DISPLACED WORKERS

THREE MICROECONOMETRIC STUDIES OF DISPLACED WORKERS.

By

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ABSTRACT

This thesis comprises three essays. The first two essays examine what inferences can be drawn about the structure of wages from the experiences of displaced workers using the Ontario Ministry of Labour Plant Closure Survey. The third essay examines the effect of unemployment benefits on household consumption during spells of unemployment, with a particular emphasis on durables purchases. It employs data from a second and new data source, the Canadian Out of Employment Panel.

The first essay revisits the issue of what can be learned about wage tenure profiles from displaced worker data. The positive relationship between wages and tenure in cross section data is consistent with the accumulation of firm specific capital. Alternatively, it may be explained by unobserved heterogeniety across workers, or by endogenous mobility. Displaced worker data is quite helpful in correcting for the first possible bias, and less so for the second. The relationship between various estimation strategies in the literature is illustrated. Estimates that control for individual heterogeniety and endogenous mobility driven by systematic differences in the pay policies of firms are presented. In this data, 10 years of tenure appears to raise wages by about 7%.

The second essay examines *intra*-industry wage differentials. Even after conditioning on a rich set of worker and job characteristics, firm of employment is a significant determinant of wages. Estimates that employ the longitudinal nature of data demonstrate that sorting of workers across firms by unobserved ability can explain about half of the observed differentials. Firm wage differentials are observed within narrow industries, consistent across broad occupational groups, and 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. These observations imply that compensating wage differentials are also a poor candidate explanation for the observed differentials. The results are more consistent with models based on rents or some firm monopsony power. The results also raise questions about the interpretation of wage regressions which ignore firm heterogeneity, and about the sources of wages losses among displaced workers.

The final essay examines how households smooth consumption over the income losses due to an unemployment spell. A model of "internal capital markets" is proposed, which suggests that households adjust the timing of the replacement of small durables to income flows. The plausibility of this model is investigated empirically, using a series of program changes in the Canadian unemployment insurance scheme for exogenous variation in transitory income. The data are consistent with the predictions of the "internal capital markets model" while rejecting both a standard life cycle model and a "rule of thumb" model of household expenditure patterns.

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PREFACE

The third chapter, `Shocks, Stocks and Socks: Consumption Smoothing and the Replacement of Durables During an unemployment Spell' was jointly authored by Professor Martin Browning for the purpose of publication. I carried out the empirical work. Professor Browning conducted the simulations and is responsible for the proof in the appendix.

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INTRODUCTION

Displaced Workers are those workers who lose their job as a consequence of a major reduction in the employment levels at their work place. The experiences of such workers are of interest for at least two reasons. First, such workers are an object of independent policy interest. They bear the costs of adjustment in a dynamic economy. While the processes of labour reallocation between dying and growing industries and from poorly managed firms to well managed firms may be, on efficiency grounds, desirable, it seems obvious that costs of such adjustment are inequitably distributed. Equity considerations dictate that we should monitor the plight of the less fortunate, and provide compensation and adjustment assistance where possible.

The second reason that displaced workers are of interest is the insight their experiences can provide regarding the operation of labour and other markets and the behaviour of economic agents. The event of separation from an employment relationship amounts to a sharp change in the circumstances of a worker. The idea is to learn from observing subsequent behaviour and outcomes. Furthermore, if displacement is a consequence of broad economic changes and unrelated to characteristics of individual workers, then this change in circumstances approximates a "natural experiment" in which a plausibly random group of workers experiences a "treatment".

There has been considerable work documenting the experiences of displaced workers . The essays comprising this thesis are more directly motivated by second consideration outlined above; that is, they attempt to draw inferences about labour markets, credit markets and household behaviour from the experiences of displaced workers. The first two essays examine what inferences can be drawn about the structure and determinates of wages from the experiences of displaced workers. The data employed are from an Ontario Ministry of Labour Plant Closure Survey. The third essay examines the effect of unemployment benefits on household consumption during spells of unemployment, with a particular emphasis on durables purchases. It employs data from a new data source, the Canadian Out of employment Panel.

A central question in labour economics is whether wages rise with tenure or seniority at specific firm.

Such a relationship is the prediction of several important models including models in which workers acquire over time with a firm renumerable skills which are valuable only to that firm (specific human capital). It is well known that in data in which individual workers are observed only once, those with higher tenures in fact report higher wages. However, this does not demonstrate that wages rise with seniority. Rather the observed pattern may result if more productive (and hence higher paid) workers are also more stable. The pattern is also generated by a variety of models in which workers search for better wages while employed. In this case the apparent association between tenure and wages is not indicative of a causal relationship. Rather, workers observed with long tenures are, on average, those who found a high paying job. The evidence reported in the first chapter of this thesis suggests that the first alternative explanation is not empirically relevant, but that the second is. The true economic value of additional years of seniority appears to be quite small.

The second essay empirically assesses the idea that there are "high" and "low" wage firms. Simple, competitive models of the labour market suggest a "law of one price": identical workers should receive the same wage regardless of where they are employed. There exists evidence that apparently similar workers in fact make quite different wages. However, this might be explained if those workers differ in characteristics that are not observable to the researcher, and workers sort into firms according to these characteristics. It might also be the case that compensation is mis-measured in a way that generates the observed differences across firms. Displaced worker data can provide important new insights into these questions. For example, if the apparent differences across firms are actually the result of unobserved worker characteristics, then the workers receiving apparently high wages should continue do so when they move to another firm as a consequence of displacement. The central result of the chapter is that firms do appear to matter.

The third essay examines how poor agents smooth consumption over income losses due to an unemployment spell. It seems unlikely that they have substantial liquid assets with which to do so, or either the ability or inclination to accumulate considerable debt. One possibility is that they adjust the timing of the replacement of small durables to the timing of income. Because old but serviceable durables continue to provide a flow of services, such a strategy minimizes the welfare cost of a cut in expenditures. This is termed an "internal capital markets" strategy because stocks of durables allow households to effectively borrow from themselves.

This hypothesis is supported by data from a survey of unemployed Canadians. In particular, marginal dollars of unemployment insurance benefits tend to increase clothing expenditures *even conditioning on total expenditures*. The same is not true of expenditures on a nondurable good, food at home.

While the essays in this thesis focus on using displaced worker data to test economic models rather than to describe the circumstances of those workers, the work does have important implications for economic policy in general and for policy with respect to displaced workers in particular. Understanding the sources of wage losses by displaced workers is crucial to designing policies intended to mitigate them. For example, if the losses are firm specific human capital, then policies which encourage retraining and investment in these workers by the firms to which they move are most sensible. On the other hand, if the losses are driven more by the loss of employment at a "good" firm or by the loss of a high quality work-firm match, then policy is more appropriately directed to supporting and assisting search activities by these workers. Similarly, an understanding of how households smooth the income fluctuations associated with the end of an employment relationship is crucial to the design of income support policies.

What Can We Learn From Displaced Worker Data About the Returns to Tenure?

1. Introduction.

In cross section data wages are positively correlated with job tenure, conditional on labour market experience. This well known and robust result has been taken by some authors as support for theories that predict that wages should grow with tenure, including firm specific capital and incentive theories. Extensive criticism of this interpretation has rested on two broad ideas. First, cross section tenures may be correlated with fixed and unobserved productivity differences across workers ("ability bias" - the more able are also inclined to more stable employment). Second, mobility (and hence tenure) may be endogenous (as suggested by models of search or matching in the labour market). Thus, cross section tenures may be correlated with unobserved productivity differences across worker-firm or worker - job matches (in the case of matching models) or with heterogeneity in firm pay strategies (in the case of general equilibrium search models)[1].

These criticisms have in turn spawned a literature that attempts to estimate a return to tenure, net of these biases. One branch of this literature has utilized data on displaced workers - workers who change jobs after being displaced from their jobs by a plant closing or major layoff.[2] Assuming that plant closings/major

[1]A third criticism is that specific capital might not be firm but rather industry specific. (Neal, 1995; Parent, 1997)

[2]Another strand of the literature has attempted to recover estimates of the wage-tenure profile from panel data. That literature has been hampered by the endogeneity of all mobility, difficulties in constructing accurate tenure measures and disagreement over the validity of proposed instruments. Altonji and Shakotko (1987) and Abraham and Farber (1987) report that the true returns to tenure are essentially negligible, but Topel (1991) subsequently finds that they are almost as large as suggested by the cross section estimate. Altonji and Williams (1992) indicate that the issue is not resolved.

layoffs are essentially unrelated to the characteristics of *individual* workers, displaced worker surveys may provide us with a sample of workers whom 1) are observed more than once (allowing one to control for individual heterogeneity) but 2) whose mobility is not correlated with unobserved personal characteristics (they do not systematically differ from workers not displaced) or with current or future job. Its important here to distinguish between the displaced workers mobility on dislocation - which one might consider exogenous - and their *past* mobility (or lack of mobility) which was certainly endogenous. That is, if one believes that the displacements are exogenous, then this is random sample of a cross section of workers (of various tenures) whom one happens to observe in a second job (in contrast, those workers who one observes switching jobs in any short panel are certainly a nonrandom sample of the workers in the panel). However, the displaced workers tenure at displacement is a function of past (im)mobility, and hence is potentially endogenous (correlated, for example, with the quality of the "match" in the pre displacement job).

Authors who have attempted to exploit this `natural control' (Kletzer 1989) to estimate the wage-tenure profile include Addison and Portugal (1989), Kletzer (1989), Ruhm (1990 and 1991),Topel (1990), Carrington, 1992, Crossley et al. (1994), Kuhn and Sweetman, (1994) , Neale (1995), and Houle and Van Audenrode (1995).[3] Despite the common direction of these papers, there are considerably differences in the way in which they attempt to use data on displaced workers to isolate components of the cross section wage-tenure profile. The contribution of this paper is to review and synthesize this line of research. Alternative identification strategies are evaluated, and empirical examples are provided on a single data set, to facilitate comparison.

A positive relationship between wages and seniority (or tenure), net of experience and conditional on both individual , firm and job match heterogeneity, is an important prediction of several models of wage dynamics, including firm specific human capital accumulation (Becker, 1975) and incentive contracts (Lazear,

[3] Another advantage of displaced worker surveys is that the tenure measure is not constructed (hence the problems discussed in Brown and Light (1995) are avoided.) Against this, a major disadvantage of displaced worker data is its retrospective nature. There is the possibility of recall bias.

1979). Furthermore, differences in the wage tenure profile across groups are an implication of models of gender wage differentials, (Crossley et. al., 1994, Hersh and Reagan, 1994) and trade union behaviour (Kuhn and Sweetman, 1994). Finally, the composition of the cross sectional returns to tenure has important implications for the costs of worker displacement. If these returns comprise largely (transferable) individual heterogeneity (ability) then the costs of displacement are much smaller than if the apparent returns to seniority represent largely (nontransferable) firm specific human capital or job match quality.

The principal results of this paper are: (1) It is possible to control for unobserved ability bias in estimating the returns to tenure with displaced worker data, and it is not necessary to assume constant returns to unobserved ability to do so. However, previous authors' who have attempted to relax this assumption have failed to use any of the additional information available in the data, and hence have simple converted one mis-specification bias into another. (2) In a richer econometric approach that utilizes all of the empirical second moments of the data, I find than any specification that imposes that the returns to observable characteristics are constant is strongly rejected by the data, and my preferred identifying assumption suggests that the returns to both observed and unobserved characteristics are greater in the post-displacement job. (3) Unobserved ability bias does not seem to be a serious problem in the data I employ. Controlling for unobserved ability bias results in estimates of the returns to tenure that are just slightly smaller than the cross section estimates, and direct estimates of the correlation between tenure and latent earnings ability are negligible. (4) the sign of "match" or endogenous mobility bias is ambiguous. In the simplest model it is fact negative (the cross section regression will *under*state the true returns to tenure) but if one allows for past involuntary separations (which consistency seems to require when using displaced worker data) then it is possible that this bias is reversed. (5) None of the estimation strategies used by other authors can adequately deal with "match" bias. In fact, the only plausible way of doing so is to condition on the source of heterogeneity in wage offers. (6) Finally, my preferred estimates correct for both unobserved heterogeneity in earnings ability and mobility propensities and endogenous mobility induced by heterogeneity in firm pay policies, and do not constrain the returns to either observed or unobserved worker characteristics to be the same in pre- and post-displacement jobs (allowing for possible learning and insurance effects). They do not however, control for endogenous mobility induced by idiosyncratic firm-worker matches. The estimated returns to tenure statistically significant but are just less than half of OLS cross section estimates at about 7% per ten years.

The organization of the rest of the paper is as follows: Section 2 describes a unique displaced worker data set - the Ontario Ministry of Labour Plant Closure Survey. Section 3 discusses unobserved ability bias and alternative solutions based on displaced worker data. Section 4 takes up endogenous mobility bias, presenting a very simple model of endogenous mobility and discussing possible identification strategies. Both sections 3 and 4 present the implementation of various estimation strategies on my data. Finally, Section 5 concludes.

2. The Ontario Ministry of Labour Plant Closure Survey

The data employed in this paper come from an Ontario Ministry of Labour Plant Closure Survey (OML PCS). In Ontario (Canada's most populous province) firms are required to notify the Ministry of La bour of layoffs and plant closures affecting more than 50 workers. The Ministry used the resulting registry to identify a representative sample of plant closures and mass layoffs which occurred during 1982. Workers at the 21 selected firms were identified from company personal records and surveyed. Because the sample frame was constructed from personnel records provided by the firms, each worker can be matched to their pre-displacement firm. The survey collected information on the standard demographics and human capital measures and hourly[4] wages both in the pre-displacement job and in re-employment. Each worker 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. All of the workers are victims of *mass* layoffs and

[4]Especially important given Topel's (1990) finding that pre-displacement tenure predicts hours changes on displacement.

most were involved in complete plant closures[5]. This strengthens the contention that job loss can be considered to be orthogonal to the characteristics of individual workers. It is possible to control for a wide variety of firm policies that may affect the quality of post-displacement jobs, such as the amount of notice given to workers, severance pay, and other kinds of firm assistance such as arranging interviews with other firms. Complete details of the OML PCS are available elsewhere; see Crossley et. al. (1994), Jones and Kuhn (1995), and Ontario Ministry of Labour (1983).

For my empirical work, I focus exclusively on the men, since I am forced to proxy experience with age and education. Tables 2.1 and 2.2 present the profile of (pre-displacement) wages and wage losses by tenure in this data. Both are increasing. The same profiles are presented in Figures 1 and 2, with smoothed median bands. Both relationships appear to be concave. There are outliers in both the marginal distributions of wages and wage losses. However, these data points occur in the middle of the (marginal) tenure distribution, so they are unlikely to be influential[6].

Table 2.3 presents summary statistics for the variables of interest in the OML PCS. The individuals in this data set are similar to those in the US DWS in most ways. They tend to be older, predominantly blue collar, and less educated than a sample of all workers. However, they differ from typical US displaced workers in two ways. First, they have a much higher rate of unionization (reflecting the much higher overall rate of unionization in Canada relative to the US). Second, they tend to have had considerably more advanced notice of layoff. The OML data set has substantial information on advance notice, which has been investigated elsewhere (Jones and Kuhn, 1995).

In Column 1 of Table 2.4, I present a basic cross-section estimate of the returns to tenure from this data. The dependent variable is the logarithm of real hourly wages on the pre-displacement job. The tenure terms are significant, and imply that 10 years of tenure raise wages by about 16.5%. By way of comparison,

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[5] Note that this information comes from administrative records: the reason for layoff is not self reported.

[6] Nonetheless, I present formal tests for influential observations when I move to estimation of the wage-tenure profile.

Neal, (1995) reports a cross section estimate of the returns to tenure for men using the 1984 -1990 waves of the CPS Displaced Worker Supplement. His estimates imply a somewhat higher return to 10 years of tenure of almost 26%. Houle and Van Audenrode (1995) report a cross section return to 10 years of tenure of 15% in the 1986 Canadian Displaced Worker Survey, which was modelled on the CPS Displaced Worker Supplement.

Specification tests for the OML PCS cross section regression are also presented in column 1 of Table 2.5. The first two rows present, for the linear and quadratic tenure terms respectively, the minimum and maximum "Dfbeta" values for observations in the sample. These statistics indicated the change in the coefficient of interest that would result from removing the observation from the sample, reported in standard deviations of the coefficient. There are no particularly influential observations. The third row presents a test for heteroscedasticity. The null of homoscedasticity is strongly rejected. Consequently, I employ White's heteroscedasticity consistent estimate of the covariance matrix throughout (White, 1980). Finally, the fourth row presents a RESET test for omitted variables[7]. I cannot reject the null that the regression function is correctly specified.

In the OML PCS, the survey occurred no more than 2 years after a worker's displacement. Consequently, some workers remained out of work at the time of the survey. Thus any use of post-displacement wage information will suffer from sample selection into re-employment. The remaining columns of Table 2.5 investigate this issue. Before turning to these columns, I note that sample selection is not a concern if the selection into re-employment is (conditional on co-variates) unrelated to tenure. Column 1 of Table A1 in the appendix presents estimates of a probit model of re-employment. Conditional on other characteristics, tenure is a significant predictor of re-employment by the survey date.

Returning to Table 2.5, column 2 repeats the cross section pre-displacement wage regression augmenting the co-variates with a dummy indicating re-employment at the survey date. This dummy is

[7] Essentially I am testing the exclusion of higher order and interaction terms of the included co-variates.

statistically significant, and indicates that, after controlling for observables, the sample of individuals who are subsequently re-employed by the survey date have 6% greater earnings power on average.

The next three columns of Table 2.5 present the cross section wage regression estimated on the re-employed sample. Column 3 presents OLS estimates. The estimates in column 4 employ a two step sample selection correction (Heckman, 1979), and column 5 presents maximum likelihood estimates of the sample selection model. The first stage of the 2 step sample selection model is the probit reported in column 1 of appendix Table A1, while the maximum likelihood estimates of the parameters the selection equation are presented in column 2 of the same table. It is well known that identification in these sample selection models hinges on having an instrument for selection. I employ the variation in the time between when the worker first received notice to the survey date. In a search framework, this is a valid instrument (can be excluded from the wage equation) if the worker's reservation wage is constant (the search environment is stationary). This is a strong assumption.

Despite the evidence presented here of significant sample selection, I note that the coefficients of column 1 and column 3 are almost identical; the sample selection does not appear to bias the coefficients of interest. Further, most of the estimators I subsequently employ explicitly correct for unobserved heterogeneity in earnings power across workers.

3. Unobserved Ability Bias.

Other authors have motivated their concern for heterogeneity bias in estimating the returns to tenure with a wage equation such as:

$$w_{it} = X_{it}\beta + \alpha(T_{it}) + Z_i + \epsilon_{ti} \tag{1}$$

w is log real wages for individual i at time t. T is tenure. X represents other (observable) individual (and possibly job) characteristics (experience, education). The error term has two components. Z is an unobservable, individual specific (ie., job transferable), market valued characteristic: "ability". Secondly, ϵ is a person, job and time specific random disturbance. Each component of the error term is assumed to be mean zero. For expositional simplicity, I present function $\alpha(T)$ as linear in the sequel; I also drop the index of individuals, i.

It is common to assume that the error components are uncorrelated with each other and with X. However, tenure is thought to be correlated with ability (Z) conditional on X. (workers who are, unobserved to the econometrician, more productive, may also be more stable; this leads them to have both higher wages and longer tenures). Equation 1 reflects a "pure human capital" model of wages; wages are determined solely by individual characteristics, but not all of those are observed (by the econometrician). To fix ideas, consider the OLS regression of log wages on tenure in a world described by equation 1. Since:

$$E(TZ|X)\neq 0 \tag{2}$$

$$a^{OLS}=\frac{cov(WT|X)}{var(T|X)}=\frac{\alpha var(T|X)+cov(TZ|X)}{var(T|X)}=\alpha+\frac{cov(TZ|X)}{var(T|X)} \tag{3}$$

Thus the cross-sectional OLS estimate of the wage-tenure profile is upward biased in the presence of unobserved ability which is positively correlated with tenure.

The problem of "bias" in the cross section regression of wages on tenure is really one of identification. The OLS cross section regression provides unbiased estimates of the parameters of a conditional expectation function (CEF) - the expectation of log wages conditional on the Xs but not on Z. Unfortunately this is not the CEF of interest. α is a parameter of a second CEF: that which describes the expectation of wages conditional on X and Z . The parameters of the first CEF (which can be unbiasedly estimated) are mixtures of those of the second (which cannot); unfortunately, the parameters of the first CEF do not imply unique values for the parameters of interest. `Bias' resulting from errors in variables, omitted variables and simultaneity can all be interpreted as a under-identification of parameters of interest.

To facilitate thinking about the problem at hand as one of identification, I augment the framework of equation 1 as follows:

$$w_1 = X_0\beta_{10} + X_1\beta_{11} + \alpha T_1 + \gamma_1 Z + \epsilon_1 \tag{4}$$

and

$$w_2 = X_0\beta_{20} + X_2\beta_{21} + \gamma_2 Z + \epsilon_2 \tag{5}$$

Where w_1 is the wage in the pre-displacement job and w_2 is the (initial) wage in the re-employment job. The indices on X designate time invariant characteristics (X_0) or the realization of time varying characteristics in pre- and post displacement jobs (X_1 and X_2 respectively).

Note that there is now a coefficient on unobserved ability and this coefficient varies across jobs. This reflects the concern of some authors (Addison and Portugal, 1989, Houle and Van Audenrode 1995) that the returns to unobserved ability may vary across jobs (in particular, $\gamma_1>\gamma_2$, reflecting the fact that pre-displacement firms have better information about the workers unobserved abilities).

The returns to observable characteristics are also indexed. It may be more tempting to constrain the returns to observable characteristics to be constant across jobs; presumably anything observed by the econometrician is also observable by all firms and hence has a (constant) market return. However, an obvious consequence of such a specification is that fixed characteristics do not appear in a wage loss equation. This is inconsistent with much of the literature (Crossley et al, 1994, Topel 1990). It is also a testable restriction, a point I return to below.

Following Chamberlain (1982) I also specify an auxiliary equation capturing the correlation between the fixed effect (unobserved ability) and the variables of interest:

$$Z_i = X_{0i}\theta_0 + X_{1i}\theta_1 + X_{2i}\theta_2 + \psi T_{1i} + u_i \tag{6}$$

This allows us to rewrite the cross section return to tenure (equation (3)) as

$$a^{OLS} = \alpha + \gamma_1 \psi \tag{7}$$

The implied relationships between observables can be captured by two reduced form wage equations:

$$W_1 = X_0 \pi_{10} + X_1 \pi_{11} + X_2 \pi_{12} + \phi_1 T_1 + e_1 \tag{8}$$

$$W_2 = X_0 \pi_{20} + X_1 \pi_{21} + X_2 \pi_{22} + \phi_2 T_1 + e_2 \tag{9}$$

Where for example:

$$\pi_{11} = \beta_{11} + \gamma_1 \theta_1 \tag{10}$$

A simple and common approach to unobserved individual heterogeneity when multiple observations are available on each individual is "first differencing" . Several authors including Topel (1990) and Crossley, et al. (1994) estimate equations for the difference in pre- and post- displacement wages. Consider subtracting equation 4 from equation 5:

$$w_2 - w_1 = X_0(\beta_{20} - \beta_{10}) + X_2\beta_{22} - X_1\beta_{21} - T_1\alpha + (\gamma_2 - \gamma_1)z + (\epsilon_2 - \epsilon_1) \tag{11}$$

The estimation of this equation will identify α if the return to ability is the same in pre- and post displacement jobs ($\gamma_2 = \gamma_1$). This is a non-trivial restriction. It has been pointed out in the context of estimating industry wage differentials by Gibbons and Katz (1992), in the context of estimating union wage differentials by Stewart (1993) and in the current context by Addison and Portugal (1989) and Houle and Van Audenrode (1995). In this context the returns to ability might differ systematically between pre- and post- displacement jobs if, for example, firms insure workers against ability differences that become apparent during employment (as in the model of Harris and Holmstrom, 1982). Consider the implications of such a model for displaced workers. When a worker involuntarily enters the market after displacement he/she can no longer insure against ability that was revealed in the pre-displacement job. Thus ability may have a larger impact on wages in the post displacement job. This has lead some authors to reject the first difference estimates and turn to another approach: using one observed wage as a proxy for unobserved ability in estimation of the determinants of the other wage. This method, which is borrowed from the applied job search literature, has been employed by, for example, Addison and Portugal, (1989), Ruhm (1991), and Houle and Van Audenrode (1995). Consider estimating an equation of the form[8]:

$$w_1 = X_0\pi_0 + X_1\pi_1 + X_2\pi_2 + \psi T + \eta w_2 + e. \tag{12}$$

Manipulating Equation 2:

$$Z = (1/\gamma_2)(w_2 - X_0\beta_{20} - X_2\beta_{21} - \epsilon_2) \tag{13}$$

and therefore

$$w_1 = X_0(\beta_{10} - \gamma_1/\gamma_2\beta_{20}) + X_1\beta_1 - (\gamma_1/\gamma_2)X_2\beta_2 + T_1\alpha + (\gamma_1/\gamma_2)w_2 + (\epsilon_1 - (\gamma_1/\gamma_2)\epsilon_2) \tag{14}$$

This approach does not yield identification of α. Note that W_2 is **not** exogenous but is correlated with the error term (in particular with ϵ_2). Essentially all that has been accomplished is to convert omitted variable bias into simultaneous equations bias. The equations for the conditional expectations of pre- and post-

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[8] In fact those authors proceed in the reverse manner, conditioning a post-displacement wage equation (including post-displacement tenure) on pre-displacement wages. Because in the rest of my discussion I are considering *initial* wages post- displacement (so that post-displacement tenure is zero) I have reversed the approach for continuity of exposition, conditioning a pre-displacement wage equation on post-displacement wages. The point I are making is independent of this change.

displacement wages are both unidentified. Thus only combinations of transformations and assumptions (such as differencing and $\gamma_2 = \gamma_1$) lead to identified specifications. Proponents of this approach claim that the coefficient of w_2 is an estimate of γ_1/γ_2, the ratio of the returns to ability pre- and post- displacement (and hence reveals something about information in the labour market). However, it is a biased estimate.

Restricting $\eta = 1$ in equation (12) (as Addison and Portugal, 1989, and Houle and Van Audenrode, 1995, do in some specifications) is equivalent to first differencing and imposes the restriction under which regressing the wage difference on tenure provides an identified wage return to tenure. However, note that neither the authors employing this approach, nor the authors who simply first difference are using all the information in the data; in particular, they are not using the covariances between period i wages and period j Xs. Chamberlain (1982) points out that the standard fixed effects specification is over-identified. To see that here note that, assuming $\gamma_2 = \gamma_1$ and $\beta_{11}=\beta_{21}=\beta_1$:

$$\pi_{11} - \pi_{21} = \beta_1 = \pi_{22} - \pi_{12} \tag{15}$$

Since all four reduced form coefficients in the above expression can be estimated, the restriction is testable (see for example, Angrist and Newey, 1991). Should the fixed effects specification be rejected, I can impose the trivial normalization $\gamma_1 = 1$, and estimate γ_2 consistently by replacing the reduced form coefficients in the next equation by their estimates and solving for γ_2 :

$$\pi_{22} = \pi_{11} - \pi_{12}/\gamma_2 + \pi_{12}\gamma_2 \tag{16}$$

One might think of this as "quasi- differencing". Note that it is the assumption that the returns to observed characteristics are constant across jobs that is providing the identification here. Alternatively, I could assume that the returns to unobserved characteristics are constant across jobs and allow the returns to observed characteristics to vary. This seems a less attractive set of assumptions. There are other assumptions that identify (or over identify) the parameter of interest. For example, consider assuming that the disturbance variances in the two wage equations are the same ($\sigma_{11}=\sigma_{22}$). This restriction might be reasonable if, for example, the person-job-time specific disturbances (ϵ_{it}) are pure measurement error, and the two wages are measured equally well. There is no convenient explicit expression for the estimator of α; it is the implicit solution to a system of nonlinear equations relating the parameters to estimable moments. If I assume that the returns to all characteristics vary across jobs this restriction just identifies the parameters of interest. If the returns to observable characteristics are constant this restriction is testable. Here I am identifying off a restriction on the variance-covariance martrix of (reduced form) disturbances. Note that since this restriction and first differencing both just over identify the model, there is a one to one mapping from one to the other: $\sigma_{11}=\sigma_{22}$ implies a unique value of γ_2/γ_1 and $\gamma_2 = \gamma_1$ implies a unique value of σ_{11}/σ_{22} (thus I cannot test one against the other, they must both fit the data equally well). They will however, give different estimates of α and are in a sense polar opposite assumptions: If I estimate the reduced form wage equations, assuming $\gamma_2 = \gamma_1$ attributes all the difference in the residual variances to differences in measurement error across jobs, while assuming $\sigma_{11}=\sigma_{22}$ attributes all the difference in the residual variances to differences across jobs in the returns to unobserved ability. Thus it is informative to compare the estimates of α.

A third approach in the literature is to include pre-displacement tenure in a post-displacement wage equation. The argument is that firm specific human capital is not transferable across jobs (firms). Therefore, if pre-displacement tenure affects post-displacement wages, it must be proxying for an individual fixed effect - ability. This approach was first suggested by Kletzer (1989) amd is also employed by Ruhm (1991), Crossley et al. (1994) and others. The estimating equation is of the form:

$$w_2 = X_0\pi_0 + X_2\pi_2 + aT + e_2 \tag{17}$$

Substituting equation (6) into equation (5):

$$w_2 = (\beta_{20}+\gamma_2\theta_0)X_0+(\gamma_2\theta_1)X_1+(\beta_{21}+\gamma_2\theta_2)X_2 + (\gamma_2\psi)T + (\epsilon_2+\gamma_2 u_i) \tag{18}$$

First note that (17) is mis-specified by the exclusion of elements of X_1 not included in X_2.

Note also that:

$$\frac{cov(W_2 T_1|X_2)}{var(T_1|X_2)} = \gamma_2\psi \tag{19}$$

Clearly this should be zero if tenure is uncorrelated with ability. However, because it is product of two terms it could would also be small if the return to unobserved characteristics in the post - displacement job is small. Again, various asymmetric information models of the labour market predict exactly that. Comparing to equation (7) note that the coefficient on pre-displacement tenure in the post-displacement wage regression is an estimate of the bias in the returns to tenure estimated by the cross section pre-displacement wage equation only if $\gamma_2 = \gamma_1$. Thus the alternative approaches employed in the literature all hinge on the identifying assumption implicit in first difference (wage loss) regressions; *none really provides additional insight as all are based on the same combination of empirical moments.*

I now illustrate the different estimation strategies presented in this section using the OML PCS data. Table 3.1 provides return-to-tenure estimates using the standard approaches in the literature. Column 1 presents the cross section OLS estimates, for comparison purposes. In the second column of Table 3.1, I present the results of augmenting the pre-displacement wage equation with the post-displacement wage as a control for unobserved differences in earnings ability. As illustrated above, this is a mis-specified regression. Nonetheless, it provides estimates of the returns to tenure that are very similar to the cross section estimates.

The third column presents the results of adding pre-displacement tenure to the post-displacement wage equation. Again, if tenure is significantly correlated with unobserved ability, and if there are significant returns to unobserved ability in the post displacement job, tenure should be significant here. In fact, pre-displacement tenure is not a significant determinant of post-displacement wages. If I subtract the post -

displacement return from the pre-displacement return as an approximate correction for ability bias, the return to 10 years of tenure is reduced from 16.5 % to 14.1 %. These estimates suggest that the cross section wage tenure relationship observed in the OML PCS data is not an artifact of unobserved ability differences across workers. Ruhm (1990) compares the pre- and post-displacement wage returns to pre-displacement tenure to get estimates of the fraction of the pre-displacement return to tenure which is attributable to unobserved ability in a sample of male workers from the PSID. Noting that post-displacement wage returns to pre-displacement tenure are almost as large as the pre-displacement returns, he concludes that individual fixed effects are more important than firm specific capital. Kletzer (1989) and Addison and Portugal (1989) report that in samples drawn from the Displaced Worker Supplement to the CPS about half of the (pre-displacement) return to pre-displacement tenure is transferable to the post-displacement job. Finally, Houle and Van Audenrode (1995) report that pre-displacement tenure has a negligible effect on post-displacement wages in the Canadian Displaced Worker Survey. Individual fixed effects are consistently unimportant in Canada, but seem to explain one half or more of the cross section tenure return in the U.S. Recall however (from Section 2) that the cross section return to tenure in Canada appears to be about two thirds of that reported in the U.S.
An interesting further line of research would be to see if these differences could be attributed to institutional differences between the countries, such as the substantial difference in union coverage.

The results found in the fourth column , which present estimates of the returns to tenure from a wage loss equation, are similar to those in the second and third column, as the preliminary discussion suggested they should be. Here the estimate 10 year returns are 13%. It should be noted that this is not a true "first difference" estimate; following the usual practice in the literature, fixed individual characteristics are included in the wage loss equation. This issue is among those investigated in the next Table (Table 3.2). Topel (1990) finds that wage losses rise with tenure in a sample drawn from the CPS Displaced Worker Supplement but not in a sample drawn from the PSID. This is exactly what one should expect given the results of Kletzer (1989), Addison and Portugal (1990) and Ruhm (1990) presented above. Neal (1995) estimates separate wage loss regressions for men and women in the CPS DWS and finds that womens' wage losses rise faster with tenure than mens'[9]. This replicates a result reported by Crossley et al. (1994) for the OML PCS.

Table 3.2 presents "quasi-differencing" estimates of the returns to tenure. To review the procedure: I specify regression functions for pre- and post- displacement wages and latent earnings ability, I estimate reduced form equations for pre- and post-displacement wages, I identify the model with one of several identifying restrictions, and finally, I recover the parameters of the wage and earnings ability regression functions by minimum distance. The reduced form estimates are presented in Table A2 of the appendix.

The first identifying restriction I consider is that the returns to unobserved ability are the same across jobs. These estimates, in column 1, reflect the first difference specification. Thus, the coefficients on tenure and tenure squared (rows 1 and 2), the test for their exclusion (row 4) and the return to 10 years of tenure (row 3) match the numbers in column 4 of Table 3.1. Because I explicitly model the relationship between latent earnings ability and the observables, I can *explicitly* test the null hypothesis that the tenure terms are not related to unobserved ability. The results of this test are presented in row (6); the null is not rejected. This is consistent with the results presented in column 3 of Table 3.2. Finally, the returns to observed individual characteristics are not constrained in this specification to be the same pre- and post-displacement. To summarize the differences, I calculate the scalar quantity $(1/n)*(b_2-b_1)'X_1'X_1(b_2-b_1)$, where n is the number of observations, b_2 is the vector of estimated post displacement returns, b_1 is the vector of estimated pre-displacement returns, and X_1 is the matrix of observable characteristics (pre-displacement). This quantity is presented in row (9) and is positive, indicating that the returns to observed characteristics are greater in the post-displacement job[10].

If the returns to unobserved characteristics are constant across jobs, it seems reasonable to assume that the returns to observable characteristics are also constant. Because this additional assumption over-identifies the model, it can be tested; the results are presented in column (5). The data overwhelmingly reject

[9] Neal also divides his sample by industry switcher and stayer. The gender difference is consistent across this split.

[10] An approximate scale for this quantity can be taken from comparison with the raw variance of log pre-displacement wages, which is 0.06.

this restriction. Column (3) reports the results of constraining the returns to observed characteristics to be constant, while allowing the returns to unobserved characteristics to vary. This might seem a more attractive restriction; presumably characteristics observed by the econometrician are also observed by both firms, while the pre-displacement firm may have better information about unobserved characteristics than the post-displacement firm (and the econometrician). However, note that this model is also over-identified (as long as there is more than one observable characteristic) and strongly rejected by the data.

As noted above, a number of authors have been concerned with the assumption of constant returns to unobserved ability. Such an assumption seems even less tenable given the that the data rejects so strongly the constancy of returns to observable characteristics. Accordingly, I now turn to my alternative identifying restriction: that the disturbance variances in the wage equations are constant (quasi-fixed effects). The results of imposing this restriction are presented in column 2 of Table 3.2. The returns to tenure under this assumption are statistically significant and almost as large as the cross section estimate (14.2% per 10 years). As in the fixed effects specification, the relationship between tenure an latent earnings ability is statistically insignificant. In rows (8) and (9) I note that the estimates imply that the returns to both unobserved and observed characteristics are greater in the post displacement job. The interpretation of this result is not immediately obvious. In a learning model, the returns to observed characteristics should be higher post displacement, but the returns to unobserved characteristics lower. In an insurance model, such as Harris and Holmstrom (1982), the returns to unobserved characteristics should be higher post displacement, but its not clear why the returns to observed characteristics should increase.

The specifications in columns (1) and (2) represent polar assumptions. Column (1) attributes all the (pre- versus post - displacement) difference in the reduced form error variance to differences in the structural disturbance term. On the other hand, column (2) attributes all of the difference to differences in the returns to unobserved ability. Since both assumptions just identify the model, there is a one to one mapping from one specification to the other. Because the data so overwhelmingly rejects the (over-identified) assumption that the returns to observable characteristics are constant, I am inclined to prefer the specification of column (2).

However, I note that estimates of the returns to tenure are not particularly sensitive to my choice between these extreme assumptions (13% per ten years versus 14.2%).

Columns 5 combines the constant disturbance restriction with constant returns to observed characteristics. This is of course strongly rejected by the data. Columns 6 reports the result of simultaneously imposing the polar assumptions of columns (1) and (2) simultaneously. The over-identification test in row (7) indicates that these assumptions are not jointly compatible with the data (the same result is contained in the t-test of column 2, row (8)).

The major findings of this section are then, as follows: It is possible to control for unobserved ability bias in estimating the returns to tenure with displaced worker data. It is not necessary to assume constant returns to unobserved ability to do so. However, previous authors who have attempted to relax this assumption have failed to use any of the additional information available in the data, and hence have simply converted one mis-specification bias into another. In a richer econometric approach that utilizes all of the empirical second moments of the data, I can estimate the returns to tenure under a number of identifying assumptions. Any specification that imposes that the returns to observable characteristics are constant is strongly rejected by the data. Other specifications result in estimates of the returns to tenure that are just slightly smaller than the cross section estimates. Under either specification, tenure does not appear to be correlated with unobserved ability. Finally, my preferred specification suggests that the returns to unobserved ability are greater in the post-displacement job.

4. Endogenous Mobility Bias.

Models of imperfect information in the labour market provide a framework for understanding workers' labour market mobility as the consequence of on-the-job search for higher paying firms or more productive matches. Appropriately, much recent discussion of the problems of estimating the return to tenure have focussed on the possible endogeneity of mobility (and hence tenure). The typical suggestion is that individuals who receive good draws from a wage offer distribution, should have both long lasting jobs and high tenures, and that this can produce a spurious wage tenure profile (see for example, Ruhm, 1990).

As several authors have noted (Topel,1991; Bronars and Formulari, 1997), this intuition is not necessarily correct. The next section clarifies the consequences of endogenous mobility for cross section relationships between wages and tenures. I then discuss the extent to which displaced worker data can be used to recover a wage-tenure profile in the face of such difficulties and again illustrate with the OML-DWS.

4.1 A Simple Model of Endogenous Job Mobility.

To think seriously about estimating on-the-job wage growth in the presence of endogenous mobility, it helps to formalize one's thinking about mobility. Consider the following unrealistically simple model of wages and mobility. Wages of worker i at time t are given by:

$$w_{it} = \mu_{it} + \alpha(T_t) + m_{it} \tag{20}$$

The first term (μ) captures "average" initial wage (or productivity), and the second term $\alpha(T)$ captures the growth of wages with tenure (T) in a particular job (with $\alpha(0)$=0). To connect the model to the previous section and to empirically implement it, I might specify:

$$\mu_{it} = X_{it}\beta + \gamma Z_i \tag{21}$$

This would lead to a specification similar to Kletzer (1989); For the present exposition, I maintain the simpler specification. The third component (m) corresponds to the deviation in the wage the workers earns in there current job (firm) from the average they could expect in all jobs (at all firms). Thus m is "match", though I am being deliberately agnostic at this stage about the source of these wage deviations.

Further, imagine that workers receive a job offer each period. A job offer is a new draw from the offer distribution of m, F(m). Denote the "match" associated with a wage offer by m^o. $F(m^o)$ is same for all workers, and for each worker, the same in each period. Because m^o is the deviation of the wage that a worker would receive in the offered job (from the offering firm) from the average they could expect over all jobs (firms), the mean of the offer distribution $F(m^o)$ is zero. Equivalently the expected wage offer to worker i is:

$$E[w_{it}] = \mu_{it} \tag{22}$$

Note however, that because mobility is endogenous, the expected accepted match, $E[m_{it}]$, is positive after the first period (t>1). A worker leaves their current job (firm) in period t if

$$\mu_{it} + m^{o}_{it} > \mu_{it} + \alpha(T_t) + m_{it} \tag{23}$$

I now point out several features of such a model.

(1): In the absence of on-the-job wage growth, a model of purely endogenous mobility does not generate a relationship between wages and tenure, conditional on experience.

This is because, in the absence of on-the-job wage growth, there is a symmetry in the expected wages of workers of different tenures. A worker moves if

$$\mu_{it} + m^{o}_{it} > \mu_{it} + m_{it} \tag{24}$$

Assume for simplicity of notation that t=0 corresponds to (permanent) entry into the labor force, so that time and experience are identical. The expected wage of a worker at time t (or equivalently now, with experience t) is

$$E[w_{it}] = \mu_{it} + E[\max(m^{o}_{1}, m^{o}_{2}, \ldots\ldots m^{o}_{t})] \tag{25}$$

at time t, a worker with tenure T has expected wage

$$E[w_{it}] = \mu_{it} + E[m^{o}_{t-T} \mid m^{o}_{t-T} = \max(m^{o}_{1}, m^{o}_{2}, \ldots\ldots m^{o}_{t})] \tag{26}$$

Since the offer distributions are i.i.d. this quantity is independent of T. Expected match is simply a function of t (experience). What of the common intuition that matching generates a positive wage-tenure profile because match is positively correlated with both tenure and wages? It is true that, unconditionally, good matches have both long completed tenures and high wages. However, here the focus is the conditional (on experience) cross section (ie., disrupted) distribution of tenures. In a cross section of *interrupted* tenures, those with short tenures are in jobs they moved to recently. They moved to these jobs because they were good matches. The crucial difference is that in a cross section of *interrupted* tenures, short tenures do not represent poor matches, where as in a sample of *completed* tenures they do.

(2) In a model with no on-the-job wage growth, and endogenous mobility, some exogenous separations (job destruction) generates a positive relationship between wages and tenure, net of experience.

In this simple model, I take a displacement or exogenous separation to mean a resetting of the matching process. The worker takes in a fresh (mean zero) draw from the match distribution, and match quality improves with time since the last exogenous displacement. Experience (t) is no longer the relevant time index. Conditional on t, tenure in the current job (firm) will be positively correlated with time since the last exogenous separation (because the recently displaced cannot have long tenures) and hence with match.

(3) In a model with positive on-the-job wage growth and purely endogenous separations, the cross sectional relationship between wages and tenure (conditional on experience) <u>understates</u> *the rate of on-the-job wage growth.*

On-the-job wage growth breaks the symmetry of expected wages expressed in (1). This is most easily seen by considering the first period. The worker moves if :

$$\mu_{it} + m_{it}^{o} > \mu_{it} + \alpha(1) + m_{it} \tag{27}$$

Note that the offered and accepted match distributions are identical in the first period, so the conditions for moving is:

$$m_1^{o} > \alpha(1) + m_0^{o} \tag{28}$$

The expected wages of a mover are:

$$E[w_1|mover] = \mu_1 + E[m_1^{o} \mid m_1^{o} > m_0^{o} + \alpha(1)] \tag{29}$$

and those of a stayer

$$E[w_1|stayer] = \mu_1 + E[m_0^{o} \mid m_0^{o} > m_1^{o} - \alpha(1)] \tag{30}$$

To an induce a worker to move, the gain in match quality must compensate them for the loss of on-the-job wage growth. Thus movers will have better matches on average, than stayers. This simple two period case illustrates that with on the job wage growth match quality can be negatively correlated with tenure, and the cross sectional relation ship between wages and tenure net of experience may understate the on-the-job wage growth. This point is made by Topel (1991; see footnote 7). However Topel considers only endogenous mobility and any study of displaced workers must consider both voluntary and involuntary separations, which leads to my final point:

(4) Combining points (2) and (3), it is obvious that, in a model with on-the-job wage growth and both

endogenous and exogenous separations, "endogenous mobility bias" has indeterminate sign.

That is the cross section relationship between wages and tenure, net of experience, can either under or overstate the rate of on-the-job wage growth.

I now apply these ideas to the common approaches to estimating the returns to seniority from displaced worker data.

4.2 Estimating Wage Growth with Tenure.

If I augment equations 4 and 5 with a term capturing the heterogeneity in wages offered to a worker by different firms (again either firm or firm-worker specific) I get a specification similar to Kletzer (1989),

$$w_1 = \alpha T + X_0\beta_{10} + X_1\beta_{11} + \gamma_1 Z + m_1 + \epsilon_1 \tag{31}$$

and:

$$w_2 = X_0\beta_{20} + X_2\beta_{21} + \gamma_2 Z + m_2 + \epsilon_2 \tag{32}$$

The cross section return to tenure can be seen to have three components:

$$a^{OLS} = \frac{cov(W_1 T_1 | X_1)}{var(T_1 | X_1)} + \alpha + \gamma_1\psi + \frac{cov(m_1 T_1 | X_1)}{var(T_1 | X_1)} \tag{33}$$

The first component of this expression is the return to tenure. The second is 'ability bias' and of positive sign assuming that there are positive wage and tenure returns to ability. The third term is endogenous mobility bias. In the simple model of the previous section, this is negative if all mobility is endogenous (all separations are voluntary) but potentially positive with a sufficient rate of exogenous separations.

Turning to the estimators based on pre- and post displacement wages used in the displaced worker literature, first differencing wages does not solve the endogenous mobility problem, as past authors have recognized.

$$w_2 - w_1 = -\alpha T + X_0(\beta_{20} - \beta_{10}) + X_2\beta_{21} - X_1\beta_{11} + (\gamma_2 - \gamma_1)Z + (m_2 - m_1) + \epsilon_1 \tag{34}$$

Obviously, if pre- and post displacement matches are uncorrelated, as in the simple model presented in the previous section, and as is often assumed in the literature (Kletzer,1989), mobility bias will not be addressed by first differencing. On the other hand to the extent that match is correlated across displacement, differencing may mitigate some of the bias.

Second, consider estimating an equation of the form

$$w_2 = X_0\pi_{20} + X_2\pi_{21} + aT + e_2 \tag{35}$$

Substituting equation 6 into equation 32

$$w_2 = (\beta_{20} + \gamma_2\theta_0)X_0 + (\gamma_2\theta_1)X_1 + (\beta_{21} + \gamma_2\theta_2)X_2 + (\gamma_2\psi)T + (m_2 + \epsilon_2 + \gamma_2 u_i) \tag{36}$$

In the simple model of the previous selection, post displacement jobs are random draws:

$$cov(m_1, m_2) = cov(T, m_2) = 0 \tag{37}$$

Thus

$$\frac{cov(W_2 T_1 | X_2)}{var(T_1 | X_2)} = \gamma_2\psi \tag{38}$$

Assuming $\gamma_2 = \gamma_1$, the regression of post-displacement wages on pre-displacement tenure isolates the ability bias from the other components of the cross section return to tenure. Of course, the wage loss regression does this too. This decomposition is of some interest because it separates sources of earnings which are lost on displacement (firm specific capital, match) from those which are transferable (ability). Good matches and firm specific capital are assets that provide earnings, which take time to acquire. We should be concerned about losses of firm specific capital, both because this represents a private loss and because it represents a social loss. [11] On the other hand, the loss of good matches may or may not represent a social loss, depending on whether "match" represents true idiosyncratic productivity differences (as in a matching model) or simply the rents accruing from finding a job at a "high wage" firm (as in a model of general equilibrium search (Burdett and Mortensen, 1997).

Its also worth noting two restrictive assumptions of the simple model of the previous section: (1) Observed and unobserved co-variates simply shift the mean of the offer distribution (equation 21) and (2) the employed and unemployed offer arrival rates are identical (so that the worker accepts any draw from the wage offer distribution in the first period). Together, these assumptions imply that "match" is uncorrelated with the unobserved and observed characteristics (because the conditional expectation of the former is a linear function of the former). If this were not true (for example, if offer arrival rates varied across education g roups or with ability), then "match" would be correlated with the Xs and the regression based approaches above would be even more seriously flawed.

Equation 31 also makes it clear that if one could condition on match, an unbiased estimate of the returns to tenure could be obtained, even (ignoring now ability bias) from cross sectional data. In fact, in the model of the previous section, conditional on match, variations in (completed) tenures are random (representing the random draws of outside offers while employed), and if displacement is also exogenous to individual characteristics, so are tenures at displacement. Is "match" observable and might one ever expect to have data

[11] Against which one might compare the social gain from labour reallocation.

on it? The answer to this question depends on the source of the wage offer dispersion faced by workers. The source of wage offer dispersion is often not well specified in search and matching models.

The first possibility is that the wage offer received by a given worker simply varies across firms. There are "high wage" and "low wage" firms, and workers search for the former. This is the source of wage offer dispersion in general equilibrium search models such as Burdett and Mortensen (1997). Note that in such the model "match" is not idiosyncratic - a "high wage" firm is a good match for all workers. None-the-less, such a notion has both theoretical foundations and some intuitive appeal. If "firm effects" are the source of wage offer dispersion, and the premia offered by a firm is the same for all workers, then one can control for "match" by controlling for firm. This requires data in which workers place of employ can be identified and multiple workers are observed at each firm and is the approach taken by, for example, Barth (1997) with cross section data.

Many discussions of match do suggest an idiosyncratic component of wages - firm-worker specific wage deviations rather than firm specific. In typical displaced worker data, with only one wage observation on each firm worker match, its obviously not possible to remove such heterogeneity by differencing within matches. It might however be possible to improve estimates of the returns to tenure if match depends on a function of worker and job characteristics.

The empirical component of this section focusses on the former, "firm effect" interpretation of endogenous mobility bias. In particular, using the OML PCS data, I extend several of the estimation strategies of section 3 by conditioning on firm of employment. The first row of Table 4.1 report again for comparison purposes the estimates of the returns to tenure obtained from my data via an OLS pre-displacement wage equation without conditioning on firms dummies (identical to column (1) of Tables 2.4, 3.1, and 3.2). The third row of Table 5.1 reports the result of augmenting the pre-displacement wage equation with firm dummies - this is the "within firm" estimate of the returns to tenure. The within firm tenure slope is considerably less than the that estimated from the pooled data, falling by almost two thirds. This suggests that endogenous mobility biases the estimated returns to tenure up, and, in light of the simple model presented in the first part of this section, this indicates that the job histories of these workers includes significant involuntary mobility. The firm fixed effects are statistically significant. I investigate this apparent heterogeneity in pay policies in another paper (Crossley, 1997).

In Crossley et. al. (1994) separate wage and tenure profiles are estimating for men and women using the same OML PCS data. Conditioning on firm of employment reduces the apparent wage-tenure profile for women as well. Though the firm matched displaced worker data used here is quite unusual, several other authors report conditioning on firm of employment in cross section wage regressions. Hersch and Reagan (1990) allow for firm fixed effects using a small sample (309 observations) of male, blue collar workers from eighteen Oregon firms. In contrast to the results reported here, they find that conditioning on firm of employment has very little effect on the estimated wage-tenure relationship. A negligible difference between cross section and within firm estimates of the returns to tenure is also reported by Barth (1997), using a Norwegian sample of over two thousand workers and over 500 firms. Finally Bronars and Formulari (1997), with a sample of almost seventeen hundred white collar workers from some two hundred and forty establishments estimate cross section and within firm wage tenure profiles for men and women separately. They report that conditioning on firm of employment raises the estimated return to tenure for men but reduces it for women. Obviously such variety of results suggests a need for further investigation.

In order to simultaneously control for firm and individual effects, I now repeat the quasi-differencing approach of Table 3.2, augmenting each equation with firm dummies[12]. The results are presented in the final two columns of Table 4.1[13]. Column 3 contains the estimates generated when I identify by constraining the returns to unobserved ability to be constant across jobs. Comparing with column 1 of Table 3.2 I see again that conditioning on firm of employment reduces the observed return to tenure, though the estimate exceeds that

[12] Note that I do not exclude the pre-displacement firm dummies from the (structural) post-displacement wage equation, because they may be correlated with omitted post-displacement firm effects. This is discussed at length in Crossley (1997).

[13] Again, the reduced form estimates are presented in Appendix Table A2.

with firm effects only (column 2). Finally, column 4 presents results with the identifying assumption that the wage disturbance variances are the same in pre- and post-displacement jobs. These are my preferred estimates. They control for both unobserved heterogeniety in earnings ability and endogenous mobility induced by heterogeniety in firm pay policies. At the same time, they do not constrain the returns to either observed or unobserved worker characteristics to be the same in pre- and post-displacement jobs, allowing for possible learning and insurance effects. The estimated returns to tenure statistically significant but are just less than half of OLS cross section estimates at about 7% per ten years. I note that estimates again imply that the returns to both observed and unobserved individual characteristics are larger in the post-displacement job. Finally, under both identifying assumptions (columns 3 and 4) the null hypothesis that tenure is unrelated to unobserved earnings ability cannot be rejected at standard significance level while the exclusion of the firm dummies from the pre-displacement wage equation is overwhelmingly rejected.

5. Conclusions and Summary.

One way to think about the returns to tenure is to imagine the following experiment. Take a sample of new labour market entrants and randomly assign them to one of two groups: mobile and immobile. Then randomly assign the workers in both groups to firms. The workers in the immobile group remain at this initial job for the remainder of the experiment. However, each period (year), the mobile workers are forced to leave their firm and move to another *randomly selected* firm. At the end of the experiment, the average difference in the wages between the mobile and immobile groups is the return to tenure. Crucially, both selection into the mobile group, and the outcome of the ensuing mobility is exogenous. Displaced worker data may approximate this experiment in one regard: the displacement may be considered exogenous - giving us a group of workers "randomly" assigned to mobility. However, it falls far short of this experiment in that the *past* mobility that generated the tenures observed at displacement can not be considered exogenous.

Individuals' mobility may occur randomly at rates that are approximately fixed for individuals but vary across individuals in a way that is correlated with their earnings power ("ability bias"). In such a world, one incident of (plausibly) exogenous mobility[14] - such as is provided by displaced worker data - provides sufficient information to identify the returns to tenure.

On the other hand, individuals' mobility may be endogenously determined by heterogeneity in wage offers, either across firms or firm - worker matches. In this case the information provided by one exogenous displacement is not sufficient to identify the returns to tenure. I emphasize that if all past mobility is endogenous, then the cross section estimates of the return to tenure will be negatively biased (towards zero); Only if involuntary departures are an important part of workers past labour market experience will the returns be overstated.

One strategy that will of course correct of endogenous mobility is to condition on the heterogeniety in wage offers. My preferred estimates correct for both unobserved heterogeneity in earnings ability and mobility propensities and for endogenous mobility induced by heterogeneity in firm pay policies. At the same time, they do not constrain the returns to either observed or unobserved worker characteristics to be the same in pre- and post-displacement jobs, allowing for possible learning and insurance effects. They do not however, control for endogenous mobility induced by idiosyncratic firm-worker matches. The estimated returns to tenure statistically significant but are just less than half of OLS cross section estimates at about 7% per ten years. I note that my preferred estimates imply that the returns to both observed and unobserved individual characteristics are larger in the post-displacement job, a result that may warrant further investigation.

Past research has emphasized that, at minimum, displaced worker data affords the opportunity to isolates the ability bias from the other components of the cross section return to tenure.. This decomposition is of some interest because it separates sources of earnings which are lost on displacement (firm specific capital , match) from those which are transferable (ability). Good matches and firm specific capital are assets that provide earnings, and which take time to acquire. We should be concerned about losses of firm specific capital,

[14]That is, an incident in which all individuals have an equal probability of moving, at least conditional on observable characteristics.

both because this represents a private loss and because it represents a social loss. On the other hand, the loss of good matches may or may not represent a social loss, depending on whether "match" represents true idiosyncratic productivity differences (as in a matching model) or simply the rents accruing from finding a job at a "high wage" firm. This issue, along with the inconsistency of results reported by researchers attempting to control for firm effects, suggests a need for further research on the nature of matches and worker mobility.

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Tables

TABLE 2.1: Mean Real Wages by Tenure Category

	Full Sample		Re-employed	
Tenure Category (years)	Number of Obs.	Mean Real Hourly Wage (std. err.)	Number of Obs.	Mean Real Hourly Wage (std. err)
0-1	54	7.55 (0.40)	31	8.01 (0.62)
1-3	179	8.11 (0.15)	132	8.38 (0.18)
3-5	140	8.50 (0.17)	92	8.83 (0.21)
5-10	203	8.44 (0.17)	149	8.73 (0.20)
10-15	197	9.49 (0.15)	132	9.87 (0.19)
15-25	264	9.93 (0.13)	168	10.18 (0.17)
25up	132	10.10 (0.16)	69	10.19 (0.26)
Total	1169	9.05 (0.07)	773	9.29 (0.09)

Notes:
(1) Hourly wage rates in the pre-displacement job, deflated by the CPI (June 1981 = 100)

TABLE 2.2: Mean Real Re-employment Wages and Wage Losses by Tenure Category

Tenure Category (years)	Number of Obs.	Mean Real Hourly Re-employment Wage (std. err)	Mean Real Hourly Loss (std. err)
0-1	28	7.71 (0.61)	0.41 (0.31)
1-3	128	8.31 (0.21)	0.08 (0.18)
3-5	92	8.44 (0.22)	0.37 (0.21)
5-10	143	8.15 (0.20)	0.57 (0.16)
10-15	125	8.72 (0.27)	1.05 (0.27)
15-25	163	8.30 (0.20)	1.83 (0.17)
25up	68	7.88 (0.32)	2.29 (0.30)
Total	747	8.30 (0.09)	0.97 (0.09)

Notes:
(1) Hourly wage rates in the post-displacement job, deflated by the CPI (June 1981 = 100)
(2) Hourly wage rate in the pre-displacement job minus the hourly wage rate in the post-displacement job, both deflated by the CPI (June 1981 = 100)
(3) Differences in the number of observations from Table 2.1 reflect missing re-employment wage information.

TABLE 2.3: Covariate Means and Standard Deviations.

Variable	Full Sample	Re-employed	Explanation
	mean (Standard Deviation) [observations]		
Individual Characteristics			
dage2	0.10 (030) [1169]	0.11 (0.31) [773]	Dummy=1 if age 26-35.
dage3	0.24 (0.43) [1169]	0.27 (0.44) [773]	Dummy =1 if age 36-45.
dage4	0.23 (0.42) [1169]	0.24 (0.42) [773]	Dummy=1 if age 46-55.
dage5	0.24 (0.42) [1169]	0.25 (0.43) [773]	Dummy=1 if age 56-65.
educ1	0.38 (0.49) [1169]	0.39 (0.49) [773]	Dummy = 1 if some secondary or high school.
educ2	0.20 (0.40) [1169]	0.24 (0.43) [773]	Dummy = 1 if completed secondary or high school.
educ3	0.16 (0.37) [1169]	0.18 (0.38) [773]	Dummy = 1 if at least some college/ university.
training	0.29 (0.45) [1169]	0.32 (0.47) [773]	Dummy = 1 if some (other) formal technical training.
married	0.79 (0.41) [1169]	0.80 (0.40) [773]	Dummy = 1 if married.
Pre-Displacement Job Characteristics			
manpro1	0.09 (0.29) [1169]	0.11 (0.31) [773]	Dummy =1 if two digit occupation code belongs to managerial and professional groups, pre-displacement job.
super1	0.07 (0.26) [1165]	0.08 (0.28) [772]	Dummy = 1 if a supervisor in the pre-displacement job (as indicated by 4^{th} digit of occupation code).
blucol1	0.81 (0.39) [1169]	0.79 (0.41) [773]	Dummy = 1 if two digit occupation code belongs to processing, construction, transportation or materials handling, pre-displacement job.
preunion	0.73 (0.44) [1169]	0.68 (0.47) [773]	Dummy = 1 if (self reported) pre-displacement job unionised.

TABLE 2.3: Covariate Means and Standard Deviations.

Variable	Full Sample	Re-employed	Explanation
dmanuf1	0.98 (0.13) [1169]	0.98 (0.15) [773]	Dummy =1 if pre-displacement job is in the manufacturing sector.
ten	12.7 (9.90) [1169]	12.0 (9.2) [773]	Tenure in pre-displacement job, years.
Post-Displacement Job Characteristics			
manpro2		0.14 (0.34) [773]	Dummy =1 if two digit occupation code belongs to managerial and professional groups, post-displacement job.
super2		0.06 (0.24) [764]	Dummy = 1 if a supervisor in the post-displacement job (as indicated by 4th digit of occupation code).
blucol2		0.65 (0.48) [773]	Dummy = 1 if two digit occupation code belongs to processing, construction, transportation or materials handling, post-displacement job.
posunion		0.46 (0.50) [773]	Dummy = 1 if (self reported) post-displacement job unionised.
dmanuf2		0.69 (0.46) [705]	Dummy =1 if post-displacement job is in the manufacturing sector.
Search Environment			
logpop	4.07 (1.73) [1169]	4.03 (1.7) [773]	Log(population) of location of pre-displacement job.
lunemp	2.02 (0.36) [1169]	1.99 (0.34) [773]	Log(unemployment rate) in location of pre-displacement job.
dnotice	0.76 (0.43) [1169]	0.80 (0.40) [773]	Dummy = 1 if reported notice of layoff > 1 month.
Selection Instrument			
outtime2	21.9 (4.2) [1165]	22.1 (4.1) [773]	Elapsed time (at survey date) since layoff announcement, calculated from individually reported information. (Months)

Notes:

TABLE 2.4: Cross Section Wage Regressions.

Regressors	Full Sample, OLS	Full Sample, OLS, Re-employment dummy	Re-employed Sample, OLS	Re-employed Sample, 2 Step Selection Correction	Re-employed Sample, Full ML Selection Correction
dage2	-0.034	-0.054	-0.045	-0.174	-0.137
	(0.035)	(0.035)	(0.044)	(0.127)	(0.045)
dage3	-0.009	-0.025	-0.035	-0.135	-0.106
	(0.024)	(0.024)	(0.030)	(0.024)	(0.034)
dage4	0.076	0.066	0.057	-0.010	0.010
	(0.020)	(0.020)	(0.026)	(0.017)	(0.031)
dage5	0.041	0.028	0.040	-0.045	-0.023
	(0.017)	(0.018)	(0.021)	(0.022)	(0.028)
educ1	0.050	0.043	0.054	0.012	0.022
	(0.015)	(0.015)	(0.020)	(0.020)	(0.023)
educ2	0.131	0.120	0.130	0.060	0.077
	(0.020)	(0.020)	(0.025)	(0.036)	(0.029)
educ3	0.188	0.181	0.219	0.175	0.188
	(0.024)	(0.024)	(0.030)	(0.020)	(0.031)
training	0.047	0.045	0.046	0.034	0.037
	(0.014)	(0.013)	(0.017)	(0.020)	(0.019)
married	0.064	0.055	0.039	-0.018	-0.004
	(0.018)	(0.017)	(0.023)	(0.076)	(0.024)
manpro1	0.120	0.122	0.103	0.115	0.112
	(0.035)	(0.034)	(0.040)	(0.032)	(0.039)
super1	0.099	0.096	0.084	0.067	0.072
	(0.026)	(0.026)	(0.033)	(0.023)	(0.034)
blcol1	-0.012	-0.007	-0.016	0.013	0.000
	(0.025)	(0.024)	(0.030)	(0.051)	(0.030)
preunion	-0.046	-0.041	-0.045	-0.009	-0.018
	(0.017)	(0.017)	(0.022)	(0.023)	(0.023)
dmanuf1	0.335	0.342	0.351	0.404	0.389
	(0.065)	(0.065)	(0.073)	(0.018)	(0.059)
ten	0.018	0.017	0.017	0.012	0.013
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
tensq	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
reemp		0.061			
		(0.013)			
_cons	1.543	1.517	1.594	1.824	1.767
	(0.069)	(0.069)	(0.078)	(0.027)	(0.073)
N	1165	1165	772	772	772
R - square	0.36	0.37	0.36	-----	------

Notes:
(1) Standard Errors in Parentheses. White heteroscedasticity consistent estimate of the covariance matrix.
(2) tensq is the square of ten (tenure in the pre-displacement job). Reemp is a dummy for re-employment at the survey date.

TABLE 2.5 Cross Section Wage Regressions: Tests and Calculations					
	Full Sample, OLS	Full Sample, OLS, Re-employment dummy	Re-employed Sample, OLS	Re-employed Sample, 2 Step Selection Correction	Re-employed Sample, Full ML Selection Correction
DFbeta (ten) [min, max]	[-0.18,0.16]				
DFbeta (tensq) [min, max]	[-0.13,0.15]				
Cook-Weisberg Test for Hetero-scedasticity.	$\chi^2_{(1)}$ =21.66 (p = 0.00)				
RESET Test for Omitted Variables	F = 0.36 (p = 0.78)				
λ (Standard Error)				-0.20 (0.02)	-0.20 (0.02)
Point Estimate of Wage growth due to 10 years of Tenure	16.5%	15.8%	15.1%	11.0%	11.8%
F-test for Exclusion of Tenure Variables	$F_{(2,1148)} = 59.41$ (p= 0.00)	$F_{(2,1148)} = 58.41$ (p= 0.00)	$F_{(2,755)} = 26.21$ (p= 0.00)	$\chi^2_{(2)} = 22.67$ (p = 0.00)	$\chi^2_{(2)}$ =41.3 (p = 0.00)
Notes:					

TABLE 3.1: Estimates Correcting for Unobserved Ability: Standard Approaches.

	(1) Cross Section	(2) Conditioning on re-employment Wage	(3) Pre-displacement Tenure in Post-displacement Wage Eqn	(4) Wage Loss
Tenure	0.018 (0.002)	0.018 (0.002)	0.002 (0.004)	0.015 (0.00)
Tenure Squared	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
Predicted wage growth 10 years of Tenure	16.5%	13.9%	14.1%	13.0%
F-Test for Exclusion of Tenure terms	$F_{(2,1148)} = 59.4$ (p= 0.00)	$F_{(2,726)} = 29 .3$ (p= 0.00)	$F_{(2,666)} = 1.6$ (p= 0.20)	$F_{(2,661)} = 11.9$ (p= 0.00)

Notes:
(1) Full regression results available on request.
(2) The cross section specification (column 1) is exactly the same as column (1) of Table 2.3. The specification of column (2) is identical to specification of column (1) except for the addition of the post-displacement wage.
(3) The dependent variable in the post-displacement wage equation (column (3)) is log (rpay2). The co-variates in this specification include demographics and post-displacement job characteristics.
(4) In column (3) the predicted wage growth from 10 years of tenure is calculated by subtracting the apparent return to pre-displacement tenure in the post-displacement job from the pre-displacement return.
(5) The dependent variable in the wage loss equation (column (4)) is log (rpay1) - log(rpay2). The co-variates in this specification include demographics and pre- and post-displacement job characteristics.
(6) Standard errors in parentheses. White heteroscedasticity consistent estimate of the covariance matrix.

TABLE 3.2: Estimates Correcting for Unobserved Ability: Fixed Effects and Quasi Fixed Effects.

(1) Restriction	$\gamma_1 = \gamma_2$ constant returns to unobserved characteristics	$\sigma_{\epsilon 1} = \sigma_{\epsilon 2}$ constant disturbance variance	$\beta_1 = \beta_2$ constant returns to observed characteristics	$\gamma_1 = \gamma_2$ and $\beta_1 = \beta_2$	$\sigma_{\epsilon 1} = \sigma_{\epsilon 2}$ and $\beta_1 = \beta_2$	$\gamma_1 = \gamma_2$ and $\sigma_{\epsilon 1} = \sigma_{\epsilon 2}$
(2) Tenure	0.0146 (0.0042)	0.0156 (0.0032)	-0.016 (0.035)	0.0048 (0.0021)	0.0157 (0.0029)	0.0146 (0.0042)
(3) Tenure Squared	-0.002 (0.0001)	-0.002 (0.0001)	0.002 (0.0008)	0.000 (0.000)	-0.002 (0.0001)	-0.002 (0.0001)
(4) Predicted wage growth 10 years of tenure	13.0%	14.2%	-12.8%	5.6%	14.6%	13.0%
(5) Test for Exclusion of Tenure terms From Wage Equation	$\chi^2_{(2)}=23.7$ ($p = 0.00$)	$\chi^2_{(2)}=51.5$ ($p = 0.00$)	$\chi^2_{(2)}=0.3$ ($p = 0.87$)	$\chi^2_{(2)}=57.7$ ($p = 0.00$)	$\chi^2_{(2)}=81.9$ ($p = 0.00$)	$\chi^2_{(2)}=23.4$ ($p = 0.00$)
(6) Test for Exclusion of Tenure terms From the Auxiliary Equation	$\chi^2_{(1)}=2.3$ ($p = 0.31$)	$\chi^2_{(1)}=2.3$ ($p = 0.32$)	$\chi^2_{(1)}=1.2$ ($p = 0.56$)	$\chi^2_{(1)}=9.9$ ($p = 0.01$)	$\chi^2_{(1)}=1.6$ ($p = 0.44$)	$\chi^2_{(1)}=2.3$ ($p = 0.31$)
(7) Test of over-identifying restrictions	just identified	just identified	$\chi^2_{(14)}=22.7$ ($p = 0.065$)	$\chi^2_{(1)}=69.4$ ($p = 0.00$)	$\chi^2_{(1)}=67.7$ ($p = 0.00$)	$\chi^2_{(1)}=43$ ($p = 0.00$)
(8) γ_2/γ_1 [t-test]	-----	1.8 [4.4]	0.13 [-7.9]	-----	5.8 [2.7]	-----
(9) $(1/n)^*(\beta_2-\beta_1)'X_1'X_1(\beta_2-\beta_1)$	0.015	0.012	------	-----	------	0.015
Notes:			negative variance estimate			

Notes:
(1) All estimates based on 683 observations.
(2) Standard errors are given in parentheses in rows (2) and (3)
(3) The t-test in row (8) is for the null $\gamma_2/\gamma_1=1$.

TABLE 4.1: Estimates Correcting for Endogenous (Past) Mobility

	(1) Cross Section	(2) Cross Section With Firm Controls	(3) Quasi Fixed Effects With Firm Controls $\gamma_1 = \gamma_2$	(4) Quasi Fixed Effects with Firm Controls $\sigma_{\epsilon 1} = \sigma_{\epsilon 2}$
Tenure	0.0175 (0.0042)	0.006 (0.002)	0.0098 (0.043)	0.0077 (0.0028)
Tenure Squared	-0.002 (0.0001)	-0.000 (0.000)	-0.0002 (0.0001)	-0.0001 (0.0001)
Point Estimate of wage growth due to 10 years of Tenure	16.5%	5.4%	8.6%	6.8%
Test for Exclusion of Tenure terms from Pre-displacement Wage Equation	$F_{(2,1148)} =$ 59.41 ($p= 0.00$)	$F_{(2,1130)} = 7.25$ ($p= 0.00$)	$\chi^2_{(2)} =9.7$ ($p = 0.01$)	$\chi^2_{(2)} =15.4$ ($p = 0.00$)
(6) Test for Exclusion of Tenure terms From the Auxiliary Equation	---------	---------	$\chi^2_{(2)} =0.8$ ($p = 0.66$)	$\chi^2_{(2)} =0.8$ ($p = 0.67$)
(8) γ_2/γ_1 [t-test]	---------	---------	--------	2.8 [4.4]
(9) (1/n)* $(\beta_2-\beta_1)'X_1'X_1 (\beta_2-\beta_1)$	———	———	0.014	0.021
Test for Exclusion of Firm Dummies	---------	$F_{(2,1148)} = 0.36$ ($p= 0.78$)	$\chi^2_{(18)} =341.7$ ($p = 0.00$)	$\chi^2_{(18)} =341.7$ ($p = 0.00$)
Observations	1165	1165	683	683

Notes:

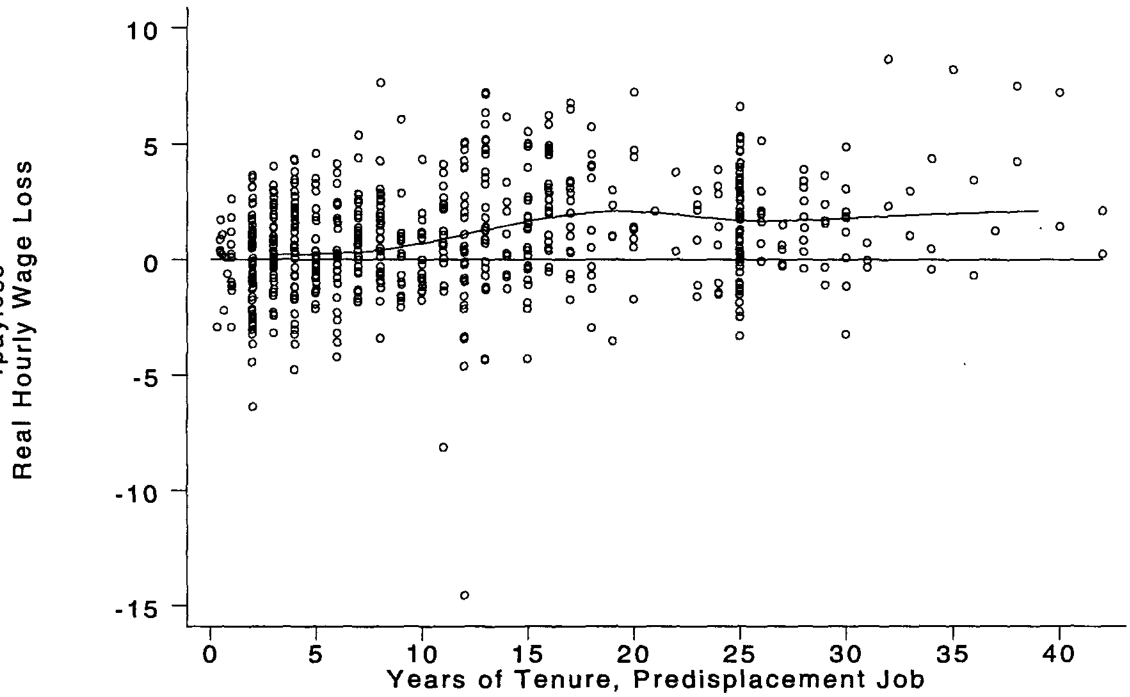

Figure 2: Wage Losses vrs. Tenure

Appendix.

TABLE A1: Re-employment Probits		
Regressor	Re-employment Probit	Selection Equation from Full ML Estimates of Selection Model
dage2	0.943 (0.210)	0.946 (0.204)
dage3	0.758 (0.153)	0.783 (0.149)
dage4	0.518 (0.136)	0.530 (0.133)
dage5	0.600 (0.122)	0.566 (0.119)
educ1	0.254 (0.103)	0.216 (0.101)
educ2	0.436 (0.136)	0.379 (0.133)
educ3	0.219 (0.149)	0.228 (0.152)
training	0.123 (0.094)	0.101 (0.091)
married	0.432 (0.110)	0.409 (0.107)
manpro1	-0.158 (0.209)	-0.164 (0.208)
super1	0.107 (0.186)	0.129 (0.186)
blcoll	-0.296 (0.155)	-0.388 (0.154)
preunion	-0.407 (0.117)	-0.416 (0.118)
dmanuf1	-1.039 (0.417)	-1.228 (0.396)
ten	0.027 (0.016)	0.021 (0.016)
tensq	-0.001 (0.000)	-0.001 (0.000)
outtime2	0.065 (0.011)	0.068 (0.010)
_cons	-0.614 (0.461)	-0.325 (0.445)
N		1165
Notes:		

TABLE A2: Reduced Forms for Difference and Quasi Difference Estimates

Regressor	Lprew	Lprew	Lprew	Lposw
dage2	-0.018	0.086	-0.087	0.053
	(0.043)	(0.054)	(0.036)	(0.053)
dage3	-0.003	0.083	-0.050	0.058
	(0.032)	(0.041)	(0.027)	(0.040)
dage4	0.075	0.125	0.023	0.106
	(0.030)	(0.038)	(0.025)	(0.037)
dage5	0.057	0.131	0.015	0.108
	(0.026)	(0.033)	(0.022)	(0.032)
educ1	0.050	0.039	0.036	0.036
	(0.022)	(0.028)	(0.018)	(0.027)
educ2	0.118	0.063	0.084	0.040
	(0.026)	(0.034)	(0.022)	(0.033)
educ3	0.202	0.216	0.114	0.166
	(0.031)	(0.039)	(0.027)	(0.039)
training	0.052	0.081	0.040	0.077
	(0.018)	(0.023)	(0.015)	(0.022)
married	0.041	0.081	0.017	0.070
	(0.023)	(0.029)	(0.019)	(0.028)
manpro1	0.098	0.073	0.120	0.075
	(0.038)	(0.049)	(0.031)	(0.046)
super1	0.036	0.028	0.040	0.056
	(0.036)	(0.046)	(0.032)	(0.047)
blcol1	0.011	-0.013	0.031	-0.018
	(0.029)	(0.037)	(0.024)	(0.036)
preunion	-0.038	-0.016	-0.149	-0.102
	(0.022)	(0.028)	(0.023)	(0.034)
dmanuf1	0.336	0.080	0.340	0.035
	(0.053)	(0.068)	(0.090)	(0.132)
manpro2	0.044	0.190	0.008	0.179
	(0.032)	(0.041)	(0.027)	(0.040)
super2	0.017	0.100	-0.023	0.039
	(0.040)	(0.051)	(0.034)	(0.050)
blcol2	-0.017	0.074	-0.007	0.077
	(0.023)	(0.030)	(0.020)	(0.029)
posunion	-0.007	0.176	-0.011	0.172
	(0.018)	(0.023)	(0.015)	(0.022)
dmanuf2	0.031	0.152	0.017	0.150
	(0.019)	(0.025)	(0.017)	(0.025)

TABLE A2: Reduced Forms for Difference and Quasi Difference Estimates

Regressor	Lprew	Lprew	Lprew	Lposw
food2			-0.226	0.068
			(0.062)	(0.092)
rbbr1			-0.050	0.067
			(0.043)	(0.064)
lthr1			-0.188	-0.074
			(0.055)	(0.080)
txtl1			-0.328	-0.108
			(0.055)	(0.081)
txtl2			-0.262	-0.150
			(0.046)	(0.068)
txtl3			-0.221	-0.084
			(0.053)	(0.078)
txtl4			-0.383	-0.165
			(0.058)	(0.084)
clth1			0.000	0.000
			(0.000)	(0.000)
papr1			-0.057	0.071
			(0.053)	(0.078)
prnt1			-0.066	0.201
			(0.089)	(0.130)
mchn1			-0.435	-0.055
			(0.081)	(0.119)
vhcl1			-0.081	-0.084
			(0.055)	(0.080)
vhcl2			-0.425	-0.184
			(0.069)	(0.102)
vhcl3			0.046	-0.066
			(0.048)	(0.070)
vhcl4			-0.399	-0.288
			(0.055)	(0.081)
elec1			-0.137	0.144
			(0.071)	(0.105)
elec2			-0.107	0.002
			(0.054)	(0.079)
chem1			-0.525	-0.239
			(0.095)	(0.139)
insr1			0.000	0.000
			(0.000)	(0.000)
motl1			-0.225	-0.148
			(0.089)	(0.130)
ten	0.017	0.002	0.007	-0.003
	(0.003)	(0.004)	(0.003)	(0.004)
tensq	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
_cons	1.547	1.450	1.931	1.688

TABLE A2: Reduced Forms for Difference and Quasi Difference Estimates

Regressor	Lprew	Lprew	Lprew	Lposw
Dfbeta (ten) [min, max]	[-0.21,0.23]	[-0.17,0.16]	[-0.26,0.36]	[-0.17,0.23]
DFbeta (tensq) [min, max]	[-0.19,0.28]	[-0.24,0.18]	[-0.22,0.25]	[-0.32,0.24]
Cook-Weisberg Test for Hetero-scedasticity.	$\chi^2_{(1)}$ =12.66 (p = 0.00)	$\chi^2_{(1)}$ =4.01 (p = 0.05)	$\chi^2_{(1)}$ =0.75 (p = 0.39)	$\chi^2_{(1)}$ =7.93 (p = 0.00)
RESET Test for Omitted Variables	F = 1.66 (p = 0.17)	F = 3.74 (p = 0.01)	F = 4.38 (p = 0.00)	F = 1.20 (p = 0.30)
Notes:				

Firms and Wages: Evidence From Displaced Workers.

1. Introduction

Obvious features of the labour market include the organization of the demand side into firms, or establishments, which number considerably fewer than workers, and the complex internal structure of these establishments, including well defined jobs and hierarchies. Despite this, most existing empirical studies of wage structure (differences in wages across workers) have focussed exclusively on worker characteristics (like age, education, experience, race or gender).[1] This "supply side" focus is partly due to the limitations of the typically available data. It may also reflect the notion that in competitive labour markets, identical workers should receive the same wage, regardless of where they are employed. Manning (1994) points out that the human capital/competitive labour market view which underlies most empirical work on wages implies that (1) equally productive workers should receive the same wage at different firms, and (2) workers with differing productivity should receive different wages, even within a single firm. Thus wages are attached to individuals.

This view can be contrasted with the activity of human resource management practitioners and academics, who expend considerable energy pursuing and describing firm and job based methods of wage determination (see for example Milkovich and Newman, 1996). It can also be contrasted with the work of the "neorealist" and "institutionalist" labour economists (Lester, 1948; Doeringer and Piore, 1985). An alternative

[1]Exceptions are the literatures on industry wage effects (Krueger and Summers, 1988, Gibbons and Katz, 1992, Gera and Grenier, 1994) and the firm size effect (Brown and Medoff, 1989, Morrisette, 1993). These literatures provide important background for this paper.

view, that wages are attached to jobs, in particular firms, is sometimes referred to as the "Company Wage Policy" view (Manning, 1994). Under this view, equally productive workers may receive different wages at different firms, and, within a firm, the same wage may be paid to workers who differ in their productivity. Recently there has been a rapid proliferation of theories that explain why wages may be attached to firms and jobs.

That firms should be an important source of wage heterogeneity is consistent with a number of alternative theoretical views of labour markets. Search theory emphasizes the role of information problems and frictions in the labour market. In the presence of these, otherwise identical firms may find it optimal to differentiate the wages they pay to workers of a given productivity (Burdett and Mortensen, 1998). The frictions prevent workers from moving instantaneously from a low wage firm to a high wage firm. The low wage firm makes greater profits per worker, but has a smaller equilibrium workforce, so that total profits are equalized in equilibrium. Efficiency wage theory suggests some firms may pay wages above those that clear the market, in order to induce effort or to recruit workers of higher average quality (See for example, Weiss, 1990).

Internal labour market theories (Doeringer and Piore, 1985) suggest that the wages of workers within firms (as opposed to initial entrants) may not be subject to competitive pressures. Instead they may be determined by considerations of incentives and internal equity and by promotion along job ladders or movement within an internal hierarchy. The attachment of wages to jobs can seen as the solution to an incentive (as in the tournament theory of Lazear and Rosen, 1981) or insurance problem (Waldman, 1984).

In light of these ideas, there has been a recent rise in interest in the empirical impact of firms and their personnel policies on wages. Examples include work by Groshen (1991a), Abowd et al (1995) and Bronars and Famulari (1997) on firm wage effects, Hartog (1986) and Shumann et al (1994) on job characteristics and Baker et al (1993, 1994a, 1994b) on the hierarchy, internal economics and wage policy of a single firm. This paper extends this literature by employing a unique displaced worker data set to reexamine the question, "Do (different) firms pay identical workers different wages?" A companion paper (Crossley, 1997a) examines the relationship between jobs and wages and the role of administered pay systems.

The focus of this paper is what are referred to in the literature as "firm wage effects". More loosely phrased, the question is 'Are there "high" and "low" wage firms?' In the empirical literature, this has operationally meant testing for significant differences across firms in the intercept of a typical human capital wage regression. For example, Groshen (1991a) finds that establishment effects account for 20-70% of intra-industry wage variation in cross-section data.

I approach this question with augmented human capital wage regressions following the existing literature on firm wage effects and the closely related literature on industry wage effects[2]. The short panel nature of the displaced worker data also admits the estimation of first difference (or wage loss) regressions. These estimates provide an important check for unobserved worker heterogeneity and are a central contribution of this paper. To preview the results, I find evidence that firm and job-level wage effects can not simply be attributed to unobserved worker heterogeneity. The second important contribution of this paper is the examination of compensating wage differentials as an explanation for firm wage effects. A range of evidence collected in this paper suggests that they are not an adequate explanation for the observed firm wage premia.

The documentation and explanation of empirical firm - and job - wage effects is important for several reasons. First, even more than inter-industry wage differentials, these effects are *prima facie* evidence against the simple competitive model of the labour market, and its underlying assumptions. Determining the exact nature and source(s) of firm wage effects may help us to identify which of the assumptions of that model are most inappropriate. The policy implications of alternative labour market models can be strikingly different from those of the simple `textbook' competitive model, as is discussed for the case of general equilibrium search models by Manning (1994) and for the case of efficiency wages by Bulow and Summers (1986).

Second, an understanding of how wage inequality is generated is crucial to the formulation of any

[2] Of course these augmented human capital wage regressions cannot be related to any alternative labour market model in a structural way. Still, there are several advantages to this approach. First it provides the most convenient and natural way to control for heterogeneity in worker characteristics. Second, it directly confronts the most common (supply side) approach to wage heterogeneity.

policy to redress it. It would seem crucial to understand what role sorting by firms plays in the generation of wage differentials between observable groups such as genders or races. [3]

While extending what we know about the wage effects of firms and jobs, this paper also provides new information on a narrower policy issue: the sources of wage losses among displaced workers. That displaced workers suffer significant wage losses and that those losses are positively correlated with tenure is well documented (Jacobson et al, 1993, Kletzer, 1990). Typically the correlation of these losses with tenure has been interpreted as suggesting that firm specific capital is important. Recently, several authors have questioned this conclusion, including Neal (1995). 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 premia. Note that if the loss of productive firm (or industry) specific human capital is the major source of wage losses, then displacement costs are social as well as private. On the other hand, if displacement costs represent the loss of rents or other wage premia, and *if* the quantity of such high wage jobs in the economy is constant, then displacement is largely redistributive.

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; extensive documentation of the data is provided in an appendix. 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 empirical firm wage premia. They are:

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

[3] The possibility that sorting of workers into high and low wage firms or, within occupations, into jobs, has been mentioned by some authors (England, 1992). There is very little evidence, however, relative to the much studied issue of occupational segregation.

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 compensation wage differentials is exposited in any labour 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 labour market. Note that these frictions need not be in the labour market. Frictions in the 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 clearing market 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) points out that the first three explanations are competitive explanations in the sense that the labour 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 "measurement" 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-i ndustry 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. Essentially the problem is omitted variable bias. 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[4]. 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.

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. Mobility is likely endogenous, which may introduce another serious bias. This is the strategy pursued by Abowd, Kramarz and Margolis (1995, forthwith AKM). AKM explore the issue of worker sorting versus "true" firm wage effects with a French panel data set. As discussed, endogeneity of mobility is a potential problem.

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, d ata 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

[4] 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.*

that, for these workers, displacement is orthogonal to the individual characteristics (particularly unobserved characteristics).[5] AKM have no information on reason for separation. In contrast, all the workers studied in this paper change jobs after a plant closure or mass layoff. Thus it is more reasonable to treat firm switches as exogenous.

AKM report that they find statistically significant firm fixed effects, but that these are dwarfed in magnitude by individual effects. In addition to the potential endogeneity problem, two other aspects of the AKM study are important in light of this result. First, AKM have rather poor 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. Secondly, France has very different wage setting institutions than Canada or the United States. Centralized wage institutions in France might be expected to lead to smaller firm wage effects than would be observed in a more decentralized labour market. For this reason, the Canadian data used in this paper may be more representative of the North American Labour market.

Finally, the literature has not evaluated compensating wage differentials as an explanation of firm wage effects. Thus this paper compliments 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. This may be a particular problem here in that it is firms that I am interested in. In my conclusions, 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 Labour 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

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

firm. The survey collected information on the standard demographics and human capital measures and hourly wages both in the pre-displacement job and in re-employment. Each worker 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 with in 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 description of the data is provided in Appendix 2.

There is now considerable evidence of inter-industry wage differentials (Krueger and Summers, 1998), 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 Appendix 2 (Table A2.2).

Because of the elapsed time between the displacements and the survey (less than 2.5 years), there is selection into re-employment at the survey date. An analysis of this problem is also presented in Appendix 2; it does not seem to significantly effect 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 labour. 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 what firm a worker works for is as good a mean square error predictor of wages as 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). Differen tials 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 10% less than average of 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[6].

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 [7], 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 attributable to the wage differentials, and has the same units as the wage differentials. The WSE is 18% in manufacturing and 12% within two digit industries. Thus a worker at firm paying one standard deviation above the industry norm could expect to earn a 12% premium. This is quite substantial when compared to the unconditional standard deviation of log wages at 30%.

There is evidence of workers sorting by productivity differences. Conditioning on observable characteristics reduces the WSE of firm differentials from 22% to 18% in manufacturing and from 16% to 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

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[6] 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.

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

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, 1998). 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 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 observed cross sectional wage differentials. I pay particular attention to "mismeasurement" explanations: unobserved ability and compensating differentials. Tables 5, 6 and 7 summarize alternative estimates of the firm wage differentials that exploit the longitudinal nature of the data (Table 5), employ alternative samples (Table 6), or augment the basic cross section specification with additional controls (Table 7)[8]. 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 tables 5,6 and 7, 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.

Tables 8 and 9 present some evidence on the correlates of the wage differentials. In Table 8 I rank the firms by their wage premia within manufacturing, and present information on the firms and the local labour markets they operated in. In Table 9 I present the results of simple regressions of the firm previously estimated firm wage premia on characteristics of firms and local labour markets[9].

Beginning with Table 5, and in all subsequent tables, I focus on the firm wage differentials within two digit industries.

4.2. Are Firm Wage Effects Unobserved Ability?

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

[9] 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.

In the previous section I presented evidence conditioning on observable characteristics reduced the apparent firm wage differentials. Clearly workers sort by productive 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 the 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. I have augmented my base cross section regression by a measure of the intelligence, strength, spatial thinking, and manual dexterity typically required in the job held by each worker. While these controls are statistically significant, they 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. I then examine the role of pre-displacement firms in determining post displacement wages. Gibbons and Katz point out that if the component of unmeasured ability that is sorted by firm is time invariant and generates the same return in different jobs, then industry wage effects can be consistently estimated by wage change regression for industry switchers. A simple empirical framework which captures these arguments is presented in Appendix 1.

Unfortunately, I cannot repeat this exercise exactly for firm wage effects. In particular, I cannot control for post displacement firms.[10] 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

[10] 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.

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 presented in the empirical framework of Appendix 1.

Table 5 summarizes the longitudinal estimates of the firm wages 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 Wald test for 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 sections estimates, presented in their entirety in the 2nd column 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. Of course, longitudinal estimates are restricted to this sample so these estimates provide a basis for comparison with across a consistent sample. They also provide, through comparison with the full sample estimates, a check of the effects of this 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 9% more in their pre-displacement jobs. Thus selection into re-employment is clearly correlated with unobserved variation in renumerable characteristics. However, as Table 5 reports, the cross section estimates on the re-employed sample are almost identical to those based on the full sample. 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 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 post-displacement four digit industries.[11]

From the Wald 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 11% less than the average of 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 4th 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", the WSE the difference and post-displacement estimates give a rough decomposition of the cross section wage differentials into the part

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[11] Many of the workers are employed in unique 4 digit industries, post displacement.

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 biassed down. In sum , the results indicate that there is significant sorting or 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 affect. 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 different occupation mix of workers. A "high wage" firm, for example, might be one that employees (relative to industry norms) a larger number of workers in risky occupations[12]. While my basic estimates control for differences between several six types of workers[13], 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 respondents 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. While these additional controls are statistically significant in the wage regression, 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 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

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

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

(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 this 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[14]. 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 premia. High tenure workers are disproportionally represented at "high wage" firms. Such a result suggests that a positive firm wage premia 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 evidenced in Table 8, there is very little difference in the mean reported hours

[14] Fringe benefits, for example.

across the plants, suggesting that firm wage differentials are not a premia paid for differences in hours of work.

While I can not approach the compensating differentials 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 labour markets has been suggested as an important determinant of displaced workers wage losses (Carrington,1992). Furthermore, if the firm wage effects reflect differences in monopsony power, they should be correlated with measures of the competitiveness of the local labour market. Local populations and unemployment rates are reported along side 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 labour market characteristics (Table 9). There may be some relationship between the size of the local market and pay, if Toronto is excluded. I have no rationale for this result.

Unlike firm wage differentials, a firm size wage effect on wages is well known. Tables 8 also reports the size of each firms 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 5th 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 employ 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 lopes rises to 0.0062 and the correlation of slopes with intercepts becomes more strongly negative at -.65. There are well known difficulties in the interpretation of the cross sectional relationship between wages and tenure. Obviously firm heterogeneity in the slopes has important implications for the literature that attempts to estimate the returns to tenure (Margolis, 1995).

5. Summary and Conclusions.

Most studies of wage heterogeneity focus on the returns to observable worker characteristics estimated in human capital wage regression framework. The focus is on the supply side of the labour market. Augmenting that framework, this paper has presented new evidence that firm of employment appears to be an empirically significant sources of wage heterogeneity. Even more than inter-industry wage differentials, these effects are *prima facie* evidence against the simple competitive model of the labour market, and its underlying assumptions.

A simple competitive model of the labour 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 firms 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 represented unmeasured characteristics of workers, then workers should continue to earn those premia if the switch firms. This is only partially borne out in this data. In fact, employing the longitudinal nature of 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 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's 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 over state 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 labour market theories that suggest that firms and their structure have an important role in determining labour 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 labour 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. Polices 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 premia. 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. While the destruction of specific human capital might represent a *social* cost of economic restructuring, if the observed losses represent instead a loss of rents (and, critically, if the number of "high wage" or rent sharing firms in the economy is not declining) the costs may be largely private, and displacement in fact "redistributive". Of course, this remains a speculative observation.

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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

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

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:

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

and that

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

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

$$E[\omega_1 z_{1j} | x_1] = E[\mu z_{1j} | x_1] \tag{8}$$

and

$$E[\omega_2 z_{1j} | x_2] = E[\mu z_{1j} | x_2] \tag{9}$$

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

$$E[w_\Delta^{/} z_j | x] = 0 \forall j \tag{10}$$

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:

$$w_t = x_t\beta + y_t\alpha + \epsilon_t \tag{11}$$

where the matrix y_t measures unobserved non-pecuniary aspects of the job. This specification of a compensating wage differential follows the empirical formulation of Brown (1989). If

$$E[y_1^{/} z_{1j} | x_1] \neq 0 \ \forall j \tag{12}$$

then:

$$E[\omega_1 z_{1j} | x_1] = \alpha' E[y_1' z_{1j} | x_1] \ \forall j \quad \textbf{(13)}$$

also:

$$E[\omega_2 z_{1j} | x_2] = \alpha' E[y_2' z_{1j} | x_2] \ \forall j \quad \textbf{(14)}$$

and

$$E[\omega_\Delta' z_j | x] = \alpha' E[\Delta y' z_{ij} | x] \ \forall j \quad \textbf{(15)}$$

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.

$$y_1 = y_2 \quad \textbf{(16)}$$

$$\Delta y = 0 \quad \textbf{(17)}$$

$$E[y_2' z_{1j} | x_2] = E[y_1' z_{1j} | x_2] \ \forall j \quad \textbf{(18)}$$

then

and

$$E[\omega_\Delta' z_j | x] = 0 \ \forall j \quad \textbf{(19)}$$

$$E[\omega_2 z_{1j}|x_2] = \alpha' E[y_1' z_{1j}|x_2] \;\; \forall j \qquad \textbf{(20)}$$

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.

$$E[y_1' y_2|x_t] = 0 \qquad \textbf{(21)}$$

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

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