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RETURNS TO TENURE OR SENIORITY?

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RETURNS TO TENURE OR SENIORITY?

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This study documents two empirical facts using matched employer–employee data for Denmark and Portugal. First, workers who are hired last, are the first to leave the firm. Second, workers' wages rise with seniority, where seniority is defined as a worker's tenure *relative* to the tenure of his colleagues. Controlling for tenure, the probability of a worker leaving the firm decreases with seniority. The increase in expected seniority with tenure explains a large part of the negative duration dependence of the separation hazard. Conditional on ten years of tenure, the wage differential between the 10th and the 90th percentiles of the seniority distribution is 1.1–1.4 percentage points in Denmark and 2.3–3.4 in Portugal.

KEYWORDS: Wage dynamics, tenure, seniority, last-in-first-out.

1. INTRODUCTION

WHY DOES LARS EARN less than Jens, if they have the same ability and work for the same firm? And why is Pedro fired, but his equally productive colleague Miguel allowed to stay, when their employer has to scale down employment? Some might think the answer to both questions is obvious: it is because Jens and Miguel have greater seniority than, respectively, Lars and Pedro; that is, Jens and Miguel have a longer tenure at their firms than their respective co-workers Lars and Pedro. This paper provides empirical evidence that supports these popular convictions. Using longitudinal linked worker-firm data for Denmark and Portugal, we show that a *worker who is hired last is likely to be fired first* (Last In, First Out; LIFO henceforth). Furthermore, we show that *there is a return to seniority in wages*, where seniority is defined as the *worker's tenure relative to the tenure of all his co-workers*. The worker's seniority is thus his rank in the tenure hierarchy of his firm. When we claim that seniority affects a worker's risk of job separation, we mean that in addition to the negative effect of tenure on the hazard rate (i.e., the negative duration dependence), being a senior worker with many more junior colleagues has a further negative effect on a worker's separation rate. Similarly, when we claim that seniority impacts

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a worker's wage, we mean that on top of the return to tenure, there is a further wage return to seniority. To the best of our knowledge, this paper is the first to document the existence of a return to seniority in wages.

Why would firms and workers agree on applying a LIFO layoff rule, and why would that lead to a wage return to seniority? Kuhn (1988) and Kuhn and Robert (1989) developed a framework that can rationalize these phenomena. Consider the standard monopoly union/right-to-manage model, where the union bargains for a wage rate above the market wage and where the firm decides on employment, taking this wage rate as given. Employment will be set below the efficient level. This outcome implies that gains from trade between the union and the firm are left unexploited, since the firm would be willing to hire additional workers for a wage between the market wage and the wage rate negotiated by the union. Kuhn and Robert showed that the firm and the union can achieve a Pareto superior outcome by agreeing on a hiring order based on seniority, and a wage schedule increasing in seniority. This agreement would require the firm to hire workers in a particular order: the most senior worker, with the highest wage rate, first. If the wage schedule is properly set, the marginal worker hired by the firm receives exactly the market wage, ensuring that employment is at its efficient level. The higher wage for inframarginal senior workers allows these workers to capture part of the firm's producer surplus. Kuhn and Robert formalized these ideas in a static framework. The working version of this paper, Buhai, Portela, Teulings, and van Vuuren (2009), developed a dynamic version of this model, akin to Bentolila and Bertola (1990), and showed that the firm and its workers agree on a wage profile where the workers hired first are fired last and earn higher wages. Moreover, they also showed that the model yields an inverse Gaussian distribution for the duration of individual job spells. Buhai and Teulings (2014) showed that distribution to match the empirical distribution of job spells closely. In this model, firing is efficient, but hiring is less than first best due to a hold up problem. Bovenberg and Teulings (2009) elaborated the implications of this model for the insurance of the workers' lifetime labor income.

Establishing a rate of return to seniority in wages is an exercise at the crossroads of two topics extensively discussed in the literature on the earnings function: the return to tenure, on the one hand (see, for example, Altonji and Shakotko (1987), Topel (1991), Altonji and Williams (2005), Dustmann and Meghir (2005), and Buchinsky, Fougère, Kramarz, and Tchernis (2010)), and the firm-size wage effect, on the other (see, for instance, Brown and Medoff (1989)). Seniority is related to tenure, since a worker's seniority is defined as his tenure relative to the tenure distribution of the rest of the firm's workforce. Hence, within a firm, seniority is positively related to tenure by construction. Seniority is also related to firm size: an increase in firm size will always increase the seniority of the firm's incumbent workers, since the newly hired workers have a lower tenure. It is therefore imperative in our exercise to pin down what identifies the return to seniority above the return to tenure and the firm-size wage effect. In related work, Neal (1995) and others have already shown that the effect of a worker's tenure at a given firm is partly a proxy for a return to industry or occupation tenure. However, their finding does not affect our measurement of the return to seniority within the firm.

The quest to estimate the wage return to tenure suffers from a well-known identification problem: the within-job-spell variation in tenure is perfectly correlated with the within-job-spell variation in experience. Hence, the first-order effects of experience and tenure cannot be identified separately using solely within-spell variation. At the same time, the between-job-spell variation is endogenous, since workers decide to change jobs at least partly motivated by a comparison of their current wage to the wage in other jobs. Various strategies have been attempted to deal with this endogeneity problem. In line with this literature, we apply the two methods most commonly used, namely, that of Altonji and Shakotko (who used job-spell fixed effects) and that of Topel (who used within-spell first differences). However, the identification problem troubling the estimation of the linear term in the return to tenure does not affect the estimation of our object of interest, the return to seniority. Unlike within-job-spell variation in tenure, within-job-spell variation in seniority is not perfectly correlated with experience, since a worker's seniority varies with the hiring and firing of other workers. Hence, seniority is not a deterministic function of tenure. This makes it possible to identify the return to seniority without resorting to between-job-spell variation, and separately from the return to tenure.

Regarding the firm-size wage effect, a worker's seniority is defined as the ratio of the total number of workers in the firm (i.e., the firm size) and the number of co-workers hired before himself (including himself). Therefore, the wage return to seniority can be distinguished from the return to the firm size only due to the variation in the number of more senior workers. Hence, in the extreme case where the LIFO rule applied perfectly, the return to seniority would not be identified: the more senior workers would never leave the firm before the respondent, such that all variation in the respondent's seniority would come from variation in firm size. Nevertheless, in practice, LIFO will not apply to each and every separation. In particular, retirement—or any other types of exogenous shocks such as leaving the firm because of a change in the spouse's location, and so forth—provides a source of variation in the number of workers with longer tenure. This type of variation identifies, therefore, the return to seniority separately from the firm-size wage effect.

We need exhaustive linked employer–employee data for establishing worker seniority, as we must know the tenure rank of each worker, in all of the firms present in our estimation sample. A full set of controls is added for the tenure in the estimation of the separation rate. We find strong effects of seniority on the job exit hazard, such that the expected increase in seniority with tenure explains a large part of the negative duration dependence of the hazard. Depending on the estimation method applied, we find small but statistically highly significant returns to seniority in wages, in the order of magnitude of 0.15% to 0.20% for every 10% increase in seniority for Portugal, and half that range for

Denmark. Conditional on ten years of tenure, going from the 10th to the 90th percentile in the seniority distribution raises the wage level by 1.1–1.4 percentage points in Denmark and 2.3–3.4 percentage points in Portugal.

Apart from Kuhn and Robert (1989), relatively little attention has been given to seniority-based promotions in the economic literature, even though it has been found that many firms use seniority as at least one of the criteria for promotions (e.g., Lazear and Oyer (2012)). At the same time, Waldman (2012) reported that wage changes are usually discontinuous, similar to the predictions of tournament models such as Lazear and Rosen (1981) or Malcomson (1984). This observation fits perfectly in a world envisioned by Kuhn (1988) and Kuhn and Robert (1989), where people move up in the hierarchy because a more senior position has been vacated by a senior worker leaving the firm.

The structure of this paper is as follows: Section 2 discusses our estimation strategy, Section 3 describes the data, Section 4 presents the estimation results, and Section 5 concludes.

2. ESTIMATION STRATEGY

2.1. *The LIFO Separation Rule*

Define the rank q_{ijt} to be the number of workers in firm j with tenure greater than or equal to tenure of worker i at time t, and define n_{jt} to be the total number of workers in firm j at time t. Then, the seniority index $\log r_{ijt}$ is defined as

$$\log r_{ijt} \equiv \log n_{jt} - \log q_{ijt}. \tag{1}$$

Thus, the seniority index for the most senior worker is equal to the log firm size $\log n_{jt}$, while the seniority index of the least senior worker is zero.

There are two ways to analyze the effect of the LIFO separation rule on the process of job separation. One approach is to investigate which workers are leaving the firm, conditional on the event that some workers are leaving that firm. The LIFO hypothesis then predicts that the workers with the shortest incomplete tenure have the highest probability of separating. However, this implication does not discriminate between LIFO and other hypotheses, as it merely confirms a standard result from the empirical literature on job durations, namely, negative duration dependence in the separation hazard. For this reason, we use a second approach: conditional on being employed by the firm, does a worker's separation rate depend on his seniority index, beyond and above the well-documented effect of his tenure? If so, then we have clear evidence in favor of LIFO.

One might presume that the LIFO hypothesis is only relevant for layoffs, the separations initiated by employers. However, as argued by McLaughlin (1991), the distinction between quits and layoffs is less clear-cut than one might think at first sight. In any model with efficient bargaining, the worker and the firm will always be able to strike a deal as long as there is positive surplus from continuation of the job, rendering the distinction between quits and layoffs meaningless. This logic also applies in a world with a LIFO rule. Knowing that the surplus has gone, either the firm might decide to lay off the worker, or the worker might decide to quit and accept another job in expectation for a future layoff. Hence, a LIFO rule will affect both quits and layoffs. We therefore do not distinguish between the two.

We model the job separation process by a mixed proportional hazard (MPH) model with discrete-time periods. The probability θ_{ijt} of worker i leaving firm j, between years t and $t+1$, conditional on T_{ijt} years of elapsed tenure, is specified as

$$\theta_{ijt} = \Lambda(\gamma_0 \log r_{ijt} + \gamma_1 \Delta \log n_{jt} + \gamma_2 Z_{ij,t-T_{ijt}} + \psi_{T_{ijt}} + \chi_j + v_i), \tag{2}$$

where $\Lambda(\cdot)$ is the logistic function and $Z_{ij,t-T_{ijt}}$ is a vector of observed characteristics of the worker and the job at the moment of job start (e.g., education and experience at the start of the job spell), and where v_i represents the unobserved worker heterogeneity, whereas χ_j represents firm heterogeneity. We include a full set of indicator variables $\psi_{T_{ijt}}$, for every tenure category (years). Experience is included in the vector $Z_{ij,t-T_{ijt}}$. Identification of the coefficient γ_0 of the seniority index $\log r_{ijt}$, separately from the parameters of the baseline hazard $\psi_{T_{ijt}}$, requires variation in $\log r_{ijt}$ that is independent of the tenure T_{ijt}. Such independent variation is available, since the seniority index also depends on the hiring, firing, and quit behavior of other workers. We add the change in firm size as a regressor to control for heterogeneity between growing and shrinking firms, since shrinking firms are expected to have higher separation rates. The LIFO separation rule implies that the separation rate is higher for junior workers (i.e., γ_0 is expected to be negative).

We model χ_j as a correlated random-effects model (see, e.g., Wooldridge (2002))

$$\chi_j = \chi_{0j} + \chi_1 \overline{Y}_j,$$

where variable χ_{0j} measures unobserved firm heterogeneity, and $\overline{Y}_j$ is a vector of within-firm averages of employee-observed characteristics over all time periods. We assume that this unobserved component χ_{0j} is distributed normally and is uncorrelated with $\overline{Y}_j$. For $\overline{Y}_j$, we use education, experience, and the percentage of women working within the firm.

Ideally, we would use a sample of individuals observed from the date of labor market entry onwards. However, for reasons discussed in our data section, we focus only on people above 25 years of age, and drop spells that are left-censored (see Lancaster (1990)). Hence, initial experience in the first job is correlated with the random worker effect v_i: a worker with a high draw of v_i can be expected to have short job spells, and hence, the initial experience in the first job started above the age of 25 is expected to be low. To correct for this, we use the conditional likelihood method developed by Wooldridge (2005).

Moreover, we allow for specifications with and without correlation between the unobserved worker and firm components. In the case of k mass points of the unobserved worker component v_i, we assume the following conditional distribution:

$$P(v_i = \nu_l | X_{i1}, \overline{\chi}_{0i}) = \frac{\exp(\eta_l + \lambda_l X_{i1} + \gamma_l \overline{\chi}_{0i})}{\sum_{m=1}^{k} \exp(\eta_m + \lambda_m X_{i1} + \gamma_l \overline{\chi}_{0i})}, \tag{3}$$

where ν_l, $l = 1, \dots, k$, are the different mass points, X_{i1} is initial experience in the first job, and $\overline{\chi}_{0i}$ is the average unobserved firm effect in the jobs of worker i; η_1, λ_1, and γ_1 are normalized to zero without loss of generality. For the specification without correlation between the unobserved worker and firm component, $\gamma_l = 0$.

The estimation is based on the method of maximum simulated likelihood so as to approximate the highly dimensional integral that results from taking into account firm random effects.[2] Up to ten job spells of an individual are used. We work with a discrete-time model, since workers are observed only once a year. This also implies that short spells are underrepresented in the duration data, since a worker has to stay at least until the next moment of observation for a spell to be recorded.[3] We cannot correct for these problems with the data at hand.

2.2. *The Return to Seniority*

The existence of a return to seniority in wages can be tested by extending the standard specification of the log earnings equation with the seniority index $\log r_{ijt}$:

$$\log w_{ijt} = \beta_0 + \beta_1 X_{ijt} + \beta_2 T_{ijt} + \beta_3 \log r_{ijt} + \beta_4 \log n_{jt} + \beta_5 Z_{ijt} + \varepsilon_{ijt}, \tag{4}$$

where $\log w_{ijt}$ is log wage. Higher-order terms in experience and tenure (and other controls) are included in the vector Z_{ijt}. The coefficient β_4 captures the firm-size wage effect documented by Brown and Medoff (1989). Substitution of $\log r_{ijt}$ as defined in (1) into (4) yields

$$\begin{aligned} \log w_{ijt} = {} & \beta_0 + \beta_1 X_{ijt} + \beta_2 T_{ijt} - \beta_3 \log q_{ijt} \\ & + (\beta_3 + \beta_4) \log n_{jt} + \beta_5 Z_{ijt} + \varepsilon_{ijt}. \end{aligned} \tag{5}$$

[2]In our analysis, we condition on the firm effects in order to calculate the conditional likelihood based on (2) and (3), and integrate over the firm effects. This is an integral with the number of dimensions equal to the number of firms. Details are provided upon request.

[3]Note that this problem does not affect our measurement of the seniority index r_{ijt}, since for that purpose we only need the distribution of tenure at a particular point in time.

The coefficient on the seniority index is thus identified separately from the coefficient on the firm size by the variation in the log number of workers in the firm with tenure greater than or equal to the tenure of worker i. It is therefore important to include log firm size in the model to make sure that the estimated effect of seniority is not merely a proxy for firm size.[4]

Following Topel (1991), the unobservable term can be decomposed into five orthogonal components: a match-, a firm-, a worker-, a time-, and an idiosyncratic effect:

$$\varepsilon_{ijt} = \varphi_{ij} + \psi_j + \mu_i + \tau_t + \nu_{ijt}. \tag{6}$$

The idiosyncratic effect ν_{ijt} includes measurement error. There are all kinds of reasons for φ_{ij}, ψ_j, and μ_i to be correlated with T_{ijt} (see, e.g., Topel (1991) or Altonji and Williams (2005)). Learning and search theories imply that good worker-firm relationships tend to survive, and bad matches are broken up, as the worker and the firm learn about the quality of their match, leading to positive correlation between $\varphi_{ij} + \psi_j + \mu_i$ and T_{ijt}. However, Topel (1991) showed that there are also reasons for a negative correlation between φ_{ij} and T_{ijt}, since workers change jobs to get a higher wage. Hence, workers who recently changed jobs are likely to have found a job that at least made up for the loss of their returns to tenure. There are two existing solutions to the problem of the endogeneity of tenure: either using within-job-spell first-differencing (FD), as applied by Topel (1991), or adding fixed effects for every job spell (FE), as applied by Altonji and Shakotko (1987).

The first-order effects of tenure and experience, β_1 and β_2, are not separately identified. Hence, one has to revert to between-job-spell variation in order to distinguish their effects. This problem has led to a debate between Altonji and Shakotko (1987) and Topel (1991), and a stream of subsequent papers. Topel (1991) established β_1 by calculating $\log w_{ijt} - (\beta_1 + \beta_2)T_{ijt}$ and regressing this variable on initial experience X_0. Altonji and Shakotko (1987) used deviations from the mean of T_{ijt} as an instrument for T_{ijt}. As discussed in Topel (1991) and Altonji and Williams (2005), there are pros and cons for each method.[5]

Happily, this identification problem does not affect the estimation of the return to seniority, β_3, since the seniority index $\log r_{ijt}$ in equation (4) (or log rank $\log q_{ijt}$ in equation (5)) is not perfectly correlated with T_{ijt}. Hence, we can identify β_3 using only within-job-spell variation in wages. Note that Topel

[4]Note that a perfect application of the LIFO rule by all workers and all firms would imply that β_3 is not identified.

[5]Some recent studies discuss, in addition, the possible endogeneity of experience. For instance, Buchinsky et al. (2010) estimated a structural model with two endogenous decisions: employment and job-to-job mobility. Dustmann and Meghir (2005), who focused on displaced workers, also took into account that interfirm mobility might be endogenous. A detailed discussion of these studies is outside the scope of our paper.

(1991) did not include time effects τ_t in his analysis of within-job-spell variation, but instead corrected for wage growth using an external source. Had he included time effects, then even the sum of the first-order terms of tenure and experience, $\beta_1 + \beta_2$, would not have been identified. Altonji and Shakotko (1987) used a time trend, making the additional assumption that their population does not change over time with respect to experience and tenure. In our application, we are not per se interested in either β_1 or β_2. Hence, for the exposition below, we include time effects, resulting in $\beta_1 + \beta_2$ dropping out of the specification. However, in our estimation section, we also report separate linear tenure and experience effects, in order to compare them across specifications with and without accounting for the seniority index. The Supplemental Material of this paper (Buhai, Portela, Teulings, and van Vuuren (2014)) discusses the identification and estimation of these effects when time indicators are included in the wage equation.[6] First-differencing (4) yields

$$\Delta \log w_{ijt} = \beta_3 \Delta \log r_{ijt} + \beta_4 \Delta \log n_{jt} + \beta_5 \Delta \log Z_{ijt} + \Delta\tau_t + \Delta\nu_{ijt}, \tag{7}$$

whereas taking deviations from the mean over a job spell yields

$$\log \widetilde{w}_{ijt} = \beta_3 \log \widetilde{r}_{ijt} + \beta_4 \log \widetilde{n}_{jt} + \beta_5 \log \widetilde{Z}_{ijt} + \widetilde{\tau}_t + \widetilde{\nu}_{ijt}, \tag{8}$$

where the upper tilde denotes deviations from the mean per-job spell (e.g., $\log \widetilde{w}_{ijt} = \log w_{ijt} - \log \overline{w}_{ij}$, with $\log \overline{w}_{ij}$ being the mean of $\log w_{ijt}$ over a job spell). Terms with β_1 and β_2 drop out in both specifications because we include the full set of time indicator variables, τ_t.

Topel (1991) found that ν_{ijt} closely approximates a random walk plus a transitory shock. The random walk can be interpreted as a sequence of permanent shocks.[7] Topel argued that his method is unbiased as long as job changes are not affected by these permanent changes. He tested for this by looking at the returns to tenure and experience based on various remaining job durations. If the permanent changes affect the estimates of the within-spell returns, then these returns should be larger when the remaining job duration increases. Topel found no evidence for this hypothesis. We perform similar checks and obtain comparable results; see the Supplemental Material of this paper.

So far, we have assumed that the timing of the variation in seniority and the corresponding variation in wages is the same. This specification is incorrect when the workers' wages are not immediately adjusted to changes in their seniority index, for example, because promotion takes time. Suppose indeed that

[6]In the working version of this paper, we time-detrended the wages prior to the regressions. The sum of the linear returns to T and X was then identified in the first stage, as there were no time indicators τ_t in the regressions. The estimated $\beta_1 + \beta_2$ is virtually the same in both cases.

[7]Abowd and Card (1989), Topel and Ward (1992), and Meghir and Pistaferri (2004) found similar results.

there is a lag in the effect of $\log r_{ijt}$ on $\log w_{ijt}$, for example,

$$\log w_{ijt} = \beta_0 + \beta_1 X_{ijt} + \beta_2 T_{ijt} + \frac{1}{2}\beta_3(\log r_{ijt} + \log r_{ij,t-1}) + \frac{1}{2}\beta_4(\log n_{ijt} + \log n_{ij,t-1}) + \beta_5 Z_{ijt} + \varepsilon_{ijt}. \tag{9}$$

Assume that both $\log w_{ijt}$ and $\log r_{ijt}$ are close to a random walk. Excluding the lagged value of r_{ijt} from the model and first-differencing the equation leads to underestimation of the estimated coefficient β_3 by a factor of 2, assuming $\Delta \log r_{ijt}$ and $\Delta \log r_{ij,t-1}$ to be uncorrelated. The same applies to β_4. When using deviations from the mean, the underestimation will be smaller, due to the persistence in $\log r_{ijt}$ and $\log n_{jt}$. Hence, the estimates for β_3 and β_4 are expected to be higher when estimating these coefficients by deviations from the mean, rather than by first-differencing. We include a robustness check in which we also use lags of log seniority and log firm size. We report robust standard errors, such that the correlation between the residuals over time implied by the autocorrelation in the error terms does not affect the validity of the standard errors.

Abowd, Kramarz, and Margolis (1999) found evidence for heterogeneity in the return to tenure between firms and between workers. We can adapt equation (4) to allow for heterogeneity in the return to tenure between spells, and for heterogeneity in the return to experience between individuals:

$$\log w_{ijt} = \beta_0 + \beta_{1i} X_{ijt} + \beta_{2ij} T_{ijt} + \beta_3 \log r_{ijt} + \beta_4 \log n_{jt} + \beta_5 Z_{ijt} + \varepsilon_{ijt}.$$

In this case, the parameters β_3 and β_4 can be estimated by first performing within-spell first-differences, and then taking deviations from the within-spell mean. Then, substitution of equation (6) yields

$$\widetilde{\Delta \log w_{ijt}} = \beta_3 \widetilde{\Delta \log r_{ijt}} + \beta_4 \widetilde{\Delta \log n_{jt}} + \beta_5 \widetilde{\Delta Z}_{ijt} + \widetilde{\Delta \tau}_t + \widetilde{\Delta \nu}_{ijt}. \tag{10}$$

3. THE DATA

For Denmark, we use the *Integrated Database for Labor Market Research* (IDA), for the years 1980–2001, which has been used in many previous studies, for example, by Mortensen (2003). IDA tracks every Danish individual between 15 and 74 years of age and contains information of all companies with employees. The labor market status of each person is recorded at November 30 of each year. The data set contains a plant identifier, which allows the construction of the total workforce of a plant, and hence of the firm as a whole. We use information on the hourly gross earnings, education, age of individuals, and on the location and industry of the plant at which they work at the start of a job, as well as on firm employment size. Industry is defined as the industry employing the largest share of the firm's workforce. Firm size is defined as the number of individuals holding a primary job in that firm and earning a positive wage.[8] The tenure of workers hired since 1980 can be calculated directly from the IDA. The tenure of workers hired between 1964 and 1980 can be calculated from a second data set on contributions to a pension plan. Job spells starting before 1964 are discarded.

For Portugal, we use *Quadros de Pessoal*, for 1986–2009, which has also been frequently used in earlier research, for example, by Cabral and Mata (2003). The data set is based on a compulsory survey of firms, establishments, and all of their workers. The available information is similar to that for Denmark, except that workers' tenure is directly reported. Industry is defined as the industry with the highest sales share of the firm or, when allocation by sales is impossible, the highest employment share. We use all workers with data records consistent over time in their main job and working for a firm located in Portugal's mainland. Wages are measured with high accuracy.

For both countries, we use data for all private-sector jobs, except for agriculture, fishing, and mining. Potential experience is the worker's age minus the years of schooling minus 6.[9] We first calculate the seniority variable $\log r_{ijt}$ for all workers in all firms. Furthermore, we calculate seniority indices separately for gender and education subgroups. These are defined as an individual's tenure rank within the tenure distribution in the subgroups of male and female employees, respectively, as well as in the subgroups of lower and higher educated workers. We define higher educated workers as those workers with more than 12 years of education. In addition to the seniority measures computed at the firm or firm subgroup-level, we are also able to retrieve establishment tenure and size, which allows us to compute establishment-level seniority for every employee.[10]

For the duration analysis, we use all employment spells of men over 25 years of age. By leaving out younger individuals from the data, we eliminate those who could still be in education, and might have part-time jobs while at school. We exclude women from the duration analysis since they are more likely to leave their job for reasons unrelated to the LIFO rule (in particular, child bearing). Observations for individuals above 55 years of age are also excluded,

[8]We perform a robustness check using the number of registered full-time-equivalent units, instead of this definition. The reported analysis in this paper discards the few firms where the correlation between the two firm-size measures is low, but none of the results are affected if using all firms.

[9]For Denmark, the data allow us to construct actual experience as well. Using actual rather than potential experience does not make a difference for the coefficients on seniority. For the sake of consistency, we report results using potential experience for both countries.

[10]In both countries, we know the firm, but not the establishment, for the unobserved part of ongoing spells at the start of our data observation windows. We assume that such workers have always been attached to their first establishment observed in the data. Hence, firm tenure is better measured than establishment tenure for these individuals.

since, for this group, retirement starts disrupting the application of the LIFO rule; spells started before the age of 55 and finished afterwards are taken as right-censored. Including both unobserved firm and worker effects in the non-linear MPH model is a highly computational-intensive task. Hence, we use only a 5 percent random sample of the observed individuals. Furthermore, we exclude all firms that exist in the data for less than ten years or have fewer than ten employees at any point of time. As discussed in Section 2, the unobserved firm effect is not well identified for smaller firms, leading to biases. Checking the sensitivity of our results to changes in all these thresholds revealed that they matter little for our results.[11]

For the wage analysis, we report estimates using all observations for male employees over 25 years old, for all the firms in our data sets; for each year, we eliminate all observations corresponding to wages lower than the relevant minimum wage, and the upper percentile of the wage distribution.

Summary statistics for the two countries, for the larger sample used in the wage regressions, are presented in Table I. We present statistics for the time-

TABLE I

DESCRIPTIVE STATISTICS FOR DENMARK AND PORTUGAL[a]

Variable	DK 1980–2001	DK 2000	PT 1986–2009	PT 2000
Age	40.90	41.51	40.51	40.51
	(10.54)	(10.65)	(10.49)	(10.52)
Years of education	12.35	12.87	6.86	6.85
	(3.14)	(2.81)	(3.73)	(3.63)
Tenure	6.15	5.77	9.30	9.26
	(6.04)	(6.08)	(9.04)	(9.28)
Experience	22.93	23.41	24.04	24.05
	(11.14)	(10.73)	(10.81)	(10.85)
Log seniority	0.70	0.66	0.86	0.86
	(0.75)	(0.75)	(0.85)	(0.84)
Log firm size	4.70	4.77	4.14	4.05
	(2.33)	(2.35)	(2.11)	(2.14)
Log wage	3.15	3.20	1.52	1.61
	(0.30)	(0.32)	(0.53)	(0.52)
Observations	12,634,236	626,867	15,371,019	725,729
Workers	1,412,646	626,867	2,931,323	725,729
Firms	221,807	60,236	458,888	124,621
Spells	3,456,711	626,867	4,662,627	725,729

[a]Standard deviations of variables appear in parentheses under their means. Wages are expressed in euro and deflated to year-2000 prices. Seniority is computed at firm-level.

[11]For Portugal, tenure is reported in months. We use this information in the estimation. For the rest, the modeling is the same for both countries.

pooled data, and for year 2000 separately. While some statistics (such as the mean age or mean potential work experience) are similar in both countries, there are also several striking differences. The education level in Denmark is five years higher than that in Portugal. Furthermore, Danes stay, on average, almost three and a half years less at a firm than do their Portuguese counterparts. The average firm size in Portugal is about half of that in Denmark. Finally, Danes earn, on average, almost five times as much as the Portuguese.

4. RESULTS

4.1. *The LIFO Separation Rule*

Table II reports the estimation results for the MPH model described in Section 2.1. We report results for specifications with up to three mass points for the worker random effects, with and without correlation between the worker and the firm effect ($\gamma_l = 0$ and $\gamma_l \neq 0$, respectively; see equation (3)), and a specification extended with second-order effects for seniority index and the change in log firm size. In the last column, we replicate our preferred specification for establishments rather than firms.

For both countries and for all specifications, the effect of the seniority index $\log r_{ijt}$ on the hazard rate is negative, in line with the LIFO hypothesis. More than two mass points for the worker random effects do not provide a substantial improvement to the fit of the model and do not change the other coefficients much. In particular, the coefficient on $\log r_{ijt}$ is hardly affected. This coefficient is similarly unchanged by allowing for correlation between the unobserved worker- and firm effects. If we compute job exit odds ratios, based on the reported coefficients for the specification with three mass points for worker random effects and correlated unobserved worker and firm effects, a 10% increase in the seniority of a new entrant in the firm (evaluated at the sample mean of the other observables and unobservables) reduces the hazard rate by about 1.6% in Denmark and 3.4% in Portugal.

Unlike seniority, the coefficients on the firm averages for observed variables change in magnitude when allowing for correlation between the unobserved effects. Workers in growing firms have a smaller probability of leaving the firm than do workers in declining firms. The same applies to firms with a relatively large share of women, higher educated workers, and experienced workers. Unobserved worker effects are more important for explaining the pattern of job separation than are unobserved firm effects, although the estimates of the unobserved heterogeneity distribution for workers are imprecise. The correlation between the worker and firm effects is negative. Second-order terms for the seniority index are insignificant. Finally, replicating the analysis for establishments rather than firms does not change, in any way, our conclusions.

Figure 1 illustrates the impact of tenure and seniority on the hazard rate. The vertical axis represents the change in the index of the hazard logistic func-

TABLE II

JOB EXIT HAZARD ON FIRM OR ESTABLISHMENT SENIORITY, REPORTED FOR MALES[a]

	Firms						Establishments
Specification:	Without Correlation			With Correlation			With Correlation
Mass Points:	1	2	3	2	3	3	3
Denmark							
$\log r_{ijt}$	−0.166	−0.174	−0.174	−0.169	−0.178	−0.270	−0.109
	(0.035)	(0.042)	(0.042)	(0.051)	(0.050)	(0.050)	(0.025)
$(\log r_{ijt})^2$						0.026	
						(0.023)	
$\Delta \log n_{jt}$	−0.009	−0.008	−0.008	−0.007	−0.007	0.077	8.e−4
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)	(0.001)
$(\Delta \log n_{jt})^2$						−0.001	
						(0.e−4)	
Correlated random-effects terms							
Firm averages							
Women	−0.248	−0.380	−0.379	−0.621	−0.585	−0.594	−0.343
	(0.073)	(0.086)	(0.087)	(0.116)	(0.118)	(0.113)	(0.094)
Education	−0.110	−0.137	−0.137	−0.178	−0.163	−0.174	−0.116
	(0.012)	(0.015)	(0.015)	(0.020)	(0.020)	(0.015)	(0.016)
Experience	−0.024	−0.028	−0.028	−0.041	−0.040	−0.044	−0.035
	(0.004)	(0.005)	(0.005)	(0.006)	(0.007)	(0.005)	(0.005)
Std. dev. of χ	0.252	0.e−4	0.e−4	1.400	1.607	1.598	0.e−4
	(0.096)	(0.123)	(0.123)	(0.086)	(0.094)	(0.036)	(0.236)
Std. dev. of v	0	0.966	0.958	2.349	4.656	2.388	0.819
	$(\cdot)$	(0.051)	(0.049)	(0.095)	(82.253)	(23.786)	(0.040)
Corr. of v with χ	0	0	0	−0.719	−0.726	−0.716	0.000
	$(\cdot)$	$(\cdot)$	$(\cdot)$	(0.005)	(0.485)	(1.970)	(0.283)
Mean log likelihood	−1.852	−1.845	−1.845	−1.815	−1.810	−1.808	−1.725
Observations				107,245			90,982
Firms				1,999			2,345
Individuals				19,591			16,368

(*Continues*)

tion (see equation (2)). The shaded area represents the 95 percent confidence bands for the baseline hazards $\psi_{T_{ijt}}$, while the dashed lines are the 1st, 5th, and respectively 9th deciles of the seniority contribution. We find negative duration dependence for both countries, although it is much stronger for Portugal than for Denmark. This result is comparable to other studies (e.g., Topel and Ward (1992) or Altonji, Smith, and Vidangos (2013)). The impact of seniority is larger for longer tenure. For Denmark, the difference in the logistic function contribution to the hazard rate between the 10th and the 90th percentile of

TABLE II—*Continued*

	Firms						Establishments
Specification:	Without Correlation			With Correlation			With Correlation
Mass Points:	1	2	3	2	3	3	3
Portugal							
$\log r_{ijt}$	−0.243	−0.312	−0.312	−0.348	−0.349	−0.472	−0.372
	(0.073)	(0.082)	(0.011)	(0.090)	(0.011)	(0.175)	(0.113)
$(\log r_{ijt})^2$						0.052	
						(0.058)	
$\Delta \log n_{jt}$	−0.306	−0.298	−0.302	−0.302	−0.297	−0.310	−0.157
	(0.056)	(0.057)	(0.057)	(0.058)	(0.002)	(0.093)	(0.074)
$(\Delta \log n_{jt})^2$						0.002	
						(0.093)	
Correlated random-effects terms							
Firm averages							
Women	−0.204	−0.298	−0.298	−0.371	−0.356	−0.373	−0.217
	(0.166)	(0.187)	(0.191)	(0.208)	(0.209)	(0.209)	(0.276)
Education	−0.042	−0.041	−0.041	−0.036	−0.036	−0.042	−0.047
	(0.022)	(0.026)	(0.026)	(0.029)	(0.030)	(0.029)	(0.037)
Experience	−0.032	−0.036	−0.038	−0.049	−0.036	−0.040	−0.057
	(0.008)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(0.014)
Std. dev. of χ	0.001	0.001	0.001	0.920	0.922	0.908	0.781
	(0.230)	(0.267)	(0.272)	(0.179)	(0.188)	(0.187)	(0.322)
Std. dev. of v	0	4.010	1.328	3.147	2.351	2.467	7.295
	$(\cdot)$	(1.265)	(0.675)	(42.960)	(4.610)	(6.637)	(10.982)
Corr. of v with χ	0	0	0	−0.663	−0.656	−0.656	−0.630
	$(\cdot)$	$(\cdot)$	$(\cdot)$	(0.024)	(0.031)	(0.036)	(0.025)
Mean log likelihood	−1.170	−1.167	−1.167	−1.162	−1.162	−1.162	−1.188
Observations				30,674			16,308
Firms				941			692
Individuals				6,209			2,820

[a]The estimation also controls for years of education, initial experience (up to a quartic term), region, and industry indicators. The last column uses seniority, tenure, employment size, and employee averages computed at establishment- rather than at firm-level. The firms and establishments in the estimation sample are selected according to the same rule (see text), from their respective universes in the data. Standard errors in parentheses.

the seniority distribution measured at eight years of tenure is 0.15. This difference is about the same magnitude as the effect of the negative duration dependence. For Portugal, the effect is about twice as large. However, the difference in the effect of negative duration dependence between the two countries is much larger, such that, in Portugal, the effect of negative duration dependence dominates the effect of seniority.

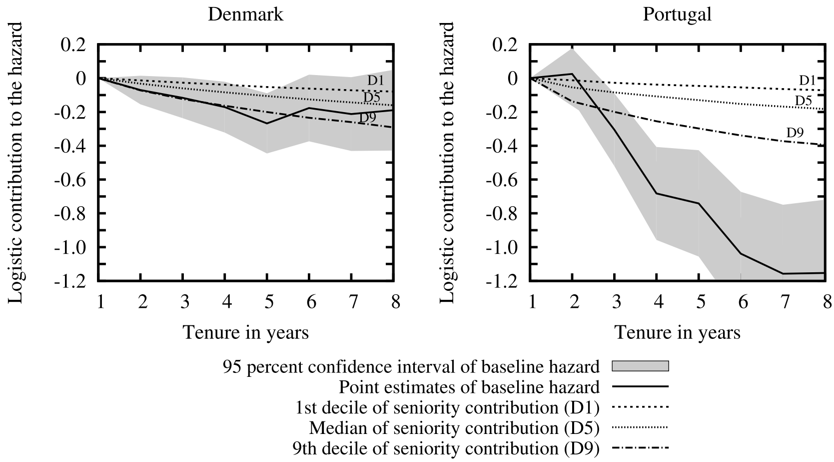

FIGURE 1.—Baseline hazards and the contributions of average seniority to the hazard for different levels of the tenure. The baseline hazards are for the model with three different mass points.

4.2. *The Return to Seniority*

Following a large empirical literature on wage dynamics, we start by checking the characteristics of the dynamic process of ν_{ijt} (see equation (6)). For both countries, the covariance of $\Delta\nu_{ijt}$ with its first lag is substantial and the covariance with higher lags is negligible. The process is, therefore, well approximated by an MA(1), a mixture of permanent and transitory shocks. The standard deviation of the permanent shocks is 0.10 for Denmark and 0.12 for Portugal.[12] These findings are of the same order of magnitude as those reported by Abowd and Card (1989) and Topel and Ward (1992) for the United States.

Table III reports our main estimates for the return to seniority on wages of male workers. As described in Section 2.2, our regressions control for up to a quartic term in tenure and experience, and for log firm size. Furthermore, we include up to a quartic in education years, and dummy variables for industry, region, and calendar time. We present two specifications, with and without the seniority index $\log r_{ijt}$. All estimated coefficients for the seniority index $\log r_{ijt}$ are positive and statistically highly significant.

The OLS results stand out in their magnitude for the coefficient on $\log r_{ijt}$. This is the only estimation method that also applies between-job-spell variation for the estimation of the return to seniority. As for our two main methods, the coefficients are somewhat larger for Altonji and Shakotko's (1987) method (job-spell fixed effects) than for Topel's (1991) (job-spell first-differences). This confirms our expectation, since Altonji and Shakotko's estimation procedure

[12]See the Supplemental Material for the results.

TABLE III

LOG WAGE REGRESSIONS ON FIRM SENIORITY, REPORTED FOR MALES[a]

	OLS		Topel		Altonji and Shakotko		Topel With Spell Fixed Effects	
	I	II	I	II	I	II	I	II
Denmark								
$\log r_{ijt}$		0.036		0.008		0.009		0.010
		(2.e−4)		(3.e−4)		(5.e−4)		(4.e−4)
$\log n_{jt}$	0.016	0.016	0.017	0.012	0.031	0.026	0.016	0.009
	(4.e−5)	(4.e−5)	(2.e−4)	(3.e−4)	(3.e−4)	(5.e−4)	(2.e−4)	(4.e−4)
X_{ijt}	−0.004	−0.004	0.048	0.048	0.032	0.032	0.028	0.028
	(2.e−4)	(2.e−4)	(4.e−4)	(4.e−4)	(3.e−4)	(3.e−4)	(3.e−4)	(0.004)
T_{ijt}	0.017	0.007	−0.006	−0.008	0.007	0.004	0.001	−0.002
	(2.e−4)	(2.e−4)	(9.e−4)	(9.e−4)	(1.e−4)	(2.e−4)	(0.005)	(0.006)
T_{ijt}^2	−0.174	−0.100	0.118	0.136	−0.025	−0.008	0.143	0.159
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)
T_{ijt}^3	0.074	0.046	−0.062	−0.070	0.008	0.001	−0.073	−0.080
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
T_{ijt}^4	−0.010	−0.006	0.010	0.011	−3.e−4	4.e−4	0.011	0.012
	(2.e−4)	(2.e−4)	(3.e−4)	(3.e−4)	(2.e−4)	(2.e−4)	(4.e−4)	(4.e−4)
R^2	0.068	0.071	0.031	0.031	0.211	0.221	0.023	0.023
Observations	12,275,995		8,597,167		12,275,995		8,597,167	
Portugal								
$\log r_{ijt}$		0.033		0.015		0.022		0.018
		(2.e−4)		(4.e−4)		(5.e−4)		(7.e−4)
$\log n_{jt}$	0.090	0.088	0.024	0.014	0.056	0.043	0.018	0.006
	(1.e−4)	(1.e−4)	(3.e−4)	(4.e−4)	(4.e−4)	(5.e−4)	(5.e−4)	(7.e−4)
X_{ijt}	−0.141	−0.142	0.069	0.069	0.061	0.059	0.082	0.081
	(4.e−4)	(4.e−4)	(7.e−4)	(4.e−5)	(6.e−4)	(6.e−4)	(5.e−5)	(5.e−5)
T_{ijt}	0.036	0.031	0.017	0.015	0.017	0.015	0.016	0.016
	(1.e−4)	(1.e−4)	(0.001)	(0.001)	(1.e−4)	(2.e−4)	(0.016)	(0.032)
T_{ijt}^2	−0.250	−0.213	−0.078	−0.061	−0.052	−0.038	−0.031	−0.021
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)	(0.007)
T_{ijt}^3	0.074	0.062	0.025	0.019	0.013	0.008	0.033	0.029
	(5.e−4)	(5.e−4)	(9.e−4)	(9.e−4)	(6.e−4)	(6.e−4)	(0.001)	(0.001)
T_{ijt}^4	−0.007	−0.006	−0.003	−0.002	−0.001	−5.e−4	−0.004	−0.003
	(1.e−4)	(1.e−4)	(1.e−4)	(1.e−4)	(1.e−4)	(1.e−4)	(2.e−4)	(2.e−4)
R^2	0.239	0.240	0.033	0.033	0.507	0.509	0.004	0.004
Observations	15,371,019		9,191,177		15,371,019		9,191,177	

[a]The dependent variable is the log real hourly wage. All regressions also control for further potential experience terms up to a quartic, for up to a quartic in years of education, and for time, region, and industry indicators. Reported coefficients for higher order polynomials in tenure are multiplied by corresponding powers of 10. For the last two columns, we report tenure effects averaged over job spells, assuming homogeneous returns to experience (see the discussion in the Supplemental Material). Standard errors in parentheses.

implicitly allows for a lagged effect of $\log r_{ijt}$ on $\log w_{ijt}$; see the discussion on equation (9). According to these methods, the effect of $\log r_{ijt}$ is twice as high in the Portuguese data. Given a 10% increase in a worker's seniority level, the estimated impact on his wage is about 0.15 to 0.20% if he is employed in Portugal, and half this range if he is employed in Denmark. The final columns report estimates for the method that allows for heterogeneity in the returns to experience and tenure, as in equation (10). For both countries, the estimated returns to seniority are hardly affected by applying this method. The reported coefficients for tenure terms (identified and estimated assuming homogeneous linear returns to experience; see the detailed discussion in the Supplemental Material) are averages over job spells; the linear job-spell-averaged tenure coefficients are not statistically significant, given very little variation left in the data. Comparing, for each specification of Table III, the estimation results with and without seniority, we note that including seniority reduces the coefficients for log firm size and for the tenure terms by 5–65% (except for the effect of tenure in Denmark, which is small anyway). The coefficients for experience are hardly affected by the inclusion of seniority. This suggests that the effects of tenure and firm size on wages are at least partly proxies for the effect of seniority.

Could measurement error in tenure explain our results? Measurement error in tenure is a general problem in the research on the wage returns to tenure. Apart from reporting errors, a main source of measurement error is the exact definition of a worker's employer. Some job changes might be classified either as "between firms," justifying the tenure clock being set back to zero, or as "within the firm," which does not affect the tenure clock. In general, measurement error reduces the estimated returns to tenure, while it may lead to an overestimation of the returns to other variables which are correlated with tenure, such as experience. But what happens if both seniority and tenure are included in the same regression, as we do here? We expect the measurement error in seniority to be larger than the measurement error of tenure, as misclassification of the years of tenure of even a single worker can affect the measurement of seniority of all his co-workers. For the same reason, the measurement error is likely to be higher for seniority than for firm size. Moreover, the empirically relevant seniority index might not be based on the total workforce of the firm, but on co-worker subgroups. Therefore, seniority is unlikely to be just a proxy for tenure or firm size. The other way around is much more likely: part of the estimated effect of tenure and log firm size might be a proxy for measurement error in the seniority variable, and the actual effect of seniority on wages can be expected to be larger than estimated here.[13]

[13]In principle, the misclassification problem here—who is the relevant employer, and thus which is the relevant seniority hierarchy—can be addressed by adapting the approach used by Keane and Sauer (2009), who modeled measurement error in the reported individual employment status, in the context of a dynamic labor supply model (see their Section 2.2, equation (6)). Further investigation of this issue is, however, beyond the scope of our current paper.

Table IV documents the impact of seniority and cumulated tenure on the within-job wage changes, for different years of tenure. The wage return to tenure is highest in Portugal: after ten years of tenure, it is 12–16%, depending on whether Topel's or Altonji and Shakotko's method is applied. The equivalent numbers for Denmark are only 0.5–4.5%. Using the Topel method that allows for spell fixed effects gives statistically insignificant job-spell-averaged tenure estimates for Denmark, and borderline significant ones for Portugal. This is not unexpected, since the estimated variances for the job-spell-averaged linear tenure terms in the last two columns of Table III were already large. Conditional on 10 years of tenure, the wage differential between the 10th and the 90th percentiles in the seniority distribution is 1.1–1.4 percentage points in Denmark and 2.3–3.4 percentage points in Portugal. Taking those numbers at face value, this implies that the effect of 10 years of tenure on wages is larger than the effect of a shift in the seniority distribution from the 10th to the 90th percentile. For shorter tenures, the effect of seniority is smaller, as there is less variation in seniority at lower tenure (lower tenures are more likely to come from junior workers, while, for higher tenures, the degree of seniority depends on the growth of the firm after the worker has been hired).

Table V reports a number of robustness checks. The first consists of including a full second-order polynomial of $\log r_{ijt}$ and $\log n_{jt}$. No consistent conclusion can be drawn from these higher-order effects across the two countries or across estimation methods. Although the first-order effect at zero seniority and small firm size becomes negative in some cases,[14] the effect of seniority remains positive in the sample mean of $\log r_{ijt}$ and $\log n_{jt}$. Hence, including second-order terms does not change our conclusions.

For the second robustness check, we include two lags of $\log r_{ijt}$ and $\log n_{jt}$. The total impact of $\log r_{ijt}$ and its lags exceeds the impact of the unlagged seniority index in the case of Topel's method, for Portugal. This is also the country where there is a substantial difference between the results for Topel's and Altonji and Shakotko's method in Table III. This squares well with our prior observation that Altonji and Shakotko's method picks up lagged effects better (see the discussion of equation (9)). We conclude that although—and as expected—the presence of lagged effects matters to some extent in terms of magnitude differences between the estimates obtained via our two methods, once again the main implication is very robust.

The third robustness check deletes the upper 25% of the changes in $\log q_{ijt}$. This exercise tests whether some large changes in $\log q_{ijt}$ drive our results. For Denmark, eliminating the upper 25% of the variation in $\log q_{ijt}$ takes out the observations that contribute most to identification, reducing the significance of the coefficient on log seniority and even rendering it statistically insignificant for one estimation method. For Portugal, the impact is less pronounced,

[14]By construction, the smallest log firm size is equal to zero. Hence, the first-order effect of seniority measures the marginal effect of seniority at the lowest seniority and the lowest firm size.

TABLE IV

EFFECT OF FIRM SENIORITY ON LOG WAGES OF MALES, BY CUMULATED YEARS OF TENURE[a]

Tenure (in years)	Topel				Altonji and Shakotko				Topel With Spell Fixed Effects			
	Tenure	Seniority			Tenure	Seniority			Tenure	Seniority		
		D1	D5	D9		D1	D5	D9		D1	D5	D9
Denmark												
2	−0.009	4.e−4	0.002	0.005	0.010	5.e−4	0.002	0.005	0.005	5.e−4	0.002	0.006
	(0.002)	(1.e−5)	(1.e−4)	(2.e−4)	(4.e−4)	(3.e−5)	(1.e−4)	(3.e−4)	(0.013)	(2.e−5)	(1.e−4)	(2.e−4)
5	−0.009	0.002	0.005	0.010	0.025	0.003	0.006	0.012	0.030	0.003	0.007	0.013
	(0.005)	(1.e−4)	(2.e−4)	(4.e−4)	(0.001)	(1.e−4)	(3.e−4)	(7.e−4)	(0.032)	(1.e−4)	(3.e−4)	(5.e−4)
10	0.005	0.004	0.009	0.015	0.045	0.005	0.010	0.018	0.089	0.006	0.011	0.020
	(0.009)	(2.e−4)	(3.e−4)	(6.e−4)	(0.003)	(3.e−4)	(5.e−4)	(0.001)	(0.064)	(2.e−4)	(4.e−4)	(8.e−4)
15	0.015	0.006	0.012	0.019	0.063	0.007	0.014	0.023	0.146	0.008	0.015	0.025
	(0.014)	(2.e−4)	(4.e−4)	(8.e−4)	(0.008)	(4.e−4)	(7.e−4)	(0.001)	(0.097)	(3.e−4)	(6.e−4)	(0.001)
20	0.011	0.008	0.014	0.023	0.082	0.010	0.017	0.028	0.185	0.009	0.017	0.028
	(0.023)	(3.e−4)	(5.e−4)	(9.e−4)	(0.016)	(5.e−4)	(9.e−4)	(0.002)	(0.133)	(4.e−4)	(7.e−4)	(0.001)
Portugal												
2	0.033	0	0.004	0.013	0.041	0	0.006	0.019	0.029	0	0.005	0.016
	(0.003)	(0.e−4)	(1.e−4)	(4.e−4)	(7.e−4)	(0.e−4)	(1.e−4)	(4.e−4)	(0.013)	(0.e−4)	(3.e−4)	(0.001)
5	0.071	0.001	0.008	0.021	0.092	0.002	0.011	0.030	0.058	0.002	0.009	0.025
	(0.007)	(4.e−5)	(2.e−4)	(6.e−4)	(9.e−4)	(4.e−5)	(3.e−4)	(7.e−4)	(0.031)	(1.e−4)	(5.e−4)	(0.002)
10	0.117	0.004	0.012	0.027	0.163	0.005	0.017	0.039	0.080	0.004	0.015	0.033
	(0.014)	(1.e−4)	(3.e−4)	(7.e−4)	(0.002)	(1.e−4)	(4.e−4)	(9.e−4)	(0.063)	(3.e−4)	(9.e−4)	(0.002)
15	0.151	0.005	0.014	0.031	0.223	0.008	0.021	0.045	0.084	0.006	0.017	0.038
	(0.022)	(1.e−4)	(4.e−4)	(8.e−4)	(0.005)	(2.e−4)	(4.e−4)	(0.001)	(0.095)	(4.e−4)	(0.001)	(0.002)
20	0.180	0.008	0.018	0.036	0.281	0.011	0.026	0.052	0.081	0.010	0.022	0.044
	(0.030)	(2.e−4)	(5.e−4)	(0.001)	(0.009)	(3.e−4)	(6.e−4)	(0.001)	(0.130)	(6.e−4)	(0.001)	(0.003)

[a] All effects are based on the estimates reported in Table III. The deciles of seniority are calculated conditional on tenure. Standard errors in parentheses.

TABLE V

ROBUSTNESS LOG WAGE REGRESSIONS ON FIRM OR ESTABLISHMENT SENIORITY, REPORTED FOR MALES[a]

	Topel				Altonji and Shakotko				Topel With Spell Fixed Effects			
	Polynomial	Lag Structure	Upper 25% Deleted	Establishments	Polynomial	Lag Structure	Upper 25% Deleted	Establishments	Polynomial	Lag Structure	Upper 25% Deleted	Establishments
Denmark												
$\log r_{ijt}$	−0.013	0.017	0.037	0.008	−0.004	0.025	4.e−4	0.022	0.015	0.013	0.001	0.002
	(5.e−4)	(4.e−4)	(9.e−4)	(2.e−4)	(8.e−4)	(0.001)	(5.e−4)	(3.e−4)	(7.e−4)	(0.002)	(5.e−4)	(3.e−4)
$(\log r_{ijt})^2$	−6.e−4				0.001				0.003			
	(2.e−4)				(2.e−4)				(2.e−4)			
$\log r_{ij,t-1}$		0.004				0.009				0.002		
		(5.e−4)				(0.001)				(7.e−4)		
$\log r_{ij,t-2}$		0.001				−0.031				0.003		
		(4.e−4)				(0.002)				(6.e−4)		
$\log n_{jt}$	0.021	0.006	−0.016	0.158	0.040	0.032	0.017	0.030	0.002	0.007	0.018	0.017
	(5.e−4)	(4.e−4)	(9.e−4)	(2.e−4)	(8.e−4)	(0.001)	(3.e−4)	(3.e−4)	(7.e−4)	(5.e−4)	(0.001)	(2.e−4)
$(\log n_{jt})^2$	−0.002				−0.002				0.002			
	(1.e−4)				(1.e−4)				(1.e−4)			
$\log r_{ijt} \times \log n_{jt}$	0.006				0.002				−0.004			
	(1.e−4)				(1.e−4)				(2.e−4)			
$\log n_{j,t-1}$		0.002				0.025				−6.e−4		
		(4.e−4)				(9.e−4)				(6.e−4)		
$\log n_{j,t-2}$		0.003				−0.001				−0.002		
		(4.e−4)				(1.e−4)				(5.e−4)		
R^2	0.032	0.046	0.034	0.030	0.230	0.156	0.233	0.232	0.023	0.034	0.025	0.023
Observations	8,597,167	4,985,101	5,568,885	8,573,782	12,275,995	6,459,830	10,129,966	12,268,564	10,129,966	4,985,101	6,450,277	8,573,782

(*Continues*)

TABLE V—*Continued*

	Topel				Altonji and Shakotko				Topel With Spell Fixed Effects			
	Polynomial	Lag Structure	Upper 25% Deleted	Establishments	Polynomial	Lag Structure	Upper 25% Deleted	Establishments	Polynomial	Lag Structure	Upper 25% Deleted	Establishments
Portugal												
$\log r_{ijt}$	0.012	0.015	0.015	0.015	0.013	0.050	0.021	0.021	−0.002	0.012	0.030	0.015
	(8.e−4)	(8.e−4)	(0.002)	(4.e−4)	(9.e−4)	(0.003)	(5.e−4)	(5.e−4)	(0.001)	(0.001)	(0.002)	(7.e−4)
$(\log r_{ijt})^2$	−0.003				−0.0002				−0.008			
	(2.e−4)				(2.e−4)				(3.e−4)			
$\log r_{ij,t-1}$		0.007				−0.015				0.004		
		(8.e−4)				(0.003)				(0.001)		
$\log r_{ij,t-2}$		−0.001				−0.010				−0.001		
		(8.e−4)				(0.002)				(0.001)		
$\log n_{jt}$	0.015	0.014	0.015	0.012	0.046	0.038	0.044	0.038	0.012	0.012	−0.005	0.007
	(8.e−4)	(7.e−4)	(0.002)	(4.e−4)	(9.e−4)	(0.003)	(5.e−4)	(4.e−4)	(0.001)	(0.001)	(0.003)	(7.e−4)
$(\log n_{jt})^2$	−4.e−4				−5.e−4				−0.003			
	(1.e−4)				(1.e−4)				(2.e−4)			
$r_{ijt} \times n_{jt}$	0.003				0.002				0.013			
	(2.e−4)				(2.e−4)				(4.e−4)			
$\log n_{j,t-1}$		0.001				0.024				6.e−4		
		(8.e−4)				(0.002)				(0.001)		
$\log n_{j,t-2}$		0.009				−2.e−4				0.008		
		(8.e−4)				(0.002)				(0.001)		
R^2	0.033	0.032	0.035	0.033	0.509	0.526	0.509	0.503	0.005	0.011	0.008	0.004
Observations	9,191,177	3,844,432	6,080,043	8,727,604	15,371,019	5,940,218	15,338,663	15,737,962	9,191,177	3,844,432	6,893,442	8,727,604

[a] The dependent variable is the log real hourly wage. All regressions also control for tenure, potential experience, and years of education up to a quartic, and time, region, and industry indicators. Standard errors in parentheses.

though there is a large change in the seniority magnitude for the Topel using spell fixed effects, compared to Table III. Large changes in $\log q_{ijt}$ are thus important for identification and estimation of the seniority effect—and we notice that particularly for the Danish data. Hence, it is crucial to work with the type of data we use: exhaustive in terms of both the panel and the cross-sectional dimensions.

Since there is no a priori reason why we should prefer a firm-level to an establishment-level analysis (see Brown and Medoff (1989)), our final robustness check looks at the impact of the worker seniority within establishments. Results obtained with all of our three estimation methods, and for both countries, reveal the same qualitative implications as the estimates obtained for the firms. There are only some quantitative differences for Denmark, relative to the firm-level seniority estimates in Table III: the magnitude of the seniority estimate is considerably larger at the establishment level when estimated with Altonji and Shakotko's method, and respectively smaller if estimated with the method of Topel with fixed spell effects.

4.2.1. *Returns to Seniority Within Gender and Education Subgroups*

The LIFO layoff rule is unlikely to apply for the workforce as a whole. Instead, one would expect the firm to apply separate layoff orders for different subgroups of its workforce. For example, a construction firm is unlikely to fire its secretaries if it has an excess supply of bricklayers, whatever the difference in seniority at the firm level between these two types of workers. One can therefore expect the theory to work better when using separate seniority indices for subgroups of the workforce. Data limitations prohibit us to consistently classify workers according to their occupation, over the whole time and cross-sectional dimensions of our data sets. Moreover, doing so would be problematic, because the promotions that accompany an increase in seniority are likely to change the occupation title of the job, thereby missing part of what is a genuine return to seniority. Hence, we have to revert to broad demographic groups, like males versus females or higher- versus lower-educated workers. For the same reason as in the case of seniority computed at the entire firm level, we report results using only male observations for the subgroups of low- and high-educated workers.

The estimates are reported in Table VI. The size variable, $\log n_{jt}$, is, in this table, the size of the relevant seniority subgroup. The results for the seniority subgroup of men are similar to the results in Table III. Seniority has a larger impact for men than for women, although the difference is small. The effect of seniority is larger for high- than for low-educated workers, except for Portugal, when using the method of Altonji and Shakotko, where the result is statistically insignificant.[15] These results are consistent with the fact that high-educated

[15]The number of observations for high-educated workers is relatively low in Portugal, and taking within-job-spell deviations from means removes most of the variation in the data, necessary

TABLE VI

LOG WAGE REGRESSIONS ON FIRM SUBGROUP-SPECIFIC SENIORITY, REPORTED FOR MALES[a]

	High Education		Low Education		Males		Females	
	Topel	Altonji and Shakotko	Topel	Altonji and Shakotko	Topel	Altonji and Shakotko	Topel	Altonji and Shakotko
Denmark								
$\log r_{ijt}$	0.014	0.019	0.004	−0.004	0.009	0.010	0.009	0.005
	(4.e−4)	(6.e−4)	(5.e−4)	(6.e−4)	(3.e−4)	(5.e−4)	(4.e−4)	(6.e−4)
$\log n_{jt}$	0.006	0.020	0.012	0.020	0.011	0.024	5.e−4	0.015
	(3.e−4)	(6.e−4)	(5.e−4)	(7.e−4)	(3.e−4)	(4.e−4)	(4.e−4)	(6.e−4)
X_{ijt}	0.047	0.043	0.032	0.005	0.048	0.031	0.037	0.018
	(5.e−4)	(4.e−4)	(0.001)	(0.001)	(4.e−4)	(3.e−4)	(6.e−4)	(0.001)
T_{ijt}	−0.013	0.003	−0.002	0.004	−0.008	0.004	0.002	0.005
	(0.001)	(2.e−4)	(0.003)	(3.e−4)	(9.e−4)	(2.e−4)	(0.001)	(3.e−4)
R^2	0.035	0.255	0.025	0.093	0.031	0.225	0.032	0.204
Observations	5,948,533	8,443,390	2,631,458	3,814,024	8,597,167	12,275,995	4,368,909	6,288,217
Portugal								
$\log r_{ijt}$	0.023	−0.001	0.014	0.017	0.014	0.013	0.008	0.002
	(0.002)	(0.003)	(4.e−4)	(6.e−4)	(4.e−4)	(5.e−4)	(4.e−4)	(6.e−4)
$\log n_{jt}$	0.009	0.040	0.012	0.042	0.012	0.046	0.011	0.048
	(0.002)	(0.002)	(4.e−4)	(7.e−4)	(4.e−4)	(5.e−4)	(5.e−4)	(5.e−4)
X_{ijt}	0.083	0.077	0.057	0.054	0.076	0.059	0.060	0.059
	(0.002)	(0.002)	(9.e−4)	(7.e−4)	(7.e−4)	(7.e−4)	(7.e−4)	(6.e−4)
T_{ijt}	0.032	0.040	0.006	0.013	0.007	0.016	0.013	0.019
	(0.005)	(9.e−4)	(0.002)	(2.e−4)	(0.002)	(2.e−4)	(0.002)	(2.e−4)
R^2	0.067	0.267	0.030	0.425	0.033	0.508	0.034	0.033
Observations	643,477	1,084,463	8,745,904	14,286,518	9,400,472	15,371,019	6,963,628	14,286,518

[a]The dependent variable is the log real hourly wage. Seniority and employment size are computed for each of the gender or education subgroup of co-workers within firms. For the education subgroups, we report estimates only for males. All regressions also control for further tenure and potential experience terms up to a quartic, up to a quartic in years of education, and time, region, and industry indicators. Standard errors in parentheses.

workers have steeper wage-tenure profiles than their low-educated peers. At the same time, these results lend support to the idea that the relevant seniority index is not defined for the firm as a whole, but for various co-worker subgroups within the firm.

5. CONCLUSION

A dynamic version of Kuhn's (1988) and Kuhn and Robert's (1989) model of layoff ordering suggests that firms and their workers are induced to agree on the firm applying a LIFO rule for its layoffs, firing junior workers with short tenure prior to senior workers with long tenure. Senior workers can use this insulation from the direct threat of being laid off to demand higher wages. This paper provides empirical evidence for these effects.

We have shown first that, other things equal, senior workers face a smaller job separation hazard, and second, that there exists a return to seniority in wages, both in Denmark and in Portugal. A 10% increase in the seniority of a new entrant in the firm reduces the hazard of separation by approximately 1.6% in Denmark and 3.4% in Portugal. These results hardly vary with the number of mass points for worker heterogeneity, whether or not one allows for correlation between the worker and firm unobservables, when using a more flexible specification of seniority, or whether LIFO is tested at establishment- rather than at firm-level. The difference in the contribution to the hazard rates between the 10th and the 90th percentile of the seniority distribution, conditional on 10 years of tenure, has the same magnitude as the effect of the negative duration dependence in Denmark. This effect is twice as large, but dominated by the effect of the negative duration dependence, in Portugal.

Similarly, we have shown that a 10% increase in seniority raises wages by up to 0.1% in Denmark and up to 0.2% in Portugal. The return to seniority in wages is therefore small, but statistically highly significant. Again, these results are stable across various estimation methods, and do not change much whether the estimation is performed at firm- or at establishment-level. Conditional on 10 years of tenure, the wage differential between the 10th and the 90th percentile of the seniority distribution increases workers' wages by up to 1.4 percentage points in Denmark and 3.4 percentage points in Portugal. The effects are larger for men than for women, and for higher-educated than for lower-educated employees. Likely, these effects are lower bounds for the true effects, since seniority is measured less precisely than are firm size and tenure, given that the measurement error in the latter two variables automatically feeds into the seniority variable.

Denmark is known for its "flexicurity" model. Its labor market is more flexible than that of Portugal, where insider interests are protected by extensive

to identify the seniority effect. Therefore, this result is not very robust. We find a much larger impact if we do robustness checks in a way similar to that presented in Table V.

employment protection legislation. One would thus expect seniority to be more important in the latter country. The estimation results confirm this idea. We have established the existence of a LIFO rule and a return to seniority for Denmark and for Portugal. Whether these phenomena exist in other countries—in particular, in the United States—remains an open question. Given the fact that the labor market institutions in the United States are more akin to those in Denmark than those in Portugal, one would expect small effects for the United States.

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