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Returns to Tenure: A Critical Assessment of the Evidence and Interpretation

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Abstract

This paper assesses the empirical evidence on returns to employer tenure. Using published studies and new illustrative estimates from British linked employer–employee payroll data, we show that estimated wage–tenure profiles vary substantially across data sources, wage measures, samples, and empirical specifications. We argue that this reflects a deeper issue: tenure coefficients should not be interpreted mechanically as causal returns to firm-specific human capital, since they may capture broader features of the employment relationship and the wage-setting environment. The literature on returns to tenure is best understood as delivering context-dependent empirical parameters, rather than convincing evidence for a single stable causal effect of remaining with the same employer.

JEL classification

C23, J31, J63

Keywords

seniority, wages, employer-employee data, mincer wage equation

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1. Introduction

A central question in labour economics is why workers' wages rise over time. One important dimension of this question concerns the returns to employer tenure, that is, the wage gain associated with remaining in the same worker–firm relationship. Tenure has long been used to study wage growth within firms, the value of continuing employment relationships, and the organisation of pay over the worker's career. Yet, despite decades of research, there remains no consensus on the magnitude, or even the existence, of causal returns to employer tenure.[1] Estimates of the wage gain associated with up to ten years of tenure range from close to zero (Abraham & Farber 1987; Adda & Dustmann, 2023) to nearly 30 percent (Topel, 1991; Buchinsky, Fougére, Kramarz & Tchernis, 2010).

This lack of consensus is striking because the parameter of interest appears conceptually well defined: the extent to which wages increase with accumulated tenure in a given worker–firm employment relationship. The difficulty is that the wage–tenure relationship is not self-interpreting. A tenure coefficient in a wage regression may reflect several aspects of the employment relationship and can be shaped by the way workers, firms, jobs, and wages are observed in the data. Consequently, studies that appear to estimate the same wage–tenure relationship may in practice identify different parameters, depending on their data, measurement choices, and identifying assumptions.

The empirical literature has responded to this problem through a wide range of methods and data sources. Early studies relied mainly on household panel data and developed approaches to distinguish tenure from general labour market experience. Later work used richer longitudinal and matched employer–employee data to account more directly for worker, firm, and match heterogeneity. More recent contributions have further emphasised the importance of firm-level wage dynamics, wage components, mobility, and the institutional setting in which pay is determined. These developments have greatly improved our understanding of wage growth within firms, but they have also made the literature harder to summarise and assess in terms of any single underlying parameter.

In this paper, we assess the empirical literature on returns to employer tenure.[2] We examine both the sources of dispersion in existing estimates and the conditions under which they can be interpreted as causal returns to employer tenure. We provide a comparison of estimates from forty-

[1] Throughout the literature, the terms return to tenure and returns to seniority are used interchangeably to denote the wage effect of time spent with a given employer, with differences reflecting terminology rather than substantive conceptual distinctions. One difference is in Buhai, Portela, Teulings and van Vuuren (2014), where seniority is explicitly redefined as rank-based seniority.

[2] To the best of our knowledge, the literature still lacks a comprehensive review of returns to tenure. Addison, Portugal and Raposo (2023) provide a valuable recent but partial discussion of the empirical challenges involved in estimating RTT, while Zwick (2011) and Sessions and Theodoropoulos (2013) cover earlier theoretical and empirical contributions on seniority wages.

seven studies across different countries, periods, datasets, wage measures, worker sample selection, and empirical specifications. We then offer both a non-critical and a critical review of this literature, distinguishing what the evidence appears to show from the reasons why these estimates are often difficult to compare.

We also provide new illustrative evidence using linked employer–employee data from the British Annual Survey of Hours and Earnings (ASHE) from 2005 to 2020. We compare estimated returns to tenure (RTT) across increasingly demanding specifications that reflect some of the most common approaches used in the literature, ranging from naïve OLS and the Altonji and Shakotko (1987) IV (A-S IV) strategy to worker–firm match fixed-effects models. This exercise shows how standard empirical choices can alter the estimated wage–tenure profile and, therefore, its economic interpretation.

The paper makes three main contributions. First, it provides a structured assessment of the empirical literature, by assembling estimates on the RTT from published studies and compares them across data sources, samples, wage measures, countries, and estimators. Second, it clarifies why these estimates should not be interpreted as directly comparable measures of a single parameter. Differences in empirical design often imply differences in the underlying object being estimated. Third, using British linked employer–employee data, it illustrates how sensitive tenure profiles can be to alternative specifications.

The structure of the paper is as follows. Section 2 sets out the main econometric challenges in estimating and interpreting returns to tenure. Section 3 reviews the empirical literature and provides both non-critical and critical assessments of the evidence. Section 4 presents illustrative estimates from British payroll-based data. Section 5 discusses further perspectives on the interpretation of tenure–wage profiles. Section 6 concludes.

2. The econometric challenge

Consider the following Mincer-wage regression model:

$$\ln(w_{it}) = \alpha + f(Tenure_{it}) + g(Exper_{it}) + u_{it} \qquad (1)$$

where w_{it} denotes the wage of worker i in period t. $f(Tenure_{it})$ and $g(Exper_{it})$ are general functions, typically polynomials, of the worker's cumulative tenure with their current employer and their labour market experience, often proxied by age. The remaining heterogeneity in wages is included in u_{it}, which includes any other control variables, fixed effects, and residuals. Estimated models of this form can thus provide average partial effects of additional periods of employer-specific tenure. Econometric identification relies on variation in tenure generated by job mobility and differences in spell durations across workers, which provides both within- and across-spell variation in wages and tenure, while labour market experience evolves mechanically over time. The fundamental identification and interpretation problems for estimates of Equation (1), however, arise from the likely endogeneity of tenure, due to unobserved worker and job-match heterogeneity and endogenous mobility decisions.

The length of job spells is not randomly assigned; rather, it is jointly determined with wages through worker mobility decisions, firm behaviour – not least due to employment and wage-setting responses to aggregate or employer-specific shocks – and the underlying or dynamic quality of job matches. Better matches tend to pay higher wages and last longer, generating a positive correlation between tenure and wages, even in the absence of any causal effect of firm-specific tenure on productivity. This constant job-match value heterogeneity was first emphasised by Abraham and Farber (1987) and Altonji and Shakotko (1987), who argued that much of the observed tenure-wage profile in OLS regressions reflects sorting and the greater survival rate of high-quality matches, rather than firm-specific human capital accumulation. Consistent with this interpretation, Topel and Ward (1992) showed that employer switching is an important component of wage growth for young men, explaining around one-third of early-career wage growth. They interpreted this wage growth as arising from improvements in the job-match component of wages, rather than from systematic sorting into firms that pay generally higher wages to all workers. Taken together, these arguments suggest that better job matches lead to both higher wages and longer employment relationships. Consequently, if job-match quality is not adequately controlled for, estimates of the RTT in Equation (1) are likely to be upward biased.

Subsequent studies, including Dustmann and Pereira (2008) and Dostie (2005), reinforced this conclusion by showing that once unobserved match quality is accounted for, either through fixed effects or the joint modelling of wages and mobility, the estimated RTT shrink dramatically or disappear altogether. Further, Altonji and Williams (1998) demonstrated that identification of tenure effects depends on strong assumptions about the evolution of match quality and mobility. Because changes in the job-match component of wages are endogenous to quits and layoffs, their analysis and weaker assumptions deliver bounds (upper and lower) on the RTT rather than point estimates. Their findings imply that conventional point estimates rely on strong identifying assumptions that cannot be empirically validated.

Closely related is the issue of worker heterogeneity. More able or more motivated workers both earn higher wages and are more likely to remain with a given employer, producing an upward bias in naïve tenure estimates. Fixed-effects estimators typically reduce estimated RTT effects relative to OLS, as shown by Williams (1991, 2009), Altonji and Williams (2005), and Dobbie, MacMillan and Watson (2014). However, even fixed effects may be insufficient if worker heterogeneity affects both wages and mobility, or if wage growth itself influences the decision to stay or leave. Structural models that explicitly account for endogenous mobility, such as in Buchinsky et al. (2010) and Dostie (2005), often find that, once these dynamics are incorporated, early-career wage growth is driven primarily by job-to-job mobility rather than by tenure accumulation within firms.

A further identification challenge arises from the mechanical relationship between tenure and labour market experience. Within a job spell, tenure and experience increase one-for-one, making it difficult to separately identify their effects on wages. As a result, estimates of RTT are highly sensitive to how general labour market experience is modelled and whether it is treated as exogenous. Dustmann and Pereira (2008) and Williams (2009) showed that when experience is more flexibly modelled or jointly instrument with tenure, the estimated RTT often become small or statistically insignificant. This sensitivity underscores that positive tenure estimates may be capturing returns to general, sectoral, or occupational experience rather than firm-specific human capital.

Measurement error in tenure further complicates inference. Much of the earlier literature relied on survey data from the US, such as the Panel Study of Income Dynamics (PSID) and National Longitudinal Survey (NLS), where tenure is subject to recall error, interval coding, rounding, and ambiguity over whether tenure refers to the job or the employer. Brown and Light (1992) demonstrated that inconsistencies in reported tenure can meaningfully affect estimated tenure coefficients, while Lefranc (2003) and Altonji and Williams (2005) showed that results are highly sensitive to how earnings and tenure are measured. Although administrative datasets reduce some of these problems, they introduce others, including ambiguity about cumulative tenure after re-hiring, difficulties in identifying multiple jobs within the same employer, and the distinction between job and employer spells (Brown & Light, 1992; Altonji & Williams, 2005; Abowd, Kramarz & Roux, 2006). Unless tenure histories are internally consistent, estimated returns may suffer from attenuation or more complex non-classical biases (Brown & Light, 1992).

2.1 Firm-year shocks and bias in tenure estimates

More recent work highlights an additional, previously underappreciated source of bias in RTT estimates: time-varying firm-level wage shocks. Snell, Martins, Stüber and Thomas (2018) argue that firm wages and employment can comove in response to business-cycle and firm-specific shocks.

Because these "equal-treatment"[3] shocks are correlated with tenure, since they theoretically can affect both firm hiring and layoffs, failing to control for them can substantially bias RTT estimates even in models with employer-employee (match) fixed effects (and implicitly both separate worker and firm fixed effects). Using German and Portuguese matched employer-employee data, Snell et al. (2018) demonstrate that accounting for these shocks, by adding firm-by-year fixed effects to the regression model, can increase estimated RTT by 20 to 40 percent relative to the traditional specifications, suggesting that those conventional approaches may still confound tenure effects with firm-level wage and employment dynamics.

Understanding how these firm-level shocks enter standard estimating equations is crucial for assessing why widely used identification strategies continue to deliver biased estimates of RTT. This is because estimating firm-specific RTT is complicated by two related econometric problems: endogenous match duration and time-varying firm-level shocks. While match fixed effects address classic upward bias from unobserved worker-firm match quality, they do not eliminate bias arising from firm-specific shocks that jointly affect wages and employment. Positive firm-year shocks raise incumbent wages (perhaps through non-base pay as we describe below) while simultaneously inducing hiring, which lowers average tenure in the firm. This creates a negative correlation between wages and tenure that biases RTT estimates downward in standard matched fixed-effect specifications. The commonly used A-S IV (1987) strategy does not solve this problem because it relies on tenure innovations within job spells being orthogonal to match-level wage shocks.[4] When firm-year shocks affect both wage growth and workforce composition, this exclusion restriction is violated, and the A-S IV may also have the same bias as OLS or match fixed-effect estimates.

Introducing firm-year fixed effects corrects this bias by absorbing time-varying firm-level wage shocks and forcing identification to come from both within-firm-year and within-match variation over time. However, this correction comes at a cost, since it sharply limits the remaining variation available to identify multiple dimensions of human capital. In particular, within an ongoing match, tenure and general experience increase one-for-one (as argued above), making it impossible to separately identify firm-specific tenure effects from experience effects without relying on job changes. Once heterogeneous returns to experience across occupations or industries are allowed as well, this problem intensifies. Occupation-specific learning interacts with firm shocks and endogenous sorting, rendering both linear tenure controls and A-S style instruments likely invalid.

[3] Equal-treatment shocks are firm-wide pay changes driven by firm-specific events that affect all workers similarly rather than reflecting individual productivity.

[4] Tenure innovations refer to within-match deviations of tenure from its match-specific mean, which arise from observing workers at different points in their job spells.

With firm-year fixed effects included, identifying occupation-specific returns would require substantial within-firm occupational mobility. This is an empirically rare event in many (non-administrative and short-panel datasets) data sets, thus leading to near-collinearity and weak identification.

2.2 Wage components and the transmission of firm shocks

Building on Snell et al. (2018), Schaefer, Singleton and Theodoropoulos (2025), using British matched employer-employee data, also demonstrate that conventional Mincer (1974) type regressions can lead to underestimated RTT when they omit time-varying firm-year shocks, even after controlling for worker-firm match quality. Positive firm-specific shocks simultaneously raise wages and induce hiring, mechanically lowering average tenure in expansionary firm-years. Because match fixed-effects specifications identify RTT solely from within-match wage changes, they misattribute shock-driven wage growth to low-tenure periods, flattening estimated tenure-wage profiles. Introducing firm-year fixed effects helps correct this problem and raises estimated RTT by around 20 percent among large British private-sector employers.

Crucially, this bias described by Schaefer et al. (2025) originates almost entirely in non-base earnings, particularly from incentive pay, rather than in base wages. Base pay is largely rigid and shows little responsiveness to firm-specific shocks, consistent also with recent evidence on the extent and macroeconomic significance of downward base wage rigidity (Grigsby, Hurst & Yildirmaz 2021; Schaefer & Singleton, 2023). As a result, estimated tenure profiles for base earnings are essentially unaffected by adding firm-year controls. In contrast, incentive pay adjusts strongly to firm-specific shocks and is disproportionately received by incumbent, higher-tenure workers, while firms also expand hiring into incentive-pay positions during expansionary years. When firm-year shocks are omitted, regressions can confound this shock-driven variation in flexible pay with tenure effects.

More broadly, these recent findings imply that measured RTT reflect not only within-match wage progression but also how firms transmit shocks through their internal pay structures. It is plausible, though to our knowledge untested, that these mechanisms are asymmetric: as the evidence gathered by Bewley (1998, 1999) and more recently by Bertheau, Kudlyak, Larsen, and Bennedsen (2025) demonstrates, employers see general wage cuts as a poor substitute to layoffs when faced with negative shocks.

3. The state of the empirical literature

In this section, we provide a systematic literature review of the published peer-reviewed empirical literature on returns to employer tenure. The literature is summarised in Table 1. Rather than focusing selectively on a small number of influential contributions, we review (potentially) all published studies on the topic. The coverage includes the classic USA-focused debate beginning with Altonji and Shakotko (1987), Abraham and Farber (1987), and Topel (1991), as well as subsequent evidence from twelve additional countries. By restricting attention to published papers only and reporting, for each study, the country, data source, sample definition, pay measure, empirical specification, and estimated RTT, we provide a structured and transparent overview of the field. This approach allows both comprehensive coverage and consistent presentation, enabling us to provide both a non-critical summary and a critical assessment of the state of the literature.

<<TABLE 1 about here>>

3.1 A non-critical review of the literature

The empirical literature on returns to employer tenure (RTT) has developed around a central question in labour economics: to what extent do wages rise as workers remain with the same employer? Although the estimated magnitudes differ across studies, the literature has established several broad patterns concerning the relationship between employer attachment and wage determination.

The first and most basic finding reflected throughout Table 1 (rows 1-47) is that wages and employer tenure are usually positively associated (rows 1-8, 17-18, 23-26). Early studies, often based on cross-sectional, pooled and early panel wage regressions, tended to report sizeable positive tenure effects and interpreted them as evidence that workers benefit from remaining with the same employer (rows 23, 38-39, 44). These estimates provided an important benchmark for subsequent work. However, later studies showed that the magnitude and interpretation of the estimated tenure premium depend heavily on the treatment of unobserved heterogeneity, mobility selection, and job-match quality (2-6, 30-34).

A second major development was the recognition that employer tenure is difficult to separate from other dimensions of human capital accumulation (rows 9, 13). Several studies show that estimated returns to employer tenure decline once occupational tenure, industry tenure, or broader career experience are included in the wage equation (rows 9, 13, 25-27). This suggests that some wage growth previously attributed to firm-specific tenure may instead reflect skills that are portable across firms but specific to occupations, industries, or career paths (rows 9, 13, 25-27, 47). The literature therefore moved from asking whether tenure matters to asking which type of tenure, experience, or attachment matters for wage growth (rows 9, 13, 25-27, 32, 36, 43, 47).

This shift did not imply that employer tenure became irrelevant. In many studies, positive and economically meaningful tenure effects remain even after richer match-specific controls are introduced (rows 5, 21-22, 40, 45-47). The evidence from countries such as Germany, Portugal, Brazil, Switzerland, Japan, the United Kingdom, and the United States suggests that employer attachment can still carry a residual wage premium in some settings (rows 2, 11, 17-18, 24-27, 32-34, 40, 45-47). What changed was the interpretation of this premium. Rather than being treated as a direct measure of firm-specific human capital, RTT came to be understood as one component of a broader wage-setting process involving experience, mobility, match formation, and firm-level pay practices (rows 14-16, 30-31, 37).

A third contribution of the literature is the documentation of substantial institutional and organisational heterogeneity. Estimated RTT differ across countries and labour-market regimes, partly reflecting differences in employment protection, internal labour markets, wage bargaining, promotion systems, and the role of seniority pay (rows 17-22, 28-40). Earlier evidence from Japan, for example, reported steep tenure-earnings profiles, consistent with long-term employment and seniority-based pay, whereas more recent work on the Japanese labour market finds smaller effects after accounting for unobserved worker and match heterogeneity (rows 38, 40).

The increasing availability of matched employer–employee data has also allowed researchers to move beyond average tenure effects or bounds (row 8). More recent studies examine how RTT vary by firm type, worker skill, sector, establishment characteristics, and the structure of pay (rows 7, 21-22, 32-34). Some find stronger tenure profiles in large firms, among unskilled workers, or in settings where internal labour markets and deferred compensation are more important (rows 17, 21, 41). Others emphasise monopsony power, rent-sharing, or firm-specific wage-setting practices (rows 34, 37, 46).

Taken together, our non-critical reading of the literature is that employer tenure remains an important feature of wage determination, but its role is context dependent. The literature has established that wage-tenure profiles are empirically relevant, that their estimated magnitude is sensitive to the identification strategy used, and that tenure effects differ across institutional, organisational, and worker settings. Rather than pointing to single universal RTT, existing evidence shows that employer attachment is rewarded under some conditions and through several possible channels.

3.2 A critical review of the literature

Although the literature is large and informative, it does not deliver a straightforward consensus. The main difficulty is that many estimates claiming to capture RTT are not estimates of the same empirical object. Studies differ in the measure of wage utilised, the population analysed, the structure and quality of the data, and the assumptions made for identification. As a result, the variation of estimates in Table 1 reflects not only genuine economic heterogeneity, but also substantial differences in what is being measured.

A first source of non-comparability is wage measurement. Most of the studies estimate tenure profiles using hourly wages (rows 2, 3, 5, 6), while others use weekly (rows 1, 4), monthly (rows 38, 41, 43), daily (rows 17, 18), or annual earnings (rows 28, 29). Some focus on base pay, whereas others use gross pay, total compensation, or measures that include overtime, bonuses, or incentive pay (rows 26, 28, 38). These distinctions matter because tenure may affect different components of compensation in different ways. A tenure profile estimated using annual earnings, for example, may capture changes in hours, bonuses, or overtime, whereas one based on hourly base pay may be closer to contractual wage progression. Recent evidence (Schaefer et al. 2025) shows that non-base earnings are particularly sensitive to firm-level shocks reinforces the importance of distinguishing between wage components. Consequently, some of the variation across studies may reflect differences in remuneration concepts rather than differences in a single underlying return to employer attachment parameter.

A second issue concerns the data sources used in the literature. Early studies often relied on household surveys, such as the PSID (rows 2-5, 8, 10, 13-14) or the British Household Panel Study (BHPS) (rows 24-27), which provide rich demographic and career information but may suffer from recall error, attrition, and imprecise measurement of tenure and wages. Administrative matched employer–employee datasets (rows 17-18, 21-22, 28-37) offer more accurate information on wages, employer identifiers, and employment spells, but they may lack education, training, hours, or information on informal and non-covered jobs. In addition, many samples are selective: some studies focus only on men (rows 1-6), private-sector workers (rows 1-3), displaced workers (row 35), full-time employees (rows 7, 18, 20, 22, 25), white-collar workers (row 7), large firms (rows 22, 33, 41), or even a single firm or sector (rows 23, 41, 44). These restrictions may be defensible for specific research designs, but they limit the comparability and generalisability of estimated tenure profiles.

A third and more fundamental concern is that different estimators use different identifying variation. OLS estimates primarily describe conditional wage-tenure correlations (rows 1-3, 6). Fixed-effects models remove some permanent worker, firm, or employer-employee match heterogeneity, but they do not necessarily account for time-varying firm-level shocks or endogenous mobility (row 7). A–S style IV approaches rely on within-match deviations in tenure and require strong exclusion restrictions (rows 2, 4, 6). Topel-type (row 5, 6, 10) estimators separate experience and tenure effects through additional assumptions about within-job wage growth and match quality. Structural models go further by jointly modelling wages, mobility, participation, search, or outside job offer arrival processes, but their conclusions can then depend on stronger assumptions (rows 14-16, 21, 30-31, 37). These approaches should, therefore, not be treated as generating alternative estimates of one common parameter, since they identify conceptually different objects.

This point is especially important because it has been shown that earlier solutions to the endogeneity of tenure were incomplete (rows 36-37). Match fixed effects address one form of unobserved heterogeneity, but they do not necessarily remove bias from firm-level wage and employment shocks (row 22). Completed-tenure controls are used to attempt to proxy match quality, but completed tenure is itself an outcome of the wage and mobility process (row 3). IV strategies based on within-spell tenure variation can fail when mobility, occupational choice, or firm shocks are correlated with wage growth (row 43). Structural models can address some of these issues, but only by imposing assumptions that are difficult to test directly. The absence of convergence in the literature, therefore, reflects a genuine identification problem rather than merely a lack of varying selection of data.

A further limitation is that employer tenure is often not cleanly separated from occupational, industry, sectoral, or career-specific experience. Notably, Neal's (1995) displaced worker evidence made this identification problem clear. Workers who lose a job and move to another firm in the same industry retain a large return from their previous employer tenure. In contrast, those who switch industries lose much more. The result implies that employer-tenure coefficients may partly capture industry-specific capital or industry specific match value, even when the empirical object is labelled as a return to employer tenure. Studies that include such additional tenure measures frequently find that the employer-tenure wage slope falls, suggesting that earlier estimates may have conflated firm-specific wage growth with more portable forms of skill accumulation. Yet the definition of these additional dimensions varies across studies (rows 9, 13, 25, 27, 43). Some use detailed occupations, some use industries, some define careers as occupation-industry combinations, and others omit these measures entirely because the data do not allow them to be observed. This makes it difficult to compare estimates even when papers address the same conceptual question.

Cross-country comparisons therefore require caution. Differences between, for example, high early estimates from Japan (row 38), small estimates from France (rows 28-31) or Sweden (row 43), and more mixed findings for the United States (rows 1-16), Germany (rows 17-22), Portugal (rows 32-34), or the United Kingdom (rows 23-27), may partly reflect true institutional variation. But they may also arise from differences in wage measures, sectoral coverage, sample restrictions, and empirical design. Estimates derived from sector-specific samples, a single measure of wages, and reduced-form OLS specifications, are not directly comparable to estimates obtained from nationally representative administrative data using structural or fixed-effects designs (rows 17, 21, 29).

The evolution of the literature also complicates synthesis. Earlier studies were often framed around the question of whether wages rise with tenure and whether this reflected firm-specific human capital (rows 1-8, 28, 38). More recent studies interpret tenure profiles through a wider set of mechanisms, including match quality, occupational learning, internal labour markets, incentive pay, monopsony, rent-sharing, peer learning, and firm-level shock transmission (rows 14-16, 21-22, 32-34). This broadening has enriched the field, but it has also moved the literature away from estimating a single RTT parameter.

Overall, the critical reading of the literature is that the evidence is highly informative but not easily reducible to one consensus estimate. The main weakness is not simply that studies reach different conclusions, but that many of their estimates are not directly comparable. They are produced using different wage concepts, populations, data structures, and identifying assumptions. The literature is therefore best interpreted as showing how sensitive wage-tenure profiles are to measurement, sample design, and econometric specification, rather than as converging on a single causal return to employer tenure.

To make this heterogeneity more transparent, Figure 1 reports estimated RTT after ten years for selected countries, distinguishing estimates by estimator type and publication year, from among the studies outlined in Table 1. The figure documents substantial variation. Although some estimates are large and positive even for a similar specification, many of the estimates are concentrated between 0% and 5%. There is also a small number of negative estimates.

<<FIGURE 1 about here>>

4. Specification sensitivity of RTT: evidence from Great Britain

In this section, we complement the previous synthesis by providing new empirical evidence on the sensitivity of RTT estimates using linked employer–employee panel data from Great Britain. We apply variants of the most common reduced-form estimation strategies used in the literature, holding constant a fixed sample of employer–employee job spells. This allows us to document substantial variation in estimated RTT across specifications that differ in their identifying assumptions and treatment of heterogeneity.

4.1 Data & estimation sample

We estimate regression models of the form discussed above using data from the Annual Survey of Hours and Earnings (ASHE) (Office for National Statistics, 2025), administered by the UK national statistical authority 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.

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 administrative employer identifiers. The ASHE has several advantages over the UK household-level surveys used previously to estimate RTT, such as the BHPS (see Table 1, specifically rows 24-27).[5]

First, employers are legally obliged to provide comprehensive information for their employees who are in the one percent of the population consistently sampled by ASHE. This information covers various aspects of the employment relationship, such as earnings without top-coding, basic and overtime hours, the start date of their employment, precise firm size, and industry classification. Further, the longitudinal aspects of ASHE in terms of both workers and firms allow us to control for match fixed effects in the regression models, which is essential to comparing the main estimation approaches for RTT, as described above. Finally, ASHE provides information overall on approximately 200,000 employees annually, which after some selection still leaves a large estimation sample of employer-employee matches.

We choose a broad measure of wages from ASHE, gross hourly pay, which is derived by dividing the employee's recorded gross weekly earnings by the sum of their weekly base and overtime hours. Before any other steps that select the matches in our estimation samples, to remove outliers or extreme cases of error in the ASHE pay or hours records, we first trim the employee-year observations that are in the top or bottom 0.5% of the gross earnings per hour distribution, and then we also trim the top and bottom 0.5% of the remaining observations according to the base earnings per hour distribution.

[5] These dimensions and advantages of the ASHE dataset have made it increasingly valuable for labour economists in investigating the role that firms play in shaping pay growth and inequalities in the UK (e.g., Jewell, Razzu & Singleton 2020; Jones & Kaya, 2023; Phan, Singleton, Bryson, Forth, Ritchie, Stokes & Whittard, 2025; Pham, Schaefer & Singleton 2026; Schaefer & Singleton, 2020).

The ASHE only provides information on the basic demographic characteristics of employees: age, gender, where they live, and where they work. As such, a key drawback is that it does not provide information on human capital variables, such as education and job training. Since education will in all but very rare cases have already been completed once an individual is observed in a job lasting multiple years, match fixed effects will help control for this and other time-invariant unobserved heterogeneity. Even so, in our estimation sample, we select only observations from 2005 to 2020 where employees are aged 21-59, to abstract from major education accumulation and early retirement or later-life occupational downgrading that could be occurring within job spells.

We also only select matches where every yearly observation in ASHE records the employee working for the firm full-time (30 or more hours per week), to ensure the general consistency and comparability of employment relationships. If we observe records in ASHE of intermittent spells for an employee at the same firm, where the firm reports a new start date of the relationship, we only keep the first match period. Finally, we only keep matches where the employer is recorded as a private company, according to the UK administrative Inter-Departmental Business Register.

The ASHE gives the month and year when an employee was hired and thus a match begins. Therefore, interrupted observation spells within the ASHE for a worker employed by a given firm do not necessarily imply a reset of tenure. We use the recorded employment start date to compute tenure in months as of each April when the employee appears in ASHE. If an employee has less than 12 months of tenure in April, we label this employee as a new hire. Since the ASHE provides employment start dates, we can use left-censored job spells (i.e., matches that started before 2005). Over the whole sample period of 2005-2020, our estimation sample contains 154,199 employee-firm matches, or spells, and a total of 671,087 employee-year observations (see sample descriptives by year in Appendix Table A1). As illustrated by Figure 2, the sample contains substantial variation in employee tenure. In most years, at least 25 percent of the sample have at least two years of tenure, the median ranges between 4.3 years in 2018 and 5.8 years in 2020. Between 2005 and 2012, the 90th percentile of employee tenure is greater than 20 years within the sample. However, this falls steadily then till 2018, in which year the 90th percentile of tenure is 18 years. There is a marked shift in the tenure distribution in 2020, which coincides with Covid-19 and a marked drop in the total ASHE sample size.[6]

[6] To the best of our knowledge, there are no recent official statistics on employer-employee tenure patterns in the UK. The ONS published mean values for the private sector from the ASHE of 6.4 and 6.7 years in 2004 and 2017, respectively, applying cross-sectional survey sample weights and without the other selections we make for our regression estimation sample. There is an earlier literature though using household surveys that points to a trend of declining job tenure in Great Britain (e.g., Booth, Francesconi & Garcia-Serrano, 1999; Gregg & Wadsworth, 2002), which is perhaps picked up as broadly continuing by Figure 2.

<<FIGURE 2 about here>>

4.2 Models and Estimation

We generate five different sets of estimates of Equation (1), specifically allowing in each for $f(Tenure_{it})$ and $g(Exper_{it})$ to be quartic functions, and for u_{it} to include year fixed effects. First, we generate the naïve OLS estimates of this model. We would expect these estimates to be substantially biased upward, since they do nothing to address the correlation of tenure with match quality. Second, we apply an A-S IV-style estimation strategy, specifically using as instruments the deviations from mean values within matches of each term in $f(Tenure_{it})$ and $g(Exper_{it})$.[7] Third, we estimate the model again using least squares but allow for match fixed effects (MFE) in u_{it}. Fourth, we expand on this baseline MFE specification by demonstrating the sensitivity to controlling for a basic proxy of within-match varying firm performance that is observed in ASHE – the precise number of employees in the firm. We allow this to enter the model as a quadratic. Fifth, we control for common time-varying shocks across groups of matches that could be plausibly correlated with both tenure and wages, by including fixed effects for the interaction between the year, the NUTS1 (Nomenclature of territorial units for statistics) region of the workplace, (e.g., London vs. North East vs. Scotland), and the firm's major industry group according to the 2003 Standard Industrial Classification.[8]

In practice for the MFE specifications, we compute the RTT estimates in two steps (see Schaefer et al., 2025). First, we estimate the regression model omitting the linear tenure and experience terms, because the fixed effects leave their coefficients unidentified. Importantly, we include match fixed effects instead of using first differences within matches because this accommodates a dataset where employment spells are often unbalanced – it is common for worker spells or matches observed in ASHE to have intermittent missing values, because of 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 Snell et al. (2018) by

[7] Potential experience (age) evolves mechanically with tenure within a job spell – we do not observe actual cumulative years of labour market work experience. Consequently, the separate identification of tenure and experience effects in this specification relies primarily on nonlinearities and heterogeneity in age at the start of spells. We thus follow the A-S IV logic for tenure, by constructing within job spell deviations from mean instruments for both the tenure and experience quartic terms.

[8] A standard crosswalk is available within the UK Data Service Secure Lab for the industry classification of firms between the Office for National Statistics 2003 and 2007 schemes, which latter scheme is consistently applied in ASHE till the end of our sample period. We group the relatively sparse primary industry sectors, A-C, into one cell.

estimating two auxiliary regressions. We use the first-step estimates to compute the gross residual log wages of employees. Then, we collect all the new hire observations within the original estimation sample; by definition, new hires have $Tenure_{it} = 0$. In the first auxiliary regression, we regress the values of the gross residuals for new hires on their initial starting experience in a match and a linear time trend. Assuming for identification, as per Topel (1991), that experience does not systematically correlate with match quality, the coefficient of starting experience in this regression then gives a consistent estimate of the average linear effect of experience. In the second auxiliary regression, we regress the gross residual log wages for all employees in the estimation sample on their tenure and, once again, a linear time trend. This gives an estimate for the summed effects of linear tenure and experience, which when combined with the estimate of the latter from the first auxiliary regression, allows us to recover a consistent estimate for the coefficient of linear tenure. Finally, to generate confidence intervals for the RTT profiles that are generated by all these models and estimators, we bootstrap sample the worker-firm matches within the estimation sample 200 times.

4.3 Results

Table 2 and Figure 3 show the cumulative log wage-tenure returns profiles for each of the five model estimates described above, computed for integer values of tenure of between 0 and 20 years. As anticipated based on the literature and the expected selection of better matches with higher average wages into longer spells, the OLS estimated profile is the steepest, without showing a sense of diminishing marginal returns after about 10 years. In contrast, the A-S IV style estimates are approximately half as steep for the first 5 years, giving an estimated and statistically significant average cumulative return then of around 5.4 log points, from accumulating time with an employer rather than moving to different firms. This return peaks after approximately 10 years at 6.5 log points, before diminishing to 4.4 after 20 years of tenure. The estimate for 10 years sits approximately in the middle of the range of other A-S IV style estimates summarised by Table 1 and Figure 1. Further, the magnitude of the difference between the OLS and A-S IV estimates that we obtain from the British payroll data is not dissimilar to the general pattern across the literature shown by Figure 1.

The estimates for our other three specifications, which all allow for match fixed effects with the further addition of basic proxy controls for firm-level and local labour market shocks, yield wage-tenure profiles that are not statistically different from zero until about 20 years, where they become marginally negative. Taken together, these estimates demonstrate that the two most common identification approaches in the literature, employing either A-S IV style instruments or a method that admits match fixed effects, can deliver substantively different results when applied to the same dataset.

<<TABLE 2 about here>>

<<FIGURE 3 about here>>

5. Some further perspectives on estimating RTT

In this section, we discuss why tenure–wage profiles are difficult to interpret as causal returns to firm-specific human capital. We also return to the identification challenges for estimating RTT, briefly discussing some further issues that have received relatively limited attention in the empirical literature reviewed above.

5.1 Mobility, rent sharing, and dynamic firm behaviour

Building on the recent and previously described work showing that firm-specific shocks can bias returns-to-tenure estimates, Ding (2022) emphasises that the modern fixed-effects estimators systematically misattribute tenure returns when they ignore dynamic firm pay policies driven by rent-sharing. Workers respond not only to permanent wage differentials across firms but also to transitory firm-level wage innovations that are common to all employees within a firm. These innovations generate wage co-movement that is not fully attributable to individual productivity yet correlated with both mobility decisions and foregone tenure, shifting the interpretation of tenure-wage profiles away from pure human capital accumulation.

A central insight of this work is that opposing mobility-related biases operate simultaneously. Negative shocks at origin firms induce quits, particularly among high-tenure workers, thus creating an upward bias in estimated tenure returns. Conversely, high-tenure movers require strong positive shocks at destination firms to compensate for lost tenure, generating a downward bias. Empirically, the destination-firm channel dominates, leading standard fixed-effects models to underestimate the underlying RTT. Using rich Norwegian administrative data and an extension of the AKM (Abowd, Kramarz & Margolis 1999) framework that incorporates firm-by-year effects, Ding shows that RTT are economically meaningful but strongly front-loaded, concentrated in the first years of a job, with later wage growth driven primarily by general experience. While Ding's analysis relies on strong mobility assumptions and offers a reduced-form interpretation of rent-sharing, it represents a substantive advance by demonstrating that dynamic firm behaviour and mobility are central to understanding bias in returns-to-tenure estimates.

5.2 Wage setting, contracts, and hiring conditions

A growing recent literature suggests that within-job wage dynamics are shaped not only by tenure-related productivity accumulation, but also by wage-setting frictions, long-term contract structures, and labour-market conditions at job entry. Hazell and Taska (2025) show that base wages for new hires are downward rigid, adjusting asymmetrically to labour market conditions: nominal wages rise in expansions but do not fall in contractions. This implies that workers hired in different macroeconomic states enter jobs at persistently different wage levels, such that observed wage growth over a job spell partly reflects cohort effects and hiring conditions rather only reflecting RTT. Evidence from broader payroll data further reinforces this view. Grigsby et al. (2021) and Schaefer and Singleton (2023) show that in the USA and Great Britain, respectively, base wages (i.e., the dominant component of labour costs) are highly downward rigid and adjust infrequently for job stayers, while non-base pay is more flexible but quantitatively secondary for most workers. As a result, within-job wage growth may reflect the timing of discrete wage resets, inflation regimes, or synchronised firm-level adjustments, rather than gradual productivity gains from remaining with the same employer.

A complementary perspective emerges from models that treat wages as outcomes of long-term contracts rather than spot-market pricing. Balke and Lamadon (2022) show that upward-sloping tenure–wage profiles can arise naturally from optimal contract backloading and partial insurance, even in the absence of firm-specific human capital accumulation. In this case, early-tenure wage growth reflects firms recovering hiring costs and incentivizing retention, while later wage dynamics are driven by productivity shocks and renegotiation thresholds rather than tenure itself. Elsby, Gottfries, Krolikowski and Solon (2025) develop a closely related framework in which wages remain fixed for long periods and adjust only when shocks or outside offers make renegotiation credible. In this environment, the influence of the hiring wage fades over time, further weakening a causal interpretation of tenure–wage profiles.

Finally, mobility frictions at the worker level further decouple tenure from productivity. Amior, Elsby and Gottfries (2025) show that workers differ persistently in mobility costs, leading less mobile workers to remain in declining markets and low-quality matches, generating long tenure without commensurate wage growth. More mobile workers, by contrast, experience wage growth primarily through relocation rather than tenure accumulation. Together, these recent theoretical perspectives reinforce a unifying message: tenure is an equilibrium outcome shaped by wage rigidity, contract design, bargaining frictions, and mobility constraints. Consequently, reduced-form estimates of RTT may conflate these forces with genuine productivity growth, cautioning against interpreting tenure–wage profiles as clean measures of firm-specific human capital accumulation.

5.3 Deeper conceptual and interpretation issues

Conceptual ambiguity about what tenure represents also plays a central role in the literature. Several studies demonstrate that RTT estimates often proxy for other forms of skill accumulation or career progression. Devereux, Hart, and Roberts (2013) show that returns to employer spells are substantially larger than returns to job spells, while Postel-Vinay and Sepahsalari (2023) find that employer tenure absorbs much of the effect of occupational and industry tenure when these are omitted from the regression model. Similarly, Dobbie et al. (2014) show that controlling for occupational tenure eliminates most of the apparent returns to job tenure. These findings suggest that many estimates attributed to firm-specific human capital may in fact reflect occupational learning, internal promotions, or sector-specific skill accumulation. Likewise, Addison et al. (2023) show that once full work histories are used, wage growth can be decomposed into returns to current tenure, previous experience, and discrete gains from job mobility, which is consistent with sorting into better matches. This suggests that what appears as tenure-related wage growth may partly be generated by the sequencing of jobs and improvements in match quality.

The literature also emphasises substantial heterogeneity in RTT across firms, cohorts, and workers. Abowd, Kramarz, and Roux (2006) find that estimated average RTT are close to zero for most firms, with positive returns concentrated in low-wage, high-mobility firms, and negative or zero returns in high-wage firms. Margolis (1996) shows that once firm- and cohort-specific compensation policies are allowed for, average tenure effects vanish. Imposing common RTT across firms and workers, therefore, risks masking heterogeneity and generating misleading average effects.

Another strand of the literature further complicates the interpretation of tenure by highlighting coworkers as a determinant of individual wage growth. This literature challenges the view that tenure-related wage growth reflects only a worker's own experience, firm-specific human capital, or traditional mechanisms such as deferred compensation or bargaining. However, studies in this strand differ in how far they push this interpretation. Evidence from Battisti (2017) shows that coworker quality raises wages, particularly for blue-collar and low-wage workers, but largely through contemporaneous peer effects rather than cumulative learning. In this view, part of what is measured as a return to tenure may reflect changes in coworker composition within firms. Yet tenure itself remains conceptually meaningful, since these peer effects do not accumulate strongly over time and do not appear to follow workers across jobs.

Cornelissen, Dustmann, and Schönberg (2017) provide an even more conservative benchmark. Once rich worker, firm–occupation, and firm-year fixed effects are included, they find that estimated average peer effects on wages are small. Their results imply that standard estimates of tenure returns are not fundamentally overturned by coworker effects, except in routine occupations where social pressure may affect wages contemporaneously. Taken together, Battisti (2017) and Cornelissen et al. (2017) suggest that coworker composition can influence wage growth but does not necessarily redefine what tenure represents. Rather, peer effects appear as an additional contemporaneous channel that may partly influence tenure estimates in some settings.

More recent work, however, pushes the coworker channel further. Hong and Lattanzio (2025) show that coworker quality affects not only current wages but also future wage growth, with effects persisting for several years and being strongest for young and low-tenure workers. Their evidence from wage growth around coworker entry and exit events suggests that wage trajectories are affected by dynamic exposure to high-quality peers. This perspective is more fully developed in Jarosch, Oberfield, and Rossi-Hansberg (2021), who reinterpret wage–tenure profiles through a structural model of learning in teams. In their framework, workers learn asymmetrically from more knowledgeable coworkers; this learning accumulates over time, and the resulting human capital is portable across firms. From this perspective, tenure matters not simply because workers accumulate firm-specific skills, but because tenure governs time spent in learning-rich environments. As a result, observed wages may understate the total value of tenure by ignoring any non-pecuniary learning compensation.

Even when positive RTT are identified, therefore, their interpretation remains contested. Tenure-related wage growth need not reflect productivity gains from firm-specific human capital alone, as emphasized by Becker (1964). Alternative explanations include self-selection models (Salop & Salop, 1976), job matching (Burdett, 1978), deferred compensation (Lazear, 1981), risk aversion (Harris & Holmström, 1982), agency and incentive mechanisms (Medoff & Abraham, 1980; Barth, 1997; Flabbi & Ichino, 2001; Sessions & Theodoropoulos, 2013), rent-sharing (Lamadon, Mogstad & Setzler, 2022), and wage renegotiation in response to outside offers (Bagger et al., 2014).

Adda and Dustmann (2023) stress that low estimated RTT do not imply the absence of firm-specific human capital, because frictions and limited outside competition prevent wages from fully reflecting productivity. The broader implication is that the tenure coefficient is a composite object: it may capture firm-specific learning, occupational and sectoral skill accumulation, match quality, compensation policies, bargaining, peer effects, and exposure to learning-rich coworker environments. Consequently, interpreting estimated tenure returns requires careful attention not only to econometric identification but also to the economic mechanisms by which wages evolve over the employment spell.

A complementary firm-side perspective comes from studies that examine productivity directly rather than inferring it from wages. For example, Caplin, Lee, Leth-Petersen, Saeverud and Shapiro (2023) use a survey of Danish firms to elicit managers' assessments of how productivity and wages evolve with tenure and previous relevant experience. They find that reported productivity growth with tenure exceeds wage growth, and that previous experience is an imperfect substitute for tenure, with substantial heterogeneity across jobs. Similarly, Gagliardi, Grinza and Rycx (2023), using Belgian matched employer–employee panel data and augmented production functions, find positive but diminishing productivity returns to workforce tenure. They also show that these returns are stronger in routine, less complex, industrial and capital-intensive environments. Taken together, these studies suggest that wage-based estimates may not fully capture the productivity value of employment relationships, and that this value depends strongly on the nature of production and work organisation.

6. Conclusion

This paper has assessed the empirical evidence on returns to employer tenure in three related ways. First, we examined why the literature has produced such divergent estimates of the wage gains associated with remaining with the same employer. Second, we clarified the conditions and different identifying assumptions under which these estimates are interpreted as causal returns to increased employer-specific tenure, rather than as the outcome of correlated mechanisms. Third, using British linked employer–employee payroll data, we illustrated how estimated wage–tenure profiles can vary with the specification of the wage equation and the most standard identification strategies. The central conclusion is that variation in estimated RTT, both across and within countries, reflects not only sampling uncertainty or genuine differences across labour markets, but also differences in how wages, workers, firms, and employment relationships are measured and modelled.

The accumulated evidence shows that tenure–wage profiles are informative, but not self-interpreting. A positive association between tenure and wages may arise for several reasons, including: workers acquiring firm-specific skills; employers rewarding attachment through internal pay structures; good matches surviving longer; firms transmitting aggregate or local shocks through pay in ways that are correlated with workforce composition. Furthermore, small or zero estimated tenure effects do not necessarily imply that employment relationships do not accumulate productivity gains. They may instead indicate that the wage return to firm-specific knowledge is limited by bargaining, mobility frictions, monopsony power, or how wages are measured.

Our review also shows that the literature has progressively moved away from treating RTT as a single stable parameter. Early studies focused on the distinction between tenure and general experience, and on the role of worker and match heterogeneity. Later work has added further layers, including occupational and industry-specific human capital, firm pay policies, rent-sharing, firm-year shocks, incentive pay, wage rigidity, and coworker learning. This evolution has improved the interpretation of tenure estimates, but it has also made clear why a simple consensus on the economic importance of firm-specific tenure for worker compensation is unlikely to emerge.

The implication is that future research should be less concerned with whether there is one average return to tenure and more concerned with the conditions under which employer attachment is rewarded. This requires closer attention to the wage component being studied, the source of identifying variation, and the institutional setting in which wages are determined. It also requires separating, where possible, wage growth generated by firm-specific learning from wage growth generated by sorting, bargaining, job mobility, pay-setting rules, and firm-level shocks.

This interpretation also matters for policy, especially where there are concerns about declining job stability and the changing institutional foundations of wage progression. Long-run institutional change may have altered the economic content of employer tenure itself. The decline of trade unions and collective bargaining coverage in many countries is likely to have weakened seniority-based pay rules, internal wage ladders, and negotiated protections for workers. At the same time, changes in the employment structure, including the growth of atypical, temporary, part-time, and more fragmented employment relationships, may have reduced the scope for stable firm-specific wage progression. These developments imply that estimated RTT may vary not only across countries and specifications but also across periods, as the institutional foundations that once linked tenure to wage growth have become less widespread. If tenure reflects valuable relationship-specific capital, job loss may destroy accumulated productive value, strengthening the case for policies that reduce inefficient separations or support displaced workers with training. If, instead, estimated tenure effects mainly reflect sorting, bargaining, firm pay policies, occupational learning, or other features of wage setting, then policy should focus less on preserving tenure per se and more on improving match quality, bargaining conditions, worker mobility, and transferable skills. Low wage RTT may also signal that firms capture much of the value generated by long employment relationships, especially in settings with mobility frictions or monopsony power. Thus, the policy relevance of RTT does not depend only on whether the estimated coefficient is large. The central issue is what the wage–tenure profile reveals about human capital accumulation, wage setting, mobility, institutional change, and the division of surplus between workers and firms.

Returns to tenure therefore remain a useful lens through which to study wage formation inside firms. Their value lies not only in potentially reflecting measures of firm-specific human capital, but in revealing how employment relationships, mobility, and firm wage policies interact over the worker's career. A more convincing empirical literature will be one that treats tenure not as a narrow causal variable, but as an entry point into understanding how wages evolve within and across firms.

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TABLE 1: Returns to Employer Tenure (RTT), Papers published as peer-reviewed articles or chapters

Paper, *Journal*	Country	Data, Years	Sample and pay measure	Specification	RTT
1. Hashimoto & Raisian, 1985, *AER*	USA	CPS, 1979 (single cross section)	Men in private sector non-agricultural industries, age group 16-70 plus, Log of usual weekly earnings	OLS	After 5 years of tenure: Small firms: 11.7% Medium firms: <0% Large firms: 7.1%
2. Altonji & Shakotko, 1987, *ReStud*	USA	PSID, 1968-1981	White male household heads, age group 18-60, no self-employed, no public-sector, Log annual real hourly wage	OLS and various IVs A-S IV (IV1) = deviations from the job-match mean IV2 = IV1 plus additional instruments from the experience-related regressors IV3 = IV2 plus other time-varying regressors in levels	Ten years of tenure: OLS = 30% IV1 (A-S IV) = 2.7% IV2 = 3.1% IV3 = 7.2% Preferred corrected IV1 (A-S IV) estimate = 6.6% (after adjusting for bias from job shopping, i.e., correlation between experience and the job-match component)
3. Abraham & Farber, 1987, *AER*	USA	PSID, 1968-1981	Male household heads age group 18–60, no self-employed, no public-sector, Log real hourly earnings	OLS, IV completed-job-duration-based IV estimator, related to Altonji–Shakotko (A-S IV)	White-collar (managerial/professional, non-union) OLS = 1.06% per year A-S IV = 0.59% per year OLS + completed job duration controls = 0.55% per year Blue-collar (non-union) OLS = 1.42% per year A-S IV = 0.29% per year OLS + completed job duration controls = 0.24% per year
4. Ruhm, 1990, *ReSTAT*	USA	PSID, 1969-1980	Male household heads, first job after a permanent layoff, Log real weekly wages	A-S IV	2.5% after 5 years
5. Topel, 1991, *JPE*	USA	PSID, 1968-1983	Men, age group 18-60, no self-employed, no agricultural and no public sector workers, Log hourly earnings	Topel 2SFD	5 years = 19.6% 10 years = 27.9% 15 years = 32.7% 20 years = 40.1%

Paper, *Journal*	Country	Data, Years	Sample and pay measure	Specification	RTT
6. Williams, 1991, *ReSTAT*	USA	Seattle (1970-1973) and Denver (1971-1974) Maintenance Study	Men, age group 18-60, Log real hourly wage	OLS, A-S IV, Topel 2SFD	OLS = 14% after 2 years A-S IV = 5.2% after 2 years Topel 2SFD = 4.1% after 2 years
7. Bronars & Famulari, 1997, *JOLE*	USA	BLS White Collar Pay Survey (WCP), 1989-1990	Full-time private sector, white collar workers, age group 18-64, Log worker's current real monthly wage	Pooled fixed effects wage regression across workers, within establishments	Wage-growth fixed-effect estimates after 5 years of tenure: Men=23.4%, Women=27.3 Wage-growth fixed-effect estimates after 10 years of tenure: Men=40.1%, Women=46.7%
8. Altonji & Williams, 1998, *RLE*	USA	PSID, 1975-1987	White male heads of household, age group 18-60, Log real wage measured either as average hourly earnings (annual earnings ÷ annual hours) or the Log of current hourly wage at the survey date.	First-differenced wage-growth model with partial identification bounds based on quits and layoffs	3% to 15% for 10 years of tenure depending on the wage measure and the identifying assumptions/bounds
9. Parent, 2000, *JOLE*	USA	NLSY, 1979-1996 PSID, 1981-1992	White men in the private sector, Log real hourly wage	A-S IV, adding industry experience and instrumenting it with its own deviation from industry match-mean	Controlling for three-digit industry. NLSY: between about -2.3% and 0.7% after 5 years, and between about -8.0% and -4.5% after 10 years, depending on spell definition (continuous vs non-continuous) PSID: between about 0.2% and 2.4% after 5 years, and between about -0.9% and 4.0% after 10 years, depending on spell definition (continuous vs non-continuous)
10. Lefranc, 2003, *IJM*	USA	PSID, 1978-1992	White male heads of household, age group 18-60, Log real hourly wage	Topel 2SFD	2 years = 4.4% 5 years = 8.5% 10 years = 11.5% 15 years = 12.5%

Paper, *Journal*	Country	Data, Years	Sample and pay measure	Specification	RTT
11. Altonji & Williams, 2005, *ILLR*	USA	PSID, 1975-2001	White male heads of household, private sector, not self-employed, Log real hourly wage	A-S IV and Topel 2SFD	9% to 11% after 10 years of tenure
12. Barlevy, 2008, *ReStud*	USA	NLSY, 1979-1993	Young male workers (<36), Log real hourly wage	Topel 2SFD	1 year = 0.5% 2 years = 1.1% 5 years = 2.7% 7 years = 3.9% 10 years = 5.6%
13. Kambourov & Manovskii, 2009, *IER*	USA	PSID, 1981-1992	White male household heads, age group 18-64, Log real hourly wage	A-S IV, Parent (2000) style IV Panel log-wage equation with employer, occupation, and industry tenure plus overall experience	2 years of tenure = 0.08% 5 years of tenure = 0.19% 8 years of tenure = 0.44% with 3-digit occupation and industry controls
14. Buchinsky et al., 2010, *ReStud*	USA	PSID, 1975-1992	Heads of households, age group 18-65, Log real hourly wage	Structural dynamic 3-equation model (wages, participation and mobility equations)	12.7% to 13.6% after 2 years of tenure 28.3% to 29.7% after 5 years of tenure 47.5% to 50.7% after 10 years of tenure
15. Pavan, 2011, *JOLE*	USA	NLSY79, 1957-1964 birth cohorts	Men, Log real hourly wage	Structural dynamic search model: a life-cycle model in which wage growth comes from three sources at once: general human capital accumulation, search over careers, and search over employers within a career	1.9% after 5 years of tenure 5.1% after 10 years of tenure
16. Buhai & Teulings, 2014, *JBES*	USA	PSID, 1975-1992	White males, 10 or more years of education, <60 years old, Log real hourly wage	Structural stochastic productivity model	0.56% per year, 2.8% over 5 years, 5.6% over 10 years

Paper, *Journal*	Country	Data, Years	Sample and pay measure	Specification	RTT
17. Dustmann & Meghir, 2005, *ReStud*	Germany	IAB, 1975-1995	1% sample, male workers, start a new job from job displacement due to business closure, Log real daily wage	Control-function wage regression (wage equation augmented with residuals from reduced-form equations for the endogenous variables)	Skilled workers: 2.4% in first 5 years, then 1.7% per year Unskilled workers: 4.0% per year in the first 5 years, then 1.1% per year
18. Zwick, 2011, *Labour Econ*	Germany	LIAB - LEED data, 1997-2004	Full-time private sector employees, age group 18-60, exclude apprentices and workplaces with <5 employees, Log real daily wage	OLS, Topel 2SFD, A-S IV	OLS = 23% after 5 years, 40% after 10 years, 56% after 15 years A-S IV = 5.4% after 5 years, 5.6% after 10 years, 6.2% after 15 years Topel 2SFD (after subtracting experience) = 6% per year
19. Dustmann & Pereira, 2008, *ILRR*	Germany	GSOEP, 1984-1999	Non-self-employed white men in the private sector, age group 18-60. Log of gross real hourly wage, Log (monthly) gross real hourly wage	OLS, A-S IV IVten1 = tenure instrumented with deviations from job means IVten2 = tenure instrumented with deviations from individual means IVtenexp = tenure instrumented from job mean deviations and experience instrumented from individual mean deviations	OLS: 12.76% after 10 years of tenure IVten1 (A-S IV) = -0.37% at 10 years IVten2 = 4.83% at 10 years IVtenexp = 0.05% at 10 years
20. Amann & Klein, 2012, *J.R.Stat.Soc. Series(A)*	Germany	GSOEP, 1984-2008	Full-time private sector male workers, Log real gross hourly wage	Non-separable control function wage equation	3.16% per year in the first 5 years and 0.28% thereafter
21. Adda & Dustmann, 2023, *JPE*	Germany	IAB, 1975-2004	Men, 2% sample of administrative social security records, Log average daily pretax wage	Dynamic structural model, simulated method of moments	trained workers = 0.07% per year in the first 5 years, then 0.15% per year untrained workers = 0.03% per year in the first 5 years, then 0.09% per year

Paper, *Journal*	Country	Data, Years	Sample and pay measure	Specification	RTT
22. Snell et al., 2018, *JOLE*	Germany	BeH, 1986-2009 (social security records)	Panel of full-time workers, full-year workers spells from the largest 100 firms, Log real daily wage	OLS with worker-firm matched fixed effects and firm-year fixed effects	FYFE = about 12.0% after 10 years
23. Brunello & Ariga, 1997, *Labour Econ*	UK	New Earnings Survey: 1975, 1976, 1979	Male workers in manufacturing, banking and finance, age group 21-59, Log hourly earnings, excluding incentive pay	OLS reduced form controlling for the position (rank) filled by the employe	Manufacturing = 29.7% at 5 years, 51.8% at 10 years, 62.1% at 15 years, 57.3% at 20 years Banking = 33.6% at 5 years, 68.2% at 10 years, 99.4% at 15 years, 122.5% at 20 years
24. Dustmann & Pereira, 2008, *ILRR*	UK	BHPS, 1991-1999	Non-self-employed white men in the private sector, age group 18-60, Log real hourly wage (real gross monthly pay divided by 4.33 times weekly hours worked)	OLS, A-S IV IVten1 = tenure instrumented with deviations from job means IVten2 = tenure instrumented with deviations from individual means IVtenexp = tenure instrumented from job mean deviations and experience instrumented from individual mean deviations	OLS = 8.8 % at 10 years of tenure IVten1 (A-S IV) = 5.4 % at 10 years IVten2 = 8.6 % at 10 years IVtenexp = 4.5 % at 10 years
25. Zangelidis, 2008, *SJPE*	UK	BHPS, 1991-2001	Male full-time workers (excluding self-employed, military, agriculture), age group 18-60, Log hourly wage adjusted for paid overtime	GLS based on Kambourov and Manovskii (2009)	At 10 years of tenure: Baseline RTT: 6.4% in GLSI (individual is the unit of observation), 3.5% in GLSII (employer-employee match) After adding occupational experience: about 5.4% to 4.6% in GLSI, about 2.9% to 2.4% in GLSII After adding both industry and occupational experience: about 5.1% to 5.3% in GLSI, about 2.8% to 3.0% in GLSII

Paper, *Journal*	Country	Data, Years	Sample and pay measure	Specification	RTT
26. Williams, 2009, *Labour Econ*	UK	BHPS, 1991-2001	Males, age group 18-60 in permanent positions in the private sector. Log real hourly wage (gross usual monthly pay deflated by CPI, usual weekly hours, paid overtime).	OLS, A-S IV, 2SFD	OLS = 14.3 percent at 10 years ASIV = 6.0 percent at 10 years 2SFD = 10.6 percent at 10 years 2SFD-IV = 7.9 percent at 10 years Overall RTT increases by 1.0 percent per year (first 10 years), becomes near zero with industry/occupational experience
27. Postel-Vinay & Sepahsalari, 2023, *EJ*	UK	BHPS/US, 1992-2017	Males and females, age group 16-64. Log hourly wage	OLS, A-S IV, based on Kambourov and Manovskii (2009) specification	OLS = 2.4 percent (5 years) IV = -2.8 percent at 5 years
28. Margolis, 1996, *Ann. d'Econ et de Stat.*	France	DAS, 1976-1987	Private-sector-type employment records in firms with 10+ employees, age group 15 to 65, Log real annual compensation	OLS, Topel 2SFD, Cohort-by-firm heterogeneity approach, with both entry wages and returns to employer tenure allowed to differ by firm and entry cohort	OLS 6.9% per year, Topel 2SFD 4.5% per year, individual fixed effects 3.2% per year, cohort-firm approach provides an almost zero return to tenure
29. Abowd et al., 1999, *ECMA*	France	DAS, 1976-1987	Private sector workers, Log real annual compensation	AKM with worker and firm fixed effects, extended to include firm-specific seniority slopes	About 1% per year on average for both men and women, but heterogeneous across firms
30. Dostie, 2005, *JBES*	France	DADS, 1976-1996	Private sector workers, Log real annual compensation	Simultaneous random-effects wage equation, plus proportional hazard job duration model	0-2 years = -5.3% 2-5 years = 0.5% 5-10 years = 0.0% 10+years = -0.5% Accounting for endogeneity, returns to tenure are negative in the first 2 years and close to zero overall
31. Abowd et al., 2006, *EJ*	France	DADS, 1976-1996	1/25 sample of French workforce, no civil servants, Log real annual compensation	Structural model of wage and mobility outcomes	0-2 years: 3.91% 2-5 years: 0.14% 5-10 years: 0.56% >10 years: 1.89% Average structural returns to seniority are essentially zero, but there is substantial heterogeneity across firms

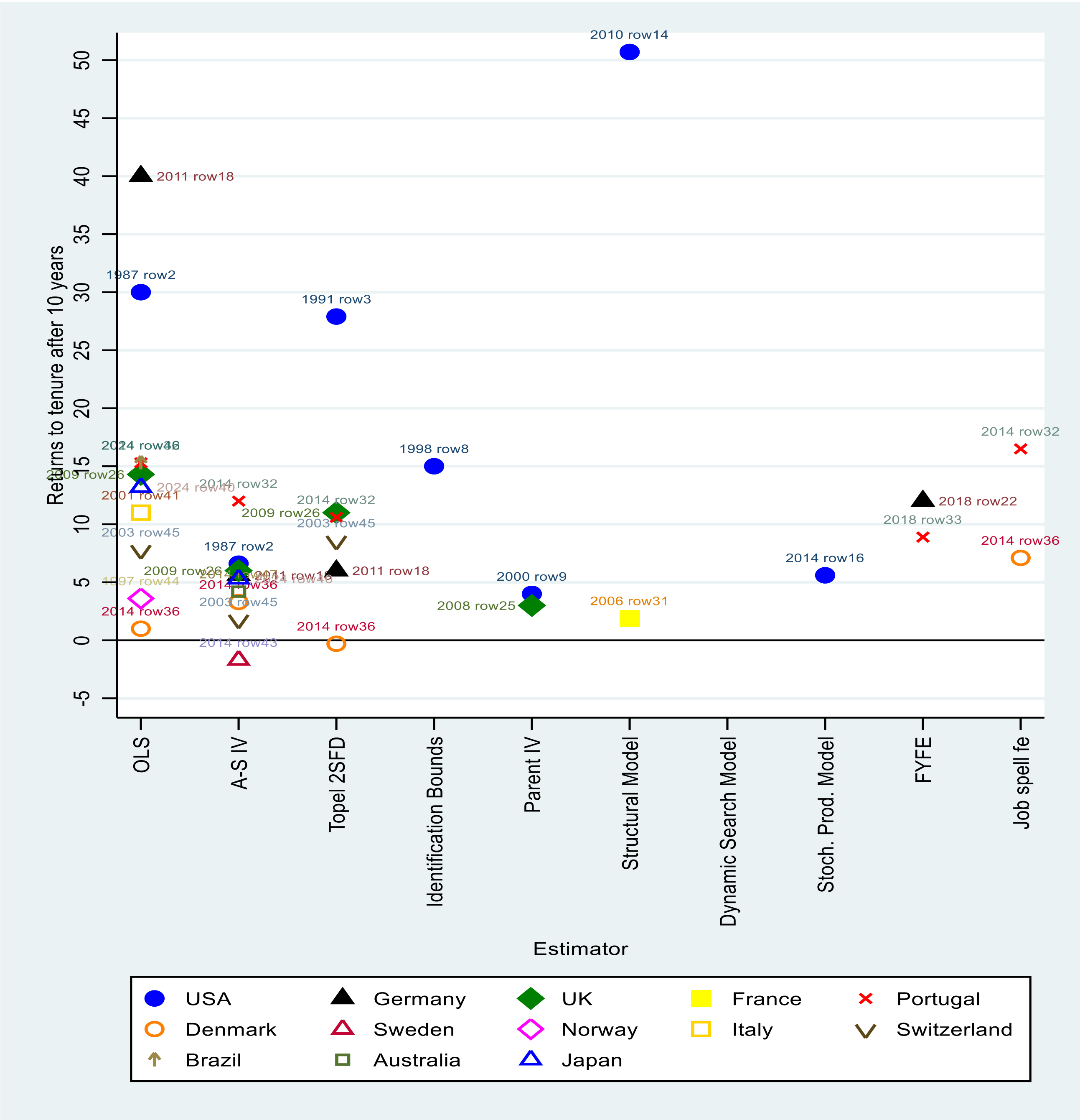
Paper, *Journal*	Country	Data, Years	Sample and pay measure	Specification	RTT
32. Buhai et al., 2014, *ECMA*	Portugal	Quadros de Pessoal (QP), 1986-2009	Private sector, men, excludes agriculture, fishing, mining, Log real hourly wage	Topel 2SFD, A-S IV, Topel with job-spell fixed-effects	Without controlling for seniority (after 10 years): OLS 17.7%, Topel 2SFD 11.4%, A-S IV 13.0%, job-spell fe 15.8% With controlling for seniority (after 10 years): OLS 15.3%, Topel 2SFD 10.6%, A-S IV 12.0%, job-spell fe 16.5%
33. Snell et al., 2018, *JOLE*	Portugal	Quadros de Pessoal (QP), 1986-2009	All workers in 127 largest private sector firms, Log real hourly wage	OLS with worker-firm matched fixed effects and firm-year fixed effects (FYFE)	About 8.9% after 10 years
34. Snell et al., 2025, *Econ Letters*	Portugal	Quadros de Pessoal (QP), 1985-2020	Universe of male workers, Log real total hourly wage	Topel 2SFD with firm-year fixed effects (FYFE)	About 16%-17% at 10 years of employer tenure, the estimated return is only slightly higher in high-monopsony firms, but the downward bias is substantially larger there
35. Bingley & Westergaard-Nielsen, 2003, *IJM*	Denmark	IDA, 1980-1998	Sample of private sector displaced workers from true firm closures who are re-employed within one year, Log wage	Cross-sectional log wage regressions, one before displacement and one after displacement with pre-displacement tenure as the key regressor	About 2% to 3% per year Net firm-specific RTT: about 0.2% in 1980 rising to about 0.7% in 1998 Net firm-specific RTT=subtracting the post-displacement tenure coefficient from the pre-displacement tenure coefficient
36. Buhai et al., 2014, *ECMA*	Denmark	IDA, 1980-2001	All Danish individuals and all companies with employees, age group 15-74, Log real hourly gross earnings	Topel 2SFD, A-S IV, Topel with job-spell fixed-effects	Without controlling for seniority (after 10 years): OLS 6.0%, Topel 2SFD 0.6%, A-S IV 5.3%, job-spell fe 9.1%. With controlling for seniority (after 10 years): OLS 1.0%, Topel 2SFD -0.3%, A-S IV 3.3%, job-spell fe 7.1%.
37. Bagger et al., 2014, *AER*	Denmark	IDA, 1985-2003	Private sector, not self-employed men, Log average hourly wage	Auxiliary Mincer wage regression motivated by a structural equilibrium search model	5% after 5 years of tenure

Paper, *Journal*	Country	Data, Years	Sample and pay measure	Specification	RTT
38. Hashimoto & Raisian, 1985, *AER*	Japan	Basic Survey of Wage Structure, 1980 (cross section)	Men in private sector non-agricultural industries, age group 16-64, Log of monthly earnings including bonuses	OLS	After 5 years of tenure: Small firms: 36.8% Medium firms: <17.5% Large firms: 40.5%
39. Brunello & Ariga, 1997, *Labour Econ*	Japan	Japanese survey on the wage structure, 1980-1984 and 1987	Male workers, Log hourly earnings	OLS reduced form controlling for the position (rank) filled by the employe	Manufacturing: 66.5% at 5 years, 143.9% at 10 years, 213.7% at 15 years, 254.8% at 20 years Banking: 51.1% at 5 years, 111.7% at 10 years, 175.0% at 15 years, 231.1% at 20 years
40. Nakamura, 2024, *Japan. Econ Review*	Japan	JHPS/KHPS, 2004-2014	Male household heads, age group 20-64, excluding government workers and the self-employed, Log real hourly wage	OLS, A-S IV	After 10 years of tenure: OLS = 13.2%, A-S IV = 5.3%
41. Flabbi & Ichino, 2001, *Labour Econ*	Italy	Personnel files from a large Italian bank, employees on payroll from 1974-1995	Men, non-managerial employees, Log pre-tax monthly earnings	OLS cross sectional earnings regression augmented with grade and performance controls (productivity proxies)	2 years: 2.4% 5 years: 5.8% 10 years: 11% 15 years: 15.8%
42. Battisti, 2012 (*book chapter*)	Italy	WHIP, 1985-1999	Young labour-market entrants, 25 years of age in 1986 and 38 years old in 1999	Simultaneous equation model: wage equation, and a hazard model for employment duration	Years of tenure 0 to 2 = 2.13% per year 2 to 4 = -1.67% per year 4 to 8 = 0.36% per year 8 plus = -0.09% per year
43. Kwon & Meyersson Milgrom, 2014, *Labour Econ*	Sweden	Panel of matched employer–employee data, 1970-1990	Private sector, white-collar workers only, Log real monthly wage	A-S IV timing-based instrument built from within-spell deviations in tenure	2 years: -0.50% 5 years: -1.10% 10 years: -1.70% 15 years: -1.80% (controlling for occupational tenure)
44. Barth, 1997, *JOLE*	Norway	Norwegian Survey of Organizations and Employees (NSOE), 1989	Private-sector employees, age group 18-60, Log hourly wage	Pooled OLS log wage model Random-effects log wage model Firm fixed-effects log wage model	10 years of tenure: Pooled OLS 3.6% Random effects 3.30% Firm fixed effects 3.37%

Paper, *Journal*	Country	Data, Years	Sample and pay measure	Specification	RTT
45. Luchsinger et al., 2003, *Swiss J. Econ Stat*	Switzerland	Labor Force Survey, 1991-1998	Men, age group 18-65, Log real hourly wage	OLS, A-S IV, Topel 2SFD	After 10 years: OLS=7.8% A-S IV=1.7% to 1.8% Topel 2SFD=8% to 8.6%
46. Pires et al., 2024, *OBES*	Brazil	RAIS, 1999-2014	Private sector firms >100 employees, working-age men, Log real hourly wage	OLS quadratic non-linear two-way fixed-effects wage equation	5.5% after 2 years 11.4% after 5 years 15.3% after 10 years
47. Dobbie et al., 2014, *Applied Economics*	Australia	HILDA, 2001-2009	Male workers, age group 15-64, Log real usual hourly earnings	A-S IV	1.5% after 10 years but disappears when occupational tenure is included. In firms with >500 employees RTT is 4.2% even controlling for occupational tenure

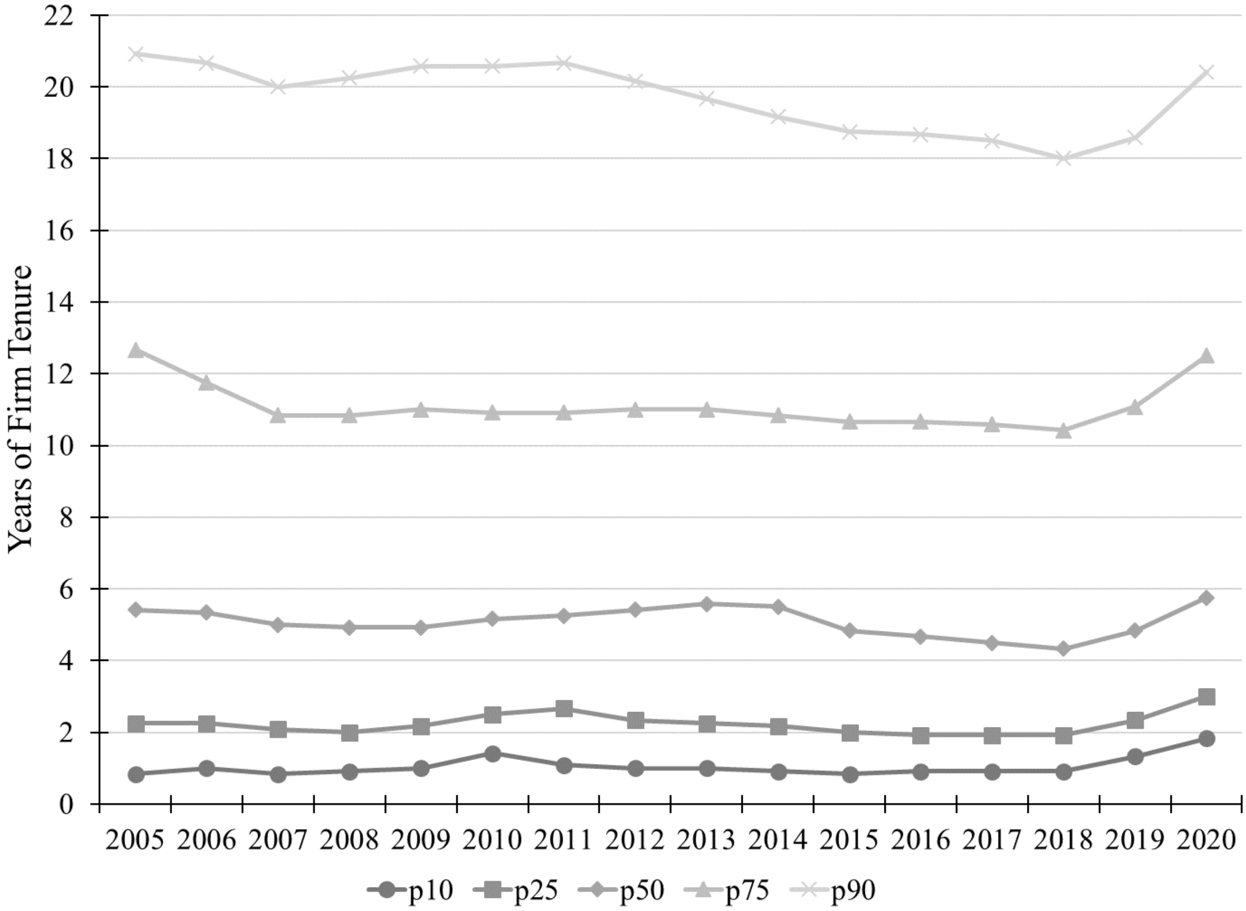
Notes. Dataset abbreviation: CPS (Current Population Survey), PSID (Panel Study of Income Dynamics), BLS (Bureau of Labor Statistics), NLSY (National Longitudinal Survey of Youth), IAB (Institute for Employment Research, German Social Security Records), LIAB (Linked Employer–Employee Data of the IAB), GSOEP (German Socioeconomic Panel), BeH (Beschäftigten-Historik, the German employment history data from the IAB/social-security records), BHPS/US (British Household Panel Study/Understanding Society), DAS (Déclarations Annuelles des Salaires), DADS (Déclarations Annuelles des Données Sociales), IDA (Integrated Database for Labour Market Research), WHIP (Work Histories Italian Panel), RAIS (Relação Anual de Informações Sociais), HILDA (Household, Income and Labour Dynamics in Australia), JHPS= Japan Household Panel Survey, KHPS (Keio Household Panel Survey). Specifications abbreviations: A-S IV (Altonji & Shakotko Instrumental Variable), Topel 2SFD (Topel's Two-Stage First-Difference), GLS (Generalized Least Squares).

FIGURE 1: Estimates of 10-year returns to tenure by estimator, country, and publication year



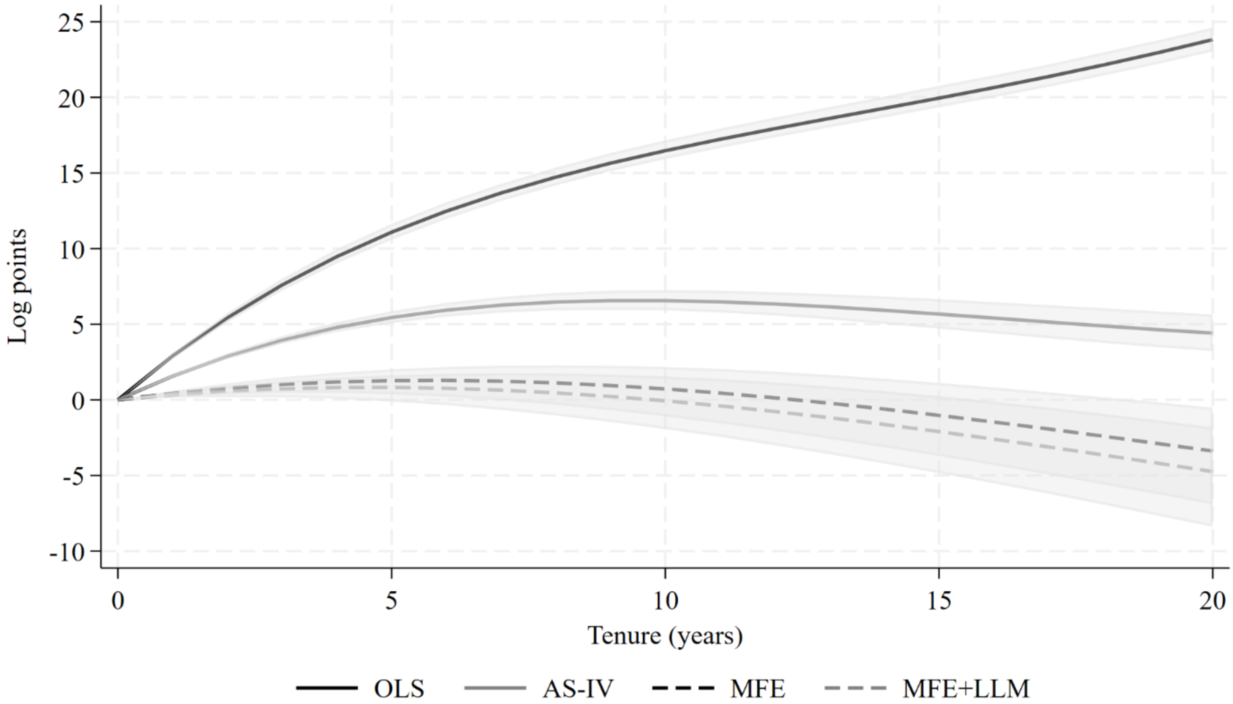
Notes: See Table 1 for details of all the studies summarised here. Some studies from the table are not reflected here where it was not straightforward to infer estimates for 10 years of tenure from the results shown in the published version.

FIGURE 2: Estimation sample percentiles of the employee tenure distribution by year



Notes: Author calculations using ASHE 2005-2020. See the text for details of the sample selection. See Appendix Table A1 (below) for these statistics, with sample sizes, means and standard deviations of employee tenure.

FIGURE 3: Estimated RTT profiles for gross hourly earnings, comparing estimators and model specifications



Notes: Author calculations using ASHE 2005-2020. See the text for details of the sample selection, models and estimation. 'OLS' refers to Ordinary Least Squares, with quartics in employee tenure and age (experience), as well as year fixed effects. 'AS-IV' shows the Altonji and Shakotko (1987) style estimates, with all the tenure and age terms instrumented by their average observed values within employee-firm matches. 'MFE' adds employee-year match fixed effects to the OLS specification, following the multi-stage regression approach to recover an estimate for the linear tenure coefficient that is described in the text. 'MFE+LLM' in addition includes as controls linear and squared employer size (N. of employees) and Industry-Region-Year fixed effects. Each set of estimates uses 671,087 employee-year observations. See also Table 2 for estimates at selected years of tenure. See Appendix Table A2 for the estimates for all years of tenure in [0,20]. The displayed 90% confidence intervals, shown by the shaded areas around each profile, are generated from (cluster) bootstrapping the matches in the sample with 200 repetitions.

TABLE 2: Estimated RTT at selected years of tenure, comparing estimators and model specifications

Years of Tenure	OLS	AS-IV	MFE	MFE+	MFE+LLM
2	5.42	2.88	0.77	0.72	0.57
	(5.14, 5.73)	(2.71, 3.07)	(0.39, 1.09)	(0.35, 1.05)	(0.19, 0.92)
5	11.09	5.45	1.27	1.16	0.83
	(10.60, 11.63)	(5.08, 5.86)	(0.37, 2.01)	(0.27, 1.91)	(-0.10, 1.63)
10	16.48	6.56	0.72	0.50	-0.06
	(15.93, 17.15)	(5.93, 7.25)	(-1.09, 2.17)	(-1.29, 1.96)	(-1.93, 1.55)
20	23.82	4.42	-3.38	-3.78	-4.74
	(23.04, 24.61)	(3.24, 5.64)	(-6.90, -0.52)	(-7.40, -0.86)	(-8.39, 1.82)

Notes: Author calculations using ASHE 2005-2020. See the text for details of the sample selection, models and estimation. See also Figure 3 for the plotted profiles and the attached notes for each estimation and model specification. 'MFE+' includes as controls linear and squared employer size (N. of employees) in addition to the 'MFE' specification. Each set of estimates uses 671,087 employee-year observations. See Appendix Table 2 for the estimates for all years of tenure in [0,20]. The values in parentheses give the 90% confidence intervals, generated from (cluster) bootstrapping the matches in the sample with 200 repetitions.

Appendix A. Additional Tables

TABLE A1: Estimation sample descriptives for employee tenure by year

Year	Mean	St. dev.	p10	p25	p50	p75	p90	N. of employee jobs
2005	8.53	8.41	0.8	2.3	5.4	12.7	20.9	36,143
2006	8.41	8.38	1.0	2.3	5.3	11.8	20.7	43,747
2007	8.01	8.18	0.8	2.1	5.0	10.8	20.0	37,531
2008	7.95	8.20	0.9	2.0	4.9	10.8	20.3	36,442
2009	8.06	8.21	1.0	2.2	4.9	11.0	20.6	43,212
2010	8.15	8.04	1.4	2.5	5.2	10.9	20.6	43,329
2011	8.17	8.07	1.1	2.7	5.3	10.9	20.7	45,427
2012	8.14	8.08	1.0	2.3	5.4	11.0	20.2	44,285
2013	8.13	8.07	1.0	2.3	5.6	11.0	19.7	45,601
2014	8.00	8.06	0.9	2.2	5.5	10.8	19.2	46,394
2015	7.76	7.96	0.8	2.0	4.8	10.7	18.8	46,052
2016	7.67	7.94	0.9	1.9	4.7	10.7	18.7	45,252
2017	7.52	7.85	0.9	1.9	4.5	10.6	18.5	45,981
2018	7.34	7.69	0.9	1.9	4.3	10.4	18.0	46,180
2019	7.77	7.70	1.3	2.3	4.8	11.1	18.6	42,189
2020	8.85	8.03	1.8	3.0	5.8	12.5	20.4	23,322

Notes: Author calculations using ASHE 2005-2020. See the text for details of the sample selection. See Figure 2 for the time series of the percentiles.

TABLE A2: Estimated RTT profiles for gross hourly earnings, comparing estimators and model specifications

Years of tenure	OLS	AS-IV	MFE	MFE+	MFE+LLM
1	2.91	1.57	0.43	0.41	0.33
2	5.42	2.88	0.77	0.72	0.57
3	7.60	3.94	1.02	0.95	0.73
4	9.47	4.79	1.18	1.09	0.82
5	11.09	5.45	1.27	1.16	0.83
6	12.48	5.93	1.29	1.15	0.77
7	13.68	6.27	1.24	1.08	0.64
8	14.73	6.47	1.12	0.94	0.46
9	15.65	6.57	0.95	0.75	0.23
10	16.48	6.56	0.72	0.50	-0.06
11	17.23	6.48	0.45	0.21	-0.39
12	17.94	6.34	0.14	-0.13	-0.77
13	18.61	6.15	-0.22	-0.50	-1.18
14	19.28	5.92	-0.61	-0.91	-1.62
15	19.95	5.67	-1.02	-1.34	-2.10
16	20.65	5.41	-1.47	-1.80	-2.59
17	21.38	5.15	-1.93	-2.28	-3.11
18	22.14	4.89	-2.40	-2.77	-3.64
19	22.96	4.64	-2.89	-3.27	-4.19
20	23.82	4.42	-3.38	-3.78	-4.74

Notes: Author calculations using ASHE 2005-2020. See the text for details of the sample selection, models and estimation, as well as Table 2 & Figure 3.