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A Quantitative Assessment and Solution

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ABSTRACT

Bias in Returns to Tenure When Firm Wages and Employment Comove:
A Quantitative Assessment and Solution[*]

It is well known that, unless worker-firm match quality is controlled for, returns to firm tenure (RTT) estimated directly via reduced form wage (Mincer) equations will be biased. In this paper we argue that even if match quality is properly controlled for there is a further pervasive source of bias, namely the co-movement of firm employment and firm wages. In a simple mechanical model where human capital is absent and separation is exogenous we show that positively covarying shocks (either aggregate or firm level) to firm's employment and wages cause downward bias in OLS regression estimates of RTT. We show that the long established procedures for dealing with "traditional" RTT bias do not circumvent the additional problem we have identified. We argue that if a reduced form estimation of RTT is undertaken, firm-year fixed effects must be added in order to eliminate this bias. Estimates from two large panel datasets from Portugal and Germany show that the bias is empirically important. Adding firm-year fixed effects to the regression increases estimates of RTT in the two respective countries by between 3.5% and 4.5% of wages at 20 years of tenure — over 80% (50%) of the estimated RTT level itself. The results extend to tenure correlates used in macroeconomics such as the minimum unemployment rate since joining the firm. Adding firm-year fixed effects changes estimates of these estimates also.

JEL Classification: J31, J63, C23

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1 Introduction and Overview

A rigorous understanding of returns to firm tenure - typically regarded as a measure of firm-specific human capital - is important for a host of reasons. In particular it can shed light on the impact of training or on the job learning on labour productivity. Alternatively, it may inform our understanding of the importance of bargaining and rent sharing in firms. The magnitude of returns to tenure can also determine the importance of the internal labour market compared to its external counterpart. Finally, returns to tenure have important implications in terms of wage inequality, the costs of unemployment and the debate on labour-market segmentation.

Traditionally labour economists use coefficient estimates on deterministic tenure in a Mincer equation as a measure of returns to tenure (henceforth RTT). This approach - which we refer to as a reduced form approach - is easy to implement and avoids making structural economic assumptions about worker entry and exit from the firm. However and as is now well known, the existence of unobservable worker-firm-match quality means that the OLS regression of wages on tenure gives upward biased estimates of RTT. Several methods — most notably the two step estimator of Topel (1991) and the IV approach of Altonji and Shaktoko (1987) — have been used to circumvent this problem (for a very recent example of an application of both of these methods see Devereux et al., 2013). More recently the emergence of very large panel datasets and advances in computing power have allowed investigators to absorb unobserved worker and match quality by adding firm-worker match fixed effects (see for example Battisti, 2012).

In this paper we identify a new and potentially pervasive source of bias, namely the existence of a time varying firm wage component that co-moves with firm employment. We show that even in a world where match quality is irrelevant, if positive firm wage/employment co-movements exist, the failure to account for them will bias estimates of returns to tenure downwards. The mechanism generating the bias is the following; firms that have a relatively high (low) wage at time t (i.e., relative to average wages at that firm) will have a relatively high (low) employment at t, hence high (low) hiring at t and hence relatively low (high) average firm tenure at t. Tenure is then spuriously negatively correlated with wages. We show that traditional estimators — ones designed to eliminate the effects of unobservable worker/firm match quality — are not immune to the bias arising from this effect.

Drivers of a firm's wage/employment co-movements may include both aggregate (business cycle) shocks and idiosyncratic shocks. In both cases the shocks that are the root cause of the problem are assumed to impact all workers in the firm, i.e., they are common components of wages. Because these components are the same for each worker in a firm we propose that if a reduced form method is undertaken then the components should be removed via the addition of firm-year interaction fixed effects to panel wage regressions. At the same time one must add match fixed effects to control for the more traditional match quality problem. In an empirical application we use large matched panel datasets from Germany and Portugal to show that the bias is empirically important — adding firm-year fixed effects

(henceforth FYFE) to wage equations (whilst controlling for worker-firm match quality) increases estimated tenure effects in the two countries by around 1.5% and 4% of wages, respectively, at 10 years of tenure, and 3.5% and 4.5% of wages at 20 years of tenure. As a proportion of RTT itself these biases exceed 80% for Portugal at ten years of tenure. For Germany the corresponding number is just below 20% rising to 50% at 20 years of tenure. An interesting aspect of our findings is that the bias is driven by idiosyncratic firm level wage/employment co-movement and not aggregate co-movement. An implication of this is that simple fixes such as controlling for the aggregate cycle via the addition of year fixed effects and/or controlling for the level of firm employment (with a single coefficient) will not work. Although investigators may have been aware of some facets of the problem we have highlighted (see for example a discussion in Topel, 1991, on high wage/employment growth firms or the discussion in Buhai, et al., 2014 on the "size" effect and tenure), to the best of our knowledge we are the first to formally analyse it, quantify its importance and propose a simple solution.

A further implication of our results is that using variables that interact macroeconomic variables such as unemployment with deterministic tenure will also result in biased inference. Canonical examples of such variables are Beaudry and DiNardo's (1991) minimum unemployment rate during a worker's tenure ("$minu$") and a new hire dummy interacted with unemployment to measure the incremental cyclicality of new hire wages.[1] In an extension to the empirics we quantify these biases as well. The empirical importance of these variates found in the literature adds a further twist because their omission will be yet another source of bias to RTT estimates. We do not model the impact on RTT of omitting these variates theoretically. Instead we quantify the impact in our empirical section.

The key result in this paper – that there is another source of pervasive bias to RTT estimates obtained via reduced form estimation – may lead the investigator to conclude that the safest way to proceed is via a fully specified structural model of worker mobility (see Buchinsky. et al., 2010, for a recent example of such a model). However one key finding of our work is that it is firm specific (heterogeneous) co-movement that drives the biases we find and not macro (aggregate) effects. A structural model with heterogeneous firm wage/hiring co-movements may be hard to specify and identify empirically. Additionally, estimates obtained from structural models are only as good as the veracity of their underlying assumptions. As far as reduced form modeling goes, our paper has a clear message. To avoid substantial RTT bias one must not only control for worker–firm match quality (as per tradition) but also for FYFE.

The paper is laid out as follows. Section 2 revisits the traditional econometric model of RTT and the implications for wages. We outline the two canonical estimation methods

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[1]Of course if wages are solely determined by a Beaudry Di Nardo style mechanism where an individual worker's wage is independent of firm hiring, then the positive comovements of wages and employment driving our bias will not exist. But there is strong empirical evidence that wage mechanisms vary across firms and sectors. We view the coexistence of e.g. $minu$ effects with equal treatment wage components in a panel as reflecting this heterogeneity and coefficient estimates on $minu$ as representing an average effect across the sample.

of Topel (1991) and Altonji and Shakotko (1987) devised to deal with unobserved worker firm match quality. We then show in section 3 that, in a simple mechanical model with exogenous separation and no human capital (match or firm specific), positive co-movement of firm wages and employment leads to downwards bias in the RTT estimates whatever method is used. Section 4 contains the empirical results whilst section 5 summarises and concludes.

2   Bias in Estimating Returns to Tenure Arising from Match Quality

We start with a traditional — and somewhat simplified[2] — archetypal model of RTT. We assume that log wages w_{ijt} for worker i in firm j at time t are given by

$$w_{ijt} = \alpha + \beta\tau_{ijt} + \gamma E_{it} + \delta^{'} X_{ijt} + \varepsilon_{ijt} \qquad \text{(Model A)}$$

$$\text{where} \quad \varepsilon_{ijt} = \phi_i + \theta_{ij} + u_{ijt} \qquad \text{(Error A)}$$

where τ_{ijt} is the worker's tenure, E_{it} is her lifetime work experience and X is a vector of other controls. The error consists of a worker fixed effect ϕ_i, job match quality θ_{ij} and an idiosyncratic error uncorrelated with the regressors. Model A makes clear what we mean by RTT. It is the incremental wage received within the firm by all incumbent workers for each year of completed tenure.[3] The definition may be loosened to allow heterogenous (across firms) tenure related wage growth. In this case the estimates could interpreted as average RTT across firms with weights given by firm employment within the sample - analagous to average treatment effects in the experimental literature.

The problem arises when the job match quality θ_{ij} is correlated with worker $i's$ tenure. When the match is good (high θ_{ij}) the worker's separation hazard may fall (see in particular Bowlus, 1995) and expected tenure will rise. This biases upwards the RTT estimate β. As noted above one solution to this problem is to explicitly model the entry and exit decisions of workers via a fully specified structural economic model. Following on our previous discussion, here we only consider reduced form estimation. In this context the most popular empirical solutions to the problem of endogenous match quality are the methods of Topel (1991) and Altonji and Shakotko (1987).

Topel's method first differences incumbents' wages to remove the (presumed constant) match quality and worker fixed effect. Regressing these incumbent wage changes on an

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[2] In particular more general specifications — as in our empirical section — would include a polynomial in tenure whereas for expositional clarity we have a single linear term.

[3] As we have noted already if there are also differential (across tenure) macro effects on wages these must be controlled for as well. The cases we explore later in the paper are differential new hire wages as modelled via inclusion of a new hire dummy interacted with the unemployment rate and the impact of implicit contracts as modeled via inclusion of $minu$.

intercept and on ΔX_{ijt} would — in this model at least — produce a consistent estimate of $\beta + \gamma$, $\widehat{\beta + \gamma}$ say. In order to separately identify β and γ, Topel (1991) proposed estimating a second stage regression of $w_{ijt} - \widehat{\beta + \gamma}\tau_{ijt}$ on X_{ijt} and the worker's initial experience on entry to the firm. Provided the latter is not correlated with job match quality this produces a consistent estimate of γ. Subtracting the latter estimate from $\widehat{\beta + \gamma}$ gives a consistent estimate of the RTT parameter β.

Altonji and Shakotko's (1987) solution to the problem involved using deviations of a worker's tenure from her ex post time at the firm as an instrument for tenure itself. Such a variable is by definition uncorrelated with both the time invariant job match quality θ_{ij} and the worker fixed effect ϕ_i.[4]

We now show that the above two methods fail when firm wages and employment co-move through time. We abstract from complications such as human capital and endogenous worker separation to make clear that the biases we identify exist even in the absence of such effects. The quantitative significance of the bias is an empirical issue which we deal with in a separate section below.

3   Bias Arising from Comovement of Firm Wages and Employment

We now consider a model that abstracts from the existence of human capital in Model A but that focuses instead on the possible effects of within firm wage/employment co-movements:

$$w_{ijt} = \alpha + \beta\tau_{ijt} + \varpi_{jt} \tag{1}$$
$$\text{with } i = 1, \ldots, L_{jt}; \quad j = 1, \ldots, n; \quad t = 1, \ldots, T;$$
$$E(L_{jt-k}\varpi_{jt}) = \sigma_{jk}, \tag{2}$$
$$L_{jt}^{\tau} = s^{\tau}(L_{jt-\tau} - sL_{jt-\tau-1}), \tag{3}$$
$$\text{Assumption: } L_{jt}^{\tau} > 0. \tag{4}$$

Equation (1) expresses log wage in terms of worker tenure τ_{ijt}, exogenous survival rate s (equal to one minus the separation rate) and a firm specific shock ϖ_{jt}. L_{jt} is firm employment at time t.

To fix ideas, and in keeping with much of the relevant data in the area, we consider t to be years. The ϖ_{jt} are "equal treatment" components of wages because all workers within firm j receive them (see Snell and Thomas, 2010, Hall, 2005, Gertler and Trigari, 2009, Michaillat, 2012 and Gertler, Huckfeldt and Trigari, 2014 for examples of macro theories based on equal treatment and see the latter for empirical evidence in support of it).

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[4]In any panel data set the worker fixed effect is subsumed in the job match fixed effect — it is not separately identified.

Equation (2) implies that the wage shock ϖ_{jt} may be correlated with current firm employment — this is the firm specific wage/employment co-movement we spoke of above. The equation also allows the wage shock to be correlated with previous employment levels in cases where $\sigma_{jk} \neq 0$. It also allows for the possibility of an aggregate business cycle in wages and employment. In (3) we specify a constant and exogenous survival (separation) rate s $(1 - s)$, where L_{jt}^{τ} is the number of workers at time t with tenure τ, and assume in (4) that separations are never so low or negative employment shocks so large that there is no net new hiring. This (linearity) assumption is made for tractability purposes; if we consider the frequency of observation to be annual then it is not an unreasonable one.[5]

To simplify we set $\beta = 0$ (see Model A) so that there is no firm specific human capital embedded in the model and nor is there a return to experience or heterogeneous match quality. This is for clarity; we wish to focus on biases away from zero of the RTT coefficient arising because of firm wage/employment co-movements. We discuss the implications for estimates of returns to experience later. We set $\tau = 0$ for "new hires" (workers with less than one year of tenure).

The model abstracts from idiosyncratic shocks and firm specific intercepts and common deterministic trends in wages. Adding idiosyncratic (worker specific) shocks to wages would not change our results as long as the number of workers in each firm is large. With regards to common deterministic trends these are typically removed in empirical work via the addition of (common) time trends whilst firm fixed effects are typically used to extract firm specific intercepts. We would not wish our results to be driven by the existence of these components and so do not model them.

As noted above we assume that the number of workers per firm per year is large. We need this in order to obtain consistent estimates of firm specific wage components at time t. Given a fixed number of firms this amounts to assuming N/T is large where N is the total number of panel observations. We wish to derive results in terms of the time series moments of firm wages and firm employment. A convenient assumption here would be to assume T itself is large (with $T/N \to 0$). Although this assumption is counterfactual — panel time spans are typically modest — we do not think it is critical. We could easily drop the large T assumption and couch our results in terms of sample time series moments rather than theoretical ones. The sign of the bias would then depend on the sign of the sample moments instead of their theoretical counterparts. As it is more common in contexts such as these to derive results based on theoretical moments we prefer to use a large T assumption. Henceforth we take probability limits as $N, T \to \infty$ and $T/N \to 0$. We assume we have a panel consisting of all the workers working in n randomly selected firms during years 1 to T.

Casual inspection of the model shows that even in a world free of any kind of human capital, tenure is endogenous; if $\sigma_{jk} > 0$ a firm which has above average wages at time t will also have above average hiring and below average tenure at t.

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[5]In the US annual average separation rates are typically in the region of 30%–40% although in Germany and Portugal they are just above 10%..

OLS using demeaned data applied to (1) gives a biased estimate of β.[6] Write the OLS estimate as

$$\widehat{\beta}_{OLS} = \frac{\sum\limits_{t=1}^{T}\sum\limits_{j=1}^{n}\sum\limits_{i=1}^{L_{jt}}(\tau_{ijt} - \overline{\tau})\varpi_{jt}/N}{svar(\tau_{ijt})}, \tag{5}$$

where $N = \sum\limits_{t=1}^{T}\sum\limits_{j=1}^{n}L_{jt}$ is the number of observations, $\overline{\tau}$ is average tenure in the sample and $svar$ denotes sample variance. The denominator in (5) is obviously positive. Denoting average firm employment per year as $\overline{L}^{f} = N/nT$ and recalling that there are L_{jt}^{τ} workers of tenure τ in firm j at time t with m being the total number of tenure categories[7], we can rewrite the numerator — $\widehat{\beta}_{OLS}^{N}$ say — as

$$\widehat{\beta}_{OLS}^{N} = \frac{1}{\overline{L}^{f}}\sum\limits_{t=1}^{T}\sum\limits_{j=1}^{n}\sum\limits_{\tau=0}^{m}(\tau - \overline{\tau})L_{jt}^{\tau}\varpi_{jt}/nT.$$

Henceforth we normalise $\overline{L}^{f}$ to unity — effectively normalising all of the firm employment levels. Substituting for L_{jt}^{τ} using (3) and simplifying gives

$$\widehat{\beta}_{OLS}^{N} = \sum\limits_{t=1}^{T}\sum\limits_{j=1}^{n}\sum\limits_{\tau=0}^{m}(\tau - \overline{\tau})s^{\tau}\{L_{jt-\tau} - sL_{jt-\tau-1}\}\varpi_{jt}/nT$$

$$\widehat{\beta}_{OLS}^{N} = \frac{1}{nT}\sum\limits_{t=1}^{T}\sum\limits_{j=1}^{n}\sum\limits_{\tau=1}^{m}\tau s^{\tau}\{L_{jt-\tau} - sL_{jt-\tau-1}\}\varpi_{jt} -$$

$$\frac{1}{nT}\overline{\tau}\sum\limits_{t=1}^{T}\sum\limits_{j=1}^{n}\sum\limits_{\tau=0}^{m}s^{\tau}\{L_{jt-\tau} - sL_{jt-\tau-1}\}\varpi_{jt}.$$

Hence $plim\{\widehat{\beta}_{OLS}^{N}\} = \frac{1}{n}\sum\limits_{j=1}^{n}\sum\limits_{\tau=1}^{m}\tau s^{\tau}\{\sigma_{j\tau} - s\sigma_{j\tau-1}\} - \frac{1}{n}\sum\limits_{j=1}^{n}\sigma_{j0}.plim\{\overline{\tau}\}$ (6)

where $plim\{\overline{\tau}\} = \frac{s - s^{m+1}}{1-s} \approx \frac{s}{1-s}.$

Note that the first term in (6) does not contain the contemporaneous covariance between firm wages and firm employment whilst the second term depends *only* on this covariance. Note also that the second term (the contemporaneous wage/employment covariance term) is multiplied by $plim\{\overline{\tau}\}$ — typically well in excess of unity — whereas the first term's summed elements are scaled by s^{τ} — which are strictly less than unity. This gives analytical force to the claims made earlier that contemporaneous wage/employment co-movements are of first order importance for the bias and that when these co-movements are positive the bias will be negative.

We examine the bias in β yielded by OLS and the two step/IV methods outlined above

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[6] Unless otherwise specified proofs of analytical results are given in the appendix.

[7] We assume all workers with tenure m years retire in the following period.

for the simple case where non contemporaneous wage/employment covariances are zero, i.e., where $\sigma_{jk} = 0$ for $k > 0$. This is not to say we believe this assumption to be true. In fact, adjustment costs and/or other frictions would imply that lagged co-movements would also be correlated with wages. But as (6) shows the first order effect on the bias is driven by contemporaneous wage/employment co-movements. We return to the case where wages are correlated with lagged employment in section 4.4 below. There we try and calibrate the bias and for that exercise it will be necessary to additionally control for lagged employment. The analytical results on bias in this section focus only on first order effects so that the impact of lagged employment may be ignored.

Our first and key result follows directly from (6); if $\sigma_{jk} = 0$ for $k > 0$ (8) implies that

$$plim\{\widehat{\beta}_{OLS}\} = \frac{-\overline{\sigma}_0 plim\{\overline{\tau}\}}{plim\{svar\{\tau_{ijt}\}\}} \qquad \text{where} \quad \overline{\sigma}_0 = \frac{\sum_{j=1}^{n} \sigma_{j0}}{n}. \qquad (7)$$

Equation (7) confirms our intuition that positive co-movement between firm wages and employment would lead to negative bias in the RTT coefficient estimate β. However it also shows that the co-movements need not be uniformly positive across firms for downward bias to exist as long as the average is positive. This is important. It may be that for some firms, shocks to their labour supply are the dominant influence on their wages rather than from labour demand. This effect might be particularly salient in large firms that have a degree of monopsony power in their hiring markets.

Now we assess the bias under the Topel (1991) method again taking β (RTT) and $\sigma_0 > 0$ with $\sigma_k = 0$ for $k > 0$. For the first stage we drop new hire (tenure 0) observations and regress first differenced wages on an intercept.[8] In this case we show in the annex that

$$plim\{\widehat{\beta + \gamma}\} = -\{\frac{\overline{\sigma}_0}{k}\}s < 0 \qquad (8)$$

where k is the proportion of incumbents in the full sample. In the second stage we regress $w_{ijt} - \widehat{\beta + \gamma}\tau_{ijt}$ on worker $i's$ experience on joining the firm. As $\widehat{\beta + \gamma}$ is (negatively) biased the error term becomes $\varpi_{jt} + \{\widehat{\beta + \gamma} - \beta - \gamma)\tau_{ijt}$. Under the assumption that a worker's entry experience is uncorrelated with his tenure, second stage OLS will give a consistent estimate (of zero) in our setup. The RTT — the difference between first and second stage estimates — will be $\widehat{\beta + \gamma}$ which as (8) shows is negative.

Turning to Altonji and Shakotko's (1987) IV method we show that $\tau_{ijt} - \overline{\tau}_{ij}$ — where $\overline{\tau}_{ij}$ is average tenure for worker i during his time at firm j — is an invalid instrument under

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[8]Usually the RTT are modelled as an nth order polynomial function in tenure. In that case we would regress on an intercept plus an $n - 1th$ order polynomial.

our model. More explicitly we show in the annex that[9]

$$plim\frac{1}{nT}\sum_{t=1}^{T}\sum_{j=1}^{n}\sum_{\tau=1}^{m}\sum_{i=1}^{L_{jt}}(\tau_{ijt} - \overline{\tau}_{ij})\varpi_{jt} = -\overline{\sigma}_0\tau(s) \tag{9}$$

where $\tau(s)$ is expected tenure of a new hire — determined by the fixed exogenous survival rate s. Not only is the instrument invalid but in this simple single regressor single instrument model — the RTT estimate it produces is downward biased.

3.1 Solution to the Problem: Firm-Year Fixed Effects

The source of the bias identified above is the common firm employment and firm wage error components.

We propose a general procedure to handle this: estimate Model A but add firm-year interaction fixed effects. To additionally control for match quality we must also include firm-worker job match dummies — in effect a two way fixed effects procedure.

However adding match fixed effects in Model A means a loss of identification of the linear experience term γ. Here we suggest following Topel. Experience should be dropped from the equation and the coefficient on the linear tenure term should be used as a consistent estimate of linear experience plus linear tenure, $\beta + \gamma$ ($\widetilde{\beta + \gamma}$ say). To separate the linear tenure and experience effects run a second stage regression of $w_{ijt} - \widetilde{\beta + \gamma}\tau_{ijt}$ on the worker's experience on entry to the firm. Under the assumption that match effects are uncorrelated with initial experience, this would yield a consistent estimate of γ. Subtracting the latter from $\widetilde{\beta + \gamma}$ would give a consistent estimate of β — the pure RTT term in Model A. An alternative simpler method — one we pursue below in the empirical section — is to regress the fitted worker-firm match fixed effects on experience on entry to the firm. This gives an estimate of the linear experience term γ which when subtracted off the (linear) tenure term in the two way fixed effects regression gives an estimate of RTT. Additionally this method allows us to test the hypothesis that different firms attract workers of different experience. To do this add firm fixed effects to the regression of fitted match on experience and see if it significantly changes the estimate of γ; if it does not then we would conclude that firms are homogeneous in terms of the experience of their workers.

3.2 Discussion

First of all we state the obvious. If one is purely interested in effects that vary only with year and tenure then firm-year effects are uninformative and removing them seems a sensible

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[9]We assume for simplicity that there are no partial firm spells in the data. We could not compute the spell average tenure otherwise.

thing to do. Once worker-firm match quality is controlled for (via match fixed effects) only the cross tenure/year movements in wages are relevant to estimating RTT; components of wages that are common to workers in firm j in year t cannot add information.

The second observation is that the use of correlates of tenure in business cycle studies is also subject to the biases we have identified here. Two canonical examples spring to mind. Beaudry and DiNardo (1991) have a contracting model in which the minimum unemployment rate since the worker joined the firm ("$minu$") is a sufficient statistic for wages (modulo human capital). Another example comes from the literature that evaluates a new-hire versus incumbent wage premium; if the new hire wage is (again modulo human capital) found to lie below the incumbent wage in recessions then this has profound implications for models of the labour market because it implies that new hires are able to price themselves into jobs during bad times. New hire/incumbent premia are typically evaluated via the inclusion of a new hire dummy interacted with the unemployment rate ("$\delta^0 u$" say) to an otherwise standard Mincer equation. Both $minu$ and $\delta^0 u$ are correlates of deterministic tenure and suffer from the same problem as the tenure variable itself; coefficient estimates will be biased if the workers in a firm suffer common wage shocks that covary with firm employment. It would be particularly interesting if the bias on these terms could be shown to be negative (leading to spurious "right signed" significance) but we cannot show this. In fact, intuitively the opposite may appear true; high firm wages may be associated contemporaneously with high firm employment, low (within firm) average tenure and high (within firm) average $minu$ and $\delta^0 u$ leading to positive bias (bias towards zero). But when one also has to control for tenure (as surely one must) the sign of the bias cannot be determined.[10] We leave the sign of the bias as an empirical issue — an issue which we flesh out in section 4 below.

Thirdly and following on from the previous discussion if variates such as $minu$ and $\delta^0 u$ are important determinants of wages their exclusion will be yet another source of bias to RTT estimates. The omission of relevant regressors that are correlated with included ones will always cause bias so this point is hardly new. Nonetheless for the sake of completeness we assess the quantitative significance of the omission of these two key macro variates on RTT in an extension to the empirics below.

Fourthly our FYFE correction allows for the possibility that firms may have heterogeneous wage and employment co-trends. The model in (1) to (4) could easily extend to capture this type of firm heterogeneity and the end result would be the same — fast (slow) growing and high (low) wage growth firms would have lower (higher) average tenure and higher (lower) average wages. This type of issue has been discussed before in the RTT literature but as far as we know it has not been analysed.

Finally since the problem is co-movement of wages and employment at the firm level it would be tempting to simply add firm employment to the panel regression to purge wages

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[10]Simulations of our simple model — available on request — show that when tenure must be additionally controlled for negative biases in $minu$ and $\delta_0 u$ are possible under plausible firm wage/comovements. But as we note in the text they are certainly not generic.

of this variable. It is easy to show that this would only remove the bias if all firms displayed the same wage/employment co-movement. Furthermore it may also be the case that wages are correlated with past firm employment levels as well which would require lagged employment levels to be included. We shall see below in the empirical section that contemporaneous wage/employment co-movements are in fact very heterogeneous in our German and Portuguese samples.

4 An Empirical Application to German and Portuguese Panel Data

4.1 Method

In this section we apply our proposed bias correction method — the addition of firm-year fixed effects — to the RTT estimates from subsamples drawn from two well-used panel data sets; the QP from Portugal and the BeH from Germany. We estimate the following models which allow RTT to be determined as a quartic function of tenure[11]:

$$w_{ijt} = \theta_{ij} + \delta_1 t + \delta_2 t^2 + \delta Age_{ijt}^2 + \sum_{k=1}^{4} \beta_k^0 \tau_{ijt}^k + e_{it}^0, \tag{10}$$

$$w_{ijt} = \theta_{ij} + \phi_t + \delta Age_{ijt}^2 + \sum_{k=1}^{4} \beta_k^1 \tau_{ijt}^k + e_{it}^1, \tag{11}$$

$$w_{ijt} = \theta_{ij} + \psi_{jt} + \delta Age_{ijt}^2 + \sum_{k=1}^{4} \beta_k^1 \tau_{ijt}^k + e_{it}^2. \tag{12}$$

where e_{it}^k are regression errors

The first equation (10) controls for worker-firm (job match) fixed effects θ_{ij} and a (quadratic) time trend (t and t^2). The second (11) replaces the quadratic trend with more general year fixed effects ϕ_t. Specifications (10) and (11) are commonly used specifications in the literature. The third specification (12) employs firm-year fixed effects ψ_{jt} — our proposed solution to the bias.

The firm-year fixed effects in (12) will remove i) aggregate (business cycle) time effects in wages ii) firm and/or sectoral level time effects in wages ii) common trends (stochastic or deterministic) in wages and iv) firm specific wage trends. The square of age (Age^2) proxies for the square of experience. Note that the effects of linear experience are absorbed into the linear tenure term β_1 but this is not an issue for us: We are interested in the change in the β's when we add FYFE and if as argued above linear experience is not correlated with

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[11]RTT are frequently modelled using a quartic in the literature and we have followed this tradition here. Adding higher order terms — which were less statistically important — did not change the qualitative nature of our results.

either match effects or common wage components then these differences should only reflect the bias in the RTT estimate.[12]

4.2 Data

We draw subsamples from the QP and BeH. For Germany we select all worker spells (see below) in the 100 largest firms (largest by average employment per year) that existed throughout the entire 1986–2009 time period. For Portugal — where firms are smaller — we selected all firms that survived this period and that had a minimum of 100 employees each per year. This gave us 127 firms. These choices were conditioned by our desire to get a large number of data points each year in each firm in order to obtain accurate estimates of the firm year fixed effects. Of course large long-lived firms may have several RTT characteristics that make them unrepresentative of firms in the economy as a whole. However we are interested in the difference between RTT estimates from various specifications rather than the absolute level. We might also argue that as most workers in the economy work in large firms results from a sample of large firms are more "representative" of an economy than would be the results obtained from a random sample of firms.

We give a brief overview of these two well-used population-based matched employer-employee panel data sets as well as the "cleaning" operations we perform on them.

General features of the data:

The BeH data set is organised by worker spells. A spell is a portion of a year spent at a single firm. For the BeH if a worker stays with one firm throughout the year the average daily wage for that "spell" forms a single datapoint. If the worker moves to a second firm within the year there will be two spells that year; the average wage at each firm would form a separate datapoint for that year. By contrast the QP is an annual survey that records data on each worker at only one month in the year (March before 1994 but October from 1994 onwards). For QP then there is only one worker "spell" per year.

The BeH draws data from the total gainfully employed members of the German population who are covered by the social security system. Not covered are self-employed, family workers assisting in the operation of a family business, civil servants (Beamte) and regular students. We focus solely on workers employed in states of the former West Germany. The BeH covers roughly 80% of the German workforce. Plausibility checks performed by the social security institutions and the existence of legal sanctions for misreporting guarantee that the earnings data are very reliable — in contrast with interview based wage data such

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[12]Buhai et al. (2014) argue that the (log of the) normalised worker tenure rank is an important determinant of wages when added to traditional tenure specifications. Although significant, the effect they find is quantitatively small (moving from being the "newest" to the "oldest" worker adds about 2% to the wage). Adding this term would likely decrease our estimates of the level of RTT. However because such a variate is orthogonal to ϖ_{jt} (the common firm wage component), the bias in RTT- which is the focus of our paper-would be unaffected.

as that in, say, the PSID (for the US) or the SOEP (for Germany).

Unfortunately the BeH only documents total spell earnings and not hours worked in that spell. We therefore only consider full time workers. Nearly all full time workers in Germany work a standard number of hours per week so the average daily wage should be very closely related to the hourly wage. To calculate the daily real wage (in 2005 prices) we use the Consumer Price Index (CPI). Unfortunately wages are censored at a maximum level equal to the contribution assessment ceiling of the compulsory pension insurance scheme. Earnings spells with wages above or close to (within 1% of) the truncation point are dropped. We drop all spells that have missing tenure. Unfortunately this means a worker only enters the data when he joins a firm after Jan. 1, 1975.[13] For this reason we drop the first 12 years and use worker spells dated at 1986 and beyond.

The QP covers all workers except the self-employed and those employed in the public sector; of course, the unemployed and the inactive are also not included. There are several wage variables, all of them expressed in monthly values (the most common type of pay in Portugal), including base wages, tenure-related payments, overtime pay, subsidies and 'other payments' (this latter category includes bonuses and profit- or performance-related pay). All QP wages have been deflated using Portugal's CPI and are expressed in 2005 euros. There is also information about normal hours and overtime hours per month. The benchmark measure of pay adopted in this study is based on the sum of all five types of pay divided by the sum of the two types of hours worked, resulting in a measure of total hourly pay. Tenure is measured (in rounded years) as the current year minus the reported start year.

The Table below gives a brief summary of the datasets' main features. Averages and standard deviations are taken over job spells.

Table 1: Data Summary Statistics

	Portugal	Germany
Average *log* monthly wage (2005 Euros)	7.01	7.93
s.d. log monthly wage	.637	.286
s.d. $log(\overline{L}_j)$	.882	.634
Average Tenure (Years)	12.9	9.2
s.d. Tenure	10.2	7.3
Number of Workers Per Firm Per Year	1489	6112
Number of Years Available (1986–2009)[14]	22	24
Number of Tenure Categories Available	51	36

Note: $\overline{L}_j$ is average annual employment of firm j.

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[13]For the analysis we only use the years 1986–2009, but for the identification of firm entrants and the calculation of firm-tenure we use BeH data from 1975 onwards. However, we exclude all spells starting Jan. 1, 1975 because the tenure could be left censored. For Portugal there no data exists for the years 1990 and 2001.

Table 1 shows some stark differences in the two labour markets. Aside from average wages being very much lower in Portugal (as we would expect) wages are over twice as volatile there. Average tenure however is very high in both countries. Separation rates are around 10%, much lower than the 30% level in the US (see for example Hobijn and Sahin, 2007). Firms are not only very much smaller in Portugal but also vary in size more than they do in Germany. We will return to some of these differential features below.

4.3 Estimates

In this section we estimate a variety of wage on tenure specifications and extract fitted values of the (quartic in) tenure component. As noted already these fitted values incorporate the effects of linear experience. For clarity and correctness we refer to these fitted values as "RTTE" — "returns to tenure plus linear experience".

The number of yearly tenure categories available from BeH and QP were 51 and 36 respectively. We start by estimating the "Base" specification (10) and comparing with the specification where we add firm year fixed effects (henceforth the FYFE specification) equation (12) above. Table 2 below displays the results for the four tenure terms

Table 2: Estimates of Tenure Effects for Germany and Portugal

	Portugal			
	$\widehat{\beta_1}$	$\widehat{\beta_2}$	$\widehat{\beta_3}$	$\widehat{\beta_4}$
Base: Equation (10)	1.720	−1.207	.256	−.026
	(.133)	(.132)	(.048)	(.006)
Year FE: Equation (11)	1.780	−1.203	.270	−.023
	(.117)	(.116)	(.043)	(.005)
FYFE: Equation (12)	2.470	−1.570	.343	−.027
	(.146)	(.133)	(.045)	(.005)
	Germany			
	$\widehat{\beta_1}$	$\widehat{\beta_2}$	$\widehat{\beta_3}$	$\widehat{\beta_4}$
Base: Equation 10)	5.585	−3.896	1.576	−.221
	(.103)	(.111)	(.055)	(.087)
Year FE: Equation (11)	5.611	−3.856	1.553	−.217
	(.100)	(.108)	(.054)	(.009)
FYFE: Equation (12)	5.691	−3.786	1.522	−.212
	(.076)	(.099)	(.048)	(.008)

Note: Standard errors, clustered by tenure, are in parentheses. $\widehat{\beta_k}$ and standard errors are scaled by 10^{k+1}.

We see that adding Year FE changes the results only marginally in all cases. By con-

trast adding FYFE to Base causes quantitatively important changes in many of the tenure estimates. This is particularly true for Portugal; here the first three tenure terms are about 25% higher in absolute value in the FYFE compared with Base and the 95% confidence intervals for the FYFE linear and quadratic terms do not include their Base counterparts. The German estimates change far less and the Base and FYFE confidence intervals for each parameter overlap.

However, changes in the individual parameter estimates per se tell us little directly about changes in the RTTE. To get a better handle on the impact of adding FYFE to overall RTTE estimates we plot the implied difference in RTTE estimates (FYFE minus Base) against tenure (in years). Henceforth we refer to these differences as the "bias" (although this is somewhat erroneous as it is in fact the negative of the bias[15]).

We do not have exact standard errors for the bias but we can obtain an upper bound for them as follows. We show in the annex that under the null hypothesis of no bias the covariance matrix of the Base RTTE estimates exceeds that of the bias by a positive definite matrix. The variance of the bias will in this case have an upper bound equal to the variance of Base RTTE estimates. Given that the RTT estimates at each tenure level are merely linear combinations of the tenure parameters we can use the Base covariance matrix to construct 90% and 95% upper bound confidence intervals around the bias. Figures 1 and 2 display the differences in RTTE estimates (FYFE minus Base) together with their 90% and 95% upper bound confidence intervals for Portugal and Germany respectively. Figures 3 and 4 plot graphs for FYFE minus Year FE again with upper bounds 90% and 95% confidence intervals[16].

The bias is positive and rises with tenure in both countries. At a tenure of 10 years the bias rises to about 4.0% of wages in Portugal and 1.7% in Germany. At 20[17] years of tenure the German bias rises to above 3.5% In terms of statistical importance, the bias is highly significant at the 95% level for Portugal but has borderline significance for Germany. However the German bias *is* significant at the 90% level. Given that the confidence intervals are upper bounds and that our prior view was that Base RTTE estimates would be downward biased (motivating a one tailed test and use of the 90% level) we could justifiably argue that the bias is significant for both countries.

Recall that our linear tenure terms also included the effects of linear experience. To get estimates of the level of RTT itself, "pure" RTT, we first of all need to get an estimate of the effect of linear experience. To do this we follow the second suggestion advanced in section 3.1: Regress fitted match effects on experience at the time of entry to the firm. Doing so gives linear experience estimates of just above 0.7% per year in Portugal and 2.2% per year in

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[15]It is also erroneous in that it implies that there are no other sources of bias. Our claim here is to have identified a major and pervasive source of bias to RTT rather than an exclusive source.

[16]The variance result we derive in the annex for the FYFE minus Base case is easily adapted to the FYFE minus Year FE case. We do not offer a proof but one is available on request.

[17]We do not analyse results beyond 20 years of tenure. The numbers of workers in tenure categories above 20 years and hence the precision of our estimates is low and falls sharply with tenure.

Germany. Subtracting these experience estimates from the tenure estimates obtained from (12) gives us our "pure" RTT estimate. For workers of ten years tenure the pure RTT is just over 5% for Portugal and about 9% for Germany. These estimates show how quantitatively important the biases are; for Portugal (and again for workers of ten years of tenure) the bias is over 80% of its pure RTT counterpart whilst for Germany it is about 20% rising to 50% at very long (20 years) tenure.

Finally and as an aside we note a feature of the estimation of the linear experience term. We found that adding firm fixed effects to the regression of fitted match values on experience at entry to the firm made little difference to the experience coefficient. This suggests there is no heterogeneity amongst firms in terms of their tendency to hire experienced or inexperienced workers.

4.4 Exploring the Source of the Bias: The Role of Firm Employment

The analytical arguments above pointed to co-movements in firm wages and employment as the source of the problem. In this section we see the extent to which current and lagged firm employment can account for the bias. We do this in two ways. First we control for current and once-lagged firm employment levels in the Base specification and see what effect it has in terms of reducing the bias. Secondly we calibrate the bias by plugging estimates from this augmented Base specification (together with other data moments from the panel) into our simple analytical model above.

A first pass at removing the bias might be to purge firm wages of the effects of firm employment by adding the latter as a regressor to the Base specification[18]. Adding a single term in (log of)[19] firm employment to Base alters the RTTE estimates little;[20] the bias is reduced by no more than 10% in either country at any tenure up to 20 years. Adding lagged firm employment is equally impotent in this respect. This suggests that if wage/employment co-movements are the source of bias then such co-movements must be heterogeneous across firms. We now explore this issue further.

We estimate a model that allows for heterogeneous and dynamic firm wage/employment

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[18]For example, in their analysis of seniority and using the QP, Buhai et al. (2014) add a single term in firm employment to control for what they call the firm size effect on wages.

[19]The theoretical analysis is in terms of employment levels normalised by average firm employment, $\overline{L}^f$. Using logs (and hence estimating elasticities) is an approximation therefore.

[20]Fuller results available on request.

co-movements. Explicitly we estimate

$$w_{ijt} = \theta_{ij} + \delta Age_{ijt}^2 + \sum_{k=1}^{4} \beta_k^0 \tau_{ijt}^k + \sum_{j=1}^{n} \gamma_j^0 d_{jt} l_{jt} +$$

$$\sum_{j=1}^{n} \gamma_j^1 d_{jt} l_{jt-1} + \delta_1 t + \delta_2 t^2 + e_{it}^0, \tag{13}$$

where d_{jt} is a dummy equal to 1 when the wage is drawn from firm j and zero otherwise and where l_{jt} is log of firm j's employment at t. We call this regression the "Employment" specification. Estimating (13) has a treble purpose. First we wish to see the extent to which absorbing wage/employment co-movements reduces bias. Second we wish to establish the extent of heterogeneity of wage/employment co-movements. Thirdly we wish to obtain "good" estimates of wage/employment elasticities for a calibration exercise. The key parameters for the calibration are the γ_0^j terms. In order to obtain consistent estimates of these we need to nest as much of the employment/wage co-movements as possible in (13). Limiting the lag length in (13) to one is driven in part by prior theoretical reasoning and in part by degrees of freedom constraints. We effectively have only 22 (24) time series observations with which to estimate each firm's γ's so two γ's per firm would seem a reasonable choice on degrees of freedom grounds. With regards to prior reasoning, hiring costs and or labour adjustment costs would suggest that current and lagged employment are the prime correlates of current wages.

We estimated (13) and recomputed the bias (Employment RTTE minus Base RTTE). The results are in Figures 5 and 6. We add the "FYFE minus Base" line to Figures 5 and 6 for comparison. Looking at the Figures we see that adding the employment terms produces RTTE estimates very close to those obtained in the FYFE specification for Germany — effectively removing the bias. For Portugal the results are less definitive but even so the extra terms reduce the bias by over 70% at 20 years of tenures.

Turning to heterogeneity, Figures 7 to 10 show the distribution across firms of the estimated employment elasticities γ_j^0 and γ_j^1 for each country. We see from the Figures that there is indeed a diverse pattern of wage/employment co-movements across firms.[21] Interestingly the average contemporaneous elasticity — an estimate of $\overline{\sigma}_0$ — is positive in both countries (.073 for Portugal and .011 for Germany). This is consistent with downward bias in estimates of RTTE.

At the risk of taking our simple model too seriously we calibrate the bias using estimates from (13) and sample moments to see if we can match the bias we saw in the Base specification. The general form for the bias is given in equation (6). Adapting it to the case where

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[21]Standard errors of the elasticities (the γ's) in both the Employment specification (and Employment/Dynamic specification used below) were low and a test of parameter equality is rejected by a very large margin.

σ_{j0} and σ_{j1} are the only nonzero employment/wage covariances gives

$$plim\{\widehat{\beta}_{OLS}\} = \left(\frac{1}{n}\sum_{j=1}^{n} s\sigma_{j1} - \frac{1}{n}\sum_{j=1}^{n} \sigma_{j0} plim\{\overline{\tau}\} \right) / plim\{svar(\tau_{ijt})\}$$

$$= \left(s\overline{\sigma}_1 - \overline{\sigma}_0 plim\{\overline{\tau}\} \right) / plim\{svar(\tau_{ijt})\}. \tag{14}$$

We plug estimates from (13) into equation (14) together with data sample moments to calibrate the bias. Explicitly we estimate $\overline{\sigma}_0$ with $\sum_{j=1}^{n}\widehat{\gamma}_j^0/n$ and $\overline{\sigma}_1$ with $\sum_{j=1}^{n}\widehat{\gamma}_j^1/n$, where $\widehat{}$ denote an OLS estimate from (13). We use Hobijn and Sahin's (2007) estimates of annual separation rates for Portugal and Germany of .11 and .12 respectively giving survival rates, s, of .89 and .88. Finally we estimate $plim\{svar(\tau_{ijt})\}$ with its sample counterpart from our data. Table 3 shows the estimated bias at 5, 10, 15 and 20 years of tenure predicted by the calibration ($Bias(Cal)$). The initial biases (FYFE minus Base) are also tabulated for comparison (denoted $Bias(Base)$).

Table 3: Calibrating The Bias

Portugal				
$\overline{\sigma}_0 = .073, \overline{\sigma}_1 = -.070, svar(\tau_{ijt}) = 104, s = .89$				
τ	5	10	15	20
$Bias(Cal)$ % of wage	-2.54	-5.08	-7.62	-10.16
$Bias(Base)$ % of wage	-2.90	-4.42	-5.03	-5.12
Germany				
$\overline{\sigma}_0 = .011, \overline{\sigma}_1 = .028, svar(\tau_{ijt}) = 53.3, s = .88$				
τ	5	10	15	20
$Bias(Cal)$ % of wage	$-.98$	-1.96	-2.94	-3.92
$Bias(Base)$ % of wage	$-.74$	-1.69	-2.64	-3.54

Table 3 shows that for Germany the calibrated bias fits its empirically estimated counterpart extremely well over all tenures. For Portugal the fit is reasonably good up to 10 years of tenure but above that is poor. There are two features of the simple model that may be behind this last result. Firstly — despite its quadratic form — the estimated RTTE for Germany is close to being linear for tenures up to 20 years whilst that for Portugal is distinctly nonlinear. Our analytical model has of course only one (linear) tenure term. Second the simple model assumes equal size firms (on average) but Table 3 shows that the standard deviation of the *log* of average firm employment is 25% higher in Portugal than in Germany. One final problem could be that higher order lags are missing for Portugal's Employment specification leading to a poor estimate of the crucial γ_j^0 parameters. This last fact may also be the reason only 70% of the bias is removed by the Employment specification.

Despite the over prediction of the bias at long tenures for Portugal the simple analytical model appears to have had some traction in explaining the origin, sign and magnitude of the empirical bias we have discovered in RTTE estimates.

4.5   Tenure Correlates: $minu$ and $\delta^0 u$

We argued above in section 3.2 that estimates of macro tenure correlates such as $minu$ and $\delta^0 u$ will be subject to similar biases as tenure itself.

To illustrate biases that may arise we once again use our simple model in (1) to (4) as the data generating process and estimate by pooled OLS:

$$w_{ijt} = \alpha + \beta \tau_{ijt} + \lambda^k u_{ijt}^k + \varpi_{jt} \tag{15}$$

where $k = minu, \delta_u^0$ and where u_{ijt}^{minu} $(u_{ijt}^{\delta^0 u})$ is the value of worker i's $minu$ $(\delta^0 u)$ at time t in firm j. As before we ignore worker idiosyncratic wage shocks to conserve notation.

We can show using textbook formulae that the bias in λ^k can be written as

$$(\widehat{\lambda}^k_{OLS} - \lambda^k) \propto \rho_{wu^k} - \rho_{w\tau}\rho_{\tau u^k}$$

where $\propto$ means "positively proportional to" and ρ_{xy} is the correlation coefficient between x and y. We can view these correlation coefficients through the lens of our simple model. If we assume once more that $\overline{\sigma}^0_j > 0$ but $\overline{\sigma}^k_j = 0$, $k > 0$, then removing the effects of aggregate shocks by adding Year FE reduces the bias in absolute value.[22] However the sign of the effect of moving to a specification with FYFE (i.e. controlling for idiosyncratic firm wage/employment co-movements) is indeterminate: The terms $\rho_{w\tau}$ and $\rho_{\tau u^k}$ are both negative (the former we have demonstrated already and the latter derives from the fact that u^k is weakly decreasing with tenure) whilst ρ_{wu^k} is positive which implies we cannot determine the sign of the impact of idiosyncratic co-movements on the bias. We repeat our earlier contention that in the context of estimating the effects of variates that move only over time and tenure, common wage components are at best noise and at worse bias-causing. These wage components should be removed whatever the sign of the bias. We now assess the impact on λ of doing so.

In macroeconomic applications involving $minu$ and $\delta^0 u$ it is important to additionally control for the aggregate business cycle. We do this by adding Year FE to our new Base specification. Explicitly we add $minu$ and $\delta^0 u$ to (11) and (12). Table 4 displays the results.

Table 4: Bias in $minu$ and $\delta^0 u$

	Germany		Portugal	
	$minu$	$\delta^0 u$	$minu$	$\delta^0 u$
Year FE	$-.405$	$-.551$	$-.928$	$-.495$
	(.046)	(.039)	(.113)	(.040)
FYFE	$-.419$	$-.526$	$-.289$	$-.441$
	(.026)	(.029)	(.029)	(.014)

All estimates are highly significant although quantitatively small. For Germany, adding FYFE changes little in either estimate but in Portugal and for $minu$ things are different; the FYFE estimate is less (in absolute value) than one-third of its Year FE value. Portugal's $\delta^0 u$ value also moves towards zero but far less — by only 10% or so of its original value.

We close by returning to a point made in the initial discussion of macro variates above. We argued that omission of relevant tenure correlated variates such as these will cause further bias to RTT. Controlling for tenure related macro effects is a two way street: failing to control for FYFE may bias their estimates whilst omitting these variates may bias estimates of RTT. Figures 11 and 12 display the differences in RTTE estimates we get when we add each respective macro variate to the FYFE specification in (12). Adding $\delta^0 u$ clearly reduces

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[22]Proof of this and the other results in this section is straightforward and is available on request.

RTT by a non negligible amount — up to 3% (1.5%) of wages for Germany (Portugal). Adding $minu$ has far less impact — less than a 1% reduction in both countries. Clearly it is important to control for tenure related macro variates if one wishes to obtain good RTT estimates.

Finally we should check that adding FYFE to the Year FE specification continues to make a difference even in the specifications containing $minu$ and $\delta^0 u$. Figures 13 and 14 show the difference in RTTE we get when we add FYFE to the Year FE specification in the $minu/\delta^0 u$ specifications (marked as "Minu"/"Deltau" on the graph). For comparison we add the line showing the impact of adding FYFE (moving from (11) to (12)) when the macro terms are absent (marked as "No Macro" on the graph). The Figures confirm emphatically that addition of FYFE remains important despite the addition of $minu$ and $\delta^0 u$ to the specifications. In fact the impacts are practically the same as those found originally in section 4 except for the case of $minu$ in Portugal. Here adding FYFE to the Year FE increases RTTE by over 6% at long tenures compared with an original impact (absent macro variates) of just over 4%.[23]

We summarise by saying that adding tenure related macro effects is important in order to get good estimates of RTT. Likewise adding FYFE is essential to get good estimates of both RTT and tenure related macro effects.

5   Summary and Closing Comments

We have shown that failing to control for co-movement of wages and employment at firm level leads to downward bias in RTT estimates obtained from reduced form wage equations. We also argued the same mechanism could also bias the coefficients on correlates of deterministic tenure such as $minu$ and $\delta^0 u$. Our estimates from all the wrokers in 100 or so large long lived Portuguese and German firms imply that these biases are quantitatively important. In other words, our findings indicate that the importance of tenure in terms of wages has been substantially underestimated in the existing literature. Given that this literature informs issues involving internal labour markets, rent sharing, training and institutional dimensions in labour market outcomes then obtaining more rigorous measurement of the effects of tenure using the methods we propose here would seem to be important.

We conclude on a warning. In another exercise we drew a set of small random worker-based random subsamples from our main dataset and re-estimated equation (12) - i.e. controlling for FYFE - each time. The variation in RTT estimates was very high. There seems to be an incidental parameter problem here. Small worker-based random samples offer few observations per firm per year. As a result the FYFE will be poorly estimated. This suggests that when obtaining reduced form estimates of RTT a (large) firm based samples rather than

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[23]This is of course a mirror image of the large impact that adding FYFE has to the $minu$ estimate in the case of Portugal.

worker based ones are required to eliminate the bias.

Appendix

Bias using Topel's Method

$$plim\{\widehat{\beta + \gamma}\} = \frac{1}{k}plim\sum_{t=1}^{T}\sum_{j=1}^{n}\sum_{\tau=1}^{m}L_{jt}^{\tau}\Delta\varpi_{jt}/nT$$

$$= \frac{1}{k}plim\frac{1}{T}\sum_{t=1}^{T}\sum_{j=1}^{n}\sum_{\tau=1}^{m}s^{\tau}(L_{jt-\tau} - sL_{jt-\tau-1})\Delta\varpi_{jt}/n$$

$$= -\{\frac{\overline{\sigma}_0}{k}\}s < 0.$$

Bias using Altonji and Shakotko's method

Note that workers joining firm j at t all have the same expected tenure. The average of firm tenure for workers joining at t will therefore be a constant $\overline{\tau}_t$ plus an $o(1)$ term. We use this fact below:

$$A = \frac{1}{(\sum L_t)/nT}\frac{1}{nT}\sum_{t=1}^{T}\sum_{j=1}^{n}\sum_{\tau=1}^{m}\sum_{i=1}^{L_{jt}}(\tau_{ijt} - \overline{\tau}_{ij})\varpi_{jt}$$

$$= \frac{1}{nT}\{\sum_{t=1}^{T}\sum_{j=1}^{n}\sum_{\tau=1}^{m}\sum_{i=1}^{L_{jt}}\tau_{ijt}\varpi_{jt} - \sum_{t=1}^{T}\sum_{j=1}^{n}\sum_{\tau=1}^{m}\sum_{i=1}^{L_{jt}}\overline{\tau}_{ijt}\varpi_{jt}\}$$

using $plim\overline{\tau}_{ijt} = \tau(s)$

$$plimA = \frac{1}{nT}.0 - \tau(s)plim\left(\frac{1}{nT}\sum_{t=1}^{T}\sum_{j=1}^{n}\sum_{\tau=1}^{m}L_{jt}^{\tau}\varpi_{jt}\right)$$

$$= -\tau(s)plim\left(\frac{1}{nT}\sum_{t=1}^{T}\sum_{j=1}^{n}\sum_{\tau=0}^{m}\varpi_{jt}(L_{jt-\tau} - sL_{jt-\tau-1})\right)$$

$$= -\overline{\sigma}_0\tau(s) < 0.$$

Proof that the variances of FYFE minus Base tenure estimates are less than those of Base

We start with model (10). We expand the error term e_{it}^0 into two components ξ_{ijt} and ω_{jt} — an idiosyncratic worker shock and a firm-year "shock". Under the null these shocks are mutually uncorrelated and uncorrelated with the regressors (we consider the match effects to be regression dummies):

$$w_{ijt} = \theta_{ij} + \delta Age_{ijt}^2 + \sum_{k=1}^{4}\beta_k^0\tau_{ijt}^k + \theta_{ij} + \delta_1 t + \delta_2 t^2 + \xi_{ijt} + \omega_{jt}.$$

If we put the four tenure terms, the square of age and the worker firm match dummies into a single regressor matrix X say with the first four columns being the occupied by the (quartic) tenure terms, we can write the Base regression model in the familiar textbook form:

$$y = X\beta + u = X\beta + \xi + \omega,$$

using obvious notation.

Now $\sqrt{N}(\widehat{\beta}_{OLS} - \beta)$ has the standard form:

$$\sqrt{N}(\widehat{\beta}_{OLS} - \beta) = \left(\frac{X'X}{N}\right)^{-1} \frac{X'(\xi + \omega)}{\sqrt{N}}.$$

Writing $\omega = Z\gamma$ where Z is a regressor matrix containing firm-year dummies[24] with coefficient vector γ we get the augmented regression model

$$y = X\beta + Z\gamma + \xi.$$

Denote the estimate for β from this new regression as β^*. We can obtain the OLS estimate of β^* in a roundabout (two-step) way. First regress y on X and Z. Then compute the fitted values $\omega^* = Z\gamma^*$. Run a second regression

$$y = X\beta + \omega^*\delta + \xi.$$

The estimate of δ will be unity whilst the estimate of β will be numerically identical to β^* and will obviously have the same distribution. The next thing to note is that under the null $plim\{\frac{X'\omega}{N}\} = 0$. So we have

$$plim\{\frac{X'\omega^*}{N}\} = 0.$$

In short the textbook X prime X matrix is asymptotically block diagonal. We can therefore write $\sqrt{T}(\beta^* - \beta)$ as

$$\sqrt{N}(\beta^* - \beta) = \left(\frac{X'X}{N}\right)^{-1} \frac{X'\xi}{\sqrt{N}} + o(1).$$

Using the fact that under the null both $\widehat{\beta}_{OLS}$ and β^* are consistent we can write the difference between the two estimates as $\beta^* - \widehat{\beta}_{OLS} = (\beta^* - \beta) - (\widehat{\beta}_{OLS} - \beta)$ so that

$$\sqrt{N}\left(\beta^* - \widehat{\beta}_{OLS}\right) = \sqrt{N}\left((\beta^* - \beta) - (\widehat{\beta}_{OLS} - \beta)\right)$$

$$= \left(\frac{X'X}{N}\right)^{-1} \frac{X'\omega}{\sqrt{N}} + o(1).$$

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[24]We must drop two FYFE dummies because X includes t and t^2. It does not matter for the proof which of the FYFE dummies we drop.

Recall from above that $\sqrt{N}\left(\widehat{\beta}_{OLS} - \beta\right) = \left(\frac{X'X}{N}\right)^{-1} \frac{X'(\omega + \xi)}{\sqrt{N}}$. But under the null ω and ξ are mutually uncorrelated. This means that

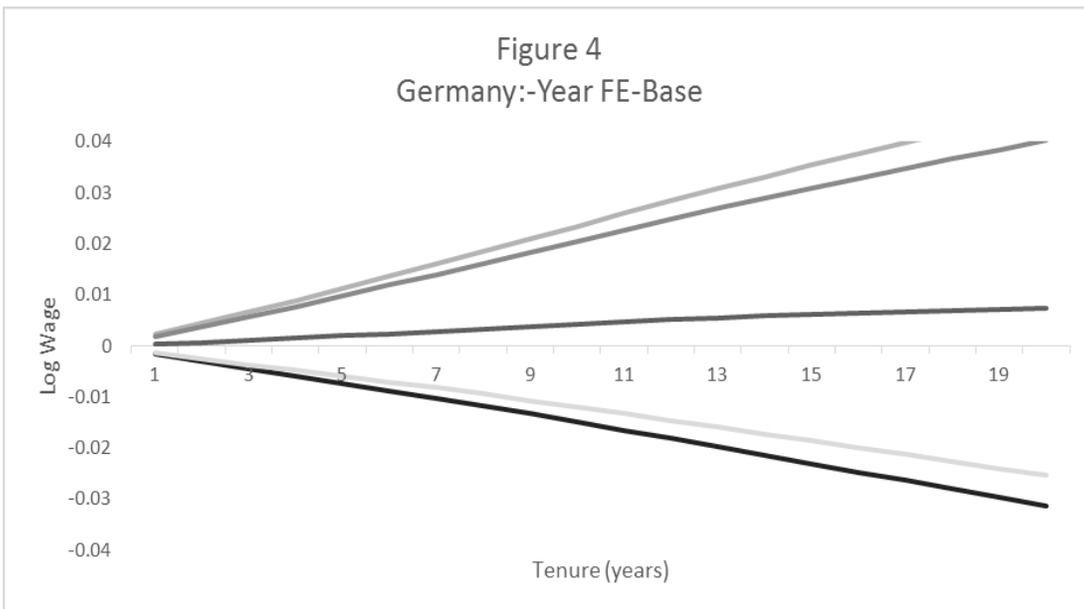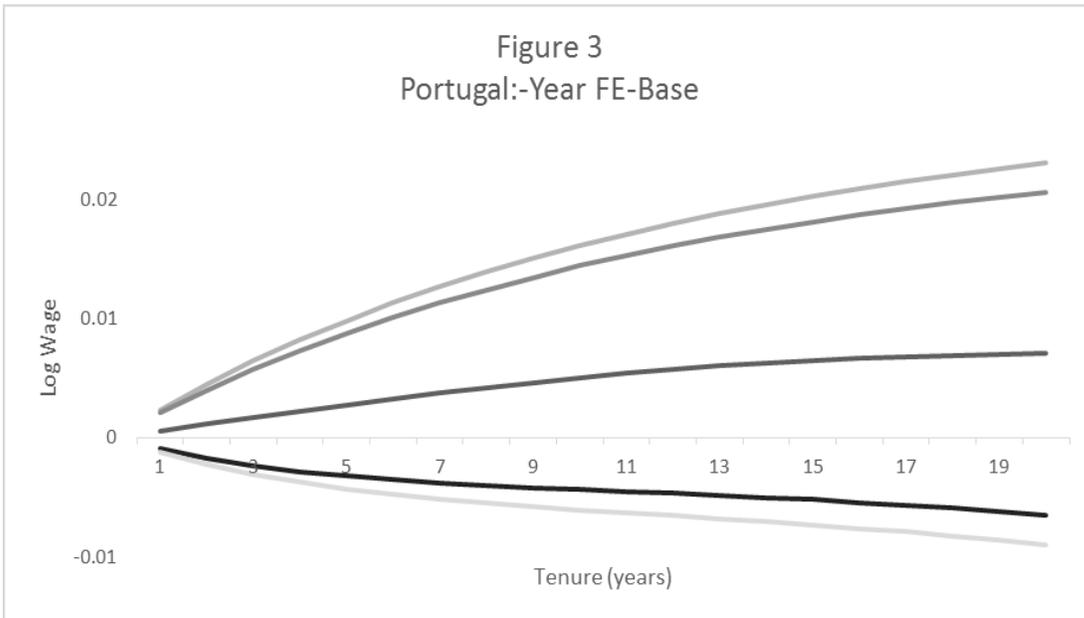$$vcov\sqrt{N}\left(\widehat{\beta}_{OLS} - \beta\right) - vcov\sqrt{N}\left(\beta^* - \widehat{\beta}_{OLS}\right) = A + o(1),$$

where A is a positive definite matrix. Finally note that the RTT estimate at tenure τ evaluated at some estimated coefficient vector β^{**} is just

$$RTT(\tau, \beta^{**}) = (\tau, \tau^2, \tau^3, \tau^4, 0, 0, \ldots, 0)\beta^{**}.$$

It follows that for large N, $var\{RTT(\tau, \widehat{\beta}_{OLS})\} > var\{RTT(\tau, \beta^* - \widehat{\beta}_{OLS})\}$. For large N then we may use the variance of $RTT(\tau, \widehat{\beta}_{OLS})$ (Base) as an upper bound for the variance of the differences in the RTT estimates (FYFE-Base).

Figure 1
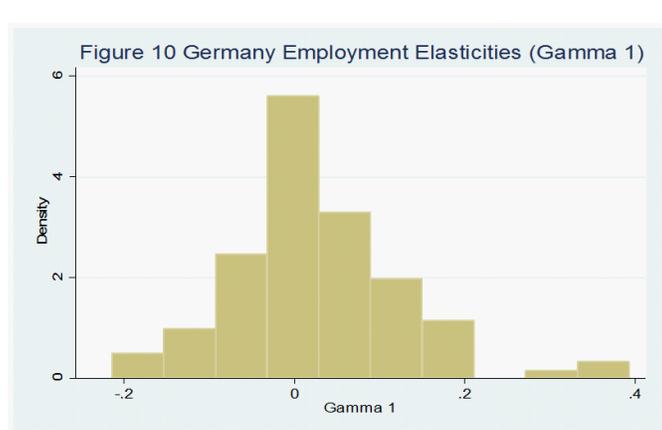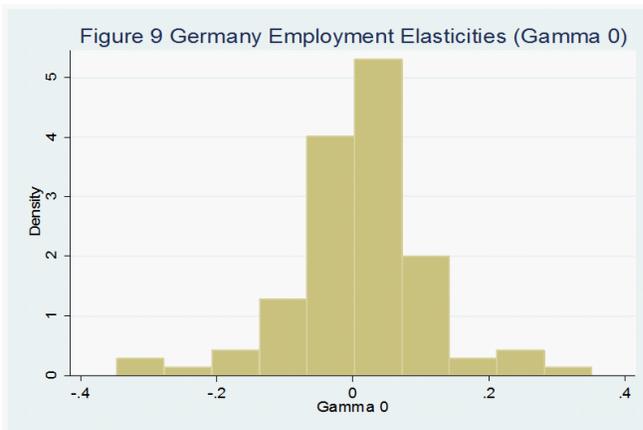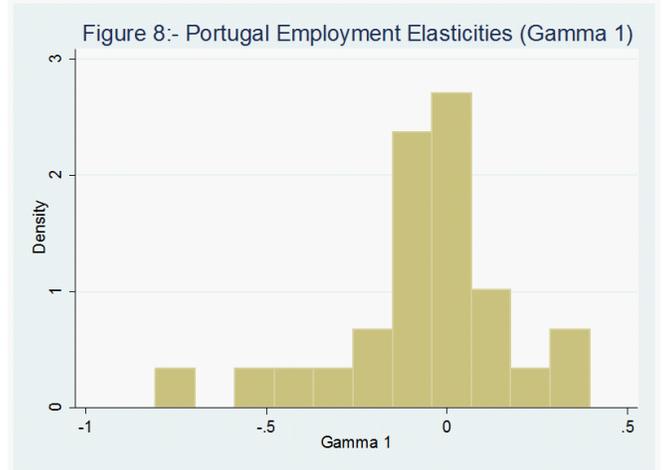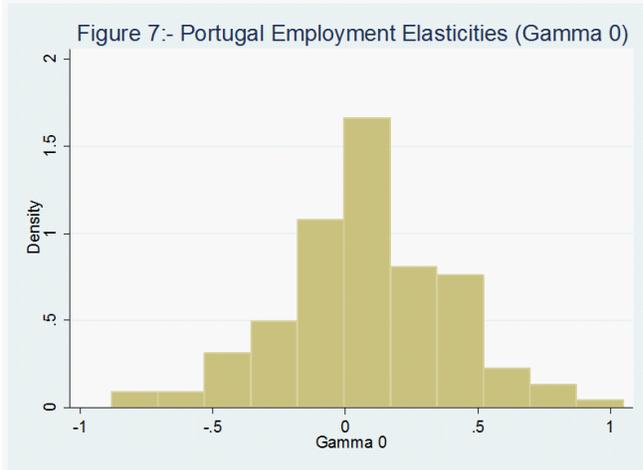Portugal:-FYFE-Base

Figure 2
Germany:- FYFE-Base
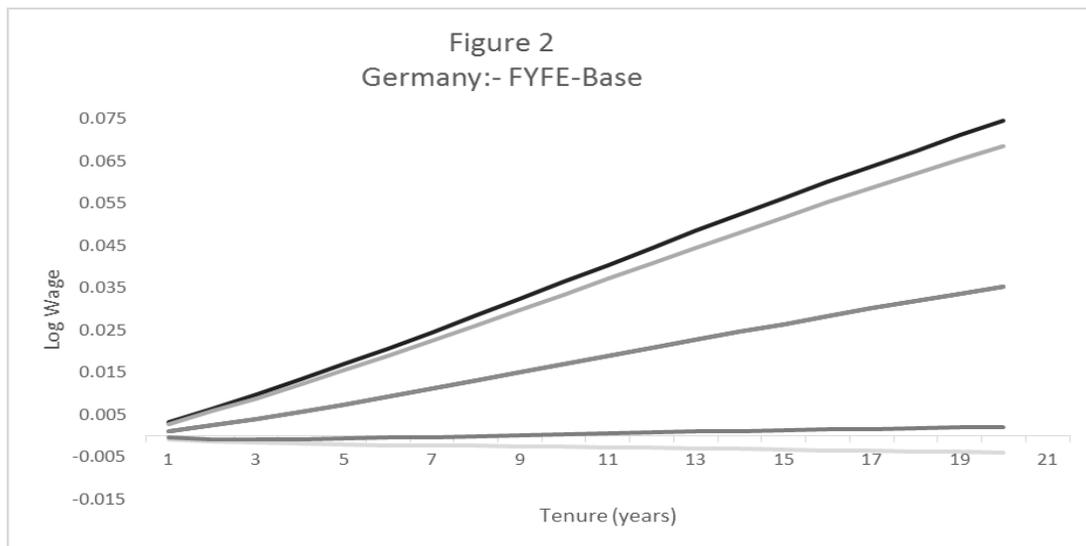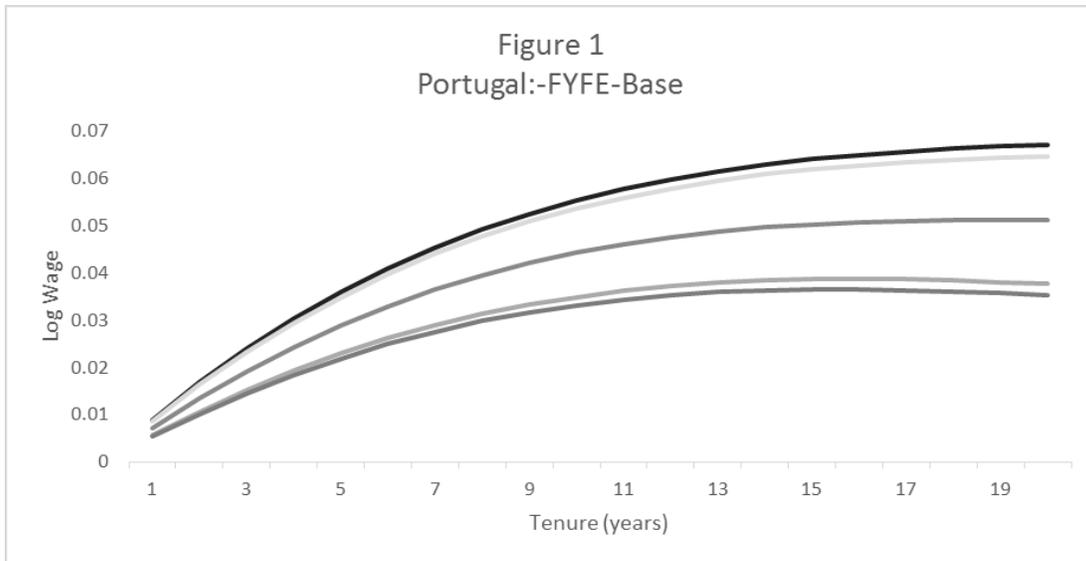
Note: The middle lines in Figures 1 to 4 above are the estimates indicated in the respective title. The two sets of outer bands are 90% and 95% confidence intervals respectively.

Figure 3
Portugal:-Year FE-Base

Figure 4
Germany:-Year FE-Base

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Figure 5
Portugal:-Employment-Base

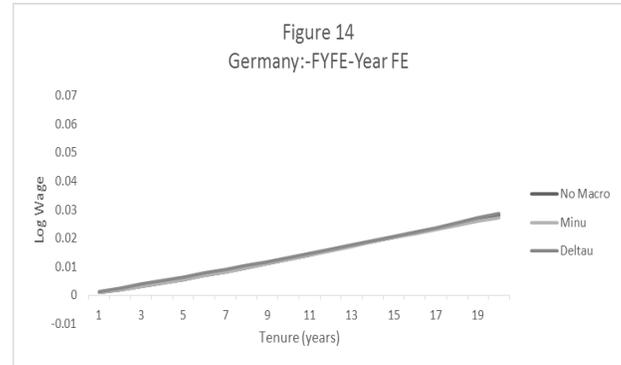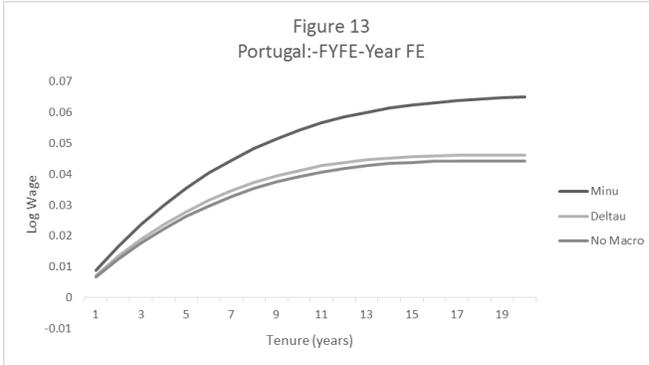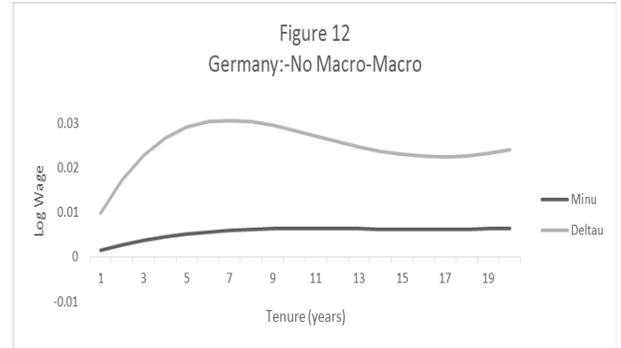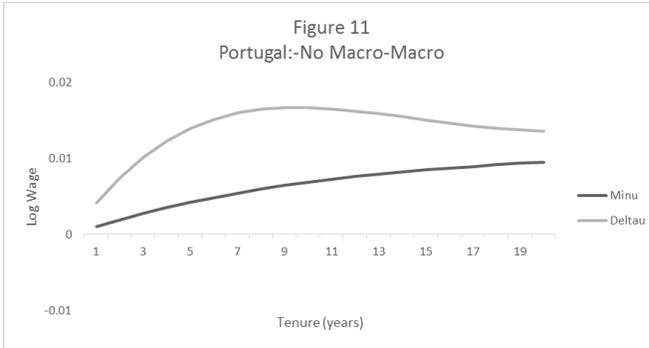Figure 6
Germany:-Employment -Base
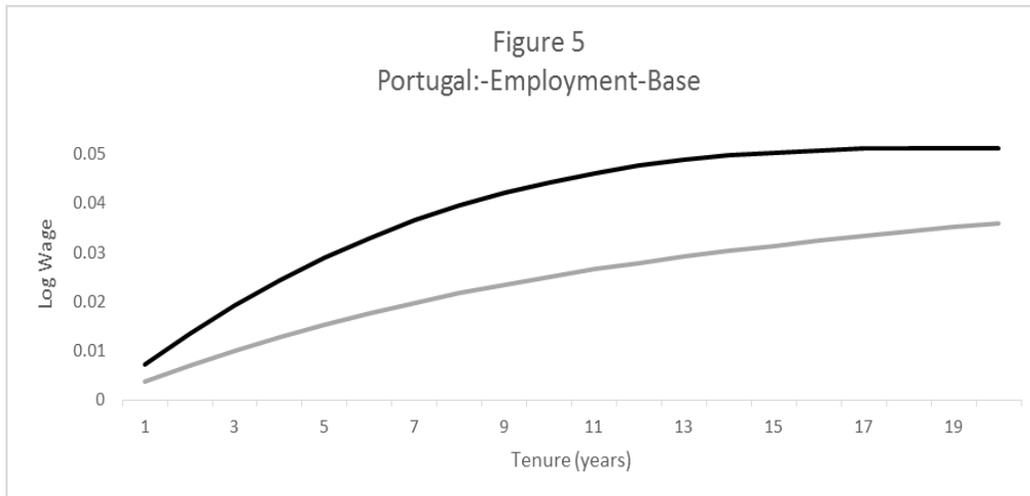
Note: The grey lines in Figures 5 and 6 above are the Employment-Base RTTE estimates. The dark lines are FYFE-Base. The vertical gap between the two lines represents the extent of the bias remaining after controlling for the firm employment terms.

Figure 7:- Portugal Employment Elasticities (Gamma 0)

Figure 8:- Portugal Employment Elasticities (Gamma 1)

Figure 9 Germany Employment Elasticities (Gamma 0)

Figure 10 Germany Employment Elasticities (Gamma 1)

Note: Figures 7 and 8 (resp. 9 and 10) give absolute frequencies of the γ_j^0 and γ_j^1 elasticities respectively from the Employment specification for Portugal (Germany).

Figure 11
Portugal:-No Macro-Macro

Figure 12
Germany:-No Macro-Macro

Figure 13
Portugal:-FYFE-Year FE

Figure 14
Germany:-FYFE-Year FE

Note: Figures 11 and 12 show the impact on tenure profiles of adding $minu$ and δu^0 to the specification that includes FYFE, equation (12). Figures 13 and 14 show tenure bias using Year FE as the base (i.e. FYFE - Year FE) for specifications including Minu and Deltau. The "No Macro" line is the FYFE-Year FE bias where neither macro variable is included.

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