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ABSTRACT

Biased Returns to Tenure: The Impact of Firm-Specific Shocks on Base and Non-base Earnings*

This study examines returns to tenure using Mincer wage regressions and longitudinal employer-employee payroll data from Great Britain. We find a pervasive downward bias in estimates of returns to tenure that rely solely on match fixed effects to control for unobserved factors influencing wages and tenure. This bias stems from the co-movement of average wages and tenure within firms, as theorised and empirically shown by Snell et al. (2018). By addressing this bias with firm-year fixed effects, we find that tenure-wage profiles increase by up to 20% in Britain's largest private-sector employers. Further analysis reveals that the bias primarily originates from non-base earnings (e.g., overtime). These findings underscore the need for caution when interpreting tenure returns from wage regressions that omit firm-year fixed effects, particularly in samples where non-base earnings are present; even if base earnings are sticky, firms may adjust other earnings components in response to shocks that influence employment levels.

JEL Classification: C23, J31, J63

Keywords: equal treatment wages, employer-employee data, mincer wage equation, UK Labour Market

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1. Introduction

A large body of empirical research has attempted to consistently estimate the returns to tenure (hereafter RTT), defined as the effect on wages of an additional year of tenure for a worker at their employer.[1] The size of RTT is important for several reasons. For instance, it informs the empirical relevance of theories of firm-specific human capital accumulation in wage growth. It also matters for our understanding of the persistent costs of job loss, a subject of much policy discussion. The standard approach for measuring RTT involves regressing wages on tenure using Mincer models (Mincer, 1974), typically controlling for worker-firm match quality to address the endogeneity of tenure; better matches tend to last longer (Topel, 1991).

However, A. Snell, P. Martins, H. Stüber and J. P. Thomas (2018) (hereafter SMST) recently identified another crucial unobservable factor that causes endogeneity: firm-specific wage shocks that generate a negative relationship between average wages and tenure. For example, a positive firm-specific shock may raise wages and lead to new hires, reducing average tenure within a firm. Since, by assumption, such shocks impact all employees within a firm similarly (see the related literature on equal-treatment wage effects, e.g., Gertler and Trigari, 2009; Snell and Thomas, 2010), SMST propose controlling for firm-specific shocks by including firm-year interaction fixed effects in Mincer wage regressions.

In this study, we use accurate and representative longitudinal employer-employee data from Great Britain to revisit the bias in RTT estimates, measured by the differences between estimates of tenure-wage profiles from standard regressions that control only for worker-firm match quality and enhanced regressions that additionally control for firm-year effects as proposed by SMST. Hereafter, when we say, "the bias" or the "the RTT bias", we are referring to bias which comes from not controlling for firm-year interaction fixed effects, unless otherwise specified. We analyse when the bias is likely to be less of a concern, providing guidance on when reduced-form regressions without firm-year fixed effects may still yield reliable estimates. This is important, as adding firm-year interaction fixed effects may pose prohibitively high demands on the data at hand: researchers require access to employer-employee panel datasets with large

[1] For example, Abraham and Farber (1987), Adda and Dustmann (2023), Altonji and Shakotko (1987), Altonji and Williams (2005), Barth (1997), Bronars and Famulari (1997), Buchinsky et al. (2010), Buhai et al. (2014), Dostie (2005), Dustmann and Meghir (2005), Topel (1991), and Williams (1991). Addison et al. (2023) contains a partial literature review of the empirical issues in estimating RTT, and Zwick (2011) describes the early theoretical and empirical literature on the relationship between establishment characteristics and seniority wages.

numbers of employee observations for each firm and year. Additionally, motivated by research showing that different components of pay respond differently to economic conditions, we examine whether the bias in RTT estimates varies across wage measures (base earnings versus base earnings plus any non-base earnings, such as overtime).[2] This comparison provides a systematic analysis of firm-specific shocks' impact on wage components, enhancing our understanding of how different wage measures evolve with tenure.

Our findings provide several novel insights. First, the extent of the bias depends substantively on the wage measure studied. The bias is close to zero for up to ten years of tenure for base earnings. In contrast, the sum of base earnings and non-base earnings shows substantial downward bias in returns to tenure estimates when firm-year interaction fixed effects are excluded, with the bias-corrected model showing a steeper tenure-wage profile by around 20% of the level at 15 years of tenure. How firm-specific shocks affect non-base earnings seems to account for this: the bias-corrected approach shows that the likelihood of receiving non-base components increases with tenure, whereas standard Mincer regressions erroneously suggest the opposite. Moreover, our results are robust to controlling for employees' hours worked, indicating that firm-specific shocks lead to the co-movement of firm-level average tenure and wage rates per hour rather than hours worked.

We also observe substantial heterogeneity in the bias of RTT estimates. In large manufacturing firms, the bias pushes estimated tenure-wage profiles substantially downwards, contrasting with approximately no evidence of this bias among large firms in other sectors. Similarly, the larger a firm is the greater the absolute downward bias in the tenure-wage profile. Overall, our findings imply that employment and non-base earnings components in large manufacturing firms are particularly sensitive to firm-specific shocks; researchers who study such firms, or groups of workers likely to work in them, should ensure their RTT estimates are not biased by firm-specific wage shocks.

While the literature on estimating RTT is vast, we are only aware of three studies that have estimated them with firm-year interaction fixed effects included in the regression models. The first study is SMST, which analysed data from a small number of large firms in Portugal and Germany. They found that controlling for firm-specific

[2] There is evidence that non-base pay components are more responsive to the business cycle than base pay, see, e.g., Devereux (2001), Martins et al. (2012), Schaefer and Singleton (2019), and Shin and Solon (2007). Relatedly, firms that face downward nominal wage rigidity tend to use non-base pay components to adjust their labour costs when hit by firm-specific shocks (e.g., Babecký et al., 2012; Schaefer and Singleton, 2023).

shocks, using firm-year interaction fixed effects in reduced-form models, increased the estimated wage-tenure profiles by about 30%. Importantly, SMST showed that this bias is present in RTT estimates even after controlling for match-quality effects. More recently, Snell et al. (2024) revisited the same source of bias, examining the role of monopsony power. They split their estimation sample into firms with high and low monopsony power, defined by a Herfindahl index of labour market concentration above or below the median value. They found that higher monopsony power increases the size of the downward bias, arguing that the co-movement of firm-level wages and average tenure is likely stronger when firms have more wage-setting power. Finally, Pires et al. (2023) used administrative-matched employer-employee data from Brazil to analyse how the RTT are related to firm wage premia estimates from a standard two-way fixed effects model (Abowd et al., 1999). They found that RTT negatively correlate with firm wage premia, and this relationship is more negative among larger firms. However, including firm-year interaction fixed effects reversed the sign of that correlation.

Our paper contributes to the literature in three ways. First, we add an important third data point on the bias in RTT estimates, to the existing evidence for Germany and Portugal. This is important, because Germany and Portugal have stronger labour market institutions and higher coverage of collective bargaining than Great Britain. These differences may affect the pass-through of firm-specific shocks to firm-level wages and average tenure, making our results potentially more relevant for other economies with comparable labour markets, such as the United States.

Second, we identify two additional important data dimensions for the bias: non-base earnings and the manufacturing industry, where the negative co-movement of firm-level average wages and tenure is particularly pronounced. This means that standard Mincer wage regressions, which can only control for match quality, are less likely to suffer from the bias in RTT estimates due to firm-specific shocks when focusing on base earnings or industries where non-base earnings are not prevalent. Third, we provide the first estimates of the RTT from Great Britain that account for firm-specific shocks: five years (ten years) of employer tenure raise employees' average base earnings per hour by 2.6 (4.5) log points. As expected in the presence of firm-specific shocks, this is larger than found in the recent study on returns to tenure by Postel-Vinay and Sepahsalari (2023), whose British household survey data did not allow computation of firm-year interaction fixed effects. They estimated various regression models that show average returns to five years of tenure ranging from -2.8

to 2.4 log points. We provide further details on other previous UK studies' findings on RTT in Appendix Section B.

The remainder of the paper proceeds as follows: Section 2 presents the econometric model, the estimation strategy, and the data. Section 3 presents the results, and Section 4 concludes.

2. Model, Estimation, and Data

2.1. Model

We first provide an insight into the potential for firm-specific shocks to generate significant bias in traditional estimates of returns to tenure (RTT). Consider the following linear regression model between log wages and tenure:[3]

$$\ln(w_{ijt}) = \alpha + \beta \tau_{ijt} + \varphi_{jt} + u_{ijt} \ , \tag{1}$$

where w_{ijt} represents the wage of worker i in firm j at time t, τ_{ijt} denotes the worker's tenure at the firm, φ_{jt} is an unobserved, mean-zero firm-specific (FS) shock, and u_{ijt} is an i.i.d. shock capturing any factors not accounted for by tenure and FS shocks (e.g., match quality). The parameter of interest is β, which measures the return to an additional year of tenure. Following Snell et al. (2024), we assume that, in the absence of any shocks, the values of firm j's employment share and the average tenure of its employees are both constant and that the number of workers in each firm-year and the number of firm-year observations are both large.

Under these assumptions, and as demonstrated in Snell et al. (2024), the bias in the ordinary least squares (OLS) estimate of β from omitting the FS shock, ϕ_{jt}, in this linear model is proportional to:

$$\sum_{k=1}^{N_K} (s_k \overline{\tau}_k - \widehat{s\tau}) \varphi_k - \overline{\tau}\, \overline{\varphi} \ , \tag{2}$$

where k denotes a particular combination of firm j and year t, hereafter a firm-year. We denote the number of total firm-years in our sample by N_K. The variable s_k denotes the share of workers in the sample who are employed in firm-year k, $\overline{\tau}_k$ is the average tenure

[3] For expositional clarity, we use the example of a linear relationship between wages and tenure, but the intuition also holds for non-linear models involving higher-order polynomials in tenure.

in firm-year k, and φ_k is the FS shock φ_{jt} in firm-year k. The term $\widehat{s\tau}$ is the steady-state value of $s_k\overline{\tau}_k$ in the absence of shocks, while $\overline{\tau}$ and $\overline{\varphi}$ are panel averages of tenure and unobserved FS shocks, respectively.

The above expression highlights when not appropriately controlling for FS shocks will lead to a downward bias in RTT estimates - the empirically relevant case: first, when FS shocks are mostly positive in firm-years when values of $s_k\overline{\tau}_k$ lie below their steady-state levels, meaning that the first term is negative; and second, when more workers in the panel experience positive rather than negative FS shocks ($\overline{\varphi} > 0$), given that average tenure is always positive ($\overline{\tau} > 0$). These conditions imply that the more firms adjust their employment in response to positive FS wage shocks, either through more hiring or laying off high-tenure employees, the more likely there will be a downward bias in the OLS estimates of RTT, such that wage-tenure profiles will be underestimated when not appropriately controlling in the regression model for the presence of FS shocks.

Another important insight from the above bias formula concerns its implications for different measures of earnings and earnings per hour. Consider an extension of Equation (1), where the wage is the sum of two components, base and non-base earnings, which we will discuss in more detail in the next section. The log of the sum of base and non-base earnings (hereafter gross earnings) can be approximately written as the log of base earnings, b_{ijt}, plus the ratio of non-base to base earnings, x_{ijt}, assuming this ratio tends to be small:

$$\ln(w_{ijt}) \approx \ln(b_{ijt}) + x_{ijt} \; . \tag{3}$$

Further, assuming each of these two variables has its own linear relationships with tenure and FS shocks, the OLS bias in the estimates of RTT is then approximately proportional to:

$$\sum_{k=1}^{N_K} (s_k\overline{\tau}_k - \widehat{s\tau})(\varphi_{b,k} + \varphi_{x,k}) - \overline{\tau}(\overline{\varphi}_b + \overline{\varphi}_x) \; , \tag{4}$$

where the subscripts b and x now differentiate whether the FS shocks affect the amount of base earnings or the ratio of non-base to base earnings. As this formula demonstrates, a downward bias in the estimated RTT for gross earnings, from not controlling for the FS shocks in the regression model, can be caused by both base and non-base earnings. Non-base earnings can then be the source of the downward bias in OLS estimates of RTT under two possible scenarios, not mutually exclusive: first when the FS shocks affecting the ratio of non-base pay to base earnings are mostly positive

($\varphi_{x,k} > 0$) and are larger than the shocks for the amount of base earnings ($\varphi_{x,k} > \varphi_{b,k}$), when tenure in firm-years is below its steady-state values, such that the first term is negative; and second, if workers in the panel, on average, tend to experience positive FS shocks to non-base earnings to a greater extent than they do in their base earnings ($\overline{\varphi}_x > \overline{\varphi}_b > 0$).

Further, suppose that w refers to gross earnings and b to average base earnings per hour (or if w referred to gross earnings per hour and b to base earnings). Then we could also extend the above bias formula by adding terms for FS shocks as they relate to total hours (base hours plus any overtime hours, since:

$$\ln(\text{gross earnings per hour}_{ijt}) \approx \ln(\text{base earnings}_{ijt}) - \ln(\text{total hours}_{ijt}) + x_{ijt} \ . \tag{5}$$

This would allow us to check whether the bias that is corrected by adding FS shocks to the regression model is driven by firms altering the gross working hours of their employees rather than hourly rates of pay.[4]

It is theoretically possible that FS shocks to base earnings and the ratio of non-base to base earnings are negatively correlated, in which case we would anticipate that the bias in the OLS estimate of β is greater in base earnings than it is in gross earnings. For instance, this could occur if firms tended to respond to a positive FS shock by converting their workers' habitual overtime into regular base hours, or by moving their incumbent workers' incentive-based contracts onto salaried work. However, it is well documented across micro-level datasets in various countries that base earnings and base earnings per hour exhibit downward nominal rigidity, and that firms use non-base earnings to adjust their labour costs. For instance, Babecký et al. (2019) found evidence from 25 European countries that firms used non-base earnings to reduce their labour costs when adjusting to the 2007-08 financial crisis and the Great Recession. Interestingly, this strategy was more evident among larger firms and firms in the financial intermediation sector, where non-base earnings tend to make up a higher share of total compensation. Relatedly, Schaefer and Singleton (2023) documented that employers in Great Britain are significantly more likely to adjust gross earnings than base earnings, with substantial evidence pointing towards downward nominal wage rigidity in base earnings per hour (see also Grigsby et al., 2021, for similar results for the United States). Consequently, our prior is that RTT estimates for gross earnings and gross earnings per hour will be more susceptible to downward bias in RTT estimates

[4]Evidence on how FS shocks affect working hours could be instructive especially for researchers who have access to administrative linked employer-employee data that lack information on hours worked, as is the case, for instance, with the frequently used matched employer-employee data from Germany.

than base earnings per hour, if firm-year effects controlling for FS shocks are omitted from regression models. Further, we expect that the downward bias in RTT estimates will be more sensitive to controlling for firm-year fixed effects, if focused on groupings of firms where non-base earnings are common or highly correlated with tenure, such as in larger firms or the manufacturing sector (see Kersley et al., 2006, for evidence from the UK and Sockin and Sockin, 2024, for the US).

2.2. Estimation

In this section, we describe our approach to estimating tenure-wage profiles for earnings and earnings per hour for Great Britain. We assume that wage variables follow a standard Mincer-wage regression of the following general form:

$$\begin{aligned}\ln(w_{ijt}) =& \beta_1 \tau_{ijt} + \beta_2 \tau_{ijt}^2 + \beta_3 \tau_{ijt}^3 + \beta_4 \tau_{ijt}^4 + \gamma_1 e_{ijt} + \gamma_2 e_{ijt}^2 + \gamma_3 e_{ijt}^3 + \gamma_4 e_{ijt}^4 \\ &+ m_{ij} + \varphi_{jt} + u_{ijt} \ , \end{aligned} \tag{6}$$

where w_{ijt} denotes the wage of worker i in firm j at time t. τ_{ijt} denotes the worker's tenure in the firm, and e_{ijt} is lifetime work experience. We assume that wages may be affected by an unobserved worker-firm (match) component, m_{ij}, encompassing both worker and firm fixed effects. Additionally, unobserved FS shocks, φ_{jt}, may affect all wages in firm j. Finally, we assume that the idiosyncratic error, u_{ijt}, is uncorrelated with the regressors.

As is widely understood and applied, it is important to control for the unobserved match quality of an employment relationship, as it likely correlates with a worker's tenure. When a match is particularly good (high m_{ij}), a worker's separation likelihood of leaving the firm may fall, and their expected tenure would rise. Without controlling for the worker-firm match component of wages, tenure is endogenous, resulting in upward-biased RTT estimates.

As discussed in the previous section, the firm-year-specific factor, or FS shock φ_{jt}, represents any factor that might cause the co-movement of average wages and firm tenure. For example, a positive firm-level shock might increase wages and hiring, leading to higher average wages and lower average tenure within the firm. This will induce a downward bias in the RTT estimates, as SMST explained and illustrated above in the simple linear model relating tenure and wages. To assess the quantitative importance of these FS shocks, we compare the RTT estimates from regression model (6) when only controlling for match-fixed effects and year-fixed effects to those obtained when we additionally control for firm-year interaction fixed effects (which supersede the year-fixed effects). Under the standard assumption in the literature that worker experience and match quality are not correlated (Topel, 1991), Equation (6), with match and firm-year fixed effects, can identify the true average RTT profile of employee wages. By contrast, models without the firm-year fixed effects are expected to generate downward-biased estimates of RTT.[5]

In practice, we compute the RTT estimates in two steps. First, we estimate Equation (6) omitting the linear tenure and experience terms, because the fixed effects - either by year or firm-year - leave β_1 and γ_1 unidentified. Unlike Snell et al. (2024), we include match fixed effects instead of using first differences within matches. Our approach accommodates a dataset where employment spells are often unbalanced – it is common for worker spells or matches observed in the British panel dataset that we use to have intermittent missing values, due to some employers not filing a return to the statistical authority in some years.

In the second step, we adapt the approach from Topel (1991) and SMST by estimating two auxiliary regressions. First, we use the first-step estimates to compute the residual log wages of employees:

$$R_{ijt} = \ln(w_{ijt}) - \hat{\beta}_2\tau_{ijt}^2 - \hat{\beta}_3\tau_{ijt}^3 - \hat{\beta}_4\tau_{ijt}^4 - \hat{\gamma}_2 e_{ijt}^2 - \hat{\gamma}_3 e_{ijt}^3 - \hat{\gamma}_4 e_{ijt}^4 \,. \tag{7}$$

Then, we collect all the new hire observations within the original estimation sample. By definition, new hires have $\tau_{ijt} = 0$. In the first auxiliary regression, we regress the values of R_{ijt} for new hires on their initial starting experience in a match and a linear time trend. Assuming, as per Topel (1991), that experience does not systematically correlate with match quality, the coefficient of starting experience provides a consistent estimate of the linear effect of experience, $\hat{\gamma}_1$. In the second auxiliary regression, we regress the residual log wages for all employees in the estimation sample on their tenure and, once again, a linear time trend:[6]

$$R_{ijt} = \alpha + \pi\tau_{ijt} + c \cdot t + \varepsilon_{ijt} \,. \tag{8}$$

[5]More experienced workers might form better matches, according to job-shopping search models (Topel, 1991). In this case, returns to experience will be overestimated and returns to tenure underestimated. Topel (1991) presents evidence suggesting that these biases are unlikely to be significant. In any case, bias due to firm-specific shocks should not be affected by job-shopping.

[6]Instead of using a linear trend, we have confirmed that our findings are virtually unchanged when using year fixed effects in both the first and second auxiliary regressions.

This gives an estimate of $\hat{\pi} = \hat{\beta}_1 + \hat{\gamma}_1$, since tenure and experience both increase together within a match. Therefore, under the mentioned assumptions, $\hat{\pi} - \hat{\gamma}_1 = \hat{\beta}_1$ provides a consistent estimate for the coefficient of linear tenure. However, the time trends in these two auxiliary regressions may differ because our panel of jobs is unbalanced, as we explain below.

We estimate the above regression model using least squares for gross and base earnings, gross and base earnings per hour, and the ratio of non-base to base earnings. Then we explore the empirical version of the bias described in the previous section, by subtracting the RTT estimates using the only match fixed effects specification (MFE), from those using the same sample of employee-year observations with a specification that also allows for firm-year interaction fixed effects (FYFE), for $\tau = \{1, ..., 20\}$. We do not focus much on the levels of the estimated RTT, since it is plausible that other confounders are missing from the model, such as measures of job seniority at firms, and since we will only be using random samples of matches within firms, such that the bias accounted for by the FYFE specification may still be attenuated somewhat.

Our approach does not control for industry- or occupation-specific tenure, which have been found to matter for life-cycle wage growth (e.g., Kambourov and Manovskii, 2009). This omission might affect our RTT estimates. To see this, note that if firm-specific tenure correlates with occupation- or industry-specific tenure, not controlling for the latter two factors will induce omitted variable bias in the least squares estimates of average RTT. However, the literature typically assumes that industry- and occupation-specific tenure are additively separable from firm tenure. In this case, our measures of RTT bias - the differences between estimates from regressions that control only for worker-firm match quality and estimates from regressions that control additionally for firm-year interaction effects - will not suffer from omitted variable bias.

Finally, as in Snell et al. (2018), we have no robust method to construct confidence intervals for the RTT profiles estimated by the approach described above, nor for the extent of differences between RTT profiles from different model specifications. We will, though, be employing large samples of employee-years, and the individual tenure and experience coefficient estimates of the models are almost universally significant.[7]

[7] One potential way to construct confidence intervals could be to block-bootstrap over the firms or firm-years in our estimation samples, but these all contain quite different numbers of employee observations. There is also no obvious way to construct standard errors for the linear tenure and experience terms that are derived from the estimation approach.

2.3. Data

We estimate the regression models discussed above using data from the Annual Survey of Hours and Earnings (ASHE) (Office for National Statistics, 2024), administered by the UK Office of National Statistics (ONS) since 2004. The ASHE is carried out in April each year and is based on employer responses for a one per cent random sample of employees who make national insurance contributions.

The ASHE is an ongoing, linked employer-employee dataset, which allows researchers to track employees over time and links them to their respective employers using unique employer identifiers. The dimensions of the ASHE dataset have made it increasingly valuable for deepening our understanding of the role that firms play in shaping pay patterns in the UK (e.g., Aghion et al., 2024; Bell et al., 2022; Duchini et al., 2024; Hall et al., 2024; Jewell et al., 2020; Jones and Kaya, 2023; Phan et al., 2023; Pham et al., 2024; Schaefer and Singleton, 2023). The ASHE has several advantages over the UK household-level surveys used previously to estimate RTT, which are summarised in Appendix Table B1. First, employers are legally obliged to provide comprehensive information on various aspects of the employment relationship, such as earnings without top-coding, working hours, tenure, firm size, and industry. Another advantage of ASHE is that it allows us to examine different measures of labour market remuneration (e.g., various earnings components: incentive pay, overtime pay, shift-premium pay, and other pay such as allowances). Additionally, the longitudinal employee and firm aspects of the ASHE allow us to control for individual, firm, and match fixed effects, which is essential to examine the bias in the estimation of RTT, as described above. Finally, the large sample size of the ASHE, providing information on approximately 200,000 employees annually, allows us to control for firm-year fixed effects that require multiple worker observations per firm and year.

Although the ASHE provides information on the basic demographic characteristics of employees, such as age and gender, it does not provide information on human capital variables, such as education and job training. However, to the extent that education has already been completed when an individual is employed in a job lasting multiple years, the match fixed effects in our regression models should account for it. Even so, to abstract from education accumulation and retirement or occupational downgrading, in our main estimation sample, we select only employees from 2004 to 2020 who are aged 21-59.

In our baseline sample, we only consider matches where an employee is continually working full-time (30 or more hours per week). If we observe intermittent spells for an employee at the same firm, where the recorded tenure restarts, we only keep the first match. In our main analysis sample, to reliably estimate firm-year interaction fixed effects, we only keep matches from firms that employ at least 5,000 employees every year that they appear in the ASHE and which are always recorded as being in the private sector. We provide sensitivity analyses later, dropping all matches in firm years with fewer than ten observations per year. We also vary the firm size criteria between 1,000 and 10,000 employees, and we consider public sector employers separately.

The ASHE provides detailed information on both base earnings and non-base earnings, such as overtime pay, premium payments for shift, night and weekend work, incentive pay for work carried out in the pay period, and any pay received through payroll for other reasons (e.g., meal or car allowances). The ASHE also provides information on base hours and overtime hours worked. Additionally, we derive hourly pay rates, by dividing base earnings by base hours, and dividing gross earnings by the sum of base hours and overtime hours. We summarise the various pay variables in Appendix Table A7. Before selecting the firms in our estimation samples, to remove outliers in the ASHE records, we first trim employee-year observations that are in the top or bottom 0.5% of the gross earnings per hour distribution, and then further trim the top and bottom 0.5% of the remaining observations according to the base earnings per hour distribution.

The ASHE lists the month and year that an employee first started working for an employer. Therefore, interrupted spells at an employer do not reset tenure. We use the employment start date to compute tenure in months as of each April. If an employee has less than 12 months of tenure in April, we call this employee a new hire. Appendix Table A1 provides the number of wage observations in our baseline estimation sample and for the whole ASHE sample of spells without selecting on large private sector firms (hereafter all-firms sample), by year. Over the whole sample period of 2004-2020, our baseline sample contains 431 consistently large firms (5,000 employees or more), generating 4,205 firm-years, for 36,052 employee-firm matches or spells, and a total of 168,661 employee-year observations (see column (1) of Appendix Table A1). As such, on average, the baseline sample contains around 40 employee gross earnings observations and all sub-components of earnings per firm-year.

Table 1 displays descriptive statistics for our baseline and the all-firms samples. By design, our baseline sample consists of large private sector firms, where the average employee has almost 45,000 co-workers. Concerning the industry distribution of

TABLE 1: Sample means (or share of employee-year observations)

	Baseline Sample	All-Firms Sample
Firm size (100s)	449	200
Private sector	1.00	0.63
Industry Sector (SIC2003):		
D (Manufacturing)	0.109	0.143
E (Electricity, Gas and Water Supply)	0.054	0.016
F (Construction)	0.023	0.027
G (Wholesale and Retail Trade)	0.276	0.124
H (Hotels and Restaurants)	0.052	0.020
I (Transport, Storage and Communication)	0.159	0.097
J (Financial Intermediation)	0.136	0.079
K (Real Estate)	0.138	0.126
L (Public Administration and Defence)	0	0.097
M (Education)	0	0.126
N (Health and Social Work)	0.022	0.112
O (Other Community Services)	0.029	0.029
Male	0.68	0.63
Age (years)	39.5	40.8
Tenure (years)	9.2	9.4
New hires	0.09	0.08
Gross earnings per week (£)	897	797
Gross earnings per hour (£)	18.18	18.92
Base earnings per hour (£)	17.01	17.87
Base hours worked per week	38.1	37.8
Total hours worked per week	39.5	39.1
Receives non-base earnings	0.55	0.49
Ratio of non-base to base earnings	0.12	0.11
No. firm-year observations	4,205	104,564
No. employee-year observations	36,052	804,956

Notes: Descriptive statistics for 2004-2020. Nominal values converted to 2020 GBP using the UK Consumer Price Index. Industry categories according to the UK Industry Standard Industry Classification 2003 (SIC2003). New hires are all employees with less than 12 months of tenure. Appendix Table A7 summarises the various pay variables.

the baseline sample, the shares of employee-year observations in Other Community Services and Construction are approximately the same as in the all-firms sample. The shares of employee-year observations in Manufacturing, as well as Health and Social Work, are relatively lower compared to the all-firms sample. In contrast, the share of employee-year observations in the rest of the industries is relatively higher in our baseline estimation sample than in the whole ASHE. A higher share of male employees is observed in our baseline sample compared to the all-firms sample. On average, the employees in our baseline sample are about a year younger, have almost the same years of tenure, and are slightly more likely to be new hires, compared with in the all-firms sample. On average, gross earnings and the share of non-base earnings are higher in the baseline sample. Gross earnings per hour, base earnings per hour, total weekly hours worked, base weekly hours worked, and the ratio of non-base to base earnings are, on average, about the same in both samples.

3. Results

3.1. Main Results

Figure 1 shows the estimated tenure profiles for each specification, either using only MFEs or also adding FYFE, as well as the implied RTT bias, given by the vertical gap between the FYFE and MFE estimates (see Appendix Table A3 for the underlying coefficient estimates). Table 2 summarises these estimated tenure-wage profiles and the implied bias for selected years of tenure.

Panel A of Figure 1 displays the RTT estimates for gross earnings. According to both specifications, the RTT are sizable and larger earlier on in a worker's time at a firm. It is evident that there is downward bias of the tenure profile of gross earnings when not correcting for firm-year fixed effects in Great Britain, and this bias gets more severe as tenure increases. For example, using only MFEs, we find cumulative returns to tenure of 0.89 log points, which increases to 1.11 log points when additionally including FYFEs. This means the RTT profiles are underestimated by 0.22 log points when not including FYFEs (Table 2, column III). The downward bias due to FS shocks leads to an underestimation of almost 25% of the level at five years of tenure. This suggests that the employment of lower tenure cohorts and gross earnings positively co-move within firms. Moreover, MFE and FYFE tenure profiles in Great Britain begin to decline after six and eight years of tenure, respectively, which is broadly in line with the evidence for Germany and Portugal (SMST) and the United States (Altonji and Williams, 2005).

As Panel B shows, RTT profiles for gross earnings per hour do not substantively decline with tenure. The average year-to-year log change in gross earnings per hour in our baseline sample is 4.7 log points for employees who are working in consecutive years at the same firm (Appendix Table A2). This suggests that the decline in gross earnings is mainly explained by working fewer hours with higher tenure, all else equal.

Again, the RTT downward bias in gross earnings per hour is sizeable: adding firm-year fixed effects raises the estimated RTT profile by 0.51 log points or around 13% of the level estimates at ten years of tenure (3.54 log points), and the bias increases with tenure.

Panel C displays the RTT estimates for total weekly hours worked, the sum of base hours and overtime hours. As our baseline sample only includes full-time employees, we expect no large bias in hours worked. Indeed, we see that the difference between the MFE and FYFE estimates is virtually zero, which is confirmed by the small estimates in Table 2. This finding implies that FS shocks do not lead to systematic co-movement of total hours worked and average within-firm tenure for full-time employees.

In Panel D, we show that the downward bias in RTT estimates is also present in base earnings per hour, though to a smaller extent than in gross earnings per hour, in line with our priors as outlined in Section 2. In fact, up to ten years of tenure, the difference in the estimated RTT profiles using MFE and FYFE is virtually zero. This suggests that the downward bias in the RTT estimates is mainly driven by non-base earnings components; average within-firm tenure and non-base earnings negatively co-move because of firm-specific shocks. This would be consistent with the notion that firms do not adjust base earnings per hour in response to FS shocks, but instead increase non-base earnings, such as incentive pay or allowances, to expand employment, especially along the new hiring margin.

TABLE 2: Estimates of tenure-wage profiles and RTT bias

	Gross earnings			Gross earnings per hour			Base earnings per hour			Ratio of non-base to base earnings			Total hours		
	MFE	FYFE	Bias	MFE	FYFE	Bias	MFE	FYFE	Bias	MFE	FYFE	Bias	MFE	FYFE	Bias
Tenure	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)	(XI)	(XII)	(XIII)	(XIV)	(XV)
2	0.521	0.573	-0.052	1.045	1.119	-0.075	1.222	1.128	0.093	-1.075	-0.834	-0.241	-0.624	-0.667	0.043
5	0.887	1.107	-0.220	2.273	2.500	-0.227	2.704	2.611	0.092	-2.667	-2.186	-0.482	-1.636	-1.696	0.060
10	0.568	1.164	-0.596	3.535	4.046	-0.511	4.355	4.477	-0.123	-5.252	-4.573	-0.679	-3.467	-3.492	0.025

Notes: Baseline sample, 2004-20. All estimates are in log points. "Tenure" shows the years that an employee is working for an employer. "MFE" shows tenure-wage profiles based on estimates from Equation (6) when only controlling for worker-firm match quality, whereas "FYFE" shows tenure-wage profiles when controlling for match-worker quality and firm-year interaction fixed effects. "Bias" shows the difference between MFE and FYFE tenure-wage profiles. See Appendix Tables A1-A2 for dimensions of the estimation samples and Appendix Table A3 for the coefficient estimates used to compute the profiles. Appendix Table A7 summarises the various pay variables.

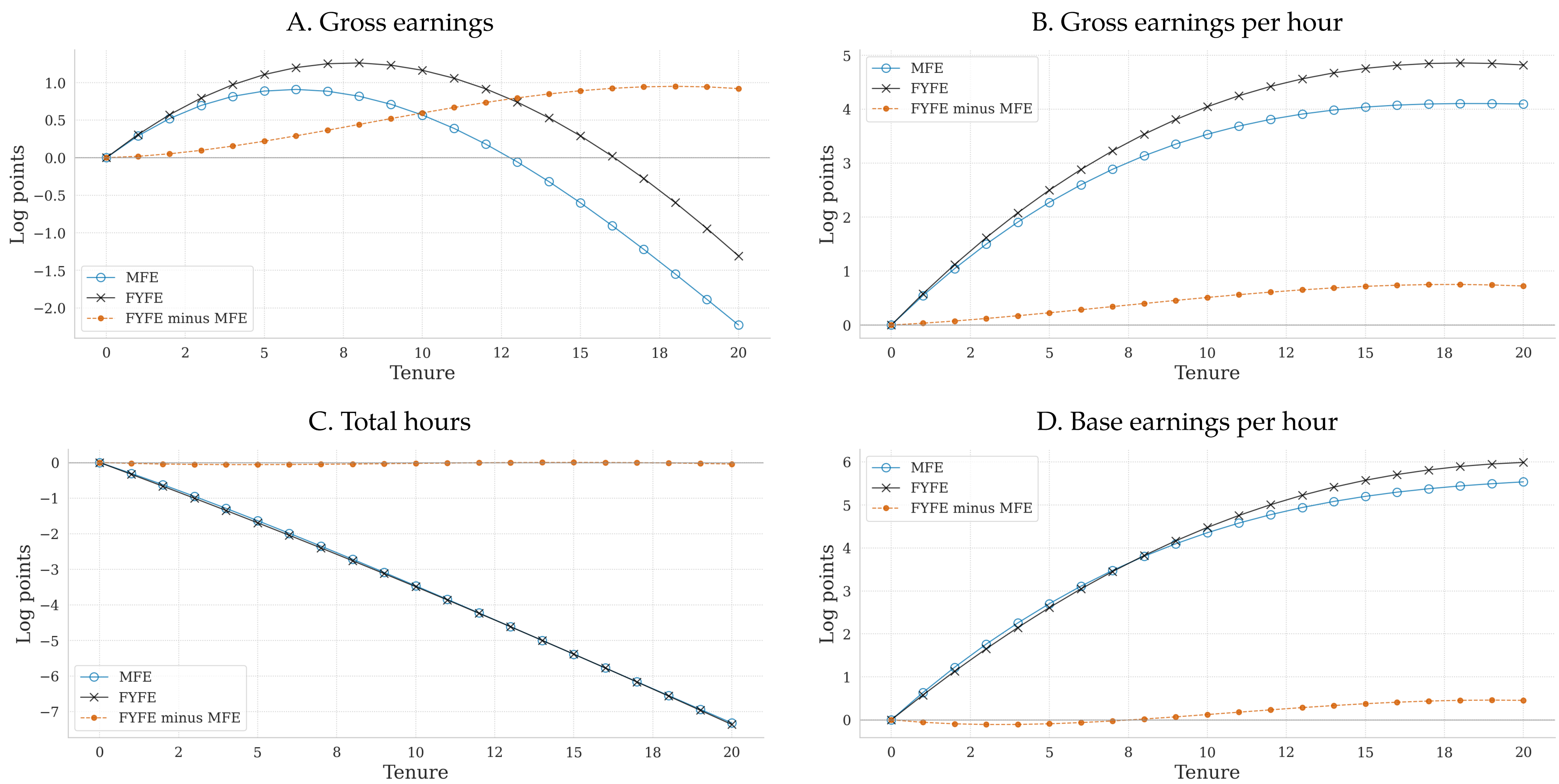

FIGURE 1: Estimated RTT profiles for different outcome variables, comparing MFE and FYFE specifications

Notes: Baseline sample, 2004-20. See the notes of Table 2 for details on the computation and Appendix Table A7 for a summary of the various pay variables.

After finding that non-base earnings are the main driver of the downward bias in RTT estimates, we now provide a deeper analysis of this earnings component. Figure 2 shows the probability of receiving any non-base earnings components along the tenure distribution. Panel A shows that RTT estimates using only MFEs are substantially downward biased, even showing a slight decline with tenure, while the corrected FYFE specification shows that the likelihood of receiving non-base earnings is increasing with tenure. This supports the notion that firms expand their hiring in response to a positive FS shock, by systematically paying more non-base earnings components to new hires than previously to high-tenure cohorts.

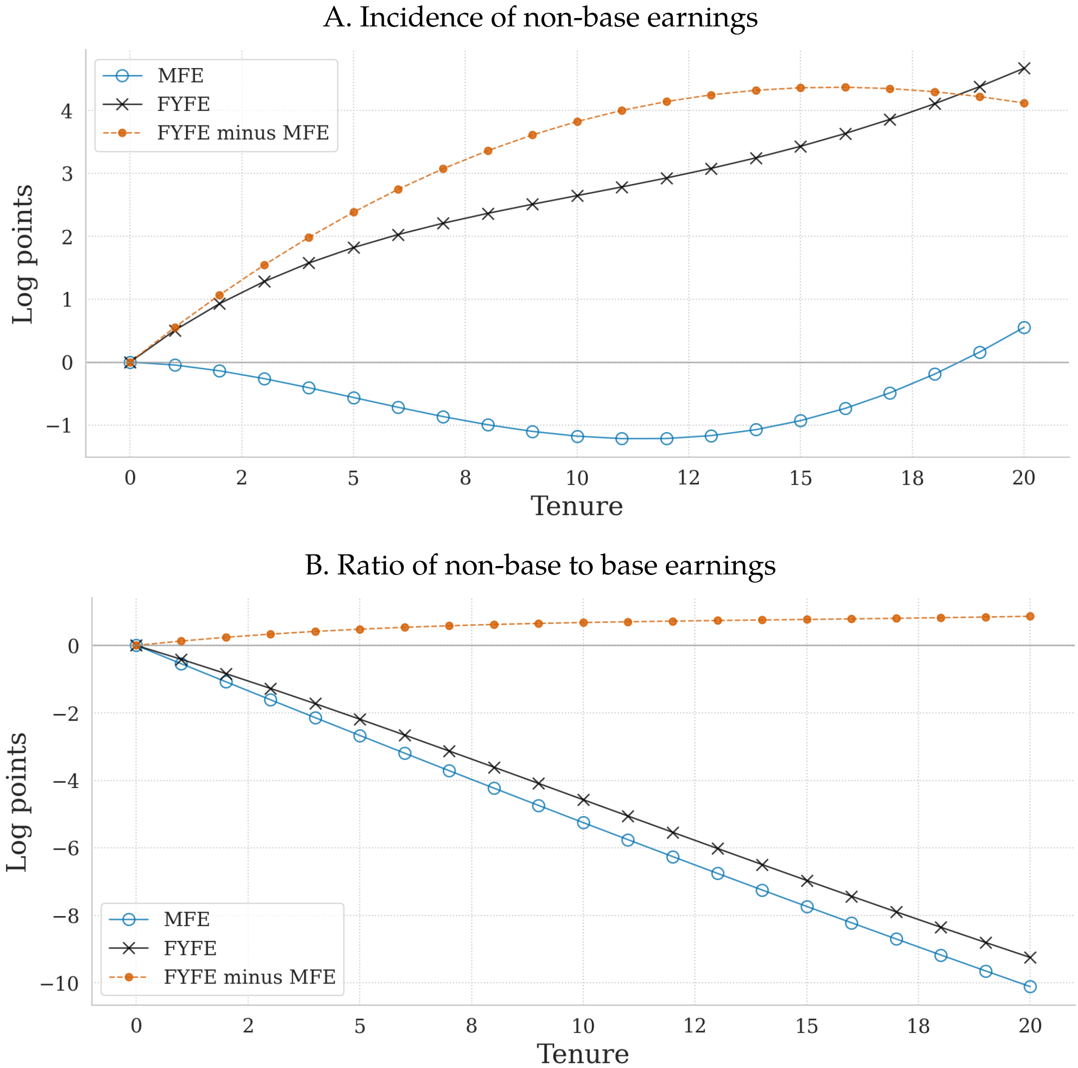

FIGURE 2: Estimated RTT profiles for the incidence of non-base earnings and the share of base in gross earnings, comparing MFE and FYFE specifications

Notes: Baseline sample, 2004-20. See the notes of Table 2 for details on the computation and Appendix Table A7 for a summary of the various pay variables.

Panel B shows estimates of RTT using the ratio of non-base to base earnings. In this way, estimates in this panel capture both the extensive and the intensive margin of non-base earnings. The RTT profiles confirm the presence of severe *upward* bias in estimates of the ratio of non-base to base earnings, implying that the importance of non-base earnings is systematically underestimated by regression models that are based on MFEs only.

To assess which non-base components of earnings generally associate with tenure in our estimation sample, Figure 3 displays the composition of non-base earnings. The left axis shows that the biggest share of non-base components within gross earnings consists of overtime pay, being followed by other pay and shift premium pay. Incentive pay attains the lowest share of non-base earnings and accounts for about 1%. These shares remain approximately constant across the tenure distribution. The right axis of Figure 3 plots the share of employees who receive a positive amount of non-base earnings. The share increases with years of tenure in the firm from 45% among new hires to over 62% among employees with 20 years of tenure.

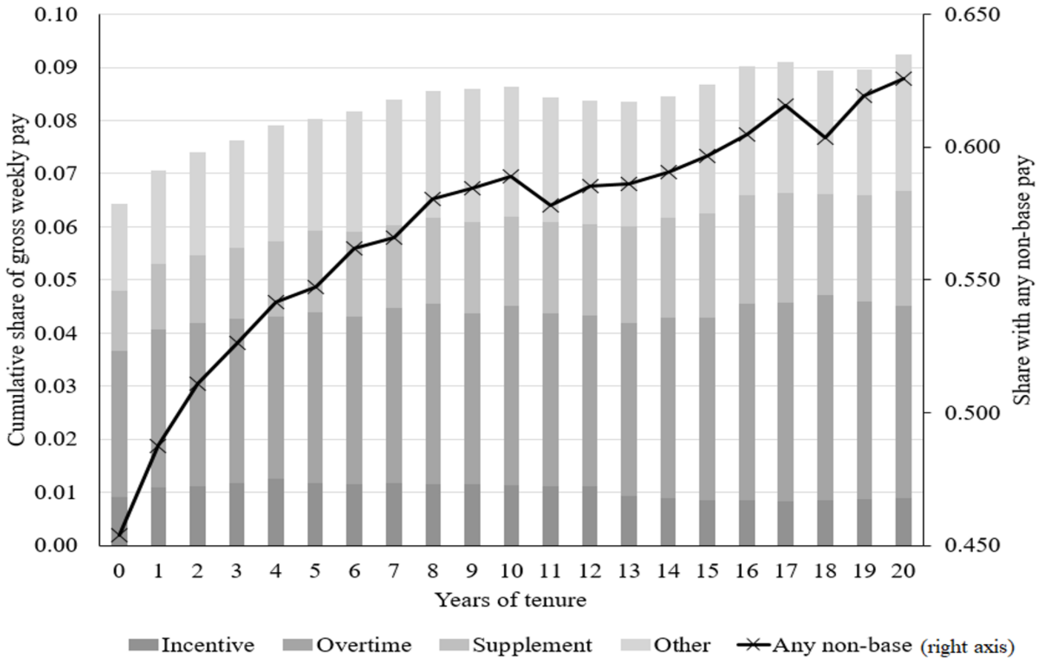

FIGURE 3: Importance and incidence of non-base components within gross earnings, by tenure

Notes: Baseline sample, 2004-20. Appendix Table A7 summarises the non-base earnings components.

3.2. Heterogeneity and Robustness Analysis

Experience. It is possible that not controlling for actual work experience affects the RTT bias. Indeed, Figure 4 shows that the returns to tenure are slightly lower when, in addition to age, we control for a second proxy of employee experience: the cumulative years that a person is observed in a job since 1975.[8] We find that the profiles for gross earnings per hour, whether controlling for experience or not, track each other closely, and the RTT bias is not affected.

Public sector employers. We also examine the bias in RTT estimates in the public sector. Wage setting and other labour market phenomena, such as career progression, are different in the public sector than in the private sector. The tenure-gross earnings per hour profile shows that RTT estimates are low in the public sector and become negative after 16 years of tenure (Figure 4). Although smaller than in the previous results for private sector firms, the vertical distance between the FYFE and MFE RTT estimates is visible. Hence, the downward bias in RTT profiles is also empirically present among public sector employers.

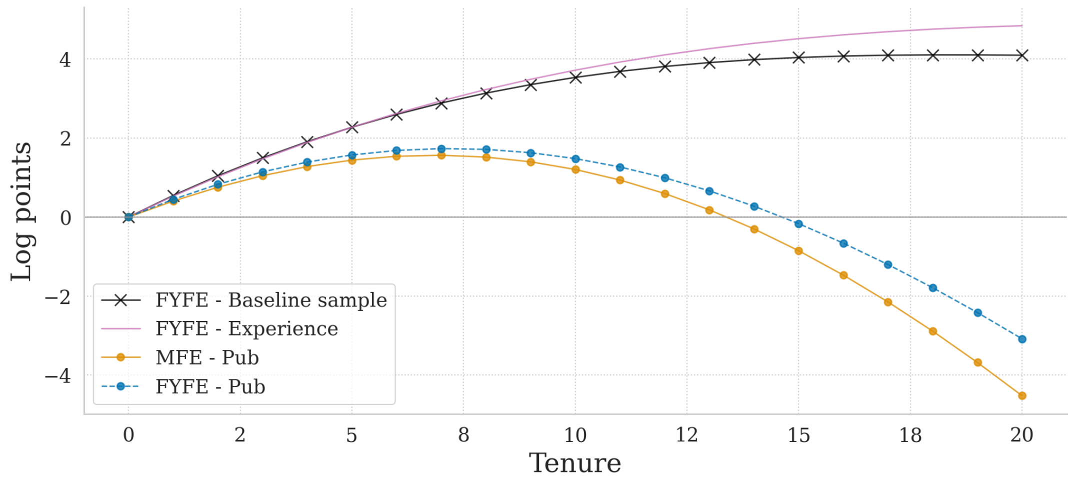

FIGURE 4: Estimated RTT profiles, FYFE specifications for gross earnings per hour: robustness checks

Notes: Appendix Table A2 for sample sizes. See Appendix Table A4 for the model coefficient estimates used to plot the profiles.

Firm sizes. Another potential concern is that our findings only apply to the baseline sample of large private sector firms (at least 5,000 employees) that we have analysed. Figure 5 displays tenure-gross earnings per hour profiles using different firm size

[8]To compute experience in the years before 2004, we use the precursor to the ASHE, the New Earnings Survey Panel Dataset (NESPD) (Office for National Statistics, 2022). The NESPD does not include firm identifiers, however, people can be longitudinally linked between 1975-2016.

cutoffs for sample selection. Panel A replicates the main estimates for convenience. Panel B presents profiles for relatively smaller firms with a minimum annual size of at least 1,000 employees, and Panel C for very large firms, with a minimum annual size of more than 10,000 employees. The FYFE RTT profiles consistently lie above the MFE profiles, implying a downward bias in the RTT estimates for all these samples of firms. Indeed, although the level of the RTT profile is higher (lower) in larger (smaller) firms, the downward bias is approximately constant at 20-25% of the level of the MFE estimates.

While the MFEs control for any permanent differences in the level of gross earnings per hour across employees, it is possible that the tenure profiles of high-tenured employees exhibit different slopes compared to low-tenured employees. This would induce selection bias in our RTT estimates. To assess whether such selection bias is an issue, we restrict the sample to employees for whom we observe at least ten firm-year matches. The resulting RTT profiles displayed in Panel D are very similar to our main results, as is the downward bias in RTT estimates.

Male share above versus below median firms. We split our sample of firms into two: 'male-above-median' firms with a share of male employees greater than the median share (68%) and 'non-male-above-median' firms with a share of male employees below 68%. Appendix Figure A1 shows that the downward bias in RTT estimates is driven by male-above-median firms. The RTT bias in gross earnings per hour is at over two percentage points more than twice as large as in the main analysis. In contrast, the RTT bias is small and negative in the sample of non-male-above-median firms.

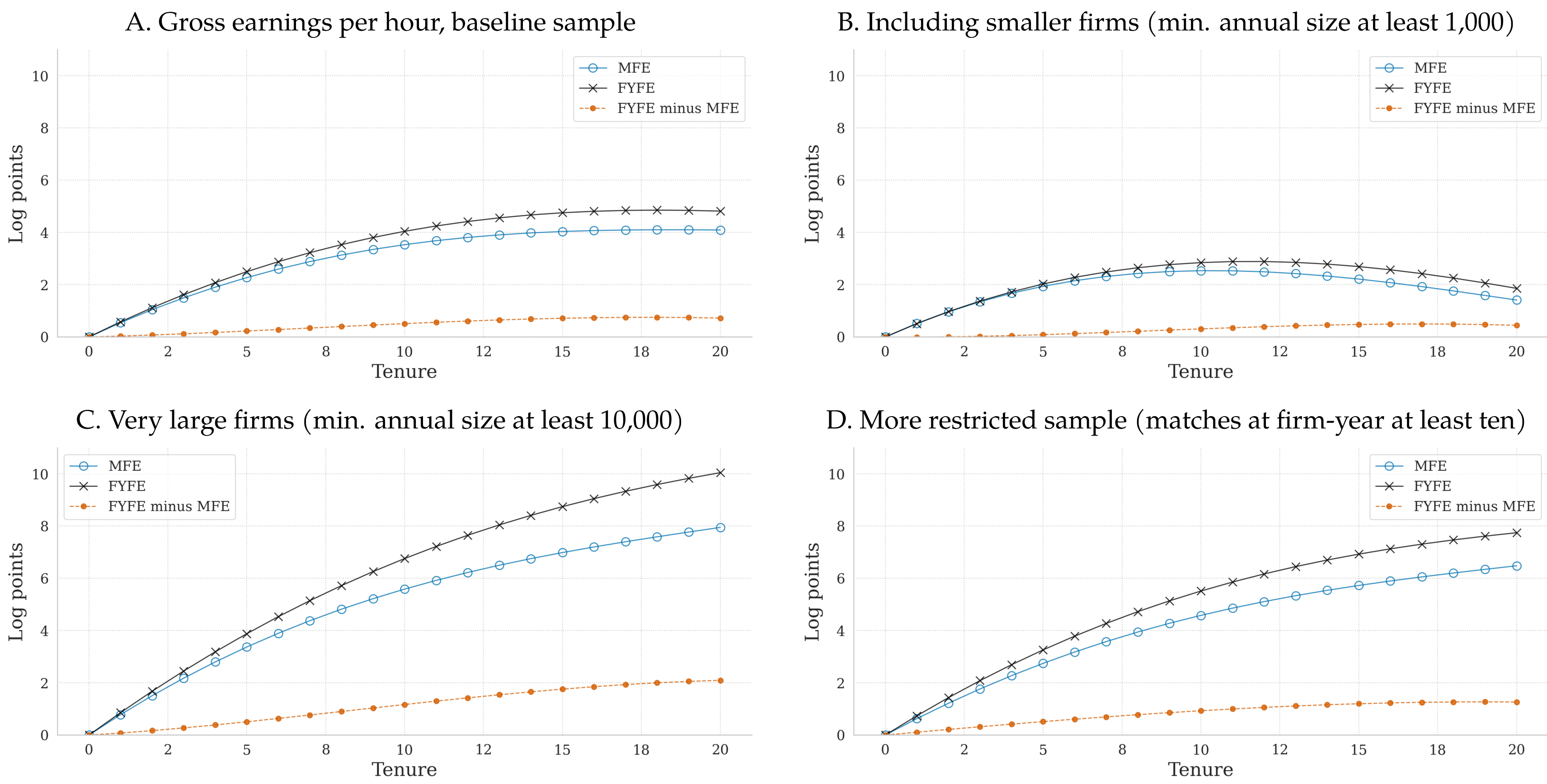

FIGURE 5: Estimated RTT profiles for gross earnings per hour, comparing MFE and FYFE specifications - varying estimation sample selection criteria

Notes: See Appendix Table A2 for dimensions of the estimation sample. For the coefficient estimates used to plot the profiles, see Appendix Tables A3-A4.

Industry sectors of firms. We explore robustness for sub-samples of the firms in the main estimation sample by industry sector. Therefore, we partition our data into five samples according to the 2003 Standard Industry Classification (SIC 2003) in the UK. The industries are manufacturing (41 firms), retail (108), communication (47), finance (39), and business services (85). Appendix Table A6 displays the RTT estimates, and Appendix Figure A2 shows the corresponding RTT profile plots, omitting the Retail industry for brevity. We find that manufacturing firms mainly drive our results about RTT bias. Therein, the RTT estimates using MFEs only are biased downwards substantially, while the bias is approximately zero in the other industries. Taken together with our main results, this finding suggests that manufacturing firms' employment and gross earnings per hour are particularly sensitive to firm-specific shocks. Specifically, non-base earnings components co-move negatively with average tenure in manufacturing firms, inducing a downward bias in RTT estimates. Snell et al. (2024) find that the bias in RTT estimates is particularly severe among firms that likely have more monopsony power. As much as manufacturing firms or larger firms also have higher monopsony power, our finding highlights the need to disentangle the importance of industry, firm size, and monopsony power in future research.

4. Conclusion

This paper examines the conditions where bias in the returns to tenure (RTT) from omitting firm-specific shocks, in a reduced form Mincer wage regression, is a concern for empirical labour economics researchers. We implement the bias-correction method proposed by Snell et al. (2018) and analyse accurate and representative employer-employee data from Great Britain. This not only adds an important third data point on the bias in RTT estimates to the existing evidence for Germany and Portugal – two countries with strong labour market institutions – but also potentially makes our results more relevant for economies with comparably flexible labour markets, such as the United States.

Confirming the results of previous research for Germany and Portugal, we find a significant downward bias in RTT estimates in Great Britain when using standard Mincer wage regressions that only control for worker-firm match quality. Adding firm-year interaction fixed effects, to correct for the bias, increases tenure-wage profiles by up to 20% in Britain's largest private-sector employers. We also show that not controlling for firm-specific shocks may still yield consistent RTT estimates in contexts where base earnings are the sole compensation.

Moreover, we provide the first estimates of the RTT in Great Britain that account for firm-specific shocks: five years (ten years) of employer tenure raise employees' average base earnings per hour by 2.6 (4.5) log points. Even so, our approach does not control for industry- or occupation-specific tenure, which have been found to matter for life-cycle wage growth and might affect RTT estimates (e.g., Kambourov and Manovskii, 2009). However, as we have argued, if industry- and occupation-specific tenure are additively separable from firm tenure, as is typically assumed in the literature, our findings regarding the bias in RTT estimates remain valid. We also uncover industry heterogeneity in the RTT bias. For instance, the bias is significantly larger in the manufacturing sector compared to other industry sectors. Future research should explore different components of non-base pay in manufacturing such as overtime pay, shift pay or rent sharing and examine their impact on the RTT.

Our results have important implications for the calibration of macroeconomic models of the labour market and for economic policy. For instance, the finding that previous RTT estimates were biased downwards suggests that skills are more employer-specific than previously thought. This relative lack of transferability of skills across firms implies that the costs of worker displacement and unemployment are relatively larger because the worker's human capital stock is more adversely affected by involuntary job separations. Therefore, a policy implication is not only to design interventions that help match workers with firms quickly, but also to ensure that this matching is long-lived.

Subsequent work could examine why tenure-wage profiles are notably negative in the finance sector, although we find zero bias among the very large firms there. Moreover, replicating our analysis for other countries besides Great Britain, that also have a high degree of labour market mobility, such as the United States, could provide further valuable insights.

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Biased Returns to Tenure: The Impact of Firm-Specific Shocks on Base and Non-Base Earnings

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Online Appendix

Appendix A. Additional Figures and Tables

TABLE A1: Numbers of employee-employer observations by year

Year	Baseline sample	All-firms sample
2004	9,016	43,376
2005	10,703	51,882
2006	11,160	52,643
2007	9,941	42,646
2008	9,391	41,311
2009	10,448	51,403
2010	10,403	51,248
2011	10,797	53,115
2012	10,840	50,727
2013	10,706	51,896
2014	10,913	51,559
2015	10,634	49,409
2016	10,319	48,271
2017	10,513	48,583
2018	8,910	47,067
2019	8,720	44,066
2020	5,247	25,754
Total	168,661	804,956

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TABLE A2: Average within-match nominal wage growth and numbers of observations for different samples

		Number of observations			
Sample	$\Delta \ln(w)$	Firms	Firm-years	Matches	Employee-years
Baseline	0.0471	431	4,205	36,052	168,661
Smaller Firms	0.0465	1,196	13,983	60,368	285,334
Larger Firms	0.0473	193	1,861	23,610	111,095
Restricted	0.0479	261	1,609	24,489	102,605
Public	0.0416	418	5,132	31,842	190,846
All-firms	0.0443	16,440	104,564	167,416	804,956
Male-above-median	0.0442	190	1,893	16,325	80,552
Non-male-above-median	0.0498	241	2,312	19,727	88,109
D: Manufacturing	0.0398	41	424	3,643	19,018
I: Communications	0.0424	47	462	5,187	27,472
J: Finance	0.0562	39	358	4,823	22,517
K: Business	0.0511	85	782	5,802	22,765
G: Retail	0.0460	108	1,110	9,222	46,296

Notes: The average year-to-year change within jobs in log nominal earnings per hour is $\Delta \ln(w)$. See notes of Appendix Tables A4-A6 for the definitions of the samples. “Matches” shows the number of worker-firm matches.

TABLE A3: Tenure-wage profile coefficient estimates

	Gross earnings		Gross earnings per hour		Base earnings per hour		Incidence of non-base earnings		Ratio non-base to base		Total hours	
	MFE	FYFE	MFE	FYFE	MFE	FYFE	MFE	FYFE	MFE	FYFE	MFE	FYFE
Tenure	-0.0033	0.0032	0.0033	0.0057	0.0060	0.0066	0.0059	-0.0002	0.0055	-0.0054	-0.0040	-0.0030
Tenure^2 ($\times 10^2$)	-0.0017	-0.0306	-0.0218	-0.0243	-0.0202	-0.0251	-0.0125	-0.0291	-0.0473	0.0011	-0.0094	-0.0063
Tenure^3 ($\times 10^4$)	-0.0033	0.0404	-0.0014	0.0227	0.0019	0.0232	-0.0244	0.2290	0.2180	0.0046	0.0421	0.0177
Tenure^4 ($\times 10^6$)	0.0111	0.0238	0.0578	0.0375	0.0467	0.0326	0.0714	-0.3580	-0.3030	-0.0075	-0.0535	-0.0137

Notes: Baseline sample, 2004-2020. "MFE" shows tenure-wage coefficient estimates from Equation (6) when only controlling for worker-firm match quality, whereas "FYFE" shows estimates when controlling for match-worker quality and firm-year interaction fixed effects. See Appendix Tables A1-A2 for dimensions of the estimation samples. Appendix Table A7 summarises the various pay variables.

TABLE A4: Tenure-wage profile coefficient estimates, robustness

	Smaller firms		Very large firms		Restricted sample		Public sector		All-firms		Exper.
	MFE	FYFE	MFE	FYFE	MFE	FYFE	MFE	FYFE	MFE	FYFE	MFE
Tenure	0.0055	0.0054	0.0081	0.0088	0.0065	0.0076	0.0043	0.0048	0.0037	0.0041	0.0055
Tenure^2 ($\times 10^2$)	-0.0351	-0.0280	-0.0290	-0.0208	-0.0209	-0.0220	-0.0245	-0.0324	-0.0288	-0.0284	-0.0203
Tenure^3 ($\times 10^4$)	0.0539	0.0213	0.0358	-0.0020	0.0116	0.0061	-0.0875	-0.0195	0.0060	-0.0246	0.0250
Tenure^4 ($\times 10^6$)	0.0088	0.0348	0.0304	0.0579	0.0571	0.0537	0.2330	0.1150	-0.0868	0.1350	-0.0024
N: Employee-years	285,334	285,334	111,095	111,095	102,605	102,605	190,846	190,846	804,956	804,956	168,661

Notes: Coefficient estimates from Equation (6), using gross hourly earnings as dependent variable. "Smaller firms" extends the baseline sample by firms with only 1,000 employees. "Very large firms" restricts the baseline sample to firms with 10,000 or more employees. "Restricted sample" restricts the baseline sample to firms for which we observe at least ten worker-firm matches every year. "Public sector" public sector firms only. "All-firms" uses the sample of all firms, as defined in Section 2.2. in the main text. "Exper." the regression model given by Equation (6) controls for MFE, FYFE, and employee experience. See Appendix Table A2 for dimensions of the estimation samples and notes of Appendix Table A3 for additional details.

TABLE A5: Tenure-wage profile coefficient estimates, robustness

	Male-above-median		Non-male-above-median	
	MFE	FYFE	MFE	FYFE
Tenure	-0.0024	-0.0008	0.0134	0.0121
Tenure^2 ($\times 10^2$)	-0.0103	-0.0066	-0.0593	-0.0525
Tenure^3 ($\times 10^4$)	0.0025	-0.0365	0.1090	0.1120
Tenure^4 ($\times 10^6$)	0.0284	0.0817	-0.0370	-0.0887
N: Employee-years	80,552	80,552	88,109	88,109

Notes: Coefficient estimates from Equation (6), using gross hourly earnings as dependent variable. “Male-above-median” firms with a share of male employees greater than the median share (68%). “Non-male-above-median” firms with a share of male employees below 68%. See Appendix Table A2 for dimensions of the estimation samples and notes of Appendix Table A3 for additional details.

TABLE A6: Tenure profile coefficient estimates, robustness

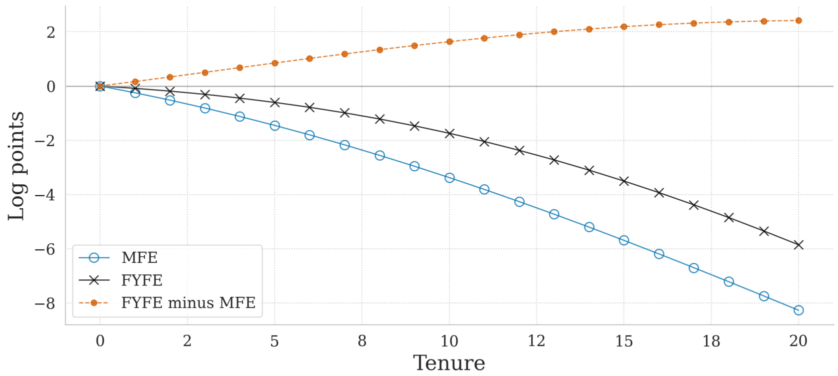
	Manufacturing		Retail		Communication		Finance		Business	
	MFE	FYFE	MFE	FYFE	MFE	FYFE	MFE	FYFE	MFE	FYFE
Tenure	-0.0039	0.0005	0.0121	0.0113	0.0078	0.0075	-0.0231	-0.0211	0.0163	0.0154
Tenure^2 ($\times 10^2$)	-0.0280	-0.0234	-0.0700	-0.0612	-0.0226	-0.0173	-0.0138	-0.0445	-0.0133	-0.0157
Tenure^3 ($\times 10^4$)	0.0493	0.0088	0.1360	0.1180	0.0552	0.0165	-0.0381	0.1110	-0.0680	-0.0191
Tenure^4 ($\times 10^6$)	-0.0238	0.0203	-0.0713	-0.0656	-0.0348	0.0233	0.0945	-0.1110	0.1800	0.0840
N: Employee-years	19,018	19,018	46,296	46,296	27,472	27,472	22,517	22,517	22,765	22,765

Notes: Coefficient estimates from Equation (6), using gross hourly earnings as dependent variable. “Manufacturing” Firms in the manufacturing industry (SIC 2003 D). “Retail” firms in the retail industry (SIC 2003 G). “Communication” firms in the communication industry (SIC 2003 I). “Finance” firms in the finance industry (SIC 2003 J). “Business” firms in the business services industry (SIC 2003 K). See Appendix Table A2 for dimensions of the estimation samples and notes of Appendix Table A3 for additional details.

TABLE A7: Overview of pay variables

	Description
ASHE variables	
Base earnings	Base weekly earnings, excl. any extra pay, before deductions
Overtime pay	Overtime pay
Shift premium pay	Premium payments for shift, night, and weekend work
Incentive pay	Incentive pay received for work carried out in the pay period
Other pay	Pay received for other reasons, e.g., meal allowances
Base hours worked	Weekly hours relating to base earnings, incl. hours paid at shift premium
Overtime hours worked	Weekly hours relating to overtime pay
Derived variables	
Non-base earnings	Sum of overtime, shift, incentive, and other pay
Gross earnings	Sum of base and non-base earnings
Total hours worked	Sum of base and overtime hours
Base earnings per hour	Base earnings divided by base hours worked
Gross earnings per hour	Gross earnings divided by total hours worked

A. Male-above-median firms



B. Non-male-above-median firms

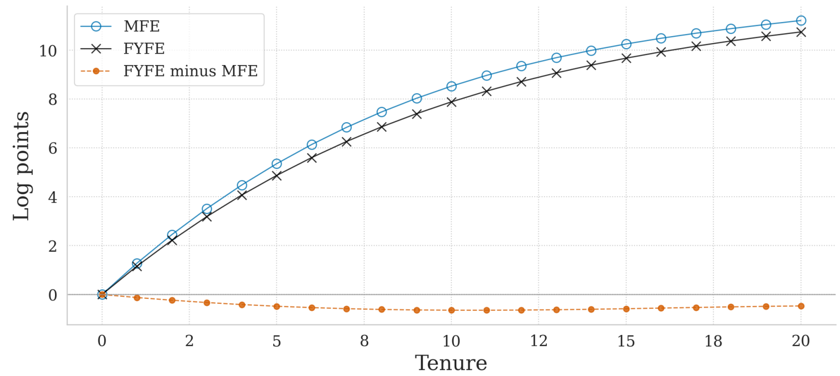


FIGURE A1: Estimated RTT profiles for gross earnings per hour, comparing MFE and FYFE specifications - male-above-median versus non-male-above-median firms

Notes: For the coefficient estimates used to plot the profiles and sample definitions, see Appendix Table A5.

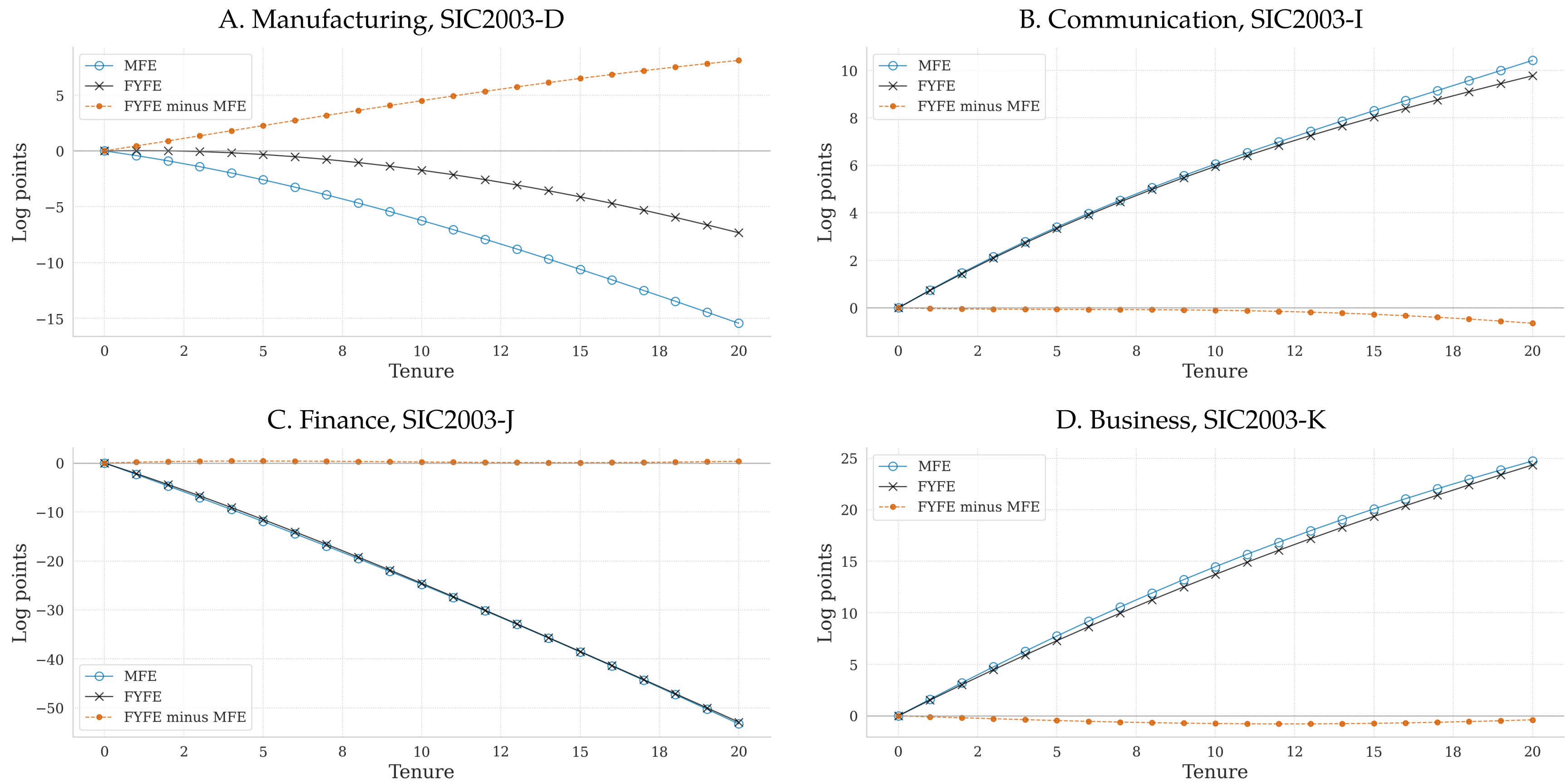


FIGURE A2: Estimated RTT profiles for gross earnings per hour, comparing MFE and FYFE specifications - industries

Notes: For the coefficient estimates used to plot the profiles and sample definitions, see Appendix Table A6.

Appendix B. Literature on Returns to Tenure in the United Kingdom

Appendix Table B1 outlines papers that provide estimates on the returns to tenure (RTT). It does so by presenting the papers chronologically. Four out of the six studies for Great Britain use household-level data (Dustmann and Pereira, 2008; Postel-Vinay and Sepahsalari, 2023; Williams, 2009; Zangelidis, 2008), one study uses employer data (Devereux et al., 2013), and only one study uses matched employer-employee data (Aghion et al., 2024). In accordance with the earlier literature, five (Devereux et al., 2013; Dustmann and Pereira, 2008; Postel-Vinay and Sepahsalari, 2023; Williams, 2009; Zangelidis, 2008) out of the six studies use the Altonji and Shakotko (1987) instrumental variable estimator (AS IV), or some variant of it, to solve the endogeneity problem caused by the fact that better matches tend to last longer. In this method, tenure is instrumented by deviations from observed job-specific means, assuming that job effects are not time-varying. Kambourov and Manovskii (2009) (KM) extend this instrumental variable estimator by additionally considering deviations from occupation- and industry-specific means. Two out of these five studies (Devereux et al., 2013; Williams, 2009) additionally use Topel's two-stage first difference (2SFD) estimator as outlined in the main text. Altonji and Williams (2005) examined the differences between the Topel (1991) and the Altonji and Shakotko (1987) estimators. They argued that Topel's estimator of the returns to tenure is biased upward due to individual heterogeneity, while the Altonji and Shakotko (1987) estimator is biased downward due to job match heterogeneity.

Since the British studies use only the two estimators and do not control for firm-specific shocks via firm-year interaction effects, the studies' results potentially suffer from RTT bias. The most recent study by Aghion et al. (2024) controls for match quality through worker-firm fixed effects but does not control for firm-year interaction fixed effects, leaving results exposed to the described RTT bias. Finally, and as shown in the fifth column of Table B1, none of the six studies examines different pay components.

In summary, the evidence provided by the literature on RTT in Great Britain is inconclusive. Some papers find large and significant returns (Devereux et al., 2013), whereas others find insignificant or even negative returns (Postel-Vinay and Sepahsalari, 2023).

TABLE B1: Returns to tenure - summary of UK evidence

Study	Data/Years	Specification	Sample and Pay Measure	Returns to Tenure
Dustmann and Pereira (2008)	BHPS/ 1991-1999	OLS, AS IV	Non-self-employed white men aged 18-60 in the private sector. Log of real hourly wage (real gross monthly pay divided by 4.33 times weekly hours worked).	OLS: 0.088 log points at 10 years of tenure. IV1: 0.054 (insignificant) IV2: 0.086 (significant) IV3: 0.045 (insignificant) at 10 years.
Zangelidis (2008)	BHPS/ 1991-2001	AS IV based on KM specification	Males aged 18-60, full-time workers (excluding self-employed, military, agriculture). Log hourly wage adjusted for paid overtime.	RTT ranges between 0.046-0.064 log points at 10 years, depending on specification. Drops to 0.035 log points (insignificant) with job-match and additional experience controls.
Williams (2009)	BHPS/ 1991-2001	OLS, AS IV, 2SFD	Males aged 18-60 in permanent positions in the private sector. Log of real hourly wage (gross usual monthly pay deflated by CPI, usual weekly hours, paid overtime).	OLS: 0.143 log points at 10 years. AS IV: 0.060 2SFD: 0.106 2SFD-IV: 0.079. RTT increases by 0.010 per year (first 10 years), becomes near zero with industry/occupation experience.

TABLE B1 continued: Returns to tenure - summary of UK evidence

Study	Data/years	Specification	Sample and pay measure	Returns to tenure
Devereux et al. (2013)	NESPD/ 1975-2010	OLS, AS IV, 2SFD	Full-time single job workers (male and female). Log hourly wage deflated by CPI (gross weekly earnings divided by basic hours).	Employer Spells (Male): OLS: 0.295 AS IV: 0.146 2SFD: 0.167 Employer Spells (Female): OLS: 0.299 AS IV: 0.170 2SFD: 0.270. RTT are approximately 1/3 higher in employer vs. job spells.
Postel-Vinay and Sepahsalari (2023)	BHPS, USoc/ 1992-2017	OLS, AS IV, based on KM specification	Males and females aged 16-64. Log hourly wage.	OLS: 0.024 log points (5 years). IV: -0.028 log points (negative) at 5 years.
Aghion et al. (2024)	ASHE-Census 2011/ 2003-2018	OLS (panel data with firm-worker fixed effects)	Workers aged 18-39 (public/private sector), less educated (high school dropout/graduate). Hourly wage deflated by CPI.	Each year of tenure increases wages by approximately 0.5 log points for less-educated workers in high-social-skill occupations.

Notes: BHPS (British Household Panel Survey), USoc (Understanding Society), NESPD (New Earnings Survey Panel Dataset), ASHE (Annual Survey of Hours and Earnings). BHPS and USoc are household surveys containing self-reported earnings. NESPD and ASHE are payroll-based employer surveys.