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Labour Market Outcomes:

A Cross-National Study

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Firms and Wages: Evidence from Displaced Workers.

Thomas F. Crossley
York University, Toronto

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Abstract: I use a unique data set of Canadian displaced workers to measure the effects of firm of employment on wages. This data set has the advantage of consisting of a sample of workers changing jobs for reasons (product demand shifts or technological changes) that are largely orthogonal to their individual levels of "ability". It is also drawn from a labor market with wage-setting institutions that are quite similar to the U.S. My main findings are that, even within narrowly-defined industries, there are economically large and statistically significant firm wage effects that cannot be accounted for by unobserved worker heterogeneity. For a number of reasons, including the evidence I present on tenure, these effects are not easily attributable to compensating differentials, thus suggesting a role for models in which job rents play a role.

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Correspondence: Department of Economics, Faculty of Arts, York University, 4700 Keele St. Toronto, Ontario, Canada, M3J 1P3. Email: crossley@yorku.ca. Website: http://dept.econ.yorku.ca/~crossley

1. Introduction

According to the simplest competitive model of labor markets, identical workers should earn identical wages, regardless of the firm at which they are employed. The empirical relevance of this model has, however, recently been questioned by a number of authors (e.g. Groshen, 1991a,b; Abowd et al., 1995; Bronars and Famulari, 1997), who find large and significant firm wage effects in different samples of workers. This suggests possibly important roles for alternative conceptualizations of the labor market, including various types of equilibrium search models (Manning, 1994; Burdett and Mortenson, 1998), efficiency-wage models (Bulow and Summers, 1986), or rent-sharing models (Christofides and Oswald, 1992).

While the available evidence on firm wage effects is highly suggestive, it must still be interpreted with some caution. For example, studies of firm wage effects based on cross-section data (e.g. Groshen, 1991a,b; Bronars and Famulari, 1997; Barth, 1997) face the same difficulty as comparable studies of industry wage effects: they may simply reflect the sorting of workers, by productivity, across firms (Krueger and Summers, 1988; Gibbons and Katz, 1992). As is well known, this problem can be addressed using panel data, but such studies are rare (Abowd et al., 1995, is the best example), and face some data-induced limitations of their own.

In this paper I use a unique data set of Canadian displaced workers to measure the effects of firm of employment on wages. While much smaller in size than the large administrative data sets used in some other studies, this data set has the important advantages of (a) containing a fairly complete set of measures of workers' demographic characteristics; (b) consisting of a sample of workers changing jobs for reasons (product demand shifts or technological changes) that are largely orthogonal to their individual levels of "ability", and (c) being drawn from a labor market with wage-setting institutions that are quite similar to the U.S. My main findings are that, even within narrowly-defined industries, there are economically large and statistically significant firm wage effects that cannot be accounted for by unobserved worker heterogeneity. For a number of reasons, including the evidence I present on tenure, these effects are not easily attributable to compensating differentials, thus suggesting a role for models in which job rents play a role.

Models of the labor market in which job rents exist can have very different implications for the effects of various policies on employment, unemployment and wages (e.g. Bulow and Summers, 1986). Knowing that job rents are quantitatively important also has implications for the design of policies designed to reduce wage inequalities, both among and within groups, (e.g. England, 1992). The evidence presented here that firms do matter (and indeed in North America at least they seem to matter *a lot*) thus may have important implications for how we view labor markets and how we design policies affecting them.

Section 2 discusses possible explanations for firm wage effects in cross section data and reviews the existing empirical literature. Section 3 briefly describes the data set; Section 4 presents results and Section 5 concludes.

2. Firm Wage Effects: Existing Evidence and Explanations.

Groshen (1991b) discusses five reasons for the existence of firm wage premia. They are:

Explanations for Empirical Firm Wage Differentials (Groshen, 1991b)

1. Firms sort workers by unmeasured productivity.
2. Compensating wage differentials.
3. Information costs and other frictions.
4. Firms pay efficiency wages.
5. Workers capture rents.

The first two explanations posit no "true" wage effects; in each case, empirical firm wage effects are simply an artifact of measurement problems. If the econometrician could completely measure productivity and non-pecuniary aspects of compensation, the firm wage effects would disappear. Sorting of workers across firms by productivity differences arises in "team production" models (Kremer, 1993; Rosen, 1982). The notion of compensating wage differentials is exposited in any labor economics text.

The third explanation posits that firm wage premia may arise either randomly (perhaps due to error) or by design and can persist because of information costs and other frictions. This explanation would of course encompass general equilibrium search models of the labor market (Burdett and Mortensen, 1998). Note that these frictions need not be in the labor market. Frictions in capital markets will allow firms to persist in paying above market wages.

The fourth explanation refers to the idea that the optimal wage for some firms (or a sector) may be above the market clearing wage (see for example, Weiss, 1990). The final explanation rests on the existence of rents (perhaps due to product market imperfections) that accrue to workers either as a result of their bargaining power or the benevolence of managers.

Groshen (1991b) suggests that the first three explanations are competitive explanations in the sense that the labor market clears. In the cases of efficiency wages and rent sharing, there should be queues for high paying jobs. From an empirical view, an important distinction is between an explanation based on unmeasured differences in worker characteristics or (possibly non-pecuniary) compensation (that is sorting or “mismeasurement” based explanations) and those based on characteristics or policies of firms. Groshen’s first two explanations are of the former type. A primary goal of this paper is determine whether observed firm effects are simply artifacts of such empirical problems.

Groshen’s empirical results have since been replicated on other cross section data sets. For example significant firm wage differentials are reported by Bronars and Famulari (1997) in a U.S. sample of white collar workers and Barth (1997) in Norwegian Data. Nonetheless, there is a limit to what we can learn about firm wage effects from cross section data. A central difficulty in studying firm wage effects (or inter-industry wage differentials) is the unobservability of productivity. Productivity is generally proxied by observable "productive characteristics", which are suggested by human capital theory: education, training, experience and seniority (tenure). If one then looks for the effects of "nonproductive characteristics" on wages, one runs the risk that these characteristics may simply capture part of the effect of the unmeasured component of productivity. As is well known, if multiple observations on each worker are available, it is possible to remove at least the fixed component of unmeasured productivity by first differencing.[1] Simply put, if firm wage effects reflect worker characteristics, they should persist when workers move to a new firm. Groshen (1991a) and other authors employing cross sectional data can only control for observable individual characteristics.

[1] In fact, what must be assumed is that unmeasured productivity can be divided into two components: one that is individual specific and time invariant (often referred to as "ability") and a second which is uncorrelated with observables. Furthermore, the fixed individual characteristic *must generate the same return in different jobs.*

If one follows a random sample of workers through time (with a panel data set) firm (and industry, and union) effects are identified entirely from the subsample of job changers. Precisely because these individuals are choosing to move, they may be drawn from firms, or moving to firms, who are outliers in the distribution of firm wage effects. This is the strategy pursued by Abowd, Kramarz and Margolis (1995, forthwith AKM), who explore the issue of worker sorting versus "true" firm wage effects with a French panel data set.

Ideally, one would like to observe in two jobs a sample of workers whose mobility was randomly assigned. If one focuses on workers displaced by plant closures and mass layoffs, data from displaced worker surveys reasonably approximate this experiment. This is the strategy pursued in this paper. This exercise parallels that undertaken by Gibbons and Katz, for industry wage effects (1992). The crucial assumption is that, for these workers, displacement is orthogonal to the individual characteristics (particularly unobserved characteristics).[2] All the workers studied in this paper change jobs after a plant closure or mass layoff. In contrast, AKM have no information on reason for separation

AKM report that they find statistically significant firm fixed effects, but that these are dwarfed in magnitude by individual effects. Two further aspects of the AKM study are important in light of this result. First, AKM have rather limited information about worker characteristics (such as education). If they are measuring time- invariant worker characteristics with considerable error, this may contribute substantially to the large individual effects they observe. Superior controls for individual characteristics is an important advantage of the current paper.

[2] Note that this assumption is almost certainly not valid for workers displaced on an individual basis.

Secondly, the importance of firm wage effects may depend considerably on national wage-setting institutions; it would be interesting to see whether AKM's result of small firm wage effects carries over to contexts, such as the U.S. or Canada, where individual firms have much more discretion over wage-setting than they do in France.

Finally, the literature has not evaluated compensating wage differentials as an explanation of firm wage effects. Thus this paper complements the work of AKM and others. The most important disadvantage of displaced worker data is that it represents a nonrandom sample of firms. All the firms from which these workers are sampled are contracting in size. In the conclusion, I discuss the possible implications of this for the interpretation of my results.

3. Data.

The data employed in this survey come from an Ontario Ministry of Labor Survey of workers displaced in 21 mass layoffs or plant closures in 1980 or 1981. The sample frame was constructed from personnel records provided by the firms; thus each worker surveyed can be matched to their pre-displacement firm. The survey collected information on standard demographics, human capital measures and hourly wages both in the pre-displacement job and in re-employment. Summary statistics for the data are presented in Table A1.1. Further descriptions of the data can be found in Ontario Ministry of Labour (1983), Crossley et al. (1994) and Jones and Kuhn (1995).

Each job observed in the data was assigned an extremely detailed (7 digit) occupation code from the Canadian Classification and Dictionary of Occupations (CCDO). This aspect of the data allows a crude view of the internal hierarchies of the firms; it is possible to identify supervisors within narrow occupations for example. In addition, the CCDO rates each detailed occupation on a broad series of measures including both required aptitudes (such as numerical, verbal and physical abilities) and conditions of work (such as injury risk and environmental exposure). These can be used to infer the characteristics of each worker's job. An extensive discussion of aspects of the data pertinent to firms and jobs can be found in Crossley (1998a).

There is now considerable evidence of inter-industry wage differentials (Krueger and Summers, 1988) at fine levels of industry disaggregation. In order to isolate firm wage effects, it will be necessary to condition on industry. In this paper I consider three levels of industrial structure. I define "sector" to correspond to SIC division. Of the 21 firms, 19 are in the manufacturing sector, while the other two are in unique sectors (financial and service). Since for these two firms I will not be able to disentangle firm effects from industry effects even at this high level of industry aggregation, I drop them from my empirical analysis and focus only on manufacturing firms. The 2 digit industries correspond to SIC major groups. At this level of industrial disaggregation, 7 manufacturing firms are in unique industries. I can estimate firm wage differentials for the 12 firms in the food and beverage, textile, transportation equipment and electrical products industries. If I control for 4 digit industry (SIC minor groups) I can estimate firm wage differentials for the 3 firms in the carpet, mat and rug industry and for the 3 firms in the motor vehicle parts and accessories industry. The break down of firms by different levels of industrial disaggregation is presented in Table A2.2.

Because of the elapsed time between the displacements and the survey (less than 2.5 years), there is some selection into re-employment at the survey date. An analysis of this problem is can also be found in Crossley (1998a); it does not seem to significantly affect the results presented in this paper, as is discussed in section 4.2.

4. Results.

4.1 Firm Wage Effects in Cross Section.

Table 1 reports mean real hourly wages at the 19 firms under consideration. Evidently, there were considerable differences in the mean wages paid by these firms. Of course, the firms might well employ very different kinds of labor. I begin my empirical exploration of inter-firm wage variation by estimating firm wage effects in cross sectional pre-displacement wage equations. In Table 2 I report the explanatory power of alternatively specified wage equations. The firm dummies alone account for 57% of the raw variation in log wages. The (weighted) standard deviation of the raw firm wage differentials is 0.224, compared to the standard deviation of log wages of 0.299. Simple human capital measures can only explain 29% of the raw variation in log wages, while a fully specified wage equation, including human capital measures, demographics and 2 digit industry controls has an R^2 of 0.65. Thus a worker's employer is a better mean square error predictor of wages than the characteristics of the worker. Adding firm controls to this human capital specification accounts for an additional 8% of the variation in log wages. Of course, worker characteristics are not randomly distributed across firms. However, even if we attribute all the possible variance in wages to individual characteristics, firms still explain a substantial fraction of the residual.

In Table 3 I report estimates of firm wage differentials conditional on different sets of industry controls. The dependent variable is the logarithm of pre-displacement real hourly wages. Each regression includes controls for human capital, demographics, and job characteristics (including union status), as well as industry and firm dummies. I report firm wage differentials, following the procedure introduced by Krueger and Summers (1988) for industry wage differentials. The wage differential for firm *i* is the coefficient on the relevant firm dummy (0 for the omitted firm) minus the weighted mean of all the coefficients on the firm dummies within a particular industry (including 0 for the omitted firm in each industry). Differentials so calculated are invariant to which firm is omitted. Following Haisken-DeNew and Schmidt (1997) I calculate the corrected standard errors for each differential. Moving from left to right in Table 3, I control for progressively finer inter-industry wage differences. Even controlling for 4 digit industries (column 3), I find statistically significant intra-industry firm wage effects. These effects also have economic significance. For example, a worker at Txtl4 could expect to earn approximately 10% less than the wages she could expect in the carpet industry.

At the bottom of each column I report summary measures of the statistical and economic significance of the firm wage differentials. The F statistics for the joint significance of the firm dummies confirms that they are statistically significant at the 0.001 level, regardless of the level of disaggregation in industry controls.[3]

The WSE is the weighted standard error of the firm differentials. To calculate this I take the weighted (by observations) sum of the squared firm differentials,[4] divide by the sum of the weights (the sample size), correct for sampling variation (Haisken-DeNew and Schmidt, 1997) and take the square root. It gives a measure of the amount of wage variation in the sample

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[3] Through out I employ a heteroscedasticity consistent estimate of the covariance matrix. A test for different error variances across firms rejects the null hypothesis of homoskedasticity at standard significance levels.

[4] Note that the mean within industry differential is zero.

attributable to the wage differentials, and has the same units as the wage differentials. The WSE is 0.18 log points in manufacturing and 0.12 log points within two digit industries. Thus a worker at a firm paying one standard deviation above the industry norm could expect to earn a 0.12 log point premium. This is quite substantial when compared to the unconditional standard deviation of log wages at 0.30.[5] It is also quite substantial when compared to premia that accrue to specific individual characteristics. For example, relative to having only elementary education, highschool completion is associated with average wages that are only 0.053 log points higher, and some tertiary education a further 0.047 log points beyond that.

There is evidence of worker sorting by productivity differences. Conditioning on observable characteristics reduces the WSE of firm differentials from 0.22 to 0.18 in manufacturing and from 0.16 to 0.12 within two digit industries.

In Table 4 I report the coefficients on worker characteristics estimated with and without the firm dummies. These are the OLS and "within" estimates respectively. The difference in some coefficients is striking. For example, the cross sectional returns to tenure are substantially reduced when estimated using only the variation in wage and tenure within firms. Cross sectional estimates of the returns to tenure are often thought to be upwardly biased because more able or better matched workers have longer tenures. The latter intuition is confused; in a cross section of *interrupted* tenures it is not necessarily the case that short tenures represent poor matches. Nonetheless, in this data it appears that high tenure workers are employed at high paying firms. I investigate this in a second paper (Crossley, 1998b). It is also apparent that the gender gap within these firms is about half of what it is in the pooled data. The implication is that a substantial

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[5]In percentage terms a 0.12 log point premia, for example, is $[1-e^{-0.12}]*100 = 11.3\%$.

fraction of the gender gap is attributable to the fact that women are disproportionately employed in low wage firms.

The next three subsections, and the remainder of the tables, investigate the plausibility of alternative explanations of the cross sectional wage differentials summarized above. I pay particular attention to "mismeasurement" explanations: unobserved ability (Section 4.2) and compensating differentials (Section 4.3). To do this, I present alternative estimates of the firm wage differentials that exploit the longitudinal nature of the data, employ alternative samples, or augment the basic cross section specification with additional controls. For each estimation, I present the F statistic for the exclusion of the firm dummies, and the weighted standard deviation (WSE) of the firm wage differentials. In the bottom panel of each tables, I present the correlations between the sets of wage differentials arrived at via the alternative specifications or samples. These correlations are weighted by the number of respondents from each firm - this roughly weights the differentials according to their precision.[6]

I also present evidence on the correlates of the wage differentials: characteristics of the firms and the local labor markets they operated in.[7] Throughout, I focus on the firm wage differentials within two digit industries.

4.2. Are Firm Wage Effects Unobserved Ability?

In the previous section I presented evidence that conditioning on observable characteristics reduced the apparent firm wage differentials. Clearly workers sort by productive

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[6] Full estimation results are available from the author.

[7] Because these variables vary only *across* (rather than within) firms, it is not possible to control for them by adding them to a wage regression with firm dummies.

characteristics. I now turn to the role of unobserved worker heterogeneity in explaining empirical firm wage effects. I consider evidence based on job characteristics and on the longitudinal aspect of the data.

One way to pick up unobserved skill differences between workers at firms in the same industry is to condition on the typical skill requirements of the narrow occupations of the workers. I present the results of this exercise in Column 2 of Table 7. My base cross section regression includes the following occupational controls: blue collar supervisor, white collar worker, white collar supervisor, professional, manager. The omitted category is a blue collar worker without supervisory responsibilities. Here I augment these controls with measures of the intelligence, strength, spatial thinking, and manual dexterity typically required in the job held by each worker. These controls are jointly statistically significant, but only the intelligence measure is individually significant. The intelligence measure is a five point scale (with 5 being the highest), and over 90% of the workers in the data are displaced from jobs ranking 2 or 3. The coefficient on this measure is 0.06, with a t-value over 6. For the current exercise, the important finding is that these controls diminish neither the statistical or economic significance of the firm wage effects (as measured by the F test for exclusion of the firm dummies and the WSE of the firm wage differentials, respectively).

Following the Gibbons and Katz (1992) analysis of inter-industry wage differentials, I exploit the longitudinal nature of displaced worker data in two ways. First, I estimate first difference wage equations, hoping to net out unobserved but fixed individual effects. Gibbons and Katz point out that if the component of unmeasured ability that is sorted by industry (in the current context: firm) is time invariant and generates the same return in different jobs, then industry (firm) wage effects can be consistently estimated by wage change regression for industry (firm) switchers. Second I examine the role of pre-displacement firms in determining post-displacement wages. If unmeasured ability is sorted across pre-displacement firms, then pre-displacement firm dummies will proxy for unmeasured ability in a post-displacement wage equation. A simple empirical framework which captures these arguments is presented in Appendix 1.

Unfortunately, I cannot repeat Gibbons and Katz's exercise exactly for firm wage effects. In particular, I cannot control for post-displacement firms.[8] This means that, under the assumption that there are true firm wage effects, my first difference regression will be misspecified by the exclusion of post-displacement firm controls and hence the first difference estimates of pre-displacement firm wage effects may be biased. However, if a worker displaced from a "high" wage firm is more likely to move to another "high" wage firm, then the first difference regression will *underestimate* pre-displacement firm wage effects. This is because the observed wage losses will be less than those that would occur if a workers were re employed at random firms. Thus, I argue that this bias has the opposite sign to the bias in the cross section regression, and that the first difference estimates give us a lower bound on the magnitude of firm wage effects. These arguments are also developed in the empirical framework of Appendix 1.

Table 5 summarizes the longitudinal estimates of the firm wage effects and compares them to the cross section estimates. Each column summarizes a different sample or estimation strategy. In each case I present the weighted standard error of the firm wage differentials, and an F test for

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[8] The data does include post-displacement firm identifiers. However, more than half of those workers who are re-employed at the survey date are employed at a unique post-displacement firm.

the significance of the firm dummies. In the bottom panel of the table I present the correlation between the alternate estimates of firm wage estimates. In calculating the correlations, each of the estimates of each firm's wage differential are weighted by the number of observations from the firm, giving greater weight to the more precisely estimated premia.

The first column of Table 5 summarizes again, for purposes of comparison, the full sample cross section estimates, from column 2 of Table 3. The next column presents a second set of cross section estimates, based on the restricted sample of workers re-employed by the survey date. Longitudinal estimates must be restricted to this sample so these estimates provide a basis for comparison. They also provide, through comparison with the full sample estimates, a check of the effects of sample selection on estimates of the firm wage premia. An initial investigation indicated that, conditional on a full set of individual and job characteristics and on place of employment, those who would go on to re-employment by the survey date earned approximately 6% more in their pre-displacement jobs.[9] Thus selection into re-employment is clearly correlated with unobserved variation in remunerable characteristics. However, as Table 5 reports, the cross section estimates of firm wage premia on the re-employed sample are almost identical to those based on the full sample.[10] Furthermore, the longitudinal estimates employed explicitly account for fixed individual differences in earnings power.

The first difference estimates are summarized in the third column of Table 5. The dependent variable is the logarithm of post-displacement real hourly wages minus the logarithm of

[9] For full details, see Crossley (1998a).

[10] This is also true of estimates of the returns to standard worker characteristics (Crossley 1998a).

pre-displacement real hourly wages. In addition to pre-displacement firm I include controls for changes in job characteristics and human capital measures and industry switches. I control only to the level of the two digit industry, because I do not have sufficient observations to estimate the effects of four digit industries after displacement.

From the F statistic, it appears that the difference (or fixed effect) estimates of the intra-industry firm wage effects are statistically significant. They also remain economically significant: based on the first difference estimates a worker at Txtl4 could expect to earn 0.11 log points less than the wages she could expect elsewhere in the textile industry. However, they are significantly smaller than the cross section estimates: the WSE falls by about half. Finally, note that the wage differentials based on the difference estimates are highly correlated with those based on cross section estimates.

Pre-displacement firms will affect post-displacement wages if either (1) firms sort workers by unmeasured ability or if (2) a worker displaced from a high wage firm is more likely to be re-employed at a high wage firm than an identical worker displaced from a low wage firm. Therefore the regression of post-displacement wages on pre-displacement wages captures the bias in both the cross section and first difference estimates of firm wage effects. Again, this is illustrated in Appendix 1. The results of this exercise are presented in the fourth column of Table 5. The dependent variable is the log of real hourly post-displacement wages. I control for human capital, demographics, post-displacement job characteristics (union status and blue collar/white collar) and post-displacement industry. After controlling for these things, pre-displacement firm is a significant determinant of post-displacement wages. This suggests that either the cross section or the first difference estimates, and possibly both, are biased.

Under the assumption that workers displaced from a “high wage” firm are no more (or less) likely to be re-employed at a “high wage” firm than those displaced from a “low wage” firm, the WSE of the difference and post-displacement estimates give a rough decomposition of the cross section wage differentials into the part due to sorting across firms by unobserved ability (captured by the post-displacement estimates) and the part that cannot be explained by sorting (captured by the difference estimates). Under this assumption, sorting may account for just less than half of the firm wage differentials observed in cross section.

If this assumption does not hold, then the difference estimates of the firm wage differentials (both positive and negative) are likely biased towards zero (see Appendix 1) and consequently the WSE of the “true” firm wage effects biased down. In sum, the results indicate that there is significant sorting of workers across firms by unobserved characteristics, but that firm wage effects can not be completely or even largely explained by sorting of workers by unmeasured ability.

4.3. Are Firm Wage Effects Compensating Differentials?

The firm wage effects could reflect compensating differentials. The longitudinal estimates of Table 5 do not shed any additional evidence on the plausibility of this explanation without a strong assumption about the source of heterogeneity in the compensating differentials. If for example, all the heterogeneity is in worker preferences (so that there is a single technological frontier) then it might be reasonable to assume that, after displacement, workers return to a job with a similar tradeoff between wage and non-pecuniary characteristics. Thus non-pecuniary characteristics of the job could be treated as a fixed effect. This story completely breaks down if there is heterogeneity in technology, that is, if firms as well as workers differ in the rates they are willing to trade off wages against other job characteristics (see Appendix 1 for a further exposition of this argument).

Rather than make such an assumption, I present a number of other pieces of evidence on the compensating wage differentials explanation. First, I simply point out that there are significant wage differentials within very narrowly defined (4-digit) industries (Table 3, column 4). This rules out as an explanation differences in job characteristics to do with the products or common technology of industries. We need to consider instead differences in the non-wage conditions of work at different auto-part manufacturers or carpet weavers.

It could be the case that technological differences across firms cause each to employ a different occupation mix of workers. A "high wage" firm, for example, might be one that employs (relative to industry norms) a larger number of workers in risky occupations.[11] While my basic estimates control for differences between several types of workers,[12] I simply do not have enough data to control for finely disaggregated occupational effects. Fortunately, the matched job characteristics by detailed occupation allow a way around this. Rather than condition on occupation, I condition on measures of the typical job characteristics in that very narrow (4 digit) occupation. In particular, I include dummies which indicate whether jobs in a respondent's 4 digit occupation typically involve a risk of injury, exposure to extremes of heat or cold, and exposure to air or noise pollution. The results are summarized in Column 3 of Table 7. These additional controls are jointly statistically significant in the wage regression, with risk of injury and exposure

[11] A less automated firm might employ a greater proportion of its workers directly in the production process.

[12] Specifically: managers, professionals, white collar supervisors, other white collar workers, blue collar supervisors, other blue collar workers.

to cold being individually significant. There appear to be positive compensating differentials for injury risk (0.04 log points) and cold (0.31 log points). None-the-less, it is obvious from the F statistic and WSE (comparing to column 1) that they do not diminish the economic or statistical significance of the firm wage differentials. Furthermore, differentials so estimated are almost perfectly correlated with those from the base specification. This would seem to preclude a model in which, within an industry, the non-pecuniary characteristics of jobs is common, but the mix of jobs varies across firms.

Thus if compensating differentials are to explain the firm wage differentials apparent in this data, it must be the case that some of the firms differ from industry norms in the non-pecuniary compensation they offer to workers in particular jobs. Being a machinist at one auto parts manufacturer must be a riskier or more unpleasant job than being a machinist at another auto parts firm. In columns 2, 3 and 4 of Table 6 I present estimates of the firm wage differentials based on the subsamples of blue collar workers, white collar workers, and white collar workers excluding managers and professionals, respectively. In all cases the differentials are statistically significant, and are of the same magnitude as, and are highly correlated with, differentials estimates on the full sample and with each other. Thus firm wage premia seem to be paid to all workers at a firm. This is inconsistent with the notion that one firm may have, for example, poorer plant safety or inferior plant air quality (in this case, the compensating differential would presumably only be paid to production workers). Interestingly, there is some evidence that the firm wage differentials are less consistent among managerial and professional workers. Bronars and Famulari (1997) report similar evidence of firm wage premia that are consistent across occupation groups. They interpret this as evidence of sorting, as predicted by models of team production. The longitudinal data employed in this paper provides a much more direct test of that hypothesis. As the previous section reported, I do find direct evidence of such sorting, but it is at best a partial explanation for firm wage differentials.

It remains possible that certain firms, within narrow industries, might offer a non-pecuniary benefit to all their workers.[13] In Tables 8 and 9 I present correlates of firm wage premia, by way of rankings and simple regressions respectively. The one strong pattern apparent in Tables 8 and 9 is the positive relationship between average tenure (at displacement) of workers at a firm and the firm's wage premium. High tenure workers are disproportionally represented at "high wage" firms. Such a result suggests that a positive firm wage premium is associated with reduced turnover, and is consistent with a rents-based explanation for the wage premia. Krueger and Summers (1988) find a similar relationship between industry wage premia and tenure; they interpret this as evidence for a rents based explanation of those premia and against a compensating differentials based explanation. In a compensating differentials equilibrium marginal workers should be indifferent between "high" and "low" wage firms, and those should not experience different worker mobility. A second model that is consistent with long tenures at high wage firms is a general equilibrium search model (Burdett and Mortensen, 1998) in which firms have some monopsony power and differentiate their compensation strategies along an isoprofit curve in wage-turnover space.

Finally I note that, as shown in Table 8, there is very little difference in mean reported weekly hours of work across the plants, suggesting that firm wage differentials are not a premia paid for differences in hours of work. While I cannot address the compensating differentials

[13] Fringe benefits, for example.

explanation as directly as the sorting explanation, the preponderance of evidence seems to suggest that compensating differentials cannot be a complete explanation of the firm wage differentials observed in this data.

4.4. Additional Specification Checks and Correlates of Firm Wage Effects.

In this final section I present a number of additional specification checks, each addressing a potential explanation for the firm wage differentials, and investigate some correlates of those differentials.

Differing conditions in very fine local labor markets have been suggested as an important determinant of displaced workers' wage losses (Carrington,1992). Furthermore, if firm wage effects reflect differences in monopsony power, they should be correlated with measures of the competitiveness of the local labor market. Local populations and unemployment rates are reported alongside the wage differentials in Tables 8 and do not seem to correspond to the pattern of wage effects. I have also approached this with simple regressions of the estimated firm wage differentials on these local labor market characteristics (Table 9). There may be some relationship between the size of the local market and pay, but only if Toronto is excluded. I have no rationale for this result.

Unlike firm wage differentials, a firm size effect on wages is well known (Brown and Medoff, 1989). Table 8 also reports the size of each firm's operations in Canada and the size of the layoffs. For the 16 firms which experienced plant closures, the latter is a measure of plant size. There appears to be no obvious relationship between these numbers and the estimated firm differentials within two digit industries. Additional evidence is presented in the first row of Table 9. Evidently the individual firm wage effects I find in this data are not a firm size effect.

While the OML data contains hourly wages for each worker, not all the workers were paid on an hourly basis (that is, in some cases the hourly wage is calculated). To examine the possibility that the firm wage effects are capturing differential wage measurement error across the firms, I re-estimated the firm wage differentials on the sample of workers who were paid on an hourly basis. The results are in the fifth column of Table 6. These firm wage differentials so estimated are just as statistically and economically significant as, and are almost perfectly correlated with, the base case.

Table 8 presents the fraction of workers at each firm who report union coverage. The wage regressions I use to estimate the firm wage differential include a control for union coverage. However, since they also contain firm dummies, the union wage differential is estimated from the differences in the wages of covered and uncovered workers *within* firms. In fact, the "within" estimate of the union wage differential (Table 4) is negative. Presumably, the union dummy is picking up occupational differences in pay within firms that are not captured by my occupational (job level) controls. Thus "high wage" firms could be unionized firms. Inspection of Table 8 reveals that unionization is likely part of the story in the transportation equipment industry, but not in the textile industry or food industries. Table 9 confirms that unionization is not an adequate explanation of the observed firm wage premia.

Finally, I have considered the possibility that firms offer compensation schemes that differ in their starting wage and rate of wage growth, but that have the same present value. The final column of Table 5 addresses this issue. In fact, I find statistically significant differences across firms in their cross sectional wage tenure relationships. With a linear specification of the wage-tenure relationship, the weighted standard error of yearly percentage growth in wages is 0.0037.

Firms with above average intercepts have flatter wage-tenure profiles; the correlation between slopes and intercepts is -0.34, which is statistically significant at the 0.01% level. Similar results are reported by Bronars and Famulari (1997) and AKM (1995). In a wage loss regression the weighted standard error of the slopes rises to 0.0062 and the correlation of slopes with intercepts becomes more strongly negative at -0.65. However, there does not appear to be sufficient "cross over" for this relationship between wage levels and wage growth across firms to explain for the apparent firm wage effects. For both the cross section and first difference estimates, the rank ordering of predicted wage levels (by firms) at 20 years of tenure[14] remains positively correlated with the rank ordering of predicted starting wages. Additionally, the apparently lower turnover at high-wage-level firms is a puzzle, if the firm wage premia are to be explained by compensating differences in wage growth (one might expect *higher* turnover at high-wage-level/low-wage-growth firms). Of course, there are well known difficulties in the interpretation of the cross sectional relationship between wages and tenure - or even first difference estimates - as wage growth (See Crossley, 1998b, for example). These difficulties can only be exacerbated if there is firm heterogeneity in offered wage profiles (Margolis, 1995).

5. Summary and Conclusions.

Most studies of wage heterogeneity focus on the returns to observable worker characteristics estimated in a human capital wage regression framework. The focus is on the supply side of the labor market. Augmenting that framework, this paper has presented new

[14]The median (interrupted) tenure in the data is 10 years, and the 75th percentile is 16 years.

evidence that firm of employment appears to be an empirically significant source of wage heterogeneity. Even more than inter-industry wage differentials, these effects are *prima facie* evidence against the simple competitive model of the labor market, and its underlying assumptions.

A simple competitive model of the labor market can be "rescued" from these results by assuming that they are simply the empirical artifact of the empirical difficulty of measuring productivity and compensation. I consider the two most common variants of this proposition: the idea that workers sort across firms according to unmeasured productivity ("ability"), and the idea that the firm wage effects represent compensation for non-pecuniary characteristics of employment at particular firms (compensating wage differentials).

If firm wage premia represent unmeasured characteristics of workers, then workers should continue to earn those premia if they switch firms. This is only partially borne out in this data. In fact, using the longitudinal nature of the data, I conclude that sorting of workers across firms by unobserved ability can explain less than half of the observed differentials. This result, that firm fixed effects are at least as important as individual fixed effects, contrasts with that of Abowd et al. (1995, denoted AKM) who report that firm effects are dwarfed by individual fixed effects in a French panel data set. There are two possible explanations for this disparity. First, I have better controls for individual worker characteristics than AKM. This reduces the size of the *unobserved* worker effects, relative to the firm effects. Second, France has more centralized wage setting institutions than Canada, so that one would expect less firm wage heterogeneity in France than in Canada.

Firm wage differentials are observed within narrow industries, are consistent across broad occupational groups, are robust to conditioning on differences in the mix of skills or job characteristics. Further, “high wage” firms exhibit high average tenures suggesting that positive wage premia are associated with reduced mobility. From these observations I conclude that compensating wage differentials are also a poor candidate explanation for the observed differentials.

There are a number of possible objections to my results. An obvious one is that my data does not represent a random sample of firms. However, I think the fact that all of these firms were engaged in layoffs makes my results all the more striking. The obvious question is why some firms would pay (or workers demand) wages above the apparent industry norm, when faced with the necessity of laying off workers. One suggestion is that dying firms are those that make “wage mistakes”, that is, offer excessive or insufficient compensation to workers of a given productivity. If this were true, then my results would overstate the contribution of firms to wage variation in the whole population. I think this is unlikely for two reasons. First, it implies that “wage mistakes” cannot be corrected, even in the face of a plant closure and mass displacement. Second, cross section wage effects are apparent in more representative data sets.

This paper was in part motivated by the growing body of labor market theories that suggest that firms and their structure have an important role in determining labor market outcomes. Team production models (Kremer, 1993; Rosen, 1982) suggest that firm wage effects should arise because of the sorting of workers by ability. While my results suggest that such sorting cannot fully explain the observed firm wage effects, I do find substantial evidence of sorting. The correlation of firm wage effects across occupations is another prediction of these models confirmed here. General equilibrium search models (Burdett and Mortensen,1998) predict “true” firm wage differentials, as do efficiency wage models (Weiss, 1990). The results presented here confirm some of the predictions of these models and should encourage this renewed emphasis on the demand side of the labor market.

The results in this paper should also raise some question about the utility of pooling data across firms. Evidently the returns to some typically studied individual characteristics are largely driven by mean differences *across* firms. This can have important policy implications. For example, I find that the gender gap within firms is about half that which is observed in the pooled data. This in turns suggests that a good part of the gender gap is the result of the sorting of women into low wage firms. Policies which attempt to promote pay equity *within* firms (policies intended to address occupational sorting as a source of the wage gap) with obviously be ineffective against *inter*-firm wage inequality.

Finally, several authors (Carrington, 1992, Neal, 1995) have recently questioned the role of firm-specific elements in generating the wage losses experienced by displaced workers. The evidence presented in this paper suggests that displacement from a particular firm may be very costly; not, however, because of the loss of specific skills but because the firm paid a wage premium. Further, the large losses of high tenure workers may result from their concentration at “high wage” firms, rather from their having accumulated more firm specific skills.

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APPENDIX 1: EMPIRICAL FRAMEWORK.

The empirical observation established by, for example, Groshen (1991a) is:

$$E[\omega' z_j | x] \neq 0 \ \forall \ j \qquad \textbf{(1)}$$

Where ω is a (n x 1) vector of residuals from a standard human capital wage regression,

$$\omega = w - E[w|x] \qquad \textbf{(2)}$$

z_j is a (n x 1) vector corresponding to the *jth* firm dummy, and x is an (n x m) matrix of observable individual and job characteristics which may (or may not) vary across observations of the same worker. Thus in cross section there appear to be "firm wage effects".

Groshen proposes five explanations for this observation, which cannot be distinguished on the basis of (1) alone. In addition to confirming this observation, in this paper I report several additional observations. In particular I estimate

$$E[\omega_\Delta z_j | x] \ \forall j \qquad \textbf{(3)}$$

Where ω_θ is the vector of residuals from a wage loss regression.

$$\omega_\Delta = \Delta w - E[\Delta w | x] \qquad \textbf{(4)}$$

Δw is the vector of wage losses on displacement (w_2-w_1) and I have introduced an index t=1,2 to indicate pre- and post-displacement respectively. Implicitly, x without a subscript may now contain both pre- and post-displacement characteristics. Alternatively I consider

$$E[\omega_2 z_{1j}|x_2] \;=\; 0 \;\;\forall\;\; j \qquad \textbf{(5)}$$

the covariance of post-displacement wages with pre-displacement firm dummies. I also examine the relationship between these conditional covariances across j.

In the sequel, I sketch several simple empirical models which capture several of Groshen's explanations for (1). I illustrate how they would generate (1) and derive their implications for (3) and (5), and the relationship between these conditional covariances across j. In the main body of the paper I present additional evidence on the plausibility of alternative models based on the relationship between the apparent firm wage effects and mobility (as measured by average tenure at the firm) , on the pattern of firm wage effects across occupational categories, and on the effect on the estimates of firm wage premia of conditioning on different sets of covariates.

The first two explanations posited by Groshen suggest that the apparent "firm wage effects" are entirely spurious, the artifact of a correlation between the firm dummies and omitted variables in the wage regression. If the omitted variable is an (fixed) individual characteristic, we have a model of workers sorting between firms on the basis of unobserved individual heterogeneity. If the omitted variable is a unobserved job characteristic, then we have a model of compensating differentials. The other explanations listed by Groshen posit a "true" firm effect. The empirical model I propose to capture these has a firm fixed effect.

(1) *Unobserved Individual Characteristics*.

Imagine that wages are generated by:

$$w_t = x_t\beta + \mu + \epsilon_t \qquad \textbf{(6)}$$

and that

$$E[\mu z_{tj}|x_t] \neq 0 \;\; \forall \; j \qquad \textbf{(7)}$$

For example, some firms hire workers that, conditional on their observable characteristics are, on average, more able. Then

$$E[\omega_1 z_{1j}|x_1] = E[\mu z_{1j}|x_1] \qquad \textbf{(8)}$$

and

$$E[\omega_2 z_{1j}|x_2] = E[\mu z_{1j}|x_2] \qquad \textbf{(9)}$$

Differencing, however, sweeps out the individual fixed effect so that this model implies

$$E[w_\Delta^{/} z_j|x] = 0\forall j \qquad \textbf{(10)}$$

Since the wage premium earned by workers at apparent `high wage' firms is due to a portable individual characteristics, these workers should experience typical wage losses.

(2). *Compensating Wage Differentials.*

Imagine that wages are generated by:

$$w_t \;=\; x_t\beta \;+\; y_t\alpha \;+\; \epsilon_t \qquad \textbf{(11)}$$

where the matrix y_t measures unobserved non-pecuniary aspects of the job. If

$$E[y_1^{\prime} z_{1j}|x_1] \;\neq\; 0 \;\; \forall j \qquad \textbf{(12)}$$

then:

$$E[\omega_1 z_{1j}|x_1] \;=\; \alpha^{\prime} E[y_1^{\prime} z_{1j}|x_1] \;\; \forall j \qquad \textbf{(13)}$$

also:

$$E[\omega_2 z_{1j}|x_2] \;=\; \alpha^{\prime} E[y_2^{\prime} z_{1j}|x_2] \;\; \forall j \qquad \textbf{(14)}$$

and

$$E[\omega_{\Delta}^{\prime} z_j|x] \;=\; \alpha^{\prime} E[\Delta y^{\prime} z_{ij}|x] \;\; \forall j \qquad \textbf{(15)}$$

Clearly the implications depend crucially on the correlation of omitted job characteristics across displacement. Consider two cases:

case 1: Imagine firms have homogenous technology, while workers have heterogenous preferences. Post-displacement workers select the same position on the (single) isoprofit surface that they occupied pre-displacement. The unobserved characteristics of their pre- and post-displacement jobs are identical.

$$y_1 = y_2 \tag{16}$$

$$\Delta y = 0 \tag{17}$$

$$E[y_2^{/} z_{1j} | x_2] = E[y_1^{/} z_{1j} | x_2] \ \forall j \tag{18}$$

then

and

$$E[\omega_{\Delta}^{/} z_j | x] = 0 \ \forall j \tag{19}$$

$$E[\omega_2 z_{1j} | x_2] = \alpha^{/} E[y_1^{/} z_{1j} | x_2] \ \forall j \tag{20}$$

case 2: Imagine workers are homogeneous in their preferences. Firms have different technologies. In equilibrium all workers receive the same utility regardless of what wage - job characteristics bundle they receive (that is, all jobs lie on a single indifference surface). Consequently they chose randomly. The unobserved characteristics of their jobs are not correlated across displacement and the unobserved characteristics of their post-displacement job is uncorrelated with their pre-displacement firm.

$$E[y_1^{/} y_2 | x_t] = 0 \tag{21}$$

$$E[y_2^{/}z_{1j}|x_t] = 0 \;\; \forall j \qquad \textbf{(22)}$$

then

$$E[\omega_2^{/}z_j|x_2] = 0 \;\; \forall j \qquad \textbf{(23)}$$

and

$$E[\omega_\Delta^{/}z_{1j}|x] = \alpha^{/}E[y_1^{/}z_{1j}|x] \;\; \forall j \qquad \textbf{(24)}$$

In a world with heterogeneity in both tastes and technology, both wage losses and post-displacement wages will be correlated with pre-displacement firms. Thus without a strong assumption about the nature of heterogeneity, neither the wage loss or post-displacement wage regression can provide evidence on the plausibility of the compensating differentials explanation of Groshen's result.

(3) *Firm Fixed Effects.*

Finally, imagine that wages are generated by:

$$w_t = x_t\beta + z_t\theta + \epsilon_t \qquad \textbf{(25)}$$

where the Z_t is a matrix of firm dummies and θ is a vector of firm effects. Z_{tj} is the jth column of Z_t

and θ_j is the jth element of θ. Note that

$$E[z_t\theta|x_t] = 0 \ \ \forall j \qquad \textbf{(26)}$$

then:

$$E[\omega_1 z_{1j}|x_1] = \theta_j E[z_{1j}^2|x_1] \ \ \forall j \qquad \textbf{(27)}$$

also:

$$E[\omega_2 z_{1j}|x_2] = E[z_{1j}' z_2|x_2]\theta \ \ \forall j \qquad \textbf{(28)}$$

and

$$E[\omega_\Delta' z_j|x] = E[z_{1j}' z_2|x]\theta - \theta_j E[z_{1j}^2|x] \ \ \forall j \qquad \textbf{(29)}$$

Clearly the implications depend crucially on the correlation of firm wage effects across displacement.[15]

Consider a search frame work, with true dispersion of wage policies across firms. Workers know the distribution of wage offers, but not their locations (that is, they know that some firms offer high wages, but not which ones). If workers are homogeneous, facing the same

[15] Because of the number of post-displacement firms, it is not possible to control for them.

offer distribution, they will all set the same reservation wage (making the standard search assumptions such as search without recall). In this case the expected wage is the same for every unemployed worker (the mean of the offer distribution above the common reservation wage). Differences in the wages a worker receives (conditional on observed characteristics) are random. Hence a worker who is exogenously displaced from a high wage firm is no more or less likely to find re-employment at a high wage firm than a worker displaced from a low wage firm. Then

$$E[\omega_2' z_{1j} | x_2] = 0 \ \ \forall j \qquad \textbf{(30)}$$

and

$$E[\omega_\Delta z_{1j} | x] = -\theta_j E[z_{1j}^2 | x] \ \ \forall j \qquad \textbf{(31)}$$

Thus the wage loss regressions provide a direct estimate of the pre-displacement firm wage effects.

This simple story breaks down if, conditional on observed characteristics, workers differ. For example they may have different knowledge about the location of high wage firms. More simply, they may differ in the utility of search, if for example, some workers have higher non-market income. If this is true, workers who set a higher reservation wage while searching for the pre-displacement job (and hence were more likely to work at a high wage firm) may set a higher reservation wage again. Hence workers displaced from a high wage firm would be more likely to be re-employed at a high wage firm. This implies

$$E[z_{j1}'z_2|x_t]\theta > 0 \text{ iff } \theta_j > 0. \qquad \textbf{(32)}$$

The wage losses of workers displaced from a high wage firm understate those they would experience if re-employed at a random firm. Thus the wage loss regression provides estimates of firm wage effects that (for both positive and negative premia) are biased towards zero.

These simple empirical models are summarized in Table A1.1.

TABLES

TABLE 1: Mean Real Wages and Real Wage Losses by Firm		
Firm, (valid observations)	Mean Real Hourly Wages (standard error of mean)	Mean Real wage losses (standard error of mean)
All Firms (1735)	8.24 (0.06)	1.07 (0.08)
Food1 (58)	8.58 (0.13)	1.39 (0.66)
Food2 (43)	6.62 (0.16)	0.47 (0.36)
Rbbr1 (415)	9.66 (0.08)	0.91 (0.14)
Lthr1 (57)	7.63 (0.26)	1.93 (0.30)
Txtl1 (217)	5.49 (0.06)	0.37 (0.18)
Txtl2 (191)	7.40 (0.18)	1.04 (0.18)
Txtl3 (66)	8.40 (0.37)	1.05 (0.42)
Txtl4 (41)	6.54 (0.19)	-0.48 (0.41)
Clth1l (53)	5.81 (0.15)	1.67 (0.25)
Papr1 (41)	9.17 (0.11)	0.90 (0.43)
Prnt1 (43)	7.52 (0.56)	0.47 (0.41)
Mchn1 (22)	6.98 (0.48)	-1.79 (0.52)
Vhcl1 (44)	9.24 (0.28)	2.11 (0.41)
Vhcl2 (25)	6.71 (0.41)	0.17 (0.49)
Vhcl3 (191)	11.06 (0.10)	2.79 (0.27)
Vhcl4 (67)	7.21 (0.11)	0.43 (0.32)
Elec1 (42)	9.22 (0.39)	-0.28 (0.63)
Elec2 (86)	8.79 (0.12)	1.95 (0.30)
Chem1 (33)	5.58 (0.26)	-0.34 (0.35)

TABLE 2: Explaining Wage Variation

Regressors	% of variation explained (R^2)
human capital measures.	0.29
human capital measures, demographics.	0.47
human capital, demographics, job characteristics.	0.51
human capital, demographics, job characteristics, two-digit industries.	0.65
human capital, demographics, job characteristics, four-digit industries.	0.7
19 firm dummies.	0.57
human capital, demographics, job characteristics, firm dummies.	0.73
human capital, demographics, job characteristics, two-digit industries and 8 firm dummies.	0.73
human capital, demographics, job characteristics, four-digit industries and 4 firm dummies.	0.73

Notes:
(1) the independent variable is log(real pre-displacement hourly wages).
(2) human capital measures: 5 age dummies, 4 education dummies, tenure and tenure squared
(3) demographics: female and married dummies, their interaction,
(4) job characteristics: dummy variables for union coverage, blue collar supervisor, white collar occupation, white collar supervisor, professional, manager.
(5) 10 two-digit industry dummies/14 4 digit industry dummies.
(6) the number of firm dummies decreases with increasing industry dissagregation as firms become unique to an industry.
(7) based on 1735 valid observations.

TABLE 3: Cross Sectional Firm Wage Differentials

	manufacturing sector	2 digit industry controls	4 digit industry controls
Food1 (58)	0.153 (0.021)	0.113 (0.011)	-----
Food2 (43)	-0.112 (0.018)	-0.152 (0.015)	-----
Rbbr1 (415)	0.144 (0.008)	-----	-----
Lthr1 (57)	-0.053 (0.0.32)	-----	-----
Txtl1 (217)	-0.262 (0.011)	-0.097 (0.009)	-----
Txtl2 (191)	-0.091 (0.011)	0.073 (0.009)	0.002 (0.008)
Txtl3 (66)	-0.041 (0.024)	0.124 (0.024)	0.053 (0.022)
Txtl4 (41)	-0.190 (0.026)	-0.025 (0.025)	-0.096 (0.024)
Clth1l (53)	-0.212 (0.028)	-----	-----
Papr1 (41)	0.135 (0.013)	-----	-----
Prnt1 (43)	-0.116 (0.039)	-----	-----
Mchn1 (22)	-0.202 (0.052)	-----	-----
Vhcl1 (44)	0.089 (0.019)	-0.024 (0.018)	-0.090 (0.018)
Vhcl2 (25)	-0.206 (0.040)	-0.319 (0.038)	-0.385 (0.039)
Vhcl3 (191)	0.250 (0.010)	0.138 (0.008)	0.071 (0.006)
Vhcl4 (67)	-0.146 (0.020)	-0.258 (0.018)	-----
Elec1 (42)	0.094 (0.034)	-0.021 (0.027)	-----
Elec2 (86)	0.124 (0.015)	0.010 (0.013)	-----
Chem1 (33)	-0.365 (0.049)	-----	-----
Weighted Standard Error of Firm Wage Differentials	0.176	0.123	0.099
Joint significance of firm dummies	F(18,1697) =85.2	F(8,1697) =81.75	F(4,1697) =41.9

Table 4: OLS and Within Estimates of the determinants of Log wages (Pre-displacement)		
Coefficient	OLS	Within Estimate
age3	0.048 (0.022)	0.047 (0.018)
age4	0.118 (0.023)	-0.115 (0.019)
age5	0.115 (0.025)	0.103 (0.020)
age6	0.066 (0.027)	0.089 (0.022)
educ1	0.086 (0.013)	0.028 (0.010)
educ2	0.152 (0.016)	0.053 (0.013)
educ3	0.200 (0.021)	0.091 (0.018)
training	0.052 (0.012)	0.026 (0.010)
tenure	0.013 (0.002)	0.005 (0.002)
tenure squared	-0.000 (0.000)	-0.000 (0.000)
female	-0.183 (0.023)	-0.104 (0.018)
married	0.058 (0.017)	0.050 (0.013)
female*married	-0.119 (0.027)	-0.063 (0.018)
blue collar supervisor	0.125 (0.025)	0.085 (0.020)
white collar occupation	0.028 (0.017)	-0.028 (0.014)
white collar supervisor	0.197 (0.055)	0.180 (0.055)
professional	0.095 (0.033)	0.047 (0.025)
manager	0.288 (0.030)	0.236 (0.033)
union	0.022 (0.014)	-0.079 (0.017)
observations	1735	872

TABLE 5: Longitudinal Estimates of Firm Wage Differentials

	Cross Section, Full Sample	Cross Section, Re-employed Sample	First Difference Estimates	Re-employment wages
sample size	1735	872	872	872
Weighted Standard Error of Firm Wage Premia	0.123	0.125	0.06	0.041
F-Stat for Firm Dummies (p-value)	81.75 (0.00)	49.74 (0.00)	4.11 (0.00)	2.42 (0.01)

Correlations of Wage Premia Estimates

	Cross Section, Full Sample	Cross Section, Re-employed Sample	First Difference Estimates	Re-employment Wages
Cross Section, Full Sample	1			
Cross Section, Re-employed Sample	0.964	1		
First Difference Estimates	0.896	0.904	1	
Re-employment Wages	0.257	0.3	0.019	1

TABLE 6: Estimates of Firm Wage Differentials - Alternative Samples

	Full Sample	Blue Collar	White Collar	White Collar, Managers, Professionals excluded	Paid Hourly
sample size	1735	1343	392	251	1179
Weighted Standard Error of Firm Wage Premia	0.123	0.131	0.113	0.125	0.121
F-Stat for Firm Dummies (p-value)	81.75 (0.00)	69.36 (0.00)	12.95 (0.00)	12.18 (0.00)	53.41 (0.00)

Correlation of Firm Wage Premia Estimates

	Full Sample	Blue Collar	White Collar	White Collar, Managers, Professionals excluded	Paid Hourly
Full Sample	1				
Blue Collar	0.99	1			
White Collar	0.92	0.85	1		
White Collar, Managers and Professionals excluded	1	0.98	0.92	1	
Paid Hourly	1	0.99	0.9	0.99	1

TABLE 7: Estimates of Firm Wage Differentials - Alternative Specifications				
	Base Specification	Job Aptitude Requirements	Job Disamenities	Slopes Vary by Firm
sample size	1735	1696	1698	1735
Weighted Standard Error of Firm Wage Premia	0.123	0.122	0.126	0.125
F-Stat for Firm Dummies (p-value)	81.75 (0.00)	75.41 (0.00)	80.12 (0.00)	7.69 (0.00)
F-Stat for Augmenting controls	---------	11.55 (0.00)	6.36 (0.00)	3.02 (0.00)
Correlation of Wage Premia Estimates				
	Base Specification	Job Aptitude Requirements	Job Disamenities	Slopes Vary by Firm
Base Specification	1			
Job Aptitude Requirements	1	1		
Job Dis-amenities	1	1	1	
Slopes Vary by Firm	0.83	0.81	0.82	1

TABLE 8: High and low Wage Firms in 2 Digit Industries

Rank	Firm	Differ-ential (%)	Size	Size of layoff	Size of Locale (1000s)	Local UE rate at Notice	% Union	Mean Usual Hours	Mean Tenure (years)
Food and Beverages									
1	Food1	11	1700	133	308	7.4	98	37.2	10.1
2	Food2	-15	1000	54	2131	5.1	97	39.1	8.6
Textiles									
1	Txtl3	12	899	113*	46	9.9	41	40.35	8.0
2	Txtl2	7	280	286	13	8.1	76	39.7	8.2
3	Txtl4	-2	150	73	35	8.2	77	39.8	4.6
4	Txtl1	-10	2800	240	73	6.1	92	40.46	11.45
Electrical Products									
1	Elec2	1	800	141	273	3.5	98	39.6	18.1
2	Elec1	-2	4800	62	2131	4.1	2	40.0	7.1
Transportation Equipment									
1	Vhcl3	14	350	253*	197	15.3	78	40.2	18.1
2	Vhcl1	-2	2500	66	5	6.2	76	40.2	13.0
3	Vhcl4	-31	350	75	2	8.2	0	40.7	3.3
4	Vhcl2	-36	34	36	2131	5.7	5	40.8	1.9

* Not a Plant closure.

TABLE 9: Correlates of Firm Wage Premia
(regressions, 12 observations)

Variable	Cross Section Premia	First Difference Premia
	slope coefficient (t-value)	slope coefficient (t-value)
log(firm size)	-0.019 (-0.57)	-0.009 (-0.46)
dummy for union firm (>50% report coverage)	0.122 (1.42)	0.054 (1.06)
% union at firm	0.183 (1.74)	0.082 (1.31)
log(mean tenure at firm)	0.161 (3.34)	0.091 (3.33)
log(local unemployment rate)	0.140 (1.29)	0.084 (1.39)
log(local population)	-0.005 (-0.28)	-0.008 (-0.79)
log(local population) dummy for Toronto	0.047 (2.03) -0.355 (-2.99)	0.020 (1.62) -0.196 (-3.03)

Notes;

TABLE A1.1: Empirical Framework.

Model	Empirical Covariance		
	$E[\omega_1'z_{j1}\|x_1]$	$E[\omega_2'z_{j1}\|x_2]$	$E[\omega_\Delta'z_{j1}\|x]$
Fixed Individual Heterogeneity	$E[\mu'z_{j1}\|x_1]$	$E[\mu'z_{j1}\|x_2]$	0
Compensating Differentials, Taste Heterogeneity Only	$\alpha'E[y_1'z_{j1}\|x_1]$	$\alpha'E[y_1'z_{j1}\|x_2]$	0
Compensating Differentials, Technology Heterogeneity only	$\alpha'E[y_1'z_{j1}\|x_1]$	0	$-\alpha'E[y_1'z_{j1}\|x]$
Firm Heterogeneity, Uncorrelated across displacement	$\theta_j E[z_{j1}{}^2\|x_1]$	0	$-\theta_j E[z_{j1}{}^2\|x]$
Firm Heterogeneity, correlated across displacement	$\theta_j E[z_{j1}{}^2\|x_1]$	$E[z_{j1}'z_2\|x_2]\theta$	$E[z_{j1}'z_2\|x]\theta- \theta_j E[z_{j1}{}^2\|x]$

Notes:
1. $E[z_{j1}'z_2|x_t]\theta > 0$ iff $\theta_j > 0$.

TABLE A2.1: Variable Means and Standard Deviations.

Variable	Mean	Std. Dev.	Range	Explanation
rpay1	8.25	2.46	2.01, 25.7	Hourly wage rate in pre-displacement job, deflated by the CPI (June 1981 = 100)
rpay2	7.69	2.72	2.22, 23.1	Hourly wage rate in post-displacement job, deflated by the CPI (June 1981 = 100)
rpayloss	1.07	2.39	-14.6, 14.8	rpay1-rpay2
lprew	2.07	0.3	0.70, 3.25	log of rpay1
lposw	1.98	0.36	0.80, 3.14	log of rpay2
lwdiff	-0.15	0.32	-1.28, 1.28	lposw-lprew
dage3	0.24	0.43	1	respondent aged 25 to 34
dage4	0.23	0.42	1	respondent aged 35 to 44
dage5	0.24	0.43	1	respondent aged 45 to 54
dage6	0.18	0.38	1	respondent aged 55 to 64
educ1	0.36	0.48	1	1 if some secondary or high school
educ2	0.21	0.41	1	1 if completed secondary or high school
educ3	0.14	0.34	1	1 if at least some college/university
training	0.25	0.43	1	1 if some (other) formal technical training
tenure	11.8	9.1	0.17,42	years in pre-displacement job
tensq				tenure squared
female	0.34	0.47	1	1 if female
married	0.77	0.42	1	1 if married
femmar	0.25	0.43	1	1 if married and female
man1	0.04	0.19	1	1 if pre-displacement job in managerial occupation
pro1	0.04	0.2	1	1 if pre-displacement job in professional occupation
wcsup1	0.01	0.11	1	1 if pre-displacement job in white collar supervisory occupation
whcol1	0.14	0.35	1	1 if pre-displacement job in white collar occupation
bcsup1	0.04	0.2	1	1 if pre-displacement job in blue collar supervisory occupation
preunion	0.7	0.46	1	1 if (self reported) pre-displacement job unionised
man2	0.04	0.19	1	1 if post-displacement job in managerial occupation
pro2	0.03	0.18	1	1 if post-displacement job in professional occupation

TABLE A2.1: Variable Means and Standard Deviations.				
Variable	Mean	Std. Dev.	Range	Explanation
wcsup2	0.01	0.1	1	1 if post-displacement job in white collar supervisory occupation
whcol2	0.15	0.35	1	1 if post-displacement job in white collar occupation
bcsup2	0.02	0.13	1	1if post-displacement job in blue collar supervisory occupation
posunion	0.23	0.42	1	1 if (self reported) post-displacement job unionised
intell1	2.53	0.62	2, 5	general intelligence required in pre-displacement job
spatial1	2.24	0.52	1, 5	spatial reasoning required in pre-displacement job
strnght1	3.01	0.5	1, 5	physical strength required in pre-displacement job
manldx1	2.51	0.5	2, 4	manual dexterity required in pre-displacement job
risk1	0.41	0.49	1	1 if injury risk in pre-displacement job
cold1	0	0.03	1	1 if exposure to cold in pre-displacement job
heat1	0.11	0.31	1	1 if exposure to heat in pre-displacement job
noise1	0.58	0.49	1	1 if exposure to noise in pre-displacement job
air1	0.33	0.47	1	1 if exposure to poor air quality in pre-displacement job
logpop	4.22	1.81	0.69,7.66	Log(population) of location of plant
unemp	7.8	3.01	3.5, 15.3	Unemployment rate in location of plant
lunemp	1.99	0.35	1.25,2.73	log unemployment rate
dreg3	0.09	0.28	0, 1	1 if pre-displacement firm located in Toronto
fsize	1111	1025	34, 4800	employees of pre-displacement firm in Canada, prior to layoff
lfsize	6.53	1.09	3.52, 8.48	log fsize
outtime2	20.4	5.2	1034	Elapsed time since layoff announcement, calculated from individually reported information

TABLE A2.2: Firms By Industry

Firm, location, (Valid Obs.), [employees in Canada]	4 Digit Industry	2 Digit Industry (SIC "major group")	Sector (SIC "division")
Food1, Hamilton (54) [1700]	1070 - Bakery Products Industries	10 - Food and Beverages	Manufacturing (100-399)
Food2, Toronto (38) [1000]	1089 - Miscellaneous Food Processors		
Rbbr1, Whitby (371) [1314]	1623 - Tire and Tube Manufacturers	16 - Rubber Products	
Lthr1, Kitchener (45) [308]	1740 - Shoe Factories	17 - Leather Products	
Txtl1, Cambridge (201) [2800]	1851 - Fibre Processing Mills	18 - Textiles	
Txtl2, Lindsay (177) [280]	1860 - Carpet, Mat and Rug Industry		
Txtl3, Cornwall (53) [899]			
Txtl4, Belleville (39) [150]			
Clth1, Carleton Place (49) [300]	2499 - Miscellaneous Clothing Industries	24 - Clothing	
Papr1, Toronto (35) [191]	2730 - Paper Box and Bag Manufacturers	27 - Paper	
Prnt1, Stratford (40) [975]	2890 - Publishing and Printing	28 - Printing	
Mchn1., Ingesoll (19) [225]	3150 - Miscellaneous Machinery and Equipment Manufactures	31 - Machinery	

TABLE A2.2: Firms By Industry

Firm, location, (Valid Obs.), [employees in Canada]	4 Digit Industry	2 Digit Industry (SIC "major group")	Sector (SIC "division")
Vhcl1, Toronto (19) [34]	3250 - Motor Vehicle Parts and Accessories Manufacturers	32 - Transportation Equipment	
Vhcl2, Parry Sound (42) [2500]			
Vhcl3, Windsor (177) [350]			
Vhcl4, Deseronto (60) [350]	3290 - Miscellaneous Vehicle Manufacturers		
Elec1, Toronto (41) [4800]	3391 -Battery Manufacturers	33 - Electrical Products	
Elec2, Missauga (85) [800]	3399- manufacturers of Miscellaneous Electrical Products		
Chem1, Paris (30) [142]	3740 - Manufacturers of Pharmaceuticals and Medicines	37 - Chemical Products	
Insr1, Toronto (27) [?]	7210 - Insurance Carriers	72 -	Finance, Insurance, Real Estate (700-799)
Motl1, Toronto (37) [64]	8810 - Hotels and Motels	88 -	Community Business and Personal Services (800-899)

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