MSc Economics

Wage cyclicality: New evidence from the Survey of Income and Program Participation

A Master's Thesis submitted for the degree of
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Affidavit

I, Richard Foltyn

hereby declare

that I am the sole author of the present Master's Thesis,

Wage cyclicality: New evidence from the Survey of Income and Program Participation

52 pages, bound, and that I have not used any source or tool other than those referenced or any other illicit aid or tool, and that I have not prior to this date submitted this Master's Thesis as an examination paper in any form in Austria or abroad.

Vienna, June 18, 2012

Signature

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Abstract

A well-known problem of the canonical Mortensen-Pissarides labor market model is the inability to match business cycle facts such as the volatility of unemployment. One of the remedies proposed in the literature (e.g. Gertler and Trigari (2009)) is to assume that wages of new hires are rigid. However, as some empirical evidence indicates that this assumption is rejected by the data (c.f. Haefke et al. (2008), this solution remains controversial. The present thesis uses the Survey of Income and Program Participation (SIPP), a data set that has been largely ignored in the literature on wage dynamics, to provide new evidence on the wage cyclicality of new hires. I find that wages of new hires react strongly to productivity changes, and in most specifications wages of new hires are more cyclical than wages of workers who remain with their employers. These findings further caution against relying on rigid wages of new hires to address the unemployment volatility puzzle.

1. Introduction

According to Pissarides (2009), Solon et al. (1994) and others, there is an established paradigm among many macroeconomists that wages are rigid which is based on time-series evidence from the 1960s and 1970s.[1] Solon et al. (1994), however, argue that this paradigm is built on a "statistical illusion," a consequence of various biases introduced by data aggregation.

The question whether wages are rigid or not, and if so, which types of wages exhibit rigidity, is of great importance for the empirical plausibility of the Mortensen-Pissarides framework of search and match labor market models, as was first argued by Shimer (2004, 2005). As is well known, the canonical Mortensen-Pissarides model is unable to account for the empirically observed volatilities in unemployment, vacancies and labor market tightness over the business cycle. One possible solution, proposed by Shimer (2005) and Hall (2005), is to introduce wage rigidities into the model. The reasoning is that if wages do not move one for one with productivity, a firm's surplus from a match will be more volatile over the business cycle, thus inducing firms to adjust vacancy creation more strongly, which in turn affects the job-finding probability and unemployment.[2]

As initially shown by Shimer (2004) and made more explicit by Haefke et al. (2008) and Pissarides (2009), wage rigidity in continuing matches does nothing to improve the model's performance. Only sticky wages of newly hired workers can bring the model's prediction closer to empirically observed volatilities. However, as Haefke et al. (2008) point out, imposing rigid wages of newly-hired workers is at odds with the data: using the CPS, Haefke et al. (2008) construct a time series of wages for newly-hired workers as well as all workers and show that wages of newly-hired workers are less persistent and react much more strongly to productivity shocks.

Empirical work on differences in wage stickiness between newly-hired and ongoing workers has started only in recent years, as earlier studies mostly investigated wages of "stayers" and "changers," i.e. workers who stay with or change their employer, respectively. This evidence, which is briefly surveyed in the next section, usually finds that wages of changers are less rigid than those of stayers. However,

[1] In line with the literature on Mortensen-Pissarides search and matching models, I define wage rigidity or wage stickiness as an elasticity of wages w.r.t. productivity that is significantly lower than 1.

[2] An alternative to imposing rigid wages, proposed by Hagedorn and Manovskii (2008), is to simply use different parameters for the workers' bargaining power and the value of nonmarket activity. Such a calibration can give results that are closer to observed business cycle facts. However, this parameterization remains controversial (c.f Costain and Reiter (2008); Hall and Milgrom (2008))

for the canonical Mortensen-Pissarides framework which does not feature on-the-job search, the rigidity of wages of workers who enter the labor market out of unemployment is of greater interest. As mentioned above, one such estimation of the wage rigidity of newly hired workers is presented in Haefke et al. (2008).

The validity of these results is challenged by Gertler and Trigari (2009) who suggest that the higher (semi-)elasticity of wages results from the failure to control for job quality. They argue that workers have to accept low-quality jobs in recessions which do not make full use of their skill level and therefore earn lower wages. Gertler and Trigari claim that once job-specific fixed effects are introduced, wages of new hires are no more volatile than those in ongoing jobs. Supporting the findings of Gertler and Trigari (2009), Hagedorn and Manovskii (2010) argue that once job selection (match quality) is controlled for, wages of new hires are no longer more cyclical than wages of stayers. Nevertheless, in the context of the unemployment volatility puzzle, focusing on the difference in wage cyclicalities between these groups of workers misses the point: the question of interest is whether wages of new hires are cyclical, not whether they are more cyclical than wages of stayers. Viewed from this perspective, the results in Hagedorn and Manovskii (2010) actually contradict those in Gertler and Trigari (2009), as Hagedorn and Manovskii find that after controlling for job selection, wages of stayers and new hires are *both* quite volatile, while in Gertler and Trigari (2009) wages of both groups are much less volatile than what was previously reported in the empirical literature.

The aim of the present paper is to revisit the discussion on the volatility of new hires' wages based on the Survey of Income and Program Participation (SIPP). The SIPP data set seems to be almost completely absent from the empirical literature on wage cyclicality so far except for a brief section in Gertler and Trigari (2009) and in Barattieri et al. (2010).[3] Some of the other few papers that use SIPP data in a labor-market-related context are Tjaden and Wellschmied (2012) who focus on wage dispersion, and Gottschalk (2005) and Gottschalk and Huynh (2010), who deal with measurement errors in the SIPP wage data. Given that the SIPP has been administered by the US Census Bureau since the 1980s and has a large number of observations at monthly frequencies, this lack of empirical studies is surprising, but might be due to the complexity of the data set, the difficulty of accessing the data, quality issues and at least one substantial change in the design of the survey.

The remainder of this paper is structured as follows: Section 2 reviews some of the existing literature on the cyclicality of wages mostly based on PSID an NLSY data. Section 3 discusses the structure of the SIPP data and the issues involved

[3] While Barattieri et al. (2010) examine the rigidity of wages reported in the SIPP, they do this mostly outside of a business cycle context. Since they only use the 1996 panel running from 1996–1999, their sample is not adequate to investigate cyclicality. It is therefore not too surprising that in their sample wages are *positively* correlated with the unemployment rate!

in getting a suitable data set for estimating the cyclicality of wages. Section 4 then attempts (and fails) to replicate the results presented in Gertler and Trigari (2009). In addition, I show that using the specification from Devereux (2001), it is possible to arrive at comparable results using the SIPP data. Lastly, I examine whether aggregating the SIPP data to quarterly observations and estimating the elasticity of wages w.r.t. productivity yields results similar to those in Haefke et al. (2008). Section 5 concludes.

2. Survey of related panel studies

Studies on the cyclicality of wages go back to the 1930s. Due to data availability, they were based on aggregate wage series which gives rise to several problems. Bils (1985) argues that empirical studies using aggregate wage data face three severe limitations:

1. Due to aggregation, it is impossible to control for changes in the composition of the labor force. The resulting composition bias arises because the business cycle affects employees with less education and work experience differently than other workers as they are more likely to be laid off in downturns. Since these workers have lower wages, more low-wage workers are included in the calculation of average wages during booms, thus introducing a countercyclical bias.

   Another composition bias pointed out by Solon et al. (1994) arises because women on average have lower wages and less variable working hours over the cycle. This leads to a procyclical bias in aggregated time series, even though Solon et al. (1994) quantify its magnitude to be small in relation to the composition bias caused by differences in education or skill.

2. Aggregation also leads to biased estimates if the business cycle affects sectors differently. For example, the work force in high-wage manufacturing is more volatile than in other sectors of the economy. When employment in manufacturing declines during recessions, a disproportionately high number of high-wage jobs is lost, resulting in a procyclical bias.

3. Lastly, aggregation assumes that wages of all workers are affected equally by the business cycle, introducing a specification bias if the assumption does not hold. This is particularly relevant for the present paper, since the goal is to investigate differences in the cyclical behavior of wages of continuing workers and new hires.

Given these limitations, it is hardly surprising that the studies using aggregate wage series cited by Bils (1985) find evidence for both procyclical and countercyclical wages. Bils (1985), on the other hand, is one of the first microdata-based studies of wage cyclicality. Using the National Longitudinal Surveys of Youth (NLSY) data which is compiled from interviews of young men from 1979 onwards, he estimates the effect of changes in Δu_t on $\Delta \log w_{i,t}$ of whites and blacks, where u_t is the unemployment rate and $w_{i,t}$ are real wages. The results are summarized in Table 2.1 on page 12. Bils (1985) uses an interaction term to distinguish job

changers and continuing workers, showing that the semielasticity of job changers is more than twice as high as the one of workers who remain with the same employer. Wages of workers who find jobs out of unemployment also react more strongly to the unemployment rate.[1] Bils's results are confirmed by Shin (1994), who extends the sample period and uses a slightly different specification and estimation method.

The study by Beaudry and DiNardo (1991) does not distinguish between continuing and newly-hired workers but investigates alternative determinants of the cyclicality of wages. In addition to contemporaneous wages, the authors include the unemployment rate at the time of hiring and the minimum unemployment rate observed during a worker's tenure as business cycle indicators.[2] They use both the Panel Study of Income Dynamics (PSID) from 1976 to 1984 and the Current Population Survey's (CPS) May supplement on pensions from 1979 and 1983. The most relevant of their findings are given in Table 2.1. For the PSID, all three measures of unemployment are when estimated separately, the minimum unemployment rate having the most pronounced effect. However, when all three variables are used jointly, the contemporaneous unemployment rate is no longer significant. These results have implications for the relative cyclicality of wages of different types of workers: If wages are determined by the unemployment rate at the beginning of a job spell, wages of stayers will be rigid (as historical unemployment rates remain constant), while wages of new hires will be highly correlated with the business cycle.

Solon et al. (1994) also investigate the cyclicality of wages (mainly of prime-aged men), but their focus is on quantifying the composition biases introduced by aggregating wage series. Their main finding is that real wages have been considerably more procyclical than aggregate time series suggest. Furthermore, they report that the semielasticity of wages with respect to unemployment is -1.4 for all men in their PSID sample, while it is -1.24 for workers staying with the same employer. Consequently, stayers' wages are quite procyclical, even if less so than wages of all men. For women, the semielasticity of wages w.r.t. unemployment is -0.53, which is statistically significantly lower than the coefficient for men.

Similar to the studies above, Devereux (2001) examines the differences in the cyclical behavior of wages of all workers and job stayers. Using PSID data, he finds that the semielasticity of wages w.r.t. to the unemployment rate of all workers is -1.16 and -0.81 for job stayers (excluding the self-employed). The coefficient declines to -0.54 if the sample of job stayers is further restricted to exclude those with two or more concurrent jobs. Hence wages of stayers are

[1] Bils (1985) uses the most recent wage of unemployed workers to compute $\Delta \log w_{i,t}$.

[2] The theoretical motivation to look at various unemployment rates is the following: if the labor market was a spot market, only the contemporaneous unemployment would be relevant to determine wages. However, if the market is better characterized by long-term contracts, the unemployment rate when a match was formed or the lowest unemployment rate during the job spell are the relevant determinants of wages, depending on the mobility of workers.

mildly procyclical, even if not as cyclical as those of all workers. The focus of Devereux's work is on wage cyclicality within employer-employee matches, so he further stratifies the sample to differentiate between workers paid by the hour and salaried workers. He finds that reported hourly wages of hourly workers who do not change employers are not procyclical at all, while their overall earnings exhibit substantial procyclicality with a semielasticity of -2.3. This is a consequence of procyclical working hours. For salaried workers, earnings of job stayers are acyclical unless workers receive additional non-salary income. Consequently, any procyclicality for this group of workers seems to be be induced by overtime pay and bonuses.

Paper	Data	Sample	Dep. var.	BC var.[a]	Results (Coef. on unempl. $\times -100$)	
Bils (1985)	NLSY79	Annual data on 3,438–5,225 males who finished school and are not self-employed; 1966-1980	$\Delta \log w_{i,t}$	Δu_t	All white/black: Stayers white/black: Changers white/black: In/out of LF white/black:	1.6/1.8 0.6/0.4 3.7/4.4 2.5/3.3
Beaudry and DiNardo (1991)	PSID/CPS	PSID: annual obs. on males with pos. and untruncated wages, 1976–1984; CPS: 1979/1983 Pension supplement of May CPS	$\log w_{i,t}$	u_t, $u_{t,1}$, $\min\{u_t\}$	PSID: Contemp. u_t, FE:[b] PSID: Initial u_t, FE:[b] PSID: Min. u_t, FE:[b] PSID: Contemp. u_t, FE:[c] PSID: Initial u_t, FE:[c] PSID: Min. u_t, FE:[c] CPS: Initial u_t: CPS: Min. u_t:	0.7 0.6 2.9 1.4 2.1 2.9 1.7 3.1
Shin (1994)	NLSY79	Same as Bils (1985), extended to 1981	$\Delta \log w_{i,t}$	Δu_t	All white/black: Stayers white/black: Changers white/black:	1.7/1.4 1.2/0.2 2.7/3.8
Solon et al. (1994)	PSID	Unbalanced panel men: aged 16+, includes years with >100 hours worked; annual obs. for 1967–1987	$\Delta \log w_{i,t}$	Δu_t	All men: All women: Stayers (men):	1.4 0.5 1.2
Devereux (2001)	PSID	Annual data on men aged 18 to 64 with at least 100 hours worked per year; 1970–1992	$\Delta \log w_{i,t}$	Δu_t	All workers, all types: Stayers, all types:[d] Stayers w/o extra job, all types:[d] Stayers (hourly), hourly wage:[e] Stayers (hourly), earnings:[e] Stayers (salaried), hourly wage:[f] Stayers (salaried), earnings:[f]	1.2 0.8 0.5 0.1 2.3 0.3[†] 0.5[†]

[†] Not statistically significant and conventional levels.
[a] Business cycle variable
[b] Fixed-effects specification includes contemporaneous, initial and min. unemployment
[c] Fixed-effects estimated separately for contemporaneous, initial and min. unemployment
[d] Excludes self-employed. "Type" refers to hourly or salaried workers.
[e] Hourly workers reporting hourly wage rate in PSID
[f] Salaried workers reporting salary / earnings.

Table 2.1.: Estimates of wage cyclicality in selected studies

3. Data source

The Survey of Income and Program Participation is a (partially overlapping) series of micro panels that has been administered by the US Census Bureau since the 1980s. Each SIPP panel is divided into four equally-sized rotation groups which are interviewed in waves. Each wave collects data on the four reference months preceding the interview date. As an example, TABLE A.1 illustrates the structure of the 2008 SIPP. This data is compiled into monthly per-person observations and published in person-month format. The number of waves administered and the number of households surveyed differs across SIPP panels. The basic characteristics of the 8 most recent panels which I use in my analysis are given in TABLE 3.1.

The combined sample of the 1990 to 2008 SIPP panels has more than 20m observations, out of which about 16m relate to persons of age 16 or higher. In addition, the SIPP survey allows for up to two job observations per month. In situations where job-specific effects are of interest (such as in the specification used by Gertler and Trigari (2009)), it is advantageous to transform the SIPP into a job-month structure. Then each observation represents monthly data on a job, while observations on persons who are unemployed or out of the labor force are effectively in a person-month format. The resulting increased number of observations is reported in the last column of TABLE 3.1.

3.1. Data processing issues

3.1.1. Extraction

The SIPP data can be accessed either via a web interface, or the raw fixed-format data files can be downloaded from the US Census Bureau's FTP server[1] or the SIPP-specific site at the NBER.[2] Since the web interface is ill-suited for even a medium-scale data extraction, the data were extracted from the raw files. Each SIPP panel is split into core wave files, topical module files, longitudinal files (for the 1990–1993 panels) and longitudinal weight files (2001–2008 panels) with a combined size of about 100 GB in uncompressed state contained in almost 200 separate files. In an attempt to facilitate access to the SIPP data, the CEPR (2012) has published STATA do-files to process and clean the raw data.[3] While these unified CEPR extracts are a useful starting point for researchers using the

[1] http://www.census.gov/sipp/access.html
[2] http://www.nber.org/data/sipp.html
[3] These are distributed under an open source license and can be obtained at http://ceprdata.org/sipp-uniform-data-extracts/.

Panel	Waves	Households†	Start	End	Min. obs.‡	Max. obs.‡	Total person-month obs.	Total job-month obs.
1990	8	19,800	1989/10	1992/09	10,490	46,570	1,362,482	1,398,590
1991	8	15,626	1990/10	1993/09	6,684	29,981	873,827	896,925
1992	9	21,577	1991/10	1995/05	9,164	41,192	1,351,226	1,387,435
1993	9	21,823	1992/10	1996/01	8,969	41,569	1,350,183	1,383,953
1996	12	40,188	1996/01	2000/03	14,850	79,463	3,042,367	3,241,559
2001	9	50,000	2000/10	2004/01	13,380	74,701	2,009,087	2,119,667
2004	12	51,379	2003/10	2008/01	8,161	92,412	3,003,831	3,159,913
2008	13	52,031	2008/06	2011/07	16,502	89,197	2,624,771	2,734,260
Total			1989/10	2011/07			15,617,774	16,322,302

† Households: number of wave 1 eligible households.
‡ Min./Max. obs. refer to the minimum and maximum number of job observations per month.
Numbers of observations refer to obs. relating to persons of age 16 or higher.

Table 3.1.: Basic characteristics of 1990–2008 SIPP panels

SIPP for the first time, the code is too unmaintainable to be used directly in any long-term project. However, they provide some valuable hints on how to organize the data and create a uniform data set across the 1990–2004 SIPP panels.[4]

3.1.2. Unified data extracts

Starting from the 1996 panel, the SIPP survey has undergone a major redesign. One consequence was a complete overhaul of the variable naming and data encoding scheme, thus making comparisons across pre- and post-1996 panels difficult. Additionally, the US Census Bureau ceased to release merged and edited longitudinal files containing the full panel. Instead, researchers are now required to merge the separate core and topical module wave files themselves. Adopting the approach from CEPR (2012), I created a subset of unified variables which mostly abstract from whether an observation is part of a pre- or post-1996 panel.

3.1.3. Imputations and corrections

In this section I briefly address some of the issues encountered when creating the unified SIPP data extract.

Weights. The SIPP attempts to be a representative data set for the whole civilian non-institutionalized US population. To this end, every observation is associated with several weights (person, family, sub-family, calendar month and full panel weights) which designate how many individuals in the population it represents. Unfortunately, these weights were not designed to be used when pooling more than one SIPP panel. For a person-month analysis which treats each month as a cross-section, this can easily be corrected by adjusting the weights for the relative number of observations per panel each month. Note that this is mostly an issue that needs to be addressed for the early 1990–1993 panels, as the later panels no longer overlap (except for a few months at most).

On the other hand, there is no straightforward way to adjust the constant full panel weights which are assigned to persons who were in the panel for its entire duration. As the degree to which panels overlap changes over the lifetime of a panel, it is not easy to construct adjustment factors that preserve the representativeness of the pooled panel and keep the panel person weights constant at the same time.

Employer IDs. Each employee is assigned a sequentially increasing employer ID (job ID) to identify jobs across waves. These employer IDs are only unique in the context of a particular employee, so it is not possible to estimate specifications with some form of firm fixed-effects based on the SIPP data. Before the survey

[4] The CEPR extracts seem to be unmaintained at the moment as they do not include the 2008 SIPP at all.

was computerized in 1996, field representatives were responsible for drawing a new job ID for each new job spell (or assigning an existing one if an employee returned to a previously held job). It is questionable how reliable this processes really worked, as a revision of job IDs based on not publicly available employer names and administrative records performed by Stinson (2003) changes thousands of job IDs in each of the pre-1996 panels.

Job starting dates and tenures. Tenure is an important control variable in most wage regressions, but it is not easy to compute reliably from the SIPP data. The pre-1996 panels had no question on job starting dates in the core module. These were only surveyed in the wave 2 topical module, thus being subject to potential matching problems. From the 1996 panel onwards, the questionnaire includes questions on the starting date of the current job spell. However, these values quite often vary across waves for the same employer ID. In such cases I chose either the mode or the minimum value. For job spells that started during the SIPP (i.e. at a later date than first reference month of wave 1), I impute missing job starting dates from the first date this job was observed.

Additionally, based on the reported job starting dates there is a substantial number of implied negative tenures (of up to −8 months) whenever the reported job starting date comes after an observation's reference month. I do not attempt to correct for this and simply drop affected job spells. The resulting distribution of maximum job tenures observed in the 1990–2008 SIPP panels is shown in FIGURE 3.1. Note that this histogram is computed from the cleaned data, i.e. after applying the steps outlined in section 3.1.4.

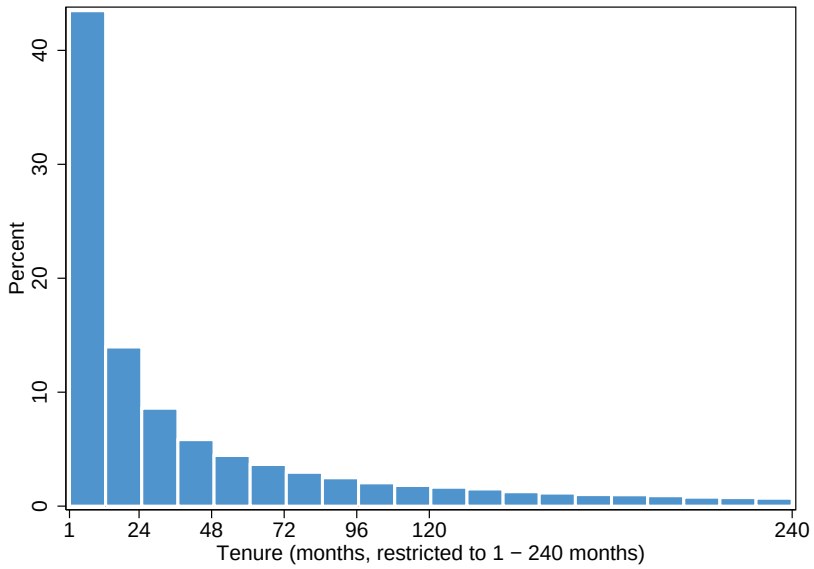

Figure 3.1.: Distribution of maximum job tenures in unified SIPP data (based on cleaned data set)

FIGURE 3.2 shows the length of job spells solely based on SIPP observations, i.e. by disregarding reported starting dates that predate the respective SIPP survey. It is evident that the observations on a particular job spell mostly cut off at the end of a wave (which has a reference period of four months). As mentioned in the SIPP User Guide, people are more likely to report changes between waves than within them. The pattern in FIGURE 3.2 is a consequence of this "seam effect."

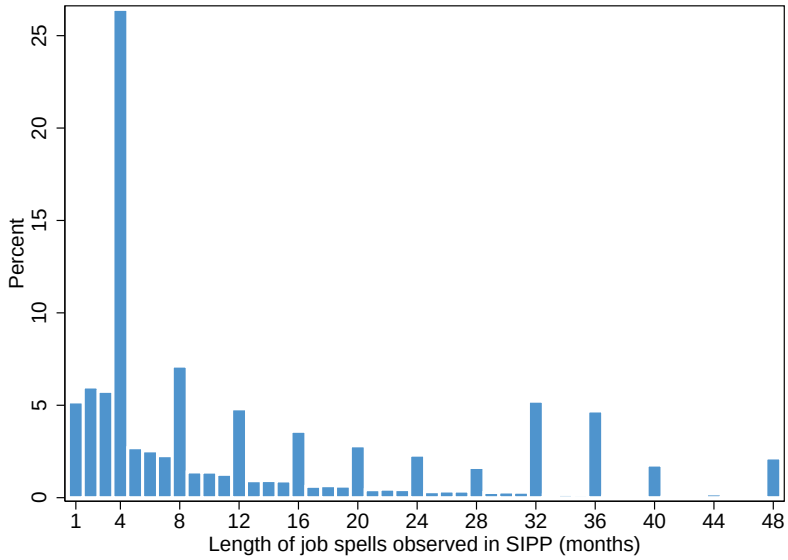

Figure 3.2.: Length of job spells observed in SIPP (based on cleaned and unified data set)

Monthly working hours. The SIPP survey instrument does not directly ask for hours worked per month. Instead the data files report the typical weekly working hours per job and the number of weeks worked. This does not work well for workers with more than one job per month as it cannot be reliably established how many weeks were spent on each job and to what extent these overlapped. If the reference month coincides with the reported job starting or ending date, I attempt to compute the maximum weeks spent on a particular job from these data. Nevertheless, average hourly wages per job computed from monthly earnings over monthly hours are not very reliable, at least for workers with more than one job. A possible alternative to avoid the substantial measurement errors in working hours is to restrict the analysis to hourly workers as in Gottschalk (2005) and Barattieri et al. (2010). Since most hourly workers report hourly wage rates directly, there is no need to compute them using the number of hours worked.

Topcoded earnings and wages. To protect the privacy of interviewees, earnings and wage variables in the public SIPP files are topcoded. Topcoding is triggered whenever earnings per job or business exceed a certain threshold *per wave*. These thresholds differ across SIPP panels; they are reported in TABLE A.2

in the appendix. Whenever wavely earnings are exceeded, the monthly earnings for a particular job or business above a monthly threshold are replaced with topcoded values.[5] For the 1990–1993 panels, all such monthly values were replaced with \$8,333. Starting in 1996, monthly earnings exceeding the values listed in TABLE A.2 are replaced with a mean value computed from earnings above the wavely threshold. These values depend on a person's gender, race and worker status (whether someone works full time or not) and are shown in TABLE A.3.

It is important to keep in mind that topcoding occurs only if the wavely threshold is exceeded. Therefore, it is possible to observe non-topcoded monthly earnings of up to \$50,000 for the 1996 and 2001 panels (\$66,666 for the 2004 and 2008 panels) if a person works only in one of the four reference months.

Hourly wages are also topcoded, but no attempt is made to achieve consistency between topcoded earnings, hours worked and topcoded hourly wages. The latter are simply replaced with a maximum value that differs across panels if a person reports a wage rate above this threshold. Hourly wage topcodes are also given in TABLE A.2.

Interestingly enough, the SIPP data set does not provide a reliable way to identify topcoded observations. Since it is impossible to calculate the worker status mentioned in TABLE A.3 from publicly available data, the best one can do is eliminate all observations with earnings that equal one of the topcodes. Even though unlikely, this might eliminate non-topcoded earnings as well.

Educational attainment. The SIPP reports educational attainment based on a categorical scale. These categories changed significantly between the pre-1996 and post-1996 panels as the SIPP moved to a system almost identical to the one used for the CPS. In order to compute experience using the standard method ($age - educ - 6$), I derive years of education from the categorical data based on the guidelines in Jaeger (1997). I re-estimated the returns to education regressions reported there to verify that this procedure yields plausible results for my SIPP sample. The results are given in TABLE A.4 and match Jaeger's results quite well.

3.1.4. Data cleaning

As the last step of data pre-processing, I dropped all observations falling into one of the categories listed in TABLE 3.2. Most observations excluded from the sample belong either to children or to unemployed and thus do not have a job ID. I also decided to drop all persons who potentially report topcoded earnings or hourly wage rates as it is impossible to examine wage cyclicality if earnings or wages hit an upper boundary. Note that there seems to be a flaw in the 2004 and 2008

[5] The monthly threshold is computed as a quarter of the wavely threshold since each wave consists of four reference months.

All job-month obs., panels 1990–2008	**20,807,177**
Drop obs. with missing job ID	11,590,731
Drop persons with age < 16 for all obs.	12,916
Drop employees with duplicate job IDs[†]	29,262
Drop employees with potentially topcoded earnings	344,358
Drop persons with too much variation in age[‡]	47,044
Drop persons with decreasing age[§]	52,537
Drop person with negative tenures	65,291
Drop job spells with negative experience	160
Remaining obs.	**8,664,878**

[†] Duplicates in terms of job ID, reference month, industry and occupation. Drops all duplicates since it is not possible to identify which job ID is the correct one.
[‡] Most likely due to some matching error, these observations exhibit strong variation in age across waves, while being consistent within waves. Effects 2004 and 2008 SIPP only.
[§] Related to previous problem. Again only affects 2004 and 2008 SIPP.

Table 3.2.: Number of observations dropped from full SIPP sample

panels as the age series of some individuals exhibit implausibly high variation across waves or are even decreasing in time. I drop all affected individuals.

Based on this cleaned data set, FIGURE 3.3 shows the fraction of new hires among all workers (for this particular purpose I define new hires as workers with a tenure of less than 3 months), while FIGURE 3.4 shows the average hourly real wage rate for new hires and stayers.

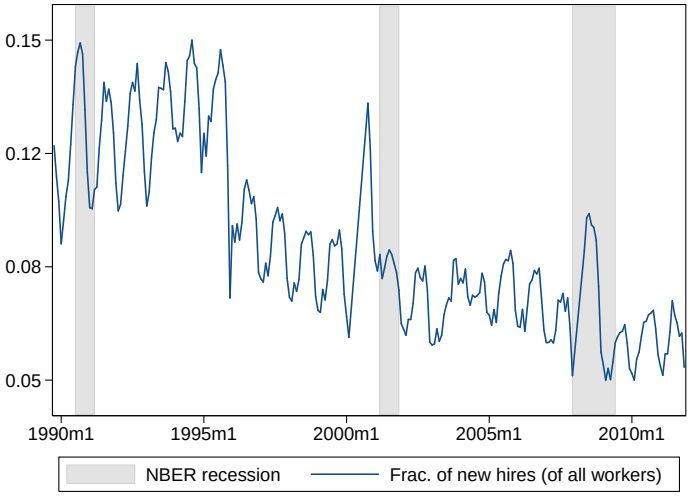

Figure 3.3.: New hires as a fraction of all workers

Section A.4 shows numerous other mostly demographic characteristics for new hires and continuing workers. Some of the time series, such as the monthly working hours shown in FIGURE A.10, seem to have breaks that might have been introduced by changes in the survey methodology (in this particular case, this break seems to coincide with the start of the 1996 panel). Note that all of these statistics were computed using the adjusted monthly person weights.

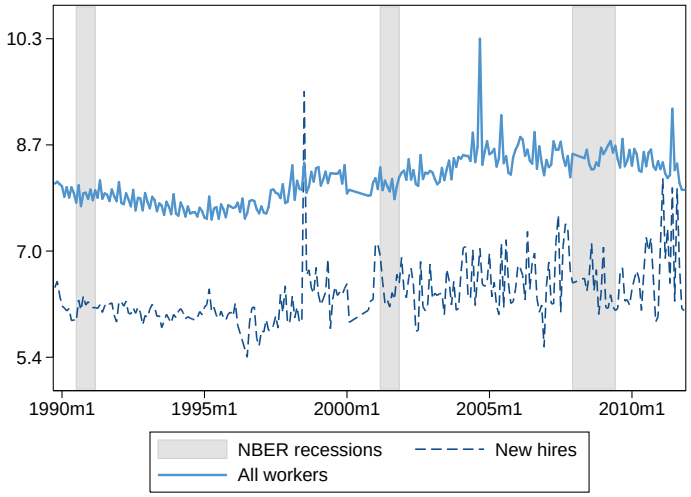

Figure 3.4.: Average hourly real wage of new hires and stayers

4. Replicating previous studies using SIPP data

4.1. Gertler and Trigari (2009)

4.1.1. Specification

As a first step, I attempt to replicate the results reported in Gertler and Trigari (2009). Since in their model stayers and new hires receive the same wage (there is a unique wage per firm), the cyclical behavior of wages of new hires and continuing workers is identical. While this is at odds with the findings of studies cited in section 2, Gertler and Trigari (2009) argue that these results fail to account for match-specific job quality. Once match quality is accounted for, wages of new hires are no more cyclical than those of stayers. Consequently, in models that do not control for job quality, the higher cyclicality of new hires results from a procyclical composition bias, as during booms high-wage firms hire an increasing share of workers.

To demonstrate this effect, Gertler and Trigari (2009) use two different specifications of Mincer-type regressions with the unemployment rate as the business cycle indicator:

$$\log w_{i,j,t} = \boldsymbol{x}_{i,t}\boldsymbol{\beta}_x + \beta_u u_t + \beta_n u_t d^{new}_{i,j,t} + \gamma_i + \epsilon_{i,j,t} \tag{4.1}$$

$$\log w_{i,j,t} = \boldsymbol{x}_{i,t}\boldsymbol{\beta}_x + \beta_u u_t + \beta_n u_t d^{new}_{i,j,t} + \gamma_{i,j} + \epsilon_{i,j,t} \tag{4.2}$$

Individual workers are indexed by i, firms by j and time periods by t. The vector $\boldsymbol{x}_{i,t}$ contains the usual individual characteristics such as age, tenure or experience, u_t is the unemployment rate, $d^{new}_{i,j,t}$ is a dummy indicating a newly-formed match of worker i and firm j and γ_i is a worker fixed effect. In the second specification, $\gamma_{i,j}$ denotes a match (or job) fixed effect.

Gertler and Trigari (2009) claim that specification (4.1) gives the usual results (i.e. β_n is negative and significant), while model (4.2) is compatible with their interpretation that once compositional effects are accounted for (via $\gamma_{i,j}$), β_n is no longer significant.

4.1.2. Results

I follow Gertler and Trigari and restrict the data set to the 1990–1993 panels. Since new hires are defined as those workers with a tenure of up to one year, I also exclude observations of persons with shorter job spells. Otherwise it will be

impossible to distinguish job fixed effects from the new hires indicator variable.[1] Additionally, due to the measurement errors in monthly hours worked and thus in average hourly wages computed from these data, I trim my sample to exclude the lower and upper 0.5 percentiles of wages in each period. As in Gertler and Trigari (2009), I use a tenure of less than one year to identify new hires. Similar to their model, I include the usual control variables: polynomials in age and education, a polynomial in tenure (for job fixed effects) and indicator variables for marital status and metropolitan area. Since I am using fixed effects, I cannot include dummies for race or gender as these are time-invariant.

I estimate eight sets of regressions. The central results are shown in Table 4.1 and Table 4.2, while the full set of control variables as well as alternative specifications using weighted fixed effects and wavely data are given in the appendix.[2] In each table the first two regressions include only men (as in Gertler and Trigari (2009)), while columns (3) and (4) include both sexes. Furthermore, I experiment with different sets of educational attainment controls, giving results for both educational attainment in years and categorical educational attainment.

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
u_t	-0.490^{***}	-0.494^{***}	-0.367^{***}	-0.369^{***}
	(0.033)	(0.033)	(0.024)	(0.024)
newhire $\times$ u_t	-0.241^{***}	-0.239^{***}	-0.238^{***}	-0.236^{***}
	(0.014)	(0.014)	(0.009)	(0.009)
Obs.	747,314	747,180	1,450,488	1,450,208
R^2 (overall)	0.163	0.195	0.118	0.172
No. of groups	26,669	26,662	52,254	52,239
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	28.02	28.02	27.76	27.76
Obs. per group (max)	36	36	36	36

Dep. variable: log real wages $\times$ 100 (deflated with CPI)
Notes: Columns (1) and (2) include only men aged 18 to 64, while columns (3) and (4) include both sexes. All columns only include observations from job spells lasting more than a year. New hires are those workers with a tenure of less than one year. Person fixed effects used for all specifications.
Legend: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table 4.1.: Main results from Gertler and Trigari (2009) replication, person FE (monthly, unweighted)

The results for specifications with person fixed effects and monthly data are shown in Table 4.1. The coefficient of the unemployment rate of -0.49 for

[1] This restriction is not required for the worker fixed effects estimation, but I want to keep the sample identical across both specifications to exclude any changes in coefficients due to changes in the sample. For the same reason I also exclude all persons with missing tenure, as tenure is a control variable in the match FE regressions.

[2] I chose to run both unweighted and weighted regressions as I do not have adequate weights for the match fixed effects models. As discussed in section 3, SIPP provides full panel weights for individuals, but not for single job spells. I therefore transform person full panel weights to job spell weights by multiplying by the fraction of hours spent on each job spell.

the males-only unweighted specification is close to Gertler and Trigari's −0.564.[3] On the other hand, the coefficient of the interaction term of new hires with unemployment is −0.24 while Gertler and Trigari report −1.042. Thus new hires' wages are significantly more volatile with person fixed effects, but less so than reported by Gertler and Trigari (2009).

The coefficients of most other control variables are in line with economic intuition. I find a significant effect of union membership, while in other studies such as Solon et al. (1994) this effect is insignificant. Wages are also higher in metropolitan areas and for married workers.

For the categorical education variables, I use those who hold a highschool diploma as the reference group. Intuitively, workers with less than high school have lower wages; interestingly, so do workers who have some years of college (but no degree). Note that in the weighted specifications most of the categorical educational attainment coefficients are insignificant, which casts doubt on the validity of the weights used. When years of schooling are used instead, the coefficient of the non-squared education term is negative. However, the total marginal effect of one year of additional education is positive, e.g. it is 1.06% at the mean level of education (13.4 years) in column (2) of Table B.1.

To estimate the fixed effects models with match fixed effects, I restructure the data set into a job-month format, which increases the number of groups to approximately 29,000. This makes it trivial to estimate the model without having to resort to dummy variables. The main results are shown in Table 4.2. In the

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
u_t	-0.414^{***}	-0.417^{***}	-0.322^{***}	-0.323^{***}
	(0.033)	(0.033)	(0.023)	(0.023)
newhire × u_t	-0.169^{***}	-0.168^{***}	-0.135^{***}	-0.133^{***}
	(0.015)	(0.015)	(0.010)	(0.010)
Obs.	751,356	751,223	1,461,081	1,460,802
R^2 (overall)	0.128	0.155	0.135	0.177
No. of groups	29,342	29,335	57,568	57,553
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	25.61	25.61	25.38	25.38
Obs. per group (max)	36	36	36	36

Dep. variable: log real wages × 100 (deflated with CPI)
Notes: Columns (1) and (2) include only men aged 18 to 64, while columns (3) and (4) include both sexes. All columns only include observations from job spells lasting more than a year. New hires are those workers with a tenure of less than one year. Match fixed effects used for all specifications.
Legend: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table 4.2.: Main results from Gertler and Trigari (2009) replication, job FE (monthly, unweighted)

unweighted specification, the coefficient of the unemployment rate slightly drops

[3] In order to be consistent with other coefficients reported in this paper, I multiply log real wages by 100 so the coefficients can be interpreted as percentage changes.

to -0.41, while the coefficient of the interaction term with new hires drops to about -0.17 (-0.21 in the unweighted specification). The coefficient is neither positive nor insignificant in any of the regressions examined, in contrast to the results reported by Gertler and Trigari (2009).

To summarize, I was not able to replicate Gertler and Trigari's result. This might be due to the lack of detail in their exposition which makes it impossible to know which control variables they included and which additional restrictions they imposed on the data set. Also, since they have only a maximum of 9 observations per group, it seems that they used only one observation per wave. For comparison, I re-estimated the above specifications using only the last observation per wave, but the results are quite close to my previous findings (even quantitatively). They are reported in TABLE B.5 to TABLE B.8 in the appendix.

Since the negative and significant sign of the interaction term is robust across person and match fixed effects regressions with various control variables and weights, it is hard to see how Gertler and Trigari (2009) could have arrived at their results.[4]

Lastly, the magnitude of the semielasticity of log wages w.r.t. unemployment is considerably smaller than what the studies listed in TABLE 2.1 have found. This might be a consequence of the methodological approach (two-stage estimation with differenced log wages vs. fixed effects) and the different time periods under consideration.

4.2. Devereux (2001)

4.2.1. Specification

In the present section I continue to assess the quality of the SIPP data and its potential to confirm previous results found in the empirical literature by replicating some of the findings in Devereux (2001) using my data set. As mentioned in section 2, Devereux investigates the differences in wage cyclicality of various types of workers, in particular how it differs between those who do not change employers and all workers. He uses the PSID with a sample of men aged 18 to 64 who work more than 100 hours a year running from 1970 to 1992. Following the standard approach in the literature (see the papers listed in TABLE 2.1, in particular Solon et al. (1994)), the Mincer equation that Devereux estimates for different types of workers is

$$\Delta \log w_{i,t} = \beta_0 + \beta_1 \Delta u_t + \boldsymbol{x}_{i,t}\boldsymbol{\beta}_2 + \beta_3 t + \nu_t + \epsilon_{i,t} \tag{4.3}$$

[4] Similarly, for a Portuguese linked employee-employer data set, Carneiro et al. (2009) also find significant differences in wage cyclicality between new hires an stayers even after controlling for job fixed effects as in Gertler and Trigari (2009).

where the row vector $\boldsymbol{x}_{i,t}$ contains a third-order polynomial in labor market experience (and tenure for stayers) as individual controls. To account for a possible year-specific error term ν_t the coefficients in (4.3) are estimated in two steps.

The first-stage equation is given by

$$\Delta \log w_{i,t} = \boldsymbol{x}_{i,t}\boldsymbol{\gamma} + \sum_{s=1}^{T} \varphi_s \mathbb{1}_{[s=t]} + \eta_{i,t} \tag{4.4}$$

where $\mathbb{1}_{[s=t]}$ is a set of dummy variables that take on the value 1 if the observation is from time period s. In the second stage, the coefficients from (4.4) are regressed on the differenced unemployment rate and a time trend:

$$\hat{\varphi}_t = \delta_0 + \delta_1 \Delta u_t + \delta_3 t + u_t \tag{4.5}$$

Note that since Devereux takes the standard approach and uses differenced wages to eliminate time-invariant individual-specific effects, it is not possible to examine the cyclicality of newly hired workers, unlike in the specification used by Gertler and Trigari (2009). Therefore the analysis is restricted to wages of all workers and stayers.

4.2.2. Results

The results from replicating Table 1 in Devereux (2001) are shown in Table 4.3. The corresponding estimates from the first stage of the two-stage approach are given in Table C.1. While the coefficients do not exactly match Devereux's findings (which would be surprising given the different time period, time frequency and sample), they reflect the general trend quite well. In column one, I use the entire sample of men aged 18 to 64 from the 1990-2004 SIPP panels who work in the non-farm business sector.[5] I take 12 lags when computing differenced wages and unemployment rates to match Devereux's annual differences. As in Devereux (2001), I restrict the sample to exclude self-employed in column (2) and include only stayers (i.e. workers who report the same primary employer as 12 months ago) in column (3). In column (4) I further restrict the sample by eliminating all workers who hold more than one job in a given month. I use the inverse variances of the time dummy estimates from stage 1 as weights in the second stage to account for varying numbers of observations across time periods and the resulting effects on precision.

The results show that wages of all workers (semielasticity of -0.58) react more strongly to changes in the unemployment rate than wages of stayers (semielasticity of -0.32). Devereux (2001) reports coefficients of -1.16 and -0.81 for these types of workers. I also report the results from a 1-stage regression approach

[5] The 2008 panel is not included in the analysis as at this point the survey is still ongoing. Hence the US Census Bureau has not published person weights for the full panel, which I use to weight observations from the other panels.

	(1)	(2)	(3)	(4)
Dep. var.:	Coefs. S1	Coefs. S1	Coefs. S1	Coefs. S1
Δu_t	-0.5781^{**}	-0.5897^{**}	-0.3181	-0.3285^{*}
	(0.1839)	(0.1829)	(0.1644)	(0.1645)
trend	0.0065^{**}	0.0065^{**}	0.0054^{**}	0.0051^{**}
	(0.0022)	(0.0021)	(0.0019)	(0.0019)
Constant	5.4987^{***}	5.4703^{***}	4.2364^{***}	4.4350^{***}
	(0.9756)	(0.9682)	(0.8541)	(0.8641)
Obs.	166	166	166	166
R^2	0.124	0.127	0.081	0.075
F-stat	11.49	11.91	7.204	6.645

Dep. variable: time dummy coefficients from stage 1 regression.
Notes: Column (1) uses observations from all males aged 18–64 who work in the nonfarm business sector. In (2) all self-employed are excluded. In (3) the sample is further restricted to stayers only, while in (4) only stayers without additional jobs are used.
Inverse variances of coefficients from first stage are used as period-specific weights in second stage.
Legend: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table 4.3.: Replication of Table 1 in Devereux (2001), stage 2 of 2-stage method

with errors clustered by time in Table C.2. The results are almost identical.

Table 4.4 lists some of the replication results of Devereux's Table 2 which are based on observations of stayers paid by the hour (the results from the first-stage regression can be found in Table C.3). While Devereux finds that hourly rates of hourly workers are almost acyclical, my results indicate that they are slightly procyclical with a semielasticity that is almost identical to the one found for all stayers.

	(1)	(2)	(3)
Dep. var.:	Coefs. S1	Coefs. S1	Coefs. S1
Δu_t	-0.3261^{*}	-0.3562^{*}	-1.1424^{*}
	(0.1445)	(0.1442)	(0.4517)
trend	0.0041^{*}	0.0044^{**}	0.0030
	(0.0017)	(0.0017)	(0.0052)
Constant	4.7911^{***}	4.7769^{***}	15.1335^{***}
	(0.7625)	(0.7636)	(2.3762)
Obs.	166	166	166
R^2	0.076	0.089	0.042
F-stat	6.719	7.916	3.617

Dep. variable: time dummy coefficients from stage 1 regression.
Notes: Column (1) gives results for stayers paid on an hourly basis. Column (2) excludes workers from (1) with more than one job. Column (3) uses total earnings as dependent variable in stage 1.
Inverse variances of coefficients from first stage are used as period-specific weights in second stage.
Legend: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table 4.4.: Replication of Table 2 in Devereux (2001), stage 2 of 2-stage method

The results hardly change if I further restrict the sample to include only those workers who do not have additional jobs (column (2)). However, when overall earnings are used as the dependent variable in column (3), I find considerably

more cyclicality, which is consistent with Devereux's results.

Overall the specifications examined here indicate that Devereux (2001)'s findings also hold for the SIPP, even though the semielasticities I find are at the lower bound of what was previously reported in the related literature. However, the studies cited in TABLE 2.1 use annual data, while the SIPP observations are monthly. Furthermore, at least some of the studies use average wages derived from earnings from all jobs, while SIPP reports job-specific earnings for up to two jobs. Since monthly working hours cannot be reliably calculated from the SIPP data for multiple job holders, it is not straightforward to create individual wage series that are comparable to previous studies. It is unclear how much the differences in semielasticities is attributable to these SIPP-specific issues.

Before concluding this section I want to briefly address the extremely low R^2 reported in the stage 1 (and 1-stage) tables in the appendix. These seem to be in line with other results in the literature, at least if R^2 is reported: Bils (1985) has R^2 statistics of around 0.02, while Devereux (2001), Shin (1994), Beaudry and DiNardo (1991) and Barlevy (2001) do not report R^2 at all. Solon et al. (1994) report R^2 of 16%–82%, but only for the second-stage regression.

4.3. Elasticity of wages with respect to productivity

In a last step, I investigate the elasticity of wages with respect to productivity. As argued by Haefke et al. (2008), this is the more relevant statistic for models in the Mortensen-Pissarides framework since productivity is taken to be an exogenous process while the unemployment rate is determined endogenously.

Since productivity is only available at quarterly frequencies, I transform the monthly SIPP data into quarterly observations before estimating a person fixed effects specification. However, there is no straightforward way to combine up to 6 monthly wage observations on up to two jobs into one quarterly wage statistic. The usual approach of dividing total earnings per period by hours worked cannot be used with the SIPP data as total earnings are unknown (only earnings per job are reported) and monthly hours worked cannot be computed reliably. I therefore experimented with several measures of hourly wages per quarter:

1. Use the last monthly primary job observation as quarterly wage. Here only one monthly observation is used.

2. Use the average of the last observed primary job as quarterly wage. In this case up to three wage rates are averaged to obtain the quarterly wage.

3. Use the average of the most often observed primary job as quarterly wage. Again, up to three observations are averaged to form on quarterly wage.

4. Compute average wages as the fraction of total earnings (from up to two jobs) over total hours (from up to two jobs).

I present evidence using the first approach in TABLE 4.5 (results including control variables are given in TABLE D.2), while findings using the second approach are shown in TABLE D.1. Weighted estimates of the first method are given in TABLE D.3.

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
newhire × lprod	0.0127^{*}	0.0190^{***}	0.0140^{**}	0.0203^{***}
	(0.0055)	(0.0044)	(0.0046)	(0.0039)
lprod	0.7343^{***}	0.7100^{***}	0.6698^{***}	0.6075^{***}
	(0.0171)	(0.0136)	(0.0139)	(0.0123)
newhire	-0.0846^{***}	-0.1106^{***}	-0.0894^{***}	-0.1154^{***}
	(0.0244)	(0.0194)	(0.0204)	(0.0173)
Obs.	2,115,130	2,095,432	1,961,130	1,217,505
R^2 (overall)	0.189	0.221	0.225	0.170
No. of groups	292,352	291,240	283,729	210,217
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	7.235	7.195	6.912	5.792
Obs. per group (max)	17	17	17	17

Dep. variable: log real wages (deflated with the GDP deflator)
Column (1) uses obs. of workers aged 18 to 64 in nonfarm business sector who are not self-employed. Column (2) excludes outliers (with wage below (above) the 0.5 (99.5) percentile. In column (3) the sample is further restricted to include workers with only one concurrent job, while in column (4) only hourly workers are considered.
Quarterly data are created by using the last monthly observation of the primary job wage each quarter.
Legend: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Table 4.5.: Wage cyclicality w.r.t. productivity (quarterly, unweighted)

While the results show that wages of new hires are statistically significantly more volatile than wages of stayers, the difference is economically small. Haefke et al. (2008) find that for their CPS sample, the elasticity of new hires is 0.79 for hourly wage while it is 0.24 for all workers. On the other hand, I find an elasticity of about 0.75 for new hires, but the elasticity for stayers is only marginally lower at 0.73 (column (1)), and hence the elasticity for all workers will be in between these values. The elasticities for both groups decrease if I exclude outliers (column (2)), exclude workers with multiple jobs (column (3)) and in addition examine only hourly workers (column (4)). These results do not change much when full panel weights are used (see TABLE D.3), even though then the level effect on wages of new hires is much more pronounced (so new hire wages are even lower than suggested in TABLE 4.5).

To summarize, I was not able to replicate the substantial difference between the elasticities of new hires and all workers found in Haefke et al. (2008). This may be due to a completely different estimation strategy as they do not have panel data but instead use a 2-stage approach to construct aggregate time series from a sequence of CPS cross sections. Additionally, they have observations from 83 quarters while in my sample the longest job spell observed lasts only 17 quarters.

Yet again, as discussed in the introduction, whether wages of stayers are less cyclical than wages of new hires is irrelevant since only the rigidity of new hires' wages determines firms' vacancy posting behavior. In this regard, my results are quite similar to those in Haefke et al. (2008), as both papers find elasticities of around 0.75. Therefore in both approaches the data seem to reject the assumption that new hires' wages are overly rigid.

5. Conclusion

The aim of the present paper was to revisit the debate on the cyclicality of wages of various types of workers (new hires, stayers, all workers) using the SIPP data set which has been mostly ignored by the empirical labor literature so far. In section 3, I highlighted some of the issues that have to be dealt with before using this data set in empirical work, in particular the methodological break in 1996, the lacking quality of some of the variables and problems related to pooling several SIPP panels, such as the absence of adequate weights.

Despite these issues, using the SIPP to re-estimate previous results from the literature can yield meaningful outcomes. I demonstrated that the findings of Devereux (2001) mostly apply to the SIPP data as well, even if the coefficients of the semielasticity of wages w.r.t. the unemployment rate are lower than what Devereux or other authors found. However, since my sample covers a different time period and uses monthly frequencies (as opposed to annual frequencies), there is no reason why the SIPP data should be able to exactly match those results.

On the other hand, I was unable to confirm the findings of Gertler and Trigari (2009), which seems to be the only paper that actually uses the SIPP data set to examine wage cyclicality. While the effects I find are much smaller in magnitude compared to other studies and even the findings in Gertler and Trigari (2009), the fact that wages of new hires are more cyclical than those of all workers across all specifications I examined at least casts doubt on the results presented in their paper. Given the lack of detail in their exposition, however, it is impossible to tell where these differences come from.

Lastly, while I was unable to replicate the substantial difference in wage elasticities w.r.t. productivity of new hires and all workers reported in Haefke et al. (2008), I found elasticities of new hires' wages that are very close to their findings. According to my results, wages of new hires increase by 0.75% for every 1% increase in productivity, which casts further doubt on the empirical validity of models that rely on the rigidity of new hires' wages to solve the unemployment volatility puzzle. These results are similar to those in Hagedorn and Manovskii (2010), who also fail to find significant differences in the wage cyclicalities of stayers and new hires after controlling for job selection. However, their estimates also indicate that wages of both stayers and new hires are quite cyclical.

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A. Tables and figures characterizing SIPP data

A.1. Panel structure

Reference Month	Rotation Group 1	2	3	4
May 08	W1 1			
June 08	W1 2	W11		
July 08	W1 3	W1 2	W1 1	
Aug. 08	**W1 4**	W1 3	W1 2	W1 1
Sept. 08	W2 1	**W1 4**	W1 3	W1 2
Oct. 08	W2 2	W2 1	**W1 4**	W1 3
Nov. 08	W2 3	W2 2	W2 1	**W1 4**
Dec. 08	**W2 4**	W2 3	W2 2	W2 1
Jan. 09	W3 1	**W2 4**	W2 3	W2 2
Feb. 09	W3 2	W3 1	**W2 4**	W2 3
Mar. 09	W3 3	W3 2	W3 1	**W2 4**
April 09	**W3 4**	W3 3	W3 2	W3 1
May 09	W4 1	**W3 4**	W3 3	W3 2
June 09	W4 2	W4 1	**W3 4**	W3 3
July 09	W4 3	W4 2	W4 1	**W3 4**
Aug. 09	**W4 4**	W4 3	W4 2	W4 1
Sept. 09	W5 1	**W4 4**	W4 3	W4 2
Oct. 09	W5 2	W5 1	**W4 4**	W4 3
Nov. 09	W5 3	W5 2	W5 1	**W4 4**
Dec. 09	**W5 4**	W5 3	W5 2	W5 1
Jan. 10	W6 1	**W5 4**	W5 3	W5 2
Feb. 10	W6 2	W6 1	**W5 4**	W5 3
Mar. 10	W6 3	W6 2	W6 1	**W5 4**
April 10	**W6 4**	W6 3	W6 2	W6 1
May 10	W7 1	**W6 4**	W6 3	W6 2
June 10	W7 2	W7 1	**W6 4**	W6 3
July 10	W7 3	W7 2	W7 1	**W6 4**
Aug. 10	**W7 4**	W7 3	W7 2	W7 1
Sept. 10		**W7 4**	W7 3	W7 2
Oct. 10			**W7 4**	W7 3
Nov. 10				**W7 4**

Reference Month	Rotation Group 1	2	3	4
Sept. 10	W8 1	*See Wave7 data in bottom*		
Oct. 10	W8 2	W8 1	*of first column*	
Nov. 10	W8 3	W8 2	W8 1	
Dec. 10	**W8 4**	W8 3	W8 2	W8 1
Jan. 11	W9 1	**W8 4**	W8 3	W8 2
Feb. 11	W9 2	W9 1	**W8 4**	W8 3
Mar. 11	W9 3	W9 2	W9 1	**W8 4**
April 11	**W9 4**	W9 3	W9 2	W9 1
May 11	W10 1	**W9 4**	W9 3	W9 2
June 11	W10 2	W10 1	**W9 4**	W9 3
July 11	W10 3	W10 2	W10 1	**W9 4**
Aug. 11	**W10 4**	W10 3	W10 2	W10 1
Sept. 11	W11 1	**W10 4**	W10 3	W10 2
Oct. 11	W11 2	W11 1	**W10 4**	W10 3
Nov. 11	W11 3	W11 2	W11 1	**W10 4**
Dec. 11	**W11 4**	W11 3	W11 2	W11 1
Jan. 12	W12 1	**W11 4**	W11 3	W11 2
Feb. 12	W12 2	W12 1	**W11 4**	W11 3
Mar. 12	W12 3	W12 2	W12 1	**W11 4**
April 12	**W12 4**	W12 3	W12 2	W12 1
May 12	W13 1	**W12 4**	W12 3	W12 2
June 12	W13 2	W13 1	**W12 4**	W12 3
July 12	W13 3	W13 2	W13 1	**W12 4**
Aug. 12	**W13 4**	W13 3	W13 2	W13 1
Sept. 12		**W13 4**	W13 3	W13 2
Oct. 12			**W13 4**	W13 3
Nov. 12				**W13 4**

Table A.1.: Struture of waves and rotation groups in the 2008 SIPP. Source: SIPP (2009)

A.2. Topcoding

Panel	Annual earnings‡	Wavely earnings	Monthly earnings‡	Hourly wages
1990–1993	99,996	33,332	8,333	99.99†
1996	150,000	50,000	12,500	29.00
2001	150,000	50,000	12,500	29.00
2004	200,000	66,666	16,666	28.50
2008	200,000	66,666	16,666	35.00

† Hourly wage topcode should be 52.00 according to US Census Bureau emails, but substantial number of observations exceeds this threshold.
‡ Calculated thresholds implied by wavely earnings, does not trigger topcoding by itself.

Table A.2.: Topcoding thresholds in the 1990–2008 SIPP. Source: SIPP (2009) and email communication with US Census Bureau.

Sex	Race	Worker status†	Topcode amount by panel (in USD)			
			1996	2001	2004	2008
Male	Non-black, non-Hispanic	FYFT	29,600	29,057	37,750	30,200
Male	Non-black, non-Hispanic	NFYFT	38,270	24,956	38,900	30,000
Male	Black, non-Hispanic	FYFT	17,530	20,769	51,400	31,800
Male	Black, non-Hispanic	NFYFT	24,015	20,769	51,400	57,900
Male	Hispanic, any race	FYFT	26,250	24,283	33,600	22,000
Male	Hispanic, any race	NFYFT	24,015	36,866	33,600	26,100
Female	Non-black, non-Hispanic	FYFT	21,990	23,420	30,000	31,900
Female	Non-black, non-Hispanic	NFYFT	49,450	25,973	43,500	38,500
Female	Black, non-Hispanic	FYFT	24,015	26,841	51,400	23,400
Female	Black, non-Hispanic	NFYFT	24,015	26,841	51,400	57,900
Female	Hispanic, any race	FYFT	24,015	31,909	33,600	26,100
Female	Hispanic, any race	NFYFT	24,015	31,909	33,600	26,100

† FYFT: Full year, full time; NFYFT: Not full year, full time

Table A.3.: Topcodes used in the 1996–2008 SIPP. Source: SIPP (2009) and email communication with US Census Bureau.

A.3. Returns to education

	All	Females	Nonwhites	Age†		
				25-35	36-46	47-64
Education in years						
SIPP	0.089^{***}	0.099^{***}	0.076^{***}	0.104^{***}	0.094^{***}	0.074^{***}
Jaeger (1997)	0.093^{***}	0.102^{***}	0.101^{***}	0.109^{***}	0.095^{***}	0.081^{***}
Categorical education						
SIPP						
Dropouts	-0.192^{***}	-0.224^{***}	-0.182^{***}	-0.181^{***}	-0.191^{***}	-0.180^{***}
Some college	0.151^{***}	0.166^{***}	0.166^{***}	0.152^{***}	0.157^{***}	0.134^{**}
College grads.	0.509^{***}	0.537^{***}	0.546^{***}	0.544^{***}	0.508^{***}	0.445^{***}
Jaeger (1997)						
Dropouts	-0.261^{***}	-0.288^{***}	-0.264^{***}	-0.247^{***}	-0.241^{***}	-0.280^{***}
Some college	0.170^{***}	0.198^{***}	0.172^{***}	0.160^{***}	0.179^{***}	0.165^{***}
College grads.	0.450^{***}	0.505^{***}	0.542^{***}	0.430^{***}	0.476^{***}	0.469^{***}

SIPP-based regressions include year dummies to account for possible changes in returns to education of the 20 years of the sample.

Table A.4.: Return to education in cleaned SIPP data compared to CPS data in Jaeger (1997)

A.4. Worker characteristics

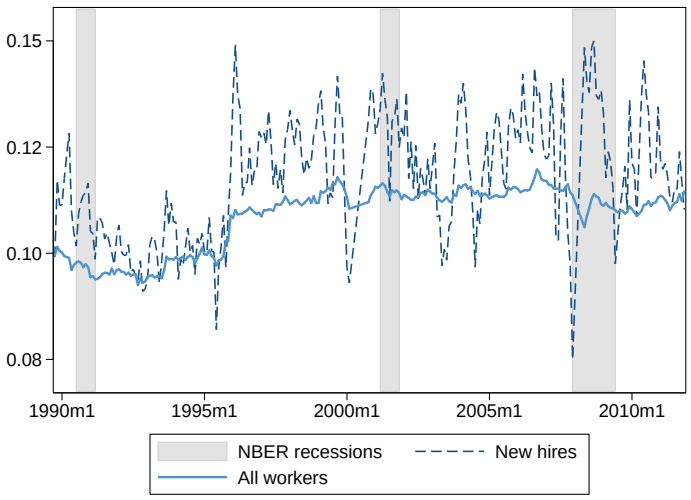

Figure A.1.: Fraction of blacks among new hires and stayers

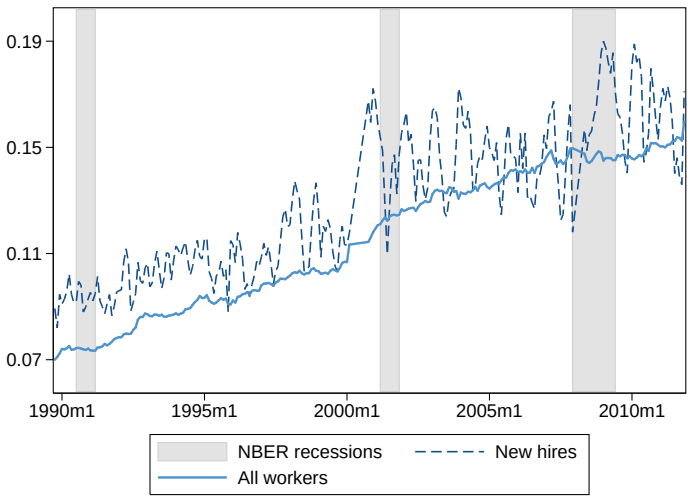

Figure A.2.: Fraction of hispanics among new hires and stayers

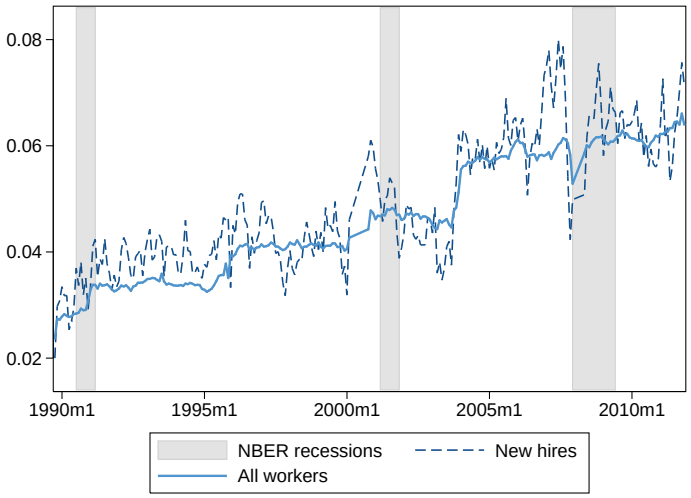

Figure A.3.: Fraction of other nonwhites among new hires and stayers

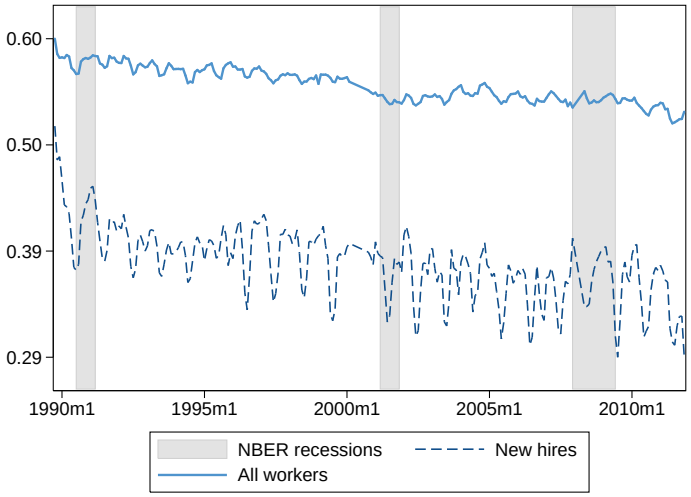

Figure A.4.: Fraction of married among new hires and stayers

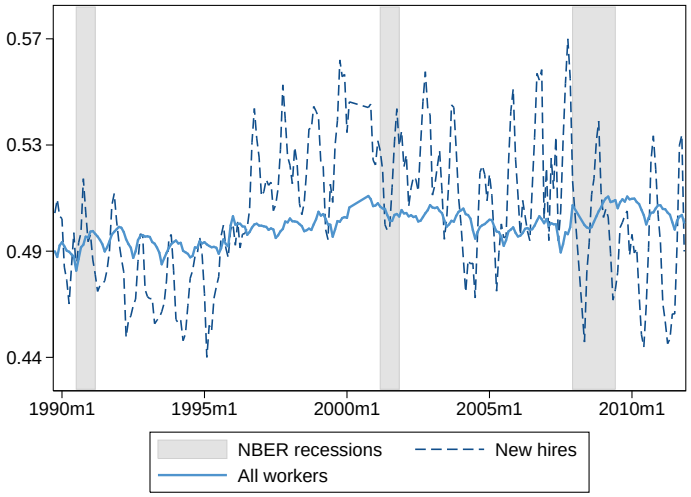

Figure A.5.: Fraction of females among new hires and stayers

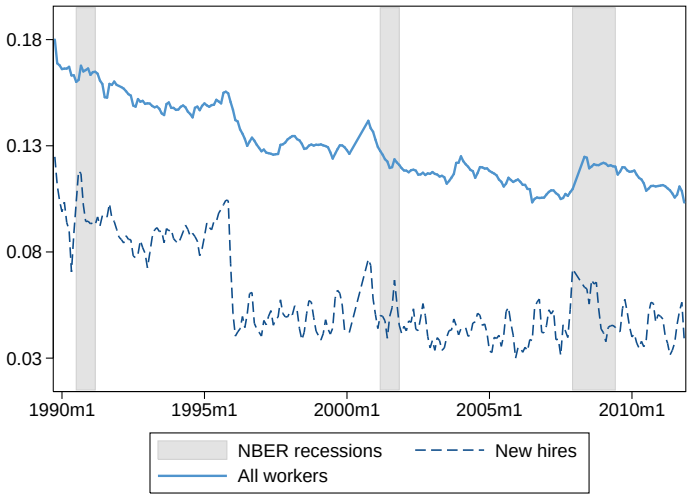

Figure A.6.: Fraction of union members among new hires and stayers

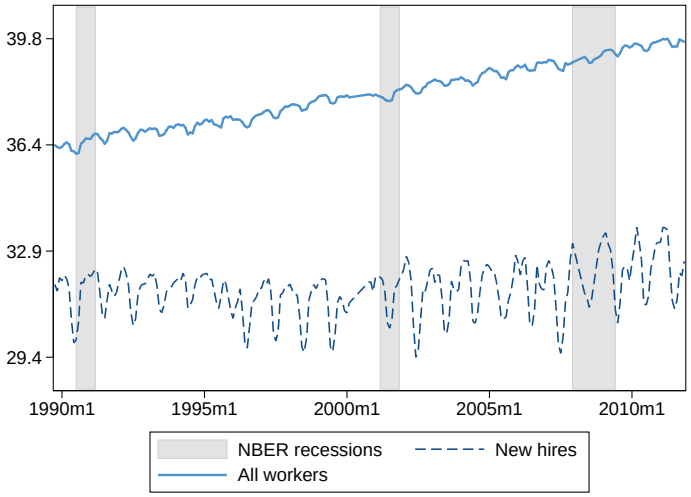

Figure A.7.: Average age of new hires and stayers

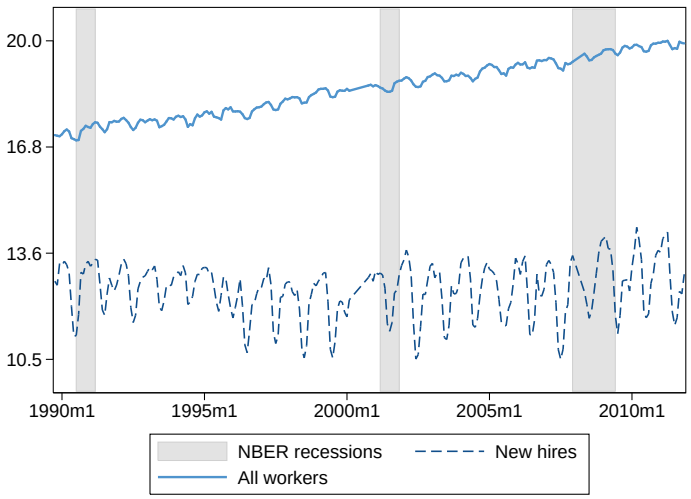

Figure A.8.: Average years of experience of new hires and stayers

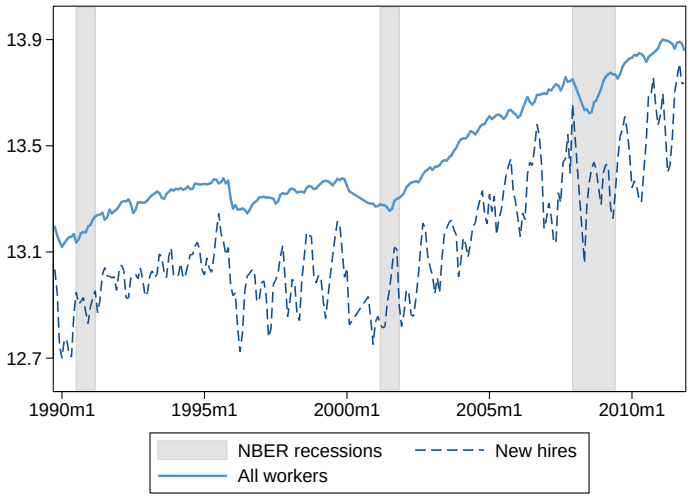

Figure A.9.: Average years of schooling of new hires and stayers

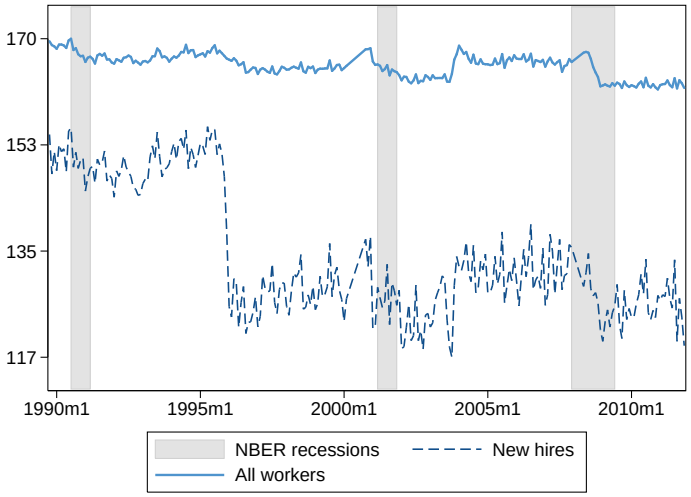

Figure A.10.: Average monthly working hours of new hires and stayers

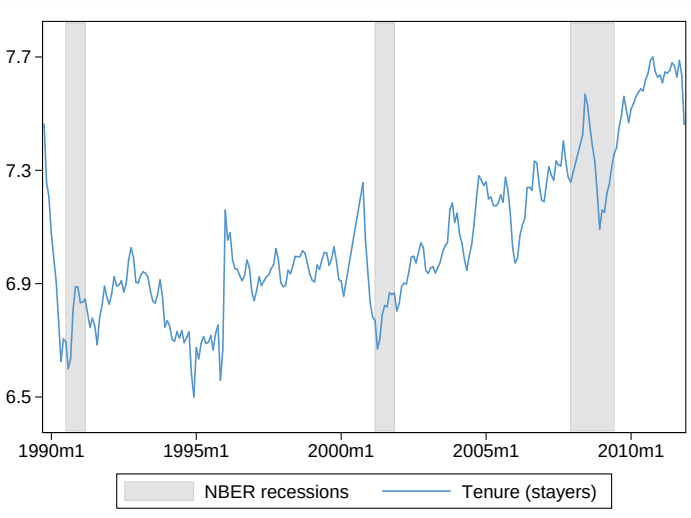

Figure A.11.: Average tenure of stayers

B. Detailed results from replicating Gertler and Trigari (2009)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
Business cycle				
u_t	-0.490^{***}	-0.494^{***}	-0.367^{***}	-0.369^{***}
	(0.033)	(0.033)	(0.024)	(0.024)
newhire $\times$ u_t	-0.241^{***}	-0.239^{***}	-0.238^{***}	-0.236^{***}
	(0.014)	(0.014)	(0.009)	(0.009)
Control vars.				
age	5.145^{***}	5.039^{***}	4.691^{***}	4.560^{***}
	(0.096)	(0.097)	(0.068)	(0.069)
age^2	-0.058^{***}	-0.057^{***}	-0.050^{***}	-0.049^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
married	1.005^{***}	0.992^{***}	0.721^{***}	0.710^{***}
	(0.169)	(0.169)	(0.119)	(0.119)
metro	1.692^{***}	1.704^{***}	2.187^{***}	2.180^{***}
	(0.332)	(0.332)	(0.255)	(0.255)
union	2.105^{***}	2.106^{***}	1.988^{***}	1.991^{***}
	(0.119)	(0.119)	(0.086)	(0.086)
Education				
Less than highschool	-1.517^{***}		-0.833^{***}	
	(0.333)		(0.234)	
Some college	-1.452^{***}		-0.283	
	(0.360)		(0.236)	
Bachelor´s degree	1.292^{**}		1.608^{***}	
	(0.461)		(0.312)	
Grad. college	2.127^{***}		2.540^{***}	
	(0.491)		(0.333)	
educ		-3.811^{***}		-3.543^{***}
		(0.419)		(0.296)
educ^2		0.182^{***}		0.179^{***}
		(0.017)		(0.012)
Constant	112.019^{***}	131.461^{***}	103.077^{***}	120.700^{***}
	(2.007)	(3.302)	(1.430)	(2.371)
Obs.	747,314	747,180	1,450,488	1,450,208
R^2 (overall)	0.163	0.195	0.118	0.172
No. of groups	26,669	26,662	52,254	52,239
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	28.02	28.02	27.76	27.76
Obs. per group (max)	36	36	36	36

Dep. variable: log real wages $\times$ 100 (deflated with CPI)
Notes: Columns (1) and (2) include only men aged 18 to 64, while columns (3) and (4) include both sexes. All columns only include observations from job spells lasting more than a year. New hires are those workers with a tenure of less than one year. Person fixed effects used for all specifications.
Marginal effects in (2): educ 1.06% (at sample mean 13.4); age: 0.52% (at sample mean 39.6)
Marginal effects in (4): educ 1.26% (at sample mean 13.4); age: 0.64% (at sample mean 39.8)

Table B.1.: Gertler and Trigari (2009) replication results, person FE (monthly data, unweighted)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
Business cycle				
u_t	-0.492^{***}	-0.492^{***}	-0.364^{***}	-0.363^{***}
	(0.091)	(0.091)	(0.064)	(0.064)
newhire $\times$ u_t	-0.255^{***}	-0.253^{***}	-0.259^{***}	-0.257^{***}
	(0.044)	(0.044)	(0.029)	(0.029)
Control vars.				
age	5.327^{***}	5.229^{***}	4.808^{***}	4.679^{***}
	(0.275)	(0.273)	(0.195)	(0.194)
age^2	-0.060^{***}	-0.059^{***}	-0.051^{***}	-0.050^{***}
	(0.003)	(0.003)	(0.002)	(0.002)
married	1.231^{**}	1.211^{**}	1.081^{***}	1.063^{***}
	(0.441)	(0.440)	(0.320)	(0.319)
metro	2.222	2.227	2.543^{**}	2.537^{**}
	(1.202)	(1.203)	(0.958)	(0.958)
union	2.143^{***}	2.144^{***}	2.032^{***}	2.036^{***}
	(0.346)	(0.346)	(0.247)	(0.247)
Education				
Less than highschool	-1.273		-0.793	
	(0.935)		(0.583)	
Some college	-1.492		0.007	
	(0.957)		(0.595)	
Bachelor´s degree	0.899		2.092^{*}	
	(1.312)		(0.865)	
Grad. college	1.877		3.129^{***}	
	(1.394)		(0.919)	
educ		-3.322^{**}		-3.536^{***}
		(1.204)		(0.766)
educ^2		0.158^{**}		0.179^{***}
		(0.053)		(0.034)
Constant	105.249^{***}	122.333^{***}	98.215^{***}	115.988^{***}
	(5.855)	(8.875)	(4.210)	(5.995)
Obs.	669,024	668,927	1,303,008	1,302,767
R^2 (overall)	0.158	0.189	0.120	0.167
No. of groups	21,972	21,967	43,385	43,373
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	30.45	30.45	30.03	30.04
Obs. per group (max)	36	36	36	36

Dep. variable: log real wages $\times$ 100 (deflated with CPI)
Notes: Columns (1) and (2) include only men aged 18 to 64, while columns (3) and (4) include both sexes. All columns only include observations from job spells lasting more than a year. New hires are those workers with a tenure of less than one year. Person fixed effects used for all specifications. Obs. are weighted by SIPP full panel weights adjusted by average relative number of obs. per panel.

Table B.2.: Gertler and Trigari (2009) replication results, person FE (monthly data, weighted)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
Business cycle				
u_t	-0.414^{***}	-0.417^{***}	-0.322^{***}	-0.323^{***}
	(0.033)	(0.033)	(0.023)	(0.023)
newhire $\times$ u_t	-0.169^{***}	-0.168^{***}	-0.135^{***}	-0.133^{***}
	(0.015)	(0.015)	(0.010)	(0.010)
Control vars.				
age	3.337^{***}	3.287^{***}	2.663^{***}	2.613^{***}
	(0.124)	(0.124)	(0.086)	(0.086)
age^2	-0.039^{***}	-0.038^{***}	-0.032^{***}	-0.031^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
married	0.785^{***}	0.779^{***}	0.513^{***}	0.506^{***}
	(0.168)	(0.168)	(0.118)	(0.118)
metro	1.992^{***}	1.984^{***}	1.173^{***}	1.164^{***}
	(0.354)	(0.354)	(0.274)	(0.274)
union	1.427^{***}	1.426^{***}	1.378^{***}	1.380^{***}
	(0.118)	(0.118)	(0.085)	(0.085)
Education				
Less than highschool	-0.950^{**}		-0.558^{*}	
	(0.327)		(0.229)	
Some college	-0.632		0.059	
	(0.355)		(0.232)	
Bachelor´s degree	0.718		0.910^{**}	
	(0.454)		(0.307)	
Grad. college	1.019^{*}		1.411^{***}	
	(0.483)		(0.328)	
tenure	0.949^{***}	0.924^{***}	1.325^{***}	1.304^{***}
	(0.083)	(0.083)	(0.058)	(0.058)
tenure^2	-0.033^{***}	-0.033^{***}	-0.039^{***}	-0.039^{***}
	(0.002)	(0.002)	(0.001)	(0.001)
educ		-3.004^{***}		-2.339^{***}
		(0.427)		(0.301)
educ^2		0.139^{***}		0.114^{***}
		(0.018)		(0.012)
Constant	146.596^{***}	161.930^{***}	146.424^{***}	157.915^{***}
	(2.809)	(3.872)	(2.006)	(2.779)
Obs.	751,356	751,223	1,461,081	1,460,802
R^2 (overall)	0.128	0.155	0.135	0.177
No. of groups	29,342	29,335	57,568	57,553
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	25.61	25.61	25.38	25.38
Obs. per group (max)	36	36	36	36

Dep. variable: log real wages $\times$ 100 (deflated with CPI)
Notes: Columns (1) and (2) include only men aged 18 to 64, while columns (3) and (4) include both sexes. All columns only include observations from job spells lasting more than a year. New hires are those workers with a tenure of less than one year. Match fixed effects used for all specifications.
Marginal effects in (2): educ 0.72% (at sample mean 13.4); age: 0.28% (at sample mean 39.5); tenure: 0.30% (at sample mean 9.6)
Marginal effects in (4): educ 0.71% (at sample mean 13.4); age: 0.14% (at sample mean 39.75); tenure: 0.64% (at sample mean 8.6)

Table B.3.: Gertler and Trigari (2009) replication results, job FE (monthly data, unweighted)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
Business cycle				
u_t	-0.396^{***}	-0.396^{***}	-0.302^{***}	-0.302^{***}
	(0.085)	(0.085)	(0.060)	(0.060)
newhire $\times$ u_t	-0.207^{***}	-0.206^{***}	-0.175^{***}	-0.174^{***}
	(0.043)	(0.043)	(0.028)	(0.028)
Control vars.				
age	3.363^{***}	3.320^{***}	2.632^{***}	2.586^{***}
	(0.279)	(0.278)	(0.191)	(0.191)
age^2	-0.038^{***}	-0.038^{***}	-0.031^{***}	-0.031^{***}
	(0.003)	(0.003)	(0.002)	(0.002)
married	0.995^{*}	0.984^{*}	0.755^{**}	0.743^{**}
	(0.401)	(0.400)	(0.286)	(0.286)
metro	2.538^{**}	2.536^{**}	1.403	1.398
	(0.983)	(0.984)	(0.798)	(0.800)
union	1.426^{***}	1.425^{***}	1.364^{***}	1.366^{***}
	(0.304)	(0.304)	(0.219)	(0.219)
Education				
Less than highschool	-0.315		-0.287	
	(0.896)		(0.555)	
Some college	-0.521		0.195	
	(0.977)		(0.572)	
Bachelor´s degree	0.431		1.195	
	(1.228)		(0.802)	
Grad. college	0.816		1.745^{*}	
	(1.313)		(0.866)	
tenure	0.920^{***}	0.902^{***}	1.350^{***}	1.331^{***}
	(0.158)	(0.158)	(0.109)	(0.109)
tenure^2	-0.033^{***}	-0.032^{***}	-0.039^{***}	-0.039^{***}
	(0.004)	(0.004)	(0.003)	(0.003)
educ		-2.529^{*}		-2.210^{**}
		(1.048)		(0.672)
educ^2		0.115^{**}		0.108^{***}
		(0.045)		(0.029)
Constant	143.392^{***}	156.718^{***}	145.278^{***}	156.223^{***}
	(5.757)	(8.394)	(4.022)	(5.621)
Obs.	670,129	670,033	1,305,715	1,305,475
R^2 (overall)	0.121	0.147	0.140	0.177
No. of groups	24,241	24,236	47,787	47,775
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	27.64	27.65	27.32	27.33
Obs. per group (max)	36	36	36	36

Dep. variable: log real wages $\times$ 100 (deflated with CPI)
Notes: Columns (1) and (2) include only men aged 18 to 64, while columns (3) and (4) include both sexes. All columns only include observations from job spells lasting more than a year. New hires are those workers with a tenure of less than one year. Match fixed effects used for all specifications. Obs. are weighted by SIPP full panel weights adjusted by average relative number of obs. per panel and by fraction of working hours of particular job spell.

Table B.4.: Gertler and Trigari (2009) replication results, job FE (monthly data, weighted)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
Business cycle				
u_t	-0.427^{***}	-0.432^{***}	-0.284^{***}	-0.287^{***}
	(0.072)	(0.072)	(0.051)	(0.051)
newhire $\times$ u_t	-0.264^{***}	-0.262^{***}	-0.259^{***}	-0.256^{***}
	(0.030)	(0.030)	(0.021)	(0.021)
Control vars.				
age	5.522^{***}	5.412^{***}	4.924^{***}	4.770^{***}
	(0.210)	(0.211)	(0.150)	(0.151)
age^2	-0.062^{***}	-0.060^{***}	-0.052^{***}	-0.051^{***}
	(0.003)	(0.003)	(0.002)	(0.002)
married	1.124^{**}	1.111^{**}	0.801^{**}	0.786^{**}
	(0.363)	(0.363)	(0.257)	(0.257)
metro	1.506^{*}	1.516^{*}	2.113^{***}	2.102^{***}
	(0.713)	(0.713)	(0.549)	(0.549)
union	2.161^{***}	2.162^{***}	2.037^{***}	2.039^{***}
	(0.257)	(0.257)	(0.187)	(0.187)
Education				
Less than highschool	-1.536^{*}		-1.148^{*}	
	(0.706)		(0.499)	
Some college	-1.346		-0.066	
	(0.764)		(0.506)	
Bachelor´s degree	1.269		1.900^{**}	
	(0.983)		(0.671)	
Grad. college	1.524		2.482^{***}	
	(1.048)		(0.718)	
educ		-3.605^{***}		-3.872^{***}
		(0.904)		(0.634)
educ^2		0.171^{***}		0.196^{***}
		(0.037)		(0.026)
Constant	102.035^{***}	120.873^{***}	95.881^{***}	115.508^{***}
	(4.395)	(7.166)	(3.156)	(5.124)
Obs.	189,655	189,619	368,911	368,836
R^2 (overall)	0.159	0.189	0.117	0.170
No. of groups	26,653	26,646	52,222	52,207
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	7.116	7.116	7.064	7.065
Obs. per group (max)	9	9	9	9

Dep. variable: log real wages $\times$ 100 (deflated with CPI)
Notes: Columns (1) and (2) include only men aged 18 to 64, while columns (3) and (4) include both sexes. All columns only include observations from job spells lasting more than a year. New hires are those workers with a tenure of less than one year. Person fixed effects used for all specifications.

Table B.5.: Gertler and Trigari (2009) replication results, person FE (wavely data, unweighted)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
Business cycle				
u_t	-0.442^{***}	-0.443^{***}	-0.301^{***}	-0.301^{***}
	(0.100)	(0.100)	(0.071)	(0.071)
newhire $\times$ u_t	-0.299^{***}	-0.297^{***}	-0.282^{***}	-0.280^{***}
	(0.049)	(0.049)	(0.032)	(0.032)
Control vars.				
age	5.607^{***}	5.509^{***}	5.002^{***}	4.850^{***}
	(0.302)	(0.301)	(0.214)	(0.214)
age^2	-0.062^{***}	-0.061^{***}	-0.053^{***}	-0.051^{***}
	(0.004)	(0.004)	(0.003)	(0.003)
married	1.406^{**}	1.389^{**}	1.086^{**}	1.063^{**}
	(0.482)	(0.481)	(0.351)	(0.351)
metro	2.000	2.004	2.310^{*}	2.301^{*}
	(1.282)	(1.283)	(1.037)	(1.038)
union	2.167^{***}	2.167^{***}	2.052^{***}	2.055^{***}
	(0.365)	(0.365)	(0.266)	(0.266)
Education				
Less than highschool	-1.367		-1.045	
	(0.976)		(0.627)	
Some college	-1.557		0.070	
	(1.007)		(0.629)	
Bachelor´s degree	0.919		2.376^{**}	
	(1.382)		(0.921)	
Grad. college	1.291		2.976^{**}	
	(1.485)		(0.992)	
educ		-3.224^{**}		-3.977^{***}
		(1.196)		(0.790)
educ^2		0.151^{**}		0.198^{***}
		(0.053)		(0.035)
Constant	97.490^{***}	114.638^{***}	92.076^{***}	112.739^{***}
	(6.406)	(9.187)	(4.604)	(6.333)
Obs.	169,459	169,433	330,866	330,802
R^2 (overall)	0.154	0.181	0.117	0.163
No. of groups	21,965	21,960	43,369	43,357
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	7.715	7.716	7.629	7.630
Obs. per group (max)	9	9	9	9

Dep. variable: log real wages $\times$ 100 (deflated with CPI)
Notes: Columns (1) and (2) include only men aged 18 to 64, while columns (3) and (4) include both sexes. All columns only include observations from job spells lasting more than a year. New hires are those workers with a tenure of less than one year. Person fixed effects used for all specifications. Obs. are weighted by SIPP full panel weights adjusted by average relative number of obs. per panel.

Table B.6.: Gertler and Trigari (2009) replication results, person FE (wavely data, weighted)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
Business cycle				
u_t	-0.355^{***}	-0.357^{***}	-0.232^{***}	-0.233^{***}
	(0.072)	(0.072)	(0.051)	(0.051)
newhire $\times$ u_t	-0.190^{***}	-0.188^{***}	-0.145^{***}	-0.144^{***}
	(0.033)	(0.033)	(0.023)	(0.023)
Control vars.				
age	3.550^{***}	3.502^{***}	2.673^{***}	2.610^{***}
	(0.275)	(0.276)	(0.193)	(0.193)
age^2	-0.042^{***}	-0.041^{***}	-0.034^{***}	-0.033^{***}
	(0.003)	(0.003)	(0.002)	(0.002)
married	0.940^{**}	0.933^{**}	0.604^{*}	0.593^{*}
	(0.360)	(0.360)	(0.255)	(0.255)
metro	2.045^{**}	2.037^{**}	1.396^{*}	1.382^{*}
	(0.759)	(0.759)	(0.589)	(0.589)
union	1.488^{***}	1.486^{***}	1.359^{***}	1.360^{***}
	(0.254)	(0.254)	(0.184)	(0.184)
Education				
Less than highschool	-0.897		-0.810	
	(0.694)		(0.491)	
Some college	-0.468		0.258	
	(0.754)		(0.499)	
Bachelor´s degree	0.769		1.163	
	(0.968)		(0.661)	
Grad. college	0.564		1.397^{*}	
	(1.033)		(0.707)	
tenure	1.125^{***}	1.099^{***}	1.603^{***}	1.575^{***}
	(0.188)	(0.188)	(0.132)	(0.132)
tenure^2	-0.032^{***}	-0.032^{***}	-0.038^{***}	-0.038^{***}
	(0.004)	(0.004)	(0.003)	(0.003)
educ		-2.908^{**}		-2.849^{***}
		(0.924)		(0.645)
educ^2		0.133^{***}		0.138^{***}
		(0.038)		(0.026)
Constant	141.046^{***}	156.349^{***}	145.816^{***}	160.123^{***}
	(6.319)	(8.568)	(4.551)	(6.133)
Obs.	191,451	191,415	373,096	373,021
R^2 (overall)	0.146	0.172	0.150	0.195
No. of groups	29,323	29,316	57,527	57,512
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	6.529	6.529	6.486	6.486
Obs. per group (max)	9	9	9	9

Dep. variable: log real wages $\times$ 100 (deflated with CPI)
Notes: Columns (1) and (2) include only men aged 18 to 64, while columns (3) and (4) include both sexes. All columns only include observations from job spells lasting more than a year. New hires are those workers with a tenure of less than one year. Match fixed effects used for all specifications.

Table B.7.: Gertler and Trigari (2009) replication results, job FE (wavely data, unweighted)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
Business cycle				
u_t	-0.356^{***}	-0.356^{***}	-0.237^{***}	-0.237^{***}
	(0.094)	(0.094)	(0.066)	(0.066)
newhire $\times$ u_t	-0.248^{***}	-0.247^{***}	-0.183^{***}	-0.182^{***}
	(0.050)	(0.050)	(0.032)	(0.032)
Control vars.				
age	3.310^{***}	3.274^{***}	2.504^{***}	2.447^{***}
	(0.341)	(0.342)	(0.238)	(0.239)
age^2	-0.040^{***}	-0.039^{***}	-0.032^{***}	-0.031^{***}
	(0.004)	(0.004)	(0.003)	(0.003)
married	1.269^{**}	1.261^{**}	0.829^{*}	0.812^{*}
	(0.452)	(0.452)	(0.322)	(0.322)
metro	2.692^{*}	2.691^{*}	1.607	1.598
	(1.099)	(1.100)	(0.896)	(0.897)
union	1.500^{***}	1.498^{***}	1.347^{***}	1.348^{***}
	(0.322)	(0.322)	(0.235)	(0.235)
Education				
Less than highschool	-0.335		-0.474	
	(0.940)		(0.603)	
Some college	-0.584		0.284	
	(1.029)		(0.604)	
Bachelor´s degree	0.524		1.382	
	(1.314)		(0.858)	
Grad. college	0.241		1.544	
	(1.414)		(0.940)	
tenure	1.225^{***}	1.206^{***}	1.687^{***}	1.663^{***}
	(0.220)	(0.220)	(0.154)	(0.154)
tenure^2	-0.033^{***}	-0.033^{***}	-0.040^{***}	-0.039^{***}
	(0.005)	(0.005)	(0.004)	(0.004)
educ		-2.430^{*}		-2.765^{***}
		(1.043)		(0.692)
educ^2		0.106^{*}		0.131^{***}
		(0.045)		(0.030)
Constant	144.054^{***}	157.531^{***}	147.739^{***}	162.150^{***}
	(7.484)	(9.621)	(5.379)	(6.705)
Obs.	170,428	170,402	332,849	332,785
R^2 (overall)	0.139	0.160	0.153	0.192
No. of groups	24,232	24,227	47,765	47,753
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	7.033	7.034	6.968	6.969
Obs. per group (max)	9	9	9	9

Dep. variable: log real wages $\times$ 100 (deflated with CPI)
Notes: Columns (1) and (2) include only men aged 18 to 64, while columns (3) and (4) include both sexes. All columns only include observations from job spells lasting more than a year. New hires are those workers with a tenure of less than one year. Match fixed effects used for all specifications. Obs. are weighted by SIPP full panel weights adjusted by average relative number of obs. per panel and by fraction of working hours of particular job spell.

Table B.8.: Gertler and Trigari (2009) replication results, job FE (wavely data, weighted)

C. Detailed results from replicating Devereux (2001)

	(1)	(2)	(3)	(4)
Dep. var.:	dlrwage	dlrwage	dlrwage	dlrwage
exper	-0.7439^{***}	-0.7469^{***}	-0.4224^{***}	-0.4188^{***}
	(0.0343)	(0.0344)	(0.0371)	(0.0361)
exper^2	0.0209^{***}	0.0210^{***}	0.0121^{***}	0.0118^{***}
	(0.0017)	(0.0017)	(0.0018)	(0.0017)
exper^3	-0.0002^{***}	-0.0002^{***}	-0.0001^{***}	-0.0001^{***}
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
tenure			-0.3227^{***}	-0.3263^{***}
			(0.0494)	(0.0475)
tenure^2			0.0099^{**}	0.0101^{**}
			(0.0036)	(0.0035)
tenure^3			-0.0001	-0.0001
			(0.0001)	(0.0001)
Obs.	1,054,200	1,034,203	846,981	821,017
R^2	0.010	0.011	0.008	0.008
F-stat	42.61	42.54	29.04	27.75

Dep. variable: differenced log real wages × 100 (deflated with CPI)
Column (1) uses observations from all males aged 18–64 who work in the nonfarm business sector. In (2) all self-employed are excluded. In (3) the sample is further restricted to stayers only, while in (4) only stayers without additional jobs are used. Coefficients of time dummies not shown.

Table C.1.: Replication of Table 1 in Devereux (2001), stage 1 of 2-stage method

	(1)	(2)	(3)	(4)
Dep. var.:	dlrwage	dlrwage	dlrwage	dlrwage
exper	-0.7400^{***}	-0.7439^{***}	-0.4092^{***}	-0.4051^{***}
	(0.0334)	(0.0333)	(0.0415)	(0.0406)
exper^2	0.0209^{***}	0.0210^{***}	0.0117^{***}	0.0114^{***}
	(0.0017)	(0.0017)	(0.0021)	(0.0021)
exper^3	-0.0002^{***}	-0.0002^{***}	-0.0001^{***}	-0.0001^{***}
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
tenure			-0.3433^{***}	-0.3468^{***}
			(0.0506)	(0.0488)
tenure^2			0.0111^{**}	0.0113^{**}
			(0.0036)	(0.0035)
tenure^3			-0.0001	-0.0001
			(0.0001)	(0.0001)
Δu_t	-0.6609^{***}	-0.6840^{***}	-0.2854	-0.2081
	(0.1770)	(0.1735)	(0.1761)	(0.1741)
trend	0.0069^{***}	0.0066^{***}	0.0049^{**}	0.0048^{*}
	(0.0019)	(0.0019)	(0.0018)	(0.0018)
Constant	6.2456^{***}	6.4345^{***}	5.3449^{***}	5.3966^{***}
	(0.9449)	(0.9379)	(0.8439)	(0.8605)
Obs.	1,054,200	1,034,203	846,981	821,017
R^2	0.004	0.004	0.003	0.003
F-stat	307.7	307.2	187.8	163.8

Dep. variable: differenced log real wages × 100 (deflated with CPI)
Column (1) uses observations from all males aged 18–64 who work in the nonfarm business sector. In (2) all self-employed are excluded. In (3) the sample is further restricted to stayers only, while in (4) only stayers without additional jobs are used. One-stage estimation method with errors clustered by time.

Table C.2.: Replication of Table 1 in Devereux (2001), 1-stage method

	(1)	(2)	(3)
Dep. var.:	dlrwage	dlrwage	dlearn
exper	-0.2988^{***}	-0.3160^{***}	-1.3556^{***}
	(0.0303)	(0.0315)	(0.0763)
exper^2	0.0079^{***}	0.0087^{***}	0.0497^{***}
	(0.0015)	(0.0015)	(0.0036)
exper^3	-0.0001^{***}	-0.0001^{***}	-0.0006^{***}
	(0.0000)	(0.0000)	(0.0000)
tenure	-0.6954^{***}	-0.6803^{***}	-1.3722^{***}
	(0.0328)	(0.0337)	(0.0732)
tenure^2	0.0318^{***}	0.0304^{***}	0.0763^{***}
	(0.0021)	(0.0022)	(0.0047)
tenure^3	-0.0004^{***}	-0.0004^{***}	-0.0012^{***}
	(0.0000)	(0.0000)	(0.0001)
Obs.	474,230	450,802	475,617
R^2	0.016	0.015	0.016
F-stat	30.80	28.11	34.96

Dep. variable: differenced log real wages × 100 (deflated with CPI) in columns (1) and (2); differenced log real total monthly earnings in column (3).
Notes: Column (1) gives results for stayers paid on an hourly basis. Column (2) excludes workers from (1) with more than one job. Column (3) uses total earnings as the dependent variable in stage 1. Coefficients of time dummies not shown.

Table C.3.: Replication of Table 2 in Devereux (2001), stage 1 of 2-stage method

D. Detailed results from replicating Haefke et al. (2008)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
newhire × lprod	0.0332***	0.0258***	0.0190***	0.0227***
	(0.0051)	(0.0041)	(0.0044)	(0.0040)
lprod	0.7535***	0.7429***	0.7294***	0.6391***
	(0.0157)	(0.0128)	(0.0130)	(0.0123)
newhire	−0.1729***	−0.1396***	−0.1119***	−0.1281***
	(0.0224)	(0.0183)	(0.0192)	(0.0175)
exper	0.0456***	0.0453***	0.0444***	0.0370***
	(0.0008)	(0.0006)	(0.0006)	(0.0006)
exper^2	−0.0016***	−0.0016***	−0.0016***	−0.0012***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
exper^3	0.0000***	0.0000***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
educ	0.0443***	0.0346***	0.0314***	0.0092
	(0.0110)	(0.0089)	(0.0091)	(0.0081)
educ^2	−0.0069***	−0.0059***	−0.0053***	−0.0023**
	(0.0010)	(0.0008)	(0.0008)	(0.0007)
educ^3	0.0003***	0.0003***	0.0003***	0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
union	0.0716***	0.0709***	0.0636***	0.0766***
	(0.0012)	(0.0010)	(0.0010)	(0.0010)
married	0.0200***	0.0188***	0.0178***	0.0146***
	(0.0015)	(0.0012)	(0.0012)	(0.0012)
metro	0.0275***	0.0264***	0.0273***	0.0288***
	(0.0022)	(0.0018)	(0.0019)	(0.0017)
Constant	−1.2378***	−1.1543***	−1.0674***	−0.7235***
	(0.0772)	(0.0627)	(0.0636)	(0.0586)
Obs.	2,106,275	2,086,930	1,908,007	1,207,491
R^2 (overall)	0.189	0.213	0.207	0.156
No. of groups	291,775	290,544	284,896	211,618
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	7.219	7.183	6.697	5.706
Obs. per group (max)	17	17	17	17

Dep. variable: log real wages × 100 (deflated with the GDP deflator)
Column (1) uses obs. of workers aged 18 to 64 in nonfarm business sector who are not self-employed. Column (2) excludes outliers (with wage below (above) the 0.5 (99.5) percentile. In column (3) the sample is further restricted to include workers with only one concurrent job, while in column (4) only hourly workers are considered.
Quarterly data are created by using the average of observations belonging to the last observed primary job each quarter.
Marginal effects for (1): educ: 3.28% (at sample mean 13.32); exper: 0.80% (at sample mean 18.41)

Table D.1.: Wage cyclicality w.r.t. productivity (quarterly, unweighted)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
newhire × lprod	0.0127*	0.0190***	0.0140**	0.0203***
	(0.0055)	(0.0044)	(0.0046)	(0.0039)
lprod	0.7343***	0.7100***	0.6698***	0.6075***
	(0.0171)	(0.0136)	(0.0139)	(0.0123)
newhire	−0.0846***	−0.1106***	−0.0894***	−0.1154***
	(0.0244)	(0.0194)	(0.0204)	(0.0173)
exper	0.0456***	0.0452***	0.0437***	0.0360***
	(0.0008)	(0.0006)	(0.0007)	(0.0006)
exper^2	−0.0016***	−0.0016***	−0.0015***	−0.0012***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
exper^3	0.0000***	0.0000***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
educ	0.0315**	0.0220*	0.0224*	−0.0008
	(0.0120)	(0.0095)	(0.0097)	(0.0080)
educ^2	−0.0056***	−0.0047***	−0.0046***	−0.0014
	(0.0010)	(0.0008)	(0.0008)	(0.0007)
educ^3	0.0003***	0.0002***	0.0002***	0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
union	0.0729***	0.0731***	0.0674***	0.0822***
	(0.0014)	(0.0011)	(0.0011)	(0.0010)
married	0.0226***	0.0211***	0.0200***	0.0154***
	(0.0016)	(0.0013)	(0.0013)	(0.0012)
metro	0.0249***	0.0244***	0.0248***	0.0262***
	(0.0024)	(0.0019)	(0.0020)	(0.0017)
Constant	−1.1318***	−0.9872***	−0.7995***	−0.5635***
	(0.0840)	(0.0665)	(0.0679)	(0.0587)
Obs.	2,115,130	2,095,432	1,961,130	1,217,505
R^2 (overall)	0.189	0.221	0.225	0.170
No. of groups	292,352	291,240	283,729	210,217
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	7.235	7.195	6.912	5.792
Obs. per group (max)	17	17	17	17

Dep. variable: log real wages × 100 (deflated with the GDP deflator)
Column (1) uses obs. of workers aged 18 to 64 in nonfarm business sector who are not self-employed. Column (2) excludes outliers (with wage below (above) the 0.5 (99.5) percentile. In column (3) the sample is further restricted to include workers with only one concurrent job, while in column (4) only hourly workers are considered.
Quarterly data are created by using the last monthly observation of the primary job wage each quarter.
Marginal effects for (1): educ: 3.34% (at sample mean 13.32); exper: 0.86% (at sample mean 18.38)

Table D.2.: Wage cyclicality w.r.t. productivity (quarterly data from last monthly observation, unweighted)

	(1)	(2)	(3)	(4)
Dep. var.:	lrwage	lrwage	lrwage	lrwage
newhire × lprod	0.0453**	0.0472***	0.0404**	0.0297*
	(0.0149)	(0.0126)	(0.0130)	(0.0122)
lprod	0.7845***	0.7475***	0.7093***	0.6033***
	(0.0382)	(0.0311)	(0.0317)	(0.0295)
newhire	−0.2279***	−0.2344***	−0.2051***	−0.1571**
	(0.0650)	(0.0551)	(0.0566)	(0.0533)
exper	0.0466***	0.0452***	0.0438***	0.0372***
	(0.0023)	(0.0019)	(0.0019)	(0.0018)
exper^2	−0.0016***	−0.0015***	−0.0015***	−0.0012***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
exper^3	0.0000***	0.0000***	0.0000***	0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
educ	0.0235	0.0098	0.0137	−0.0082
	(0.0300)	(0.0280)	(0.0270)	(0.0302)
educ^2	−0.0059*	−0.0046	−0.0046	−0.0014
	(0.0028)	(0.0026)	(0.0025)	(0.0028)
educ^3	0.0003***	0.0003***	0.0003***	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
union	0.0598***	0.0615***	0.0548***	0.0631***
	(0.0036)	(0.0031)	(0.0032)	(0.0033)
married	0.0225***	0.0229***	0.0216***	0.0195***
	(0.0041)	(0.0036)	(0.0035)	(0.0037)
metro	0.0297***	0.0271***	0.0248***	0.0263***
	(0.0079)	(0.0066)	(0.0066)	(0.0060)
Constant	−1.3054***	−1.0995***	−0.9384***	−0.5443***
	(0.1899)	(0.1616)	(0.1606)	(0.1612)
Obs.	1,283,034	1,271,445	1,198,598	725,923
R^2 (overall)	0.199	0.232	0.235	0.184
No. of groups	125190	124888	123544	92760
Obs. per group (min)	1	1	1	1
Obs. per group (avg)	10.25	10.18	9.702	7.826
Obs. per group (max)	17	17	17	17

Dep. variable: log real wages × 100 (deflated with the GDP deflator)
Column (1) uses obs. of workers aged 18 to 64 in nonfarm business sector who are not self-employed. Column (2) excludes outliers (with wage below (above) the 0.5 (99.5) percentile. In column (3) the sample is further restricted to include workers with only one concurrent job, while in column (4) only hourly workers are considered.
Quarterly data are created by using the last monthly observation of the primary job wage each quarter.
Observations are weighted with adjusted full panel weights.

Table D.3.: Wage cyclicality w.r.t. productivity (quarterly data from last monthly observation, weighted)