Job Search Technique & Worker Quality: Do Job Search Methods Yield Higher Earnings due to Human Capital Differences?

Author:
Vikram Rai

Supervisor:
Dr. Allen Head

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Queen's University
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Contents

1 Introduction

Worker quality, measured by specific or general human capital, cannot be observed directly by econometricians, nor is survey data capable of effectively capturing the heterogeneity of similarly educated and experienced workers. This is a large problem for applied labour research in such topics as wages and unemployment duration because worker quality has a direct impact on all labour market outcomes. Worker quality decreases the duration of unemployment spells because more productive workers find employment faster and are able to use more effective search methods which reduce asymmetric information and other job search frictions. Worker quality also increases earnings, since more productive workers earn more in a competitive labour market, even in the presence of labour market frictions.

Significant unobserved heterogeneity between workers with similar observed characteristics is a challenge for obvious reasons: it enters each model essentially as an omitted variable, creating bias in undetectable ways. Though specification tests are capable of determining whether or not there is evidence of such bias, there is no clear way of decomposing that bias in models of, for example, unemployment duration or promotion path. This is particularly problematic for research that attempts to measure the effect of a treatment, such as the receipt of employment insurance, when receipt of that treatment is extremely likely to be correlated with the unobserved heterogeneity between similar workers. A good deal of the policy questions facing labour economics today involves precisely this problem: measuring how a specific treatment effects the behaviour of workers. Given that, a reliable proxy for worker quality is of utmost value. That said, "worker quality" is a vague and somewhat uninformative term, despite how heavily empirical work in job search has refered to it as the largest challenge in that field. Let us explore more fully what it really means.

Drawing on human capital theory, there are two possible formations of worker quality, which are not mutually exclusive. The human capital literature, which previously has not been connected explicitly to unobserved heterogeneity in empirical labour market research, posits two types of human capital: general human capital, which can be applied to any number of tasks and exists prior to the acquisition of education, training, and experience; and specific human capital, which is acquired by workers through education, training, and experience (learning by doing). "Worker quality", as envisioned by labour economists, can incorporate either or both types of human capital. First, it may encompass only general human capital, a time-invariant set of skills that is not derived from any of the worker's observable characteristics. If this is the form that the unobserved heterogeneity takes, then a wage equation over time conveniently conforms to common specifications for panel data, including random and fixed effects. Second, it may be the case that unobservable worker quality differences do in fact come from a worker's observable characteristics in that some workers attain a higher level of worker quality from the same level of investment in specific human capital. If this occurs, then some workers will receive a greater return to their education, training, and experience than others. In this formulation, worker quality changes over time and increases with education and experience; consequently, labour market experience and age will reinforce difference in worker quality. Finally, of course, there may be a combination of both effects in the labour force.

Because human capital takes both specific and general forms, the hypothesis that higher levels of human capital leads to more effective search is complicated by the possibilities that different types of specific human capital make workers more productive in different industries. Firms in specific industries may be more likely to accept applicants that use certain search methods simply because of prior knowledge that productive workers in that industry search for work through that channel. Intuitively, this certainly makes sense; the proverbial grapevine is not the same for lawyers and factory workers. This creates ambiguity as to whether job search methods are simply more effective in obtaining offers, or workers are signalling higher specific human capital by using specific search methods, or whether employers are either engaging in some matching process or are screening workers by search method. Many labour market questions are concerned both with creating incentives for unemployed workers to search for work and monitoring the search effort of unemployed workers, but if employers' responses are more significant in this process than simply the volume of direct applications made by workers, then much of that effort is unproductive.

Using prior empirical research and search theory as a baseline, I posit that workers who use non-traditional methods of finding employment—traditional meaning responding to public advertisements for open positions—are higher quality workers. Higher quality workers enjoy three main benefits on the labour market: first, they earn more because they are more productive, and in a competitive labour market, workers are paid their marginal product; second, they spend less time unemployed, because job search skills—and the ability to access more effective job search techniques—are correlated with worker quality, perhaps because being a worker of high quality serves as a gateway to those search methods; and third, they tend to enjoy quicker advanced and greater assumption of responsibilities in employment, which can be measured by receiving managerial responsbilities. The second difference is a consequence of their ability to exercise the channels of friends, relatives, acquaintances and other firms to "skip the queue" that low-quality workers spend time in before obtaining a job, and this requires social capital that also makes them more productive workers. The models in this paper will focus on the dynamics around the first observation, that workers who use non-traditional search methods earn more because they are higher quality workers, not because they are extracting rents or because the causality is in the opposite direction.

This paper is organized as follows: in the following section, I will review the literature in this area and the human capital research I will use to link job search technique to earnings and labour market outcomes. The next section describes a simple theoretical model, which shows how an offer function that responds to varying levels of human capital can produce two distributions of wages in a standard search model. Following that, I describe the dataset used and give an overview of the methodology I will use to measure individual-specific effects and determine whether or not they have a significant impact on labour market outcomes, before giving the empirical results and discussing any inference and conclusions that can be made.

2 Literature Review

This study draws on areas of literature, which have struggled to deal with the absence of an observable measure of worker quality. The field of empirical research in labour economics contains many studies which try to isolate the effect of a treatment on some labour market outcome or the behaviour of workers. Treatments studied vary from marriage, injury, and childbirth, to unemployment insurance, job tenure and worker's compensation benefits. Unobserved heterogeneity becomes an emprical challenge when it is correlated with the treatment in question—it is well-known that an omitted variable correlated with any of the regressors will bias parameter estimates.

A series of studies attempts to assess the effect of different employment insurance regimes across the world, all explicitly acknowledging the challenge posed by unobserved heterogeneity and searching for some way of dealing with this unobserved heterogeneity. Belzil (2001), which studies unemployment insurance reforms in Canada, uses a hazard model estimated by maximum likelihood in which the unobserved heterogeneity terms are integrated out in the estimation process, to produce an estimate of the effect of employment insurance on subsequent job duration. This is necessary since the correlation of worker quality with employment insurance receipt would appear in the estimates as spurious state dependence—workers who receive employment insurance would be shown to have shorter subsequent job duration, when in fact the determining factor is simply that lower quality workers apply for employment insurance. Belzil's 2001 study features the longest and most explicit discussion of the impact of unobserved heterogeneity in this literature, but the issue is addressed in a series of other studies by the same author (Belzil 1992, 1994, 1995a, 1995b, 1996).

Tatsiramos (2009) encounters similar difficulties in a broader study of a variety of employment insurance schemes in Europe, but is able to use the hypothesized correlation of unobserved heterogeneity with the selection equation for benefit receipt to solve an identification problem in estimating matching effects. This is a good deal more complicated than would be necessary if a direct measure of worker quality existed. Centeno (2004) conducts a similar study in the US context, and limits the study only to one unemployment spell per subject, to avoid bias due to unobserved heterogeneity being correlated with future spells. Unlike the other studies, Centeno (2004) is also concerned with unobserved heterogeneity between the same worker over time, which arise if individuals accumulate human capital at different rates from the same observable characteristics. These empirical studies are consistent in identifying that worker quality is unobservable and effects both wages and employment.

There is also a large series of studies analyzing the empirical efficacy of different search methods, the majority of which focus not on wages but on the likelihood of being employed and the subsequent duration of that employment. Addison & Portugal (2002) examine outcomes from job search methods in Portugal and determine that among the least effective search mechanisms is the state employment agency; if search is related to human capital, as I intend to argue, then this result stems not from the incompetence of the Portugese public service, but from adverse selection into the state employment agency. A few empirical studies focus specifically on the search method of social contacts; Bentolila, Michelacci, & Suarez (2010) produce a similar finding, presenting evidence that using social contacts reduces unemployment duration by as much as three months on average; curiously, however, this is accompanied by a wage discount, not, as anticipated by most theory, higher earnings. Cingano & Roslia (2012) present evidence that the employment status of workers own networks of social contacts has a significant impact on reducing unemployment duration. Holzer (1988), which offers both a theoretical model and an empirical exploration using data on youth cohorts, notes that a large portion of openings are filled by workers referred by a member of the employer's social network. The empirical studies into the effectiveness of different job search methods consistently associated non-traditional methods with better labour market outcomes, although they are not consistent as to which outcomes improve.

The theoretical studies on this topic are consistent on two fronts: first, that considering multiple search methods with differing outcomes on the part of both firms and workers implies a much higher equilbrium level of search intensity than previously believed, and the second, that for some reason, not all workers use the most effective search methods. Mortensen (1977) and Burdett (1980) outline the basic search theory which can be extrapolated to multiple search methods. Mortensen & Vishwanath (1994) apply this theory using informational asymmetries among equally productive workers; workers receive wage information according to some distribution, and earn more depending on their source of information. This model is entirely stochastic, but arrives at the conclusion that the simple presence of separate information channels will produce different wages. Holzer (1988) models search methods assuming that individuals choose how to search for jobs based on the wage distribution associated with each model; this analysis, however, presents the choice as a simple cost-benefit analysis only considering aggregate GE constraints, but no budget constraint for what individuals can exert in searching for work. As a result, this analysis does not consider the possibility that some search methods are not available to some workers. Cahuc & Fontaine (2009) take a somewhat contrarian position, arguing that the generally accepted concern of unemployment benefits creating moral hazard (see Lippman & McCall (1980), Mortensen (1977), and Burdett (1980) for theory on this; Belzil (2001) and Centeno (2004) for empirical studies)) is misplaced, and produce a theoretical model showing that in the presence of multiple search methods, search intensity is actually higher than the welfare-maximizing equilibrium.

Importantly, both Cahuc & Fontaine (2009) and Holzer (1988) also consider search effort exerted by employers, which can be extremely significant; this relates to the hypothesis mentioned previously that, if it is known by employers that workers with greater human capital tend to use non-traditional search methods, than search methods can serve as an effective screening mechanism for firms to block low-quality workers from consideration. This is also important if the search and matching process differs by industry. Rogerson, Shimer & Wright (2005) note in a long review of search and matching models that the literature overwhelmingly assumes a fixed search intensity whenever frictions in search are considered, but most models can be extended to endogenous search intensity. However, varying search intensity is primarily used to make modelling moral hazard easier, rather than to make any observations about search intensity itself—for example, with endogenous search intensity, a simple endogenous search function predicts that workers respond to an increase in unemployment benefits by reducing search intensity. Search intensity is rarely related to wages or human capital, only unemployment, employment, and the duration of unemployment and employment spells; the few papers in the literature which do relate search methods to wages, such as Mortensen & Vishwanath (1994), do not explore the possibility of productivity differences between workers or perform any empirical analysis.

3 Theoretical Framework

The chief purpose of this section is to present a simple theoretical framework relating wages to search method, under the assumption that specific time-invariant human capital endowments are required to access different job search methods. Both Holzer (1988) and Diamond & Vishwanath (1994) present a partial-search model with no equilibrium that can be adapted for this purpose. We begin by assuming no disutility from working, no labour-leisure tradeoff, and that for i search methods, there are i offer functions, each of which determines whether or not using the i^{th} search method results in a wage offer. Each offer function is associated with some cost C_j, from J workers, each with a different level of human capital H_j to "spend", representing the search effort of workers drawing from their human capital endowments. In this sense, the model is similar to game-theoretic models of wage discrimination, except that the source of the hurdle is not imposed by firms, but by the nature of the search method. Wage offers otherwise arrive exogenously according to some distribution $F(w^r)$. Previous models included a labour-leisure trade-off, which I omit for the sake of simplicity; including such a trade-off would not alter the following results. Within this framework, each unemployed worker j will attempt to maxmize the following objective function in each period:

$$\max_{W_j, w^R} U_t = \pi_i(C_j)[1 - F(w^R)] \times E(\phi(w)|w^r) + (1 - \pi_j)[1 - F(w^R)] \times U_{t+1} \quad (3.1)$$

$$\text{s.t.} H_j \geq C_j$$

Here, π is an offer function on [0, 1] depending on the value of C_j, which simply scales the outcome of the wage draw, and $F(w^R)$ is the distribution from which wages are drawn, while ϕ is the value function for being unemployed and U_{t+1} is the value function of being unemployed in the next period. H_j is the human capital endowment of the j^{th} individual, where j = 1, 2, . . . , n, and serves as an upper-bound on how effort each individual can spend searching. By the well-ordering principle, I can arrange each individual according to their human capital endowments into the interval [H_1, H_n], such that $H_1 \leq\ H_j\ \forall\ j\ \in\ [1, n]$, and $H_n \geq\ H_j\ \forall\ j\ \in\ [1, n]$. The first-order conditions of this objective function give the following relationships:

$$\phi(w^R) = U_{t+1} \quad (3.2)$$

$$C_j \geq \int_{w^R}^{\infty} [\phi(w) -\ U_{t+1}] f(w)\, \mathrm{d}x \quad (3.3)$$

Equation 3.2 gives the straightforward result that the value of wages at the reservation wage must be equal to the value of remaining unemployed in the next period.

The second first-order condition, in Equation 3.3, indicates that the cost of search effort must be greater than the value of declining a wage offer in the next period. For the sake of simplicity, suppose $i = 1, 2$; there are only two methods of searching, one available to high-quality workers associated with an offer function more likely to generate offers, and another associated with an offer function less likely to generate offers; that is, $\pi_1(C_j) \geq \pi_2(C_j) \; \forall \; j \in [1, n]$; more formally, we can also say the first offer function has strict first-order stochastic dominance over the second offer function. Note that, up until this point, the only assumption I have had to make regarding the distribution of human capital endowments is that there exist least and most endowed individuals. This assumption coupled with two exogenous offer functions that require different levels of human capital to access, proves enough to generate two different wage distributions.

Now we must specify the value functions $\phi(w)$ and U_{t+1}. Here we follow a familiar procedure, detailed in Rogerson, Shimer & Wright (2005), to determine two value functions, one for employed workers and one for unemployed workers:

$$\phi_j(w) = w_j + \beta\phi(w)\pi_i(C_j) \tag{3.4}$$

$$U_{j,t+1} = b + \beta \int_0^\infty \max[U_j, \; \phi(w)\pi_i(w)] \; \mathrm{d}x \tag{3.5}$$

Here, b is the value of unemployment benefits, w is the wage the employed worker is currently receiving, and β is the discount factor. The only difference between this and the standard formulation is the presence of the offer function $\pi_i(w)$ as well as the wage draw. The construction of the offer function I will outline implies two separate wage distributions. The offer functions $\pi_1(H_j)$ and $\pi_2(H_j)$ are such that $\exists$ H$_j*$ $\in$ [H$_1$, H$_n$] such that, $\forall$ H$_j \leq$ H$_{j*}, \pi_1$(H$_j$) = π_2(H$_j$), and $\forall$ H$_j$ >H$_{j*}, \pi_1$(H$_j$) > π_2(H$_j$). Solving the value function in Equation 3.4 results in an expression for the reservation wage, which can be written in any of the following forms:

$$w^R = b \ + \ \frac{\beta}{1 \ - \ \beta} \int_{w^R}^{\infty} \pi_i(w \ - \ w^R) \mathrm{d}F(w) \tag{3.6}$$

$$w^R = b \ + \ \frac{\beta \pi_i}{1 \ - \ \beta} \int_{w^R}^{\infty} [1 \ - \ F(w)] \mathrm{d}w \tag{3.7}$$

This result clearly shows that workers who posses a level of human capital greater than H$_{j*}$ will have a higher reservation wage. By partial differentiation of Equation 3.6, we get that:

$$\frac{\partial w^R}{\partial F(w)} = \frac{\beta \pi_i}{1 \ - \ \beta}(w \ - \ w^R) \tag{3.8}$$

Though this is not an equilibrium expression, let $\frac{\partial w^R}{\partial F(w)} = \kappa$ and isolate w:

$$w = \kappa \pi_i \frac{(1 \ - \beta)}{\beta} \ + \ w^R \tag{3.9}$$

Following the same procedure with Equation 3.7 and let $\alpha = \frac{\partial w^R}{\partial w}$ yields a similar result:

$$w = \alpha \pi_i \frac{(1 \ - \beta)}{\beta} \tag{3.10}$$

Again, neither of these equations are equilibrium or even partial equilibrium expressions, but since π_i and F(w) are modeled entirely exogenously, its implication is clear: the wage distribution for workers who can access a greater offer function will be higher. This result is generalizable to any number of offer functions, since the well-ordering principle will always allow for any number of pivotal values of j on the interval [1, n], or if **j** was a vector of pivotal values of specific human capital. Furthermore, this is generalizable to the case of multi-dimensional human capital (for example, if different industries had different pivotal values of j due to responses to specific, not general, human capital), where the offer function π(H) is a N-dimensional vector of functions on the space of human capital **H**, because the space H is always a complete space. Any closed subset will be compact and so the Euclidean logic applied above will hold. Therefore, we can conclude that existence of multiple offer functions which require higher levels of human capital to access better offers leads to higher wage distributions increasing in the amount of human capital.

4 Data & Methodology

The data for used in this paper comes from Statistics Canada's *Survey on Labour Income & Dynamics* (SLID), a panel micro-data set that follows over 32,000 individuals for six years, with a new panel beginning every two years. I will use the most recent panel, which ran from 2002-2007. The data-set includes a host of personal characteristics, major life events, employment and unemployment periods, and detailed information on the job(s) of each person. A description of the full set of controls used is given in Appendix A; the key variable of interest will be described in this section. The basic wage equation with which we are familiar is specified as follows:

$$logW_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 Y_i + \beta_3 exp_{it} + \beta_4 exp_{it}^2 + \epsilon_{it} \tag{4.1}$$

In this model, the matrix X_{it} contains a set of time-varying person-specific controls, excepting experience, and the matrix Y_i contains person-specific time-invariant controls, such as gender, race, immigrant status, marital status, etc. The empirical we wish to address is that this equation has proven unable to account for worker quality, which persists despite including education. This is captured by the inclusion of ϵ_{it}, which we cannot measure, in Equation 4.2.

$$\epsilon_{it} = \alpha_{it} + u_i \tag{4.2}$$

One should recognize this specification of the error term as conforming to a random effects model, which generally takes form given below in Equation 4.3.

$$Y_{it} = X_{it}\beta + \alpha_{it} + u_i \tag{4.3}$$

This model assumes that the person-specific and time-varying components of the error term are independent of each other, but does not suffer from the drawbacks of other estimation methods; note, for example, that we cannot used a fixed effects model to do any sort of interesting data with this panel data set-up, since many of the variables of interest will be time-invariant dummy variables, which fixed effects cannot estimate parameters for. We can parameterize u_i any way we like using maximum-likelihood estimation, since we must assume there exists a population mean μ, a mean person-specific effect u_i that follows some distribution (Gaussian, or normal in the case of MLE) with some variance σ_i^2 between subjects. This entire process is modelled as in Equation 4.4.

$$\widehat{\alpha_{ij}} = \mu + \alpha_{it} + \eta_i \tag{4.4}$$

The MLE estimate of the population mean is then simply:

$$\widehat{\mu} = \frac{1}{Nn} \sum_{i=1}^{N} \sum_{t=1}^{n} \alpha_{it} \tag{4.5}$$

where N is the number of persons and n is the number of years.

The SLID includes a key variable that will allow this model to be better calibrated. This is a categorical variable taking 9 values, breaking down which types of public advertisements individuals responded to, and also including networking, public service assistance, and other relevant categories. The random effects estimator will be computed using two formulations: one with each category included as a dummy, and one with a binary indicator for whether or not the job search method used was one requiring higher human capital, based on theoretical priors and results from previous studies (called the "High-Leaning Classification"). For the binary classification, when the prior was not clear based on theory, I erred on the side of categorizing a search method as "high", or one that would have a prior hypothesized effect of increasing earnings. That is why it is labelled "High-Leaning Classification". Table 1 indicates how the categorical variable is grouped to build a dummy, based on this categorization method. Because the panel runs from 2002-2007, the "internet" search category is presumed to correspond to individuals with higher human capital, since its prevelance in the job market was limited during that period. The same data for a more recent panel would require different priors.

The latter form of the model will be estimated largely because the sub-sample size of some search categories is small enough (< 50) to throw into question inference based on those parameter estimates. If the model is correctly specified and these variables account for much of the person-specific variation, then the estimate of u_i will be significant and small relative to the random effects estimate of the other parameters, estimated using itereated FGLS.

Job Search Method	High-Leaning Classification
Contacted employer directly	High
Friend or relative	High
Placed/answered newspaper ad	Low
Employment agency	Low
Referred from another employer	High
Contacted directly by employer	High
Union	High
Required for Social Assistance	Low
Searched the internet	High

Table 1: Breakdown of job search techniques by hypothesized human capital

Briefly, note that although fixed effects estimation is not a useful tool for hypothesis testing in this model, given the presence of some time-varying regressors, estimating a fixed effects model does have one use. Implementing a simple Hausman test comparing the fixed effects estimates to the random effects will test for the exogeneity of the person-specific effects; that should shed light on whether or not the random effects model sufficiently captures the person-specific effects, which the fixed effects model completely eliminates. If the random effects model is found to be superior, that is, obviously, evidence in favour of the specification described above.

Two possible routes exist for discerning whether or not individuals of different quality receive a different return to their observable characteristics, and I will implement both. The first is simply to segregate the regression by job-search type, using either narrow measure of worker quality described above, and compare coefficients. Given that there are likely to be sub-sample size issues that would make assessing any decomposition difficult, I will additionally employ an experimental random-effects ordered probit model, pioneered by Crouchley (1995) in order to determine whether or not search method is a useful predictor of education attainment, which can also be understood as prior human capital accumulation. If job search methods accurately predict prior education attainment, then we have strong evidence that job search methods reveal something about unobservable human capital, since they are significant for both earnings and educational attainment.

5 Empirical Results

5.1 Descriptive Statistics

Before delving into the main estimates, I wish to present a few descriptive statistics to provide a general picture of how earnings varies by job search method. Because of sample size restrictions and confidentiality concerns imposed by Statistics Canada on the use of the SLID, I reduced the job search variable to three categories for this section; a number of the nine categories from the full measure had sample sizes below the threshold Statistics Canada allows to be extracted. Table 2 gives the overall distribution of wages and the distribution by the search categories as grouped. The search methods are split into three groups, in increasing order of the prior hypothesized effect on earnings.

It is clear that the priors do hold—employment agencies and welfare/workfare requirements fare poorly, direct and public applications somewhat better, and more specific contacts, such as unions, direct contact from an employer, and referral from another employer, perform even better. The strong performance of unions is likely related to the structure of unions in Canada; large unions such as CUPE and CAW retain members through multiple positions and even during unemployment spells, so this result may not be generalizable to other countries. In general, I suspect the significance of union contacts in the models estimated in the next section is almost certainly idiosyncratic to Canada, but further research is required to verify that. These results are broadly consistent with previous empirical studies, eg Holzer

Sub-Sample of Log-Earnings	Mean (between) (within)	Observations (Individuals)	Average Person-Years
All	10.673 (1.337) (0.551)	154,022 (32,185)	4.9
Category 1	10.544 (0.952) (0.601)	1,553 (316)	4.9
Category 2	10.612 (0.952) (0.599)	8,592 (1719)	4.9
Category 3	10.805 (0.868) (0.577)	1,325 (269)	4.9

Cat. 1: Employment agencies and work required for SA

Cat. 2: Direct applications, internet applications, and newspaper ads

Cat. 3: Union referrals, referrals from another employer, directly contacted by the employer, & other

Standard deviations and number of individuals in parentheses

Table 2: Distribution of log-earnings by job search method

(1988) and Addison & Portugal (2001), excepting unions, which obviously were not as significant a factor in Holzer (1988), an American study on youth cohorts.

Three other features are worth noting. First, the within-groups variation is much smaller than the between-groups variation for each search category and for earnings as a whole. This will become important later, as the standard random effects estimator, computed by iterated GLS, is simply the matrix-weighted average of the within- and between-groups estimators. Second, since the search variable is reported only for those workers who were employed for the duration of the survey, it is time-invariant and significantly reduces the sample size, from 154,022 observations to 11,470 observations. The weighted mean of log-earnings for all three search categories is almost exactly the same as the mean of log-earnings for the full dataset, so I do not anticipate any sample selection issues. Furthermore, the sample size is still quite large and the panel still fairly strongly balanced. Third, the between-person variation is larger than the differences in means across search categories. Therefore, we should not be surprised if explanatory variables other than search method are more significant.

5.2 Random Effects

The main estimates for the random effects model is reported in column 1 of Table 3. A description of the full set of controls for this estimate is reported in Appendix A, while the variables of interest are reported here. This includes indicator variables for demographic and work information such as sex, race, disability status, and whether or not the job was managerial or permanent, as well as categorical variables for the individual's major source of income (eg, investment income, salary, wages), education, the reason the position was not permanent if applicable, and whether or not their work was related to their education. Age and experience were both initially included with squared terms to capture ambiguity in the direction of their impact; in every case, age was found to improve the fit of the model more, and including both created collinearity problems, so experience was omitted in all reported regressions.

The job search category "Contacted employer directly" is omitted to avoid multi-collinearity, so the parameter estimates reported are best interpreted as the difference in log earnings from contacting the employer directly. The search categories are jointly significant with a p-value < 0.03 from a Wald test of no effect. Briefly,

VARIABLES	(1) Log-Earnings	(2) Log-Earnings
Age	-0.0290***	-0.0280***
	(0.00772)	(0.00771)
Age^2	0.000304***	0.000300***
	(0.000103)	(0.000103)
Sex	-0.0274	-0.0219
	(0.0349)	(0.0346)
Friend or relative	-0.0188	
	(0.0426)	
Newspaper ad	-0.0386	
	(0.0600)	
Employment agency	-0.245**	
	(0.0958)	
Referral from another employer	0.186	
	(0.153)	
Contacted directly	0.0540	
	(0.0712)	
Union	0.407**	
	(0.162)	
Required for SA	-0.388	
	(0.945)	
Searched the internet	0.0965	
	(0.106)	
Other	-0.00708	
	(0.0729)	
HLC Search		0.103**
		(0.0500)
Constant	9.425***	9.312***
	(0.427)	(0.430)
Observations	8947	8947
R-squared (between groups)	0.25	0.25
Number of idcode	2210	2210

Standard errors in parentheses
*** $p < 0.01, **p < 0.05, *p < 0.1$

Table 3: Main estimates for random effects model

we can see that our a priori intuition was correct—referrals from employers, being directly contacted by employers, and obtaining a job through a union results in higher wages relative to simply applying for a position directly. Similarly, employment agencies and work requirements for social assistance are associated with lower wages. However, while these effects are jointly significant, it is worth noting that they are both less significant statistically and smaller in magnitude than many of the controls; for example, the province dummies are all jointly significant with a χ^2 of over 4,000 on a Wald test of no effect, and somewhat larger in magnitude than most of the reported coefficients for job search method. The estimates of spatial differences are reported in Table 4; the province of Newfoundland & Labrador is used as the base and omitted. Nevertheless, these results do indicate that job search method confers more explanatory power on wages than age, sex, or visible minority status.

This same model was estimated using the binary measure of job search methods, described in the previous section. This is done for several reasons: first, the sample size of some groups is small (eg, "required for SA" has less than 50 observations); second, it is easier for a group of 9 dummies to be jointly significant than a single dummy (especially in the maximum-likelihood models reported later, where likelihood ratio tests are preferrable to Wald tests on linear restrictions), so the significance of the category may be spurious. As column 2 demonstrates, the results are still significant with p-value < 0.05, and the estimated coefficient is substantial, though again has less of an impact than disability status and province. The province dummies for this model are report in column 2 of Table 4.

The random effects models reported up to this point all use the iterated FGLS

VARIABLES	(1) Log-Earnings	(2) Log-Earnings
NS	0.149	0.143
	(0.101)	(0.100)
PEI	0.108	0.0966
	(0.0906)	(0.0905)
NB	0.145	0.133
	(0.0927)	(0.0925)
QC	0.200**	0.198**
	(0.0797)	(0.0795)
ON	0.473***	0.469***
	(0.0765)	(0.0760)
MAN	0.252***	0.245***
	(0.0927)	(0.0924)
SK	0.146	0.141
	(0.0914)	(0.0910)
AL	0.304***	0.297***
	(0.0817)	(0.0815)
BC	0.259***	0.251***
	(0.0895)	(0.0891)

Table 4: Province dummies in random effects models are highly significant and large in magnitude

or GMM estimator for the coefficients described in Breusch (1987) and Hausman & Taylor (1981), which explicitly do not include the group mean as one of its moments. Lee et al (2011) note that maximum-likelihood random effects estimator is more appropriate both when the group mean is of interest and the explanatory variables are categorical variables in longitudinal or panel data. The MLE random effects estimates of the coefficients of categorical varibales is easier to interpret and conforms better to prior theoretical explanations. The MLE estimates are reported in Table 5. The full set of controls is the same as in the previous model. The maximum-likelihood random effects model produces similar results; employment agencies fare very poorly, and unions fare extremely well. The search category is significant with a p-value < 0.05 using both a Wald test and a likelihood-ratio rest. Also similarly, its estimated effect is greater than the estimated effect of age, sex, or disability.

5.3 Robustness & Specification Checks

As noted in the methodology section, the random effects model is never sufficiently parameterized, despite strong evidence from a Breusch-Pagan lagrange multiplier test that the data exhibit random effects—the null hypothesis of no time-invariant individual effects is rejected with a p-value of < 0.001, or a χ^2 of over 4000 for almost twenty different sets of controls; detailed results from a Breusch-Pagan test are reported in Table 6 for the full random effects model. There certainly are individual effects in this model; we can check a Hausman test to see if the specification of the random effects model in the previous section is appropriate, although I will argue that the test is not appropriate in this case.

VARIABLES	(1) Log-Earnings	(2) u_i	(3) $\epsilon_i t$
Age	-0.0290***		
	(0.00768)		
Age2	0.000304***		
	(1.02E-04)		
Sex	-0.0273		
	(0.0346)		
Friend or relative	-0.0187		
	(0.0423)		
Newspaper ad	-0.0385		
	(0.0596)		
Employment agency	-0.245***		
	(0.0951)		
Referral from another employer	0.186		
	(0.152)		
Contacted directly	0.0542		
	(0.0707)		
Union	0.407**		
	(0.161)		
Required for SA	-0.388		
	(0.94)		
Searched the internet	0.0961		
	(0.105)		
Other	-0.00718		
	(0.0724)		
Disability	-0.0867***		
	(0.0251)		
Perm Job	0.932**		
	(0.398)		
Constant		0.708***	0.612***
		(0.0136)	-0.00532

Observations: 8947

Number of persons: 2210

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Maximum-likelihood random-effects estimates of log-wages

In every model estimated—this is over 30 different combinations of controls from

Breusch-Pagan test of H_0: var(u_i) = 0		
	Variance	Standard Deviation
Log-Earnings	1.039	1.019
ϵ_{it}	0.392	0.626
u_i	0.600	0.775
$\chi^2 = 4809.25$		
P-value < 0.00001		

Table 6: Breusch-Pagan LM test of no individual effects on random effects model

those available and reported in Appendix A—a Hausman test comparing the random effects model to the fixed effects model rejected the null hypothesis of no systematic differences between the two estimators. If the random effects model were efficient and consistent, then we would have observed the opposite result; this is usually interpreted to mean that the random effects model is not fully parameterized, despite the large sample size and the inclusion of all variables believed to have an impact on earnings. A closer look at the Hausman test may explain why this occurred. The differences between coefficients contributing to the χ^2 value are reported in Table 7.

This table includes the full output of all time-varying coefficients in the estimated model. It is worth noting that, while the Hausman test rejects the random effects model, the coefficients in the fixed effects model do not conform to theoretical expections. Among other irregularities, education appears to have a negative impact on wages. It is safe to say that the fixed effects estimates look very noisy. Secondly, the Hausman tests finds extremely strong evidence of systematic differences between the two estimators, but these difference lie almost exclusively in the estimates of the

Hausman test on log wages (H_0: β is consistent and efficient)				
Variable	FE (b)	RE (β)	Difference (b - β)	ΔSE SE(b) - SE(β)
Total hours	.0001346	.000175	-.0000404	5.61e-06
Full-time	.0115093	-.0253296	.0368389	.0090041
Age	-.01435	-.0289895	.0146394	.0112531
Age^2	.0006534	.0003044	.000349	.0001503
HS	-.1276661	.0171255	-.1447916	.0283021
Bachelor's	-.1415378	.00919	-.1507279	.0429345
More than Bachelor's	-.1081764	.0830969	-.2912733	.0577423
Edu related to work	.0037742	.0573827	-.0536084	.010436
Edu somewhat related to work	.0402824	.073312	-.0330297	.0079588
Disability	-.0249762	-.1860433	.061067	.0103731
Perm Job	.7403711	.9301229	-.1897518	.0725367
Self-Employed	.0387459	.097438	-.0586921	.0422948
rsnp1	.7153933	.8895843	-.174191	.071972
rsnp2	.6834921	.8715561	-.188064	.0727229
rsnp3	.7958733	.9791315	-.1832582	.0729606
rsnp4	.610993	.7582721	-.1472791	.077045
rsnp5	.7014006	.9267623	-.2253617	.073565
NS	.6567084	.1492274	.507481	.2097573
PEI	.2939853	.1078801	.1861052	.1819895
NB	.4101536	.1447277	.2654259	.2302401
QC	.1205335	.1996309	-.0790973	.2459567
ON	.5153307	.4727977	.042533	.1817853
MAN	.6228986	.2523918	.3705068	.2113035
SK	.7913904	.1462601	.6451303	.2073128
AL	.3928594	.3043075	.0885519	.1634042
BC	.2845295	.2593438	.0251857	.1992967

See Appendix A for an explanation of the "RSNP" variables.
$\chi^2 = 114.53$
P-value < 0.001

Table 7: Differences between coefficients and standard errors in random effects and fixed effects model is largest for province dummies

coefficients for the province dummies. Most individuals only change provinces once over the six-year period, which results in a low contribution to the within-person R^2. This coincides with a very low within-person R^2 for all the random effects models estimated (< 0.09), and low with-person variation for all search categories as reported in the descriptive statistics in Table 2. Finally, if the Hausman tests were correct, we would expect the estimated individual effect u_i to be correlated with the explanatory variables in the model, but regressing the individual effect (the fitted values for u_i from both random effects models) on the explanatory variables yields only an overall R^2 of 0.04. The most important assumption for random effects is that the estimated individual not be correlated with the regressors, and it appears that that assumption holds, despite the Hausman test saying otheriwse.

Hahn, Ham & Moon (2011) note that empirical micro-economics researchers have often ignored the Hausman test because it is known to reject the null implausibly when the within-groups (or within-person, in this case) variation is quite small. They also provide a theoretical explanation for the intuition researchers have followed in ignoring the Hausman test. If the within-person variation is small, then the fixed effects estimates will not not be asympotically normal—this violates the basic premise of the Hausman test, which is that the fixed effects estimates are consistent. Their logic is simple to follow. If $\hat{\beta}$ is the between-groups estimator and $\overline{\beta}$ is the within-groups estimator, then, per Hausman (1978)[1], the Hausman statistic can be written

[1]Hausman (1978) notes that comparing random and fixed effects is equivalent to comparing the within- and between-groups estimators, providing the construction of Hausman's test statistics shown above.

as:

$$N(\tilde{\beta} - \overline{\beta})(\hat{\Omega}_{\tilde{\beta}} + \hat{\Omega}_{\overline{\beta}})^{-1}(\tilde{\beta} - \overline{\beta}) \tag{5.1}$$

If β is the parameter associated with X_{it} and γ is the paramater associated with the unobserved individual effect u_i in a typical panel data setup, then let $\beta_B = plim\overline{\beta} = \beta + \gamma$. Then the validity of the Hausman statistic in Equation 5.1 depends on the asymptotic normality of $(\sqrt{N}(\tilde{\beta} - \beta), \sqrt{N}(\overline{\beta} - \beta_B))$. If $\tilde{X}_{it}$ is the X matrix from the within-groups model, then the within-groups variation is simply $\sum_{1=1}^{N}\sum_{t=1}^{T}\tilde{X}_{it}^{T}\tilde{X}_{it}$. When this is small, the asymptotic normality of $\sqrt{N}(\tilde{\beta} - \beta)$ is not likely to hold, since $\tilde{\beta} = (\sum_{1=1}^{N}\sum_{t=1}^{T}\tilde{X}_{it}^{T}\tilde{X}_{it})^{-1}(\sum_{1=1}^{N}\sum_{t=1}^{T}\tilde{X}_{it}\tilde{Y}_{it})$. Consequently, the Hausman test may not be valid when the within-groups R^2 is as low as it is in the models I have reported. Intuitively, this is perhaps unsurprising, since over a period as small as six years, we would not expect much variation in earnings for a single person. Since a residual analysis shows no evidence of the assumption that corr(u_i , $\mathbf{X}$) $= 0$ is violated, we can conclude that the random effects model is sufficiently parameterized. The result of regressing the fitted values for u_i in the random effects and maximum-likelihood models against the regressors in those models are both not significant, with p-values > 0.1 on a Wald test of no effect, using both the full category models and the models with search method grouped into a binary variable. Likelihood-ratio tests for the MLE models similarly produce a p-value > 0.1.

5.4 Job Search & Human Capital Accumulation

The previous section does confirm that there is a time-invariant individual effect, and that job search methods are a significant predictor of time-invariant individual effects, which I interpret to be a form of human capital that distinguishes individuals with otherwise similar observable characteristics. There are two related issues I wish to explore: firstly, can it be said that job search method is related to human capital accumulation broadly, and secondly, do individuals who use more effective search methods earn more because they are inexplicably more productive, or because they have "better" observable characteristics? To answer the first question, I turn to the random-effects ordered probit model mentioned in the methodology section, originating in Crouchley (1995). Ordered probit models are a well-established method for dealing with a categorical response variable for which there is some natural order; the response variable I am estimating here is education, which was initially a categorical variable for four different levels of education. Because education is coded as "highest education level attained" while the job search variable refers to the current job the individual holds, it will typically be the case that the response variable in this model will have been realized temporally earlier than search method, the regressor of interest. This, combined with the panel data set-up, should account for most endogeneity concerns. The results in Table 8 show how the use of different search methods predicts to the probability of prior educational attainment. The full set of controls for this model is given in Appendix A, and is the same as those for the random effects models, excepting a few exclusions. The ordered educational attainment groups are "high school or less," "some university or college," "Bachelor's degree," and "more than Bachelor's degree".

Interpreting the results of the random effects probit model requires computing the probability that the normal-score is less then the difference between each coefficient and the estimated cut for a given outcome. Based on this model, for someone who has found their job through a union, the probability of having at least a Bachelor's degree is 99.1%. Likewise, the probability that someone who obtained their job through social assistance has at least a Bachelor's degree is 1.9%. The other results produce less stark average probabilities, but nevertheless, these results clearly support the hypothesis that search methods are significant in explaining prior education attainment. This is most easily interpreted as meaning that search methods area reliable proxy for a general human capital that both exists before workers obtain their highest level of education, and is significant as a predictor of their prior educational attainment and current earnings.

In regards to the second question, there are two ways of determining whether or not this is the case, both of which I borrow from the literature on discrimination. First, the random-effects and maximum-likelihood models are re-estimated using the same binary measure of search methods as before, except in this case, instead of including the search methods in the regression, the regressions are run separately for both values of the search variable. Put in as simple terms as possible, including dummy variables for search allows us, as in the previous section, to see whether or not the intercept of log-wages was different for different search methods, but tells us

VARIABLES	Edu	Cut 1	Cut 2	Cut 3	ρ
Age	0.810***				
	(0.0208)				
Age^2	-0.0100***				
	(0.000303)				
Sex	-0.148**				
	(0.0676)				
Friend or relative	-2.353***				
	(0.101)				
Newspaper ad	0.153				
	(0.130)				
Employment agency	-0.380**				
	(0.182)				
Referral from another employer	-1.574***				
	(0.350)				
Contacted directly	0.0471				
	(0.112)				
Union	-2.798***				
	(0.295)				
Required for SA	-19.86				
	(751.0)				
Searched the internet	0.116				
	(0.162)				
Other	0.0502				
	(0.106)				
Constant		10.13***	13.68***	16.98***	0.902***
		(0.310)	(0.340)	(0.360)	(0.00335)

Observations: 9575

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Random-effects ordered-probit model of effect on education of job search method

nothing about whether or not the slopes are different. Normally, the possibility of different returns to regressors for different values of a binary variable would be addressed by including interaction terms, but this is not possible without creating collinearity problems with many regressors are dummies. Segregated regressions is the obvious solution. A different paramter estimate for any particular variable would mean that individuals using more effective search methods consistently earn different returns to (for example) age or education, which has a number of possible explanations. The results from estimating the models separately are reported in Table 9; the first two columns refer to the estimates for the less effective search-method population.

An important advantage of estimating the two models separately is that it is now possible to use a fixed effects model, advantageous because at least one of the fixed and random effects models must be consistent. The only reason we were unable to use it before is that the job search method variable is not time-varying, and so it was impossible to get an estimate of its effect on earnings from such a model. Given that there is at least some ambiguity as to whether or not random effects is appropriate, however, a fixed effects model that confirms the result serves as a powerful robustness check. These results are also given in Table 9; the third column refers to less effective search methods for the fixed effects specification. It is fairly clear from these tables that the slope coefficients of all parameters is quite different, even within specification, for the select sub-samples, and the R^2 for the fixed effects model is well within an acceptable range for fixed effects models. The MLE models from earlier are also re-estimated separately for different search methods. These results are reported in Table 10.

VARIABLES	(RE-Low) lnearnings	(RE-High) lnearnings	(FE-Low) lnearnings	(FE-High) lnearnings
Age	-0.0445*	-0.0243***	-0.0513	-0.00236
	(0.0234)	(0.00819)	(0.0378)	(0.0146)
Age^2	0.000538*	0.000249**	0.00128***	0.000479**
	(0.000311)	(0.000109)	(0.000486)	(0.000196)
Sex	-0.0512	-0.0216		
	(0.0983)	(0.0371)		
Disability	-0.0318	-0.0992***	-0.0276	-0.0265
	(0.0632)	(0.0274)	(0.0654)	(0.0297)
Perm Job	0.0333	1.424***	-0.0436	1.185**
	(0.659)	(0.499)	(0.638)	(0.512)
PEI	0.200	0.138	0.543**	0.783***
	(0.343)	(0.105)	(0.024)	(0.237)
NS	0.254	0.0753	0.507	0.394*
	(0.349)	(0.0937)	(0.861)	(0.210)
NB	0.456	0.0890	-1.766*	0.557**
	(0.325)	(0.0966)	(0.926)	(0.257)
QC	0.421	0.160*	-2.234**	0.326
	(0.270)	(0.0833)	(0.868)	(0.271)
ON	0.558**	0.461***	-1.508**	0.653***
	(0.257)	(0.0799)	(0.673)	(0.210)
MN	0.441	0.209**	-2.493***	0.886***
	(0.284)	(0.0991)	(0.826)	(0.244)
SK	0.553*	0.0910	2.525**	0.934***
	(0.315)	(0.0951)	(1.071)	(0.234)
AB	0.402	0.278***	-2.519***	0.543***
	(0.286)	(0.0850)	(0.793)	(0.188)
BC	0.620**	0.201**	-2.925***	0.555**
	(0.298)	(0.0935)	(0.791)	(0.229)
Constant	10.44***	8.890***	10.97***	7.681***
	(0.821)	(0.524)	(1.236)	(0.624)
Observations	1203	7744	1203	7744
Number of Persons	302	1908	302	1908
R-squared			0.189	0.197

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Random and fixed effects estimates segregated by search method

As the results from all three specifications show, users of more effective search methods have greater returns to earnings than those who use search methods which are (at least) correlated with lower earnings. This supports the hypothesis that it is not search methods themselves which provide a greater return to earnings, but something about the workers that choose, or perhaps more appropriately, are able to choose those search methods. This would not be the case unless the same characteristics which made workers choose certain search methods also made employers pay them a higher wage—in a competitive labour market, even with frictions, the only explanation for this is that those workers are more productive.

6 Limitations

The SLID is an extremely high-quality data set that makes possible research that would not be otherwise; however, as would be expected for surveys, it is not without issues that limit the strength of conclusions based on its use. Two of the variables used may pose problems for the models reported in this paper. First, the key variable describing how individuals obtain jobs is not reported for the vast majority of individuals. Out of the 17,000 individuals in each panel, only at most 2,200 had observations for any period. There may be a sample selection issue, or there may simply be a data collection issue, since the SLID surveys are self-reported. In either case, this should be considered a potential source of bias. Moreover, low sub-sample size issues were most prevelant in search categories explicitly associated with a lower

VARIABLES	(Low) Log-Earnings	σ_u	σ_e	(High) Log-Earnings	σ_u	σ_e
Age	-0.0428*			-0.0243***		
	(0.0229)			(0.00818)		
Age^2	0.000511*			0.000249**		
	(0.000304)			(0.000109)		
Sex	-0.0505			-0.0216		
	(0.0942)			(0.0370)		
Disability	-0.0328			-0.0992***		
	(0.0626)			(0.0274)		
PEI	0.214			0.138		
	(0.330)			(0.105)		
NS	0.253			0.0753		
	(0.337)			(0.0935)		
NB	0.471			0.0890		
	(0.313)			(0.0964)		
QC	0.433*			0.160*		
	(0.260)			(0.0832)		
ON	0.571**			0.461***		
	(0.249)			(0.0797)		
MN	0.459*			0.209**		
	(0.275)			(0.0989)		
SK	0.559*			0.0909		
	(0.304)			(0.0950)		
AB	0.414			0.278***		
	(0.276)			(0.0848)		
BC	0.649**			0.201**		
	(0.290)			(0.0934)		
Constant		0.699***	0.602***		0.710***	0.612***
		(0.0393)	(0.0146)		(0.0147)	(0.00571)
Observations	1203	1203	1203	7744	7744	7744
Number of idcode	302	302	302	1908	1908	1908

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Maximum likelihood estimates segregated by search method

return to earnings. The lowest sub-sample size for search categories was that of "Required for Social Assistance", which is an extremely important category for making any inference about why search methods are correlated with earnings.

The other key concern I would like to highlight is that the SLID lacks a useful indicator of the industry of each person or their employer. Several industry groupings usings NAICS and NOCS are available, but none of them have a high enough response rate to be useful. When added to the model estimated, exclusions reduced the total number of observations to < 100, from 8904. Not only is industry likely to be extremely significant for wages, but is also likely true that search methods interact with industry in some way—it may be the case that accessing the same search methods requires a specific type of human or social capital in different industries, in which case we would observe different parameter estimates for search categories by industries, perhaps both in magnitude and sign. Search methods may be different across industries and professions because firms in specific industries wish to hire individuals whose search methods demonstrate that they would be productive workers in their industry. The specific human capital necessary to be productive differs by industry, and so to would the effect of search methods. I am hopeful that future panels of the SLID will offer more data on the industry of the firm or job in question.

7 Conclusions

Job search method has an effect on earnings, but the effect is not as simple as a typical treatment that produces a differential from a base. There is some statistical evidence that job search methods effect earnings directly by increasing the likelihood of securing employment, and consequently increasing reservation wages, and indirectly, by increasing the returns to other regressors. The most successful search methods were, in order of estimated effect, unions, referrals from another employer, and being directly contacted by employers. Contacting employers directly and responding to ads on the internet or in media were similarly less effective, and jobs obtained through employment agencies or for Social Assistance requirements were consistently estimated to yield the lowest wages. Two of these conclusions are likely artefacts of Canadian institutions—employment agencies includes, for example, EI job placement assistance, a program idiosyncratic to Canada, while the efficacy of unions is certainly related to Canada's well-known high rate of unionization; policy-makers should pay attention to the effect of these policies on earnings and evidence of potential adverse selection problems for government-sponsored employment assistance programs. Additionally, job search methods proved to be an effective predictor of previously obtained education, indicating that similar unobservable characteristics lead to education and the choice of job search method.

That said, while search methods were significant, their effects were not large in magnitude compared to other, well-established factors. The most significant factor for predicting wages, by far, was province of residence—regional inequality is a commonly discussed issue in Canada, and its prevelance is borne out by the data. Likewise, other factors, such as what an individual's major source of income was (eg, pension, salary, investment income), also tended to be more significant determinants of wages. It is important not to overestimate the usefulness of these results; the primary benefit of relating job search method to human capital is that it can reliably used to distinguish between individuals with the same observable characteristics. It is not an argument, however, for ignoring those observable characteristics. There is significant interaction between job search method and an individual's observable characteristics, and any attempt to understand earnings through observations about job search method is incomplete without recognizing that.

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A Appendix: Description of controls

SLID Code	Description
age26	Age of respondent in reference year
disabs26	Binary variable, 1 if disability status in reference year
eovm15	Binary variable, 1 if visible minority
fllprt1	Binary variable, 1 if job was full-time
fsein28	Binary variable, 1 if self-employed incorporated
hleveg18	Categorical variable for education; grouped into "did not graduate high school", "graduated high school", "less than Bachelor's", "Bachelor's",, and "More than Bachelor's"
immst15	Binary variable, 1 if immigrant
majri42	Categorical variable for primary income source, grouped into "no income", "wages and salaries", "self-employment income", "government transfers", "private retirement pensions", and "other income"
manag1	Binary variable, 1 if perceived managerial
managl1	Binary variable, 1 if management level
prmjb1	Binary variable, 1 if permanent job
pvres25	Categorical variable for all ten provinces
reled1	Categorical variable for relationship between work and education, grouped into "closely related", "somewhat related", and "not related at all"
sex99	Sex of respondent
tothrw1	Total hours worked in reference year

Table 11: Full set of control variables used in estimated models

List of Tables