alters the estimated elasticity of the matching function with respect to job market tightness and the time-series behavior of matching efficiency. In particular, labor market mismatch in $2008-2010$ is considerably larger than the conventional measure indicates. To make sense of the empirical evidence, we develop a search-and-matching model in which the separation decision is accompanied by a recall option. While new matches require costly search and are mediated by a matching function, recalls are free and triggered both by aggregate shocks and job-specific shocks that continue to evolve even after separation. The recall option is lost when the unemployed worker accepts a new job. A quantitative version of the model captures well our cross-sectional and cyclical facts through selection of recalled matches.

1 Introduction

Unemployment is a state of job search, and is measured accordingly. Due to informational imperfections, jobless individuals do not gain immediately the kind of employment that they desire and that the market offers somewhere. A common interpretation of these search frictions is that jobs and workers are extremely heterogeneous. Hence, it takes time for an unemployed worker to locate and to arrange a suitable job. The relevant dimensions of job heterogeneity include pay, hours, location, task, work environment, and very many others. If, however, an unemployed worker who previously lost a job later returns to his old employer, then much of this heterogeneity may be irrelevant, since worker and firm already know what to expect from one another.

Using data from the Survey of Income and Program Participation (SIPP), we document that recalls of former employees are surprisingly common: Over 40% of workers separating into unemployment (EU flow) return to their previous employer after a jobless spell. This fraction significantly exceeds that of the flow into unemployment due to Temporary Layoffs (from now on: TL). In other words, recalls are much more pervasive than TL. The reason is that, even within the group of "Permanently Separated" (PS) workers (i.e., workers who lose their job with no indication of a recall date or chance and thus start looking for another job), about 20% are eventually recalled by their last employer. The recall rate is even higher at 50% if we restrict the sample to the workers who stay unemployed until finding a job (without dropping out of the labor force). It is still substantial at 25%−30% even for all separated workers including those who immediately leave the labor force such as retirees.

We show that recall matters for labor market outcomes: Recalled workers were on average with their previous employer for twice as long (6 years vs. 3 years), experience shorter unemployment duration (by over a month), and switch occupation much less often (3% for recalled workers vs. over 50% for job switchers). Among PS workers, especially at long unemployment duration, a recall generates a dramatically more favorable real wage outcome. Among TL workers, the few who did not return to their previous employer tend to experience significant wage gains, presumably because the option value of recall raises their reservation wage to accept a new job offer. We also find that negative unemployment duration dependence exists only for those who are eventually recalled. In other words, the hazard rate of exit from unemployment to a new employer is essentially constant with unemployment duration.

We also investigate the cyclical implications of recalls. First, we find that the share of recalls out of all hires is countercyclical and increased especially sharply during the Great Recession. In the two recessions in the sample period, the probability of finding a new employer drops much more than the recall probability. This finding bears important implications for our understanding of the labor-market matching process. Specifically, the matching function approach in modeling labor market frictions relies on the presumption that all hires result from a costly search process. However, if recalls circumvent the search friction, hires through recalls cannot be treated as the output from the matching function. To quantify the effects of

the large recall rate and its cyclical fluctuations, we estimate the matching function assuming that recalls are not mediated by the matching function. Specifically, we exclude recalls from hires (the dependent variable) and subtract TL from unemployment (one of the covariates). We find that this modification leads to significant changes in the elasticity estimate and the time-series behavior of the regression residual, which is commonly interpreted as a measure of matching efficiency. Most strikingly, compared with the traditional estimation result, our corrected measure of matching efficiency suggests that labor market "mismatch" was considerably larger overall during the Great Recession, but did not continue deteriorating as much during the subsequent recovery period.

To better understand the economic mechanisms behind the empirical evidence, we introduce a recall option in the standard search-and-matching model of the labor market à la Mortensen and Pissarides (1994). We assume that each job is hit by idiosyncratic and aggregate productivity shocks. Job separation is endogenous following both kinds of shocks. After separation, the productivity of the match may keep evolving. As long as the previous employee is still unemployed and available, he can agree with his previous employer to re-match, due to intervening changes in the aggregate state and idiosyncratic match quality. Recall is free and instantaneous for both parties. New firms, and pre-existing firms who either cannot or do not want to recall the previous employee, must pay a cost to post a vacancy and to search for a new worker.

In our model, after an endogenous separation, the worker is not concerned about being replaced in his old job by a new hire, because we assume that the firm can always create new positions to hire new workers. Conversely, the firm must keep track of the worker availability for a recall, because a worker can only work for one employer at each point in time. We also assume that accepting a new job voids the recall option of the former employer. The unemployed worker, while waiting for a possible recall, may be searching for another job. When he finds one and accepts it, it is as if the quality of his (re-)match with his previous employer dropped to a permanently low level. The chance of this kind of match quality shock is endogenous to the economy, because it is the chance that the unemployed worker finds and accepts another job while waiting for a recall. Thus, as in the standard stochastic search and matching model, the job finding probability is the key equilibrium object in our model and tracking its evolution poses no additional complications. Essentially, the recall option makes match quality evolve also during unemployment, according to a transition law that depends on job market tightness.

In this environment, firms that post new vacancies meet searching workers who, in many cases, still holding a recall option. Therefore, if the unemployed worker's outside option (when receiving a new offer) varies with the chance of recall by the former employer, under Nash Bargaining, a new vacancy will earn different profits depending on which worker it meets. If most unemployed workers have a good recall option, their bargaining stance is stronger and the incentives to create new jobs are reduced. This means that the distribution of unemployed worker by quality of the match with their former employers is an endogenous and infinitely-dimensional state variable that pins down entry and equilibrium. To avoid this

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complication, we assume that an unemployed worker must give up any recall option with a former employer before bargaining a wage and acquiring the recall option with a new firm. Therefore, all new hires from unemployment receive the same wage and generate the same profits for the firm, although they receive different gains from the new job, depending on the value of the recall option they have to give up. The decision of accepting a new job then becomes independent of their recall option value, because acceptable new jobs yield positive rents when activated, while mothballed old jobs would still yield negative rents. Otherwise, they would have been already recalled.

We calibrate the model in steady state and explore quantitatively its cyclical properties. We estimate by a simulated method of moments the parameters of the idiosyncratic shock process, which is the engine of turnover and recalls through dynamic selection. We find that this exploratory analysis matches qualitatively all the cross-sectional and aggregate/cyclical facts that we highlight and performs well even quantitatively.

The rest of the paper is organized as follows. In Section 2, we place our contribution in the context of the relevant literature. In Section 3. we describe the new facts on recall of separated workers by their former employers. In Section 4, we examine the cyclical implications of recalls, by focusing on the effects on the matching function estimation. In Section 5, we lay out and analyze a search-and-matching model with recall. Section 6 provides quantitative evaluations of the model.

2   Related Literature

Several authors already documented that recall of newly separated workers is surprisingly frequent and fast, and explored the implications for unemployment duration dependence. The literature on recall is entirely microeconomic in focus, thus relies on detailed samples that are limited in scope and/or time. To the best of our knowledge, we are the first to study recall in a large, nationally representative survey covering a significant time span, which allows to make a connection to the broader macroeconomic debate on cyclical unemployment.

Katz (1986) was the first to notice, in 1981-1983 PSID data, that observed negative duration dependence in unemployment is the result of a strongly declining hazard rate of exit to recall, masking a positive to nil duration dependence in the exit to new jobs. Katz and Meyer (1990) take advantage of a supplemental survey of new UI recipients from Missouri and Pennsylvania in 1979-1981. The vast majority said that they expected to be recalled, although a much smaller share had an explicit date of expected recall (thus, according to the Census definition, were on TL). Ex post, a sizable share were actually recalled. Katz and Meyer (1990) exploit these reported expectations in a competing hazard model, to quantify their effect on the incentives to search for new jobs. They also find that pre-displacement tenure predicts recall, which in turn predicts more favorable wage outcomes. Fallick and Ryu (2007) use the same data as Katz (1986), and replicate Katz and Meyer (1990)'s competing hazard exercise without information on subjective recall expectations but controlling for unobserved heterogeneity.

Our sample is based on SIPP, which covers 20 years and the US labor force, not only UI recipients and/or a deep recession. We do not have a measure of ex ante recall expectations without a definite date (i.e., beyond TL status), but we still find that a significant fraction of PS workers, who say that the previous employer did not indicate a possible recall data, ended up being recalled nonetheless. In comparison to this microeconomic literature, we confirm and extend the empirical relationship of recall with tenure, exit from unemployment, and wages, but we also show that the strongest relationship is with occupational mobility.

Recall played an even smaller role in the macroeconomics literature on unemployment. Bils et al. (2011) extend the canonical search-and-matching model to allow for heterogeneity in the reservation wage (value of leisure) across workers and study the amplification of aggregate shocks. When calibrating the separation rate, they use SIPP and only count permanent separations that do not result in a recall within four months, and target an average unemployment rate of 6%, which presumably (they do not say) exclude the contributions to unemployment of those workers who are eventually recalled within four months. We investigate whether the recall option affects the incentives for existing and new firms to post new vacancies and engage in costly search, that is, whether recall and search interact with each other, in which case that their calibration strategy is potentially problematic. In our view, while firms can always post more vacancies, and do not face a trade-off between recall and search, in the aggregate the recall option does make more or fewer workers willing to search for new jobs, hence affects the stock of workers available for new hires and their outside option. In addition, we show that Bils et al. (2011)'s choice of a four month unemployment duration cutoff to define a recall, probably due to data issues in SIPP that we discuss in detail, leads to significantly underestimate true recalls.

Fernandez-Blanco (2011) studies a similar model to ours, but only in steady state, and assumes commitment to contracts by firms. He analyzes the trade off between providing workers with insurance (flat wage path) and incentives not to search while unemployed, waiting for a recall. In contrast, we introduce aggregate shocks and assume Nash Bargaining to stay close to the canonical business cycle model of a frictional labor market, and we aim to also match our new facts about the unemployment duration dependence, wage and occupational changes following a recall. As Fernandez-Blanco (2011) points out, one can interpret unemployment without active job search by workers who have a strong expectation of recall as "rest unemployment" in the language of Alvarez and Shimer (2011). Fujita (2003) extends the Mortensen and Pissarides (1994) model by introducing a fixed entry cost. The job can be mothballed in his model, as in our model. However, his model does not allow for a recall of the same worker and the paper only examines the model's cyclical implications on aggregate variables such as job flows, unemployment, and vacancies.

Shimer (2012) examines the "heterogeneity hypothesis" to explain the strong cyclical volatility of the average monthly job finding probability of unemployed workers. That is, he asks whether these cyclical movements are the result of composition effects in the unemployment pool, or rather all types of unemployed workers experience cyclical job finding opportunities. He finds that the best case for this hypothesis can be made when breaking

down the unemployed between TL and PS, as their proportions are slightly cyclical and their relative job finding chances are very different; but he still finds that this channel explains a small fraction of cyclical movement in the average job finding probability. The dimension of heterogeneity we consider is based on the type of exit (recall vs. different employer) as opposed entry (TL vs. PS). We argue that this heterogeneity is important quantitatively with respect to the matching function estimation.

Shimer (2012) leaves open the possibility that composition effects in terms of unobservable worker characteristics may be important. In order to investigate this question directly, one needs high-frequency longitudinal data with multiple unemployment spells, so that one can extract fixed-effects. Moreover, the sample period needs to be long enough to cover at least several business cycles. The CPS has too short a panel dimension to cover multiple spells, and SIPP also has too short a time dimension to cover more than three business cycles. Hornstein (2012) tackles this question indirectly. He formulates a statistical model of unemployment duration dependence due to either unobserved heterogeneity of individual job finding rates and the resulting selection or pure duration dependence such as skill loss or discouragement. He concludes that unobserved heterogeneity explains almost all of the negative duration dependence and that the cyclicality of the job finding rates of the long-term unemployed "types" is the main cause of overall unemployment volatility. In our data and setting, the long-term unemployed are mostly those workers who are not recalled ex post: they take longer to find a job, suffering larger wage losses. We further document that these non-recalled workers were on average shorter-tenured before separation and that their job finding hazard exhibits no duration dependence. Thus our paper provides a direct content to the traits that are "unobserved" in Hornstein's approach.

3   Evidence on Temporary Layoffs and Recalls

In this section, we present our empirical results from two nationally representative surveys: the SIPP and the monthly CPS. We document how frequently a worker who lose his job and enters unemployment either expects to be recalled by his last employer, or eventually returns to his last employer, whether he expects it or not. That is, we examine the importance of recalls for both the inflow into and the outflow from unemployment. We then discuss the economic implications of recalls.

While our new results are from SIPP, we first revisit some evidence from the CPS to present what is known and possible to learn there, and why SIPP affords significant progress in studying recall. Unlike the SIPP, the CPS does not ask questions that allow us to identify employers across non-employment spells, hence recalls. The CPS provides only information on workers on Temporary Layoff (TL). Since the CPS is the standard source of labor market information including the official unemployment rate, and TL are also measured in SIPP, it is useful to compare observations on TL in the two surveys, and then focus on recalls in SIPP.

Attention paid by labor market researchers to recalls waned due to the observed decline

Figure 1: Unemployment Stocks by Reason
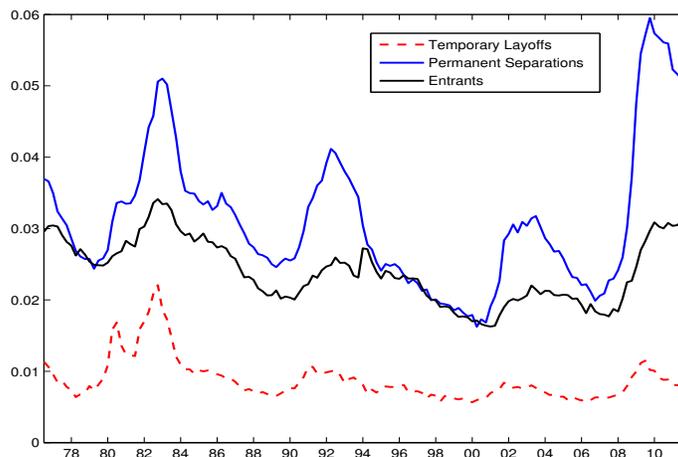
Notes: Source, Monthly CPS. Expressed as a fraction to the total labor force.

in the level and cyclicality of TL, which tracked the decline in the relative importance of the manufacturing sector, where TL were common. We present empirical evidence that should lead us to rethink this assessment for two reasons that we now summarize and further elaborate upon in the next two subsections.

First, the decreasing incidence of TL is observed in the stock of unemployment in the CPS. But TL are still a much larger fraction of the flows in and out of unemployment than of the stock. The reason for the stock-flow discrepancy is that TL spend much less time unemployed than average. So, if one is interested in worker flows, TL still matter.

Second, and more importantly, TL are only part of the story. We document that, in SIPP, PS workers, who have no clear expectation of recall, nonetheless return to their former employer with surprisingly high frequency, which has not declined over the last two decades. Although this frequency of PS recall is still much lower than that of TL recall, a large share of recalls originate from the stock of PS, who did not expect a recall. Therefore, focusing on TL alone, whether in stocks or flows, paints an incomplete picture. When we measure all recalls, their importance and implications for the matching process and the cost of unemployment change significantly.

3.1 Facts from the CPS

For our purposes, the main source of the information in the CPS is unemployment by reason, combined with worker transition data. The CPS transition data do not allow us to identify recalls. In the CPS, there are six reasons for unemployment: (i) on temporary layoff, (ii) permanent job losers, (iii) persons completed temporary jobs, (iv) job leavers, (v) reentrants, and (vi) new entrants. We reclassify these six groups into three groups. We treat the group (i)

Figure 2: Transition Rates Between Employment and Unemployment by Reason: Matched Records
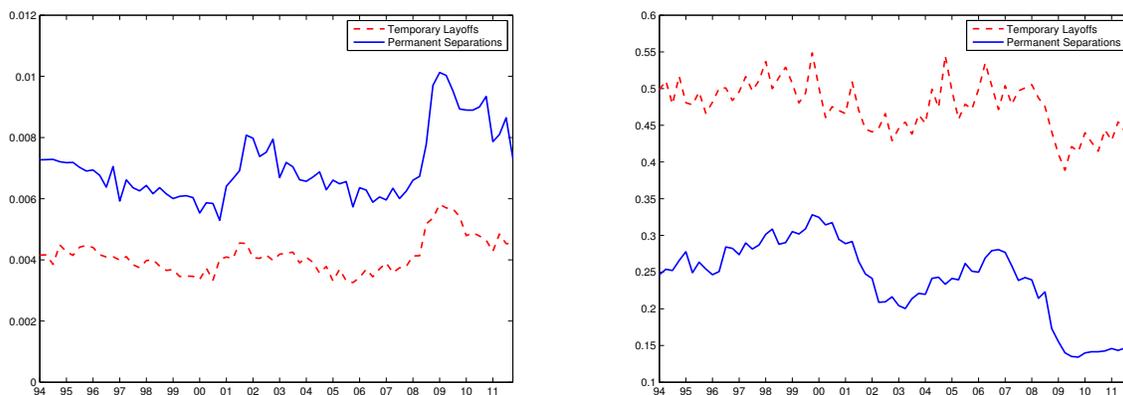
(a) EU Transition Rate

(b) UETransition Rate

Notes: Source, Monthly CPS. Based on matched records and expressed as quarterly averages of the monthly rates.

on its own. Groups (ii) through (iv) are lumped together and called "permanent separations" (PS). The last two groups (v) and (vi) are treated as one group and called "entrants."

### 3.1.1 Unemployment Stocks by Reason

It is often argued that the role of TL has diminished since the mid 1980s (e.g., Groshen and Potter (2003)). Figure 1 plots unemployment stocks by reason. Each stock is expressed as a fraction to the labor force and thus the sum of these three lines equals the official unemployment rate. One can see that unemployment due to TL is relatively small in the unemployment stock especially after the mid 1980s, and the increase in TL during the last three recessions has been modest.

3.1.2 Transition Rates

The small share of TL in the unemployment stock does not necessarily mean that TL is equally unimportant in hiring and separation flows. In fact, this small share is due to the fact that TL quickly exit from the unemployment pool.

Figure 2 plots the transition rates between employment and unemployment derived from the matched records. Panel (a) breaks down employment-to-unemployment (EU) transition rates into TL and PS. Note that each line is calculated by dividing EU flow for each reason by the total employment stock. This figure thus tells the relative size of the two flows. Observe that the separation flow associated with TL amounts to roughly one half of the flow associated with PS. In terms of the cyclicality, the separation rate for TL moves more or less in parallel with that of PS. Panel (b) presents unemployment-to-employment transition (job

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Figure 3: Unemployment Entry and Exit by Reason: Duration Data
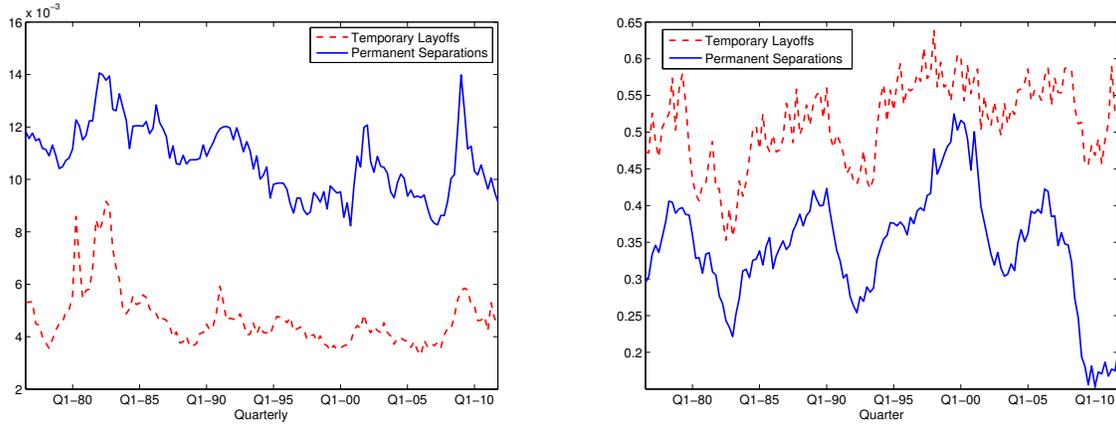
(a) Short-Term Unemployment

(b) Exit Probability

Notes: Source, Monthly CPS. Short-term unemployment: unemployed less than 5 weeks. Short-term unemployment is expressed as the fraction to the total employment stock.

Figure 4: Median Duration by Reason
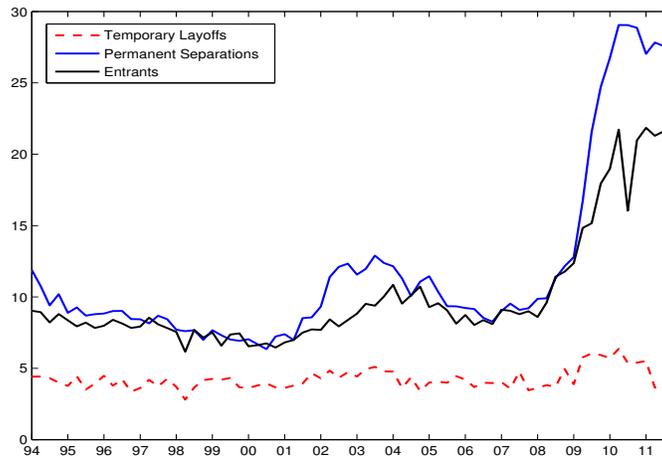
Notes: Source, Monthly CPS.

finding) rates by reason. Workers on TL face a dramatically higher job finding rate, compared to PS workers. Note also that although both series exhibit the familiar procyclicality, the procyclicality is more pronounced for PS. During the post-GR recovery, the exit probability recovered for TL but not for PS.

Similar conclusions obtain from using the short-term unemployment data instead of the matched records (Figure 3). Panel (a) shows short-term unemployment (unemployed less

Figure 5: Industry Breakdown of Temporary Layoffs
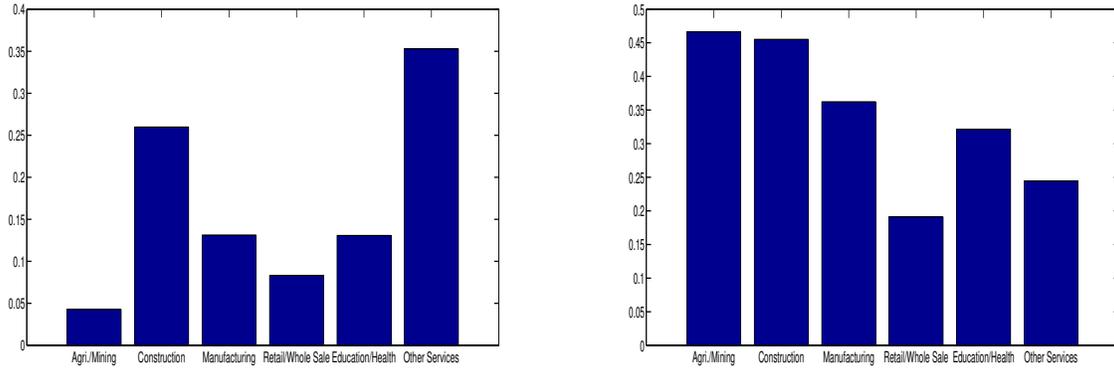
(a) Industry Shares of TL Separation Flow   (b) Share of TL Separation Flow in Each Industry

Notes: Source, monthly CPS. Panel (a) presents the shares of each industry in the total TL separation flow. Panel (b) presents the share of TL separations out of all EU separations within each industry. Average shares between January 2003 and December 2011. Other services include Transportation and Utilities; Information; Financial Activities; Professional and Business Services; Leisure and Hospitality; Other Services; Public Administration; Armed Forces.

than 5 weeks) for TL and PS.[1] The exit probability from each of the unemployment pool, presented in Panel (b), is inferred by using short-term unemployment and each type of stock, as in Shimer (2012). Note that the exit probability does not specify the destination of the workers, as opposed to the data plotted in the previous figure. Nevertheless, Figures 2 and 3 give similar results in terms of relative size of TL and PS flows and their cyclicality.

Lastly, Figure 4 presents median duration, broken down by the reasons. We can confirm here that median duration of those on TL is much shorter on average and less cyclical.

3.1.3 Industry Composition and Seasonality of Temporary Layoff

It is important to note that TL are not concentrated in a particular sector (i.e., manufacturing). Panel (a) of Figure 5 presents the industry breakdown of the TL separation flow. While the contributions of the construction and manufacturing sectors are large as expected, TL are also observed in other sectors as well. Note that this figure gives the breakdown of all TL separation flows by industry, and thus does not take into account the relative size of each industry. For example, the highest share of "Other Services" does not mean that this sector is the most frequent user of TL. To see how common TL are within each industry, Panel (b) displays the share of the TL separation flow out of all EU separations within each industry. As expected, in agriculture/mining, construction, and manufacturing, TL are used very frequently. More importantly, though, the shares of the TL flow in remaining indus-

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[1]Due to the redesign of the CPS in 1994, the raw data exhibit a break in these series at the start of 1994. We adjust the break, following the adjustment procedure proposed by Elsby et al. (2009).

Figure 6: Seasonality of Temporary Layoffs by Industries
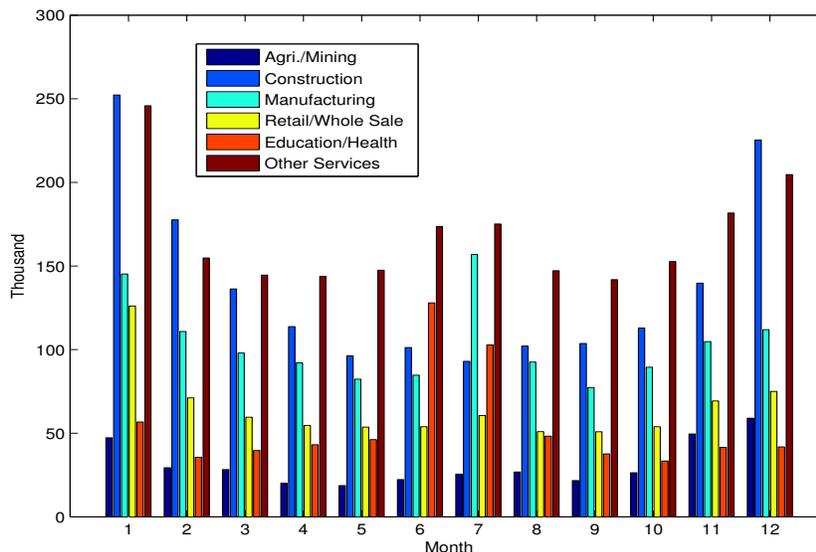
Notes: Source, monthly CPS. Average shares between January 2003 and December 2011.

tries are substantial.[2] Figure 6 summarizes the seasonal pattern of TL. All industries except education/health share the pattern that the TL flow increases in winter months. In addition, some sectors (manufacturing and other services) shed more workers also during summer months. In the education/health sector, TL are concentrated in June. Overall, this figure suggests the presence of significant seasonal variations in the TL flow. However, Figures 2 and 3, which plot seasonally-adjusted data, demonstrate that there are also non-seasonal, business cycle variations in separation and job finding rates associated with TL.

3.2 Facts from SIPP

We now discuss our main empirical facts from SIPP (Survey of Income and Program Participation). Again the biggest advantage of SIPP over the CPS is that we can see if a worker returns to the same employer or not.

3.2.1 Sample Selection and Identification of Recalls

SIPP is a collection of panels. The following eight panels are used in the analysis: 1990, 1991, 1992, 1993, 1996, 2001, 2004, and 2008 panels. The survey was redesigned in 1996, in a manner that introduced significant changes for our purposes. We thus sometimes distinguish

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[2]Remember that at the aggregate level, the share of the TL flow out of all EU flow is roughly 30%, as suggested by Panel (a) of Figure 2 and this average share is consistent with the shares presented in Panel (b).

Table 1: Coverage of SIPP Panels

Panel	Number of Waves	Number of Months Covered	First Reference Month
1990	8	32	Oct. 1989
1991	8	32	Oct. 1990
1992	9	36	Oct. 1991
1993	9	36	Oct. 1992
1996	12	48	Dec. 1995
2001	9	36	Oct. 2000
2004	12	48	Oct. 2003
2008	13	52	May 2008

Notes: Each wave (interview) covers a four-month period. The 2008 panel is still ongoing and we use the data up to wave 10. The results for the 2008 panel use the data up to wave 10.

between the first four panels and the last four panels. The length of each panel is roughly either three or four years. The first four panels have some overlapping survey periods. Each interview in a panel covers the preceding four-month period and is called a wave. Table 1 summarizes each panel's length and the period covered.[3]

We drop individuals who miss any wave of the panel. In other words, individuals in our sample have complete, three-year or four-year, history. The Census Bureau provides population weights, called panel weights, specifically calculated for the balanced-panel data, making this sample nationally representative. After applying these sample selection criteria, we identify spells that start with employment followed by non-employment and then again by employment (called an $E\not{E}E$ spell).

It is important to make sure that right censoring of the panel does not affect our results. Thus, we further restrict our sample to those cases in which the transition into non-employment (separation) in the $E\not{E}E$ spell occurs in the first year (in the case of three-year panels) or the first two years (in the case of four-year panels) of each panel, which ensures that each subsequent non-employment spell could last at least two years and still be measured by the survey. An alternative way of dealing with the censoring problem is to focus on hires that occur in the last year or last two years of each panel. This procedure also ensures that non-employment spells could last at least two years. We further checked the robustness of our results with respect to the different window size, i.e., including more separations (hires) that occur later (earlier) in the panel. Those results are similar and available upon request.

We define labor market status (employed, unemployed, and not-in-the labor force) in SIPP in a manner similar to the CPS: we classify separated and unemployed workers into two groups, on TL and PS, as we did in the CPS. Unfortunately, the classification of the

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[3]The 2008 panel is still ongoing and we use the data up to wave 10 in the current draft of the paper.

labor market status prior to the 1996 SIPP redesign is not consistent with the CPS, and therefore, we focus on the post 1996 data whenever we condition our analysis on the labor market status. In particular, after the redesign, SIPP applies a more precise definition of TL, raising the number of TL unemployed workers. Prior to the redesign, the share of those who report TL among those who are unemployed throughout the non-employment spell in our EUE sample was too low (roughly 20%). The CPS data suggest that the share should be more like 35%.

SIPP assigns a unique job id to each employer for each worker. Therefore, when a worker returns to the same employer, we can identify this event as a recall. In particular, we have an accurate picture of recalls for the 1990-1993 panels. As discussed in Stinson (2003), job ids in 1990-1993 panels were subject to miscoding. However, the Census Bureau investigated the problem and produced accurate job ids using confidential employer name information and administrative data containing individual-level job counts. The revision of job ids made it possible for us to correctly identify recalls in the earlier panels. We therefore view the aggregate recall rate of all separated workers from the 1990-1993 panels as completely reliable.

The identification of a recall in the 1996-2008 SIPP panels is subject to two important sources of measurement error, both leading to significant underestimates of recall rates. First, we discover a "seam effect" in SIPP for PS workers. Consider all PS workers who stay unemployed and regain employment within one or two months, hence experience either a EUE or a EUUE spell. In some cases, the spell is entirely contained within a wave (4-month interval between interviews), hence is reported at once in the same interview. For others, the initial and final employment in the spell belong to different waves and are reported in different (consecutive) interviews. Whether a spell crosses the "seam" between waves or not should be a completely random event. In the SIPP, however, the recall rate of the workers who experience these short within-wave spells is about 20%, as opposed to only 5% for the identical spells that cross the interview seam. Evidently, reporting labor market history all at once in one interview preserves more accurate information. This suggests that recall rates for all PS jobless spells that cross SIPP waves, including necessarily all jobless spells that last more than two months, are underestimated. As we discuss below, we impute these missing recalls due to the seam effect by using the information in the within-wave spells.

Second, after 1996 SIPP misses recalls *altogether* when a worker returns to the same employer after a long non-employment spell spent looking for a job elsewhere or being out of the labor force. The reason is that SIPP drops the job id if the worker reports being jobless for the entire wave (4-month interval between interviews). One important exception is when a worker is on TL, in which case SIPP keeps track of the last job id and we do not miss a recall even when it happens after a long unemployment spell. In other words, in those panels, the recall rate for those not on TL and recalled after a long non-employment spell are underestimated. In these cases, the seam effect is irrelevant, as the recall rate is set to zero by the survey design. We attempt to recover the missing late recalls of PS workers in the post 1996 panels by means of imputation based on regression analysis, using the observations from the 1990-1993 SIPP panels and part of the observations from the 1996-2008 panels.

Table 2: Recall Rates: Separations Occurred in the First Year or Two Years of Each Panel

Panel	Separations in waves	$E\not\!E$		$E\not\!E \cdots \not\!E E$	
		Recall rates	Counts	Recall rates	Counts
1990	1−3	0.264	4,695	0.371	3,325
1991	1−3	0.303	3,272	0.423	2,310
1992	1−3	0.293	3,975	0.407	2,827
1993	1−3	0.286	3,670	0.398	2,587
1996	1−6	0.246	11,039	0.318	8,350
2001	1−3	0.248	5,276	0.329	3,906
2004	1−6	0.244	5,175	0.329	3,731
2008	1−3	0.286	5,473	0.413	3,724

Notes: Source, SIPP. Third column gives the number of recalls relative to all separations into non-employment, denoted by $\not\!E$ (including unemployment and inactivity). Fifth column gives the number of recalls relative to all the spells that end with employment. The results for the 2008 panel are based on the observations up to wave 10.

The imputation is performed separately for the long spells (three months or more) and the short spells (one or two months) that cross the wave seam. For each of the two groups, we use a "reference sample" to estimate a logit regression that predicts recalls given the observable characteristics such as non-employment duration, switching of occupation, etc. We then impute the missing recalls in the 1996-2008 panels. The reference sample of the long jobless spells, who tend to lose job ids and thus may not be measurably recalled unless they are on TL, is the analogous sample of long-term unemployed in the 1990-1993 panels. Because unemployment status, TL vs. PS in particular, is not reliable before 1996, we do not use it in the estimation. Hence, we impute recalls also to post-1996 spells that are on TL (even though they are measured accurately) to avoid selection by unemployment status, which is obviously non-random and likely correlated with recalls. For the short spells that suffer from the seam effect after 1996, the strongest predictor of recall is occupational mobility. For the occupational stayers, we run the imputation regression on the analogous sample before 1996: all short spells of occupational stayers before 1996 that do not cross the seam. Here we can use unemployment status, TL vs PS, as we only exploit post-1996 data where it is measured correctly. If we observe an occupational switch after a short spell that crosses a wave after 1996, we directly impute a zero recall rate. This conservative choice follows from the observation that, among these short spells, over 99% of the occupational switchers before 1996 and 100% after 1996 (who do not cross the seam) are not recalled. Details of the imputation procedure is described in the Appendix.

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Table 3: Recall Rates: Hires Occurred in the Last Year or Two Years of Each Panel

Panel	Hires in waves	$\not\!E E$ Recall rates	Counts	$E\not\!E\cdots\not\!E E$ Recall rates	Counts
1990	7−9	0.307	5,103	0.415	3,698
1991	7−9	0.263	3,395	0.381	2,325
1992	7−9	0.254	4,267	0.361	2,963
1993	7−9	0.269	3,989	0.378	2,778
1996	7−12	0.237	11,089	0.309	8,327
2001	7−9	0.254	4,861	0.336	3,604
2004	7−12	0.220	4,870	0.303	3,449
2008	8−10	0.259	4,937	0.390	3,238

Notes: Source, SIPP. Third column gives the number of recalls relative to all hires from non-employment, denoted by $\not\!E$ (including unemployment and inactivity). Fifth column gives the number of recalls relative to all the spells that end with employment. The results for the 2008 panel are based on the observations up to wave 10.

3.2.2 Recall Rates

Table 2 presents the recall rates by panel. Remember that we collect $E\not\!E E$ spells in each panel. We count the number of cases in which the worker returned to the same employers, relative to all $E\not\!E E$ spells. However, we also calculate the recall rates by including separations that do not end with employment within the period covered in each panel(denoted by $E\not\!E$). For example, a transition into unemployment occurs in the first year of a panel and the worker continues to be in the unemployment pool without going back to work until the end of the panel. In this case, there is no way to know if the worker is recalled or not. However, we count these cases as non-recall. Note that this treatment only reduces the recall rate. The third column presents recall rates including all separations into non-employment and the fifth column presents recall rates when we focus on $E\not\!E E$ spells.[4]

One can immediately see that recall rates are surprisingly high, regardless of which panel we look at. Even relative to all separations, close to 30% of workers return to the same employer. Due to the low frequency nature of the data, it is difficult to clearly see business cycle variations in the recall rates. However, it is interesting to note that recall rates increased in the 2008 panel relative to those in the 2004 panel. One possible reason is that the composition of separation flows shifted toward workers that are strongly attached to a particular firm, which raises recall rates ex post. Another possibility is that a decline in a recall expectations, especially among PS workers led them to leave the labor force altogether.

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[4]Another kind of observations arises when a spell ends with employment but information to determine recall or non-recall is missing. These cases are included in the calculation of the third column, being treated as non-recalls, but excluded from the calculation of the fifth column in the table.

Table 4: Recall Rates: Separations into Unemployment Occurred in the First Year or Two Years of Each Panel

Panel	Separations in waves	EU		$EU\cdots UE$	
		Recall rates	Counts	Recall rates	Counts
1996	1−6	0.408	3,725	0.45	3,388
2001	1−3	0.402	1,764	0.45	1,555
2004	1−6	0.422	1,610	0.49	1,369
2008	1−3	0.414	2,669	0.53	2,096

Notes: Source, SIPP. Third column gives the number of recalls relative to all separations into unemployment, denoted by U. Fifth column gives the number of recalls relative to all the spells that end with employment. The results for the 2008 panel are based on the observations up to wave 10.

Table 5: Recall Rates by Reasons for Separations into Unemployment

Panel	Separations in waves	Temp. Layoffs		Perm. Separations	
		Recall Rates	Counts	Recall Rates	Counts
1996	1−6	0.845	1,482	0.172	1,906
2001	1−3	0.867	679	0.167	876
2004	1−6	0.864	663	0.177	706
2008	1−3	0.873	997	0.232	1,099

Notes: Source, SIPP. The sample for $EU\cdots UE$ in Table 4 is split into two groups based on the reason of unemployment in the first month of the unemployment spell. The results for the 2008 panel are based on the observations up to wave 10.

Table 3 presents recall rates relative to hires that occur toward the end of each panel. This table confirms that recalls are common also from the viewpoint of the employer.

As mentioned before, the aggregate recall rate of all separated workers is very accurately estimated in the 1990-1993 SIPP panels, and probably underestimated in later panels. From now on, we report evidence conditioning on variables, primarily employment status, that are reliably available only after the 1996 re-design of the SIPP. Therefore, from now on reported statistics refer to the 1996-2011 period.

Table 4 focuses on those who are in the unemployment pool, a subset of the $E\not{E}E$ sample. This sample restriction raises recall rates: labor force attachment is strongly associated to recall. The third and fourth columns include the cases that never go back to employment, again treated as non-recall. The last two columns restricts attention to those spells that end with employment.

Table 5 splits the EUE sample into two groups by reason for unemployment, TL or PS. Because we focus on completed unemployment spells, from the counts of TL and PS in the table we can see that the share of TL here is close to half, significantly larger than that

Table 6: Unemployment Duration: $EU \cdots UE$

Panel	Sep. in waves	Overall			Recall			Non-Recall		
		Mean	SD	Median	Mean	SD	Median	Mean	SD	Median
1996	1−6	2.50	2.14	2.00	2.26	1.80	2.00	2.70	2.36	2.00
2001	1−3	2.65	2.62	2.00	2.16	1.95	1.00	3.06	3.00	2.00
2004	1−6	2.48	2.35	2.00	2.09	1.74	1.00	2.85	2.77	2.00
2008	1−3	4.00	4.83	2.00	2.81	3.30	2.00	5.32	5.83	3.00

Notes: Source, SIPP. The results for the 2008 panel are based on the observations up to wave 10.

in the EU inflow (see Figure 2); PS workers are much more likely to leave the labor force and not complete the unemployment spell. As expected, the recall rate for TL workers is very high and much higher than for PS workers. This is true for all panels. However, more importantly, even among PS workers, the recall rate is substantial: nearly 20% of workers who do not have an expectation of recall nevertheless return to the same employer.

3.2.3 Recall and Unemployment Duration

Table 6 summarizes the information about unemployment duration in the EUE sample. We calculate mean duration, standard deviation, and median duration for those who are recalled and those who move to a new employer. First note that recalls occur quicker than new hires. Similarly, the dispersion of unemployment duration is smaller for those recalled. We can also observe a clear countercyclicality of average duration: the average duration increased from 2.50 months in the 1996 panel to 2.65 months in the 2001 panel which corresponds to a recession year. A more striking increase can be observed for the 2008 panel, as is consistent with the well-known evidence in the monthly CPS. Interestingly, however, the increase in the average duration is especially concentrated among non-recalls. We can see a similar pattern for the standard deviation: dispersion of unemployment duration is countercyclical and the countercyclicality is especially pronounced among non-recall hires. The pattern here therefore highlights the important heterogeneity between recall and new hires that are hidden at the aggregate level.

3.2.4 Hazard Functions

Figure 7 plots the discrete hazard functions, calculated nonparametrically, for exit from unemployment by duration, again, based on the sample of EUE events. Specifically, we compute the probability of a recall (Panel (a)) and moving to a new employer (Panel (b)) at a particular duration (month) conditional on not having left the unemployment pool before then.

There is a clear negative duration dependence in the hazard function for recalls, while the hazard function for exiting unemployment by finding a job at a different employer is weakly hump-shaped, and much closer to be flat. To shed some light on this pattern, Figure

Figure 7: Hazard Functions: 1996-2008 Panels
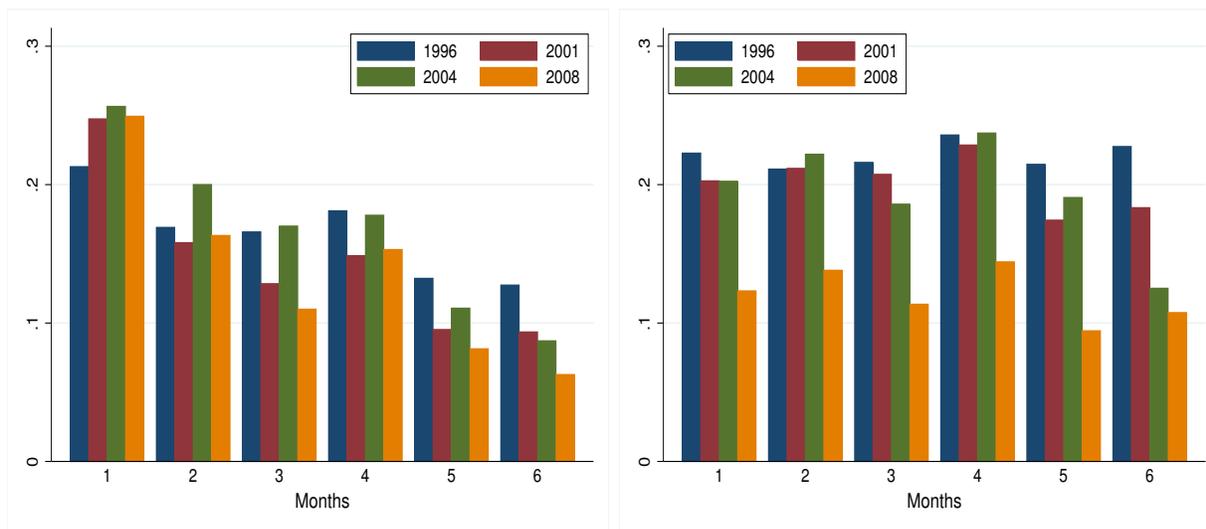
(a) Recall   (b) New Job

Notes: Source, SIPP. Based on the sample of $EU \cdots UE$ spells, where separations into unemployment occur in the first year or two years of each panel. Legends indicate the panel year.

8 further splits this sample of unemployed workers who find work but are not recalled based on the reason for their unemployment, TL or PS. Panel (a) shows that the exit probability to a new hire when a worker is on TL exhibits a clear upward sloping pattern. This pattern is consistent with the fact that, in the first few months of unemployment, the worker on TL has a strong expectation of recall and thus the probability of finding a new job is small, but after several months of unemployment, the worker is more likely to find a job elsewhere, as the recall expectation becomes less likely to be met. For PS workers who find a new job plotted in Panel (b), the exit hazard rates decline significantly with unemployment duration only between months 2 and 3.

Next, Figure 9 presents the share of recalls at each unemployment duration bin and shows that at the short duration bins a large fraction of exits from unemployment is due to recalls. This result, together with the fact that the hazard function for recall exhibits a clear negative duration dependence, suggests that negative duration dependence of unemployment could be strongly related to recalls. In particular, the heterogeneity between "short-term" and "long-term unemployment types" may be directly related to the chance/expectation of being recalled or not. In turn, this chance depends on worker characteristics, but recall puts some empirical flesh on these unobserved traits. Because we focus only on workers who remain in the labor force, thus eventually (within two years) all find jobs, our conclusions do not apply to the entire unemployment stock, which includes workers who drop out of the labor force and (re-)entrants. Nonetheless, it remains true that for all job losers the hazard rate of recall declines faster in unemployment duration than the hazard rate of exit to new jobs .

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Figure 8: Hazard Functions for New Hires: TL vs. PS, 1996-2008 Panels
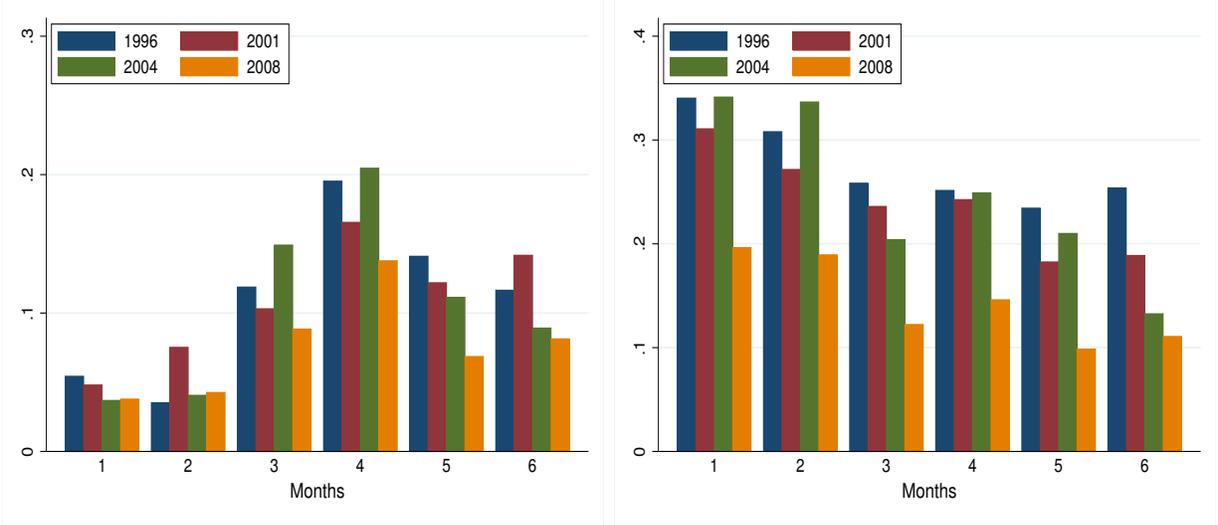
(a) TL

(b) PS

Notes: Source, SIPP. Based on the sample of $EU \cdots UE$ spells, where separations into unemployment occur in the first year or two years of each panel. Legends indicate the panel year.

Figure 9: Share of Recalls: 1996-2008 Panels
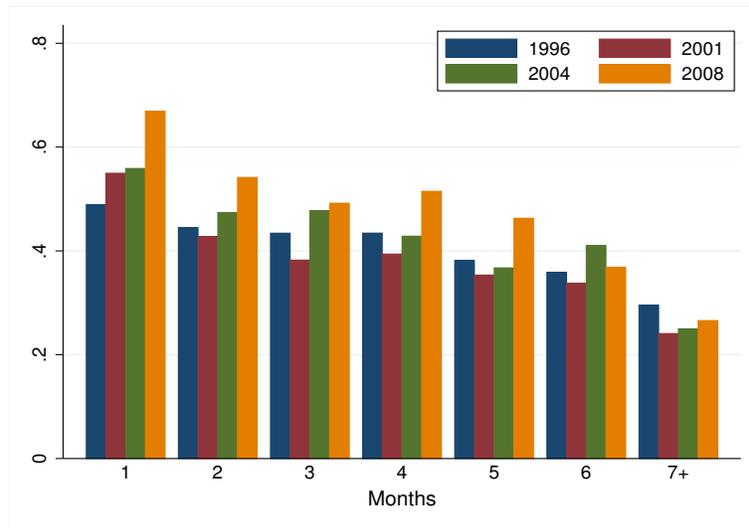
Notes: Source, SIPP. Based on the sample of $EU \cdots UE$ spells, where separations into unemployment occur in the first year or two years of each panel. Legends indicate the panel year.

3.2.5 Recall and Employer Tenure

To shed some light on the determinant of recalls, Figure 10 illustrates the relationship between employer tenure before separation and subsequent recall rates. We can see that those

Figure 10: Relationship Between Recall Rates and Firm Tenure
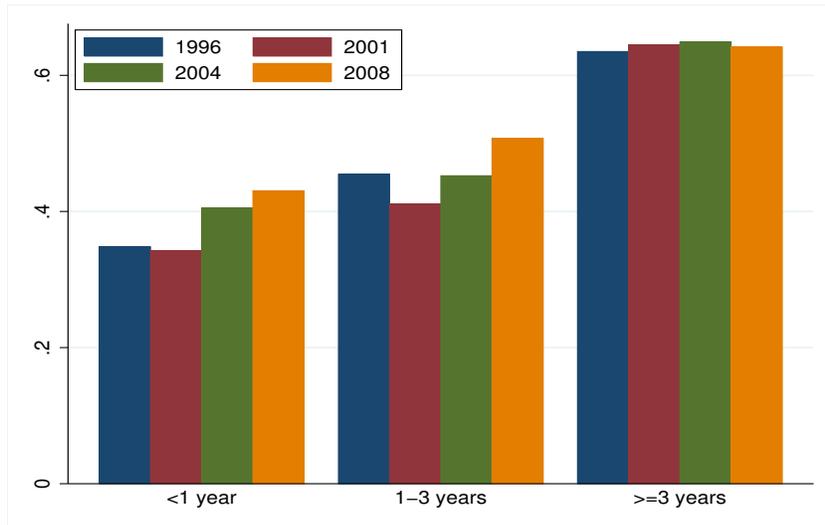
Notes: Source, SIPP. Based on the sample of $EU \cdots UE$ spells, where separations into unemployment occur in the first year or two years of each panel. Legends indicate the panel year.

who had longer tenure at the time of separation are more likely to be recalled. This pattern makes sense if tenure correlates with match-specific human capital.

3.2.6 Occupation Switches and Wage Changes

Table 7 examines detailed joint probabilities and associated outcomes in terms of the occupation switching rate and wage change, between first and second employment separated by unemployment in the EUE spells. The sample is divided based on (i) temporary layoffs (T) vs. permanent separations (P), (ii) unemployment duration of 3 months or less (S) vs. duration of 4 months or longer (L), and (iii) recall (R) vs. new hires (N). Because we are splitting the sample into the 8 detailed groups, we pool observations from 1996-2008 panels.

In terms of the probability of each event, (T,S,R), i.e., a worker who loses a job, is on Temporary Layoff, exits unemployment in a Short period of time ($\leq$3 months) and is Recalled, as well as (P,S,R), (P,S,N), and (P,L,N) are the most likely events. A relatively high probability of (P,S,R) means that even if the worker is classified as having experienced a "permanent separation" he/she is often recalled, and when this happens, it happens quickly.

The next two columns report the three-digit occupation switching probabilities for each event. Moving to a new employer after an uninterrupted unemployment spell always results in a very high probability of occupation switch. This finding is consistent with the result in Moscarini and Thomsson (2007), who find a high probability of occupation switch after a job-to-job transition in the CPS. The (T,S,R) case results in a very small chance of occupation switch. The other two cases with recall (T,L,R) and (P,S,R) result in slightly higher

Table 7: Joint Probabilities and Corresponding Outcomes: 1996-2008 Panels

Event			Counts	$\Pr(1,2,3)$	$\Pr(OS\|1,2,3)$ Occ. Switch Probability	$\mathbb{E}(\Delta\ln w\|1,2,3)$ Average Wage Change
T	S	R	2,691	0.325	0.024	0.010
T	S	N	310	0.039	0.653	0.027
T	L	R	364	0.039	0.143	0.036
T	L	N	174	0.022	0.523	0.047
P	S	R	419	0.055	0.238	−0.019
P	S	N	2,403	0.329	0.793	−0.032
P	L	R	434	0.052	0.627	−0.026
P	L	N	1,116	0.139	0.845	−0.116

Notes: Source, SIPP. Based on the sample of $EU\cdots UE$ spells, where separations into unemployment occur in the first year or two years of each panel. Event 1: temporary layoff (T) vs. permanent separation (P); Event 2: unemployment duration ≤ 3 months (S) vs. unemployment duration > 4 months (L); Event 3: recall (R) vs. new hires (N). All observations from 1996 through 2008 panels are pooled. Nominal hourly wage is converted into real hourly wage by using the PCE deflator.

occupation switching rates. Finally, in the (rare) (P,L,R) cases of a permanently separated worker who is recalled after a long unemployment spell, we observe significant occupational mobility, even after a recall. Because SIPP drops the job id in this (and any) long jobless spell, occupation codes in this case are coded "independently" (that is, questions are asked with no reference to the information given in the previous interview), which is known to inflate switching rates.

Finally, the last column reports average log real hourly wage differences before and after an unemployment spell. First, it is interesting to note that being on TL tends to result in better wage outcomes. In particular, finding a new job after being on TL results in a larger wage gain than from recall around 2%. This pattern makes sense given that those who had a clear expectation of a recall by the previous employer accept only an offer that dominates the value from returning to the same employer. Among PS workers, it is clear that moving to a new employer, particularly after a longer period of unemployment, results in a large wage loss (over 10%). On the other hand, returning to the previous employer results in a much smaller wage loss (around 3%). This fact, combined with the much longer pre-separation tenure of workers who are eventually recalled, strongly suggests that most of the wage loss due to a PS originates from a loss in firm-specific human capital.

To summarize, Table 7 demonstrates that "recalls vs. new hires" is an important economic distinction since it is systematically related to workers' economic outcomes.

4 Effects of Recalls on Matching Function Estimation

The matching function approach in modeling labor market frictions relies on the presumption that all hires result from a costly search process. It is, however, reasonable to assume that recalls circumvent the search friction. In this section, we assess the biases that could result from ignoring the different nature of recalls in the estimation of the matching function. For this estimation, we make the explicit (and novel) identification assumption that recalls are not mediated by the matching function, thus should not be included in the dependent variable of the estimation (hires). We find that, relative to the standard practice of including all accessions, this assumption leads to significant changes in the elasticity estimate of the matching function and in the measurement of matching efficiency (the residual term of the matching function).

4.1 Matching Function Estimation

First, let us write all hires as consisting of new hires and recalls:

$$H_t = R_t + M_t, \tag{1}$$

where H_t, R_t, and M_t, respectively, represent all hires, recalls, and new hires in period t. New hires are subject to a search friction which is modeled by a standard Cobb-Douglas matching function:

$$M_t = \mu_t \tilde{u}^{1-\alpha} v_t^{\alpha} \tag{2}$$

where μ_t corresponds to so-called matching efficiency, $\tilde{u}_t$ gives the number of job seekers, v_t is the number of vacancies, and α is the elasticity of new hires with respect to vacancies. It is important to note that $\tilde{u}_t$ can be different from unemployment (denoted by u_t) to the extent that some workers do not undertake job search expecting to be recalled. A natural way to distinguish between $\tilde{u}_t$ and u_t is to assume that those who are on TL do not undertake job search, expecting to be recalled. Thus we write:

$$\tilde{u}_t = u_t - u_t^{TL}, \tag{3}$$

where u_t^{TL} gives the number of unemployment due to TL, which we can directly measure from the CPS. Note that this assumption does not exclude the possibility that those in $\tilde{u}_t$ are recalled. Equations (1) and (2) imply:

$$\ln \frac{H_t}{\tilde{u}_t} + \ln(1 - s_t) = \bar{\mu} + \alpha \ln \left(\frac{v_t}{\tilde{u}_t} \right) + \varepsilon_t, \tag{4}$$

where $s_t = \frac{R_t}{H_t}$ gives the share of recalls out of all hires, which we call "recall rate" in this paper, and $\ln \mu_t$ is split into the constant term $\bar{\mu}$ and the demeaned residual term ε_t, which represents the matching efficiency term. We can measure directly s_t from SIPP. The remaining variables are also readily obtainable from the CPS. Thus we can readily estimate

Equation (4). One can think of the left-hand side variable as the job finding rate adjusted for recalls, and the explanatory variable on the right-hand side is adjusted market tightness. To see the sources of the bias in the standard estimation procedure that omits recalls, one can rewrite (4) as:

$$\ln \frac{H_t}{u_t} = \bar{\mu} + \alpha \ln \left(\frac{v_t}{u_t} \right) + \tilde{\varepsilon}_t,$$ (5)

where

$$\tilde{\varepsilon}_t = (1 - \alpha) \ln \left(\frac{\tilde{u}_t}{u} \right) - \ln(1 - s_t) + \varepsilon_t.$$ (6)

To the extent that $\frac{\tilde{u}_t}{u}$ and s_t are correlated with the unadjusted market tightness series $\frac{v_t}{u_t}$, the regression on (5) is subject to the omitted variable bias. Specifically, we showed the evidence earlier that the recall rate appears to be countercyclical and thus $-\ln(1 - s_t)$ is negatively correlated with the tightness series. Below we will construct a quarterly series for the recall rate and confirm this more formally. Furthermore, we will show that the share of unemployment due to TL is countercyclical, and thus $\frac{\tilde{u}_t}{u}$ is also negatively correlated with tightness. These two correlations imply that the regression on (5) results in an underestimate of the elasticity α. The measurement of matching efficiency is also biased: True matching efficiency ε_t differs from the one based on the standard estimation procedure (5) due to the bias in the elasticity estimate as well as the omission of the term $(1 - \alpha) \ln \left(\frac{\tilde{u}_t}{u} \right) - \ln(1 - s_t)$.

4.2 Data

In Section 3.2.2, we constructed recall rates aggregated at the panel level. To take into account of fluctuations of the recall rate in the estimation, we now construct a quarterly recall rate series. s_t in (4) gives the share of recalls out of all hires from unemployment. In Table 3, we computed the share of recalls out of all hires including those from inactivity. To be consistent with the matching function estimation, we focus on hires from unemployment. Remember also that to avoid the left censoring of $EU \cdots UE$ spells (which will skew our sample towards short spells), we drop the spells in which a transition into employment occurs in the first year of each panel. This way, we ensure that potential unemployment duration is longer than a year. Our estimation utilizes the data from the 1996 panel on, because as mentioned above, there is a break in the measurement of the labor market status. Unfortunately, however, after dropping the observations from the first year of each panel, we end up with only 42 quarterly observations that span between 1997Q1 and 2011Q3. To supplement incomplete SIPP-based recall rate series, we gauge the time series behavior of the recall rate by using the information available from the CPS, namely, the share of hires associated with TL out of all UE transitions.[5]

The blue (thick) and red solid lines in Figure 11 plot the SIPP based recall rate and the share of the TL hiring flow in the CPS. Both series are seasonally adjusted. As mentioned,

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[5]UE flows are based on the matched records. Hires associated with TL can be identified by using the reason-for-unemployment variable.

Figure 11: SIPP Recall Rate vs. CPS Share of TL Hires
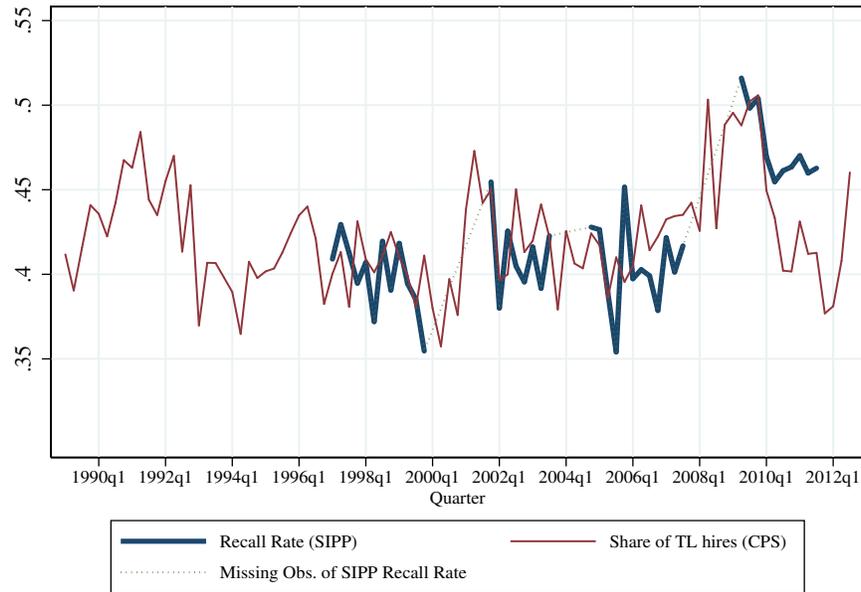
Notes: Share of TL hires: fraction of UE transitions of those on TL to all UE transitions, inflated by a constant factor of 1.78 to match the level of SIPP recall rate on average. Dotted line linearly interpolates the SIPP recall rate series.

the SIPP-based recall rate series starts in 1997Q1 and ends in 2011Q3. Note also that the dotted portion of the blue line corresponds to the missing observations in the SIPP recall rate and linearly interpolates the actual recall rate (the interpolation is only for the illustrative purpose). The red line gives the CPS-based TL hiring flow (as a share of all UE flow), and its average level is inflated by a constant factor (1.78) to match the SIPP-based recall rate. The constant factor is obtained by calculating the average ratio between the actual SIPP recall rate and the share of the TL hiring flow in the CPS over the quarters for which both series are available. As we showed earlier, the TL hiring flow captures only a part of recalls, and our assumption is that the time series behavior of the SIPP recall rate is well approximated by the CPS TL flow. This last assumption holds reasonably well for the overlapping periods, as the correlation between the two series is almost 0.6 (more precisely 0.585). In the estimation below, we use the share of the TL hiring flow as an approximation to the actual recall rate series, since this series gives us uninterrupted, longer time series. However, we also consider the estimation using the actual recall rate and show that we obtain a similar result.

One can see that both series indicate that the recall rate increased in the beginning of the Great Recession and then declined thereafter. Moreover, observe that the SIPP recall rate exhibits a downward movements between 1997 and 1999, and then jumps to a significantly higher level in the next observation for 2001Q4. These movements, which are consistent with the behavior of the TL hiring flow in the CPS, indicate the countercyclicality of recalls.

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Panel (a) of Figure 12 plots logged job finding rates. The blue dashed line plots the overall job finding rate while the red line plots the job finding rate adjusted for recalls, the left hand side variable of Equation (4). Note that this adjusted job finding rate gives the probability that an unemployed workers (excluding those on TL) finds a new job. Thus, the average level of the adjusted series is significantly lower than the unadjusted series. Furthermore, while the two series are highly correlated, the fluctuations of the adjusted series are more pronounced, which, for example, can be seen in the larger drop in the adjusted job finding rate during the Great Recession. The standard deviations of logged unadjusted and adjusted job finding rates are 0.195 and 0.231, respectively over the period between 1989Q1 and 2012Q3, confirming the casual observation. The larger volatility of the adjusted series mainly comes from the countercyclicality of the recall rate, equivalently, the procyclicality of the second term on the left hand side of Equation (4), $\ln(1 - s_t)$. Intuitively, increases in the recall rate during recessions mean that the "true" job finding rate (i.e, probability of finding a "new" job) declines more than what the unadjusted series indicates. Shimer (2012) isolates in the same CPS monthly data the effect of the changing unemployment composition between TL, PS and entrants, on the average exit rate from unemployment. Because the share of TL is countercylical and their average exit rate is higher than average, changes in the composition by itself creates the procyclical movements in the average exit rate. He finds, however, that this composition effect is quantitatively modest. An implication of his finding is that excluding TL from the stock of unemployment and their hires from the outflow should not make a big difference to the ratio (the transition rate). However, we go one step further and also exclude from the outflow (hires from unemployment) the share of PS hires that are recalls, thus focusing on the exit rate to a new job. This adjustment makes a more pronounced difference in the opposite direction than the composition effect. That is, the volatility puzzle of the job finding rate (Shimer (2005)) is even larger after adjusting for recalls.

Panel (b) presents unadjusted and unadjusted labor market tightness. We use JOLTS vacancy series after the first quarter of 2001 (the first release of JOLTS is December 2000). Before then, we use the Conference Board's help-wanted index series, constructed by Barnichon (2010) based on the Conference Board's help-wanted index. The level of the Conference Board's series is adjusted to match the level of the JOLTS series in 2001Q1. The unemployment data is taken from the monthly CPS and unemployment due to TL is also readily available from the monthly CPS releases. The graph indicates that excluding TL from the denominator of the tightness measure does not make a large difference. The two series move closely with each other, although excluding TL raises the level of tightness by definition.

As we mentioned above, correlation between the recall rate and market tightness induces the omitted variable bias in the standard matching function estimation. Figure 11 and panel (b) of Figure 12 indicate the presence of this correlation. The correlation coefficient between the actual SIPP recall rate and (unadjusted) market tightness is -0.785, while that between the approximated recall rate based on the share of TL hires and tightness is -0.464. In either case, the negative correlation is substantial. In the following section, we quantify the

Figure 12: Job Finding Rate and Market Tightness
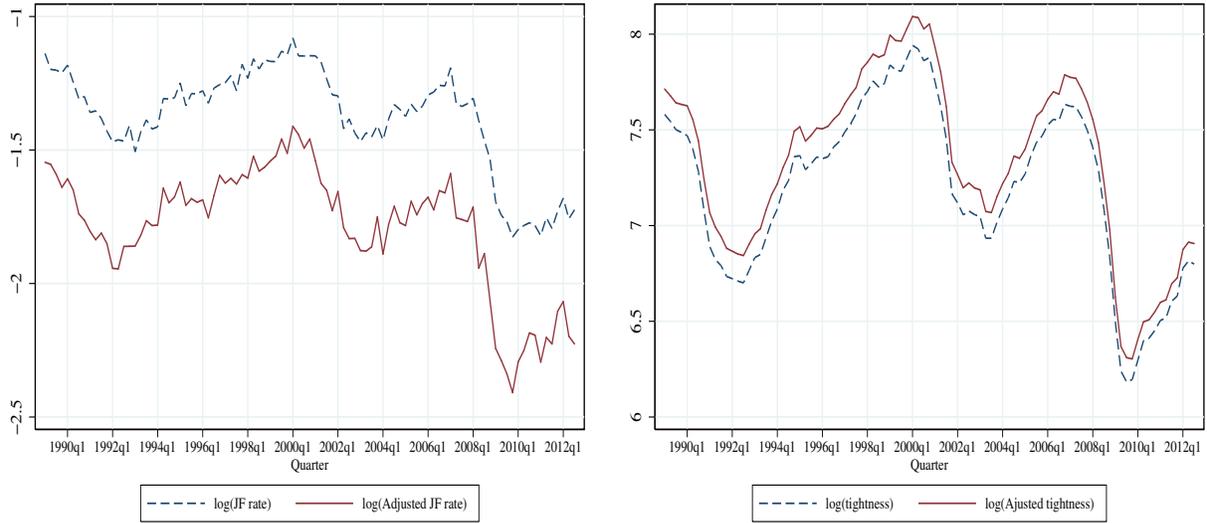
(a) Job Finding Rate   (b) Market Tightness

Notes: JF rate: overall UE transition rate, the explanatory variable of Equation (5). Adjusted JF rate: job finding rate adjusted for recalls, the explanatory variable of Equation (4), where s_t is approximated by the $1.78 \times$ share of TL hires.

consequences of this omitted variable bias on the elasticity estimate and the measurement of matching efficiency.

4.3   Elasticity Estimates

Table 8 presents the regression results. The first column of this table reports the regression result of Equation (4) when the share of TL hires is used for s_t. The second column reports the regression result of the standard matching function, Equation (5). As can be seen, the elasticity estimate increases considerably when recalls are taken into account. The intuitive reason for this result is that the job finding rate becomes more volatile once we properly account for cyclicality of recalls. Note that our elasticity estimate from the standard matching function regression is in line with the results in the existing literature that uses the CPS data, although the estimates from those studies are somewhat lower than our estimate 0.4, mainly due to the difference in the sample period.[6] The third column reports the result when the actual recall rate is used for s_t. For the comparison purpose, we reestimate the standard matching function regression by adjusting the sample period. When the actual SIPP recall rate is used, the difference in the elasticity estimate widens (it goes up from 0.42

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[6]See, for example, Shimer (2005), Barnichon and Figura (2011), and Sahin et al. (2012). Their estimates range typically between 0.25 and 0.35.

Table 8: Estimation Results

Estimated Equation	(4)	(5)	(4)	(5)
Elasticity	0.47	0.40	0.54	0.42
	(0.019)	(0.018)	(0.018)	(0.014)
Constant	-5.25	-4.29	-5.77	-4.43
	(0.146)	(0.139)	(0.136)	(0.104)
Adj-R^2	0.86	0.84	0.93	0.93
Sample Size	95	95	42	42
Measure of s_t	CPS TL hires	n.a.	SIPP recall	n.a.

Notes: Estimated equation (4): matching function with recalls; (5): standard matching function. Estimation under the first column uses the inflated share of TL hires out of all UE transitions in the CPS for s_t. Estimation under the third column uses the actual SIPP recall rate for s_t. The results under second and fourth columns differ only due to the difference in the sample period. The numbers in parentheses are HAC standard errors.

in the standard estimation to 0.54).[7]

4.4 Matching Efficiency

Figure 13 plots the matching efficiency, or residual, series from the two regressions. We take the 4-quarter moving average to smooth out high frequency variations of the two series. The dashed line corresponds to the residual term in the standard matching function regression (5). The most striking feature of this series is that since late 2009, matching efficiency has kept deteriorating to an unprecedented level. This result is again overall consistent with the findings by other studies (e.g., Barnichon and Figura (2011)). This result is intuitive given the behavior of the job finding rate and market tightness over this period. Comparing the blue dashed lines in the two panels in Figure 12, one can see that market tightness has recovered significantly since late 2009 when it hit the bottom, while, over the same period, the job finding rate has shown only a tepid recovery. Matching efficiency thus has deteriorated over this period.

Our assessment of the extent of "mismatch" changes significantly, once we consider the matching efficiency series that takes into account of recalls. Note first that there is a noticeable deterioration of matching efficiency at the *onset* of the Great Recession. In the case of the standard measure, on the other hand, the large deterioration of matching efficiency occurs only at the *end* of the Great Recession.[8] According to our adjusted measure, the extent of mismatch overall has not recovered since the initial declines at the start of the Great Recession, although temporary improvement can be observed in 2009-2010.

There is a large gap between the two matching efficiency series between 2007 and 2009.

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[7]The fact that the bias gets larger when the actual recall rate is used makes sense given that the negative correlation between the actual recall rate and market tightness is more pronounced, as mentioned above.

[8]According to the NBER, the Great Recession started in December 2007 and ended in June 2009.

Figure 13: Matching Efficiency
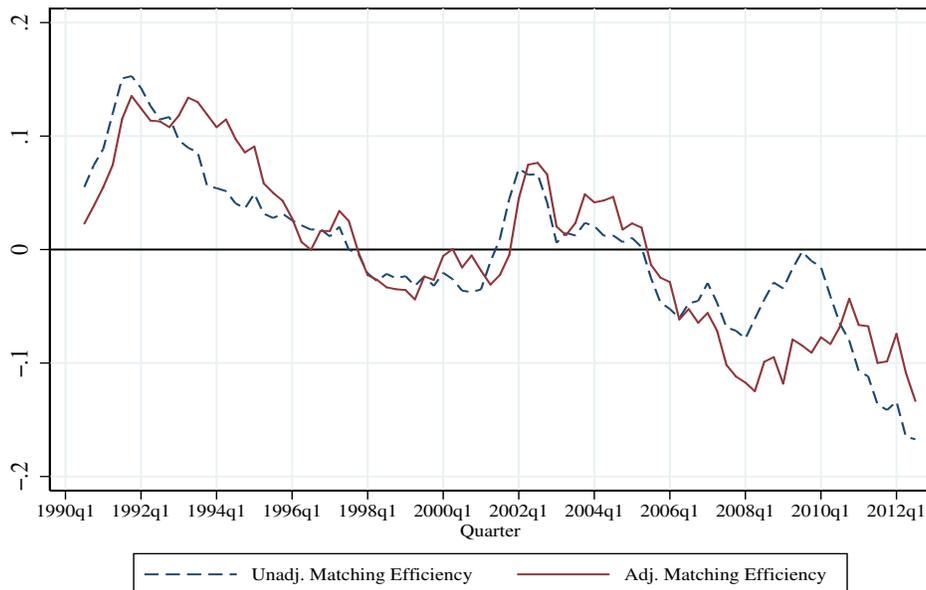
Notes: Adj. matching efficiency: ε_t in Equation (4); Unadj. matching efficiency: $\tilde{\varepsilon}_t$ in Equation (5). Four quarter (backward) moving average is taken to smooth out the high frequency variations of the series.

The large increase and the subsequent drop in the recall rate over this period (see Figure 11) are responsible for creating this gap. When the (unadjusted) job finding rate dropped sharply during the Great Recession, the recall rate increased significantly, which implies that the declines in the underlying (adjusted) job finding rate for a new job were even larger. This is when matching efficiency deteriorated significantly.

While the Great Recession provides the most striking period in terms of the difference between the two matching efficiency series, one can also observe qualitatively similar pattern in the other recessionary periods. That is, at the start of the recession when the recall rate increases, the true extent of mismatch is larger and then the relationship reverses as the recall rate subsequently drops. One can see this pattern in the early 1990's as well as early 2000's. In summary, the analysis in this section demonstrates the importance of recalls in our understanding of the state of the labor market. In particular, whether or not one takes into account of recalls makes quantitatively significant difference in our assessment of the extent of mismatch in and after the Great Recession.

5   A Stochastic Search Model with Recall

In order to make sense of this evidence and to understand its relevance to unemployment dynamics, we introduce a recall option in the Mortensen and Pissarides (1994) economy, and

we study its stochastic equilibrium when hit by aggregate productivity shocks.

5.1 Setup

Time is continuous. All agents are risk neutral and discount payoffs at rate $r > 0$. Firms produce output using a CRS technology, and sell it in a competitive market. The flow output from each match equals $p\varepsilon$. $p > 0$ is an aggregate component, common to all firms, while ε is an idiosyncratic component. Each of the two components p, ε evolves according to a Markov chain: at Poisson rate λ_p a new draw of aggregate productivity p' is taken from $dP(p'|p)$ and at Poisson rate λ_ε a new match value ε' is drawn from $dG(\varepsilon'|\varepsilon)$ while the worker is employed. Here we introduce our main modeling innovation, which has no counterparts in the existing literature and here gives rise to a recall option. After a separation, the value ε of the potential re-match between the old employer and the worker continues to evolve, according to the same Poisson rate of arrival λ_ε and a conditional distribution $dH(\varepsilon'|\varepsilon)$, possibly different than $dG(\varepsilon'|\varepsilon)$. The lowest possible match quality is equal to zero and an absorbing state for the match, so when ε drops to zero the match becomes permanently infeasible, as it will produce nothing for ever. So exogenous separations may be thought of as transitions to $\varepsilon' = 0$. In contrast, the rest of P, G and H are recurrent.

There are search frictions in the labor market. In order to create new matches, unemployed workers must pay a search cost to find vacancies, also posted at a cost. Old matches can be reassembled at no cost at any time, as long as the worker and job are still unmatched. An unemployed worker, who holds a match of quality ε with its former employer ($\varepsilon = 0$ if the old match can no longer be recalled), receives a flow payoff b and has three options: wait and do nothing, ask to recall the old match, or pay a search cost c_U to try and contact a new vacancy, that he finds at rate $\phi(\theta) = \theta q(\theta)$, where θ is the vacancy/unemployment ratio, job market tightness, and $q(\cdot)$ is a decreasing and convex function. When the unemployed worker and vacant firm do meet, they draw from a distribution F an initial match quality, that they observe provided that they start production, and then can retain if they separate. The search cost c_U can preempt job search by some workers who are likely to be recalled soon by their former employers, based on their current match quality; these 'waiting' workers do not search, but are still classified as unemployed (they are on "temporary layoff"). If the worker accepts the new offer, he forfeits the recall option with his former employer(s), but, immediately after starting production, he acquires a future recall option with the new employer.

Similarly, a vacant job that holds a match of quality ε with its former employee ($\varepsilon = 0$ if the former employee took another job) has three options: wait and do nothing ("mothball" the vacancy), recall the last employee, if still available (unemployed), or pay a search cost c_V and post the vacancy to contact, at rate $q(\theta)$, a random unemployed worker who is searching, and draw a new match from F. Firms are in excess supply and there is free entry, driving to zero the expected value of posting a new vacancy and searching for a new employee.

Wages in ongoing matches are set by a surplus-sharing rule, to be specified later. The

only requirement we impose for now is that separations and acceptance of new matches only depend on total match surplus. We assume that firms have no commitment power, not even to once-and-for all lump-sum transfers, and wages are continuously renegotiated. When an unemployed worker and a vacancy meet each other and draw a new match quality ε', the new and the former employer may want to engage in some sort of competition for the worker, but they cannot credibly do so due to a lack of commitment. The new employer, whatever it promises the prospective hire to induce him to give up the recall option, will renege immediately after the worker accepts. Therefore, the worker simply compares the values that he would obtain by bargaining separately with the two firms. Similarly, the last employee of the vacant job may want to compete with the new hiring prospect in order to retain his recall option. As we will see, this competition will be ruled out by CRS in production and free entry.

5.2 Equilibrium

Our main goal in this section is to show that the minimal state space for an equilibrium of this economy comprises only aggregate productivity p and, for each match, the quality ε of the current or last job's match quality, if any. This property makes equilibrium characterization and computation very tractable. To this purpose, we need to make a careful choice of assumptions on wage-setting. We proceed by assuming that equilibrium has this property and then verifying that the guess is consistent with all equilibrium restrictions.

Let $U(p, \varepsilon)$ denote the value of unemployment, where $p\varepsilon$ is the productivity of the last employer, if any (otherwise $\varepsilon = p\varepsilon = 0$), $W(p, \varepsilon)$ the value of employment to the worker, $V(p, \varepsilon)$ the value of a vacant job, where $p\varepsilon$ is the productivity of the last employee, if any (otherwise $\varepsilon = p\varepsilon = 0$), $J(p, \varepsilon)$ the value of a filled job, $w(p, \varepsilon)$ the wage. Recall occurs whenever both parties gain from it.

When an unemployed worker, searching for a new job, receives an outside offer, the capital gain from job search, conditional on searching and on contacting an open vacancy, is

$$\int \mathbb{I}\{W(p, \varepsilon') \geq U(p, \varepsilon')\} \max \langle W(p, \varepsilon') - U(p, \varepsilon), 0 \rangle dF(\varepsilon') \tag{7}$$

where $\mathbb{I}$ is the indicator function. The new offer at match quality ε' is acceptable only if it yields the worker both a positive surplus $W(p, \varepsilon') - U(p, \varepsilon')$ from forming the new match (in which case the new firm agrees) and a value $W(p, \varepsilon')$ that exceeds that $U(p, \varepsilon)$ of waiting for a recall of the old match, which has current quality ε.

The first key observation is that the continuation value $W(p, \varepsilon')$ after accepting the new offer is independent of the value $U(p, \varepsilon)$ of the recall option that the worker may have in hand. The reason is that no competition for the worker takes place between the old and new employer due to the lack of commitment power. In turn, this implies that the returns from hiring an unemployed worker who accepts a new match do not depend on the value of his recall option. This "memoriless" property is the key to the simplicity of the equilibrium

under consideration. A firm contemplating posting a vacancy does not need to keep track of the distribution of old match qualities among jobless workers. If the bargaining environment did allow the worker to carry part of his recall option value over to the new match, the profits from hiring new workers would depend on the recall prospects of the job-searching unemployed. Firms then would have to track their cross-section distribution, which is an infinitely-dimensional object, changing stochastically with the aggregate state.

Although the value of the recall option, as measured by $U(p, \varepsilon)$, does not impact wages in a new job, it could impact the *probability* that the unemployed worker accepts a new match, which still matters for vacancy posting and job creation. If $U(p, \varepsilon') < W(p, \varepsilon') < U(p, \varepsilon)$ the worker may want to continue waiting for a recall, although the new match is valuable. If so, the firm has to keep track of the probability that this event occurs, which varies with the aggregate shock and in fact depends on the distribution of recall options among the unemployed, so it is history-dependent.

To avoid this complication, we look for an equilibrium where any new match that is acceptable to an unemployed worker with no recall option is also acceptable to an unemployed worker with a positive recall option. The key insight is that, if the worker who makes contact with a vacancy is jobless, his recall value must be low enough not to justify recall; so, the surplus from his old match over continuing unemployment at that match quality must still be negative. Because a new match is implemented only if the surplus it generates over separating and keeping the *new* match quality is positive, then it must pay the worker more than the recall option.

Formally, we guess and later verify that the functions W, U and $W - U$ are increasing in ε. Thus consider ε and $\varepsilon' > \varepsilon$. Then

$$U(p, \varepsilon') \geq U(p, \varepsilon)$$

and

$$W(p, \varepsilon') - U(p, \varepsilon') \geq 0 \geq W(p, \varepsilon) - U(p, \varepsilon)$$

which must be the case for any acceptable new match, because ε' must yield a positive surplus to be acceptable, and ε must yield a negative surplus, otherwise that job would have been recalled and the worker would not be unemployed. Together, these imply $W(p, \varepsilon') \geq U(p, \varepsilon)$. Hence, in any acceptable match

$$\mathbb{I}\langle W(p, \varepsilon') \geq U(p, \varepsilon')\rangle = 1 \Rightarrow \max\langle W(p, \varepsilon') - U(p, \varepsilon), 0\rangle = W(p, \varepsilon') - U(p, \varepsilon). \quad (8)$$

Therefore, the probability that a new offer is accepted is independent of the value of the recall option the unemployed worker has in hand, and only depends on the new match quality draw. We can eliminate the max from the continuation value of search (7), which then reads simply

$$\Omega(p, \varepsilon) = \int \mathbb{I}\{W(p, \varepsilon') \geq U(p, \varepsilon')\} [W(p, \varepsilon') - U(p, \varepsilon)] dF(\varepsilon').$$

The associated probability of acceptance of a new match is

$$A\left(p\right) = \int \mathbb{I}\left\{W\left(p,\varepsilon'\right) \geq U\left(p,\varepsilon'\right)\right\} dF\left(\varepsilon'\right)$$

independent of the current recall value ε.

5.2.1 Bellman Equations: Firm

The flow value of a filled job equals flow output minus the wage plus capital gains or losses after each type of shock, which may induce the match to separate:

$$rJ\left(p,\varepsilon\right) = p\varepsilon - w\left(p,\varepsilon\right) + \lambda_p \int \left[\max\left\langle J\left(p',\varepsilon\right), V\left(p',\varepsilon\right)\right\rangle - J\left(p,\varepsilon\right)\right] dP\left(p'|p\right)$$

$$+ \lambda_\varepsilon \int \left[\max\left\langle J\left(p,\varepsilon'\right), V\left(p,\varepsilon'\right)\right\rangle - J\left(p,\varepsilon\right)\right] dG\left(\varepsilon'|\varepsilon\right). \tag{9}$$

The value of a vacant job solves

$$rV\left(p,\varepsilon\right) = \lambda_p \int \left[\max\left\langle J\left(p',\varepsilon\right), V\left(p',\varepsilon\right)\right\rangle - V\left(p,\varepsilon\right)\right] dP\left(p'|p\right)$$

$$+ \lambda_\varepsilon \int \left[\max\left\langle J\left(p,\varepsilon'\right), V\left(p,\varepsilon'\right)\right\rangle - V\left(p,\varepsilon\right)\right] dG\left(\varepsilon'|\varepsilon\right)$$

$$+ \mathbb{I}\left\{\phi\left(\theta\left(p\right)\right)\Omega\left(p,\varepsilon\right) - c_U \geq 0\right\} \phi\left(\theta\left(p\right)\right) A\left(p\right) \left[V\left(p,0\right) - V\left(p,\varepsilon\right)\right]$$

$$+ \max\left\langle 0, -c_V + q\left(\theta\left(p\right)\right) \int \mathbb{I}\left\{J\left(p,\varepsilon'\right) \geq V\left(p,\varepsilon'\right)\right\} \left[J\left(p,\varepsilon'\right) - V\left(p,\varepsilon\right)\right] dF\left(\varepsilon'\right)\right\rangle$$

$$\tag{10}$$

where in the fourth lines we used (8). The job can recall the former employee after any shock, but also lose the recall option if the former employee successfully locates a new acceptable offer. This occurs if the expected capital gain from job search is positive (third line), a contact occurs (at rate ϕ), and the new match is acceptable, which has chance equal to the integral in the fourth line. The firm that owns this job can also pay the vacancy cost to meet a new worker, and hires him if the new match draw ε' guarantees a positive surplus and a higher value to the firm than the continuation. This term, on the last line, does not contain a max operator inside the intregral, for the same reasons that we illustrated in the case of the unemployed worker.

5.2.2 Free entry

By free entry, firms post new vacancies, which start from $\varepsilon = 0$, until their net value is zero: for all p, $V\left(p,0\right) = 0$. When $\varepsilon = 0$, an absorbing state, the match will never be productive again and the vacancy is worthless. Since $\varepsilon = 0$ is an absorbing state, $J\left(p,0\right) = V\left(p,0\right) = 0$

and $J(p, \varepsilon') = V(p, \varepsilon') = V(p, 0)$ for all $\varepsilon' \sim dG(\varepsilon'|0)$. Using these facts in (10) we obtain a familiar-looking free-entry condition:

$$\frac{c_V}{q(\theta(p))} = \int \mathbb{I}\{J(p, \varepsilon') \geq V(p, \varepsilon')\} J(p, \varepsilon') \, dF(\varepsilon') \tag{11}$$

and therefore

$$rV(p, \varepsilon) = \lambda_p \int [\max\langle J(p', \varepsilon), V(p', \varepsilon)\rangle - V(p, \varepsilon)] \, dP(p'|p)$$

$$+ \lambda_\varepsilon \int [\max\langle J(p, \varepsilon'), V(p, \varepsilon')\rangle - V(p, \varepsilon)] \, dG(\varepsilon'|\varepsilon)$$

$$- V(p, \varepsilon) \mathbb{I}\{\phi(\theta(p)) \Omega(p, \varepsilon) - c_U \geq 0\} \phi(\theta(p)) A(p) \tag{12}$$

where the last term is the loss to the firm when its previous employee finds another job while waiting for a recall.

Conversely, for $\varepsilon > 0$ we have $V(p, \varepsilon) > 0$. A vacant job that still retains a positive match quality with a former employee has a positive chance of recalling him in the future, because match quality can rise to any higher level with positive probability in finite time. Since both mothballing the vacancy and recalling a worker are costless, the value of this vacant job is positive even when just waiting and not searching. Thus, this job will not post a vacancy, but wait. Put more simply, by constant returns to scale in production, no firm has an incentive to fill a job that could still be subject to recall with a new employee, but rather creates another job to look for the new worker. In contrast, a worker can only work for one firm, thus an unemployed worker's former employer can be replaced by a competitor who hires him.

5.2.3 Bellman Equations: Worker

The employed worker's value solves the Hamilton-Jacobi-Bellman equation

$$rW(p, \varepsilon) = w(p, \varepsilon) + \lambda_p \int [\max\langle W(p', \varepsilon), U(p', \varepsilon)\rangle - W(p, \varepsilon)] \, dP(p'|p)$$

$$+ \lambda_\varepsilon \int [\max\langle W(p, \varepsilon'), U(p, \varepsilon')\rangle - W(p, \varepsilon)] \, dG(\varepsilon'|\varepsilon). \tag{13}$$

After each shock, the worker may decide to quit.

The HJB equation of the unemployed worker is

$$rU(p, \varepsilon) = b + \lambda_p \int [\max\langle W(p', \varepsilon), U(p', \varepsilon)\rangle - U(p, \varepsilon)] \, dP(p'|p)$$

$$+ \lambda_\varepsilon \int [\max\langle W(p, \varepsilon'), U(p, \varepsilon')\rangle - U(p, \varepsilon)] \, dH(\varepsilon'|\varepsilon)$$

$$+ \max\langle 0, \phi(\theta(p)) \Omega(p, \varepsilon) - c_U\rangle. \tag{14}$$

After each shock, the worker may decide to reactivate the old job; in addition, he can decide to search for a new job, that he accepts if it offers a positive surplus (to the worker, hence to the firm).

5.2.4 Wages

We can close the model with a variety of wage-setting mechanisms. One prominent example is a linear surplus-sharing rule:

$$\beta[J\left(p,\varepsilon\right) - V\left(p,\varepsilon\right)] = \left(1-\beta\right)\left[W\left(p,\varepsilon\right) - U\left(p,\varepsilon\right)\right]. \tag{15}$$

This rule satisfies our requirement that separations and match acceptance only depend on total match surplus. If job search is costless, $c_U = 0$, (15) is also the generalized Nash Bargaining solution, thus it maximizes joint surplus and is privately efficient. If, however, job search is costly, $c_U > 0$, then wages affect the incentives to search. Because a vacant firm suffers a non-insurable loss when the former employee, waiting for a recall, takes another job, the firm may have an incentive to raise the wage, after recall, above the level implied by (15), in order to discourage job search ex ante. This is, however, a promise that the firm has to make and then deliver if the worker does get recalled. We assume this promise is not credible. In this sense, the bargaining problem is different than that with on-the-job search (Shimer (2006)), where the firm is already paying the worker, so it can continuously deliver on the promise while producing. Alternatively, the firm could pay the former employee not to search while unemployed, a kind of employer-sponsored unemployed benefit that is lost when accepting a new job. We also rule out this option by assumption.

Imposing (15) and after much algebra we obtain an expression for the wage:

$$\begin{aligned}
w\left(p,\varepsilon\right) = {} & b + \beta\left(p\varepsilon - b\right) + \left(1-\beta\right)\max\left\langle 0, \phi\left(\theta\left(p\right)\right)\Omega\left(p,\varepsilon\right) - c_U\right\rangle \\
& + \beta\mathbb{I}\left\{\phi\left(\theta\left(p\right)\right)\Omega\left(p,\varepsilon\right) \geq c_U\right\}\phi\left(\theta\left(p\right)\right)A\left(p\right)V\left(p,\varepsilon\right) \\
& + \lambda_\varepsilon \int\left[\beta V\left(p,\varepsilon'\right) - \left(1-\beta\right)U\left(p,\varepsilon'\right)\right]\left[dG\left(\varepsilon'|\varepsilon\right) - dH\left(\varepsilon'|\varepsilon\right)\right].
\end{aligned} \tag{16}$$

The wage equals the opportunity cost of time b plus the worker's bargaining share β of the flow surplus from working, $p\varepsilon - b$, plus a share $1-\beta$ of the continuation value of job search from unemployment. All this is standard. In addition, two new terms appear in this model with recall. First, the wage is augmented by a fraction β of the potential loss that the vacant firm would incur, after separation, should the worker find another job. Intuitively, separation gives the firm a positive value of the vacancy $V\left(p,\varepsilon\right)$, the value of the recall option, because match quality ε can rebound to feasible values. This option value is eroded by the chance that the worker searches and finds another job, becoming unavailable for a recall. This erosion reduces the outside option of the firm, increases match surplus, thus the wage.

Finally, the wage is affected by the change in match quality evolution after separation, as captured by the difference between the transition c.d.f.s G (on the job) and H (on the job). Suppose, for example, that G first-order stochastically dominates H because interrupting the employment spell causes some skill loss. Then the last term in the wage function is positive if $\beta V\left(p,\varepsilon'\right) - \left(1-\beta\right)U\left(p,\varepsilon'\right)$ is increasing in ε'. That is, if the value of unemployment is less sensitive to match quality than the value of the vacancy, after weighting for bargaining

shares, the worker will suffer less than the firm from accelerated match quality depreciation after separation. This gives the worker additional bargaining power, and raises the wage.

Equilibrium of the model is described by J, V, W, U, w, and θ that solve (9), (11), (12), (13), (14) and (16) as functions of aggregate and idiosyncratic productivities. It is straightforward to solve exactly this system of functional equations through any nonlinear iteration algorithm. We exploit this tractability to explore the quantitative properties of the model.

6 Quantitative Properties of the Model

We calibrate the model in steady state and then explore its business cycle properties. A unit time interval in the model is set equal to a week. For the steady state, we simulate the model's equilibrium to generate weekly observations (a total of one million person-week observations) and then sample these observations every four weeks. The resulting monthly panel data set is used to compute the model-based statistics. We do so to be consistent with the structure of SIPP interviews, and at the same time to be as close as possible to the continuous time in which the model economy lives. The simulation procedure for the business cycle analysis is described in subsection 6.2. For the time being, we calibrate internally (estimate) by simulated moments two parameters, the persistence and volatility of innovations to the idiosyncratic productivity of a match, and we fix the rest of the parameter values. This allows us to illustrate the qualitative properties of the solution. The quantitative performance of this limited, exploratory analysis is already acceptable. We postpone a more in-depth quantitative analysis to a future revision.

6.1 Calibration

The discount rate is set to $r{=}0.001$, which roughly corresponds to 5% at annual frequency. The flow value of unemployment b is set equal to 70% of steady state average level of output as in Hall (2009). We assume that unemployed job search is costless, $c_U = 0$, in which case, the linear sharing rule coincides with the Nash Bargaining solution.

The arrival rate of idiosyncratic shocks ε is set to $\lambda_\varepsilon = 3/13$, so that shocks arrive on average every 13/3 weeks (i.e., one month). Conditional on the arrival of a shock, the match experiences exogenous destruction with probability $\delta = 0.003$. When hit by this shock, the match productivity transits from any state $\varepsilon > 0$ to the lowest state $\varepsilon' = 0$. Since the lowest state is absorbing, this transition makes the future recall impossible. The remainder of the total monthly EU transition rate (0.015) is targeted as endogenous separations. With complementary probability 99.7%, $\log \varepsilon$ experiences an innovation drawn from an AR(1) process with serial correlation ρ_ε and the volatility σ_ε of innovations. These are the two parameters that we estimate. We approximate this AR(1) on a discrete grid of 49 points for $\log \varepsilon$ using Tauchen's method, append the lowest state $\varepsilon = 0$ and related transition

probability δ into it, to obtain the Markov chain G. After separation, match quality evolves according to the same stochastic law of motion, so $H = G$.

Consistently with our empirical exercise, the contact rate of unemployed workers with open vacancies derives from a standard Cobb-Douglas matching function:

$$\theta q \left(\theta\right) = \mu\theta^{\alpha}$$

where $\theta = v/u$ is job market tightness and μ is a matching scale parameter. To quantify μ we proceed as follows. First, we take the ratio between the vacancy rate (as a fraction of employment) in JOLTS and the contemporaneous unemployment/employment ratio in the CPS. We then take an average of this time series to estimate steady state job market tightness $\bar{\theta}$. This equals 0.52 in $2001:1-2008:12$, and 0.42 in $2000:12-2013:3$, which includes the Great Recession. We take $\bar{\theta} = .5$. Next, we guess a value for the steady-state contact rate of vacancies with job searchers, $\bar{q} = \mu/\bar{\theta}^{\alpha}$. We feed $\bar{q}$ and the worker's job contact rate $\bar{\theta}\bar{q}$ into the worker's and firm's Dynamic Programming problem, which we solve by value function iteration. We then find the optimal threshold $\underline{\varepsilon}$ for acceptance of a new match (as well as for separation and recall), thereby the exit rate from unemployment to new jobs, $\mu\bar{\theta}^{\alpha}\left[1 - F\left(\underline{\varepsilon}\right)\right]$. We iterate on the guess $\bar{q}$ of the contact rate until the exit rate from unemployment to new jobs generated by the model equals the empirical target 15% per month, which is our estimate for the average probability of exit from unemployment to *new* jobs from SIPP. Multiplying the value of $\bar{q}$ upon convergence by $\bar{\theta}^{\alpha} = 0.5^{\alpha}$, we obtain our estimate of the average matching scale μ. The solution to the DP problem also yields the expected surplus to the firm from a new match. Imposing free entry, we back out the vacancy posting cost c_V that rationalizes those values of contact and exit rates. We set the matching elasticity with respect to vacancies α to 0.5, based on our own estimate of the matching function discussed in the previous section. We set the worker bargaining share β equal to $1 - \alpha$, a tradition that originates in the Hosios efficiency condition, although this might not apply to our economy with recall.

We now turn to our empirical targets. By construction, we match exactly the average job market tightness $\bar{\theta} = 0.5$ and monthly probability of finding a new job (15%). In addition, we target six empirical moments: a total job finding probability of 30% per month, the total separation probability of 1.5% per month, and the hazard rate of exit of unemployed workers to new jobs and to recall at one month and at six months of unemployment duration (see Figure 7). Our choice of empirical targets is motivated by the following considerations. The job finding and separation rates are the core of the model; they yield the unemployment rate and the probability of recall. The four moments on duration dependence are very informative about the selection effect by match quality which is, in our model, the source of recall. In the data, completed unemployment spells exhibit negative duration dependence only when the spell ends with recall and we aim to replicate this property. "Targeting" these six moments means finding the values of the persistence ρ_ε and volatility σ_ε of idiosyncratic shocks that minimize the norm of the log-difference between simulated and empirical moments.

Table 9 summarizes the best calibration. Persistence ρ_ε and volatility σ_ε of idiosyncratic

Table 9: Parameter Values: Weekly Calibration

Symbol	Description	Value
r	Discount rate	0.001
b	Flow value of unemployment	0.7
c_U	Search cost	0
λ_ε	Arrival rate of idiosyncratic shock	3/13
δ	Exogenous job destruction	0.003
ρ_ε	Persistence of idiosyncratic shock	0.94
σ_ε	Standard deviation of idiosyncratic shock	0.04
μ	Matching scale parameter	0.0671
c_V	Vacancy posting cost	0.4659
β	Worker bargaining share	0.5
α	Matching function elasticity	0.5
λ_p	Arrival rate of aggregate shock	1/13
ρ_p	Persistence of aggregate shock	0.76
σ_p	Standard deviation of aggregate shock	0.025
n.a.	Mean output level	1

shocks are meant to be monthly, because such shocks hit on average once a month. Upon convergence, we find that the required $\bar{q}$ is close to 10% per week. Note that our estimate of the job-filling rate is much lower than that implied by JOLTS, and often mentioned in the literature, where hires include recalls. The average contact rate of unemployed workers with open vacancies is about 4.5% per week. Not all contacts result in acceptable new matches, but more hires than new matches occur through recalls.

We also consider additional moments that we do not target in the real data: the share of all hires that are recalls (recall rate), the mean length of all completed unemployment spells and that of all completed spells that end in either a new match or a recall. Finally, we obtain the hazard rate of exit to new jobs and to recall at each unemployment duration from 2 to 5 months.

Tables 10 and 11 report the results on the model fit. Qualitatively, the calibrated model can explain the different negative duration dependence of unemployment by type of exit. In line with the canonical search and matching model, exit to new jobs is mediated by a matching function and occurs at constant probability, which does not change over the course of an unemployment spell and only depends on job market tightness. Recall, on the other hand, becomes less and less likely as unemployment duration increases, due to selection: if an unemployed has not been recalled, chances are that his match has further deteriorated after separation, hence the likelihood of a rebound to trigger a recall is lower. Quantitatively, this simple calibration with a parsimonious idiosyncratic process does a remarkable job at fitting both targeted and untargeted empirical moments.

To calibrate the aggregate productivity process p, we proceed as follows. We use quarterly

Table 10: Steady-State First Moments of the Model

Unemployment rate	Separation rate	Job finding rate (overall)	Job finding rate (new hires)	Recall probability	Recall rate (share)
0.049	0.015	0.282	0.147	0.135	0.479

Notes: Based on the simulations of the steady state version of the model, solved at weekly frequency. The above statistics are constructed from the monthly panel data constructed by sampling the observations from the weekly panel data every four weeks.

estimate of the Solow residual by Fernald (2012), which is corrected for capacity utilization and covers the period between $1947-2013$. We take logs and HP-filter the TFP series with the smoothing parameter 1,600 and fit an AR(1) to its deviations from trend. We estimate a serial correlation of 0.727 and a standard deviation of residuals equal to 0.0289 at quarterly frequency. Next, we assume that in continuous time aggregate shocks arrive at rate $\lambda_p=1/13$, so that the shock arrives once per quarter (every 13 weeks) on average. In other words, each quarter the economy is hit by $n \sim Poi(1/13)$ aggregate shocks. Conditional on arrival of each shock, the new realization is AR(1). We choose values for the parameters, serial correlation and volatility, of this AR(1), simulate a time series of 260 quarters, draw $n \sim Poi(1/13)$ AR(1) shocks within each quarter, record the value of the simulated process at the end of each quarter, ignoring the infra-quarter realizations to reproduce the time aggregation in the data, and fit an AR(1) to these simulated quarterly data. We iterate on values of the parameters of the shocks arriving in continuous time in order to hit the quarterly empirical targets 0.727 and 0.0289. We find the parameter values at 0.76 and 0.025, respectively. These values are not very different from the quarterly empirical targets, given that in the simulation exactly one shock occurs in most quarters, as if the model was in discrete time. The occasional occurrence of $n = 0, 2, 3..$ shocks within a quarter in the simulation explains the (small) discrepancy between parameters and targets. We approximate this AR(1) on a discrete grid of 20 points for p using Tauchen's method.

6.2   Cyclical Properties of the Model

We now examine the cyclical properties of the model's equilibrium. The second moments of the aggregate time series are computed as follows. We first solve for the dynamic stochastic equilibrium, namely values and tightness as functions of the aggregate productivity shock, and then simulate a large panel data set consisting of 30,000 workers over 4,800 weekly periods. We discard the first 800 observations to randomize the initial conditions. We then sample the data every four weeks to obtain the monthly panel. The monthly sampling yields 10,000 monthly observations across the same 30,000 workers. Based on this panel data, we obtain the aggregate time series of the following data: (i) unemployment rate, (ii) separation rate, (iii) overall job finding rate, (iv) job finding rate for new hires, (v) recall probability, and (vi) recall rate (share of recalls out of all hires). We convert the monthly time series into the quarterly series through simple time averaging. Lastly, we take the natural logarithm of

Table 11: Mean Duration and Hazard Rates

	Overall hires	Recalls	New hires
	Mean duration		
Months	3.425	2.613	4.170
	Hazard rates		
1 month	0.349	0.201	0.148
2 months	0.322	0.173	0.149
3 months	0.292	0.139	0.153
4 months	0.265	0.115	0.150
5 months	0.249	0.096	0.154
6 months	0.248	0.084	0.165

Notes: See notes to Table 10.

the quarterly series and HP-filter the logged data with the smoothing parameter of 1,600.

Table 12 presents the standard deviations and the correlation matrix of the six variables. The volatility of the unemployment rate is 0.128 and roughly comparable to its empirical counterpart. In terms of the volatility of the underlying transition rates (the separation rate and overall job finding rate), the separation rate is three times as volatile as the overall job finding rate. This is counterfactual. Thus, even though the model generate the volatility of unemployment comparable to the data, the underlying mechanism is not entirely consistent with the observed data. Remember that our calibration does not target the second moments, and therefore this result is not surprising. The fluctuations of the job finding rate itself appear too small relative to its empirical counterpart, suggesting that adding the recall option to the MP model does not solve the volatility puzzle of Shimer (2005). However, when the job finding rate for new hires is separately considered, the volatility is somewhat higher than that for all hires. Moreover, the fact that the job finding rate for new hires is larger than that for the overall job finding rate is consistent with the earlier empirical finding in Section 4 that excluding recalls from hires raises the volatility of the job finding rate. Lastly, the model generates the fairly volatile recall probability and recall rate.

Turning to correlations, the model replicates the countercyclical separation rate and procyclical job finding rate, as in the standard MP model.[9] But an interesting feature of the model with recall is the fact that the recall probability is nearly acyclical (the correlation coefficient with unemployment is 0.069). Observe that the correlation of the overall job finding rate with respect to unemployment is somewhat smaller than that of the job finding rate for new hires only. This is because the acyclical variability of the recall probability plays a role in reducing procyclicality of the overall job finding rate. This pattern appears to be at least qualitatively consistent with the new empirical evidence thet we provided earlier. The acyclicality of the recall probability results from two effects that offset with

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[9]See Fujita and Ramey (2012) for the cyclical properties of various versions of the MP model.

Table 12: Cyclical Properties of the Model

	(i) Unemp. rate	(ii) Sep. rate	(iii) JF rate (total)	(iv) JF rate (new hires)	(v) Recall prob.	(vi) Recall rate
			Standard Deviations			
	0.128	0.138	0.045	0.059	0.085	0.052
			Correlation Matrix			
(i)	1.000	0.901	−0.476	−0.820	0.069	0.526
(ii)		1.000	−0.557	−0.631	−0.150	0.243
(iii)			1.000	0.409	0.769	0.263
(iv)				1.000	−0.266	−0.756
(v)					1.000	0.811
(vi)						1.000

Notes: (i) Unemployment rate, (ii) separation rate, (iii) total job finding rate, (iv) job finding rate of new hires, (v) probability of recall, (vi) recall rate (share of recalls out of all hires). The model is solved and simulated at weekly frequency. The aggregate monthly time series are constructed by sampling the data every four weeks (month). The statistics are based on logged and HP filtered quarterly time series. The smoothing parameter for the HP filtering is set to 1,600.

each other. On the one hand, lower aggregate productivity reduces labor demand and thus also lowers the recall probability. On the other hand, larger endogenous separation flows in the recession period create a larger pool of "recallable" workers in the unemployment pool, who are likely to be recalled for idiosyncratic reasons even if the economy does not improve. Importantly, the acyclical recall probability and the strongly procyclical job finding rate for new hires imply that the recall rate is higher in recessions. That is, job finding of new hires slows down sharply during recessions, while the measured recall probability is held roughly constant, and these two observations imply that the share of recalls increases during recessions. The model generates this key observation we highlighted in the earlier section despite its fairly parsimonious structure.

7 Conclusions

In this paper, we document that US workers who separate from their jobs have an exceptionally high probability of going back to the same employer and that the share of recalls out of all hires is countercyclical. These recalls entail both workers on temporary layoff and permanently separated workers. Recall is more likely the longer the worker had spent at that employer before separation and is associated with dramatically different outcomes in terms of unemployment duration (both the level and shape of hazard), post-re-employment wages and occupational mobility. Recalls are relatively stable over the business cycle, so that the hazard rate of exit from unemployment to new jobs is even more volatile and the importance

of vacancies in the matching process is even more significant than previously estimated. A relatively modest modification to the canonical Mortensen and Pissarides (1994) model of unemployment, embedded in a business cycle framework, captures well these empirical patterns through selection of workers to be recalled.

We believe that these findings cast our knowledge of the aggregate labor market under a somewhat different light. In future work we will explore in greater detail the implications of our empirical findings for the importance of firm- and occupation-specific human capital. We will also revisit more deeply, under the lens of our new stochastic search-and-matching model with recall, classic questions in this field, such as the cyclical volatility of unemployment, the unobserved heterogeneity between short- and long-term unemployment, and the implications of establishment closings on earnings prospects of the displaced workers who lose the recall option.

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A Imputation Procedure

As mentioned in the text, the imputation of the missing recalls is performed separately for short spells (non-employment duration of one or two months) and for longer spells (non-employment duration of three months or longer).

A.1 Long Spells

The imputation of the longer spells is based on a logit regression that predicts recall outcomes using the following variables:

- Age, age^2.

- Education categories: less than high school, high school graduate, some college, and college degree.

- Gender dummy, union dummy at initial employment, and employer-provided health care (EPHC) dummy at initial employment

- Address change dummy, union status change dummy, EPHC change dummy.

- Non-employment duration categories: $3-6$ months, $7-9$ months, $10-12$ months, 13 months or longer. We find that using non-employment duration as a categorical variable (instead of a continuous variable) helps improve the fit of the imputation regression.

- Occupation switch and industry switch dummies. Both switches are based on the three-digit level classification. Interaction of the two switching dummies are also included.

- Initial occupation and industry dummies. Occupation is classified into 79 categories and industry is classified into 44 categories.

- Log wage level at initial employment.

- Log wage change between initial and last employment. The change is captured as a categorical variable based on the following intervals: $(\infty, -0.5]$, $(-0.5, -0.05]$, $(-0.05, 0.03]$, $(0.03, 0.5]$, $(0.5, \infty]$. We find that categorizing log wage changes into bins (instead of using the log wage change itself) improves the fit of the imputation regression. The basic idea is that a large wage change (whether positive or negative) strongly predicts non-recall. However, we also find that negative and positive wage changes predict slightly different probabilities of recall/non-recall and thus positive and negative changes are treated separately. The middle category is centered around a negative value because the average wage change of all observation is negative.

- National unemployment rate: This to control for the aggregate labor market condition.

- Month-of-separation dummies. This is to control for seasonality.

The reference sample for the long spells is all observations from 1990-1993 panels. All observations within the same long spell category in 1996-2008 panels are imputed from this logit regression. The Pseudo R^2 of the regression is 0.3054.

Table 13: Recall Rates: Separations Occurred in the First Year or Two Years of Each Panel (Pre-Imputation)

Panel	Separations in waves	$E\not{E}$		$E\not{E}\cdots\not{E}E$	
		Recall rates	Counts	Recall rates	Counts
1990	1-3	0.264	4,695	0.371	3,325
1991	1-3	0.303	3,272	0.423	2,310
1992	1-3	0.293	3,975	0.407	2,827
1993	1-3	0.286	3,670	0.398	2,587
1996	1-6	0.146	11,039	0.189	8,350
2001	1-3	0.158	5,276	0.209	3,906
2004	1-6	0.167	5,175	0.226	3,731
2008	1-3	0.183	5,473	0.264	3,724

Notes: Source, SIPP. Third column gives the number of recalls relative to all separations into non-employment, denoted by $\not{E}$ (including unemployment and inactivity). Fifth column gives the number of recalls relative to all the spells that end with employment. The results for the 2008 panel are based on the observations up to wave 10.

A.2 Short Spells

Within the short spells (with one or two months of non-employment duration) in 1996-2008 panels, the spells that occur within a wave are reliable. Further, when labor market status is reported to be TL, we trust the recall/no recall indicator. In the remaining sample, the spells that occur across a wave, we assume that those with an occupation switch are non-recall while those that report the same occupation are imputed by running a logit regression. The reference sample for this regression is within-wave spells in the 1996-2008 panels. The regression uses basically the same variables as above with a few differences. First, we do not use occupation and industry switch dummies (the sample is only for occupation stayers). Second, initial occupation and industry dummies (a total of 123 dummies) are dropped to maintain the efficiency of the estimation, given that this sample has a fewer observations. Third, we also use a labor market status variable.[10] Lastly, we also add panel dummies. We add this variable because the short spells are imputed within the 1996-2008 panels. The Pseudo R^2 of the regression is 0.3707.

A.3 Multiple Imputation

After estimating the logit regressions, we simulate discrete recall outcomes (0 or 1) for all spells with unreliable recall outcomes, based on the predicted probabilities. We repeat this

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[10]We could not use the labor market status variable for the imputation of the long spells, because the labor market status variable is not consistent between the 1990-1993 panels and 1996-2008 panels.

process 50 times. All calculations that use imputed recall outcomes are averages of these 50 replications.

B   Pre-Imputation Data

Table 2 in the main text presented the recall rates for separations occurred in the first year or two years of each panel. Table 13 considers the corresponding recall rates based on the pre-imputation data (raw data). One can see sudden drops the recall rates at the 1996 panel.