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DOES UNMEASURED ABILITY EXPLAIN
INTER-INDUSTRY WAGE DIFFERENCES?

Robert Gibbons
Lawrence Katz

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massachusetts
institute of
technology
50 memorial drive
cambridge, mass. 02139

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by

Robert Gibbons
MIT and NBER

and

Lawrence Katz
Harvard University and NBER

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ABSTRACT

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, which is based on first-differenced regressions using matched Current Population Survey (CPS) data. We argue that these existing approaches implicitly hypothesize that unmeasured productive ability is equally rewarded in all industries. Second, we construct a simple model in which unmeasured ability is not equally valued in all industries; instead, there is matching. This model illustrates two endogeneity problems inherent in the first-differenced regressions using CPS data: whether a worker changes jobs is endogenous, as is the industry of the new job the worker finds. Third, we propose two new empirical approaches designed to minimize these endogeneity problems. We implement these procedures on a sample that allows us to approximate the experiment of exogenous job loss: a sample of workers displaced by plant closings. We conclude from our findings using this sample that neither of the contending explanations fits the evidence without recourse to awkward modifications, but that a modified version of the true-industry-effects explanation fits more easily than does any existing version of the unmeasured-ability explanation.

Robert Gibbons
Department of Economics
MIT
Cambridge, MA 02139

Lawrence F. Katz
Department of Economics
Harvard University
Cambridge, MA 02138

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 labor-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 labor market.

One explanation of persistent measured wage differences among observationally similar workers in competitive labor 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 2 we summarize the existing empirical work on the

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[1] See Dickens and Katz (1987a,b), Helwege (1987), Krueger and Summers (1987,1988) and Murphy and Topel (1987a,b).

[2] See Rosen (1986) on compensating differences, Katz and Summers (1989) and Nickell and Wadhwani (1989) 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.

unmeasured-ability explanation of inter-industry wage differences. This work uses matched Current Population Survey (CPS) 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 serves to improve 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. This model illustrates two endogeneity problems inherent in using first-differenced regressions on CPS data to estimate industry wage differentials: whether a worker changes jobs may be endogenous, as may be the industry of the new job the worker finds. We also discuss how analogous endogeneity problems may affect first-differenced estimates of compensating differentials, the union wage premium, and employer-size wage effects.

In Sections 4 and 5 we propose and implement two empirical strategies designed to minimize the importance of these endogeneity problems in the estimation of inter-industry wage differentials. To reduce the possibility that the job separations included in our sample were caused by changes in market perceptions of workers' abilities, 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 this sample of (approximately) exogenous job changers. In Section 5 we study the impact of a worker's pre-displacement industry on his or her post-displacement earnings, again for our sample of workers displaced in plant closings.

If estimated industry wage differentials largely reflect unmeasured differences in worker quality, one would expect that workers exogenously displaced from high-wage industries would maintain their wage differentials over those exogenously displaced from low-wage industries. Consistent with this, we find in Section 5 that workers displaced by plant closings maintain about 45% of their pre-displacement industry wage premiums when they are reemployed. On the other hand, we find in Section 4 that first-differenced and cross-section industry differentials are very similar even for this sample of (approximately) exogenously displaced workers. This latter finding is, of course, consistent with a true-industry-effects model, but is quite difficult to reconcile with a pure unmeasured-ability model. We conclude that neither of the contending explanations fits the evidence without recourse to awkward modifications, but that a modified version of the true-industry-effects explanation (in which the traits that help a worker get selected into high-wage industries are moderately persistent) fits more

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[4] This sample restriction is motivated by our earlier work, Gibbons and Katz (1989). We discuss it further below.

easily than does any version of the unmeasured-ability explanation that we have been able to construct.

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 (and that industry affiliation and other regressors are measured without error), first-differenced regressions yield unbiased estimates of true industry effects.[5] The existing empirical work on the unmeasured-ability explanation of inter-industry wage differences attempts to exploit this property of first-differenced regressions.

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.[6] Krueger and Summers conclude that their empirical finding casts "serious doubt on 'unmeasured labor quality' explanations for inter-industry wage

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[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 an 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 from first-differenced estimation. See Stewart (1983) for a related argument in the context of the estimation of union wage differentials from panel data.

[6] 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.

differences" (p.260).[7]

Murphy and Topel (1987a,b) 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 percent of the cross-sectional differential. Murphy and Topel (1987a) 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.[8] 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

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[7] 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.

[8] Murphy and Topel (1987b) restrict their sample to individuals who changed their industry or occupation between the two previous calendar years and who were still employed in their new industry-occupation cell at the time of the second sample date. This sample restriction 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. This approach also helps eliminate false industry transitions.

biased downward.[9]

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. 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 labor mobility generated by mismatching (caused either by changes in perceptions of worker abilities

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[9] Murphy and Topel (1987a) propose an approximate correction for this problem.

or by changes in assessments of idiosyncratic worker-job match values) appears to be quantitatively important: Jovanovic and Moffitt (1989) estimate that the bulk of labor 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. More recently, Heckman and Sedlacek (1985) have analyzed both wages and mobility in a dynamic version of Roy's model in which mobility is endogenously determined by shifts in the demand for labor across sectors. Our model complements the Heckman-Sedlacek approach by emphasizing learning about individual workers' abilities rather than shifts in relative demand.[10]

Information about ability is symmetric throughout the model and is imperfect ex ante but improves ex post: 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 and the ex post productivity observation result in imperfect matching of workers to industries ex ante and improved matching ex post; high-ability (low-ability) workers endogenously gravitate to the industries with ability-sensitive (ability-insensitive) technologies.

We assume that neither the ex ante signal nor the ex post 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 reported by the CPS, and think of the signal as representing resume information about academic performance and

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[10] 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.

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 ex ante signal (but our conclusions are by no means limited to this simple setting; see Gibbons and Katz, 1987). Specifically, ability η is either high or low: $\eta \epsilon \{\eta_H, \eta_L\}$. Output in industry A is more sensitive to ability than is output in industry B:

(1) $$y_{AH} > y_{BH} > y_{BL} > y_{AL} ,$$

where y_{ij} is the output in industry i of a worker of ability η_j. (Note that ability entirely determines output; there is no effort-elicitation problem.) These output levels are constant over time. Given perfect information and a competitive labor 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, $s \epsilon \{s', s''\}$, where $s' > s''$, and leads to the posterior probability $p(s)$ that the worker is of high ability, where $1 > p(s') > p(s'') > 0$. We assume that the signal is accurate enough that the following conditions on expected productivity hold:

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

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

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

We consider a competitive labor 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 ex ante signal nor the ex post 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. It therefore follows that in this model: (i) although there are no true industry effects, there are persistent measured inter-industry wage differences: $w_{tA} > w_{tB}$ for $t \epsilon \{1,2\}$; and (ii) 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}). Furthermore, depending on the parameters of the model, the wage changes for industry switchers can be similar in magnitude to the cross-section industry wage differentials.[11]

We conclude from this model that two endogeneity problems are inherent in the first-differenced regressions using matched CPS data summarized in Section 2: whether a worker changes jobs may be endogenous, as may be the industry of the new job the worker finds. In a typical individual-level data set (including but certainly not limited to the CPS), both of these endogeneity problems seem likely to be severe. Furthermore, even if one constructs a sample that avoids the first problem (we argue below, for example, that workers displaced by plant closings can usefully be viewed as exogenously displaced), the second problem may remain; see Model 1 in

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[11] As suggested earlier, these results are not an artifact of the simple two-sector model analyzed in this section. Gibbons and Katz (1987) show that a richer model with n sectors that differ in the sensitivities of their technologies to ability and with gradual learning about worker ability generates similar qualitative predictions concerning measured cross-section industry differentials and the wage changes of self-selected industry switchers. The biases emphasized in our simple model will arise in any model in which inter-industry mobility at least partly acts to improve the allocation of workers to industries as new information about their abilities arrives.

Appendix B for an example.

The potential biases arising from the self-selection of job changers highlighted by the model in this section are also likely to be important for longitudinal estimates of other wage gaps.[12] For example, puzzling estimates of compensating differentials using cross-section data 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 similar difficulties may help explain the often perverse longitudinal estimates of compensating differentials (e.g., Brown, 1980). In particular, 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 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.

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 and industry changes in the estimation of inter-industry wage differentials.

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

4. Wage Changes Following Exogenous Job Loss

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 that 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 also eliminated from the sample since 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 selfemployment, 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 5,224 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.[13] 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.

In earlier work using this sample of displaced workers (Gibbons and Katz, 1989), we motivated and documented an important distinction between two sub-samples of the data: workers displaced by plant closings and those displaced by layoffs.[14] We developed an asymmetric-information model of

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[13]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 $j=0.2$, might be reasonable. Taking $s=0.7$ and $m=0.1$, as an example, we then have an error rate of 14% in the DWS but of 44% in the CPS.

[14]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

endogenous wage-setting and turnover in which, if firms have discretion over whom to lay off, they dismiss their least-able workers. In the model's 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 no adverse inference about ability is warranted). Empirically, we found that laid-off workers indeed receive lower re-employment wages than do observationally equivalent workers displaced by plant closings. 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 in the pre-displacement wages of observationally equivalent workers from these two sub-samples.

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. 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.[15] First, we estimate industry differentials from the cross-section wage

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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.

[15] Krueger and Summers (1988) apply their empirical strategy to a sample from the 1984 DWS that includes those displaced by both layoffs and plant closings, using broad industry definitions. We confirm the spirit of their findings, using more data, more detailed industry definitions, and a sample less likely to include endogenous job losers.

function

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

where $\ln w_{ijt}$ is log weekly earnings for individual i in industry j 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_{ijt} 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.[16] The estimated industry differentials for the plant-closings sample are substantial in magnitude, highly 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

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[16] 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 is 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	.510 (.043)	.429 (.051)
Primary Metals	.223 (.053)	.262 (.055)
Fabricated Metals	.177 (.049)	.196 (.052)
Machinery, except Electrical	.273 (.040)	.248 (.042)
Electrical Machinery	.131 (.048)	.083 (.047)
Trans. Equipment	.274 (.045)	.272 (.047)
Lumber, Furniture	.045 (.045)	.069 (.051)
Other Durables	.164 (.047)	.091 (.046)
Food	.195 (.045)	.170 (.048)
Textiles, Apparel	-.005 (.040)	.053 (.045)
Paper, Printing	.146 (.050)	.076 (.051)
Chemicals, Petroleum	.267 (.044)	.186 (.045)
Transportation	.329 (.041)	.130 (.044)
Utilities	.262 (.064)	.285 (.057)

Table 1: Continued

Industry	(1) Cross-section[a]	(2) First-Differenced[b]
Wholesale Trade	.154	.085
	(.036)	(.034)
Retail Trade	---	---
FIRE	.263	.162
	(.052)	(.040)
Bus., Prof. Services	.217	.045
	(.038)	(.036)
Personal Services	.013	-.008
	(.043)	(.039)
Other Services	.067	-.036
	(.046)	(.036)
R^2	.451	.131
n	2,576	2,576

[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 8 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 nonwhite dummy, year of displacement dummies, 3 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 dummy variables. The base industry is retail trade. The reported regression also includes 8 occupation change dummy variables; 3 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.

industry wage differentials is 0.13.

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

$$\text{(7)} \qquad \Delta \ln w_{ijt} = \Delta X_{it}\, \delta + \Sigma\, \beta_j\, \Delta D_{ijt} + \Delta u_{ijt} .$$

Under the assumption that unmeasured productive ability is time-invariant and equally rewarded in all industries (i.e., the error term u_{ijt} can be written as $\theta_i + v_{ijt}$, where θ_i is the ability of worker i and v_{ijt} 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 reemployment weekly earnings (at the survey date) to pre-displacement weekly earnings) as the dependent variable.[17] 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,

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[17] The estimates are essentially unchanged if the non-industry regressors in (7)---i.e., the X_{it}'s---are not differenced, so that the wage change is regressed on non-industry regressors and the industry-change dummies.

which plots the β_j's against the corresponding α_j's. The (unweighted) regression line through the points in Figure 1 has an intercept of -.01, a slope of .79, and an R^2 of .72.[18] The reverse-regression estimate of this slope is 1.10.

Third, we follow Murphy and Topel in estimating the single coefficient (of interest) ϕ in the equation

$$(8) \qquad \Delta \ln w_{it} = \Delta X_{it} \delta + (\hat{\alpha}_{it} - \hat{\alpha}_{i,t-1}) \phi + v_{it} ,$$

where $\hat{\alpha}_{it}$ is the estimated cross-section industry wage differential for the industry in which individual i is employed at time t (i.e., $\hat{\alpha}_{it}$ in equation (8) equals α_j estimated in equation (6) if individual i is employed in industry j at time t), and v_{it} is an error term. If the estimated cross-section industry wage differentials are entirely due to the sorting of workers across industries by unmeasured ability that is equally valued in all industries, then ϕ should equal zero. On the other hand, if the estimated cross-section industry wage differentials are entirely due to true industry effects, then ϕ should equal one. Our estimate of ϕ is .740, with a standard

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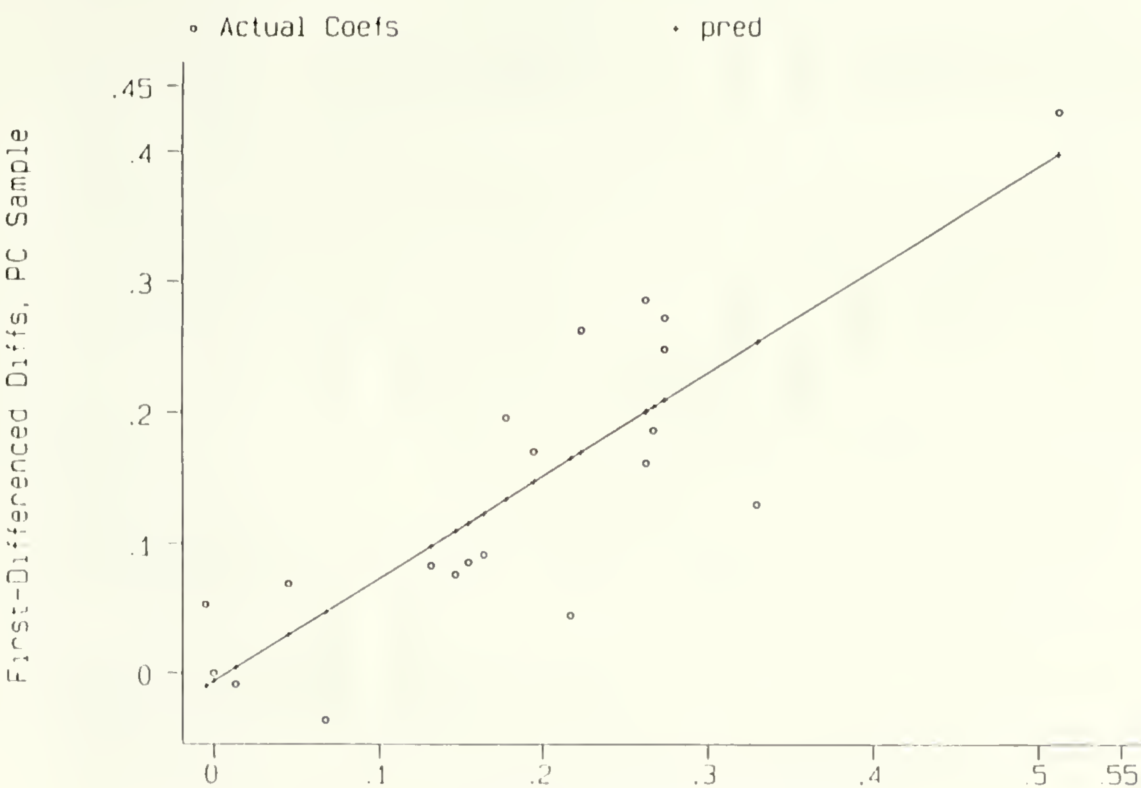
[18] One potential problem with these findings is that workers may take temporary jobs after displacement that do not fully utilize their talents. In an attempt to avoid this problem, we re-estimated equations (6) and (7) on the sub-sample of workers displaced at least two years. The results differ only slightly from those reported above. The standard deviations of the resulting industry wage differentials estimated from cross-section and first-differenced equations are 0.14 and 0.13, respectively. The regression through the points in the plot analogous to Figure 1 has an intercept of -.03, a slope of .87, and an R^2 of .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.

FIGURE 1



First-Differenced vs. C-S Ind Difs, PC Sample error of .070.[19]

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 the estimated cross-section differentials (α_j's) and the estimated first-differenced differentials (β_j's) than we do for the plant-closings sample. The regression of the β_j's against the corresponding α_j's for the layoffs sample yields a slope coefficient of .971 and an R^2 of .81; this implies a reverse-regression estimate of the slope of 1.21. The estimate of ϕ from equation (8) for the layoffs sample is .970 with a standard error of .078. Thus, industry switchers in the layoffs sample appear to earn 97 percent of the relevant cross-section differential, while the analogous figure for the plant-closing sample is approximately 75 to 80 percent. This difference between the two samples suggests 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 were likely exogenous.

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[19]The regression also included 8 change-in-occupation dummies, experience, experience interacted with a female dummy, years since displacement, and year-of-displacement dummies. Note that because the $\hat{\alpha}_{it}$ regressors are both estimated and grouped variables, the standard error of ϕ reported in an OLS regression will be incorrect. As discussed above in connection with equation (7), the estimate of ϕ is essentially unchanged if the non-industry regressors in equation (8) are not differenced.

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 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 model we developed in Section 3 could explain such an empirical result for a dataset 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 changes of such industry switchers mimic the difference in the average wages for the relevant industries.

We find it difficult to develop an unobserved-ability model that fits the evidence presented in this section. (See Appendix B for brief summaries of two failed attempts, one emphasizing firm-specific human capital and the other emphasizing sectoral shifts.) In the absence of such a model, we conclude that the existing variants of the unmeasured-ability explanation of inter-industry wage differentials are rejected by the facts concerning the wage changes of (exogenously displaced) industry switchers.

5. The Effect of Pre-displacement Industry on Post-displacement Wage

We now turn to the second endogeneity problem identified in Section 3: the possibility that an exogenously displaced worker's reemployment industry may be endogenous. Before describing our empirical analysis, it may be useful to clarify two points. First, as noted above, this second endogeneity problem can persist even if one solves the first endogeneity problem by constructing a sample of exogenously displaced workers; see Model 1 in Appendix B for a simple example based on unobserved ability; see the discussion later in this section for an example based on a different kind of unmeasured person-specific trait. Second, it is important to note that the potential importance of this second endogeneity problem does not alter our interpretation of the empirical results presented in the previous section (where we attempted to eliminate only the first endogeneity problem): we find it difficult to develop a plausible unobserved-ability model that fits our empirical findings, whether or not reemployment industry is endogenous.

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

$$(9) \qquad \ln w_{ijt} = X_{i,t-1}\,\delta + \Sigma\, \gamma_j\, D_{ij,t-1} + \epsilon_{ijt}\ ,$$

where: w_{ijt} 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;[20] $D_{ij,t-1}$ is a dummy variable equal to one if individual i was displaced from industry j; and ϵ_{ijt} is an error term.[21] The coefficients of interest in equation (9) are the γ_j's, which measure the impact of pre-displacement industry on post-displacement earnings.

Most unmeasured-ability explanations for measured cross-section industry differentials predict that, conditional on only 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 (9), these models predict the analogous prediction is that the γ_j's should be positively related to the α_j's. In a model of true industry effects, however, this impact 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 the empirical evidence on this point using our plant-closings sample.

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

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[20]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.

[21]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 whereas they analyze all the workers in 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-Displ. Industry	(1) Pre-Displ.	(2) Post-Displ.
Mining	.510 (.043)	.208 (.053)
Primary Metals	.223 (.053)	.099 (.066)
Fabricated Metals	.177 (.049)	.070 (.061)
Machinery, except Electrical	.273 (.040)	.162 (.049)
Electrical Machinery	.131 (.048)	.168 (.060)
Trans. Equipment	.274 (.045)	.168 (.056)
Lumber, Furniture	.045 (.045)	.074 (.057)
Other Durables	.164 (.047)	.121 (.058)
Food	.195 (.045)	.119 (.055)
Textiles, Apparel	-.005 (.040)	.049 (.050)
Paper, Printing	.146 (.050)	.187 (.062)
Chemicals, Petroleum	.267 (.044)	.174 (.054)
Transportation	.329 (.041)	.278 (.051)
Utilities	.262 (.064)	.141 (.080)

Table 2: Continued

Pre-Displ. Industry	(1) Pre-Displ.	(2) Post-Displ.
Wholesale Trade	.154	.162
	(.036)	(.044)
Retail Trade	---	---
FIRE	.263	.183
	(.052)	(.065)
Bus., Prof. Services	.217	.199
	(.038)	(.048)
Personal Services	.013	.064
	(.043)	(.053)
Other Services	.067	.050
	(.046)	(.058)
R^2	.451	.327
n	2,576	2,576

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 8 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, 3 region dummies, and interactions of the female dummy with marriage 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.

equation (9). 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 .06, a slope of .42, and an R^2 of .60.

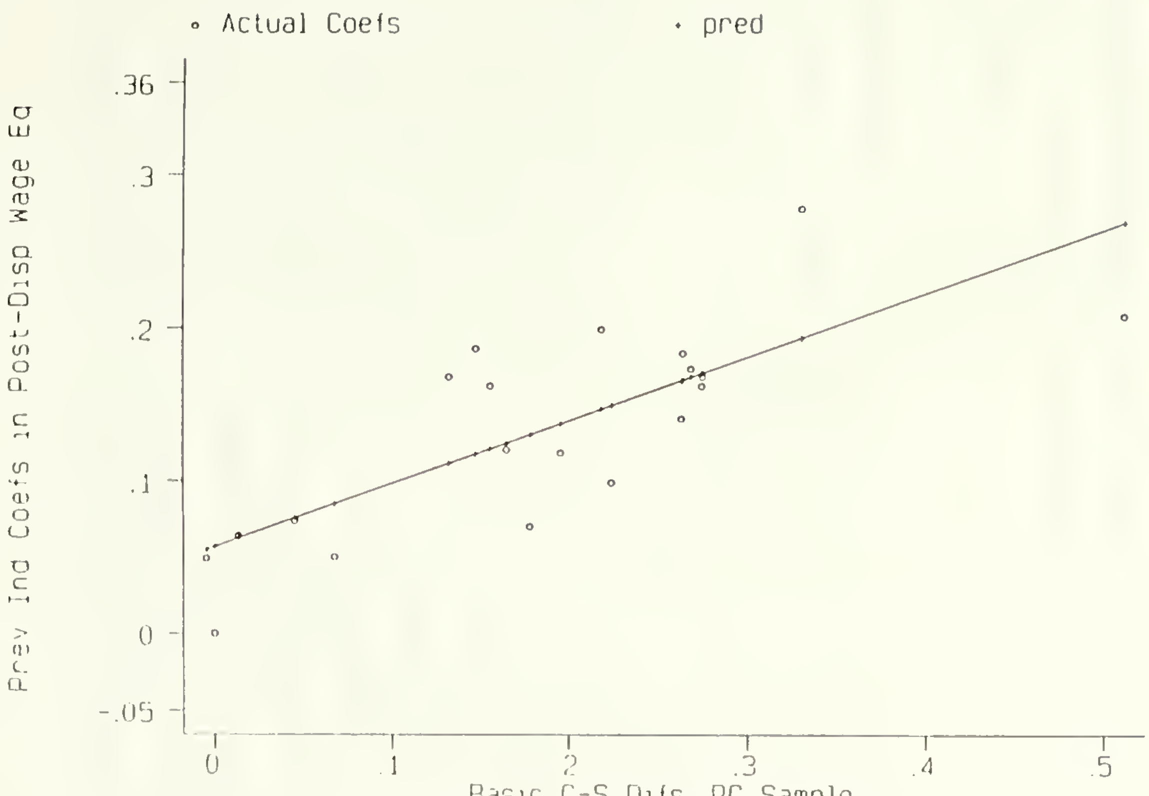
We also estimate an equation in the spirit of the Murphy-Topel first-differenced regression, equation (8):

$$(10) \qquad \ln w_{it} = X_{it}\,\delta + \hat{\alpha}_{i,t-1}\,\psi + \xi_{it} ,$$

where $\hat{\alpha}_{i,t-1}$ is the estimated industry wage premium for the industry from which individual i was displaced and ξ_{it} is an error term. We estimate that ψ is .467. Consistent with this estimate, we also find that the <u>weighted</u> regression line through the points in Figure 2 (where the weight for a given industry is the number of workers displaced by a plant closing in that industry) has a slope of .468 (as well as an intercept of .05 and an R^2 of .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 <u>pre</u>-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 true-industry-effects model in which the new jobs found by exogenously displaced workers are randomly distributed among industries. Of course, there is also direct evidence against such random sorting: column (2) of Table A1 reveals that 31% of workers displaced by plant closings found

FIGURE 2



The Transferability of Industry Difs, PC Sample their new jobs in their (1.5-digit) pre-displacement industries.

For those workers who find jobs in their pre-displacement industries, equation (9) is virtually the post-displacement analog of the pre-displacement cross-section earnings function, equation (6). The exact post-displacement analog of equation (6) estimated on the entire plant-closing sub-sample yields estimates quite similar to the pre-displacement estimates reported in column (1) of 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 (9) should be 31% of the corresponding α_j coefficients, rather than 42 to 47% as we find. Thus, our findings lead us not only to reject the view that the new jobs found by exogenously displaced workers are randomly distributed across industries but also to question seriously the view that the new jobs found by the sub-sample of workers who leave their pre-displacement industry are randomly distributed.

One way to account for these findings is to hypothesize that workers are systematically sorted among industries on the basis of an unobservable worker-specific trait, but that (unlike ability) this trait does not directly influence wages (so that, after controlling for industry affiliation, the trait would not enter the error term in a cross-section earnings equation). Since workers are systematically sorted among industries on the basis of observables (such as education), it is not implausible that they will be at least somewhat systematically sorted on the basis of such an unobservable, although it would be more persuasive to demonstrate that workers are systematically sorted among industries on the basis of an observable that does not directly affect the wage.

Overall, our empirical findings using a sample of workers displaced by plant closings are difficult to reconcile with either pure unmeasured-ability or pure industry-effects explanations for inter-industry wage differentials. The first-differenced estimation evidence is consistent with industry-effects explanations, but is not likely in an unmeasured-ability model. The impact of pre-displacement earnings on post-displacement wages suggests a modified explanation featuring true industry effects and persistent individual effects (worker traits) that do not directly influence wages but rather influence which workers get sorted into the high-wage jobs that pay wage premiums or compensating differentials.

6. Interpretation and Discussion

As noted in the Introduction, one member of the class of true-industry-effects explanations of the measured inter-industry wage differentials rests on compensating differentials for non-wage job attributes. It might seem that such compensating differentials could provide a plausible interpretation for the empirical findings reported in Sections 4 and 5: The finding that the wage changes of industry switchers are quite similar to cross-section differentials would follow because compensating differentials are true industry effects. And the worker-specific trait that makes workers displaced from high-wage industries more likely to end up in high-wage jobs after displacement would be interpreted as (infra-marginal) willingness to work in an environment viewed as unpleasant by marginal workers; displaced workers who find new jobs in unpleasant environments will again earn high wages.

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.[22] 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 (1987a). 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 (1987b). 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.

The second and third of these observations also pose problems for unmeasured-ability models of inter-industry wage differences. A fundamental assumption in such models (as in Section 3) is that industry technologies are

[22] 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.

differentially sensitive to ability: industries with ability-sensitive technologies hire proportionately more high-ability workers, and so pay higher average wages. The model in this paper considers only a single occupation. In a multi-occupation model, the strong correlation in wage differences across occupations requires that the sensitivity of an industry's technology to ability be fairly uniform across occupations. It seems unlikely, for instance, that industries that require especially skilled managers also require especially skilled laborers.

Several other stylized facts about inter-industry differences seem at best orthogonal to unmeasured-ability models: there are strong pairwise correlations between industries that pay high average wages and industries that earn large profits, have high capital-to-labor ratios, and are populated by large firms; see Katz and Summers (1989). We see no good reason why, for example, the presumption should be that high-profit industries should necessarily be those with especially ability-sensitive technologies.

Unfortunately, we know of no model that fits all the facts (without resorting to ad hoc assumptions). Efficiency-wage models, for instance, do not motivate the observed high correlation of the industry wage premium across occupations. And rent-sharing models, for their part, 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 labor market.

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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

Means (Standard Deviations)

Variable	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 (5.55)	5.24 (6.42)	3.42 (4.36)
Change in Log Real Weekly Earnings	-0.167 (0.50)	-0.164 (0.49)	-0.170 (0.50)
Log of Pre-displacement Weekly Earnings	5.80 (0.51)	5.79 (0.52)	5.81 (0.51)
Log of Current Weekly Earnings	5.63 (0.58)	5.62 (0.54)	5.64 (0.57)
Weeks of Joblessness after displacement	21.61 (25.89)	20.26 (25.53)	22.91 (26.12)
Female = 1	0.34	0.37	0.31
Years of Schooling	12.56 (2.32)	12.37 (2.36)	12.74 (2.27)
Age - Education - 6 at Displacement	12.48 (10.55)	13.59 (11.05)	11.40 (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
Elect. Machinery	0.048	0.036	0.060
Trans. 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
Bus., Prof. 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

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 emphasizes 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 the 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 <u>at the first-period firm</u> by an amount k. (For simplicity we take k to be independent of both the worker's ability and the industry technology, but these assumptions can be relaxed considerably.) 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 $y_{BL} - y_{AL} < \sigma k$: the return on firm-specific human capital is more valuable than achieving the efficient match between a 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 <u>any</u> 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 mis-matched 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 mis-matched workers need to be determined. We assume that mis-matched 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 <u>second</u> period: their firm-specific human capital is destroyed and efficient matching becomes the only determinant of third-period industry choice; those who are mis-matched switch industries, exactly as in the second period of the model described in the Section 3. This model therefore demonstrates that the second endogeneity problem (endogenous industry affiliation) can persist even if the first endogeneity problem (endogenous job separation) has been resolved. The crucial question, however, 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, consider first 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, along the lines of Gibbons and Katz (1987). 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 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 was the case in Model 1, we see no robust reason why such a relationship should hold, especially in a multi-industry framework.