Recall and Unemployment*

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Abstract

Using data from the Survey of Income and Program Participation (SIPP), we document that recalls of former employees are very frequent and associated with dramatically different unemployment and post-unemployment outcomes, relative to those of separated workers who change employer. Specifically, over 25% of all workers who are separated from their jobs (including those who immediately leave the labor force), 40% of all separated workers who remain initially unemployed, and roughly half of separated workers who remain continuously unemployed until they regain employment within two years (completed spells), go back to work for their previous employer after the jobless spell. These shares vastly exceed that of separated workers who enter unemployment on a temporary layoff, despite the fact that a significant fraction of them end up either taking another job or leaving the labor force. The reason, and one of our main findings, is that of all the workers who are permanently separated and start looking for another job, who are the bulk of the unemployed, close to 20% eventually return to their last employer. Recalled workers had twice the tenure with their previous employers before being separated, spend just over half the time unemployed, experience a more favorable real wage change (if permanently separated) and switch occupation much less often after the jobless spell. Finally, of all workers who lose jobs but remain unemployed, only those who are eventually recalled show negative unemployment duration dependence; those who change employer leave unemployment at slower but roughly constant hazard over their spell, which is also more procyclical than the hazard rate of recall. To make sense of this evidence, we introduce a recall option and aggregate productivity shocks in the standard search-and-matching model of the labor market, à la Mortensen and Pissarides (1994). While new matches require costly search and are mediated by a matching function, recalls are free and triggered both by aggregate shocks and by job-specific shocks that continue after separation. The recall option is lost when the unemployed worker accepts a new job.

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1 Introduction

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

Using data from the Survey of Income and Program Participation (SIPP), we document that one quarter of all workers who are separated from their jobs (including those who leave the labor force permanently), over 40% of all separated workers who remain unemployed, and almost one third of all unemployed workers (including new-entrants and re-entrants) are eventually recalled by their previous employer. The 40% recall rate of the separated workers who enter unemployment (employment-to-unemployment, or EU, flow) exceeds the third or so of the same flow who start unemployment on Temporary Layoff (from now on: TL). This is despite the fact that a significant fraction of TL are not eventually recalled, but end up either taking other jobs or leaving the labor force. The reason, and one of our major findings, is that close to 20% of the workers who are permanently separated (PS), i.e. lose their job with no indication of a recall date or chance, and start looking for another job, are nonetheless eventually recalled by their last employer. Because TL workers leave unemployment much faster than PS, they are a small fraction of the unemployment stock. But eventually recalled workers are large fraction of both the EU and UE flows. If we focus on workers who stay unemployed and never quit the labor force between two jobs, close to half of them return to work for their previous employer.

We also show that recall matters for labor market outcomes. Again focusing on separated workers who stay unemployed, on average, those who are eventually recalled were with their previous employer for twice as long (6 years vs. 3 years), experience shorter (by over a month) unemployment duration, and switch occupation much less often (3% vs. over 50% for job switchers). Among PS workers, especially at long unemployment duration, a recall generates a dramatically more favorable real wage outcome relative to the previous employment spell; the opposite is true for workers on TL, presumably because the option of being recalled raises their reservation wage to accept a new job offer, indeed a rare event. Of all workers who lose jobs but remain in the labor force, only those who are eventually recalled show negative unemployment duration dependence; the hazard rate of exit from unemployment to a different employer than the previous one is essentially constant with unemployment duration.

Finally, we find that the recall rate of separated workers who enter unemployment is countercyclical, especially if we focus on those workers who remain unemployed and do not leave the labor force, and less so if we include those who drop out of the labor force; hence, it appears that, in recessions, some discouraged unemployed workers leave the labor force due to a drop in recall chances, and the share of recalls in total hires rises because other hires decline much faster than recalls.

These facts raise the following questions. First, do cyclical fluctuations in the rate at which firms recall former employees contribute to explain the cyclical fluctuations in unemployment duration? How much of the persistently high unemployment after the Great Recession is due to a drop in recall rates? Second, assuming that a recall is essentially free for firms, how large is the cost of hiring a new worker, namely the cost of posting a vacancy and screening applicants? Third, what is the true effect of employer tenure on wages and turnover, if we assume that employer tenure resumes after a recall and is not reset to zero by the intervening unemployment spell? In particular, our evidence shows that occupational tenure is highly correlated with employer tenure when taking recalls into account.

To make sense of this evidence, and to help us answer these questions, we introduce a recall option in the standard search-and-matching model of the labor market, à la Mortensen and Pissarides (1994). We assume that each job is hit by idiosyncratic productivity shocks, and the economy is hit by aggregate productivity shocks. Job separation is endogenous following both kinds of shocks. After separation, the productivity of the match may keep evolving. As long as the previous employee is still unemployed and available, he can agree with his previous employer to re-match, due to intervening changes in the aggregate state and possibly in idiosyncratic match quality. Recall is free for the firm. New firms, and pre-existing firms who either cannot or do not want to recall the previous employee, must pay a cost to post a vacancy and to search for a new worker. The goal is to calibrate the model to our new empirical evidence on recall and to use it for a standard quantitative business cycle exercise, in order to infer the implications of the recall option.

In our model, after an endogenous separation, the worker is not concerned about being replaced in his old job by a new hire, because we assume that the firm can always create new positions to hire new workers. That is, free entry operates both on the intensive and the extensive margins. Conversely, the firm must keep track of the worker availability for a recall, because a worker can only work for one employer at each point in time, and we assume that accepting a new job voids the recall option of the former employer. The unemployed worker, while waiting for a possible recall, may be searching for another job. When he finds one, it is as if the quality of his (re-)match with his previous employer dropped to a permanently low level.

The chance of this kind of match quality shock is endogenous to the economy, because it is the chance that the unemployed worker finds and accepts another job while waiting for a recall. But the job-finding probability is already a key object of interest in the stochastic search model, so tracking its evolution entails no additional complications. Essentially, the recall option makes match quality evolve also during unemployment, according to a transition law that depends on job market tightness.

In this environment, firms that lose the recall option, as well as new firms, post new vacancies to meet searching workers. Some of these workers still have a recall option. If their outside option when receiving a new offer varies with the chance of recall by their former employer, under Nash Bargaining, a new vacancy will earn different profits depending on which worker it meets. If most unemployed workers have a good recall option, their bargaining stance is stronger and the incentives to create new jobs are reduced. Thus, the distribution of unemployed worker by quality of the match with their former employers, an endogenous and infinitely-dimensional object, is a state variable that pins down entry and equilibrium. To avoid this complication, we assume that an unemployed worker must give up any recall option with a former employer before bargaining a wage and acquiring the new recall option with the a firm. Therefore, all new hires from unemployment receive the same wage and generate the same profits for the firm, although they receive different gains from the new job, depending on the value of the recall option they have to give up.

The rest of the paper is organized as follows. In Section 2 we place our contribution in the context of the relevant literature. In Section 3 we describe the new facts on recall of separated workers by their former employers. In Section 4 we describe and analyze a search-and-matching model with recall.

2 Related Literature

Several authors noticed before that recall of newly separated workers is surprisingly frequent and fast, and have explored some of the implications of these facts. To the best of our knowledge, no one has documented the systematic difference between recalled and non-recalled workers in terms of: previous employer tenure, negative duration dependence of unemployment, and subsequent occupational turnover. Katz and Meyer (1990) find more favorable wage outcomes following a recall, but they only study unemployment insurance recipients and only from two US states for three years, while we study a nationally representative sample of all workers, for over 20 years. Nonetheless, our numbers are comparable to theirs. They also show that in their special sample many more newly unemployed workers expect to be recalled than they actually get recalled. SIPP does not have a similar measure of expectations, besides TL status. But we do find that a significant fraction of PS workers, who say that the previous employer did not indicate a possible recall date, end up being recalled nonetheless.

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

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

Shimer (2012) examines the "heterogeneity hypothesis" to explain the strong cyclical volatility of the average monthly job finding probability of unemployed workers. That is, he asks whether these cyclical movements are the result of composition effects in the unemployment pool, or rather all types of unemployed workers experience cyclical job-finding opportunities.

He finds that the best case for this hypothesis can be made when breaking down the unemployed between TL and PS, as their proportions are slightly cyclical and their relative job finding chances are very different; but he still finds that this channel explains a small fraction of cyclical movement in the average job finding probability. We can explore a similar hypothesis by breaking down the unemployed based on the type of exit (recall vs. different employer) as opposed entry (TL vs. PS). Because the majority of recalled workers are PS and thus, unlike TL, do not expect to be recalled with high chance, this is not a "heterogeneity" decomposition like Shimer's. Instead, we can check whether cyclical fluctuations in the hiring methods used by firms (cheap recall vs. costly search of new hires) contributes to overall cyclical volatility.

Shimer leaves open the possibility that composition effects in terms of unobservable worker characteristics may be important. In order to investigate this question directly, one needs high-frequency longitudinal data with multiple unemployment spells, to extract fixed-effects, over a long time horizon, to cover several business cycles. The CPS has too short a panel dimension to cover multiple spells, and SIPP too short a time dimension to cover more than three business cycles. Hornstein (2011) tackles this questions indirectly. He formulates a statistical model of unemployment duration dependence, which can arise either from unobserved heterogeneity of individual job finding rates and the resulting selection, or from pure duration dependence, such as skill loss or discouragement. He allows for time-varying job-finding rates for each group of workers (by unobserved heterogeneity) and a time-varying transition rate between groups (genuine duration dependence). He uses the business cycle variation in observed unemployment duration dependence and in overall exit from unemployment, extracted from semi-aggregated statistics from the CPS, to separate the contributions of these channels. He concludes that unobserved heterogeneity explains almost all of the negative duration dependence, and the cyclicality of the job-finding rates of the long-term unemployed "types" is the main cause of overall unemployment volatility. In our data and setting, the long-term unemployed are mostly those workers who are not recalled ex post: they take longer to find a job, and their wages suffer more. We also show that these non-recalled workers were on average shorter-tenured before separation and then, during their unemployment spell, exhibit no duration dependence. Therefore, we will explore whether the observation of a recall may add some empirical content to the unobserved heterogeneity of unemployed in terms of job-finding outcomes. Hornstein exploits time variation in the distribution of unemployment duration in the CPS, thus cannot distinguish between exits from unemployment to employment or non-participation. Our empirical evidence from the SIPP, based on individual histories, shows that recall rates are still high but significantly lower and definitely more cyclical if we consider all job losers, including those who give up job search and participation at some point during the jobless spell, either temporarily or for good.

3 Empirical Evidence on Temporary Layoffs and Recalls

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

3.1 Facts from the CPS

For our purposes, the main source of the information in the CPS is unemployment by reason, combined with worker transition data. The CPS transition data do not allow us to identify recalls. However, unemployment due to TL is closely related to recalls, given that workers on TL are supposed to have a clear expectation of a recall. At the same time, as we will find in SIPP, recalls are not equal to TL, since workers on TL may not be recalled, and conversely, workers not on TL may be recalled ex post.

In the CPS, there are six reasons for unemployment: (i) on temporary layoff, (ii) permanent job losers, (iii) persons completed temporary jobs, (iv) job leavers, (v) reentrants, and (vi) new entrants. We reclassify these six groups into three groups. We treat the group (i) on its own. Groups (ii) through (iv) are lumped together and called “permanent separations” (PS). The last two groups (v) and (vi) are treated as one group and called “entrants.”

3.1.1 Transition Rates

Figure 1 presents the transition rates between employment and unemployment derived from the matched records. Panel (a) breaks down employment-to-unemployment (EU) transition rates into TL and PS. Note that each line is calculated by dividing EU flow for each reason by the total employment stock. This figure thus tells the relative size of the two flows. Observe that the separation flow associated with TL amounts to roughly one half of the flow associated with PS. In terms of the cyclicality, the separation rate for TL moves more or less in parallel with that of PS. Panel (b) presents unemployment-to-employment transition (job finding) rates by reason. Workers on TL face a dramatically higher job finding rate, compared to PS workers.

(a) EU Transition Rate (b) UE Transition Rate

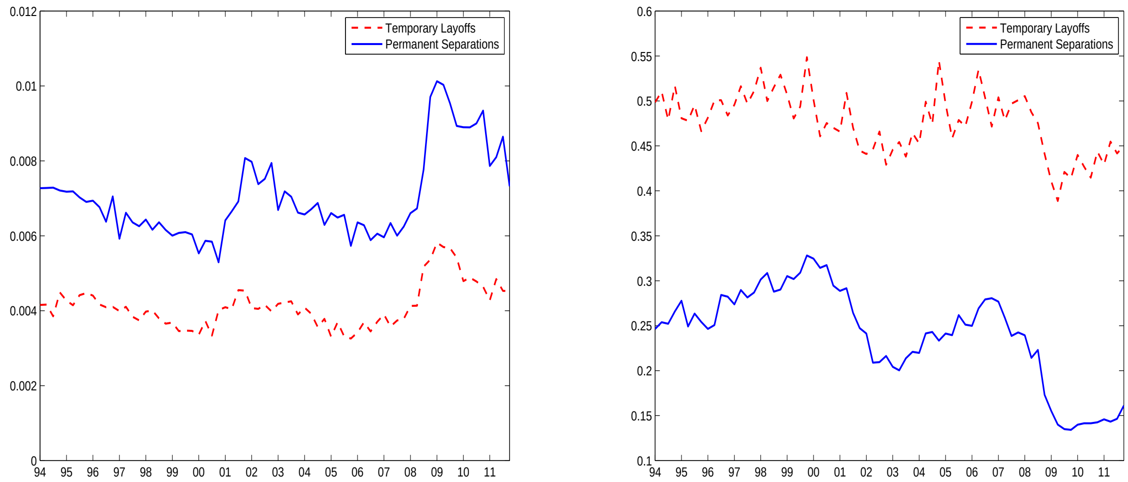

Figure 1: Transition Rates Between Employment and Unemployment by Reason: Matched Records

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

(a) Short-Term Unemployment (b) Exit Probability

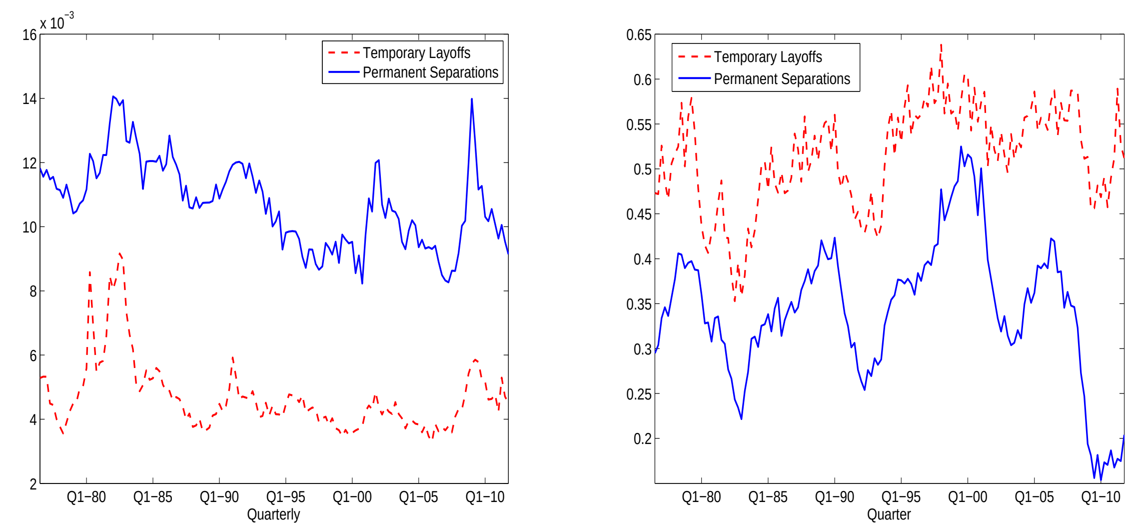

Figure 2: Unemployment Entry and Exit by Reason: Duration Data

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

Note also that both series exhibit the familiar procyclicality, although this is more pronounced for PS.

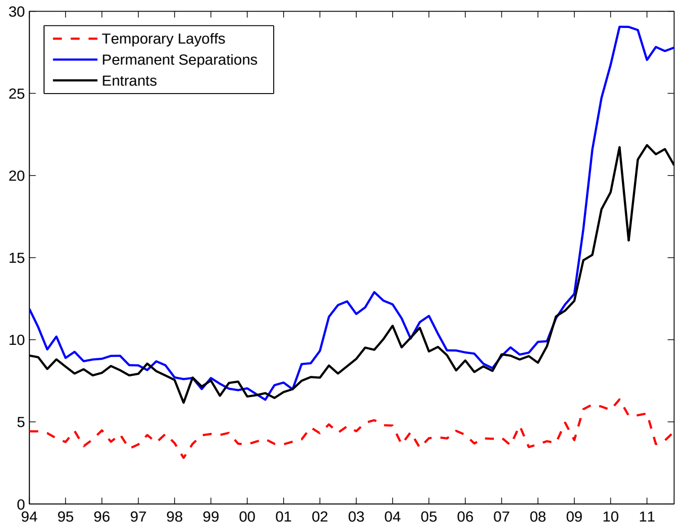

Figure 3: Median Duration by Reason

Notes: Source, Monthly CPS.

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

Lastly, Figure 3 presents median duration, broken down by the reasons. We can confirm here that median duration of those on TL is much shorter. Here the cyclicality of median unemployment duration for TL is less pronounced.

3.1.2 Unemployment Stocks by Reason

It is often argued that the role of TL has diminished since the mid 1980s (e.g., Groshen and Potter (2003)). Figure 4 plots unemployment stocks by reason. Each stock is expressed as a fraction to the labor force and thus the sum of these three lines equals the official unemployment rate. One can see that unemployment due to TL is relatively small in the unemployment stock especially after the mid 1980s.

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

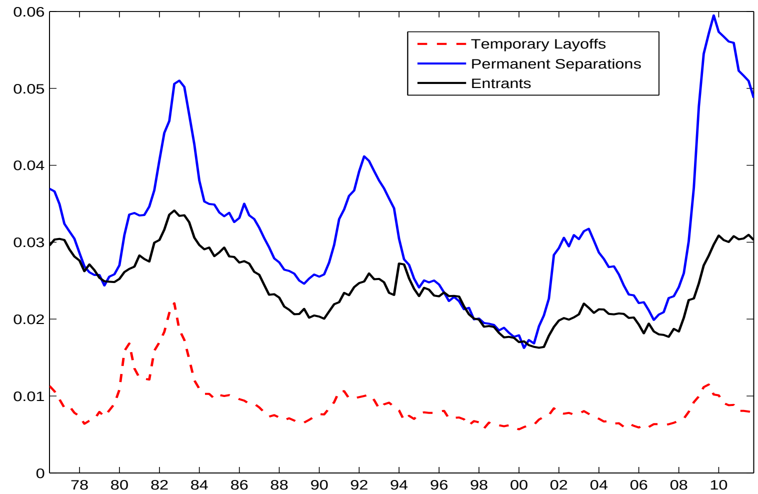

Figure 4: Unemployment Stocks by Reason

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

However, it is important to note that the small share of TL in the unemployment stock does not necessarily mean that TL is equally unimportant in hiring and separation flows. As shown in Panel (a) of Figures 1 and 2, the separation flow associated with TL is roughly one half of that associated with PS. The small share of the stock of TL in the unemployment stock is due to the fact that they quickly exit from the unemployment pool as shown above.

3.1.3 Industry Composition and Seasonality of Temporary Layoff

It is important to note that TL are not concentrated in a particular sector (i.e., manufacturing). Figure 3.1.3 presents the industry breakdown of the TL separation flow, using the short-term unemployment data. While shares of the construction and manufacturing sectors are large as expected, TL are also observed in other sectors as well. Figure 6 summarizes the seasonal pattern of TL. All industries except education/health share the pattern that the TL flow increases in winter months. In addition, some sectors (manufacturing and other services) shed more workers also during summer months. In the education/health sector, TL are concentrated in June. This figure shows that there are significant seasonal variations in the TL flow. However, Figures 1 and 2, which plot seasonally-adjusted data, demonstrate that there are non-seasonal variations in separations and job finding associated with TL.

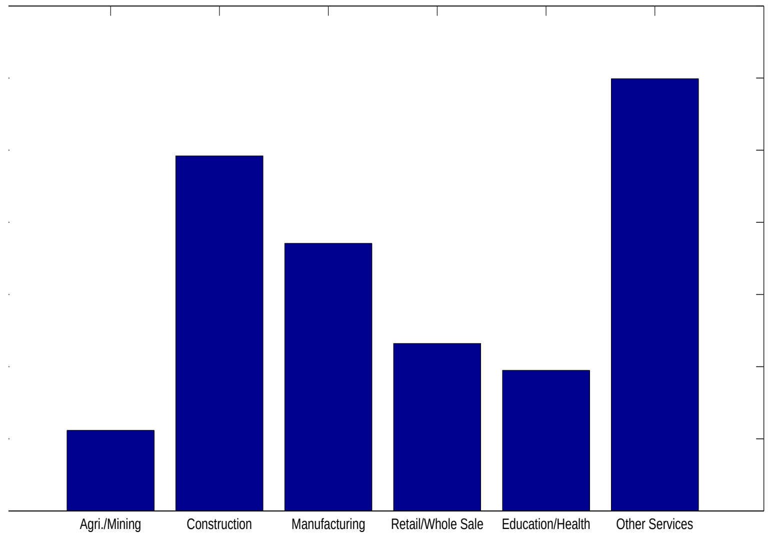

Figure 5: Industry Breakdown of Temporary Layoffs

Notes: Source, monthly CPS. Average shares between January 1989 and December 2011. Other services include Transportation and Utilities; Information; Financial Activities; Professional and Business Services; Leisure and Hospitality; Other Services; Public Administration; Armed Forces.

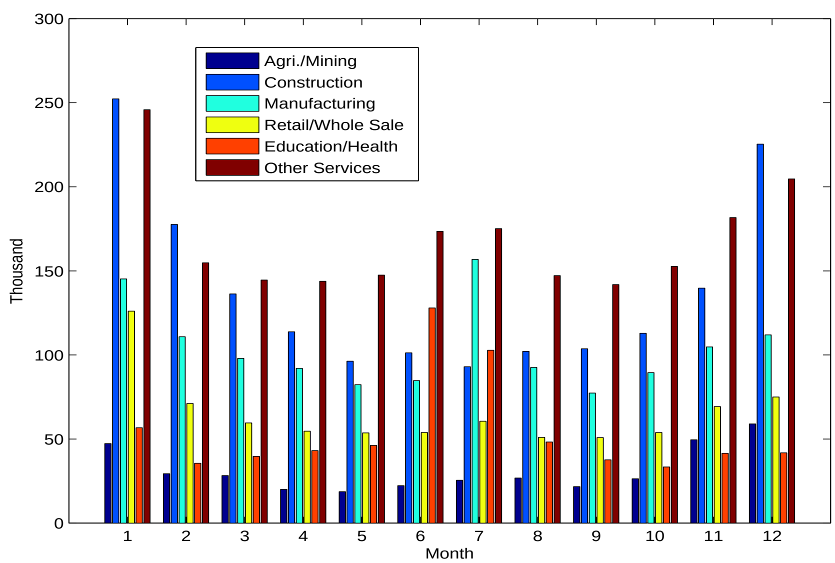

Figure 6: Seasonality of Temporary Layoffs by Industries

Notes: Source, monthly CPS. Average shares between January 1989 and December 2011.

Table 1: Coverage of SIPP Panels

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

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

3.2 Facts from SIPP

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

3.2.1 Sample Selection and Identification of Recalls

SIPP is a collection of panels. The following 8 panels are used in the analysis: 1990, 1991, 1992, 1993, 1996, 2001, 2004, and 2008 panels. The survey was redesigned in 1996, in a manner that introduced significant changes for our purposes. We thus sometimes distinguish between the first four panels and the last four panels. The length of each panel is roughly either three or four years. The first four panels have some overlapping survey periods. Each interview in a panel covers the preceding four-month period, and is called a wave. Table 1 shows each panel's length and the period covered.[2]

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

It is important to make sure that right censoring of the panel does not affect our results.

[2]The 2008 panel is still ongoing and we use the data up to wave 10, which is the most recent release as of October 2012, in the current draft of the paper.

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

We define labor market status (employed, unemployed, and not-in-the labor force) in SIPP in a manner similar to the CPS: we classify unemployed workers into two groups, on TL and PS, as we did in the CPS. Unfortunately, the classification of the labor market status prior to the 1996 SIPP redesign is not consistent with the CPS, and therefore, we focus on the post 1996 data, whenever we condition our analysis on the labor market status. In particular, after the redesign, SIPP applies a more precise definition of TL, raising the number of TL unemployed workers. Prior to the redesign, the share of those who report TL among those who are unemployed throughout the non-employment spell in our $E\not{E}E$ sample was low (roughly 20%). However, the CPS data suggest that the share should be more like 40%.[3]

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

The identification of a recall in the 1996-2008 SIPP panels is subject to two important sources of measurement error, both leading to significantly underestimating recall rates. First, we discover a "seam effect" in SIPP for permanently separated (PS) workers. Consider all PS workers who stay unemployed and regain employment within one or two months, hence experience either a EUE or a EUUE spell. In some cases, the spell is entirely contained

[3]In Figure 1, one can see that TL separation flows are roughly 40% of the total EU flows. In the post-1996 SIPP sample, the TL share for the $EU \cdots UE$ sample is somewhat higher than 40% (as can be seen in Table 5). Note, however, that the $EU \cdots UE$ sample is restricted to those who find a job. Given that PS workers are more likely to drop out of the labor force, including those workers lowers the TL share, making it close the CPS figure.

within a wave (4-month interval between interviews), hence is reported at once in the same interview. For others, the initial and final employment in the spell belong to different waves, and are reported in different (consecutive) interviews. Whether a spell crosses the "seam" or not should be a completely random event. In the SIPP, however, the recall rate of the workers who experience these short within-wave spells is about 20%, as opposed to only 5% for the identical spells that cross the interview seam. Evidently, reporting labor market history all at once in one interview preserves more accurate information. This suggests that recall rates for all PS jobless spells that cross SIPP waves, including necessarily all jobless spells that last more than two months, are underestimated. As we discuss below, we impute these missing recalls due to the seam effect by using the information in the within-wave spells.

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

The imputation is performed separately for the long spells (three months or more) and the short spells (one or two months) that cross the wave seam. For each of the two groups, we use a "reference sample" to estimate a logit regression that predicts recalls given the observable characteristics such as non-employment duration, switching of occupation, etc. We then impute the missing recalls in the 1996-2008 panels. The reference sample of the long jobless spells, who lose job ID and thus cannot be measurably recalled unless they are on TL, is the analogous sample of long-term unemployed in the 1990-1993 panels. Because unemployment status, TL vs PS in particular, is not reliable before 1996, we do not use it in the estimation. Hence, we impute recalls also to post-1996 spells that are on TL and that we know to have accurately measured recalls, to avoid selection by unemployment status, which is obviously non-random and likely correlated with recalls. For the short spells that suffer from the seam effect after 1996, the strongest predictor of recall is occupational mobility. For the occupational stayers, we run the imputation regression on the analogous sample before 1996: all short spells of occupational stayers before 1996 that do not cross the seam. Here we can use unemployment status, TL

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

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

Notes: Source, SIPP. Third column gives the number of recalls relative to all separations into non-employment, denoted by $\not{E}$ (including unemployment and inactivity). Fifth column gives the number of recalls relative to all the spells that end with employment. The results for the 2008 panel are based on the observations up to wave 10, which is the latest release as of June, 2012.

vs PS, as we only exploit post-1996 data where it is measured correctly. If we observe an occupational switch after a short spell that crosses a wave after 1996, we directly impute a zero recall rate. This conservative choice follows from the observation that, among these short spells, over 99% of the occupational switchers before 1996 and 100% after 1996 (who do not cross the seam) are not recalled. Details of the imputation procedure is described in the Appendix.

3.2.2 Recall Rates

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

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

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

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

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

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

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

Notes: Source, SIPP. Third column gives the number of recalls relative to all separations into unemployment, denoted by U. Fifth column gives the number of recalls relative to all the spells that end with employment. The results for the 2008 panel are based on the observations up to wave 10, which is the latest release as of June, 2012.

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

Table 5: Recall Rates by Reasons for Separations into Unemployment

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

Notes: Source, SIPP. The sample for $EU \cdots UE$ in Table 4 is split into two groups based on the reason of unemployment in the first month of the unemployment spell. The results for the 2008 panel are based on the observations up to wave 10, which is the latest release as of June, 2012.

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

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

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

Table 5 splits the EUE sample into two groups by reason for unemployment, TL or PS. As expected, the recall rate for TL workers is very high and much higher than for PS workers. This is true for all panels. However, more importantly, even among PS workers, the recall rate is substantial: nearly 20% of workers who do not have an expectation of recall nevertheless return to the same employer.

3.2.3 Recall and Unemployment Duration

Table 6 summarizes the information about unemployment duration in the EUE sample. We calculate mean duration, standard deviation, and median duration for those who are recalled and those who move to a new employer. First note that recalls occur quicker than new hires. Similarly, the dispersion of unemployment duration is smaller for those recalled. We can also observe a clear countercyclicality of average duration: the average duration increased from 2.50 months in the 1996 panel to 2.65 months in the 2001 panel which corresponds to a recession

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

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

Notes: Source, SIPP. The results for the 2008 panel are based on the observations up to wave 10, which is the latest release as of June, 2012.

year. A more striking increase can be observed for the 2008 panel, as is consistent with the well-known evidence in the monthly CPS. Interestingly, however, the increase in the average duration is especially concentrated among non-recalls. We can see a similar pattern for the standard deviation: dispersion of unemployment duration is countercyclical and the countercyclicality is especially pronounced among non-recall hires. The pattern here therefore highlights the important heterogeneity between recall and new hires that are hidden at the aggregate level.

3.2.4 Hazard Functions

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

There is a clear negative duration dependence in the hazard function for recalls, while the hazard function for exiting unemployment by finding a job at a different employer is weakly hump-shaped, and much closer to be flat. To shed some light on this pattern, Figure 8 further splits this sample of unemployed workers who find work but are not recalled based on the reason for their unemployment, TL or PS. Panel (a) shows that the exit probability to new hires when a worker is on TL exhibits a clear upward sloping pattern. This pattern is consistent with the fact that, in the first few months of unemployment, the worker on TL has a strong expectation of recall and thus the probability of finding a new job is small, but after several months of unemployment, the worker is more likely to find a job elsewhere, as the recall expectation becomes less likely to be met. For PS workers who find new jobs and (the majority) are not recalled, in Panel (b), the hazard rate of exit declines significantly with unemployment duration only between months 2 and 3.

Next, Figure 9 presents the share of recalls at each unemployment duration bin, and shows that at the short duration bins a large fraction of exits from unemployment is due to recalls.

(a) Recall (b) New Job

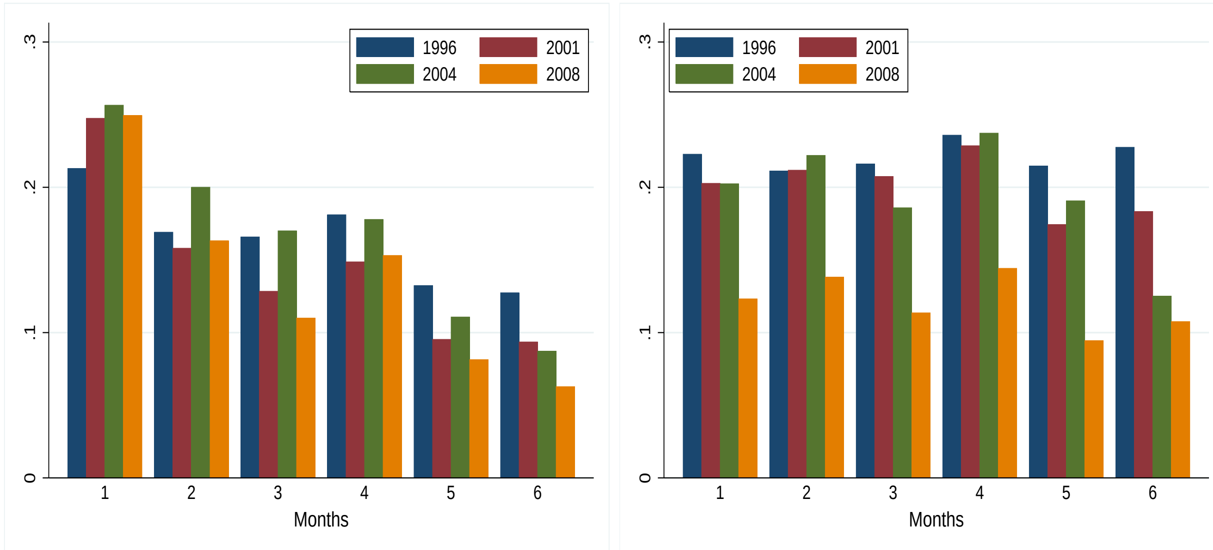

Figure 7: Hazard Functions: 1996-2008 Panels

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

(a) TL (b) PS

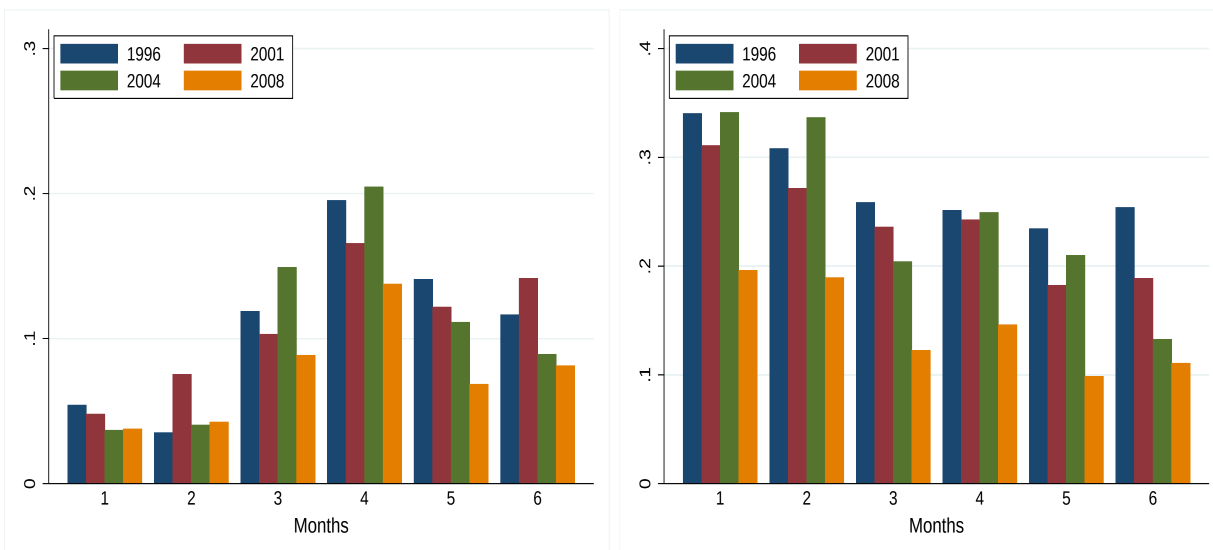

Figure 8: Hazard Functions for New Hires: TL vs. PS, 1996-2008 Panels

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

This result, together with the fact that the hazard function for recall exhibits a clear negative duration dependence, suggests that negative duration dependence of unemployment could be strongly related to recalls. In particular, the heterogeneity between "short-term" and "long-

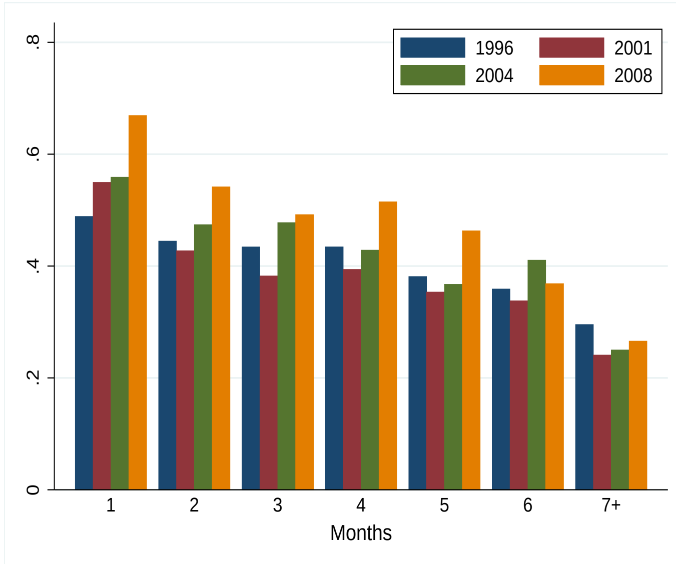

Figure 9: Share of Recalls: 1996-2008 Panels

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

term unemployment types" may be directly related to the chance/expectation of being recalled or not. In turn, this chance depends on worker characteristics, but recall puts some empirical flesh on these unobserved traits. Because we focus only on workers who remain in the labor force, thus eventually (within two years) all find jobs, our conclusions do not apply to the entire unemployment stock, which includes workers who drop out of the labor force and (re-)entrants. Nonetheless, it remains true that the relative hazard rate of recall vs. new jobs for all job losers declines in unemployment duration.

3.2.5 Recall and Employer Tenure

To shed some light on the determinant of recalls, Figure 10 illustrates the relationship between employer tenure before separation and subsequent recall rates. We can see that those who had longer tenure at the time of separation are more likely to be recalled. This pattern makes sense if tenure correlates with match-specific human capital.

3.2.6 Occupation Switches and Wage Changes

Table 7 presents detailed joint probabilities and associated outcomes in terms of the occupation switching rate and wage change, between first and second employment separated by unemployment in the EUE spells. The sample is divided based on (i) temporary layoffs (T) vs. permanent separations (P), (ii) unemployment duration of 3 months or less (S) vs. duration of 4 months

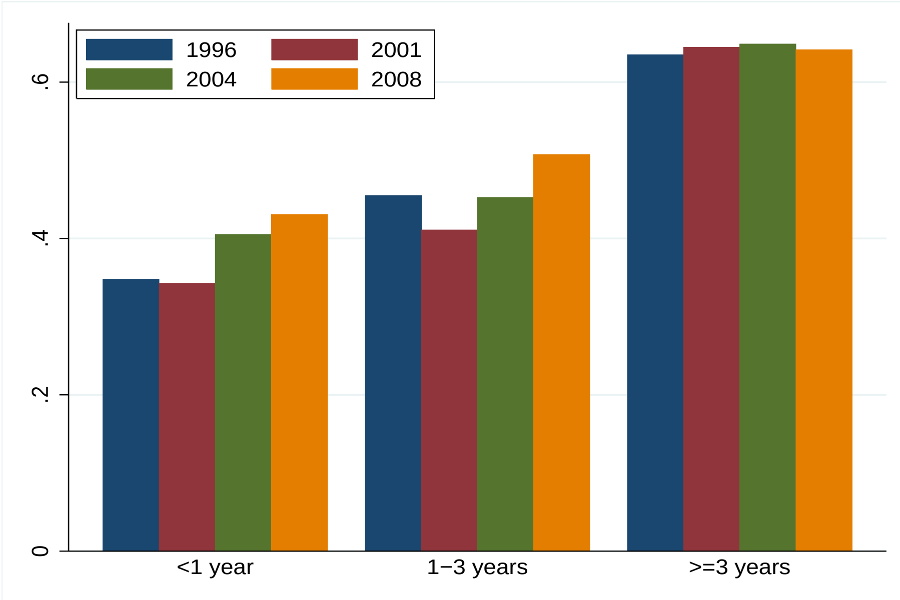

Figure 10: Relationship Between Recall Rates and Firm Tenure
Notes: Source, SIPP. Based on the sample of $EU \cdots UE$ spells, where separations into unemployment occur in the first year or two years of each panel. Legends indicate the panel year.

or longer (L), and (iii) recall (R) vs. new hires (N). Because we are splitting the sample into the 8 detailed groups, we pool observations from 1996-2008 panels.

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

The next two columns report the three-digit occupation switching probabilities for each event. Moving to a new employer after an uninterrupted unemployment spell always results in a very high probability of occupation switch. This finding is consistent with the result in Moscarini and Thompson (2007), who find a high probability of occupation switch after a job-to-job transition in the CPS. The (T,S,R) case results in a very small chance of occupation switch. The other two cases with recall (T,L,R) and (P,S,R) result in slightly higher occupation switching rates. Finally, in the (rare) (P,L,R) cases of a permanently separated worker who is recalled after a long unemployment spell, we observe significant occupational mobility, even after a recall. Because SIPP drops the job id in this (and any) long jobless spell, occupation codes in this case are coded independently, which is known to inflate switching rates.

Finally, the last column reports average log real hourly wage differences before and after an

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

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

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

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

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

average.

3.2.7 Recall and the Business Cycle

We found a very significant difference in unemployment duration (both average and its dependence) between recalled workers and those who either find employment elsewhere or eventually give up job search. It is then natural to ask if recalls account for a significant part of the rise in unemployment duration or in non-participation in recessions, particularly in the Great Recession of 2008-2009 and its aftermath. A leading hypothesis to explain negative unemploy-

Table 8: Average Recall Rates by Year of Separation: 1996-2008 Panels

Panel	Separation Year	EU Recall rates	EU Counts	$EU \cdots UE$ Recall rates	$EU \cdots UE$ Counts
1996	1996	0.428	1,838	0.472	1,665
1996	1997	0.433	1,801	0.473	1,647
1996	1998	0.412	1,655	0.463	1,470
2001	2001	0.412	1,943	0.470	1,704
2001	2002	0.383	1,549	0.458	1,293
2004	2004	0.469	821	0.559	688
2004	2005	0.408	878	0.479	747
2004	2006	0.388	674	0.463	565
2008	2008	0.427	1,173	0.535	936
2008	2009	0.413	3,122	0.545	2,366
2008	2010	0.392	1,104	0.543	797

Notes: Source, SIPP. The first reference month for 2008 panel is April 2008 and thus separations occurred in 2008 cover May through December. Separations in 2010 include those that occur between January and July.

ment duration dependence is selection by unobservables. If some workers leave unemployment faster than others, for reasons intrinsic to their skills, then, as we track a cohort of unemployed who lost their jobs at the same time, we observe a sample that is made of increasingly slow job-finders. Part of this workers' unobserved heterogeneity may be their ex ante (at the time of separation) chance of being recalled, which predicts short unemployment according to our new empirical evidence. If recall rates decline in recessions across the board, the unemployment exit hazard should become lower and even steeper, as we observe in the data.

In principle, actual recall rates may rise or fall in recessions, due to contrasting forces. On the one hand, the general decline in labor demand reduces the number of recalls, just like any other hire. On the other hand, firms might focus more on recalls and less on new hires, hence recalls decline but by less than new hires. Therefore, the recall rate should rise in a recession, especially in the EUE sample of workers who do get eventually hired.

Table 8 reports recall rates for all unemployment spells that begin in the indicated year. The denominator is either the number of all separations into unemployment (EU sample), or its subset that includes all unemployment spells that end with employment (EUE sample). The numerator in each case is the number of cases in the denominator that end with employment at the last employer, a recall. No clear cyclical pattern emerges, in part due to the very low frequency of the observations, forced by the structure of the sampling design in the SIPP. We focus our attention on the Great Recession, which is of obvious interest due to its severity and

Table 9: Composition-Adjusted Recall Rates by Year of Separation: 1996-2008 Panels

Panel	Separation Year	EU Recall Rates	$EU \cdots UE$ Recall Rates
1996	1996	0.433	0.466
1996	1997	0.439	0.473
1996	1998	0.412	0.462
2001	2001	0.413	0.461
2001	2002	0.377	0.443
2004	2004	0.464	0.536
2004	2005	0.392	0.445
2004	2006	0.358	0.415
2008	2008	0.426	0.520
2008	2009	0.402	0.519
2008	2010	0.364	0.490

Notes: Source, SIPP. Based on a logit regression. The first reference month for 2008 panel is April 2008 and thus separations occurred in 2008 cover May through December. Separations in 2010 include those that occur between January and July. For other years, separations occur throughout the year.

duration. Recall rates increased between 2006 and 2008 and stayed high afterwards, especially in the EUE sample. A natural interpretation of these findings is as follows. The sharp decline in hiring was concentrated on new hires, and a significant number of workers who lost hope of being recalled left the labor force. These are workers who, while unemployed, realize that their general human capital and preference for leisure do not justify continuous participation, but would have been willing to work again only for their former employer, to exploit the returns to specific human capital accumulated in their previous employment spell.

Even in this EU sample, however, average recall rates are potentially affected by composition bias. The composition of the EU flow in terms of observable and unobservable worker characteristics that predict eventual recall may change with business cycle conditions. In order to isolate the pure effect of the business cycle on firms' propensity to recall previous employees, we attempt to purge our samples from composition effects. For this purpose, we estimate a logit regression for recall on worker characteristics, and then focus on pure time effects, keeping the sample composition constant. It is important to make clear how this regression differs from the one that we performed to impute recalls missed by SIPP. In that imputation procedure, the dependent variable is actual, observed recall/no recall, and the covariates include ex post outcomes, observed after the end of the jobless spell, such as a change of occupation, which is very indicative of no recall. In the regression to control for the cyclical composition of the

Table 10: Average Recall Rates by Year of Separation (PS workers only): 1996-2008 Panels

Panel	Separation Year	*EU* Recall rates	*EU* Counts	$EU \cdots UE$ Recall rates	$EU \cdots UE$ Counts
1996	1996	0.165	1,055	0.185	939
1996	1997	0.150	1,036	0.168	927
1996	1998	0.143	984	0.166	849
2001	2001	0.152	1,168	0.182	976
2001	2002	0.152	981	0.195	767
2004	2004	0.176	435	0.227	338
2004	2005	0.141	504	0.172	413
2004	2006	0.122	402	0.157	312
2008	2008	0.173	674	0.237	492
2008	2009	0.157	1,836	0.235	1,227
2008	2010	0.140	654	0.223	409

Notes: Source, SIPP. The first reference month for 2008 panel is April 2008 and thus separations occurred in 2008 cover May through December. Separations in 2010 include those that occur between January and July.

EU flow, the dependent variable is recall/no recall, both observed and imputed, and the covariates only include ex-ante variables, known at the time of separation. These are age, age^2, month dummies (to control for seasonality), education dummies (4 categories), gender, dummy for employer provided health care, firm tenure, union dummy, and, crucially, separation year dummies.[5]

We run the regression separately on the two samples, *EU* and *EUE*. To cover as many years as possible, we use the expanded sample that includes separations that occurred in the first two years for the three year panel, or the first three years for the four year panels. Because our goal is to extract business cycle effects, we exclude from the covariates aggregate variables that may contain a business cycle element, such as TL short-term unemployment and PS short-term unemployment. After running the regression, we calculate predicted recall probabilities for all workers (cases) in the sample, only by turning on the dummy for a particular year of separation, and take the average of all predicted probabilities. That is, the sample for each separation year is made of all recall/no recall observations for jobless spells, including those that did not occur in that separation year. The only difference is that a dummy for a particular separation year is turned on or not. So the composition of the *EU* inflow stays the same across all separation years in the above table.

[5] We initially included the initial log wage level but this reduced the number of observations and was not statistically significant anyway, we dropped the variable.

Table 11: Composition-Adjusted Recall Rates by Year of Separation (PS workers only): 1996-2008 Panels

Panel	Separation Year	EU Recall Rates	$EU \cdots UE$ Recall Rates
1996	1996	0.150	0.163
1996	1997	0.149	0.164
1996	1998	0.141	0.164
2001	2001	0.142	0.166
2001	2002	0.153	0.189
2004	2004	0.166	0.201
2004	2005	0.132	0.151
2004	2006	0.120	0.149
2008	2008	0.154	0.203
2008	2009	0.156	0.227
2008	2010	0.147	0.225

Notes: Source, SIPP. Based on a logit regression. The first reference month for 2008 panel is April 2008 and thus separations occurred in 2008 cover May through December. Separations in 2010 include those that occur between January and July. For other years, separations occur throughout the year.

Table 9 presents the recall rates by year of separation, adjusted for sample composition. Compared with the results in Table 8, we see recall rates rise even more in the Great Recession, both for the EU and the EUE samples. The composition of the EU inflow in terms of observable worker characteristics changed towards job losers who are less likely to be recalled. We know that recalls happen relatively quickly, so this composition effect contributed to lengthen average unemployment duration.

Tables 10 and 11 replicate 8 and 9 by focusing only on PS workers, and excluding TL workers whom we know have a high probability of recall, which stays high in recessions. Now recall rates are lower and rise more sharply in the Great Recession. Controlling for sample composition has the opposite effect than on the whole sample.[6]

3.2.8 Discussion

Armed with these findings, we return to our motivating question. Did recall play any role in the exceptional increase in unemployment duration in the recent Great Recession? Since recalls are quick, a decline in the propensity of firms to recall their former employees can lead to a loss

[6]The last month for separation in 2010 is July and the last month for a possible transition into employment is November 2011. So those who separated in 2010 in the table had at least roughly 1.5 years to find a job (whether a recall or new hire).

of attachment and longer joblessness.

Table 9 shows that the fraction of all job losers who initially remain unemployed and are eventually recalled (the "EU recall rate") is roughly acyclical, while the recalled fraction of those same job losers who eventually find a job (the "EUE recall rate") rose sharply during the Great Recession. These statistics, however, do not measure the probability that an individual job losers is eventually recalled.

To understand why, consider Figure 7. Of all EUE spells, the fraction who are recalled after τ months of unemployment declines with duration τ, while the fraction that finds another job remains roughly constant. In the Great Recession, the hazard rate of recall remained roughly the same, or even slightly increased, in the first month of unemployment, and declined at longer durations, so that it became steeper, while the hazard rate of exit from unemployment to new jobs dropped much more dramatically at all durations, by about half, but remained constant over duration. If separated workers are ex ante identical in terms of recall probability, the declining hazard rate of recall is due to pure duration dependence (loss of skill, loss of attachment). In this case, the hazard rates in Figure 7 equal individual transition probabilities. In this interpretation, during the Great Recession, the probability of recall did decline at all durations except in the first month, and not as much as that of finding new jobs. Note that our sample in Figure 7 comprises only EUE spells, a selected subsample of the EU inflow. Because we give workers two years after separation to find another job, and all those who stay unemployed for up to two years eventually regained employment, our selection essentially only excludes from the EU inflow those who eventually left the labor force. Under the assumption that all workers of the same unemployment duration are homogeneous, and hazard rates are driven by pure duration dependence, this makes no difference for our conclusions.

Under this hypothesis of pure duration dependence, the reason why the overall EU recall rate stayed nonetheless constant is that exit from unemployment to other jobs (Figure 7) and to non participation (as observed in CPS data) also declined. So workers spent longer unemployed and had more time to be recalled. But these recalls happened later, as also clear from Table 6. The EUE recall rate equals the EU rate times the EUE/EU ratio, namely the fraction of the cohort of job losers who eventually regain employment. Our evidence (see counts in Table 8) shows that this ratio strongly declined in the Great Recession, mostly because hiring rates by new employers collapsed. Therefore, as the EU recall rate stayed constant, the EUE recall rate rose. We conclude that a pure duration dependence mechanism provides a natural explanation for our facts, and in the Great Recession recall probabilities declined, contributing to longer unemployment duration.

Evidence from the CPS shows that the hazard rate of exit from unemployment to non-participation increases with unemployment duration, and declined at all durations in the Great Recession. In this pure duration-dependence interpretation, the first fact is due to a discouragement effect, such as the limited duration of unemployment benefits, which was in fact extended during the Great Recession, potentially explaining the second fact.

An alternative hypothesis behind Figure 7 is unobserved heterogeneity and selection. Suppose there exist two unobservable (to the econometrician) types of workers, $i = L, H$. Each type i has transition probabilities to the former employer (recall) ρ_i, to other employers f_i, and to non-participation x_i, which do not change with unemployment duration, but only depend on type i. Without loss in generality, we label types so that type H has the higher (but still less than sure) chance of exit from unemployment to other states: $\rho_L + f_L + x_L < \rho_H + f_H + x_H < 1$. In addition, with chance δ_i an unemployed of type i switches type. Let π_0 be the proportion of types H in the EU inflow of size u_0, and π_τ the fraction of the same cohort still unemployed after τ months who are of type H. Then it is easy to derive a recursive equation for the remaining proportion π_τ of type H unemployed workers at duration τ:

$$\pi_{\tau+1} = \frac{\pi_\tau \left[1 - (\rho_H + f_H + x_H)\right](1 - \delta_H) + (1 - \pi_\tau)\,\delta_L}{1 - \pi_\tau\,(\rho_H + f_H + x_H) - (1 - \pi_\tau)\,(\rho_L + f_L + x_L)}.$$

Simple algebra shows that $\delta_L = 0$ is sufficient for this proportion of H types to decline over unemployment duration. If L types cannot become H, and H types leave unemployment faster (to any destination, including non-participation), then necessarily the proportion π_τ of type H left in a cohort of unemployed declines with their duration τ.

What does this imply for transition probabilities? The fraction of the unemployed of duration τ who are recalled in that month equals $\rho_H \pi_\tau + \rho_L (1 - \pi_\tau)$, the fraction who are hired by other firms equals $f_H \pi_\tau + f_L (1 - \pi_\tau)$, while the fraction who leave the labor force equals $x_H \pi_\tau + x_L (1 - \pi_\tau)$. Figure 7 shows that the first of these three fractions declines and the second fraction is constant in unemployment duration τ. These three facts and a declining π_τ imply

$$\rho_H - \rho_L > x_L - x_H > 0 = f_H - f_L.$$

Combined, these inequalities say that both types find new jobs at the same rate, type H are recalled more often by their former employer and leave the labor force less often, but overall leave unemployment faster. These can explain the observed hazard rates through a selection mechanism. Because our sample in Figure 7 comprises only EUE spells, the relative speed of exit from the labor force of the two types, x_L and x_H, will determine the initial proportion of type H in our EUE sample π_0, given their initial proportion in the overall EU inflow. But, whatever π_0 is, the previous argument applies.

Next, we turn to the changes observed during the Great Recession. The EU recall rate, the fraction of the u_0 newly unemployed workers who are eventually recalled

$$u_0^{-1}\sum_{\tau=0}^{\infty}u_\tau\left[\rho_H\pi_\tau+\rho_L\left(1-\pi_\tau\right)\right]$$

in the data remains roughly constant over the business cycle. As average unemployment duration rose in the Great Recession, u_τ/u_0 increased at all durations $\tau\geq1$, so the average probability of recall $\rho_H\pi_\tau+\rho_L\left(1-\pi_\tau\right)$ must have declined at at least many durations. If transition probabilities stay constant, this requires the proportion of type L to be larger, so π_0 to be lower in the Great Recession. Our EUE sample selection cannot play any role in this, because the selection does not change if transition probabilities stay constant. A larger proportion of type L in our sample would imply a lower hazard of recall in the first month of unemployment, and a higher hazard rate of exit from unemployment to non-participation at all durations. In the Great recession, the opposite happened. The hazard rate at one month duration rose slightly, and exit rates to non participation declined. We conclude that a pure composition effect cannot reconcile all the facts. Recall probabilities of either or both types of unemployed had to go down in the Great Recession, even if hazard rates are caused by selection.[7]

In that scenario, it is then possible that the composition of the EU inflow in the Great Recession shifted towards H types, who are more likely to be recalled. This would also explain why the recall hazard rate both stayed constant in the first month of unemployment, as the composition effect offset the reduction in recall probability, and became steeper in duration, due to stronger selection, and also why the hazard rate of exit to non-participation fell at all durations, as type H are less likely to leave the labor force. We conclude that a worker who entered unemployment in the Great Recession faced a lower probability of recall. Given that recalls are fast and entail better wage and occupational turnover outcomes, this fact contributed to exacerbate the costs of the recession.

Why did individual recall probabilities decline in the Great Recession remains an open question. We formulate three (possibly complementary hypotheses). First, and most obvious, the profitability of all jobs, old and new, declined. So, fewer jobs were created or re-activated. Second, more employers closed shop during the Great Recession. When a firm or even an establishment closes, recall is no longer feasible. Business Employment Dynamics flow data

[7] One margin of heterogeneity and dynamic selection is observable, namely, TL vs PS. We showed that these two groups do experience different recall rates. Figure 8, however, shows that the hazard rate of exit of each group, especially TL, to new jobs is not constant over unemployment duration, as would be required by a pure selection story. In addition, Figures 1 and 2 show that the proportions of TL and PS in the EU inflow are roughly acyclical, so there is no strong evidence of changing selection over the business cycle due to a changing composition of the initial cohort of unemployed by TL vs PS separation status.

from the BLS show that the share of gross job destruction and total job losses due to closing establishments actually sharply *declined* during the Great Recession. Most of gross and net job destruction happened at continuing establishments. This fact is still consistent with the hypothesis if most job destruction happened due to a slowdown in hires, but closings were an unusually large fraction of separations (as opposed to job destruction). Third, the dramatic decline in the probability of finding a new job reduced the pressure on employers to recall former employees, before those could find another job. We leave an in-depth empirical investigation of these hypotheses for future research. Our ensuing theoretical analysis sheds light on these mechanisms.

4 A Stochastic Search and Matching Model with Recall

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

4.1 Setup

Time is continuous. All agents are risk neutral and discount payoffs at rate $r > 0$. Firms produce output using a CRS technology, and sell it in a competitive market. The flow output from each match equals $p\varepsilon$. $p > 0$ is an aggregate component, common to all firms, while ε is an idiosyncratic component. Each of the two components p, ε evolves according to a Markov chain: at Poisson rate λ_p a new draw of aggregate productivity p' is taken from $dP\left(p'|p\right)$ and at Poisson rate λ_ε a new match value ε' is drawn from $dG\left(\varepsilon'|\varepsilon\right)$ while the worker is employed. After a separation, the value ε of the potential re-match between the old employer and the worker continues to evolve, according to the same Poisson rate of arrival λ_ε and another conditional distribution $dH\left(\varepsilon'|\varepsilon\right)$. The lowest possible match quality is equal to zero and an absorbing state for the match, so when ε drops to zero the match becomes permanently infeasible, as it will produce nothing for ever. So exogenous separations may be thought of as transitions to $\varepsilon' = 0$. In contrast, the rest of P, G and H are recurrent.

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

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

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

4.2 Equilibrium

We study an equilibrium where all values and strategies are only a function of the aggregate state p and the current or last job's match quality ε. We denote by $U(p, \varepsilon)$ the value of unemployment, where $p\varepsilon$ is the productivity of the last employer, if any (otherwise $\varepsilon = p\varepsilon = 0$), $W(p, \varepsilon)$ the value of employment to the worker, $V(p, \varepsilon)$ the value of a vacant job, where $p\varepsilon$ is the productivity of the last employee, if any (otherwise $\varepsilon = p\varepsilon = 0$), $J(p, \varepsilon)$ the value of a filled job, $w(p, \varepsilon)$ the wage. Nash bargaining implies

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

Private efficiency implies that recall by mutual consent occurs whenever either party gains from it.

The key to understand equilibrium is to describe what happens when an unemployed worker, searching for a new job, receives an outside offer. The capital gain from job search, conditional on searching and on contacting an open vacancy, is

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

where $\mathbb{I}$ is the indicator function. The new offer at match quality ε' is acceptable only if it yields the worker, thus the new firm, both a positive surplus over forming and immediately breaking up the match and a larger continuation value than waiting for a recall of the old match, which has current quality ε.

The continuation value $W\left(p,\varepsilon'\right)$ after accepting the new offer is calculated from the same function pinned down by Nash Bargaining with the new employer, with threat point given by unemployment after the new match, as if the current recall option was worthless. The reason is that no competition for the worker takes place between the old and new employer due to the lack of commitment power. The worker can only extract a share β of the surplus from each match in isolation. Therefore, the continuation value after accepting a new offer does not depend on the old recall option ε. In turn, this implies that the returns from hiring an unemployed worker do not depend on the value of his recall option. This "memoriless" property is the key to the simplicity of the equilibrium under consideration, because a job contemplating posting a vacancy does not need to keep track of the distribution of old match qualities among jobless workers. If the bargaining environment did allow the worker to carry part of his recall option value over to the new match, the profits from hiring new workers would depend on the recall prospects of the job-searching unemployed, and firms would have to track their cross-section distribution, which is an infinitely-dimensional object, changing stochastically with the aggregate state. The value of the recall option thus affects the worker's net returns from, and incentives to engage in, job search.

We now guess and later verify that the functions W, U and $W - U$ are increasing in ε. Thus consider ε and $\varepsilon' > \varepsilon$ such that

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

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

$$\mathbb{I}\left\langle W\left(p,\varepsilon'\right) \geq U\left(p,\varepsilon'\right)\right\rangle = 1 \Rightarrow \max\left\langle W\left(p,\varepsilon'\right) - U\left(p,\varepsilon\right),0\right\rangle = W\left(p,\varepsilon'\right) - U\left(p,\varepsilon\right) \qquad (2)$$

and we can eliminate the max from the continuation value of search, which then reads simply

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

4.2.1 Bellman Equations: Firm

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

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

The value of a vacant job solves

$$\begin{aligned} rV\left(p,\varepsilon\right) &= \lambda_p \int \left[\max\left\langle J\left(p',\varepsilon\right), V\left(p',\varepsilon\right)\right\rangle - V\left(p,\varepsilon\right)\right] dP\left(p'|p\right) \\ &\quad + \lambda_\varepsilon \int \left[\max\left\langle J\left(p,\varepsilon'\right), V\left(p,\varepsilon'\right)\right\rangle - V\left(p,\varepsilon\right)\right] dG\left(\varepsilon'|\varepsilon\right) \\ &\quad + \mathbb{I}\left\{\phi\left(\theta\left(p\right)\right) \int \mathbb{I}\left\{W\left(p,\varepsilon'\right) \geq U\left(p,\varepsilon'\right)\right\}\left[W\left(p,\varepsilon'\right) - U\left(p,\varepsilon\right)\right] dF\left(\varepsilon'\right) - c_U \geq 0\right\} \cdot \\ &\quad \cdot \phi\left(\theta\left(p\right)\right)\left[\int \mathbb{I}\left\{W\left(p,\varepsilon'\right) \geq U\left(p,\varepsilon'\right)\right\}\mathbb{I}\left\{W\left(p,\varepsilon'\right) \geq U\left(p,\varepsilon\right)\right\} dF\left(\varepsilon'\right)\right]\left[V\left(p,0\right) - V\left(p,\varepsilon\right)\right] \\ &\quad + \max\left\langle 0, -c_V + q\left(\theta\left(p\right)\right) \int \mathbb{I}\left\{J\left(p,\varepsilon'\right) \geq V\left(p,\varepsilon'\right)\right\}\left[J\left(p,\varepsilon'\right) - V\left(p,\varepsilon\right)\right] dF\left(\varepsilon'\right)\right\rangle \end{aligned} \tag{4}$$

where $\mathbb{I}$ is the indicator function, and in the fourth lines we used (2). The job can recall the former employee after any shock, but also lose the recall option if the former employee successfully locates a new acceptable offer. This occurs if the expected capital gain from job search is positive (third line), a contact occurs (at rate ϕ), and the new match is acceptable, which has chance equal to the integral in the fourth line. The firm that owns this job can also pay the vacancy cost to meet a new worker, and matches if the new match draw ε' guarantees a positive surplus, and a higher value to the firm than the continuation. This term, on the last line, does not contain a max operator, for the same reasons that we illustrated in the case of the unemployed worker.

4.2.2 Free entry

By free entry, firms post new vacancies, which start from $\varepsilon = 0$, until their net value is zero: for all p, $V\left(p,0\right) = 0$. When $\varepsilon = 0$, an absorbing state, the match will never be productive again and the vacancy is worthless. Since $\varepsilon = 0$ is an absorbing state, $J(p,0) = V(p,0) = 0$ and $J(p,\varepsilon') = V(p,\varepsilon') = V(p,0)$ for all $\varepsilon' \sim dG(\varepsilon'|0)$. Using these facts in (4) we obtain a familiar-looking free-entry condition:

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

and

$$\begin{aligned} rV(p,\varepsilon) &= \lambda_p \int [\max\langle J(p',\varepsilon), V(p',\varepsilon)\rangle - V(p,\varepsilon)] dP(p'|p) \\ &+\lambda_\varepsilon \int [\max\langle J(p,\varepsilon'), V(p,\varepsilon')\rangle - V(p,\varepsilon)] dG(\varepsilon'|\varepsilon) \\ &-V(p,\varepsilon)\mathbb{I}\left\{\phi(\theta(p)) \int \mathbb{I}\{W(p,\varepsilon') \geq U(p,\varepsilon')\}[W(p,\varepsilon') - U(p,\varepsilon)] dF(\varepsilon') - c_U \geq 0\right\} \cdot \\ &\cdot\phi(\theta(p)) \left[\int \mathbb{I}\{W(p,\varepsilon') \geq U(p,\varepsilon')\} dF(\varepsilon')\right] \end{aligned} \tag{6}$$

where the last term is the loss when the former worker finds another job. In this term, using (2) we ignore the restriction $W(p,\varepsilon') \geq U(p,\varepsilon)$ because an acceptable new job $W(p,\varepsilon') \geq U(p,\varepsilon')$ automatically satisfies it when the worker is searching.

Conversely, for $\varepsilon > 0$ we have $V(p,\varepsilon) > 0$ and, with positive probability, $J(p,\varepsilon') > V(p,\varepsilon') > 0$, because mothballing the vacancy has neither explicit nor opportunity costs. A vacant job that still retains a positive match quality with a former employee has a positive chance of recalling him in the future, because match quality can rise to any higher level with positive probability in finite time. Since recall is free, the value of this vacant job is positive even when just waiting and not searching. Thus, this job will not post a vacancy, but wait. Put more simply, by constant returns to scale in production, no firm has an incentive to fill a job that could still be subject to recall with a new employee, but rather creates another job to look for the new worker. In contrast, a worker can only work for one firm, thus an unemployed worker's former employer can be replaced by a competitor who hires him.

4.2.3 Bellman Equations: Worker

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

$$\begin{aligned} rW(p,\varepsilon) &= w(p,\varepsilon) + \lambda_p \int [\max\langle W(p',\varepsilon), U(p',\varepsilon)\rangle - W(p,\varepsilon)] dP(p'|p) \\ &+\lambda_\varepsilon \int [\max\langle W(p,\varepsilon'), U(p,\varepsilon')\rangle - W(p,\varepsilon)] dG(\varepsilon'|\varepsilon). \end{aligned} \tag{7}$$

After each shock, the worker may decide to quit.

The HJB equation of the unemployed worker is

$$\begin{aligned} rU(p,\varepsilon) &= b + \lambda_p \int \left[\max\left\langle W(p',\varepsilon), U(p',\varepsilon)\right\rangle - U(p,\varepsilon)\right] dP(p'|p) \\ &\quad + \lambda_\varepsilon \int \left[\max\left\langle W(p,\varepsilon'), U(p,\varepsilon')\right\rangle - U(p,\varepsilon)\right] dH(\varepsilon'|\varepsilon) \\ &\quad + \max\left\langle 0, -c_U + \phi(\theta(p)) \int \mathbb{I}\left\{W(p,\varepsilon') \geq U(p,\varepsilon')\right\} \left[W(p,\varepsilon') - U(p,\varepsilon)\right] dF(\varepsilon') \right\rangle \end{aligned}$$

After each shock, the worker may decide to reactivate the old job; in addition, he can decide to search for a new job, that he accepts if it offers a positive surplus (to the worker, hence to the firm). Using (2) and rearranging:

$$\begin{aligned} (r + \lambda_p + \lambda_\varepsilon) U(p,\varepsilon) &= b + \lambda_p \int \max\left\langle W(p',\varepsilon), U(p',\varepsilon)\right\rangle dP(p'|p) \\ &\quad + \lambda_\varepsilon \int \max\left\langle W(p,\varepsilon'), U(p,\varepsilon')\right\rangle dH(\varepsilon'|\varepsilon) \\ &\quad + \max\left\langle 0, -c_U + \phi(\theta(p)) \int \mathbb{I}\left\{W(p,\varepsilon') \geq U(p,\varepsilon')\right\} W(p,\varepsilon') dF(\varepsilon') \right. \\ &\quad \left. - \phi(\theta(p)) U(p,\varepsilon) \int \mathbb{I}\left\{W(p,\varepsilon') \geq U(p,\varepsilon')\right\} dF(\varepsilon') \right\rangle \end{aligned} \tag{8}$$

4.2.4 Wages

Imposing the Nash Bargaining solution (1) and after much algebra we obtain an expression for the wage:

$$\begin{aligned} w(p,\varepsilon) &= \beta p\varepsilon + (1-\beta) b + (1-\beta) \max\left\langle S(p,\varepsilon), 0\right\rangle + \beta \mathbb{I}\left\{S(p,\varepsilon) \geq 0\right\} A(p) V(p,\varepsilon) \\ &\quad + \lambda_\varepsilon \int \left[\beta V(p,\varepsilon') - (1-\beta) U(p,\varepsilon')\right] \left[dG(\varepsilon'|\varepsilon) - dH(\varepsilon'|\varepsilon)\right] \end{aligned} \tag{9}$$

where we define the expected gain from job search,

$$S(p,\varepsilon) = \phi(\theta(p)) \int \left[W(p,\varepsilon') - U(p,\varepsilon)\right] \mathbb{I}\left\{W(p,\varepsilon') \geq U(p,\varepsilon')\right\} dF(\varepsilon') - c_U$$

and the probability that the unemployed worker finds and accepts a new offer when searching:

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

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

Finally, the wage is affected by the change in match quality evolution after separation, as captured by the difference between the transition c.d.f.s G (on the job) and H (on the job). Suppose, for example, that G first-order stochastically dominates H because halting production entails some skill loss. Then the last term in the wage function is positive if $\beta V(p, \varepsilon') - (1-\beta) U(p, \varepsilon')$ is increasing in ε'. That is, if the value of unemployment is less sensitive to match quality than the value of the vacancy, after weighting for bargaining shares, the worker will suffer less than the firm from accelerated match quality depreciation after separation. This gives the worker additional bargaining power, and raises the wage.

4.3 Computation

Equilibrium is described by a set of functional equations in the unknown functions W, U, J, V, w, θ. To compute equilibrium we proceed as follows. We discretize the state space for p and ε, taking N_p and N_ε values, respectively. Hence, each value function and the wage function are positive $N_p \times N_\varepsilon$ arrays, while job market tightness θ is a column vector of length N_p. Given a guess $W^{(n)}, U^{(n)}, J^{(n)}, V^{(n)}, w^{(n)}, \theta^{(n)}$ for these arrays, we first set the first column of $V^{(n)}$ to all zeros per free entry, then update the wage to $w^{(n+1)}$ using (9), then firm profits $J^{(n+1)}$ using (3), job market tightness $\theta^{(n+1)}$ using (5), the value of a vacant job $V^{(n+1)}$ using (6), the value of employment $W^{(n+1)}$ using (7), the value of unemployment $U^{(n+1)}$ using (8). We initialize the algorithm with random positive matrices at $n = 0$, and iterate until the discrepancy between the entries of all these matrices at steps n and $n + 1$, all in absolute value and added together, falls below a tolerance level. Upon convergence, we verify the property that W, U and $W - U$ are increasing in ε for all p.

4.4 Numerical Examples (Under Construction)

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A Imputation Procedure

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

A.1 Long Spells

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

- Age, age^2.
- Education categories: less than high school, high school graduate, some college, and college degree.
- Gender dummy, union dummy at initial employment, and employer-provided health care (EPHC) dummy at initial employment
- Address change dummy, union status change dummy, EPHC change dummy.
- Non-employment duration categories: 3−6 months, 7−9 months, 10−12 months, 13 months or longer. We find that using non-employment duration as a categorical variable (instead of a continuous variable) helps improve the fit of the imputation regression.
- Occupation switch and industry switch dummies. Both switches are based on the three-digit level classification. Interaction of the two switching dummies are also included.
- Initial occupation and industry dummies. Occupation is classified into 79 categories and industry is classified into 44 categories.
- Log wage level at initial employment.
- Log wage change between initial and last employment. The change is captured as a categorical variable based on the following intervals: $(\infty, -0.5]$, $(-0.5, -0.05]$, $(-0.05, 0.03]$, $(0.03, 0.5]$, $(0.5, \infty]$. We find that categorizing log wage changes into bins (instead of using the log wage change itself) improves the fit of the imputation regression. The basic idea is that a large wage change (whether positive or negative) strongly predicts non-recall. However, we also find that negative and positive wage changes predict slightly different

probabilities of recall/non-recall and thus positive and negative changes are treated separately. The middle category is centered around a negative value because the average wage change of all observation is negative.

- National unemployment rate: This to control for the aggregate labor market condition.
- Month-of-separation dummies. This is to control for seasonality.

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

A.2 Short Spells

Within the short spells (with one or two months of non-employment duration) in 1996-2008 panels, the spells that occur within a wave are reliable. Further, when labor market status is reported to be TL, we trust the recall/no recall indicator. In the remaining sample, the spells that occur across a wave, we assume that those with an occupation switch are non-recall while those that report the same occupation are imputed by running a logit regression. The reference sample for this regression is within-wave spells in the 1996-2008 panels. The regression uses basically the same variables as above with a few differences. First, we do not use occupation and industry switch dummies (the sample is only for occupation stayers). Second, initial occupation and industry dummies (a total of 123 dummies) are dropped to maintain the efficiency of the estimation, given that this sample has a fewer observations. Third, we also use a labor market status variable.[8] Lastly, we also add panel dummies. We add this variable because the short spells are imputed within the 1996-2008 panels. The Pseudo R^2 of the regression is 0.3707.

A.3 Multiple Imputation

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

B Pre-Imputation Data

Table 2 in the main text presented the recall rates for separations occurred in the first year or two years of each panel. Table 12 presents the corresponding recall rates based on the

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

Table 12: Recall Rates: Separations Occurred in the First Year or Two Years of Each Panel (Pre-Imputation)

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

Notes: Source, SIPP. Third column gives the number of recalls relative to all separations into non-employment, denoted by $\not{E}$ (including unemployment and inactivity). Fifth column gives the number of recalls relative to all the spells that end with employment. The results for the 2008 panel are based on the observations up to wave 10, which is the latest release as of June, 2012.

pre-imputation data (raw data). One can see sudden drops the recall rates at the 1996 panel.