Measuring What Employers Do about Entry Wages over the Business Cycle:
A New Approach

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Abstract: Rigidity in real hiring wages plays a crucial role in some recent macroeconomic models. But are hiring wages really so noncyclical? We propose using employer/employee longitudinal data to track the cyclical variation in the wages paid to workers newly hired into specific entry jobs. Illustrating the methodology with 1982-2008 data from the Portuguese census of employers, we find real entry wages were about 1.8 percent higher when the unemployment rate was one percentage point lower. Like most recent evidence on other aspects of wage cyclicality, our results suggest that the cyclical elasticity of wages is similar to that of employment.

At least since Keynes (1936), macroeconomists have theorized that wages are inflexible and that limited cyclical variability of wages may account for the cyclical volatility of employment and unemployment. The idea continues to figure prominently in the current literature. For example, the very first sentence of the recent *Journal of Political Economy* paper by Gertler and Trigari (2009, p. 38) says, "A long-standing challenge in macroeconomics is accounting for the relatively smooth behavior of real wages over the business cycle along with the relatively volatile behavior of employment."

Much of the current interest in the cyclical behavior of wages, especially hiring wages, has grown out of a debate about the ability of the canonical Mortensen-Pissarides

(1994) model to generate realistically large cyclical fluctuations in unemployment. In that model, wages within a worker/employer match are determined by Nash bargaining. The resulting procyclicality in real wages means that, during a recession, lowered wages give employers an incentive to hire the unemployed workers that have been laid off by other employers. A problematic implication, pointed out by Shimer (2005), is that, under standard parameter values, the model generates much smaller cyclical fluctuations in unemployment than actually do occur.

In keeping with the long macroeconomic tradition of accounting for large cyclical employment and unemployment fluctuations by assuming rigid wages, many recent theorists – such as Shimer (2004), Hall (2005), Hall and Milgrom (2008), Gertler and Trigari (2009), and Kennan (2010) – have suggested stickiness in real hiring wages as a way of modifying the Mortensen-Pissarides model to generate realistically large quantity fluctuations. Ultimately, however, whether this is the right way to go should be subject to empirical investigation. How much *do* real hiring wages actually vary over the business cycle?

Our literature review in section I concludes that we presently lack persuasive evidence on this question. In section II, we propose a new empirical approach – using matched employer/employee longitudinal data to identify firms' entry jobs and then tracking the cyclical variation in the real wages paid to workers newly hired into those jobs. In section III, we illustrate our approach with an application to data from the annual census of employers in Portugal. In section IV, we briefly summarize our findings, and we discuss their macroeconomic implications.

I. Existing Evidence on Cyclicality in Hiring Wages

Of course, there already is a great deal of evidence on real wage cyclicality in general. For example, using 1967-1987 data on workers in the U.S. Panel Study of Income Dynamics, Solon, Barsky, and Parker (1994) estimated that a one percentage point increase in the unemployment rate is associated with a 1.2 percent reduction in real wages. Numerous other studies using longitudinal microdata from the United States and

elsewhere have produced similar results.[1] But what do we know about the cyclicality of *hiring* wages in particular?

Pissarides (2009) has dismissed theories based on cyclically rigid hiring wages because, in his view, the evidence shows that hiring wages are quite procyclical. His view is based on a portion of the longitudinal literature that has found that workers who change employers during expansions tend to experience much better wage growth than workers who change employers during recessions. Examples of such studies include Bils (1985) and Shin (1994) for the United States; Devereux and Hart (2006) for the United Kingdom; and Martins (2007) and Carneiro, Guimarães, and Portugal (forthcoming) for Portugal. Similarly, Haefke, Sonntag, and van Rens (2008) find that individuals moving from nonemployment to employment during expansions tend to receive higher wages than individuals moving from nonemployment to employment during recessions.

Gertler and Trigari (2009), however, explain why such evidence does not rule out acyclical wage setting by firms. Even if every firm maintains cyclically rigid wages in each job title, workers taking new jobs will show procyclical wages if, during expansions, they face better opportunities to move into higher-paying industries, higher-paying firms within industries, or higher-paying jobs within firms. In contrast, during recessions, a larger share of job changers are displaced workers, who often suffer wage reductions relative to the jobs they have lost. As Gertler and Trigari (p. 73) put it, "Suppose, for example, that a highly skilled machinist takes a job as a low-paid cab driver in a recession and then is re-employed as a high-paid machinist in a boom. In this case there is a cyclical movement in job match quality for the individual" even if the wages in both the machinist job and the cab driver job are acyclical.[2] As Gertler and Trigari emphasize, this process of so-called "cyclical upgrading" in worker/employer matches (and cyclical *down*grading in recessions) has long been discussed and documented. Early references include Reynolds (1951), Reder (1955), Okun (1973), and Hall (1974). Recent analyses

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[1] See Solon, Barsky, and Parker (1992) for a review of most of the U.S. literature. More recent U.S. studies include Devereux (2001) and Shin and Solon (2007). Studies of other countries include Devereux and Hart (2006), Martins (2007), Carneiro, Guimarães, and Portugal (forthcoming), Peng and Siebert (2007, 2008), and Shin (2012)..

[2] Bewley (1999, p. 151) earlier made the exact same point: "the findings could result from the effects of recession on the level of jobs accepted during recessions rather than from the effects on hiring pay for particular jobs."

include Solon, Whatley, and Stevens (1997), McLaughlin and Bils (2001), Devereux (2004), and Bjelland, Fallick, Haltiwanger, and McEntarfer (2011).

We should note that, from the workers' perspective, their improved opportunities for moving to better jobs in an expansion are a true form of wage procyclicality. But that is not what theoretical analyses such as Hall (2005) are about. Rather, those analyses are attempting to explain the amplitude of cyclical fluctuations in unemployment by assuming rigidity in the hiring wages that particular firms pay in particular jobs. Gertler and Trigari therefore are correct in criticizing the existing empirical literature for rejecting such theories on the basis of evidence confounded by cyclical upgrading/downgrading.

To summarize, we agree with Gertler and Trigari's point that empirical assessment of recent theories of hiring-wage rigidity requires an approach that identifies cyclical variation in hiring wages within particular jobs. Towards that end, in the next section we propose an approach directly based on tracking hiring wages in particular jobs.

In this same *Journal*, in a different approach to the same Portuguese data base that we use, Carneiro, Guimarães, and Portugal (forthcoming) estimate worker-level regressions of log wages on the unemployment rate, a "new-hire" dummy variable that equals 1 if the worker's tenure is less than 12 months, the dummy's interaction with the unemployment rate, and controls including worker, firm, and occupation fixed effects. A complication is that, with multiple dimensions of fixed effects, it is difficult to discern which within-group variation is being used to identify what. For recent studies that express concerns about the economic interpretation of wage regressions with both worker and firm fixed effects, see Abowd, McKinney, and Schmutte (2010) and the references therein. Our study complements the one by Carneiro, Guimarães, and Portugal by providing relatively transparent evidence based on tracking hiring wages in particular entry jobs over recessions and expansions. As it turns out, their results and ours are not radically different. Like our study and nearly every other longitudinal study of wage cyclicality, Carneiro, Guimarães, and Portugal find that real wages are quite procyclical. They also estimate that hiring wages are about 20 percent more procyclical than the wages of incumbent workers.

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II. A New Approach

The alternative approach we propose is to identify specific entry jobs within particular firms, track the wages paid to newly hired workers in those jobs, and measure how those entry wages vary over the business cycle. A precedent for this approach was applied by Solon, Whatley, and Stevens (1997, p. 412), who found that "the entry wage for 'laborers,' the most common occupation for newly hired workers" in the Byers iron tubing and pipe manufacturing company, was procyclical over the 1919-1932 period. That result, however, applied to only one company observed over 70 years ago. In the present paper, we take a similar approach with a large number of firms observed between 1982 and 2008 in an annual census of employers in Portugal. Because we hope that our approach eventually will be applied to other countries besides Portugal, here we describe the general methodology. Idiosyncrasies that arise in the application to the Portuguese data set will be addressed in section III.

Let w_{jt} denote the typical real wage paid in period t to workers newly hired into job j, which is a particular job into which a particular firm does a substantial amount of its hiring of new employees. A convenient statistical model for w_{jt} is

(1) $\qquad \log w_{jt} = \alpha_j + \beta_t + \varepsilon_{jt}$

where α_j is a job fixed effect, β_t is a period fixed effect common to all entry jobs, and the zero-mean error term ε_{jt} represents temporary job-specific departures from the general period effect. The object of our analysis is to estimate a time series for β_t and relate it to business cycle conditions.

If the data set were a "balanced panel" with every one of the N sampled jobs observed in all periods, the estimation of β_t would be particularly simple. With the mean of α_j normalized to zero, the least squares estimator of each β_t would be simply

(2) $\qquad \hat{\beta}_t = \sum_{j=1}^{N} \log w_{jt} / N$.

A figure that plotted the $\hat{\beta}_t$ time series along with a cycle indicator such as the unemployment rate would give an eyeball impression of the cyclical behavior of entry

wages. A quantitative summary could be obtained by applying least squares to the regression of $\hat{\beta}_t$ on the unemployment rate with controls for secular time trends. If non-stationarity were a concern, the regression could be estimated in first differences instead of levels.

If the panel is unbalanced because observations of w_{jt} are missing for some jobs in some years, it is still straightforward to estimate β_t in equation (1) by applying least squares to the regression of the available observations of $\log w_{jt}$ on a vector of period dummy variables (for which the estimated coefficients are the $\hat{\beta}_t$ series) and a vector of job dummies. Alternatively, exactly the same $\hat{\beta}_t$ are obtained by applying least squares to the "de-meaned" regression of $\log w_{jt}$ on a vector of period dummies, in which each variable is expressed as a deviation from the job's time mean of the variable over the periods in which that job is observed. Either way of calculating $\hat{\beta}_t$ accounts for period-to-period changes in which jobs are in the sample by controlling for job fixed effects. This $\hat{\beta}_t$ series is the same as the sample means in equation (2) except that it is regression-adjusted for changes in sample composition. Again, the $\hat{\beta}_t$ series can be plotted along with the unemployment rate, and it can be regressed on the unemployment rate and time trend variables.

Although complications inevitably arise in practice (as will be seen in the next section), the basic methodology is almost absurdly simple. That is what we like about it. Our method looks in a direct and transparent way at what employers actually do about entry wages over the business cycle.

III. An Application to Matched Firm/Worker Data from Portugal

A. Description of the data

In this section, we illustrate our method for measuring entry-wage cyclicality with an application to Portuguese data from 1982-2008. As shown in figure 1, Portugal's annual unemployment rate varied widely over that period, with peaks in the mid 1980s, mid 1990s, and mid 2000s. Another relevant feature of the Portuguese labor market over

that period is its system of collective wage bargaining, which is explained in detail in Cardoso and Portugal (2005). The main message of that paper is that the bargained wage rates function as wage *floors*, with employers commonly paying wages above those floors. In the authors' words (p. 899), "the wage cushion works as a mechanism to overcome the constraints imposed by collective bargaining, allowing firms wide scope for action in their wage-setting policy…. This operation of institutional forces and market forces in the Portuguese economy may help explain why a typically European institutional framework is compatible with high wage flexibility and low unemployment."

Figure 1. Annual Unemployment Rates in Portugal, 1982-2008
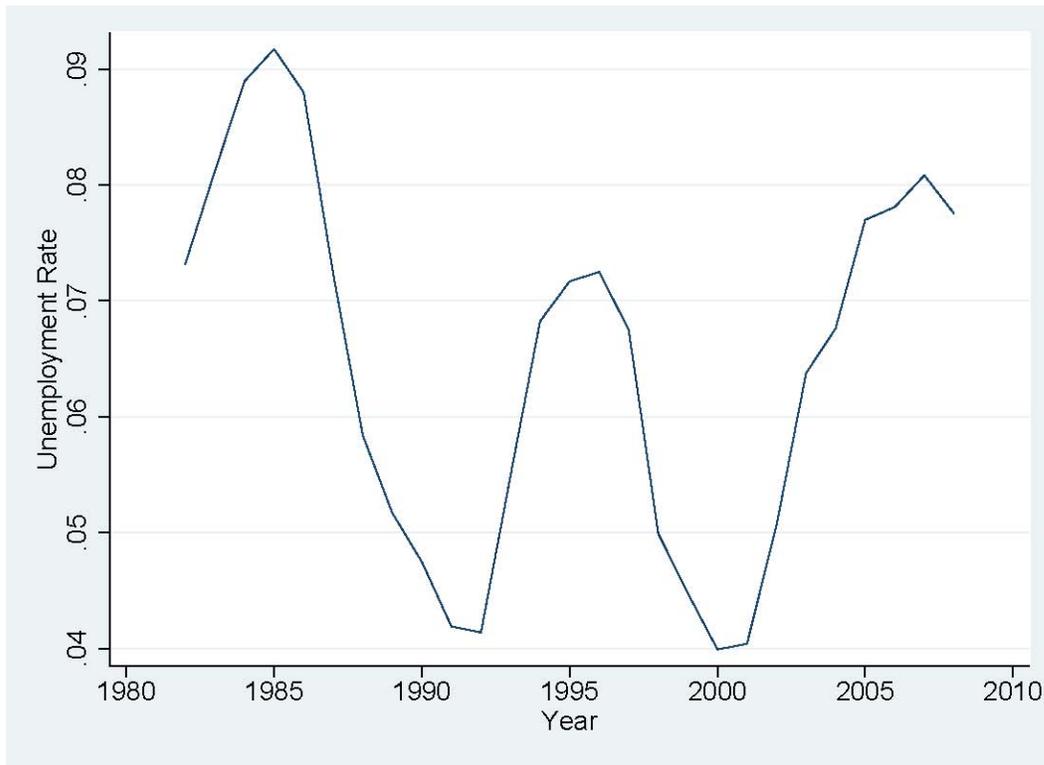
Our data come from Quadros de Pessoal, an annual mandatory census of all employers in Portugal (except that most of the public sector is excluded). The census was conducted in March in each year through 1993 and in October from 1994 on. Employer identification numbers enable longitudinal matching of employers. An unusual and valuable feature of this data base is that information on every individual employed by

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the firm as of the census date is available for every year between 1982 and 2008 except 1990 and 2001. The employee information includes current wage rate, detailed occupation code, and tenure with the firm. Employee identification numbers enable longitudinal matching of individual workers, even when they change employers, from 1986 on.

The data base suits our purposes very well. The combination of occupation and tenure information for every employee enables us to identify jobs into which each firm commonly recruits new employees, and we also observe the wages that the new employees in those jobs are paid. And by tracking those entry-job wages longitudinally, we can observe how they vary over the business cycle.

To identify a set of jobs into which, year after year, employers are observed to hire new workers, we apply a series of sample selection criteria. In our initial set of selection criteria, we first consider only firms that employed at least 50 workers in at least five years of the 1982-2008 period. Second, we define jobs within firms in terms of five-digit occupation codes (such as supermarket shelf stocker, bank teller, clothing machine operator). When the same occupation code appears for different firms, we treat each occurrence as a different occupation/firm job. We link the occupation codes used from 1995 on with a different system of codes used through 1994, but, in the process, we lose some jobs for which the pre-1995 codes branch out into multiple post-1995 codes. We also require that all workers in the job are at the same "job level," an eight-category variable coded as (1) apprentices, interns, trainees; (2) non-skilled professionals; (3) semi-skilled professionals; … (8) top executives (top management). Third, defining newly hired workers as those with no more than four months of tenure with the firm, we include a job in our main sample of entry jobs if, in at least half the years the firm is present in the data base, the particular job accounted for at least three new hires and at least 10 percent of the firm's new hires in that year. Of course, this particular set of selection criteria is quite arbitrary, and we later explore the robustness of our results to varying the criteria.

By applying such criteria, we are focusing on what Doeringer and Piore (1971) called "port-of-entry" jobs. We do not mean, however, to subscribe to their stark description in which firms hire into only a limited number of such jobs, with other jobs

filled almost exclusively by internal promotions and reassignments. More recent studies – such as the case study of a particular firm by Baker, Gibbs, and Holmstrom (1994) – have suggested that hiring from outside a firm can take place in a broad spectrum of jobs. Our focus on jobs that recurrently show new hires in the annual employer census is driven mainly by a pragmatic concern – to identify cyclical variation in hiring wages by job, we need those wages to be observed in multiple years spanning different business cycle conditions.

The main sample resulting from our initial selection criteria consists of 1,345 jobs in 1,138 firms. The firms are spread across a wide variety of industries, with the most common being manufacturing, construction, retail trade, hotels and restaurants, education, and "other business activities" (including cleaning, security, and temporary work agencies). Our panel of wages for newly hired workers in entry jobs is "unbalanced," with a total of 10,213 job/years. The unbalancedness occurs because some firms begin or end between 1982 and 2008; because, in existing firms, it may happen that the workers observed in the job as of a particular census month include no one hired within the last four months; and because, as explained below, we sometimes are unable to identify a modal hiring wage.

Table 1 displays sample size statistics by year along with the seasonally adjusted unemployment rate for the census month.[3] The number of entry-job wages observed per year ranges from a low of 137 in 1982 to a high of 810 in 2006. The sample sizes trend upward partly because of growth in the Portuguese economy and because of a trend from informal to formal employment. In addition, our requirement that the firm appear with at least 50 employees in at least five years of the 1982-2008 period causes us to omit firms that became defunct in the early years of our sample period and firms that began in the last few years. These sample composition issues underscore the importance of our practice (described in the previous section) of accounting for year-to-year changes in sample composition by controlling for job fixed effects.

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[3] These are the Eurostat unemployment rates reported at http://sdw.ecb.europa.eu/quickview.do?SERIES_KEY=132.STS.M.PT.S.UNEH.RTT000.4.000 as of June 2009. As these go back only to 1983, we imputed a March 1982 value based on the annual unemployment rate from Statistics Portugal.

Table 1. Sample Sizes by Year

| Year	Number of Entry Jobs	Number of Newly Hired Workers	Seasonally Adjusted Unemployment Rate in Census Month
1982	137	2,770	7.32
1983	160	3,670	7.67
1984	137	2,830	8.85
1985	181	2,836	9.05
1986	210	3,135	9.31
1987	219	4,110	7.71
1988	231	4,187	6.14
1989	244	6,313	5.35
1991	238	6,535	4.20
1992	265	8,248	3.91
1993	269	7,406	5.06
1994	299	8,761	7.01
1995	345	9,777	7.15
1996	353	10,276	7.15
1997	378	12,335	6.68
1998	455	15,184	4.78
1999	469	14,525	4.26
2000	513	18,803	3.87
2002	577	18,485	5.67
2003	710	26,368	6.48
2004	790	22,864	7.04
2005	803	26,048	7.99
2006	810	24,426	8.00
2007	744	30,645	7.84
2008	676	29,091	7.84
Total	10,213	319,628	

The last row of table 1 shows a total of 319,628 newly hired workers observed in the 10,213 job/years, so on average more than 30 newly hired workers are observed per job/year. On account of procyclical hiring, the number of newly hired workers per observed entry job shows a clear tendency to be lower in years of high unemployment. Importantly, however, there is not a clear tendency for the number of observed entry jobs to decline in those years. If there were, we would need to worry that rigidity in hiring wages in some firms might be causing those firms to disappear from our sample in recession years because their hiring into their entry jobs dropped to zero. Instead, the pattern is that hiring declines but remains positive, so that we still observe hiring wages.[4] Note that, if we followed much of the related literature – such as Carneiro, Guimarães, and Portugal (forthcoming) – in treating workers rather than jobs as the unit of analysis (or, equivalently, if we weighted jobs by the number of newly hired workers), variation in the number of hires by job could cause problems of endogenous sample selection.

Obviously, our analysis pertains to only a small fraction of all the jobs and firms encompassed in the Portuguese census of employers. It does, however, focus on those jobs that are most clearly recognizable as perennial entry jobs. Perhaps it is best to view our study as a sort of summary of 1,345 case studies of particular entry jobs. To give a sense of who are the workers newly hired into those jobs, figure 2 displays annual statistics for mean years of schooling, mean age, and proportion female for the newly hired workers in our sample (i.e., the 2,770 sample workers in 1982, the 3,670 in 1983, etc.). For comparison, the figures also show the corresponding population statistics for all workers and all newly hired workers in the census of employers. Not surprisingly, the figures show that both our sample of newly hired workers and the population of newly hired workers are less educated, younger, and more frequently female than the overall population of workers. The most persistent difference between our sample workers and the population of newly hired workers is our sample's higher proportion female.

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[4] We thank David Card, Jonathan Guryan, and Steven Rivkin for raising this issue at the NBER Summer Institute.

Figure 2. Average Characteristics of Sample and Population Workers by Year
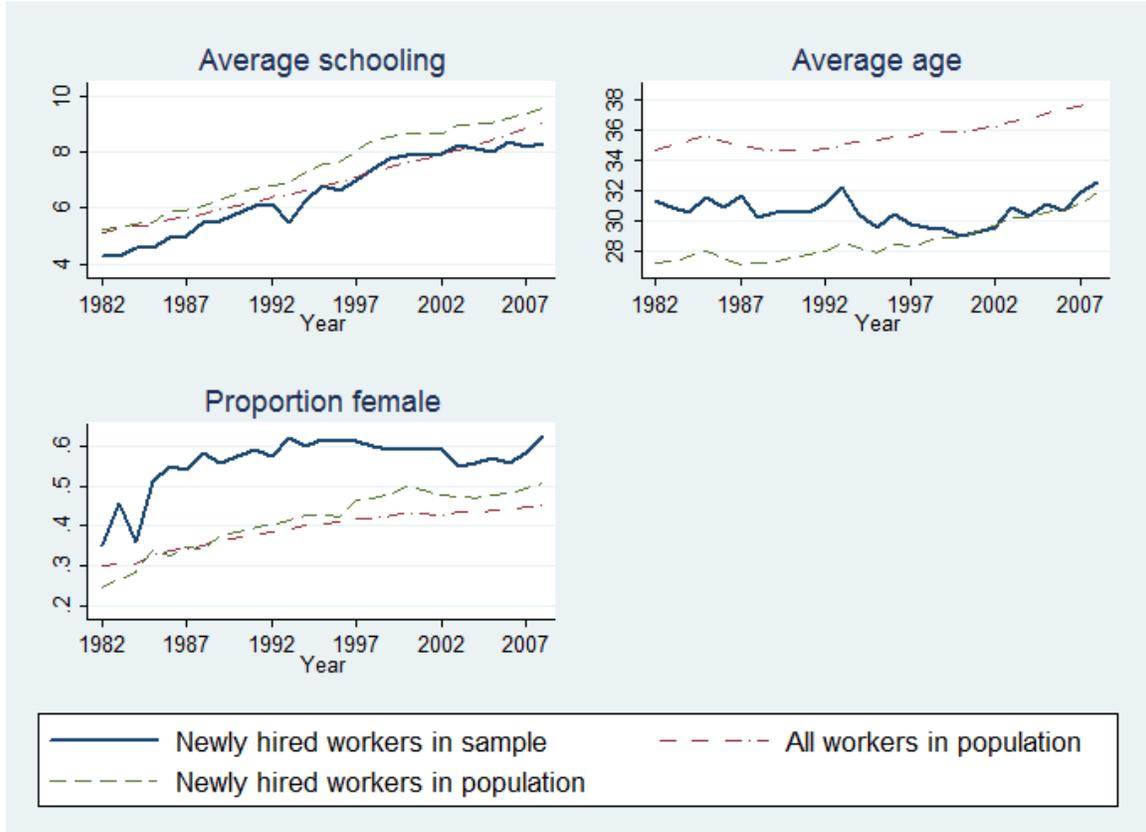
As explained in the previous section, the dependent variable in our first-stage regressions is the typical logarithmic real hiring wage in job j in period t. The wage measure we start with is each newly hired worker's monthly base pay ("corresponding to the normal hours of work") divided by the worker's normal monthly hours. This is the worker's base pay in the sense that it leaves out supplementary payments such as overtime, bonuses, and late-shift premia. We later explore the robustness of our results to including such supplements. We use Statistics Portugal's monthly consumer price index (for March through 1993, for October afterwards) to convert the wage to real terms. Our main measure of the *typical* log hiring wage in a job/year is the modal value (with log wages measured to two decimal places). As mentioned above, we have dropped job/years in which we did not identify a modal wage. This occurred because of ties, most often because the job/year contained only two newly hired workers who received different wages. Figure 3 displays, for the 319,628 newly hired workers in our sample,

the histogram of the difference between the worker's log wage and the modal log hiring wage in the job/year. As the figure shows, typically most of the newly hired workers in a job/year are paid at the modal rate. Nevertheless, we later explore the robustness of our results to using alternative measures of the typical hiring wage.

Figure 3. Sample Distribution of Difference between Individual Worker's Log Wage and Modal Log Hiring Wage in Job/Year
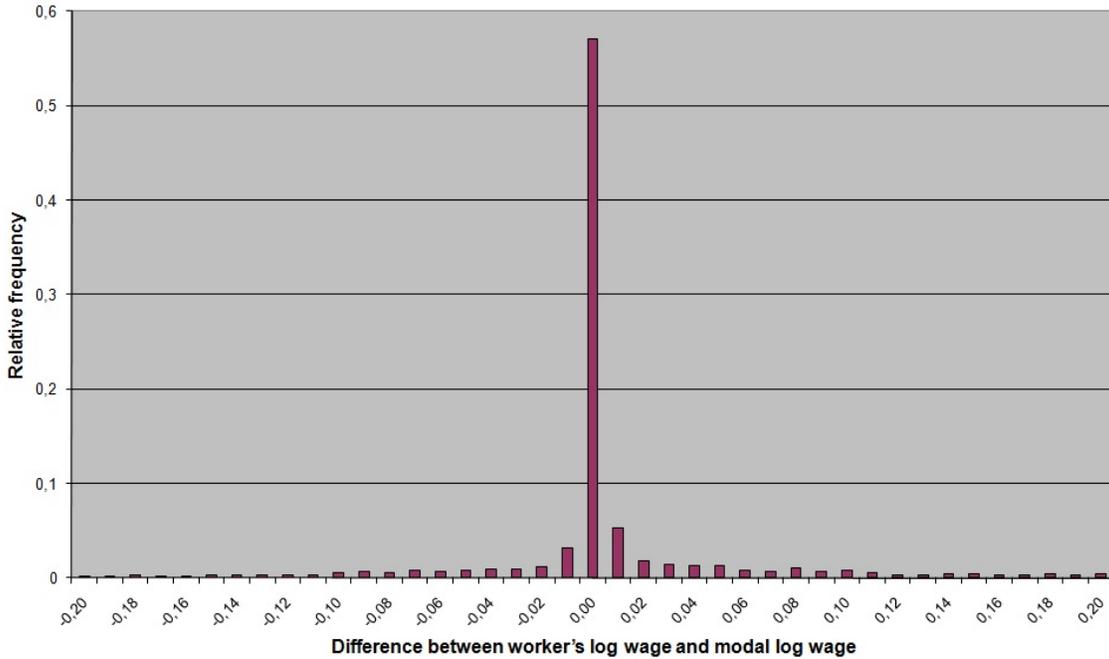
B. Empirical Analysis

As explained in section II, we begin by estimating the year effects β_t in equation (1) by applying least squares to the regression of the logarithm of job-specific real entry wages w_{jt} on year dummy variables with controls for job fixed effects. As discussed in section II, if the panel of entry jobs were balanced with entry wages observed for all jobs in all periods, the estimated year effects would be simply $\hat{\beta}_t = \sum_{j=1}^{N} \log w_{jt} / N$. With our highly unbalanced panel, our estimates essentially regression-adjust that simple statistic for year-to-year changes in sample composition.

The resulting $\hat{\beta}_t$ series is plotted in figure 4, with the value for 1982 normalized to zero. Recall from section III.A that the series pertains to March for the observations from 1982 through 1993, it pertains to October for the observations from 1994 through 2008, and it is missing the years 1990 and 2001. The figure also displays the seasonally adjusted unemployment rates for the corresponding months. Eyeballing the figure in a way that takes account of the upward secular trend in wages gives the impression that the entry-wage series and the unemployment rate are inversely related, but that the relationship is very loose.

Figure 4. Estimated Period Effects for Log Entry Wages in Available Months and Corresponding Unemployment Rates, March 1982-October 2008
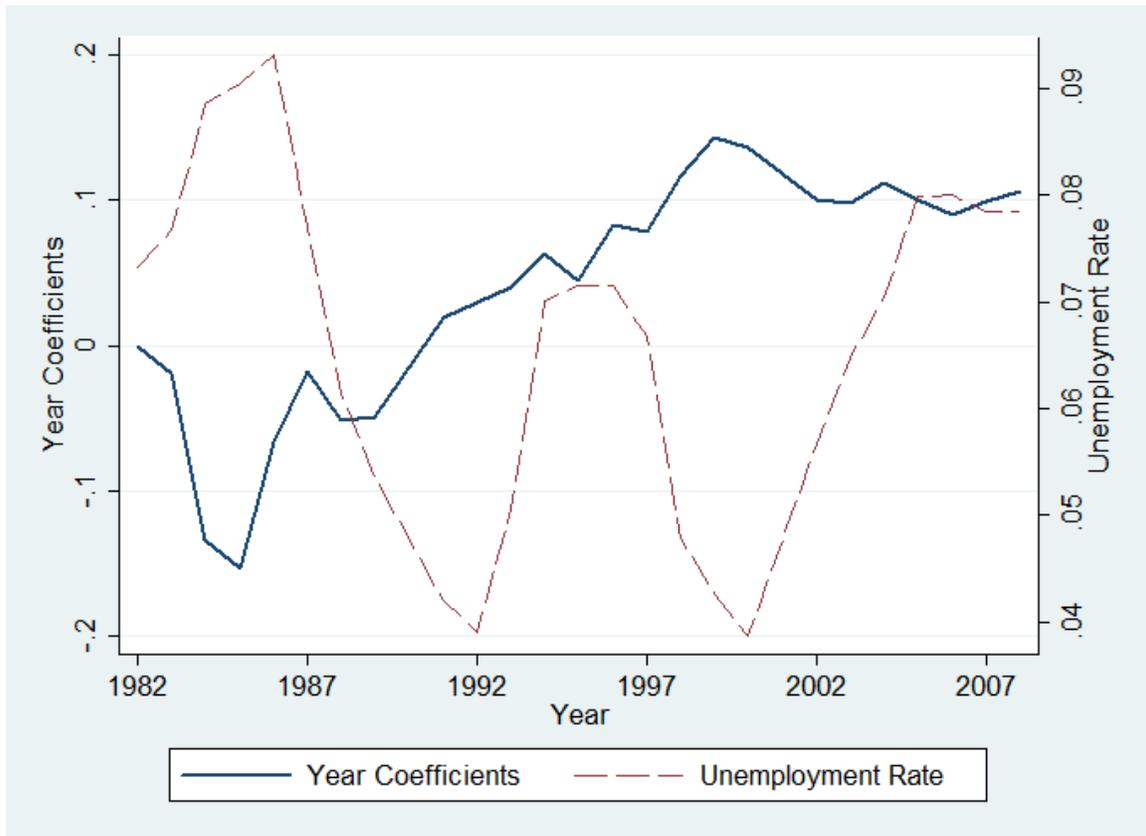
As discussed in section II, we can quantify the cyclicality of entry wages by estimating regressions of the $\hat{\beta}_t$ time series on the unemployment rate and secular time trend controls. The first row of table 2 shows the estimated coefficient of the

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unemployment rate when this regression controls for a linear time trend and is estimated by weighted least squares (weighting by the number of entry jobs observed per year, in an effort to correct for the heteroskedasticity resulting from the wide variation in the per-year sample size). The procyclical coefficient estimate of -1.81 (with estimated standard error 0.38)[5] implies that, when the unemployment rate is one point higher (say, .07 instead of .06), real entry wages tend to be about 1.8 percent lower.

We would have liked to re-estimate the regression with the logs of the employment/population ratio and real GDP per capita as alternative cycle indicators instead of the unemployment rate. Unfortunately, we have been unable to locate monthly or even quarterly versions of these variables for Portugal over our sample period. The Bank of Portugal staff, however, has imputed quarterly series for employment and real GDP (neither in per capita form).[6] When we repeat the regression in the first row of table 2 with the unemployment rate replaced by log real GDP (using first-quarter values up through 1993, when our wages are measured in March, and fourth-quarter values from 1994 on, when our wages are measured in October), the estimated coefficient is 0.55 (with estimated standard error 0.09). When we use quarterly log employment as the regressor, the estimated coefficient is 1.13 (0.32). When the regressor is the difference between log GDP and log employment, a crude productivity measure, the estimated coefficient is 0.69 (0.12). As discussed later in this section, we have used annual data to estimate Okun's Law-style regressions for Portugal over our sample period. These indicate that, when the unemployment rate goes down by one percentage point, real GDP per capita tends to be about 4 percent higher, and the employment/population ratio goes up by about 1.6 percent. In light of these relationships, our estimates of wage procyclicality with respect to GDP, employment, and GDP per worker line up fairly closely with our estimates using the unemployment rate.

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[5] The reported standard error estimates are the old-fashioned kind, i.e., not corrected for heteroskedasticity or serial correlation. Serial correlation diagnostics indicate a first-order autocorrelation of about 0.4 and negligible second- and third-order autocorrelations. Despite having only 25 time series observations in our second-stage regressions, we have experimented with Newey-West standard error estimation robust to heteroskedasticity and first-order serial correlation. The resulting standard error estimates are very close to the old-fashioned ones.

[6] The series are available at http://www.bportugal.pt/en-US/EstudosEconomicos/Publicacoes/BoletimEconomico/BEAnteriores/Pages/default.aspx?theme=2009. See de Castro and Esteves (2004) for an explanation of how the series were constructed.

Table 2. Estimates of Cyclicality of Log Entry Wages in Portugal, 1982-2008

Estimation Method	Estimated Coefficient of Unemployment Rate (and Estimated Standard Error)	
1. Weighted least squares for regression of log modal entry wage year effects on unemployment rate and linear time trend	-1.81 (0.38)	
2. Same as (1) but ordinary least squares	-1.66 (0.47)	
3. Ordinary least squares for first differences	-1.48 (0.78)	
4. Same as (3) with change from March 1993 to October 1994 omitted	-1.91 (0.83)	
5. Same as (1) with one-year lag of unemployment rate as additional regressor	Contemporaneous: -1.75 (0.74) Lagged: -0.31 (0.77)	
6. Same as (1) with quadratic time trend	-1.33 (0.52)	
7. Same as (1) for blue-collar jobs	-1.72 (0.42)	
8. Same as (1) for white-collar jobs	-1.98 (0.38)	
9. Same as (1) with log average wage	-1.92 (0.45)	
10. Same as (9) with wage measure combining base wage with regular supplements	-1.97 (0.46)	
11. Same as (9) with wage measure including all non-base pay	-1.97 (0.49)	
12. Same as (1) with more inclusive definition of entry jobs	-2.01 (0.39)	
13. Same as (1) with new-hire criterion reduced to three months of tenure	-2.03 (0.45)	
14. Same as (1) for 1986-2008	-1.59 (0.38)	

Continuing to further analyses using the unemployment rate, although heteroskedasticity diagnostics applied to the residuals suggest that our weighting procedure is reasonable, in the second row of table 2 we use ordinary least squares instead of weighted least squares. The resulting coefficient estimate, -1.66 (0.47), is fairly close to the weighted result. In the third row, we apply ordinary least squares to the

first-differenced version of the regression. A linear time trend in levels implies that, in first differences, we need to control for the varying time interval between adjacent observations, which is usually 12 months but is 24 months for the 1989-1991 and 2000-2002 changes and 19 months for the change from March 1993 to October 1994. The resulting estimate, -1.48 (0.78), is somewhat less procyclical than the previous estimates. In the fourth row, to check whether our results might be distorted by seasonal factors in wage determination, we reestimate the first-difference regression omitting the 1993-1994 difference, which is from March 1993 to October 1994. All the remaining observations are either March-to-March or October-to-October and hence automatically seasonally adjusted. The resulting estimate, -1.91 (0.83), is somewhat more procyclical.

In the fifth row, we revert to the analysis in the first row except that, as a simple check for dynamics, we include the 12-month lag of the unemployment rate as an additional regressor.[7] The estimated coefficient of the lagged term, -0.31 (0.77), is small and statistically insignificant, and the estimated coefficient of the contemporaneous term, -1.75 (0.74), is significantly procyclical. Combining the estimates, the implied wage response to a one-point increase in the unemployment rate in *both* periods is -2.06.

In the sixth row, we redo the regression from row 1 except that we control for a quadratic time trend. The estimated coefficient of the squared trend variable is statistically insignificant, and the estimated coefficient of the unemployment rate, -1.33 (0.52), becomes somewhat less procyclical. In the seventh and eighth rows, we redo the regression from row 1 separately for blue-collar and white-collar jobs. The two estimates are fairly similar to each other and to the overall estimate.[8]

The next five rows repeat the analysis from row 1 but with alternative approaches to the first-stage estimation of the $\hat{\beta}_t$ series. In the ninth row, instead of representing the

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[7] The analysis of Portuguese wage cyclicality in Carneiro, Guimarães, and Portugal (forthcoming) uses *only* a lagged unemployment rate, the annual average rate for the preceding calendar year. Up through 1993, when wages are measured in March, their unemployment rate regressor is an average from 3 to 15 months before; from 1994 on, when wages are measured in October, their unemployment rate regressor is an average from 10 to 22 months before.

[8] At some cost to the precision and stability of the estimates, we also have tried disaggregating to the most commonly observed industries in our sample. The resulting estimates are -2.31 (0.56) for real estate and other business activities, -2.19 (0.40) for wholesale and retail trade, -1.30 (0.39) for manufacturing, and -0.52 (0.65) for construction. The estimate for construction jumps to -1.67 (0.82) with the inclusion of a (significant) squared term in the time trend. On the other hand, allowing for a quadratic trend cuts the estimates for real estate, etc. and trade by about half.

"typical" job/year wage w_{jt} with the modal wage of new hires, we use their average wage. In the second-stage regression, this results in a slightly more procyclical estimate of -1.92 (0.45). This analysis still uses the individual wage measure described in section III.A – the worker's monthly base pay ("corresponding to the normal hours of work") divided by the worker's normal monthly hours. In the tenth and eleventh rows, we use alternative wage measures. In the tenth row, we use the average of a wage measure that adds in supplements "with a regular monthly nature, on account of subsistence, job transport, tenured-related or productivity, punctuality, hard-working/dangerous or dirty tasks, night shifts." In the eleventh row, we go further by measuring the worker's wage as the ratio of all the worker's pay in the census month (including overtime, bonuses, etc.) to all the worker's hours in the census month. The estimates based on these alternative wage measures, -1.97 (0.46) and -1.97 (0.49), are slightly more procyclical than those using our original base pay measure.

As we acknowledged in section III.A, our criteria for defining our main sample of perennial entry jobs were quite arbitrary. Therefore, in the twelfth row, we revert to using modal base pay, but we try using a more inclusive sample of entry jobs in the first stage. Instead of requiring that the firms employ at least 50 workers in at least five years, we require that they employ at least 25. And instead of requiring that the job involves at least three new hires and at least 10 percent of the firm's new hires in at least half the years the firm is in the data base, we require at least two new hires and at least 10 percent of the firm's new hires in at least a quarter of the years. In the second stage, this results in a slightly more procyclical estimate of -2.01 (0.39). In the thirteenth row, we repeat the analysis in the first row except that we define newly hired workers as those with tenure of no more than three months instead of four. The resulting estimate, -2.03 (0.45), is somewhat more procyclical than the one based on the four-month threshold.

Finally, because some analyses later in this section will involve only the years 1986-2008, in the last row we redo the analysis of row 1 with only those years. The resulting coefficient estimate, -1.59 (0.38), is a little less procyclical.

In general, throughout table 2, most of the estimated coefficients of the unemployment rate are in the general vicinity of -1.8. Recognizing that, with procyclical labor force participation, the negative of the change in the unemployment rate is an

attenuated version of proportional changes in employment, these estimates imply that the cyclical elasticity of entry wages and the cyclical elasticity of employment are of similar magnitudes. In particular, we have estimated Okun's Law-style relationships among the unemployment rate, the log of the employment/population ratio, and the log of real GDP per capita based on first differences of annual data for Portugal over our sample period.[9] Our results indicate that a one-point increase in the unemployment rate is associated with a 1.6 percent reduction in the employment/population ratio. This procyclicality of employment is quite similar to the procyclicality we have estimated for real entry wages.

The substantial procyclicality we have found in real entry wages becomes more striking when one considers that the "cyclical upgrading" discussed in section I may cause us to underestimate the true procyclicality of entry wages. If, in a recession, employers are able to recruit a higher quality of workers at any given wage, the effective wage they pay per efficiency unit of labor is that much lower. That this is the likely direction of bias is suggested not only by the large quantitative literature cited in section I, but also by Bewley's (1999) interviews with employers.[10]

Do our sample data on observable characteristics of newly hired workers show any sign of cyclical upgrading? Visual inspection of figure 2 does not reveal any clear-cut cyclicality in the education, age, or gender of our sample of newly hired workers. More formally, we have re-estimated some of the regressions in table 2 with average years of education of the newly hired workers, instead of their log hiring wage, as the dependent variable. In accordance with the cyclical upgrading story, these estimates do show countercyclicality in the education of new hires by job, but the statistical significance of the estimates is sensitive to the specification of secular trends. Our results for age and gender are qualitatively similar.

Another perspective on the estimates in table 2 is that they are fairly similar to the estimates of the overall cyclicality of workers' real wages in the U.S. literature that uses longitudinal data to relate workers' year-to-year changes in log wages to changes in the

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[9] Like the annual unemployment rates plotted in our figure 1, the annual statistics on the employment/population ratio and real GDP per capita are drawn from the OECD data base available at http://stats.oecd.org.

[10] See pages 279-281 for direct quotations from the employers. Bewley's summary (p. 278) is, "Most employers claimed that labor was far more abundant [in the recession] than during the previous boom…. The overall quality of applicants improved as well…."

unemployment rate.[11] This similarity is surprising for two reasons. First, the estimates are for different countries that differ in many ways, including wage-setting institutions and the severity and nature of macroeconomic shocks. Second, they are measuring different facets of wage cyclicality. On one hand, the well-known literature on implicit contracts explains why wages might be smoothed in long-term employment relationships. Such smoothing of incumbent workers' wages could cause overall wage cyclicality to be less pronounced than the cyclicality of hiring wages in entry jobs. On the other hand, workers' procyclical opportunities to move to higher-paying jobs (both within and across firms) contribute to estimates of workers' overall wage procyclicality, while our estimates of entry-wage cyclicality are designed to hold jobs constant.

To address the first issue of between-country differences, in table 3 we use longitudinal worker data from the Portuguese census of employers to replicate the analyses in the U.S. longitudinal literature. In the tradition of Bils (1985), we estimate the regression of workers' year-to-year change in log real wages on change in the unemployment rate.   Following Solon, Barsky, and Parker (1994), we break the estimation into two steps.   In the first stage, we apply ordinary least squares to the regression of the worker's change in log real wages on year dummy variables and changes in the worker's second, third, and fourth powers of age, which belong in the regression if the log wage/age profile follows a quartic in levels.[12] In the second stage, much as in the third row of table 2, we apply ordinary least squares to the regression of the estimated year effects on change in the unemployment rate and the length of the time interval between the adjacent censuses.[13] The estimates are based on the period 1986-2008 because worker identifiers are not available before 1986.

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[11] Solon, Barsky, and Parker (1994), for example, obtain estimates of about -1.4 for men and less procyclical estimates for women. The literature review in Solon, Barsky, and Parker (1992) demonstrates that those estimates are typical of the U.S. longitudinal literature. Kim and Solon (2005) explain that these estimates probably are biased in a countercyclical direction by mean-reverting measurement error in wage data based on household survey reports. Such measurement error is not a factor in the Portuguese data.

[12] A first-order term would be perfectly collinear with the year effects.

[13] We apply ordinary instead of weighted least squares in the second stage for reasons discussed in Dickens (1990). In our worker-level analysis, as opposed to the entry-job analysis in table 2, our first-stage estimation involves millions of observations. As a result, sampling error in the estimated year effects is inconsequential, and the error term in the second-stage regression is dominated by unobserved time effects common to all workers.

Table 3. Estimates from Regressions of Portuguese Workers' Change in Log Wages on Change in Unemployment Rate, 1986-1987 to 2007-2008

Estimation Method	Estimated Coefficient of Change in Unemployment Rate (and Estimated Standard Error)
1. All longitudinally matched workers, with controls for age and time	-1.25 (0.38)
2. Same as (1) but with wage measure including non-base pay	-1.51 (0.37)
3. Same as (1) but for sample restricted to workers changing employers	-2.31 (0.33)
4. Same as (1) but for sample restricted to workers staying with same employer (with additional control for tenure with employer)	-1.20 (0.40)
5. Same as (4) but also staying in same job (as well as firm)	-1.14 (0.37)

The resulting coefficient estimate for the unemployment rate, shown in the first row of table 3, is -1.25 (with estimated standard error 0.38). This estimate is somewhat less procyclical than the entry-wage counterpart for 1986-2008 shown in the last row of table 2. This suggests that, between the two factors that cause wage cyclicality from the workers' perspective to differ from cyclicality of job-specific entry wages, the wage-smoothing factor is at least as important as the cyclical-upgrading factor. The estimate of -1.25, which is similar in magnitude to the estimates in the U.S. longitudinal literature, is based on the same measure of workers' wages used in most of our entry-wage analyses – the ratio of the worker's monthly base pay to the worker's normal monthly hours. This measure leaves out overtime pay and other such non-base payments, which typically are included in the wage measures used in the U.S. longitudinal literature. In the second row of table 3, as in the eleventh row of table 2, we include such pay by measuring the wage as the ratio of the worker's total monthly pay to the worker's total monthly hours. The resulting coefficient estimate for the unemployment rate, -1.51 (0.37), is even more procyclical.

As discussed in section I, the wage cyclicality literature based on longitudinal worker data has estimated especially strong wage procyclicality for workers who change

employers. In the third row of table 3, we redo the estimation in row 1 with a sample restricted to workers who change employers between censuses. As in the previous literature, we find particularly procyclical wages for changers, with an estimated unemployment rate coefficient of -2.31 (0.33). Of course, the wage procyclicality for workers who change employers is expected to exceed other measures of wage procyclicality because, for this group of workers, cyclical upgrading/downgrading is especially salient and wage smoothing is not a factor. It is worth noting that this estimate is more procyclical than the estimates in table 2. As discussed in section I, Pissarides (2009) interpreted the strong procyclicality of employer changers' wages as evidence of strongly procyclical hiring wages, but Gertler and Trigari (2009) criticized that interpretation on the ground that employer changers' wage procyclicality might be driven largely by cyclical upgrading/downgrading.   Indeed, although the hiring-wage procyclicality we have estimated directly in table 2 is substantial, it is not as great as the procyclicality we have estimated for employer changers' wages. This pattern is consistent with Gertler and Trigari's critique of Pissarides's interpretation.

In the fourth row of table 3, we redo the row 1 analysis for a sample restricted to workers who stay with the same employer from one census to the next. In the first-stage regression, we control for change in the worker's squared tenure with the employer as well as in the worker's polynomial in age.   The second-stage estimate of the unemployment rate coefficient, -1.20 (0.40), is almost as procyclical as our estimate of overall wage cyclicality[14] and somewhat less procyclical than the corresponding estimate of entry-wage cyclicality in the last row of table 2. Finally, in the last row of table 3, we follow the analysis of U.K. data in Devereux and Hart (2006) by further restricting to workers who stay not only with the same employer, but also in the same job title. This restriction reduces the estimated coefficient only slightly to -1.14 (0.37), according with Devereux and Hart's finding of substantial wage procyclicality even for stayers in the same job in the same firm.

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[14] This comparison is a little surprising because, in the U.S. literature, the estimates of stayers' wage cyclicality sometimes are noticeably smaller than the estimates of overall wage cyclicality. But some studies – such as Solon, Barsky, and Parker (1994) and Shin and Solon (2007) – report only modest differences.

To summarize, longitudinal worker-based estimates of real wage cyclicality in Portugal are fairly similar to those in the U.S. literature. In Portugal, however, unlike the United States at present, it is possible to use detailed employer/employee longitudinal data to conduct a direct and transparent analysis of the cyclical behavior of wages paid to newly hired workers in a large number of entry jobs. As discussed in sections I and II, these data are well suited for assessing recent theories of firms' practices vis-à-vis setting hiring wages. Our analyses of the Portuguese data have produced a robust finding that real entry wages by job tend to be about 1.8 percent lower when the unemployment rate is one percentage point higher.

V. Summary and Discussion

Using longitudinally matched data from Portugal's annual census of employers, we have tracked the cyclical behavior of the real wages paid to newly hired employees in over a thousand jobs. We hope that eventually our methodology will be applied to additional countries as the requisite data become available.

Our estimates suggest that real entry wages in Portugal tend to be about 1.8 percent lower when the unemployment rate is one percentage point higher. As noted in section III.B, this procyclicality in entry wages is substantial in the sense that the cyclical elasticity of this price variable is of approximately the same magnitude as the cyclical elasticity of employment. This finding, like practically all the longitudinal evidence on workers' wage cyclicality, counters a view often stated by influential macroeconomists that wages are much less cyclical than employment and unemployment. Most of our estimates of entry-wage cyclicality, however, are less procyclical than our estimated wage cyclicality for workers who change employers. This contrast is consistent with Gertler and Trigari's (2009) criticism of Pissarides's (2009) interpretation of the cyclicality of employer changers' wages.

One reason our findings matter is that some recent (as discussed in the introduction) and older attempts at theoretically explaining large cyclical changes in unemployment have emphasized rigidity of wages. Like most recent studies of other aspects of wage cyclicality, our study of real entry wages in Portugal has found that wages are not rigid, but rather respond considerably to business cycle conditions. Of

course, this finding does not preclude that, in some sense, wages are "insufficiently" variable. Pursuing that idea, however, will require not a theory of wage *rigidity*, but a theory of why wages are not even more variable than they are.[15] Putting the point another way, the very existence of large cyclical fluctuations in employment and unemployment indicates that wages do not vary enough to prevent those fluctuations. As emphasized by Pissarides (2009), the continuing challenge to macroeconomic theorists is to develop a logically coherent and empirically relevant explanation of why employment, unemployment, and wages vary over the business cycle in the ways that they actually do.

Finally, returning to a question posed in the introduction, can the Mortensen-Pissarides model in particular account for the cyclical variability of unemployment in light of the magnitude of the entry-wage cyclicality that we have found? At first glance, it appears the answer may be no. For example, when Kennan (2010) calibrates his modification of the Mortensen-Pissarides model, most of his calibrations match the empirical variation in the unemployment rate by assuming that the real hiring wage declines by less than 0.7 percent when the unemployment rate rises by one percentage point.[16] This is well under half the cyclical variability that we estimate for hiring wages.

As noted by many writers, however, in a world of long-term employment relationships, decisions about hiring (and acceptance of job offers) depend not only on the initial hiring wage, but also on agents' expectations about the future course of the relationship. This, of course, is just an application of the old insight that, in a long-term employment relationship, the current wage is not necessarily allocative (Becker, 1962; Barro, 1977; Hall, 1980). The answer therefore depends not only on the cyclicality in initial hiring wages, but also on the persistence of those wages as the worker/employer match continues. As explained by Kudlyak (2009), if workers hired at a low wage during

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[15] Hall and Milgrom (2008), Kennan (2010), Snell and Thomas (2010), and Perry and Solon (1985) are examples of steps in that direction.

[16] In particular, in three of the calibrations shown in the right panel of Kennan's table 2, he gets the unemployment rate to increase by more than two percentage points by having initial hiring wages drop by only 0.4 to 1.4 percent. Of course, one could refer to any number of other variants of the Mortensen-Pissarides model, such as those in Hall and Milgrom (2008) and Gertler and Trigari (2009). We emphasize examples from Kennan's models for two reasons. First, Kennan's reporting of his calibrations' implications for the covariation between real wages and the unemployment rate is unusually clear. Second, the differences in that covariation across his models are particularly well suited for illustrating our concluding point – that future research on the connection between employers' hiring incentives and initial hiring wages will need to take account of the durability of employment relationships and the evolution of wages and productivity over the course of those relationships.

a recession are somehow locked into a long-term employment relationship at a persistently low wage, the labor cost relevant to employers' recruiting decisions could be as cyclical as, or even more cyclical than, the initial hiring wage. On the other hand, if the shortfall in hiring wages during a recession vaporizes soon after the employment relationship is initiated (either because the cheaply-hired workers quit or because retaining them requires substantial raises once the recession eases), even strong cyclicality in the initial hiring wage might have only a minor impact on hiring decisions and hence allow for large cyclical variation in unemployment.

This latter possibility is illustrated in one version of Kennan's calibrations in which the initial hiring wage decreases by 3.5 percent when the unemployment rate goes up by 2.3 percentage points. This version of the model assumes that the elasticity parameter in the matching function is 0.5 and that the cyclical variation in the present discounted value of newly hired workers' wages is realized entirely through a short-lived dive in initial hiring wages during a recession. The wages of workers hired cheaply during the recession are increased to normal levels once the recession passes. In this way, Kennan achieves sufficient inflexibility in the present discounted value of wages to generate realistic cyclical variability in unemployment while also achieving realistic cyclical variability in initial hiring wages. He does not explicitly justify assuming that long-term wage contracts would take this particular form, but several other writers – such as Thomas and Worrall (1988), Beaudry and DiNardo (1991), and Rudanko (2009) – have presented models that point in that direction.

Ultimately, assessing the allocative importance of initial hiring wages in the Mortensen-Pissarides model – or in any model that recognizes the prevalence of long-term employment relationships – will require a deeper understanding of the durability of employment relationships, the evolution of wages and productivity in those relationships, and the dependence of both on current, past, and anticipated business cycle conditions. The empirical literature pioneered by Beaudry and DiNardo (1991), which is summarized in Thomas and Worrall (2007) and extended in Kudlyak (2010), has begun to explore these issues, and we believe further research in that area could be highly productive.

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