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Do Employers Provide Insurance against Low Frequency Shocks? Industry Employment and Industry Wages

Paul J. Devereux, *University of California, Los Angeles*

I use panel data to examine whether long-term changes in industry wages are positively related to long-term changes in industry employment. Previous research using repeated cross-sectional data found no systematic relationship between these variables. Using standard fixed effects models to deal with individual heterogeneity, I find a robust positive relationship between changes in composition-constant industry wages and industry employment. This suggests that growing industries attract less skilled individuals in a manner that biases down the estimated relationship between industry employment and wages in repeated cross-sectional data. The results imply that supply curves facing industries are elastic but upward sloping.

I. Introduction

Much economic research has focused on how wages respond to short-term business cycle shocks. However, there is very little research about the labor market response of wages to low frequency, persistent shocks that affect the demand for labor. A good example of these kinds of shocks

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is demand shocks that have led to structural declines in many manufacturing industries over the last few decades. The goal of this research is to examine how wages within industries respond to low frequency shocks to the demand for labor across industries.

The empirical evidence in the literature suggests that wages are rigid with respect to changes in industry demand.¹ Helwege (1992) finds that the interindustry wage structure is stable despite extremely heterogeneous growth rates across industries. Furthermore, recent work by Weinberg (2001) suggests that there is no systematic relationship between industry wages and low frequency changes in industry demand for labor. He uses repeated cross-sections of the Current Population Survey (CPS) and finds no robust relationship between changes in industry employment and changes in industry wages over the 1971–81 and 1981–91 periods. Weinberg's result implies that the long-run supply curve of effective industry-specific skill is relatively flat for most industries.²

On the other hand, there is a lot of evidence that industry-specific skills are an important feature of the labor market and industry growth and decline does affect the value of human capital.³ This suggests a puzzle: if industry-specific human capital or industry matching creates barriers to interindustry mobility, why would industry wages not respond to changes in the value of workers' human capital? If individuals have portfolios of industry-specific skills, a simple Roy model (Roy 1951) implies that industries face upward-sloping labor supply curves. This occurs because growing industries must induce workers to leave other industries by offering higher wages. Thus, at least in this Roy framework, it is difficult to reconcile industry-specific skills being important with a horizontal industry labor supply curve. One requires some form of wage rigidity (both between and within employment matches) in order to reconcile these two results. The conclusion of this article is that reconciliation is unnecessary because industry wages do respond to industry-level shocks to labor demand.

Previous research using repeated cross-sections cannot satisfactorily deal with unobserved individual heterogeneity or any individual-specific industry match effect. In this article, I use panel data from the 1971–2001 interviewing years of the Panel Study of Income Dynamics (PSID) that

¹ However, Shaw (1989) shows that, during the late 1970s, wages are higher for individuals in industries with greater deviations of employment growth rates from trend growth.

² He concludes that there are either few barriers to mobility between industries or that wages are rigid and employers insure workers against low frequency shocks.

³ Carrington (1993), Neal (1995), Parent (2000), and Weinberg (2001) all provide evidence that suggests that industry-specific human capital is an important feature of labor markets.

allow the use of standard fixed effects methods to condition out these forms of heterogeneity.

In their study of cyclical upgrading, McLaughlin and Bills (2001) show that most industries are characterized by positive selection and that typically the worst workers in declining industries move and become the worst workers in growing industries. This would imply that the average quality of workers increases as industries decline and decreases as industries grow. This type of selection would tend to bias down any positive relationship between changes in industry wages and changes in industry employment. To the extent that ability is fixed, the addition of individual fixed effects using panel data can deal with this issue.⁴

However, composition changes may also be related to industry-specific skills. In a simple Roy model, when employment expands in an industry, workers enter this industry from other industries. These entrants were optimally in a different industry previously, and so it is likely that their industry-specific skills in the industry they enter are relatively low (at least compared to incumbent workers). Thus we would expect that new entrants will be less productive than existing workers in an industry and will earn a lower wage. I deal with these industry-specific skills by using panel data and adding individual-industry fixed effects.

The potential importance of composition bias is clear when one considers the two primary sources of industry labor demand shocks—product demand shocks and technological change. If either is skill biased, industry growth or decline may be accompanied by a large change in the skill mix within the industry. While the fixed effects approaches used in this article are an advance on the literature, they will not deal with all forms of composition bias. For example, if skill-biased technological change is accompanied by intensive firm-provided training that increases industry-specific skills, this will violate the fixed ability assumption implicit in the fixed effects formulation. Unfortunately, this sort of issue is impossible to address in the absence of detailed data on training.

I find that, for the most part, composition changes bias down the correlation between industry employment changes and industry wage changes. This implies that when industries are expanding, they tend to attract individuals who are of lower quality than the average worker in that industry. The composition-constant estimates suggest that during the 1980s and 1990s industry wages are positively related to industry growth

⁴ This is related to the composition bias that affects measures of the responsiveness of wages to the business cycle. In that literature, the problem is the composition of individuals who work changes over the business cycle. Solon et al. (1994) show that the measured cyclical responsiveness of wages to the business cycle is increased substantially when individual fixed effects are added. In the current context, the problem is that the types of workers in an industry may change as it grows or declines.

rates. During the 1970s, there is no evidence of a positive relationship between industry growth rates and industry wage changes. However, when changes in industry employment are adjusted to take account of shocks to labor supply that arise owing to changes in the demographic structure of the labor market, the estimates are quite similar across decades and suggest a robust positive relationship. The results are consistent with a model in which workers have industry-specific human capital that is reduced in value when there are persistent negative shocks to labor demand in their industry. They also imply that industry labor supply curves are elastic but upward sloping.

The structure of this article is as follows: In the next section, I describe the econometric strategy that exploits the individual-level panel data from the PSID to deal with composition bias. In Section III, I describe the data, and Section IV contains the main empirical results. In Section V, I reconcile the results to others in the literature, and Section VI examines issues of simultaneity. Section VII summarizes the results and concludes.

II. Econometric Strategy

The Basic Estimator

I follow the literature in using persistent changes in industry employment to proxy for shocks to labor demand. Later, in Section VI, I argue that the available evidence suggests that shifts in labor supply are unlikely to be an important determinant of industry employment changes. Also in Section VI, I present estimates in which efforts have been made to correct industry employment changes to take account of labor supply shifts.

The goal of the empirical work is to examine how the industry wage rate varies in response to low frequency, persistent changes in industry employment. The estimation is carried out in two steps using individual-level panel data from the PSID. In the first step, the micro data are used to construct a composition-constant measure of the average wage rate in industry j at time t . In the second step, changes in this composition-constant industry wage rate are related to employment changes in the industry.

First Stage

In the first step, the estimation equation is as follows:

$$w_{ijt} = \beta_1 + \beta_2 x_{it} + \phi_{jt} + f_i + \theta_{ij} + \varepsilon_{ijt}. \quad (1)$$

The log wage of an individual i in industry j at time t (w_{ijt}) is modeled as a function of personal characteristics (x_{it}), indicator variables for the

industry-year cell (ϕ_{jt}), and an error term.⁵ The x_{it} variables included are a cubic in actual labor market experience, indicators for high school graduate and college graduate, interactions of all these variables with each other, and an indicator for whether the individual is white. The equations are estimated separately for women and for men and are estimated separately for each time period under study. The error term has a permanent individual-specific component (f_i), an individual-industry match component (θ_{ij}), and an idiosyncratic component (ε_{ijt}). Note that because I use panel data, I can include individual fixed effects (f_i) in equation (1). These individual fixed effects control for permanent unobserved individual attributes that affect wages. Furthermore, I can go further and include individual-industry fixed effects (θ_{ij}) in equation (1). These condition out all fixed individual-specific factors and also any fixed individual-industry match component (such as industry-specific skills). The coefficients on the industry-year dummies, $\hat{\phi}_{jt}$, are stored and used in the second-stage regressions. These coefficients can be interpreted as composition-constant average wage rates for each industry-year cell given that individual fixed effects (or individual-industry fixed effects) and time-varying individual characteristics have been conditioned out in equation (1).

Second Stage

The second-stage regressions are carried out at the industry-year level. That is, if there are T time periods, and J industries, there are $T \times J$ observations in the second-stage analysis. To account for permanent differences in wages and employment across industries, I use both long-difference and fixed effects estimators in the second-stage analysis. The fixed effects estimators include controls for both industry and year dummies.

Long-difference estimator.—When using the long distance estimator, equation (1) is first estimated. Then, the second step is carried out in long differences (10-year differences, to be precise):

$$\hat{\phi}_{jt+10} - \hat{\phi}_{jt} = \delta_1 + \delta_2(E_{jt+10} - E_{jt}) + u_{jt}. \quad (2)$$

In equation (2), E_{jt} refers to log industry employment at time t , and u_{jt} is an error term. Thus, the differenced variable approximates the percentage change in employment. Each second step observation is weighted by the average number of individuals in the industry in the two years. The long differences are used so as to capture secular changes in employment and wages rather than high frequency changes. One disadvantage of long differences is that they use information from only 2 years and hence provide less precise coefficients than estimation methods that

⁵ I have experimented with allowing the coefficients on the x_{it} variables to differ over time. This makes no appreciable difference to the results.

use information from all years in the 10-year period. Therefore, I also estimate fixed effects models that use information from all periods.

Fixed-effects estimator.—The coefficients on the industry-year dummies, $\hat{\phi}_{jt}$ from equation (1), are regressed on log industry employment, a set of year dummies, and a set of industry dummies.

$$\hat{\phi}_{jt} = \delta_1 + \delta_2 E_{jt} + \delta_3 YEAR_t + \delta_4 IND_j + v_{jt}. \quad (3)$$

I weight each second step observation by the number of individual observations in that industry-year. While the fixed effects estimator uses more information than the long-differences estimator, there are a couple of issues that need to be addressed in practice.

The first issue is that the fixed effects estimates capture both the effects of transitory variation in industry employment and the low frequency changes that are the interest of this article. For this reason, I also report estimates where industry employment is replaced by a measure that is stripped of transitory variation. To do so, I fit a set of 5-year splines to log industry employment for each industry over the full 1971–2001 period. The predicted value of log industry employment from this regression is then used as an industry employment measure that is purged of short-term, transitory changes. I refer to this as smoothed log industry employment.⁶

A second issue with the fixed effects estimator is that there is likely to be serial correlation in the error v_{jt} . This could arise because of industry-specific business cycles that imply that wages are more closely correlated in two adjacent periods than in two periods further apart. I deal with this issue by assuming that v_{jt} follows an autoregressive (AR) process. I estimated some preliminary regressions using the residuals in order to evaluate whether an AR(1) process was sufficient. I found that adding a second order lag typically did not increase the explanatory power of the regression (in terms of the adjusted R -squared) and the second order lag was typically statistically insignificant. In contrast, the first order lag was typically of the order of 0.3 and statistically significant. Thus, I have estimated equation (3) by generalized least squares (GLS) under the assumption that v_{jt} follows an AR(1) process.⁷

⁶ I have also tried fitting a quadratic time trend and smoothing using 5- and 3-year moving averages. The results are not very sensitive to the method of smoothing used.

⁷ In the 1991–2001 period, there was no data collection in 1998 and 2000. To preserve equal spacing between periods, I use data from survey years 1991, 1993, 1995, 1997, 1999, and 2001 when using the GLS estimator.

III. The Data

Individual-Level Panel Data

The data set I use in this study is the PSID.⁸ This panel data set contains observations on people of all ages and covers a long time period—my sample runs from 1971 to 2001 (the PSID files from 1994 to 2001 are early release files). I restrict the sample to individuals aged 25–55 who have 6 or more years of actual labor market experience.⁹ I delete all observations in which the individual is self-employed because the focus here is on the decisions being made by employers. I also delete cases in which the individual worked less than 26 weeks in the calendar year so as to restrict the sample to individuals with strong labor market attachment. The reported two-digit industry sometimes changes in cases where the tenure data strongly suggest that no employer change occurred. In these cases (4% of the total for women, 9% of the total for men), I have fixed the industry as being the modal industry during that spell with the employer.¹⁰

There are two different hourly wage measures available in the PSID, and I report results for both.¹¹ Average hourly earnings equal previous calendar-year earnings divided by annual hours and are available for all individuals who work even briefly during the previous year. The current wage is available only for individuals paid hourly or salaried. For hourly workers, the current wage is the hourly wage rate at the interview date. For workers paid weekly, monthly, or annually, the PSID reports a wage rate that is calculated by dividing weekly, monthly, or annual salary by a fixed number of hours. For example, if a worker is paid weekly, the weekly payment is divided by 40 hours irrespective of how many hours the worker works each week. The current wage has the advantage of being specific to the main job the employee works on but excludes income from

⁸ I use only the random subsample of the PSID in the analysis. The results from using the full PSID sample are generally similar.

⁹ The relatively restricted age range is used in order to make it more likely that the fixed effects assumptions are reasonable. Learning models imply that even if ability is fixed, market perceptions of individual ability may change as new information is revealed (Gibbons et al. 2002). The assumption made in this article is that most learning is done in the first 5 years that individuals are in the labor market. Workers aged over 55 are omitted as health changes or partial retirement may imply that their skills are changing over time.

¹⁰ The results are similar if the industry information is left as it was reported by respondents.

¹¹ I deflate all nominal amounts using the personal consumption expenditures deflator for gross domestic product (base year is 1996).

Table 1
Descriptive Statistics (1971–2001)

Variable	Number of Observations	Mean	Standard Deviation	Minimum	Maximum
Women:					
White	32,923	.90	.30	.00	1.00
Years of experience	32,923	15.68	6.95	6.00	42.00
Age	32,923	38.25	8.37	25.00	55.00
Average hourly earnings	32,359	2.43	.54	.92	4.59
Current wage	25,678	2.40	.48	.92	4.59
High school dropout	32,923	.09	.28	.00	1.00
High school graduate	32,923	.59	.49	.00	1.00
College graduate	32,923	.33	.47	.00	1.00
Survey year	32,923	87.73	7.84	71.00	101.00
Industry adjusted	32,923	.04	.21	.00	1.00
Men:					
White	42,559	.92	.27	.00	1.00
Years of experience	42,559	19.00	8.22	6.00	42.00
Age	42,559	37.77	8.37	25.00	55.00
Average hourly earnings	42,213	2.80	.51	.92	4.59
Current wage	34,568	2.76	.48	.92	4.61
High school dropout	42,559	.12	.32	.00	1.00
High school graduate	42,559	.54	.50	.00	1.00
College graduate	42,559	.34	.47	.00	1.00
Survey year	42,559	86.39	8.24	71.00	101.00
Industry adjusted	42,559	.09	.29	.00	1.00

overtime and bonuses.¹² Also, this wage measure is unavailable for wives before 1976 and is available for heads who are not paid hourly only after 1975. Thus, I use only post-1976 data when using the current wage. Descriptive statistics by gender for selected variables are in table 1.

Data on Industry Employment

For the purposes of this study, all workers are assigned to one of 26 industry groupings. In table 2, I list these 26 groupings along with the change in log employment for 1971–81, 1981–91, and 1991–2001. As can be seen in table 2, the growth rate of industry employment differs greatly across industries. For example, between 1981 and 1991, business services grew by approximately 60%, and tobacco manufacturing declined by about 35%. The industry employment data I use comes from establishment surveys and is provided by the Bureau of Labor Statistics (BLS).

¹² A small proportion of both average hourly earnings and current wage observations is topcoded. I impute topcoded values for both of these measures by multiplying the topcoded value by 1.33. To reduce the influence of outliers and measurement error, I delete wage observations that are less than \$2.50 or greater than \$100 in 1997 dollars.

Table 2
Employment Changes by Industry (1971–81, 1981–91, 1991–2001)

Industry Number	Industry	1971–81 ΔE	1981–91 ΔE	1991–2001 ΔE
1	Mining and Extraction	.63	–.50	–.20
2	Metal Industries	.02	–.27	.03
3	Machinery, Including Electrical Motor Vehicles and Other	.26	–.18	.01
4	Transport	.08	.01	–.07
5	Other Durables	.10	–.06	.03
6	Food and Kindred Products	–.06	.00	.01
7	Tobacco Manufacturing	–.09	–.36	–.37
8	Textiles	–.12	–.25	–.49
9	Paper and Allied Products	.00	.01	–.08
	Chemicals, Petroleum, Rubber, and Plastic Products	.14	.00	.00
10	Other Manufacturing	.08	–.09	–.04
11	Construction	.12	.10	.36
12	Transportation	.10	.18	.25
13	Communication	.20	–.07	.28
14	Other Public Utilities	.20	.12	–.12
15	Retail Trade	.29	.24	.20
16	Wholesale Trade	.29	.12	.11
17	Finance, Insurance, and Real Estate	.34	.23	.15
18	Repair Services	.40	.34	.29
19	Business Services	.66	.63	.63
20	Personal Services	–.02	.29	.13
21	Other Services	.45	.39	.35
22	Printing, Publishing, and Al- lied Services	.16	.19	–.03
23	Medical and Health Services	.54	.39	.24
24	Education	.22	.37	.35
25	Government	.22	.14	.13

SOURCE.—Bureau of Labor Statistics.

NOTE.—The employment changes are differences in logs.

IV. Results

The estimates from the regressions in equations (2) and (3) are in table 3 and table 4. Table 3 contains the results for women. Each coefficient in the table refers to a different regression. There are separate regressions for the 1971–81 period, the 1981–91 period, and the 1991–2001 period. Also, I report long-difference estimates and GLS fixed effects estimates with both industry employment and smoothed industry employment. For each estimator, there are three columns; column 1 reports the estimates when only observable characteristics are included in equation (1); column 2 reports results when individual fixed effects are also included; column 3 reports results when the individual fixed effects are replaced with individual-industry fixed effects.

Note that the current wage relates to the interview date, while average hourly earnings refer to the previous calendar year. To time the variables appropriately, average hourly earnings are linked to industry employment

Table 3
Effects of Industry Employment Changes on Industry Wages (Women)

Sample	Long Differences			GLS Employment			GLS Smoothed Employment		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
1991–2001:									
Average hourly earnings	–.104 (.238)	.084 (.119)	.228 (.182)	–.103 (.067)	.148* (.053)	.273* (.071)	–.085 (.069)	.166* (.054)	.306* (.072)
Current wage	.033 (.170)	.112 (.112)	.246 (.190)	–.022 (.064)	.186* (.050)	.295* (.061)	.007 (.069)	.216* (.053)	.338* (.065)
1981–91:									
Average hourly earnings	–.006 (.068)	.153* (.064)	.237* (.113)	.087 (.055)	.163* (.048)	.238* (.047)	.079 (.056)	.166* (.049)	.234* (.048)
Current wage	.056 (.070)	.152* (.075)	.224 (.115)	.137* (.050)	.183* (.038)	.241* (.039)	.146* (.052)	.182* (.039)	.254* (.040)
1971–81:									
Average hourly earnings	.059 (.098)	.033 (.065)	–.075 (.102)	–.002 (.067)	.046 (.058)	.003 (.069)	–.016 (.068)	.037 (.059)	–.025 (.071)
Current wage ^a	–.107 (.187)	.118 (.145)	–.019 (.174)	.136 (.141)	.178 (.099)	.160 (.112)	.054 (.159)	.196 (.111)	.131 (.126)

NOTE.—(1) = controls for individual characteristics (education, experience, race). (2) = controls for individual characteristics plus individual fixed effects. (3) = controls for individual characteristics plus individual-industry fixed effects. The estimates are coefficients on log industry employment from the estimation of eqq. (2) and (3) in the text. Each estimate is from a different regression. Regressions are run at the industry-year level. See text for details and for descriptions of the industry employment variables. Standard errors are in parentheses.

^a 1976–81.

* Significant at the 5% level.

Table 4
Effects of Industry Employment Changes on Industry Wages (Men)

Sample	Long Differences			GLS Employment			GLS Smoothed Employment		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
1991–2001:									
Average hourly earnings	.189 (.137)	.202 (.109)	.177 (.145)	.114* (.053)	.148* (.053)	.131* (.054)	.106* (.055)	.144* (.055)	.128* (.055)
Current wage	.206* (.090)	.214* (.070)	.168 (.094)	.208* (.067)	.211* (.042)	.180* (.043)	.242* (.072)	.256* (.044)	.218* (.046)
1981–91:									
Average hourly earnings	.131 (.083)	.203* (.080)	.248* (.097)	.109* (.046)	.179* (.037)	.223* (.038)	.116* (.048)	.187* (.039)	.250* (.040)
Current wage	.102 (.074)	.263* (.086)	.204* (.087)	.049 (.039)	.210* (.032)	.182* (.038)	.062 (.042)	.232* (.035)	.208* (.041)
1971–81:									
Average hourly earnings	–.084 (.040)	–.162 (.086)	–.042 (.100)	.012 (.060)	–.028 (.054)	.040 (.059)	–.080 (.063)	–.131* (.057)	–.071 (.064)
Current wage ^a	–.262 (.234)	.124 (.133)	–.036 (.157)	–.365* (.108)	.039 (.074)	–.056 (.086)	–.311* (.135)	.090 (.091)	–.058 (.105)

NOTE.—(1) = controls for individual characteristics (education, experience, race). (2) = controls for individual characteristics plus individual fixed effects. (3) = controls for individual characteristics plus individual-industry fixed effects. The estimates are coefficients on log industry employment from the estimation of eqq. (2) and (3) in the text. Each estimate is from a different regression. Regressions are run at the industry-year level. See text for details and for descriptions of the industry employment variables. Standard errors are in parentheses.

^a 1976–81.

* Significant at the 5% level.

in the previous calendar year (where the industry is that in which the individual worked in the previous year).¹³ This implies that, for example, the 1991–2001 results for the current wage are for wage and employment changes between 1991 and 2001; the analogous results for average hourly earnings refer to wage and employment changes between 1990 and 2000.

GLS Estimates

The long-difference estimates are somewhat imprecise so I will discuss the GLS estimates first. First, consider the results for women in table 3. For the 1991–2001 period, the estimates that control only for observed worker heterogeneity are negative and statistically insignificant. In contrast, the estimates that control for individual fixed effects or for industry-individual effects are positive and statistically significant. These estimates suggest a magnitude of around 0.2—a 2% increase in industry wages tends to accompany a 10% increase in industry employment.

For the 1981–91 period, the results are somewhat similar. Evidence for a positive relationship is weak when only observed heterogeneity is controlled for (there is a statistically significant effect for the current wage but not for average hourly earnings). However, once one includes individual or individual-industry fixed effects, the coefficients become larger and statistically significant. Once again, the estimates suggest a magnitude somewhere around 0.2. Given the enormous variance in industry employment growth rates during this period displayed in table 2, these are changes of some economic significance. For the 1971–81 period, the estimates are all statistically insignificant and vary in sign, irrespective of the specification used.

The equivalent results for men are in table 4. For the 1991–2001 period, the estimates for specifications that include individual or individual-industry fixed effects are statistically significant, with magnitudes between 0.1 and 0.3. Unlike the female sample, there is no strong evidence of composition bias here; the estimates do not change much in magnitude with the introduction of the fixed effects. The evidence for composition effects is stronger for the 1981–91 period; for average hourly earnings, coefficients rise from a statistically significant 0.1 without fixed effects to a magnitude of about 0.2 with the fixed effects. For the current wage, the estimate without fixed effects is small and statistically insignificant; with fixed effects it rises to a statistically significant 0.2. Finally, in the 1971–81 period, the estimates with individual or individual-industry fixed effects are generally statistically insignificant and vary in sign.

The GLS estimates using smoothed employment in the final three col-

¹³ Because there were no surveys in 1998 and 2000, previous-year industry is unavailable in 1999 and 2001. For these two years, I set previous-year industry as being the industry at the interview date.

umns of tables 3 and 4 are quite similar to the unsmoothed estimates. This does not imply that the responses of wages to transitory industry shocks are the same as to more persistent shocks. When one includes both smoothed employment and the deviation of employment from smoothed employment in the same specification, the smoothed employment coefficients remain close to those in tables 3 and 4, but the coefficient on the deviation of employment from smoothed employment is almost always statistically insignificant.¹⁴

Overall, the estimates for men and women are quite similar. In both the 1981–91 and 1991–2001 periods, individual fixed effects estimates are about 0.2. For the 1971–81 period, the coefficients vary in sign and generally are statistically insignificant, irrespective of the specification used. Thus, during this decade, one cannot rule out the possibility that wages were not responsive to industry employment. The results in tables 3 and 4 suggest that conditioning out unobserved individual and individual-industry heterogeneity is important and that, for women since 1981 and men in the 1981–91 period, composition bias is an extremely relevant issue.

Long Differences

When one compares the long-difference estimates in tables 3 and 4 to the GLS estimates, the magnitudes of the coefficients are generally very similar. The long-difference estimates are imprecisely estimated as a result of using information from only 2 years of data. However, long differences do allow a simple graphical illustration of the relationship between changes in industry employment and industry wages. Figures 1 and 2 contain plots for men and women respectively using average hourly earnings as the wage measure. In the plots, industries are labeled by the number assigned to the industry in table 2. The plots also contain the regression line fitted to the points.¹⁵ In figure 1, we see that the addition of individual or individual-industry fixed effects leads to a positive relationship between changes in wages and employment for women in the 1980s and 1990s.

¹⁴ For example, for the individual fixed effects specification for average hourly earnings, the estimates of 1981–91 for smoothed employment are 0.169 (0.049) for women and 0.188 (0.039) for men. The accompanying estimates for the deviation of employment from smoothed employment are 0.081 (0.161) and 0.104 (0.105), respectively.

¹⁵ For presentational purposes, unlike in tables 3 and 4, there is no weighting applied in figs. 1 and 2. For this reason, a few industries in each period are omitted because there are fewer than 10 workers on average over the two years, and so the average wage changes are considered extremely unreliable. Note that the unweighted estimates underlying the regression lines are very similar to the weighed estimates in tables 3 and 4.

FIG. 1.—Plots of industry employment changes (X -axis) against industry wage changes (Y -axis) for women

FIG. 2.—Plots of industry employment changes (X -axis) against industry wage changes (Y -axis) for men

For men, in figure 2, the effect of adding the fixed effects is less obvious as there is a positive relationship for all three specifications.

The plots contain interesting information about specific industries. Of course, one should note that there is much sampling error in average wage changes, so one cannot take the exact placement of any one point too seriously. Below, I discuss a few of the industries that have particularly high or low employment changes in one or more periods.

Mining (industry 1).—About half of all mining jobs are in oil and gas extraction. Their outcomes are closely tied to the price of oil, and so employment increased in the 1970s and declined during the 1980s and 1990s as oil prices declined. Employment in coal mining fell during the 1980s, largely owing to new labor-saving technology, and fell during the 1990s owing to shifts in demand away from more labor-intensive high-sulfur coal to cleaner low-sulfur coal. As would be expected, the wage change for miners is reasonably high in the 1970s and low in the 1980s and 1990s (see fig. 2).

Metal industries (industry 2).—Metal industries, such as the steel industry, lost a lot of jobs in the 1980s owing to a mix of improved technology and increased competition from abroad. As shown in figure 2, this was accompanied by low wage changes for men.

Textiles (industry 8).—Plunkert (1990, 11) writes that “throughout the 1980’s, [textiles and apparel] faced increasingly intense competition from imported goods, particularly goods from low-wage countries in the Far East.” In response to growing imports, the textile industry invested heavily in labor-saving capital equipment. Taken together, both these factors led to large employment falls during the decade. A similar story prevailed in the 1990s: Hatch and Clinton (2000, 8) write, “Apparel and other textile products, influenced less by technological innovations than by imports, lost a third of its workforce during the decade. This industry was especially harmed in the latter half of the decade, when most Asian countries devalued their currencies, making their products much less costly in the United States.” Between 1974 and 1994, the Multifiber Arrangement (MFA) placed some restrictions on imports of textiles and apparel. Beginning in 1995, the MFA expired, and there began a 10-year transition period, by the end of which all import quotas are scheduled to be eliminated (Mittelhauser 1997). Also, the implementation of the North American Free Trade Agreement (NAFTA) in 1994 has led to some shifting of manufacturing to Mexico. Both these legal changes were anticipated at the time to reduce employment in the U.S. textiles and apparel industry (Murray 1995). Figures 1 and 2 suggest that the decline in textile employment was accompanied by low composition-constant wage changes for women in the 1980s and 1990s (and low wage changes for men in the 1980s). Composition effects seem important in the 1990s, as might be expected given technological change.

Construction (industry 12).—Construction employment surged in the 1990s, accompanied by large increases in housing starts, completions, and permits. This can be attributed to an increase in demand for homes induced by increases in stock market wealth and low mortgage rates. This increase appears to have been accompanied by relatively high wage changes for men and women.

Communication (industry 14).—During the 1990s, communication employment increased rapidly. “Cable and other pay television services continued to grow even during the economic recession and became one of the fastest growing industries. Employment gains in the telephone communications industry during the 1990s offset losses that occurred in the 1980s following the breakup of AT&T. The demand for cellular service doubled in the last 3 years of the 1990s with more than 76 million subscribers in June of 1999. In addition, the increased popularity of pagers and the Internet helped raise employment levels” (Hatch and Clinton 2000, 9). The rapid increase in communications employment in the 1990s appears to have been accompanied by fast wage growth for both men and women.

Finance, Insurance, and Real Estate (industry 18).—Deregulation of banking in 1980 reduced barriers to entry and was followed by rapid growth in the financial sector. The number of insurance agents and real estate agents also grew over this period. This was accompanied by high wage growth for men and women during this period.

Business Services (industry 20).—This industry grew rapidly in all three decades as companies responded to competitive pressures by outsourcing more of their labor requirements to contract and temporary help firms. This growth was accompanied by high wage growth in the 1980s and 1990s but not particularly so in the 1970s.

Differences in Estimates across Groups

Unions may affect the responsiveness of wages to industry employment changes if wages are more flexible in nonunionized environments. To examine this possibility, I estimated separate regressions by union status for the 1981–91 period (information on unionization is missing in the 1999 and 2001 early release PSID files). I found the estimates for men to be similar for unionized and nonunionized workers. For women, I find statistically higher estimates for unionized workers. One should remember, however, that a relatively small proportion of women are unionized. Both groups of women exhibit a positive relationship between wages and industry employment.

I have also split the sample by age (individuals aged 18–34 and 35–55).¹⁶

¹⁶ The small sample size in the PSID does not permit a finer disaggregation by age. Individuals aged 18–24 are not used elsewhere in this article. I include them here because of the specific interest in differences in coefficients across age groups.

The estimates for the younger group are larger than for the older group. For average hourly earnings, the individual fixed effects GLS estimates for young women are 0.201 (0.063) for the 1980s and 0.222 (0.125) for the 1990s (compared to 0.027 [0.065] and 0.175 [0.08] respectively for the older group). The equivalent estimates for men are 0.233 (0.051) and 0.301 (0.095) for the younger group and 0.095 (0.046) and 0.112 (0.062) for the older group. While the differences are at the margin of statistical significance, they do suggest that younger individuals are better placed to take advantage of opportunities when industries are expanding but suffer bigger wage losses when industries are declining.

V. Reconciliation of Results to the Literature

I have found a positive relationship between industry employment changes and industry wage changes during the decades of the 1980s and 1990s. The results for the 1980s contrast with those of Weinberg (2001), who finds small and statistically insignificant relationships using the March CPS (he does not study the 1990s). There are many differences between my analysis and that of Weinberg. These include the fact that I use the PSID while he uses the CPS; we also use different age ranges, different industry groupings, and different measures of industry employment.¹⁷ Possibly most important, we use different specifications; he uses only long differences, and because he uses repeated cross-sectional data, he cannot take account of unobserved individual or individual-industry heterogeneity.

I begin by replicating Weinberg's estimates, making use of the thorough data documentation provided in his paper. I use the same data source (March CPS), similar sample restrictions, the same two-digit industry cells, and the same control variables. Also, as with him, the measure of industry employment is total hours worked in the industry calculated as the weighted sum of hours worked in the industry. This variable is calculated directly from the March CPS.¹⁸ The results are in table 5. Note that the dating in table 5 refers to the survey year, so that 1982–92 uses earnings in calendar years 1981–91 as reported in March 1 year later.

¹⁷ He uses 69 industries, compared to the 26 I use. Given the coarseness of the two-digit industry classification in the PSID, it is impossible to carry out analysis using his industry classification. Also, given the smaller sample sizes in the PSID, using more detailed industry groups would be problematic.

¹⁸ Unlike him, however, I do not also include a measure of the industry unemployment rate in the regression as it is difficult to interpret the effects of an industry demand measure conditional on another variable that could be interpreted as a demand variable. Also unlike him, I do not use pre-1976 CPS surveys, as weeks worked were bracketed and usual weekly hours are unavailable. Strong assumptions are hence required to impute average hourly earnings and hours worked during these years and would make the results less reliable.

Table 5
Reconciliation to Literature

	Estimate from Weinberg (2001) (1)	CPS Estimates— Weinberg Setup (2)	CPS Estimates— PSID Setup (3)
Women:			
1982–92	.057 (.032)	.043 (.045)	.044 (.105)
1977–82	.107* (.043)	.081 (.049)	.100* (.041)
Men:			
1982–92	.037 (.033)	.063 (.036)	.095 (.074)
1977–82	.051 (.039)	.066 (.060)	.025 (.093)

CPS = Current Population Survey. PSID = Panel Study of Income Dynamics. The estimates are coefficients on log industry employment from the estimation of eqq. (2) and (3) in the text. Each estimate is from a different regression. Regressions are run at the industry-year level. See text for details and for descriptions of the industry employment variables. Standard errors are in parentheses.

* Significant at the 5% level.

As can be seen in column 2 of table 5, my estimates using a similar setup are reasonably close to Weinberg's.¹⁹ In the third column, I show the estimates when the setup is made closer to mine: I use the 26 industry groups rather than 69, restrict the sample to individuals between 25 and 55 who have 6 or more years of potential experience, and use my control variables (replacing experience with potential experience). Now, the estimates are less precise, as there are only 26 industry groups, but the estimates have not changed that much (at least compared to the standard errors). Thus, it appears that differences between my results and his are not primarily the result of different industry groupings, different sample definitions, or the use of different sets of observed covariates.

Comparison of CPS Estimates to PSID Estimates

In this section, I use the CPS and PSID data to further explore these issues. Table 6 contains estimates using the 1976–81, 1981–91, and 1991–2001 survey years of the CPS. Column 1 of table 6 reports the long-difference estimates when the industry employment measure is aggregate hours calculated from the CPS. Comparing the estimates in column 1 of table 6 to those in column 3 of table 5, we see some sizable differences in the coefficient values that arise because of the change from 1982–92 to 1981–91, and so on. This indicates the importance of the transitory component in the CPS data that may arise owing to sampling variation or transitory shocks. This is reflected in the large standard

¹⁹ The standard errors are higher in col. 2 than in col. 1 primarily because I report robust standard errors.

Table 6
Comparison of CPS Estimates to PSID Estimates

	CPS				PSID
	Long Differences (Total Hours) (1)	GLS (Total Hours) (2)	GLS Smoothed (Total Hours) (3)	GLS Smoothed BLS Employment (4)	GLS Smoothed BLS Employment (5)
Women:					
1991–2001	−.069 (.051)	.001 (.028)	.001 (.031)	−.006 (.028)	−.085 (.069)
1981–91	.047 (.046)	.091* (.029)	.083* (.030)	.128* (.027)	.079 (.056)
1976–81	.169* (.046)	.118* (.034)	.148* (.040)	.134* (.040)	−.019 (.141)
Men:					
1991–2001	.093 (.79)	.076* (.028)	.158* (.034)	.110* (.029)	.106 (.055)
1981–91	.013 (.048)	.071* (.024)	.074* (.026)	.069* (.027)	.116* (.048)
1976–81	.119 (.110)	.049 (.045)	.146* (.061)	.082 (.066)	−.357* (.120)

NOTE.—CPS = Current Population Survey. PSID = Panel Study of Income Dynamics. The estimates are coefficients on log industry employment from the estimation of eqq. (2) and (3) in the text. Each estimate is from a different regression. Regressions are run at the industry-year level. See text for details and for descriptions of the industry employment variables. Standard errors are in parentheses.

* Significant at the 5% level.

errors so that none of the differences between the estimates are statistically significant.

Columns 2–5 of table 6 switch from long differences to the GLS industry fixed effects estimates. Column 2 uses total hours worked as the employment variable. We see the benefit of using all the years of data in the much decreased standard errors. In column 3 the employment variable is smoothed hours worked.²⁰ We see that the effect of smoothed hours is generally more positive than the effects of actual hours. This possibly reflects the fact that there is a lot of sampling variation in aggregate hours calculated from the CPS. Looking at the smoothed hours estimates, we see that there is a statistically significant positive relationship between industry hours and industry wages for men in all time periods and women in 1976–81 and 1981–91. Thus, the positive relationships found in the PSID are also to be found in the CPS.

In column 4, I replace aggregate hours with the smoothed BLS industry employment data that was earlier used with the PSID data. This tends to give results that are fairly similar to the earlier specifications using the aggregate hours variable. This suggests that the choice between the BLS

²⁰ As before, the smoothing is done by fitting 5-year splines to the aggregate hours worked data.

employment measure and manually constructing aggregate hours from the CPS may not matter much, at least once one takes out some of the transitory variation in aggregate CPS hours by smoothing.

Finally in column 5, I present the estimates from the PSID with the same control variables used in the CPS. For the 1980s and 1990s, the PSID estimates are quite similar to the CPS estimates and are never statistically different. There are differences for the 1976–81 period, as the negative results in the PSID are quite different from the positive coefficients in the CPS (this may be a function of the short time period and relatively small PSID sample; as seen in table 4, the statistically significant negative effect for men disappears when the whole 1971–81 period is used). However, for the 1980s and 1990s, the magnitudes of the estimates in columns 4 and 5 are similar. This suggests that the differences between my conclusions for the 1980s and Weinberg's do not arise from some fundamental difference between the CPS and PSID samples. Rather, the differences arise chiefly from two factors. First, sampling variability and transitory shocks imply that using industry hours calculated from the CPS and long-differences estimators lead to imprecise and possibly downwardly biased estimates of the relationship between industry employment and industry hours. Second, as shown in tables 3 and 4, it appears that composition bias also tends to lead to a downward bias.

VI. Analysis of Simultaneity

The assumption made so far is that the persistent changes in industry employment are primarily the result of shocks to labor demand rather than labor supply. In this section, I address this issue and make the argument that supply shocks are probably an unimportant source of industry employment change over this period. I also implement a couple of corrections for supply shocks.

Do Industry Employment Changes Largely Reflect Shocks to Labor Demand?

Evidence from the existing literature strongly suggests that changes in industry employment are largely determined by shocks to labor demand. Weinberg (2001) demonstrates a strong negative correlation between 10-year changes in aggregate industry hours and changes in the industry unemployment rate. This is consistent with changes in industry hours being driven by changes in labor demand rather than shifts in the supply of labor to individual industries. Also, if industry employment growth is driven by supply shocks, then workers displaced from growing industries should have low postdisplacement wages. Consistent with the demand-

side story, postdisplacement wages are higher for workers displaced from growing industries (Carrington 1993; Weinberg 2001).

This evidence can be augmented by looking more closely at the determinants of employment changes in individual industries. We already discussed labor demand shocks in several industries in Section IV. Obvious shocks to labor demand also exist in other industries. For example, the health industry grew rapidly during the 1980s owing to the aging of the population, technological advances in treatments, and greater paperwork requirements for government and for insurance companies. Also, health insurance coverage increased over the decade, and the proportion of cost paid directly by patients declined from 27% to 19% (Hiles 1992). This implied a positive shift in demand for health services. The growth rate slowed somewhat in the 1990s as the growing popularity of HMOs and greater use of prospective payment led to decreased health expenditures. Likewise, employment changes in other industries can be traced to clear shocks to labor demand.

Adjusting Industry Employment for Supply Shocks

Assume a simple linear industry labor supply curve in differences of the following form:

$$dE = \varepsilon^s dW + dS. \quad (4)$$

Here, E refers to log employment, W to the log wage, S is a supply shift in log terms, and ε^s is the elasticity of supply facing the industry. We can rewrite the supply curve to be consistent with the estimation equation in the article:

$$dW = \frac{1}{\varepsilon^s} dE - \frac{1}{\varepsilon^s} dS. \quad (5)$$

If we do not control for dS , we will get a biased estimate of $1/\varepsilon^s$ if dS is correlated with dE . The estimate will be biased upward if the correlation is negative and biased downward if the correlation is positive. In general, we would expect a positive correlation between dS and dE as growing industries are probably those that have positive supply shocks. Possible supply shocks are changes in individual preferences toward working in particular industries, changes in the gender and educational composition of the workforce, and changes in the relative wages of different demographic groups. While one cannot rule out shifts in preferences, they are unobservable and impossible to deal with in this context. I outline below corrections to industry employment that take account of the other sources of supply shocks. To deal with these issues, I take a similar approach to that taken by Bound and Johnson (1992) and Weinberg (2001).

First, consider the effects of changes in the demographic composition of employment. Using the March CPS, I define eight demographic groups, g , by sex, race (white or nonwhite), and education (college degree or no college degree). First, I calculate the proportion of each group in each industry, j , over the full 1971–2001 period (θ_{gj}). Then I calculate the number of people in each group-year cell over the period. The next step, for any two periods, is to multiply the change in the log of the number of individuals in each group (dE_g) by θ_{gj} and add across groups for each industry. This gives an estimate of the growth rate of each industry under the scenario that the only thing that changes is the demographic composition of the labor market. Define dE_j^* as an estimate of the change in log employment in industry j that would have occurred in the absence of supply shocks.

$$dE_j^* = dE_j - dS_j = dE_j - \sum_g \theta_{gj} dE_g. \quad (6)$$

This correction decreases the implied demand for industries that intensively employ groups who are becoming more prevalent in the labor market.

A second possibility is that changes in relative wages across demographic groups influence industry employment growth. For example, the wages of women rose relative to men during the 1980s. All else equal, this would tend to reduce employment growth of industries that intensively employ women. We can augment the correction to take account of changes in relative wages across demographic groups. The estimate of the industry growth rate under the assumption that only relative wages change equals $\varepsilon^d \sum_g \theta_{gj} dW_g$, where ε^d is the elasticity of labor demand and dW_g is the change in the log wage of group g . Putting both supply corrections together, we get

$$dE_j^* = dE_j - \sum_g \theta_{gj} dE_g - \varepsilon^d \sum_g \theta_{gj} dW_g. \quad (7)$$

This adjustment increases the implied demand growth for industries that intensively employ groups whose wages have increased.

Results

The results are in table 7. The first panel contains GLS estimates for the specifications with individual fixed effects in the first stage; the second panel contains the analogous estimates when individual-industry fixed effects are included. The estimates in column 1 are uncorrected and are exactly the same as in tables 3 and 4.

The estimates in column 2 are obtained using equation (6). We see that this correction does not have big effects for 1991–2001 and 1981–91—the estimates are quite similar to those in column 1, sometimes smaller

Table 7
Adjusting for Supply Shocks (GLS Estimates)

	Women			Men		
	(1)	(2)	(3)	(1)	(2)	(3)
Individual fixed effects included:						
1991–2001:						
Average hourly earnings	.148* (.053)	.190* (.059)	.112* (.052)	.155* (.054)	.150* (.057)	.120* (.051)
Current wage	.186* (.050)	.230* (.055)	.134* (.047)	.211* (.042)	.176* (.045)	.159* (.039)
1981–91:						
Average hourly earnings	.163* (.048)	.135* (.056)	.135* (.051)	.172* (.038)	.130* (.041)	.151* (.039)
Current wage	.183* (.038)	.183* (.044)	.173* (.039)	.210* (.032)	.197* (.036)	.201* (.033)
1971–81:						
Average hourly earnings	.046 (.058)	.106 (.057)	.086 (.057)	–.023 (.054)	.121* (.055)	.098 (.056)
Current wage ^a	.178 (.099)	.276* (.099)	.289* (.108)	.039 (.074)	.103 (.073)	.122 (.074)
Individual-industry fixed effects included:						
1991–2001:						
Average hourly earnings	.273* (.071)	.337* (.079)	.208* (.069)	.124* (.054)	.130* (.057)	.077 (.051)
Current wage	.295* (.061)	.377* (.066)	.217* (.060)	.180* (.043)	.161* (.046)	.129* (.041)
1981–91:						
Average hourly earnings	.238* (.047)	.238* (.054)	.219* (.049)	.218* (.039)	.178* (.044)	.198* (.041)
Current wage	.241* (.039)	.261* (.045)	.234* (.040)	.182* (.038)	.186* (.039)	.191* (.036)
1971–81:						
Average hourly earnings	.003 (.069)	.112 (.069)	.082 (.069)	.038 (.059)	.211* (.060)	.180* (.061)
Current wage ^a	.160 (.112)	.260* (.113)	.267* (.116)	–.056 (.086)	.041 (.083)	.057 (.084)

NOTE.—(1) = no correction for supply shocks. (2) = correction for changes in demographic composition of labor force. (3) = correction for both changes in demographic composition of labor force and changes in relative wages. The estimates are coefficients on log industry employment from the estimation of eqq. (2) and (3) in the text. Each estimate is from a different regression. Regressions are run at the industry-year level. See text for details and for descriptions of the industry employment variables. Standard errors are in parentheses.

^a 1976–81.

* Significant at the 5% level.

and sometimes larger. The correction does make a difference for the 1970s estimates. The adjusted estimates are uniformly larger than the unadjusted ones and in many cases are statistically significant. Indeed, now the estimates from the 1970s look quite similar to those from the other decades.

The estimates in column 3 are obtained by using equation (7).²¹ An

²¹ Because hourly wage data are unavailable from the CPS before 1976, the supply correction in column 3 for average hourly earnings is carried out under

elasticity of labor demand of -1 is assumed.²² This correction tends to reduce the size of the coefficient on industry employment. However, the differences are not very large, and the conclusion of a robust positive relationship between industry employment and industry wages remains. Thus, the main effect of adjusting industry employment for supply factors is to increase the coefficients from the 1970s to be more in line with the other decades.

VII. Conclusions

By using panel data, I have been able to study how changes in the composition of individuals within industries affect the estimated correlation between changes in industry employment and industry wages. I find that, for the most part, composition changes bias down the correlation between industry employment and industry wages. This implies that when industries are expanding, they attract individuals who are of lower quality than the average worker in that industry. Likewise, declining industries lose workers who have lower skills than the average worker in that industry. The implied pattern of mobility in which the worst workers leave declining industries to become the worst workers in expanding industries is consistent with the finding of McLaughlin and Bils (2001) in their study of industry mobility over the business cycle. The results show the value of using panel data that allows one to adjust for both observable and unobservable quality differences across workers.

The composition-constant estimates suggest that during the 1980s and 1990s industry wages are robustly related to industry growth or decline. During the 1970s, there is no evidence of a positive relationship between industry growth and industry wages. However, once industry employment is adjusted to take account of labor supply shocks, a positive relationship tends to emerge. The results are consistent with a model in which workers have industry-specific human capital that is reduced in value when there are persistent negative shocks to their industry. They also imply that while labor supply curves facing industries are elastic, they are not horizontal.

the assumption that changes in employment between 1970 and 1975 did not occur as a result of changes in relative wages. This is an issue only for the average hourly earnings results, as the current wage results pertain to 1976–81 in any case.

²² I have also experimented with other demand elasticities, and the estimates are sensitive but not terribly sensitive to the value chosen. For example, for women, the coefficient for average hourly earnings with individual fixed effects for 1981–91 is 0.135 (0.051) with the elasticity at -1 , 0.124 (0.045) if the elasticity is assumed to be -2 and 0.137 (0.054) if the elasticity is assumed to be -0.5 . For men, the coefficient for 1981–91 is 0.151 (0.039) with the elasticity at -1 , 0.159 (0.036) if the elasticity is assumed to be -2 and 0.142 (0.040) if the elasticity is assumed to be -0.5 .

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