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Does Unmeasured Ability Explain Inter-Industry Wage Differentials?

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This paper provides empirical assessments of the two leading explanations of measured inter-industry wage differentials: (1) true wage differentials exist across industries, and (2) the measured differentials simply reflect unmeasured differences in workers' productive abilities. First, we summarize the existing evidence on the unmeasured-ability explanation. Second, we construct a simple model which shows that if matching is important then endogenous job-change decisions can create important self-selection biases even in first-differenced estimates of industry wage differentials. Third, we analyze a sample that approximates the experiment of exogenous job loss. We find that (i) the wage change experienced by a typical industry switcher closely resembles the difference in the relevant industry differentials estimated in a cross-section, and (ii) pre-displacement industry affiliation plays an important role in post-displacement wage determination.

1. INTRODUCTION

Several recent studies have shown that there are large and persistent wage differentials among industries, even after controlling for a wide variety of worker and job characteristics.[1] The pattern of these differentials is remarkably stable over time and similar across countries with distinct labour-market institutions. These facts suggest that the differentials are neither transitory disequilibrium phenomena nor artifacts of particular collective bargaining arrangements or government interventions in the labour market.

One explanation of persistent measured wage differences among observationally similar workers in competitive labour markets rests on differences in workers' productive abilities that are not captured in individual-level data sets: high-ability workers earn high wages; industries that employ proportionately more high-ability workers pay higher average wages to observationally equivalent workers. An alternative explanation of measured inter-industry wage differences, of course, is that true wage differentials exist across industries, even for identical workers. Such industry wage differentials arise in models of compensating differences, rent sharing, and efficiency wages, among others.[2]

This paper provides empirical assessments of these unmeasured-ability and true-industry-effects explanations of the measured inter-industry wage differences.[3] In Section

1. See Dickens and Katz (1987*a*, *b*), Helwege (1989), Krueger and Summers (1987, 1988) and Murphy and Topel (1987, 1990).
2. See Rosen (1986) on compensating differences, Blanchflower, Oswald and Garrett (1990), Katz and Summers (1989) and Nickell and Wadhwani (1990) on rent sharing, and Katz (1986) on efficiency wages.
3. See Murphy and Topel (1989) for an alternative approach to assessing the impact of ability bias on estimates of inter-industry wage differentials.

2 we summarize the existing empirical work on the unmeasured-ability explanation of inter-industry wage differences. This work uses longitudinal data to study the wage changes experienced by industry switchers. Such first-differenced estimation of industry wage effects eliminates biases caused by unmeasured productive ability, *provided* that ability is equally valued in all industries and that market perceptions of worker quality are time-invariant.

In Section 3 we develop a model in which unmeasured productive ability is *not* equally valued in all industries; instead, there is matching. In this model, learning causes the market's perception of the potential match between a worker's productive ability and each industry's technology to vary over time. Endogenous mobility decisions then determine the worker's wage and industry affiliation at each date: inter-industry mobility improves the allocation of workers to industries as new information about their abilities becomes available. The model yields measured inter-industry wage differences that are solely attributable to unmeasured productive ability, but also predicts that (self-selected) industry switchers will experience wage changes that are of the same sign as and possibly of similar magnitude to the difference in the relevant industry differentials estimated in a cross-section.

The model in Section 3 illustrates the potential importance of self-selection biases arising from the endogeneity of job and industry changes in the existing first-differenced estimates of industry wage differentials. (We discuss below how analogous endogeneity problems may affect first-differenced estimates of compensating differentials, the union wage premium, and employer-size wage effects.) In principle, there are two ways to cope with these endogeneity problems. First, one could continue to use the data used in existing first-differenced estimates (namely, a random sample of all job-to-job transitions) but apply empirical techniques that attempt to correct for self-selection. Second, one could continue to estimate first-differenced regressions but restrict attention to a sample designed to minimize the importance of self-selection. In this paper, we pursue the latter approach; in Gibbons, Katz, and Lemieux (1991) we pursue the former. Thus, the empirical work in this paper (Sections 4 and 5) should *not* be viewed as testing the model in Section 3. Rather, the model should be viewed as illustrating the problems with existing estimates and as motivating our choice of an alternative sample.

In Sections 4 and 5, we approximate the natural experiment of exogenous job loss by using data on workers displaced by plant closings.[4] In Section 4 we use a first-differenced regression to determine the wage changes experienced by industry switchers from our sample of workers displaced by plant closings. We find that first-differenced and cross-section industry differentials are very similar even for this sample of (approximately) exogenously displaced workers. This finding is consistent with a true-industry-effects model, but is quite difficult to reconcile with a pure unmeasured-ability model. In Section 5 we study the impact of a worker's *pre*-displacement industry on his or her *post*-displacement earnings. We find that workers displaced by plant closings maintain about 45% of their pre-displacement industry wage premiums when they are re-employed.

We conclude that neither of the contending explanations fits the evidence without recourse to awkward modifications. Perhaps the simplest single explanation for our findings is a modified version of the true-industry-effects explanation, in which the traits that help a worker find employment in a high-wage industry once are likely to do so

4. This sample restriction is motivated by our earlier work, Gibbons and Katz (1991). We discuss it further below.

again. One such trait could be infra-marginal tolerance for unpleasant working conditions; we discuss this and other possibilities in Section 6.

2. SUMMARY OF EXISTING EVIDENCE ON THE ROLE OF UNMEASURED ABILITY

The simplest unmeasured-ability explanation of inter-industry wage differences is based on two observations. First, there is evidence that workers are sorted across industries by measured human capital: Dickens and Katz (1987a) and Topel (1989) find that observable dimensions of human capital that are associated with higher wages—such as education and experience—are also associated with employment in high-wage industries. Second, there may be a great deal of variation in unmeasured human capital: among all workers with a college degree, for instance, only some have performed well at demanding institutions. The simplest unmeasured-ability explanation of inter-industry wage differences thus amounts to the conjecture that the forces that cause sorting by measured human capital cause similar sorting by unmeasured human capital. In this case, estimates of industry wage differentials using cross-section individual-level data sets will overstate true industry differentials.

Given longitudinal data on the wages of a given individual as he or she switches industries, first-differenced (or fixed-effects) estimation eliminates the impact of a worker-specific, time-invariant fixed effect on the estimated industry differentials. Under the assumption that unmeasured productive ability is time-invariant and equally rewarded in all industries, first-differenced regressions eliminate the impact of unmeasured ability on the measured industry effects.[5] Much of the existing empirical work on the unmeasured-ability explanation of inter-industry wage differences attempts to exploit this property of first-differenced regressions.[6]

Krueger and Summers (1988) present estimates of the effects of industry switches on wages through a first-differenced regression on matched May Current Population Survey (CPS) data. After attempting to correct for false industry transitions (by utilizing outside information on the frequency of such false transitions), Krueger and Summers estimate that the industry wage differentials from the first-differenced regression are significant, of the same sign as, and close in magnitude to the cross-section regression estimates. In other words, (after controlling for other observables) workers moving from high- to low-wage industries experience a wage decrease, while those moving from low- to high-wage industries experience a wage increase. Moreover, the size of these wage changes is similar to the difference between the relevant industry wage differentials estimated in a cross-section.[7] Krueger and Summers conclude that their empirical finding casts "serious doubt on 'unmeasured labour quality' explanations for inter-industry wage differences" (p. 260).[8]

5. To foreshadow the argument below, however, note that the assumption that ability is fixed for a worker does *not* imply that ability is a worker-specific fixed effect in a cross-section earnings equation. Only if ability is equally valued in each industry (as we assume here, temporarily) does it become a fixed effect, and thus disappear in first-differenced estimation. See Stewart (1983) and Lemieux (1991) for a related argument in the context of the estimation of union wage differentials from panel data.

6. An alternative approach is to add further proxies for ability, such as measures of IQ and Knowledge of the World of Work, to a cross-section earnings function that also includes industry dummies; see Blackburn and Neumark (1991).

7. More precisely, Krueger and Summers find that the standard deviations of their estimated cross-section and first-differenced industry log wage differentials are both approximately 0·12, and that the correlation between their cross-section and first-differenced estimates is 0·96.

8. It is worth noting, however, that their findings are quite sensitive to their proposed correction for false industry transitions. We address the issue of false transitions in Section 4.

Murphy and Topel (1987, 1990) also use longitudinal data to estimate first-differenced regressions. They use a sample of males from matched March CPS data. In contrast to Krueger and Summers, Murphy and Topel find that industry-switchers receive only 27 to 36% of the cross-sectional differential. Murphy and Topel (1987) conclude that "nearly two-thirds of the observed industry differences are estimated to be caused by unobserved individual components" (p. 135). One possible reason for this much lower estimate is that Murphy and Topel use information on each worker's aggregate annual earnings (i.e. earnings across all jobs held during the year) and primary industry affiliation for the year, rather than information on a worker's earnings and industry affiliation at a point in time. Thus, Murphy and Topel estimate the relation between (i) the change in the wage differentials associated with a worker's primary industry affiliations for consecutive years and (ii) the change in the worker's aggregate annual earnings.[9] Because the two annual-earnings measures used to construct the wage-change variable for the first-differenced regression are likely to contain earnings from the same job, the estimate of the impact of the change in a worker's industry differential on the change in earnings is likely to be biased downward.[10]

3. A SIMPLE MODEL OF ENDOGENOUS INTER-INDUSTRY MOBILITY

In this section we develop a simple model to illustrate the difficulties in using first-differenced regressions on a sample of potentially self-selected industry switchers to attempt to differentiate between the true-industry-effects and unmeasured-ability explanations for industry wage differentials. This model generates inter-industry wage differences among observationally equivalent workers that are *solely* attributable to unmeasured differences in workers' productive abilities, yet the model also predicts that workers who change industries experience wage changes of the same sign as and of similar magnitude to the difference in average wages between the relevant two industries, just as would be the case in a true-industry-effects model in the absence of a correlation between unmeasured ability and industry affiliation.

The first key element of the model is that, unlike the model implicitly underlying the empirical work discussed in Section 2, here unmeasured productive ability is *not* equally valued in different industries; rather, there is matching.[11] The second key element of the model is that, as will become clear below, mobility is *not* exogenous: whether a worker changes jobs is endogenous, as is the industry of the new job the worker finds. In support of these elements of the model, we note that labour mobility generated by mismatching (caused either by changes in perceptions of worker abilities or by changes in assessments of idiosyncratic worker-job match values) appears to be quantitatively important: Jovanovic and Moffitt (1990) estimate that the bulk of labour mobility for young males in the United States is caused by mismatch rather than by sectoral demand shifts.

Matching models of wage determination have been important in the literature since Roy's (1951) study of the income distribution. Roy's two-sector model involves a two-

9. Murphy and Topel (1990) restrict their sample to individuals who changed their industry or occupation between interview dates $t-1$ and t and who were still employed in their new industry-occupation cell at interview date $t+1$. This sample restriction helps eliminate false transitions, and also is likely to eliminate some moves to transitory jobs such as the low-wage, short-term jobs that high-ability workers might take in the process of searching for new high-wage jobs that allow them to utilize their talents.

10. Murphy and Topel (1987) propose an approximate correction for this problem.

11. More precisely, in Section 2, industry technologies may be differentially sensitive to ability, but ability is equally rewarded in all industries in equilibrium.

dimensional definition of ability (i.e. Roy's is a two-factor model). The model we describe below, in contrast, is a two-sector model but involves a one-dimensional definition of ability (i.e. ours is a one-factor model). In such a one-factor model, a worker who is more able in one industry is also more able (but perhaps not as well matched) in another industry. This property of one-factor models disciplines our empirical analysis: it imposes restrictions on the observed patterns of wages and mobility decisions that are consistent with the model. In the n-sector, n-factor case, in contrast, any observed pattern of wages and mobility decisions can be reconciled with some configuration of model parameters, unless one imposes strong distributional assumptions.

Recent work has begun to account for endogenous mobility decisions in matching models of sectoral wage differentials. Heckman and Sedlacek (1985), for example, have analysed both wages and mobility in a dynamic version of Roy's model in which mobility is endogeneously determined by shifts in the demand for labour across sectors. Our model complements the Heckman–Sedlacek approach by emphasizing learning about individual workers' abilities rather than shifts in relative demand.[12]

Information about ability is symmetric throughout the model. Information is imperfect initially but improves over time: the market observes a noisy (and non-manipulable) signal about each worker's ability at the time of hiring, and a subsequent productivity observation provides more information. The noisy ex ante signal results in imperfect matching of workers to industries; the productivity observation results in improved matching. Over time, high-ability (low-ability) workers endogenously gravitate to the industries with ability-sensitive (ability-insensitive) technologies.

We assume that neither the initial signal nor the subsequent productivity observation is observable by an econometrician using standard individual-level data. (Think of the model as describing a cohort of workers with a given number of years of education, as recorded by the CPS, and think of the signal as representing resume information about academic performance and institutional quality.) The econometrician observes only a worker's wage and industry affiliation in each period.

Formally, the model involves two ability levels, two industries, two periods, and two values of the noisy signal (but the biases emphasized in our simple model will arise in any model in which inter-industry mobility improves the allocation of workers to industries as new information about their abilities arrives). Specifically, ability η is either high or low: $\eta \in \{\eta_H, \eta_L\}$. Output in industry A is more sensitive to ability than is output in industry B:

$$y_{AH} > y_{BH} > y_{BL} > y_{AL}, \tag{1}$$

where y_{ij} is the output in industry i of a worker of ability η_j. Ability entirely determines output—there is no effort-elicitation problem. These output levels are constant over time.

Given perfect information and a competitive labour market, high-ability (low-ability) workers would be employed in industry A (B) and would earn high (low) wages, but there would be no mobility across industries. We assume, however, that information is imperfect but symmetric. All parties observe the noisy signal s before hiring occurs. The signal can take two values, $s \in \{s', s''\}$, where $s' > s''$. A higher signal leads to a higher posterior probability, $p(s)$, that the worker is of high ability: $1 > p(s') > p(s'') > 0$. We assume that the signal is accurate enough that the following conditions on expected productivity hold:

$$p(s')y_{AH} + [1-p(s')]y_{AL} > p(s')y_{BH} + [1-p(s')]y_{BL}, \tag{2}$$

12. See Bull and Jovanovic (1988) for a model of earnings dynamics that incorporates mobility generated both by learning about match quality and by sectoral-demand shifts.

and,

$$p(s'')y_{AH}+[1-p(s'')]y_{AL}<p(s'')y_{BH}+[1-p(s'')]y_{BL}. \tag{3}$$

Thus, productive efficiency dictates that high-signal (low-signal) workers begin their employment in industry A (B).

We consider a competitive labour market populated by risk-neutral workers. We assume that output is observed by all parties, so ability is publicly known after period one. In this setting, there is no loss of generality in restricting attention to single-period compensation contracts that specify the period's wage before the period's production occurs. In each period, firms in each industry bid wages up to expected output in that industry (conditional on the publicly observed information available at that date) and workers choose to work in the industry that maximizes their current wage.

In period one, high-signal workers are employed in industry A and earn the wage

$$w_{1A}=p(s')y_{AH}+[1-p(s')]y_{AL}, \tag{4}$$

while low-signal workers are employed in industry B and earn the wage

$$w_{1B}=p(s'')y_{BH}+[1-p(s'')]y_{BL}. \tag{5}$$

Note that $w_{1A}>w_{1B}$ because $p(s')>p(s'')$ and equation (2) holds. After the first period of production, output perfectly reveals ability and the match between workers and industries improves. In period two, high-ability workers are employed in industry A and earn the wage $w_{2A}=y_{AH}$, while low-ability workers are employed in industry B and earn the wage $w_{2B}=y_{BL}$.

Recall that we assume that neither the initial signal nor the subsequent productivity observation is observable by an econometrician using standard individual-level data; the econometrician observes only a worker's wage and industry affiliation each period. Thus, although there are no true industry effects, there are persistent measured inter-industry wage differences: $w_{tA}>w_{tB}$ for $t\in\{1,2\}$. Furthermore, workers who move from the high-wage industry A to the low-wage industry B experience a wage decrease (from w_{1A} to w_{2B}), while workers making the reverse transition experience a wage increase (from w_{1B} to w_{2A}). In fact, depending on the parameters of the model, the wage changes for industry switchers can be identical in magnitude to the cross-section industry wage differential; see Appendix *B*. Finally, in this model, the inter-industry wage differential grows with experience (i.e. $w_{2A}-w_{2B}>w_{1A}-w_{1B}$), but this is not true in general; see Appendix B for a simple counter-example.

We conclude from this model that self-selection biases arising from the endogeneity of job and industry changes may be important in the first-differenced regressions using matched CPS data summarized in Section 2. Related biases also may be important for longitudinal estimates of other wage gaps.[13] Puzzling estimates of compensating differentials using cross-section data, for example, are often attributed to omitted-variable bias in which workers with high unmeasured ability both earn higher wages and work on jobs with better working conditions than do workers with low unmeasured ability. Our model suggests that there may be a similar explanation for the often perverse longitudinal estimates of compensating differentials (e.g. Brown (1980)): workers moving in response to good news concerning their abilities are likely to move to jobs with both higher wages and better working conditions, while the reverse is likely to occur for workers moving in response to bad news concerning their abilities. Similarly, fixed-effects estimation is also

13. See Solon (1988) for an alternative model that illustrates self-selection biases in longitudinal estimation of wage gaps.

unlikely to purge estimates of union wage differentials (e.g. Freeman (1984)) and employer-size wage effects (e.g. Brown and Medoff (1989)) of unmeasured-ability bias for samples of potentially endogenous movers.

4. WAGE CHANGES FOLLOWING EXOGENOUS JOB LOSS

In the next two sections we propose and implement empirical strategies designed to reduce the importance of the biases arising from the endogeneity of job changes in the estimation of inter-industry wage differentials. We first provide evidence on the wage changes of industry switchers following exogenous job loss. Models of true industry effects predict that first-differenced regression estimates of industry differentials on such a sample should be similar to cross-section estimates. While some unmeasured-ability models (such as the model developed in Section 3) also yield this prediction for a sample of endogenous movers (e.g. workers who change jobs in response to new information about their abilities), these unmeasured-ability models do not yield this prediction for a sample of workers in which job separations are exogenous.

To construct a sample of (approximately) exogenous job changers, we use data from the January 1984 and 1986 CPS Displaced Workers Surveys (DWS). This data set provides information on current wage and industry as well as on pre-displacement wage and industry for workers who permanently lost a job during the five years prior to the survey date. We examine a sample of workers between the ages of 20 and 61 at the survey date who were displaced from a full-time, private-sector, non-agricultural job because of a plant closing, slack work, or a position or shift that was eliminated. Workers displaced from construction jobs were eliminated from the sample because it is difficult to formulate an appropriate definition of permanent displacement from a construction job.

In order to study the longitudinal evidence provided by industry switchers, we restricted the sample to individuals who were re-employed at the survey date; these are the only individuals for whom pre- and post-displacement earnings information is available in the DWS. (The CPS does not provide current earnings information for those workers who entered self-employment, so our sample consists of workers who found new jobs in wage-and-salary employment.) We also restricted the sample to those who had re-employment earnings of at least \$40 a week. These restrictions produced a sample of 5224 displaced workers. Basic descriptive statistics for this sample are presented in column (1) of Table A1 in Appendix A.

Since this DWS sample contains only workers who actually lost jobs, the ratio of false industry transitions to reported industry transitions is likely to be much smaller than in the matched CPS samples utilized in earlier work. Thus, in our data there is likely to be a much smaller downward bias from measurement error in first-differenced estimates of the relationship between a worker's wage change and the change in the relevant industry wage differentials.[14] We make no attempt to correct our estimates for bias arising from false industry transitions, both because we believe the bias is likely to be small and because we know of no persuasive way to perform an approximate correction for the DWS sample.

14. To make this point more concrete, suppose that the probability of job change is j, the conditional probability of switching industries given job change is s, and the (independent) probability of industry miscoding is m. Then (ignoring miscoding that makes true industry switchers appear not to have switched industries) the fraction of recorded industry transitions that do not correctly record a true switch is $m/[(js+(1-js)m]$. The key point is that in the DWS we have $j=1$, whereas in the matched CPS a much smaller j, such as $j=0{\cdot}2$, might be reasonable. Taking $s=0{\cdot}7$ and $m=0{\cdot}1$, as an example, we then have an error rate of 14% in the DWS but of 44% in the CPS.

In other work using this sample of displaced workers (Gibbons and Katz (1991)), we motivated and documented an important distinction between two sub-samples of the data: workers displaced by plant closings and those displaced by layoffs.[15] We developed an asymmetric-information model of endogenous wage-setting and turnover in which, if firms have discretion over whom to lay off, they dismiss their least-able workers. In equilibrium, the market infers that laid-off workers are of low ability and so offers them low re-employment wages, relative to the re-employment wages of those displaced by plant closings (for whom we assume that no adverse inference about ability is warranted). Empirically, we found that laid-off white-collar workers indeed receive lower re-employment wages than do observationally equivalent white-collar workers displaced by plant closings, and that there is no analogous difference for blue-collar workers. (This difference in findings for white- and blue-collar workers is consistent with blue-collar workers being likely to be covered by formal last-in-first-out layoff procedures.) In addition, we found that (consistent with our asymmetric-information model, in which it is the layoff event that conveys information to the market) there is no difference between the *pre*-displacement wages of laid-off workers and those of observationally equivalent workers displaced by plant closings.

We conclude from our earlier work that at least some laid-off workers are not exogenously displaced. Rather, they are effectively fired for poor performance. In an attempt to construct a sample of exogenously displaced workers, therefore, we hereafter focus on workers displaced by plant closings.[16] Descriptive statistics for the plant-closings and layoffs sub-samples are given in columns (2) and (3) of Table A1.

In this section, we use our 1984–86 DWS plant-closings sample to mimic the empirical strategies of Krueger and Summers and of Murphy and Topel. First, we estimate industry differentials from the cross-section wage function

$$\ln w_{it} = X_{it}\delta + \sum \alpha_j D_{ijt} + u_{it}, \tag{6}$$

where $\ln w_{it}$ is log weekly earnings for individual i at time t, X_{it} is a vector of individual characteristics, region dummies and occupation dummies, D_{ijt} is a dummy variable equal to one if individual i was employed in industry j at time t, and u_{it} is an error term. We estimate equation (6) for the plant-closings sample using *pre*-displacement earnings, industry, occupation, and individual characteristics. Column (1) of Table 1 presents estimated cross-section industry wage differentials relative to the base industry (retail trade), using what we hereafter refer to as "1·5-digit" industry definitions.[17] The estimated industry differentials for the plant-closings sample are substantial in magnitude, highly

15. We classify workers as displaced by a plant closing if they were displaced because their plant or company closed down or moved. We classify workers as displaced by a layoff if the plant or company from which they were displaced was still operating at the time of displacement, and the reason for displacement was slack work or position or shift abolished. The vast majority of those we classified as displaced by layoffs reported themselves as having been displaced because of slack work.

16. Krueger and Summers (1988) also analyze a sample from the 1984 DWS but include workers displaced by layoffs as well as those displaced by plant closings. (In fact, laid-off workers make up half the sample.) Given the evidence that laid-off white-collar workers are not exogenously displaced and the potential importance of endogenous job-change decisions for first-differenced estimates of industry differentials (as described in Section 3), we eliminate laid-off workers from the sample. We also present evidence below that suggests that failing to eliminate laid-off workers from the sample indeed imparts an upward bias to first-differenced estimates.

17. We disaggregated our sample into 20 distinct industries. These industry definitions are slightly finer than the CPS "major industries." The 1980 Census Industry Classification Codes for the 3-digit industries contained in each of our 1·5-digit industries are presented in Table A2 of Appendix A; the distributions of our entire DWS sample and of the plant closing and layoffs sub-samples by 1·5-digit pre-displacement industry are given in Table A3 in Appendix A. The size of our DWS sample prevented us from using a more detailed industry classification scheme. Our basic findings are quite similar when traditional 1-digit industries are used and qualitatively similar (but a bit noisier) when CPS "detailed industries" (i.e. 2-digit industries) are used.

TABLE 1

Industry wage differentials from cross-section and first-differenced regressions

January 1984 and 1986 CPS displaced workers survey plant closing sub-sample

Industry	(1) Cross-section[a]	(2) First-differenced[b]
Mining	0·510	0·429
	(0·043)	(0·051)
Primary Metals	0·223	0·262
	(0·053)	(0·055)
Fabricated Metals	0·177	0·196
	(0·049)	(0·052)
Machinery, except Electrical	0·273	0·248
	(0·040)	(0·042)
Electrical Machinery	0·131	0·083
	(0·048)	(0·047)
Transportation Equipment	0·274	0·272
	(0·045)	(0·047)
Lumber, Furniture	0·045	0·069
	(0·045)	(0·051)
Other Durables	0·164	0·091
	(0·047)	(0·046)
Food	0·195	0·170
	(0·045)	(0·048)
Textiles, Apparel	−0·005	0·053
	(0·040)	(0·045)
Paper, Printing	0·146	0·076
	(0·050)	(0·051)
Chemicals, Petroleum	0·267	0·186
	(0·044)	(0·045)
Transportation	0·329	0·130
	(0·041)	(0·044)
Utilities	0·262	0·285
	(0·064)	(0·057)
Wholesale Trade	0·154	0·085
	(0·036)	(0·034)
Retail Trade	—	—
FIRE	0·263	0·162
	(0·052)	(0·040)
Business, Professional Services	0·217	0·045
	(0·038)	(0·036)
Personal Services	0·013	−0·008
	(0·043)	(0·039)
Other Services	0·067	−0·036
	(0·046)	(0·036)
R^2	0·451	0·131
n	2576	2576

[a] The dependent variable is log (pre-displacement weekly earnings). The reported estimates are the coefficient values for the pre-displacement industry dummy variables. The base industry is retail trade. The reported regression also includes eight pre-displacement occupation dummies, a spline function in previous tenure (with breaks at one, two, three, and six years), years of schooling, pre-displacement experience and its square, a marriage dummy, a female dummy, a non-white dummy, year of displacement dummies, three region dummies, and interactions of the female dummy with marriage and the experience variables.

[b] The dependent variable is log(post-displacement weekly earnings/pre-displacement weekly earnings). The reported estimates are the coefficient values for the difference between the post-displacement and pre-displacement industry dummy variables. The base industry is retail trade. The reported regression also includes eight occupation change dummy variables; three dummy variables for post-displacement employment in agriculture, construction, or public administration; experience and experience interacted with the female dummy variable; years since displacement, and year-of-displacement dummy variables. The numbers in parentheses are standard errors.

statistically significant, and quite similar to those estimated in other data sets. Earnings in mining, transportation equipment, primary metals, transportation, and chemicals, for example, are substantially above those in textiles, retail trade, furniture, and most service industries, even with controls included for years of schooling, potential experience, years of seniority, occupation, region, and gender. The standard deviation of the estimated 1·5-digit industry wage differentials is 0·12.[18]

Second, we follow Krueger and Summers in estimating the first-difference of equation (6),

$$\Delta \ln w_{it} = \Delta X_{it}\delta + \sum \beta_j \, \Delta D_{ijt} + \Delta u_{it}. \tag{7}$$

Note that the β_j coefficients reflect the *relative* log-wage changes experienced by industry switchers. The *absolute* wage loss experienced by the typical displaced worker is reflected in the coefficient on the intercept in equation (7).

Under the assumption that unmeasured productive ability is time-invariant and equally rewarded in all industries (i.e. the error term u_{it} in equation (6) can be written as $\theta_i + v_{it}$, where θ_i is the ability of worker i and v_{it} is white noise), the first-differenced regression yields unbiased estimates of the industry differentials. If the estimated cross-section industry wage differentials—the α_j coefficients in equation (6)—are entirely due to the sorting of workers across industries by unmeasured ability that is equally valued in all industries, then the β_j coefficients in equation (7) should all equal zero. On the other hand, if the estimated cross-section industry wage differentials are entirely due to true industry effects, then the β_j coefficients in equation (7) should be identical to the α_j coefficients in equation (6).

Column (2) of Table 1 presents estimates of the β_j coefficients in equation (7) using the plant-closings sample with the change in log weekly wages (i.e. the log of the ratio of re-employment weekly earnings (at the survey date) to pre-displacement weekly earnings) as the dependent variable. The estimated industry differentials from the first-differenced regression are quite similar to the cross-section estimates in column (1). We summarize the relation between these two sets of estimates in Figure 1, which plots the β_j's against the corresponding a_j's. The (unweighted) regression line through the points in Figure 1 has an intercept of $-0{\cdot}01$, a slope of 0·79, and an R^2 of 0·72. The reverse-regression estimate of this slope is 1·10.

One potential problem with these findings is that workers may take temporary jobs after displacement that do not fully utilize their talents. In an attempt to avoid this problem, we re-estimated equations (6) and (7) on the sub-sample of workers displaced at least two years. The results differ only slightly from those just reported. The standard deviations of the resulting industry wage differentials estimated from cross-section and first-differenced equations are 0·13 and 0·12, respectively. The regression through the points in the plot analogous to Figure 1 has an intercept of $-0{\cdot}03$, a slope of 0·87, and an R^2 of 0·76. These results also suggest that potential sample-selection biases resulting from the omission from our sample of workers unemployed at the survey date are likely to be small.

In order to assess the empirical support for our theoretical arguments that (a) endogenous job change can create important biases in first-differenced estimates of industry wage differentials and (b) some laid-off workers are effectively fired for poor performance rather than exogenously displaced, we re-estimated equations (6) and (7) using the layoffs sub-sample from the DWS. We find an even stronger similarity between

18. All reported standard deviations of industry wage differentials have been corrected for sampling error following the approach of Krueger and Summers (1988).

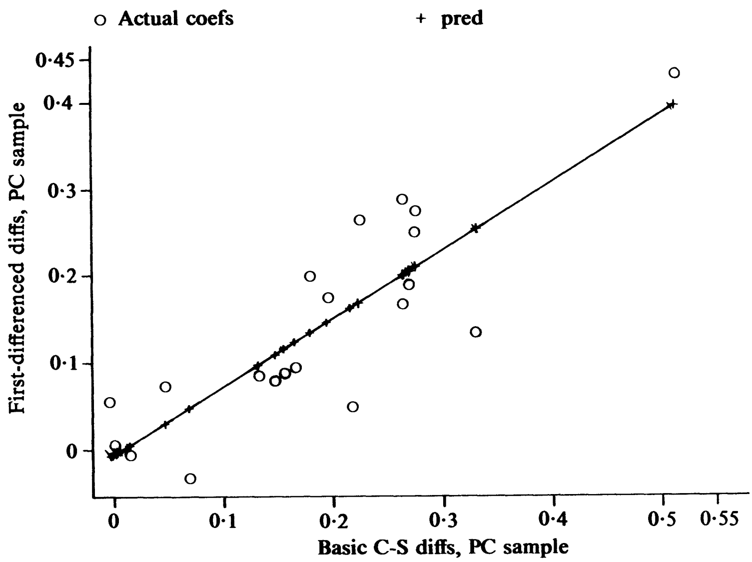

FIGURE 1
First-differenced vs. C – S ind diffs, PC sample

the estimated cross-section differentials (α_j's) and the estimated first-differenced differentials (β_j's) than we do for the plant-closing sample. The regression of the β_j's against the corresponding α_j's for the layoffs sample yields a slope coefficient of 0·971 and an R^2 of 0·81. Thus, industry switchers in the layoffs sample appear to earn 97% of the relevant cross-section differential, while the analogous figure for the plant-closing sample is approximately 75 to 80%. This difference between the two samples is consistent with the possibility that endogenous job change may impart a significant upward bias in first-differenced regression estimates of industry differentials on samples not restricted to exogenous job changers. Furthermore, this comparison of the layoffs and plant-closings sub-samples probably understates the true bias from endogenous job change because some of the displacements in the layoffs sample (such as those in unionised jobs) were likely exogenous.

We draw two conclusions from the evidence presented in this section. First, industry switchers experience wage changes that are of the same sign as and of similar magnitude to the difference in the relevant industry differentials estimated in a cross-section. This evidence is quite consistent with an important role for true industry effects in explaining the inter-industry wage structure. Furthermore, this evidence leads us to reject the simplest unmeasured-ability explanation: the vast majority of inter-industry wage differences *cannot* be explained by the sorting of workers across industries by unmeasured productive ability that is time-invariant and equally valued (at the margin) in all industries.

Second, this evidence on the wage changes of industry switchers comes from a sample in which all job changes were caused by plant closings. The matching model we developed in Section 3 could explain such an empirical result for a data set that consists mainly of workers who switched industries in response to changes in market perceptions of their abilities, but quits (and even layoffs) are excluded from our plant-closings sample. This leaves open the possibility that a variation on the model developed in Section 3 could account for the evidence presented here. Such a model would have to explain (i) which workers displaced in a plant closing switch industries, (ii) why those who switch after a plant closing did not do so before, and (iii) why the wage change of a typical industry switcher mimics the difference in the average wages for the relevant industries.

One simple conjecture is that those who switch industries after a plant closing are those who recently learned that they are better matched in another industry but have not yet found a new job there. Since column (2) of Table A1 reveals that 69% of workers displaced in a plant-closing change industries, we find this conjecture implausible. We have been unable to develop any other one-factor unobserved-ability model that fits the evidence presented in this section. (See Appendix C for brief summaries of two failed attempts, one emphasizing firm-specific human capital, the other sectoral shifts.) Of course, as noted in Section 3, one could develop an *n*-sector, *n*-factor unobserved ability model that is consistent with any observed pattern of wages and mobility decisions. We therefore offer only a circumspect conclusion: the existing one-factor variants of the unmeasured-ability explanation of inter-industry wage differentials are rejected by the facts concerning the wage changes of (approximately exogenously displaced) industry switchers.

5. THE EFFECT OF PRE-DISPLACEMENT INDUSTRY ON POST-DISPLACEMENT WAGE

We now turn to the possibility that an exogenously displaced worker's re-employment industry may be endogenous. Before describing our empirical analysis, it is important to note that the potential endogeneity of an exogenously displaced worker's re-employment industry does not alter our interpretation of the empirical results presented in the previous section: we find it difficult to develop a plausible unobserved-ability model that fits our empirical findings, *whether or not* re-employment industry is endogenous.

To eliminate the influence of re-employment industry and any other potentially endogenous re-employment variable on the re-employment wage, we estimate the following equation:

$$\ln w_{it} = X_{i,t-1}\delta + \sum \gamma_j D_{ij,t-1} + \varepsilon_{it}, \tag{8}$$

where: w_{it} is the *post*-displacement weekly earnings of individual i at date t; X_{it-1} is a vector of (almost entirely) *pre*-displacement individual characteristics and occupation dummies, but including neither pre- nor post-displacement industry dummies;[19] $D_{ij,t-1}$ is a dummy variable equal to one if individual i was displaced from industry j; and ε_{it} is an error term.[20] The coefficients of interest in equation (8) are the γ_j's, which measure the impact of pre-displacement industry on post-displacement earnings.

The essence of an unmeasured-ability explanation for measured cross-section industry differentials implies that, conditional on workers' observed pre-displacement characteristics, workers exogenously displaced from high-wage industries should have higher *post*-displacement wages than should those exogenously displaced from jobs in low-wage industries. In terms of equations (6) and (8), unmeasured-ability explanations imply that the γ_j's should be positively related to the α_j's. In a model of true industry effects,

19. Two of the individual characteristics are measured as of the survey date and so are post-displacement variables: years since displacement and a dummy variable for whether the individual is married with spouse present.

20. Addison and Portugal (1989) and Kletzer (1989) also estimate similar post-displacement regressions, but do not focus on pre-displacement industry affiliation. Our approach differs in that we study a sample of exogenously displaced workers (those displaced by plant closings) using our combined 1984–86 DWS sample whereas they analyze a sample that includes many potentially endogenously displaced workers (those displaced by layoffs) using the 1984 DWS data set.

TABLE 2

The effect of pre-displacement industry on pre- and post-displacement wages

January 1984 and 1986 CPS displaced workers survey plant closing sub-sample

Pre-displacement industry	(1) Pre-displacement	(2) Post-displacement
Mining	0·510	0·208
	(0·043)	(0·053)
Primary Metals	0·223	0·099
	(0·053)	(0·066)
Fabricated Metals	0·177	0·070
	(0·049)	(0·061)
Machinery, except Electrical	0·273	0·162
	(0·040)	(0·049)
Electrical Machinery	0·131	0·168
	(0·048)	(0·060)
Transportation Equipment	0·274	0·168
	(0·045)	(0·056)
Lumber, Furniture	0·045	0·074
	(0·045)	(0·057)
Other Durables	0·164	0·121
	(0·047)	(0·058)
Food	0·195	0·119
	(0·045)	(0·055)
Textiles, Apparel	−0·005	0·049
	(0·040)	(0·050)
Paper, Printing	0·146	0·187
	(0·050)	(0·062)
Chemicals, Petroleum	0·267	0·174
	(0·044)	(0·054)
Transportation	0·329	0·278
	(0·041)	(0·051)
Utilities	0·262	0·141
	(0·064)	(0·080)
Wholesale Trade	0·154	0·162
	(0·036)	(0·044)
Retail Trade	—	
FIRE	0·263	0·183
	(0·052)	(0·065)
Business, Professional Services	0·217	0·199
	(0·038)	(0·048)
Personal Services	0·013	0·064
	(0·043)	(0·053)
Other Services	0·067	0·050
	(0·046)	(0·058)
R^2	0·451	0·327
n	2576	2576

The dependent variable in column (1) is log(pre-displacement weekly earnings). The dependent variable in column (2) is log(post-displacement weekly earnings). The reported estimates are the coefficient values for the pre-displacement industry dummy variables. The base industry is retail trade. The numbers in parentheses are standard errors. Each of the reported regressions includes eight pre-displacement occupation dummies, a spline function in previous tenure (with breaks at one, two, three, and six years), years of schooling, experience and its square, a marriage dummy, a female dummy, a nonwhite dummy, year of displacement dummies, three region dummies, and interactions of the female dummy with marrigae and the experience variables. The experience variables in column (1) use pre-displacement experience, while the experience variables in column (2) use current experience. The regression reported in column (2) also includes years since displacement.

however, the estimated effect of pre-displacement industry on the post-displacement earnings of exogenously displaced workers depends crucially on the process by which (potentially rationed) jobs in high-wage industries are allocated. We discuss several alternative allocation processes below. Before doing so, however, we present our empirical evidence.

Column (1) of Table 2 repeats our estimates of the α_j coefficients from equation (6), and column (2) presents our estimates of the γ_j coefficients in equation (8). In Figure 2, we plot the γ_j's against the corresponding α_j's. The unweighted regression line through the points in Figure 2 has an intercept of 0·06, a slope of 0·42, and an R^2 of 0·60. The analogous weighted regression line (where the weight for a given industry is the number of workers displaced by plant closings in that industry) has an intercept of 0·05, a slope of 0·47, and an R^2 of 0·68.

The empirical results reported in this section suggest that pre-displacement industry affiliation plays a fairly important role in determining a worker's post-displacement wage—something like 42% to 47% as important a role as the influence of pre-displacement industry affiliation on the worker's *pre*-displacement wage, for example. These substantial differentials maintained by workers displaced from jobs in high-wage industries over those displaced from jobs in low-wage industries are inconsistent with a model in which the cross-section industry differentials solely reflect true industry effects *and* the new jobs found by exogenously displaced workers are randomly distributed among industries. Of course, there is also direct evidence against such random sorting: as noted above, 69% of workers displaced by plant closings found jobs in new (1·5-digit) industries, so 31% found new jobs in their pre-displacement industries.

For those workers who find their new jobs in their pre-displacement industries, equation (8) is virtually the post-displacement analogue of the pre-displacement cross-section earnings function, equation (6). We estimated (but have not reported) the exact post-displacement analogue of equation (6) on the entire plant-closing sub-sample; the estimates are quite similar to the pre-displacement estimates reported in column (1) of

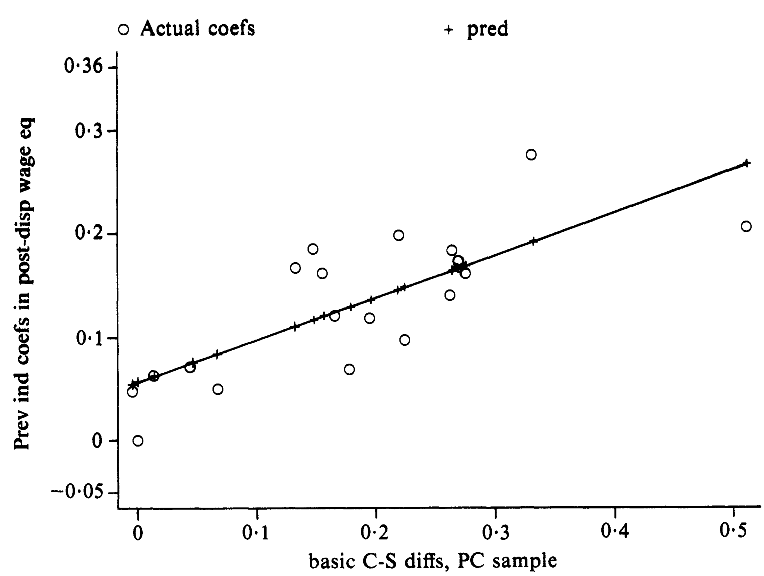

FIGURE 2

The transferability of industry diffs, PC sample

Table 2. Therefore, if the estimated cross-section industry wage differentials—the α_j coefficients in equation (6)—were entirely due to true industry effects, and if workers who did not stay in their pre-displacement industries were randomly sorted among other industries, then on average the γ_j coefficients in equation (8) should be 31% of the corresponding α_j coefficients, rather than 42 to 47% as we find.

6. DISCUSSION

Our empirical findings using a sample of workers displaced by plant closings are difficult to reconcile with either pure (one-factor) unmeasured-ability or pure industry-effects explanations for inter-industry wage differentials. The first-differenced estimates are consistent with industry-effects explanations but run counter to an unmeasured-ability model. The impact of pre-displacement industry on post-displacement wages, however, is suggestive of an unmeasured-ability model. A single model consistent with both pieces of evidence features both true industry effects and persistent individual effects (worker traits). The first-differenced evidence suggests that these traits have only a small (or perhaps no) direct effect on wages, but the post-displacement evidence suggests these traits substantially influence mobility decisions.

One such worker trait might be infra-marginal tolerance for unpleasant working conditions: if the cross-section industry wage differentials largely reflect compensating differentials for non-wage job attributes then exogenously displaced industry switchers should experience wage changes like those documented in Section 4 (because compensating differentials are true industry effects), but pre-displacement industry affiliation should affect the post-displplcement wage as documented in Section 5 (because a worker who found the compensating differential attractive once will do so again). There are two problems with this interpretation, however. First, column (2) of Table 2 reveals that workers displaced from mining (an industry one might think pays a compensating differential) take exceptionally little of their large pre-displacement wage premium with them into new industries.[21] And second, the cross-sectional wage differences themselves are not easily explained by compensating differentials, for three reasons.

The first reason that inter-industry wage differences are not easily explained by compensating differentials is that the inclusion of controls for observable differences in working conditions has little impact on estimated inter-industry wage differences; see Krueger and Summers (1988) and Murphy and Topel (1987). Of course, these controls are incomplete. The second reason is that inter-industry wage differences are highly correlated across occupations: in industries where one occupation is highly paid, all occupations tend to be highly paid; see Dickens and Katz (1987*b*). It seems unlikely that whenever working conditions are poor for production workers they also are poor for secretaries, salesmen, and managers. Finally, the third and most important reason is that Pencavel (1970) and many others have shown that there is a strong negative correlation between industry wage differentials and quit rates, which suggests that workers in high-wage industries earn rents.

A second worker trait that might reconcile the findings in Sections 4 and 5 is the ability to invest in human capital, as in Neal (1990). Suppose that workers differ in (unobserved) ability and that industries differ in the opportunity to invest in industry-specific human capital. Neal presents conditions under which more able workers choose

21. It is possible, of course, that mining pays a large compensating differential but that there are relatively few infra-marginal workers, and/or that the infra-marginal workers could not find new jobs in dirty or hazardous conditions. It is also possible that mining's wage premium is due to extensive unionization rather than a compensating differential.

industries that allow more investment in specific capital. Industry switchers displaced from high-wage industries suffer larger wage losses (consistent with Section 4) because they had more specific capital in their pre-displacement industries, but also earn more on their new jobs (consistent with Section 5) because they have more ability. Unfortunately, Neal's model is inconsistent with several disaggregated versions of the findings in Section 4, as follows.

We estimated the first-differenced regression (7) using workers displaced by plant closings who not only switched industries but also found their new jobs higher up the industry wage hierarchy (i.e. in industries with larger coefficients in column (1) of Table 1). The estimates were qualitatively similar to those reported in column (2) of Table 1. We also estimated (7) using workers displaced by plant closings from pre-displacement jobs in the lowest third of the industry wage hierarchy. Again, the estimates were quite similar to those for the full sample. Thus, of all workers displaced from a given pre-displacement industry, those who found new jobs in industries higher up the industry wage hierarchy experienced wage gains relative to those who found new jobs in the pre-displacement industry, even though the latter workers did not lose any industry-specific capital. We find this result hard to reconcile with Neal's model.

In summary, we know of *no* model that fits all the facts (without resorting to *ad hoc* assumptions). Unmeasured-ability models do not motivate findings of strong pairwise correlations between industries that pay high average wages and industries that earn large profits, have high capital-to-labour ratios, and are populated by large firms (Katz and Summers (1989)).[22] Efficiency-wage models do not motivate the observed high correlation of the industry wage premium across occupations. And rent-sharing models do not motivate the observed similarity of the industry wage structure across countries with very different market systems, such as Eastern and Western Europe. Perhaps no single theory can provide a complete explanation of inter-industry wage differences because different theories are of greatest importance in different sectors of the labour market.

APPENDIX A

TABLE A1

Descriptive statistics for displaced workers data set

January 1984 and 1986 CPS displaced workers surveys workers re-employed at survey date in wage and salary employment

Variable	Means (standard deviations) Entire sample	Reason for displacement: Plant closing	Layoff[a]
Plant Closing = 1	0·49	1·00	0·00
Pre-displacement tenure in years	4·32	5·24	3·42
	(5·55)	(6·42)	(4·36)
Change in Log Real Weekly Earnings	−0·167	−0·164	−0·170
	(0·50)	(0·49)	(0·50)
Log of Pre-displacement Weekly Earnings	5·80	5·79	5·81
	(0·51)	(0·52)	(0·51)
Log of Current Weekly Earnings	5·63	5·62	5·64
	(0·58)	(0·54)	(0·57)

22. Blanchflower, Oswald and Garrett (1990) similarly find that inter-establishment wage differentials within detailed industries show quite similar patterns.

TABLE A1—continued

Means (standard deviations)

Variable	Entire sample	Reason for displacement: Plant closing	Reason for displacement: Layoff[a]
Weeks of Joblessness after displacement	21·61	20·26	22·91
	(25·89)	(25·33)	(26·12)
Female = 1	0·34	0·37	0·31
Years of Schooling	12·56	12·37	12·74
	(2·32)	(2·36)	(2·27)
Age—Education—6 at Displacement	12·48	13·59	11·40
	(10·55)	(11·05)	(9·92)
White Collar in Previous Job = 1	0·41	0·40	0·41
Change 1·5-Digit Industry = 1	0·71	0·69	0·73
Sample Size	5224	2576	2648

[a] Reason for displacement was slack work or shift or position eliminated.
All weekly earnings figures are deflated by the GNP deflator.

TABLE A2

Construction of 1·5-digit industry aggregates from 1980 census industry classification codes

1·5-digit industry	1980 census industry classification codes
Mining	40–50
Primary Metals	270–280
Fabricated metals	281–300
Machinery, except Electrical	310–332
Electrical Machinery	340–350
Transportation Equipment	351–370
Lumber, Furniture	230–242
Other Durables	371–392
Food	100–122
Textiles, Apparel	131–152
Paper, Printing	160–172
Chemicals, Petroleum	179–212
Transportation	400–431
Utilities	440–472
Wholesale Trade	500–571
Retail Trade	580–641
FIRE	700–713
Business, Professional Services	720–742, 841, 882–892
Personal Services	750–799
Other Services	800–840, 842–881

TABLE A3

Pre-displacement industry distributions for displaced workers samples

January 1984 and 1986 CPS displaced workers surveys workers re-employed at survey date in wage and salary employment

		Reason for displacement	
Industry	Entire sample	Plant closing	Layoff
Mining	0·049	0·054	0·044
Primary Metals	0·033	0·028	0·038
Fabricated Metals	0·037	0·034	0·040
Machinery, except Electrical	0·083	0·067	0·099
Electrical Machinery	0·048	0·036	0·060
Transportation Equipment	0·058	0·044	0·071
Lumber, Furniture	0·036	0·042	0·030
Other Durables	0·040	0·040	0·039
Food	0·033	0·044	0·023
Textiles, Apparel	0·059	0·075	0·044
Paper, Printing	0·032	0·032	0·032
Chemicals, Petroleum	0·049	0·047	0·051
Transportation	0·061	0·063	0·060
Utilities	0·020	0·017	0·024
Wholesale Trade	0·071	0·075	0·068
Retail Trade	0·103	0·124	0·083
FIRE	0·029	0·028	0·031
Business, Professional Services	0·074	0·065	0·082
Personal Services	0·043	0·047	0·040
Other Services	0·040	0·039	0·041
n	5224	2576	2648

APPENDIX B

In this appendix we provide parameter values such that the model in Section 3 yields wage changes for industry switchers that are identical to the cross-section industry wage differential. We also develop a slight extension of the model in Section 3 to document that the cross-section differential need not grow with experience.

Let q denote the probability that $s = s'$. Then the average wage paid in industry A is

$$w_A = \frac{q w_{1A} + \{qp(s') + (1-q)p(s'')\} w_{2A}}{q + \{qp(s') + (1-q)p(s'')\}}$$

and the average wage paid in industry B is

$$w_B = \frac{(1-q) w_{1B} + \{q[1-p(s')] + (1-q)[1-p(s'')]\} w_{2B}}{(1-q) + \{q[1-p(s')] + (1-q)[1-p(s'')]\}}.$$

The cross-section wage differential is thus $w_A - w_B$. The first-differenced estimate, in contrast, is $[(w_{2A} - w_{1B}) - (w_{2B} - w_{1A})]/2$, independent of the proportion of workers moving in each direction. If we set $q = \frac{1}{2}$ and $p(s') = 1 - p(s'')$ then the two estimates are identical.

We now show that the industry wage differential need not grow with experience. Suppose there are three levels of ability: η_L, η_M, and η_H. Let the technology in industry B be (arbitrarily close to) independent of ability: $y_{BH} = y_{BM} = y_{BL} = y_B$. Assume that η_H surely generates the signal s', and that η_M and η_L surely generate the signal s''. Finally, assume that the efficient industry choice given s'' is industry B, but that the efficient choice for η_M is industry A. In this model, the average wage in industry B is y_B in both periods, but the average wage in industry A falls over time, from y_{AH} in the first period to a weighted average of y_{AH} and y_{AM} in the second.

APPENDIX C

This appendix contains brief summaries of two failed attempts to develop an unobserved-ability model that fits the empirical finding documented in Section 4: holding other observables constant, the wage change experienced by an exogenously displaced industry switcher closely approximates the difference between the relevant industry differentials estimated in a cross-section. Recall from the discussion in Section 3 that such an unobserved-ability model must not only account for the cross-section industry differentials but also explain (i) which exogenously displaced workers switch industries, (ii) why those who switch did not do so before displacement, and (iii) why industry switchers experience the wage changes we documented. The first model described below emphasizes the role of firm-specific human capital, the second sectoral shifts.

Model 1

This model adds firm-specific human capital to the model developed in Section 3. In order to clarify the process of wage determination, we also extend the model to three periods. As in Section 3, information about a worker's ability is symmetric but imperfect in the first period, first-period output reveals ability perfectly, and information is then perfect in the second period. For the same reason, information is perfect in the new third period considered here.

Suppose that in the first period each worker has an opportunity to invest in firm-specific human capital that increases second- and third-period productivity *at the first-period firm* by an amount k. (For simplicity we take k to be independent of both the worker's ability and the industry technology.) Suppose further that firms induce workers to undertake this (costly but efficient) investment by contracting to share the returns, so that second- and third-period wages at the first-period firm are increased by σk, where $0<\sigma<1$. Finally, suppose that k is large enough that $y_{AH}-y_{BH}<\sigma k$ and $y_{BL}-y_{AL}<\sigma k$: a worker's return on firm-specific human capital is more valuable than achieving the efficient match between the worker's ability and an industry's technology.

In this model there will be learning about ability from first-period output, but the learning will not induce *any* mobility because workers cannot afford to abandon their firm-specific human capital. In a more general model (such as would result if ability were continuously variable and k were of intermediate size), workers who are sufficiently badly mismatched will move to their efficient matches for the second and third periods, while those who are sufficiently well matched will stay with their first-period employers. Both in our simple model and in the more general case, the second- and third-period wages of mismatched workers need to be determined. We assume that mismatched workers who stay with their first-period employers earn the sum of (i) the wage that other employers in that industry would be willing to pay plus (ii) the return on firm-specific human capital that the current employer has contracted to pay. Thus, the second- and third-period wage of a low- (high-) ability worker in industry A (B) is $y_{AL}+\sigma k(y_{BH}+\sigma k)$.

Now consider what happens to workers displaced by a plant closing after the *second* period: their firm-specific human capital is destroyed and efficient matching becomes the only determinant of third-period industry choice; those who are mismatched switch industries, exactly as in the second period of the model described in Section 3. The crucial question is whether the wage changes of these exogenously displaced industry switchers behave in the required fashion. The answer is that they do not.

To compute the wage change of an industry switcher, first consider the worker's third-period wage. Depending on the worker's ability, this wage will be either y_{AH} or y_{BL}—the worker's productivity in the efficient match, in the absence of the recently destroyed firm-specific human capital. Now consider the worker's second-period wage. Depending on the worker's ability, this wage will be either $y_{BH}+\sigma k$ or $y_{AL}+\sigma k$—the worker's productivity in the inefficient match, plus the (contractually enforced) return to human capital. The wage change experienced by an industry switcher thus consists of two components: a wage increase due to improved matching, and a wage decrease due to the loss of human capital. We see no robust reason why the net effect of these changes should mimic the difference in the cross-section industry differentials, especially if one moves beyond the two-industry framework analyzed here.

Notice that if the plant closing were to occur after the first rather than the second period, then the wage changes experienced by industry switchers in this model would be identical to those identified in Section 3. We take it to be implausible that plant closings should routinely occur just as investments in firm-specific human capital mature, but we use this observation to clarify the key difference between the two models. In the text, learning about ability immediately induces mobility, so there is no opportunity for this learning to affect the *pre*-displacement wage. Here, learning has no effect on mobility until the worker's firm-specific human capital is destroyed but therefore does affect the pre-displacement wage.

Model 2

This model explores the possibility that sectoral shocks could induce inter-industry mobility in a way that produces wage changes of the necessary kind. We find that such demand-shift models, while useful for other purposes, cannot easily match the stylized fact documented in Section 4.

To see this, consider a model with *n* industries and continuously variable ability. Suppose a single industry receives a negative shock. The wages *offered* to all workers in the industry will fall. Workers who were sufficiently well matched before the shock (in the sense of earning well above their second-best wage offer) will accept these reduced wage offers and stay in the industry; the rest will leave. Workers who leave will necessarily experience wage decreases (compared to their *pre*-shock wages) because they switch industries to accept offers that were available but were rejected before the shock, but these workers will (typically) find their new jobs in industries both higher and lower in the industry wage structure than their pre-displacement industry.

If industry switchers are to experience relative wage changes of the same sign as *and* of similar magnitude to the difference in the relevant industry wage differentials estimated in a cross-section, those who move up the industry wage structure must lose an appropriate amount less than do those who move down. That is, those who move up should also be those whose second-best pre-displacement wage offers (which they now switch industries to accept) were only marginally inferior to their pre-displacement wages, while those who move down should also be those whose second-best pre-displacement wage offers were dramatically inferior to their pre-displacement wages. As in Model 1, we see no robust reason why such a relationship should hold, especially in a multi-industry framework.

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