DO WAGES RISE WITH JOB SENIORITY? A REASSESSMENT

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The authors provide new estimates of the return to job seniority using a dataset similar to that employed in Joseph Altonji and Robert Shakotko, "Do Wages Rise with Job Seniority?" *Review of Economic Studies* (1987) and Robert Topel, "Specific Capital, Mobility, and Wages," *Journal of Political Economy* (1991). They consider the strengths and weaknesses of these studies' treatment of economy-wide trends, their dating conventions for tenure and wages, their handling of wage observations that might span multiple jobs, and their estimation approaches. Re-estimation points to an effect of ten years of tenure on the log wage equal to .11, which is above the preferred estimate of Altonji and Shakotko but far below that of Topel. Changes in earnings distributions and in the employment relationship motivate the examination of more recent data. Perhaps surprisingly, the return to tenure has probably increased over time.

Whether seniority has a large effect on wage growth has been the subject of continuing controversy. At stake is the empirical relevance of theories emphasizing a role for worker-financed firm-specific capital in wage growth and turnover behavior, as well as models of wages that emphasize the use of deferred compensation as an incentive, insurance, or sorting mechanism (see Carmichael 1989; Hutchens 1989; Malcomson 1999; Gibbons and Waldman 1999; and Farber 1999). The size of the return to seniority is also important in assessing the costs of dislocation from work, a subject of much policy discussion and research.[1] Perhaps most important, the strong

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A data appendix containing additional results and copies of the computer programs and data files used to generate the results in the paper is available from the first author at joseph.altonji@yale.edu.

[1]Studies that have examined the wage losses from layoffs and discussed the distinction between the value of lost seniority and the job match loss include Addison and Portugal (1989), Altonji and Williams (1998), Carrington (1993), Hamermesh (1987), Kletzer (1989), Jacobson, LaLonde, and Sullivan (1993a,b), Neal (1995), Ruhm (1991), and Topel

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relationship between seniority and wage rates in a cross-section of workers is a prominent feature of the earnings distribution that needs to be understood. However, nonrandom selection in who acquires seniority and in what types of jobs makes estimation of the return to seniority difficult.

In the 1970s and early 1980s, several studies—one widely cited example being Mincer and Jovanovic (1981)—concluded that there is a large return to seniority on the basis of the strong positive relationship between tenure and wage rates in cross-sectional or pooled cross-section–time-series data. Several papers in the mid-1980s, notably Altonji and Shakotko (1987) (hereafter, AS) and Abraham and Farber (1987) (hereafter, AF), challenged this conclusion, and the literature appeared to be moving toward a new consensus that the returns to seniority are relatively small in the United States.[2] However, an influential paper by Topel (1991) argued that AS and AF reached the wrong conclusions because of inappropriate methods, data, or both. He argued that the returns to tenure are large—on the order of what one obtains from a simple OLS regression.

Topel's results have been widely cited and appear to have been accepted by many researchers. For example, Polachek and Siebert (1993) stated, "Apparently the dramatic results of Altonji and Shakotko are in part due to mismeasuring tenure.... Thus the specific capital model appears to survive the challenge." Devine and Kiefer (1991) summarized the results from the various studies, including AS, AF, and Topel (1991), and stated, "The findings have gone full circle and beyond—the most recent results suggest that the early OLS results attributed too small a share of wage growth to tenure and too much to labor market experience." Felli and Harris (1996), citing the three studies, wrote, "Whether wages increase with tenure ... is an open question."

In this paper we first evaluate the relative strengths and weaknesses of the earlier studies, then perform multiple replications using the original data to test the soundness of our hypotheses concerning how the choices of data and methodologies affect the empirical results. Our analysis not only provides refined estimates of returns to seniority, but also has some implications for how best to conduct other similar empirical studies.

After sketching Topel's and AS's models, we consider the alternative methods the two studies used to control for secular wage trends and how the choices made affect the empirical results. Much of the difference in the trends arises from (1) Topel's use of a CPS-based wage index for 1968–83 with PSID wage data that refer to 1967–82, and (2) AS's use of a larger control set in the PSID. The remainder reflects unexplained differences between the CPS and PSID and the effect of sample selection rules and missing data on the replication sample. We also discuss the implications of attrition on the basis of fixed and time-varying person-specific and job-specific error components for how one should detrend the data. Further, we draw on recent literature comparing wage trends in the CPS to trends in other data. We conclude that while there is no perfect solution to the trend estimation problem, the appropriate trend is much closer to the trend AS estimated than to the trend that Topel used. Topel's treatment of secular wage growth leads to a substantial positive bias in the AS estimator and a smaller positive bias in his estimator. Our analysis of the pluses and minuses of alternative ways to handle time trends in panel data is of independent interest.

Some equally thorny issues concern the choice of timing of the tenure and wage data. AS and AF used the year t average hourly wage with year t tenure, whereas Topel used the average hourly wage in year t–1 with year t tenure after excluding jobs with seniority less than one. Problems arise

(1990). Jacobson, LaLonde, and Sullivan (1993b) and Farber (1999) surveyed the literature on dislocated workers.

[2]See also Topel (1986), Marshall and Zarkin (1986), and Williams (1991).

with both approaches because average wage observations at the beginning and end of jobs may be mixtures of more than one job. Simply adjusting the two tenure measures to reflect the location of the survey date within the year narrows the impact of the choice on AS's estimator by a modest amount. Both procedures for handling tenure in conjunction with the average annual wage have advantages and disadvantages that are hard to weigh. However, the results based on the wage rate at the time of the survey are much closer to those based on AS's procedure.

We go on to briefly review the impact of measurement error on AS's and Topel's results, and then shift to a discussion of differences between AS's IV1 estimator and Topel's estimator, revisiting Topel's test of whether his estimator is sensitive to bias from individual heterogeneity. AS's instrumental variables estimator usually leads to a lower estimate of the return to seniority than the two-step first difference estimator (2SFD) proposed by Topel. The gap narrows when we apply a rough bias correction to IV1. While the estimated tenure effects from both estimators are biased down by job match heterogeneity, 2SFD is substantially upward biased by individual heterogeneity.

After summarizing the findings from our replications, we draw conclusions concerning the relative desirability of the estimators and data used by AS and Topel. An auxiliary analysis reconsiders Topel's finding that relaxing certain restrictions in the AF estimator leads to large OLS-like seniority returns. Our main conclusion is that the returns to seniority are modest, and much closer to AS and AF's results than to Topel's. In contrast to Devine and Kiefer's summary of the literature mentioned above, we find that OLS is subject to a large upward bias and should not be used to estimate the return to tenure.

Before concluding, we present estimates based on a new PSID extract for 1975–2001. We do so for three reasons. First, the tenure and wage data are better for these years, particularly from 1981 on, than for the periods examined by the earlier studies. Second, it is likely, given dramatic changes in the returns to schooling, experience, and ability, that the returns to tenure and the relative biases in the estimators have changed as well. Third, the question of whether changes in the stability of jobs and the nature of the employment relationship in the 1980s have led to a decline in the return to tenure is of independent interest. The results suggest that the return to tenure may have increased, but it is still modest. The estimates for our preferred wage measure point to a return to ten years of tenure of about .09 for 1988–2001, but are higher for the wage measure used in the earlier studies.

1. Background to AS and Topel

1.1 The Wage Model

Many studies of the returns to seniority, including AS, AF, and Topel, have used the basic wage model

$$W_{ijt} = \beta_0 t + \beta_1 X_{ijt} + \beta_2 T_{ijt} + \varepsilon_{ijt}, \tag{1.1}$$

where W_{ijt} is the log real wage of person i in job j in period t, X_{ijt} is total labor market experience, and T_{ijt} is tenure with the employer. For expositional purposes and to facilitate some of the theoretical analysis, we include a linear time trend in the wage equation, but in most of the empirical work we do not restrict the trend. We sometimes suppress subscripts where the meaning is clear. All variables are deviations from sample means. The equation abstracts from a set of control variables and from nonlinear terms in experience and tenure that all three studies include. This makes it easier to compare the estimators and analyze biases.

The error term is decomposed as

$$\varepsilon_{ijt} = \mu_i + \phi_{ij} + \eta_{ijt} + u_{it}, \tag{1.2}$$

where μ_i is a fixed individual-specific error component, ϕ_{ij} is a fixed job match–specific error component, η_{ijt} is a time-varying job match–specific component, and u_{it} is the sum of measurement error in the wage and a person-specific component that affects wages at all employers. AS, AF, and Topel all ignored u_{it} on the grounds that it is unlikely to be related to turnover behavior. AS assumed movements in η_{ijt} are small or transitory and thus unlikely to have a strong relationship with turnover behavior. Topel argued that his analysis would be insensitive to η_{ijt} if it is a random walk and showed that the data are consistent with this.[3]

The key parameters of interest are β_1 and β_2, where β_1 is the partial effect of experience on the wage and β_2 is the partial effect of tenure. That is, β_1 is the causal effect of one more year of labor market experience on wages, holding tenure as well as job match quality ϕ_{ij} and the other error components constant. Since ϕ_{ij} is held fixed, β_1 does not include the return to job shopping over a career. β_2 is the causal effect on the wage of one more year of tenure, holding years of experience, job match quality, and the other error components constant. Therefore, $\beta_2 T_{ijt}$ is the wage loss the worker would suffer if he or she were to move to a new job with the same values for the error components.

Many studies have used OLS to estimate these parameters, and they have consistently found large returns to seniority. For example, AS and Topel reported that 10 years of seniority raises the log wage by .267 and .300, respectively. However, using OLS to estimate β_1 and β_2 is inappropriate, because both experience and tenure are likely to be correlated with the unobserved individual and job match heterogeneity. For example, tenure will be positively correlated with μ_i in the likely event that individuals with low productivity (low μ_i) have high quit and layoff propensities.[4] Individual heterogeneity associated with μ_i will bias OLS estimates of the wage-tenure profile upward.

To better understand the biases from unobserved individual and match heterogeneity, we follow Topel and specify auxiliary regressions between the unobserved components and experience and tenure. For the fixed job match error component ϕ_{ij}, let the auxiliary regression be

$$\phi_{ij} = b_1 X_{ijt} + b_2 T_{ijt} + \xi_{ijt}. \tag{1.3}$$

Consider the likely signs of b_1 and b_2. First, b_1 is likely to be positive because matching models and conventional search models (for example, Burdett 1978) imply that job shopping over a career will induce a positive correlation between X_{ijt} and ϕ_{ij}. Second, the sign of b_2 is ambiguous. On the one hand, workers will be less likely to quit high-wage jobs than low-wage jobs.[5] Furthermore, if firms share in the returns to a good match, ϕ_{ij} will be negatively correlated with the layoff probability. Both of these considerations suggest that tenure is positively correlated with ϕ_{ij} and b_2 is positive. However, Topel emphasized that the selection induced by voluntary job changes will lead low tenure values to be associated with large values of ϕ_{ij}, so b_2 could be negative.

[3]We ignore η in most of the comparison between the AS and 2SFD estimators. However, modifying AS's estimator in a way that should make it less sensitive to η if it is a random walk does not have a large effect on the results. Adding heterogeneity in the experience slope β_1 that is uncorrelated with turnover will not affect any of the estimators. We rule out person-specific or job match–specific heterogeneity in β_2, which is likely to be negatively related to T_{ijt}, and a positive source of bias in all three estimators. Topel presented evidence that this heterogeneity was not important for his analysis. We have not revisited this issue. In contrast, Abowd, Kramarz, and Margolis (1999) showed that there are differences in tenure slopes among a set of French firms. Farber (1999) argued that models stressing heterogeneity in tenure slopes deserve more attention in the literature.

[4]See AS for evidence that estimates of μ_i and ϕ_{ij} enter logit models for both quits and layoffs with negative signs. One of AF's key findings is that completed job tenure has a strong positive association with the level of wages on a job.

[5]The existence of differences in match quality across firm-worker pairs (see Johnson 1978; Jovanovic 1979), the presence of noncompetitive elements in the wage structure, and differences across firms in the optimal compensation level for a given type all imply that individual workers face a distribution of wages. See Groshen (1991), Jacobson, LaLonde, and Sullivan (1993a,b), and Abowd, Kramarz, and Margolis (1999) for evidence of firm-specific wage components.

The problem of individual heterogeneity may be analyzed similarly with the auxiliary regression

$$\mu_i = c_1 X_{ijt} + c_2 T_{ijt} + \omega_{ijt}. \tag{1.4}$$

With additional assumptions, one can show that $c_1 < 0$ and $c_2 > 0$. First, both AS and Topel assumed that

$$\text{Cov}(\mu_i, X_{ijt}) = 0. \tag{1.4a}$$

This implies that worker quality is independent of year of birth once one conditions on controls, and also that high- and low-wage workers have similar labor force attachment—at least for a sample of white male heads of households. AS investigated the second assumption and found that their results were insensitive to using potential experience as an instrument for actual experience.

Second, consider the auxiliary regression

$$T_{ijt} = d_1 \mu_i + d_2 X_{ijt} + v_{ijt}, \tag{1.5}$$

where $d_1 > 0$, $d_2 > 0$, and v_{ijt} is uncorrelated with μ_i and X_{ijt} by definition of an auxiliary regression. Given that $\text{Cov}(\mu_i, X_{ijt}) = 0$, it follows from (1.5) that d_2 is the coefficient γ_{XT} from a least squares regression of T_{ijt} on X_{ijt}. A little algebra establishes that c_1 and c_2 in (1.4) are

$$c_1 = -\gamma_{XT} c_2 < 0; \tag{1.6}$$

$$c_2 = d_1 \frac{\text{Var}(\mu_i)}{d_1^2 \text{Var}(\mu_i) + \text{Var}(v_{ijt})} > 0.$$

Using these results, we now discuss the OLS estimator as well as AS's and Topel's estimators of the wage model (1.1). We consider AF's approach in Section 8.

1.2 The OLS Estimator

Using equations (1.1) to (1.4), it is easy to show that the biases in the OLS estimators of β_1 and β_2 are

$$\beta_1^{\text{OLS}} - \beta_1 = b_1 + c_1$$

$$\beta_2^{\text{OLS}} - \beta_2 = b_2 + c_2.$$

Unfortunately, neither bias can be signed. The bias in experience is ambiguous because the job match (b_1) and individual heterogeneity (c_1) terms are of opposite signs. Similarly, the bias in tenure is ambiguous because the job match heterogeneity (b_2) may either offset or reinforce the upward bias in β_2^{OLS} from individual heterogeneity (c_2). However, if c_2 is large and positive and b_2 is either positive or small and negative, then the net bias in β_2^{OLS} will be positive, and the estimated effect of seniority on wages will be overstated in OLS wage regressions.

1.3 Altonji and Shakotko's IV1 Estimator

Altonji and Shakotko proposed an instrumental variables estimator to address the problems of individual and job match heterogeneity in the wage equation. Let $DT_{ijt} = T_{ijt} - \overline{T}_{ij}$ denote the deviation of tenure from the mean of the sample observations on tenure for job match *ij*. Abstracting from η_{ijt}, this variable is a valid instrument because it is orthogonal to the error components μ_i and ϕ_{ij}, which are fixed within the job. We refer to the estimator that uses DT_{ijt}, X_{ijt}, and t as instruments as the IV1 estimator. Using this estimator, AS found that 10 years of tenure leads to a wage increase of 2.7%, about 1/10 of their OLS estimate.

The IV1 estimator is free of bias from μ_i. However, the likely positive correlation between X_{ijt} and ϕ_{ij} leads to positive bias in β_1^{IV1} and negative bias in β_2^{IV1}. One can show that the bias in β_2^{IV1} induced by ϕ_{ij} is

$$-b_1 - \frac{\gamma_{XT}}{1 - \gamma_{XT}} (b_1 + b_2),$$

where γ_{XT} is the least squares coefficient in the regression of T on X. AS (p. 450) proposed a modification of the IV1 estimator to correct for this bias that is based on the fact that ϕ_{ij} is orthogonal to DT_{ijt}, and the correlation between t and ϕ_{ij} arises only because t is correlated with X_{ijt}, which means that only an estimate of $E(\phi_{ij} | X_{ijt})$ is required. They estimated $E(\phi_{ij} | X_{ijt})$ as the product of (1) an assumed value of the average change in ϕ_{ij} per quit and (2) the expected number of times a person will quit by experience level X_{ijt} based on a logit model of quits as a function of a cubic in experience. Using the modified estimator, IV1*, AS obtained .066 as their preferred estimate of the effect of ten years of tenure.

Unfortunately, AS's modification of IV1 ignores the fact that the gain from quits is likely to vary with the experience level, and it also ignores offsetting job match losses associated with layoffs. We make only a limited use of this bias correction below.

1.4 Topel's Two-Step First Difference (2SFD) Estimator

Prior to estimation, Topel (1991) detrended the data by subtracting an index of aggregate real wage growth from wages. The wage index was from an early draft of Murphy and Welch (1992), who created it using CPS cross-sections. In our discussion below, it is useful to keep in mind that this detrending procedure is similar to regressing the Murphy-Welch index on a time trend using the sample composition to weight the various years, and then using the coefficient estimate $\hat{\beta}_0$ to detrend the data. After detrending, the estimation of β_1 and β_2 proceeds in two steps. The first step estimates the combined effect of the linear experience and tenure terms ($\beta = \beta_1 + \beta_2$) by applying OLS to a within-job wage growth equation for stayers:

$$W_{ijt} - W_{ijt-1} - \hat{\beta}_0 = \beta + \varepsilon_{ijt} - \varepsilon_{ijt-1} + \beta_0 - \hat{\beta}_0. \tag{1.8}$$

Because current experience is the sum of the initial experience on the job $X0_{ijt}$ and T_{ijt}, in a second step one estimates the linear experience coefficient (β_1) by applying OLS to

$$W_{ijt} - \hat{\beta}_0 t - \hat{\beta} T_{ijt} = X0_{ijt}\beta_1 + e_{ijt}, \tag{1.9}$$

where $e_{ijt} = \varepsilon_{ijt} + t(\beta_0 - \hat{\beta}_0) + T_{ijt}(\beta - \hat{\beta})$ and $\hat{\beta}$ is the OLS estimate from (1.8). Finally, the linear tenure slope (β_2) is estimated as $\hat{\beta} - \hat{\beta}_1$. We refer to this procedure as the two-step first difference (2SFD) estimator. When the specification of the tenure and experience effects is linear and an outside estimate of β_0 is used, Topel showed that the IV1 estimator is approximately equivalent to using (1.8) to estimate β and estimating (1.9) by instrumental variables with X as an instrument for $X0$. The approaches are identical in the linear case if one replaces (1.8) with an equation for deviations from job means.

Since μ_i and ϕ_{ij} are included in e_{ijt}, and both may be correlated with $X0$, the 2SFD estimator will produce biased results. Specifically, Topel showed that job matching produces a downward bias in the estimator of β_2 equal to $-b_1 - \gamma_{X0T}(b_1 + b_2)$, where γ_{X0T} is the least squares coefficient in the regression of T on $X0$.

Given values of the coefficients in equations (1.3) and (1.4), we can compare the "job matching" bias in the IV1 and 2SFD estimators. For example, Topel reported that because in his sample γ_{X0T} is about -0.25 and γ_{XT} is .5, the downward bias in the tenure coefficient (β_2) is larger for the IV1 estimator than the 2SFD estimator, provided that $b_1 + b_2$ is positive. Evidence from Topel and Ward (1992) suggests that $b_1 + b_2$ is positive. However, because Topel's estimate of $b_1 + b_2$ is only .0020 (p. 159), his empirical results and the expressions for bias above imply that the difference between the IV1 and 2SFD estimators due to bias from ϕ_{ij} is $-(.5/(1-.5)-(-.25)) \times .0020 = -.0015$. Multiplying by 10, this implies that the difference between the estimators in the effects of job heterogeneity contributes only $-.015$ to the difference between IV1 and 2SFD in the estimated value of 10 years of seniority.

The overall bias in 2SFD for β_2 also depends on the importance of bias from μ_i. Some algebra establishes that the bias term $\gamma_{X0\mu}$ in Topel's equation (13) is equal to

$$\gamma_{X0\mu} = -c_2 \frac{\mathrm{Var}(T|X)}{\mathrm{Var}(T)},$$

where c_2 is the coefficient in the auxiliary regression (2.4) for μ_i above, and $\mathrm{Var}(T|X)$

is the variance of T_{ijt} conditional on X_{ijt}.[6] In the replicated sample we use below, $\mathrm{Var}(T|X)/\mathrm{Var}(T)$ is .676, so the bias from individual heterogeneity in the 2SFD estimator of the linear tenure coefficient is about 2/3 the size of the bias in OLS from this source.

Topel found that ten years of tenure leads to a log wage increase of .246. This implies a percentage increase in the wage level of 28%. Recognizing the potential bias from individual heterogeneity, he investigated by instrumenting $X0$ in (1.9) with X and found that the effect of ten years of tenure on the log wage only declines slightly to about .22. This is substantially larger than AS's IV1 and IV1* estimates of .027 and .066.[7]

Topel made a serious effort to explain the discrepancy between his findings and those of AS and AF. He argued that AS's tenure estimates are biased down due to (1) the greater effect of unobservable job heterogeneity on the IV1 estimator as compared to the 2SFD estimator, (2) measurement error in the tenure data, and (3) the use of an exogenous time trend. In contrast, Topel argued that the difference between his estimates and those of AF arises because AF used an inappropriate methodology. He concluded that the return to seniority is large and that the OLS estimates may even be downward-biased. Most of the remainder of this paper revisits the issue of why the studies obtained such different results and discusses the relative merits of their approaches.

2. Replication of Topel's Basic Results

We begin by replicating Topel's basic findings. We started with a working data set covering the 1968–83 PSID survey years that Topel used. Topel graciously provided a complete and well-documented set of programs for the May 1988 draft of his paper, which used a data set covering the 1968–81 survey years only.[8] We updated the programs to use the additional sample years.

The replication sample has 8,585 within-job wage change observations and 10,529 wage level observations, which is quite close to Topel's sample size.[9] In Table 1 we report the sample means of the variables used in the analysis. Column (1) is reproduced from Topel's Table A1. Column (2) reports the sample means for the replication sample. The remaining columns refer to other samples that we will discuss below.

The means of education, marital status, union membership, residence in an SMSA, and a disability affecting work match almost exactly. There is a substantial discrep-

[6]The parameter $\gamma_{X0\mu} = \mathrm{Cov}(X0,\mu)/\mathrm{Var}(X0)$. $\mathrm{Cov}(X0,\mu) = \mathrm{Cov}(X–T, \mu) = –\mathrm{Cov}(T,\mu) = –d_1\mathrm{Var}(\mu)$. $\mathrm{Var}(X0) = \mathrm{Var}(X–T) = \mathrm{Var}(X)–2\gamma_{XT}\mathrm{Var}(X) + \mathrm{Var}(T) \approx \mathrm{Var}(T)$ in Topel's sample because γ_{XT} is .5. Thus, $\gamma_{X0\mu} \approx -d_1\mathrm{Var}(\mu)/\mathrm{Var}(T)$. Using this result, equation (1.6) for c_2, together with the fact that (1.5) implies that $\mathrm{Var}(T|X) = d_1^2\ \mathrm{Var}(\mu) + \mathrm{Var}(v_{ijt})$, leads to the expression for $\gamma_{X0\mu}$ in the text.

[7]We infer the value of .22 from Topel's report on page 164 that the estimate is about 3% lower when X is used as an instrument for $X0$ in (1.9).

[8]The 1968–83 extract was supplied to Topel by Altonji and Nachum Sicherman. The wage data refer to the 1967–82 calendar years. These data are a superset of an extract for the 1968–81 surveys created by AS and used by AF. We were able to replicate a number of the results in the earlier draft.

[9]Topel reported 13,138 observations in the text (p. 154). However, he also reported a sample of 8,683 within-job wage change observations in the note to his Table 2, and 10,685 observations in the note to his Table 3. The latter values are probably the actual sample sizes, because the ratio of the within-job wage changes to wage level observations, of .8126, corresponds closely to the ratio of .8154 (8,585/10,529) in the replicated sample. We do not know the source of the discrepancies between Topel's sample and our replication. In the second paragraph on page 174, Topel states that he deleted some additional jobs in which there were ambiguities about starting and ending dates. An earlier draft of his paper makes the same statement, and we implemented the checks in the programs that Topel supplied to us corresponding to that draft. It is possible that some additional checks were put into place when Topel extended the sample to take advantage of the data from 1982 and 1983. He states that "these deletions had very minor effects on the results and none on the conclusions."

Table 1. Summary Statistics, PSID, 1968–1983.

Variable	Topel (1991) Sample (1)	Replicated Topel Sample: Tenure < 1 Excluded (2)	Samples Using Year t Wage, Year t Tenure: Tenure < 1 Included (3)	Samples Using Year t Wage, Year t Tenure: Exclude Last Obs. on Job and Earnings Mixtures at Start of Job (4)
EARN_MW68	1.131 (0.497)	1.538 (0.436)	1.492 (0.464)	1.571 (0.423)
ΔEARN_MW68	0.026	0.026 (0.230)	0.024 (0.243)	0.018 (0.226)
EARN_MW67		1.525 (0.435)	1.479 (0.463)	1.559 (0.422)
ΔEARN_MW67		0.022 (0.229)	0.018 (0.243)	0.014 (0.225)
EARN		1.645 (0.437)	1.600 (0.464)	1.678 (0.425)
ΔEARN		0.030 (0.230)	0.028 (0.244)	0.022 (0.226)
Experience	20.021 (11.045)	19.736 (10.565)	17.918 (10.676)	19.724 (10.35)
Tenure	9.978 (8.944)	10.630 (9.036)	8.666 (8.935)	10.827 (8.869)
Education	12.645 (2.809)	12.689 (2.807)	12.671 (2.784)	12.653 (2.839)
Married Now	0.925 (0.263)	0.929 (0.260)	0.916 (0.277)	0.936 (0.244)
Union	0.344 (0.473)	0.342 (0.473)	0.325 (0.466)	0.352 (0.476)
SMSA	0.644 (0.478)	0.635 (0.481)	0.646 (0.478)	0.647 (0.478)
Disability Affecting Work	0.074 (0.262)	0.071 (0.257)	0.068 (0.253)	0.068 (0.252)
Wage Change Observations	8,683	8,585	9,481	7,921
Wage Level Observations	10,685	10,529	12,274	9,528

Notes: Standard deviations in parentheses. Topel (1991) reported the number of wage change observations in the notes to his Table 2 (p. 157) and the number of wage level observations in the notes to Table 3 (p. 158). Samples are created from the Panel Study of Income Dynamics, survey years 1968 through 1983.

ancy in the mean of Topel's earnings measure, which we call EARN_MW68, but this appears to be due to a difference in the base of the price deflator used rather than to differences in the samples.[10] However, we doubt that this is important, because the mean of the change in EARN_MW68 in the replication sample is identical to Topel's

[10]As will be discussed further below, EARN is the log of real annual earnings divided by annual hours, while EARN_MW68 is equal to EARN minus the log of the real wage index constructed by Murphy and Welch for the years 1968 to 1983.

Table 2. OLS, IV1, and 2SFD on PSID Topel Replication Sample, 1968–1983.
(Dependent Variable: EARN_MW68)

	Replicated Topel Sample			*Topel (1991)*		
Variable	*OLS* *(1)*	*IV1* *(2)*	*2SFD* *(3)*	*OLS* *(4)*	*IV1* *(5)*	*2SFD* *(6)*
Linear Tenure Coefficient	0.0530 (0.0081)	0.0321 (0.0063)	0.0620 (0.0082)		0.032 (0.006)	0.0545 (0.0079)
Linear Experience Coefficient	0.0409 (0.0123)	0.0389 (0.0128)	0.0571 (0.0178)			0.0713 (0.0181)
2 Years of Tenure	0.0923 (0.0127)	0.0527 (0.0103)	0.1033 (0.0132)			
5 Years of Tenure	0.1881 (0.0219)	0.0973 (0.0194)	0.1950 (0.0237)	0.2313 (0.0098)	0.098 (0.017)	0.1793 (0.0235)
10 Years of Tenure	0.2732 (0.0250)	0.1188 (0.0277)	0.2474 (0.0287)	0.3002 (0.0105)	0.122 (0.024)	0.2459 (0.0341)
15 Years of Tenure	0.3145 (0.0262)	0.1232 (0.0367)	0.2588 (0.0300)	0.3203 (0.0110)	0.131 (0.028)	0.2832 (0.0411)
20 Years of Tenure	0.3495 (0.0287)	0.1467 (0.0485)	0.2904 (0.0317)	0.3563 (0.0116)	0.161 (0.035)	0.3375 (0.0438)
5 Years of Experience	0.1777 (0.0406)	0.1861 (0.0423)	0.2305 (0.0601)			
10 Years of Experience	0.3045 (0.0530)	0.3434 (0.0553)	0.3703 (0.0808)			
30 Years of Experience	0.4205 (0.0524)	0.5380 (0.0633)	0.4622 (0.0783)			

Notes: White standard errors in parentheses for the OLS and IV1 estimators. Standard errors for the 2SFD estimator account for the fact that it is a two-step estimator and for person-specific heteroskedasticity and serial correlation in the error terms. Columns (1) through (3) use the replicated Topel sample, while columns (4) through (6) contain estimates reported by Topel (1991). The specifications in columns (1) and (3) do not contain a time trend. The specification in column (2) contains an exogenous time trend. Columns (4) and (6) are taken from Table 3 of Topel. Column (5) is taken from Table 6, column (2) of Topel. All specifications include a quartic in tenure, a quartic in experience, years of education, and dummies for marital status, union membership, current disability, residence in an SMSA, residence in a city with a population of more than 500,000, and eight Census regions.

reported value of .026. The means of experience are close—20.02 versus 19.74. The one somewhat worrisome difference is that the mean of tenure is 9.98 in Topel's sample and 10.63 in our replication, a difference of .65 years.

In Table 2 we directly compare estimates reported by Topel to those obtained using our replicated sample. (Cells are empty for estimates that are not reported in his study.) In columns (1) and (4) we report the OLS estimates.[11] Our replicated estimate of the effect of 5 years of tenure is .1881, which is substantially below Topel's estimate of .2313. However, at ten years of tenure the estimates are .2732 versus .3002, and the estimates are quite close at the higher levels of tenure. In columns (2) and (5) we report the IV1 estimates when we include an exogenous time trend. The estimates for the replication sample are almost identical to Topel's. In columns (3) and (6) we present the 2SFD estimator. The repli-

[11]Unless otherwise noted, we report White standard errors that account for individual-specific heteroskedasticity and serial correlation in the error terms as well as for the fact that the 2SFD estimator is a two-step estimator.

cated results yield almost exactly the same effect at five and ten years, and a slightly lower effect at 15 and 20 years. We have also replicated many other results reported in Topel's paper. The comparisons between columns (2) and (5) and between (3) and (6) are typical of what we found. We conclude that our replication sample is close enough to Topel's to give reliable information.

3. Controlling for Economy-Wide Time Trends and Changes in Sample Composition

3.1 Introduction

Empirical studies often must control for the effects of secular change. AS, AF, and many other studies have included a time trend or year dummies in their wage models to control for economy-wide changes in real wages. Topel argued for an alternative approach that deflates wages by subtracting the log of a real wage index created from CPS data by Murphy and Welch (1992). In the tables below we refer to the PSID wage measure as EARN, the index as MW68, and the trend-adjusted real wage measure as EARN_MW68.

In this section, we replicate Topel's finding that the much lower trend in MW68 leads to a large increase in the tenure effect, particularly for the IV1 estimator; analyze the reasons for the difference in the trend in MW68 and trend estimates based on the PSID; discuss potential sources of bias from treating the time trend as exogenous in a wage equation and a way to fix the problem by instrumenting the time trend; and review evidence on possible bias in wage trends based on the CPS.

3.2 Sensitivity of the Results to Treatment of the Time Trend

In Panel A of Table 3 we report results for the OLS, IV1, and 2SFD estimators for two different treatments of the time trend. The models include 4th-order polynomials in tenure and experience, as well as the other control variables used by Topel. Due to space constraints, we only report estimates of the effect of ten years of tenure based on the models.[12] In columns (1A)–(3A) we use EARN_MW68 as the dependent variable and exclude year dummies from the equation. The IV1 and 2SFD estimators both imply a return to ten years of tenure of about .25. The implied effect of 20 years of tenure is larger for the IV1 estimator than for the two-step estimator (.39 versus .29, not reported).

In columns (5A)–(7A) we instead use EARN as the dependent variable, adding year dummies to control for the time trend. The annual growth in year dummies is .0090 in the case of OLS and .0084 in the case of IV1.[13] The OLS estimator is virtually the same as when EARN_MW68 is used. However, the IV1 estimate declines by half—from .235 to .123. There is also a small decline in the 2SFD estimator.[14] Topel reported very similar results in his Table 6.

[12]Estimates of the return to 2, 5, and 20 years of T and 10 and 30 years of experience are .062 (.011), .122 (.020), .278 (.051), .329 (.056), and .436 (.070), respectively, for model 2B, .098 (.013), .183 (.024), .240 (.032), .353 (.081), and .420 (.078) for model 3B, .046 (.010), .079 (.019), .064 (.041), .344 (.056), and .593 (.059) for 6B, .097 (.013), .178 (.024), .210 (.032), .336 (.080), and .391 (.078) for 7B, .029 (.008), .0443 (.014), .037 (.040), .349 (.041), and .575 (.048) for 6C, and .062 (.010), .111 (.018), .144 (.030), .345 (.070), and .374 (.066) for 7C. The detailed results and coefficient estimates for all models in the paper are available from the authors. Note that the AS and Topel papers used different functional forms but that this explains little of the differences in the results of the two studies.

[13]The linear trends we report are based on regressions of either the MW index or the year dummy estimates on a linear trend and a constant. In all cases we use year weights that reflect the distribution of observations across years in the replication sample underlying panel A of Table 3. We do this because the mix of observations across years has an effect on the linear trend estimate.

[14]Equation (1.9) reveals that the 2SFD estimator requires an estimate of the time trend or year dummies. We use the OLS or IV tenure exogenous estimates of the year dummies corresponding to the particular panel when implementing the 2SFD estimators. So here we use the year dummy estimates from the specification in column (5A).

Table 3. Investigating Detrending and the Dating of Wages and Tenure, PSID Topel Replication Sample, 1968–1983.

Panel A: Year t – 1 Wage, Year t Tenure, Tenure < 1 Excluded									
		EARN_MW68				*EARN, Year Dummies Exogenous*			
		OLS	*IV1*	*2SFD*	*2SFD X0 Endo*	*OLS*	*IV1*	*2SFD*	*2SFD X0 Endo*
		(1A)	*(2A)*	*(3A)*	*(4A)*	*(5A)*	*(6A)*	*(7A)*	*(8A)*
10 Years of Tenure	1	0.273	0.235	0.247	0.216	0.278	0.123	0.222	0.119
		(0.025)	(0.029)	(0.029)	(0.045)	(0.024)	(0.028)	(0.029)	(0.042)
Dating Corrected 10 Years of Tenure	2	0.243	0.216	0.206	0.175	0.248	0.100	0.180	0.076
Panel B: Year t – 1 Wage, Year t Tenure, Tenure < 1 Excluded									
		EARN_MW67				*EARN, Year Dummies Endogenous*			
		OLS	*IV1*	*2SFD*	*2SFD X0 Endo*	*IV T Exo*	*IV1*	*2SFD*	*2SFD X0 Endo*
		(1B)	*(2B)*	*(3B)*	*(4B)*	*(5B)*	*(6B)*	*(7B)*	*(8B)*
10 Years of Tenure	3	0.275	0.177	0.222	0.129	0.278	0.078	0.210	0.082
		(0.025)	(0.029)	(0.029)	(0.044)	(0.025)	(0.025)	(0.029)	(0.042)
Dating Corrected 10 Years of Tenure	4	0.245	0.157	0.181	0.088	0.248	0.055	0.1688	0.039
Panel C: Year t Wage, Year t Tenure, Tenure < 1 Included									
		EARN_MW67				*EARN, Year Dummies Endogenous*			
		OLS	*IV1*	*2SFD*	*2SFD X0 Endo*	*IV T Exo*	*IV1*	*2SFD*	*2SFD X0 Endo*
		(1C)	*(2C)*	*(3C)*	*(4C)*	*(5C)*	*(6C)*	*(7C)*	*(8C)*
10 Years of Tenure	5	0.280	0.096	0.123	–0.012	0.283	0.033	0.126	–0.011
		(0.021)	(0.026)	(0.023)	(0.042)	(0.021)	(0.022)	(0.023)	(0.041)
Dating Corrected 10 Years of Tenure	6	0.293	0.101	0.133	–0.003	0.297	0.039	0.136	–0.0004
Panel D: Year t Wage, Year t Average Tenure, Omit Last Observation on Job and First Year Mixtures									
		EARN_MW67				*EARN, Year Dummies Endogenous*			
		OLS	*IV1*	*2SFD*	*2SFD X0 Endo*	*IV T Exo*	*IV1*	*2SFD*	*2SFD X0 Endo*
		(1D)	*(2D)*	*(3D)*	*(4D)*	*(5D)*	*(6D)*	*(7D)*	*(8D)*
10 Years of Tenure	7	0.210	0.097	0.119	0.010	0.210	0.009	0.119	0.012
		(0.026)	(0.029)	(0.026)	(0.043)	(0.026)	(0.026)	(0.026)	(0.042)
Dating Corrected 10 Years of Tenure	8	0.220	0.098	0.123	0.016	0.220	0.012	0.125	0.017

Notes: The rows labeled "10 Years of Tenure" are estimates of the effect of 10 years of tenure based on the wage model estimates. The rows labeled "Dating Corrected 10 Years of Tenure" adjust for discrepancy between the dating of the average hourly wage measure and tenure. See Section 4 in the text for details. The wage specification is as described in the notes to Table 2. The sample sizes are 10,529 for columns (1), (2), (5), and (6) and for the second step of the 2SFD estimator (equation 1.9) in columns (3), (4), (7), and (8) in panels A and B. The sample for the first stage of the 2SFD model (1.8) in columns (3), (4), (7), and (8) contains 8,585 wage change observations. The sample sizes are 12,274 for columns (1C), (2C), (5C), and (6C) and for the second step of the 2SFD estimator in columns (3C), (4C), (7C), and (8C). The sample for the first stage of the 2SFD model in columns (3C), (4C), (7C), and (8C) contains 9,481 wage change observations. The sample sizes are 9,528 for columns (1D), (2D), (5D), and (6D) and for the second step of the 2SFD estimator in columns (3D), (4D), (7D), and (8D). The sample for the first stage of the 2SFD model in columns (3D), (4D), (7D), and (8D) contains 7,921 wage change observations. Standard errors are in parentheses—see Table 2.

Clearly, in the replication sample the method used to detrend matters. What does theory predict about the consequences of the time trend used? In the case of OLS, the consequences should be small because both the simple and partial correlations between t and X_{ijt} and between t and T_{ijt} are small. This is what we find in columns (1A) and (5A) of Table 3. In the appendix, we show that the change in the IV1 estimate of the tenure effect should be approximately –2.27 times the difference in the trend in MW68 index and the trend in the year dummies. In the replication sample the OLS estimate of the trend in the year dummies is .00835 and the trend in the Murphy-Welch index is .00281. This implies that the IV1 estimate of the effect of ten years of seniority will decline by about 2.27∗(0.00835 – 0.00281)∗10, or 0.1259, when one replaces EARN_MW68 with EARN and adds year dummies (or a linear time trend) to the model. The actual decline in Table 3, Panel A is 0.1124.

In the appendix we also show that the difference in the 2SFD estimates should be about –.79 times the difference in the trends, so the effect of ten years of seniority will decline by about .79∗(0.00835 – 0.00281)∗10, or 0.0438, when one replaces EARN_MW68 with EARN and adds year dummies (or a linear time trend) to the model. The actual difference using the estimates in column (3A) and column (6A) is a somewhat smaller 0.0252, perhaps because the analytical formula in the appendix ignores nonlinearity in the specification of tenure and experience.

In sum, we confirm that controlling for economy-wide real wage changes using the MW68 real wage index vis-à-vis estimating year effects with the PSID sample has no effect on OLS, makes a substantial difference in the IV1 estimator, and has a modest effect on the 2SFD estimator. These results are generally consistent with the theoretically predicted effects. We now turn to the more difficult question of which of the two procedures is preferred.

3.3 Sources of the Difference in CPS-Based and PSID Time Trends

It is helpful to begin by decomposing the difference in the time trend between the CPS index and that obtained using the PSID replication sample into four sources: (1) Topel's dating of the MW index; (2) differences between the CPS and a representative PSID sample; (3) the effect of controlling for union and marital status; and (4) differences between the representative PSID sample and the replication sample.

Dating of the Murphy-Welch Index

MW68 is equal to an early version of the Murphy and Welch index for the years 1968–81 and the published version for 1982 and 1983. Topel used index values for 1968 to 1983 to detrend PSID earnings data for 1967 to 1982. This means, for example, that PSID earnings in 1970 were detrended using the CPS-based index value for 1971. To examine the impact of this choice, we regressed MW68 on a linear time trend, weighting to reflect the distribution across years of the observations in the replication sample. By comparing the estimated trend values in rows 1 and 2 of Table 4, we observe that using the 1968–83 wage index values rather than 1967–82 values understates the 1967–82 trend by .0025. This understatement reflects the fact that growth in the published Murphy-Welch index between 1967 and 1982 is 4.8% higher than between 1968 and 1983. When we re-estimate the wage models using EARN minus the log of the published Murphy-Welch index for 1967–82 (hereafter EARN_MW67), the IV1 estimate of the effect of 10 years of tenure declines from .235 based on EARN_MW68 to .177 (Table 3, column 2B). Thus, the gap between IV1 using an index to detrend wages and IV1 using year dummies is reduced by about half if one uses the values of the Murphy-Welch index for the calendar years corresponding to the PSID wage data.[15] The 2SFD estimate declines to .222.

[15]Results are not sensitive to whether we use the early version or the published version of the Murphy-Welch wage index series.

Table 4. Time Trend Investigation, PSID Topel Replication Sample, 1968–1983.

		Trend Estimate
Topel Replication Sample		
Earn_MW68, CPS Wage Index from 1968 to 1983 Used to Deflate Earnings	(1)	.0028
Earn_MW67, CPS Wage Index from 1967 to 1982 Used to Deflate Earnings	(2)	.0053
Representative PSID Sample		
Earn, without Holding Union and Marital Status Constant, Weighted	(3)	.0065
Earn, Holding Union and Marital Status Constant, Weighted	(4)	.0081
Earn, Holding Union and Marital Status Constant, Unweighted	(5)	.0082
Earn, Holding Union and Marital Status Constant, Matched to Replication Sample, Unweighted	(6)	.0108
Topel Replication Sample		
Earn, Holding Union and Marital Status Constant, Year Dummies Exogenous	(7)	.0090
Earn, Holding Union and Marital Status Constant, Year Dummies Endogenous	(8)	.0114

Notes: Rows 1 and 2 come from a regression of the Murphy-Welch real wage index onto a constant and time trend, using year weights that reflect the distribution of observations across years in the replication sample. The estimates in rows 3–7 are obtained by first regressing log earnings onto a quartic in experience, other controls, and year dummies modeled as exogenous. The estimated year dummies are then regressed onto a constant and a time trend using year weights that reflect the distribution of observations across years in the replication sample. The estimate in row 8 is similar, except that the year dummies in the first step are instrumented using deviations from individual means for each year dummy.

PSID Sample Selection Criteria and Control Variables

Next we look at the effect on the time trends of the explicit and implicit sample selection criteria underlying the replication sample. The replication sample only includes white men who were heads of households in one or more of the years 1981, 1982, and 1983, who held jobs in the private sector, who were not self-employed, and for whom it was possible to construct job indicators and a tenure measure in a given year. To investigate whether the head of household restriction matters, we created a comparable sample from the PSID for the survey years 1968–83 that is not limited to persons who were heads of household in one or more of the years 1981, 1982, and 1983 and is not selected based on the availability of tenure information. We then regressed Earn on our basic control set with marital status and union status excluded plus a quartic in experience and dummies for each calendar year using person weights.[16] The trend in the year dummies is .0065 (Table 4, row 3). Thus, there is a discrepancy of .0012 in the trend in MW67_82 and the trend in a representative sample of household heads from the PSID.

Note that the CPS indices do not hold union status and marital status constant, while the wage models used to estimate the effect of tenure do. Adding controls for marital status and union status raises the trend estimate to .0081 (Table 4, row 4). The increase in part reflects the decline in unionization from 33.7% of the sample in 1967 to 23.4% in 1982. When we drop person weights, the estimate rises slightly to .0082 (row 5).

[16]We obtained weights for the SRC sample by dividing the person weights for an individual in all years by the weight for the individual in the 1968 survey. Note that individuals who marry into PSID sample families receive a weight of 0. These individuals receive a weight of 1 in the unweighted case.

The PSID trend rises to .0108 when we restrict the PSID to observations that are in the replication sample (row 6). This change in sample represents the combined effect of restricting the sample to persons who were heads of household in one or more of the years 1981, 1982, and 1983 and the loss of additional cases because of missing data or ambiguities in the data used to identify employer changes and tenure. Further analysis shows that the trend is .0031 higher for the observations in the replication sample than for those in the full sample. This value falls to .0019 when we add an interaction between a linear trend and an indicator for attrition. (The trend is .00106 lower for persons who leave the sample than for the full sample, although this difference is not statistically significant.) Consequently, we conclude that about .0012 (.0031 − .0019) of the trend difference between the full sample and the replication sample is associated with attrition.

Finally, we emphasize that the trend of .0090 (row 7) in the actual replication sample is smaller than the trend in the observations that are in both the replication sample and the full sample (row 6).

To sum up, we have a total difference of .0062 in the trend in MW68 and the trend in the year dummy estimates based on the replication sample (row 7 – row 1). Using the correct years of the MW index accounts for 40.3% of this difference. Controlling for union and marital status accounts for 27.4% of the difference. This leaves a difference of .002, or 32.3%, that is unaccounted for, of which .0012 is a difference between the trend in MW67 and the trend in the representative PSID sample of household heads (row 3 – row 2) and .0008 (row 7 – row 5) is the difference between the trend in the replication sample and a matched sample of the representative and the replication samples.

The results suggest that if one were to adjust the MW67 index up to account for head of household status and changes in unionization and marital status, it would not matter very much whether one controlled for the trend using year dummies or used the MW67 index after adjustment. This is especially true given that the trend falls to only .0084 in the replication sample once tenure is added to the model. The results also suggest that one must be careful in applying a secular wage adjustment from outside the sample if different sample selection rules are used. Part of the unexplained difference might reflect the interaction between differences in sample composition and heterogeneity in economy trends, but we also show that part is associated with lower wage growth rates among those who ultimately drop out of the PSID sample. In the next section, we discuss possible sources of bias in the PSID and in the CPS that may account for the two unexplained discrepancies.

3.4 Potential Sources of Bias in Treating Time as Exogenous

Using exogenous year dummies in the wage regressions may be problematic if these dummies are correlated with the unobservable error components detailed in (1.2). We consider three possibilities. First, time may be positively correlated with the job match component. Specifically, if time is correlated with average X_{ijt}, and average X_{ijt} is correlated with ϕ_{ij}, then time is likely correlated with ϕ_{ij}. This is not a major concern in the representative PSID sample or the CPS, because both, after weighting, are representative of the U.S. population, and the change in average experience with calendar time is minimal. In principle, it could be a more important issue in the replication sample. However, the simple correlation between t and ϕ_{ij} is likely to be weak because the relationship between time and average experience is very weak.[17]

[17]In the replicated sample, the coefficient on X_{ijt} in the auxiliary regression of t on X_{ijt} is actually negative (−.0024) and is not statistically significant. The relationship is weak primarily because much of the variation in X_{ijt} is cross-sectional but also because the PSID is a self-replicating sample. The male children in the original PSID families enter the regression sample when they set up separate households, and original members of the sample leave when they

In any case, the covariance between t and ϕ_{ij} will not lead to bias in the tenure and experience coefficients of the OLS and IV1 estimators. The covariance between t and ϕ_{ij} arises because t is correlated with the amount of time a given cohort of workers has been in the labor market, not because of the passage of time per se. Consequently, the covariance is zero conditional on X_{ijt}, T_{ijt}, and X_{ijt}, or T_{ijt} and $X0_{ijt}$. That is,

$$\mathrm{Cov}(t, \phi_{ij}|X_{ijt}) = \mathrm{Cov}(t, \phi_{ij}|T_{ijt}, X_{ijt}) = \mathrm{Cov}(t, \phi_{ij}|T_{ijt}, X0_{ijt}) = 0.$$

It follows that whether or not t is positively correlated with ϕ_{ij} has no effect on the probability limit of the OLS estimator or the IV1 estimator. However, inclusion of a time trend or year dummies in the second step of the 2SFD estimator is likely to have an effect, because T_{ijt} is omitted from (1.9) and t is correlated with T_{ijt} conditional on $X0_{ijt}$.

A second objection to the use of exogenous year dummies in the wage regressions is that changes in sample composition may induce a positive correlation between time and unobservable individual heterogeneity (μ_i). Becketti et al. (1988) and Fitzgerald, Gottschalk, and Moffitt (1998) found that attrition is higher among low-income individuals. Consequently, there is reason to believe that the trend in the full PSID sample could be biased upward. We do not believe this is an important problem in our case, because the replication sample is drawn from those who were heads of household in at least one of the years 1981, 1982, and 1983. Sample attrition on the basis of μ_i does not lead to an upward trend in μ_i, because persons who left the PSID prior to 1981 are excluded in all years.[18]

In any event, one can deal with correlation between t and μ_i by using the deviation of t from its mean for person i as an instrument for time. This variable is uncorrelated with μ_i by construction and is uncorrelated with ϕ_{ij} conditional on T_{ijt} and $X0_{ijt}$.[19] In practice we replace the time trend with a vector of year dummies YD_t and use $\widetilde{\mathrm{YD}}_{it}$, the deviations of the elements of YD_t from their means for each individual, as instrumental variables for YD_t. The instrumental variables estimate of the trend in the year effects is .0099 in the representative PSID sample, which compares to .0082 when we treated the year dummies as exogenous. The corresponding numbers for the PSID replication sample are .0114 and .0090 (when tenure is excluded). Results in (6A) and (6B) of Table 3 show that the IV1 estimate of the return to 10 years of tenure declines from .123 when YD_t is treated as exogenous to .078 when we instrument using $\widetilde{\mathrm{YD}}_{it}$. The counterparts to the OLS (labeled IV T Exo) and the 2SFD estimators in columns (5B) and (7B) are very similar to those in columns (5A) and (7A). We conclude that there is not much of an *a priori* case and little evidence that an upward trend in the mean of μ_i leads to an overestimate of the time trend in the replication sample. If anything, the evidence points to an underestimate.

This leaves a third problem, which is the potential for bias due to an attrition-in-

retire, die, or reach the age 60 cutoff used by AS and Topel. Furthermore, the sample includes men who marry members of original PSID sample households. The split-offs and persons who marry into the PSID sample tend to enter the sample early in their careers, while the heads in 1968 were a cross-section of the population.

[18]Some of the observations in the later years are contributed by persons who married into PSID families or who split off from the original households. The observations in the early years might be even be more select than those in the late years, because they are contributed by persons who remained in the sample until at least 1981. This does not apply to the samples from the 1975–2001 data analyzed in Section 10, because we use the available data on persons who later become nonrespondents.

[19]Biases in the tenure and experience coefficients from other factors could contaminate the time trend coefficient even if the time trend is unrelated to the error term. However, the correlations between t and X_{ijt} and T_{ijt} are too weak for this to be a serious issue in the replication sample, as evidenced by the fact that the estimated time trend is nearly the same for OLS and IV1 despite the large difference in the tenure and experience effects.

duced association between the job match component ϕ_{ij}, which is implicitly indexed by t, and the sum of the time-varying components u_{it} and η_{ijt}. The fact that the growth in wages is more rapid in the full PSID sample for those who remain in the PSID sample until 1981 or later than for those who drop out prior to 1981 could be a symptom of such an association. Suppose attrition is negatively related to $\phi_{ij} + u_{it} + \eta_{ijt}$. Then $E(\phi_{ij} + u_{it} + \eta_{ijt})$ will rise with time since entry into the PSID sample. Because membership in the replication sample is conditional on surviving until at least 1981, years since entry will be correlated with time in the replication sample even after conditioning on experience. It will also be positively correlated with the deviation of time from the individual means.

The issues raised by sample selection on ϕ_{ij}, u_{it}, and η_{ijt} are broader than the question of how to control for secular wage growth, and the implications for whether one should use the MW index or year dummies to control for economy-wide changes are not clear-cut. Even if the MW index is a perfect control for economy-wide changes, the attrition-induced trend in $u_{it} + \eta_{ijt}$ will cause wage growth within jobs to exceed $\beta_1 + \beta_2$, leading to an overestimate of the return to tenure. Very little of this effect would be offset by correlation between $X0_{ijt}$ and ϕ_{ij}, because $X0_{ijt}$ has a very weak correlation with t in the replication sample. On the other hand, if one introduces year dummies into a wage-level equation, year dummy coefficients will be biased by the fact that $E(\phi_{ij})$ and $E(u_{it} + \eta_{ijt})$ depend positively on t because of attrition, but this will reduce the effect of $E(u_{it} + \eta_{ijt})$ on within-job wage growth and reduce the overestimate of the return to tenure. In summary, if attrition is mainly based on ϕ_{ij}, then simply removing β_0 using the MW index might minimize bias from $\phi_{ij} + u_{it} + \eta_{ijt}$; but if attrition is mainly due to $u_{it} + \eta_{ijt}$, then the year dummies may serve as a partial control for its effects on $E(u_{it} + \eta_{ijt})$. Thus it is not clear which approach to dealing with aggregate trends is more robust with respect to sample selection based on $\phi_{ij} + u_{it} + \eta_{ijt}$.

3.5 Bias in Trends Based on the CPS Wage Series

Aside from the potential for differences between the trends that reflect differences between the underlying populations for the PSID sample and the CPS, there is the issue of whether the CPS should be treated as the "gold standard." Abraham et al. (1998) noted that the growth in wages in the March CPS is lower than growth in wages as measured by the National Income and Product Accounts (NIPA) hourly wage series. They examined a number of possible explanations for the divergence, including the fact that the NIPA is job-based while the CPS is person-based, as well as differences in population coverage, earnings coverage, and hours reporting. They showed that much of the discrepancy results from a decline in the NIPA weekly hours measure relative to the CPS measure. They found some evidence that an increase in over-reporting of hours in the CPS may account for a substantial share of the divergence between NIPA and the CPS hourly earnings measures. It is possible, of course, that there is also an increase in over-reporting of hours in the PSID. In any event, we have re-estimated our models in panels A and B of Table 3 using the NIPA wage series deflated by a GNP implicit price deflator for personal consumption expenditures in place of the MW67 wage index. The IV1 estimate is .0621 using the NIPA index, which is below the estimate of .123 obtained by treating year dummies as exogenous and very close to the estimate obtained when the year dummies are instrumented using $\widetilde{YD}_{it}$.

Finally, it is possible that the average trend is greater for the replication sample than for CPS sample members. The PSID sample is restricted to heads of household, while the CPS includes other men. This could make a difference if the trend in earnings has been higher for high-income individuals, as most research on inequality suggests, because heads of household tend to have higher earnings than other household members. Also, the PSID largely excludes persons who immigrated to the

United States after 1968, while the CPS includes them. Since recent immigrants earn less than natives, the immigration growth in the 1970s likely depresses growth in the CPS index relative to the PSID, although we doubt that the effect is large.

In summary, it is quite possible that there is bias in trend estimates based on the CPS. It is also possible that time trends for the populations that underlie the PSID and CPS samples differ. We have also shown for the period under study that the control set matters. These are reasons to prefer estimating the time trend within the PSID sample over imposing one from another data set.

3.6 Conclusions about Detrending Procedures

Only 32% of the discrepancy in trend estimates based on AS's and Topel's procedures remains if one uses the correct years of the MW index and accounts for the fact that union and marital status are controlled for in the earnings regressions. The percentage is even smaller if one compares the trend in MW68 to the trend in the year dummies obtained in the replication sample when seniority is in the regression. Using MW67 with an adjustment for an estimate of the part of the trend associated with changes in unionism and marital status will lead to results that are fairly close to those based on including year dummies in the wage regressions. There may be a case for instrumenting the year dummies using deviations from means as a way to deal with bias from attrition on the basis of fixed unobservables, and we do this below. Which detrending procedure is more robust with respect to the potential for bias from attrition on the basis of time-varying error components is an open question. It might be best to try to model such attrition, but we leave this to future research.

4. The Dating of the Tenure and Wage Measures

In the PSID, employer tenure, union status, and other job-specific variables refer to the survey date (typically March, April, or May), while the wage measure is annual earnings divided by annual hours in the previous calendar year. Consequently, AS used the wage measure from the survey in year t and tenure and union status from the survey in $t-1$. In contrast, Topel took both the wage and the tenure and union status measures from the year t survey. He excluded observations if $T_{ijt} < 1$ because "wages refer to average hourly wages in the year preceding the survey."

The key issue, of course, is not the particular surveys that the tenure information come from but the consistency between the tenure measures and the wage measure, as well as the impact of the rules on sample composition. Both dating procedures could lead to bias, for several reasons. The first has to do with the fact that all the estimation methods suggest that the return to seniority declines sharply after the first year or two. If April 1 is the average survey date, then Topel's tenure measure is overstated by .75 years on average, while AS's is understated by about .25 years. Adding a constant to the tenure variable affects the interpretation of the coefficients.

Second, both studies use some wage observations that are likely to be mixtures of the wage on the old job and the wage on the new job if a job change occurred during the year. First observations on jobs have the same problem in both studies. If one ignores measurement error in tenure and assumes (a) that the interview date is uniformly distributed between an early date and a late one, (b) that tenure is uniformly distributed conditional on being less than 1, and (c) that there is no time between jobs, then measured wage growth in the first year on the job will overstate actual growth by roughly

$$g \cdot \int_{t_{Imin}}^{t_{Imax}} ((.5 \cdot t_I) \cdot t_I \,/\, (t_{Imax} - t_{Imin}))\, dt_I,$$

where g is the average growth in wages between jobs, and t_{Imin}, t_{Imax}, and t_I are the earliest, latest, and actual interview dates (measured as the fraction of the year that has passed). Tabulations of the changes in the survey wage rate (see below) across jobs in Altonji and Williams (1998) suggest that g is only about .0383 for all separations. Interviews in the PSID are heavily concentrated in March, April, and May. If one assumes that interview dates are uniformly distributed between March 1 (t_{Imin} = 2/12) and May 30 (t_{Imax} = 5/12), and one sets g to 3 times the mean value of .0383, then the bias in the estimate of growth during the first year on the job is .005186. This is probably close to an upper bound and suggests that the fact that observations are a mixture across jobs is a relatively minor problem given the estimation methodologies both studies use.

AS's dating convention also leads to potential problems with the last observation on a job. Under the assumptions above, a formula similar to (4.1) holds with t_I replaced by $(1 - t_I)$ in the integral. In this case, the bias in growth for last observations on jobs is .029. Since these account for 18.5% of the wage growth observations, this could lead average wage growth to be overstated by .0054, which is substantial. However, the value we have used for g is probably substantially overstated.

Finally, and perhaps most important, the decision about dating and whether to eliminate t – 1 observations if $T_{ijt} < 1$ has an important effect on the mix of observations. Excluding wage observations from t – 1 if T_{ijt} at the survey if t is less than 1 eliminates some short jobs altogether and limits observations on some relatively short jobs to only one observation on the earnings level and none on earnings growth. Because short jobs tend to pay less than other jobs and exhibit lower growth rates (conditional on the low tenure), we suspect that the restriction increases in estimates of the returns to seniority. However, it is hard to say precisely what the effect would be.[20]

We start the examination of the impact of the dating conventions by using Topel's dating convention and simply subtracting .75 from the value of tenure. We report the "dating-corrected" estimates of the effect of 10 years of tenure in rows 2 and 4 of Panel A and B of Table 3. As expected, this reduces IV1 and 2SFD estimates somewhat—by about .02 and .04, respectively.

In Panel C of Table 3 we report results for the various estimators following AS's practice of using year t wages with year t tenure and including observations with T_{ijt} < 1. There are 1,889 wage-level observations and 965 wage growth observations in this sample that are not in the sample for Panels A and B. For these additional observations, the means of EARN and the growth in EARN are 1.33 and .0085, respectively, which are well below the overall means for these variables reported in Table 2. The mean of tenure is 2.85. There are also 144 wage observations and 0 wage growth observations that are in Panels A and B but not in Panel C.

Columns (1C)–(4C) report results that use EARN_MW67 as the wage measure and exclude year dummies. Comparison of columns (2B) and (2C) shows that AS's dating rule and inclusion of low-tenure observations leads to a decline in the IV1 estimate of the effect of ten years of tenure from .177 to .096. In the 2SFD case the effect declines from .222 to .123 (columns 3B and 3C). In row 6 of Panel C we report estimates of the effect of 10 years of tenure that adjust for the fact that AS's dating rule understates tenure by about .25 years. (These are obtained by re-estimating after adding .25 to tenure and to experience without changing the sample.) As expected, these estimates of the effect of 10 years of tenure are slightly larger than the unadjusted estimates in row 5. Comparing the adjusted IV1 estimates in column (2B), row

[20]Furthermore, requiring that tenure and wages be measured in the same year means that the first wage observation on some individuals (for example, those from the 1968 survey) are lost if tenure is less than 1 in the first year we observe. For example, if tenure at the survey date in 1968 is estimated to be .5, one cannot infer a value of tenure for 1967, because a job change occurred in 1967. The same problem affects the initial observation on persons who enter the sample in other years. In practice, we are able to infer tenure in most cases.

4, with those in column (2C), row 6 leaves a difference of .056 in the effect of timing that cannot be accounted for simply by mismeasurement of average tenure. The corresponding value for 2SFD is .048.

In columns (5C)–(8C) of Panel B we report estimates using year dummies to control for secular wage growth and $\widetilde{YD}_{it}$ as instruments for the dummies. The IV-Tenure Exogenous, IV1, and 2SFD estimates of the effect of 10 years of "dating-corrected" tenure on wages are .297, .039, and .136, respectively. A comparison of the dating-corrected estimates in row 4 of Panel B with those in row 6 of Panel C suggests that the IV-Tenure exogenous estimate is actually larger using AS's dating procedure than using Topel's, while the IV1 and 2SFD fall by between .02 and .04.

In Panel D we report estimates for a sample in which we (a) use information on the survey date to create a measure of average tenure over the year, (b) use the tenure and date of survey information to eliminate 2,046 observations that involve jobs that began between January 1 and the date of the survey, on the ground that these could be a mixture of earnings from different jobs, and (c) eliminate 700 observations that correspond to the last wage observation, on the ground that they could be mixtures across jobs.[21] Eliminating last observations comes at the cost of eliminating a number of wage growth observations on short jobs. The results are similar to those in Panel C.

As should be clear by now, there is no perfect solution to the problem posed by the fact that the average hourly wage over the year may be a mixture from different jobs. For this reason, in Table 5 we report results using WAGE, an alternative wage measure that refers to the same point in time as the tenure measure. WAGE is the log of the reported hourly wage at the survey date for persons paid by the hour and is based on the salary per week, per month, or per year reported by salary workers. Observations with $T_{ijt} < 1$ are included because the reported wage refers to the job held at the survey, which is when tenure is measured. WAGE is unavailable prior to 1970 and is limited to hourly workers prior to 1976.[22]

When we use year dummies with the instruments $\widetilde{YD}_{it}$ to account for secular change (columns 4–6), the IV1 estimate of the effect of ten years of tenure is .0402, which is almost exactly the same as the dating-corrected estimate based on AS's dating convention in Table 3, Panel C, column (6C). The 2SFD estimate is .0995, which is below the dating-corrected estimate based on Topel's dating convention as well as the corrected estimate of .1359 based on AS's convention.

In sum, the results suggest that adjusting the tenure measures so that they correspond to the middle of the year in which the wage is measured clearly is desirable and reduces the discrepancy between Topel's and AS's dating convention. Once this is done and we control for secular wage growth using year dummies with instruments, the IV1 results fall in the narrow range of .012 to .055 (Table 3, columns 6B, 6C, and 6D) and are close to the estimate of .04 in Table 5. The IV1 estimator uniformly suggests a small effect of tenure on wages. The 2SFD estimates are much more sensitive to the dating of tenure and wages as measured by EARN, although regardless of the procedure used they are far below the OLS estimates.

5. Measurement Error in Tenure

As noted earlier, Topel considered measurement error in the AS tenure measure

[21]The results are very similar if we simply add .25 to the tenure measure rather than use the survey date information to adjust tenure at the survey date to the average value.

[22]We account for the fact that it is capped at $9.98 per hour prior to 1978 by replacing capped values for the years 1970–77 with predicted values based on a regression of the log of the reported wage on a constant and EARN. The regression is estimated using the sample of individuals in 1978 for whom the reported wage exceeds $9.98.

Table 5. Log Hourly Wage Regressions, PSID Topel Replication Sample, 1968–1983. (Dependent Variable: WAGE)

	Wage Year Dummies Endogenous Year t Wage, Year t Tenure T < 1 Included		
Tenure or Experience	*IV, Tenure Exogenous (1)*	*IV1 (2)*	*2SFD (3)*
2 Years of Tenure	0.0652 (0.0109)	0.0279 (0.0079)	0.0459 (0.0101)
10 Years of Tenure	0.2038 (0.0228)	0.0402 (0.0223)	0.0995 (0.0238)
20 Years of Tenure	0.2580 (0.0295)	0.0290 (0.0421)	0.0875 (0.0306)
10 Years of Experience	0.2603 (0.0416)	0.3185 (0.0427)	0.2823 (0.0563)
30 Years of Experience	0.3793 (0.0448)	0.5349 (0.0508)	0.3341 (0.0548)

Notes: Standard errors in parentheses—see Table 2. The specification is as described in the notes to Table 2. The mean of WAGE is 1.41 and the mean of ΔWAGE is 0.015. The first stage of the 2SFD estimator uses 6,309 within-job first differences, while the IV with tenure exogenous, IV1, and second stage of the 2SFD estimator use 8,872 observations.

to be a major factor leading to the differences between the conclusions of the two studies. In his Table 6, Topel reported that the estimated effect of tenure rose from .074 at ten years of tenure to .122 when he used the IV1 estimator with an exogenous time trend and replaced AS's tenure measure with his tenure measure, a ratio of .61. The difference at 20 years of tenure is .052 versus .161. AS performed several experiments to check on the seriousness of error in their tenure measure.[23] They found that eliminating the effects of bracketing of tenure values in the early years and unusual changes in tenure or smoothing the tenure variable increased their basic estimate from .027 to about .044, also a ratio of .61. They concluded on the basis of these calculations and other results that measurement error was important but had little effect on their substantive conclusions.[24]

AS's and Topel's results with regard to the effects of measurement error are easily reconciled. As the numbers provided above suggest, the bias from measurement error is similar in the two samples in percentage terms. The difference between the studies in the absolute magnitude of the bias from

[23]AS used a complicated procedure to identify job matches and separations and to measure T. They included programming checks to make sure that tenure refers to the employer rather than the job and to try to guard against cases in which persons left an employer and then returned. However, for their main results they did not smooth tenure during the years 1968–74 when it is bracketed and did not require tenure to increase by 1 for each year on the job. These were bad decisions. See Brown and Light (1992) on the quality of the PSID tenure data.

[24]As a final check on measurement error bias and as a general specification test, they computed the mean of predicted wage growth by level of tenure in the previous year for job stayers, quits, and layoffs (using the cell means of tenure) and compared the predictions to actual growth. Random measurement error in tenure should have little effect on these sample means. The IV1 estimator performs well in these prediction tests. The OLS estimator performs miserably, with a pattern of errors that suggests that the OLS estimator has a strong positive bias.

measurement error reflects the fact that Topel's use of period t tenure information with period $t-1$ wage information led to a larger value in the IV1 estimator. A similar bias in percentage terms led to a larger bias in absolute terms.[25]

6. Differences in the Estimators

Our results show that when the trend in the PSID is handled using year dummies, and earnings and tenure are taken from the same year, 2SFD is consistently larger than the IV1 estimator, although both are much smaller than OLS. In the replication sample, the gap between IV1 and the 2SFD estimator is about .10 at ten years of tenure and .15 at twenty years of tenure (not reported). As discussed in Sections 2.3 and 2.4, both the IV1 and 2SFD estimators are biased downward by job match heterogeneity, and the 2SFD estimator is biased upward by individual heterogeneity. In this section we investigate the relative importance of these biases in the two estimators.

As noted above, the downward bias from correlation with the job component ϕ_{ij} is likely to be larger for IV1 than for the 2SFD estimator. The IV1* estimator is an attempt to correct IV1 for the effects of ϕ_{ij} under alternative assumptions about the gain from quits. We follow AS and compute the IV1* estimator, which incorporates a crude adjustment for the correlation between X_{ijt} and ϕ_{ij}. We assume a job match gain per quit of .05, which is consistent with AS's data. Because we ignore layoffs, which tend to reduce the growth in ϕ_{ij} with X_{ijt}, we overstate the relationship between ϕ_{ij} with X_{ijt} and overstate the size of the bias. The IV1* estimate of the effect of ten years of tenure is .12, while the comparable IV1 and 2SFD estimates are .039 and .136 (Table 3, Panel C, columns 6C and 7C). We conclude that the IV1 estimator is biased downward by job match heterogeneity, although we wish to stress that the adjustment in the IV1* estimator is not rigorous.

As noted in Section 1.4, the 2SFD estimator is also biased downward by job match heterogeneity, although to a slightly lesser degree than the IV1 estimator. It is biased upward by individual heterogeneity. In Section 1.4 we showed that this upward bias in the 2SFD estimator is about 2/3 of the bias in tenure in the OLS or IV-exogenous tenure estimators. If the upward bias is small, then the 2SFD estimator might be more accurate than IV1. As noted previously, Topel investigated this issue by instrumenting $X0$ with X in the second step of his estimator, and concluded that unobserved individual heterogeneity did not substantially bias his estimates upward.

In columns (4) and (8) of all panels of Table 3 we revisit this issue and use X as an instrument for $X0$ for alternative treatments of the time trend and the dating of wages and tenure. In column (4A), we replicate Topel's result showing that this instrumentation seems to have little effect on his estimates. However, this result only holds when we use EARN_MW68, year $t-1$ wages, and year t tenure. When we simply use the more appropriate EARN_MW67 in the same sample, the dating-corrected estimate of the value of ten years of tenure is .0881 (row 4, column 4B). This is substantially below the estimate for the 2SFD estimator (with $X0$ exogenous) of .181 in column (3B), and even below the IV1 estimate of .157. This strongly suggests that the correlation between μ_i and $X0$ leads to substantial upward bias in 2SFD. Topel's conclusion that unobserved individual heterogeneity is relatively unimportant does not hold even with his timing convention and detrending procedure once the correct

[25]We also re-analyzed the issue of measurement error using the AS sample after imposing the constraint that tenure within a job increases by 1 per year and eliminating (a) all jobs in which the implied starting value of tenure is negative and (b) all jobs that start in the sample and have an implied starting value of tenure greater than 1.25. In this data set we find that the absolute value of the effect of measurement error on IV1 rises when one uses a treatment of the time trend or the dating of wages and tenure that leads to a large IV1 estimate. For example, when we estimate that model with EARN_MW68 rather than a time trend in the AS sample, the estimated effect of 10 years of tenure is .0936 using AS's tenure measure and .172 using the smoothed tenure measure.

years of the Murphy-Welch index are used.

When we use the dating convention of AS, the effect of 10 years of tenure is –.003 (column 4C), which compares to .133 (column 3C) using the 2SFD estimator with $X0$ exogenous. When we replace EARN_MW67 with EARN, use endogenous year dummies, and instrument $X0$ with X, the effect of ten years of tenure is –.0004, which compares to .136 using the 2SFD with $X0$ treated as exogenous. In both cases, these effects lie below the IV1 estimates. Bias from individual heterogeneity appears to be important in the PSID.[26]

Taken together, the evidence suggests that the downward bias in the 2SFD estimator from job match heterogeneity is more than offset by an upward bias from individual heterogeneity. However, the difference between the IV1 and IV1* estimators and the 2SFD estimator is small compared to the difference between these estimators and OLS.

7. Conclusions about AS's and Topel's Analyses

The three main sources of the difference between AS's and Topel's results are the detrending procedure, the choice about dating of tenure and wages, and the estimators. Most of the difference due to the detrending procedure disappears when one uses the correct years for the MW wage index and takes into account the effect of controlling for union and marital status on the trend. Regarding the estimators, 2SFD leads to higher estimates in the replication sample. When the trend in the PSID is properly accounted for, Topel's testing procedure suggests that the upward bias from individual heterogeneity in his estimator is substantial. On the other hand, job match heterogeneity imparts more of a downward bias to the IV1 estimator than to the 2SFD estimator. Taken together, these considerations suggest that the effect of 10 years of tenure lies above the IV1 estimate, but not as high as the 2SFD results. We think the weight of the evidence for the samples and wage data used by AS and Topel points to an intermediate value for the effect of ten years of tenure of perhaps .11. This is above AS's IV1* estimate of .066 but far below Topel's results. However, our results in Table 5, column (5) and (6) for the reported wage measure at the survey date, which is preferred to average hourly earnings over the year, imply a return to 10 years of tenure of only about .06.

8. Topel's Analysis of the Abraham and Farber Estimator

Abraham and Farber (1987) added an estimate of the expected completed tenure T^*_{ij} of each job to the basic wage equation in (1.1) as a control for heterogeneity and obtained estimates of the return to tenure by estimating the equation

$$W_{ijt} = \beta_0 t + \beta_1 X_{0ijt} + (\beta_1 + \beta_2)\, T_{ijt} + \psi T^*_{ij} + \varepsilon^1_{ijt}. \tag{8.1}$$

OLS estimation of (8.1) leads to estimates of the returns to seniority well below estimates that exclude T^*_{ij}. Partly on the basis of this evidence, AF concluded that the returns to tenure are relatively small. Topel noted that (8.1) is equivalent to estimating

$$W_{ijt} = \beta_0 t + \beta_1 X_{0ijt} + (\beta_1 + \beta_2)\,(T_{ijt} - \overline{T}_{ij}) + (\beta_1 + \beta_2)\,(\overline{T}_{ij}) + \psi T^*_{ij} + \varepsilon^1_{ijt} \tag{8.2}$$

by OLS with the coefficients on $\overline{T}_{ij}$ and $T_{ijt} - \overline{T}_{ij}$ restricted to be the same, where $\overline{T}_{ij}$ is the average observed tenure on job j. He noted that $\overline{T}_{ij}$ may be correlated with the unobservables conditional on T^* in a sample that includes incomplete longitudinal histories. Imposing the parameter restrictions implicit in (8.1) on (8.2) will lead this bias to be transmitted to the coefficient on $T_{ijt} - \overline{T}_{ij}$. When Topel estimated (8.2), he

[26]In AW (1997), Table A4, we explored whether the fact that IV1 uses deviations from means of tenure and 2SFD uses first differences explains the larger IV1 results and concluded that it does not. As Topel noted, consideration of the effects of the time-varying error component η_{ijt} might lead one to prefer the use of first differences, particularly if it is a random walk. On the other hand, the use of deviations from job means might be less sensitive to minor misdating of job start and end dates.

strongly rejected the restrictions implicit in (8.1) and obtained estimates of the tenure parameter that were similar to the OLS value he obtained when using the linear specification of the tenure effect. Note that the OLS estimate of .138 he reported in his Table 7 for the linear specification is far below the OLS estimate of .302 that he reported in his Table 3 when he used a 4^{th}-order polynomial for tenure.

As it turns out, Topel's findings are somewhat sensitive to his use of the period $t-1$ wage with period t tenure. More important, when one relaxes the linear specification of the tenure effect in (8.1) and (8.2) by using parameter estimates from a within-job wage growth equation to remove the nonlinear tenure and experience effects prior to estimation of these two equations, the OLS estimates rise dramatically, but the estimates of the tenure effect based on (8.2) rise only slightly. The use of an estimate of job duration to control for heterogeneity yields an estimate of the return to ten years of tenure between .06 and .13 even if one uses the unrestricted version of AF estimator that Topel advocated. These values are between 1/4 and 1/2 of the OLS estimates for the Topel's 4^{th}-order polynomial specification. The details are in AW (1997).

9. Results for a More Recent Sample

Given dramatic changes in the returns to schooling and experience documented by Murphy and Welch (1992) and others, it is likely that the returns to tenure have changed. Furthermore, the quality of the tenure data and the data on the survey wage (WAGE) is better after 1975, and there are further improvements in the tenure data in the 1980s. Finally, the growth of temporary employment and contract work and reductions in job security suggest a change in the nature of the employment relationship that may have led to a change both in the return to tenure and in consequences of individual heterogeneity and job match heterogeneity. For all of these reasons, we examine a more recent period.

In Table 6 we present estimates for calendar years 1975–82 (panel A), 1975–87 (panel B), 1988–2001 (Panel C), and 1975–2001 (panel D).[27] In columns (1)–(3) of Table 6 we present results for the various estimators using WAGE as the dependent variable for the two sample periods. The specifications are the same as those in Table 5, and all use $\widetilde{YD}_{it}$ as instruments for YD_t.

The IV-Tenure Exogenous estimates that only use these instruments in column (1) suggest little change, as the return to ten years of tenure is .262 for 1975–82, .285 for 1975–87, and .268 for 1988–2001. In contrast, the IV1 estimate of the value of ten years of tenure in column (2) is –.003 and statistically insignificant in the first period, .097 for 1975–87, and .061 for 1988–2001. As noted earlier, these estimates are probably downward-biased by job shopping. They are consistent with a modest increase in the return to tenure in the early to mid-1980s, perhaps followed by a small decrease in the 1990s, although the difference between 1975–87 and 1988–2001 is not statistically significant.

The 2SFD estimates of the effect of ten years of tenure in column (3) are .145 for 1975–87 and .150 for 1988–2001. These estimates are larger than the estimate of .100 in Table 5, column (6) that uses WAGE in the replication sample for 1975–82. However, the 2SFD estimates are biased upward by individual heterogeneity. We take a value of .09, which lies between the IV1 and 2SFD estimates for 1988–2001, as our point estimate of the return to ten years of tenure based on this wage measure for the most recent period.

[27]We used extensions of the algorithms developed in Altonji and Williams (1997) and Devereux (1996) to create the experience and tenure measures. Our computer programs and data are available on request. Complications arise from the institution of biannual surveys that began in 1997. From 1975 to 1996, both the WAGE sample and the EARN samples use data that originated from the same calendar year. This continues in 1997, 1999, and 2001 for the WAGE sample. In the EARN sample, earnings refer to the previous calendar year, and so the post-1996 observations refer to 1998 and 2000. The tenure and experience are adjusted to account for this fact.

Table 6. OLS, IV1, and 2SFD for PSID 1975–1982, 1975–1987, 1988–2001, and 1975–2001.

	Dependent Variable: Wage			*Dependent Variable: Earn*		
	IV, Tenure Exogenous	*IV1*	*2SFD*	*IV, Tenure Exogenous*	*IV1*	*2SFD*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Panel A: 1975–1982						
10 Years of Tenure	0.2617	−0.0028	0.1064	0.3778	0.0421	0.2131
	(0.0251)	(0.0376)	(0.0309)	(0.0280)	(0.0357)	(0.0379)
30 Years of Experience	0.4562	0.7527	0.4549	0.4184	0.7527	0.3997
	(0.0527)	(0.0879)	(0.0611)	(0.0578)	(0.0889)	(0.0734)
Panel B: 1975–1987						
10 Years of Tenure	0.2854	0.0965	0.1446	0.3942	0.1174	0.2228
	(0.0216)	(0.0245)	(0.0245)	(0.0244)	(0.0253)	(0.0306)
30 Years of Experience	0.5076	0.5911	0.4708	0.4950	0.6615	0.4482
	(0.0454)	(0.0582)	(0.0524)	(0.0494)	(0.0614)	(0.0610)
Panel C: 1988–2001						
10 Years of Tenure	0.2675	0.0610	0.1502	0.3877	0.1172	0.2711
	(0.0244)	(0.0219)	(0.0200)	(0.0261)	(0.0254)	(0.0332)
30 Years of Experience	0.4147	0.6039	0.4068	0.4465	0.6846	0.3140
	(0.0634)	(0.0686)	(0.0644)	(0.0655)	(0.0743)	(0.0862)
Panel D: 1975–2001						
10 Years of Tenure	0.2787	0.1121	0.1461	0.3920	0.1583	0.2466
	(0.0173)	(0.0163)	(0.0157)	(0.0188)	(0.0180)	(0.0230)
30 Years of Experience	0.4638	0.6063	0.4657	0.4528	0.6542	0.4130
	(0.0397)	(0.0423)	(0.0413)	(0.0409)	(0.0451)	(0.0512)

Notes: Standard errors are in parentheses—see Table 2. The specification is as described in the notes to Table 2, except that the eight Census region dummies have been replaced by three area dummies. In all panels, the dependent variable is Wage in columns (1)–(3), and Earn in columns (4)–(6). All panels include year dummies that have been instrumented using deviations from individual means for each year dummy. The Wage samples for 1975–1982, 1975–1987, 1988–2001, and 1975–2001 contain 5,833, 10,369, 11,262, and 21,631 observations, respectively. The samples for the first stage of the Topel estimator in column (3) contain 3,825, 6,995, 7,535, and 15,174 wage change observations. The Earn samples for 1975–1982, 1975–1987, 1988–2001, and 1975–2001 contain 6,830, 11,819, 10,683, and 22,501 observations. The samples for the first stage of the Topel estimator in column (6) contain 4,731, 8,355, 6,749, and 15,762 wage change observations. The sample periods are the calendar year that the wage and earnings data refer to, rather than the years of the PSID survey. For example, the data on Earn in 1991 are from the 1992 survey, and the data from Wage in 1991 are from the 1991 survey.

Using Earn as the dependent variable in columns (4)–(6), we find larger tenure effects. It should be kept in mind that data on Earn from the 2001 PSID wave refer to 2000. When tenure is treated as exogenous (column 4), the estimate of the effect of ten years of tenure is about .39 and varies little across periods. The IV1 estimate rises from .042 (.036) for 1975–82 to .117 (.025) for both 1975–87 and 1988–2001. The 2SFD estimate rises from .213 (.038) to .223 (.031) for 1975–87 and to .271 (.033) for the later period. We would take .16 or .17 as our preferred estimate-based Earn for 1988–2001. As we have already discussed, these estimates are subject to the problem that Earn may be a mixture of wages from two jobs, and the results based on the survey wage rate should be preferred *a priori*.[28]

[28]There are minor differences in the samples for the two wage measures that stem primarily from the

There is a large literature showing that much of the rise in inequality has been within education/experience cells, and there is some evidence that the returns to aptitude and achievement measures have risen over time.[29] There is also evidence that the relationship between unobserved personal characteristics and wages has changed. This would imply an increase in the market price on the individual heterogeneity component μ_i, the variance in the distribution of μ_i, or both. Secular increases or decreases in the variance of the job match heterogeneity component or its relationship with experience and tenure will affect all three estimators.

One way to isolate the change in the return to tenure and experience from the above changes is to compute the combined return to ten years of experience and tenure using the within-job wage change equation (1.8) for different periods because these estimates are not affected by μ_i.[30] We use the estimated year dummies obtained from the IV-Exogenous tenure estimator to remove the effect of secular growth. The within-job estimates for WAGE imply a combined return to ten years of tenure and experience of .480 (.052) for 1975–82, .504 (.041) for 1975–87, and .474 (.057) for 1988–2001. The corresponding results for EARN are .506 (.063), .593 (.052), and .533 (.085). Thus, the point estimates suggest that the sum of the effects of tenure and experience increased during the early to mid-1980s and then declined, but the changes are small relative to standard errors.

10. Conclusion

Our main conclusion is that the data used by both AS and Topel imply a return to ten years of tenure of about .11, which is much closer to AS's preferred estimate of .066 and to AF's results than to Topel's estimate of .245 or the OLS estimates. Use of the survey wage rate in place of average hourly earnings leads one to revise this estimate back downward to about .06. The OLS-type estimators lead to a large overestimate of the return to tenure and should not be used.

Broad changes in earnings distributions and in the employment relationship motivate our examination of more recent data. Perhaps surprisingly, the return to tenure is probably larger over the 1975–87 and 1988–2001 periods than over the period analyzed by earlier studies, but our estimate of the size of the return is sensitive to the choice of wage measure. For the later periods we are able to perform a more complete analysis using the survey wage rate, which is the preferred wage measure because it refers to a point in time and thus does not average wages across jobs. Focusing on 1988–2001, using this measure we obtain a return of .061 using IV1, which is probably downward-biased, and a return of .150 using 2SFD, which is probably upward-biased. We would choose an intermediate value of .09 as our estimate based on this wage measure. We would revise this estimate upward to perhaps .14 because the IV1 and 2SFD estimates are both substantially higher for the average hourly earnings measure of wages, although there are problems with that measure. This value is far below estimates that treat

fact that persons must be employed or on temporary layoff to have a survey wage. These differences do not appear to explain the larger tenure estimates using EARN.

The IV1 estimator produces larger estimates for the 1975–2001 period than for either sub-period and is not simply an average of the results for the two sub-periods. When we pool the observations across sub-periods but construct the values of DT_{ijt} separately for each sub-period, we obtain results that are intermediate between those for the sub-periods. Consequently, the IV1 estimator puts more weight on long jobs when we use the 1975–2001 period. This raises the possibility that there is heterogeneity in the tenure slope or the experience slope (or both), and persons who move less often have higher slopes.

[29]See the surveys by Levy and Murnane (1992) and Katz and Autor (1999).

[30]We normalize μ_i to have a mean of 0 in the sample, but if the mean of μ_i is higher for stayers than movers, an increase in the factor loading relating wages to μ_i could lead to an increase in the estimate of within-job wage growth that has nothing to do with the return to experience or seniority.

tenure as exogenous, and it is on the high side for most recent work on the return to seniority.[31]

[31]See Carrington (1993), Neal (1995), and Parent (2000) for evidence showing only a modest link between tenure and job losses from dislocation. See Altonji and Williams (1998), Abowd, Kramarz, and Margolis (1999) (French data), Lillard (1999), Parent (2000), and Abowd and Chang (2002) for other evidence of a small return to seniority. Bouchinsky et al. (2001) and Dustman and Meghir (2003) (German data) are exceptions.

Appendix

The Effect of Bias in the Time Trend on the IV1 and 2SFD Estimators

For simplicity, treat year-to-year variation in the aggregate wage change as part of the error and focus on the linear trend component. Redefine the wage measure W_{ijt} as the real wage net of the Murphy-Welch wage index, and rewrite (1.1) as

$$W_{ijt} = \beta_0^* t + \beta_1 X_{ijt} + \beta_2 T_{ijt} + \varepsilon_{ijt},$$

where β_0^* is the difference between the actual time trend (β_0) and the coefficient ($\hat{\beta}_0$) from a regression of the Murphy-Welch wage index on t. In the linear case the IV1 estimators of $\beta = \beta_1 + \beta_2$ and β_1 are

$$\hat{\beta}^{IV1} = (DT'DT)^{-1}\, DT'DW \quad \text{and} \quad \hat{\beta}_1^{IV1} = (X'X0)^{-1}X'(Y - T\hat{\beta}^{IV1}),$$

where we use obvious matrix notation and DT and DW are vectors of deviations from job means of T_{ijt} and W_{ijt}. From the first equation it is easy to show that

$$plim\,\hat{\beta}^{IV1} = plim\,\hat{\beta}^{2SFD} = \beta_1 + \beta_2 + \beta_0^*.^{32}$$

From the second equation it is easy to show that bias in the experience coefficient is

$$plim\,\hat{\beta}_1^{IV1} - \beta_1 = b_1 + \frac{\gamma_{XT}}{1-\gamma_{XT}}\,[b_1 + b_2] - \frac{\gamma_{XT}}{1-\gamma_{XT}}\,[\beta_0^*] + \frac{\gamma_{Xt}}{1-\gamma_{XT}}\,\beta_0^*,$$

where γ_{Xt} is the coefficient in the auxiliary regression of t on X_{ijt}. Since $\hat{\beta}_2^{IV1} = \hat{\beta}^{IV1} - \hat{\beta}_1^{IV1}$, the bias in the tenure coefficient contributed by the terms involving β_0^* is

$$\beta_0^* + \frac{\gamma_{XT}}{1-\gamma_{XT}}\,[\beta_0^*] - \frac{\gamma_{Xt}}{1-\gamma_{XT}}\,\beta_0^*.$$

We can evaluate the bias from using the wrong time trend by substituting the estimates γ_{XT}, γ_{Xt}, and β_0^* obtained from using the replication sample. In that sample, γ_{XT} is about .56 and γ_{Xt} is only –.0024, so the bias implied by the above expression is about $-2.27\beta_0^*$.

A similar analysis of the 2SFD estimator implies that using the wrong time trend biases β_2 by

$$\beta_0^* + \frac{\gamma_{X0T}}{1-\gamma_{X0T}}\,[\beta_0^*] - \gamma_{X0tij}\,\beta_0^*.$$

[32]The expression for $\hat{\beta}^{IV1}$ is simply the mean wage growth within jobs after adjusting for the Murphy-Welch trend estimate. This is $\beta_1 + \beta_2 + (\beta_0 - \hat{\beta}_0)$.

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