RECALL AND UNEMPLOYMENT

Shigeru Fujita
Giuseppe Moscarini

ABSTRACT

Using data from the Survey of Income and Program Participation (SIPP) covering 1990-2011, we document that a surprisingly large number of workers return to their previous employer after a jobless spell and experience more favorable labor market outcomes than job switchers. Over 40% of all workers separating into unemployment regain employment at their previous employer; over a fifth of them are permanently separated workers who did not have any expectation of recall, unlike those on temporary layoff. Recalls are associated with much shorter unemployment duration and better wage changes. Negative duration dependence of unemployment nearly disappears once recalls are excluded. We also find that the probability of finding a new job is more procyclical and volatile than the probability of a recall. Incorporating this fact into an empirical matching function significantly alters its estimated elasticity and the time-series behavior of matching efficiency, especially during the Great Recession. We develop a canonical search-and-matching model with a recall option where new matches are mediated by a matching function, while recalls are free and triggered both by aggregate and job-specific shocks. 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.

Shigeru Fujita
Research Department
Federal Reserve Bank of Philadelphia
Ten Independence Mall
Philadelphia, PA 19106
shigeru.fujita@phil.frb.org

Giuseppe Moscarini
Yale University
Department of Economics
PO Box 208268
New Haven, CT 06520-8268
and NBER
giuseppe.moscarini@yale.edu

1 Introduction

Unemployment is commonly understood to be a state of job search, and is measured accordingly. Due to informational imperfections, jobless individuals do not immediately find the kind of employment that they desire and that the market offers somewhere. One leading interpretation of these search frictions is that jobs and workers are extremely heterogeneous, so unemployed workers need time and effort to locate and arrange suitable jobs. The relevant dimensions of job heterogeneity include pay, schedule, location, task, work environment, and many others. If, however, a worker who separates from an employer and goes through a jobless spell eventually returns to work there, then much of this heterogeneity may be irrelevant, since employer and employee 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 in the US labor market are surprisingly common: Over 40% of the employed workers who separate into unemployment (EU flow) return, after the jobless spell, to their last employer. This share of the inflow into unemployment, which we will refer to as the "recall rate," significantly exceeds the fraction of the same EU flow that is due to Temporary Layoffs (from now on: TL), namely, workers who report being laid off with a recall date or expectation. In other words, recalls are much more pervasive than TL. The reason is that, even within the group of "Permanently Separated" (PS) workers—those who lose their job with no indication of a recall, and start looking for another job—about 20% are eventually recalled by their last employer. The recall rate is even higher, over 50%, for the workers who remain unemployed until they find a job (without dropping out of the labor force, EUE spells). It is still substantial, about 30%, for all separated workers, including those who leave the labor force, either immediately after separation, such as retirees, or after some unsuccessful job search, i.e., discouraged workers.

To study the implications of recall for individual labor market experiences we then restrict our attention to more "attached" job losers, those who complete their unemployment spell without leaving the labor force (EUE spells), so that we can compare pre- and post-unemployment outcomes, with and without recall. Recalled workers had been with their last employer on average twice as long before separation (6 vs. 3 years of tenure), 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 drastically more favorable real wage outcome. Conversely, among TL workers, the few who are not recalled experience significant wage gains, presumably because the option value of recall raises their reservation wage to accept a new job offer. Finally, we observe negative unemployment duration dependence only for those who are eventually recalled: the hazard rate of exit from unemployment to a

new employer is almost constant over unemployment duration. Importantly, this feature of the data holds even when we consider all separations into unemployment (EU flow rather than EUE complete unemployment spells), including those who end up leaving the labor force.

Next, we draw the implications of recall for cyclical unemployment. In recessions, the probability that an unemployed worker is recalled drops, but much less than the probability that he finds a new employer; therefore, the recall rate rises, and so does the share of recalls out of all hires (the outflow from unemployment). The increase was especially sharp during the Great Recession. 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 search frictions, as suggested by common sense and by the evidence on tenure, occupation switching and wage outcomes that we just summarized, then recalls cannot be treated as the output of a matching function. To quantify the effects of the large recall rate and of its countercyclical fluctuations, we estimate the matching function under the novel assumption that the frictional matching process that this function represents does not mediate recalls, so we exclude recalls from hires (the dependent variable). This modification leads to significant changes in the estimated elasticity and the time-series behavior of the regression residual, which is commonly interpreted as a measure of matching efficiency. Compared with the traditional estimation result, our corrected measure of matching efficiency suggests that labor market "mismatch" was considerably larger during the Great Recession and smaller in its aftermath.

To better understand the economic mechanisms behind the new empirical evidence, we introduce a recall option in the standard search-and-matching model of the labor market à la Mortensen and Pissarides (1994). Jobs are hit by idiosyncratic and aggregate productivity shocks, which give rise to endogenous separations. Our new assumption is that, after separation, the productivity of the match keeps 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 and/or idiosyncratic components of match productivity. Recall is free and instantaneous for both parties. In contrast, new firms and pre-existing firms, which either cannot or do not want to recall a previous employee, must pay a cost to post a vacancy and to search for a new worker.

After an endogenous separation, the firm can keep the vacant position indefinitely open at no cost, hoping for conditions to improve and trigger a recall. If the firm wants to hire new workers, it can always create new vacancies. Constant returns ensure that recall and new job creation decisions are made independently. Thus, a separated worker does not need

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to be concerned about being replaced in his old job by a new hire. Conversely, the worker can only work for one employer at each point in time. We limit the scope of recall to the last match by assuming that, when a separated worker accepts a new job, the previous match can no longer be recalled. Hence, a firm must be concerned about losing a former employee to a new employer. The probability of this event, which is a type of irreversible negative match quality shock, is endogenous to the economy, because it is the (new-)job-finding probability. Thus, as in the standard stochastic search and matching model, this probability is the key equilibrium object in our model and tracking its evolution poses no additional complications.

We assume that wages share match surplus, and find a simple equilibrium where the option value of recall affects neither the probability of accepting a new job nor the wage this pays. The only relevant state variables are the exogenous productivity components ε and p.

We calibrate the model to match steady-state moments computed from our microeconomic evidence. In particular, we estimate by a simulated method of moments the parameters of the idiosyncratic shock process, which is the engine of turnover and recalls. We find that the calibrated model matches qualitatively all the cross-sectional and aggregate/cyclical facts highlighted. Even the quantitative performance is decent, given the parsimonious structure of the model. On the micro dimension, the hazard rate of recalls declines with unemployment duration, as we observe in the data, due to dynamic selection: The longer the worker stays unemployed without being recalled, the more likely it is that the (unobserved and persistent) quality of his previous match has deteriorated since separation, hence the less likely that a recall is forthcoming. From a macro viewpoint, the recall rate in the model is countercyclical, as in our data, for the same reasons. The introduction of recalls amplifies the response of vacancy creation and thus unemployment to aggregate shocks.

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 empirical evidence on recalls. In Section 4 we examine the effects of the cyclicality of the recall rate on the estimation of a matching function. In Section 5, we lay out our search-and-matching model with recall and analyze its quantitative performance.

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 and 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, and

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 the underlying upward-sloping or flat exit hazard 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 of survey participants (75%) said that they expected to be recalled, although much fewer workers (18%) had a definitive recall date; 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 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's competing hazard exercise without information on subjective recall expectations, but controlling for unobserved heterogeneity.

Our sample is based on the SIPP, which covers the entire US labor force for over 20 years, not only UI recipients and/or a single deep recession. Our measure of recall expectation is based on the formal definition of TL and we find that a significant fraction of those who say that the previous employer did not indicate a possible recall date (i.e., PS workers) end up being recalled nonetheless.[1] In comparison to this microeconomic literature, we confirm and extend the empirical relationship of recall with tenure, exit from unemployment, and wages. We also show, however, that the strongest relationship is with occupational mobility.

Recall plays an even smaller role in the macroeconomic 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 the SIPP, only count permanent separations that do not result in a recall within four months, and target an average unemployment rate of 6%. This strategy presumably (although 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 to engage in costly search, that is, whether recall and search interact, in which case the calibration strategy in Bils et al. (2011) is potentially problematic. In addition, we show that Bils et al.'s choice of a four month unemployment duration cutoff to define a recall leads to significantly underestimate true recalls, because of data issues in the SIPP that we discovered and discuss in detail.

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[1]In order for the worker to be classified as TL in the official data such as the Current Population Survey (CPS), the worker must either have been given a date to report back to work or, if not given a date, expect to be recalled to his/her last job within 6 months. This definition is likely to be more stringent than the recall expectation measured in the data Katz and Meyer (1990) used.

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 with 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. We also aim to match our new facts about unemployment duration dependence 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 this is the result of composition effects in the unemployment pool, or rather all types of unemployed workers experience cyclical job finding opportunities. He finds that, among all observable worker characteristics in the CPS, the best case for the heterogeneity hypothesis can be made when breaking down the unemployed between TL and PS, as their proportions are 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 from unemployment, recall vs. different employer, as opposed to entry, TL vs. PS. We argue that this heterogeneity is quantitatively important for matching function estimation.

Shimer (2012) leaves open the possibility of sizable composition effects in terms of unobservable worker characteristics. In order to investigate this hypothesis directly, one needs high-frequency longitudinal data with multiple unemployment spells to extract some sort of 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 each SIPP panel also has too short a time dimension to cover multiple 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 in the CPS, and that the cyclicality of the job-finding rates of the long-term unemployment "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 job loser expects to be recalled by his last employer and, whether he expects it or not, returns to his last employer. That is, we examine the importance of recalls for flows both into and out of unemployment. We then discuss the economic implications of recall.

While our new results are from the SIPP, we first revisit evidence from the CPS to present what is possible to learn there, and why the 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 official source of labor market information, including the unemployment rate, and TL are also measured in the SIPP, it is useful to compare observations on TL in the two surveys and then focus on recalls in the SIPP.[2]

Labor market researchers paid decreasing attention to recalls, due to the observed decline 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. 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 into and out of unemployment than of the stock. The reason for the stock-flow discrepancy is that TL spend much less time in the unemployment pool than average. So, if one is interested in worker flows, TL still matter, even today. Second, and more importantly, TL are only part of the story. We show that in the 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 significant 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.

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[2]See footnote 1 for the formal definition of TL.

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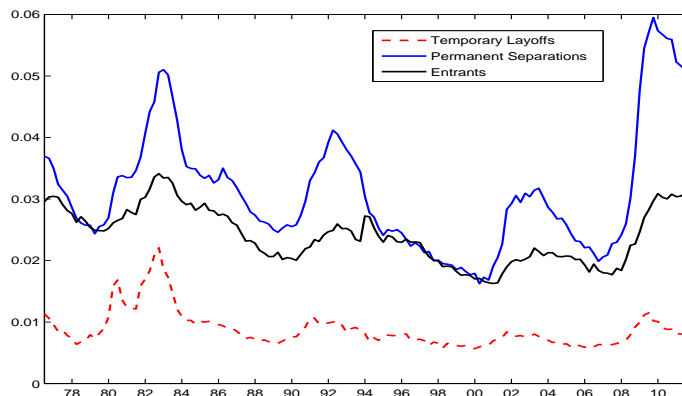
Notes: Source, Monthly CPS. Expressed as a fraction of the total labor force.

We now elaborate on these points in detail.

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. Neither 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. We treat group (i) 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 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 of 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. Moreover, the increase in TL during the last three recessions has been modest.

3.1.2 Flows in and out of Unemployment by Reason

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

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Figure 2: Unemployment Entry and Exit by Reason: Duration Data

(a) Entry Probability

(b) Exit Probability
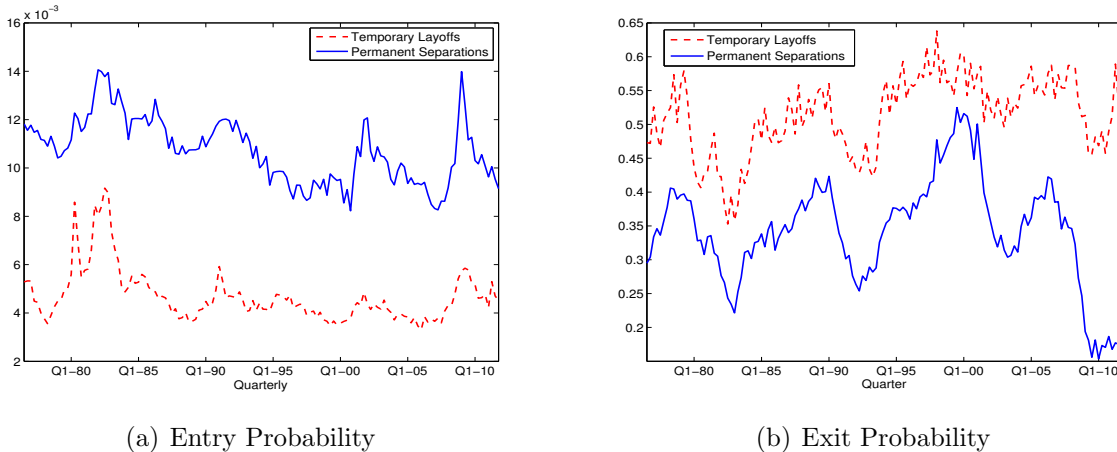
Notes: Source, Monthly CPS. Short-term unemployment: unemployed less than 5 weeks. Short-term unemployment is expressed as the fraction of the total employment stock.

fact that TL quickly exit from the unemployment pool.

Figure 2 shows quarterly averages of monthly probabilities of entry into and exit out of unemployment, which we infer by using short-term unemployment (less than 5 weeks) as in Shimer (2012). In each panel, we show probabilities by type of inflow, TL and PS.[3] In panel (a), the TL inflow amounts to slightly less than one half of the PS inflow, and the two move more or less in parallel over business cycles, with a marked countercyclical pattern. In panel (b), workers on TL enjoy a much higher job-finding probability than PS workers; note also that both series exhibit the familiar procyclicality, but this is more pronounced for PS workers. During the post Great Recession recovery, the UE probability recovered for TL but not for PS workers.

In the Appendix, we provide supplementary evidence that further corroborates this over-all picture. First, we report monthly transition probabilities between employment and un-employment derived from the monthly CPS matched records. Unlike the results based on short-term unemployment, this method allows to distinguish between exit from unemploy-ment to employment, as opposed to non-participation. There we focus on the data after the 1994 CPS redesign, after which the measurement of TL and PS is consistent. For the overlapping 1994-2012 period, we obtain the same qualitative patterns: entry rates are coun-tercyclical, and about double for PS than TL, and the opposite is true of the exit rate, which is also much more cyclical for PS, and never recovered after 2008, unlike for TL. As a con-sequence, we show that the median duration of unemployment is much higher and more

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[3]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).

cyclical for PS (as well as for entrants) than for TL. Overall, TL experience shorter and less cyclical unemployment spells.

Second, we study the industry composition and seasonality of TL. We find that TL are not concentrated in a particular sector (e.g., manufacturing), but are common is most major sectors. TL show a seasonal pattern, which is not synchronized across sectors. Therefore, some of this seasonality cancels out in the aggregate TL flows, but does affect its average level. Figure 2, which plots seasonally-adjusted data, demonstrates that there are also non-seasonal,business cycle variations in separation and job-finding probabilities associated with TL. Similarly, in our main analysis based on the SIPP, we will find that the share of hires from unemployment that are recalls, whether from TL or not, exhibits a countercyclical pattern. Therefore, TL and recalls are not simply a seasonal phenomenon.

3.2 Facts from the SIPP

We now present our main empirical findings on recalls from the Survey of Income and Program Participation. Again the biggest advantage of the 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

The SIPP is a collection of panels. The following eight 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 between the first four 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 A.1 in the Appendix summarizes each panel's length and the period covered.[4]

We drop individuals who miss any wave of the panel. Therefore, individuals in our sample, in principle, have complete history over the entire panel. The Census Bureau provides population weights, called longitudinal weights, specifically calculated for the panel, making this sample nationally representative. We also exclude so-called type-Z imputed observations from our analysis.[5] After applying these sample selection criteria, we identify spells that start

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[4]The 2008 panel has 13 waves. But our results are based on the data up to wave 10, which was the latest data available at the time of our analysis.

[5]We thank Martha Stinson for suggesting this conservative procedure. The type-Z respondents are ones who answered very few questions of the survey and thus have many of their responses imputed. The concern is that the type-Z respondents spuriously have higher recall rates, thus biasing the aggregate recall rate upward. Our results are not actually affected by the inclusion of these observations. However, we believe that excluding them is a prudent practice. Dropping the type-Z observations reduced the sample size for

with employment followed by non-employment and then again by employment ($E\not{E}E$).

To make sure that the relative short duration of each panel does not affect our results through censoring of jobless spells, we further restrict our sample to those cases in which the transition into non-employment (i.e., 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. This ensures that each subsequent non-employment spell could last at least roughly 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 out of the labor force) in the SIPP in a manner similar to the CPS: we classify workers who separate from their job into unemployment in two groups, TL and PS, as we did in the CPS. Unfortunately, the classification of labor market status prior to the 1996 SIPP redesign does not appear to be consistent with the CPS: the share of those who report TL among those who are unemployed throughout the non-employment spell in our EUE sample from the SIPP was roughly 20%, as opposed to 35% in the CPS. After the 1996 redesign, the SIPP applied a more precise definition of TL, raising this share significantly. Therefore, whenever we condition our analysis on labor market status, we focus on post-1996 data.

The SIPP assigns a unique job id to each employer for each worker. Therefore, when a worker returns to the last employer after a jobless spell, we can identify this event as a "recall". 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 makes it possible for us to correctly identify recalls in these early panels. We therefore view the aggregate recall rate of all separated workers from the 1990-1993 panels, covering years 1990-1995, as completely reliable.

The identification of recalls in the 1996-2008 SIPP panels is subject to two important sources of measurement error, both leading to significantly underestimate the incidence of recalls. First, we discover a "seam effect" in the SIPP. 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) and thus is reported at once in the same interview. In the other

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our analysis roughly by 7-8%.

cases, initial and final employment in the spell belong to different waves and are reported in two different (consecutive) interviews, four months apart. Whether a spell crosses the "seam" between waves or not should be a completely random event. In the SIPP, however, after 1996 the recall rate of the PS 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 spells that last more than two months, are underestimated. As we discuss below, we use information from within-wave spells after 1996 to impute these recalls that are missing due to the seam effect.

Second, after the 1996 redesign, the SIPP drops the job id if the worker reports being jobless for the entire wave (4-month interval between interviews). Thus, we miss recalls *altogether* when a worker returns to the same employer after a long jobless spell, spent either looking for a new job or out of the labor force. One important exception is when a worker is on TL, in which case the 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 post-1996 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.

We perform the imputation of recalls after 1996 separately for the short spells (one or two months of non-employment) that cross the wave seam and for the long spells (three or more months of non-employment). For each of the two groups, we use a "reference sample" to estimate a logit regression that predicts recall given observable spell characteristics, such as non-employment duration, switching of occupation, etc. We then impute the missing recalls in the 1996-2008 panels. Going in reverse order, the reference sample of the long jobless spells, whose job ids are dropped by the SIPP and thus cannot be measurably recalled, unless they are on TL, is the analogous sample of long-term nonemployed in the 1990-1993 panels. Because labor market status, particularly TL or PS, is not compatible between pre- and post-1996 panels, we do not use information about this status in the estimation. Hence, we also impute recalls for those on TL (even though their job ids are measured accurately) to avoid selection by unemployment status, which is obviously non-random and likely correlated with recalls.

For the short spells that are TL, we can trust the information on recalls, regardless of whether or not spells cross a seam. For non TL, we trust within-wave spells, as mentioned above. We apply the following imputation procedure for recalls following short and cross-

Table 1: Recall Rates: Separations Occurred in the First Year or Two Years of Each Panel

Panel	Separations in waves	$E\not\!E$		$E\not\!E E$	
		Recall rates	Counts	Recall rates	Counts
1990	1−3	0.298	4,176	0.371	3,325
1991	1−3	0.343	2,870	0.423	2,310
1992	1−3	0.330	3,515	0.407	2,827
1993	1−3	0.324	3,220	0.398	2,587
1996	1−6	0.268	10,032	0.317	8,341
2001	1−3	0.270	4,807	0.328	3,904
2004	1−6	0.274	4,570	0.329	3,730
2008	1−3	0.325	4,821	0.413	3,718

Notes: Source, SIPP. Number of recalls relative to all separations into non-employment, denoted by $E\not\!E$ (including unemployment and inactivity), and relative to all the spells that end with employment, denoted by $E\not\!E E$.

wave jobless spells that do not start on TL. For these short spells, the strongest predictor of recall is occupational mobility. Thus, we impute recalls of the occupational stayers using the regression where the reference sample is within-wave occupation stayers in the same post-1996 panels. Here, we can use labor market status variable (PS or out of the labor force), since we only exploit the post-1996 data. Next, if we observe an occupational switch after crossing a seam, we directly mark no recall. This conservative choice follows from the observation that, among these short spells, over 99% of the occupational switchers in the pre-1996 panels and 100% (that do not cross a seam) in the post-1996 panels are not recalled. In the Appendix we provide details of the imputation procedure, as well as diagnostic evidence of its quality, based on an "in-sample forecast." We first discard valid recall records, re-impute them, and then compare the imputed records with the true records; we recover true recall rates nearly perfectly.

3.2.2 Recall Rates

Table 1 presents recall rates, namely shares of the relevant non-employment spells that end in a recall. Our main results are in the fifth column, where we consider all $E\not\!E E$ spells that begin and end with measured employment. In the third column, we also report recall rates including separations that do not end with employment within the period covered in each panel (denoted by $E\not\!E$). For example, a separation occurs in the first year of a panel and the worker continues to be jobless 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

Table 2: Recall Rates: Hires Occurred in the Last Year or Two Years of Each Panel

Panel	Hires in waves	$\not{E}E$		$E\not{E}E$	
		Recall rates	Counts	Recall rates	Counts
1990	7−9	0.349	4,469	0.415	3,698
1991	7−9	0.302	2,948	0.381	2,325
1992	7−9	0.287	3,757	0.361	2,962
1993	7−9	0.302	3,522	0.378	2,778
1996	7−12	0.260	10,008	0.308	8,315
2001	7−9	0.281	4,365	0.336	3,602
2004	7−12	0.249	4,267	0.304	3,448
2008	8−10	0.299	4,292	0.391	3,235

Notes: Source, SIPP. Number of recalls relative to all hires from non-employment, denoted by $\not{E}E$, and relative to all the spells that end with employment, denoted by $E\not{E}E$.

that this treatment only reduces the recall rate.

One can immediately see that recall rates are surprisingly high, regardless of which panel we look at. Even relative to all separations ($E\not{E}$ spells), close to 30% return to the same employer. Here our focus is on cross-sectional facts but it is interesting to note that recall rates increased significantly 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. In Section 4, we will look more closely at the business cycle facts. We also explore the underlying mechanism of the countercyclical recall rate by using our model.

Table 2 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. Note that a similar time series pattern, particularly the increase in the recall rate in the 2008 panel, can be observed here as well.[6]

As mentioned before, the aggregate recall rate of all separated workers is measured accurately in the 1990-1993 SIPP panels, while recalls in the later panels are under-reported. We

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[6] Information on recalls is also available from the Quarterly Workforce Indicators (QWI), which are publicly available aggregates from the LEHD matched employer-employee dataset at the Census. Data are available from most, yet not all, U.S. states, starting at different dates by state, after 1996 and through 2012. One major limitation of QWI for our purposes, and indeed for any study of worker turnover, is that the underlying LEHD records are collated from quarterly snapshots and tend to miss within-quarter spells of employment or unemployment. Because most recalls are quick, this time aggregation may miss many recall episodes. In fact, the share of hires that are recalls in the QWI is 16%, which is still quite significant, but just over half as large as in the SIPP (see Table 2).

Table 3: Recall Rates: Separations into Unemployment Occurred in the First Year or Two Years of Each Panel

Panel	Separations in waves	EU		EUE	
		Recall rates	Counts	Recall rates	Counts
1996	1−6	0.399	4,133	0.448	3,384
2001	1−3	0.388	1,983	0.456	1,553
2004	1−6	0.424	1,770	0.490	1,369
2008	1−3	0.428	2,805	0.528	2,093

Notes: Source, SIPP. Number of recalls relative to all separations into unemployment, denoted by EU, and to all the spells that end with employment, denoted by EUE.

suspect that, even after our best attempt at imputation, recalls may still be underestimated since 1996. Indeed, Tables 1 and 2 show a significant drop in recall rates of separations and hires between the 1993 panel (covering 1993-1995) and the 1996 panel (1996-1999) even after imputation, which thus appears to fill only part of the time series break in the original data, reported in Table A.2 in the Appendix. From now on, we report evidence conditioning on variables, primarily employment status, that are reliably available only after the 1996 SIPP re-design. Therefore, the rest of our analysis concerns mostly the 1996-2008 panels.

Table 3 restricts attention to those who are separated and first flow into the unemployment pool. The fifth column (EUE) focuses on the completed unemployment spells, the cases where the worker remains unemployed (without leaving the labor force) until he/she gains employment. The third column (EU) adds to the EUE sample the cases where the worker stays in the unemployment pool until the end of the panel and where the worker drops out of the labor force after being unemployed initially. Note that we treat the former case as non-recall, even though we do not know if they returned to the same employer after the end of the panel. However, in the latter case, it is possible to see if the worker was recalled or found a new job unless the worker is nonemployed until the end of the panel.[7]

Table 4 splits the EUE sample into two groups by reason for unemployment, TL and PS. First note that, because this table is only about completed unemployment spells, the share of TL, which is over 40%, is significantly larger than that in the EU separation flow (see Figure 2). This makes sense, given that PS workers are much more likely than TL to leave the labor force and not to complete the unemployment spell by returning to employment within two years. As expected, the recall rate for TL workers is very high, much higher

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[7]For example, suppose a worker has a history $EUNNUE$, where N denotes Non-participation in the labor force. This case, because he/she drops out of the labor force in the third month, is not part of the EUE sample, but rather of the EU sample. We can identify a recall in this case and thus treat it as such.

Table 4: 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	0.844	1,481	0.172	1,903
2001	1−3	0.868	678	0.168	875
2004	1−6	0.864	663	0.176	706
2008	1−3	0.874	996	0.232	1,097

Notes: Source, SIPP. The EUE sample in Table 3 is split here into two groups based on the reason of unemployment in the first month of the unemployment spell.

than for PS workers. This is true in all panels. However, and more importantly, even among PS workers, the recall rate is substantial: nearly 20% of workers who do not have, upon separation, an expectation of recall nevertheless return to the same employer.

Having documented that recalls are remarkably frequent in the US labor market, we now ask what difference a recall makes to the worker's experience.

3.2.3 Recall and Unemployment Duration

Table 5 summarizes the information about unemployment duration in the EUE sample of completed unemployment spells. We calculate mean, standard deviation, and median duration for those who are recalled and those who join a different employer. First note that recalls occur sooner than hires by a new employer. Similarly, the dispersion of unemployment duration is smaller for those who are eventually recalled. Average duration is clearly countercyclical. In the 1996 and 2004 panels, which cover only expansion years, mean duration is 2.50 and 2.48 months, respectively. On the other hand, it is higher at 2.65 months in the 2001 panel, which includes a shallow recession, and at 4 months in the 2008 panel, which covers the Great Recession and the subsequent anemic recovery. This overall pattern is consistent with the well-known evidence in the monthly CPS. Interestingly, however, the increase in average duration is more than twice as large for non-recalls than for recalls. Similarly, from the standard deviation, the dispersion of unemployment duration across workers is countercyclical, and the countercyclicality is especially pronounced among non-recall hires. The pattern here therefore highlights an important heterogeneity between recall and new hires that is hidden at the aggregate level.

3.2.4 Hazard Functions

Figure 3 plots the discrete hazard functions, calculated nonparametrically, for exit from unemployment by duration. The sample includes all separations into unemployment (i.e.,

Table 5: Unemployment Duration

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	1.94	1.00	3.06	3.01	2.00
2004	1−6	2.48	2.35	2.00	2.08	1.72	1.00	2.87	2.78	2.00
2008	1−3	4.00	4.84	2.00	2.80	3.26	2.00	5.34	5.85	3.00

Notes: Source, SIPP. Based on the *EUE* sample. See notes to Table 3.

EU sample), including unemployment spells that are not completed before the end of the panel. Specifically, we compute the fraction of unemployed workers, at each duration (month) since they lost their last job, who exit unemployment to a recall (first row), to a new employer (second row), to non-participation (third row), and to any of those (fourth row, which sums the first three rows). The columns condition the hazard on labor market status in month 1 of unemployment, in order: all, TL, and PS. Each color represents a different SIPP panel.
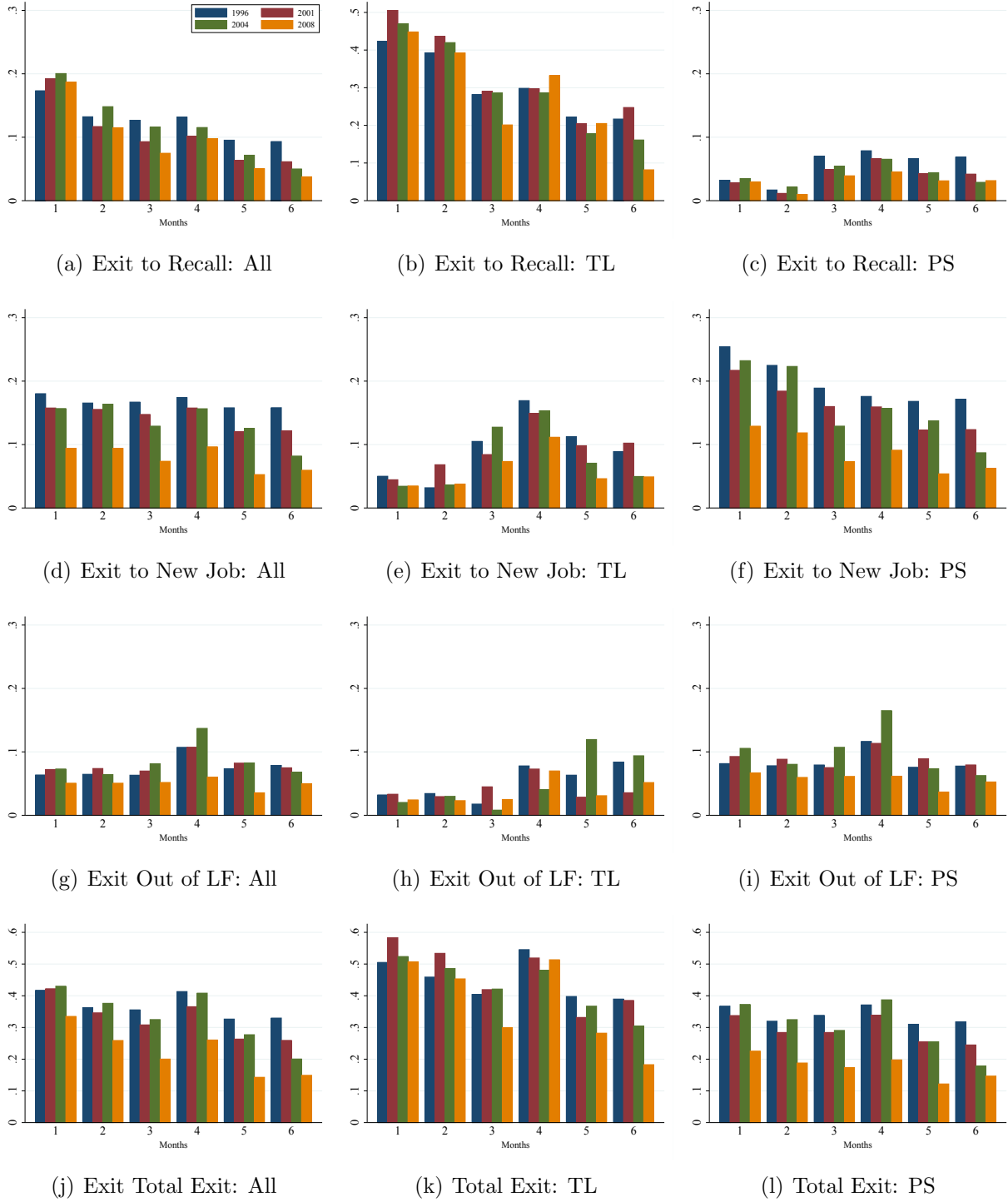
In the first column, from top to bottom row, the exit hazard to different outcomes of job search, there is clear negative duration dependence in recalls, while the hazard function for those who exit unemployment by finding a job at a different employer is weakly hump-shaped and much closer to be flat. Exit to non-participation, what we could call "discouragement," is also flat with duration, with a small bump at four months that probably reflects a seam effect in the SIPP.[8] As a result, overall duration dependence in the last row is only mildly negative and entirely due to the declining chance of a recall as unemployment continues.

In the second column we examine the experience of those who begin the unemployment spell on TL. Their chance of being recalled is initially very high and sharply declines with duration. Their expectation of being recalled is clearly reflected in the next two rows: the exit to other jobs and non-participation is negligible in the first few months of unemployment, and then rises.

In the third column, we examine the experience of those who begin the unemployment spell with no expectation of recall (PS). In the first two months of unemployment, their chance of finding a new job is high and barely declining. After three months, that chance drops, but the chance of a recall rises. Overall, the chance of recall is small but non-negligible,

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[8]Our conjecture is based on the aforementioned under-reporting of recall, and on the following reasoning. Suppose that a worker had a true history $[\cdots E|EUUU|E\cdots]$, where bars denote seams between waves. In the last of those six months, the worker is interviewed and reports information about the previous four months. If he/she reports being unemployed throughout the four months, we will detect a hire after 4 rather than 3 months of unemployment. Because seams are randomly distributed, one quarter of all 3-month completed unemployment spells are vulnerable to this problem. If this hypothesis is correct, a fraction of this quarter of the hazard should be reallocated from 4 to 3 months.

Figure 3: Hazard Functions: 1996−2008 Panels

(a) Exit to Recall: All

(b) Exit to Recall: TL

(c) Exit to Recall: PS

(d) Exit to New Job: All

(e) Exit to New Job: TL

(f) Exit to New Job: PS

(g) Exit Out of LF: All

(h) Exit Out of LF: TL

(i) Exit Out of LF: PS

(j) Exit Total Exit: All

(k) Total Exit: TL

(l) Total Exit: PS

Notes: Source, SIPP. Based on the *EU* sample. See also notes to Table 3. Labor market status (PS or TL) is based on the status at the time of separation into unemployment.

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Figure 4: Share of Recalls at Each Duration: 1996-2008 Panels

(a) All   (b) TL   (c) PS
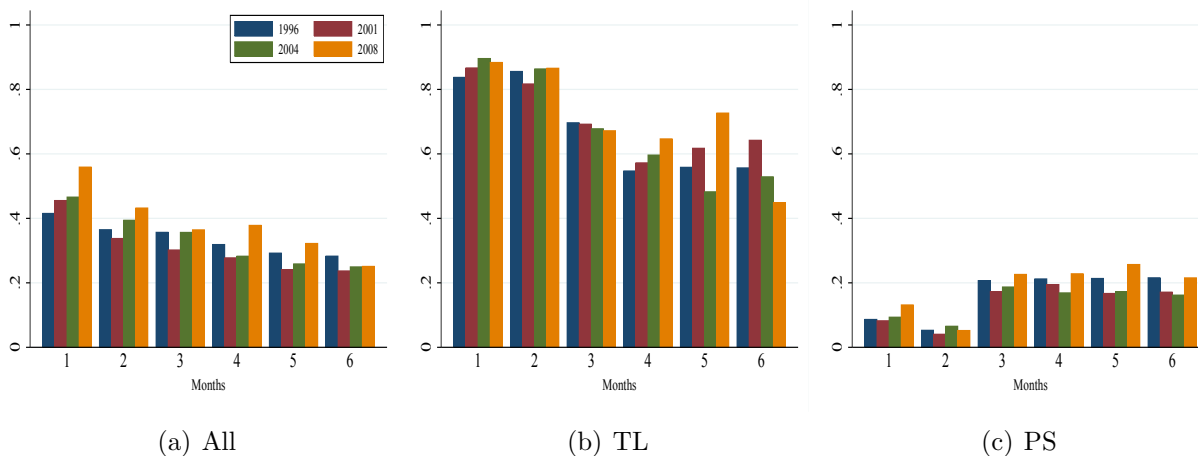
Notes: Source, SIPP. Fraction of recalls at each duration. See also notes to Figure 3

and it appears that PS workers are recalled after they failed to secure a new job quickly. Again, exit to non-participation is flat with a bump at four months and so is overall exit from unemployment.

Figure 4 completes the picture by illustrating the share of hires that are recalls at each duration. As should be clear from Figure 3, this share is declining with duration overall, but only due to the declining chance of recall of TL. 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 is strongly related to recalls. In particular, the heterogeneity between "short-term" and "long-term unemployment types" may be directly related to the expectation/chance of being recalled or not. In turn, this chance depends on worker characteristics, but recall puts some empirical flesh on these unobserved traits.

In all three columns of Figure 3, the effects of high aggregate unemployment show mainly in the probability of finding a new job, which drops dramatically at all durations in the 2008 panel that covers 2008-2011. Exit to non-participation also declines in the 2008 panel, but not nearly as strongly, consistently with the well-known decline in the transition rate into non-participation observed in the CPS during and after the Great Recession. In contrast, the exit rate to recall is barely lower in the 2008 panel for all types of separations. Therefore, the share of recalls in hires is countercyclical, as clear in Figure 4. From this evidence, it appears that recalls stabilize cyclical fluctuations in the overall job-finding probability, for TL and PS workers alike, and that the probability of finding new jobs is not only lower but also even more cyclical than previously thought. We will further elaborate on this theme below.

Figure 5: Relationship Between Recall Rates and Firm Tenure
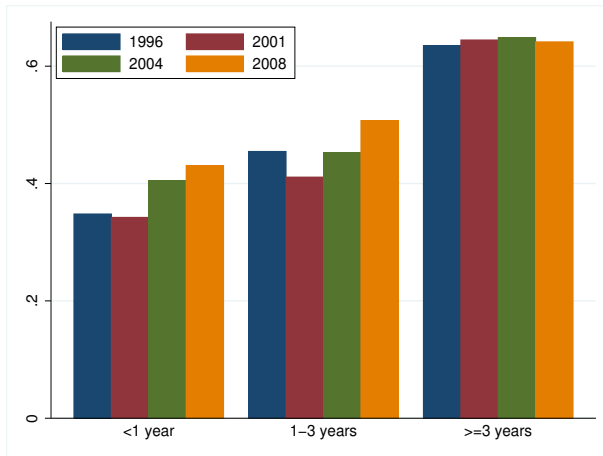
3.2.5 Recall and Employer Tenure

To shed some light on the determinants of recalls, Figure 5 illustrates the relationship between employer tenure before separation and subsequent recall rates. We can see that those who had longer tenure at the time of separation are more likely to be eventually recalled. This pattern makes sense if tenure correlates with match-specific human capital.

3.2.6 Occupation Switches and Wage Changes

Table 6 examines detailed joint frequencies and associated outcomes in terms of the occupation switching probability 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 hire (N). Because we are splitting the sample into the 8 detailed groups, we pool all observations in 1996-2008 panels.

In terms of the probability of each event, the most likely events are the (T,S,R) case, where a worker is on Temporary Layoff, exits unemployment in a Short period of time ($\leq$3 months) and is Recalled, as well as the (P,S,N) and (P,L,N) cases where Permanently separated workers regain employment at a different employer. 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 a continuous unemployment spell always results in a very high probability of occupation switch. This finding is consistent with the result in

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Table 6: Joint Probabilities and Corresponding Outcomes: 1996-2008 Panels

Event 1	Event 2	Event 3	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	2,690	0.325	0.025	0.010
T	S	N	309	0.039	0.647	0.027
T	L	R	363	0.039	0.140	0.028
T	L	N	174	0.022	0.531	0.061
P	S	R	422	0.055	0.241	−0.018
P	S	N	2,394	0.328	0.793	−0.032
P	L	R	428	0.052	0.619	−0.020
P	L	N	1,122	0.139	0.846	−0.118

Notes: Source, SIPP. Based on the sample of EUE 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.

Moscarini and Thomsson (2007), who find a high probability of occupation switch after a direct employer-to-employer 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 occupational mobility. 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 in this case is coded "independently." That is, questions are asked with no reference to the information given in the previous interview, a protocol that is known to inflate switching rates. Therefore, from this part of the table we take the qualitative difference in occupation switching probabilities as informative, but the levels after no recall may be too high.

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. Second, for workers on TL, finding a new job TL results in larger wage gains than recall. 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 large wage loss (over 10%). On the other hand, returning to the previous employer results in a much smaller wage loss (around 2%, whether the recall is quick or not). This fact, combined with

the 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.[9]

In summary, Table 6 demonstrates that "recalls vs. new hires" is an important economic distinction since it is systematically related to workers' economic outcomes. We now will examine the implications of recall for the measurement of aggregate labor market fluctuations.

4 Effects of Recalls on Matching Function Estimation

The matching function approach to 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 search frictions. Our evidence that recalls associate with shorter jobless spells, lower occupational mobility and better wage outcomes corroborates this assumption. In this section, we assess the biases that 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 and 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 and in the measurement of matching efficiency, as captured by the residual term of the matching function.

4.1 Matching Function Estimation

First, let us write all hires H_t as consisting of new matches M_t and recalls R_t:

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

The creation of new matches is subject to search frictions, modeled by a standard Cobb-Douglas matching function:

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

where μ_t is a TFP-like term that can be interpreted as matching efficiency, $\tilde{u}_t$ the number of job seekers, v_t the number of vacancies, and α the elasticity of new hires with respect to

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[9]Katz and Meyer (1990) (Table V) find that, among new job finders, those who expect to be recalled experience a much larger wage loss than those who did not expect a recall. This result is in apparent contradiction with our result that TL workers do better when not recalled, a result that we interpreted as originating from the high option value of recall for those on TL. Remember that Katz and Meyer (1990) focus on UI recipients. They also show that the expectation of a recall in their survey was much more common than TL status (that is, those who reported to have an expectation of recall did not meet the requirements to be classified as TL). Consequently, their sample of "expected to be recalled" may include some of our (P,L,N) cases: workers who lost a job not as TL but only loosely expected to be recalled, woke up late to the reality of no recall, and then faced dire prospects and large wage losses.

vacancies. It is important to note that $\tilde{u}_t$ can potentially be different from unemployment (denoted by u_t) to the extent that some workers, expecting to be recalled, do not undertake job search. In the estimation below, we consider two cases in terms of measurement of $\tilde{u}_t$. In the first case, we assume that $\tilde{u}_t = u_t$ meaning that all unemployed workers search for jobs, as is usually assumed in writing the standard matching function. In the second, we assume that those who are on TL and thus expect to be recalled do not undertake any job search:

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

where u_t^{TL} gives the number of unemployed on TL, which we can directly measure from the CPS. Note that this assumption does not exclude the possibility that those in $\tilde{u}_t$, albeit searching for a new job, are nonetheless recalled. Equations (1) and (2) imply:

$$\ln \frac{H_t}{\tilde{u}_t} + \ln(1 - s_t) = \bar{\mu} + \alpha \ln \left(\frac{v_t}{\tilde{u}_t}\right) + \varepsilon_t, \tag{3}$$

where $s_t = \frac{R_t}{H_t}$ is the share of hires that are recalls, and $\ln \mu_t$ is split into the constant term $\bar{\mu}$ and the de-meaned residual term ε_t, which represents the matching efficiency term. We can measure directly s_t from the SIPP and obtain the remaining time series from the CPS. Thus we can readily estimate Equation (3). One can think of the left-hand side variable as the job-finding probability adjusted for recalls and the explanatory variable on the right-hand side as adjusted market tightness. To see the source of the bias in the standard estimation procedure that omits recalls, rewrite (3) as:

$$\ln \frac{H_t}{u_t} = \bar{\mu} + \alpha \ln \left(\frac{v_t}{u_t}\right) + \tilde{\varepsilon}_t, \tag{4}$$

where

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

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 in (4) is subject to omitted variable bias. Specifically, we document that the recall rate appears to be countercyclical (more precisely, positively correlated with the unemployment rate) and thus $-\ln(1 - s_t)$ is negatively correlated with the tightness series, which is strongly procyclical as well known. Below we will construct a quarterly series for the recall share of hires from unemployment and confirm this more formally. Furthermore, the term $\frac{\tilde{u}_t}{u}$ is countercyclical when $\tilde{u}_t$ is taken to be the number of PS workers (i.e., in the case of $\tilde{u}_t = u_t - u_t^{TL}$) and thus is negatively correlated with tightness. These two correlations imply that the regression on (4) results in an underestimate of the elasticity α.[10] The measurement

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[10]In the specification where $\tilde{u}_t = u_t$, the second correlation is absent. We confirm below that the bias is larger when $\tilde{u}_t = u_t - u_t^{TL}$.

Figure 6: Recall Rates
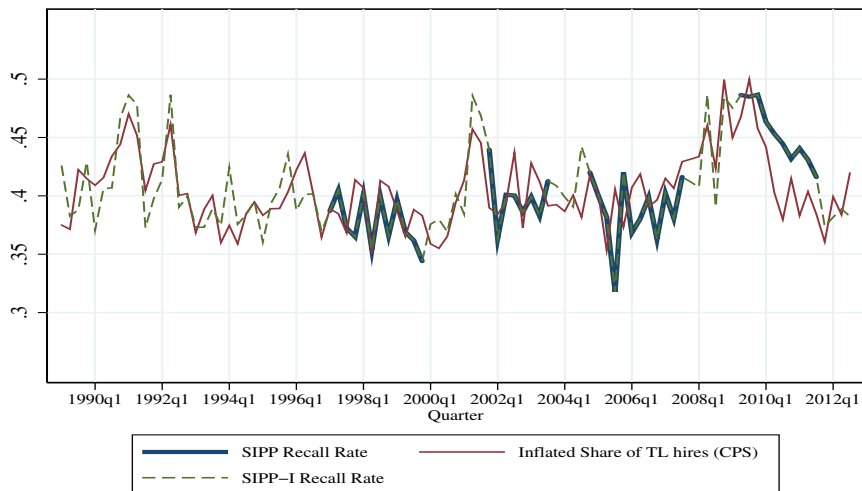
Notes: Share of TL hires is a fraction of UE transitions of those on TL to all UE transitions, inflated by a constant factor of 1.71 to match the level of SIPP recall rate on average. The SIPP-I recall rate series fills the missing observations in the SIPP recall rate based on the regression of the SIPP series on the share of TL hires from the CPS.

of matching efficiency is also biased: True matching efficiency ε_t differs from the one based on the standard estimation procedure (4) 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. We now construct a quarterly recall rate series to measure s_t in (3), the share of recalls out of all hires from unemployment. In Table 2 we reported the share of recalls out of all hires including those from inactivity. To be consistent with the standard matching function estimation, we focus on hires from unemployment. After dropping the observations from the first year of each panel, to avoid the left censoring of EUE spells, from the 1996 panel on, when labor market status (TL vs PS, as well as unemployment vs non-participation) is measured accurately, we end up with only 42 quarterly observations that span 1997Q1 to 2011Q3. We supplement this incomplete time series with information available from the CPS, namely, the share of hires who were on TL out of all UE transitions.[11]

The blue (thick) and red (thin) solid lines in Figure 6 are, respectively, the SIPP-based

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

recall share and the CPS-based TL share of the hiring flow from unemployment. Both series are seasonally adjusted. The latter is inflated by a constant factor (1.71), to exactly match the average SIPP-based recall share, obtained by calculating the average ratio between the two series over the quarters for which both are available. As mentioned, the SIPP-based recall share series starts in 1997Q1 and ends in 2011Q3 with missing observations. 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 share of hires is well approximated by the CPS TL hiring flow. This last assumption holds reasonably well for the overlapping periods, as the correlation between the two series is 0.564 despite high-frequency noise.

We exploit this strong correlation to "fill" gaps in the SIPP-based series. We regress the blue line on the red line, when both are available, and use the estimated regression coefficients to impute the recall share of hires from unemployment, the dashed green line in Figure 6, in quarters when the original SIPP-based series is not available. In the matching function estimation below, we use the TL share of the hiring flow as an approximation to the actual recall share series, since the CPS gives us uninterrupted, longer time series. However, we also consider the estimation using the actual and partially imputed recall share series, which gives similar results. We call the partially imputed series the SIPP-I recall share.

All series in Figure 6 indicate that the recall share increased at the beginning of the Great Recession and then declined thereafter. Moreover, the SIPP recall share 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 the recall share[12]

Panel (a) of Figure 7 plots logged job finding probabilities. The blue solid line applies to all hires, the red solid and green dashed lines exclude recalls. Thus the numerator excludes recalls from UE transitions and the denominator excludes the unemployed on TL. The green dashed line uses the SIPP-I recall share in constructing the numerator while the red solid line uses the inflated TL hiring share. These two series behave similarly. The adjusted probability of finding a new job from unemployment (excluding TL) is the left-hand side variable of Equation (3). The average level of the adjusted series is significantly lower than the unadjusted series with the difference being explained by recalls. Furthermore, while the red and the blue/green series are highly correlated, the fluctuations of the latter adjusted series are more pronounced, for example during the Great Recession. The standard deviation of the HP filtered logged job-finding probability over the period of 1989Q1-2012Q3 confirms the casual observation. It is 7.9% for the standard, unadjusted measure; 10.0%

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[12]In the QWI, the share of all hires from unemployment that are recalls is strongly countercyclical in 1996-2012; we thank Ryan Michaels for putting this evidence together. See also Footnote 6 on the QWI.

Figure 7: Job-Finding Probability and Market Tightness
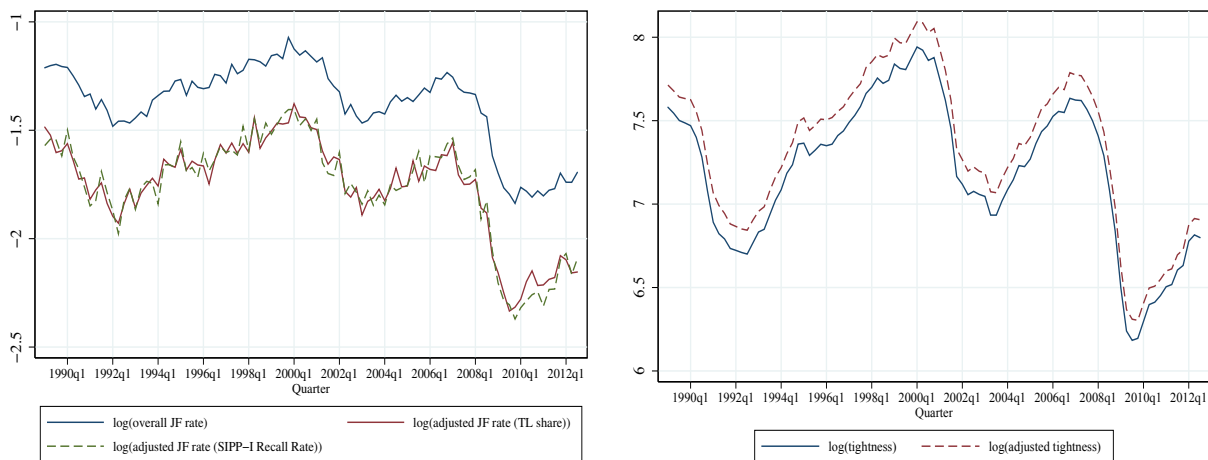
(a) Job-Finding Probability　　　　　(b) Market Tightness

Notes: Overall JF probability: UE transition probability, the explanatory variable of Equation (4). Adjusted JF probability: UE transition probability adjusted for recalls, the explanatory variable of Equation (3), where s_t is approximated either by the inflated share of TL hires in the CPS or the SIPP-I recall rate. Adjusted labor market tightness excludes unemployment due to TL from total unemployment.

for the one adjusted by the CPS TL hiring share; and 11.1% for the one adjusted by the SIPP-I recall share. The larger volatility of the adjusted series mainly comes from the countercyclicality of the recall share, equivalently, the procyclicality of the second term on the left-hand side of Equation (3), $\ln(1 - s_t)$. Intuitively, increases in the recall share of UE transitions during recessions mean that the "true" job-finding probability (i.e, 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 countercyclical and their average exit rate is higher than average, changes in the composition by itself creates the procyclical movements in the av! erage ex it 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), a fact that we confirm below. 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 exit 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 adjusted labor market tightness, where the adjustment

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Table 7: Estimation Results

Specification	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
Elasticity	0.403	0.478	0.448	0.494	0.464	0.427	0.527	0.535
	(0.018)	(0.022)	(0.020)	(0.022)	(0.019)	(0.015)	(0.0148)	(0.016)
Constant	-4.281	-5.340	-5.118	-5.384	-5.160	-4.481	-5.719	-5.713
	(0.137)	(0.165)	(0.145)	(0.164)	(0.142)	(0.111)	(0.108)	(0.122)
Adj-R^2	0.848	0.841	0.837	0.856	0.855	0.945	0.949	0.942
Sample Size	95	95	95	95	95	42	42	42
Adjust recall	No	Yes	Yes	Yes	Yes	No	Yes	Yes
Recall rate	$-$	SIPP-I	TL Share	SIPP-I	TL-Share	$-$	SIPP	SIPP
Search pool	u_t	u_t	u_t	$u_t - u_t^{TL}$	$u_t - u_t^{TL}$	u_t	u_t	$u_t - u_t^{TL}$

Notes: Sample period: 1989Q1-2012Q3, with gaps in specifications (vii) and (viii), which use the observed recall share in the SIPP, 42 quarters. The numbers in parentheses are HAC standard errors.

again refers to the denominator, the number of job searchers. For the numerator, we use JOLTS vacancy series after the first quarter of 2001 (the first release of JOLTS is December 2000). Before then, we use the 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 adjusting tightness by excluding TL from the denominator does not make a large difference. The two series move closely with each other, although excluding TL raises the level of tightness by construction.

As we mentioned above, correlation between the recall share and market tightness, visually obvious from Figure 6 and panel (b) of Figure 7, induces omitted variable bias in the standard matching function estimation. Specifically, the correlation coefficients between (unadjusted) tightness and each of the three recall share series, SIPP, SIPP-I, and the TL share of hires, are -0.75, -0.47, and -0.34, respectively. We now quantify the consequences of this bias in the elasticity estimate on the measurement of matching efficiency.

4.3 Results: Elasticity Estimates

Table 7 presents the OLS regression results. The column (i) presents the result from the estimation of the standard matching function. The columns from (ii) through (v) present the estimation results of Equation (3). The specification differs with respect to the measure of the recall share and the pool of job seekers $\tilde{u}_t$. The columns (vi) through (viii) focus on the sample period where the actual SIPP recall share of hires is available.[13]

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[13]When the SIPP-I series is used in the estimation, this covariate itself is partially imputed from another regression, so the standard errors under specification (ii) and (iv) are likely underestimated and should be corrected for noise in the imputation regression; but the correction requires making some assumptions about correlations of unobservables and is unlikely to change the significance of the point estimate, as the reported standard error is tiny anyway.

The estimated elasticity of the standard matching function 0.4 is somewhat higher than what is found in the existing literature that uses the CPS data, due to the difference in the sample period.[14] When we exclude recalls from the dependent variable, the elasticity estimate increases considerably (either to 0.48 or 0.45 depending on the recall share measure used). The intuitive reason is that the probability of finding new jobs is more cyclically volatile than the standard measure that includes recalls. When we exclude workers on TL from the unemployment pool (columns (iv) and (v)), the bias becomes even larger. Our adjustments make an even larger difference, .1 or more, to the estimated elasticity when we use the actual SIPP-based recall share on a shorter sample period (42 quarters) due to the missing observations.[15]

4.4   Matching Efficiency

Figure 8 plots the residual series from the regressions (i) through (iii) in Table 7, passed through a 4-quarter moving average to smooth out high frequency variations.[16] This residual is often interpreted as a measure of matching efficiency, since it quantifies the flow of hires that cannot be explained by the stocks of unemployed and vacancies searching for each other. The blue solid line plots the residual from the standard matching function regression (4), the red solid line and the green dashed line, which move closely together, from the regressions with job-finding probability adjusted, respectively, by the TL hiring share in the CPS and the SIPP-I recall share. All three series exhibit the well-known declining trend, reflecting the falling dynamism of the US labor market. Off trend, all three series rise in coincidence with the 1990 and 2001 recessions. Strikingly, their behavior only diverges significantly during and after the Great Recession.[17] The standard measure suggests less mismatch during the recession and dramatic continuous deterioration since late 2009. This result is again overall consistent with the findings by other studies (e.g., Barnichon and Figura (2011)), and is intuitive given the behavior of the job-finding probability and market tightness over this period. The measures of mismatch that take recalls into account depict a more severe mismatch during the recession, but less mismatch after the recession.

The main reason for these findings is that recalls were relatively resilient during the Great Recession, in part because new jobs were so scarce that workers remained available

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

[15]The large difference in the elasticity estimate in this case reflects the fact that the negative correlation between the actual recall rate in the SIPP and market tightness is more pronounced.

[16]Our discussion from here on focuses on the comparison of the results from (i) through (iii). That is, we use either the SIPP-I series or the TL hiring share as a measure of the recall rate and we set $\tilde{u}_t = u_t$. All results below are insensitive to excluding workers on TL from unemployment, i.e., $\tilde{u}_t = u_t - u_t^{TL}$.

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

Figure 8: Matching Efficiency
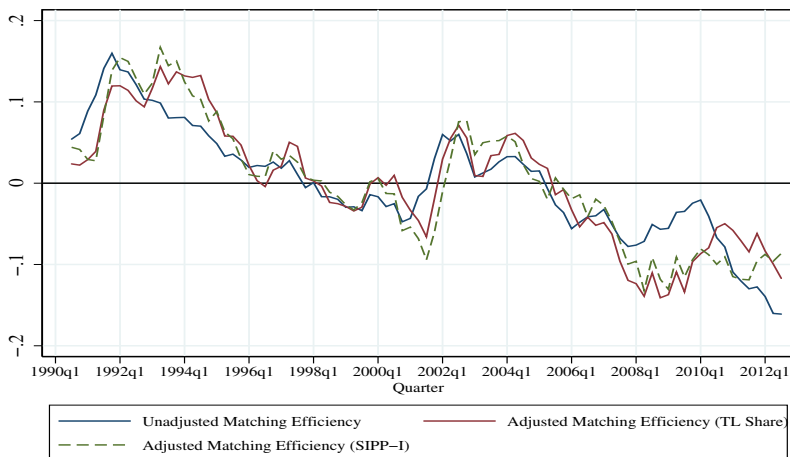
Notes: Adjusted matching efficiency: ε_t in Equation (3); Unadjusted matching efficiency: $\tilde{\varepsilon}_t$ in Equation (4). Four-quarter moving average.

for a recall for much longer, so recalls sustained the overall hiring rate. Conversely, as the economy recovered in 2010-2012, recalls did not rise as much as new matches. As job market tightness fell to an unprecedented extent, and then recovered, it tracked more closely the probability of new match creation than the overall hiring rate including recalls.

While the Great Recession provides the most striking instance, a qualitatively similar pattern can be detected in the other recessionary periods, early in each decade. That is, at the start of a recession, when the recall rate increases, the true extent of mismatch is larger, and then the relationship reverses as the recall rate subsequently drops.[18]

In the Appendix, we quantify this difference in a more familiar metric, the unemployment rate. We measure the size of unemployment that we can attribute to mismatch, with or without correction for recalls. We find that mismatch accounts for a percentage point rise in unemployment during the Great Recession, but adjusting for recalls cuts this in half. That is, half of what we would normally considered increased mismatch (lack of jobs given vacancies posted) is due to mismeasurement of new matches that vacancies can create.

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

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[18]The Appendix presents the results from the GMM procedure proposed by Borowczyk-Martins et al. (2013) to correct for the potential endogeneity in the estimation.

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 of one consumption good 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 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 thereafter. 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 which are also posted at a cost as in the standard model. 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, propose to recall the last match, or pay a search cost c_U to try and contact a new vacancy, which 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, which they observe provided that they start production and then can retain if they choose to separate. If the worker accepts the new offer and starts producing, he forfeits the recall option with his former employer(s) and simultaneously acquires a future recall option with this new employer. 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").

Similarly, a vacant job that holds a match of quality ε with its former employee (where $\varepsilon = 0$ if either the former employee took another job or the separation was "drastic") 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 with him 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. For now, we only require that separation and acceptance of a match 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 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 join and 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 any new hiring prospect in order to retain his recall option. As we will see, this competition will be ruled out by constant returns to scale 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 verify that the guess is consistent with all equilibrium restrictions.

Let $U(p, \varepsilon)$ be the value of unemployment, where $p\varepsilon$ is the productivity of the last employer, if any (otherwise $\varepsilon = p\varepsilon = 0$), $W(p, \varepsilon)$ be the value of employment to the worker, $V(p, \varepsilon)$ be the value of a vacant job, where $p\varepsilon$ is the productivity of the last employee, if any (otherwise $\varepsilon = p\varepsilon = 0$), $J(p, \varepsilon)$ be the value of a filled job, $w(p, \varepsilon)$ be the wage. Production continues and a recall occurs whenever both parties gain: $W(p, \varepsilon) \geq U(p, \varepsilon)$ and $J(p, \varepsilon) \geq V(p, \varepsilon)$.

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

$$\Omega(p, \varepsilon) := \int \mathbb{I}\left\{W(p, \varepsilon') \geq U(p, \varepsilon')\right\} \max\left\langle W(p, \varepsilon') - U(p, \varepsilon), 0\right\rangle dF(\varepsilon') \qquad (5)$$

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 the value $U(p, \varepsilon)$ 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 will be independent of the value $U(p, \varepsilon)$ of the recall option that the worker may currently 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. 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 "memoryless" 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 holding no recall option is also acceptable to an unemployed worker who holds 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 of the previous match, otherwise he would not be jobless; 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

$$\mathbb{I} \langle W(p, \varepsilon') \geq U(p, \varepsilon') \rangle = 1 \Rightarrow \max \langle W(p, \varepsilon') - U(p, \varepsilon), 0 \rangle = W(p, \varepsilon') - U(p, \varepsilon). \qquad (6)$$

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 (5):

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

The associated probability of acceptance of a new match by the unemployed worker (and also by the vacant firm given privately efficient rent-sharing) is written as:

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

Note that this is independent of the current recall value, as encoded in 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:

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

$$+ \lambda_\varepsilon \int [\max \langle J(p, \varepsilon'), V(p, \varepsilon') \rangle - J(p, \varepsilon)] \, dG(\varepsilon'|\varepsilon). \tag{7}$$

The value of a vacant job solves

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

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

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

$$+ \max \left\langle 0, -c_V + q(\theta(p)) \int \mathbb{I}\{J(p, \varepsilon') \geq V(p, \varepsilon')\} [J(p, \varepsilon') - V(p, \varepsilon)] \, dF(\varepsilon') \right\rangle, \tag{8}$$

where in the fourth line we used (6). The firm can recall the former employee after any shock, but also loses the recall option if the former employee successfully locates a new acceptable offer. This occurs (third line) if the expected capital gain from job search is positive, a contact occurs (at rate ϕ), and the new match is acceptable, which has a chance equal to $A(p)$. 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 integral 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 at $\varepsilon = 0$, until their net value is zero: for all p, $V(p, 0) = 0$. 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, $J(p, 0) = V(p, 0) = 0$ and $J(p, \varepsilon') = V(p, \varepsilon') = V(p, 0)$ for all $\varepsilon' \sim dG(\varepsilon'|0)$. Using these facts in (8), we obtain a familiar-looking free-entry condition:

$$\frac{c_V}{q(\theta(p))} = \int \mathbb{I}\{J(p, \varepsilon') \geq V(p, \varepsilon')\} J(p, \varepsilon') \, dF(\varepsilon') \tag{9}$$

and therefore (8) simplifies to

$$rV(p, \varepsilon) = \lambda_p \int [\max\langle J(p', \varepsilon), V(p', \varepsilon)\rangle - V(p, \varepsilon)] \, dP(p'|p) + \lambda_\varepsilon \int [\max\langle J(p, \varepsilon'), V(p, \varepsilon')\rangle$$
$$- V(p, \varepsilon)] dH(\varepsilon'|\varepsilon) - V(p, \varepsilon) \, \mathbb{I}\{\phi(\theta(p)) \Omega(p, \varepsilon) - c_U \geq 0\} \phi(\theta(p)) A(p), \tag{10}$$

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 a 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 that worker.

5.2.3 Bellman Equations: Worker

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

$$rW(p, \varepsilon) = w(p, \varepsilon) + \lambda_p \int [\max\langle W(p', \varepsilon), U(p', \varepsilon)\rangle - W(p, \varepsilon)] \, dP(p'|p)$$
$$+ \lambda_\varepsilon \int [\max\langle W(p, \varepsilon'), U(p, \varepsilon')\rangle - W(p, \varepsilon)] \, dG(\varepsilon'|\varepsilon). \tag{11}$$

After each shock, the worker may decide to quit. The HJB equation of the unemployed

worker is

$$rU\left(p,\varepsilon\right)=b+\lambda_{p}\int\left[\max\left\langle W\left(p',\varepsilon\right),U\left(p',\varepsilon\right)\right\rangle -U\left(p,\varepsilon\right)\right]dP\left(p'|p\right)$$

$$+\lambda_{\varepsilon}\int\left[\max\left\langle W\left(p,\varepsilon'\right),U\left(p,\varepsilon'\right)\right\rangle -U\left(p,\varepsilon\right)\right]dH\left(\varepsilon'|\varepsilon\right)+\max\left\langle 0,\phi\left(\theta\left(p\right)\right)\Omega\left(p,\varepsilon\right)-c_{U}\right\rangle .$$
(12)

After each shock, the worker may decide to reactivate the old job; in addition, he can decide to search for a new job, which is created if it offers a positive surplus.

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 for some $\beta \in (0,1)$:

$$\beta[J\left(p,\varepsilon\right)-V\left(p,\varepsilon\right)]=\left(1-\beta\right)\left[W\left(p,\varepsilon\right)-U\left(p,\varepsilon\right)\right].$$
(13)

This rule satisfies our requirement that separations and match acceptance only depend on total match surplus. If job search is costless (i.e., $c_U = 0$), (13) is also the generalized Nash Bargaining solution, thus maximizes joint surplus, and is privately efficient. If, however, job search is costly (i.e., $c_U > 0$), 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 (13), 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 that 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 (13) and after much algebra we obtain an expression for the wage:

$$w\left(p,\varepsilon\right)=b+\beta\left(p\varepsilon-b\right)+\left(1-\beta\right)\max\left\langle 0,\phi\left(\theta\left(p\right)\right)\Omega\left(p,\varepsilon\right)-c_{U}\right\rangle$$

$$+\beta\mathbb{I}\left\{\phi\left(\theta\left(p\right)\right)\Omega\left(p,\varepsilon\right)\geq c_{U}\right\}\phi\left(\theta\left(p\right)\right)A\left(p\right)V\left(p,\varepsilon\right)$$

$$+\lambda_{\varepsilon}\int\left[\beta V\left(p,\varepsilon'\right)-\left(1-\beta\right)U\left(p,\varepsilon'\right)\right]\left[dG\left(\varepsilon'|\varepsilon\right)-dH\left(\varepsilon'|\varepsilon\right)\right].$$
(14)

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 of these are 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 $V(p, \varepsilon)$, 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, making recall infeasible. This reduction in the firm's outside option 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 (off the job). Suppose, for example, that G first-order stochastically dominates H because unemployment causes skill loss. Then the last term in the wage function is positive if $\beta V(p, \varepsilon') - (1 - \beta) U(p, \varepsilon')$ 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 match quality depreciation after separation. This gives the worker additional bargaining power, thereby raising the wage.

The equilibrium of the model is described by J, V, W, U, w, and θ that solve (7), (9), (10), (11), (12) and (14) as functions of aggregate and idiosyncratic productivities. It is straightforward to solve this system of functional equations through any nonlinear iteration algorithm. We exploit this tractability to explore the quantitative properties of the model.

5.3   Calibration

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. We use the resulting monthly panel data set to compute the cross-sectional 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. A similar simulation procedure is used for the business cycle analysis, which is described in subsection 5.4.

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, $c_U = 0$, so that 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, match productivity transits from any state $\varepsilon > 0$ to the lowest state $\varepsilon' = 0$, which is absorb-

ing, so this transition makes a future recall impossible. The remainder of the total monthly EU transition probability, which is targeted at 0.015, is generated 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 volatility σ_ε of innovations. We estimate these two parameters via the simulated method of moments, as explained below. We approximate this estimated 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 to it, to obtain the Markov chain G. After separation, match quality evolves according to the same stochastic law of motion, with no skill depreciation: $H = G$.

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

$$\theta q(\theta) = \mu \theta^\alpha,$$

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, $\mu\bar{\theta}^\alpha [1 - F(\underline{\varepsilon})]$. We iterate on the guess $\bar{q}$ of the contact rate until the exit probability from unemployment to new jobs generated by the simulated model equals the empirical target 15% per month, which is our estimate for the average probability of exit from unemployment to *new* jobs from the 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 condition for constrained efficiency, although this condition might not apply to our economy with recall.

We now turn to our empirical targets. Consistently with the model, where workers always participate in the labor force, empirical moments are based on completed unemployment spells EUE. Note that this differs from the data we used to estimate the matching function,

Table 8: Parameter Values: Weekly Calibration

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

where we included entrant unemployed. 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. Our choice of empirical targets is motivated by the following considerations. Job-finding and separation probabilities are at 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, 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 8 summarizes our best calibration. Persistence ρ_ε and volatility σ_ε of idiosyncratic shocks are meant to be monthly, because such shocks hit on average once a month. Upon convergence, we find that the required average contact rate $\bar{q}$ 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, 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 9 and 10 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 a probability that does not change over the course of an

Table 9: Steady-State First Moments of the Model and Data

	Unemp. rate	Separation prob.	JF prob. (overall)	JF prob. (new hires)	Recall prob	Recall rate (share)
Model	0.049	0.015	0.282	0.147	0.135	0.479
Data	0.060	0.014	0.257	0.139	0.117	0.460

Notes: Based on the simulations of the steady-state version of the model, solved at weekly frequency. The model statistics are constructed from the monthly panel data constructed by sampling the observations from the weekly panel data every four weeks. Sample period for the data are 1989Q1-2012Q3.

unemployment spell, but only depends on job market tightness and the job acceptance rate.[19] 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 quality 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 non-targeted empirical moments.

To calibrate the aggregate productivity process p, we proceed as follows. We use quarterly estimate of the Solow residual by Fernald (2012), which is corrected for capacity utilization and covers the period $1947-2013$. We take logs and HP-filter this series with 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. The resulting parameter values, 0.76 and 0.025 respectively, 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

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[19]The job acceptance rate does not depend on unemployment duration either. As discussed in Subsection 5.2, the worker's acceptance decision is independent of the value of the recall option.

Table 10: Mean Duration and Hazard Rate (Model)

	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 9. The empirical counterparts are in Table 5 and Figure 3. Note that Figure 3 includes those who drop out of the labor force, whereas the calibration targets hazard rates of the *EUE* sample.

of 20 points for p using Tauchen's method.

5.4   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 Bellman values and tightness as functions of the state variables, 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: (i) unemployment rate, (ii) separation probability, (iii) overall job-finding probability, (iv) job-finding probability 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 the quarterly series and HP-filter the logged data with the smoothing parameter of 1,600.

Table 11 presents the standard deviations and the correlation coefficients between the unemployment rate and the remaining five variables. For empirical measures of the recall rate, we use two measures, one based on the SIPP-I series (first row along "Data" section in the table) and the other based on the TL share of hires in the CPS (second row). Accordingly, we also consider the two different empirical measures of the recall probability and job-finding probability for new hires. The volatility of the unemployment rate is 0.128 and roughly

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Table 11: Cyclical Properties of the Model

	Unemp. rate	Separation prob.	JF prob. (total)	JF prob. (new hires)	Recall prob.	Recall rate
	Standard Deviations					
Model	0.128	0.138	0.045	0.059	0.085	0.052
Data	0.115	0.072	0.079	0.123	0.087	0.077
				0.109	0.088	0.064
	Correlation with Unemployment					
Model	1.000	0.901	−0.476	−0.820	0.069	0.526
Data	1.000	0.823	−0.944	−0.821	−0.517	0.380
				−0.803	−0.690	0.208

Notes: Empirical JF probability for new hires, recall probability, and rate based on: SIPP-I series (first row along "Data" in the table) and the share of TL hires (second row).

comparable to its empirical counterpart. This happens, however, in part for the wrong reason, as the separation probability into unemployment is twice as volatile in the model as in the data, and the opposite is true of the overall job-finding probability. Remember that our calibration does not target the second moments, and therefore this result is not surprising. Hence, adding the recall option to the MP model does not solve the volatility puzzle of Shimer (2005). However, when considering separately the job-finding probability for new hires, its volatility is somewhat higher than that for all hires, both in the model and in the data, which is consistent with the earlier empirical finding in Section 4. Lastly, the model replicates well the fairly volatile recall probability and recall rate.

Turning to correlations, the model replicates the countercyclical separation probability and procyclical job-finding probability, as in the standard MP model.[20] But an interesting feature of the model with recall is the fact that the recall probability, the chance that an unemployed worker is recalled at each point in time, not to be confused with the recall rate (here, the share of hires that are recalls), is nearly acyclical (the correlation coefficient with unemployment is 0.069), while it is definitely procyclical in the data. Observe that the acyclical recall probability, relative to the chance of finding new jobs, predicted by the model tames the procyclicality of the overall job-finding probability, which is qualitatively consistent with the new empirical evidence that we provided earlier, although quantitatively too strong. Importantly, the acyclical recall probability and the strongly procyclical probability of finding a new job imply that the recall share of new hires is higher in recessions. The model generates this key observation we highlighted in the earlier section despite its fairly parsimonious

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

structure.

In the model, the recall probability is acyclical due to the combination of two effects. First, lower aggregate productivity reduces labor demand and thus also the probability that any newly separated worker is recalled at each point in time. 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; this partially offsets the poorer recall prospects of those who were close to be recalled before the aggregate shock, which is part of the first effect. The reason why the model exaggerates the correlation of unemployment and recalls is that it also exaggerates the volatility of the separation probability, so the second effect in the model is too strong.

6 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 from unemployment 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 the exit 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 different light. In future work we will explore 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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Appendix

A Supplementary Evidence from the CPS

A.1 Transition Probabilities and Unemployment Duration

Figure A.1 plots quarterly transition probabilities between employment and unemployment derived from the matched records. Panel (a) breaks down employment-to-unemployment (EU) transitions into TL and PS. Note that each line is calculated by dividing the EU flow for each reason by the total employment stock. This figure thus tells the relative size of the two inflows. The TL inflow amounts to roughly one half of the PS inflow, and the two move more or less in parallel over business cycles. Panel (b) presents unemployment-to-employment transition (UE) probabilities by reason. Workers on TL enjoy a much higher job-finding probability than PS workers. Note also that both series exhibit the familiar procyclicality, but the procyclicality is more pronounced for PS workers. During the post Great Recession recovery, the UE probability recovered for TL but not for PS workers.

Therefore, Figure A.1 and Figure 2 in the text give similar results in terms of relative size of TL and PS flows and their cyclicality. Figure A.2 confirms that median duration of those on TL is much shorter on average and less cyclical.

Figure A.1: Transition Probabilities Between Employment and Unemployment by Reason: Matched Records
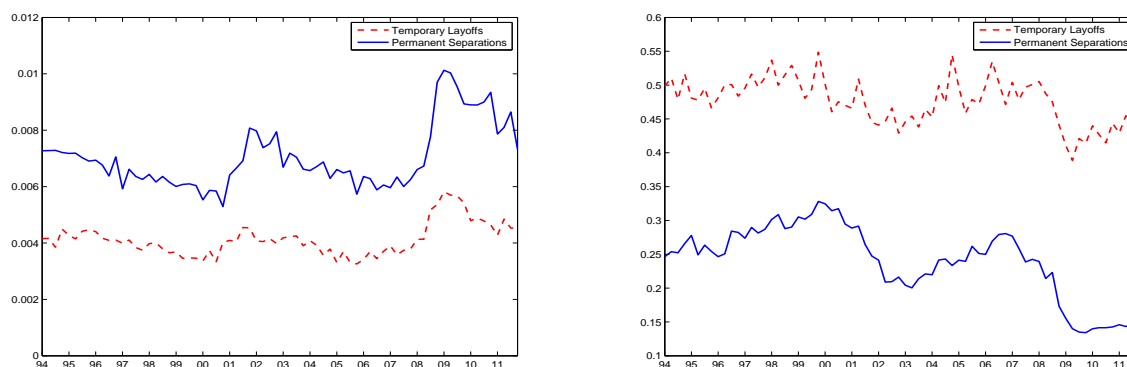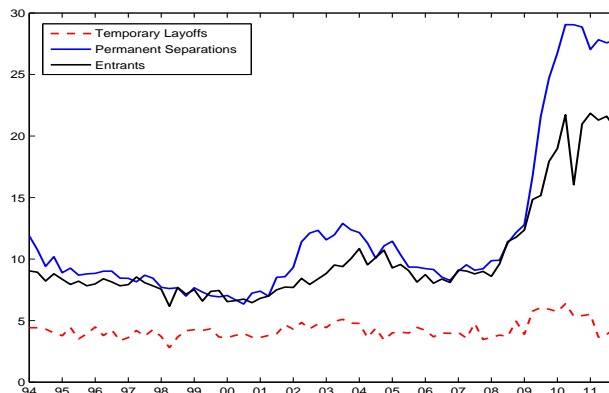

(a) EU Transition Probability

(b) UE Transition Probability

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

Figure A.2: Median Duration (Weeks) by Reason

Notes: Source, Monthly CPS.

A.2 Industry Composition and Seasonality of Temporary Layoff

It is important to note that TL are not concentrated in a particular sector (e.g., manufacturing). Panel (a) of Figure A.3 presents the industry breakdown of the aggregate TL separation flow into unemployment. While the contributions of the construction and manufacturing sectors are, as expected, large, TL are not at all unusual also in other sectors. To take into account the relative size of each industry and 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.[21] As expected, in agriculture/mining, construction, and manufacturing, TL are very frequent. More importantly, though, the shares of the separation flows that are TL in the other industries are substantial.[22]

Figure A.4 summarizes the seasonal pattern of TL. All industries except education/health share the feature that the TL flow increases in winter months. In addition, some sectors (manufacturing and other services) shed more workers temporarily 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 A.1 and 2, which plot seasonally-adjusted data, demonstrate that there are also non-seasonal, business cycle variations in separation and job-finding rates associated with TL. Similarly, in our main analysis based on the SIPP, we will find that the share of hires from unemployment that are recalls, whether from TL or not, exhibits a countercyclical pattern. Therefore,

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[21]The shares plotted in Panels (a) and (b) are averages over the period between January 2003 and December 2011, during which the industry classification used by the CPS remains consistent.

[22]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 A.1 and this average share is consistent with the shares in Panel (b).

Figure A.3: Industry Breakdown of Temporary Layoffs
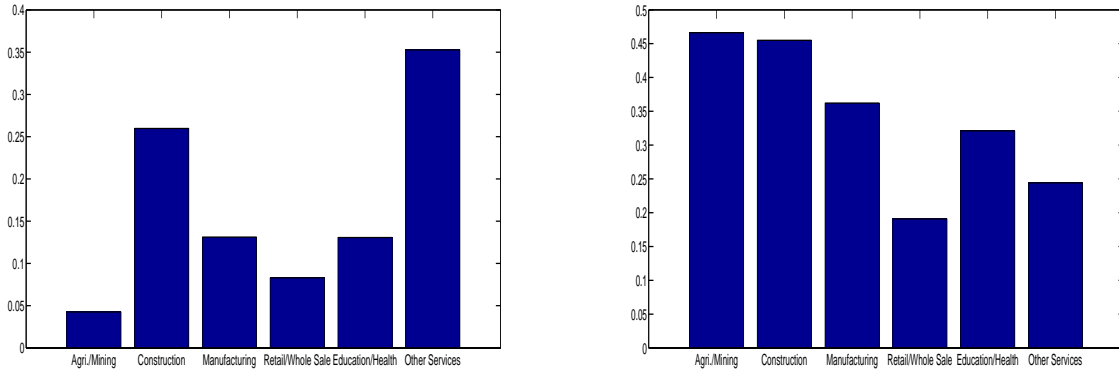
(a) Industry Shares of TL Separation Flow

(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. The graphs give average shares over the period 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.

Figure A.4: Seasonality of Temporary Layoffs by Industries
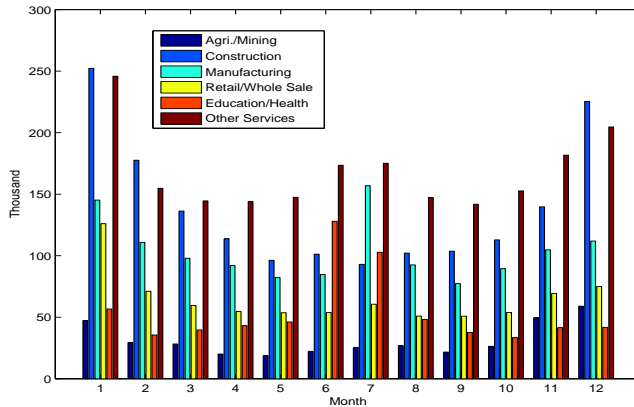
Notes: Source, monthly CPS. Average shares between January 2003 and December 2011.

TL and recalls are not simply a seasonal phenomenon. Furthermore, even their seasonal component does affect the average level of turnover in and out of unemployment. Since TL (thus, presumably, also recalls) are not synchronized between industries, but rather staggered within the year, part of this industry-specific seasonality cancels out when aggregating all industries to obtain economy-wide job finding and separation flows.

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Table A.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 results for the 2008 panel use the data up to wave 10, which was the latest wave available at the time of our analysis.

B   Imputation Procedure

Table A.1 summarizes each panel's length and the period covered. To ensure that censoring of the panel does not affect our results, our main analysis restricts our sample to those cases in which the transition into non-employment occurs in the first year in the 1990-1993 panels or the first two years in the 1996-2008 panels.

As mentioned in the text, the job id is available in the SIPP and has been verified by the Census Bureau in the 1990, 1991, 1992 and 1993 panels. For panels starting in 1996, the job id is dropped when the worker has no job over a wave, except when he is on TL; we also found that, for shorter non-employment spells (one or two months) that end within a wave, recall is much lower when the spell crosses the wave seam. Indeed, in Table A.2, which reports recall rates based on the raw data, one can see sudden drops in the recall rates at the 1996 panel. For both reasons, for panels starting in 1996 we need to impute recall following both long spells not on TL and short spells that cross a wave. When imputing recalls after a long spell, we choose to discard the genuine information that we have on recall of TL from 1996 on, and impute those too, so that all long spells from 1990-1993 panels are used to impute recall after all long spells from 1996 on.

B.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 or higher.

- 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 A.2: Recall Rates: Separations Occurred in the First Year or Two Years of Each Panel (Pre-Imputation)

Panel	Separations in waves	$E\!\!\not{E}$ Recall rates	Counts	$E\!\!\not{E}E$ Recall rates	Counts
1990	1-3	0.298	4,176	0.371	3,325
1991	1-3	0.343	2,870	0.423	2,310
1992	1-3	0.330	3,515	0.407	2,827
1993	1-3	0.324	3,220	0.398	2,587
1996	1-6	0.160	10,032	0.190	8,341
2001	1-3	0.172	4,807	0.209	3,904
2004	1-6	0.189	4,570	0.226	3,730
2008	1-3	0.208	4,821	0.264	3,718

Notes: Source, SIPP. Number of recalls relative to all separations into non-employment, denoted by $E\!\!\not{E}$ (including unemployment and inactivity), and relative to all the spells that end with employment, denoted by $E\!\!\not{E}E$.

B.2 Short Spells

Within the short spells (one or two months of non-employment duration) in the 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.[23] 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.

B.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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[23]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.4 Imputation Diagnostics

To assess the quality of our imputation, we perform an "in-sample forecast." We split randomly our reference samples that were deemed accurate into two equal subsamples, A and B. We then reset all recall information in subsample B to "missing." We merge the subsamples again and repeat our imputation procedure. We then compare imputed recall outcomes for subsample B to the true recall observations. The imputation round, we remind the reader, is intentionally noisy, because the logit model generates a probability in $(0, 1)$, while we need to impute an outcome in $\{0, 1\}$. We find that the imputation introduces Type I and Type II errors relative to true data each equal to roughly 15%. So the imputation recovers the truth 70% of the time, and introduces no bias, hence the share of recalls is imputed almost exactly.

We also find that the imputation procedure recovers very accurately the average recall rate of workers on TL for a long spells after 1996 (note that our imputation regression does not use the labor market status variable, as mentioned above). Because the recall probability of TL is close to 1, this means that our predicting variables for recall also predict very accurately TL status. In fact, we chose the variables that we include in the regression precisely to make sure that it would correctly recover recalls of TL long spells after 1996.

B.5 Chained Multiple Imputation

We also experimented with a "chained" method, which is a type of MCMC procedure. We first impute missing observations on the last wage before, and on the first wage after, the non-employment spell, using multiple imputation from regressions on the same variables as above. The regressions are "chained": first we use all variables but the post-spell wage to impute the pre-spell wage, when missing; next we use the sample with completed (observed or imputed) pre-spell wage to impute the post-spell wage, when missing; we then go back and repeat the same step, and so forth, and collect the results after 10 initial burn-in iterations, when coefficients and imputed values have settled.

Next, we use this completed sample of pre- and post-spell wages, either observed or imputed, to repeat the multiple imputation of recalls described before. We then use the imputed recalls to repeat the chained multiple imputation of pre- and post-spell wages described above, now also using recall in the chained regressions. We then use the new imputed wages to impute, again, recall, and so on. After 10 iterations we collect the final results.

When performing the diagnostics, we find that the quality of chained imputations is worse than that of the simpler, univariate imputation. Now the error percentage is 35%. Moreover errors are not symmetric: the model makes the Type I error (impute recall when there was none) 20% of the time and the Type II error 15% of the time, implying that the imputed average recall rate is 5% higher than the true one. The only major difference in the imputation estimated regression coefficients between the univariate and the multivariate (chained) case is the effect of pre-spell wage, much stronger on recall in the chained case. So, high wage workers are predicted more often to be recalled, and this leads us to overpredict wages for those who are recalled but whose wages are missing, which, in turn, raises the recall prediction further. For this reason, we present results from the univariate imputation.

B.6 Missing Values in Predicting Variables

When we apply our imputation procedure, we find that some of the otherwise valid $E\!\!\!/E$ records could not be imputed because one or more of the right-hand-side variables are missing. The most important one is the wage information. Since wage changes are strong predictor of recalls, we chose to run our imputation regression restricting the sample to those cases with non-missing wage information, even though it means dropping otherwise valid $E\!\!\!/E$ records.

We drop these cases from both denominator and numerator of the recall rates. Our assumption here is that those observations are missing at random. The concern is the violation of this assumption that the probability of missing wages is correlated with higher or lower probability of recall. The chained multiple imputation procedure discussed above is our attempt to impute wages for those cases and use those imputed wages in the regression for imputation of recalls. However, as mentioned above, we find that this chained imputation procedure performs worse than our simpler univariate imputation procedure, when our diagnostic test is applied to both datasets. Moreover, as discussed above, our diagnostic test on the univariate imputation procedure indicates that the missing-at-random assumption is adequate.

C   Estimation of the Matching Function

C.1   GMM

The evidence that "mismatch" (residual from the matching function estimation) was countercyclical, off a declining trend, also raises the issue of endogeneity of the job-finding probability and the resulting bias in the elasticity estimate. As pointed out by Borowczyk-Martins

Table A.3: Elasticity Estimates from Various Specifications

Method	Recall Adjustment	Full Sample	89Q1−00Q4	01Q1−12Q3
OLS	No	0.403**	0.264**	0.442**
		(0.018)	(0.011)	(0.022)
OLS	SIPP-I	0.478**	0.329**	0.515**
		(0.022)	(0.021)	(0.027)
OLS	TL Share	0.447**	0.322**	0.466**
		(0.020)	(0.018)	(0.027)
GMM	No	0.364	0.287**	0.661**
		(0.926)	(0.058)	(0.175)
GMM	SIPP-I	0.608**	0.357**	0.721*
		(0.140)	(0.113)	(0.320)
GMM	TL Share	0.493	0.654	0.459
		(0.667)	(0.419)	(0.513)

Notes: HAC Standard errors are in parenthesis. ** (*) indicate statistical significant at 1% (5%) level. Three specifications estimated via OLS correspond to specifications (i) through (iii) in Table 7.

et al. (2013), in the canonical Mortensen and Pissarides (1994) search and matching model, shocks to matching efficiency influence the firm's vacancy posting decision and thus the job-filling probability through the free entry condition. As a result, the regressor in the matching function estimation, job market tightness, is correlated with the error, of which matching efficiency is an estimate, and the OLS estimate of the elasticity of hires with respect to vacancies will be biased. Suppose that matching efficiency suddenly worsens, for whatever reason. On the one hand, as a direct effect of lower matching efficiency, the firm's incentive to post vacancies is reduced. On the other hand, if changes in matching efficiency are persistent, the negative shock means worse prospects for the unemployed in the near future. This reduces future Nash bargained wages and raises on impact the value of a new hire, thus raising the firm's incentive to post more vacancies. Ignoring these effect leads to a bias in the estimated elasticity.[24]

Borowczyk-Martins et al. (2013) propose a GMM procedure to correct for endogeneity. They use monthly data from JOLTS for vacancies and hires covering the period of 2001-2013 and find that their preferred GMM estimated elasticity is lower than the OLS one (.69 vs .84).[25] Table A.3 reports the results of estimating the matching function by the two

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[24]The sign of the bias cannot be determined theoretically because the two effects that are discussed work in the opposite directions.

[25]We thank the authors for providing the code of their estimator.

methods and different sample periods. When we apply their procedure to our data (with no recall adjustments), which differs in source, frequency, and time coverage, we find the lower elasticity estimates than theirs.

With their GMM procedure, the estimation becomes less stable in general: As can be seen from the lower part of the table, elasticity estimates sometimes become insignificant. When the TL hiring share is used as a measure of the recall rate, none of the estimated elasticities are statistically significant. It is also true for the full sample without recall adjustment. However, when using the SIPP-I series, the results are relatively stable. Our OLS estimates are biased down: for the full sample, the point estimate increases from 0.48 t 0.61 and for the other two subsamples, the GMM correction raises the elasticity.

C.2   Counterfactual Unemployment Rates

To quantify this difference in a more familiar metric, the unemployment rate, we perform the following exercise.[26] First, we exploit the steady-state approximation to the unemployment rate which is expressed as the ratio between the separation probability and the sum of the separation and the overall job-finding probabilities,. Shimer (2012) showed this "conditional steady-state" values to be very close to the actual observed series. With our recall adjustment, the overall job-finding (UE transition) probability is written as the sum of the transition probability into a new job and the recall probability, where only the former variable is modeled by the matching function.[27] We then fix the matching efficiency series at its average level, which gives the counterfactual unemployment rates under the absence of the fluctuations of matching efficiency. The difference between the actual steady-state unemployment rate and the counterfactual unemployment rate gives a measure of "mismatch" expressed in unemployment rate percentage points.

Figure A.5 reports in panel (a) the actual steady-state approximation, which is close to the true unemployment rate and shown by a black solid line, and the three counterfactual steady-state unemployment rates. The blue line corresponds to the case where the overall job-finding probability is modeled by the matching function, and red solid and green dashed lines correspond to the cases with the recall adjustments.

To better illustrate the impact of our adjustments, panel (b) plots the difference between the observed steady-state unemployment rate and each of the three counterfactual unemployment rates. First note that the contribution of mismatch is estimated to be larger when the standard matching function is used to model the overall job-finding probability. The larger differences are particularly visible in the early 90s and in the post Great Recession period.

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[26]We thank Rudiger Bachmann for suggesting the exercise.

[27]We also assume here $\tilde{u}_t = u_t$.

Figure A.5: Counterfactual Unemployment Rates
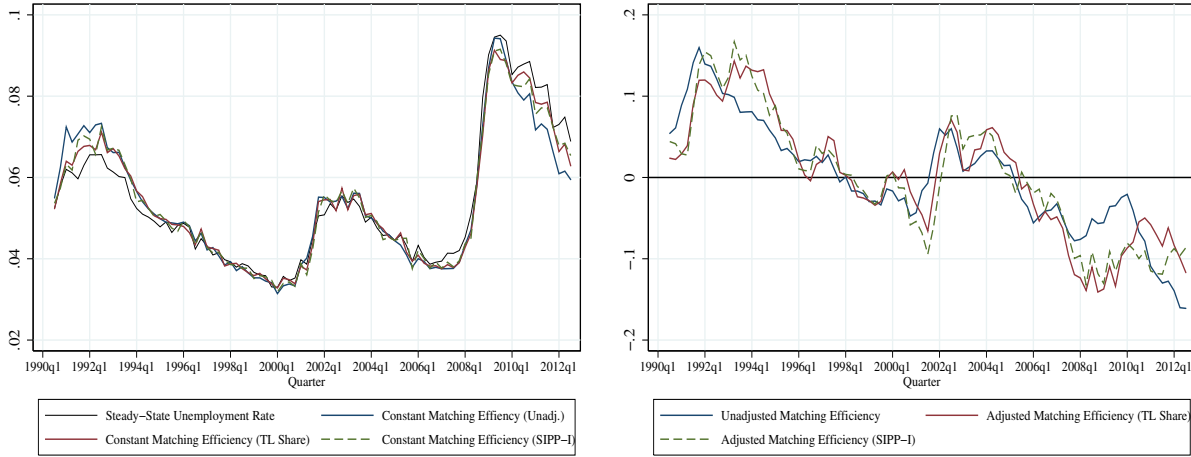
(a) Counterfactual Unemployment

(b) Difference from Conditional Steady State Values

Notes: (b) plots the four quarter moving averages of the difference between the conditional steady-state unemployment rate and each of the counterfactual unemployment rates.

With our recall adjustments, some of the unemployment fluctuations that were attributed to mismatch disappear. In particular, in the post Great Recession period, the standard matching function suggests that mismatch contributed to raising the unemployment rate by 1 percentage point. Both of our two recall adjustments reduce the contribution to less than 0.5 percentage points.[28]

The analysis here 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 a quantitatively significant difference in our assessment of the extent of mismatch in and after the Great Recession.

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[28]According to Figure 8, the recall adjustment does not make a large difference in overall variability of the residuals themselves. This fact, however, is not inconsistent with their contributions to unemployment being significantly different. With the recall adjustment, part of the fluctuations in the overall job-finding probability are accounted for by those in the recall probability itself, thus reducing the unexplained portion of the fluctuations in the overall job-finding probability.