Winter 1998

Unions, Wages, and Skills

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ABSTRACT

Studies uniformly conclude that union wage effects are largest for workers with low measured skills. Longitudinal analysis using 1989/90–1994/95 Current Population Survey matched panels produces union premium estimates equivalent across skill groups, following appropriate sample restrictions and control for worker-specific skills. Evidence from the National Longitudinal Survey of Youth on aptitude scores confirms that union workers with high measured skills have relatively low unmeasured skills. Differential selection by skill class and skill homogeneity in union workplaces results from employer and employee sorting in response to wage standardization, union organizing where skills are homogeneous, and unionized employers' reluctance to hire the most as well as least able workers.

I. Introduction

The relationship between unionism, wages, and worker skills has been the focus of considerable research. A universal finding is that union-nonunion wage differentials are larger for lower-skilled than for higher-skilled workers.[1] The standard explanation for this result is that unions standardize wages by decreasing

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[Submitted July 1996; accepted May 1997]

1. See, for example, Johnson and Youmans (1971) and Ashenfelter (1978). Lewis provides a survey, concluding that union wage gaps decrease with occupational skill level and schooling (Lewis 1986, pp. 128–31, 136–39). As discussed subsequently, Card (1996) arrives at a similar conclusion using longitudinal analysis.

differentials across and within job positions (Freeman 1980) so that lower-skilled workers receive a larger premium relative to their alternative nonunion wage. There exists a related literature on omitted ability bias and the estimation of union wage effects. The conventional argument is that a wage premium encourages skill upgrading by unionized employers, so that standard OLS estimates of the premium are biased upward owing to a failure to account fully for worker skills positively correlated with union status.

In this paper, we argue that union wage effects need not lead to skill upgrading or to uniformly higher (or lower) unmeasured skills among union than among nonunion workers. Rather, the principal outcome associated with unionization is lower dispersion in worker skills in union than in nonunion workplaces. Selection by employers and employees leads union workers with low credentials or levels of measured skills to have high unmeasured ability, and those with high credentials or measured skills to have low unmeasured ability, relative to their nonunion counterparts. In contrast to standard cross-sectional estimates of union wage effects, our longitudinal estimates controlling for otherwise unmeasured worker-specific skills differ little with respect to the level of measured premarket skill.

In the next section we develop the key arguments underlying the analysis. In Section III, we describe our data and empirical approach, and examine the impact of sample restrictions on premium estimates. Cross-sectional and longitudinal evidence on union wage effects across skill groups is presented in Section IV. In Section V we examine direct evidence on differences between union and nonunion workers in aptitude scores.

II. Skill Homogeneity, Unmeasured Skill, and Union Premium Estimates

We use the term "unmeasured" to refer to worker skills that are observable to employers and productive in the workplace, but not observable or measured directly by researchers. Biased estimates of the union premium arise where unmeasured (omitted) skills are correlated with union status and the wage. Potential ability bias has led researchers to estimate union wage effects using selectivity methods (for example, Robinson 1989) or longitudinal models that control for unmeasured worker-specific skills (Mellow 1981; Freeman 1984; Robinson 1989; Jakubson 1991; Lemieux 1993; Card 1996). The conventional view is that the existence of a union premium leads to skill upgrading as employers select high-ability workers from the union queue. As a result, standard premium estimates are biased upward. Economists surveying the literature on union wage effects have emphasized this point (Lewis 1986, pp. 46–47, 60–61; Hirsch and Addison 1986, pp. 116–17, 127–29; Booth 1995, pp. 171–72). Recently, Wessels (1994) has argued rather convincingly that the conventional view may be wrong. He shows that a wage premium can decrease, increase, or leave unchanged worker skills. Given union bargaining power, an employer that upgrades worker skills through selective hiring would then face an even higher wage in the following bargaining round. Quoting Wessels: "[The standard story] assumes that unions will sit idly by and let union firms whittle away any union wage differential by hiring better-quality workers. I would argue that it is more likely that unions would respond to better-quality workers by raising wages even more. Employers, anticipating this, may respond by hiring lower quality workers'' (Wessels 1994, p. 616). In Wessels' formal model, union employers will hire lower (higher) skill workers if labor quality augments capital productivity and the elasticity of substitution between labor and capital is less than (greater than) unity.[2]

Skill homogeneity in union workplaces can result for (at least) three reasons. First, homogeneity may result from union wage standardization policies and subsequent selection by employers and employees. Unions narrow wage differences across high- and low-skill job categories and decrease dispersion within job categories through reliance on contractually determined wages based on job position and seniority, rather than on individual, merit-based wage setting (Freeman 1980, 1982). In response to such wage standardization, employer and employee selection will lead to unmeasured union-nonunion skills being negatively related to measured skill levels. To make the point clear, assume unionized firms pay a single wage to all workers and that schooling represents a premarket attribute measuring worker skill with error. As workers queue for union jobs, there will be a long queue among less-schooled workers, for whom the union wage represents a large premium, and a short queue among highly educated workers, for whom alternative wages tend to be high. Among the low-schooling group few workers would reject the high-paying union job, but employers will reject the less-able applicants in the queue. Employer selection, therefore, leads to high levels of unmeasured skills among those with low credentials. Among highly educated workers, the most productive workers will not be in the queue owing to higher alternative wages, leading to low levels of unmeasured ability among those with high measured skills.[3] Selection leads to a predictable bias in estimates of union wage effects. Estimates from wage level regressions among workers with low measured skills are likely to be biased upward owing to positive selection on ability. Estimates among workers with high measured skills are likely to be biased downward owing to lower unmeasured skill among union than nonunion workers. The standard conclusion that the union premium is higher for low-skill than for high-skill workers may be overstated or no longer hold once one nets out the effects of unmeasured skills.

A second explanation for skill homogeneity emphasizes the effect of dispersion on the likelihood of unionization, rather than the other way around. Unions are most likely to organize and maintain political support in establishments where worker preferences and skills are relatively homogeneous (Farber and Saks 1980; Hirsch 1982; and Demsetz 1993). True skills are likely to be more homogeneous than are measured skills.

A third explanation for skill homogeneity within union workplaces deemphasizes union goals and wage standardization and instead focuses on the employer response to a union tax on wages. As shown by Wessels (1994), depending on the nature of

2. Intuitively, there is little incentive for employers to upgrade skills if it leads to future wage increases and production technology is such that high-ability workers have a small impact on productivity owing, for example, to a highly routinized production process.

3. Abowd and Farber (1982) and Card (1996) provide two-sided selection models whereby being a union member requires a worker to desire covered employment and to be chosen by the employer from the union queue.

technology and the elasticity of substitution between capital and labor, higher union wages need not lead to skill upgrading. If labor quality augments capital and the elasticity of substitution between capital and labor decreases with measured skill level (for such evidence, see Hamermesh 1986), the Wessels model predicts a more compressed distribution of skills in union workplaces. For example, an elasticity of substitution greater than unity for high school graduates and less than unity for college graduates would lead the firm to hire relatively high ability high school graduates and low ability college graduates in response to a union wage increase.[4] Skill homogeneity can result, therefore, from a uniform union tax on wages, absent any attempt at wage standardization. As before, union-nonunion differences in unmeasured ability decline with the level of measured skill. Although standard wage level estimates of the premium may decline with respect to measured skill, ''true'' union wage effects would not vary with the level of skill.

All three explanations for union skill homogeneity lead to the expectation of an inverse relationship between union-nonunion measured and unmeasured skills or, stated alternatively, greater compression in true skills than in measured skills. The finding that true union wage effects by skill class are more similar than standard wage level estimates is consistent, therefore, with any of these explanations. A finding of identical wage effects across skill groups appears most consistent with a variant of the Wessels model. A uniform union tax, however, need not be inconsistent with union contracts that standardize wages and lessen top-to-bottom wage differences within firms relative to what would exist in a nonunion workplace, as long as employer-employee sorting follows in response to such standardization. But uniform union wage effects are not consistent with a model in which unions both seek and acquire larger proportional gains to low-skill than to high-skill members, following labor market sorting. The union endogeneity explanation (namely, skill homogeneity increases unionization) may help account for relatively uniform union wage effects, but does require that union wage effects be equivalent across skill levels.

Card (1996) has recently provided a careful examination of union wage effects and skills using longitudinal analysis from a 1987–88 CPS panel of male workers. Following adjustment for misclassification error of union status, based on information in a January 1977 CPS validation survey, Card concludes that *all-worker* OLS wage level and longitudinal union-nonunion wage differential estimates are similar, and that there exists positive selection among workers with lower measured skill and negative selection among those with higher measured skill. Card continues to find that longitudinal union wage effects decline with skill level, although by less than do standard estimates not controlling for worker fixed effects. Card relies on a two-sided employer-employee selection model, as described previously, to explain these results. Our analysis differs from Card in several respects. We utilize substantially larger and more recent data—CPS panels for 1989/90–1994/95. Instead of relying on a measurement error adjustment based on an external survey and assumed constant across skill grades, we make several sample restrictions that sharply reduce bias from measurement error and increase reliability of our estimates. In contrast to

4. In the Wessels model, the value given to labor quality depends in part on its enhancement of capital productivity. If the elasticity of substitution is greater than (less than) unity, a union tax on labor increases (decreases) capital's share and increases (decreases) the weight given to labor quality.

Card, we conclude that union wage effects are highly similar across groups of workers with high and low levels of measured skills, after accounting for unmeasured skills and following labor market sorting. We also present direct evidence from the National Longitudinal Survey of Youth (NLSY) on union-nonunion differences in the Armed Forces Qualifying Test (AFQT)—a productivity-related worker attribute not measured in the CPS.[5]

III. Model, Data, and the Use of Sample Restrictions to Reduce Measurement Error

The relationship between skills and the union wage premium is examined by estimating log wage equations in levels and in difference form. The former provides conventional estimates of union premiums by skill category, while the latter purges premium estimates of bias owing to omitted worker-specific skills transferable between union and nonunion jobs. As our measure of skill group, we follow Card (1996) and sort workers into skill categories based on their predicted wage, formed by estimating a wage level equation for nonunion workers with premarket personal and location variables (see footnote 10). All union and nonunion workers are then placed in one of four quartiles based on the log wage predicted from the equation.[6]

Union wage effects are first estimated using standard wage level equations:

$$(1)\quad \ln W_{its} = X_{jits}\beta_{js} + Union_{its}\Theta_s + Year_{yits}\tau_{ys} + \Gamma_{is} + e_{its}.$$

Here $\ln W_{its}$ is the log real wage of worker i in year t in skill group s, vector X consists of variables (indexed by i) measuring personal and job-related characteristics, and β are the respective coefficients, *Union* is a dummy variable equal to 1 if the worker is a union member, Θ_s is a measure of the union log wage differential by skill group s, and *Year* are year dummies (indexed by y). The error term is divided into an individual-specific ''skill'' component (Γ_{is}) assumed fixed over time (one year in our case), and a random, well-behaved component (e_{its}).

If the omitted fixed effect Γ is correlated with union status, then estimates of Θ are likely to be biased. For example, let $\Gamma = \alpha + \Omega Union + \mu$. Using the standard measure of omitted variable bias (which assumes that Γ is uncorrelated with other variables in (1)), the OLS wage premium estimate $\Theta' = \Theta + \Omega$, where Θ is the ''true'' union premium and Ω is the skill bias. Further, if the correlation between *Union* and Γ_s are allowed to differ by skill category s such that $\Theta'_s = \Theta_s + \Omega_s$, the magnitude and possibly direction of the bias will differ across measured skill categories. Or, as we subsequently show, standard estimates of Θ'_s decline with measured skilled level largely because Ω_s is strongly positive for low-skill workers and small

5. An earlier version of our paper (Hirsch and Schumacher 1996) was written independently of Card's study. For a related study using Canadian data, see Lemieux (1993).

6. It is important to measure skill based on premarket characteristics rather than on labor market outcomes (for example, occupational attainment) that are positively correlated with the wage and unmeasured skills. In an earlier version of our paper (Hirsch and Schumacher 1996), we focused on education as a measure of skill. As shown subsequently, basic results are highly similar to those based on the predicted wage.

or negative for high-skill workers. True union wage effects Θ_s may vary little across measured skill groups.

Letting Δ represent changes between adjacent years, a wage change equation will take the form (dropping the individual subscript i):

$$(2)\quad \Delta \ln W_{ds} = \Delta X_{jds}\beta_{js} + \Delta Union_{ds}\Theta_s + Year_{ds}\tau'_{ds} + \Delta e_{ds}.$$

Here d indexes the one-year time period over which changes occur and $Year_{ds}$ represents dummies that measure differences in average wage growth. The fixed effect Γ falls out, potentially allowing unbiased estimates of Θ_s.

The data used in the paper are from the Current Population Survey (CPS) Outgoing Rotation Group (ORG) earnings files for January 1989 through May 1995. The structure of the CPS permits one to match given individuals in the same month, one year apart. We construct a large panel data set for 1989–90 through 1994–95 from the CPS ORG.[7] Our sample consists of private-sector wage and salary workers, ages 16 and over. Excluded are those who are not in private-sector wage and salary employment in consecutive years, workers whose primary activity is schooling in either year, those whose weekly earnings are top-coded by the Census (at $1,923) in either year (since measured wage change would then be determined by the earnings assignment in the open-ended category), workers with an implied wage (weekly earnings divided by hours worked per week) less than $1 or more than $99.99 (the latter corresponds to someone receiving weekly earnings near the cap and working less than 20 hours), workers covered by a collective bargaining agreement who are not union members (covered nonmembers are more likely to have union status measured with error and have wages higher than nonunion workers but lower than union members), workers whose earnings or union status have been allocated by the Census in either year (these two restrictions are examined below), and workers who cannot be matched across years. Because the Census reinterviews households in fixed locations, individuals whose household moves or who move out of a household during the year are not in the sample. Young workers are most likely to be underrepresented (Peracchi and Welch 1995; Card 1996).

To insure comparability between the wage level and wage change samples, the panel data set is used for estimation of both Equations 1 and 2, with the levels equations based on second-year observations for each worker. Wage level estimates using the full CPS ORG data set are very similar. We include standard control variables in X—schooling, potential experience (the minimum of age − schooling − 6 or age − 16) and its square, gender (separately and interacted with the experience variables), marital status (two dummies included), race and ethnicity identifiers (4), part-time status, region (8), metropolitan size (6), industry (12), occupation (11), and year (5). In the wage change equations, most of the variables in X are time invariant. Included in addition to changes in union status are changes in experience squared, part-time status, broad industry, and broad occupation, plus year dummies. By construction, region and city size do not change because households that move cannot be matched in the CPS panel. Experience changes by one for all and is cap-

7. Details on construction of the CPS panel are provided in Hirsch and Schumacher (1996), along with separate estimates by gender. The Census adopted new area samples after May 1995, not permitting the matching of households for June-December 1994–95.

tured in the intercept. Remaining variables either cannot change by construction of the panel (for example, gender) or are treated as invariant because recorded changes may result from measurement error (for example, schooling).

Measurement error in the union variable presents a potentially serious problem for the longitudinal analysis. Mismeasured union status lowers the signal-to-noise ratio in $\Delta Union$, biasing estimates of Θ toward 0. We reduce error resulting from the misclassification of union status through a series of sample restrictions. Although such restrictions reduce bias from measurement error, they do so at the cost of reducing the sample size and, to a lesser extent, representativeness. Such an approach is feasible using large CPS panels for multiple years, but is less so using a single year's CPS panel (as in Card 1996) or smaller data sets such as the PSID or NLSY.

Table 1 presents wage level and longitudinal union premium estimates based on alternative groups of measured union switchers and using alternative sample restrictions. Results in lines 1a and 1b are based on our primary sample (n = 153,778), using the restrictions stated above. The union premium estimate from the wage level equation is 0.18 log points. Our most important method for reducing measurement error bias in $\Delta Union$ is to distinguish (using interaction terms) between four types of reported union changers: workers who change both (three-digit) occupation and industry as well as union status, those who change industry but not occupation, those who change occupation but not industry, and those who change neither industry nor occupation. Our principal results focus on wage changes among the first group—union switchers who also change detailed industry and occupation—because this group is least likely to include misclassified union switchers. Separate interaction terms are included to control for the other three groups of union switchers and for wage change associated with broad occupation and industry change, independent of union status change.[8]

Our preferred longitudinal union premium estimate is 0.12 log points, based on the sample of industry-occupation changers (lines 1a and 2). By contrast, the estimated premium is 0.08 among workers reporting industry-only changes, 0.03 among occupation-only changers, and 0.02 among those changing neither industry nor occupation. These results are consistent with known measurement problems in the CPS, which exaggerate the amount of year-to-year occupational and, to a lesser extent, industry switching (Polivka and Rothgeb 1993). A ''standard'' longitudinal estimate based on the pooled group of union switchers (line 1b) is only 0.06, half the size of our preferred estimate. Basing longitudinal estimates on union status changers who also change industry and occupation appears to be a fruitful (and simple) strategy for reducing measurement error bias. All longitudinal estimates presented subsequently are based on union switchers who change occupation and industry.

We next examine alternative sample restrictions. The earliest year in our CPS data set is 1989, when the Census first included a flag for individuals whose union status is allocated. Our primary sample (line 2) omits workers whose union status is allocated in either of the two years, as well as workers whose earnings are allocated by

8. Wage change regressions including not only changes in broad occupation and industry, but also dummies for whether workers change detailed industry/occupation, industry only, or occupation only, produced virtually identical results. In results not shown, wage *level* estimates of Θ are 0.19 among union members in year 2 who changed neither industry nor occupation, and 0.15–0.16 among union members who changed industry or occupation or both.

Table 1

Wage Level and Change Estimates of the Union Differential, Alternative Sample Restrictions

	Wage Level			Wage Change		
	n	Union	Standard Error	[# switch]	ΔUnion	Standard Error
Alternative union switcher groups using primary sample						
1a. Primary sample	153,778	0.182	(.0032)	153,778	—	—
Union switchers	—	—	—	[6,622]	—	—
Industry/occupation change	—	—	—	[1,805]	0.116	(.0081)
Industry only change	—	—	—	[638]	0.082	(.0136)
Occupation only change	—	—	—	[1,684]	0.031	(.0084)
No industry/occupation change	—	—	—	[2,495]	0.024	(.0069)
1b. Primary sample with all union switchers	153,778	0.182	(.0032)	[6,622]	0.056	(.0042)
Alternative sample restrictions and industry/occupation union switcher estimates						
2. Primary sample	153,778	0.182	(.0032)	153,778	0.116	(.0081)
3. Full sample	217,793	0.178	(.0027)	217,793	0.098	(.0068)
4a. Union not allocated	213,922	0.182	(.0028)	209,792	0.115	(.0073)
4b. Union allocated	3,871	0.103	(.0178)	8,001	0.008	(.0253)
5a. Earnings not allocated	212,364	0.180	(.0028)	207,049	0.101	(.0069)
5b. Earnings allocated	5,429	0.095	(.0190)	10,744	0.057	(.0338)
6a. No proxy respondent	76,252	0.186	(.0048)	54,152	0.115	(.0149)
6b. Proxy respondent	77,526	0.177	(.0043)	99,626	0.116	(.0098)

The sample includes private-sector wage and salary workers ages 16 and over employed during consecutive years, from the CPS Outgoing Rotation Group (ORG) earnings files for January 1989 through May 1995. Deleted are workers not matched across years, workers whose principal activity is schooling, covered nonmembers, workers with top-coded earnings, and workers with extreme wages or wage changes. The ''primary sample'' omits workers with allocated union status or allocated earnings, but includes workers with proxy respondents. Wage level equations are based on 1990–95; wage change equations 1989/90–1994/95. *n* is sample size and [# switch] is number of union switchers. In lines 2 through 6, union change coefficients are based on union switchers who change detailed occupation and industry. Separate interaction terms are included for other union switchers, as in line 1a. Other variables included in wage level regressions are schooling, potential experience (minimum of age − schooling − 6 or age − 16) and its square, gender (separately and interacted with the experience variables), marital status (two dummies included), race and ethnicity identifiers (4), part-time status, region (8), metropolitan size (6), industry (12), occupation (11), and year (5). The wage change equations include, in addition to the change in union status variables, change in experience squared, change in part-time status, change in industry, change in occupation, and year dummies.

the Census in either year.[9] The importance of these sample restrictions is seen in Table 1. The "full" sample (line 3), which includes workers with allocated union status and earnings, produces a similar premium estimate in levels (0.18), but 0.02 lower in changes (0.10 versus 0.12). Wage level estimates based either on the sample with union status allocated in the second year of the panel (line 4b) or those with earnings allocated in year two (line 5b) are each 0.10, substantially lower than the 0.18 from the restricted or full samples. Longitudinal premium estimates are particularly sensitive to mismeasured union status and earnings. The sample with union status allocated in either year (line 4b) has a coefficient on $\Delta Union$ of only 0.01, reflecting serious measurement error bias. The sample with a designation that earnings were allocated in either year (line 5b) yields a $\Delta Union$ coefficient of only 0.06. In the latter case, measurement error is in the dependent variable, but Θ is biased downward because such error is not random. The Census "hot-deck" procedure often assigns earnings to union workers based on nonunion "donors" and to nonunion workers based on union "donors," biasing downward measured union-nonunion wage differences.

An additional sample restriction issue examined is whether to include individuals in the CPS for whom information was provided by a "proxy" respondent (often a spouse). This issue is potentially important, since roughly half the responses in a given year are by proxy. Using panel analysis, an even greater proportion of the sample is based on proxy respondents in at least one of the two years. The good news is that estimates of union wage effects are highly similar for samples of workers who provided their own responses and samples with proxy respondents. Lines 6a and 6b of Table 1 divide our primary sample into "no proxy" and "proxy" samples. Wage level estimates of Θ are 0.19 and 0.18, respectively, while longitudinal estimates of Θ are 0.12 in both cases. No distinction between workers with and without proxy respondents is made in the analysis that follows.

IV. Union-Nonunion Wage Differentials by Measurable Skill Group

Wage level and change equation estimates of union wage effects by skill level are provided in Table 2. Column 1 shows estimates of Θ from the full sample. The next four columns present separate estimates of Θ by skill group, measured alternatively by the predicted wage and schooling. Measured skill quartiles are formed from a predicted wage based on premarket wage correlates.[10] The far

9. Earnings allocation information is not provided in the ORG beginning in 1994. Hence, sample deletions from the 1993–94 sample are based only on allocation information for 1993, and no deletions are made from the January-May 1994–95 sample. Card (1996) deletes those with allocated earnings, but was not able to omit those with allocated union status.

10. The predicted wage is calculated from the coefficients of a wage equation including schooling, potential experience and its square, gender (separately and interacted with the experience variables), marital status, race and ethnicity identifiers, region, metropolitan area size, and year, estimated for the sample of all nonunion workers. Union and nonunion workers are then divided into skill quartiles measured by their predicted log wage. Estimates of Θ_s shown in Table 2 are obtained from wage equations estimated separately by skill group.

Table 2
Wage Level and Change Estimates of the Union Differential, by Skill Category

		Predicted Wage Quartile				
	All	First	Second	Third	Fourth	*F* [*p*-value]
Base specification (with occupation dummies)						
1a. Level (union)	0.182	0.223	0.216	0.198	0.130	91.515
	(.0032)	(.0067)	(.0067)	(.0057)	(.0070)	[0.000]
1b. Change (Δunion)	0.116	0.104	0.113	0.143	0.104	0.001
	(.0081)	(.0143)	(.0158)	(.0159)	(.0195)	[0.976]
Specification (without occupation dummies)						
2a. Level (union)	0.131	0.203	0.183	0.160	0.043	258.925
	(.0033)	(.0068)	(.0065)	(.0058)	(.0070)	[0.000]
2b. Change (Δunion)	0.114	0.103	0.109	0.142	0.098	0.036
	(.0081)	(.0144)	(.0158)	(.0159)	(.0195)	[0.850]
Sample size	153,778	38,070	38,122	38,631	38,955	
Number of union industry/occupation changers	1,805	504	456	472	373	
Percent union industry/occupation to all changers	27.3	31.6	28.4	26.7	22.5	

	All	Years Schooling: Dropouts	High School	Some College	College Graduate	F [p-value]
Base specification (with occupation dummies)						
3a. Level (union)	0.182	0.237	0.198	0.168	0.084	113.805
	(.0032)	(.0079)	(.0042)	(.0063)	(.0119)	[0.000]
3b. Change (Δunion)	0.116	0.107	0.117	0.120	0.111	0.022
	(.0081)	(.0191)	(.0115)	(.0159)	(.0269)	[0.883]
Sample size	153,778	17,905	63,246	41,384	31,243	
Number of union industry/occupation changers	1,805	303	827	476	199	
Percent union industry/occupation to all changers	27.3	27.0	28.4	28.1	22.4	

See note to Table 1. The predicted wage quartiles are based on coefficients from a log wage equation estimated with the nonunion sample and variables measuring premarket skills, demographic characteristics, and location (see text for details). The base specification corresponds to that shown in Table 1, line 1. The F and p values correspond to the null that coefficients in the lowest and highest skill or schooling categories are equivalent. Standard errors are in parentheses.

right column of Table 2 provides F-ratio and probability values corresponding to the null that Θ_s is equivalent in the low-and high-skill quartiles or schooling groups. The sample used in Table 2 corresponds to the "primary" sample from Table 1, with the longitudinal estimates of Θ based on workers changing industry and occupation as well as union status.[11]

Cross-sectional union premium estimates decrease substantially with skill level. In line 1a, estimates decline with respect to measured skills, with log differentials of 0.22, 0.22, 0.20, and 0.13 as we move from the lowest to highest skill quartile. The null of equivalent union premiums in the first and fourth quartiles is decisively rejected. The cross-sectional pattern is more readily evident when occupation dummies are omitted (line 2a), with estimates of 0.20, 0.18, 0.16, and 0.04, respectively. Absent occupational controls, the wage level model includes relatively few direct measures of skill. Exclusion of occupation dummies not only lessens differences in estimated union premiums across skill classes, but also causes the average Θ to decrease substantially (from 0.18 to 0.13) because union workers are concentrated in production and laborer occupational categories with low average pay, while being underrepresented in professional and managerial occupations. Because wage regressions with occupation dummies are standard in the literature, results in line 1a provide the more appropriate benchmark.[12] Line 3a segments the sample not by the predicted wage but by schooling category—dropouts, high school graduates, some college, and college graduates. This produces a similar pattern with estimates of 0.24, 0.20, 0.17, and 0.08, respectively, for the various schooling classes.

In sharp contrast to wage level estimates of Θ, longitudinal estimates are highly similar across skill groups—0.10, 0.11, 0.14, and 0.10 moving across skill groups from the bottom to top quartiles (line 1b). One cannot reject the null of equality between the low- and high-skill categories (or, in results not shown, of equivalence among all skill groups). We obtain identical estimates from the specification without occupational change variables (line 2b). Using schooling as our measure of skill group, we obtain estimates of 0.11, 0.12, 0.12, and 0.11 as we move from low to high schooling (line 3b).[13]

Our finding of equivalent longitudinal or "quality-adjusted" union premiums across premarket skill groups differs from results in Card (1996). Card concludes that fixed-effects estimates, adjusted for union misclassification error, decline with measured skill, although (as in this study) the spread in union wage effects across skill groups is reduced substantially as one moves from OLS to a fixed-effects estimator. Our results, based on a substantially larger CPS panel data set (1989/90–1994/95 versus 1987–88), differ from Card's primarily owing to our focus on union

11. An earlier version of the paper (Hirsch and Schumacher 1996) provides separate analysis for males and females. A similar pattern exists for each group.

12. Card (1996) does not include occupation dummies in his analysis, and thus obtains wage level estimates similar to his longitudinal premium estimates. Absent control for occupation, we likewise obtain similar estimates using wage level and wage change analysis. Wage level specifications with less and more detailed occupational controls yield very similar results. We also examined occupational skill variables from the *Dictionary of Occupational Titles* measuring required verbal, numerical and spatial abilities. Following control for broad occupation, inclusion of *DOT* skill variables had little effect on union premium estimates.

13. To conserve space, we do not present coefficients from wage change models with separate estimates for union joiners, union leavers, union stayers, and nonunion stayers. As does Card (1996), we find roughly symmetrical wage gains and losses for union joiners and leavers.

switchers who also change industry and occupation. Card's adjustment for measurement error assumes a constant rate of misclassification across skill groups.[14] Among all measured union switchers, however, a smaller percentage of high-skill than low-skill workers report changing occupation and industry. For example, 22.5 percent of union switchers in the top wage quartile change industry and occupation, as compared to 31.6 percent in the bottom quartile. If not changing industry and occupation is a good proxy for the likelihood of misclassification, then applying a common misclassification rate to all measured union switchers will bias downward estimates for high-skill relative to low-skill groups.

A clear inference from our results is that unmeasured skills are high among union workers with low measured skills, and low among union workers with high measured skills, as compared to nonunion workers with similar measured characteristics. ''True'' quality-adjusted union wage effects vary little with respect to measured premarket skills. The conventional wisdom that union wage effects are substantially smaller for high-skill workers has little support once one accounts for worker-specific skills. As discussed earlier, skill homogeneity found in union workplaces is likely to derive from several sources. A dual selection process, along the lines modeled by Abowd and Farber (1982) and Card (1996, pp. 977–78) will produce such a pattern. Given a union premium and the compression of wage differences within union workplaces, employers hire only the most able among workers with low measured skills, while more able workers with high measured skills are unlikely to be in the union queue. By this scenario, it is union wage-leveling policies that lead to a compression in the union ability distribution. An alternative explanation, also consistent with our evidence, relies on the model provided by Wessels (1994), which shows that employer response to a uniform union tax on labor *may* lead to compression in the distribution of worker ability. This explanation for skill homogeneity emphasizes the role of employers given a uniform union tax, absent union compression of the wage distribution. It is worth emphasizing that the findings here are conditional on labor market sorting that has taken place in response to unionization. It does not follow that a randomly selected college graduate would receive as high a union premium, or a randomly selected high school dropout as low a premium, as reflected in the longitudinal estimates. These estimates reflect the union premium observed for given workers switching into or out of union jobs. Such a focus is appropriate, since we are most interested in union wage effects among actual or potential covered workers and not among workers who are unlikely to select or be selected for union employment.

V. Union Status and Skills: Evidence from the NLSY

In this section, we turn to the NLSY for additional evidence on union status and skills. The AFQT, a widely used measure of individual aptitude, was administered to the NLSY sample in 1981 (and renormed in 1989), with individuals

14. Card (1996) has large standard errors owing to use of a single CPS panel. He notes that identical rates of misclassification will produce greater bias among groups with the lowest rate of measured union transitions.

Table 3
Mean AFQT Scores and Union-Nonunion AFQT Differentials

		Predicted Wage		Education	
	All	Below Median	Above Median	School ≤12	School >12
Panel A: AFQT mean percentile scores by skill category and union status					
Union					
Weighted mean	40.50	29.45	48.17	35.10	53.75
Standard deviation	25.80	22.37	25.23	23.49	26.42
n	765	394	371	534	231
Nonunion					
Weighted mean	51.66	34.01	63.27	35.99	67.84
Standard deviation	28.66	23.56	25.63	23.70	23.97
n	4,112	2,044	2,068	2,282	1,830
All					
Weighted mean	50.09	33.36	61.20	35.83	66.61
Standard deviation	28.54	23.45	26.09	23.66	24.51
n	4,877	2,438	2,439	2,816	2,061
Panel B: WLS regression union-nonunion AFQT differential, by skill class					
Union coefficient	−9.230	−1.511	−13.316	−0.422	−9.699
Standard error	1.089	1.243	1.482	1.066	1.726
n	4,877	2,438	2,439	2,816	2,061

AFQT is measured in percentiles. All means and regression results use NLSY sample weights. The predicted wage is determined by coefficients from a log wage equation estimated with the nonunion sample and variables measuring premarket skills, demographic characteristics, and location (see text for details). Regressions in Panel B also include dummies for age, gender, race, and ethnicity.

ranging in age from 16 to 24 when tested. The AFQT represents the average of four tests included in the broader Armed Services Vocational Aptitude Battery, and is expressed as a percentile score. Our AFQT sample includes 4,877 private-sector wage and salary workers employed in 1991, ages 26 to 34.

The AFQT measure is useful in at least two ways. First, AFQT scores should be positively correlated with skills that are unmeasured in the CPS. If our interpretation of the CPS evidence is correct, then we should observe a decrease in relative union-to-nonunion AFQT scores as we move from workers with low to high skills. Second, AFQT provides an alternative measure of worker premarket skills. Thus, segmenting workers on the basis of low and high aptitude scores should produce a pattern of wage level and wage change union premium estimates similar to those produced when we segment on the basis of other premarket skills.

Table 3 presents means and regression results measuring union-nonunion differences in the AFQT. Because the NLSY oversamples minorities, all means and regression results are weighted using NLSY sample weights. In order to insure adequate sample sizes of union and nonunion workers, we divide the sample into only two skill groups based, alternatively, on the median of the predicted wage, schooling (workers with a high school diploma or less and those with at least some college), and the median of AFQT scores.

The full sample weighted mean of AFQT is the 50.1 percentile. Segmenting on the predicted wage we obtain a lower-half mean of 33.4 and upper-half mean of 61.2, while we obtain means of 35.8 and 66.6 based on low and high levels of schooling, respectively. Similar AFQT scores are found among lower-skill union and nonunion workers (union status is measured by collective bargaining coverage), with AFQT about 4 points lower among union workers when the division is based on predicted wages and 1 point lower when based on schooling. In sharp contrast, among higher-skilled workers (measured by predicted wages or schooling), union workers score an average 14–15 points lower than do nonunion workers. Panel B of Table 3 presents the coefficient on union status from weighted least squares (WLS) regressions with individual AFQT scores as the dependent variable.[15] Union-nonunion differences in AFQT are consistent with the means presented above. For the lower-skill groups of workers, union coefficients are close to 0 and insignificant. But among those with high predicted wages (schooling), AFQT scores are 13.3 (9.7) percentile points lower among union than nonunion workers.

Assuming that AFQT is positively correlated with the unmeasured skills controlled for in CPS wage change equations, the AFQT evidence supports our hypothesis that relative union-nonunion unmeasured skills are inversely related to measured skills. That being said, AFQT scores do not capture all or even most unmeasured characteristics valued in the labor market, in particular such attributes as skills acquired on the job, worker motivation, and personality.[16] The similarity in AFQT scores among less-skilled union and nonunion workers, coupled with evidence from this group of a sharp drop in union premium estimates as one moves from wage level to longitudinal analysis, suggests that there are important worker-specific skills correlated with union status but measured by neither standard CPS variables nor the AFQT.

We next provide evidence from the NLSY, analogous to that from the CPS, on differences in the union premium by skill group. The NLSY has disadvantages relative to the CPS—it includes a limited age range, and sample sizes of union changers are very small. Hence, we regard evidence from the NLSY as secondary to our primary evidence from the CPS. The NLSY has advantages, however. First, the AFQT aptitude score provides an alternative measure of worker-specific premarket skills, one that is not directly measured in the CPS. Second, the NLSY panel retains

15. Control variables included are dummies for age in 1981 when the test was administered (AFQT scores increase with age), gender, race, and Hispanic status. Similar union-nonunion differences are found when the sample is segmented by gender or race.

16. Wage level estimates of the union premium by skill group are affected little by the addition of AFQT to the wage equation. This suggests that aptitude scores reflect relatively few of the relevant unmeasured skills in wage level analysis. Cawley et al. (1996) provide evidence that AFQT and other aptitude measures are more weakly related to earnings and occupational choice than are schooling and family background measures. Neal and Johnson (1996), however, show that AFQT provides a good measure of black-white differences in premarket skills (not conditional on schooling or other variables), accounting for much of the racial wage gap subsequently observed in the labor market.

workers who have changed households and/or moved geographically. And third, measurement error in the union change variable may be reduced by extending the change period beyond one year (thus increasing the ratio of signal to noise) and by measuring wage changes among those who change union status *and* who record at least one job change during the period. This strategy provides a way to reduce measurement error in the union change variable without requiring that occupation and industry also change.

Table 4 presents union coefficients from NLSY wage level and change equations, with the sample segmented on the basis of predicted wages, schooling, and AFQT. The NLSY specification follows closely that used in the CPS analysis. The principal difference, besides the union variable, is that the wage change equations span a longer time interval (1989–91) and include dummy variables measuring changes in employer, region, and residence in a metropolitan area. Union status is measured by collective bargaining coverage, while the change in union status is equal to *ΔUnion*Chgjob,* where *Chgjob* equals 1 if the individual has changed employers over the period (separate control variables are included for *Chgjob* and *ΔUnion*(1 − Chgjob)*).

Using the full sample with private-sector earnings in both years (including 130 workers without an AFQT score), the union premium from the levels equation is 0.13, while that from the change equation is 0.08. Although similar qualitatively, NLSY premium estimates are lower than the full-sample CPS estimates of 0.18 and 0.12 from levels and change equations, respectively. When we segment the sample based on the predicted wage (the 1–45 and 55–99 percentiles), wage level estimates are 0.13 and 0.15 for the low- and high-skill groups, while longitudinal estimates are 0.05 and 0.08, respectively. Standard errors are large, with no differences close to statistical significance. When the sample is segmented by schooling group, wage level estimates are 0.18 and 0.02 for the groups with low and high schooling, respectively, while the corresponding longitudinal estimates are 0.06 and 0.11.

Finally, we segment workers based on low or high age-adjusted AFQT scores, measured by the residual from a WLS regression of the AFQT percentile score on a full set of age dummies.[17] Among workers with both low and high AFQT scores, the wage level premium estimate is 0.15. Longitudinal estimates, however, are 0.07 and 0.17 for the low- and high-ability groups, respectively. The (admittedly imprecise) NLSY results based on segmentation by predicted wages, schooling, and AFQT strongly reinforce our conclusions regarding the inverse relationship between relative union-nonunion measured and unmeasured skills.

17. A value of 0 indicates a score equal to the mean of one's age group. The low and high AFQT groups have scores at least 5 percentile points below and above the mean for their age group, respectively. The larger sample size for the low AFQT group reflects the NLSY oversampling of minorities and disadvantaged youth. The AFQT is not directly observable to employers. If AFQT reflects some productivity-related skills visible to employers, wages will vary accordingly. Hence, wage level and change estimates based on segmentation by AFQT should be similar to those based on segmentation by predicted wages and schooling. Hirsch and Schumacher (1996) examine blue-collar/white-collar differences in the union premium.

Table 4
Wage Level and Change Estimates of Union Differential, by Predicted Wage, Schooling, and AFQT Categories

Panel A: Predicted Wage Category	All	Low Skill	High Skill	F [p-value]
Level	0.130	0.129	0.154	0.288
	(0.022)	(0.030)	(0.034)	[0.591]
Change	0.077	0.048	0.084	0.269
	(0.032)	(0.042)	(0.054)	[0.604]
Sample size	3,775	1,676	1,726	
Number of changers	241	131	86	

Panel B: Schooling Category	All	School ≤12	School >12	F [p-value]
Level	0.130	0.178	0.022	10.426
	(0.022)	(0.026)	(0.041)	[0.001]
Change	0.077	0.060	0.106	0.433
	(0.032)	(0.039)	(0.060)	[0.511]
Sample size	3,775	2,223	1,552	
Number of changers	241	174	67	

Panel C: AFQT Category	All	Low AFQT	High AFQT	F [p-value]
Level	0.125	0.147	0.146	0.000
	(0.022)	(0.025)	(0.047)	[0.989]
Change	0.086	0.070	0.173	1.842
	(0.033)	(0.037)	(0.073)	[0.175]
Sample size	3,645	2,052	1,232	
Number of changers	232	173	43	

Shown are coefficients (standard errors) on a union dummy variable in the level equations and the change in union status in the change equations. The F and p values correspond to the null that coefficients in the "low" and "high" categories are equivalent. NLSY wage level equations are estimated for 1991, while change equations measure changes between 1989 and 1991. All NLSY results are from weighted least squares regression, using NLSY sample weights. The NLSY sample is composed of workers employed in the private sector in both 1989 and 1991. The levels specification is highly similar to that used with the CPS (see Table 1 note and text). In the NLSY change equation, the union status variable is $\Delta Union * Chgjob$. Additional variables included are changes in union status for those who do not change jobs, a change job dummy, and changes in experience squared, part-time status, metropolitan residence, region, industry, and occupation. "Low Skill" and "High Skill" categories in Panel A include workers in this sample with predicted wages in the 1–45 and 55–99 percentiles, respectively, of the prediction wage sample. "Low AFQT" and "High AFQT" categories in Panel C include persons with AFQT scores at least 5 percentile points below or 5 points above the mean for their age group, respectively.

VI. Conclusions

A universal finding in the empirical literature is that union wage effects are highest for workers with low levels of measured skills, and lowest among workers with high measured skills. In contrast to estimates from wage level regressions, our longitudinal estimates using CPS panel data are very similar across groups of workers with different measured levels of premarket skills. The clear inference is that there exists positive sorting among workers with low measured skills and negative sorting among those with high measured skills, leading to substantial skill homogeneity in union workforces. Direct evidence from the NLSY on AFQT scores confirm that as measured skills increase, aptitude scores decrease for union relative to nonunion workers. Longitudinal estimates are shown to be sensitive to sample selection criteria and the designation of "true" union switchers. Deleting workers with allocated union status and earnings, and basing union premium estimates on reported union switchers who also change industry and occupation, provide what we believe is a reliable strategy to reduce what is otherwise considerable measurement error bias in longitudinal premium estimates.

Our analysis does not identify the specific routes through which skill compression within union workplaces is realized. Skill homogeneity results in part from sorting that occurs in response to union wage standardization policies that decrease earnings differences across and within job categories. Employer selection truncates the bottom tail of the skill distribution, while employee sorting results in there being relatively few high-skill workers in the union queue. Low dispersion in skills is also likely to reflect union success in organizing establishments where worker preferences and skills are relatively similar. A compressed distribution of skills and equivalent union wage effects across skill levels can also be explained by a variant of a model proposed by Wessels (1994), wherein a uniform union tax on wages may lead to an upgrading of ability among lower-skill workers but not among higher-skill workers. Our results cast doubt on the thesis that unions both seek *and* acquire larger proportional wage gains for low-skill than for high-skill members. Once labor market sorting has occurred, union wage premiums are highly similar for workers with different levels of skills.

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