WORKING PAPERS
RESEARCH DEPARTMENT

WORKING PAPER NO. 17-29
RECALL AND UNEMPLOYMENT

Shigeru Fujita
Research Department
Federal Reserve Bank of Philadelphia

Giuseppe Moscarini
Yale University and NBER

June 2017

RESEARCH DEPARTMENT, FEDERAL RESERVE BANK OF PHILADELPHIA
Ten Independence Mall, Philadelphia, PA 19106-1574 • www.philadelphiafed.org/research-and-data/

Recall and Unemployment*

Shigeru Fujita[†] Giuseppe Moscarini[‡]

June 2017

Abstract

We document in the Survey of Income and Program Participation covering 1990-2013 that a surprisingly large share of workers return to their previous employer after a jobless spell and experience very different unemployment and employment outcomes than job switchers. The probability of recall is much less procyclical and volatile than the probability of finding a new employer. We add to a quantitative, and otherwise canonical, search-and-matching model of the labor market a recall option, which can be activated freely following aggregate and job-specific productivity shocks. Recall and search effort significantly amplify the cyclical volatility of new job-finding and separation probabilities.

JEL Codes: E24, E32, J64
Keywords: Recall, unemployment, duration, matching function, business cycles

*We thank Behzad Kianian for excellent research assistance, Martha Stinson and Mark Klee for technical assistance on the SIPP, our discussants Larry Katz, Ryan Michaels, Toshi Mukoyama, and Rob Valletta, as well as Rudiger Bachmann, Javier Fernandez-Blanco, Fabien Postel-Vinay, and participants at many seminar and conference presentations for extensive comments that are reflected in this revision. We thank in particular the editor, Luigi Pistaferri, and three anonymous referees for their generous input, which greatly improved the paper. The usual disclaimer applies. Moscarini also thanks the NSF for support under grant SES 1123021 and the Federal Reserve Bank of Philadelphia for its hospitality while working on this project. The views expressed herein are the authors' and do not reflect the views of the Federal Reserve Bank of Philadelphia or the Federal Reserve System. This paper is available free of charge at www.philadelphiafed.org/research-and-data/publications/working-papers/.

[†]Federal Reserve Bank of Philadelphia, Research Department. shigeru.fujita@phil.frb.org

[‡]Yale University, Department of Economics and Cowles Foundation; NBER. giuseppe.moscarini@yale.edu

1 Introduction

Unemployment is commonly understood as a state of job search, and is measured accordingly. Due to information imperfections, workers cannot immediately find the kind of employment that they desire and that the market offers somewhere. One leading interpretation of these search frictions is the extreme heterogeneity of jobs − by pay, schedule, location, task, work environment − and workers − by various types of skills, work ethics, collegiality, and so on. Therefore, it takes time and effort from both sides to identify and arrange a suitable match. 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 each other. In this paper, we show that recalls in the U.S. labor market are a pervasive phenomenon, with a distinct cyclical pattern and significant implications for individual worker experiences and aggregate unemployment volatility.

Using data from the Survey of Income and Program Participation (SIPP), we document that recalls of former employees in the U.S. 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 flow 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.[1] In other words, recalls are 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 more "attached" job losers, who complete their unemployment spell without leaving 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 restrict our attention to EUE spells, so that we can compare pre- and post-unemployment outcomes, with and without recall. Recalled workers were employed at their last job on average twice as long as new hires (6 vs. 3 years of tenure), experience shorter unemployment duration (by over a month), switch occupations much less often upon re-employment (3% vs. over 50% for job switchers), and stay with the employer significantly longer after the jobless spell. Negative unemployment duration dependence emerges mostly for those who are eventually

[1] In order for the worker to be classified as TL in both the SIPP and 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.

recalled; the hazard rate of exit from unemployment to a *different* employer than the last one is only mildly declining 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. A natural interpretation of this evidence is that recalls circumvent to a large extent search-and-matching frictions, and thus cannot be treated as the output of a matching function, which is only about new matches.

Next, we study the empirical relationship between recall and unemployment over the business cycle. In recessions, the probability that an unemployed worker is recalled drops, just like, but by much less than, the probability that he finds a new employer; therefore, the recall rate rises, and so does the share of recalls in the total number of hires. This increase was especially sharp during the Great Recession.

To summarize our empirical findings: when we exclude recalls from the exit rate from unemployment to employment, the probability that an unemployed worker finds a *new* job in the U.S. labor market is on average much lower, more cyclical, and nearly independent of the duration of the unemployment spell, when compared both to the stylized facts from the CPS, where this distinction is not possible, and to our own SIPP sample when not excluding recalls. In this sense, employment reallocation in the U.S. labor market is not as fluid as, and finding a new job is harder than, commonly thought, although on the bright side recall is pervasive and beneficial; the puzzle posed by Shimer (2005) is even deeper; and the negative duration dependence of unemployment is primarily tied to the fading likelihood of a recall.

Building on these facts, we quantify the importance of recalls for aggregate labor market fluctuations. We introduce a recall option in the 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 key innovation is the assumption that, after separation, the productivity of the match keeps evolving. As long as the former employee is still unemployed and available, he can agree with his previous employer to re-match, induced by intervening changes in the aggregate and/or idiosyncratic components of match productivity. Recall is free and instantaneous for both parties. In contrast, firms that either cannot or do not want to recall a former employee must pay a cost to post a vacancy and search for a new worker. Similarly, unemployed workers must spend costly search effort to contact those vacancies and draw a new match.

After an endogenous separation, the firm can keep the idle 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 to scale in production ensure that recall and new job creation decisions are made independently. Thus, a separated worker does not need to be concerned about being replaced in his old job by a new hire. Conversely, a worker can only work for one employer at a time, and cannot scale up his labor supply like firms do with their labor demand. 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 should be concerned about losing a former "mothballed" employee to a new employer. The probability of this event, the (new-)job-finding probability, is the key equilibrium object in our model, as in the standard stochastic search-and-matching model, but here in part for a new reason: a higher job-finding probability reduces not only unemployment, but also recall opportunities. Recall is similar to on-the-job search, in the sense that the worker can search while still attached to an employer, but is also different, because unemployed job search (while waiting for recall) and wage payments are mutually exclusive; therefore, current wages cannot affect incentives to search for other jobs.

We assume that wages are set by Nash Bargaining and analyze a simple equilibrium, where the option value of recall affects neither the probability of accepting a new job nor the wage the new job will pay. The only state variables are the exogenous productivity components. We calibrate the model parameters by a simulated method of moments, so that its steady-state equilibrium replicates cross-sectional moments computed from our microeconomic evidence. The calibrated model reproduces quantitatively all of our cross-sectional facts. The hazard rate of recalls declines with unemployment duration, as we observe in the data, due to dynamic selection: The longer a worker remains unemployed without being recalled, the more likely it is that the (unobserved and persistent) quality of his previous match has deteriorated since separation, and hence the less likely it is that a recall is forthcoming.

Finally, we introduce aggregate productivity shocks in the calibrated model, and simulate its stochastic simple equilibrium, with and without recall option and/or search effort. The existence of a recall option greatly amplifies cyclical fluctuations in job separations and, when interacted with search effort, in the job-finding probability. Firms are more willing to lay off workers when they can recall them, but this is especially true in recessions, when these workers remain available for recall longer due to lack of alternatives. In model parlance, in recessions the match surplus from continuing production over temporary separation declines further. This surplus also determines the propensity to accept new matches; hence, its additional decline further reduces the average job-finding probability and depresses vacancy creation. In turn, a lack of new jobs, a lower propensity to accept them, and the recall option itself all discourage costly search for new jobs by workers, again further depressing vacancy creation. Moreover, recalls are much less procyclical and volatile than new hires, as in the data.

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 measurement issues that arise in the estimation of recall in the SIPP and present our preferred estimates. In Section 4, we illustrate the empirical relationship between our measured recalls and employment and unemployment duration. In Section 5, we describe business-cycle patterns of aggregate recalls. In Section 6, we lay out our search-and-matching model with recall that we analyze quantitatively in Section 7. Brief conclusions take stock of the results. The Appendix presents additional materials for empirical evidence and quantitative exercises.

2 Related literature

Several authors 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 often in scope and always in time span. To the best of our knowledge, we are the first to study recall in a large, nationally representative survey covering several decades and thus to connect recall 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 Unemployment Insurance (UI) benefit recipients from Missouri and Pennsylvania in 1979-1981. The vast majority of survey participants (75%) said that they expected to be recalled, although only 18% had a definitive recall date; ex post, a sizable share were actually recalled.[2] Katz and Meyer 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.[3]

Our sample is based on the 1990-2008 panels of the SIPP, which cover the entire U.S.

[2]The definition in the CPS (see Footnote 1) is likely to be stricter than the recall expectation measured in the data that Katz and Meyer (1990) used.

[3]This seminal work inspired a sizable literature, which is too large to survey exhaustively here. Fallick and Ryu (2007) use the same data as Katz (1986) and replicate Katz and Meyer's competing hazard exercise without the information on subjective recall expectations, but controlling for unobserved heterogeneity. A similar approach is taken by Jansson (2002) and Alba-Ramirez et al. (2007) for (resp.) Sweden and Spain. Recalls amount to 45% of all completed unemployment spells in Sweden and one third of all hires in Spain, and only recalls exhibit negative duration dependence of unemployment. Kodrzycki (2007) studies a sample of workers who suffered mass layoffs in Massachusetts in the early 1990s, were eligible for expensive retraining under the Job Training Partnership Act, and thus arguably were not expected to be recalled at all. She finds that 4% of them were, against all odds, recalled, and did much better, even years later, than those who were not recalled. Nekoei and Weber (2015) find in Austrian administrative data that 58% of temporary layoffs and 19% of permanent separations are recalled, with an average recall rate of 35%.

labor force for almost a quarter century and three business cycles (1990-2013), not just UI benefit recipients, a single region, or a single recession episode. In comparison to this microeconomic literature, we confirm in our comprehensive sample the importance of recall, even for PS workers, and its empirical relationship with tenure and exit from unemployment, including its hazard rate (and wages; see our working paper, Fujita and Moscarini (2013)). We also show, however, that the strongest relationship is with occupational mobility, and that recall also predicts subsequent attachment.

Recall plays a negligible 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. To calibrate the separation probability, they use the SIPP, but 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) excludes the contribution to unemployment of those workers who are separated and then recalled within the four-month period. We investigate whether the recall option affects the incentives of the firm and the worker to search for new matches, that is, whether recall and search interact, as suggested by the microeconomic literature, in which case, the calibration strategy by Bils et al. (2011) is problematic. In addition, we show that their choice of a four-month unemployment duration cutoff to define a recall leads to true recalls being significantly underestimated, because of data issues in the SIPP that we will discuss in detail.

Fernandez-Blanco (2013) 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 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 with our model our estimated unemployment duration dependence preceding a recall. As Fernandez-Blanco (2013) 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 he only examines the cyclical implications for aggregate variables, such as job flows, unemployment, and vacancies.

On the empirical side of macroeconomic investigation, Shimer (2012) examines the "heterogeneity hypothesis" to explain the strong cyclical volatility of the overall job-finding probability of unemployed workers. That is, he asks whether this volatility is the result of composition effects in the unemployment pool, or rather whether all types of unemployed workers experience very 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 overall 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.

Shimer 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 monthly 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 selection by unobserved heterogeneity of individual job-finding probabilities 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 probabilities of the long-term unemployment "types" is the main cause of overall unemployment volatility. In our data, the long-term unemployed are mostly those workers who are not recalled ex post. Thus, we put some empirical flesh on the traits that are "unobserved" in Hornstein's approach. Ahn and Hamilton (2015) explain the cyclical volatility of the average job-finding probability through the composition of the inflow into unemployment by unobserved job-finding ability that they estimate with a dynamic unobserved component model. They find that the closest observable worker characteristic is Permanent Separation status.

3 Measurement of recall in the SIPP

3.1 Definitions: Labor force status and job identifiers

The SIPP is a collection of panels, each named after the year when it begins. In our analysis, we use the following eight panels: 1990, 1991, 1992, 1993, 1996, 2001, 2004, and 2008. Table A.1 in the Appendix reports the period covered by each panel, which varies from three to five years. Each interview in a panel covers the preceding four-month period, called a "wave." The first four panels, 1990-1993, have overlapping survey periods. 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, pre- and post-1996.

The SIPP assigns a unique numerical ID to each employer for each worker, for up to two jobs held simultaneously (EENO1, EENO2). This ID is the counter of the number of firms that the individual worked for until that point in time in the panel. For brevity, from now on we will refer to it as "job ID," although it is important to remember that it identifies an employment relationship and does not change when the worker is either promoted or otherwise asked to change duties by that employer. When a worker separates from an employer and, after a jobless spell, returns to the same employer, we call this event a "recall." We do not study "second round" recalls that occur after one or more spells of employment at a different company, possibly without any non-employment in between.

To build the sample of relevant jobless spells, we adopt the following criteria. First, we focus on individuals who are assigned "longitudinal weights" by the Census Bureau. This allows us to study the history of workers who participated in the entire survey. These weights are designed to make this sample nationally representative in terms of observable worker characteristics over the panel period. We also exclude so-called type-Z imputed observations.[4] We discuss below in more detail additional sample selection issues that can potentially impact our calculations and show that the effects are likely to be small.

The SIPP contains variables indicating the starting and ending dates of each job and weekly labor force status. For our analysis, we use a monthly panel. Specifically, we measure labor force status (employment "E," non-employment "$\not{E}$" that can be either unemployment "U" or out of the labor force "OLF") for each individual in the second week of each month, in line with the measurement in the CPS. We identify "$E\not{E}E$" completed spells of non-employment, of any positive number of months (a shorthand for $E\not{E}...\not{E}E$), where the individual experiences both a separation and an accession with a non-employment spell in between. To benchmark the frequency of recalls, we also identify spells of non-employment that either begin but do not end within the panel ($E\not{E}$) or are ongoing when the panel begins and end in employment ($\not{E}E$) within the panel. Later, we consider the cases where a worker separates into or is hired from unemployment (U), hence EU, UE, and EUE spells (again, the latter is a shorthand for $EU...UE$).

In measuring recalls, we restrict attention to jobless spells that begin with a separation in the first year (in the case of three-year panels) or the first two years (in the case of longer panels) of each panel. We adopt this sample selection to avoid right-censoring of jobless

[4] 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 have spuriously higher recall rates, thus biasing the aggregate recall rate upward. Our results are actually unaffected by the inclusion of these observations. However, we believe that excluding them is a prudent practice. Dropping the type-Z observations reduces the sample size for our analysis roughly by 7%.

spells due to the ending of the panel, ensuring that the jobless spell could last roughly two years and still be measured by the survey.[5] Similarly, to avoid left-censoring of spells that are ongoing at the beginning of each panel, when we benchmark recalls against all hires (rather than separations), we focus on jobless spells that end (with a hire) in the last year or last two years of each panel. 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.

While the SIPP uniquely provides all the pieces of information we need, it contains two types of measurement errors that are particularly relevant to our analysis, one in non-employment status (unemployment U vs. non-participation OLF) before the 1996 panel, and one in job IDs following certain types of non-employment spells since the 1996 panel.

The SIPP redesign in 1996 changed the definitions of labor force states, making them consistent with the monthly CPS, but not entirely comparable to pre-1996 SIPP panels. Figure A.3 in the Appendix shows a permanent downward jump in OLF between the 1993 and 1996 panels, matched by an upward jump in U and TL. In contrast, we do not observe a similar discontinuity in the monthly CPS around its own 1994 redesign. We are not aware of any literature documenting or even suggesting measurement error of TL vs. OLF status at entry into unemployment in the two major national surveys *after* their redesign. Since 1996, the definition of TL is identical in these two datasets and consistent over time. In Table A.6 in the Appendix, we compare the TL share of the flow into unemployment in the SIPP and in the monthly CPS over the periods covered by each SIPP panel after 1996. The shares are of similar magnitude and relatively stable over time. We conclude that the problem exists in the SIPP before 1996, when many unemployed workers (on TL by the definition of the post-1996 panels) were erroneously classified as non-participants in the pre-1996 SIPP panels, presumably because they were neither employed nor engaged in an active job search. Because no solution to this measurement issue is available, whenever we condition our analysis on labor market status, we focus on post-1996 SIPP data.

Stinson (2003) showed that job IDs in the SIPP 1990-1993 panels were subject to substantial miscoding, and then corrected the problem using confidential employer name information and administrative data containing employer-level job counts. This revision of job IDs makes it possible for us to correctly identify recalls in these early panels. We therefore view the aggregate recall rate computed from the 1990-1993 panels as reliable. This is a critical assumption on which we build our entire empirical analysis. We will provide below corroborating evidence supporting this assumption.

[5] Even with this sample selection, some of the non-employment spells are necessarily right-censored as a result of a long non-employment spell, and we treat these cases as non-recall.

In the mid-1990s, the Census Bureau introduced Computer-Assisted Personal Interviewing (CAPI) and redesigned the SIPP in its first new panel after that date, 1996. Among other improvements, CAPI reduced the level of post-collection edits and imputation and thus helped to maintain longitudinal consistency, including of job IDs, obviating the ex-post revision that had been necessary for the 1990-1993 panels. These improvements, however, did not eliminate one issue that already existed concerning job IDs. When a worker separates from an employer and is jobless for an entire four-month wave, the SIPP, by default, assigns different job IDs to the employers before and after the jobless wave(s), even if in reality they are the same company. The one important exception is when the worker is placed on TL, in which case the SIPP carries over the information about the last job ID, even after a long unemployment spell.[6] Stinson (2003) resolved this problem in the 1990-1993 panels by retrospectively considering information on each individual record for the entire panel, which was not available to interviewers and coders when the survey was ongoing.

To recap: we regard job IDs as reliable in the pre-1996 panels, while the labor market status in the SIPP is consistent with the CPS and over time only in the post-1996 panels. We will discuss other potential sources of mismeasurement that lead to the underestimation of recall rates and propose an imputation procedure to recover the missing recalls after 1996. But we first present evidence from the raw micro data.

3.2 Preliminary evidence on the incidence of recalls

We begin with empirical evidence on the frequency of recall among completed jobless spells $E\not{E}E$. Table 1 contains our main findings. The first two columns report the number of completed spells and the fraction that end in recall in the raw data. We note that this recall rate is very high, especially before 1996, but also after 1996, given that we know, for reasons explained in the previous section, that it is underestimated.

Three tables in the Appendix provide more detailed evidence on these results and on their robustness. First, Table A.2 reports the same recall rate, the share of all separations that start a complete jobless spell that ends in recall, in the 1990-1993 panels that we take as accurate, broken down by various worker and job characteristics. Recall is much more prevalent among older workers and union members, working in goods-producing sectors. But younger, non-unionized workers in service sectors still are recalled frequently and represent the vast majority of the U.S. workforce. The aggregate recall rates thus reflect more closely the latter groups. Gender and education do not make much of a difference for the recall

[6] Presumably, the purpose of this change was to lighten the survey collection and processing load, and the rationale was that in those cases the next employer should be new anyway, because any recall of unemployed workers who are not on TL tends to happen either quickly or not at all. However, using the pre-1996 cleaned job IDs, we show that this assumption is not totally warranted.

Table 1: Incidence of recall among job separations followed by complete jobless spells

Panel	Actual		Actual + Imputed							
	Spell Count	Recall Rate	Spell Count	Recall Rate	Spell Count	Recall Rate	Spell Count	Recall Rate	Spell Count	Recall Rate
	$E\not{E}E$		$E\not{E}E$		EUE		TL		PS	
1990	3,325	0.371	3,325	0.371						
1991	2,310	0.423	2,310	0.423						
1992	2,827	0.407	2,827	0.407						
1993	2,587	0.398	2,587	0.398						
1996	8,341	0.190	8,341	0.319	3,384	0.449	1,481	0.846	1,903	0.172
2001	3,904	0.209	3,904	0.328	1,553	0.455	678	0.867	875	0.168
2004	3,730	0.226	3,730	0.328	1,369	0.491	663	0.865	706	0.177
2008	4,935	0.262	4,935	0.412	2,756	0.532	1,354	0.866	1,402	0.236

Source: SIPP. Separations occur in waves 1-3 in the 1990-1993 and 2001 panels, in waves 1-6 otherwise.

rate. Next, Table A.3 reports recall rates as a share of all non-employment spells, $E\not{E}$ and EU, many of which are incomplete spells; the workers in those cases end up in retirement or persistent non-participation and hence are not even available for a recall. The share of all jobless spells that end in recall is only slightly lower as compared to complete spells in Table 1. Finally, Table A.4 reports the fraction of *hires* that are recalls, ending both incomplete and complete jobless spells. The numbers are almost identical to those of separations: a large share of all hires are of former employees. We again observe a drop in recall rates following the 1996 redesign. Before addressing this drop, we briefly discuss two potentially important measurement issues which may bias our estimates of average recall even before 1996: selective attrition and seam bias.

Accurate job IDs in the pre-1996 panels are not sufficient to guarantee accurate measurement of recall rates. A specific concern is survey attrition. The SIPP is not address-based, like the CPS, but aims to track respondents also when they move. Nonetheless, this may not always be possible, and some correlation between geographical mobility and attrition may be unavoidable. If TL workers are more likely to stay put, waiting for a recall, than PS workers, or are more likely to answer the survey because they less busy looking for work, they may be less subject to survey attrition and increasingly skew the unemployed sample toward workers who are more likely to be recalled.[7] Our average recall rates use longitudinal weights to correct for differential attrition by observable worker characteristics. We then estimate a Probit regression of attrition on a rich set of demographics and on labor force

[7]Indeed, attrition rates in the SIPP are high. Slud and Bailey (2006) estimate in the 1996 panel that 30% of all respondents to wave 1 did not complete the survey. They examine implications for some variables, but not for recall. For our longer sample period, we find even slightly higher attrition rates.

status (TL, PS, and OLF) at separation as a proxy of unobservable heterogeneity in the propensity to be recalled. We find that, although the attrition rate is significantly larger in expansions and for job losers (relative to employed workers), within job losers PS workers are only 0.5% more likely to leave the survey than TL at every wave (see Table A.8).

It is well known that many types of transitions, especially between labor force states, tend to be reported in the SIPP at the "seam" between two waves (see Bound et al. (2001) for a detailed statistical analysis). We can detect this phenomenon in all panels, even 1990-1993, because Stinson's (2003) validation of job IDs in those panels focused on the identity of the employers, not on the *timing* of labor market transitions, which is measured with error. However, we see no reason why in those early panels the seam effect should bias the *average* recall rate and, in fact, we find that it does not. In Table A.14, we consider "short" $E\not{E}E$ spells with non-employment duration of less than or equal to two months, and we split this sample into two types: one where the entire $E\not{E}E$ spell occurs within a wave and the other where it crosses the seam between waves. Recall rates before 1996 are essentially the same for these two samples (48% vs. 49%). However, we do need to worry about post-1996 observations. Indeed, job IDs tend to change disproportionately when the non-employment spell crosses a seam: the recall rate drops from 48% of within-wave spells, the same as before 1996, to 32% when similar spells cross a seam.

To recap, in the SIPP, we find no evidence of mismeasurement of recall in the 1990-1993 panels, but we also identify two reasons why recall rates are underestimated in the post-1996 panels. First, job IDs are reset by default after an entire wave of non-employment, making it impossible to directly detect a recall. Second, recall rates are much lower when a short spell of non-employment crosses a seam, likely due to job ID miscoding. We now propose and implement a procedure to impute those "missing recalls."

3.3 Imputation of recall in post-1996 SIPP panels

To perform the imputation, we first split the sample into "short" and "long" spells of non-employment, lasting (resp.) up to two months and three months or longer. In each case, we use a "reference sample" to estimate a logit regression that predicts recall given observable worker and spell characteristics, such as non-employment duration, switching of occupations, and many others, and then use the estimated coefficients to perform multiple randomized imputations for each relevant spell. Tables A.11 and A.13 in the Appendix report the specification and results of the imputation regressions.

For the short spells that begin as TL, we assume that job IDs, hence recalls, are measured accurately, whether or not these spells cross a seam. This is because the SIPP preserves the job ID for TL workers. For short spells that do not begin as TL, we assume that job IDs are accurate when the spell does not cross the seam, because the within-wave recall rate is identical to the pre-1996 benchmark (Table A.14). For the remaining short spells that do not start as TL and then cross a seam, the strongest predictor of recall in the logit is 3-digit occupational mobility. To be conservative, when we observe such an occupational switch after crossing a seam, namely, when the worker reports two different occupations in the two consecutive interviews, we directly mark no recall. This choice follows from the observation that, among these short cross-seam spells, less than 10% of the occupational switchers in the pre-1996 panels are recalled (Table A.14). This choice is conservative because crossing a seam introduces error also in occupational codes, turning stayers, who were likely to be recalled, into switchers. So our final imputed recall rates are still likely to be biased downward.

The reference sample for the imputation of recall after short $E\not{E}E$ spells that do not start as TL, do not result in an occupational change, and cross the seam, is the sample of analogous short spells that do not cross the seam. Here, we only exploit the post-1996 data for the imputation regression, so that we can use the labor market status variable (PS or OLF), which is reliable after 1996. The recall rate after imputation is reported in the last row of Table A.14. The imputation here does not make a major difference.[8]

The reference sample for the imputation of recall after long jobless spells post-1996 is the analogous sample in the 1990-1993 panels (i.e., $E\not{E}E$ with three or more months of non-employment $\not{E}$). Because measurement of labor market status in the SIPP is not comparable before and after 1996, we do not use that information in the estimation. Hence, we also impute recalls for those on TL, even though their job IDs and recalls are measured accurately in the post-1996 panels. This is necessary to avoid selection by labor market status, which is obviously non-random and likely correlated with recalls.[9] Table A.12 reports the results. The imputation raises the recall rate from 0.11 to 0.34, a level comparable to the one in the pre-1996 data. For TL workers, we impute a recall rate of 72%, which is very close to the actual one (77%) that we observe without error. This is an important result that validates our imputation procedure, given that it does not utilize explicitly *any* information on unemployment status (TL/PS). Evidently, the other spell and worker characteristics used in the imputation regression capture correctly the TL status and thus recall. In contrast to

[8]Part of the reason can be attributed to our assumption that, for short spells that do not start as TL and cross a seam, a change in occupation means a change in employer. Given this assumption, the imputation eliminates a few recalls that were in the raw data and assigns a recall only to few occupation switchers on TL, reducing (from 2% to 1%) the recall rate of all occupational switchers.

[9]The key assumption is that the relationship between observable worker characteristics and probability of recall did not change over the last 25 years. While there is no direct test of this assumption, the average recall rate (as a share of total hires) in the Quarterly Workforce Indicators (QWI), assembled from error-free, administrative data, shows a pronounced countercyclicality and a very modest downward trend in 1995-2012, just like our imputed recall rate in the SIPP. See subsection 5.2 for details.

the case of short spells, the imputation of long spells makes a large difference in the aggregate recall rate.

To summarize, we impute recalls only after 1996 and only when the jobless spell either lasts three months or more, or lasts one or two months, begins not on TL, ends after crossing exactly one seam, and does not generate an occupational transition. Quantitatively, almost all action occurs in the former case, long spells, whose imputed recall rates are three times the observed ones. We can validate these imputed rates independently, as they are almost identical to the results from two reliable subsamples: the reference sample before 1996, and the TL subsample after 1996. The impact of the seam bias on short spells is much smaller, and the imputation only affords a modest correction.

In the Appendix, we provide additional evidence of the validity of our imputation procedure based on another "in-sample forecast." We discard randomly half of the (valid) observations in each reference sample and re-impute them; we recover the observed recall rates nearly perfectly on average, with equal Type I and Type II errors of about 15%.

In Table 1, we present the estimated recall rates, by SIPP panel after 1996, resulting from our imputation procedure. Close to 40% of all completed jobless spells $E\not{E}E$ end up in a recall. The center columns of Table 1 further restrict attention to "attached" workers, who separate into unemployment but stay in the labor force. Recall rates are now close to 50% of all complete unemployment spells EUE. These are strikingly large numbers. Table A.3 in the Appendix repeats the exercise of Table 1 for all separations into non-employment $E\not{E}$, including permanent ones like retirement. Their recall rate is about 30% and rises again over 40% for all separations that begin with unemployment, EU. Finally, Table A.4 in the Appendix computes recalls as a share of all *hires* from non-employment; the results are almost identical to those for separations from Tables 1 and A.3. In both separation- and hiring-based measures, a visible drop in recall rates remains between pre- and post-1996 panels even after imputation, suggesting that our procedure is conservative.

So far, we used the TL/PS classification merely to correctly build various types of jobless spells and to impute recalls after some short spells. This distinction is also of independent interest to draw the important distinction between ex-ante expectations of a recall (TL), which is a traditional subject of investigation, and ex-post recall outcomes, which we measure for the first time in a comprehensive manner. The last columns of Table 1 break down the complete unemployment spells EUE in the middle columns by detailed unemployment status, TL or PS, at the time of separation. While the vast majority of TL are recalled, as they (and we) expected, a sizable fraction of them still change employer. More interestingly, close to 20% of PS workers, who did not expect to be recalled upon separation, are recalled nonetheless. This share is close to a quarter in the 2008 panel. Because PS separations are much more frequent than TL, the contribution of these "unexpected recalls" of PS workers to the overall recall rate is sizable. The cross-sectional correlation between TL and recall is 0.67, high but still very far from one. As we will see shortly, TL and recall differ even more in terms of cyclicality. This key result reveals an important distinction between ex-ante expectations of recall, as measured by TL status, and ex-post outcomes, as measured by recall. It also provides additional reasons not to dismiss recall as a relic of the manufacturing-based, unionized economy of the 1970s and early 1980s. Finally, it motivates our focus on recall as a distinct phenomenon from the better-known TL, and the need to work with the SIPP as the best source of information on recalls.

4 Recall and labor market experience

Having measured recall and shown that it occurs frequently, we now provide evidence that the labor market experience of recalled workers markedly differs from that of new hires. First, recalls are associated with stronger attachment to the employer, both before and after the jobless spell, so they appear to reflect some form of firm-specific knowledge. Second, recalls are widespread in the population and not overwhelmingly concentrated among few individuals. Third, recalls occur quickly, while workers who are not recalled spend much longer being unemployed. Fourth, the probability of recall starts high and sharply declines with unemployment duration; in contrast, unemployment spells that end in new hires exhibit modest duration dependence. This evidence will inform our modeling strategy. The first and third facts will motivate our assumption that recalls are free and instantaneous, while new hires are generated by a matching function, customarily used to formalize the costly and time-consuming meeting process between job vacancies and the unemployed that is due to imperfect information about match quality. The second fact will motivate our choice to model recall as the result of selection by ex-post match heterogeneity, affecting ex-ante homogeneous workers. The last two facts will inform our modeling of this selection process.

4.1 Employer attachment and recall

It is well known that the hazard rate of separation from a job is strongly declining in tenure. A standard rationale is that tenure with an employer measures some form of match-specific quality, due to either selection of good matches or accumulation of specific human capital. A recall, by definition, brings a worker back to the employer where he/she already has some tenure. Hence, we expect a positive correlation between tenure, both before and after separation, and recall. Indeed, Table 2 shows that workers who had longer tenure at the

Table 2: Job tenure before separation and probability of recall

Tenure/Panel	1996	2001	2004	2008
<1 year	0.350	0.342	0.403	0.439
$1-3$ years	0.454	0.414	0.456	0.523
≥ 3 years	0.635	0.645	0.649	0.639

Source: SIPP. Based on EUE sample.

time of separation are more likely to be recalled.[10]

To investigate whether a recall predicts employment duration with the same employer *after* the first completed $E\not{E}E$ spell in a panel, i.e., the employment spell that begins with the "second" E in $E\not{E}E$, we use validated job IDs from the 1990-1993 panels. To compute the length of this second employment spell, we ignore subsequent separations that are followed by a recall within the time span of the panel, and we continue increasing tenure until either the worker moves to non-employment, or to another job, or the panel ends, which right-censors the spell. Table 3 reports the resulting average duration of the second employment spell in a panel including both completed and right-censored spells. Employment spells that begin in the first five waves of each panel are less likely to be right-censored, even more so in the 1992-1993 panels that have one more wave than 1990-1991. From this table, it is very clear that recall predicts more stability in the ensuing employment relationship. Hence, it looks as if pre-displacement tenure is not "reset," but resumed upon recall.

In Table A.5 in the Appendix, we compute recall rates by focusing on "stable" employment relationships. We select complete jobless spells that are bracketed on either side by at least three months of continuous employment in the same firm, instead of one month in our baseline calculations. This selection cuts the sample size in half, because it drops spells where new hires separate again within three months, and separation begets separation. Under this stricter selection, recall rates are even higher. This evidence is consistent with the fact that new employment relationships are more fragile than recalls.

4.2 Temporal correlation of recalls

Our main focus is on the share of non-employment *spells* that end in a recall. Are these spells concentrated among a relatively small number of *workers*, who "cycle" in and out of employment, or rather is the incidence of recall in the workforce widespread? We answer this question with data from the 1990-1993 SIPP panels, where job IDs are accurate. Of all workers who individually experience at least one recall in a panel, 77% experience only one recall and contribute 58% of all recall events. If we compute the recall rate using only the

[10]Here our calculation focuses on EUE spells, but the same pattern emerges from $E\not{E}E$ spells.

Table 3: Mean employment duration (in months) after the first complete jobless $E\not{E}E$ spell

	Recall	New Hire	Recall	New Hire
	1990 Panel		1991 Panel	
All Spells	10.7	6.8	11.6	7.1
Separation ≤ Wave 5	14.9	9.4	15.9	10.6
	1992 Panel		1993 Panel	
All Spells	13.5	7.5	12.3	8.1
Separation ≤ Wave 5	19.0	10.4	18.1	12.1

Source: SIPP. Mean duration can be right-censored by the end of the panel. Second and fourth rows consider only the cases where a transition into non-employment occurs at or before Wave 5.

spells of those who are recalled at most once, so excluding altogether the "serially recalled" workers, the average recall rate drops to a still sizable 33% of completed non-employment spells, compared to about 40% for all such spells (Table 1).[11]

4.3 Unemployment duration and recall

We now turn to the association between recall and unemployment. As explained earlier, unemployment (as opposed to non-employment) is measured accurately only after 1996, so we focus on those panels. Table 4 summarizes the information about unemployment duration in the sample of completed unemployment spells (EUE sample) by their destination (recall or new hire). First note that recalls occur more quickly than new hires.[12] Similarly, the dispersion of unemployment duration is smaller for those who are eventually recalled. Average duration is clearly countercyclical for both. For recalled workers, 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 4.21 months in the 2008 panel, which covers the Great Recession and the subsequent anemic recovery period. Interestingly, the proportional increase in average duration is twice as large for non-recalls than for recalls. Similarly, from the standard deviations, the dispersion of unemployment duration across workers is countercyclical, especially for non-recall hires.

[11] This second sample selection runs into a censoring problem due to the end of the panel that leaves many non-employment spells incomplete and exaggerates temporal correlation. Presumably, the spells of rarely recalled workers are more likely to be censored, because the "serially recalled" workers cycle quickly in and out of employment. If we focus on recalls following separations that occur in the first three waves (twelve months) of each panel, the share of single recalls rises to 83%. See Table A.16 for details.

[12] Mean unemployment duration in Table 4 (first column) is estimated very precisely. As a sample mean, its standard error is simply "SD" in the second column of each panel divided by the square root of "Count" in the third column. In all cases, the result is under .2 months. The same order of precision applies to mean employment duration in Table 3.

Table 4: Unemployment duration and recall

Panel	Overall			Recall			New Hire		
	Mean	SD	Count	Mean	SD	Count	Mean	SD	Count
1996	2.50	2.14	3,384	2.26	1.79	1,605	2.70	2.37	1,779
2001	2.65	2.62	1,553	2.15	1.93	742	3.06	3.01	811
2004	2.48	2.35	1,369	2.09	1.75	719	2.86	2.76	650
2008	4.21	5.51	2,756	2.95	3.49	1,523	5.65	6.86	1,233

Source: SIPP. Based on the EUE sample.

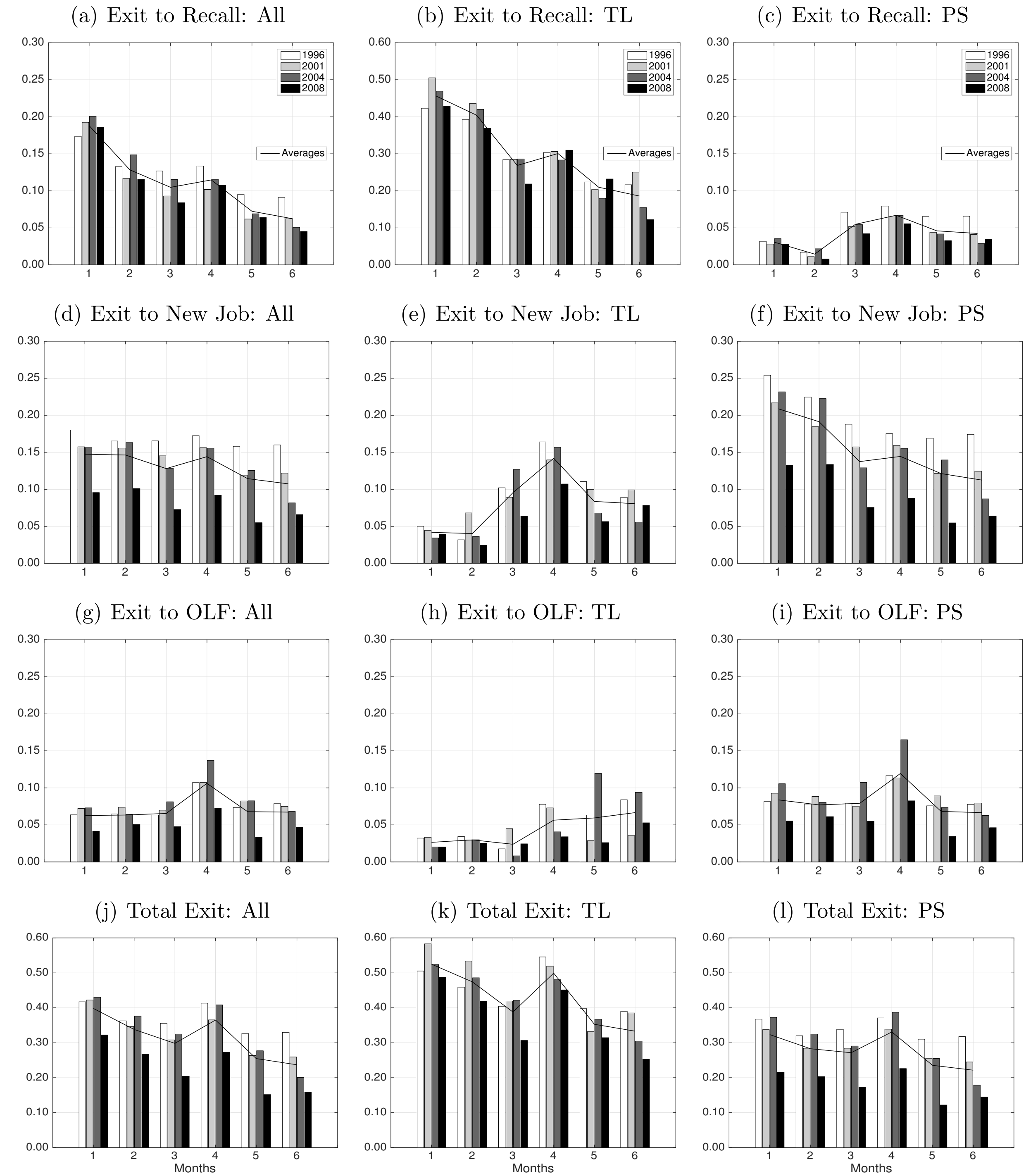
Recent U.S. experience rekindled interest in the prospects of the long-term unemployed. Figure 1 plots the discrete hazard functions, calculated non-parametrically, for exit from unemployment by duration. The sample covers all separations into unemployment (i.e., EU sample), including unemployment spells 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), a new employer (second row), non-participation (third row), and any of those (fourth row, summing the first three). The columns condition the hazard on labor market status in the first month of unemployment, in order: all and, for illustration, TL and PS. Each bar represents a different SIPP panel and the line represents the (unweighted) average across four panels. Of course, the three outcomes (new job, recall, exit to OLF) are mutually exclusive. The hazards are therefore competing, and the occurrence of an event censors the spell for other outcomes.

The first column illustrates the exit hazards from unemployment to different outcomes of job search, which add up, in the last row, to the total hazard rate. The strongest negative duration dependence appears in recalls (Figure 1(a)), while the hazard for those who exit unemployment by finding a job at a different employer (d) is much flatter.[13] An even flatter hazard appears for exit to non-participation (g). As a result, overall duration dependence is negative mostly due to the fading chance of a recall as unemployment continues. This is a novel and, we believe, important finding, but we stress it is limited to the first six months of unemployment, which still includes the vast majority of unemployed workers.

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 (Figure 1(b)). Their expectation of being recalled is clearly reflected in the next

[13] Figure 1 in Kroft et al. (2013), based on monthly CPS data from 2008-2011, shows that the transition rate from unemployment to employment drops by about two-thirds (half) when moving from zero (resp., one) to five completed months of unemployment. Their result corresponds to the sum of (a) and (d) in our Figure 1, although we define months of unemployment as incomplete and hence start the hazard from one month. We observe almost identical proportional drops in the hazard rate with duration in the SIPP 2008 panel, which also covers the post-2007 period.

Figure 1: Hazard functions: 1996−2008 panels



Source: SIPP. Based on the *EU* sample. Labor market status (PS or TL) is based on the status at the time of separation into unemployment.

two rows: the exits to new jobs (e) and non-participation (h) are negligible in the first few months of unemployment and then rise when the expected recall does not materialize.

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 declines only slightly (Figure 1(f)). After three months, that chance further drops, but the chance of a recall rises (c). Overall, the chance of recall is small but non-negligible, and it appears that PS workers are recalled after they failed to secure a new job quickly. Again, exit to non-participation is flat in duration (i). Total exit from unemployment (l) consequently is mildly falling in duration.

Figure A.1 in the Appendix completes the picture by illustrating the share of hires that are recalls at each duration. As should be clear from Figure 1, this share is declining in duration, but only due to the declining chance of recall of TL. Taken all together, these results suggest 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.

Figure 1 also breaks down hazard rates by SIPP panel. The most salient comparison is between the 2008 panel (black), which covers a period of extremely high national unemployment, and previous periods, especially the 1996 and 2004 panels (white and dark gray) when unemployment was low. Exit rates to new jobs drop by about half in the 2008 panel at all durations, while exit rates to recall barely drop. This illustrates a dramatic difference in the cyclicality of the two types of re-entry into employment, an important finding that we will return to shortly. The well-known cyclical volatility of job-finding probabilities (Shimer (2005)) is actually significantly more pronounced if we exclude recalls from accessions. Exit to non-participation also declines in the 2008 panel, although not nearly as much, consistent with the decline in the transition rate from unemployment to non-participation observed in the CPS during and after the Great Recession.[14]

From the last piece of 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. To complete our empirical investigation, we now move to explore systematically the relationship between recall and business cycles.

[14]Note that the decline in the labor force participation rate observed in and after the Great Recession is not inconsistent with the lower transition rate from unemployment to non-participation, because the transition rate from non-participation to employment declined even more.

5 Aggregate time series evidence on recalls

5.1 Survey of Income and Program Participation

The recent debate on unemployment fluctuations revolves around job-finding rates. To study how recalls impact the behavior of the overall job-finding rate, this section considers the recall rate defined as the share of *hires* that are recalls. On average, this share roughly matches that of separations that end in recall (compare Table 1 for separations and Table A.4 for hires). We now study its cyclical properties. This evidence will inform our theoretical analysis. After dropping the observations from the first year of each panel to avoid the left-censoring of $E\not{E}E$ spells, we end up with 69 quarterly observations of the hire recall rate, spanning 1990Q4 to 2013Q2. Only since 1997Q1, hence for 49 observations, we can also rely on the distinction between unemployment U and OLF and calculate recall shares of hires from U. Figure 2(a) illustrates the resulting time series, seasonally adjusted by regression on seasonal dummies, logged and filtered with a cubic time trend, because gaps in the time series make HP-filtering infeasible, along with the seasonally adjusted unemployment rate from the BLS, also logged, and HP-filtered with parameter 10^5 as in Shimer (2005) (and also other series discussed below). It is possible to detect visually a rise in the recall rate during recession times, after 2001 and especially during the Great Recession of 2008-2009, as well as a sharp decline during the tight labor market of the mid-2000s. As just discussed, this is all the result of procyclical probabilities of finding employment, either at the previous or at a new employer, with the latter being much more volatile. Because the probability of finding a new job is very procyclical and that of leaving the labor force is small, the competing hazard of being recalled is countercyclical, conditional on exiting unemployment.

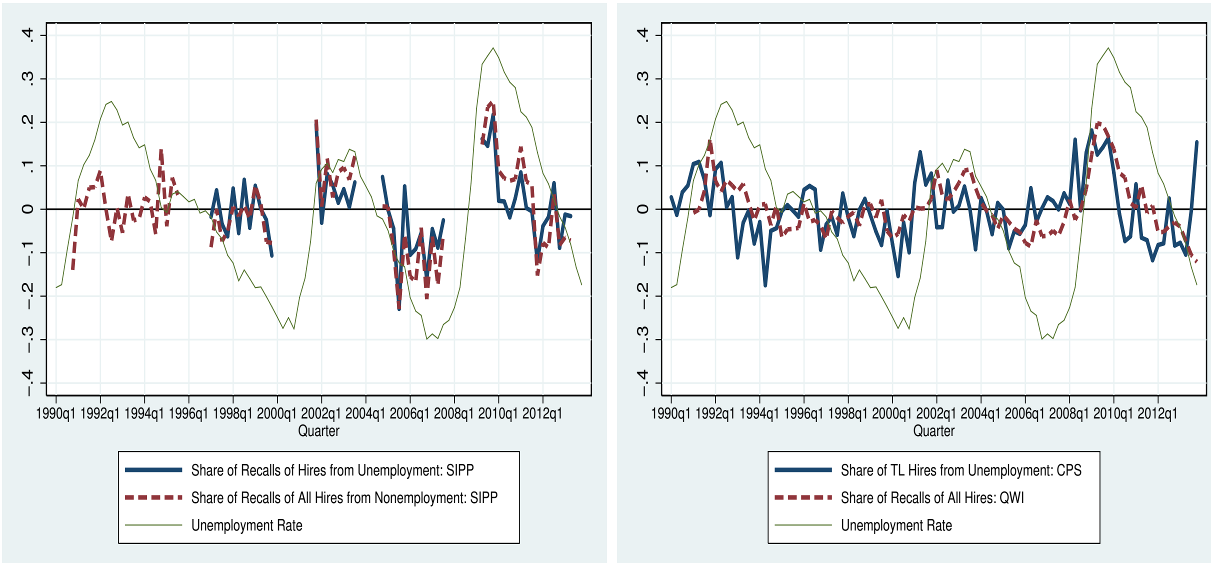
We supplement this fragmented time series evidence with additional evidence from the Quarterly Workforce Indicators and from the monthly CPS, which allow us to construct uninterrupted time series. While both sources are dominated by the SIPP to measure recall, they do contain useful ancillary information to understand its cyclicality.

5.2 Quarterly Workforce Indicators

The Longitudinal Employer-Household Dynamics (LEHD) program at the Census Bureau provides a matched employer-employee administrative dataset of quarterly employment and earnings for virtually the entire U.S. private and state sector. The LEHD has a limited time span, as states joined the program only gradually, starting in the early 1990s with several states such as CA, ID, MD, OR, WA, and WI. Other states joined later, many in the 2000s, and now nearly all states are in the program. The Census Bureau publishes aggregate tabulations of major labor market variables from the LEHD under the name Quarterly

Figure 2: Cyclicality of recall and unemployment rates

(a) SIPP Recall Rates | (b) QWI Recall Rate and CPS TL Hires



All series are seasonally adjusted. The unemployment rate, QWI recall rate, and the share of TL hires from unemployment are logged and detrended by the HP filter with smoothing parameter of 10^5. SIPP recall rates are logged and detrended by a cubic polynomial trend. The QWI recall rate is the average across the U.S. states where the recall series are available at each point in time.

Workforce Indicators (QWI). See Abowd et al. (2009) for a detailed description of LEHD and QWI. One such QWI tabulation is "Recall," the probability that a hire by an employer in quarter t had earnings from the same employer in any of the three quarters $t-2$, $t-3$ or $t-4$ (but not in quarter $t-1$, because the worker is "hired" in t). Calculated as a share of total gross hires, the QWI recall rate most closely corresponds to the results in our Table A.4. We study its level and cyclicality.

The QWI recall rate averages about 17% of all hires, less than half of our estimate from the SIPP. Measurement of recalls in QWI differs from the SIPP in two important respects, which contribute in opposite ways to the average recall rate. Both issues arise because the underlying LEHD dataset lacks information about labor market spells. On the one hand, QWI do not distinguish between hires from non-employment and from other firms. So recalls in QWI include those that occur after the worker spent a few months with another employer. In this sense, it is a broader notion, and the recall rate should be higher in QWI than in the SIPP, where we focus on recalls from non-employment. On the other hand, QWI suffer from severe time-aggregation bias, because of their quarterly measurement. Specifically, QWI fail to detect altogether any non-employment spell that starts and completes with a recall within a full calendar quarter. Because recalls are quick and follow mostly short non-employment spells, many of them are missed. Applying the LEHD-QWI sampling procedure to our SIPP data reduces the estimated recall rate to a level consistent with that in the QWI, providing further support to the accuracy of our measurement of recalls. Details are in the Appendix.

The QWI recall rate is strongly countercyclical. We collect an unbalanced state panel of quarterly recall rates and unemployment rates for the 32 states where the QWI recall series are available at least since 1999. We seasonally adjust the series, take log and HP-filter both state-level recall and unemployment rates, with smoothing parameter 10^5. We use these state-level data in the regression analysis in the next section. In Figure 2(b), we present, as a summary aggregate measure, the time series of the unweighted average recall rate from these states. Its correlation coefficient with the national unemployment rate is 0.74.

5.3 Monthly Current Population Survey

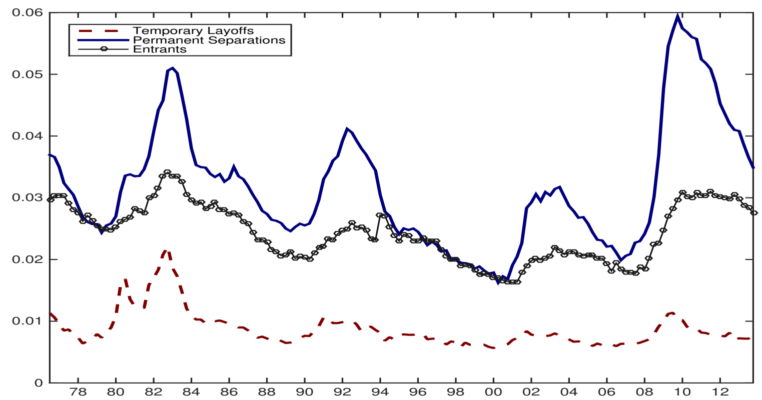
The monthly CPS data are the most widely used source of information for labor market flows. Unlike the SIPP and the QWI, they do not contain employer information, and thus cannot be used to measure recall. They can, however, provide a long unbroken time series of the share of hires who were on TL, out of all hires from unemployment.[15] We plot this series (quarterly averages of monthly shares) in Figure 2(b), logged and HP-filtered with parameter 10^5. While countercyclical, like the SIPP recall rate, its correlation with the latter is only 0.29, positive but far from perfect, highlighting once again a significant difference between ex-ante TL and ex-post recall.

The time span of the monthly CPS affords a longer perspective on the issue of recalls and TL. Labor market researchers paid decreasing attention to TL, due to the observed decline in its level and cyclicality (Groshen and Potter (2003)), which tracked the decline in the relative importance of the manufacturing sector, where TL and recalls were common (up to 70% recall rate in 1965-1976, Lilien (1980)). Our empirical evidence should lead us to rethink this assessment for two reasons.

First, the decreasing incidence of TL in the CPS is observed in the stock of unemployment, but not much in the flows. Figure 3 plots unemployment stocks by reason, all 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 indeed a relatively small share of the unemployment stock, especially after the mid-1980s. Moreover, the increase in the TL stock during the last three recessions has been modest. But TL are still a much larger fraction of the flows in and out of unemployment than in the stock of unemployment. The reason for the stock-flow discrepancy is that TL spend much less time in the unemployment

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

Figure 3: Unemployment stocks by reason



Source: Monthly CPS. Expressed as a fraction of the total labor force.

pool than average. So, if one is interested in worker flows, TL still matter, even today. Figure 4 shows quarterly averages of monthly probabilities of entry into and exit out of unemployment, by type of inflow, TL and PS. To construct these series, we use the duration data as in Shimer (2012), combined with the data on reason for unemployment.[16] In Figure 4(a), the TL inflow amounts to slightly less than one-half of the PS inflow, a ratio that has remained fairly stable since 1976. The two inflow rates move more or less in parallel over business cycles, both showing a marked countercyclical pattern. In Figure 4(b), workers on TL enjoy a much higher exit probability than PS workers; note also that both exit probabilities exhibit the familiar procyclicality, but it is more pronounced for PS workers. After the Great Recession, the exit probability recovered rather quickly for TL, but slowly for PS.

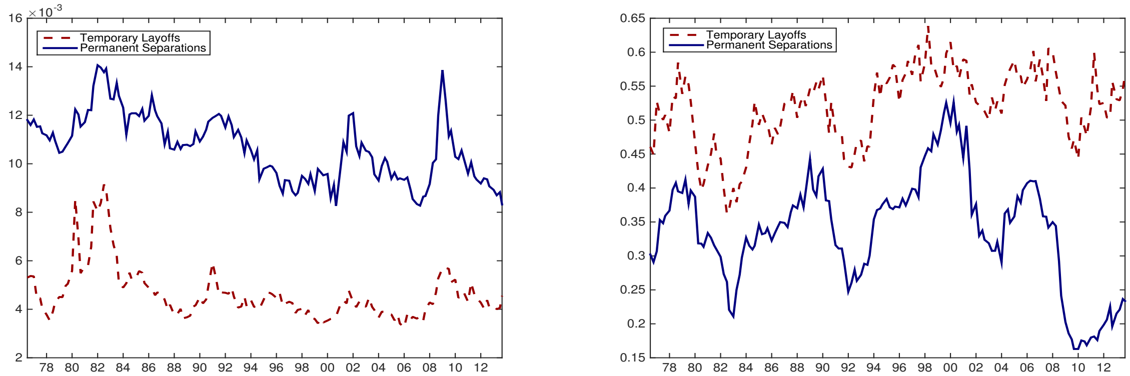
Second, and more important, TL are only part of the story. We showed in the SIPP that PS workers, who have no clear expectation of recall, nonetheless return to their former employer with surprisingly high frequency, about 20%, and this frequency has not declined over the last two decades. Although this frequency of PS recall is still much lower than that of TL recall, a significant share of recalls originate from the (much larger) stock of PS workers, who did not expect a recall. Therefore, even though one can see from Figure 4(a) that the relative importance of TL in the *EU* inflow has diminished in the last three

[16] The entry probability is computed as short-term unemployment (less than 5 weeks) of each type normalized by the total employment stock; the exit probability is the outflow of each type normalized by the corresponding unemployment stock (PS or TL). By construction, exit probabilities include not only those who find a job but also those who drop out of the labor force. Due to the redesign of the CPS in 1994, the raw data exhibit a break in these series at the start of 1994. We adjust the break, following the adjustment procedure proposed by Elsby et al. (2009).

Figure 4: Unemployment entry and exit by reason

(a) Entry Probability

(b) Exit Probability



Source: Monthly CPS data on unemployment duration and reason for unemployment. See footnote 16 for more details.

recessions, "recallable" workers remain quantitatively very important. Furthermore, as we will document shortly, the job-finding probabilities of unemployed workers who started the spell as TL and PS are both cyclical, while the exit rate from unemployment to recalls is much less cyclical than that to new jobs.

Figure B.1 in the Appendix provides supplementary evidence from the monthly CPS matched records, which allow also to distinguish between exit from unemployment to employment, as opposed to non-participation. The PS flow into unemployment is twice as large as the TL flow, and both transition probabilities into unemployment are countercyclical. The job-finding probability of unemployed on TL is much higher but much less cyclical than for PS and entrants.

5.4 Summary: Business-cycle moments

In Table 5, we present the volatility of the various detrended recall rate measures, as well as their elasticity (regression coefficient) with respect to unemployment rates, which is our preferred measure of cyclicality. In the first four columns, we report the results based on our recall measures from the SIPP. For the QWI recall rate (fifth column), the volatility refers to the unweighted average series described above, while the elasticity is from a panel regression where we regress state-level recall rates on state fixed effects and unemployment rates. In the last column, we consider the CPS-based TL share of hires. Interestingly, all measures are similarly and highly volatile, only slightly less than the unemployment rate, as can be inferred from Figure 2. The SIPP measures are more volatile than QWI and CPS-based

Table 5: Cyclicality of recall rates

	$\not{E}E$ Recall Rate	UE Recall Rate	$E\not{E}E$ Recall Rate	EUE Recall Rate	Recall Rate (QWI)	Share of TL Hires (CPS)
Volatility	0.097	0.084	0.105	0.082	0.062	0.074
Elasticity w.r.t	0.348**	0.244**	0.412**	0.222**	0.246**	0.083
Unemployment	(0.051)	(0.052)	(0.055)	(0.052)	(0.012)	(0.047)
R^2	0.406	0.316	0.390	0.316	0.162	0.042
# of Obs.	69	49	69	49	2,317	99

Source: SIPP, CPS, and QWI. All series are seasonally adjusted, logged, and detrended. The QWI result is from the state-level fixed effect regression described in the text. The remaining regressions use the time series of the aggregate recall rate and the national unemployment rate. Robust standard errors are in parentheses. ** (*) indicates statistical significance at 1% (5%) level.

measures. In contrast, the unemployment elasticities are high and similar in the SIPP and QWI, which measure genuine recalls, but much smaller for the CPS share of TL accessions. This is one of our central findings: the distinction between TL and recalls is quantitatively important in terms not only of average levels but especially of their volatility and cyclicality. In short, the incidence of recalls is higher and much more countercyclical than that of TL.

6 A stochastic search model with recall

In order to make sense of this evidence and understand its relevance to unemployment dynamics, we introduce a recall option in the Mortensen and Pissarides (1994) economy and study quantitatively its equilibrium response to aggregate productivity shocks.

6.1 Setup

Time is continuous. All agents are risk neutral and discount payoffs at rate $r > 0$. Firms produce a homogeneous consumption good using a CRS technology and sell it in a competitive market. The flow output from each firm-worker match equals $p\varepsilon$, where $p > 0$ is an aggregate component common to all firms, while ε is an idiosyncratic component. Both p and ε evolve according to a Markov chain: at Poisson rate λ_p a new draw of aggregate productivity p' is taken from $dP\left(p'|p\right)$, and at Poisson rate λ_ε, a new match value ε' is drawn from $dG\left(\varepsilon'|\varepsilon\right)$ while the worker is employed. Here we introduce our main modeling innovation, which gives rise to a recall option: worker and firm can suspend production and, as long as the worker does not take another job, the value ε of the (potential re-)match between the employer and the worker continues to evolve, according to the same Poisson rate of arrival λ_ε and a conditional distribution $dH\left(\varepsilon'|\varepsilon\right)$, possibly different from $dG\left(\varepsilon'|\varepsilon\right)$. The lowest possible match quality is equal to zero and an absorbing state, so when ε drops to $\varepsilon' = 0$, the match becomes permanently infeasible, as it will produce nothing thereafter. Exogenous separations may be thought of as transitions to $\varepsilon = 0$. In contrast, the rest of P, G, and H are irreducible.

There are search frictions in the labor market. In order to create new matches, unemployed workers spend search effort $s \geq 0$ at cost $c(s)$ to find, at rate $s\phi$, open vacancies, which are posted at a flow cost $\kappa > 0$ as in the standard model. $c(\cdot)$ is twice continuously differentiable, increasing, and convex, with $c(0) = c'(0) = 0$. Old matches that separated can be reassembled at any time at no cost to either party, if still unmatched. Let u denote unemployment (rate) and $\bar{s}$ the average search effort of the unemployed, so that $\bar{s}u$ is aggregate search effort by unemployed workers. Let $\theta = v/(\bar{s}u)$ denote labor market tightness, the ratio of open vacancies to aggregate search effort. We assume that the flow of new contacts between open vacancies and job searchers equals $m(v, \bar{s}u)$, where m is a standard continuous and homothetic matching function. Thus by random matching each open vacancy is contacted by a searching worker at rate $q(\theta) = m/v$, where q is continuous, decreasing, and convex and $\phi = \phi(\theta) = \theta q(\theta)$ is the worker contact rate per unit of search effort.

When an unemployed worker and vacant firm do meet for the first time, they draw from a distribution F an initial match quality $\tilde{\varepsilon}$. If they accept the match and start producing, the worker must forfeit the recall option with his former employer(s) and simultaneously acquires a job and a future recall option with this new employer. 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 irreversible) can either (i) wait and do nothing ("mothball" the vacancy); (ii) recall the last employee if still unemployed; or (iii) pay κ and re-post the vacancy, to contact at rate $q(\theta)$ a random unemployed worker who is searching and draw from F a new match productivity $\tilde{\varepsilon}$. Free entry in vacancy creation drives to zero the expected value to a firm of searching for a new employee.

Wages in ongoing matches are set by generalized Nash Bargaining, with the worker receiving a share $\beta \in (0, 1)$ of match surplus. Unlike in the standard model, the outside options when bargaining are not obvious, as now separation is not irreversible and hence is not a credible threat in a non-cooperative foundation. We assume that the outside option is temporary separation until the next productivity shock occurs and triggers a possible recall; in the meantime, parties can look for other partners and better matches. Firms have no commitment power, not even to a once-and-for-all lump-sum transfer, and wages are continuously renegotiated, so that search effort by either side is not contractible.

In order to match, a firm and worker who meet and draw match quality $\tilde{\varepsilon} \sim F$ gain a positive surplus not only over the alternative of rejecting the new match and continuing search, but also over waiting for $\tilde{\varepsilon}$ to improve through the law of motion H. That is, the new match quality $\tilde{\varepsilon}$ must be good enough to begin production right away; otherwise, it is lost. This assumption of "No Mothballing Before Production" captures the idea that a new match requires some initial phase of discovery and experimentation through production. Therefore, an unemployed worker and a vacancy-posting firm that have just met for the first time cannot just "keep in touch."

Our model nests the standard Mortensen and Pissarides (1994) model as a special case with no recall option ($dH\left(\varepsilon'|\varepsilon\right) = 0$), costless unemployed job search ($c(s) = 0$ and s is normalized to one), and a degenerate distribution of new matches (F is a mass point at the upper bound of the support of G).

We restrict attention to an equilibrium where value and policy functions are defined on a very simple state domain: aggregate productivity p and, for each match, the quality ε of the current or (if unemployed) last match. In this simple equilibrium, idle workers who are searching will be willing to accept new job offers independently of their value of recall in hand. Thus, firms posting new vacancies will not need to keep track of the evolving distribution of recall values held by the unemployed, which is then not a state variable. Bellman values are time-independent functions of p and ε only. Labor market tightness θ is a function of p only. We assume that equilibrium has these properties and then verify that the guess is consistent with all equilibrium restrictions. These properties will make equilibrium characterization and computation very tractable.

6.2 Match acceptance, separation, and mothballing

Let $U(p,\varepsilon)$ denote the worker's value of unemployment, where $p\varepsilon$ is the productivity of the last match, if any (otherwise $\varepsilon = p\varepsilon = 0$), $W\left(p,\varepsilon\right)$ the worker's value of employment, $V\left(p,\varepsilon\right)$ the value of a vacant job, where $p\varepsilon$ is the current (potential) productivity of the last employee, if any (otherwise $\varepsilon = p\varepsilon = 0$), $J(p,\varepsilon)$ the value of a filled job, $w\left(p,\varepsilon\right)$ the wage.

Since $\varepsilon = 0$ is an absorbing state and that match will never be recalled, $V\left(p,0\right)$ equals the value of an unattached, brand new vacancy, which is zero by free entry, i.e., $V\left(p,0\right) = 0$. Next, we examine the decision to either dissolve or mothball the match. Neither the worker nor the firm has any incentives to give up a recall option, unless match quality drops to zero, because waiting entails no explicit or opportunity costs either to the firm, by free entry, or to the worker, who can search for other jobs whether or not he has a recall option in hand. They decide by mutual consent to mothball the match when they are both indifferent: $J(p,\varepsilon) = V(p,\varepsilon) \Leftrightarrow W(p,\varepsilon) = U(p,\varepsilon)$. Except for a transition to the absorbing state $\varepsilon' = 0$, separation is never irreversible but always results initially in mothballing. In this notation, we can study the decision to accept a new match and formalize our "No Mothballing Before

Production" assumption as follows: *for new matches* $\tilde{\varepsilon} \sim F$ *only*,

$$J(p, \tilde{\varepsilon}) \leq V(p, \tilde{\varepsilon}) \Rightarrow J(p, \tilde{\varepsilon}) = 0.$$

Next, we examine the decision whether to accept or reject a new match. Search effort and the recall option give rise to moral hazard. The old employer could offer the former employee a flow payment, a firm-sponsored unemployment insurance, to discourage search for new jobs or to compensate the worker for rejecting any new offer. In turn, new employers could promise a higher wage to respond to the old employer's counteroffer. We rule out any such competition because it would require commitment. The worker, anticipating this, will simply compare the values that he would obtain by bargaining independently with either the current or the new employer. Similarly, the last employee of a currently vacant job may want to compete with any new hire prospect (in order to keep his old job available) and retain his recall option. This competition is ruled out by constant returns to scale in production and free entry, because the firm can always create a new job for the new applicant and keep the old job "mothballed" for the former employee.

Therefore, after meeting and jointly drawing an initial match quality $\tilde{\varepsilon} \sim F$, the firm and the worker, who carries a quality ε from his last mothballed match with another firm, create the new match if and only if this yields the firm more than both giving up the new vacancy (which has zero value by free entry) and mothballing the new match immediately ($J(p, \tilde{\varepsilon}) > V(p, \tilde{\varepsilon})$), and the worker more than continuing job search, either with no match in hand or waiting for a recall of the old match ε, and also more than mothballing the new match immediately: $W(p, \tilde{\varepsilon}) \geq \max \langle U(p, 0), U(p, \varepsilon), U(p, \tilde{\varepsilon}) \rangle$. Clearly, $U(p, \tilde{\varepsilon}) \geq U(p, 0)$ for all $\tilde{\varepsilon} \geq 0$, because the worker can always reject recall of old matches and mimic a worker who has no recall option. Similarly, $V(p, \tilde{\varepsilon}) \geq V(p, 0) = 0$ for the firm.

To recap, a new match $\tilde{\varepsilon}$ will be acceptable if and only if:

$$J(p, \tilde{\varepsilon}) \geq V(p, \tilde{\varepsilon}) \text{ and } W(p, \tilde{\varepsilon}) \geq \max \langle U(p, \varepsilon), U(p, \tilde{\varepsilon}) \rangle,$$

i.e., if it yields both parties a positive surplus from forming the new match and producing output immediately, and also yields the worker a positive surplus over (the option to recall) the old match, whose quality evolved to the current value ε. By the private efficiency of Nash Bargaining, $J(p, \tilde{\varepsilon}) \geq V(p, \tilde{\varepsilon}) \Leftrightarrow W(p, \tilde{\varepsilon}) \geq U(p, \tilde{\varepsilon})$. Hence, denoting by $\mathbb{I}\{\cdot\}$ the indicator function, the worker incentive constraint is weakly more binding, and the probability that a new contact results in an acceptable match equals:

$$a(p, \varepsilon) = \int \mathbb{I}\{W(p, \tilde{\varepsilon}) \geq \max \langle U(p, \varepsilon), U(p, \tilde{\varepsilon}) \rangle\} \, dF(\tilde{\varepsilon}).$$

6.3 Bellman equations

We can now write the (Hamilton-Jacobi-)Bellman equations that these values solve. For the employed worker, it is written as:

$$\begin{aligned} rW(p,\varepsilon) =& w(p,\varepsilon) + \lambda_p \int \left[\max \langle W(p',\varepsilon), U(p',\varepsilon)\rangle - W(p,\varepsilon)\right] dP(p'|p) \\ &+ \lambda_\varepsilon \int \left[\max \langle W(p,\varepsilon'), U(p,\varepsilon')\rangle - W(p,\varepsilon)\right] dG(\varepsilon'|\varepsilon). \end{aligned} \tag{1}$$

Endogenous separation may follow either aggregate (p') or idiosyncratic (ε') shocks.

A worker may be unemployed and searching for one of three reasons: (i) the match was hit by an exogenous destruction shock, which sets $\varepsilon = 0$ and voids any recall possibility; (ii) the match was mothballed following a productivity shock, either aggregate or idiosyncratic, but might still be recalled; and (iii) off the equilibrium path, the firm and worker disagree on the wage and, as a threat point, suspend production and search until the next shock hits. Whatever the reason, an unemployed worker who currently holds an old match value ε and contacts an open vacancy expects a capital gain equal to:

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

Under our assumptions, the search problem of the unemployed worker has a unique solution:

$$s^*(p,\varepsilon) = \arg\max_{s\geq 0} \left\{s\phi(\theta(p))\Omega(p,\varepsilon) - c(s)\right\}$$

so that the value of unemployment solves:

$$\begin{aligned} rU(p,\varepsilon) = b &+ \lambda_p \int \left[\max \langle W(p',\varepsilon), U(p',\varepsilon)\rangle - U(p,\varepsilon)\right] dP(p'|p) \\ &+ \lambda_\varepsilon \int \left[\max \langle W(p,\varepsilon'), U(p,\varepsilon')\rangle - U(p,\varepsilon)\right] dH(\varepsilon'|\varepsilon) \\ &+ s^*(p,\varepsilon)\phi(\theta(p))\Omega(p,\varepsilon) - c(s^*(p,\varepsilon)). \end{aligned} \tag{2}$$

In words: after each shock to either p or ε, the worker may propose to reactivate the old job, and at all times, he can search for a new job.

We now move on to the firm, starting with the value of a filled job. The flow return equals flow output, minus the wage, plus capital gains or losses after each type of shock, which may induce the match to separate:

$$\begin{aligned} rJ(p,\varepsilon) = p\varepsilon - w(p,\varepsilon) &+ \lambda_p \int \left[\max \langle J(p',\varepsilon), V(p',\varepsilon)\rangle - J(p,\varepsilon)\right] dP(p'|p) \\ &+ \lambda_\varepsilon \int \left[\max \langle J(p,\varepsilon'), V(p,\varepsilon')\rangle - J(p,\varepsilon)\right] dG(\varepsilon'|\varepsilon). \end{aligned} \tag{3}$$

The value of a vacant job solves a more complex equation:

$$\begin{aligned} rV(p,\varepsilon) = \lambda_p \int & \left[\max \langle J(p',\varepsilon), V(p',\varepsilon)\rangle - V(p,\varepsilon)\right] dP(p'|p) \\ & + \lambda_\varepsilon \int \left[\max \langle J(p,\varepsilon'), V(p,\varepsilon')\rangle - V(p,\varepsilon)\right] dH(\varepsilon'|\varepsilon) \\ & - s^*(p,\varepsilon)\,\phi(\theta(p))\,a(p,\varepsilon)\,V(p,\varepsilon). \end{aligned} \tag{4}$$

The firm can offer to recall the former employee after any shock, but can also lose the recall option and be left with an unattached vacancy which, by free entry, is worth zero. This occurs (third line of (4)) if the former employee contacts another open vacancy (at rate $s^*(p,\varepsilon)\,\phi(\theta(p))$) and draws a new acceptable match, which has a chance equal to $a(p,\varepsilon)$. The firm could also pay the flow vacancy cost κ to meet a new worker and hire him if the new match draw $\tilde{\varepsilon}$ guarantees a positive surplus and a higher value to the firm than the continuation value of waiting for a recall. Again, the net value of this option is zero by free entry, and does not appear in (4). Therefore, $V(p,\varepsilon)$ measures only the value of the recall option for the firm, while the corresponding value for the worker $U(p,\varepsilon)$ also contains an option value of searching for another job in addition to the value of leisure.

6.4 Free entry condition and equilibrium state space

Firms post new vacancies until their net value is zero: for all p, $V(p,0) = 0$. After matching, if the quality ever drops to $\varepsilon = 0$, an absorbing state, the match will never be productive again and the vacancy becomes worthless, just like new ones: $J(p,0) = V(p,0) = 0$.

Free entry thus implies that the vacancy posting cost κ equals the contact rate $q(\theta(p))$ times the expected surplus from a contact. Here is where the assumption on the state space has bite. If the incentives of a job applicant to accept a new match draw depend on the value of the recall option that he holds, then both the probability that a vacancy is filled and the profits from filling it, hence the free entry condition, will depend on the distribution of recall values (or equivalently the qualities of last jobs held) among unemployed workers. This is an infinitely dimensional object, which evolves stochastically with aggregate productivity p.

To prove that agents can ignore this state variable and confirm the guess of a simple state space (p,ε), we have to show that *on the equilibrium path*, where a worker is unemployed only when his last match has negative surplus, thus not due to bargaining disagreement, both the probability $a(p,\varepsilon)$ that a new match is acceptable and the profits $J(p,\tilde{\varepsilon})$ that the firm earns from it are independent of the value $U(p,\varepsilon)$ of the recall option that the worker may currently have in hand. By Nash bargaining, this also requires that the worker's continuation value $W(p,\tilde{\varepsilon})$ from the match, hence his new wage, be independent of $U(p,\varepsilon)$.

The argument for the continuation value $W(p, \tilde{\varepsilon})$ follows from the lack of commitment and ex-post competitions for a worker between firms. The new employer bargains with all workers in the same way, as if they had nothing in hand, and offers all new hires a value $W(p, \tilde{\varepsilon})$. This is accepted if and only if $W(p, \tilde{\varepsilon}) \geq U(p, \tilde{\varepsilon})$, by the assumption of "No Mothballing Before Production." This value $W(p, \tilde{\varepsilon})$ is independent of the current recall option encoded in ε.

The argument for the probability of accepting a new match $a(p, \varepsilon)$ to be independent of the value of the recall option in hand $U(p, \varepsilon)$, thus of ε, is more subtle, and by revealed preferences. If the old match ε and new match $\tilde{\varepsilon}$ satisfy $U(p, \tilde{\varepsilon}) < W(p, \tilde{\varepsilon}) < U(p, \varepsilon)$, the worker will continue waiting for a recall, although the new match $\tilde{\varepsilon}$ would be acceptable absent the recall option. The critical observation is that any new match that is acceptable to an unemployed worker who does not hold a recall option is also acceptable to an unemployed worker who does. If the worker who makes contact with a new vacancy is jobless, his recall value $U(p, \varepsilon)$ must be low enough not to justify recall of the previous match. Otherwise, he would have recalled the match and not be jobless and searching. Thus, the surplus from his old match over continuing unemployment at that match quality must still be negative. Note that this is not true for workers who separate due to bargaining disagreement, but the disagreement does not exist on the equilibrium path. By assumption, a new match occurs only if it pays to start production right away, rather than to just mothball it. Therefore, if the surplus it generates over separating and keeping the *new* match quality is positive, then, to be acceptable, the new match must pay the worker more than the recall option he already had in hand.

Formally, we guess and later verify that the functions U and $W - U$ (hence W) are increasing in ε. Consider a worker who decides in this period not to recall the old match ε (i.e., $W(p, \varepsilon) - U(p, \varepsilon) \leq 0$), searches for new vacancies, and draws a new match $\tilde{\varepsilon}$ that is acceptable (i.e., $W(p, \tilde{\varepsilon}) - U(p, \tilde{\varepsilon}) \geq 0$). Combining the two inequalities, $W(p, \tilde{\varepsilon}) - U(p, \tilde{\varepsilon}) \geq 0 \geq W(p, \varepsilon) - U(p, \varepsilon)$. As $W(p, .) - U(p, .)$ is increasing, this implies $\tilde{\varepsilon} \geq \varepsilon$. As $U(p, .)$ is increasing, this in turn implies $U(p, \tilde{\varepsilon}) \geq U(p, \varepsilon)$. Putting everything together, when an unemployed worker holding a mothballed match of quality ε finds a new match $\tilde{\varepsilon}$ that would be acceptable even if he did not have a recall option, he will accept it anyway: $W(p, \tilde{\varepsilon}) \geq U(p, \varepsilon)$. To conclude: a searching worker will accept a new match independently of his value of recall, as encoded in ε.

Firms post vacancies until job market tightness θ equates the expected hiring cost to the expected surplus from an acceptable new match:

$$\kappa = q(\theta) \int \mathbb{I}\{J(p, \tilde{\varepsilon}) \geq V(p, \tilde{\varepsilon})\} J(p, \tilde{\varepsilon}) \, dF(\tilde{\varepsilon}), \tag{5}$$

which shows that indeed $\theta = \theta(p)$ is uniquely determined as a function of p only. Although the current new vacancy is worth zero, the firm knows that it will gain the surplus $J(p, \tilde{\varepsilon})$ over it only if the new match draw $\tilde{\varepsilon}$ is good enough also to start production right away, $J(p, \tilde{\varepsilon}) > V(p, \tilde{\varepsilon}) > 0$, because new matches cannot be mothballed before production.

It follows that the probability $a(p, \varepsilon)$ that a worker who is unemployed accepts a new offer is independent in equilibrium of the value of the recall option ε he has in hand, and thus depends only on the aggregate state. We can write it as:

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

Again, we stress that this "memoryless" property applies only on the equilibrium path because it relies on all unemployed workers holding a negative surplus from recalling their last match. Still, to calculate the outside options for wage bargaining, which we assumed to be temporary separation until the next productivity shock, we need to know the current match quality ε. These types of separations, however, are off the equilibrium path and hence do not affect the pool of unemployed from which new vacancies draw and thus crucially do not affect the free entry condition.

6.5 Nash Bargaining and wages

We assumed that the outside options are the continuation values of separating until at least the next productivity shock hits. Examining the Bellman equations, these are precisely $U(p, \varepsilon)$ and $V(p, \varepsilon)$. Therefore, the Nash Bargaining solution is:

$$w(p, \varepsilon) = \arg\max_{w} [W(p, \varepsilon) - U(p, \varepsilon)]^{\beta} [J(p, \varepsilon) - V(p, \varepsilon)]^{1-\beta} . \tag{6}$$

Taking an FOC yields[17]

$$\beta J(p, \varepsilon) = (1 - \beta) [W(p, \varepsilon) - U(p, \varepsilon)] . \tag{7}$$

[17] Shimer (2006) points out that in the case of costly search *on the job* the Nash problem (6) may not be concave, so the necessary FOC that yields the standard linear sharing rule (7) may not be sufficient. Intuitively, the firm may want to offer a higher wage than that implied by (7) in order to discourage job search by its employees, gaining on net due to retention, at the expense of future employers. This issue does not arise in our context, and (7) is sufficient for (6), because search effort occurs only *off the job*. The firm cannot commit to a future wage conditional on a recall, in order to influence the worker's current incentives to search off the job while waiting for that recall. Once the recall occurs, bygones are bygones, wages are renegotiated ex post, and on-the-job search is not ruled out by assumption.

Using the Bellman equations and (7), and after much algebra, we can solve for the wage:

$$
\begin{aligned}
w(p,\varepsilon) = {} & (1-\beta)\,b + (1-\beta)\left[s^*(p,\varepsilon)\,\phi(\theta(p))\,\Omega(p,\varepsilon) - c\left(s^*(p,\varepsilon)\right)\right] + \beta p\varepsilon \\
& + \beta s^*(p,\varepsilon)\,\phi(\theta(p))\,a(p,\varepsilon)\,V(p,\varepsilon) \\
& + \lambda_\varepsilon \int \left[\beta V(p,\varepsilon') - (1-\beta)\,U(p,\varepsilon')\right]\left[dG(\varepsilon'|\varepsilon) - dH(\varepsilon'|\varepsilon)\right]. \qquad (8)
\end{aligned}
$$

The worker is paid the flow value of being unemployed, which includes leisure b and the surplus from searching optimally for a new match, plus his bargaining share β of flow output minus this opportunity cost and (second line) of the potential loss to the firm of the recall value, should the worker indeed find a new viable match. Note that the employed worker would search if temporarily separated because of bargaining disagreement (off the equilibrium path), in which case his propensity to accept a new job *would* depend on the recall option ε. Finally, in the third line, the wage contains a term that captures the differential evolution of match quality on and off the job. Suppose $G(\cdot|\varepsilon) \succ_{FSD} H(\cdot|\varepsilon)$, i.e., starting from ε, match quality improves on the job relative to off the job, for example because of match-specific skill depreciation during unemployment. Then the last wage component is positive if and only if $\beta V(p,\varepsilon') - (1-\beta)\,U(p,\varepsilon')$ is increasing in ε', i.e., if (what the worker can appropriate of) the firm's recall option is more sensitive to match-specific shocks than (what the firm can appropriate of) the worker's recall option.

6.6 Equilibrium

The equilibrium of the model is described by functions J, V, W, U, w of ε and p, and a function θ of p, which solve (1), (2), (3), (4), (5) and the sharing rule (7) or, equivalently, the wage equation (8). It is straightforward to solve this system of functional equations exactly through any nonlinear iteration algorithm, after discretizing the support of ε and p. We exploit this tractability to explore the quantitative properties of the model.

Before doing that, we show that our model nests the standard search-and-matching framework as a special case without recall and worker search effort. Equations (1), (3), (5), and (7) are unaffected. Eliminating the recall option ($H = 0$), free entry $V(p,\varepsilon) = 0$ for all (p,ε) replaces (4), and $U(p,\varepsilon) = U(p)$. Appropriately modifying (2), the Nash Bargaining wage solution (8) reduces to:

$$
w(p,\varepsilon) = (1-\beta)\,b + (1-\beta)\left[s^*(p,\varepsilon)\,\phi(\theta(p))\,\Omega(p,\varepsilon) - c\left(s^*(p,\varepsilon)\right)\right] + \beta p\varepsilon.
$$

Eliminating worker search effort ($c(s) = 0$, and normalizing $s = 1$), we recover two well-known cases. In the steady state, we obtain $w(\varepsilon) = \beta\varepsilon + (1-\beta)\,b + \beta\theta\kappa$, which is the standard wage function of the classic models of Pissarides (1985) with initial, but fixed match heterogeneity F and no further idiosyncratic shocks, and of Mortensen and Pissarides (1994), where all new matches are the same but are then subject to idiosyncratic shocks. With aggregate but no idiosyncratic shocks, the wage is:

$$w(p,\varepsilon) = \beta p\varepsilon + (1-\beta)b + \beta\lambda_p \int \max\langle J(p',\varepsilon), 0\rangle \, dP(p'|p) + \beta\theta(p)\kappa,$$

which corresponds to Shimer's (2005) stochastic version of Pissarides (2000).

7 Quantitative analysis

7.1 Calibration

We calibrate the model in the steady state and then explore its business-cycle properties. A unit time interval in the model is set equal to a week. We simulate the model's steady-state equilibrium to generate a weekly panel. After discarding the observations in a "burn-in" period, we re-sample the data every four weeks and compute the cross-sectional model-based statistics. We do so to be consistent with the structure of SIPP interviews, while also being as close as possible to the continuous-time setup of the model economy. A similar simulation procedure is used for the business-cycle analysis, whose results are described in subsection 7.2. The computational methodology is presented in the Appendix.

We begin with normalizations, externally calibrated parameters, and functional forms. The discount rate is set to $r = 0.1\%$, which roughly corresponds to 5% at annual frequency. We normalize to one the unconditional means of idiosyncratic and aggregate productivity, ε and p. In the steady state, the latter takes the constant value $p = 1$. The contact rate of unemployed workers with open vacancies, per unit of time spent searching, derives from a standard Cobb-Douglas matching function: $\theta q(\theta) = \mu\theta^{\alpha}$, where job market tightness θ is the ratio between open vacancies and aggregate search effort of the unemployed, and μ is a matching scale parameter. In the steady state, the scale of μ and $\overline{\theta}$ are not separately identified, so we normalize $\overline{\theta} = 1$. We set $\alpha = 0.5$, a standard number in the literature, and the worker bargaining share $\beta = 1 - \alpha$, a tradition that originates in the Hosios condition for constrained efficiency, although this condition need not apply to our economy. We set $\lambda_{\varepsilon} = 3/13$, so that idiosyncratic shocks to $\varepsilon > 0$ arrive on average every 13/3 weeks (i.e., one month), and $\lambda_p = 1/13$, so that aggregate shocks to p arrive on average once per quarter.

Next, we move on to the parameters that we calibrate internally. Conditional on the arrival of an idiosyncratic shock, the match experiences exogenous destruction with probability δ: match productivity transits from any state $\varepsilon > 0$ to the lowest state $\varepsilon' = 0$, which is absorbing, making any future recall impossible. The remainder of the EU transitions are

Table 6: Parameter values: Weekly calibration

Symbol	Description	Value	Symbol	Description	Value
r	Discount rate	0.001	μ	Matching scale parameter	0.067
b	Flow value of unemployment	0.9	κ	Vacancy posting cost	0.722
c_0	Search cost scale	0.29	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	0.0005	λ_p	Arrival rate of aggregate shock	1/13
ρ_ε	Persistence of idiosyncratic shock	0.97	ρ_p	Persistence of aggregate shock	0.97
σ_ε	SD of idiosyncratic shock	0.035	σ_p	SD of aggregate shock	0.008
			–	Mean output level	1

endogenous separations. With probability $1-\delta$, $\log\varepsilon$ experiences an innovation drawn from an AR(1) process with parameters ρ_ε and σ_ε. This compound process determines G. After separation, match quality evolves according to the same stochastic law of motion with no skill depreciation: $H = G$. We constrain search effort s in $[0,1]$ and interpret it either as the fraction of time spent for job search or the flow probability of search by the unemployed worker. The search effort cost function is quadratic: $c(s) = c_0 s^2/2$.

We calibrate seven parameters—ρ_ε, σ_ε, δ, μ, c_0, κ, and b—by minimizing the log unweighted distance of a vector of nine moments generated by the steady-state equilibrium from their empirical counterparts. Consistent with the model, where workers always participate in the labor force, these moments are computed from completed unemployment spells EUE, hence excluding entrants. We start with seven transition moments. The first two are standard in the literature, so to facilitate comparison with it, we draw them from the matched records of the monthly CPS 1990-2014. The total EU separation probability is 1.4% per month, and the total UE job-finding probability is 27.7% per month. The remaining five moments are computed from the SIPP 1996-2013. The recall share of hires is 46.4%, which implies a new-job-finding probability of 14.85% per month. The hazard rate of exit from unemployment to employment is 35% after one month and 25% after six months. The analogous hazard rates of exit to just recall are, respectively, 20% and 10%.

Our choice of these empirical targets is motivated by the following considerations. Job-finding and separation probabilities are at the core of the model; they directly impact the unemployment rate and the probability of recall. The four moments on duration dependence are 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 mostly when the spell ends with recall, and we aim to replicate this property.

The eighth target is aggregate search effort, or equivalently the share of the unemployed who search full time. We take it to be the average share of PS workers in the unemployment pool (excluding entrants), which is 80% in the CPS over the period 1990−2014. Although every unemployed worker in the model will spend some fraction of his time searching, this fraction will be increasing in the distance of ε to the recall threshold; hence, search effort and probability of recall will be negatively correlated across workers. In the data, the coarse categorization PS/TL satisfies this negative correlation, so we view our choice as a reasonable approximation.

A critical parameter to calibrate in this literature is the flow value of leisure b. Bruegemann and Moscarini (2010) work with steady-state equilibrium equations that apply to a large class of search models. They study the comparative statics response of the job-findingprobability to changes in aggregate productivity p and show that this response, an upper bound to the volatility in the stochastic simulation of the same model, depends directly on the ratio b/p. Their findings generalize the insight from Hagedorn and Manovskii's (2008) calibration of Shimer's (2005) specific model. Here, a similar argument, omitted and available upon request, shows that the source of amplification is the ratio between the value of leisure b net of average search costs paid when unemployed, and average labor productivity corrected by match selection through endogenous separations. We follow Hall and Milgrom (2008) and target this "replacement ratio" to be 0.71.

The parameters of the idiosyncratic shock process, ρ_ε, σ_ε, and δ, are the only drivers in the model of job separation and recall and of unemployment duration dependence, which we showed in the data to be mostly about declining recall chances. More persistent (higher ρ_ε) and smaller (lower σ_ε) innovations to match-specific productivity raise the equilibrium separation cutoff, thus the probability of separation, and reduce the probability of recall. The latter effect is stronger the longer a worker has been unemployed (by selection), which shows up as negative duration dependence of recall. A higher chance δ of exogenous, irrevocable separation has similar effects on the chances of separation and recall but no impact on unemployment negative duration dependence. Therefore, observed duration dependence separately identifies the sources of exogenous and endogenous separations and recalls.

Table 6 summarizes our best calibration. The implied value of leisure (b) is 0.9, while the average search cost paid is 0.11. Thus, the net benefit is 0.79 and the replacement ratio to average productivity of active matches (1.06 in our calibration) is 0.75, slightly above the target at 0.71. The flow surplus from employment, however, remains substantial.

We can now examine the implications of our calibration for a few untargeted moments.

An exogenous job destruction probability δ equal to 0.05% per week implies that the share of workers with no recall option at the time of job separation is about 17% in the steady state. To gauge its plausibility, we observe that, when an establishment closes, its employees presumably lose any recall option. According to the Business Employment Dynamics assembled by the BLS, each quarter, establishment closings result in about 1.5 million job losses. This is about 25% of total *EU* separations in the CPS, about 2 million

Table 7: First moments in the model steady-state equilibrium and empirical targets

	Job-Finding Prob.	Separation Prob.	Recall Rate	Search Prob.	Replacement Ratio
Model	0.291	0.014	0.499	0.791	0.75
Data	0.277	0.014	0.464	0.800	0.71

Sample period for CPS-based measures is 1990-2014.

per month, thus 6 million per quarter. Some of the workers affected by closings may not be part of the *EU* inflow because they either are reassigned to a different establishment owned by the same firm, find another job right away, or drop out of the labor force.

Our calibrated idiosyncratic productivity process (in logs) has persistence 0.97 monthly, meaning 0.7 annually, and a standard deviation of innovations equal to 3.5% monthly, 8% annual, and 14% ergodic. The employer-level (log) TFP estimated by Foster et al. (2008) has exactly the same persistence, 0.7 annual, and higher volatility, 21% over 5 years. Given possible sources of measurement error, we find this to be in the ballpark; that is, our model does not require implausibly volatile or transient measures of productivity.

The implied average contact rate of new vacancies per unit of search effort, namely of a full-time job searcher ($s = 1$), which is $\overline{\theta}\overline{q} = \overline{q}$ given the normalization $\overline{\theta} = 1$, equals 6.7% per week. Combined with an 80% probability of accepting a new match, in the calibrated model the probability for a vacancy of hiring a *new* worker from unemployment is about 5% per week, or 20% per month. This is only about one quarter of the 80% average vacancy-filling probability measured in the Job Openings and Labor Turnover Survey (JOLTS). Three differences in definitions can help to explain this discrepancy. First, our notion of a vacancy does not coincide with JOLTS's: the model's cyclical dynamics do not depend on the scale in which we measure vacancies, and for that reason we normalized it to equal steady state unemployment ($\overline{\theta} = 1$). If we rescale vacancies in the model to match $\overline{\theta} = 0.5$, which is roughly the average ratio between the number of vacancies from JOLTS and the number of unemployed workers in the CPS during 2001-2016, then the implied job-filling rate by new workers in the model doubles to 40% per month. Second, the model's predictions concern the rate at which workers join *new* firms, while hires in JOLTS include recalls, about one-third of all hires from non-employment, according to our evidence in Table A.4. This correction reduces the JOLTS-filling rate by one-third, to 53%, much closer to our 40%. Finally, hires in JOLTS also include those from non-participation and from other firms, which are outside of the scope of the model. Job-to-job transitions alone account for about half of all hires, based on evidence from the CPS and the SIPP. If we exclude those hires from the numerator of the JOLTS job-filling rate, presumably we should also discount a share of open vacancies

Table 8: Mean unemployment duration and hazard rates in the model s.s. equilibrium

	Overall Hires	Recalls	New Hires
Mean duration (months)	3.41	2.80	4.01
Hazard rate at month 1	0.346	0.204	0.142
2	0.318	0.169	0.148
3	0.290	0.139	0.150
4	0.274	0.124	0.151
5	0.264	0.110	0.154
6	0.257	0.098	0.159

The empirical counterparts are in Table 4 and Figure 1. Note that Figure 1 includes those who drop out of the labor force, whereas the calibration targets are hazard rates of the EUE sample.

in the denominator, by an amount which is difficult to assess without a model.

Tables 7 and 8 report the fit of the model. Quantitatively, this simple calibration with a parsimonious idiosyncratic process does a remarkable job at fitting both targeted and non-targeted empirical moments.

7.2 Cyclical properties of the model

We now examine the cyclical properties of the model's equilibrium. To calibrate the aggregate productivity process p, we assume that, conditional on an arrival at Poisson rate λ_p=1/13, it follows an AR(1) process in logs. We calibrate the innovations' serial correlation at 0.97 and standard deviation at 0.008 in order to replicate the cyclical properties of Average Labor Productivity (ALP), including the "cleansing" effect of recessions. To measure ALP, we follow Shimer (2005) and use "output per job in the nonfarm business sector" from the BLS (series PRS85006163) over the same sample period (1990-2014), as in the case of the CPS transition rate series discussed above. The cyclical component is measured by its logged and HP filtered series with smoothing parameter of 10^5. Rather than calibrating the aggregate driving process to a specific series, such as the Solow residual or identified monetary policy shocks, we take this agnostic view, because our model features a single aggregate shock, while in the data there are several. To facilitate comparison with the literature, we target ALP, but in our model this is an endogenous object. Our focus is on comovement and amplification of labor market variables, not on the origin of aggregate economic fluctuations. We measure comovement, both in the data and in the model, with the semi-elasticity (regression coefficient) of each relevant variable on unemployment rather than the unconditional correlation between the two, which is contaminated in the data by additional shocks.

The main goal of our modeling exercise is to understand the impact of the recall option

Table 9: Standard deviations in the model stochastic equilibrium and empirical targets

Model	Search Cost	ALP	Separation Prob.	Job-Finding Prob.	Measured Tightness	Recall Rate
Recall	Yes	0.016	0.152	0.088	0.169	0.068
	No	0.017	0.095	0.040	0.066	0.034
MP	Yes	0.017	0.087	0.106	0.144	–
	No	0.018	0.061	0.041	0.072	–
Data		0.016	0.103	0.145	0.350	0.082

"Measured" tightness equals the ratio between vacancies and unemployment.

on aggregate labor market fluctuations. To this end, we also present results from versions of the model where we remove search effort and/or recall. We label the model without recall"MP," essentially identical to Mortensen and Pissarides (1994) with the only difference that we also allow for an interesting acceptance margin, as in Pissarides (1985), while in Mortensen and Pissarides (1994) all new matches are acceptable.[18] We re-calibrate the two versions of the MP model (with and without search effort) in the steady state by targeting the average job-finding probability and separation probability. These two moments alone are insufficient to identify all parameters. For example, the values of both δ and σ_ε govern EU separations. In our benchmark model in which shocks to ε keep hitting even after separation, these two parameters also determine the frequency and duration dependence of recall. In the MP model, we keep the calibration of our benchmark model and only modify three parameters, which speak directly to these two moments: σ_ε, the scale parameter of the matching function μ, and the vacancy posting cost κ. We also reset the value of leisure b to maintain our target replacement ratio. When introducing search effort in the MP model, we recalibrate the scale of its cost function to target the share of PS with average search effort, as in our recall model. We report in the Appendix the resulting values of the parameters for these calibrated models without recall. Finally, we compute their stochastic equilibrium in response to the same sequence of aggregate shocks as the benchmark model with recall and search effort.

We log and HP-filter (with parameter 10^5) all time series, empirical and model-generated, sampled quarterly. Tables 9 and 10 present the standard deviations of the series and their semi-elasticities (regression coefficients) w.r.t. the unemployment rate, in the Recall models, MP models, and the empirical data.

[18]This exercise is of independent interest, as the first quantitative exploration of business cycles in a canonical search-and-matching model simultaneously featuring endogenous rates of match contact, acceptance, and separation. See Fujita and Ramey (2012) for the cyclical properties of various versions of the Mortensen and Pissarides (1994) model, where new matches are always accepted.

Both the recall option and search effort amplify countercyclical fluctuations in the probability of endogenous separation. In our benchmark model with recall and search effort, the volatility of the unemployment rate (not shown) is 0.199, comparable to its empirical counterpart. This happens, however, in part for the wrong reason: the separation probability into unemployment is 1.5 times as volatile in the model (first row) as in the data (last row), while close to the opposite is true for the overall job-finding probability. Given that our calibration does not target aggregate second moments, this result is not surprising. On the other hand, the MP model without recall and search effort (fourth row), which is the natural term of comparison, underestimates by much more the volatility of *both* job-finding and separation probabilities, and therefore the volatility of the unemployment rate. As we know from Shimer (2005), this MP model does even worse on both dimensions.[19] We conclude that adding to the MP model the recall option, in a way that is consistent with our empirical evidence, does not fully resolve the unemployment volatility puzzle of Shimer (2005), but it goes in the right direction.

The intermediate models, in the second and third rows of Table 9, reveal that both recall and search effort amplify the volatility of the separation probability, while they have opposite effects on the volatility of the job-finding probability. Removing search effort from the recall model, in the second row of Table 9, improves the volatility of the separation probability, but rolls back any gains in the volatility of the job-finding probability, which is the main focus of the literature. The recall rate as well becomes too stable.

Removing the recall option while leaving search effort (third row of Table 9) reduces the volatility of the separation probability and raises that of the job-finding probability. This MP model without recall but *with* search effort appears to do best and appears to negate the importance of recall and thus our entire exercise. But this version of the MP model is simply a term of comparison with our recall model, useful only to inspect the mechanism. Nothing in the logic of the MP model suggests how to calibrate the cost of search effort, which is critical to this intermediate result. We simply matched the share of unemployed workers who are on PS, as in the recall model, for the sake of comparison. But the TL/PS distinction does not really belong in the MP model, and even the PS share alone provides weak identification for the search technology (for example, our choice to model it as a quadratic cost). Conversely, the TL/PS distinction is natural in the recall model, given the strong empirical correlation between TL and recall (and PS and no recall). In both the SIPP and the CPS, the definition of TL explicitly ties an expectation of recall to the measurement of search effort. Finally, the cyclical volatility of the recall rate (last column of Table 9) provides additional empirical

[19]For the separation probability, this is by construction, given that it is assumed to be constant in his model.

Table 10: Elasticity with respect to unemployment

Model	Search Cost	Separation Prob.	Job-Finding Prob.	Vacancies	Recall Rate
Recall	Yes	0.690	−0.404	0.174	0.252
	No	0.820	−0.294	0.458	0.096
MP	Yes	0.412	−0.663	0.122	–
	No	0.622	−0.501	0.180	–
Data		0.493	−0.756	−0.854	0.222

discipline to evaluate the specification and calibration of the search technology.

In our model, "true" labor market tightness is the ratio of vacancies to aggregate search effort. In the data, we can only observe (the ratio between) vacancies and unemployment. As Shimer (2005) points out, this vacancy/unemployment ratio, "measured" tightness, is roughly 20 times more volatile than ALP (0.35 vs. 0.016 in our data). We can replicate measured tightness in the model. Its volatility is larger, roughly half of the empirical counterpart, in the recall model (0.169) than in the MP model with search effort (0.144).

We now turn to comovement in Table 10. Our benchmark recall model with search effort and its polar opposite MP model without search effort perform similarly. As is clear from the intermediate models, recall and search effort have countervailing effects. The entries in the "Vacancies" column measure the slope of the empirical Beveridge curve. Along this dimension, all models perform poorly, with a wrong, positive sign. The recall model without search effort performs worst, although introducing search effort brings the elasticity down to a level comparable to the two versions of the MP model. This poor fit of the Beveridge curve is a well-known implication of countercyclical separations. The literature has shown that on-the-job search and associated job-to-job transitions overcome this problem (Fujita and Ramey (2012)). The same is likely to apply to the model with recall. Finally, our benchmark model replicates well the mildly countercyclical behavior of the recall rate, which acts as a stabilizer of total hires. This effect is due mostly to search effort, which falls in a recession, making recall a more likely outcome of unemployment.

7.3 Discussion

We now interpret our quantitative results. Table 11 provides additional informative moments. First, why does the recall option amplify the volatility of separations? When deciding whether to separate, a firm is concerned that the mothballed worker may find another job and become unavailable for recall. This concern is stronger in expansions, when both search effort and the probability of contacting new vacancies rise, and hence a firm is more reluctant

Table 11: Cyclicality of "new hires" job-finding probability

Model	Search Cost	Job-Finding Prob. (New Hires)	Search Prob.	Acceptance Prob.	Tightness
			Standard Deviation		
Recall	Yes	0.137	0.072	0.034	0.098
	No	0.048	–	0.024	0.066
			Elasticity w.r.t Unemployment		
Recall	Yes	−0.659	−0.349	−0.135	−0.476
	No	−0.377	–	−0.134	−0.542

to separate and hoards even more labor. Conversely, in recessions, a firm is more willing to mothball its unproductive workers, who have nowhere to go. Because the separation cutoff is also the cutoff for the acceptance of new matches, by the same logic, the probability of accepting a new job offer also becomes more cyclically volatile. Intuitively, the ability of the worker to search for other jobs while waiting for a recall and to void the recall option raises the surplus from staying together. This force is stronger in expansions and further encourages firms to post vacancies, as their offers are more likely to be accepted, so (true) tightness also moves more. For all these reasons, the recall option makes the probability of finding *new* jobs more cyclically volatile. The *total* job-finding probability, however, contains many recalls, which are much more stable, so it is slightly less volatile in the model with recall. Note the interesting tension between model and data: accounting for recalls improves amplification in the model but also makes its task harder by raising the target volatility of the new job-finding probability estimated from the data.

Recall is stable in the model as a result of three, partially opposing forces. First, a positive aggregate shock encourages production: a whole set of unemployed workers is recalled on impact when the economy improves and the separation/acceptance cutoff falls; as the expansion unfolds, some previously unlikely recalls become plausible for idiosyncratic reasons. Second, the quality of idle matches is strongly countercyclical. We compute at each point in time the ratio between the average "shadow" productivity of the unemployed, based on the still-evolving quality of the last match and the actual productivity of the marginal match at the cutoff. The regression coefficient of this ratio with respect to the unemployment rate is positive at 0.015. The intuition behind this cyclical selection is simple. The probability δ with which match quality drops permanently to its lowest (zero) value, voiding any recall option, is acyclical, while the endogenous separation probability into the unemployment pool, from which recall is possible, is countercyclical. So, in a recession, a larger fraction of unemployed workers are recallable. Finally, the incentives to search for new jobs are procyclical, while the idiosyncratic shocks leading to recall are acyclical. Therefore, in a recession, separated workers spend much longer being unemployed and are more likely to be available for a recall.

Turning to search effort, it is clearly procyclical: in expansions there are more jobs available, and the return from working is higher. As workers search harder for new vacancies, firms post more of them, so tightness responds strongly to aggregate shocks. Therefore, endogenous search effort amplifies the response of the job-finding probability, both directly and through its effect on vacancy postings. Search effort also raises the volatility of separations, strongly interacting with recall. Without a search effort margin, in an expansion, a firm is less concerned about mothballing a worker who cannot increase his search effort to take advantage of good aggregate conditions; thus, separations decline by less. The opposite is true in a recession.

Whether job search effort by the unemployed is pro- or countercyclical (here meaning negatively or positively correlated with the unemployment rate) is an important issue of difficult empirical resolution. The main stylized fact traditionally cited in this regard is the number of job search methods used by a typical unemployed worker, which increases with the unemployment rate (e.g., Shimer (2004) and an older literature reviewed in Moscarini (2001)); yet, different search methods may differ in efficacy and in intensity of use. Recently, Mukoyama et al. (2017) provided direct evidence, based in part on the American Time Use Survey (ATUS), that unemployed workers search for jobs harder at times of high unemployment. The short time span of the ATUS requires an imputation of time spent on job search, based on observations available only during the single (and special) 2008-2009 recession. On the extensive margin, after recessions non-employment shifts away from non-participants to the unemployed, who search more on average; within unemployment, the composition changes toward the long-term unemployed, who are in the ATUS the most likely to spend longer per day searching for a job when faced with a loose labor market, or with looming expiration of their unemployment benefits. Our model abstracts from the participation margin, and thus the first composition effect is not directly relevant. On the intensive margin, the time that the average unemployed worker reports searching for employment in the ATUS is clearly countercyclical, off a suspiciously low average of less than one hour per day. But within short-term unemployment, which is where recall has bite, the average daily time spent on job search is procyclical (Gomme and Lkhagvasuren (2015)), as in our model.

Finally, in our model, aggregate search effort effectively makes matching efficiency positively correlated with job market tightness. Borowczyk-Martins et al. (2013) point out that, under these circumstances, an OLS regression of the job-finding rate on job market tightness is bound to overestimate the matching function elasticity. They propose and implement a

GMM procedure to correct for this bias, and indeed find strong evidence that it is positive.

To conclude, all four models fail to replicate the negative correlation between unemployment and vacancies that constitutes the empirical Beveridge curve. This is a well-known feature of search models like Mortensen and Pissarides (1994), where endogenous separations increase in recessions the pool of unemployed workers who are available for a fresh re-match, thus spurring vacancy postings. This effect is never strong enough, however, to make tightness countercyclical. Models of purely exogenous separations like Shimer (2005) do well with the Beveridge curve but miss by construction the remarkable countercyclical volatility of the *EU* separation probability. As explained, recall amplifies fluctuations in the separation probability, so per se it makes the problem even worse. Search effort, however, compensates, because it is naturally procyclical, so effective aggregate search effort is not as countercyclical as unemployment. The argument that the MP model is mostly about job creation, justifying the simplifying assumption of a constant separation rate, does not apply to our version with recall, where temporary separations directly interfere with new hires. Future research will need to address this interesting tension.

7.4 Alternative calibration

One might argue that an alternative strategy to examine the impact of recall is to change the calibration rather than the model. Based on the idea that recalls are not mediated by a matching function, we can ignore them, and the separations that precede them, altogether when estimating transition rates. We calibrate the canonical search-and-matching MP model by matching the average probability that an unemployed worker finds a *new* job, which is only 15% per month as opposed to 27.7%, and that an employed worker is permanently separated into unemployment, 0.9% per month as opposed to 1.4%.

This different steady-state calibration hardly affects the model's ability to amplify aggregate shocks. The implied elasticity of measured job market tightness to unemployment changes only by a tiny amount, from 3.63 to 3.70. These values double the 1.78 obtained by Shimer (2005), because the value of b/p is nearly doubled in our calibration, but are still much too low, in line with the low volatility of the job-finding probability in our stochastic MP model with no search effort. The reason for this invariance result can be traced to the properties of the standard model. As the literature made clear, the amplification of aggregate shocks in the MP model can be bound above by performing comparative statics on its steady-state equilibrium. Shimer (2005) calculates the elasticity of the steady-state job-finding probability $\phi(\theta)$ with respect to aggregate labor productivity p to be, in our notation:

$$\frac{1}{1-b/p}\cdot\frac{r+\delta+\beta\phi(\theta)}{(r+\delta)(1-\alpha)+\beta\phi(\theta)}.$$

The second fraction is always close to one as long as the bargaining share β is non-negligible, because the empirical job-finding probability $\phi(\theta)$ is always, even in our alternative calibration, an order of magnitude larger than $r+\delta$. Simply put, in the U.S. economy, it is much easier to find a job than to lose one, and in fact easy enough to make discounting irrelevant.

We conclude that, in order to make quantitative progress in our understanding of cyclical unemployment, our new evidence on recall must be incorporated in the model and not only in the calibration targets. Recall is not only a matter of measurement but also a matter of incentives, for firms to mothball their workers and for workers to search for a new jobs, both of which change over the business cycle.

8 Conclusions

In this paper, we document that U.S. workers who separate from their jobs have a surprisingly high probability of going back to the same employer and that the share of such recalls out of all hires from unemployment is countercyclical. Recalls involve mostly workers on temporary layoff but also many 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) and post-re-employment attachment. Recalls are relatively stable over the business cycle, so that the hazard rate of exit from unemployment to new jobs is even more volatile 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. Recall, through its effect on expectations and job search effort, amplifies the business-cycle volatility of the average job-finding and separation probabilities.

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 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.

References

Abowd, John, Bryce Stephens, Lars Vilhuber, Fredrik Andersson, Kevin McKinney, Marc Roemer, and Simon Woodcock, "The LEHD Infrastructure Files and the Creation of the Quarterly Workforce Indicators," in Timothy Dunne, Bradford Jensen, and Mark Roberts, eds., *Producer Dynamics: New Evidence from Micro Data*, University of Chicago Press, 2009, chapter 5, pp. 149–230.

Ahn, Hie Joo and James D. Hamilton, "Heterogeneity and Unemployment Dynamics," 2015. Unpublished Manuscript, UC San Diego.

Alba-Ramirez, Alfonso, Jose Arranz, and Fernando Munoz-Bullon, "Exits from unemployment: Recall or new job," *Labour Economics*, 2007, *14* (5), 788 – 810.

Alvarez, Fernando and Robert Shimer, "Search and Rest Unemployment," *Econometrica*, 2011, *79* (1), 75–122.

Bils, Mark, Yongsung Chang, and Sun-Bin Kim, "Worker Heterogeneity and Endogenous Separations in a Matching Model of Unemployment Fluctuations," *American Economic Journal: Macroeconomics*, 2011, *3*, 128–154.

Borowczyk-Martins, Daniel, Gregory Jolivet, and Fabien Postel-Vinay, "Accounting for Endogeneity in Matching Function Estimation," *Review of Economic Dynamics*, 2013, *3* (16), 440–451.

Bound, John, Charles Brown, and Nancy Mathiowetz, "Measurement Error in Survey Data," in Heckman J.J. and Leamer E.E., eds., *Handbook of Econometrics*, 1 ed., Vol. 5, Elsevier, 2001, chapter 59, pp. 3705–3843.

Bruegemann, Bjorn and Giuseppe Moscarini, "Rent Rigidity, Asymmetric Information, and Volatility Bounds in Labor Markets," *Review of Economic Dynamics*, 2010, *13*, 575–596.

Elsby, Michael, Ryan Michaels, and Gary Solon, "The Ins and Outs of Cyclical Unemployment," *American Economic Journal: Macroeconomics*, 2009, *1* (1), 84–110.

Fallick, Bruce and Keunkwan Ryu, "The Recall and New Job Search of Laid-Off Workers: A Bivariate Proportional Hazard Model with Unobserved Heterogeneity," *Review of Economics and Statistics*, 2007, *89* (2), 313–323.

Fernandez-Blanco, Javier, "Labor Market Equilibrium with Rehiring," *International Economic Review*, 2013, *54* (3), 885–914.

Foster, Lucia, John Haltiwanger, and Chad Syverson, "Reallocation, Firm Turnover, and Efficiency: Selection on Productivity or Profitability?," *American Economic Review*, 2008, *98* (1), 394–425.

Fujita, Shigeru, "The Beveridge Curve, Job Creation, and the Propagation of Shocks," 2003. Unpublished Manuscript, UC San Diego.

_ **and Garey Ramey**, "Exogenous versus Endogenous Separation," *American Economic Journal: Macroeconomics*, 2012, *4* (4), 68–93.

_ **and Giuseppe Moscarini**, "Recall and Unemployment," 2013. NBER WP 19640.

Gomme, Paul and Damba Lkhagvasuren, "Worker Search Effort as an Amplification Mechanism," *Journal of Monetary Economics*, 2015, *76* (October), 106–122.

Groshen, Erica and Simon Potter, "Has Structural Change Contributed to a Jobless Recovery?," *Current Issues in Economics and Finance*, 2003, *9* (8).

Hagedorn, Marcus and Iourii Manovskii, "The Cyclical Behavior of Equilibrium Unemployment and Vacancies Revisited," *American Economic Review*, 2008, *98* (4), 1692–1706.

Hall, Robert E. and Paul R. Milgrom, "The Limited Influence of Unemployment on the Wage Bargain," *American Economic Review*, 2008, *98* (4), 1653–1674.

Hornstein, Andreas, "Accounting for Unemployment: The Long and Short of It," 2012. Federal Reserve Bank of Richmond Working Paper No. 12-7.

Jansson, Fredrik, "Rehires and Unemployment Duration in the Swedish Labour Market: New Evidence of Temporary Layoffs," *Labour*, 2002, *16* (2), 311–345.

Katz, Lawrence, "Layoffs, Recall, and the Duration of Unemployment," 1986. NBER Working Paper 1825.

_ **and Bruce Meyer**, "Unemployment Insurance, Recall Expectations, and Unemployment Outcomes," *Quarterly Journal of Economics*, 1990, *105* (4), 973–1002.

Kodrzycki, Yolanda, "Using Unexpected Recalls to Examine the Long-Term Earnings Effects of Job Displacement," 2007. Federal Reserve Bank of Boston WP No. 07-02.

Kroft, Kory, Fabian Lange, and Matthew Notowidigdo, “Duration Dependence and Labor Market Conditions: Evidence from a Field Experiment,” *Quarterly Journal of Economics*, 2013, *128* (3), 1123–1167.

Lilien, David, “The Cyclical Pattern of Temporary Layoffs in United States Manufacturing,” *Review of Economics and Statistics*, 1980, *62* (1), 24–31.

Mortensen, Dale and Christopher Pissarides, “Job Creation and Job Destruction in the Theory of Unemployment,” *Review of Economic Studies*, 1994, *61* (3), 397–415.

Moscarini, Giuseppe, “Excess Worker Reallocation,” *Review of Economic Studies*, 2001, *58* (3), 593–612.

_ **and Kaj Thomsson**, “Occupational and Job Mobility in the US,” *Scandinavian Journal of Economics*, 2007, *109* (4), 807–836.

Mukoyama, Toshihiko, Christina Patterson, and Aysegul Sahin, “Job Search Behavior over the Business Cycle,” *American Economic Journal: Macroeconomics*, 2017, *forthcoming.*

Nekoei, Arash and Andrea Weber, “Recall Expectations and Duration Dependence,” *American Economic Review Papers and Proceedings*, 2015, *105* (5), 142–46.

Pissarides, Christopher, “Short-Run Equilibrium Dynamics of Unemployment, Vacancies, and Real Wages,” *American Economic Review*, 1985, *75* (4), 676–690.

_ , *Equilibrium Unemployment Theory*, 2nd edition ed., MIT Press, 2000.

Shimer, Robert, “Search Intensity,” 2004. Unpublished manuscript, University of Chicago.

_ , “The Cyclical Behavior of Equilibrium Unemployment and Vacancies,” *American Economic Review*, 2005, *95* (1), 25–49.

_ , “On-the-Job Search and Strategic Bargaining,” *European Economic Review*, 2006, *50* (4), 811–830.

_ , “Reassessing the Ins and Outs of Unemployment,” *Review of Economic Dynamics*, 2012, *15*, 127–148.

Slud, Eric and Leroy Bailey, “Estimation of Attrition Biases in SIPP,” *ASA Proceedings Survey Research Methods*, 2006, pp. 3713–3720.

Stinson, Martha, “Technical Description of SIPP Job Identification Number Editing in the 1990-1993 SIPP Panels,” July 2003. Unpublished Manuscript.

Appendices (not for publication)

A Supplementary evidence from the SIPP

Table A.1 summarizes the time span covered by each panel. For the 2008 panel, we use the data up to wave 15.

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	16	64	May 2008

Each wave (interview) covers a four-month period.

A.1 Additional facts about recall

Table A.2 presents recall rates by demographic groups for $E\not{E}E$ spells in the SIPP 1990-1993 panels.

Table A.2: Recall rates by observable characteristics: $E\not{E}E$ spells, 1990-1993 panels

	Mean Recall Rates	S.E. of Mean
Age		
16−24	0.293	0.007
25−54	0.459	0.007
55−	0.626	0.016
Gender		
Male	0.414	0.007
Female	0.406	0.007
Education		
Less than High School	0.414	0.009
High School	0.439	0.008
Some College	0.369	0.008
College or Higher	0.422	0.012
Union Membership		
Non-Union	0.380	0.005
Union	0.651	0.014
Industry		
Durable Goods Manufacturing	0.521	0.016
Nondurable Goods Manufacturing	0.448	0.019
Construction	0.495	0.016
Retail/Wholesale Services	0.302	0.009
Other Services	0.426	0.007

Source: SIPP, 1990-1993 panels. Share of recalls in $E\not{E}E$ spells where separations occur in the first three waves (12 months) of each panel; “Other Services” category includes all other industries.

Table A.3 extends Table 1 by including incomplete spells such as those that remain non-employed at the end of the panel. *EU* spells include the cases that drop out of the labor force after entering into the unemployment pool initially.

Table A.3: Incidence of recall among all separations, complete and incomplete jobless spells

Panel	Actual		Actual + Imputed			
	Spell Count	Recall Rate	Spell Count	Recall Rate	Spell Count	Recall
	$E\not{E}$		$E\not{E}$		EU	
1990	4,176	0.298	4,176	0.298		
1991	2,870	0.343	2,870	0.343		
1992	3,515	0.330	3,515	0.330		
1993	3,220	0.324	3,220	0.324		
1996	10,332	0.160	10,332	0.270	4,133	0.400
2001	4,807	0.172	4,807	0.270	1,983	0.387
2004	4,570	0.189	4,570	0.273	1,770	0.424
2008	6,298	0.215	6,298	0.338	3,575	0.451

Source: SIPP. Non-employment spells start in waves 1-3 in the 1990-1993 and 2000 panels, and in waves 1-6 otherwise.

Table A.4 reports the recall rate as a share of hires from non-employment, using the raw (pre-imputation) and imputed data. The corresponding results for the separation-based measure are in Tables 1 and A.3.

Table A.4: Recall rates: hires occurred in the last year or two years of each panel

Panel	Actual				Actual + Imputed			
	Spell Count	Recall Rate	Spell Count	Recall Rate	Spell Count	Recall Rate	Spell Count	Recall Rate
	$\not{E}E$		$E\not{E}E$		$\not{E}E$		$E\not{E}E$	
1990	4,469	0.349	3,698	0.415	4,469	0.349	3,698	0.415
1991	2,948	0.302	2,325	0.381	2,948	0.302	2,325	0.381
1992	3,757	0.287	2,962	0.361	3,757	0.287	2,962	0.361
1993	3,522	0.302	2,778	0.378	3,522	0.302	2,778	0.378
1996	10,008	0.147	8,315	0.175	10,008	0.261	8,315	0.310
2001	4,365	0.159	3,602	0.190	4,365	0.283	3,602	0.337
2004	4,267	0.145	3,448	0.178	4,267	0.248	3,448	0.302
2008	7,329	0.188	5,937	0.230	7,329	0.316	5,937	0.386

Source: SIPP. Non-employment spells end in waves 7-9 in 1990-1993 and 2000 panels, in waves 7-12 in 1996 and 2004 panels, and waves 8-15 in the 2008 panel.

Table A.5 shows how the recall rates are affected by a stricter sample selection criterion that non-employment spells must be preceded and followed by an employment spell that lasted at least three months.

Table A.5: Recall rates for jobless spells bracketed by at least one or three months of continuous employment

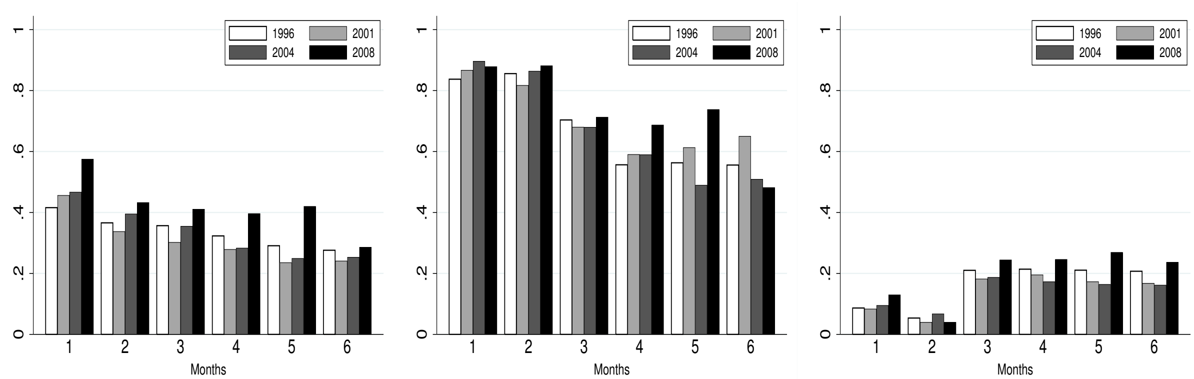
Panel	$E\not{E}\cdots\not{E}E$		$EEE\not{E}\cdots\not{E}EEE$	
	Count	Recall Rate	Count	Recall Rate
1990	3,325	0.371	1,506	0.398
1991	2,310	0.423	1,072	0.445
1992	2,827	0.407	1,365	0.457
1993	2,587	0.398	1,296	0.456

Source: SIPP. The third and fourth columns consider only the cases where a worker is employed at the same firm for at least three months continuously before and after a non-employment spell.

Figure A.1 presents shares of recalls out of all hires from unemployment at each duration by the labor force status. This figure complements the results on unemployment hazard (Figure 1).

Figure A.1: Share of recalls at each duration: 1996-2008 SIPP panels

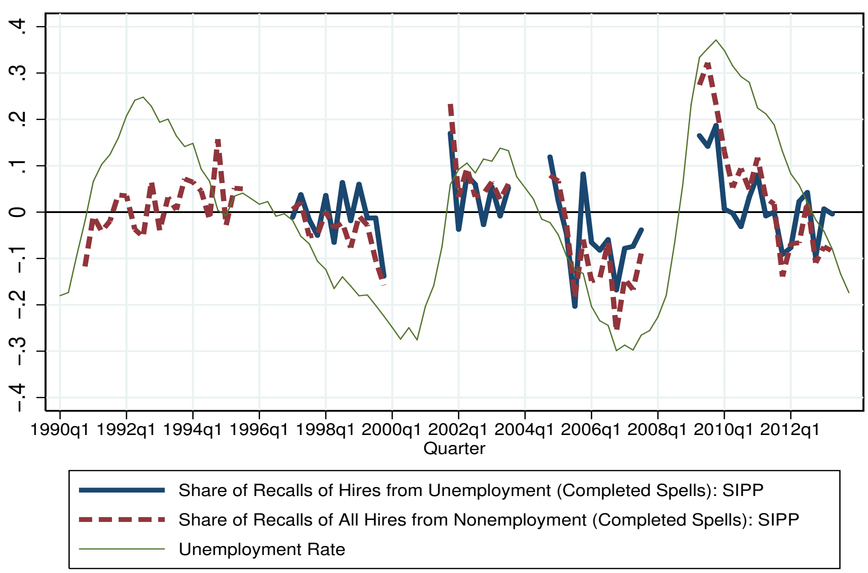
(a) All (b) TL (c) PS



Source: SIPP. Fraction of recalls at each duration. See also notes to Figure 1.

Figure A.2 plots the time series of the recall rate, measured as the share of all completed jobless spells that end in recall.

Figure A.2: Recall rates and unemployment: EUE and $E\not{E}E$



All series are seasonally adjusted. The unemployment rate is detrended by the HP-filter with smoothing parameter of 10^5. SIPP recall rates are logged and detrended by the cubic polynomial trend.

A.2 Measurement of recall

A.2.1 Misclassification of unemployment before the 1996 panel

Table A.6 shows that the TL share of the inflow into unemployment is remarkably similar and relatively stable in the SIPP and the monthly CPS over the same period.

Table A.6: Share of EU flow classified as on Temporary Layoff vs. Permanent Separation

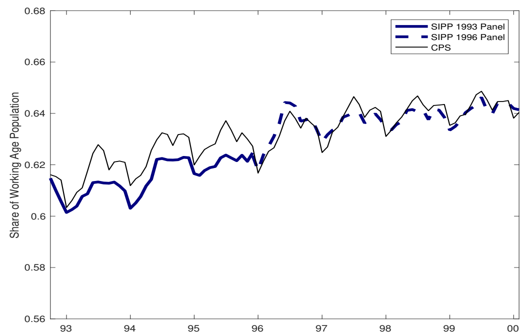
Periods covered by SIPP Panels SIPP Panels	SIPP TL/(TL+PS)	CPS TL/(TL+PS)
1996	0.34	0.38
2001	0.32	0.36
2004	0.35	0.37
2008	0.34	0.36

Source: SIPP and monthly CPS matched files. The time period for the CPS is matched with the period covered by each SIPP panel.

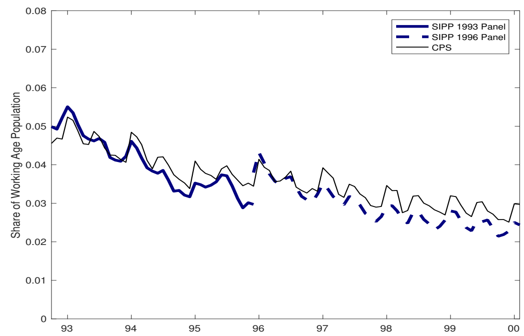
Figure A.3 illustrates the inconsistent definitions of labor market status, especially TL and OLF, in the SIPP before and after the 1996 panel redesign. The CPS redesign took place in 1994, but there is no discernible break in the TL and OLF series.

Figure A.3: Stock distribution of labor market status: SIPP vs. CPS

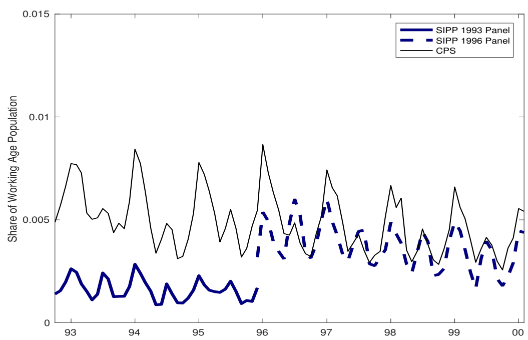
(a) Employment



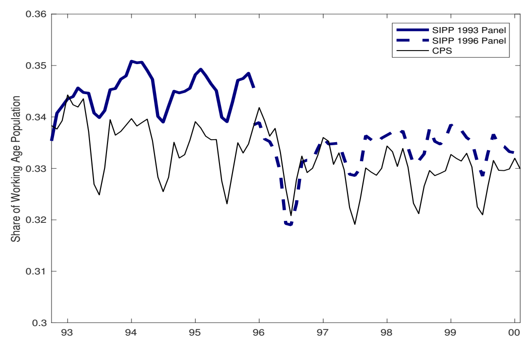
(b) Unemployment



(c) Temporary Layoffs



(d) Out-of-the-Labor Force



Sources: SIPP 1993 and 1996 panels; monthly CPS.

A.2.2 Attrition

In Table A.7, we present evidence on attrition rates to all 1990-2008 panels. "Gaps" refers to workers who miss one or more waves (interviews covering four months) but then reappear in the survey, while "Final" refers to workers who leave the survey for good. Our estimated attrition rate in the 1996 panels is higher than Slud and Bailey's (2006) because our sample covers all respondents, including those who enter the survey after Wave 1. The final attrition rate in the 2004 panel is exceedingly high because, due to budgetary reasons, midway through the panel the Census Bureau was forced to drop a random half of the sample. The attrition rate in the 2008 panel is higher because that panel was much longer than previous ones.

Table A.7: Attrition rates by SIPP panel

Panel	Gaps	Final Attrition Rates
1990	0.06	0.20
1991	0.06	0.20
1992	0.07	0.23
1993	0.07	0.25
1996	0.15	0.36
2001	0.15	0.36
2004	0.21	0.69
2008	0.34	0.44

Source: SIPP.

All these observations notwithstanding, these attrition percentages appear to be very large. Yet, we do not think this is an issue for us for four distinct reasons.

First, the longitudinal weights that we use in all of our analyses are meant to correct for attrition. While these weights provided by the Census Bureau are based only on observable worker characteristics, they certainly go some way towards reducing the problem. Second, much of the attrition in the SIPP occurs late in each panel. We select only workers who separate into non-employment early in the panel, so that non-employment duration is not right-censored by the end of the panel. Most of those workers regain employment within a year. In addition, the previous observation that recall rates are similar whether we consider all separations early in a panel and all hires late in a panel speaks against selective attrition. Third, Table A.6 shows that the share of TL in the flow from employment into unemployment that we compute from the SIPP using longitudinal weights is similar to the corresponding share in the monthly CPS. To check that the CPS does not suffer from selective attrition in terms of TL/PS inflow status, Table B.1 (presented later) reports no trend in the TL share of the flow into unemployment as we move across rotation groups, which suffer from increasing attrition as is well documented.[A.1]

Finally, the main concern for our purposes is that an omitted variable (some source of unobserved heterogeneity) causes workers to be more likely to both enter unemployment systematically as recall-prone and leave the SIPP. An excellent empirical measure of propensity to be recalled is labor market status (particularly TL vs. PS) at the time of separation. If PS workers are more likely to change address and attrite from the survey, as it seems plausible, the measured share of TL, and consequently of recalls, will be inflated. TL, as

[A.1] Similarly, we could report the same share of the *EU* flow in the SIPP by wave $n = 1, 2...$ However, this share is procyclical, and we only have one "wave n" per panel, so four total from 1996 to 2008. If all "wave n" observations happen to be at a similar state of the business cycle, the associated share could reflect cyclical movements rather than attrition.

Table A.8: Propensity of attrition from the SIPP (base category: TL)

Attrition dummy	(1)	(2)
PS	0.050**	0.076
	(0.020)	(0.064)
OLF	0.067**	0.099
	(0.019)	(0.060)
Employed	0.017	0.062
	(0.019)	(0.060)
Unemployment rate (UR)		−3.957**
		(0.906)
PS × UR		−0.217
		(0.957)
OLF × UR		−0.728
		(0.914)
Employed × UR		−1.095
		(0.911)

Source: SIPP. Estimates from Probit regression of attrition. Standard errors in parentheses. Both specifications include full dummies for gender, race, age, marital status, and education. ** indicates statistical significance at 5% level. Total number of observations: 2,494,536.

well as possibly PS workers who have some chance of recall, have stronger reasons not to move: they are hoping to go back to their job.[A.2] To address this concern, we run a Probit regression of attrition on labor force status dummies (TL, PS, Employed, OLF) and individual demographics. Although we argued that longitudinal weights control for selection by worker observables, these weights are only available for those respondents who complete the survey, while here we are studying the probability of completion and hence we must control for observables directly. In a separate specification, we also control for the unemployment rate that we compute from the same sample and for its interactions with the labor force status dummies. This latter specification allows us to examine whether selection is cyclical and related to one of our main findings that the recall rate is countercyclical.

The coefficient estimates and standard errors from the Probit regression are presented in Table A.8. To run the regression, we select observations referring only to the last available month in each wave and create an attrition dummy that equals one when the individual record prematurely ends there (before the planned end of the panel).[A.3] We also discard

[A.2]However, keep in mind that the SIPP in principle tracks people over time even after they move to a different address (in contrast to the CPS that surveys households at fixed locations). Nevertheless, one can think of various possibilities that make the interview harder when respondents move to a different location. See "Following Rules" in SIPP Users' Guide for details.

[A.3]If we used all monthly observations, we would have to include also the first three months in the last individuals who are never employed in the entire panel because we are interested in recall shares of flows into and out of employment; so we lose workers who enter the panel jobless, but had been employed before, and never regain employment during the panel. These very long-term jobless workers are unlikely to be recalled, so this sample selection tends to inflate our measured recall rate. This bias, however, is offset by the identical very long spells whose separation or hire happens to fall within the panel (thus being observed in the data) and that do not generate any recall.

The first column shows that PS and OLF are indeed associated with a higher propensity of attrition than TL in a statistically significant manner. But when we calculate separately the marginal effects implied by the coefficient estimates, these turn out to be quantitatively small: PS and OLF are only 0.49 and 0.7 percentage points more likely (over a four-month period) to drop out of the survey than TL, respectively. Moreover, when unemployment is included in the regression to control for the cyclical effects, the coefficients on the labor force status become insignificant. The second column shows that higher unemployment is associated positively with attrition. However, the interaction terms show no indication that PS and OLF are more likely to drop out when unemployment is high.

A.2.3 Seam bias and "bunching" of reported transitions

In Section 3, we investigate the seam effect in employment transition. A possible cause of this seam effect is "bunching" of reported labor force state transitions at the start of the wave. Suppose a spell "...$E\not{E}$ | $\not{E}\not{E}\not{E}E$ | ..." is reported as "...$E\not{E}$ | $EEEE$ | " because the respondent backdates the start of the last employment spell to the beginning of the four-month period on which he/she is reporting. After all, at the time of the interview the respondent is employed and thus might as well tell the interviewer that he has been employed all along since they last spoke. This error may lead to an underestimate of the duration of some non-employment spells that cross the seam. This has consequences for both the correlation between non-employment duration and recall, and our imputation procedure of recall for the post-1996 panels. We investigate the incidence and consequences of bunching for our recall rates and show that, in fact, this should not be a major concern for our purposes.

We can investigate the nature of the seam bias by comparing spells that complete within a wave with those that cross the seam. This can be done for non-employment duration of either one or two month(s), because any longer spell necessarily crosses a seam. Table A.9 shows the frequency distribution of completed spells $E\not{E}E$ with one month of non-employment, distinguished by the timing of that month in the wave. Stars in the table are placeholders

wave of the respondents in the survey, in which attrition is zero by construction. We select the last month of each wave because this is when attrition may or may not occur.

Table A.9: Distribution of one-month jobless spells $E\not{E}E$ by timing of the seam

	Count	Frequency	Recall Rate Occupation	
			Stayers	Switchers
	1990−1993 Panels			
$*\|E\not{E}E*\|*$	$1,313$	0.17	0.81	0.00
$*\|*E\not{E}E\|*$	$1,512$	0.20	0.80	0.01
$*\|**E\not{E}\|E$	$2,728$	0.36	0.79	0.11
$E\|\not{E}E**\|*$	$2,080$	0.27	0.75	0.10
	1996−2008 Panels			
$*\|E\not{E}E*\|*$	$2,313$	0.20	0.78	0.00
$*\|*E\not{E}E\|*$	$2,522$	0.22	0.82	0.00
$*\|**E\not{E}\|E$	$3,619$	0.32	0.75	0.03
$E\|\not{E}E**\|*$	$2,999$	0.26	0.63	0.02

Source: SIPP. "|" denotes the seam between waves.

for any employment status. The first two types of spells in the table complete within a wave, while the last two cross the seam and are indeed much more frequent than the first two, both before and after 1996 panel, which supports the evidence of bunching. In the last two columns, we report the recall rate, namely the share of each type of spell on the rows that end in a recall, and we distinguish between those who return to the same 3-digit occupation and those who do not, irrespective of the employer change. Recall rates for occupational stayers are very similar across all four types of short spells, both before 1996 when job IDs are accurate and after 1996. Note that the recall rate for occupational switchers is non-negligible around 10% in the 1990-1993 panels only when the spell crosses the seam. Because job IDs were validated before 1996, this strongly suggests that in those cases occupations of the two jobs that bracket the month of non-employment and the seam were sometimes incorrectly coded as different, and those spells actually belong to occupational stayers, whose recall rates are clearly high. Thus, in the 1990-1993 panels, while the timing of recalls within a wave and duration of non-employment are significantly affected by bunching and the resulting seam bias, the average recall rate is not.

A more interesting pattern emerges in the post-1996 panels, when the seam effect has a negative impact on recall rates. One possible explanation is that the duration of cross-seam spells is underestimated due to bunching, and we know that the chance of recall declines as time goes by after a separation. Instead, this bias is related to a higher rate of occupational switching when crossing a seam. In fact, in Table A.14 where we present recall rates before and after imputation for the short spells, recall rates are very similar

Table A.10: Distribution of two-month jobless spells $E\not{E}\not{E}E$ by timing of the seam

	Count	Frequency	Recall Rate Occupation	
			Stayers	Switchers
		1990−1993 Panels		
$**\|E\not{E}\not{E}E\|**$	792	0.17	0.79	0.01
$**\|*E\not{E}\not{E}\|E*$	1,826	0.38	0.79	0.08
$**\|**E\not{E}\|\not{E}E$	486	0.10	0.58	0.08
$*E\|\not{E}\not{E}E*\|**$	1,650	0.35	0.75	0.11
		1996−2008 Panels		
$**\|E\not{E}\not{E}E\|**$	1,284	0.20	0.74	0.00
$**\|*E\not{E}\not{E}\|E*$	2,274	0.35	0.67	0.01
$**\|**E\not{E}\|\not{E}E$	915	0.14	0.40	0.00
$*E\|\not{E}\not{E}E*\|**$	1,966	0.31	0.66	0.03

Source: SIPP. "|" denotes seam between waves.

within each column, independently of the seam and the time period, but differ a lot between columns, and hence only strongly depend on the occupation switch. Thus, the frequency of measured occupational switchers within wave and across seams must be making all the difference after 1996. In Table A.9 (and also in Table A.10 discussed below), the rate of occupational switching is indeed significantly higher in post-1996 spells that cross a seam relative to all other spells (both before and after 1996).[A.4] Presumably, independent coding of job IDs and occupations in different interviews, four months apart, creates false employer and occupational transitions, as opposed to within-wave spells, reported in the same interview. Moscarini and Thomsson (2007) show that independent coding of occupations in the pre-1994 (redesign) monthly CPS inflated measured rates of occupational mobility by an order of magnitude.

Table A.10 repeats the exercise for two-month completed non-employment spells. Here, only one kind ($|\ E\not{E}\not{E}E\ |$) can complete within a wave, while the remaining three cases necessarily cross a seam. The results before the 1996 panel are similar to cases with a non-employment duration of one month. We disproportionately observe completed spells that cross a seam. The one exception is in the third case, $**E\not{E}\mid\not{E}E**$, when the two months of non-employment bracket the seam, which is rare. One would think that this case would often be coded as $**E\not{E}\mid EE**$, due to "bunching" that backdates the start of the second employment spell to the beginning of the wave. Indeed, the third type $**E\not{E}\mid EE**$ of

[A.4] For brevity, we do not directly report these relative shares of occupation switchers, but they can be inferred from the information given in the tables.

one-month completed non-employment spell in Table A.9 is particularly frequent, so some of those are actually spells of duration two months or longer that are cut short by bunching. The recall rates of occupational stayers, however, are relatively unaffected by this bunching and, more generally, by the seam, because they are all around 80%, with some drop in the third case, suggesting that the "bunched" transitions were a bit more likely to be a recall. Again, the recall rates of occupational switchers before 1996 are significantly positive only when crossing the seam, suggesting measurement error in occupational mobility (as recall is accurately measured then).

A.3 Imputation of recall in post-1996 panels

A.3.1 Methodology

To impute recalls for the long spells ($E\not{E}E$ spells with non-employment duration of three months or longer) in the post-1996 panels, we use the corresponding data in the 1990-1993 panels as a reference sample. We run the logit regression on this reference sample to predict recalls in the post-1996 data. The following variables are included in the regression: quadratic polynomials in age; education categories: less than high school, high school graduate, some college, and college degree or higher; gender dummy; union membership dummy at initial employment; 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; occupation switch and industry switch dummies at the three-digit level classification, and interactions of the two switching dummies; initial occupation and industry dummies (79 occupational categories and 44 industry categories); log wage change between initial and last employment, captured as a categorical variable based on the following intervals: $(\infty, -0.5]$, $(-0.5, -0.05]$, $(-0.05, 0.03]$, $(0.03, 0.5]$, $(0.5, \infty]$; national unemployment rate to control for aggregate labor market conditions; and month-of-separation dummies to control for seasonality. We find that using non-employment duration and log wage changes as categorical variables, instead of continuous variables, helps to improve the fit of the imputation regression. We also find that negative and positive wage changes predict slightly different probabilities of recall/non-recall and thus treat positive and negative changes separately. The middle category is centered around a negative value because the average wage change of all observations is negative.

Table A.11 reports the estimated marginal effects of covariates on the probability of recall after a long jobless spell. Table A.12 describes the effects of the imputation on the recall rate of long jobless spells.

Table A.11: Marginal effects in imputation regression: Long spells

Variables	Marginal Effect	Robust S.E.
Age	0.0015**	0.0004
Education (High School Dropouts)		
High School	0.0021	0.0091
Some College	0.0040	0.0090
College or Higher	−0.0185	0.0138
Female	0.0267**	0.0078
Non-employment duration (3 to 6 months)		
7 to 9 Months	0.0016	0.0089
10 to 12 Months	−0.0322**	0.0117
13 or More Months	−0.1051**	0.0124
Occupation Switch	−0.0450**	0.0099
Industry Switch	−0.3315**	0.0110
Union Member	0.0623**	0.0158
Union Member Status Change	−0.0764**	0.0146
Employer Provided Health Insurance	0.0188*	0.0102
EPHI Status Change	−0.0631**	0.0260
Address Change	−0.0890**	0.0093
Log Real Wage Change ($-0.05 < \Delta \ln w \leq 0.03$)		
$\Delta \ln w \leq -0.5$	−0.2662**	0.0132
$-0.5 < \Delta \ln w \leq -0.05$	−0.2111**	0.0102
$0.03 < \Delta \ln w \leq 0.5$	−0.1298**	0.0096
$\Delta \ln w > 0.5$	−0.1946**	0.0152
Log (Real Wage)	0.0124	0.0091

Source: SIPP. Sample size: 14,478. Pseudo R^2: 0.3053. Based on the imputation regression on the sample of long spells (non-employment duration of three months or more) in 1990-1993 panels. See the text for the full list of covariates included in the imputation regression. For education, non-employment duration, and wage change category variables, the base category is in parentheses. ** (*) indicates statistical significance at 5% (10%) level.

Table A.12: Recall rates before and after imputation: Long spells

	Recall Rates	Total # of Obs.
90-93	0.35	15,141
96-08	0.11	22,641
96-08 Imputed	0.34	
Temporary Layoffs		
96-08	0.77	2,237
96-08 Imputed	0.72	

Source: SIPP. Long spell: Non-employment duration of ≥ 3 Months.

For short spells ($E\not{E}E$ spells with non-employment duration of one or two months) in the post 1996 panels, we impute recall if the spell satisfies three requirements: (i) it does not begin as TL; (ii) it crosses a seam; and (iii) it does not lead to an occupational switch. Again, we run a logit regression. The reference sample is made of the 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 much fewer observations. Third, we also use a labor market status variable, TL vs. PS, which was not feasible for long spells as discussed earlier. Lastly, we also add panel dummies, because the short spells are imputed within the 1996-2008 panels. Table A.13 reports the estimated marginal effects of covariates on the probability of recall after a short jobless spell. Table A.14 describes the effects of the imputation on the recall rate of short jobless spells.

After estimating the logit regressions, we simulate discrete recall outcomes (0 or 1) for all spells that are deemed unreliable, based on the predicted probabilities. All calculations that use imputed recall outcomes are averages of 50 replications of this simulation.

Table A.13: Marginal effects in imputation regression: Short spells

Variables	Marginal Effect	Robust S.E.
Age	0.0016*	0.0010
Education (High School Dropouts)		
High School	-0.0010	0.0346
Some College	0.0349	0.0329
College or Higher	0.1167**	0.0381
Female	-0.0471**	0.0216
Non-employment duration (One Month)		
Two Months	-0.0734**	0.0265
Union Member	-0.0897*	0.0485
Employer Provided Health Insurance	-0.0631**	0.0260
Address Change	-0.0407	0.0381
Log Real Wage Change ($-0.05 < \Delta \ln w \leq 0.03$)		
$\Delta \ln w \leq -0.5$	-0.4359**	0.0580
$-0.5 < \Delta \ln w \leq -0.05$	-0.6152**	0.0298
$0.03 < \Delta \ln w \leq 0.5$	-0.6473**	0.0242
$\Delta \ln w > 0.5$	-0.5887**	0.0420
Log (Real Wage)	-0.0870**	0.0209

Source: SIPP. Based on the imputation regression on the sample of non-TL short spells (non-employment duration of two months or less) of occupation stayers in 1996-2008 panels. Sample size: 1,296. Pseudo R^2: 0.3574. See the text for the full list of covariates. For education, non-employment duration, and wage change category variables, the base category is in parentheses. ** (*) indicates statistical significance at 5% (10%) level.

Table A.14: Recall rates before and after imputation: Short spells

	All	Occupation	
		Switchers	Stayers
90-93: Within	0.48	0.01	0.80
90-93: Across	0.49	0.10	0.77
96-08: Within	0.48	0.00	0.79
96-08: Across	0.32	0.02	0.68
96-08: Across, Imputed	0.34	0.01	0.72

Source: SIPP. Short spells: Non-employment duration of $\leq$ 2 Months. "Within": Entire $E\not{E}$ spell occurs within a wave. "Across": $E\not{E}$ spell crosses a seam between two waves.

A.3.2 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 that we had discarded. 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 a binary 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%. Thus, the imputation recovers the truth 70% of the time, and the share of recalls is imputed almost exactly without introducing any systematic bias.

Table A.15: In-sample validation of the imputation procedure

	Actual	Predicted	MAE
Non-TL Short Spells (Occ. Stayers)	0.482	0.474	0.294
Long spells	0.355	0.354	0.298

Source: SIPP. MAE: Mean Absolute Errors.

A.3.3 Temporal Correlation of Recall

Table A.16 reports the counts and frequencies of the workers that we observe to experience $n = 1, 2, 3, 4$ or 5+ completed non-employment spells that end in recall by the end of the panel and their respective contributions to the aggregate recall counts.[A.5] The two halves of the table refer to two different samples; one underestimates and the other overestimates the true extent of temporal correlation. We estimate significant temporal correlation in recall, but the vast majority of all recalls are unique events in the three years covered by the panel.

On the left hand side of the table, we restrict attention to workers who experience n recalls, where all n separations occur in the first three waves (twelve months) of each panel. In that sample, 83% of all recall events are accounted for by the workers who experienced a recall only once, 15% by workers who are recalled twice, and the remaining 2% by the rest. This sample selection underestimates temporal correlation because it tracks "repeatedly recalled" workers only if their non-employment spells all begin early (before the end of wave 3) in the panel and ignores later additional spells.

[A.5] Note that the counts in Table 1 are the total number of $E\not{E}E$ events, while Table A.16 reports the total number of recalls.

Table A.16: Distribution of number of recalls per worker

Recalls/ Worker	Separations in Waves 1−3				All Separations			
	Workers		Recalls		Workers		Recalls	
	Count	Freq.	Count	Freq.	Count	Freq.	Count	Freq.
1	3,758	0.91	3,758	0.83	6,825	0.77	6,825	0.58
2	334	0.08	668	0.15	1,498	0.17	2,996	0.26
3	33	0.01	99	0.02	388	0.04	1,164	0.10
4	1	0.00	4	0.00	102	0.01	408	0.03
5+	0	0.00	0	0.00	53	0.01	292	0.02
Total	4,126	1.00	4,529	1.00	8,866	1.00	11,685	1.00

Source: SIPP, 1990-1993 panels. Sample: $E\not{E}E$ spells ending in recalls.

On the right hand side of the table, we include all completed spells of non-employment in those panels, so any subsequent separations into non-employment after the first one may occur later in the panel (in waves 4 and beyond), which may lead to additional recalls. This sample selection runs into a censoring problem, as the end of the panel leaves many non-employment spells incomplete, thus exaggerating temporal correlation: Presumably, the spells of rarely recalled workers are more likely to be censored, because repeatedly recalled workers cycle quickly in and out of employment. Of all workers who experience one or more recalls in a panel, 77% experience only one, and they contribute 58% of all recall events. If we exclude the spells of workers cycling in and out of employment from both the numerator and the denominator of the recall rate, the recall rate drops to a still sizable 33%, compared to about 40% in Table 1. We repeat the exercise by focusing on all hires that are recalls, as in Table A.4, and obtain very similar results; the results are available upon request.

B Supplementary evidence from other datasets

B.1 Monthly CPS

B.1.1 Transition probabilities and unemployment duration

Table B.1 shows that the TL/PS composition of the flow into unemployment does not change significantly across CPS rotation groups. Although attrition in the CPS by rotation group is known to be severe, we do not find that it is selected on TL/PS status, just like in the SIPP.

Table B.1: Share of EU flow in monthly CPS classified as on Temporary Layoff (vs. Permanent Separation), by rotation group

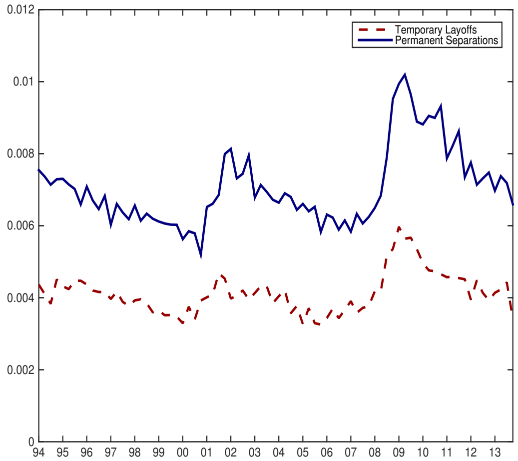
Rotation Group	1st	2nd	3rd	5th	6th	7th
$\frac{TL}{TL+PS}$	0.37	0.36	0.37	0.36	0.36	0.37

Source: monthly CPS matched file between Jan. 1996 and Dec. 2013. EU transition occurs between the month in sample in the "Rotation" column and the subsequent month. Outgoing rotation groups 4 and 8 cannot be matched one month forward.

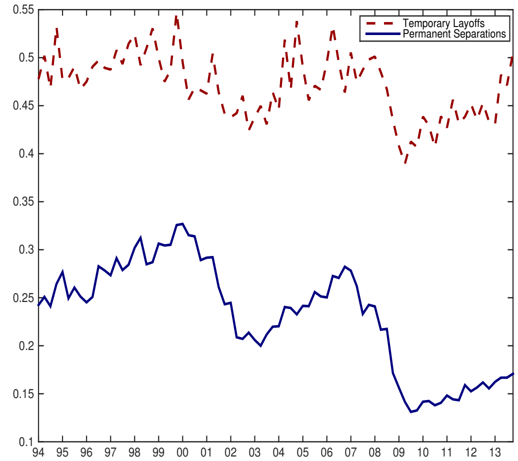
Figure B.1 plots monthly EU transition probabilities (averaged over quarterly periods) derived from the matched records. Panel (a) breaks down EU transitions into TL and PS, by dividing the EU flow for each reason by the total employment stock. This figure thus tells the relative size of the two inflows. 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. Figure B.1 and Figure 4 in the text give similar results in terms of relative size of TL and PS flows and their cyclicality. Figure B.2 confirms that median duration of those on TL is much shorter on average and less cyclical.

Figure B.1: Transition probabilities between employment and unemployment by reason: Matched monthly CPS records

(a) EU Transition Probability

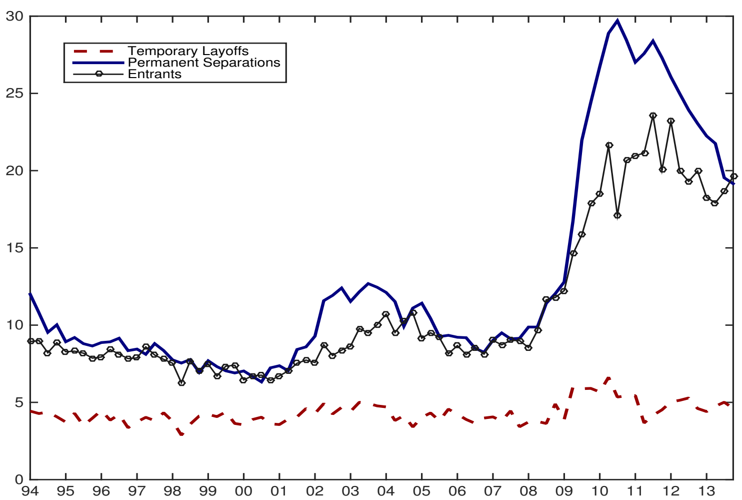


(b) UE Transition Probability



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

Figure B.2: Median unemployment duration (in weeks) by reason



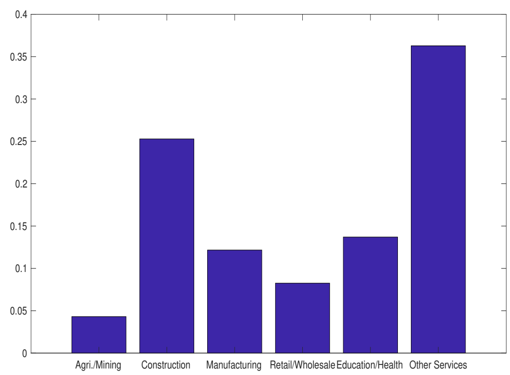
Source: Monthly CPS. Quarterly averages of monthly data.

B.1.2 Industry composition and seasonality of Temporary Layoffs

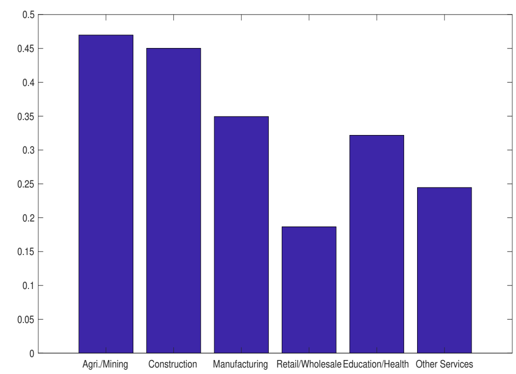
Panel (a) of Figure B.3 presents the industry breakdown of the aggregate TL separation flow into unemployment. While the contributions of the construction and manufacturing sectors are, as expected, large, TL are not at all unusual in other sectors. To take into account the relative size of each industry 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.[B.1] As expected, in agriculture/mining, construction, and manufacturing, TL are very frequent. More important, though, the shares of the separation flows that are TL in the other industries are substantial.[B.2]

Figure B.3: Industry breakdown of Temporary Layoffs

(a) Industry Shares of TL Separation Flow



(b) Share of TL Separation Flow in Each Industry



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 2013. Other services include Transportation and Utilities; Information; Financial Activities; Professional and Business Services; Leisure and Hospitality; Other Services; Public Administration; and Armed Forces.

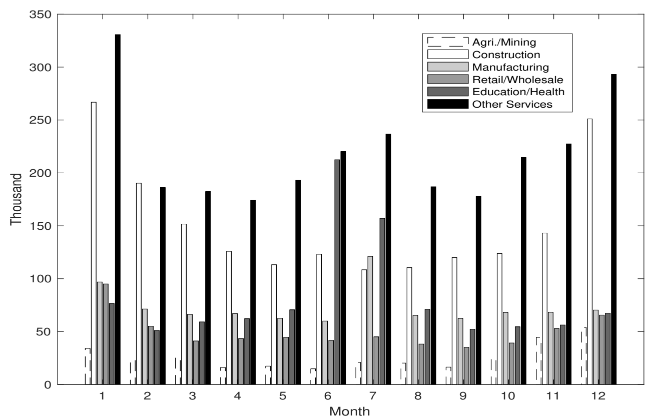
Figure B.4 summarizes the seasonal pattern of TL. All industries, except education/health, share the feature that the TL flow increases in winter months. In addition, some sectors (manufacturing and other services) shed more workers temporarily also during summer months. In the education/health sector, TL are concentrated in June. Overall, this figure suggests the presence of significant seasonal variations in the TL flow. However, Figures B.1 and 4, which plot seasonally adjusted data, demonstrate that there are also non-seasonal,

[B.1] The shares plotted in Panels (a) and (b) are averages over the period between January 2003 and December 2013, during which the industry classification used by the CPS remains consistent.

[B.2] Remember that at the aggregate level, the share of the TL flow out of all *EU* flow is roughly 30%, as suggested by Panel (a) of Figure B.1, and this average share is consistent with the shares in Panel (b).

business cycle variations in separation and job-finding probabilities associated with TL. Similarly, in our main SIPP-based analysis, we 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. 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 generate economy-wide job-finding and separation flows.

Figure B.4: Seasonality of Temporary Layoffs by industry



Source: Monthly CPS. Short-term (less than 5 weeks) TL unemployment by industry. Averages between January 2003 and December 2013.

B.2 Reconciliation of recall rates in the QWI and the SIPP

To make things simple, round up spell durations to months. QWI misses recalls after: (i) all jobless spells that last one or two months; (ii) two-thirds of all three-month jobless spells, i.e., those that do not exactly "fill" one calendar quarter; and (iii) one-fourth of the four-month jobless spells, i.e., those that are divided equally by a seam between quarters. Every jobless spell of duration five months and up necessarily implies a full calendar quarter of zero earnings and correspondingly is detected in QWI. The QWI's recall rate from joblessness is a share of all hires, $\not{E}E$ in our notation, so we calculate the contribution of spells (i)-(iii) to the recall rate in our $\not{E}E$ sample from the 1990-1993 SIPP panels, where we do not need to impute any recalls. We start with short (one or two months) completed jobless spells. In Table A.4, before 1996 there are a total of 14,696 $\not{E}E$ hires that are eligible for a recall (that occurs in the last three waves), of which about 30%, or 4,500, do end in a recall. Since the four 1990-1993 panels have eight or nine waves each, we multiply these numbers by three and estimate about 45,000 hires from non-employment before 1996, of which 13,500 are recalls. In the same early panels, we counted separately 12,702 completed spells of non-employment $E\not{E}E$ that last one month, and we estimated their recall rate at about one-half, so these short spells alone add up to about 6,000 recalls. Treating these cases as uninterrupted employment spells, i.e., "ironing them out," as QWI would do because these spells entail positive earnings and no change in employer ID for the calendar quarter, reduces both the number of recalls and the number of hires by 6,000. So the recall rate drops from 13,500/45,000=30% to (13,500-6,000)/(45,000-6,000)=19%. It drops even more if we iron out also the jobless spells $E\not{E}\not{E}E$ where the worker leaves the labor force for two months, as well as two-thirds of completed jobless spells with a duration of three months, and one-fourth of those with a duration of four months. Combined with the added recalls from employment in QWI, which we do not count in the SIPP, the 17% recall rate in QWI seems perfectly consistent with our results in Table A.4.

C Model equilibrium: Computation

Equilibrium computation requires simulating, both in and out of the steady state, a weekly panel of individual worker histories. We then sample the data every four weeks to generate a monthly panel, from which we compute relevant statistics.

C.1 Steady state

We approximate the AR(1) for idiosyncratic shocks on a discrete grid of 99 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.

In the first step, we seek a value for the steady-state contact rate of vacancies with job searchers, $\overline{q}$. Given the normalization of steady-state tightness $\overline{\theta} = 1$, this is also the worker's job contact rate per unit of search time (effort), $\overline{\theta}\overline{q} = \overline{q}$, and the scale parameter of the matching function, $\overline{\theta}\overline{q} = \mu = \overline{q}$. For any value of $\overline{q}$, we feed both contact rates into the worker's and firm's Dynamic Programming (DP) problem, which we solve by value function iteration. We find the optimal threshold $\underline{\varepsilon}$ for acceptance of a new match (as well as for separation and recall), thereby the acceptance probability of new offers, $[1 - F(\underline{\varepsilon})]$. Multiplying this acceptance probability by contact rate $\overline{\theta}\overline{q} = \overline{q}$ and by the targeted average search effort of 0.8 yields the average probability of exit from unemployment to *new* jobs. This step requires only value iteration and no simulation. We search for the value of $\overline{q}$ such that the resulting exit probability from unemployment to new jobs equals the empirical target 14.85% per month, which is our estimate for the average probability of exit from unemployment to new jobs from the SIPP. The solution to the DP problem also yields the search probability as a function of ε and the expected profits to the firm from a new match. Using the latter in the free entry condition, we back out the vacancy posting cost κ that rationalizes those values of contact and exit rates.

The second step feeds all these calibrated parameters into the simulation of a weekly panel, from which we sample the data every four weeks and recover the targeted moments. We simulate 50,000 workers over 800 weeks and discard the first 400 weeks as a burn-in period. By computing the frequency distribution of both the unemployed and the employed by their current match quality, we obtain the average search effort and the average productivity of active jobs. These two can be used to obtain the replacement ratio between value of leisure net of average search costs and average match productivity, which we target at 0.71.

C.2 Calibration of the alternative models

In the main text, we compare the cyclical properties of four different models to better understand the underlying forces of the benchmark model. Tables C.1 and C.2 put together the parameter values and the first moment properties for those different versions of the model. Recall that for the benchmark model, the values of seven parameters ($b, c_o, \rho_\varepsilon, \sigma_\varepsilon, \delta, \mu$, and κ) are estimated by minimizing the distance between nine empirical moments and steady-state moments. For the other three versions of the model, we drop the four moment conditions associated with unemployment hazard rates (job-finding and recall probabilities at first and six months). We also maintain the same values for ρ_ε and δ at 0.97 and 0.0005, respectively. As summarized in Tables C.1 and C.2, we choose values of five parameters to target the six steady-state first moments. Note that in the model without search cost, $c_0 = 0$ by construction and the moment condition for the search probability is irrelevant. Note also that, across all versions, we maintain the same replacement ratio at 0.75 as well as the same average transition rates. (As noted before, the target level of the replacement ratio is 0.71. However, our estimation procedure resulted in 0.75 for this value, which we keep for the calibrations of the other three models.)

Table C.1: Calibrations for benchmark and alternative models

Parameters	Recall Search Cost	Recall No Search Cost	No Recall Search Cost	No Recall No Search Cost
b	0.9	0.79	0.91	0.79
c_0	0.29	-	0.36	-
σ_ε	0.035	0.027	0.019	0.019
μ	0.067	0.053	0.141	0.108
κ	0.722	0.394	0.288	0.234

The rest of the parameters remain fixed.

Table C.2: First moment properties

	Recall Search Cost	Recall No Search Cost	No Recall Search Cost	No Recall No Search Cost
Job-Finding Prob.	0.29	0.29	0.27	0.27
Market Tightness (θ)	1	1	1	1
Separation Prob.	0.014	0.014	0.015	0.014
Recall Rate	0.50	0.48	–	–
Search Prob.	0.79	–	0.80	–
Replacement Ratio	0.75	0.75	0.75	0.75

C.3 Business cycles

We choose values for the parameters, serial correlation and volatility, of the AR(1) process for aggregate log TFP p, so that the time series simulated in continuous time and sampled every quarter has serial correlation and standard deviation of innovations equal to the quarterly empirical targets. We approximate this AR(1) on a discrete grid of 20 points for p using Tauchen's method.

To compute the second moments of the aggregate time series, we first solve for the dynamic stochastic equilibrium, namely Bellman values and tightness as functions of the state variables, simulate the panel dataset of 100,000 workers over 4,800 weekly periods, and discard the first 800 observations to randomize the initial conditions. Finally, we aggregate worker-level data to obtain monthly 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 further convert the monthly time series into quarterly series through simple time averaging, as we do with their empirical counterparts that are available at monthly frequency. Lastly, we take the natural logarithm of the quarterly series and HP-filter with smoothing parameter 10^5.