



THE EFFECTIVENESS OF TRAINING FOR DISPLACED WORKERS WITH LONG PRIOR JOB TENURE

STEPHEN R.G. JONES





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Executive Summary

Workers displaced from long-tenure jobs often have difficulty finding new employment and can take a substantial drop in earnings in the new job. Canadian evidence shows that displaced workers with at least five years' tenure on the old job have an average earnings loss of 25-30 per cent, even many years after the initial job separation. These losses are large and persistent; they dwarf the transitory losses from the initial period of nonemployment. Policy response for these long-term problems has centred on education, training and skill development. How effective are such policies likely to be?

This turns out to be a complicated question to answer. Displaced workers differ in many ways, not all observable, and any one displaced worker can only be observed either with training or without training. Policy needs to answer the counterfactual question of what would have happened, if this particular worker had made the other choice, to assess the net effect of training. The paper surveys and assesses a variety of strategies that have been employed to determine training effectiveness, using results from field experiments and from econometric work based on non-experimental data.

Unfortunately, findings from this large research enterprise are not encouraging. Both experimental and non-experimental research shows that the returns to training for displaced workers are low, almost surely less than the (well-estimated) returns to formal schooling which lie in the 6-9% range. On a cost-benefit basis, the body of evidence does not show that training pays off for most of the displaced population.

Alternative means to compensate the losers from economic adjustment might include modified or expanded EI coverage, without any necessary link to training expenditures, and perhaps consideration of alternative policies, such as Wage Insurance. Since evidence on training programs for displaced workers gives only limited promise, it is important to search for other creative ways to ensure that the costs of economic restructuring do not fall disproportionately on a narrow group.

The Effectiveness of Training for Displaced Workers with Long Prior Job Tenure

Stephen R.G. Jones

Displaced workers are individuals permanently laid off from a long-tenure job. Such workers pose a major policy challenge for three reasons. First, many face great difficulty finding a suitable new job and may experience a significant jobless spell. Second, when re-employed, many of the displaced take a substantial earnings drop, relative to the old job. Third, displaced workers' earnings losses appear very persistent in the months and years that follow. Aggregated over the period following a displacement, these earnings losses can dwarf the initial short-term income loss from unemployment and have hence become the central focus of the literature on training effectiveness.

Policy response naturally begins with the EI program, the centerpiece of Canadian adjustment programs. However, much support provided by EI—especially EI Part I—is passive support for a presumed temporary spell of short-term joblessness. While important for the job search of a displaced worker, and while other adjustment programs such as job search assistance may usefully supplement basic income support, EI does not address the larger problem posed by substantial long-lived earnings losses. Policy to tackle such earnings deficits is rather based on education, training, and skill development as means of bolstering the human capital of the displaced. A key issue is then the evaluation of the effectiveness of training. This forms the main focus of this report.

Magnitude of the Problem

The Incidence of Displacement

Although there is considerable US evidence on displacement, based largely on the biannual Displaced Worker Survey (DWS),¹ Canadian evidence is relatively thin. Without a regular Canadian DWS, it is hard to track patterns and changes in the number of displaced workers and the nature of their experience.² The most comprehensive Canadian evidence comes from Morissette et al. (2007) who use the Statistics Canada Longitudinal Worker File (LWF). These data integrate four administrative sources and cover 10 per cent of Canadian workers for 1983-2002, although this time frame means that most of the job separations they study are at least a decade old. The age of these data is obviously an important qualifier of the analysis' relevance to the current policy debate.

Definitions of displacement vary, with some being based on plant closures and others relying on mass layoffs to identify separations that are exogenous from the worker's point of view. Moreover, results vary depending on whether the prior job tenure of the group of displaced workers is limited. For prime age (25-49) displaced workers, Morissette et al. find that the incidence of permanent layoffs lies in the range 6.6-9.1 per cent for men and 3.4-5.3 per cent for women. Of these, about 10% were firm closures and about 20 per cent were layoffs linked to either firm closures or mass layoffs. On the broader layoff definition, these incidence rates mean that some 50,000 men were displaced in 2002, about one sixth of the 300,000 men laid off (for any reason) in that year. Analogous figures for women are about 23,000 displacements and 137,000 lay-offs in 2002. An important point is the concentration of displacement within a relatively small group: most layoffs are not displacement.³

Seniority is important for the study of displacement since, as we shall see, losses rise sharply with prior job tenure. In terms of incidence, Morissette et al. find that, on the firm closure layoff definition, about 10 per cent of all displacements had tenure of five years or more. Such long tenure workers represent less than 1 in 100 of all annual layoffs.

Magnitude of the Problem

Unemployment Spells and Earnings Losses

Canada lacks clear evidence through time on the unemployment experience of displaced workers following their job separations. One would like to track the distribution of unemployment durations, compared with that for the non-displaced unemployed, and to see the evolution of such policy-relevant magnitudes as the proportion of EI eligibility used and the frequency of EI exhaustion among the displaced. Without regular DWSs, though, the available evidence is thin. Campolieti (2009) presents data on the experiences of job losers, but the analysis encompasses a much wider group than those typically viewed as displaced. Gray and Finnie (2009a, 2009b) use the Longitudinal Administrative Database, covering much the same time period as Morissette et al., and report that surprisingly few of the older displaced rely heavily on EI. And Bernard (2009) provides recent Canadian evidence on unemployment durations, although attention is not restricted to displaced workers.

Overall, in both Canada and the US, research on displaced workers has paid relatively little attention to short-term unemployment and non-employment following job separation, focusing rather on earnings losses in new employment. Nonetheless, some evidence suggests that Canadian displaced workers face longer unemployment spells than the average for the newly unemployed as a whole, reflecting their lack of familiarity with job search, potential mismatch of their skill sets with current labour market needs, and perhaps a reluctance to settle for lower wages. Both EI and job search assistance are likely of some help in cushioning this impact of displacement. But such transitory problems are secondary to the more enduring problem of lost earning potential.

Morissette et al. assess earnings losses that displaced workers experience in new employment. There are many permutations of sample, definitions of displacement and estimation method, but the pattern of results is quite consistent. For the high seniority group, Morissette et al.

compare earnings in year 5 after job separation with those in year 4 prior to displacement, calculating the change as a proportion of the pre-displacement earnings.⁴ The mean earnings drop is 25-35 per cent (men) and around 35 per cent (women) based on all displacements, and 25-34 per cent (men) and 35-37 per cent (women) based only on displacements owing to firm closure. For displaced workers at all seniority levels, and using all displacements, the earnings drops are 16-22 per cent (men) and 22-31 per cent (women).⁵ Clearly, these are large losses five years after a displacement.

For older workers not covered by the Morissette et al. sample aged 25-49, a complicating issue is the potential for self-selection out of the labour force and into (possibly early) retirement. Such selection would prevent potential earnings losses from being observed, although the resulting estimation bias could go in either direction.⁶ The best evidence comes from Schirle (2007) who models this selection process carefully. She concludes it is not empirically critical: older workers face potential displacement earnings losses that are similar to those experienced by the prime-age displaced.

In summary, the balance of Canadian evidence is that many displaced workers cannot avoid an earnings loss. For workers with significant prior tenure, these losses are substantial. A ballpark figure from the best Canadian research would be around 25-30 per cent for those with at least five years' tenure, together with a figure of perhaps 20 per cent for displaced workers at all seniority levels. Such earnings losses are typically long-lived, with no sign in the data that they dissipate even five years after the displacement.

Causes of the Problem: Potential for Skill Upgrading?

There is conclusive evidence that displaced workers typically suffer substantial and potentially permanent earnings losses. Does this mean that these earnings losses are the result of a skill deficit? And, as a distinct issue, would skill upgrading help displaced workers deal with these earnings losses?

The causes of displacement earnings losses are probably many and varied. Human capital that is *specific* to a job or firm may grow with tenure and be associated with substantial wage growth, and yet be non-transferable to a new job or firm following displacement. Key to assessing the importance of this cause is whether human capital is specific or general and, if specific, whether the specificity applies at the level of the firm, occupation or industry. A leading alternative is a long-term contracting framework wherein risk-neutral firms provide insurance against wage fluctuation to their risk-averse workers by making wages smoother than the corresponding path of productivity. Similarly, incentive pay schemes may use deferred compensation to overcome agency problems (related to work effort) within the firm. These agency models have low wages (under-compensation) at low tenure counterbalanced by high wages (and over-compensation) at high tenure levels, a pattern consistent with displaced earnings losses that are increasing in tenure. Finally, there are also theories of wage premia based on union wage effects, efficiency wages, and models of rent-sharing.

It is also important to note the considerable econometric literature that tries to disentangle the empirical wage-tenure relationship. In this work, the key is that wages and job tenure are *co-determined*, both resulting from workers' and firms' choices in equilibrium. Consequently, while a positive cross-section correlation between wages and tenure could arise from one or more of the above theories, it could also be *spurious*, driven by unobserved heterogeneity in worker, firm or worker-firm match type.⁷

This is obviously not the place to survey this econometric work extensively. Rather, I simply summarize this literature by suggesting that the simple least-squares estimate from cross-sectional data probably represents an upper bound on the true returns to tenure. If correct, this body of research would conclude that, while part of the displacement earnings loss could be attributed to a loss of a tenure-related wage premium, a substantial part of the loss would be for other reasons.

I now turn to the question of whether displaced workers need skills upgrading. If earnings reflect accumulated skills (with some firm or industry specificity) that are lost on displacement, then skill upgrading and replacement is a natural policy response. A thornier issue arises when lost specific human capital is *not* the main reason for earnings deficits. What if the earnings drop after displacement is due to long-term contracting reasons, such as deferred compensation, or industry-specific rents, or even a loss of a premium unionized job? Is there an argument that an upward-sloping wage profile for incentive reasons constitutes a "valid" cause of a pre-displacement wage premium, while a union rent does not? I have three comments.

First, it is hard to determine the cause of a pre-displacement earnings premium for any individual displaced worker. This parallels the practical, empirical difficulties many have encountered in attempting to identify the cause of displacement (trade, technical change, shifting final product demand).

Second, pragmatic policy might aim to compensate the losers from adjustment, regardless of the cause. Such compensation garners political support for policies that favour change, dynamism and economic growth, the argument being that, while the benefits of economic adjustment may be widespread, the costs of adjustment may narrowly be borne by a few. That said, a clear case where policy might economize on compensation costs is where pre-displacement earnings were unusually high for transitory reasons. Compensatory policy focused on long tenure displaced workers would naturally achieve this goal.

Third, does it then follow that the best mitigating policy should be to promote skills upgrading? There are comparatively few other policy levers that can raise post-displacement wages, although perhaps job search assistance can help secure better worker-firm matches without directly augmenting skills. But I think the answer to this question must be *contingent*, based on empirical evidence about effectiveness and efficiency of investments in skills. If the displaced were essentially unable to learn new skills to a level that paid off in terms of higher earnings, say, then perhaps some other form of compensation policy would be more suitable. But if there were a sufficient payoff to training and the acquisition of skills, then skill upgrading could help compensate the displaced. The effectiveness of training for this population is key.

Training

Private and Social Returns

The private gain from training may exceed the social benefit, particularly in cases of *substitution*, whereby trainees benefit from jobs that they compete away from non-trainees. Trainees could displace non-trainees, and “displacement” is very much the *mot juste*. With many data sources, it may be hard to assess the importance of this effect without direct evidence on the labour market outcomes of non-trainees. General equilibrium effects could undo apparent training rewards but it might be hard for the analyst to discern.

Substitution may also be relevant for job search assistance. If their employment-related services genuinely improve match quality, then a worker’s private gain might be mirrored by a social gain flowing from enhanced productivity. But if the private benefits of job search assistance derive mostly from outcompeting those without such assistance—without a concomitant increase in productivity—then the private benefits of job search assistance could mask little or no social return.⁸

Training

Barriers to Private Provision?

The case for *public* provision of training for long-tenure displaced workers also depends on an elaboration of the barriers to private provision. Credit constraints and other imperfections in capital markets, specifically the inability of individuals to borrow against future labour income, are often thought relevant to an explanation of the distribution of education achievements by income level. However, the evidence for credit constraints remains indirect and disputed, especially as higher family income can also improve the household environment conducive to the growth of cognitive and non-cognitive skills for education. Indeed, Carneiro and Heckman (2002) conclude that teenage ability far outweighs family income as a determinant of participation in post-secondary education. This opens up a voluminous literature of which a reasonable summary might be that the importance of credit constraints for human capital choices remains much disputed.⁹

With regard to displaced workers, *self-financed* training in particular may be subject to credit constraints and some aspects of public policy appear to be structured in light of this.¹⁰ But again the evidence is mixed. Chapman et al. (2003) use the 1995 Canadian Out of Employment Panel (COEP) to examine self-financed training among job losers and suggest that liquidity does affect these choices. Yet they also note that credit constraints are not reported as the main reason why the COEP job losers do not undergo self-financed training. The extent that training is rationed owing to credit constraints remains an open issue.

Public intervention in training could also have a role even absent credit constraints, provided the private outcome involves equilibrium *underprovision* of training. Such non-optimality can arise for several reasons. Wage compression (for institutional or legal reasons) may prevent workers from “paying for training” by accepting low initial wages: a minimum wage might thus discourage appropriate provision of training. Empirically, though, most evidence from

the US and Canada suggests that this effect is small. Other types of wage compression, relative to marginal products, can also generate potential suboptimality of private training (Acemoglu and Pischke, 1998, 1999, 2003). Examples would include imperfect labour markets, transaction costs, asymmetric information, contracting reasons relating to the eliciting of effort and diligence, and the interaction of general and specific skills. To give one example, “poaching” threats may make firms wary of too much investment in their workforce for fear of losing skilled workers to competitors. Asymmetric information may make it hard for workers credibly to signal that they have useful general skills to other employers, since the current employer may wish to keep such productivity information private, and this in turn produces suboptimal incentives for skill investments.

Two points on the optimality of private training bear emphasis. First, proponents of these non-competitive models of training with wage compression do not have specific recommendations to offer: “...we currently lack the type of detailed empirical information necessary to make precise policy recommendations” (Acemoglu and Pischke, 1999, pF128). Second, if inefficiencies in training arise for *non*-credit market reasons, the fix is probably not a program of loans. Indeed, attempts to solve non-credit problems by credit market subsidies could be counter-productive, perhaps generating overinvestment. Understanding the origins of suboptimal training investments is a prerequisite for knowing how to fix the problem.

Issues in the Evaluation of Training Methodologies

A large and developed literature exists on the economics and econometrics of active labour market programs, particularly training schemes, and research in this area can rightfully be regarded as at the forefront in the development of techniques for empirical program evaluation. Methodological issues fundamental to the assessment of causation, and to the separation of program *impacts* from program *outcomes*, are comprehensively assessed in Heckman et al. (1999) and more recently in Imbens and Wooldridge (2009).

The fundamental problem in an assessment of causation is that, for a given individual, one can only observe the effect *with* treatment or the effect *without* treatment.¹¹ The counterfactual of what would have happened to a treated individual, had they not received the treatment, is unobserved. Comparison of the treated and the untreated may offer insight but is problematic if the treatment itself is endogenous: one group may *self-select* into treatment (training) and this self-selection may be associated with different characteristics than for the group that did not self-select. Individuals may also be selected into treatment by program administrators, perhaps in a well-meaning effort to help those in greatest need, or perhaps by selecting those most likely to succeed (“creaming”). If differences between the groups influence response to treatment, the untreated group may be a poor proxy for the true counterfactual. To tackle these problems using nonexperimental data, research methodologies have included instrumental variables, panel data estimates, selection-correction models and more recently matching and regression discontinuity estimators.

The major alternative in program evaluation is to use experimental data where assignment to treatment is random (independent of observed individual characteristics and potential outcomes). For some problems, experimentation can yield good estimates of the counterfactual and hence more reliable estimates of program impact. In the specific context of training, LaLonde's influential paper (1986) showed that a variety of econometric methods, applied to data drawn from a true experiment, were unable to match results from the randomized experiment itself.

Subsequent work has improved on these methods, however, and many have argued that randomized social experimentation may deliver answers only to very specific questions, such as the mean difference between two groups (Heckman, 1991). When labour market interventions are multi-stage, the scope for clean experimental evaluation may be limited.¹² "Randomization bias" may contaminate experimental results: the behaviour of potential participants may be altered by the random assignment itself, perhaps by selection on risk aversion. Further, experiments are undoubtedly expensive and may pose significant practical, political, technological or ethical problems. Finally, experimental estimates usually reflect a partial equilibrium effect of an intervention. If treatment confers an advantage in employment prospects, say, a positive partial effect for the treated may or may not be at the expense of employment of the untreated control group, depending on the extent of displacement or substitution of non-trainees.¹³

Overall, I remain agnostic in the debate about experimental and nonexperimental methods and results. Although random assignment does not solve all problems, it has at times yielded more robust and credible estimates than were possible from nonexperimental data alone. Moreover, it has generated advances in statistical and econometric methods to deal with problems arising from such nonexperimental data.¹⁴

Effectiveness of Training Programs for Displaced Workers

Most of the voluminous literature on training program effectiveness, based on US evidence, has studied *disadvantaged populations* with few skills, checkered attachment to the labour market, and generally poor wage and employment prospects. Lessons from such a population do not apply directly to a population of Canadian displaced workers that exhibits past strong attachment to the labour market, evidenced particularly in long tenure and strong earnings on the old job. Accordingly, I will restrict my review of empirical results almost completely to research based on samples of displaced workers.¹⁵ Moreover, I follow the training literature in a focus on long-term outcomes, specifically earnings replacement and recovery following displacement. This contrasts with attention to short-term re-employment outcomes, about which there has been less research. This longer-term focus reflects the greater overall importance of earnings changes over subsequent years and decades, relative to the transitory costs of initial joblessness.¹⁶

Important results on training effectiveness for the displaced fall into four groups. First, I review findings from (experimental) evaluations of displaced worker programs conducted in the US in the late 1980s, including demonstration projects in Buffalo, Texas and New Jersey (Leigh, 1990, 1994). Second, there are credible nonexperimental studies of the effect of community college courses on displaced workers based on administrative records from Washington

State (Jacobson et al., 2005a, 2005b, 2005c). Third, I assess some preliminary results for the US Workforce Investment Act (WIA), legislation that replaced the Job Training Partnership Act in 1998 (Hollenbeck et al., 2005; Heinrich et al., 2009; and Decker 2009). Finally, I review the small Canadian literature on the effectiveness of training in light of the international evidence.

Dislocated Worker Demonstration Projects

The US Department of Labor's Dislocated Worker Demonstration Project began in October 1982 to study measures to aid the adjustment of workers displaced from manufacturing. It followed the Downriver program in Denver which had studied service delivery for the displaced following closing of an auto part plant in August 1980. Although the Downriver program used treatment and comparison groups, workers were *not* randomly assigned on an individual basis; rather, *plants* were determined to be treatment or comparison plants, and workers were then selected at random from within these plants. The assignment by plant casts doubt on the positive results on earnings found in some Downriver programs (Leigh, 1990).

Although there were seven sites involved in the Dislocated Worker Demonstration Project, the chief impact analysis was confined to Buffalo. The "target plant" sample in Buffalo was drawn from workers laid off from six steel and auto plants in the 12 months starting October 1982, the sample being male, white and married and with average prior job tenure of more than ten years: most had experienced a lengthy period of unemployment prior to the Project. A formal lottery generated random assignment to available program slots and resulted in 281 treated and 516 controls. The treatment consisted of job search assistance (JSA) with a possible follow-up of either classroom training (CT) or on-the-job training (OJT). Leigh notes (1990, p30) that the CT in Buffalo was typically very short-duration, while the OJT was chiefly a placement tool involving an employer wage subsidy. Some 55 per cent of participants received neither CT nor OJT.

The Buffalo Demonstration Project showed an impact on average weekly earnings of \$134 from JSA alone for the target plant sample, an effect that is statistically significant (5 per cent level) and economically important (Leigh, 1990, Table 3.3). With a mean prior wage of \$10.78, this earnings impact was substantial, being about 28 per cent of pre-layoff earnings (based on a 40 hour week). However, there were *no significant effects* of supplementing this with either CT or OJT. Since Leigh reports that average costs per participant were \$851 for JSA compared with \$3282 for CT+JSA and \$3170 for OJT+JSA, a conclusion from the Buffalo results is that the only potentially cost effective treatment was JSA.

An important limitation of the Buffalo Demonstration Project stems from the construction of the treatment and control groups. Specifically, individuals who were randomly assigned for treatment but chose not to participate were included as members of the comparison group. This creates a difficult self-selection problem that undermines random assignment. Moreover, the participation rate among recruited target-plant workers was only 16 per cent (Leigh, 1990, p27), so this selection was quantitatively important. Although efforts were made to model individual participation decisions, including a selectivity variable in the final outcome specification (Corson et al., 1985), difficulties with exclusion and other identification restrictions mean that such results lack the credibility of those from the initial random assignment design.

Results for a broader range of displaced workers were provided by the Texas Worker Adjustment Demonstration (WAD). The two main sites were Houston, where the displaced were mostly male petrochemical workers with relatively high prior earnings, and El Paso, where mostly female Hispanic workers had been displaced from low-paying jobs in light manufacturing. Under the WAD, individuals were randomly assigned to three groups: JSA (termed “Tier I” in the WAD evaluation), JSA+training (termed “Tier I/II”), and no treatment (beyond other services available in the community). Unlike the Buffalo Demonstration Project, recruited non-participants were not included in the control group. Further, unlike Buffalo, the participation rate under the WAD was fairly high, with 71 per cent of those assigned to a treatment group choosing to participate (Leigh 1990: 32).

Results show some overall increase in annual earnings and weeks worked for the treated, with a substantial difference along gender/location lines. While men in both Houston and El Paso had short-term (annual) earnings impacts of \$750 and \$770, neither was statistically significantly different from zero. In contrast, the women in El Paso had a program-induced gain in earnings of \$1070, a large and statistically significant figure.¹⁷ Analysis of the time-frame of earning gains, though, suggests that much of this female impact was very short-lived (Leigh, 1990: Table 3.4).

The relative performance of JSA and training can be assessed using the WAD only for the Houston site, and only for men. Overall, the Tier I package of JSA yielded an earnings impact of \$860, while the augmented Tier I/II combination of JSA+training yielded an impact of \$680 (Leigh, 1990: Table 3.5). Thus, the net effect of the augmentation of JSA with training appears to be *negative*. Two interpretations were proposed: first, that skill training takes time, so that serious job search may wait until training is completed, which could result in poor short-term earnings impacts; and second, that the training offered, mainly CT in skilled manual trades (e.g. air conditioning installation and maintenance), was poorly matched to the nature of the Houston target group, who were largely white-collar workers laid off from high paying petrochemical jobs. While both interpretations probably have some merit, the latter was judged critical by the authors of the initial evaluation and highlights the key role of the *match* of training to workers’ characteristics and to labour market demand conditions. Whatever the reasons for the negative return to training beyond JSA, though, the results are in line with the Buffalo evidence. With average costs of \$1531 for Tier I (JSA) and \$4991 for Tier I/II, and with no apparent benefits from the more costly treatment, only JSA might be justified on a cost-benefit basis by these data. Whether training better matched to the clientele would yield greater benefits, and whether these benefits could cover the cost of such provision of training, are not questions that the WAD evaluation can answer.

The third major demonstration project conducted in the 1980s was the New Jersey Unemployment Insurance Reemployment Demonstration that ran in 1986-87. Like the Texas WAD, it had the aim of evaluating an on-going system, rather than of assessing a one-time crisis intervention, but unlike the WAD, it also offered some advantage in the length of the follow-up period available for analysis. The intake group was targeted to be UI claimants with at least three years of prior job tenure and the NJ Demonstration used several screens after four weeks of benefit claim to filter out unemployed who did not qualify as displaced. Random assignment was implemented in week five of benefit claim following a (mandatory) job search workshop and a counseling/assessment session, and amounted to one of three treatments: JSA only, JSA+CT/OJT (subject to some conditions on chosen training schemes), and JSA+Reemployment Bonus.

Training was here limited to upgrading existing skill sets, rather than learning an entirely new profession. The bonus treatment was a payment directly to the claimant of 50 per cent of the remaining UI entitlement if lasting full-time reemployment was achieved within two weeks of the bonus treatment offer, with a schedule that reduced the bonus by 10 per cent each week (and reaching zero 11 weeks after the offer).

Results from the NJ Demonstration imply an earnings impact in the quarter following the initial UI claim of \$160 for the JSA+bonus group, compared with \$125 for JSA alone and \$82 for the JSA+training group, with the first two impacts being statistically significant (Leigh, 1990: Table 3.7). All three figures rose in the second quarter but declined substantially by four quarters after the UI claim and were insignificantly different from zero. The effects of these initiatives seem to be quite transitory. Moreover, longer-term follow-up over the subsequent six years confirmed that those randomly assigned to the offer of retraining did no better than those assigned to receive only JSA (Corson and Haimson 1995). However, it should be noted that, among the group assigned to JSA+training, only 15 per cent actually engaged in training, pointing out the role of *take-up* as well as the initial assignment.¹⁸

Overall, although random assignment and the demonstration project structure offered promise, firm conclusions from the various dislocated worker demonstration projects remain limited. Design issues, low take-up and the specificity of many of the sample populations have restricted the lessons that can be learned. The best summary is that these demonstrations found small positive effects of job search assistance, probably large enough to merit provision based on cost-benefit analysis. However, the demonstrations found uniformly small effects, sometimes negative, for training programs beyond basic job search assistance, and a robust conclusion is that these more expensive programs could not meet a simple cost-benefit test.

Nonexperimental Studies of Displaced Worker programs

There are evident limits to the questions that can be resolved directly from the demonstration evidence, at least without the addition of important additional statistical assumptions and econometric modeling. Moreover, with small and often distinctive (non-representative) samples, the external validity of the demonstration results is open to question. Finally, the follow-up period to study dynamics following an intervention is often very short. As a consequence, research has also used nonexperimental data to study the effects of existing training schemes and other policies for the displaced. Key issues in this research are how to control for differences between trainees and non-trainees, both along observed dimensions and also allowing appropriately for unobserved heterogeneity.

A leading example of such research is the analysis of administrative data on Community College training in Washington State conducted by Jacobson et al. (2005a, 2005b, 2005c). Using a threshold of three years' prior tenure, Jacobson et al. study some 97,000 cases of displacement with separations between 1990 and 1994.¹⁹ Of these, about 16 per cent had earned at least one Community College credit by the end of 1996. Importantly, substantial federal funding for displaced worker retraining had not yet been implemented so that most of the Community College courses taken in the 1990-94 Washington State sample were *self-financed*. Comparing trainees and others, Jacobson et al. report that participants in Community College were younger than

other displaced workers, had somewhat lower job tenure (in line with the age difference), were more likely female and somewhat more likely to be rural. Interestingly, they also note that participants were substantially more likely than non-participants to have attended Community College in the past.

Overall, these Community College participants complete an average of two-thirds of a year of schooling. To evaluate the effects, Jacobson et al. employ two models, together with a hybrid combination of the two. The first model is a *binary program evaluation approach*, comparing adjusted post-program earnings of Community College participants and non-participants. The adjustment amounts to a regression on observed characteristics, plus allowance for unobserved characteristics that are fixed or that change at a steady rate (based on individual earnings growth in the pre-displacement period). The second model estimates a *return to Community College* that is assumed to be proportional to the credits earned. Given strong enough proportionality assumptions, one could estimate a return to Community College credits using only participants, although there would be valuable further information in the earnings data from the non-participants.²⁰

The hybrid approach used by Jacobson et al. builds on the proportionality model but allows for a further discrete effect of *any* Community College participation (as in the binary framework). Perhaps the benefits of Community College are strongly non-proportional, based on networking contacts quickly acquired at College. Or perhaps the unobserved heterogeneity is not fully captured by the adjustment Jacobson et al. are able to make based on past earnings. In some cases of this type, the hybrid model may correctly estimate the return to Community College credits when either of the two preceding models would be biased.

Jacobson et al. allow for a distinctive pattern of displacement effects on earnings through time, with a potential dip prior to displacement, a more substantial drop in the quarter after the separation, and a relatively rapid rise in the next few quarters, followed finally by slow to zero increase thereafter. Further, they allow for dynamic effects after Community College training, with a potential transition period after Community College ends (a period when earnings often fall initially), followed by a period of potential growth in the year or more thereafter. In terms of the sample size, the quality and accuracy of the administrative data, and the resultant capacity for flexibility in the econometric modeling, the papers by JLS make an important contribution.

Estimation allowing for a transition period after Community College yields a long-run positive effect of schooling of 9.3 per cent of post-displacement earnings for men and 7.6 per cent for women, both under the binary specification (Jacobson et al., 2005c: Table 2, p285). When estimated assuming proportionality, analogous estimates of the impact on earnings are 11.7 per cent for men and 10.4 per cent for women. For both cases, estimated effects immediately after Community College are negative, with the effects becoming positive several quarters after the end of the courses. Both sets of returns are substantial, perhaps slightly higher than generally accepted estimates of the return to formal schooling (in the 6-9 per cent range, as in, e.g., Card, 1999).²¹ These figures represent the strongest set of non-experimental results for proponents of training for the displaced.

In the richer hybrid model, JLS find high estimates of the value of “just showing up” since this binary indicator appears to raise earnings by 6.8 per cent for men and 5.5 per cent for women.

While this could be a networking-type effect, Jacobson et al.'s preferred interpretation is that its magnitude indicates residual selection into training. Observed controls may not be capturing all of the individual heterogeneity in factors such as motivation and dynamism, and remaining differences may be reflected in more motivated individuals being more likely to enroll for Community College training.²²

When earnings time trends are also included as controls, in addition to fixed effects, the pattern of results remain fairly consistent but the level of returns tends to rise (Jacobson et al., 2005c: Table 3). In the most general specification, the return to a year of Community College training represents an earnings gain (as a percentage of post-displacement earnings) of 9.4 per cent for men and 13.1 per cent for women. Jacobson et al. interpret the rise in the estimated returns when worker-specific time trends are included as a form of compensation in the enrolment decisions of workers: those with relatively slower prior earnings growth tend to self-select into more courses as a means to compensate for their slower earnings path. Naturally, allowance for this type of selection raises the estimated effect of training.

A further important contribution of Jacobson et al. concerns *course content*. Specifically, they aggregate courses into two groups: "Group 1" comprises academic courses in mathematics and science, together with more vocational courses related to technical trades, technical professions and health; and "Group 2" comprises all other Community College courses, notably academic courses in the humanities and social sciences, and vocational courses in less technical fields. Results (Jacobson et al., 2005c: Table 5) from a variety of specifications show that the Group 1 courses have much larger impacts than those from Group 2. Indeed, in their preferred model, JLS estimate earnings gains from a year of Group 1 credits to be nearly 14 per cent (of post-displacement earnings) for men and a staggering 29 per cent for women. However, the analogous estimates for Group 2 credits are around 4 per cent but with standard errors large enough that they cannot reject the null hypothesis of no earnings effect whatsoever. These gains can be decomposed into about one third wage gains following Group 1 courses with the remainder being a consequence of increased work hours.

The age pattern of Community College enrolment and returns in JLS is also worth noting. Older men and women are both much less likely to enroll or to complete at least one course than the younger displaced, and they also record a lower average number of credits completed. This is standard in models of age and schooling, since the older have fewer years remaining to reap the benefits of education, may have higher foregone earnings costs, and may find schooling harder after a longer period out of the classroom. However, the completion probability for at least one credit (conditional on Community College enrolment) is not age-dependent, and the number of credits earned, given some completion, is also flat across the age distribution. It seems to be choices about enrolment, not success rates for enrollees, which govern the overall age difference in Community College training completion.²³

The evidence from Jacobson et al. is probably the best available nonexperimental source on the effects of training for displaced workers and it does offer some promise of success, at least for the right type of course. That said, there are limitations and it is important to keep these in mind when evaluating results for policy purposes. First, Jacobson et al. are rightly cautious about the size of the "just showing up" effect in their hybrid model, deeming such estimates as "implausibly large" (Jacobson et al., 2005c, p298). Such returns to even *one* Community College

credit are probably a sign of self-selection into training that has not been fully accounted for in the model specification. Second, Jacobson et al. follow Ashenfelter's (1978) analysis of the Manpower Development and Training Act, specifically his specification test based on "backcasting", which estimates whether training appears to have effects *before it actually occurs*. In a variety of specifications, (Jacobson et al., 2005c, Table 7; and JLS, 2005b, Table 4), Jacobson et al. find that participation in Community College schooling predicts earnings *prior* to enrolment, a clear flag suggesting model misspecification. It is hard to sign the bias likely indicated by this, however, since it depends on the potential persistence of the unobserved factors beyond the fixed effects and the worker-specific time trends. Participation in Community College retraining tended to be high for workers with unusually large unexplained earnings drops between the pre- and post-displacement periods. If these drops were temporary—a phenomenon dubbed "Ashenfelter's Dip" in the literature (Ashenfelter, 1978)—then estimated returns to Community College are likely too large, while if they reflect permanent losses from displacement, the estimated returns to Community College are probably an underestimate of the true value.

Finally, it bears emphasis that the Jacobson et al. results are only directly applicable to the set of displaced workers studied who chose to participate in Community College training, even if all of the other issues surrounding nonexperimental data are correctly handled. The results would not apply directly to displaced workers in Washington State at this time who chose not to participate. They would also not apply directly to the hypothetical set of workers who might be induced to participate if, say, public policy had offered enhanced subsidies to the cost of Community College education. Since hours changes appear to drive a majority of the earnings gain impacts from the Group 1 courses, there may also be suspicions about substitution, with trainees potentially replacing other workers so that the partial equilibrium effects might exceed the general equilibrium outcome.²⁴ And of course the broader external validity of the estimated results must remain. We do not know how well the results would apply in other places and at other times.

Workforce Investment Act (WIA) Results

The most recent US evaluation results on training effectiveness derive from the WIA of 1998. The goal of the WIA was to replace the piecemeal system of training programs under the Job Training Partnership Act (JTPA) with a unified and consolidated system that nonetheless gave local and state agencies flexibility in program design and promoted client choice. The WIA program currently serves over 2 million people, costs over \$3Bn annually, and has three key elements: an adult program, a youth program, and a dislocated worker program. The first two of these elements are targeted largely at the disadvantaged, so it is the dislocated worker program that is our main focus. Decker (2009) provides a valuable overview of the first decade of the WIA.²⁵

To date, no large scale experimental evaluations have been conducted on the effects of the WIA on dislocated workers. However, two substantial studies have assessed these effects using nonexperimental data and methods. Heinrich et al. (2009) use matching estimation methods to assess the effects of WIA services (as a package) and WIA training (as a specific component) for dislocated workers, finding very disappointing results. Initial effects on earnings are negative, which may not be surprising given the Jacobson et al. results earlier, but there is only

modest improvement even three or four years out (2009: Table VI.1). Indeed, they conclude that “it appears possible that *ultimate gains from participation are small or nonexistent*” (2009: p58, *emphasis added*). By demographics, they found somewhat worse performance for men than women but little other variation by race, age and veteran status, possibly because sample sizes were quite small for these sub-groups.

The other study of the WIA’s effects on dislocated workers’ earnings (Hollenbeck et al., 2005) seems contradictory, since it suggests positive impact estimates of \$1008 per quarter for men and \$895 for women, with the largest effects occurring immediately. However, Decker (2009) notes that Hollenbeck’s methodology uses the program exit point as the start of the observation period so that any earnings foregone during WIA program participation is essentially ignored in the calculation of a return. Once this methodological difference is amended, Decker suggests that the two papers are in fact fairly consistent. When direct and opportunity costs of WIA services are included, Decker argues that Hollenbeck’s results on earnings impacts show a substantially negative return to WIA.²⁶

In closing, I note that both sets of authors remain somewhat tentative about the nonexperimental results from the WIA, a viewpoint reinforced by Decker’s (2009) overview. Until an evaluation with some random assignment is conducted, as with the US Demonstration Programs on the 1980s, doubts will remain that results reflect as much on the sensitivities of the methodology as on the nature of the underlying WIA-induced outcomes.

Canadian Evidence on Adjustment Policies

Finally, I comment on assessments of Canadian adjustment policies. A valuable account of the various Federal Targeted Programs for this type of population is provided in the Expert Panel on Older Workers (2008: Table 1, p20).²⁷ Most such programs were short-lived and aimed at narrowly defined groups, particularly those affected by the restructuring of the fishing industry, and insofar as the programs were formally evaluated, the outcomes were quite discouraging.²⁸ Such programs were discontinued in 1998, in view of this perceived lack of success, although a number of alternative pilots have been tried. In October 2006, the Targeted Initiative for Older Workers (TIOW) was launched, and the TIOW is currently funded to March 2012, aiding the adjustment of workers aged 55-64. I know of no extant evaluation of the TIOW, nor whether one is planned.

There is some recent unpublished Canadian research that has looked at *self-financed* training and its effects. Frenette et al. (2010) identify training by exploiting the tuition credits and education deductions present in the T1 tax files that comprise part of the LWF. Such credits indicate some self-financed training within the calendar year covered by the T1 form, including the count of months for which an education deduction is claimed, although for technical reasons they adopt a binary indicator of training uptake (rather than a proportion of the year). Note that this training must be limited to postsecondary education and that the tax data captures attendance rather than completion. Methodologically, Frenette et al. model selection into the treatment (i.e., into postsecondary attendance) using a latent variable that depends on the distance between the individual’s home and the closest postsecondary institution. This distance proxies for some of the costs of such education, building on work by Card (1995). The identify-

ing assumption (exclusion restriction) is that this distance measure does not affect the returns to postsecondary education, given the selection process.

In preliminary results for those aged 25-44 in 1997, Frenette et al. find evidence of positive selection into postsecondary education. Controlling for this selection, there remain substantial effects from postsecondary education on earnings. For men, on a base of approximately \$30000 in paid earnings in 1997 (all figures in 2007 dollars), estimated effects of formal training on earnings rise through time and peak nine years post-displacement at an earnings gain of \$6400. Analogous figures for women, on a \$20000 base, the peak effect comes with a \$7100 earnings gain after five years. There are obviously very large effects, relative to the literature, and warrant further investigation. That said, it is worth noting that Frenette et al. find no significant effects for men aged 35-44 (the older part of their sample), so there are potentially interesting differences across demographic groups.

Finally, despite these apparent benefits, Frenette et al. report that the take-up rate is very low for men, even among the younger group that appears to benefit the more. Take-up following displacement is larger for women, both young and older. It remains puzzling why men appear so reluctant to pursue this nominally beneficial training. One possibility is that the geographic controls do not truly control for selection into formal training (i.e., the identifying assumption is not valid) and that the returns to training are overstated by results from the sample of displaced workers who choose to pursue postsecondary education. It will be interesting to see future work using these data and methods.

Another set of unpublished work concerns a variety of active labour market measures implemented by Emploi-Quebec (SOM, 2003, 2006) using both federal funds from the Canada-Quebec Labour Market Development Agreement and provincial funds in supplement. Specifically, the focus was on training, wage subsidies and active counselling as means to facilitate transition back to employment and to enhance earnings on the new job. On some readings, this work has been interpreted as a counter-example to the broad sweep of conclusions from North American research on the efficacy of active labour market measures.

It is difficult to evaluate research that has not been subject to peer review and published in standard outlets, partly because documentation of the research design and assessment of the conclusions may be incomplete. In the case of these Quebec studies, the voluminous unpublished reports do furnish some documentation, however, and highlight two key points.

First, the authors were aware of a number of selection-related biases in estimation (e.g., SOM, 2003, pp8-9). Nonetheless, the underlying design remained one where, relative to a treatment group whose members received active measures, the control group was comprised of eligible individuals who did not participate in these programs. Clearly, individual choices to participate would therefore affect membership in the treatment/control group and a fundamental self-selection bias is unavoidable. As such, it is probably incorrect to term such a research design “quasi-experimental” (a term first used on page 246 of the 2003 report), since there was no plausibly exogenous variation that generated assignment into treatment and control groups.

Second, the report nonetheless attempted to address selection bias, both by using propensity score matching methods and by a version of Heckman selection-bias correction estimation.

For both, predicted probabilities of participation in active measures are used to separate the treatment and control groups, so the estimation results hinge on the quality of these predictions. The report (SOM, 2003, p9) documents that these predictions were based on personal demographics, job characteristics, and utilization patterns of EI in the pre-participation period. While it is hard to know what more could have been done, absent a truly experimental or quasi-experimental design, none of these proposed instruments seems unrelated to the likely outcomes of the active measures. Therefore, the exclusion restriction required of a valid instrument would fail and the estimates raise issues of credibility that haunt evaluations without random assignment.

Summary of Evidence on Training Effectiveness

Overall, my summary assessment is that most training programs have had results on earnings that are modest to poor. There has been less work on initial re-employment after displacement, probably because these costs are small relative to the long-term cost of earnings losses. Nonetheless, to the extent that short-term results are a key policy target, the long-term focus of the existing literature could constitute something of a mismatch of evaluative resources.

Since there are few experimental evaluations for displaced workers, results are necessarily thin, but based on these evaluations and available nonexperimental studies, conclusions on effectiveness are mixed and perhaps inconclusive, at best. It is not clear that past training for displaced workers has paid off as an investment. Moreover, even if returns to training were significantly positive—which is probably an over-optimistic assessment—on the order of the return to formal education, say, the investment in training necessary for the long-tenure displaced to cover their earnings shortfall is staggering, perhaps an order of magnitude higher than any such investments in the past. With any important role for self-selection into training based on returns,²⁹ and with the possibility of diminishing returns to training at an individual level, training may not solve the problem of displacement.

Concluding Remarks

The robust conclusion of this paper is that training of displaced workers is not a panacea. Job search assistance is the single adjustment mechanism that appears to pass clear cost-benefit tests in terms of earnings replacement across a range of studies, and even in this case, there are still doubts about general equilibrium effects if the assisted merely substitute for the unassisted. For training schemes beyond Jacobson et al., though, the broad sweep of results is much less sanguine. In limited cases and for particular displaced populations, training that was technically oriented has yielded returns on a par with those typically found in the literature on formal education, that is to say, returns in the high single digits. But given such returns, and the size and duration of earnings losses following displacement, the level of investment in training needed to bridge these gaps fully would be huge. Moreover, with allowance for selection into training, and if returns to training for any individual are diminishing, existing estimates of the marginal return to training may dramatically underestimate the required investment to meet

the full displacement earnings shortfall. Although training might be a good investment even without complete compensation, existing calculations suggest that substantial foregone earnings might undercut such internal rate of return assessments for many displaced workers.

One general lesson from this review is the importance of building evaluation strategies into policy programs. Much earlier Canadian work did not have such assessments in place, and hence has yielded little in terms of robust conclusions. But the US experience is quite clear in showing the benefits of such evaluation and the problems that can arise in its absence.

A second conclusion is to raise the issue of the goal of training programs. Based on long-term calculations, where earnings losses in reemployment trump more transitory losses from periods of nonemployment, almost all of the evaluative literature in economics has addressed the extent of earnings replacement. If a major goal of policy is the short-run, however, then more attention needs to be paid to jobless durations following displacement and to the probability of reemployment, regardless of the salary and terms that such employment might offer.

Finally, these problems with training effectiveness prompt serious consideration of alternative means of compensating the losers from economic adjustment. Such steps might include modified or expanded EI coverage, even without any link to training expenditures. Current policies base eligibility only on recent work history, although displaced worker earnings often decline in the final year or two prior to displacement from the long-term employer. Obviously, a longer time frame for EI work histories might benefit such displaced workers. Moreover, the duration of EI may not be sufficient to assist the displaced following a separation. Indeed, if earnings losses are permanent, no temporary EI framework can really meet this need. It is for these types of reasons that many have proposed serious evaluation of a Wage Insurance (WI) program for the long-tenure displaced.³⁰ Such a program would raise new challenges, and perhaps some sort of pilot or demonstration of WI would be appropriate. Certainly, since evidence on training programs for displaced workers gives quite limited promise, it is important to search for other creative ways to ensure that the costs of economic restructuring do not fall disproportionately on a narrow group.

Endnotes

1. See, e.g., the sequences of papers using the DWS by Farber (1993, 1998, 2004, 2005).
2. Canada had one DWS in 1986, covering separations in the period 1981-85.
3. Morisette et al. also supply figures for those aged 50-64, a group they omitted from their analysis for fear of confounding issues of non-participation. Nonetheless, this older sample yields similar results. Rates of layoff and displacement are close to those for the prime-aged group and the relative magnitude of layoffs, narrow definition displacement and broad definition displacement is also consistent for the 50-64 year olds (2007, Appendix Table 1).
4. Average earnings changes include individuals who experienced an earnings increase, as well as those who suffered a loss. Excluding those who gained, the average loss across just the set of losers would naturally be higher. This might be important for policies toward displacement that target only those that experience losses.
5. If displacements are restricted to firm closure, then a qualification from Morisette et al. is that these type of earnings losses hold for the full sample (i.e., for all tenure levels) provided the model is estimated using only fixed-effects. However, when person-specific trends are also included, estimated earnings losses become insignificantly different from zero more than three years after displacement.
6. If older individuals with large potential earnings losses withdraw from the labour force, the exclusion of retirees would underestimate displacement earnings losses. But if long-tenured older workers have the best options for early retirement, they may leave the labour force even if potential earnings losses from displacement are small; this could lead to an overestimate of displacement earnings loss.
7. The heterogeneity would be unobserved to the researcher, but observed by the market participants. For example, firms could potentially retain unusually good workers, leading to long tenure positively correlated with high wages.
8. It is also possible that social benefits to training might exceed the private gains. The costs of displacement might be geographically concentrated in particular communities or regions, leading to multiplier-type effects that could compound individual displacement losses. Training could perhaps reduce reliance on EI and income assistance, and perhaps training can have wider effect in reducing illegal activity, increasing community participation and so on. On balance, though, I think the displacement or substitution effect is likely the most important quantitatively.
9. In addition to Carneiro and Heckman (2002), key papers that argue against an important role for borrowing constraints include Cameron and Heckman (2001), Keane and Wolpin (2002) and Cameron and Taber (2004). Contrary evidence in favour of borrowing constraints is presented by Kane (1996) and Ellwood and Kane (2000). More broadly on the issue of credit and liquidity constraints and their empirical importance, see Jappelli (1990) and Souleles (2000).
10. A recent Canadian example might be the "Learn\$ave" experiment conducted by SRDC that subsidized saving for approved education or training (or for small business start-up costs). See Leckie et al. (2009).
11. More generally, when treatment is non-binary, one can only observe one level of treatment for a given individual.
12. For example, Heckman and Smith (2004) decompose participation in a social program into five stages: eligibility, awareness, application, acceptance and enrollment. At which stage(s) should we randomize?
13. Heckman et al. (2000) provide a detailed empirical account of substitution and dropout bias, addressing the situation where control group members can find substitutes for the training program, and where treatment group members can drop out of the training program if they find a better alternative. Evidence suggests that classroom training was particularly vulnerable to these effects, while on-the-job training (with public subsidy) was less affected owing to a lack of ready substitutes.
14. A valuable account of this debate is provided by Glazerman et al. (2003) who assess 12 studies of earnings impacts that sought to replicate experimental evaluation using nonexperimental methods.
15. Note, though, that the results from evaluations of training for disadvantaged populations are also quite pessimistic.
16. A valuable overview of the work on dislocated workers in the US is provided by the survey of Wandner (2010).
17. There were very few women in the Houston sample.
18. Perhaps this pattern of results highlights Heckman's general point that there is a limit to the questions that can be well answered by single randomization designs when the question of interest is multi-stage in nature.

19. Workers also had to be attached to the state labour force between 1987 and 1995 to qualify for this sample.
20. This is analogous to labour supply estimation, where one can estimate an hours-wages relationship using only participants with positive hours, but where efficiency can be gained by additionally using non-participant data.
21. Similarly, Heckman et al. (2007) suggest as a summary that an additional year of formal schooling might raise earnings by about 10%. With adjustment for the full costs of education, this translates into an internal rate of return of the order of 7%.
22. The high value of “just showing up” in JLS’s results contrasts with evidence of credential or “sheep-skin” effects in the literature on formal education, where returns to education are estimated to be large only if key phases are completed and a credential achieved (for Canadian evidence, see Ferrer and Riddell, 2002). If Community College training is truly analogous to formal education, this apparent discrepancy probably reinforces the interpretation that the JLS “just showing up” effect is driven by misspecification.
23. Older workers may anticipate more difficulty in course completion and may hence choose not to enroll. That is, the success rate is based on the self-selection sample of those that did enroll.
24. This may also apply to earnings gains from higher wage rates if trainees crowd out non-trainees for a fixed set of higher wage jobs.
25. Interestingly, Decker (2009, p24) notes that the preliminary nonexperimental results from the WIA actually suggest somewhat better results for the adult program as a whole—with modestly increased earnings and employment for both men and women—than for the dislocated worker group.
26. Other nonexperimental work on the WIA include that of the US General Accounting Office, which noted that “Little is known on a national level about the outcomes of those being trained” (GAO, 2005, p1), and Moore and Gorman (2009), which studied one California WIA program using regression methods and found very weak labour market benefits.
27. See also the descriptive accounts contained in the various reports by HRDC (1995, 1996a, 1996b and 1999).
28. Riddell, a member of the Expert Panel on Older Workers, recently commented that “Evaluation of these programs (which are generally of poor quality) are not encouraging: results range from disappointing to dismal” (2009, p14).
29. Kambourov et al. (2009, 2010) assess the importance of selection in a relative assessment of government and firm-sponsored training, arguing that occupation switchers tend to select into government-sponsored programs. Since switchers lose some specific human capital, assessment of the returns to public training must take this selection process into account.
30. Some of the issues relating to Wage Insurance are discussed in the Expert Panel on Older Workers (2008) and in Jones (2009).

Glossary of Acronyms

COEP	Canadian Out of Employment Panel
CT	Classroom Training
DWS	Displaced Worker Survey
EI	Employment Insurance
JSA	Job Search Assistance
JTPA	Job Training Partnership Act
LWF	Longitudinal Worker File
OJT	On the Job Training
TIOW	Targeted Initiative For Older Workers
WAD	Worker Adjustment Demonstration
WI	Wage Insurance
WIA	Workforce Investment Act

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About the Author

Stephen Jones is Professor and Associate Chair in the Department of Economics at McMaster University. He was educated at Cambridge and Berkeley and has also worked at the University of British Columbia, the University of California, Berkeley, the Institute for Advanced Study (Princeton), the London School of Economics and the research institute DELTA in Paris. His academic research has covered many areas of labour economics, with particular focus on issues relating to unemployment, and has been published in two monographs and numerous papers in academic journals and collections of conference proceedings. He has been Co-Editor and on the Editorial Board of the *Canadian Journal of Economics*. He was Program Director for the "Labour Market Institutions and Unemployment" theme within the Canadian International Labour Network (CILN) and served as Principal Investigator and Director of CILN in the period 1999-2002. He currently serves as Program Director for "Unemployment and Labour Market Adjustment" program of the Canadian Labour Market and Skills Researcher Network (CLSRN).

About the EI Task Force

The Mowat Centre has convened a research-driven Employment Insurance Task Force to examine Canada's support system for the unemployed. The Task Force will develop an Ontario proposal for modernizing the EI system—conscious of the national context—that works for individuals and businesses.

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