Recall and Unemployment

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Supplementary Appendix

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A Supplementary evidence from the SIPP

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

Table A.1: Coverage of SIPP Panels

Panel	Number of Waves	Number of Months Covered	First Reference Month
1990	8	32	Oct. 1989
1991	8	32	Oct. 1990
1992	9	36	Oct. 1991
1993	9	36	Oct. 1992
1996	12	48	Dec. 1995
2001	9	36	Oct. 2000
2004	12	48	Oct. 2003
2008	16	64	May 2008

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

A.1 Additional facts about recall

Table A.2 presents recall rates by demographic groups for $E\cancel{E}E$ spells in the SIPP 1990-1993 panels.

Table A.2: Recall rates by observable characteristics: $E\cancel{E}E$ Spells, 1990-1993 panels

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

Source: SIPP, 1990-1993 panels. Share of recalls in $E\cancel{E}E$ spells where separations occur in the first three waves (12 months) of each panel; "Other Services" category includes all other industries.

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

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

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

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

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

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

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

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

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

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

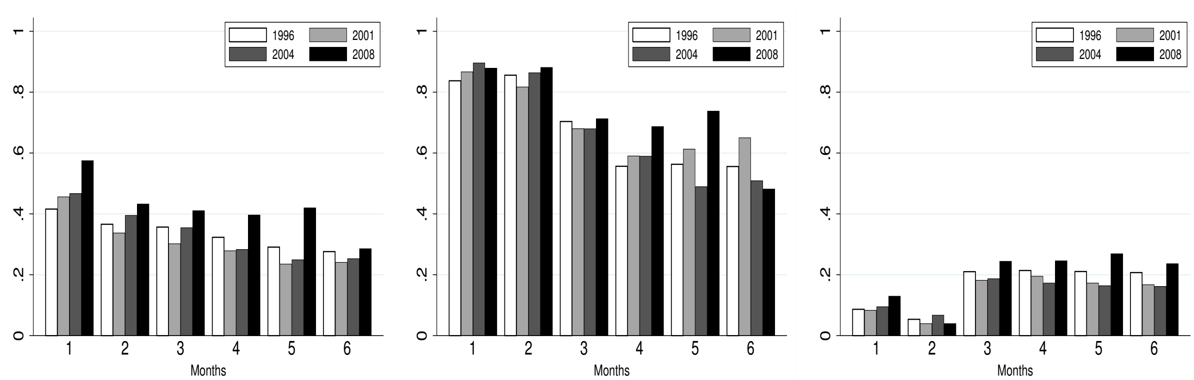
Panel	$E\not{E}\cdots\not{E}E$		$EEE\not{E}\cdots\not{E}EEE$	
	Count	Recall Rate	Count	Recall Rate
1990	3,325	0.371	1,506	0.398
1991	2,310	0.423	1,072	0.445
1992	2,827	0.407	1,365	0.457
1993	2,587	0.398	1,296	0.456

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

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

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

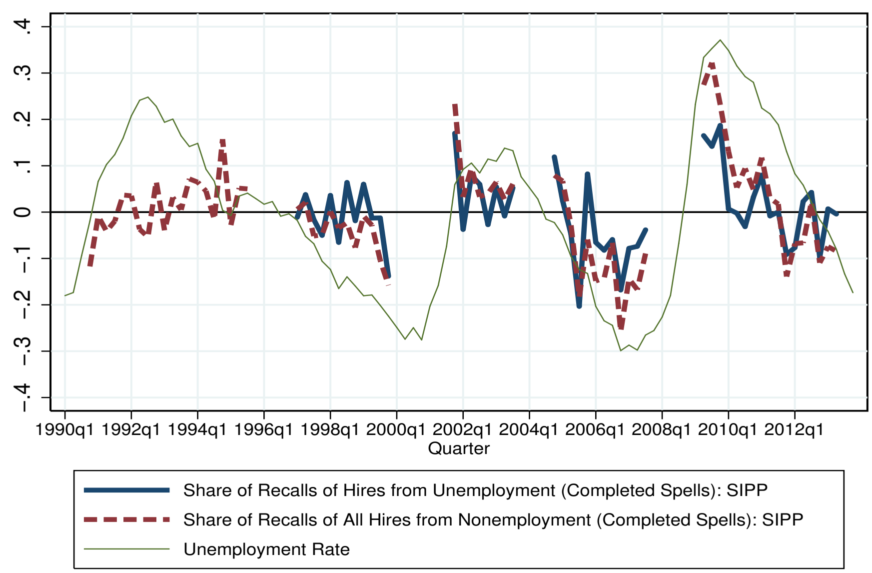
(a) All (b) TL (c) PS



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

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

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



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

A.2 Measurement of recall

A.2.1 Misclassification of unemployment before the 1996 panel

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

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

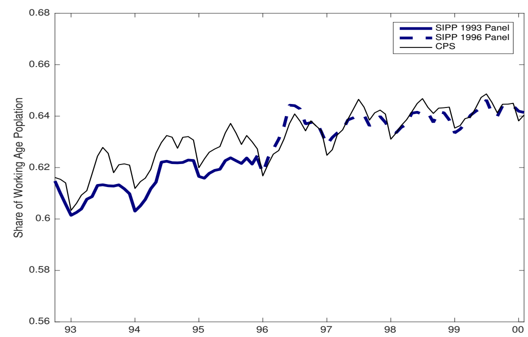
Periods covered by SIPP Panels SIPP Panels	SIPP TL/(TL+PS)	CPS TL/(TL+PS)
1996	0.34	0.38
2001	0.32	0.36
2004	0.35	0.37
2008	0.34	0.36

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

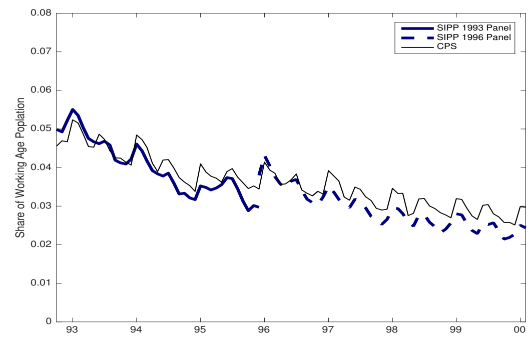
Figure A.3 illustrates the inconsistent definitions of labor market status, especially TL and OLF, in the SIPP before and after the 1996 panel redesign. The CPS redesign took place in 1994, but there is no discernible break in the TL and OLF series.

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

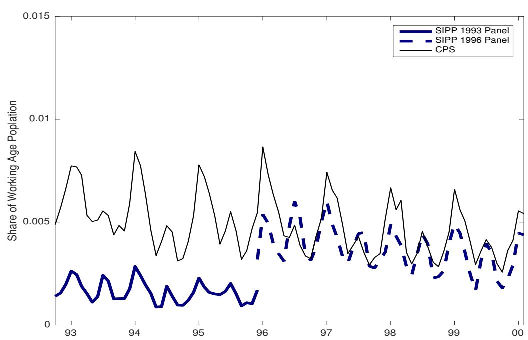
(a) Employment



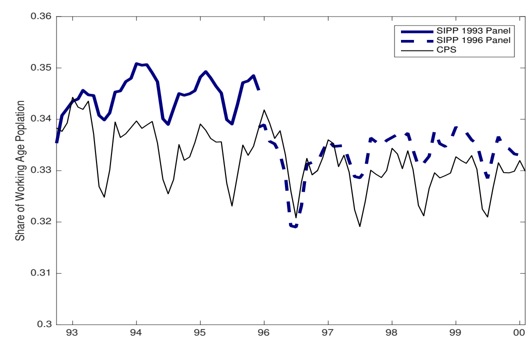
(b) Unemployment



(c) Temporary Layoffs



(d) Out-of-the-Labor Force



Sources: SIPP 1993 and 1996 panels; monthly CPS.

A.2.2 Attrition

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

Table A.7: Attrition rates by SIPP panel

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

Source: SIPP.

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

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

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

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

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

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

Source: SIPP, 1996-2008 panels. Estimates from Probit regression of attrition. Standard errors in parentheses. Both specifications include full dummies for gender, race, age, marital status, and education. Total number of observations: 2,494,536.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

A.3 Imputation of recall in post-1996 panels

A.3.1 Methodology

To impute recalls for the long spells ($E\not{E}E$ spells with non-employment duration of three months or longer) in the post-1996 panels, we use the corresponding data in the 1990-1993 panels as a reference sample. We run the logit regression on this reference sample to predict recalls in the post-1996 data. The following variables are included in the regression: Quadratic polynomials in age; Education categories: less than high school, high school graduate, some college, and college degree or higher; Gender dummy; Union membership dummy at initial employment; Employer-provided health care (EPHC) dummy at initial employment; Address change dummy; Union status change dummy; EPHC change dummy; Non-employment duration categories: 3−6 months, 7−9 months, 10− 12 months, 13 months or longer; Occupation switch and industry switch dummies at the three-digit level classification, and interactions of the two switching dummies; Initial occupation and industry dummies (79 occupational categories and 44 industry categories); log wage change between initial and last employment, captured as a categorical variable based on the following intervals: $(\infty, -0.5]$, $(-0.5, -0.05]$, $(-0.05, 0.03]$, $(0.03, 0.5]$, $(0.5, \infty]$; National unemployment rate to control for aggregate labor market conditions; And month-of-separation dummies to control for seasonality. We find that using non-employment duration and log wage changes as categorical variables, instead of continuous variables, helps to improve the fit of the imputation regression. We also find that negative and positive wage changes predict slightly different probabilities of recall/non-recall and thus treat introduce positive and negative changes. The middle category is centered around a negative value because the average wage change of all observation is negative.

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

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

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

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

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

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

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

For short spells ($E\not{E}E$ spells with non-employment duration of 1 or two months) in the post 1996 panels, we impute recall if the spell satisfies three requirements: (i) it does not begin as TL; (ii) it crosses a seam; (iii) it does not lead to an occupational switch. Again, we run a logit regression. The reference sample is made of the within-wave spells in the 1996-2008 panels. The regression uses basically the same variables as above with a few differences. First, we do not use occupation and industry switch dummies (the sample is only for occupation stayers). Second, initial occupation and industry dummies (a total of 123 dummies) are dropped to maintain the efficiency of the estimation, given that this sample has much fewer observations. Third, we also use a labor market status variable, TL vs. PS, which was not feasible for long spells as discussed earlier. Lastly, we also add panel dummies, because the short spells are imputed within the 1996-2008 panels. Table A.13 reports the estimated marginal effects of covariates on the probability of recall after a short jobless spell. Table A.14 describes the effects of the imputation on the recall rate of short jobless spells.

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

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

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

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

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

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

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

A.3.2 Diagnostics

To assess the quality of our imputation, we perform an "in-sample forecast." We split randomly our reference samples that were deemed accurate into two equal subsamples, A and B. We then reset all recall information in subsample B to "missing." We merge the subsamples again and repeat our imputation procedure. We then compare imputed recall outcomes for subsample B to the true recall observations that we had discarded. The imputation round, we remind the reader, is intentionally noisy, because the logit model generates a probability in $(0, 1)$, while we need to impute a binary outcome in $\{0, 1\}$. We find that the imputation introduces Type I and Type II errors relative to true data each equal to roughly 15%. Thus the imputation recovers the truth 70% of the time and but the share of recalls is imputed almost exactly without introducing any systematic bias.

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

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

Source: SIPP. MAE: Mean Absolute Errors.

A.3.3 Temporal Correlation of Recall

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

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

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

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

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

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

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

B Supplementary evidence from other datasets

B.1 Monthly CPS

B.1.1 Transition probabilities and unemployment duration

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

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

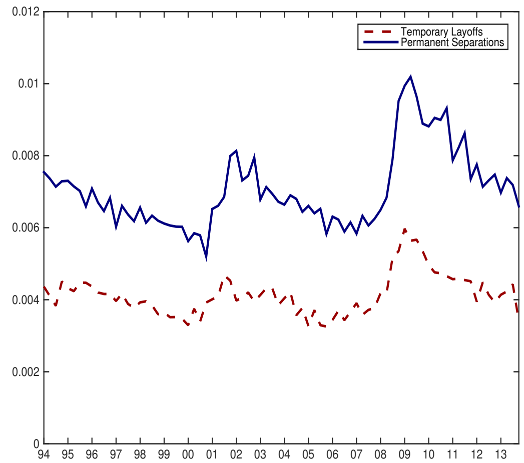
Rotation Group	1st	2nd	3rd	5th	6th	7th
$\frac{TL}{TL+PS}$	0.37	0.36	0.37	0.36	0.36	0.37

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

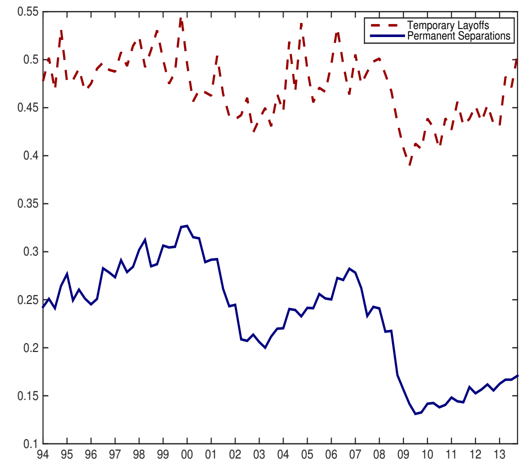
Figure B.1 plots monthly EU transition probabilities (averaged over quarterly periods) derived from the matched records. Panel (a) breaks down EU transitions into TL and PS, by dividing the EU flow for each reason by the total employment stock. This figure thus tells the relative size of the two inflows. The TL inflow amounts to roughly one half of the PS inflow, and the two move more or less in parallel over business cycles. Panel (b) presents unemployment-to-employment transition (UE) probabilities by reason. Figure B.1 and Figure 4 in the text give similar results in terms of relative size of TL and PS flows and their cyclicality. Figure B.2 confirms that median duration of those on TL is much shorter on average and less cyclical.

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

(a) EU Transition Probability

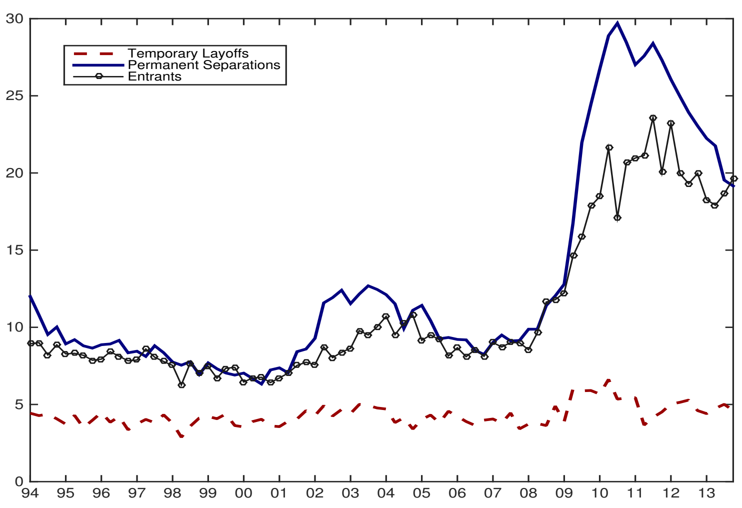


(b) UE Transition Probability



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

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



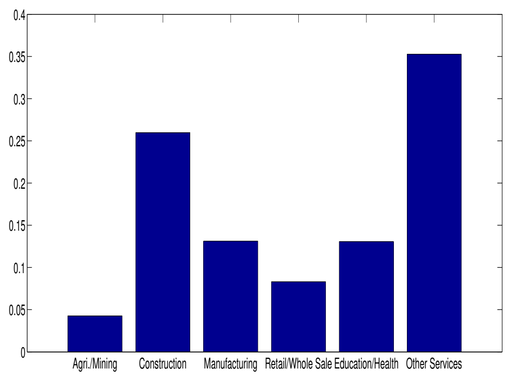
Source: Monthly CPS. Quarterly averages of monthly data.

B.1.2 Industry composition and seasonality of Temporary Layoffs

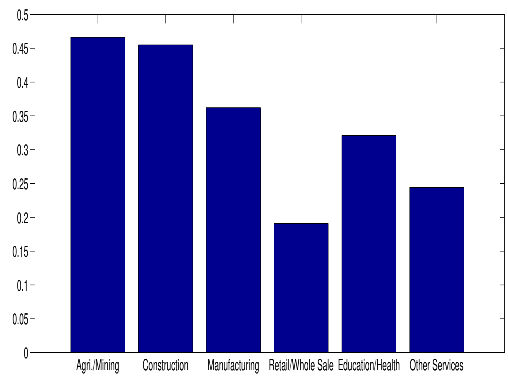
Panel (a) of Figure B.3 presents the industry breakdown of the aggregate TL separation flow into unemployment. While the contributions of the construction and manufacturing sectors are, as expected, large, TL are not at all unusual in other sectors. To take into account the relative size of each industry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

Figure B.3: Industry breakdown of Temporary Layoffs

(a) Industry Shares of TL Separation Flow



(b) Share of TL Separation Flow in Each Industry



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

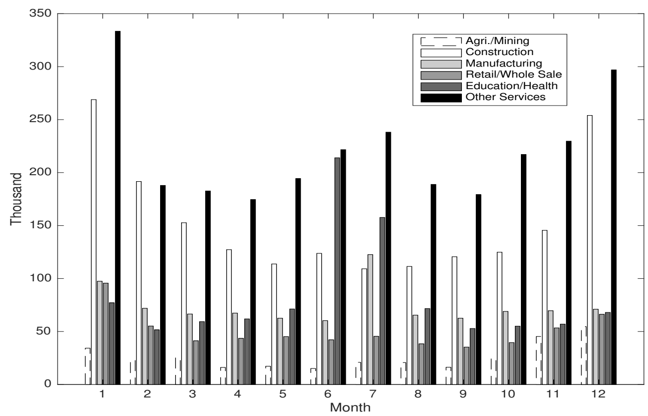
Figure B.4 summarizes the seasonal pattern of TL. All industries, except education/health, share the feature that the TL flow increases in winter months. In addition, some sectors (manufacturing and other services) shed more workers temporarily also during summer months. In the education/health sector,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

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

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

business cycle variations in separation and job-finding probabilities associated with TL. Similarly, in our main SIPP-based analysis, we find that the share of hires from unemployment that are recalls, whether from TL or not, exhibits a countercyclical pattern. Therefore, TL and recalls are not simply a seasonal phenomenon. Furthermore, even their seasonal component does affect the average level of turnover in and out of unemployment. Since TL (thus, presumably, also recalls) are not synchronized between industries, but rather staggered within the year, part of this industry-specific seasonality cancels out when aggregating all industries to generate economy-wide job-finding and separation flows.

Figure B.4: Seasonality of Temporary Layoffs by industry



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

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

To make things simple, round up spell durations to months. QWI misses recalls after: (i) all jobless spells that last one or two months; (ii) two thirds of all 3-month jobless spells, i.e., those that do not exactly "fill" one calendar quarter; and (iii) one fourth of the four-month jobless spells, i.e., those that are divided equally by a seam between quarters. Every jobless spell of duration 5 months and up necessarily implies a full calendar quarter of zero earnings, and correspondingly is detected in QWI. The QWI's recall rate from joblessness is a share of all hires, $\not{E}E$ in our notation, so we calculate the contribution of spells (i)-(iii) to the recall rate in our $\not{E}E$ sample from the 1990-1993 SIPP panels, where we do not need to impute any recalls. We start with short (one or two months) completed jobless spells. In Table A.4, before 1996 there are a total of 14,696 $\not{E}E$ hires that are eligible for a recall (happen in the last three waves), of which about 30%, or 4,500, do end in a recall. Since the four 1990-1993 panels have eight or nine waves each, we multiply these numbers by three and estimate about

45,000 hires from non-employment before 1996, of which 13,500 are recalls. In the same early panels, we counted separately 12,702 completed spells of non-employment $E\not{E}E$ that last one month, and we estimated their recall rate at about one half, so these short spells alone add up to about 6,000 recalls. Treating these cases as uninterrupted employment spells, i.e., "ironing them out", as QWI would do because these spells entail positive earnings and no change in employer id for the calendar quarter, reduces both the number of recalls and the number of hires by 6,000. So the recall rate drops from 13,500/45,000=30% to (13,500-6,000)/(45,000-6,000)=19%. It drops even more if we iron out also the jobless spells $E\not{E}\not{E}E$ where the worker leaves the labor force for two months, as well as two thirds of completed jobless spells of duration three months, and one fourth of those with a duration of four months. Combined with the added recalls from employment in QWI, which we do not count in the SIPP, the 17% recall rate in QWI seems perfectly consistent with our results in Table A.4.

C Model equilibrium: Computation

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

C.1 Steady state

We approximate the AR(1) for idiosyncratic shocks on a discrete grid of 99 points for $\log \varepsilon$ using Tauchen's method, append the lowest state $\varepsilon = 0$ and related transition probability δ to it, to obtain the Markov chain G.

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

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

C.2 Calibration of the alternative models

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

Table C.1: Calibrations for benchmark and alternative models

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

The rest of the parameters remain fixed.

Table C.2: First moment properties

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

C.3 Business cycles

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

To compute the second moments of the aggregate time series, we first solve for the dynamic stochastic equilibrium, namely Bellman values and tightness as functions of the state variables, simulate the panel dataset of 100,000 workers over 4,800 weekly periods, and discard the first 800 observations to randomize the initial conditions. Finally, we aggregate worker-level data to obtain monthly time series of: (i) unemployment rate, (ii) separation probability, (iii) overall job-finding probability, (iv) job-finding probability for new hires, (v) recall probability, and (vi) recall rate (share of recalls out of all hires). We further convert the monthly time series into quarterly series through simple time averaging, as we do with their empirical counterparts that are available at monthly frequency. Lastly, we take the natural logarithm of the quarterly series and HP-filter with smoothing parameter 10^5.

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