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COMPARATIVE ADVANTAGE, LEARNING,  
AND SECTORAL WAGE DETERMINATION

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### ABSTRACT

We develop a model in which a worker's skills determine the worker's current wage and sector. Both the market and the worker are initially uncertain about some of the worker's skills. Endogenous wage changes and sector mobility occur as labor-market participants learn about these unobserved skills. We show how the model can be estimated using non-linear instrumental-variables techniques. We then apply our methodology to study the wages and allocation of workers across occupations and across industries. For both occupations and industries, we find that high-wage sectors employ high-skill workers and offer high returns to workers' skills. Estimates of these sectoral wage differences that do not account for sector-specific returns are therefore misleading. We also suggest further applications of our theory and methodology.

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## 1. Introduction

We analyze the theoretical and econometric implications of comparative advantage and learning for the wages and sector affiliations of individuals, and for changes in these variables over workers' careers. After developing the theory and econometrics, we turn to two empirical applications of our methodology, concerning the wages and allocations of workers across occupations and across industries.

Our focus on comparative advantage is motivated by a large and established literature. Many have found that the average characteristics of individuals vary by sector.<sup>1</sup> Furthermore, several have found that the measured returns to individuals' observable characteristics vary by sector.<sup>2</sup> Finally, Heckman and Scheinkman (1987) rejected the hypothesis that the returns to individuals' time-invariant unmeasured characteristics are constant across sectors.

Our focus on learning is motivated by a smaller and more recent literature. While Jovanovic (1979), Harris and Holmstrom (1982), and others showed long ago that learning models could provide new interpretations for important features of the data (such as the return to seniority and the increase in the variance of wages with experience), recent work has built on these foundations to derive and test novel implications, many of which have survived confrontations with data.<sup>3</sup>

Our theoretical model emphasizes the role of worker skills that cannot be measured by an econometrician. To clarify the exposition of the econometrics, we develop the theory in stages.

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<sup>1</sup> For example, Dickens and Katz (1987) found differences in average education levels by industry and Blackburn and Neumark (1992) found sorting by test scores across industries.

<sup>2</sup> For example, Mincer and Higuchi (1988) found differences in returns to tenure and experience across industries in Japan and the US and Freeman and Medoff (1984) found differences in returns to education and experience for union and non-union workers.

<sup>3</sup> For example, Farber and Gibbons (1996) derive and test the prediction that the residual from a regression of an individual's score on an ability test (AFQT) on observable characteristics and the first wage should have increasing explanatory power for subsequent wages as experience increases. Continuing in this vein, Altonji and Pierret (2001) derive and test the prediction that the effect of observable characteristics (like education) should decrease with experience while the effect of initially unobservable characteristics like AFQT (not only the AFQT residual) should increase with experience. Chiappori, Salanié, and Valentin (1999) derive and test a new prediction from the Harris-Holmstrom model, that comparing two individuals in the same job in period 1 and the same (higher) job in period 3, future wage and promotion prospects are brighter for the individual who was promoted later (*i.e.*, in period 3 rather than period 2).

We begin with two models in which workers' skills are equally valued in all sectors. In the first of these models, all labor-market participants have perfect information about workers' skills; in the second, information is initially imperfect but output observations convey additional information over time and so endogenize wage changes. We then develop two other models in which different sectors place different values on workers' skills and workers sort themselves into different sectors on the basis of comparative advantage. In the first of these latter two models, labor-market participants again have perfect information about workers' skills; in the second, information is again imperfect, so learning endogenizes not only wage changes but now also sector mobility.

Our richest model, with comparative advantage and learning, resembles the learning and matching models of wages and turnover pioneered by Jovanovic (1979), Ross, Taubman, and Wachter (1981), and MacDonald (1982). In Jovanovic's model, a worker's performance is independent across jobs, whereas in our model (like Ross *et. al.* and MacDonald), a worker's performance in one job determines not only the expected value of staying in that job but also the expected value of moving to a given new job. We differ from Ross *et. al.* and MacDonald by introducing a one-dimensional notion of ability that determines a worker's productivity in every sector, much as in Murphy (1986). The resulting model of learning and sorting is a natural generalization of the two-period, two-sector, two-type model in Gibbons and Katz (1992).

As is well known, in our simplest theoretical model (in which worker skills are equally valued in all sectors and there is no learning by labor-market participants), the returns to time-varying worker characteristics can be estimated using first-differences to eliminate individual fixed effects that are unmeasured by the econometrician. Similarly, in this simplest model, first-differences can be used to estimate sectoral wage differentials without bias from unmeasured fixed effects. Unfortunately, first-difference estimation is not appropriate for any of the three other theoretical models we develop. Simply put, in these models, a worker's fixed ability does not translate into a fixed effect in a wage equation, so first-differencing the wage equation does

not correct ability bias. The contribution of this paper is to move beyond merely warning of this problem (as in Gibbons and Katz, 1992, for example) to proposing a solution.

Our theoretical models rely heavily on the assumption of normality. Many models that rely on normality can be estimated by maximum likelihood or by two-step methods, but estimating our dynamic model of wage determination and sector affiliation would be computationally difficult because it entails more than two sectors and more than two periods. In addition, it is not necessary to estimate the full model when the parameters of interest are those that determine the returns to skills and wage differences across sectors. We therefore undertake the more modest task of estimating the wage equations in each sector.<sup>4</sup>

We show that our richest theoretical model produces a random-coefficients econometric model in a panel-data setting, which can be estimated using a non-linear instrumental-variables technique. Even in this richest model, consistent estimates of the effects of both measured and unmeasured skills on wages require neither distributional assumptions nor standard exclusion restrictions. (That is, we use normal distributions to develop the full theory, but we do not need these assumptions to estimate the parameters of interest related to sectoral wage differentials and sector-specific returns to skills.) Instead, the estimation strategy utilizes natural restrictions available in panel data with three or more observations per person. Our econometric approach is similar to other panel-data models in which first-differenced estimates are inconsistent, such as Holtz-Eakin, Newey, and Rosen (1988) and Lemieux (1998).

After developing the theory and econometrics, we implement two empirical investigations, concerning sorting and wage differentials across occupations and industries, using individual-level panel data from the National Longitudinal Survey of Youth. Our richest theoretical model is consistent with several familiar facts about wage determination: a typical individual's wage increases with experience, the variance of the wage distribution across

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<sup>4</sup> Other results exist on the identifiability of related models in the absence of normality. For example, Heckman and Honore (1990) show that the Roy model is identified with panel data and exogenous shifts in the price of skills over time. But Heckman and Honore focus on the estimation of a sequence of static models; they do not address learning and job mobility.

individuals increases with experience, and the skewness of the wage distribution increases with experience.<sup>5</sup> But variants of Mincer's (1974) theory can also explain these basic facts, so we focus on our model's further predictions, concerning the returns to skills and the resulting allocation of workers across sectors. For both occupations and industries, we find important variation in sector-specific returns to observed and unobserved skills. Furthermore, in both cases, high-wage sectors employ high-skill workers and offer high returns to workers' skills, so estimates of sectoral wage differences that do not account for sector-specific returns to skill and the sorting of workers across sectors on the basis of unmeasured skills are misleading and difficult to interpret.

Although our empirical work explores two standard definitions of sectors (namely, occupations and industries), other definitions are possible. For example, sectors could be jobs inside a firm (Gibbons and Waldman, 1999; Lluís, 2001), states or regions within a country (Borjas, Bronars, and Trejo, 1992), or entire countries (Borjas, 1987). In fact, the individuals in our model need not be workers. Instead, they could be firms, where what we call worker ability is reinterpreted as firm productivity, much as Jovanovic (1982) reinterpreted Jovanovic (1979).

## 2. Theory and Econometrics

The four theoretical models analyzed below are special cases of the following model. If worker  $i$  is employed in sector  $j$  at time  $t$ , the worker's output is

$$(1) \quad y_{ijt} = \exp(X_{it} \beta_j + \psi_{ijt}),$$

where  $X_{it}$  is a vector of human-capital and demographic variables measured by the econometrician and  $\psi_{ijt}$  represents determinants of productivity that are not measured by the econometrician. The worker characteristics  $X_{it}$  and the slope vector  $\beta_j$  are known by all labor-

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<sup>5</sup> An illustrative discussion of these implications of the model and a comparison with alternative labor market models is presented in Neal and Rosen (2000).

market participants at the beginning of period  $t$ ; the realized output  $y_{ijt}$  is observed by all labor-market participants at the end of period  $t$ . The error term  $\psi_{ijt}$  has the components

$$(2) \quad \psi_{ijt} = Z_i + b_j (\eta_i + \varepsilon_{ijt}) + c_j,$$

where  $Z_i$  denotes the portion of worker  $i$ 's productive ability that is equally valued in all sectors,  $\eta_i$  denotes the portion that is differentially valued across sectors, and  $\varepsilon_{ijt}$  is a random error. The coefficients  $\{b_j, c_j : j = 1, \dots, J\}$  are fixed and known to all labor-market participants. The noise terms  $\varepsilon_{ijt}$  are normal with zero mean and precision  $h_\varepsilon$  (i.e., variance  $\sigma_\varepsilon^2 = 1/h_\varepsilon$ ) and are independent of each other and of all the other random variables in the model.

In developing the theory and econometrics, we treat  $Z_i$  and  $\eta_i$  differently. We assume throughout that  $Z_i$  is observed by all labor-market participants; this is the standard case of a fixed effect that the econometrician cannot observe but market participants can. For  $\eta_i$ , however, we consider two cases: perfect information (no learning by market participants, as with  $Z_i$ ) and imperfect information (learning). One could also imagine investigating learning about  $Z_i$ . Farber and Gibbons (1996) study this problem in the absence of sector-specific returns to ability (i.e.,  $b_j=b$  and  $\beta_j = \beta$  for every  $j$ , so that a worker's unmeasured ability is  $Z_i + b\eta_i$  and is equally valued in every sector), but the combined problem of learning about  $Z_i$  and about  $\eta_i$  awaits future research.

In the imperfect-information case, all labor-market participants share symmetric but imperfect information about  $\eta_i$ . In particular, given their initial information ( $Z_i$  and  $X_{i1}$ ), all participants in the labor market share the prior belief that  $\eta_i$  is normal with mean  $m$  and precision  $h$ . Subsequent productivity observations,  $y_{ijt}$ , refine this belief. Information in the labor market therefore remains symmetric and improves over time. For simplicity, we assume that subsequent realizations of measured skills,  $X_{it}$ , are conditionally independent of  $\eta_i$  given  $Z_i$  and  $X_{i1}$ . (This assumption is not only convenient but realistic, because the major time-varying element of  $X_{it}$  is experience.) Thus, market participants can compute

$$(3) \quad s_{it} = \frac{\ln y_{ijt} - X_{it}\beta_j - Z_i - c_j}{b_j},$$

which yields  $s_{it} = \eta_i + \varepsilon_{ijt}$ , a noisy signal about the worker's ability that is independent of the worker's sector during period  $t$ . We call  $s_{it}$  the worker's normalized productivity observation for period  $t$ . Let  $s_i^t = (s_{i1}, \dots, s_{it})$  denote the history of the worker's normalized productivity observations through period  $t$ . Then (from Chapter 9 of DeGroot, 1970) the posterior distribution of  $\eta_i$  given history  $s_i^t$  is normal with mean

$$(4) \quad m_t(s_i^t) = \frac{hm + h_\varepsilon(s_{i1} + \dots + s_{it})}{h + th_\varepsilon}$$

and precision  $h_t = h + th_\varepsilon$ .

To close the model, we assume that workers are risk-neutral and that there is no cost to firms or workers at the beginning or end of a job (*i.e.*, no hiring, firing, or mobility costs), so we can restrict attention to single-period compensation contracts. For simplicity, we further restrict attention to contracts that specify the period's wage before the period's production occurs (as opposed to piece-rate contracts). Competition among firms causes each firm in a given sector to offer a given worker a wage equal to the expected value of the worker's output in that sector, given the worker's observed characteristics and history of previous output realizations.

## 2.1 Sorting without Comparative Advantage

In this sub-section we ignore the possibility of comparative advantage by assuming that  $b_j = b$  for every  $j$ , so that a worker's unmeasured ability is  $Z_i + b\eta_i$  and is equally valued in every sector. Continuing in this vein, we also assume in this section that  $\beta_j = \beta$  for every  $j$ . But we allow the intercepts  $c_j$  to vary by sector, in keeping with the possibility that measured sector premia may reflect true sector effects. Of course, all else constant, jobs in sectors with high



values of  $c_j$  may be more attractive (depending on the source of  $c_j$ , such as rent-sharing versus compensating differentials). If some sectors are more attractive, issues such as queuing and rationing arise. Because our main interest is in the richer model with comparative advantage in Section 2.3, we do not formally address queuing or rationing here.

It is not controversial that workers' productive abilities are imprecisely measured in standard micro data sets. But if unmeasured skills are to explain estimated sector wage differentials then these skills must be non-randomly allocated across sectors. This, too, is plausible, for example because different sectors use different technologies that require workers' skills in different proportions. But if this unmeasured-skill explanation for measured sectoral wage differentials is correct, it suggests that the few skills that are measured in standard micro data sets (hereafter "measured skills") should be systematically related to the sector in which the worker is employed. We investigate this prediction about measured skills in our empirical work on occupations and industries in Sections 3 and 4. For now, however, we confine our attention to econometric approaches to estimating the role of unmeasured skills.

In the perfect-information case without comparative advantage, all firms know that the worker's ability is  $Z_i + b\eta_i$ . As always, the wage offered to worker  $i$  by firms in sector  $j$  in period  $t$  is the worker's expected output in that sector, but the only uncertainty in this case is the error term  $b\varepsilon_{ijt}$  in (2). Recall that if  $\log \theta$  is normally distributed with mean  $\mu$  and variance  $\sigma^2$  then  $E(\theta) = \exp\{\mu + (1/2)\sigma^2\}$ . Therefore the log wage offered to worker  $i$  in sector  $j$  in period  $t$  is

$$(5) \quad \ln w_{ijt} = X_{it}\beta + Z_i + b\eta_i + c_j + (1/2)b^2\sigma_\varepsilon^2.$$

Turning to the imperfect-information case without comparative advantage, in each period, firms in sector  $j$  bid worker  $i$ 's wage up to the worker's expected output in that sector (conditional on the publicly observable information available at that date), so the log wage is

$$(6) \quad \ln w_{ijt} = X_{it}\beta + Z_i + bm_{i,t-1} + c_j + (1/2)b^2\sigma_\varepsilon^2,$$

where  $m_{i,t-1}$  is shorthand for  $m_{t-1}(s_i^{t-1})$  and  $\sigma_t^2 = [h + th_\varepsilon]/(h_\varepsilon[h + (t-1)h_\varepsilon])$ . In both the perfect- and the imperfect-information cases, the worker's ability  $\eta_i$  is unmeasured by the econometrician (as is  $Z_i$ ); in the latter case,  $\eta_i$  is also unobserved by labor-market participants (unlike  $Z_i$ ). Note that, since  $t$  represents the number of years of experience in the model, the error component  $(1/2)b^2\sigma_t^2$  will be captured by a linear function in labor market experience that we include in all estimated models.

## 2.2 Estimation without Comparative Advantage

In the absence of both learning and comparative advantage, the source of possible bias in conventional cross-section estimates of sectoral log wage differentials is the potential partial correlation between sector affiliation and unmeasured skills ( $Z_i$  and  $\eta_i$ ) conditional on measured skills ( $X_{it}$ ). In this simplest case, the worker's fixed ability ( $Z_i + b\eta_i$ ) creates a worker fixed-effect in the wage regression, which can be eliminated in standard fashion. For example, a first-differenced regression eliminates the fixed effect  $Z_i + b\eta_i$  in (5).

Even without comparative advantage, however, learning implies that fixed ability is not a fixed effect in the earnings equation. The key property of our learning model is that Bayesian beliefs are a martingale. That is, the conditional expectation  $m_t(s_i^t)$  in (4) obeys the law of motion

$$(7) \quad m_{it} = m_{i,t-1} + \xi_{it},$$

where  $\xi_{it}$  is a noise term orthogonal to  $m_{i,t-1}$ . In somewhat more intuitive terms, the market begins period  $t$  with the information contained in  $s_i^{t-1}$  and then extracts new information about  $\eta_i$  from the output observation  $y_{ijt}$  (or, equivalently,  $s_{it}$ ). But the new information that can be extracted from  $y_{ijt}$  is precisely the part that could not be forecasted from  $s_i^{t-1}$ . Hence, the innovation  $\xi_{it}$  is orthogonal to the prior belief  $m_{i,t-1}$ .

Farber and Gibbons (1996) explored some of the implications of this martingale property. But they focused on several specific predictions that can be derived regarding regressions in which the dependent variable is the level of earnings, not the log of earnings. In this paper, in contrast, we use the log of earnings as the dependent variable, so the specific Farber-Gibbons predictions do not hold, but the martingale property of the market's beliefs continues to create endogeneity problems, as follows.<sup>6</sup>

Formally, a first-differenced regression eliminates  $Z_i$  but not  $m_{i,t-1}$  from (6). Instead, first-differencing (6) for a worker who switches from sector  $j$  to sector  $j'$  yields

$$(8) \quad \ln w_{it} - \ln w_{i,t-1} = (X_{it} - X_{i,t-1})\beta + b(m_{i,t-1} - m_{i,t-2}) + (c_{j'} - c_j) + (1/2)b^2(\sigma_t^2 - \sigma_{t-1}^2) ,$$

where  $m_{i,t-1} - m_{i,t-2} = \xi_{i,t-1}$ . But  $\xi_{i,t-1}$  may be correlated with the change in sector affiliation through whatever (unmodeled) process led unmeasured ability to be correlated with sector affiliation in the first place.<sup>7</sup> Thus, with learning, first-differenced estimates of sectoral wage differentials are biased if the change in the residual is correlated with the change in sector affiliation. Fortunately, this endogeneity problem is simple to correct because the new information summarized in  $\xi_{i,t-1}$  is not related to wage, skill, or sector information in period  $t - 1$  or earlier. (See Section 2.4 for more discussion of this issue.) For example, equation (8) can be estimated by two-stage least squares using the interaction of the worker's score on an ability test (taken before the worker entered the labor market) and the worker's sector affiliation at  $t-1$  as a valid instrumental variable for changes (between  $t - 1$  and  $t$ ) in sector affiliation.

<sup>6</sup> Relative to Farber and Gibbons, we also use the more specific production function (1), the more specific error structure (2), and the more specific distributional assumptions given in the text below (2). We impose these more specific assumptions in order to explore several issues related to the returns to skills across sectors, which Farber and Gibbons could not address with their more general model.

<sup>7</sup> For example, suppose that there are only two levels of ability, high and low, but that sectors differ in the proportion of high-ability workers they employ. Consider a high-ability worker whose employment exogenously ends in sector  $j$ . Suppose that such a worker is equally likely to be re-employed in any of the economy's jobs for high-ability workers. Then there is some probability that the worker's new job is again in sector  $j$ , but if the worker changes sectors then it is likely that the new job is in a sector with a large number of high-ability jobs. In this case, positive information about a worker's ability will tend to be associated with shifts to high-wage sectors (where high-skill jobs are more plentiful), and the reverse for negative information.

### 2.3 Sorting with Comparative Advantage

In this section we relax the assumption that a worker's ability is equally valued in every sector. By introducing comparative advantage, we endogenize sector affiliation. By subsequently introducing learning, we endogenize not only wage changes but also sector mobility.

To analyze comparative advantage, we now return to the production function specified in (1) and (2), where the slope coefficients  $\beta_j$  in (1) and  $b_j$  in (2) vary by sector. We index the  $J$  sectors so that  $b_j$  strictly increases in  $j$ : sector  $j + 1$  values the worker's ability  $\eta_i$  more than does sector  $j$ . In keeping with the notion that ability is productive, we assume that  $b_1 > 0$ . Given a fixed  $X_{it}$  there exist critical values of  $\eta_i$  that determine the efficient assignment of workers to sectors. Denoting these critical values by  $\{v_j(X_{it}) : j = 0, 1, \dots, J\}$ , the efficient assignment rule assigns worker  $\eta_i$  to sector  $j$  if and only if  $v_{j-1}(X_{it}) < \eta_i < v_j(X_{it})$ , where  $v_0(X_{it}) = -\infty$ ,  $v_n(X_{it}) = \infty$ , and  $v_j(X_{it})$  strictly increases in  $j$ . See Figure 1 for a graphical representation of this efficient assignment rule.

We again analyze first perfect and then imperfect information. In the perfect-information case, firms in sector  $j$  bid worker  $i$ 's wage up to the expected output in that sector:

$$(9) \quad \ln w_{ijt} = X_{it}\beta_j + Z_i + b_j\eta_i + c_j + (1/2)b_j^2\sigma_\varepsilon^2,$$

analogous to (5) but with the sector-specific returns  $\beta_j$  and  $b_j$ . If the worker faces no mobility constraints, worker  $i$  will choose to work in sector  $j$  if  $v_{j-1}(X_{it}) < \eta_i < v_j(X_{it})$ . Thus, taking the model literally, sector mobility in the perfect-information case is driven entirely by changes in  $X_{it}\beta_j$ . One could envision exogenous shocks to sector demand that produce additional sector mobility in this model, but we will not formally model such shocks, for the same reason that we did not model queues or rationing above: our ultimate interest is in the model with comparative advantage and learning, which gives a coherent account of sectoral mobility without reference to queues, rationing, or sectoral shocks. Whatever the reason that worker  $i$  is employed in sector  $j$

in period  $t$ , in the perfect-information case with comparative advantage we assume that the worker's wage is given by (9).

In the imperfect-information case, we again assume that information in the labor market is symmetric but imperfect, as described above. As in the model of learning without comparative advantage, all participants in the labor market share the prior belief that  $\eta_i$  is normal with mean  $m$  and precision  $h$ , conditional on their initial information  $Z_i$  and  $X_{i1}$ . Inferences from the productivity observations,  $y_{ijt}$ , are greatly simplified because the information content of an output observation is constant across sectors; that is, (2) involves  $b_j(\eta_i + \varepsilon_{ijt})$  rather than  $b_j\eta_i + \varepsilon_{ijt}$ . This functional form is what allows us to define the normalized productivity observation for worker  $i$  in period  $t$ ,  $s_{it}$  from (3), to be a noisy signal about the worker's ability that is independent of the worker's sector during period  $t$ . Relaxing this assumption about the functional form of (2) would complicate the analysis because workers' sector choices would then depend on the benefit from faster learning as well as on the benefit from increased expected output given current beliefs. Relaxing the assumption that all labor-market participants observe  $Z_i$  (so that there could be learning about both  $Z_i$  and  $\eta_i$ ) would cause similar complications. Under our assumptions, the posterior distribution of  $\eta_i$  given the history  $s_i^t$  is normal with mean  $m_{it}$  given by (4) and precision  $h_t = h + th_e$ , regardless of the worker's history of sector affiliations.

In this fourth model, with learning and comparative advantage, we finally have an internally consistent account for sector affiliation, wages, sector mobility, and wage changes, as follows. In each period, firms in sector  $j$  bid a worker's wage up to the worker's expected output in that sector, conditional on the publicly observable information about the worker available at that date:

$$(10) \quad \ln w_{ijt} = X_{it}\beta_j + Z_i + b_j m_{i,t-1} + c_j + (1/2)b_j^2 \sigma_t^2,$$

analogous to (6) but with sector-specific returns  $\beta_j$  and  $b_j$ . The model also includes sector-specific (experience) effects since the posterior variance  $\sigma_t^2$ , which declines with time (labor

market experience), is interacted with  $b_j^2$ . The worker chooses to work in the sector offering the highest current wage. Thus, worker  $i$  chooses sector  $j$  in period  $t+1$  if  $v_{j-1}(X_{it}) < m_{it} < v_j(X_{it})$ .

## 2.4 Estimation with Comparative Advantage

We now develop a non-linear instrumental-variables procedure to estimate the parameters  $\{\beta_j, b_j, c_j; j = 1, \dots, J\}$  in (9) and (10). This procedure does not rely on normality and can be implemented using standard computer packages. To discuss the estimation of the model, define the sector indicators  $D_{ijt}$  where:

$D_{ijt} = 1$  if person  $i$  is employed in sector  $j$  at time  $t$ ,

$D_{ijt} = 0$  otherwise.

The wage equation (10) for each sector  $j$  can then be written as a single equation where measurement error  $\mu_{it}$  is assumed to be independent of sector affiliation:

$$(11) \quad \ln w_{it} = \sum_j D_{ijt} c_j + \sum_j D_{ijt} X_{it} \beta_j + Z_i + \sum_j D_{ijt} b_j m_{i,t-1} + (1/2) \sum_j D_{ijt} b_j^2 \sigma_i^2 + \mu_{it}.$$

Estimates of the sector slopes and intercepts  $\{\beta_j, c_j; j = 1, \dots, J\}$  obtained by estimating equation (11) with OLS are inconsistent. The problem is that expected ability influences sector affiliation, so  $m_{i,t-1}$  is correlated with the set of sector dummies  $\{D_{ijt}, j = 1, \dots, J\}$ .

The endogeneity problem in equation (11) is different from the usual fixed-effect case for two reasons. First, as noted in Section 2.2,  $m_{i,t-1}$  is a martingale rather than a fixed effect. This martingale property does not depend on the normality assumptions in our theoretical model; all Bayesian beliefs are martingales. In the absence of comparative advantage, we could handle this martingale problem as described in Section 2.2. But, second, comparative advantage causes  $m_{i,t-1}$  to be interacted with the set of sector dummies  $\{D_{ijt}, j = 1, \dots, J\}$  in (11).

Other panel-data models in which first-differenced estimates are inconsistent have been considered in the literature. For example, Holtz-Eakin, Newey, and Rosen (1988) discuss the

estimation of models in which the fixed effect is interacted with year dummies. They show that consistent estimates can be obtained by quasi-differencing the equation of interest and then using appropriate instrumental-variables techniques. Similarly, Lemieux (1998) estimates a model in which the return to a time-invariant unobserved characteristic is different in the union and the nonunion sectors.

The estimation strategy we follow also relies on quasi-differencing combined with IV techniques. More precisely, we eliminate  $m_{i,t-1}$  by exploiting the dynamic structure of the problem, as follows. First, solving (11) for  $m_{i,t-1}$  yields:

$$(12) \quad m_{i,t-1} = \frac{\ln w_{it} - \sum_j D_{ijt} c_j - \sum_j D_{ijt} X_{it} \beta_j - Z_i - (1/2) \sum_j D_{ijt} b_j^2 \sigma_t^2 - \mu_{it}}{\sum_j D_{ijt} b_j} .$$

The lagged version of equation (12) yields an expression for  $m_{i,t-2}$ . Substituting this expression for  $m_{i,t-2}$  into the law of motion  $m_{i,t-1} = m_{i,t-2} + \xi_{i,t-1}$  gives a new expression for  $m_{i,t-1}$ , which can be substituted into the wage equation (11) to yield:

$$(13) \ln w_{it} = \sum_j D_{ijt} c_j + \sum_j D_{ijt} X_{it} \beta_j + \frac{\sum_j D_{ijt} b_j}{\sum_j D_{ij,t-1} b_j} (\ln w_{i,t-1} - \sum_j D_{ij,t-1} c_j - \sum_j D_{ij,t-1} X_{i,t-1} \beta_j) + e_{it} ,$$

where

$$e_{it} = Z_i + \frac{1}{2} \sum_j D_{ijt} b_j^2 \sigma_t^2 + \mu_{it} + \sum_j D_{ijt} b_j \xi_{i,t-1} - \frac{\sum_j D_{ijt} b_j}{\sum_j D_{ij,t-1} b_j} \left( Z_i + \frac{1}{2} \sum_j D_{ij,t-1} b_j^2 \sigma_{t-1}^2 + \mu_{i,t-1} \right) .$$

Equation (13) is a non-linear wage equation from which  $m_{i,t-1}$  has been eliminated. Non-linear least-square estimates of the sector slopes and intercepts  $\{\beta_j, b_j, c_j; j = 1, \dots, J\}$  are nevertheless inconsistent for two reasons. First, the lagged version of equation (11) shows that  $\ln w_{i,t-1}$  is correlated with  $\mu_{i,t-1}$  by construction (but not with  $\xi_{i,t-1}$ , which is unknown at the moment

$w_{i,t-1}$  is decided). Second, the sector dummies  $D_{ijt}$  are correlated with  $v_{i,t-1}$  because expected ability influences sector affiliation. Fortunately, both of these problems can be handled by finding appropriate instruments for  $\ln w_{i,t-1}$  and the set of sector dummies  $\{D_{ijt}, j = 1, \dots, J\}$ . Such instrumental variables must of course be independent of the error term  $e_{it}$  in equation (13).

The most obvious candidate instrumental variables are wage, skill, or sector information from period  $t-1$  or earlier, as well as interactions of these variables. Since the evolution of wages and sector affiliation over time is driven by the evolution of  $m_{it}$ , these wage, skill, and sector histories should help predict  $m_{i,t-1}$  and thus  $\ln w_{i,t-1}$  and  $\{D_{ijt}, j = 1, \dots, J\}$ . We chose the interaction between sector affiliation at time  $t-1$  and  $t-2$ ,  $\{D_{ij,t-1}, j = 1, \dots, J\}$  and  $\{D_{ij,t-2}, j = 1, \dots, J\}$ , as our main instrumental variables. These interactions between sector affiliation at  $t-1$  and  $t-2$  are uncorrelated with the error term  $e_{it}$  in equation (13) given the model's assumption that sector affiliation is determined only by perceptions about the sector-sensitive components of ability ( $X_{it}$  and  $\eta_i$ ) and is independent of any part of ability that is not differentially valued across sectors ( $Z_i$ ). In Appendix B we discuss in more detail why the model suggests using these variables as instruments. We also show evidence of their predictive power. For efficiency reasons discussed below, we also include a set of interactions of sector affiliation at time  $t-2$  with the explanatory variables  $X_{it}$  (as summarized by a skill index and year of experience) in the instrument set.<sup>8</sup>

We estimate the parameters in equation (13) using non-linear instrumental-variables (NLIV) techniques. Consider  $e$ , a vector in which all the individual error terms  $e_{it}$  are stacked, and  $V$ , a matrix in which the individual instrument vector  $v_{it}$  (e.g., sector histories) are stacked. Since the error terms  $e$  should be uncorrelated with the instruments  $V$ , the orthogonality condition  $(1/N)e'V = 0$  should hold. The NLIV method consists of setting the sample analogs of

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<sup>8</sup> Since both the terms  $X_{it}\beta_j$  and  $(1/2)b_j^2\sigma_i^2$  are interacted with sector affiliation, which is endogenous, it is natural to include some instruments for sector affiliation interacted with those terms in the instrument set. In the estimation we replace  $X_{it}\beta_j$  by a skill index discussed below and use experience to proxy for  $\sigma_i^2$ . This leads to adding interactions between the second lag of sector affiliation (the instruments for sector affiliation) and the skill index and experience to the main set of instruments (interactions between sector affiliation at  $t-1$  and  $t-2$ ).



$(1/N)e'V$  as close as possible to zero by finding the values of the parameters  $c_j$ ,  $b_j$ , and  $\beta_j$  (for  $j = 1, \dots, J$ ) that minimize the quadratic form

$$(14) \quad S = (1/N) (e'V)M(e'V),$$

where  $M$  is a weighting matrix. Note that the parameters  $\{\beta_j, b_j, c_j; j = 1, \dots, J\}$  are implicitly included in  $S$  because the elements  $e_{it}$  of  $e$  are computed as the difference between  $\ln w_{it}$  and the explanatory factors on the right-hand-side of equation (13). Under the assumption that  $e_{it}$  is homoskedastic and uncorrelated, it is well known that the most efficient estimate is obtained by using the inverse of the variance of  $e'V$  as weighting matrix,  $M=(V'V)^{-1}$ . However, equation (13) shows that  $e_{it}$  is a relatively complex function of the sector dummies. So  $e_{it}$  will be heteroskedastic even if the “structural” error terms  $\xi_{it}$  and  $\mu_{it}$  are homoskedastic.

To allow for heteroskedasticity, we use a standard GMM two-step procedure in which the parameters are first estimated using  $M=(V'V)^{-1}$ . These consistent but inefficient parameters are used to compute a heteroskedasticity-robust variance matrix  $\Sigma$  of  $e'V$ . Efficient GMM estimates are then obtained using  $M=\Sigma^{-1}$ . Furthermore, it is easily shown (Hansen, 1982) that  $N$  times the minimized value of  $S$  follows a  $\chi^2$  distribution with  $q$  degrees of freedom, where  $q$  is the number of overidentifying restrictions (the difference between the number of instruments and the number of parameters). In complicated non-linear models like ours, however, this overidentification test is better thought as an omnibus specification test than as a standard test of the validity of instruments commonly performed for linear models. We discuss in more detail in Appendix B why these tests should be interpreted with care.

In the linear case where  $e_{it}$  is homoskedastic and uncorrelated, NLIV is just the well-known two-stage least-squares estimator. One difficulty with NLIV is that since we project a non-linear function of the model variables and of the parameters ( $e$ ) into a linear set of instruments ( $V$ ), the instruments must be chosen in a way that predicts sufficiently well the

explanatory righthand side of equation (13).<sup>9</sup> In addition to the sector histories discussed above, we thus include as instruments a set of interactions between the explanatory variables  $X_{it}$  (as summarized by a skill index and years of experience) and the period  $t - 2$  dummies for sector affiliation  $\{D_{ij,t-2}, j = 1, \dots, J\}$ .

In the perfect-information case, where unmeasured ability  $\eta_i$  is observed by labor market participants, the quasi-differenced equation (13) remains the same except that the innovation term  $\xi_{i,t-1}$  drops from the error term  $e_{it}$ . The remaining endogeneity problem is due to the correlation between  $\ln w_{i,t-1}$  and the error component  $\mu_{i,t-1}$ . In this case, we simply use the full set of interactions between the sector dummies at time  $t$  and  $t - 1$  as instruments for  $\ln w_{i,t-1}$ .<sup>10</sup>

### 3. Data

The data set used in this paper is the National Longitudinal Survey of Youth, or NLSY. Individuals in the NLSY were between the ages of fourteen and twenty-one on January 1, 1979. We use up to seventeen yearly observations per worker (from 1979 to 1996).<sup>11</sup> One advantage of the NLSY is that it allows us to follow workers from the time they make their first long-term transition to the labor force.

We use the same sample-selection criteria as those used by Farber and Gibbons (1996). We classify individuals as having made a long-term transition to the labor force when they spend at least three consecutive years primarily working, following a year spent primarily not working. Someone is classified as primarily working if she/he has worked at least half the weeks since the

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<sup>9</sup> See Newey (1990) for more discussion and proposed (nonparametric) solutions to this problem. Note that choosing the functional form or the number of instruments can also be problematic in the linear model (Donald and Newey, 1999).

<sup>10</sup> In the absence of learning, either the interactions between sector affiliation at time  $t$  and  $t-1$  or at time  $t-1$  and  $t-2$  can be used as instruments. In practice, this choice has little impact on the results since both sets of instruments predict very well the wage (see Appendix B). Since sector affiliation is exogenous in this model, we do not need to include the additional interaction terms between the skill index, experience, and sector affiliation at time  $t-2$  discussed above. Note that Lemieux (1998) uses an identical strategy to estimate union wage differentials when unmeasured ability is known to all labor-market participants but is differently rewarded in the union and non-union sectors: the interaction between the union status at time  $t$  and  $t - 1$  is used as an instrument for the lagged wage.

<sup>11</sup> There was no interview in 1995.

last interview and averaged at least thirty hours per week during the working weeks. Note that the “last interview” does not necessarily refer to the previous calendar year if an individual had not been interviewed the year before. Self-employed workers are deleted, as are members of the NLSY military subsample. Readers are referred to Appendix 1 in Farber and Gibbons (1996) for more details on the criteria used to construct our NLSY sample.

Farber and Gibbons used NLSY data from 1979 to 1991 interview years, whereas our data are through 1996. Except for the longer sampling frame, the only noteworthy difference between our sample and Farber and Gibbons’s has to do with union coverage in 1994. For some reason, the question on union coverage in the current or most recent job at interview time (job number 1 in the work history file) was not asked in that year. Although the error was caught and fixed during the field period, many respondents were simply not asked this question even though they should have been. Consequently, the raw data shows a large number “valid skips”.<sup>12</sup> We provide a correction of our own to partially fix this problem and recover quite a few of those missing observations. More precisely, if an individual in 1994 is working for the same employer as the one he worked for in the previous interview, we assign the value of the union coverage dummy for the previous interview year to the current one. If the individual interviewed in 1994 has started working for a new employer since the last interview, we check to see whether she/he is still working for that employer in 1996. If so, we assign the value of the union coverage dummy for that year to the 1994 interview.

From this NLSY sample we focus on the subsample of observations at which the individual was working at the interview date for at least the previous three years. This sample restriction enables us to use the first and second lags of various variables in the estimation, as explained in Section 2.4. We exclude workers in agricultural jobs. Since we (later) divide manufacturing into durable and nondurable goods manufacturing, we also exclude a few workers who hold jobs in manufacturing industries that are hard to classify as producing durable as

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12 Personal communication from Steve McClaskie of the Center for Human Resource Research.

opposed to nondurable goods.<sup>13</sup> We are left with a sample of 35,438 observations on 5,904 workers that satisfy these sample-selection criteria.

To summarize the relationship between the wage premia and observable skills, we construct a "skill index" for each worker. We first estimate a flexible log (hourly) wage equation using our sample.<sup>14</sup> The base explanatory variables used in the log wage equation are the AFQT score, years of education, education category dummies (dropout, high school graduates, some college, and college degree), (actual) experience, experience squared, dummy variables for race, gender, marital status, union status, and a set of dummies for year, industry, and occupation. We also include sets of pairwise interactions between the education category dummies, gender, and race, as well as interactions between gender and experience, gender and marital status, and race and experience. Detailed regression results are reported in Appendix Table A with and without the AFQT variable included.

We then use the estimated coefficients from that equation to predict the wage of each worker. The skill index is the predicted wage based solely on the education, experience, and AFQT score of the worker. That is, although characteristics such as occupation, industry, union status, and demographic characteristics are included in the initial wage equation, they are not used to construct the skill index for the worker. We normalize the skill index to have zero mean.

#### **4. Wages and Returns to Skills Across Occupations**

We believe that the concepts of sorting and comparative advantage are likely to play a more important role for occupations than for industries, so we first estimate our models for occupations. As we mention in Section 5, other factors such as compensating wage differences and rent-sharing may mask the importance of comparative advantage in the case of industries. Furthermore, our one-factor model is well suited to cases where there is a natural ordering of

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<sup>13</sup> These industries are: stone, clay, and glass; tobacco manufacturing; leather and leather products; and not specified manufacturing. Workers in these industries represent less than one percent of the full sample.

<sup>14</sup> The wage variable in all estimated models is the hourly wage on the current job at the time of the survey.

sectors from least skill-sensitive to most skill-sensitive. We believe this ordering is more likely apply to occupations (e.g. going from operatives to craft workers to managers) than industries.

#### **4.1 Occupational Wage Premia without Comparative Advantage**

Throughout the paper, we divide workers into seven conventional occupation aggregates.<sup>15</sup> In Table 1 we report the raw occupation log wage differentials (relative to the service occupation) and the average values of measured skills (education and experience) and other measured characteristics (race, sex, and marital status) by occupation. As is well known, there are large differences across occupations in mean wages and in mean values for education and other characteristics. There is also a strong link between these two variables: the correlation between the raw wage premium and the mean level of education is 0.81 (bottom row of Table 1).

The mean skill index for each occupation is reported in Column 7. In keeping with the positive correlation between the wage premium and mean education, we find that the correlation between the raw wage premium and the mean skill index is 0.96. But the cross-occupation variation in mean log wages in Column 1 (standard deviation of 0.181) is almost twice as large as cross-occupation variation in the skill index in Column 7 (standard deviation of 0.099), suggesting that there may be more to the story than just observable skills. In this spirit, Column 1 of Table 2 reports an OLS regression of the log wage on the skill index and six occupation dummies (operatives and laborers are the base occupation). All the models reported in Table 2 also include controls for industry affiliation (nine dummy variables), gender, race, marital status, union status, and a full set of year dummies.

The skill index is highly significant and has a coefficient of one (by construction), but the occupation coefficients remain highly significant, although smaller than the raw wage differentials reported in Table 1. Of course, such a regression merely replicates the common

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<sup>15</sup> Using a more detailed classification does not alter our basic findings and comes at the cost of less precise estimates of the occupation effects. Precision is an issue for the some of the non-linear instrumental variables models presented below.

finding that the occupation coefficients are significant even after controlling for measured characteristics. We report it as our point of departure.

In this OLS model, the standard deviation of the estimated occupational wage premia is .086. Column 2 reports first-differenced estimates of these premia; their standard deviation falls to .021. Of course, these first-differenced estimates might be attenuated by false transitions. One approach to the false-transitions problem is to estimate a fixed-effect regression rather than a first-differenced regression.<sup>16</sup> We present fixed-effect estimates in Column 3; the occupational wage premia have a standard deviation of .032. Another approach to the false-transitions problem is to re-compute the first-differenced estimates on the sub-sample of observations in which the worker reports taking a new job (with a new employer). The resulting wage premia (in Column 4) have a standard deviation of .036. In sum, the estimates in Columns 3 and 4 are consistent with the view that more than half of the variation in occupational wage premia (after controlling for measurable skills) may be due to unmeasured ability bias, even in our simplest model without comparative advantage or learning.

In Columns 5 and 6 of Table 2 we explore the possibility of further bias associated with learning (but not comparative advantage). As described at the end of Section 2.2, the problem is that learning about ability may be correlated with the change in sector affiliation (such as where job loss is exogenous but re-employment is not). As suggested in Section 2.2, we can use wage, skill, and sector information from period  $t-1$  or earlier to instrument for the change in sector affiliation between periods  $t-1$  and  $t$ . In Columns 5 and 6 we use as instruments the full set of interactions between occupation dummies at times  $t-1$  and  $t-2$ . For the full sample (Column 5), none of the individual occupation effects is significant. Furthermore, the standard deviation of the estimated occupation effects is quite small (.009), and we cannot reject that these premia are all zero (p-value of .46). The results for the sub-sample of new jobs (Column 6) are relatively

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<sup>16</sup> Fixed-effect estimates use information from both first differences and longer differences and so are less affected by measurement error than first-difference estimates are (Griliches and Hausman, 1986).

similar. Now some of the estimated occupation effects are individually significant, but we still cannot reject the null hypothesis that all premia are jointly equal to zero (p-value of 0.12).

Note that the estimates of the model for the sample of new jobs are much less precise than when all observations are being used. As we will see in the next Section, limiting the analysis to new jobs appears to be a much more efficient way of eliminating false transitions in the case of industries than occupations. The problem is that people can clearly change occupation by being promoted or re-assigned to a different task while staying with the same employer. We lose these legitimate changes when we focus on new jobs only. By contrast, it is much more difficult for an employee to change industry while staying with the same employer. This means we should lose little legitimate information by focusing on new jobs in the case of industries.

In sum, our results suggest that accounting for both unmeasured ability and learning eliminates most of the occupational wage premia. The results in Columns 2-4 indicate that controlling for measured and unmeasured skills explains over 80 percent of the raw standard deviation of wages across occupations (0.181). The remaining premia are no longer significant when learning is accounted for in Columns 5 and 6, though these results are less precise than in the more standard models of Columns 1 to 4.

## **4.2 Occupational Wage Premia and Occupational Skill Premia**

Our exploration of occupation wage premia without comparative advantage strongly suggests that learning combined with the sorting of both measured and unmeasured skills accounts for the bulk of occupational wage premia. In this section we explore the sources of this sorting by adding comparative advantage to the analysis. We indeed find important differences in the returns to measured and unmeasured skills across occupations. This finding suggests caution in interpreting the standard occupational wage premia reported in Table 2 (and elsewhere in the literature). In addition, as we describe below, such occupation-specific returns to skill make estimated occupational wage premia difficult to interpret, even after controlling for

differences in returns to skills across sectors. As a result, we now shift our focus to these differential returns to skill. In particular, we investigate whether high-skill workers are concentrated in high-return occupations, as our theory suggests.

Table 3 extends our analysis of occupational wage premia in Table 2 by reporting not only these premia but also occupation-specific returns to skills. All models reported in Table 3 also include the same set of additional controls (gender, race, year dummies, etc.) as in Table 2. Column 1 of Table 3 reports OLS estimates of the wage premia, while Column 2 reports the occupation-specific returns to observable skill. Most of the estimated returns to skill are quite plausible. For example, all occupations but the clerical occupations have a significantly larger return to skill than operatives and laborers. Managers and sales occupations have the largest returns to skill, although the returns for professionals may be a bit smaller than expected.

In spite of these significant differences in occupation-specific returns to observable skills (p-value of .00 on the joint test of equality of returns), the associated occupational wage premia are quite similar to those from Column 1 of Table 2 (which did not allow for occupation-specific returns to skill). For example, the standard deviation of the estimated occupational wage premia is .096 – slightly larger than the .086 in Column 1 of Table 2. But our analysis in Table 2 suggested an important role for unmeasured skills, so we next investigate occupation-specific returns to unobservable skills.

The remaining models reported in Columns 3 through 6 of Table 3 allow for occupation-specific returns to both measured and unmeasured skill. In all models, we include (but do not show in the Table) a set of interactions between occupation and experience to capture the term  $(1/2)b_j^2\sigma_\epsilon^2$  in equation (13).<sup>17</sup> We allow returns to measured and unmeasured skill to be different but proportional. In terms of the parameters of the model, this means that  $\beta_j=kb_j$  for all

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<sup>17</sup> Strictly speaking, this term should appear in only the learning model. We include it in all models with unmeasured skills for the sake of comparability across specifications.



occupations  $j$ , where  $k$  is a proportionality parameter. As we report in the tables, proportionality can never be rejected, but  $k$  is typically statistically different from 1.<sup>18</sup>

In the first model, reported in Columns 3 and 4, we analyze the model with comparative advantage but without learning, so the only endogenous variable is the lagged wage. In these models we use the full set of interactions between occupational affiliation at time  $t$  and  $t-1$  as instrumental variables. Relative to the OLS model of Columns 1 and 2, two features of the results in Columns 3 and 4 are striking. First, none of the occupational wage premia remains significant once occupation-specific returns to unmeasured skills are accounted for in the estimation. Recall that some of the premia were significant in the corresponding model for all workers without learning and *without comparative advantage* (first-difference estimates in Column 2 of Table 2). This suggests that introducing comparative advantage can account for most of the remaining occupational wage premia, just as learning did in the last column of Table 2. Second, most of the occupation-specific returns to skill remain significantly different from one (the normalized return to skill for operatives and laborers). Furthermore, the pattern of returns to skill across occupations now shows professionals, managers, and sales occupations with the highest returns.

The joint tests at the bottom of the Table confirm this pattern of results. The null hypothesis that the occupational wage premia are all zero cannot be rejected (p-value of .44) while the null hypothesis that returns to skill are all the same can be rejected at standard significance levels (p-value of .036).

As a final step, Columns 5-6 report estimates of our richest theoretical model – equation (13), which allows for both comparative advantage and learning, so that both the lagged wage and the current occupation are endogenous. As discussed earlier, we use the full set of interactions between occupational affiliation at time  $t-1$  and  $t-2$  as instruments (plus interactions

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<sup>18</sup> We test for proportionality by estimating an unrestricted model with separate returns to measured and unmeasured skills and performing a non-linear Wald test (null hypothesis is that the ratio  $b_j/\beta_j$  is constant across occupations). The p-value from this test is reported in the third row from the bottom of Table 3.

between the skill index and occupational affiliation at time t-1 and t-2). The results reported in Columns 5 and 6 are similar though slightly less precise than in the corresponding model without learning (Columns 3 and 4). For instance, returns to skill among professional, managers and sales occupation are 20 to 25 percent larger than for operative and laborers. The difference is not significant, however, except in the case of professionals. Though the joint test of equality of the returns across occupations can no longer be rejected at the five percent level, it is still rejected at the 10 percent level (p-value of .085). By contrast, as in the model without learning, none of the occupational wage premia are significant. The joint test that all occupational premia are the same still cannot be rejected (p-value of .28).

### **4.3 Interpretation**

The evidence reported in Tables 1 through 3 strongly suggests that comparative advantage and sorting based on observable and unobservable skills play important roles in explaining raw occupational wage premia. Table 1 shows strong and systematic sorting of highly-skilled into highly-paid occupations (correlation coefficient of .96). Perhaps not surprisingly, Table 2 shows that controlling for measured and unmeasured skills in conventional ways (OLS and first-differences) successively reduces the standard deviation of occupational wage premia from 0.181 to 0.086 and to between 0.020 and 0.034 (depending on the estimator used to control for time-invariant unmeasured skills). Remaining occupational wage premia are no longer significant once comparative advantage is explicitly accounted for by introducing occupation-specific returns to measured and unmeasured skills (Columns 3-4 of Table 3). The pattern of occupation-specific returns to skill is strongly consistent with measured skill sorting across occupations. For example, professionals who are the most skilled occupation (Table 1) also exhibit the largest return to skill (Column 4 of Table 3).

Interestingly, professionals also have the highest return to skill when learning is introduced in the model of Columns 5 and 6. More generally, the correlation between average measured skills and returns to skill is 0.76 and 0.65 in the models with and without learning,

respectively. Note that introducing learning does not change the results substantially once comparative advantage is properly accounted for in Table 3. One possible explanation for this finding is that though learning about ability may be quite important in the first few years in the labor market, it may not be as important further into workers' careers (Neal 1999). This may explain why learning plays a limited role in our NLSY sample where we have up to 15 years of labor market observations per worker.

As discussed earlier, introducing learning has more impact on the estimated occupation wage premia when comparative advantage is not properly accounted for (Table 2). We suspect that since models without comparative advantage are misspecified, instrumenting for occupational affiliation as we do in the model with learning may help correct for some the biases induced by the failure to control for comparative advantage.<sup>19</sup>

## **5. Wages and Returns to Skills Across Industries**

A substantial literature has established that there are large and persistent wage differentials among industries, even after controlling for a wide variety of worker and job characteristics (Katz 1986; Dickens and Katz 1987; Krueger and Summers 1987, 1988). One possibility is that these inter-industry wage differentials largely reflect differences in workers' productive abilities that are not captured by the variables available in standard individual-level data sets. An alternative explanation is that measured inter-industry wage differences are "true wage differentials" reflecting compensating differentials, non-competitive rent-sharing, or efficiency-wage considerations. Vigorous debate has centered on the extent to which industry wage differences reflect competitive factors such as unmeasured ability and compensating differentials (Murphy and Topel, 1987 and 1990) as opposed to labor-market rents, and on

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<sup>19</sup> For example, Wooldridge (1997) shows in a different context that using IV methods can sometimes yield a consistent estimate of the average treatment effect in a model with heterogeneous treatment effects (a random-coefficients model). The analogy with our case is that we also have a random-coefficients model since the "effect" of sector affiliation on wages depends systematically on skills in our model with comparative advantage.

whether such measured wage differentials potentially may justify certain types of industrial or trade policies (Katz and Summers 1989; and Topel 1989).

In this context, our model with comparative advantage and learning can be viewed as a renewed attempt at “explaining” inter-industry wage differentials by the systematic allocation of unmeasured skills across industries. For reasons mentioned earlier, we nonetheless expect comparative advantage to play less of a role in explaining sectoral wage differences across industries than across occupations.

### **5.1 Industry Wage Premia without Comparative Advantage**

We divide workers into ten conventional industry aggregates.<sup>20</sup> In Table 4 we report the raw industry log wage differentials (relative to the retail trade industry) and the average values of measured skills (education and experience) and other measured characteristics (race, sex, and marital status) by industry. Like others, we find large differences across industries in mean wages and in mean values for education and other characteristics. Like Dickens and Katz (1987), we find substantial correlation between these raw wage premia and these mean characteristics. For example, the correlation between the wage premium and the mean level of education is .49. To move beyond individual skill measures such as education, we use the same skill index as in the previous section. The mean skill index for each industry is reported in the final column of Table 4. The correlation between the wage premium and the mean skill index is .85. So, at first pass, sorting on observable skill appears to play a slightly smaller role in explaining inter-industry wage differences than it did for occupations (when the correlation coefficient was .96). A related point is that that the cross-industry variation in mean log wages (.151) is much larger than the cross-industry variation in the skill index (.045), whereas the cross-occupation variation in the skill index (0.099) represented more than half the cross-occupation variation in wages (0.181).

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<sup>20</sup> Using a more detailed classification does not substantially alter our basic findings and comes at the cost of less precise estimates of the industry effects.

Table 5 reports the estimates of the models without comparative advantage. All the models reported in Table 5 (and Table 6) also include controls for occupational affiliation (six dummy variables), gender, race, marital status, union status, and a full set of year dummies. The results without learning reported in Columns 1 through 4 are relatively similar to those obtained by Krueger and Summers (1988) and others. For instance, OLS estimates of the inter-industry wage differentials in Column 1 are large and significant, with a standard deviation of .105. Furthermore, more than half of the standard deviation of the OLS wage premia across industries remains when unmeasured skills are controlled for using fixed effects (.054 in Column 3) or first differences for new jobs (.059 in Column 4). As discussed for occupations, the smaller standard deviation of industry wage premia obtained from first differences for all workers (.040) is likely due to false transitions among workers staying with the same employer.<sup>21</sup>

Columns 5 and 6 report first-differenced IV models, to allow for the possibility of learning (but not comparative advantage). The instrumental variables used in this model are the full set of interactions between industry affiliation at time  $t-1$  and  $t-2$ . The standard deviation of the estimated inter-industry wage differentials falls somewhat (from about .055 in Columns 3 and 4 to about .042 in Columns 5 and 6). Unlike the case of occupations, however, the null hypothesis of no industry wage premia is still strongly rejected ( $p$ -value of .0001), even in these IV estimates.

Other interesting patterns emerge from the comparison of results for industry and occupations. For example, the standard deviation of raw wages differences is *smaller* for industries than for occupations (in Column 1 of Tables 1 and 4), but the standard deviation of the wage premia across sectors is substantially *larger* for industries than for occupation in the standard models without comparative advantage or learning (OLS, first-difference and fixed-

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<sup>21</sup> Our results are also consistent with Krueger and Summers' (1988) finding that first-differenced estimates of the industry wage effects can be significantly biased downwards because of misclassification errors in industry affiliation. Standard first-differenced estimates are misspecified when the whole sample is used but well-specified for job changers. Since misclassification errors in industry changes are much less likely to occur when a job change is observed than otherwise, we believe misclassification errors are the primary source of misspecification in the first-differenced estimates for the whole sample.

effect estimates in Columns 1 through 4 of Tables 2 and 5). This comparison strongly suggests that the sorting of measured and unmeasured skills across sectors plays more of a role in explaining the raw wage differentials across occupations than across industries.

A more subtle point is that controlling for skills has much more impact for some industries than others. Take the case of two relatively “high-wage” industries, construction and professional and business services (PBS). Despite high wages, construction has relatively low measured skills, while PBS has the highest measured skills of all industries (Table 4). The raw log wage differences indicate that PBS pays 0.114 more than construction. Just controlling for measured skills reverses this pattern. The OLS estimates indicate that construction now pays .026 more than PBS (Column 1 of Table 5). Controlling for unmeasured skills increases the gap in favor of construction to between 0.062 and 0.089, depending on the estimator being used (Columns 3 and 4).

This differential effect of controlling for skills (even without learning or comparative advantage) suggests that no single theory can likely explain the wage premia for all industries. In sectors like PBS, the systematic sorting of skills that follows from our model of comparative advantage likely accounts for a large share of the premium; in sectors like construction, compensating wage differences and unionization (rent-sharing) are more plausible explanations. We next explore this hypothesis formally by introducing comparative advantage in the estimated models.

## **5.2 Industry Wage Premia and Industry Skill Premia**

Table 6 extends our analysis by reporting not only the industry wage premia but also industry-specific returns to skills. Columns 1 and 2 report OLS estimates of the wage premia and returns to measurable skills, respectively. As in the case of occupations, there is substantial heterogeneity in the returns to skill across industry. In spite of this heterogeneity in industry-specific returns to skills, controlling for this heterogeneity reduces the standard deviation of the estimated industry wage premia only slightly, from .105 (in Column 1 of Table 5) to .099.

Roughly speaking, industries with high wage premia tend to exhibit high return to skill, though construction is an important exception.

A similar pattern holds when industry-specific returns to both measured and unmeasured skills are introduced in Columns 3 and 4.<sup>22</sup> Unlike the case of occupations, however, several of the industry wage premia remain positive and significant. Interestingly, the joint test of equality of industry wage premia is strongly rejected while equality in industry-specific returns to skill cannot be rejected ( $p=.13$ ). This is the opposite of the case with occupations, where equality of the wage premia could not be rejected while equality in the occupation-specific returns to skill was rejected. This finding is consistent with our interpretation that comparative advantage plays a more important role in explaining sectoral wage premia for occupations than for industries. But this is not to say that comparative advantage plays no role in wage and affiliation decisions across industries. For example, comparing Column 3 to Column 1, the two “high wage” industries that experience the largest decrease in estimate wage premium are finance, insurance and real estate (FIRE) and professional and business services (PBS). These two industries also happen to have the largest estimated returns to skill in Column 4 and relatively high skill levels (Table 4).

The models reported in the remaining columns of Table 6 are qualitatively similar to the model for all workers without comparative advantage of Columns 3 and 4. In all cases, the joint test of equality of industry wage premia is strongly rejected while equality in industry-specific returns to skill cannot be rejected. As in the case of occupations, accounting for learning has relatively little effect on the results.

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<sup>22</sup> As in the case of occupations, we constrain returns to measured and unmeasured skills to be proportional in all the models with industry-specific returns to skill. The Table shows that proportionality is never rejected ( $p$ -values ranging from .45 to .96) and that the proportionality parameter is always positive and well determined. The instrumental variables are also selected in the same fashion as in the models for occupation. In the models without learning, we use interactions between industry affiliation at time  $t$  and  $t-1$  as instruments. In the models with learning, the instruments used are the interactions between industry affiliation at time  $t-1$  and  $t-2$  and the interactions of industry affiliation at time  $t-2$  with the skill index and experience.

### **5.3 Interpretation**

The existing literature on inter-industry wage differentials suggests that neither a simple unmeasured-ability explanation (in which ability is equally valued in all industries and market perceptions of worker quality are time invariant) nor a pure rent-based explanation appears fully consistent with evidence from longitudinal analyses of the wage changes of industry switchers (Krueger and Summers, 1988) or the pre- and post-displacement wages of workers displaced by plant closings (Gibbons and Katz, 1992). These findings have motivated recent work that has focused on econometric approaches for estimating industry wage differentials while accounting for heterogeneous matches between workers (Neal 1995; Bils and McLaughlin 2001; Kim 1998). Our approach is also in this vein.<sup>23</sup>

Our results reinforce the view that a single explanation does not fit all industries. For instance, the industry wage premia in mining, manufacturing and construction remain large and statistically significant even in our richest model with comparative advantage and learning. By contrast, introducing these two factors essentially eliminates the wage premia in industries such as FIRE and PBS.

## **6. Conclusion**

We develop a model of wages and sector choices that generalizes the static model of sorting with perfect information to the case in which some skills are unobserved by both the market and the worker. Wage changes and sector mobility arise endogenously as the market and the incumbent firm learn about a worker's skills. We show how this model can be estimated using non-linear instrumental-variables techniques.

We illustrate our theoretical and econometric approach by studying both occupations and industries. Broadly speaking, the results suggest that the measured occupational wage differentials in a cross-section regression are largely due to unmeasured and unobserved worker

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<sup>23</sup> A complementary approach focuses on correlations between ability and investments in sector-specific skills (Neal 1998).



skills. We find strong evidence that the sorting of skills into “high-wage” occupations is explained by high returns to skills in these occupations. Although comparative advantage appears to play a fundamental role in occupational wage differences, the role of learning is more limited. One possible explanation for this finding is that though learning may be quite important in the first few years in the labor market, it may not be as important later on.

The results for industries are mixed, which is consistent with the existing literature. Our richest model with comparative advantage and learning explains relatively well the cross-sectional premia in industries like finance, insurance and real estate and professional and business services. More traditional explanations like compensating differences and rent-sharing seem to be better suited for industries such as mining, manufacturing, and construction.

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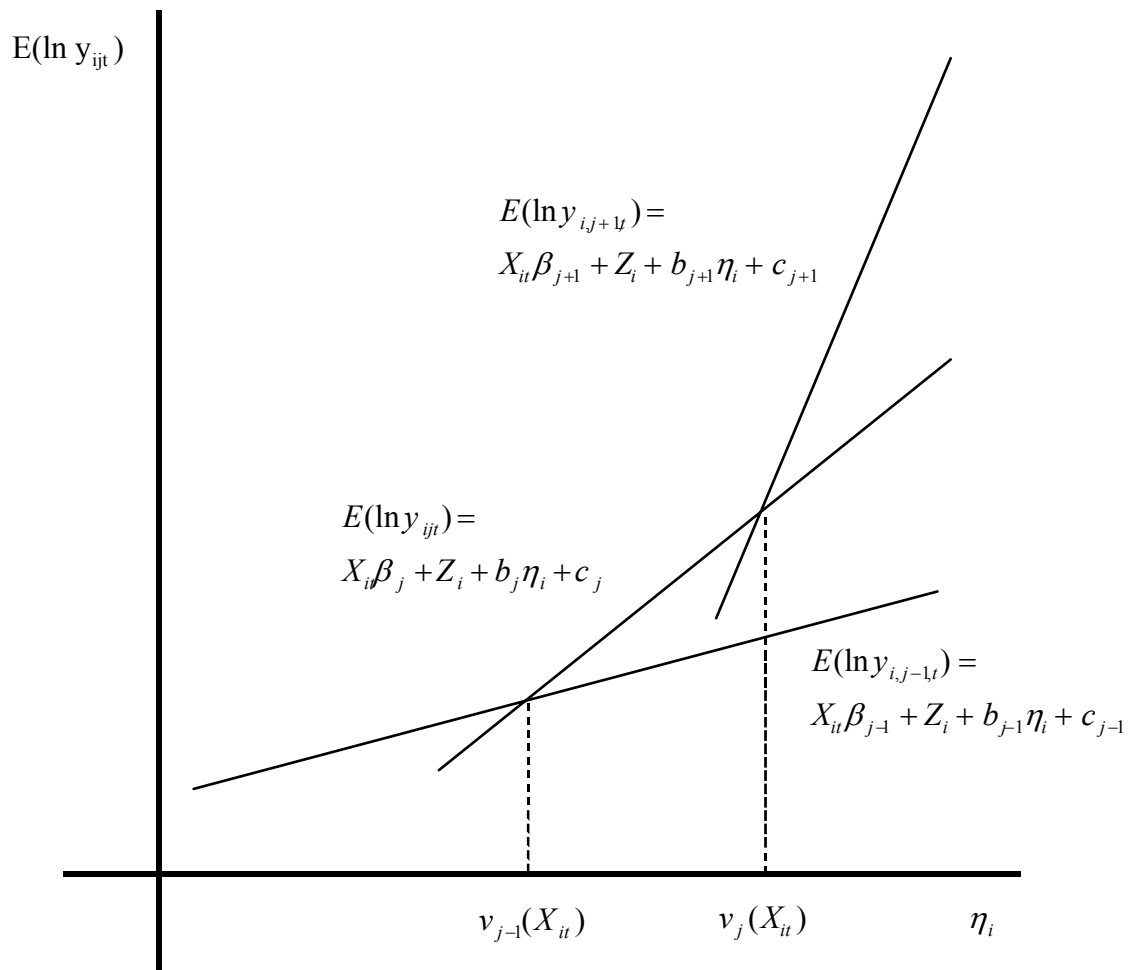


FIGURE 1

TABLE 1  
Average Characteristics by Occupation<sup>a</sup>

	Raw Wage Differences <sup>b</sup>	Years of:		Fraction:			Skill Index	Sample Propor- tion
		Educ- ation	Exper- ience	Female	Non- white	Married		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1. Professionals	0.522	15.66	7.38	0.534	0.136	0.583	0.1479	0.212
2. Managers	0.425	14.38	8.31	0.432	0.137	0.562	0.1049	0.138
3. Sales occupations	0.388	14.38	7.49	0.400	0.118	0.550	0.0828	0.052
4. Clerical occupations	0.116	13.17	7.27	0.786	0.233	0.503	-0.0907	0.203
5. Craft workers	0.290	12.17	7.96	0.077	0.157	0.499	-0.0358	0.118
6. Operatives and laborers	0.090	11.92	7.44	0.244	0.265	0.489	-0.0979	0.171
7. Service Occupations	0.000	12.73	7.10	0.532	0.310	0.424	-0.0989	0.108
Standard deviation (across occupations)	0.181	1.26	0.39	0.210	0.069	0.050	0.099	
Correlation with raw wage diff.	1.00	0.81	0.53	-0.12	-0.94	0.93	0.96	

a. Based on a sample of 35,438 observations for 5,904 workers. See text for more details.

b. Mean log wage in the industry relative to service occupations (mean log wage in service occupations is 1.431).

TABLE 2  
Estimates of Occupation Wage Differentials Without Comparative Advantage<sup>a</sup>

Estimation Method:	No Learning				Learning	
	OLS	FD	FE	FD	FDIV <sup>b</sup>	FDIV <sup>b</sup>
Sample:	All	All	All	New Jobs	All	New Jobs
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Occupation Effects:</b>						
1. Professionals	0.208* (0.008)	0.042* (0.007)	0.043* (0.008)	0.095* (0.016)	0.019 (0.014)	0.002 (0.030)
2. Managers	0.213* (0.008)	0.022* (0.007)	0.049* (0.007)	0.052* (0.015)	-0.001 (0.013)	0.048 (0.025)
3. Sales occupations	0.191* (0.010)	0.006 (0.009)	0.018 (0.010)	0.044* (0.018)	-0.012 (0.017)	0.054 (0.031)
4. Clerical occupations	0.044* (0.007)	-0.003 (0.006)	-0.018* (0.007)	0.010 (0.014)	0.007 (0.013)	0.050* (0.025)
5. Craft workers	0.144* (0.008)	0.038* (0.006)	0.039* (0.007)	0.085* (0.013)	0.010 (0.010)	0.041* (0.021)
6. Service occupations	0.021* (0.008)	-0.019* (0.007)	-0.045* (0.008)	-0.000 (0.014)	0.010 (0.014)	0.023 (0.023)
7. Operatives and Laborers	0.000	0.000	0.000	0.000	0.000	0.000
Skill Index	0.999* (0.011)	0.811* (0.050)	1.185* (0.046)	0.764* (0.107)	0.598* (0.037)	0.804* (0.108)
R-square	0.418	0.035	0.789	0.070	0.031	0.061
Observations	35438	35438	35438	9198	35438	9198
Overidentification Test (p-value)	---	---	---	---	.0001	.0001
Test of equality of Occup. Effects (p-value)	.0000	.0001	.0001	.0001	.4563	.1165
Standard deviation of occup. effects	0.086	0.021	0.032	0.036	0.009	0.021
Adjusted std. deviation <sup>c</sup>	0.086	0.020	0.032	0.034	---	---

a: Standard errors are in parentheses. "\*" indicates that the estimated coefficient is significantly different from zero at the 95 percent confidence level. All specifications also include controls for gender, race, marital status, year effects, industry (9 dummies), and a dummy for collective bargaining coverage.

b: Instrumental variables (for changes in occupation) are the full set of interactions between the occupation dummies at time t-1 and t-2.

c: The adjusted standard deviation is the actual standard deviation of the estimated occupation effects corrected for the sampling variation in these estimates.

TABLE 3  
Estimates of Occupation Wage Differentials With Comparative Advantage<sup>a</sup>

Estimation Method: Sample:	No Learning				Learning	
	OLS All		NLIV All		NLIV All	
Occupation effects:	Main (1)	Interacted <sup>b</sup> (2)	Main (3)	Interacted <sup>b</sup> (4)	Main (5)	Interacted <sup>b</sup> (6)
1. Professionals	0.263* (0.008)	1.120* (0.034)	0.069 (0.034)	1.271* (0.086)	0.029 (0.049)	1.259* (0.116)
2. Managers	0.233* (0.008)	1.332* (0.036)	0.046 (0.034)	1.201* (0.077)	0.032 (0.049)	1.196 (0.110)
3. Sales occupations	0.199* (0.011)	1.476* (0.047)	0.054 (0.038)	1.218* (0.083)	0.087 (0.061)	1.224 (0.128)
4. Clerical occupations	0.066* (0.008)	1.027 (0.036)	0.033 (0.031)	1.162* (0.073)	0.004 (0.044)	1.099 (0.097)
5. Craft workers	0.168* (0.008)	1.138* (0.044)	0.040 (0.026)	1.023 (0.065)	-0.024 (0.036)	0.971 (0.086)
6. Service occupations	-0.037* (0.009)	1.107* (0.042)	0.047 (0.035)	1.127 (0.076)	0.088 (0.060)	1.204 (0.133)
7. Operatives and Laborers	0.000	1.000	0.000	1.000	0.000	1.000
Proportionality factor <sup>b</sup> between measured and unmeasured skills	---		0.8652* (0.0710)		0.7847* (0.0899)	
Endogenous Variables <sup>c</sup>	---		w1		w1,D0	
Instrumental Variables <sup>c</sup>	---		D0*D1		D1*D2, SK*D2, E*D2	
Observations	35438		35438		35438	
Overidentification test (p-value)	---		.0000		.0000	
Test of equality of Occupation Effects (p-value)	.0000		.4372		.2807	
Test of equality of Interaction slopes (p-value)	.0000		.0357		.0852	
Test of proportionality <sup>b</sup> (p-value)	---		.1391		.5403	
Standard deviation of occupation effects	0.096		0.020		0.040	
Adjusted std. Deviation <sup>d</sup>	0.095		---		---	



Notes to Table 3

a: Standard errors are in parentheses. In the case of main effects, "\*" indicates that the estimated coefficient is significantly different from zero at the 95 percent confidence level. In the case of interactions, "\*" indicates that the estimated coefficient is significantly different from one at the 95 percent confidence level. All specifications also include controls for gender, race, marital status, year effects, industry (9 dummies), collective bargaining coverage, and (except in columns 1-2) occupation-specific experience effects.

b: In columns 1-2, the interaction terms indicate the effect of measured skills in the occupation relative to operatives and laborers (effect normalized to 1 in service occupations). In columns 3-6, the interaction terms indicate the effect of both measured and unmeasured skills in the occupation, again relative to operatives and laborers, but these occupation-specific slopes for measured and unmeasured skills are constrained to have the same proportionality factor across all occupations. The estimated proportionality factor is reported at the bottom of the first panel; the test of the proportionality restriction is reported in the second panel.

c: In the endogenous variables,  $w_1$  stands for the lagged wage and  $D_0$  stands for contemporaneous values of the occupation dummies  $\{D_{ijt}, j=1, \dots, 6\}$ . In the instrumental variables,  $D_1$  and  $D_2$  stand for the first and second lag of occupations dummies,  $SK$  stands for the skill index, and  $E$  stands for experience.

d: The adjusted standard deviation is the actual standard deviation of the estimated occupation effects corrected for the sampling variation in these estimates.

TABLE 4  
Average Characteristics by Industry<sup>a</sup>

	Raw Wage Differences <sup>b</sup>	Years of:		Fraction:			Skill Index	Sample Proportion
		Educ- ation	Exper- ience	Female	Non- white	Married		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1. Mining and Durable Manuf.	0.359	13.15	7.76	0.303	0.153	0.577	0.0106	0.142
2. Non-Durable Manufacturing	0.203	13.02	7.40	0.405	0.170	0.529	-0.0304	0.094
3. Construction	0.283	11.99	8.06	0.085	0.166	0.486	-0.0457	0.062
4. Transpo., Comm., Utilities	0.387	13.44	8.09	0.333	0.253	0.529	0.0261	0.066
5. Finance. Insur., Real Estate	0.348	14.10	7.43	0.659	0.172	0.505	0.0140	0.089
6. Profess. and Bus. Services	0.397	14.44	7.52	0.471	0.173	0.525	0.0628	0.084
7. Personal Service	-0.032	12.63	7.26	0.400	0.236	0.436	-0.0685	0.041
8. Wholesale Trade	0.222	14.39	7.34	0.659	0.219	0.545	0.0239	0.216
9. Retail Trade	0.000	12.92	7.16	0.455	0.190	0.426	-0.0654	0.145
10. Public Admin.	0.392	14.11	8.06	0.484	0.307	0.593	0.0494	0.062
Standard deviation (across industries)	0.151	0.78	0.34	0.160	0.047	0.051	0.045	
Correlation with raw wage diff.	1.00	0.49	0.70	-0.02	0.04	0.80	0.85	

a. Based on a sample of 35,438 observations on 5,904 workers. See text for more details.

b. Mean log wage in the industry relative to retail trade (mean log wage in the retail trade industry is 1.445).

TABLE 5  
Estimates of Industry Wage Differentials Without Comparative Advantage<sup>a</sup>

Estimation Method:	No Learning				Learning	
	OLS	FD	FE	FD	FDIV <sup>b</sup>	FDIV <sup>b</sup>
Sample:	All (1)	All (2)	All (3)	New Jobs (4)	All (5)	New Jobs (6)
Industry Effects:						
1. Mining and Durable Manufacturing	0.280* (0.008)	0.111* (0.007)	0.148* (0.008)	0.165* (0.014)	0.065* (0.015)	0.146* (0.023)
2. Non-Durable Manufacturing	0.182* (0.008)	0.075* (0.008)	0.104* (0.009)	0.108* (0.015)	0.036* (0.016)	0.071* (0.024)
3. Construction	0.268* (0.010)	0.134* (0.009)	0.151* (0.010)	0.173* (0.017)	0.103* (0.020)	0.144* (0.030)
4. Transpo., Comm., Utilities	0.275* (0.009)	0.093* (0.009)	0.131* (0.010)	0.144* (0.017)	0.055* (0.021)	0.112* (0.029)
5. Finance, Insurance, Real Estate	0.248* (0.008)	0.061* (0.010)	0.110* (0.010)	0.090* (0.016)	-0.015 (0.024)	0.062* (0.027)
6. Prof. and Business Services	0.242* (0.009)	0.064* (0.008)	0.089* (0.009)	0.084* (0.015)	-0.003 (0.015)	0.049* (0.022)
7. Personal Services	0.006 (0.011)	-0.012 (0.009)	-0.001 (0.010)	-0.001* (0.017)	-0.017 (0.017)	0.014 (0.026)
8. Wholesale Trade	0.092* (0.007)	0.054* (0.007)	0.067* (0.007)	0.079* (0.012)	0.035* (0.014)	0.066* (0.021)
9. Retail Trade	0.000	0.000	0.000	0.000	0.000	0.000
10. Public Admin.	0.244* (0.010)	0.100* (0.010)	0.149* (0.011)	0.150* (0.022)	0.029 (0.024)	0.081* (0.039)
Skill Index	1.000* (0.012)	0.811* (0.050)	1.185* (0.046)	0.749* (0.107)	0.828* (0.050)	0.767* (0.107)
R-square	0.418	0.036	---	0.072	---	---
Observations	35438	35438	35438	9198	35438	9198
Overidentification Test (p-value)	---	---	---	---	.0001	.0003
Test of equality of Industry Effects (p-value)	.0000	.0000	.0000	.0000	.0001	.0001
Standard deviation of industry effects	0.105	0.040	0.054	0.059	0.037	0.046
Adjusted st. deviation <sup>c</sup>	0.104	0.039	0.054	0.057	0.032	0.038

Notes to Table 5

a: Standard errors are in parentheses. "\*" indicates that the estimated coefficient is significantly different from zero at the 95 percent confidence level. All specifications also include controls for gender, race, marital status, year effects, occupation (6 dummies), and collective bargaining coverage.

b: Instrumental variables (for changes in industry affiliation) are the full set of interactions between the industry affiliation dummies at time t-1 and t-2.

c: The adjusted standard deviation is the actual standard deviation of the estimated industry effects corrected for the sampling variation in these estimates.

TABLE 6  
Estimates of Inter-Industry Wage Differentials With Comparative Advantage<sup>a</sup>

Estimation Method: Sample:	No Learning						Learning			
	OLS All		NLIV All		NLIV New Jobs		NLIV All		NLIV New Jobs	
	Main	Interacted <sup>b</sup>	Main	Interacted <sup>b</sup>	Main	Interacted <sup>b</sup>	Main	Interacted <sup>b</sup>	Main	Interacted <sup>b</sup>
Industry effects:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1. Mining and Durable Manufacturing	0.282 <sup>*</sup> (0.008)	1.182 <sup>*</sup> (0.042)	0.163 <sup>*</sup> (0.033)	1.124 <sup>*</sup> (0.062)	0.254 <sup>*</sup> (0.051)	1.138 (0.100)	0.144 <sup>*</sup> (0.047)	1.158 (0.100)	0.182 <sup>*</sup> (0.060)	0.978 (0.119)
2. Non-Durable Manufacturing	0.187 <sup>*</sup> (0.009)	1.208 <sup>*</sup> (0.046)	0.093 <sup>*</sup> (0.033)	1.089 (0.060)	0.123 <sup>*</sup> (0.051)	1.114 (0.096)	0.112 <sup>*</sup> (0.049)	1.144 (0.100)	0.167 <sup>*</sup> (0.089)	1.282 (0.179)
3. Construction	0.248 <sup>*</sup> (0.011)	0.823 <sup>*</sup> (0.054)	0.124 <sup>*</sup> (0.040)	1.005 (0.087)	0.173 <sup>*</sup> (0.056)	0.968 (0.133)	0.146 <sup>*</sup> (0.065)	1.115 (0.152)	0.169 <sup>*</sup> (0.069)	0.875 (0.147)
4. Transpo., Comm., Utilities	0.276 <sup>*</sup> (0.009)	0.969 (0.048)	0.114 <sup>*</sup> (0.041)	1.094 (0.079)	0.201 <sup>*</sup> (0.069)	1.182 (0.135)	0.053 (0.063)	1.275 (0.156)	0.190 <sup>*</sup> (0.097)	1.223 (0.201)
5. Finance, Insurance, Real Estate	0.237 <sup>*</sup> (0.009)	1.296 <sup>*</sup> (0.049)	0.037 (0.040)	1.212 <sup>*</sup> (0.081)	0.125 <sup>*</sup> (0.061)	1.362 <sup>*</sup> (0.131)	-0.040 (0.052)	1.156 (0.137)	0.029 (0.072)	1.241 (0.181)
6. Prof. and Business Services	0.224 <sup>*</sup> (0.009)	1.341 <sup>*</sup> (0.049)	0.052 (0.036)	1.176 <sup>*</sup> (0.070)	0.092 (0.052)	1.223 <sup>*</sup> (0.108)	0.028 (0.046)	1.132 (0.105)	0.084 (0.062)	1.098 (0.133)
7. Personal Services	-0.023 (0.012)	0.625 <sup>*</sup> (0.056)	-0.050 (0.054)	0.970 (0.123)	-0.047 (0.078)	0.939 (0.192)	0.066 (0.105)	1.232 (0.232)	0.112 (0.145)	1.167 (0.311)
8. Wholesale Trade	0.102 <sup>*</sup> (0.007)	0.877 <sup>*</sup> (0.032)	0.069 (0.033)	1.148 <sup>*</sup> (0.069)	0.109 <sup>*</sup> (0.051)	1.165 (0.108)	0.095 (0.051)	1.248 <sup>*</sup> (0.118)	0.180 <sup>*</sup> (0.080)	1.219 (0.166)

9. Retail Trade	0.000 ---	1.000 ---	0.000 ---	1.000 ---	0.000 ---	1.000 ---	0.000 ---	1.000 ---	0.000 ---	1.000 ---
10. Public Administration	0.251* (0.010)	0.936 (0.047)	0.119* (0.036)	1.155* (0.078)	0.183* (0.064)	1.233 (0.162)	0.099 (0.061)	1.300 (0.176)	0.039 (0.077)	1.119 (0.195)
Proportionality <sup>b</sup> Parameter	---		0.987* (0.063)		0.724* (0.103)		0.860* (0.077)		0.671* (0.111)	
Endogenous Variables	---		w1		w1		w1,D0		w1,D0	
Instrumental Variables <sup>c</sup>	---		D0*D1		D0*D1		D1*D2,SK*D2,E*D2		D1*D2,SK*D2,E*D2	
Observations	35438		35428		9198		35438		9198	
Overidentification test (p-value)	---		.0001		.0002		.0000		.0169	
Test of equality of Industry Effects (p-value)	.0000		.0000		.0000		.0018		.0259	
Test of equality of Industry Slopes (p-value)	.0000		.1331		.2473		.6980		.1996	
Standard deviation of industry effects	0.099		0.061		0.086		0.058		0.038	
Adjusted st. deviation <sup>d</sup>	0.098		0.048		0.065		---		---	
Test of proportionality <sup>b</sup> (p-value)	---		.9645		.4487		.4649		.5597	

a: In the case of main effects, "\*" indicates that the estimated coefficient is significantly different from zero at the 95 percent confidence level. In the case of interactions, "\*\*" indicates that the estimated coefficient is significantly different from one at the 95 percent confidence level. All specifications also include controls for gender, race, marital status, year effects, occupation (6 dummies), collective bargaining coverage, and industry-specific experience effects (except in the model of columns 1-2).

b: In columns 1-2, the interaction terms indicate the effect of measured skills in the industry relative to retail trade (effect normalized to 1 in this industry). In columns 3-10, the interaction terms indicate the effect of both measured and unmeasured skills in the industry, again relative to retail trade, but these industry-specific slopes for measured and unmeasured skills are constrained to be proportional across all industries. The estimated constant of proportionality between measured and unmeasured skills is reported at the bottom of the first panel; the test of the proportionality restriction is reported in the second panel.

c: In the endogenous variables,  $w_1$  stands for the lagged wage and  $D_0$  stands for contemporaneous values of the occupation dummies  $\{D_{ijt}, j=1, \dots, 6\}$ . In the instrumental variables,  $D_1$  and  $D_2$  stand for the first and second lag of occupations dummies,  $SK$  stands for the skill index, and  $E$  stands for experience.

d. The adjusted standard deviation is the actual standard deviation of the estimated industry effects corrected for the sampling variation in these estimates.

**Appendix Table A**  
**Log Wage Equations estimated to construct the skill index**

	(1)	(2)
<u>Variables used to construct the skill index:</u>		
AFQT	---	0.272 (0.010)
Years of educ	0.038 (0.003)	0.028 (0.003)
Educ. Categ.		
HS dropout	-0.126 (0.028)	-0.046 (0.028)
HS grad	-0.140 (0.018)	-0.095 (0.018)
Some coll.	-0.138 (0.017)	-0.124 (0.017)
Experience	0.050 (0.004)	0.047 (0.003)
Exp. squared (/100)	-0.054 (0.018)	-0.058 (0.018)
<u>Other control variables:</u>		
Female	-0.100 (0.012)	-0.090 (0.012)
Married	0.083 (0.006)	0.079 (0.005)
Non-white	-0.071 (0.017)	-0.008 (0.017)
Union	0.168 (0.005)	0.176 (0.005)
Year dummies	yes	yes
Industry dummies	yes	yes
Occupation dummies	yes	yes
<u>Interaction terms:</u>		
Fem*HS dropout	-0.001 (0.018)	-0.012 (0.018)
Fem*HS grad.	0.002 (0.012)	0.005 (0.012)
Fem*Some coll.	0.021 (0.013)	0.037 (0.013)
Fem*Experience	-0.000 (0.001)	-0.001 (0.001)
Fem*Married	-0.081 (0.008)	-0.076 (0.008)
Fem*Non-white	0.021 (0.010)	0.021 (0.010)
Exp*HS dropout	-0.015 (0.002)	-0.014 (0.002)
Exp*Hs grad	-0.007 (0.002)	-0.007 (0.001)
Exp*Some coll	-0.001 (0.002)	-0.001 (0.002)
NW*HS dropout	0.087 (0.019)	0.050 (0.019)
NW*HS grad	0.001 (0.014)	-0.006 (0.014)
NW*Some coll	-0.004 (0.015)	-0.004 (0.015)
NW*experience	-0.002 (0.002)	-0.003 (0.002)
Constant	1.046 (0.049)	1.060 (0.049)
R-squared	0.405	0.418



---

Note: All models are estimated on a sample of 35,438 observations for 5,904 workers. The skill index is constructed by computing a predicted wage from equation (2) using only the variables AFQT, education (both years of education and education categories) and experience, and holding the other variables (female, married, non-white, union, year, industry and occupation) at their average sample values.

## Appendix B: Choice of instruments

This appendix explains the choice of the interaction between the sector affiliation at time  $t-1$  and  $t-2$  as the main set of instruments in model with comparative advantage and learning. The intuition for this choice is most easily understood using of Figure 1. First note that in the most general model with comparative advantage and learning, Figure 1 remains as is except that the unmeasured ability  $\eta_i$  is replaced by its expected value conditional on the available information,  $m_{i,t-1}$ . Consider the case of sectors  $j$  and  $j-1$  in the Figure 1. There are four possible “sector histories” in these two sectors. Workers can either “stay” in sector  $j-1$  or  $j$  in both time periods, or switch (from  $j$  to  $j-1$  or from  $j-1$  to  $j$ ). Workers with expected ability close to the critical value  $v_{j-1}(X)$  are more likely to switch sector than either workers with expected ability clearly lower than  $v_{j-1}(X)$  (sector  $j-1$  stayers) or clearly higher than  $v_{j-1}(X)$  (sector  $j-1$  stayers). In terms of expected ability and wages, “switchers” currently in sector  $j-1$  should earn *more* than other workers in sector  $j-1$ , while “switchers” currently in sector  $j$  should earn *less* than other workers in sector  $j$ .

Appendix Tables B1 (occupations) and B2 (industries) show that the data is broadly consistent with this prediction. These tables show the average log wages as a function of current and previous sector affiliations. For example, consider operatives and laborers as sector  $j-1$  and craft workers as sector  $j$ . As expected, “stayers” in the craft occupation earn more (0.08) than craft workers who switched from operatives and laborers to craft (-0.09). By contrast, workers who “switch down” from craft to operatives and laborers earn more (-0.11) than “stayers” in the operative and laborers occupation (-0.17).

This example shows how the interaction between sector affiliation at time  $t$  and  $t-1$  helps predict wages even after controlling for the current sector affiliation. Similar reasoning can be used to show how the interaction between sector affiliation at time  $t-1$  and  $t-2$  can be used to predict sector affiliation at time  $t$ . Recall that like the lagged wage, the current sector affiliation is endogenous in the model with comparative advantage and learning.

Consider, for example, the choice of sector at time  $t$  of individuals observed to be in sector  $j$  at time  $t-1$ . Remember from Figure 1 that the expected ability of sector  $j$  workers (at time  $t-1$ ) who were in sector  $j-1$  at time  $t-2$  should be lower (close to  $v_{j-1}(X)$ ) than the expected ability of sector  $j$  workers who were also in sector  $j$  at time  $t-2$ . Consider a positive productivity signal that increases expected ability  $m_i$ . Since sector  $j$  stayers (at time  $t-1$  and  $t-2$ ) are closer to the upper critical value  $v_j(X)$  than the switchers who just came from sector  $j-1$ , the stayers are more likely to move to sector  $j+1$  at time  $t$  than the switchers. This shows how the interaction between sector affiliation at time  $t-1$  and  $t-2$  can be used to predict sector affiliation at time  $t$ .

In summary, sector histories (interaction between sector dummies at time  $t-1$  and  $t-2$ ) should help predict both the wage at  $t-1$  and the sector affiliation at time  $t$  in the model with comparative advantage and learning. Appendix Tables B1 and B2 suggest they do so in the case of the wage. A more formal test consists of testing the predictive power of the sector histories on the lagged wage and current affiliation after controlling for all other exogenous variables of the model. In a linear model, this test is just the standard F-test of the predictive power of the excluded instruments in the first-stage equation. Testing for the predictive power of instruments is more complicated in a non-linear context. In the linear model  $y = x\beta + e$ , the “first-stage” consists of projecting all the right hand side variables ( $x$ ) on the set of instruments, where the  $x$  variables also happen to be (minus) the derivative (or gradient) of  $e$  with respect the parameters  $\beta$  (since  $e = y - x\beta$ ). In the non-linear model, the equivalent of the first-stage is thus a regression of the gradient of  $e$  with respect to the structural parameters. Those derivatives will typically be non-linear combinations of various  $x$  variables. One way to measure the predictive power of the instruments is thus to compute F-tests on the excluded instruments in these “gradient” regressions.

The more intuitive approach we follow is to linearize the gradients as a function of the various explanatory variables. Once this linearization is performed, we can simply compute the usual F-test on the “first-stage” equations for the lagged wage and the current sector affiliation. To be consistent with the estimated models of Table 3 and Table 6, our excluded instruments are the interactions between sector affiliation at time  $t-1$  and  $t-2$  as well as interactions of sector affiliation at time  $t-2$  with the skill index and experience. The F-statistics are reported below. In all cases, they largely exceed the critical values (at the 95 percent confident level) of 1.26 for industries and 1.39 for occupations:

### F-test of the predictive power of instruments

	Lagged wage	Current sector (range of values)	# of excluded instruments
Occupations:	16.27	16.27-38.69	60
Industries:	11.46	17.45-26.68	117

As mentioned in the text and tables, in the case with comparative advantage but no learning we use the sector affiliations at time  $t$  and  $t-1$  (instead of  $t-1$  and  $t-2$ ) as instruments. In this case, the F-statistics are 10.70 and 8.27 for occupations and industries, respectively, which largely exceeds the critical values.

### Overidentification test

Having established that the instruments predict well the lagged wage and sector affiliation, it is also important to know whether the instruments are valid in the sense that they are uncorrelated with the error term in equation (13). As discussed in Section 2.4, the overidentification test is the natural way of testing this hypothesis. In a complicated non-linear model like ours, however, this test is an omnibus test that can fail either because the model is misspecified or because the model is well-specified but the instruments are invalid. Intuitively speaking, the model is overidentified when there are more instruments than parameters. So the model may either fail the overidentification test because not enough parameters are included (model misspecified) or because some of the instruments are invalid. In the absence of further information, we cannot conclude that the instruments are invalid just because our models do not generally pass the overidentification test.

For example, we use throughout the paper a convenient single-index model in which wages in all sectors are linear functions of the same underlying index of measured and unmeasured skills. While this assumption is used for reasons of convenience and clarity, it is well-known to be restrictive in models with more than two sectors (see Heckman and Scheinkman, 1987). By contrast, Lemieux (1998) argues that the single index model imposes little restriction when only two sectors are used.

It is easy to see in the three sector case illustrated in Figure 1 why the single index model is restrictive. Consider sector  $j-1$  and  $j$ . The relative slope and intercepts in the two sectors can be estimated using the subsample of workers that are in these two sectors at time  $t$  and  $t-1$  (and  $t-2$  in the model with learning). Similarly, observations from sectors  $j$  and  $j+1$  can be used to estimate the relative slope and intercepts in these two sectors. Say we normalize the slope in sector  $j-1$  to 1 and estimate the slope of sector  $j$  relative to  $j-1$  to be 1.2. If the slope in sector  $j+1$  relative to  $j$  is also 1.2, it follows that the slope of sector  $j+1$  relative to  $j-1$  is 1.44.

According to the model, it is unlikely that a worker would switch directly from sector  $j-1$  to  $j+1$  or vice versa. But since those transitions are actually observed in the data, it is possible to use the subsample of workers in the two sectors in two consecutive periods to estimate the relative slope and intercepts in sectors  $j-1$  and  $j+1$ . To the extent that the resulting slope (for example) is statistically different from 1.44, the overidentification test will fail since different identification strategies yield different estimates of the slopes. In this case we would conclude, however, that the failure of the overidentification is a consequence of the fact that the single index model is an oversimplification.

In light of this example, we re-estimate the model of columns 3 and 4 of Table 3 using only workers who either stay in the same occupation at  $t$  and  $t-1$  (66 percent of the sample) or move across adjoining occupations according to the ranking used in the tables (14 percent of the sample).<sup>24</sup> Consistent with our expectations, the p-value on the overidentification test moves from 0.0000 (Table 3) to 0.2776. In other words, the overidentification restrictions can no longer be rejected at standard significance levels.

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<sup>24</sup> Note that except for clerical and service occupations, occupational mobility tends to be concentrated between adjoining sectors. There is a total of 11969 occupation transitions (34 percent of all observations) in the data. Of those 4789 are between “adjoining” occupation (i.e. between occupation 1 (professionals) and 2 (managers), 2 and 3 (sales), etc.). Of the 7180 transitions among “non-adjoining” occupations, 5316 involve either clerical or service occupations. This pattern of transitions among occupations suggests that these two particular occupations are difficult to rank in terms of an occupational hierarchy based on a single index of skills.

Another possible source of misspecification is that, as mentioned in Section 2, we assume throughout the paper that this period's wage is specified before production occurs. This, in essence, assumes away the presence of piece rate contracts or of any form of contracts where the level of pay is determined *ex post*, such as contracts where part of the compensation is based on a subjective evaluation of performance. One consequence of having some workers in our sample being paid piece rates or commissions would thus be to affect the validity of our overidentifying restrictions. To explore whether this might be one reason why the restrictions are all rejected, we re-estimated our occupation models by first restricting the sample to workers who were in occupations in which the percentage of piece rate workers is below 10%. To impose this sample selection restriction, we looked at the percentage of workers in the sample who are paid commissions or piece rates between 1988 and 1990, the three years in which this information is available in the NLSY. We then exclude from our total sample observations that belong to one of the 3-digit occupations where piece rates account for at least 10% of the observations between 1988 and 1990.<sup>25</sup> As it turns out, the overidentifying restrictions are still decidedly rejected, although the p-value does increase somewhat to 0.0003 for the model with comparative advantage only.<sup>26</sup> There is hardly any change for the model with both learning and comparative advantage.

If we further restrict the sample to occupations where piece rates account for less than 5% of the sample (this reduces the sample size to 18,108 observations), we basically get the same results as in the previous paragraph for the full sample: the overidentifying restrictions are rejected in both models. For the subsample of new jobs only, though, both models pass the test, with a p-value of 0.2185 for the comparative advantage model and 0.0827 for the model.

In summary, our assumptions that wages are paid in advance and depend on a single index of skill are restrictive and clearly contribute to the rejection of the overidentification restrictions of the model. It would be useful to enrich the empirical model in future research to accommodate departures from these assumptions. But despite the limitations of our approach, we think our model and instrumentation strategy are a useful way of analyzing the role of comparative advantage and learning in sectoral wage determination.

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<sup>25</sup>This reduces the sample size from to 24,726 observations. Not surprisingly, the main deletions are made in sales-related occupations, where we are more likely to find workers paid commissions, and also among operatives, which account for the bulk of piece rate workers. A list of all deleted 3-digit occupations is available upon request.

<sup>26</sup>The improvement is more substantial if we look at new jobs. In fact, we cannot reject the restrictions (p-value=0.0904) for the model with comparative advantage. The restrictions are still rejected for the full model with learning and comparative advantage (p-value=0.0175).

**Appendix Table B1: Average Value of Log Wage in year t as a function of occupations in year t and t-1.**

Occupation Categories	First lag of Occupation Categories							Total
	Professional Managers	Sales	Clerical	Craft Workers	Operatives & laborers	Services		
Professional	0.30	0.36	0.26	0.07	0.18	0.02	-0.05	0.26
Managers	0.36	0.21	0.26	0.01	0.12	-0.16	-0.22	0.16
Sales	0.23	0.22	0.24	-0.09	-0.20	-0.35	-0.45	0.13
Clerical	0.05	-0.05	-0.14	-0.15	-0.10	-0.27	-0.41	-0.15
Craft workers	0.15	0.13	-0.12	-0.12	0.08	-0.09	-0.21	0.03
Operatives & Laborers	0.01	-0.22	-0.25	-0.25	-0.11	-0.17	-0.39	-0.17
Services workers	-0.06	-0.26	-0.51	-0.45	-0.27	-0.39	-0.24	-0.26
Total	0.27	0.16	0.14	-0.13	0.03	-0.17	-0.25	0.00

**Appendix Table B2: Average Value of Log Wage in year t as a function of industry in year t and t-1.**

Industry categories	First lag of Industry Categories										
	Mining, Durables	Non-Durables	Constr- uction	Transpo. and util.	FIRE	Bus&Prof Services	Personal Services	Wholesale trade	Retail trade	Public sector	All
Mining, durables	0.15	0.03	-0.05	0.15	0.09	0.24	-0.10	0.05	-0.21	0.24	0.11
Non-Durables	0.01	-0.03	-0.22	-0.08	0.03	0.02	-0.22	-0.08	-0.18	-0.05	-0.05
Construction	-0.05	-0.12	0.09	0.06	-0.01	-0.04	-0.15	-0.11	-0.18	-0.13	0.03
Transportation, Utilities	0.17	0.08	-0.04	0.20	0.17	0.10	-0.18	-0.09	-0.19	0.10	0.14
Fin., Insur., Real estate	0.02	-0.12	0.00	-0.08	0.13	0.16	-0.33	-0.08	-0.22	0.05	0.10
Business and Prof. Services	0.21	0.02	-0.11	0.03	0.16	0.25	-0.18	0.01	-0.22	0.02	0.15
Personal Services	-0.18	-0.41	-0.16	-0.17	-0.40	-0.26	-0.26	-0.34	-0.38	-0.01	-0.28
Wholesale trade	0.05	-0.12	-0.19	-0.04	-0.11	-0.04	-0.30	-0.00	-0.22	0.01	-0.03
Retail Trade trade	-0.29	-0.31	-0.39	-0.23	-0.33	-0.24	-0.31	-0.29	-0.23	-0.25	-0.25
Public Sector	-0.01	0.05	-0.07	0.07	0.00	0.16	-0.05	-0.01	-0.08	0.19	0.14
All	0.10	-0.05	0.02	0.14	0.10	0.16	-0.25	-0.02	-0.23	0.15	-0.00