Recall and Unemployment*

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Abstract

Using data from the Survey of Income and Program Participation (SIPP) covering 1990-2013, we document that a surprisingly large number of workers return to their previous employer after a jobless spell, and experience very different unemployment and employment outcomes than job switchers. Furthermore, the probability of recall is much less cyclical and volatile than the probability of finding a new job. Building on these facts, we introduce a recall option in a canonical search-and-matching business-cycle model of the labor market. The recall option is lost when the unemployed worker accepts a new job. New matches are mediated by a matching function, which brings together costly vacancy postings and costly search effort by unemployed workers. In contrast, recalls are frictionless and free, and triggered both by aggregate and job-specific shocks. A quantitative version of the model captures well our cross-sectional and cyclical facts through selection of recalled matches. Model analysis shows that recall and search effort significantly amplify the cyclical volatility of job finding and separation rates.

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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 to 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 what to expect from each other. In this paper, we show that recall in the US labor market is a pervasive phenomenon with a distinct cyclical pattern, and has significant implications for individual worker experiences and for unemployment volatility.

Using data from the Survey of Income and Program Participation (SIPP), we document that recalls of former employees in the US labor market are surprisingly common: Over 40% of the employed workers who separate into unemployment (EU flow) return, after the jobless spell, to their last employer. This share of the inflow into unemployment, which we will refer to as the "recall rate," significantly exceeds the fraction of the same EU flow that is due to Temporary Layoffs (from now on: TL), namely, workers who report being laid off with a recall date or expectation. In other words, recalls are 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 then 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), experienced shorter unemployment duration (by over a month), switch occupation 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 only for those who are eventually recalled; the hazard rate of exit from unemployment to a new employer is almost constant over unemployment duration. Importantly, this feature of the data holds even when we consider all separations into unemployment (EU flow rather than EUE complete unemployment spells), including those who end up leaving the labor force. In the U.S., it is harder to find a new job than commonly thought, even at short durations; on the bright side, recall is pervasive and beneficial. A natural interpretation of this evidence is that recalls circumvent to a large extent search and matching frictions, 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, 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 out of all hires (the outflow from unemployment). This increase was especially sharp during the Great Recession.

Building on these facts, we quantify the importance of recalls for aggregate labor market fluctuations. We introduce a recall option in a 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 previous employee is still unemployed and available, he can agree with his previous employer to re-match due to intervening changes in the aggregate and/or idiosyncratic components of match productivity. Recall is free and instantaneous for both parties. In contrast, firms that either cannot or do not want to recall a previous employee must pay a cost to post a vacancy and to search for a new worker. Similarly, unemployed workers can spend costly search effort to raise their probability of contacting those vacancies and drawing a new match.

After an endogenous separation, the firm can keep the vacant position indefinitely open at no cost, hoping for conditions to improve and to 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 need not 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: as it reduces unemployment, it also destroys recall opportunities. Recall is similar to on-the-job search, in that the worker can search while still attached to an employer, but 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 find 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 relevant state variables are the exogenous productivity components.

We calibrate the model so that its steady state equilibrium replicates cross-sectional moments computed from our microeconomic evidence. In particular, we estimate by a simulated method of moments the parameters of the idiosyncratic shock process, which is the engine of turnover and recalls. The model is overidentified and yet matches quantitatively all the cross-sectional facts highlighted above. The hazard rate of recalls declines with unemployment duration, as we observe in the data, due to dynamic selection: The longer the worker stays unemployed without being recalled, the more likely it is that the (unobserved and persistent) quality of his previous match has deteriorated since separation, hence the less likely that a recall is forthcoming.

Finally, we introduce aggregate productivity shocks in the calibrated model, and simulate its stochastic 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 rate. Firms are obviously more willing to lay workers off 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, the match surplus from continuing production over temporary separation declines further in recessions. This surplus also determines the propensity to accept new matches; hence, its additional decline further reduces the average job finding rate and depresses vacancy creation. In turn, lack of jobs, lower acceptance chances, and the recall option itself all discourage costly search for new jobs by workers, again further depressing vacancy creation. Recalls are much less procyclical and more stable than new hires.

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 present evidence of the meaning and implications of our measured recalls for 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 and in Section 7 analyze its quantitative performance. Brief conclusions take stock of the results. The Appendix presents additional materials for empirical evidence and quantitative exercise.

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, which allows us to make a connection to the broader macroeconomic debate on cyclical unemployment.

Katz (1986) was the first to notice in 1981-1983 PSID data that observed negative duration dependence in unemployment is the result of a strongly declining hazard rate of exit to recall, masking the underlying upward-sloping or flat exit hazard to new jobs. Katz and Meyer (1990) take advantage of a supplemental survey of new UI recipients from Missouri and Pennsylvania in 1979-1981. The vast majority of survey participants (75%) said that they expected to be recalled, although only 18% had a definitive recall date; ex post, a sizable share were actually recalled.[1] 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.[2]

Our sample is based on the 1990-2008 panels of the SIPP, which cover the entire US labor force for almost 25 years, thus not only UI recipients, a single region, or a single deep recession. 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, in a WP version, Fujita and Moscarini (2013), with wages). We also show, however, that the strongest relationship

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

[2] This seminal work inspired a sizable literature, too large to survey completely here. Fallick and Ryu (2007) use the same data as Katz (1986) and replicate Katz and Meyer's competing hazard exercise without information on subjective recall expectations, but controlling for unobserved heterogeneity. 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 to one of third of all hires in Spain, and only recalls exhibit negative duration dependence of unemployment. Kodrzycki (2013) studies a sample of workers who suffered mass layoffs in Massachusetts in the early 1990s and qualified for expensive retraining under the Job Training Partnership Act, so 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 ended in recall, for an average of 35% of all spells.

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 rate 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 in Bils et al. (2011) is problematic. In addition, we show that Bils et al. (2011)'s choice of a four-month unemployment duration cutoff to define a recall leads to significantly underestimate true recalls, because of data issues in the SIPP that we 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 unemployed, waiting for a recall. In contrast, we introduce aggregate shocks and assume Nash Bargaining to stay close to the canonical business cycle model of a frictional labor market. We also aim to match 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 the paper 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 (2012) leaves open the possibility of sizable composition effects in terms of unobservable worker characteristics. In order to investigate this hypothesis directly, one needs high-frequency longitudinal data with multiple unemployment spells to extract some sort of fixed effects. Moreover, the sample period needs to be long enough to cover at least several business cycles. The 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 either to selection by unobserved heterogeneity of individual job-finding rates, or to pure duration dependence, such as skill loss or discouragement. He concludes that unobserved heterogeneity explains almost all of the negative duration dependence in the CPS, and that the cyclicality of the job-finding rates of the long-term unemployment "types" is the main cause of overall unemployment volatility. In our data, 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 Holstein's approach. Ahn and Hamilton (2015) explain the cyclical volatility of the average job-finding rate 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 presents each panel's length, which varies from three to five years, and the period it covers. 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 job ID to each employer for each worker, for up to two jobs held simultaneously (EENO1, EENO2). When a worker separates from an employer and, after a jobless spell, first returns to work there, 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. In other words, individuals in our sample, in principle, have complete history over the entire panel. 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.[3] 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 "LOF") 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, 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 who separate into or are hired from unemployment (U), hence EU, UE and EUE spells.

When building these different types of jobless spells, we detect an issue in the accuracy of reported labor force status in the pre-1996 panels of the SIPP. Specifically, some of the unemployed workers who are on "on Temporary Layoff" (TL) are erroneously classified as LOF. Hence, U is underestimated and LOF is overestimated before 1996. Within the unemployment pool, if the worker expects to be recalled by the same employer at the time of separation, then he/she is classified as TL. Importantly, these workers do not need to be looking for a job to be classified as unemployed. We classify those who are *not* on TL as "Permanent Separations" (PS). This label is semantically accurate only for the flow from employment, while the stock of these "Permanent Separators" includes also entrants into the labor force, who may have never held and thus never separated from a job. But, in our analysis, the TL/PS distinction is mostly relevant to flows between E and U.

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.

[3] 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%.

Figure A.3 in the Appendix shows a permanent downward jump in LOF 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.[4] We are not aware of any literature documenting or even suggesting measurement error of TL vs LOF status at entry into unemployment in the two major national surveys *after* their redesign. Since then, the definition of TL is identical in these two datasets and consistent over time. In Table A.5 in the Appendix, we compare the TL share of the inflow 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 nonparticipants in the pre-1996 SIPP panels, presumably because they were neither employed nor engaged in 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.

Going back to recalls, to avoid right-censoring of jobless spells due to the ending of the panel we further 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. This ensures that the jobless spell could last roughly two years and still be measured by the survey. 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.

Stinson (2003) showed that job IDs in the 1990-1993 panels were subject to substantial miscoding, and then corrected the problem using confidential employer name information and administrative data containing individual-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.

The procedure followed to collect job ID information changed at the time of 1996 redesign, but mismeasurement of job IDs remained, and no formal attempt was made to correct those errors (such as the one made for the 1990-1993 panels). A clear example of mismeasurement

[4] As is well known, the main impact of the CPS redesign in 1994 is on the measurement of unemployment duration due to the introduction of dependent coding.

is when a worker separates from a particular employer, is jobless for an entire 4-month wave, and comes back to the same employer. In this case, the SIPP by default assigns different job IDs, because the job ID of the first employment is lost in the wave throughout which the worker is jobless. One exception is when a worker is on TL, in which case the SIPP keeps track of the last job ID, even after a long unemployment spell.[5] 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. But we first present evidence from the raw microdata.

3.2 Preliminary evidence on the incidence of recalls

Table 1 reports the number of separations that begin early in each panel, as explained earlier. Columns under $E\not{E}$ consider all separations while those under $E\not{E}E$ focus a completed spells. Recall rates represent the share of all spells following those separations that end in recall. Note that the former sample includes the latter, and the rest are all treated as non-recall. We note that this recall rate is very high, especially before 1996, but also after 1996 when we know (and indeed see in the table) that it is underestimated. The recall rate is high even in the left part of the table, as a share of all non-employment spells, many of which do not complete but end up in retirement or persistent non-participation, hence are not even available for a recall.

Table 2 reports the same recall rates 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 good-producing sectors. But younger, non-unionized workers in service sectors still experience recall frequently, and represent the vast majority of the US workforce. The aggregate recall rates thus reflect more closely these groups. Gender and education do not make much of a difference for the recall rate.

Table A.2 in the Appendix replicates Table 1 for the fraction of *hires* that are recalls, and conveys the same general message: just like a large share of all separations end up in recall, a large share of all hires are of former employees. We again observe a drop in recall rates following the 1996 Survey 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.

[5]In the post-1996 data, the SIPP resets the job IDs after long non-TL spells, possibly in order to lighten the survey collection and processing load. The rationale could be that in those cases the new employer is different anyway, because any recall tends to happen quickly. However, using the pre-1996 cleaned job IDs, we show that this assumption is not totally warranted.

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

Panel	Separations in waves	Total Counts $E\not{E}$	Recall rates $E\not{E}$	Total Counts $E\not{E}E$	Recall rates $E\not{E}E$
1990	1−3	4,176	0.298	3,325	0.371
1991	1−3	2,870	0.343	2,310	0.423
1992	1−3	3,515	0.330	2,827	0.407
1993	1−3	3,220	0.324	2,587	0.398
1996	1−6	10,032	0.160	8,341	0.190
2001	1−3	4,807	0.172	3,904	0.209
2004	1−6	4,570	0.189	3,730	0.226
2008	1−6	6,298	0.215	4,935	0.262

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

Accurate job IDs in the pre-1996 panels are not sufficient to guarantee accurate measurement of recall rates. As mentioned, we focus on individuals who complete all waves of their panel. Non-random survey attrition may increasingly skew the sample towards workers who are more likely to be recalled. We use longitudinal weights precisely to correct for differential attrition by worker demographics and other permanent observable characteristics. This, however, does not take care of unobservables correlated with recall. 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 do not complete the survey. They examine implications for some variables, but not for recall. We extend this estimation to our entire sample period and find even slightly higher attrition rates (see Table A.6 in the Appendix). We investigate whether attrition may have an impact on our estimates of recall, both its average level and its cyclically. Specifically, we estimate a Probit regression of attrition on a rich set of demographics and labor force status (TL, PS, and LOF) at separation, which is likely to be a good proxy for unobservable heterogeneity in the propensity to be recalled. As we will see, TL status at the time of separation is a particularly strong predictor of recall. We also consider a specification that includes interaction terms between labor force status and the national unemployment rate. Our findings indicate that the marginal effects are all very small. While the attrition rate is significantly larger in expansions and for job losers relative to employees, among job losers PS are only .5% more likely to leave the survey than TL at every wave (see Table A.7).

It is well known that many types of transitions, especially between labor force states,

Table 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

Notes: 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.

tend to be reported at the "seam" between two SIPP waves (see Bound et al. (2001) for a detailed statistical analysis). We can detect this phenomenon in all panels, even 1990-1993 where job IDs were validated. Hence, the *timing* of labor market transitions 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 3 we consider "short" $E\not{E}E$ spells with non-employment duration of less than or equal to 2 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, what we do need to worry about are

Table 3: Recall Rates Before and After Imputation: Short Spells

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

Notes: Source, SIPP. Short spells: Nonemployment Duration of ≤ 2 Months. "Within": Entire $E\not{E}$ spell occurs within a wave. "Across": $E\not{E}$ spell crosses a seam between two waves.

post-1996 observations, where 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, 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 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 two: "short" and "long" spells of non-employment, of duration (resp.) less than or equal to 2 months, and 3 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 occupation, and many others, and then use the estimated coefficients to perform multiple randomized imputations for each relevant spell. TablesA.11 and A.10 in the Appendix report the specification and results of the imputation regressions.

For the short spells that begin as TL, we trust job IDs, hence the information on recalls, 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 trust job IDs when the spell does not cross the seam, because the within-wave recall rate is identical to the pre-1996 benchmark (Table 3). For the remaining short spells that do not start as TL and then cross a seam, the strongest predictor of recall is 3-digit occupational mobility. To be conservative, when we

Table 4: 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	

Notes: Source, SIPP. Long spell: Nonemployment Duration of ≥ 3 Months.

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 3). This choice is conservative because crossing a seam tends to inflate rates of occupational mobility, so some of those occupational movers are truly occupational stayers, of which some are recalled. 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, cross the seam, and do not result in an occupational change, are the 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 labor market status variable (PS or LOF), which is reliable after 1996. The recall rate after imputation is reported in the last row of Table 3. The imputation here does not make a major difference.[6]

For the long spells, the reference sample for the post-1996 imputation is the analogous sample in the 1990-1993 panels (i.e., $E\not{E}E$ spells with 3 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 after 1996 for those on TL (even though their job IDs and recalls are measured accurately) to avoid selection by unemployment status, which is obviously non-random and likely correlated with recalls. Table 4 reports the results. The imputation raises the recall

[6]Part of the reason can be traced to our assumption that, for jobless spells that do not start as TL, cross a seam, and complete quickly, a change of occupation means a change in employer. This assumption is very conservative, because in the clean pre-1996 sample 10% of these spells actually end with a recall. Based on 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.

Table 5: Recall Rates: Separations Occurred in the First Year or Two Years of Each Panel. Imputed Results in 1996−2008 Panels.

Panel	Separations in waves	Total Counts	Recall rates	Total Counts	Recall rates
		$E\not{E}$		$E\not{E}E$	
1990	1−3	4,176	0.298	3,325	0.371
1991	1−3	2,870	0.343	2,310	0.423
1992	1−3	3,515	0.330	2,827	0.407
1993	1−3	3,220	0.324	2,587	0.398
1996	1−6	10,032	0.270	8,341	0.319
2001	1−3	4,807	0.270	3,904	0.328
2004	1−6	4,570	0.273	3,730	0.328
2008	1−6	6,298	0.338	4,935	0.412
		EU		EUE	
1996	1−6	4,133	0.400	3,384	0.449
2001	1−3	1,983	0.387	1,553	0.455
2004	1−6	1,770	0.424	1,369	0.491
2008	1−6	3,575	0.451	2,756	0.532

Notes: Source, SIPP.

rate from 0.11 to 0.34, a level comparable to the one in the pre-1996 data. For TL workers, our imputation procedure recovers a recall rate at 72%. This 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 labor force status (TL/PS). Evidently, the other spell and worker characteristics used in the imputation regression capture correctly the TL status and thus recall. Note also that, in contrast to the case of short spells, the imputation of long spells clearly 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 3 months or more, because then the completed spell always crosses a seam, and in many cases it also covers an entire wave, 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 analogous 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 pro-

Table 6: Recall Rates by Reason for Separation into Unemployment

Panel	Separations in waves	Temporary Layoffs Total Counts	Temporary Layoffs Recall rates	Permanent Separations Total Counts	Permanent Separations Recall rates
1996	1−6	1,481	0.846	1,903	0.172
2001	1−3	678	0.867	875	0.168
2004	1−6	663	0.865	706	0.177
2008	1−6	1,354	0.866	1,402	0.236

Notes: Source, SIPP. The EUE sample in Table 5 is split here into two groups based on the reason of unemployment in the first month of the unemployment spell.

cedure 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 5 we present the average recall rates, by SIPP panel, after 1996 resulting from our imputation procedure. Close to 30% of all separations into non-employment $E\not{E}$ (including permanent ones like retirement) and close to 40% of all completed jobless spells $E\not{E}E$ end up in a recall. These are strikingly large numbers. Table A.3 in the Appendix shows that the same numbers apply to shares of all hires from non-employment. In both tables, a visible difference in recall rates remains between pre- and post-1996 panels, suggesting that our imputation procedure is indeed quite conservative.

The bottom part of Table 5 further restricts attention to "attached" workers, who separate into unemployment but stay in the labor force. Recall rates are now well above 40% of all EU separations into unemployment, and 50% of all complete unemployment spells EUE.

So far, we used the TL/PS classification merely to correctly build various types of jobless spells and to impute recall 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. Table 6 breaks down the incidence of ex post recall by unemployment status at the time of separation, TL or PS. 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 more frequent than TL, the contribution of these "unexpected recalls" 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 cyclically. This key result reveals an important distinction between ex

Table 7: Recall Rates and Firm Tenure

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

Notes: Source, SIPP. Based on EUE sample.

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 almost no duration dependence. This evidence will inform our modeling strategy. The first and third fact 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 fourth fact will inform this selection process in the model.

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.

Table 8: Mean Employment Duration in Months after the First $E\not{E}E$ spell

	Recall	New Hire	Recall	New Hire
	1990 Panel		1991 Panel	
All Spells	10.7	6.8	11.6	7.1
Separation $\leq$ 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 $\leq$ Wave 5	19.0	10.4	18.1	12.1

Notes: 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.

A recall, by definition, brings a worker back to the employer where he/she already has some tenure, and thus the match should last longer than average both before and after a recall. Table 7 illustrates the relationship between employer tenure before separation and subsequent recall rates. Indeed, those who had longer tenure at the time of separation are more likely to be recalled.[7]

Next, we investigate whether a recall predicts employment duration with the same employer *after* the first completed jobless spell in a panel, i.e., the E spell that begins with the "second" E in $E\not{E}E$. For this purpose, we use data in the 1990-1993 panels, where job IDs are accurate and recalls do not need to be imputed. When this second employment spell ends with a second separation followed by a recall within the time span of the panel, we extend employment duration, "skipping" the second non-employment spell and continuing after that with the third employment spell, and so forth; otherwise, we terminate the employment spell right then, when the worker moves either to non-employment or to another job. If instead the second employment spell lasts through the end of the panel, we compute its right-censored duration. Table 8 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 the 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.

The evidence presented in Tables 7 and 8 confirms that in fact recalls are different from new hires, and are associated with longer match duration both before and after the non-employment spell, hence with better mutual knowledge and lower information frictions. Table A.4 in the Appendix presents another piece of evidence pointing to the same implication.

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

Table 9: Distribution of Number of Recalls Per Worker

Recalls/ Worker	Separations in Waves 1–3				All Separations			
	Workers		Recalls		Workers		Recalls	
	Counts	Freq.	Counts	Freq.	Counts	Freq.	Counts	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

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

There, we compute the recall rates by selecting the completed spells that were preceded and followed by at least three months of continuous employment within one firm, instead of one month in our baseline calculations. This selection cuts the sample size in half, because separation begets separation. But, overall, this sample selection allows us to focus on more "stable" relationships, excluding the cases with repeated quick cycles of separations and recalls. It is also important to note, however, that this selection also drops spells where new hires separate again within 3 months. In fact, recall rates increase with this more strict selection, suggesting that the latter effect is quantitatively larger. This evidence is consistent with a more fragile nature of new employment relationships relative to recalls.

4.2 Temporal correlation of recalls

Our main focus is on the share of non-employment *spells* that end in a recall. A natural question is whether these spells are concentrated among a relatively small number of *workers*, who "cycle" in and out of employment, or rather the incidence of recall in the workforce is widespread. We answer this question with data from the 1990-1993 SIPP panels, where job IDs are accurate but the distinction between unemployment and inactivity is blurry, so we focus on spells of non-employment rather than unemployment.

Table 9 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.[8] The two halves of the table refer to two different samples; one under-estimates and the other over-estimates the true extent of temporal correlation. We estimate significant temporal correlation in recall,

[8]Note that the counts in Table 1 are the total number of $E\not{E}E$ events, while Table 9 reports the total number of recalls.

but the vast majority of all recalls are unique events in the three years covered by the panel.

In the left half 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.

In the right half 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 many more additional recalls. This sample selection runs into a censoring problem, due to the end of the panel that leaves many non-employment spells incomplete. Presumably, the spells of rarely recalled workers are more likely to be censored, because repeatedly recalled workers cycle quickly in and out of employment. This sample thus exaggerates temporal correlation. The workers who experience only one recall in the panel represent 77% of all workers who experience one or more recalls and contribute 58% of all recalls 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, this 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.2, and obtain very similar results; results are available upon request.

4.3 Unemployment duration and recall

We now turn to the association between recall and unemployment. As explained earlier, unemployment (as opposed to nonemployment) is measured accurately only after 1996, so we focus on those panels. Table 10 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 sooner than new hires. 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. Interestingly, the cyclical % 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, and the countercyclicality is

Table 10: Unemployment Duration

Panel	Sep. in waves	Overall Mean	Overall SD	Overall Counts	Recall Mean	Recall SD	Recall Counts	New Hire Mean	New Hire SD	New Hire Counts
1996	1−6	2.50	2.14	3,384	2.26	1.79	1,605	2.70	2.37	1,779
2001	1−3	2.65	2.62	1,553	2.15	1.93	742	3.06	3.01	811
2004	1−6	2.48	2.35	1,369	2.09	1.75	719	2.86	2.76	650
2008	1−6	4.21	5.51	2,756	2.95	3.49	1,523	5.65	6.86	1,233

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

especially pronounced among non-recall hires.

Recent US 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), to a new employer (second row), to non-participation (third row), and to 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 color represents a different SIPP panel.

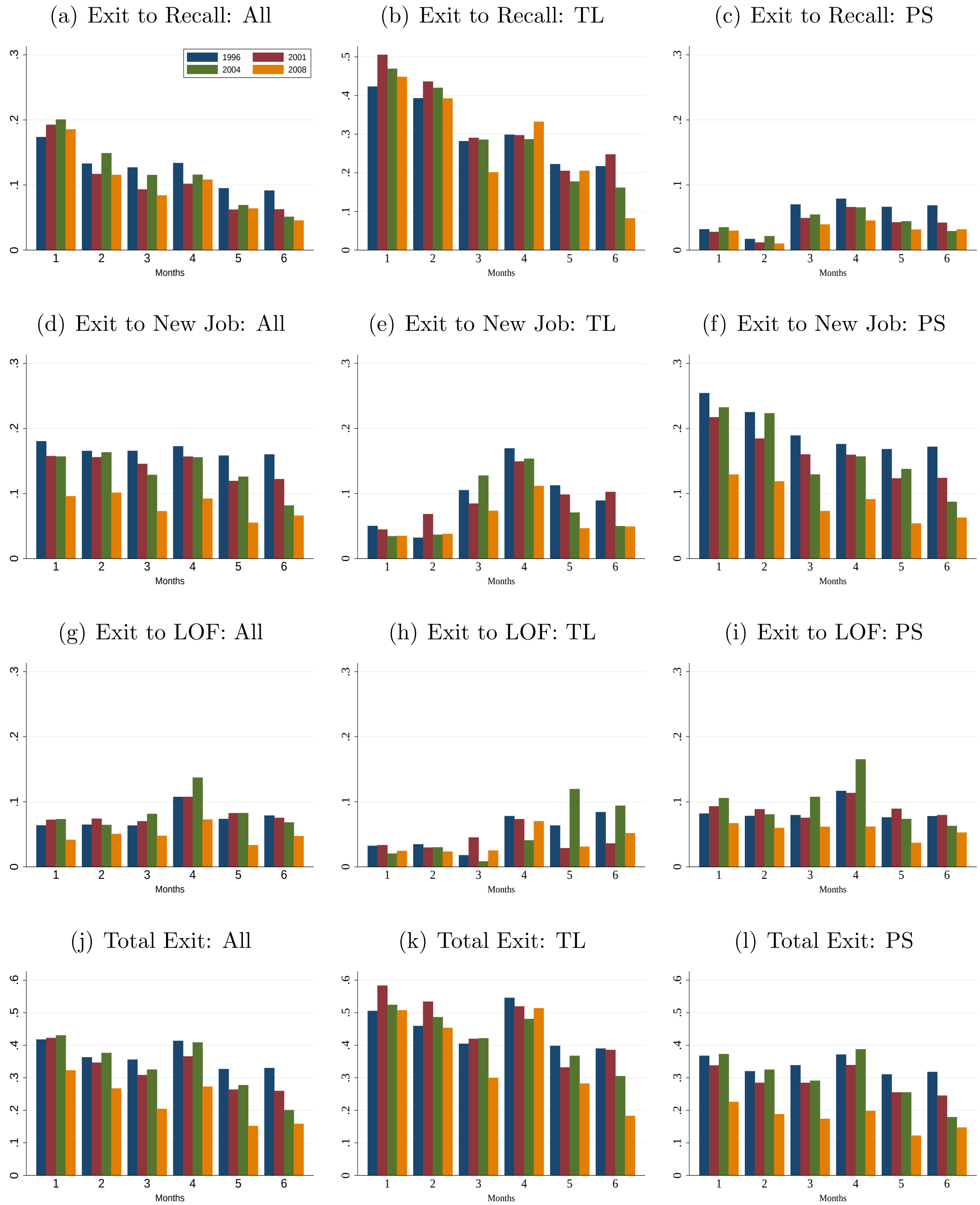
In the first column, where we present the exit hazard to different outcomes of job search, there is clear negative duration dependence in recalls, while the hazard function for those who exit unemployment by finding a job at a different employer is essentially flat. The same flat hazard appears for exit to non-participation, which we could call "discouragement". As a result, in the last row, overall duration dependence is negative[9] and entirely due to the declining chance of a recall as unemployment continues.

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

[9]Figure 1 in Kroft et al. (2013), based on monthly CPS data from 2008-2011, show that the exit rate from unemployment drops by about two thirds (half) when moving from 0 (resp., 1) to 5 completed months of unemployment. In our Figure 1 we define months of unemployment as incomplete, hence start from 1. 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



Notes: 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.

chance of finding a new job is high and barely declining. After three months, that chance drops, but the chance of a recall rises. Overall, the chance of recall is small but non-negligible, 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, as is overall exit from unemployment.

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 overall, 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 (yellow), which covers a period of extremely high national unemployment, and previous periods, especially the 1996 and 2004 panels (blue and green) 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 cyclically 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 rates (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, consistently with the well-known decline in the transition rate into non-participation observed in the CPS during and after the Great Recession.[10]

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.

5 Aggregate time series evidence on recalls

5.1 SIPP

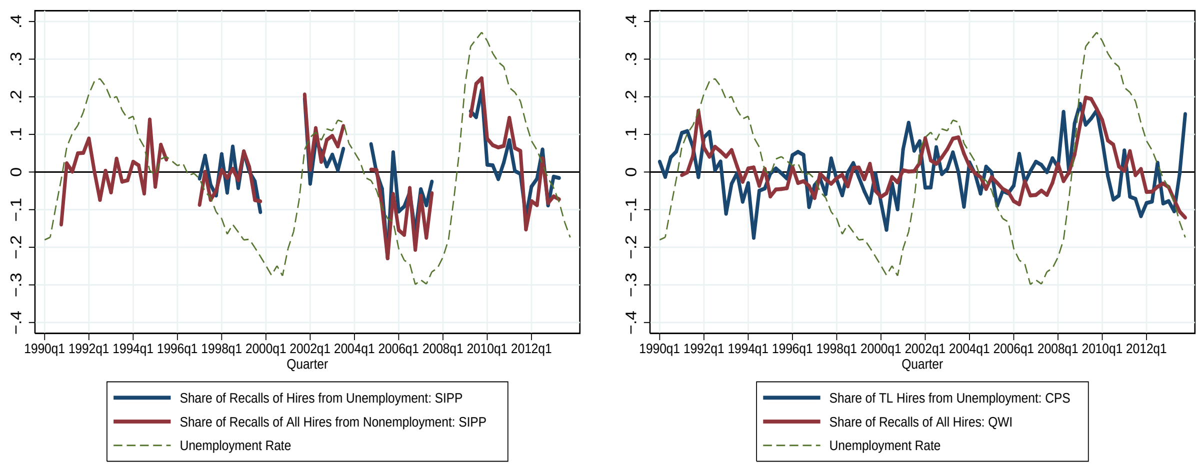
The recent debate on unemployment fluctuations revolves around job finding rates. To study how recalls impact the behavior of the overall job finding rate, this section considers

[10]Note that the decline in the labor force participation rate observed in and after the Great Recession is not inconsistent with the lower drop-out rate, because the inflow rate into participation declined even more.

Figure 2: Cyclically of Recall Rates and Unemployment Rate

(a) SIPP Recall Rates

(b) QWI Recall Rate and CPS TL Hires

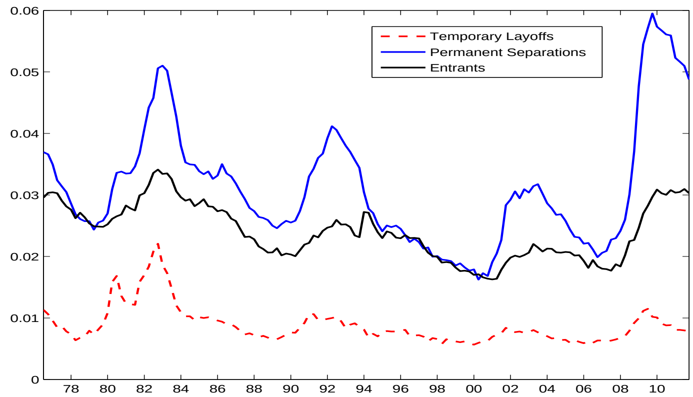


Notes: All series are seasonally adjusted. The unemployment rate, QWI recall rate, and the share of TL hires from unemployment are logged and detrended by the HP filter with smoothing parameter of 10^5. SIPP recall rates are logged and detrended by a cubic polynomial trend. The QWI recall rate is the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

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 Tables 1 and A.2). 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 1990:Q4 to 2013Q2. Only since 1997:Q1, hence for 49 observations, we can also rely on the distinction between unemployment U and LOF, 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.

The SIPP has temporal gaps, partly due to its own design (coverage of later panels is not overlapping) and partly due to our need to discard part of each panel to avoid spell censoring. So we supplement this partial time series evidence with additional pieces of evidence from the

Figure 3: Unemployment Stocks by Reason



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

monthly CPS and from the Quarterly Workforce Indicators, which allow the construction of uninterrupted time series. While both sources are dominated by the SIPP to measure recall, as discussed below, they do contain useful ancillary information to understand its cyclically.

5.2 Monthly CPS

The matched files of the monthly CPS can be used to calculate a long unbroken time series of the share of hires who were on TL, out of all hires from unemployment.[11] Given the strong but far from perfect association between TL status and recall, this time series provides useful ancillary evidence. We plot this series (quarterly averages of monthly shares) in Figure 2(b), logged and HP filtered with parameter 10^5. The correlation with the SIPP recall rate is 0.29, positive but far from perfect, highlighting once again a significant difference between ex ante TL and ex post recall. The TL share of hires from unemployment is clearly countercyclical as in the SIPP recall rate.

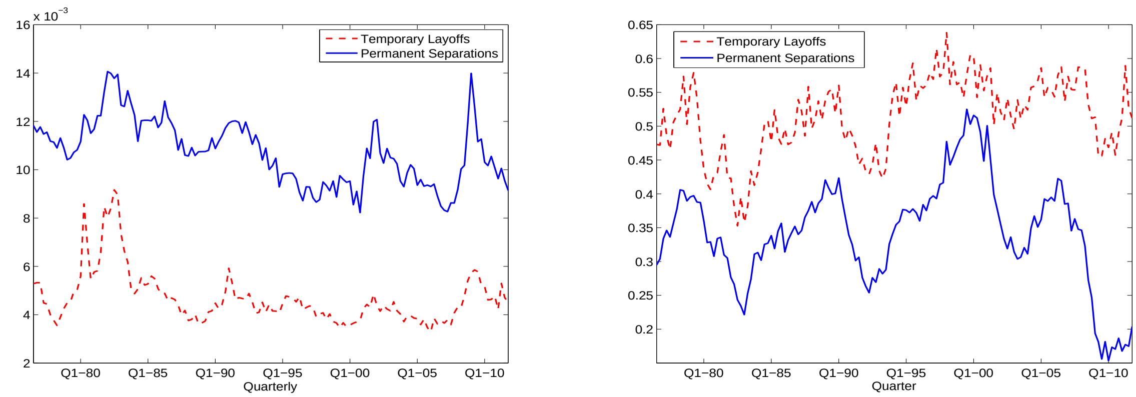
The time span of the monthly CPS affords a longer perspective on the issue of recalls and TL. Labor market researchers paid decreasing attention to TL, due to the observed decline in its level and cyclically which tracked the decline in the relative importance of the manufacturing sector, where TL were common (e.g., Groshen and Potter (2003)). 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. Indeed, Figure 3 plots unemployment stocks in the CPS by reason.

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

Figure 4: Unemployment Entry and Exit by Reason: Duration Data

(a) Entry Probability (b) Exit Probability



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

Each stock is expressed as a fraction of the labor force and thus the sum of these three lines equals the official unemployment rate. One can see that unemployment due to TL is 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 than of the stock of unemployment. The reason for the stock-flow discrepancy is that TL spend much less time in the unemployment pool than average. So, if one is interested in worker flows, TL still matter, even today. Figure 4 shows quarterly averages of monthly probabilities of entry into and exit out of unemployment, which we infer by using short-term unemployment (less than 5 weeks) as in Shimer (2012). In each panel, we show probabilities by type of inflow, TL and PS.[12] In panel (a), the TL inflow amounts to slightly less than one half of the PS inflow, and the two move more or less in parallel over business cycles, with a marked countercyclical pattern. In panel (b), workers on TL enjoy a much higher exit probability than PS workers; note also that both exit probabilities exhibit the familiar procyclicality, but it is more pronounced for PS workers. During the post Great Recession recovery, it recovered rather quickly for TL, but quite slowly for PS.

Second, and more importantly, 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, 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

[12]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).

recall, a significant share of recalls originate from the (much larger) stock of PS workers, who did not expect a recall. Furthermore, as we will document shortly, the job finding rates of unemployed workers who started the spell as TL and PS are similarly cyclical, while the exit rate from unemployment to recalls is much less cyclical than that to new jobs. To understand the importance of recalls for cyclical unemployment, the traditional measure of TL in the CPS tells only part of the story. Therefore, focusing on TL alone, whether in stocks or flows, paints an incomplete picture. When we measure all recalls, their importance and implications for the matching process and cyclical unemployment change significantly.

Figure A.4 in the Appendix provides supplementary evidence that further corroborates this overall empirical picture. We report monthly transition probabilities between employment and unemployment derived from the monthly CPS matched records. Unlike the results based on short-term unemployment, this method allows to distinguish between exit from unemployment to employment, as opposed to non-participation. With this alternative practice of constructing the data, we find the same qualitative patterns as in the previous analysis: the PS flow into unemployment is twice as large as the TL flow, and both transition rates are countercyclical. The job finding probability is much higher in level for TL, but is much more cyclical for PS. As a consequence, we show that the median duration of unemployment is much higher and more cyclical for PS (as well as for entrants) than for TL. Overall, TL experience shorter and less cyclical unemployment spells.

5.3 QWI

The Longitudinal Employer-Household Dynamics program at the Census Bureau provides a matched employer-employee dataset (LEHD) based on employment and earnings information that all formal US employers must report every quarter on all of their employees to the Department of Labor of the state where the establishments are located, to enforce experience rating of the state's Unemployment Insurance system. This information is merged by the Census Bureau with information from other censuses and surveys of employers and workers. The LEHD has 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.

This administrative dataset cover 96% of private and state employment and has no measurement error in employer ID (a state-specific business tax number) and earnings. The Census Bureau publishes aggregate tabulations of major labor market variables from the LEHD, under the name Quarterly 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 it is a hire). Calculated as a share of total gross hires, the QWI recall rate most closely corresponds to the results in our Table A.2. The QWI recall rate thus defined 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 does not contain detailed information on the worker's employment status. Only long non-employment spells can be inferred, when a worker shows no positive earnings for an entire calendar quarter.

For this very reason, on the one hand, QWI cannot make a distinction 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 than in the SIPP, where we focus on recalls from non-employment. On the other hand, QWI suffers from severe time aggregation bias, because of its quarterly measurement. Specifically, QWI fails 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. As discussed in the Appendix, 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

Interestingly, the QWI recall rate too is strongly countercyclical. We collect an unbalanced state panel of quarterly recall rates and unemployment rates, for the 32 US 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. In Figure 2(b), we present, as a summary aggregate measure, the time series of the unweighted average recall rate from these states. One can clearly see the countercyclicality: its correlation coefficient with the national unemployment rate is 0.74.

5.4 Summary: Business cycle moments

In Table 11 we present volatilities of the various detrended recall rate measures, as well as their elasticities with respect to unemployment rates, which are our measure of cyclically. In the first two columns we report the results based on our two direct measures from the SIPP. In the third column we consider the CPS-based (approximate) measure described earlier, the TL share of hires. For the QWI recall rates, we run a panel regression using state-level data on recall rates and unemployment rates (The volatility of the QWI recall rate reported in the table is based on the unweighted average series discussed above). Interestingly, all three measures are similarly and highly volatile, only slightly less than the unemployment

Table 11: Cyclically of Recall Rates

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

Notes: 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.

rate, as can be inferred from Figure 2. The SIPP measure is the most volatile. 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 cyclically. 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 to understand its relevance to unemployment dynamics, we introduce a recall option in the Mortensen and Pissarides (1994) economy and study its stochastic equilibrium when hit by aggregate productivity shocks.

6.1 Setup

Time is continuous. All agents are risk neutral and discount payoffs at rate $r > 0$. Firms produce a homogenous 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 $dH(\varepsilon'|\varepsilon)$, possibly different than $dG(\varepsilon'|\varepsilon)$. 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 recurrent.

There are search frictions in the labor market. In order to create new matches, unemployed workers spend search effort s at cost $c(s)$ to find, at rate $s\phi$, open vacancies, which are also posted at a flow cost $\kappa > 0$ as in the standard model. $c(\cdot)$ is twice continuously differentiable, increasing, and convex, with $c(0) = 0$. 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 forfeits 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 wait and do nothing ("mothball" the vacancy), or recall the last employee if still unemployed, or 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. 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 worker bargaining weight $\beta \in (0, 1)$. Unlike in the standard model, the outside options when bargaining are not obvious, as now separation is not irreversible, hence 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 once-and-for-all lump-sum transfers, and wages are continuously renegotiated, so search effort by either side is not contractible.

To conclude the description of the environment, we make one final assumption: "No Mothballing Before Production." This stipulates that, in order to match, a firm and worker who meet and draw match quality $\tilde{\varepsilon} \sim F$ must 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 captures the idea that a new match requires some initial phase of discovery and experimentation through production. Therefore, a worker and a firm who 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 ($dH(\varepsilon'|\varepsilon) = 0$), costless unemployed job search($c(s) = 0$ and s is normalized to one), and degenerate distribution of new matches (F is a mass point).

We restrict attention to 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. Most importantly, in 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 $\varepsilon = p\varepsilon = 0$), $W(p,\varepsilon)$ the worker's value of employment, $V(p,\varepsilon)$ 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, $w(p,\varepsilon)$ the wage.

Since $\varepsilon = 0$ is an absorbing state and that match will never be recalled, $V(p,0)$ equals the value of an unattached, brand new vacancy. In turn, the latter value equals zero by free entry, i.e., $V(p,0) = 0$.

Next, we examine the decision to either dissolve or mothball the match. Neither worker nor firm have any incentives to give up a recall option, unless match quality drops to zero, because waiting entails no explicit nor 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 "No Mothballing Before Production": *for new matches* $\tilde{\varepsilon} \sim F$ *only*,

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

Next, we examine the decision to accept 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, due to a lack of commitment. The worker, anticipating this, will simply compare the values that he would obtain by bargaining independently with either firm, old and new. 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 to retain his recall option. This competition will be 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 ($J\left(p, \tilde{\varepsilon}\right) > V\left(p, \tilde{\varepsilon}\right)$), 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: $W(p, \tilde{\varepsilon}) \geq \max\left\langle U(p, 0), U\left(p, \varepsilon\right), U\left(p, \tilde{\varepsilon}\right)\right\rangle$. Clearly, $U\left(p, \tilde{\varepsilon}\right) \geq U(p, 0)$ 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, $V\left(p, \tilde{\varepsilon}\right) \geq V\left(p, 0\right) = 0$ for the firm. To recap, a new match $\tilde{\varepsilon}$ will be acceptable if and only if

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

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, $J(p, \tilde{\varepsilon}) \geq V\left(p, \tilde{\varepsilon}\right) \Leftrightarrow W(p, \tilde{\varepsilon}) \geq U\left(p, \tilde{\varepsilon}\right)$. Hence, denoting by $\mathbb{I}\left\{\cdot\right\}$ the indicator function, the worker incentive constraint is weakly more binding, and the probability that a new match is acceptable equals

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

6.3 Bellman equations

We can now write the (Hamilton-Jacobi-)Bellman equations that these values solve. For the employed worker

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

Endogenous separation may follow each shock, whether aggregate (p') or idiosyncratic (ε').

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 has been mothballed due to a productivity shock, either aggregate or idiosyncratic, but might still be recalled; (iii) off the equilibrium path, 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 vacancies expects a capital gain

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

We assume that the search problem of the unemployed worker has a 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

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

In words: after each shock, to p or ε, the worker may propose to reactivate the old job; 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:

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

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

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

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) if the former employee finds another vacancy (at rate $s^*(p,\varepsilon)\,\phi(\theta(p))$) and draws a new acceptable match, which has an acceptance 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, which have no workers to recall ($\varepsilon = 0$), 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, of 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 $U(p,\varepsilon)$.

The argument for the continuation value $W(p,\tilde{\varepsilon})$ follows from the lack of commitment and of ex post competition 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 is more subtle, and by revealed preferences. If the old match ε and new match $\tilde{\varepsilon}$ are such that $U(p,\tilde{\varepsilon}) < W(p,\tilde{\varepsilon}) < U(p,\varepsilon)$, then the worker may want to continue waiting for a recall, although the new match 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; so, the surplus from his old match over continuing unemployment at that match quality must still be negative. Note that this is not true of workers who separate due to bargaining disagreement, but those do 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, in turn this 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:

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

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 in equilibrium accepts a new offer is independent of the value of the recall option ε he has in hand, only depends on the aggregate state, and 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, that we assumed to be separating for one period, we need to know the current match quality ε. This type of separations, however, are off the equilibrium path, hence do not affect the pool of unemployed from which new vacancies draw.

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 a FOC[13] yields

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

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

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

[13] Shimer (2006) points out that in the case of costly search *on the job* the Nash problem (6) may not be concave, so the 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 no search effort is available on the job.

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 net of 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 $a(p,\varepsilon)$ 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 J, V, W, U, and w as functions of ε and p, and θ as a function of p that 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 through any nonlinear iteration algorithm. We exploit this tractability to explore the quantitative properties of the model.

Before doing that, we show that our model nests its predecessors, when we assume away recall ($H = 0$) and search cost for workers ($c(s) = 0$, so $s = 1$ always). Equations (1), (3), (5), (7) depend neither on recall nor on search costs. Absent recall, free entry $V(p,\varepsilon) = 0$ for all (p,ε) replaces (4), and $U(p,\varepsilon) = U(p)$. Appropriately modifying (2), the NB wage solution reduces to

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

In steady state ($\lambda_p = 0$, $p \equiv 1$) we then 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 (2005)'s stochastic version of Pissarides (2000).

7 Quantitative analysis

7.1 Calibration

We calibrate the model in steady state and then explore its business cycle properties. A unit time interval in the model is set equal to a week. 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 be 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 $r = 0.1\%$, which roughly corresponds to 5% at annual frequency. We normalize to one the unconditional means of idiosyncratic and aggregate productivity, ε and p. In steady state, the latter takes the constant value $p = 1$. The contact rate of unemployed workers with open vacancies, per unit 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 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 needs 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 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 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 is 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 job searching, 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} and δ, the scale of the matching function μ and of the search effort cost function c_0, the vacancy cost κ, and the flow value of leisure b—by

Table 12: 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

minimizing the log unweighted distance of a vector of nine moments generated by the steady state equilibrium from their empirical counterparts. Consistently 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, resp., 20% and 10%, which imply that the hazard rate of finding new job is flat at 15%.

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 yield 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 only 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. Our study is one of the first to attempt to inform a job search technology with *direct* empirical evidence on search effort. 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.

Finally, we need a target to calibrate the flow value of leisure b. Brugeman and Moscarini

Table 13: Steady-State First Moments in the Model 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

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

(2010) work with steady state equilibrium equations that apply to a large class of search models, to study the comparative statics response of the job finding probability to changes in aggregate productivity p. They 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 (2008)'s calibration of Shimer (2005)'s 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.

Table 12 summarizes our best calibration. 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. Not all unemployed time is spent searching, and not all contacts result in acceptable new matches. Therefore, the resulting probability of filling an open vacancy is much lower than conventional values from the Job Openings and Labor Turnover Survey, where recalls and new hires cannot be distinguished. The implied value of leisure is $b = 0.9$, while average search costs paid are 0.11, for a net benefit of 0.79 and a replacement ratio to the average productivity of active matches (1.06 in our calibration) of about .75, slightly above the .71 target. The flow surplus from employment is substantial.

Tables 13 and 14 report the results of model fit. 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

Table 14: Mean Duration and Hazard Rate (Model)

	Overall Hires	Recalls	New Hires
	Mean duration		
Months	3.41	2.80	4.01
	Hazard rates		
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

Notes: The empirical counterparts are in Table 10 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.

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. For the same reason, we measure comovement, both in the data and in the model, with the semielasticity (regression coefficient) of each relevant variable on unemployment. This moment captures comovement better than the unconditional correlation, contaminated in the data by additional shocks.

The main goal of our modeling exercise is to understand the impact of the recall option on aggregate labor market fluctuations. To this purpose, we also present results from versions of the model where we remove search effort and/or recall. We label "MP" the model without recall, 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.[14] We re-calibrate the two versions of

[14]This exercise is of independent interest, as the first quantitative exploration of business cycles in a

Table 15: Standard Deviations

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

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

the MP model (with and without search effort) in 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, where shocks to ε keep hitting 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, matching scale μ, and vacancy cost κ. We also reset the value of leisure b to maintain our target replacement ratio. With search costs, we recalibrate their scale to target the share of PS in the MP model without recall. The values of the parameters for the other models are reported in the Appendix. Finally, we compute the stochastic equilibrium of these other variants of the model when hit by 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 15 and 16 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.

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), and close to the opposite is true for the overall job-finding probability. Our calibration does not target aggregate second moments, and therefore this result is not surprising. On the other hand, the MP model without recall and search effort (fourth row), which is the natural

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.

Table 16: 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

term of comparison, underestimates by much more the volatility of *both* job finding and separation probability, hence of the unemployment rate. As we know from Shimer (2005), this MP model without persistent idiosyncratic shocks does even worse on both dimensions, by construction for the separation rate which is assumed constant. 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 goes in the right direction.

The intermediate models, in the second and third row of Table 15, reveal that both recall and search effort amplify the volatility of the separation rate, while they have opposite effects on the volatility of job finding rate. Removing search effort from the recall model, in the second row of Table 15, improves the volatility of the separation rate, but rolls back any gains in the volatility of the job finding rate, 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 15) 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 of 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 Census surveys, 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 15) provides additional empirical 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 (0.133), both with search effort.

We now turn to cyclicality in Table 16. 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. While all models predict it with the wrong sign, a well-known implication of countercyclical separations, the two polar models show more promise, while the intermediate models are clearly counterfactual. 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 17 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 disappear. This concern is stronger in expansions, when both search effort and the probability of contacting new vacancies rise, hence a firm is more reluctant to separate, and hoards even more labor. Conversely, in recessions a firm is more willing to mothball its unproductive workers, because they have nowhere to go. Because this 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 volatile. Intuitively, the ability of the worker to search for other jobs while waiting for a recall, and give up the recall option, raises the surplus from staying together, more so in expansions. This 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.

Recall is stable in the model as a result of three, partially opposing forces. On the one hand, 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;

Table 17: Cyclically of "New Hires" Job Finding Rate

Model	Search Cost	JF 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

as the expansion occurs, some previously unlikely recalls become plausible for idiosyncratic reasons. On the other hand, 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 and the actual productivity of the marginal match at the cutoff. The regression coefficient of this ratio with respect the unemployment rate is positive at 0.015. The intuition behind this procyclical negative 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 rate to unemployment, from where recall is possible, is countercyclical. So, in a recession, a larger fraction of unemployed workers is recallable. Finally, the incentives to search for new jobs are procyclical, while the idiosyncratic shocks leading to recall are the same, hence in a recession separated workers spend much longer unemployed and are more likely to be available for a recall.

Turning to search effort, it is clearly procyclical, as there are more jobs, and higher returns from working, in expansions. 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. Vice versa in a recession.

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 spur 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 rate. As explained, recall amplifies fluctuations in the separation rate, 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 does not apply to our version with recall, where temporary separations directly interact with new hires. Future research will need to address this interesting tension.

8 Conclusions

In this paper, we document that US 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 entail 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, amplify 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.

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Appendices (not for publication)

A Supplementary evidence from the SIPP

Table A.1 summarizes the time span covered by each panel. The 1990 and 1991 panels cover slightly less than 3 years. But after these two panels, all panels cover 3 years or more, with the 2008 panel being the longest covering 5 years. The 1990-1993 panels have some overlapping periods, while the 1996-2008 panels covers non-overlapping periods.

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	15	60	May 2008

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

A.1 Additional facts about recall

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

Panel	Hires in waves	Total Counts $\not{E}E$	Recall rates $\not{E}E$	Total Counts $E\not{E}E$	Recall rates $E\not{E}E$
1990	7−9	4,469	0.349	3,698	0.415
1991	7−9	2,948	0.302	2,325	0.381
1992	7−9	3,757	0.287	2,962	0.361
1993	7−9	3,522	0.302	2,778	0.378
1996	7−12	10,008	0.147	8,315	0.175
2001	7−9	4,365	0.159	3,602	0.190
2004	7−12	4,267	0.145	3,448	0.178
2008	8−15	7,329	0.188	5,937	0.230

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

Table A.3: Recall Rates: Hires Occurred in the Last Year or Two Years of Each Panel. Recall Rates Imputed in 1996−2008 Panels

Panel	Hires in waves	Total Counts $\not{E}E$	Recall rates $\not{E}E$	Total Counts $E\not{E}E$	Recall rates $E\not{E}E$
1990	7−9	4,469	0.349	3,698	0.415
1991	7−9	2,948	0.302	2,325	0.381
1992	7−9	3,757	0.287	2,962	0.361
1993	7−9	3,522	0.302	2,778	0.378
1996	7−12	10,008	0.261	8,315	0.310
2001	7−9	4,365	0.283	3,602	0.337
2004	7−12	4,267	0.248	3,448	0.302
2008	8−15	7,329	0.316	5,937	0.386

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

Tables A.2 and A.3 report the recall rate as a share of hires from non-employment, using (resp.) the raw (pre-imputation) and imputed data. To minimize the left censoring of the $E\not{E}E$ spells, we consider hires that are observed later in the panel (as presented in the second column). The corresponding results for the separation-based measure are in Tables 1 and 5.

Table A.4 show how the recall rates are affected by a more strict sample selection criterion that non-employment spells must be preceded and followed by an employment spell that lasted at least three months. Compared to our benchmark case, this sample restriction somewhat raises the recall rate, as discussed in the main body of the paper.

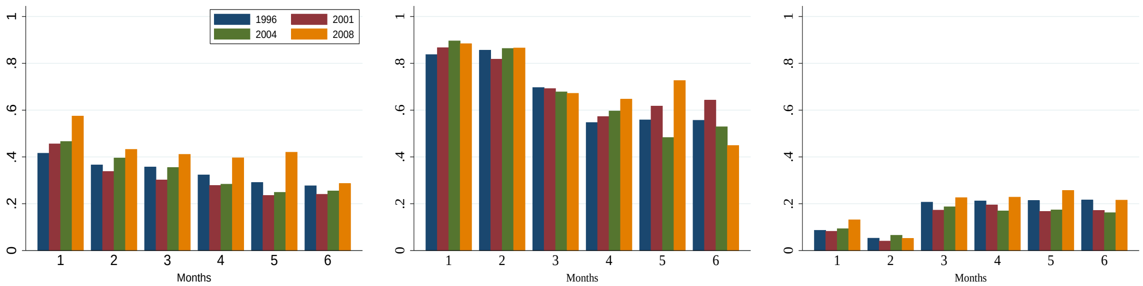
Table A.4: Recall Rates for Jobless Spells Bracketed by at Least One or Three Months of Continuous Employment

Panel	Separations in waves	$E\not{E}\cdots\not{E}E$ Total counts	$E\not{E}\cdots\not{E}E$ Recall rates	$EEE\not{E}\cdots\not{E}EEE$ Total counts	$EEE\not{E}\cdots\not{E}EEE$ Recall rates
1990	1−3	3,325	0.371	1,506	0.398
1991	1−3	2,310	0.423	1,072	0.445
1992	1−3	2,827	0.407	1,365	0.457
1993	1−3	2,587	0.398	1,296	0.456

Notes: 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: Share of Recalls at Each Duration: 1996-2008 SIPP Panels

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

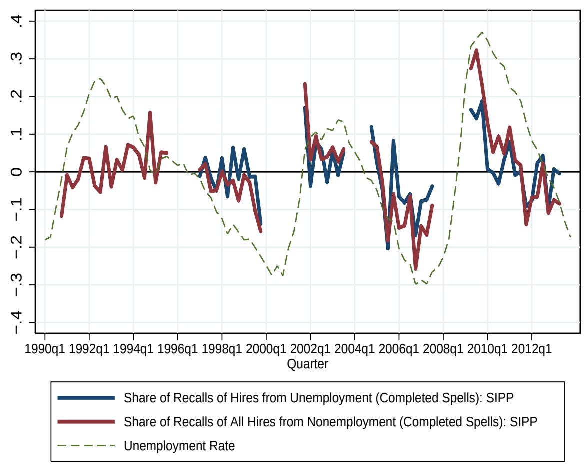


Notes: Source, SIPP. Fraction of recalls at each duration. See also notes to Figure 1.

Figure A.1 presents shares of recalls out of all hires from unemployment at each duration by the labor force status. The share of recalls decline as unemployment duration increases, indicating that the worker loses the attachment with a particular employer over time.

Finally, 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 $E\not{U}E$



Notes: 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 the cubic polynomial trend. The QWI recall rate is the average across the states where the recall series are available at each point in time.

A.2 Measurement of recall

A.2.1 Misclassification of unemployment before the 1996 panel

Table A.5 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.5: Share of *EU* Flow Classified as "On Temporary Layoff" vs. Permanent Separation

Periods covered by 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

Notes: 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 LOF, 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 LOF series.

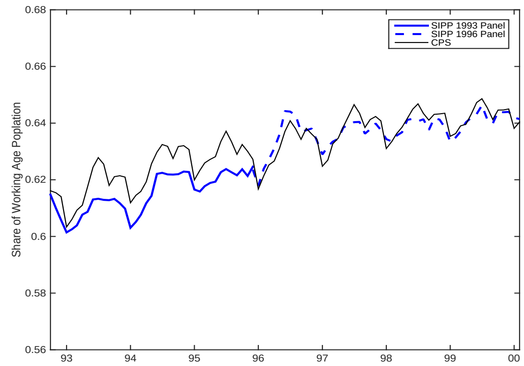
A.2.2 Attrition

In Table A.6 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 (2006)'s because we include 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 3 to 6 waves longer than previous ones. 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.

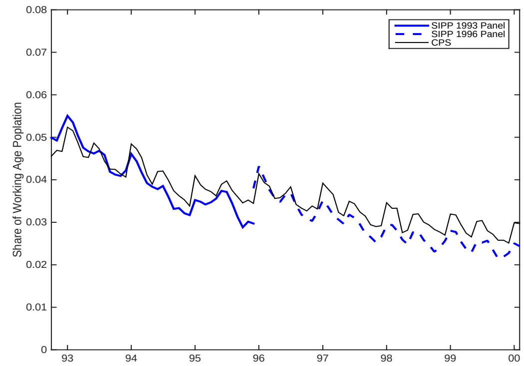
First, the longitudinal weights that we use in all of our analysis are meant to correct for attrition. While these weights provided by the Census are based only on observable worker characteristics, they certainly go some way towards reducing the problem. Second, much of the attrition in the SIPP occurs late in each panel. We select only workers who

Figure A.3: Stock Distribution of Labor Market Status: SIPP vs. CPS

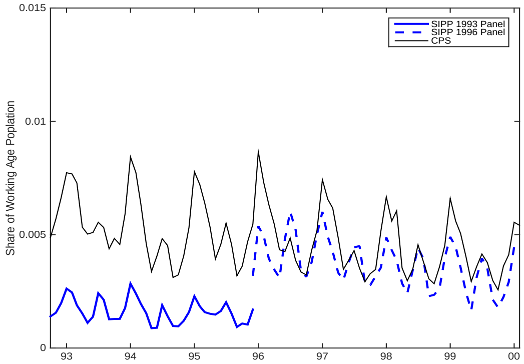
(a) Employment



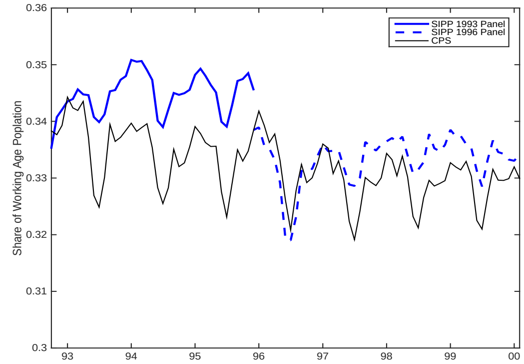
(b) Unemployment



(c) Temporary Layoffs



(d) Out-of-the-Labor Force



Notes: Sources, SIPP 1993 and 1996 panels; monthly CPS.

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.5 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 A.13 (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.[15]

[15] Similarly, we could report the same share of the EU flow in the SIPP by wave $n = 1, 2...$ But, this share is procyclical, and we only have one "wave n" per panel, so four total from 1996 to 2008. If all "wave n"

Table A.6: 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

Notes: Source, SIPP.

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 to 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 are more likely to change address and to attrite from the survey, as it seems plausible, the measured share of TL, and consequently of recalls, will be inflated. TL, as well as possibly PS who have some chance of recall, have stronger reasons not to move: they are hoping to go back to their job.[16] To address this concern, we run a Probit regression of attrition on labor force status dummies (TL, PS, Employed, LOF) 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, 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.7. 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

observations happen to be at a similar state of the business cycle, the associated share could reflect cyclical movements rather attrition.

[16]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.

Table A.7: Propensity of Attrition from SIPP (Base Category: Temporary Layoffs)

	Attrition Dummy	
PS	0.050**	0.076
	(0.020)	(0.064)
LOF	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)
LOF × UR		−0.728
		(0.914)
Employed × UR		−1.095
		(0.911)
No. of Obs.	2,494,536	

Notes: Source, SIPP. Estimates from Probit regression of attrition. Omitted category: Temporary Layoffs. Standard errors in parentheses. Both specifications include full dummies for gender, race, age, marital status, and education. ** (*) indicates statistical significance at 5% level.

record prematurely ends there (before the planned end of the panel).[17] We also discard 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, hence be observed, within the panel, and that do not generate any recall.

The first column shows that PS and LOF 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 LOF 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

[17] If we used all monthly observations, we would have to include also the first three months in the last wave of the respondent 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.8: Distribution of One-Month Jobless Spells $E\not{E}E$ by Timing of Seam

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

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

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 LOF 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 he/she is reporting on. After all, at the time of the interview the respondent is employed and thus might as well tell the interviewer that she has been employed all along since they last spoke. This error may lead to underestimate 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 1 or 2 month(s), because any longer spell necessarily crosses a seam. Table A.8 shows the frequency distribution of completed spells $E\not{E}E$ with one month of non-employment,

Table A.9: Distribution of Two-Month Jobless Spells $E\not{E}\not{E}E$ by Timing of Seam

	Counts	Frequency	Recall Rates Occ. Stayers	Occ. Switchers
		1990−1993 Panels		
$\ast\ast|E\not{E}\not{E}E|\ast\ast$	792	0.17	0.79	0.005
$\ast\ast|\ast E\not{E}\not{E}|E\ast$	1,826	0.38	0.79	0.076
$\ast\ast|\ast\ast E\not{E}|\not{E}E$	486	0.10	0.58	0.076
$\ast E|\not{E}\not{E}E\ast|\ast\ast$	1,650	0.35	0.75	0.111
		1996−2008 Panels		
$\ast\ast|E\not{E}\not{E}E|\ast\ast$	1,284	0.20	0.74	0.000
$\ast\ast|\ast E\not{E}\not{E}|E\ast$	2,274	0.35	0.67	0.013
$\ast\ast|\ast\ast E\not{E}|\not{E}E$	915	0.14	0.40	0.003
$\ast E|\not{E}\not{E}E\ast|\ast\ast$	1,966	0.31	0.66	0.029

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

distinguished by the timing of that month in the wave. Stars in the table are placeholders 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 is 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 rate is clearly high. Thus, in the 1990-1993 panels, while the timing of recalls within a wave and the 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 3 recall rates are very similar within each column, independently of the seam and the time period, but differ a lot between columns, hence only strongly depend on the occupation switch. Rather, it is the frequency of measured occupational switchers within wave and across seams to make all the difference after 1996. Indeed, in Tables A.8 and A.9 the rate of occupational switching is significantly higher in post-1996 spells that cross a seam relative to all other spells (both before and after 1996). 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.9 repeats the exercise for two-month completed non-employment spells. Here, only one kind ($\mid E\not{E}\not{E}E \mid$) 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 non-employment duration of one month. We disproportionately observe completed spells that cross a seam. The one exception is in the third case, $\ast\ast E\not{E} \mid \not{E}E\ast\ast$, when the two months of non-employment bracket the seam, which is rare. One would think that this case would often be coded as $\ast\ast E\not{E} \mid EE\ast\ast$, due to "bunching" that backdates the start of the second employment spell to the beginning of the wave. Indeed, the third type $\ast\ast E\not{E} \mid EE\ast\ast$ of one-month completed non-employment spell in Table A.8 is particularly frequent, so some of those truly are spells of duration two months or longer, 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 3 months or longer) in the post-1996 panels, we use the corresponding data in the 1990-1993 panels as a reference sample.[18] 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: (i) A Quadratic

[18]Note that we choose to discard the genuine information that we have on recalls of TL in the post-1996 panels, given the inconsistency of the labor force status information between pre- and post-1996 panels as discussed in the main text.

in age; (ii) Education categories: less than high school, high school graduate, some college, and college degree or higher; (iii) Gender dummy, union dummy at initial employment, and employer-provided health care (EPHC) dummy at initial employment; (iv) Address change dummy, union status change dummy, EPHC change dummy; (v) Non-employment duration categories: 3−6 months, 7−9 months, 10− 12 months, 13 months or longer; (vi) Occupation switch and industry switch dummies at the three-digit level classification, and interactions of the two switching dummies; (vii) Initial occupation and industry dummies, 79 occupational categories and 44 industry categories; (ix) log wage change between initial and last employment, captured as a categorical variable based on the following intervals: $(\infty, -0.5]$, $(-0.5, -0.05]$, $(-0.05, 0.03]$, $(0.03, 0.5]$, $(0.5, \infty]$; (x) National unemployment rate, to control for aggregate labor market conditions; (xi) 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 positive and negative changes are treated separately. The middle category is centered around a negative value because the average wage change of all observation is negative.

The Pseudo R^2 of the regression is 0.3053. The marginal effects of the imputation regression is presented in Table A.10.

For short spells ($E\not{E}E$ spells with non-employment duration of 1 or 2 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; (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 a 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. The Pseudo R^2 of the regression is 0.3574. The marginal effects are summarized in Table A.11.

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

Table A.10: 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
Gender (Male)		
Female	0.0267**	0.0078
Nonemployment 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 (No Switch)		
Switch	−0.0450**	0.0099
Industry Switch (No Switch)		
Switch	−0.3315**	0.0110
Union Member (Nonmember)		
Member	0.0623**	0.0158
Union Membership Status Change (No Change)		
Yes	−0.0764**	0.0146
Employer Provided Health Insurance (No EPHI)		
EPHI	0.0188*	0.0102
EPHI Status Change (No Change)		
Yes	−0.0631**	0.0260
Address Change (No Change)		
Yes	−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

Notes: Source, SIPP. Based on the imputation regression on the sample of long spells (non-employment duration of 3 months or more) in 1990-1993 panels. Sample size: 14,478. Pseudo R^2: 0.3053. See the text for the full list of covariates included in the imputation regression. The base category is in parenthesis. ** (*) indicates statistical significance at 5% (10%) level.

Table A.11: 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
Gender (Male)		
Female	−0.0471**	0.0216
Nonemployment Duration (One Month)		
Two Months	−0.0734**	0.0265
Union Member: Non Member		
Member	−0.0897*	0.0485
Employer Provided Health Insurance (No EPHI)		
EPHI	−0.0631**	0.0260
Address Change (No Change)		
Yes	−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

Notes: Source, SIPP. Based on the imputation regression on the sample of non-TL short spells (non-employment duration of 2 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 included in the imputation regression. The marginal effect represents the average change in the probability of recall with respect to a discrete change from the base category or a change by one unit if the variable is a continuous variable. The base category is in parenthesis. ** (*) indicates statistical significance at 5% (10%) level.

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

Table A.12: In-Sample Validation of Imputation

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

Notes: Source, SIPP. MAE: Mean Absolute Errors.

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%. So the imputation recovers the truth 70% of the time, and introduces no bias, hence the share of recalls is imputed almost exactly.

We also find that the imputation procedure recovers very accurately the average recall rate of workers on TL for a long spell after 1996. Table 4 in the main text shows that imputed recall rate of this sample is 72% while that in the original data (which we can trust yet cannot be used in our imputation), the recall rate is 77%. We view this as a reassuring result that suggests the reliability of our imputation procedure, given that the imputation regression does not use the labor market status variable.

B Supplementary evidence from other datasets

B.1 Monthly CPS

B.1.1 Transition probabilities and unemployment duration

Table A.13 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.

Figure A.4 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. Workers on TL enjoy a much higher job-finding probability than PS workers. Note also that both series exhibit the familiar procyclicality, but this is more pronounced for PS workers. During the post Great Recession recovery, the UE probability recovered for TL but not for PS workers. Therefore, Figure A.4 and Figure 4 in the text give similar results in terms of relative size of TL and PS flows and their cyclically. Figure A.5 confirms that median duration of those on TL is much shorter on average and less cyclical.

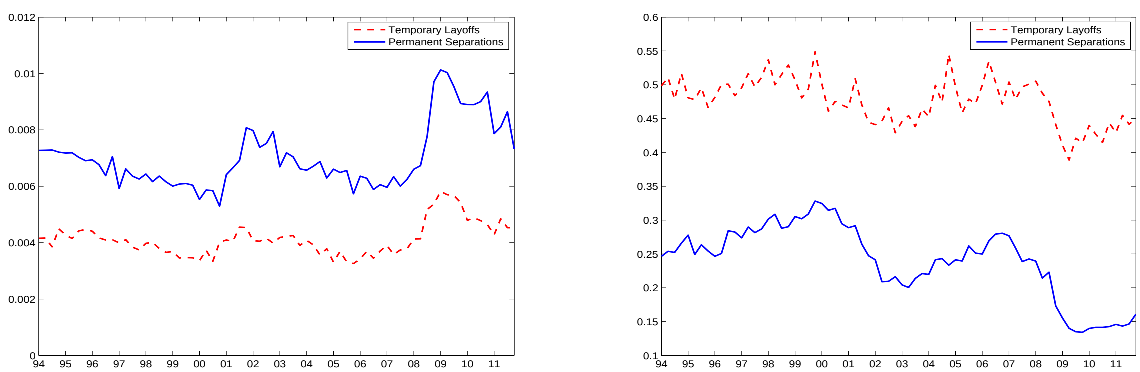
Table A.13: Share of *EU* Flow in Monthly CPS Classified as on Temporary Layoff (vs. Permanent Separation), by Rotation Group

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

Notes: 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 A.4: Transition Probabilities Between Employment and Unemployment by Reason: Matched Records

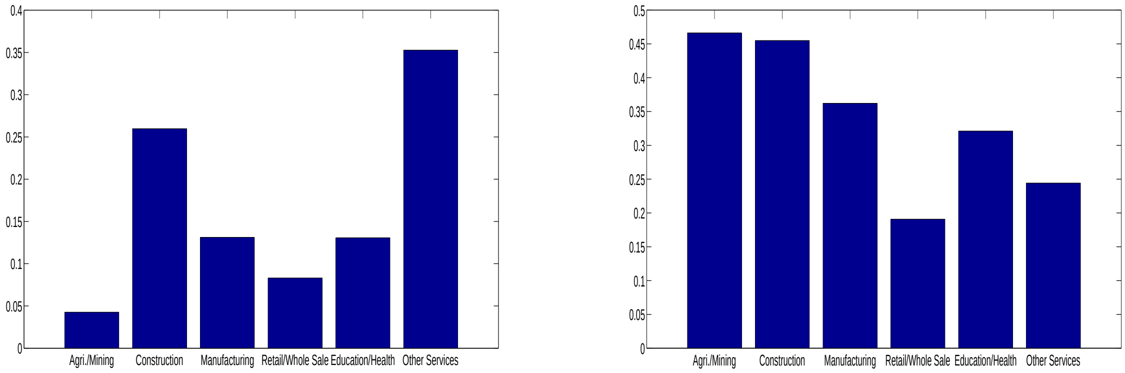
(a) EU Transition Probability (b) UE Transition Probability



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

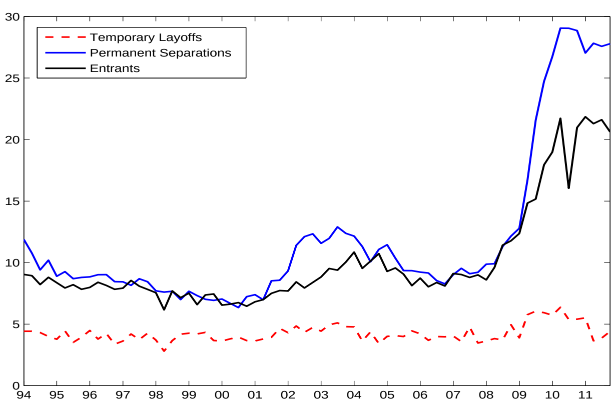
Figure A.6: Industry Breakdown of Temporary Layoffs

(a) Industry Shares of TL Separation Flow (b) Share of TL Separation Flow in Each Industry



Notes: Source, monthly CPS. Panel (a) presents the shares of each industry in the total TL separation flow. Panel (b) presents the share of TL separations out of all *EU* separations within each industry. The graphs give average shares over the period between January 2003 and December 2013. Other services include Transportation and Utilities; Information; Financial Activities; Professional and Business Services; Leisure and Hospitality; Other Services; Public Administration; Armed Forces.

Figure A.5: Median Unemployment Duration (Weeks) by Reason

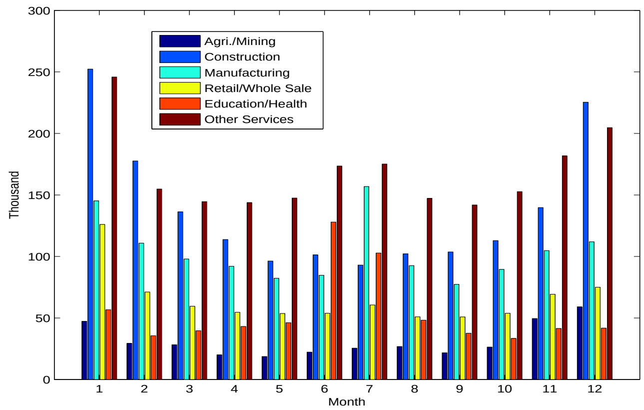


Notes: Source, Monthly CPS.

B.1.2 Industry composition and seasonality of Temporary Layoffs

It is important to note that TL are not concentrated in a particular sector (e.g., manufacturing). Panel (a) of Figure A.6 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

Figure A.7: Seasonality of Temporary Layoffs by Industries



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

industry, Panel (b) displays the share of the TL separation flow out of all EU separations within each industry.[19] As expected, in agriculture/mining, construction, and manufacturing, TL are very frequent. More importantly, though, the shares of the separation flows that are TL in the other industries are substantial.[20]

Figure A.7 summarizes the seasonal pattern of TL. All industries, except education/health, share the feature that the TL flow increases in winter months. In addition, some sectors (manufacturing and other services) shed more workers temporarily also during summer months. In the education/health sector, TL are concentrated in June. Overall, this figure suggests the presence of significant seasonal variations in the TL flow. However, Figures A.4 and 4, which plot seasonally-adjusted data, demonstrate that there are also non-seasonal, business cycle variations in separation and job-finding rates associated with TL. Similarly, in our main 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.

[19]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.

[20]Remember that at the aggregate level, the share of the TL flow out of all EU flow is roughly 30%, as suggested by Panel (a) of Figure A.4 and this average share is consistent with the shares in Panel (b).

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 1 or 2 months; (ii) two thirds of all 3-month jobless spells, those that do not exactly "fill" one calendar quarter; and (iii) one fourth of the 4-month jobless spells, those that are divided equally by a seam between quarters. Every jobless spell of duration 5 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 (i), short (one or two months) completed jobless spells. In Table A.2, before 1996 there are a total of 14,696 $\not{E}E$ hires that are eligible for a recall (happen in the last three waves), of which about 30%, or 4,500, do end in a recall. Since the three panels have 9 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 panels, we count 12,702 completed spells of unemployment EUE of duration up to two months, and we estimated the associated recall rate at about one half, so these alone add up to about 6,000 recalls. Treating these cases as uninterrupted employment spells, i.e., "ironing 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 30% to (13,500-6,000)/(45,000-6,000)=19%. The recall rate 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 of duration 3 months, and one fourth of those with duration 4 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.3.

C Model equilibrium: Computation

Equilibrium computation requires simulating, both in and out of steady state, a weekly panel of individual worker histories. We then sample the data every four weeks to generate a monthly panel of monthly, 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 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, sampled monthly, and recovers from it the targeted moments. We simulated 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 A.14 and A.15 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 A.14 and A.15, 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

Table A.14: 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

Notes: The rest of the parameters remain fixed.

Table A.15: 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

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.[21]

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 to 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

[21] 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.

(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.