Web Appendix for "Quantifying the Contribution of Search to Wage Inequality"

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A Creating the Data Set

A.1 Aligning the 1993 and 1996 SIPP

Our two samples from the SIPP differ regarding data collection and sample size. Unlike the 1993 sample, the 1996 SIPP uses *computer-assisted interviewing techniques* to increase data quality. The computer assures that employer identification numbers stay constant across interviewing waves. Moreover, the 1996 SIPP uses dependent interviewing across waves with respect to employer IDs asking: *"Last time we recorded that you worked for [Employer name]. Do you still work for [Employer name]?"*. Both features likely reduce misreporting in employer changes. In the 1993 sample, interviewers assign employer IDs manually for each wave and use no dependent interviewing across waves. To address the issue in the 1993 sample, we use employers IDs

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constructed by Stinson (2003) which combine the survey data with administrative records to accurately identify these changes.

The 1996 sample also has a considerably larger initial sample size, providing information on 95,402 sample members compared with 56,800 in the 1993 SIPP.[1]

Some information we use from both panels is grouped differently in the 1993 and 1996 SIPP. First, the grouping for the state of residence provides somewhat more detailed information for smaller states in the 1996 panel. Second, the 1993 panel contains monthly information on membership in the armed forces. This information is only available on a 4 months basis in the 1996 panel. Therefore, we have to drop entire individual waves from the 1996 SIPP when the individual reports to have been member of the armed forces during that time. For those readers interested in more of the details of sample creation and sample selection than are provided here and in the next section, STATA and Matlab codes for all our empirical work is available for download on the author's web pages.

A.2 Calculating Hourly Wages and Sample Selection

The SIPP asks respondents whether they are paid by the hour and their corresponding hourly pay rate in each month. We use this hourly pay rate whenever it applies. The SIPP also reports total monthly earnings per job, whether the job lasted the entire month and the number of hours worked per week. When computing monthly earnings of those workers that are not paid by the hour, we assume that workers do not alter their earnings response based on the length of a month and use smooth 4.3 weeks per months.[2] SIPP records starting date and end date of each job that does not last the entire month. We use this information to calculate hourly wages for those months.

As we also report in the main text, we select our sample from the original merged 1993 and 1996 SIPP by using only observations from individuals aged 23-55 (prime working age), for whom we require complete information on the individual's employment status, age and employer id. We only consider an individual's primary job and drop workers that are recalled by former employers[3] or have missing reporting months

[1]The 1993 SIPP was the last sample that published a *Full Panel Longitudinal Research File*. The imputation methods in the *Core files* do not use longitudinal information for imputation purposes and include records for individuals that did not respond in a given wave. We circumvent these problems by using only records that appear in the *Full Panel Longitudinal Research File* as suggested by the CENSUS Bureau.

[2]Neither the reported wage, nor earnings or hours worked are reported by dependent interviewing across waves in either of the two samples.

[3]We chose to exclude those observations because recalled workers likely possess a different search

Table 1: Comparing Sample data to Original Data

	Mean wage	EE rate	$Var(\ln(w_{it}) - \ln(w_{it-1})))$
SIPP	13.71	0.0218	0.1223
Sample	13.43	0.0143	0.0548

Note: The table compares our sample data to the original data. The first column reports the nominal mean wage, the second the rate of job to job transitions and the third column the variance of log-wage growth.
Source: Authors' calculations based on SIPP data.

during a job spell. Moreover, we drop workers reporting to be school enrolled, the self-employed, family-workers, members of the armed forces, workers at non profit companies and anyone whose wage information was imputed by the SIPP. Finally, we truncate the wage distribution at the top and bottom 1 percent to take care of outliers and top-coding.[4] These restrictions leave us with 2,039,345 person/month observations.

In particular our choices of excluding individuals who are recalled by former employers and those with imputed wages introduce changes to the data set which have bearing on some of our key calibration targets. Table 1 compares our final data set to the original, non stratified SIPP samples. Mean hourly wages are almost identical. The aforementioned exclusions limit our data to a sub-sample of the population that has relatively stable work profiles. Job to job transition rates are considerably lower in the stratified data and lower than rates usually reported from CPS data. Moreover, the variance of log wages at job to job transitions is considerably lower in our final sample. Here, the exclusion of imputed wages along with the truncation of the wage distribution at the bottom are largely responsible for the more than fifty percent reduction.

B More on the Empirics of On the Job Search

B.1 Measuring Job to Job Flows

In order to calibrate the job offer arrival rate on the job, it is crucial to accurately identify job to job transitions in the data. One of the biggest advantages in working with SIPP data is that workers are asked to report an employment status for each

technology than what is represented in our model specification.

[4]Earnings are topcoded at \$33333 and \$50000 for a four month period in the 1993 and 1996 sample, respectively.

week of the reporting period separately. This allows us to identify any unemployment spell lasting longer than one workweek.

In a given month we count as employed someone who reports holding a job for the entire month. This definition includes paid as well as unpaid absences as result of vacations, illnesses or labor disputes. It does exclude; however, those who report having been on layoff for at least a week. There is no standard definition for job to job movements in empirical work. We experiment with several different definitions. Our first measure is analogous to the definition in Fallick and Fleischman (2004) and equates job to job transitions with firm changes. We use a monthly employer identifier based on company names. We refer to this definition by $JTJ1$. Given that a firm is a match in our model and given that employees may transit between jobs within a given firm, we find it useful to somewhat broaden the concept beyond employer id changes. For $JTJ2$ we therefore follow Moscarini and Thomsson (2007) in identifying job to job movements by changes in the three digit occupational code. Moreover, we define $JTJ3 = JTJ1 \cup JTJ2$ as the union set from the two definitions.

Table 2 reports job to job flow rates based on the different definitions. For comparison, we also report averages from monthly estimates for the years 1994-2003 taken from Fallick and Fleischman (2004), who use CPS data for individuals ages 24 to 54. As we noted in Appendix A, our choice of excluding individuals with imputed wages and those who are recalled by former employers is equivalent to restricting the sample to people with relatively stable employment profiles, which lets EE-rates drop under any definition. For better comparability with the numbers based on the CPS, in Panel (a) we first report estimates of flow sizes from our raw sample. Identifying job to job movements by either employer changes or changes in the occupational code alone yields roughly comparable flow sizes. However, only our broadest definition of job to job employment transitions comes close to the magnitude found in the CPS. Reassuringly, the share of transitions yielding lower hourly wages and their conditional average loss are very similar regardless of the definition used. This still holds true when repeating the estimation on our final sample in Panel (b). Our calibration is based on the 1.43 percent probability found when applying $JTJ3$ on our baseline sample. It is the only definition which, using the raw sample, yields estimates of comparable size to those from the CPS which many other studies use. Regarding the decrease after applying sample selection criteria, we are confident that this figure is more representative for the kinds of transitions included in our model environment.

Table 2: Different Definitions of JTJ Flow Rates

(a) Full 1993/1996 Merged SIPP Data Set

	$JTJ1$	$JTJ2$	$JTJ3$	CPS
JTJ	1.75	1.27	2.18	2.29
Share loss	36.2	37.2	35.3	
Ave. loss	23.8	23.6	22.6	

(b) Sample for Baseline Calibration

	$JTJ1$	$JTJ2$	$JTJ3$
JTJ	1.03	0.73	1.43
Share loss	36.4	34.4	34.4
Ave. loss	20.1	19	19.6

Note: The Table shows percentage probabilities for job to job transitions based on SIPP data from 1993 to 1999. For reference we also cite monthly averages from CPS data for the years 1994 to 2003 for workers between 25 and 54. The different flow definitions can be found in the text. Panel (a) reports figures for the whole merged 1993/1996 SIPP data set before applying any sample selection criterion. Panel (b) refers to the sample underlying our baseline calibration from which most other numbers in this paper are computed. *Share loss reports* the percentage of *EE* transitions which result in lower hourly wages under the given flow definition. *Ave. loss* reports the corresponding conditional average wage loss.
Source: Authors' calculations based on SIPP data and Fallick and Fleischman (2004) for CPS data.

B.2 Wages and On the Job Search

We argue in the paper that the magnitude of job to job flows in itself is insufficient to identify the on the job offer arrival rate. Instead, the question is how many of these job changes actually yield higher wages for the worker. In the main text, in Section III.B, we established as stylized fact from our data that about one third of all job to job transitions yield lower nominal wages for the worker than his previous job. In this section, we establish the robustness of this result by considering a number of different data stratifications to demonstrate that those wage cuts are not driven by any subsample of the population but instead extend across workers of all kinds. All results are summarized in Table 3.

B.2.1 Wage Gains from Employment Changes

First, we use CPI data to deflate monthly wages. The share of losses in wage changes increases to 47 percent while the mean loss reduces to 14.6 percent. Evidently, many transitions result in unchanged nominal wages between jobs. Of course, the worker should only care about real wages in making his decision. Meanwhile, an argument can be made that in the presence of some wage rigidity, the worker expects a real wage loss on his current job as well, and therefore compares nominal wages. Results are unaltered when trying to control for benefit payments at the new job. We subsequently exclude from the sample transitions from non Union to unionized jobs, transitions into jobs that provide health insurance, and transitions into jobs that provide educational subsidies. None of these modifications significantly changes the moments of interest.

Next, we consider potential business cycle effects by splitting our sample into different years. The willingness of workers to accept a wage reduction upon transition might depend on the aggregate state of the economy. Between, 1993 and 1999, the time of our sample, the US economy experienced the longest uninterrupted expansion in post WWII history which was to last until March 2001. Yet, we again observe no significant variation in the share of losses or the size of losses between years.

Women are known to have less stable work relationships than men and might; therefore, be responsible for an overproportional share of loss making job to job transitions. Nonetheless, in the data both sexes have an equal probability of experiencing a wage cut after moving. The same holds for stratifications by age groups. Young workers have a looser attachment to the labor market and may initially experiment with different career paths or search for jobs with higher non monetary benefits. But none of these phenomena cause the youngest age group to experience markedly more job to job transitions with wage losses.

We also stratify our sample by earnings and tenure. We split the main sample into its lowest and highest quartile and the observations in-between. Again, we do not expect the outcome to be random, because high wage earners are more likely to incur a loss when they are forced to look for alternative employment. Nonetheless, low wage earners are far from insulated to wage losses when switching jobs and even in the lowest quartile, 23 percent of all job to job transitions result in nominal wage losses. Considering tenure is informative in two ways. For one, one might hypothesize that a subsample of the population with a loser labor market attachment who never accumulate longer periods of tenure are disproportionately responsible for the observed job losses. Still, wage losses upon transition are a pervasive phenomenon across all of the tenure distribution. Alternatively, one might be assume that the observed losses are the result of losses in match-specific capital for high-tenured workers. In his case, they should be increasing in tenure on the previous job. This hypothesis, also, is not borne out in the data.[5]

Lastly, we restrict our sample to workers who report being paid by the hour. This might help to rule out potential measurement error resulting from hour calculations of hourly wages for people where only earnings are reported. In that case, the share of losses drops to twenty percent and conditional losses to seven. Still, this figure appears to understate the phenomenon for the population as a whole. First, the group of workers paid by the hour is a highly selective subsample of the population with relatively low wages. Mean hourly wages in the SIPP are \$13.5, but drop to \$11.1 within that group. Second, we are interested in total worker compensation. When workers are asked about their hourly pay rate, the question reads: "What was your regular hourly pay rate at the end of month X". Hence, respondents are unlikely to include any bonuses or performance payments. Contrary, when asking about total monthly earnings, the question explicitly states: "Be sure to include any tips, bonuses, overtime pay, or commissions".

B.2.2 Alternative Explanations

Postel-Vinay and Robin (2002) propose an alternative explanation for those wage losses. They lay out a model where wages can only be renegotiated by mutual agreement and the firm has all the bargaining power. Wage raises on the job occur as a result of counter-offers to bids by other firms. They demonstrate that in such a framework workers will accept wage cuts upon job to job transitions, if the option value of working at the other firm is sufficiently high. Workers only move to higher ranked firms than their current employer and very productive firms offer the potential of large future wage gains.

A testable implication of these types of models is that expected future wage growth with the new employer should be an increasing function of the wage cut accepted. The left panel of Figure 1 plots cumulative wage growth with the new job against the initial wage change for our population of job to job transitions. There is no relationship between the initial wage change and consecutive wage growth. In the right panel, 1 we restrict the sample to agents whom we observe for at least two years with their new job (This time, the initial wage cut is included in the sum). We again find no evidence, that agents that accepted an initial wage cut are compensated

[5]Unfortunately, the tenure measure is of low quality in the SIPP. Of those being employed in the first month of the interview, more than 8 percent of workers report not having been employed with their current employer previously implying unrealistically high worker turnover rates. Moreover, the tenure variable is employer specific in the SIPP and not linked to a job as in our model.

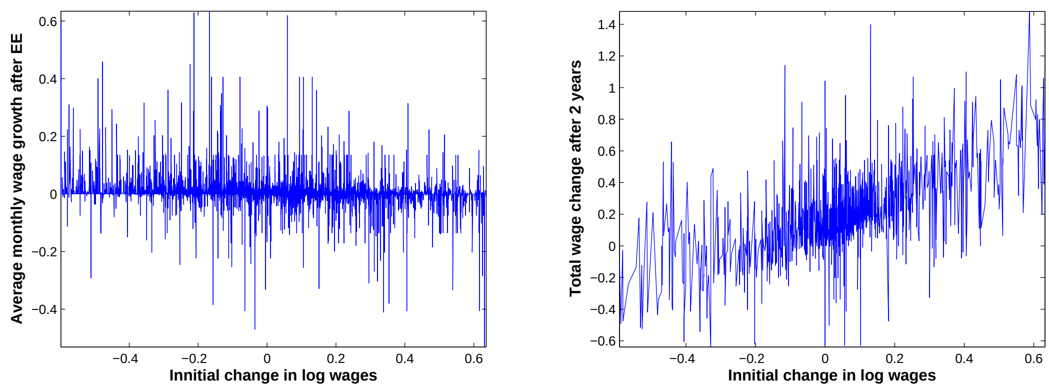

Figure 1: Initial Wages Change and Subsequent Wage Growth

Note: The left panel plots cumulative wage growth in the two months after a job to job movement against the initial wage change, excluding the latter from the calculation. The figure was generated using all observed job to job transitions. In the right panel, we only include job to job transitions where the worker was subsequently observed for at least 24 months. The cumulation of wage growth now includes the initial change upon transition. *Source:* Authors' calculations based on SIPP data.

by steeper wage profiles on the new job.[6] Hagedorn and Manovskii (2010) provide further evidence against the mechanism. They show that wage growth of job stayers in the US is uncorrelated to local labor market tightness whereas the model by Postel-Vinay and Robin (2002) would predict it to be an increasing function of the probability to receive a job offer.[7]

C Estimating the Measurement Error Process

For the identification of the amount of reallocation shocks, the identification of the wage offer distribution, the identification of innovations to individual wage potential and the amount of frictional wage dispersion, we need to identify the process of measurement error. Table 4 reports the results of regressing within job change in log wages (after taking out year dummies) on its lags. The regression indicates that the autocovariance of wage growth is falling at higher lags and close to zero after eleven months. Therefore, we follow Meghir and Pistaferri (2004) and postulate an $MA(q)$

[6]It is of course possible that the higher expected wage increases lie further in the future than the two years we observe. Given that Dustmann and Meghir (2005) find wage-tenure profiles to be basically flat after two years; however, we find this not to be very likely.

[7]The same holds true for models that stress the importance of learning about match quality over time.

process for measurement error (i.e. $r_{i,t} = \Theta(q)\iota_{i,t} = \iota_{i,t} - \sum_{j=1}^{q} \theta_j \iota_{i,t-j}$) fixing q at 12.

In Section V.B of the published paper, we use the assumption that the measurement error process does not change upon a reported job to job transition to identify the size of job heterogeneity. One can think of two alternative assumptions, one leading to an over- and one leading to an underestimation of job heterogeneity. First consider the case where measurement error is constant within job spells and upon each job to job movement the worker draws a new measurement error shock. In this case, the variance of measurement error and job dispersion are not separately identified. However, given the high autocovariance of within job wage growth displayed in Table 4, we find the assumption of constant measurement error to be at odds with the data.

Alternatively, one may assume that measurement error follow a MA process within job spells, but the process begins anew upon a job to job transition. In this case, the variance of wage growth of job stayers would be inflated relatively to job switchers and we would underestimate the variance of job heterogeneity. This theory would imply that the variance of within job wage growth is larger at later stages of the job than in the period directly after the job to job transition (when the MA process is still building up). To assess this implication more formally, we compare the variance of within job wage growth in the first ten months after a job to job transition and the later months. The respective numbers are 0.0111 and 0.0115, so virtually the same.

Finally, to simulate our model, we require actual estimates of $\Theta(12)$ and σ_ι. We obtain these by maximizing the sum of individual likelihoods of within job wage growth in the data. More specifically, we treat $\iota_{i,t}$ as unobserved state and obtain the individual likelihood for wage growth of individual i from the following state space representation

$$g_{it} = \begin{bmatrix} 1 \\ \theta_1 - 1 \\ \theta_2 - \theta_1 \\ \theta_3 - \theta_2 \\ \theta_4 - \theta_3 \\ \theta_5 - \theta_4 \\ \theta_6 - \theta_5 \\ \theta_7 - \theta_6 \\ \theta_8 - \theta_7 \\ \theta_9 - \theta_8 \\ \theta_{10} - \theta_9 \\ \theta_{11} - \theta_{10} \\ \theta_{12} - \theta_{11} \\ -\theta_{12} \end{bmatrix}' \mathbf{R}_{it} + \epsilon_{it}$$

$$\mathbf{R}_{it+1} = \begin{bmatrix} 0&0&0&0&0&0&0&0&0&0&0&0&0&0 \\ 1&0&0&0&0&0&0&0&0&0&0&0&0&0 \\ 0&1&0&0&0&0&0&0&0&0&0&0&0&0 \\ 0&0&1&0&0&0&0&0&0&0&0&0&0&0 \\ 0&0&0&1&0&0&0&0&0&0&0&0&0&0 \\ 0&0&0&0&1&0&0&0&0&0&0&0&0&0 \\ 0&0&0&0&0&1&0&0&0&0&0&0&0&0 \\ 0&0&0&0&0&0&1&0&0&0&0&0&0&0 \\ 0&0&0&0&0&0&0&1&0&0&0&0&0&0 \\ 0&0&0&0&0&0&0&0&1&0&0&0&0&0 \\ 0&0&0&0&0&0&0&0&0&1&0&0&0&0 \\ 0&0&0&0&0&0&0&0&0&0&1&0&0&0 \\ 0&0&0&0&0&0&0&0&0&0&0&1&0&0 \\ 0&0&0&0&0&0&0&0&0&0&0&0&1&0 \end{bmatrix} \mathbf{R}_{it} + \begin{bmatrix} \iota_{it+1} \\ 0 \\ 0 \\ 0 \\ 0 \\ 0 \\ 0 \\ 0 \\ 0 \\ 0 \\ 0 \\ 0 \\ 0 \\ 0 \end{bmatrix}$$

Our calibration imposes the following moment restriction: $\sigma_\epsilon = 0.016$. Table 5 reports our estimation results.

D Robustness Exercises

This section performs three robustness exercises for our quantitative model. We show that our results are almost unaffected by calibrating to a higher replacement rate. Second, we show that our main conclusions are unchanged when reducing the share of reallocation shocks by half. Third, we show that our modeling of reallocation shocks is not in contrast to the large average wage gains of young workers reported by Topel and Ward (1992).

HKV show that the amount of frictional wage dispersion in the standard job ladder model depends crucially on the replacement rate in unemployment. Indeed, equation (A1) in the appendix to the published paper shows that the minimum wage is an increasing function in this parameter. In our calibration, we follow Shimer (2005) and choose a total replacement rate of 0.4. However, the literature has not settled on an appropriate value yet, and substantially higher values have been suggested. Therefore, we follow Hall and Milgrom (2008) and adjust the value of leisure upwards to imply a replacement rate of 0.71. Neither the identified deep parameters, nor the sources of wage inequality over the life-cycle change economically significant as a result from this experiment.[8] The reason for the robustness is that the ability to search on the job, the process of individual wage potential and the option value generated by stochastic innovations to wage potential make workers accept almost all wage offers in our baseline calibration. Put differently, rejected offers have a very low probability of realization. In consequence, shifting the threshold somewhat to the right of the distribution still leads to very low probabilities of jobs being declined. Therefore, the equilibrium distributions are almost unaffected and the inferred parameters are almost unchanged. In fact, we observe substantial changes only with replacement rates close to one.

Given its crucial quantitative importance, we also perform a robustness exercise for the reallocation shocks. We drop the share of loss making job to job transitions as calibration target and reduce the share of reallocation shocks from 10 to only 5 percent. As expected, this calibration performs worse in matching moments of wage growth reported in Table 7 of the published paper. Yet, our main conclusions are quite robust to this exercise. The share of model implied wage inequality attributed to the search friction rises by 14 percentage points, and the share explained by initial worker heterogeneity drops by 3 percentage points. Also, the variance of the job offer distribution and innovations to individual wage potential are almost unchanged. The finding reflects our result from Section 2 that these types of models are mostly affected at the margin of introducing reallocation shocks.

Topel and Ward (1992) find that early in their careers, workers experience large wage gains resulting from job to job movements. One may conjecture that this finding is in contrast to our modeling of reallocation shocks. However, our model lines up closely with their data regarding the profile of wage gains at job to job transitions as a function of experience. Table 6 shows that we match both the average wage

[8] The Mm-ratio changes by less than 10^{-3} at each wage quintile.

increase during the first ten years of labor market experience as well as its decreasing profile with labor market experience.[9] To understand this fact, one should keep in mind that in our model inexperienced workers are located at rather low paying jobs, implying that reallocation shocks are more likely to yield wage gains than for older workers at better jobs.

E Numerical Algorithm

The numerical algorithm consists of two nested loops followed by simulations. Codes are available on the authors' webpages.

- We begin the algorithm by guessing functions for $b(A_t)$ and $Z(A_t)$.
- Next, we discretize the workers' log wage potential by 1500 grid points. We find 15 to be a non binding upper bound. The distribution of the log job component is discretized into 100 equi-likely grid points.
- Given the initial guesses, we can start the inner loop, which calculates the value functions using value function iteration. Expectations regarding next period's idiosyncratic wage potential are calculated using Gaussian quadrature with 10 nodes for evaluating the innovations and linear interpolation[10] between grid points.
- The value functions of the workers allow us to to obtain policy rules for match formation. Using these, we compute the stationary distribution of the economy by distribution function iteration. For the distribution function we use a finer grid for workers' wage potential of 5000 grid points. We then update policy functions.
- Next, we update $b(A_t)$ and $Z(A_t)$ and iterate until convergence.
- The last step are the simulations, that employ the policy functions and equilibrium job offer rates. We use linear inter and extrapolation on the worker and job grid.

[9] The model also does a good job in matching potential labor market experience into actual labor market experience. The model performs less well in matching the large amount of job holdings after the first five years. One should keep in mind that both their data set and sample selection is different from ours, and their sample includes workers that are still in education.

[10] We opt for linear interpolation at this step, as it considerably decreases the computational burden and does not appear to alter the results compared to spline interpolation. Also, spline extrapolation is known to be unreliable.

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Table 3: Wage Cuts after Job to Job Transitions

Stratification		**Share loss**	**Mean loss**
Whole sample			
	Nominal	0.344	-0.196
	Real	0.471	-0.146
Job characteristics			
	Union	0.345	-0.196
	+ Health insurance	0.352	-0.196
	+ Education	0.351	-0.196
Year			
	1993	0.346	-0.196
	1994	0.36	-0.216
	1995	0.364	-0.211
	1996	0.33	-0.192
	1997	0.343	-0.187
	1998	0.324	-0.196
	1999	0.345	-0.178
Gender			
	Male	0.352	-0.202
	Female	0.334	-0.189
Age			
	23-34	0.348	-0.198
	35-43	0.336	-0.196
	44-55	0.345	-0.192
Income			
	Lowest 25 percent	0.233	-0.16
	25-75 percent	0.353	-0.198
	Top 25 percent	0.454	-0.214
Tenure			
	Less than 6 months	0.357	-0.209
	6-12 months	0.309	-0.185
	1-3 years	0.344	-0.182
	3-10 years	0.35	-0.173
	10 and more years	0.343	-0.161
Paid by the hour		0.232	-0.078

Note: The Table shows the share of workers incurring a cut in hourly wages after a job to job movement for the whole population and different subsamples in the 1993/1996 SIPP. Mean loss reports the mean wage loss in log points conditional on suffering a wage cut upon movement. All figures refer to nominal wages, except in the row labeled *Real*.
Source: Authors' calculations based on SIPP data.

Table 4: Autocovariance Structure of Wage Growth

Lag	*Coef.*
1	-0.433
2	-0.232
3	-0.180
4	-0.362
5	-0.248
6	-0.147
7	-0.143
8	-0.171
9	-0.122
10	-0.143
11	-0.075
12	0.023
13	-0.070
14	-0.048

Note: The table reports the coefficients from regressing within job wage growth (after controlling for time fixed effects) on its own lags.
Source: Authors' calculations based on SIPP data.

Table 5: Estimates for Measurement Error

Parameter	Estimate
σ_ι	0.119
θ_1	0.538
θ_2	0.468
θ_3	0.376
θ_4	0.093
θ_5	0.109
θ_6	0.115
θ_7	0.070
θ_8	0.058
θ_9	0.056
θ_{10}	-0.008
θ_{11}	0.021
θ_{12}	0.093

Note: The table shows the estimation results for the measurement error process.
Source: Authors' calculations based on SIPP data.

Table 6: Comparing Wage Growth in the Model to Topel and Ward (1992)

Market Experience (years)	Average wage gain at job transition (model)	Average wage gain at job transition (TW)
$0 - 2.5$	0.132	0.145
$2.5 - 5$	0.099	0.099
$5 - 7.5$	0.079	0.064
$7.5 - 10$	0.068	0.046
$0 - 10$	0.102	0.094

Note: The table compares for different ranges of labor market experience the average change in log wages at a job to job transition in our model to those reported in Topel and Ward (1992) (TW).