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What works for whom in public employment policy?

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W.E. UPJOHN INSTITUTE
for Employment Research

What works for whom in public employment policy?

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What works for whom in public employment policy?

1 INTRODUCTION

1.1 Employment Insurance 1996 Reforms

The Canadian Employment Insurance (EI) Act, passed in June 1996, established two parts to the EI program. Part I, Employment Insurance Benefits (EIB), is cash unemployment compensation. It provides temporary income support to Canadians between jobs.[2] Part II provides for Employment Benefits and Support Measures (EBSM), which are active labour market programs to help the unemployed return to work.[3] The overriding policy objective is to provide "eligible Canadians with better opportunities to obtain and keep employment and be productive participants in the labour force." The EBSM should also advance other policy objectives, including reduced EIB expenditures.

A major goal of the EI Act was harmonization of federal and provincial employment programs while maintaining local flexibility in design and delivery of services. The act provided for bilateral agreements between the provinces and the federal government, as well as for contracts between provinces and third parties for the design and delivery of services. The bilateral agreements are known as Labour Market Development Agreements (LMDA).

1.2 Labour Market Development Agreements

By the fall of 1999, LMDA had been concluded between the federal government and each province and territory except Ontario, where the agreement did not go into effect until January 1, 2007. The delivery of EBSM in this province was handled by the federal government until that date. The federal government is represented by Human Resources and Skills Development Canada (HRSDC) in these agreements. The initial agreements were either comanaged or devolved. In the comanaged situation, the province or territory and the HRSDC shared responsibility for labour market activities.[4] In the devolved situation, the province or territory had sole responsibility for the design and delivery of interventions. Since February 2010, all LMDA have been devolved. The interventions are designed and delivered within the principles and guidelines established by the legislation and the LMDA.

In keeping with the new approach to results-based management in the federal government, known as "Modern Comptrollership," HRSDC has developed and continues to explore new results-based indicators to reflect outcomes rather than only throughputs or inputs. Modern Comptrollership places emphasis on the department and its minister to account to Parliament and the Canadian public for the use of resources. As a consequence, with the

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[2]Benefits may also be paid, for example, to persons on maternity leave or on sick leave.

[3]An overview of Employment Insurance (EI) Part I, income benefits for the unemployed in Canada, is available at http://www.hrdc-drhc.gc.ca/dept/guide98/ei2.shtml. EI Part II is the subject of this report.

[4]See <http://www.hrdc-drhc.gc.ca/dept/guide98/hri2.shtml>.

implementation of the EBSM came the introduction of a new results-based accountability framework—a systematic approach to managing departmental performance by results.

EI Part I is delivered by HRSDC through Service Canada centers, but delivery of EBSM have been devolved to the provinces and territories. The LMDA required formative and summative evaluations of EBSM in each province and territory. The formative evaluations were completed, and the first summative evaluations have been done in all jurisdictions except for one province, where the completion is expected in Summer 2011. Those evaluations answered some questions but raised others about program effectiveness and evaluation design. This paper recapitulates results from the summative evaluations and contrasts these results with findings in the international literature with an eye toward informing the next round of EBSM evaluations under the LMDA.

1.3 Canadian Active Labour Market Programs

The EBSM comprise four main Employment Benefits (EBs): Skills Development (SD), Targeted Wage Subsidies (TWSs), Self-Employment (SE), and Job Creation Partnerships (JCPs). The main support measure under Employment Services interventions is Employment Assistance Services (EAS). Eligibility for EBs is limited to current EI recipients (referred to as "active claimants"), and unemployed persons who have had an EI claim in the past three years or received maternity or parental benefits in the past five years before applying for EBSM assistance. The latter group is referred to as "EI former claimants," or "reachbacks." Employment Assistance Services (EAS) are available to all unemployed Canadians and legal residents regardless of their involvement with EI Part I. Following are brief overviews describing how each program works and recent levels of participation and program expenditures.[5]

Skills Development (SD) is the primary job skills training program available through the EBSM. Typically, SD training is funded through client vouchers to third party providers, with clients paying a negotiated portion of the total cost. Individuals needing training to upgrade their skills for employment may be eligible for financial assistance through SD to help with training, related costs, and living expenses. A portion of SD assistance may be repayable on a conditional basis.

Skills Development, which includes SD-Regular and SD-Apprentices, helps participants obtain employment by providing direct financial assistance that enables them to select, arrange and pay for training in skills ranging from basic to advanced. SD-Regular participants receive financial assistance to defray basic living costs and training costs, including tuition. Participants in SD-Apprentices interventions are supported during the classroom portion of apprenticeship training, primarily through EI Part I. These individuals may also receive Part II support for additional classroom-related expenses.

[5]This overview draws freely on a technical document prepared by Nicholson (2010) under contract with HRSDC and continuously updated by HRSDC (2009).

As is consistent with the high priority placed on addressing skills shortages across Canada, SD traditionally accounts for the largest proportion of Employment Benefits interventions and expenditures, and these trends intensified in 2008/09. SD interventions rose 11.4 percent, to 159,011. This total accounted for 83.7 percent of all Employment Benefits interventions delivered in budget year 2008/09, up from 82.1 percent the previous year.

Targeted Wage Subsidies (TWSs) help participants obtain employment through the acquisition of work experience and on-the-job training. TWSs encourage employers to hire individuals whom they would not normally hire in the absence of a subsidy. Financial assistance is provided to the employer to cover a portion of the participants' wages, as well as other employment-related costs. These subsidies are typically targeted at hard-to-employ workers and may account for up to 60 percent of total wages. The subsidies last for up to 52 weeks and can be extended to 78 weeks for workers with disabilities. Earnings from subsidized jobs are insurable under EI and therefore enable workers to renew their EI eligibility.

In 2008/09, 15,412 TWS interventions were delivered across the country, a decrease of 2.9 percent from the previous budget year. This type of intervention has declined in each of the last eight years. The TWS share of all Employment Benefits interventions fell from 9.1 percent the previous year to 8.1 percent in 2008/09. Total TWS expenditures fell to $87.4 million in 2008/09.

Self-Employment (SE): Eligible individuals who have a good idea for their own business may qualify for financial support, planning assistance, and ongoing support while they get their business up and running. Applicants must attend an orientation, provide a viable long-term business plan, start a new business, and agree to work full-time on this business while receiving financial assistance. Workers are provided with technical assistance in setting up their own businesses. They are able to collect their remaining EI entitlements during this process and may in some cases collect additional EI Part II benefits. Earnings under SE are not insurable under EI and therefore do not provide for renewed EI eligibility.

Self-Employment enables participants to obtain employment by helping them to start their own business or become self-employed. It provides financial assistance for basic living expenses and other personal needs while the participants are developing and implementing their business plan. SE also funds coordinators who ensure participants have access to business planning advice and expertise.

The number of SE participants rose for the first time in five years, climbing 2.2 percent to 10,380 in 2008/09. Even with this increase, SE interventions have declined 13.6 percent since 2004/05. SE represented 5.5 percent of all Employment Benefits interventions in 2008/09, down from 5.8 percent the previous year. Expenditures for SE declined year-over-year, falling 3.0 percent to $135.6 million.

Job Creation Partnerships (JCPs): Eligible individuals may have an opportunity to work on special projects developed in partnership with the provinces/territories, the private

sector, labour, and community groups. These projects create incremental and meaningful work opportunities and help develop the community and local economy. These are jobs in the public and nonprofit sectors that are intended to benefit the community. JCPs give participants the opportunity to gain work experience to improve their prospects for obtaining and maintaining employment. Financial assistance is provided to short-term projects that offer work experience to participants. Employees on these jobs continue to receive their EI benefits in lieu of wages and may have these benefits "topped up" to prevailing wage rates for the specific occupations. Wages earned under JCP activities are not insurable under EI.

In 2008/09, there were 5,275 participants in JCPs, an increase of 3.0 percent year-over-year. This was the first increase in JCPs since 2002/03. Even with this small gain, the number of JCP participants has declined by 49.9 percent since 2002/03. JCP interventions made up 2.8 percent of total Employment Benefits interventions, down from 2.9 percent last year. Expenditures were relatively stable at $49.3 million compared with $49.5 million in 2007/08.

Employment Assistance Services (EAS): These are job search assistance services provided to help unemployed workers find employment. Services include job interview referrals, job skills inventory, aptitude testing, job search assistance, access to information and resource centers, job finding clubs, individualized and group counseling, and group information sessions. The services are often provided through third-party service delivery agreements.

Employment Services are available to any unemployed person in Canada who requires assistance to enter or return to the labour force. There are three types of Employment Services interventions: 1) Employment Assistance Services (EAS), 2) Group Services, and 3) Individual Counseling. Total expenditures for Employment Services rose 2.2 percent to $566.3 million in 2008/09. The number of interventions delivered rose at a significantly higher rate of 14.4 percent to 878,254. As the economy worsened during the latter half of the year, an increasing number of individuals sought Employment Services to facilitate a quick return to the labour market, or, conversely, needed multiple interventions as they developed a return-to-work action plan.

EAS interventions comprise a variety of services that support participants as they prepare to enter or re-enter the labour force. These services range from job search assistance, provided to job-ready clients, to the development of in-depth return-to-work action plans for clients facing multiple employment barriers. EAS interventions may be combined with other EBSM programming for which the client is eligible. A total of 512,198 EAS interventions were delivered in 2008/09, which was a year-over-year increase of 15.1 percent. EAS interventions accounted for 58.3 percent of all Employment Services interventions delivered during the year.

1.4 Overview

The next section reviews results from the Canadian evaluations as summarized by Nicholson in a technical document contracted by HRSDC (2010), and lists the knowledge gaps left from these studies concerning the effectiveness of programs for participant subgroups, in differing labour market conditions, for various program features, and in differing combinations

and sequences with each other. The third section is the body of this paper, where we attempt to present the findings on the effectiveness of active labour market programs in other developed countries, relying on high-quality evaluation studies. In Section Four, the knowledge gaps in the evaluation literature from both Canada and other countries are presented. Finally, in Section Five, based on knowledge of the LMDA evaluations in Canada and the evidence from other countries, we will suggest evaluation strategies that could be tried in the next round of Canadian EBSM evaluations under LMDA.

2 EFFECTIVENESS OF ACTIVE LABOUR MARKET PROGRAMS IN CANADA

2.1 Evidence from LMDA Summative Evaluations

This section reviews the evidence from the LMDA evaluations as summarized by HRSDC (2009) and Nicholson (2010). Results were available for evaluations in the following provinces and territories: Alberta, British Columbia, Newfoundland and Labrador, New Brunswick, Northwest Territories, Nova Scotia, Ontario, Quebec, Prince Edward Island, Saskatchewan, and Yukon. The regions spanned by these 11 evaluations cover 97 percent of the total Canadian labour force. The reference period used in the summative evaluations to assess program net impacts falls between 1998 and 2004.

The summaries of evaluations focused on three outcome measures (annual hours of employment, annual earnings, and number of weeks per year in receipt of EI benefits), by type of EBSM, for active and former EI claimants. The summaries focused on evidence from comparison group–designed quasi-experimental evaluations. The most common estimation methodology used in the LMDA summative evaluations is called difference-in-differences. The outcomes of participants are contrasted to those of similar individuals who did not participate in the program, with a second contrast computed before and after the program participation time frame.

The EBs examined were Skills Development, Targeted Wage Subsidies, Self-Employment, and Job Creation Partnerships. The SM evaluated was Employment Assistance Services. Following are the main findings for each program.

Skills Development (SD). Active claimants who participated in SD increased their earnings in seven jurisdictions, representing 33 percent of the national labour force covered by the evaluations. The typical estimated gain in annual earnings was in the $2,000 to $5,000 range. In proportional terms, the earnings gains were about 10 to 20 percentage points. The impact on employment was positive (an increase of 211 hours per year, or about five weeks) for active claimants in one jurisdiction, representing 15 percent of the national labour force covered by these evaluations. Smaller increases were reported in the remaining jurisdictions reporting on this measure, representing 85 percent of the labour force covered. However, the smaller estimates were not statistically significant at standard levels. Both positive and negative net impacts were found for EI use among active claimants. Predominantly positive findings were found for EI use in six jurisdictions encompassing 57 percent of the nation's workforce, with

estimated decreases ranging between 1.2 and 3.2 weeks in EI use per year. Average increases of 1.8 weeks in EI use per year were estimated for two jurisdictions, representing 17 percent of the national labour force covered by the evaluations.

For former claimants, SD had mixed effects on employment, earnings, and EI use. Point estimates ranged from a decrease of 235 hours to an increase of 342 hours, estimated earnings impacts ranged from a decrease of $3,900 per year to an increase of $5,300 per year, and estimated EI impacts ranged from a decrease of 2.5 weeks per year to an increase of 4.3 weeks per year.

These relatively positive results may in part be explained by the focus of many SD interventions on obtaining credentials. A majority of SD participants report that their program provided some sort of credential for completion, and there is empirical evidence that such credentials may serve as a signal about productivity to prospective employers (Martin and Grubb 2001). Another reason for the preponderance of positive results may be the relatively weak labour markets that the comparison groups faced in many of the evaluations.

Targeted Wage Subsidies (TWSs). Significant employment and earnings gains were reported for former claimants who participated in TWSs. Four jurisdictions, representing 87 percent of the national labour force, reported increases in employment between 194 and 419 hours per year. Similarly, former claimants who participated in TWSs recorded increases in annual earnings ranging from $2,600 to $4,400 in four jurisdictions, representing 85 percent of the national labour force covered by the evaluations. Significant postprogram earnings gains were found for active claimants who participated in TWSs in two jurisdictions, representing 15 percent of the national labour force covered by the evaluations. The typical estimated gain in annual earnings was in the $4,200 to $4,800 range. The impact on EI use was mixed for active claimants, ranging between a decrease of 1.8 weeks per year and an increase of 2.7 weeks per year.

For former claimants, the impact was negative, with increases in EI use of between 0.2 and 8.5 weeks per year in four jurisdictions, representing 84 percent of the labour force covered. The negative impact on EI use may, in part, reflect eligibility effects. Employment under a TWS program is insurable under EI, so eligibility is more or less automatic for most participants. Even if there are employment gains after the intervention, it is still possible that some of those who lose their subsidized jobs will collect EI. Significant employment and earnings gains for former claimants were found in the majority of the evaluations that studied this intervention. Specifically, significant increases in employment were found in four out of five evaluations that assessed this outcome, and increases in earnings were found in four out of six evaluations. The estimated gains in earning were in the range of $2,600–$4,400. The results for active claimants were more modest, as only one out six evaluations found a positive impact on earnings, and only one out of seven evaluations reported an increase in earnings.

Self-Employment (SE). SE showed positive net impacts on employment for both active and former claimants. The jurisdictions with positive employment outcomes represented 98

percent of the national labour force covered by the evaluations for active claimants and 87 percent of the national labour force covered for former claimants. Increases of 20 to 30 percent in annualized hours worked were reported, with much larger gains being reported in some provinces. These strong gains suggest that many SE participants remain self-employed after the formal intervention ends and that they generally report working full-time on such jobs. Increases in the annual number of hours SE participants worked were often not accompanied by increases in earnings. In some cases, the evaluations reported significant declines in earnings (up to $2,600 per year). In contrast, however, some increases in earnings (up to $4,700 per year) were found for former claimants in one jurisdiction, representing 26 percent of the national labour force covered. Both active and reachback claimants experienced significant decreases in EI use in the postprogram period. Specifically, reductions in EI use of up to 16 weeks per year for active claimants were found in seven jurisdictions, representing 86 percent of the national labour force. Similarly, reductions in EI use of up to four weeks per year for former claimants were found in three jurisdictions, representing 87 percent of the national labour force covered by the evaluations.

The principal finding is that outcomes from participation in SE are extremely heterogeneous. In some cases, the ventures can be wildly successful, creating employment not only for the individual involved but for many others as well. In other cases, SE can have serious negative consequences for the individuals involved—their businesses may be unsuccessful, and they may incur a wage penalty when they seek to re-enter paid employment. Negative results were found for women, but these were often not statistically significant because of small sample sizes for women entering self-employment. However, the negative impacts from spells of self-employment are considerably smaller than those from unemployment itself.

Job Creation Partnerships (JCPs). The net impact estimates for JCPs were generally quite varied. For active claimants, a positive net impact was found on hours worked in one jurisdiction (an increase of 285 hours, or about seven weeks per year), representing 20 percent of the national labour force of the jurisdictions in which this outcome was assessed. Mixed results were found for impacts on earnings. Increases of $3,600 per year were found in one jurisdiction, representing 17 percent of the national labour force, while in another jurisdiction, representing 58 percent of the national labour force, decreases of $2,500 per year were reported. No significant results were reported for EI weeks for these claimants. Employment results for former claimants who participated in JCPs were mixed (ranging from a decrease of 259 hours, or about six weeks worked per year, to an increase of 85 hours, or about two weeks worked per year). JCPs had negative impacts on earnings for former claimants (decreases ranging from $2,100 to $3,800 per year) in three jurisdictions, representing 40 percent of the national labour force. A negative impact was found for EI weeks in one jurisdiction (an average increase of 1.5 weeks), representing 60 percent of the national labour force covered by the evaluations.

The most obvious conclusion to be drawn from the JCP evaluation results is that the estimates were generally quite varied and often not statistically significant. For active claimants, only two of the earnings estimates were significantly different from zero, and one of them was negative. For former claimants, three significant declines in earnings following JCP

participation were reported. Estimates of impacts on EI receipt were rarely statistically significant.

Employment Assistance Services (EAS). These programs are generally short and relatively low-cost. Often EAS are combined in action plans with other interventions. Because of this complexity, evaluations of EAS have tended to focus on the group of "EAS-only" claimants. According to the British Columbia data, these represent perhaps 65 percent of individuals who received any EAS-related services, but a much smaller fraction of total EAS services provided (because those with an Employment Benefits intervention tend to have more EAS interventions than do members of the EAS-only group). The extent to which the EAS-only group is representative of all EAS participants has not been explicitly addressed in the evaluations, but on a priori grounds it seems plausible that this group might have more successful employment experiences than the other EAS participants.

Results for active claimants were generally not statistically significant for employment and earnings, with the exception of one jurisdiction where an earnings increase was estimated. In part, this may have resulted from the difficulty of detecting such impacts given the small sample sizes available in the evaluations. For EI weeks, five out of eight evaluations generated statistically significant impacts; these included both positive and negative results. Given the mixed results, no overall conclusions can be drawn about the impact of EAS-only in the EBSM context. EAS participants did report strong levels of program satisfaction, job readiness, and interest in further training.

2.2 Lessons Learned and Knowledge Gaps

We list the lessons learned and the remaining gaps in knowledge about effects of EBSM operated under LMDA in the provinces and territories. Likely gaps concern effects by participant characteristics, program features, labour market conditions, and bundling or sequencing of services.

Skills Development. Sample size restrictions generally prevented the evaluations from estimating effects of SD separately for subgroups of participants. A few of the evaluations did report that women had somewhat more favorable overall impacts on employment and earnings than men, though these results were generally not reported separately for SD participants only. In the evaluations that were able to estimate gender-specific impacts for SD participants only, gains for men often exceeded those for women. Hence, the EBSM results may not precisely mirror the international finding that women are more likely to benefit from training than men.

Estimated impacts of SD on former claimants were more variable than were SD impact estimates for active claimants. This larger variance in results may in part be explained by the difficulties that some of the evaluations had in identifying a proper comparison group for former claimants, some of whom had been out of the labour force for some time.

Targeted Wage Subsidies. A general wage subsidy should increase both wages and employment. Katz (1996) provides a "guesstimate" that each 10 percent of subsidy should increase wages by about 6 percent and employment by about 2 percent. However, when a subsidy is targeted at only one class of workers, the situation becomes more complicated, because the displacement of unsubsidized workers by subsidized ones becomes relevant. By some estimates such displacement effects may be as large as 80–90 percent.

The more consistent positive impacts on employment and earnings among former claimants are not surprising. Many former claimants have been out of the labour force for substantial periods of time, and a temporary subsidy reduces employers' costs of getting them "up to speed" in their jobs. On the other hand, active claimants have recent employment experiences, so their potential gains from TWSs are not as great. Given these caveats, the actual results reported for TWSs in the evaluations were modestly promising.

The temporary nature of many wage subsidy programs adds further complications. In most economic models, the effect of a temporary subsidy should be smaller than that of a permanent one because firms would not make the kinds of labour-using investments they might if the subsidy were permanent. But more complex models suggest that such differences will be smaller when learning on the job is important. In these cases, the subsidy may help to compensate for an initial period of low productivity for new workers and help to overcome firms' reluctance to make such hires.

These conceptual issues about the concurrent effects of wage subsidies have not played an important role in the EBSM evaluations, however, because the evaluations focused exclusively on outcomes after the subsidies end. That is, the subsidy period was viewed as being the TWS "treatment," and this program was evaluated in ways similar to any other active labour market program. TWSs should have positive employment effects since skills and attitudes developed during the subsidy period may make employees more attractive.

Many evaluations of short-term subsidy programs have found beneficial outcomes. For example, the random-assignment Job Training Partnership Act (JTPA) evaluation in the United States found that female subsidy recipients experienced earnings gains of about 15 percent relative to the control group, and males experienced gains of about 10 percent (Bloom et al. 1993). In many cases, these gains persisted into the second postprogram year. Similar positive results were found in the National Supported Work evaluation and in some of the analysis of some states' welfare reform initiatives (Gueron and Pauly 1991). Although evidence from formal evaluations is less readily available outside of the United States, a survey of OECD experiences offers the assessment that subsidy programs have a greater impact per dollar spent than either training programs or direct government job creation (Martin and Grubb 2001). In the same way, a meta-analysis of 95 studies of European active lLabour market policy reports that evaluations of private sector incentive programs such as wage subsidies are more likely to report a positive impact from participation in these programs than from participation in training (Kluve 2006).

One complication in conceptualizing the wage subsidy component of the EBSM program is in understanding precisely how it is targeted. Although most of the evaluations report that TWSs are more appropriate for younger and harder-to-employ workers, few details are provided about how such targeting is achieved. The fact that the EBSM recipients studied in the evaluations must have been EI claimants further complicates the targeting issue. In some respects, EI claimants have characteristics more similar to dislocated workers than to the disadvantaged workers typically served by temporary wage subsidies. Precisely how the theory of wage subsidies applies to such workers is an open question. Similarly, the evidence on the effectiveness of such subsidies in achieving labour market gains among dislocated workers is much less well-developed.

Self-Employment. Unfortunately, the employment gains from participating in SE were often not accompanied by increases in earnings. Once all sources of self-employment income were taken into account, one evaluation reported significant increases in earnings, and another evaluation reported decreases in earnings for former claimants. For active claimants, the only significant impact found in the evaluations with respect to earnings was negative. Whether the differences between these positive and negative findings can be explained by differences in the ways in which the earnings data were collected is an open question. Clarifying whether SE provides a good income source for participants or, instead, raises difficulties in returning to paid employment should be an important goal of future evaluations.

SE participants generally experienced significant decreases in EI receipt in the postprogram period. Because weeks in self-employment are not insurable under EI, it is likely that these outcomes largely reflect eligibility effects rather than a decline in EI collection among eligible workers. If this decline was accompanied by declining earnings, the incomes of workers pursuing self-employment may experience serious declines, especially in the short run. Although this sort of impact was not found in all of the evaluations, the possibility that those in self-employment may experience large short-run declines in income suggests caution in expanding self-employment interventions beyond carefully targeted subgroups of claimants.

Job Creation Partnerships. Some studies have reported that participation in public sector employment can help to improve training outcomes for low-skilled workers. That is, the effects of training are more likely to stick for this group if they can experience a period of relatively undemanding work prior to joining the formal labour market (Heckman, LaLonde, and Smith 1999). However, interactions between SD and JCPs were not explicitly studied in the EBSM evaluations.

Employment Assistance Services. A complication in evaluating EAS in the EBSM context is that often these services are combined in action plans with other interventions. Evaluations have tended to focus on EAS-only claimants, and evidence suggests that these claimants may be more successful than those who received EAS in combination with other services. The evaluation findings for EAS were generally not strong. As noted, the employment and earnings impacts, with the exception of one jurisdiction, were not statistically significant,

and although the impacts on EI weeks were significant in five out of eight cases, the signs of the impacts were both positive and negative.

A natural question to ask is why these results seem to differ so much from the small though relatively stable findings reported in many job search evaluations. Three possibilities might be mentioned. First, it may be that it is especially difficult to evaluate EAS using nonexperimental methods (many of the best job search studies used random assignment). Measuring the impact of this low-cost intervention may require a very precise matching of participant and comparison cases in the preprogram period, and it may not be possible to achieve that precision with the propensity score methods used here. A second possibility is that the actual services delivered under EAS are more heterogeneous than the package of job search services usually studied. The fact that many claimants received numerous specific EAS services supports this view. Finally, many previous job search studies have been done in the United States, where the provision of such services also plays a monitoring and enforcement role with respect to continuing eligibility for unemployment benefits. That role may be less significant in Canada, and this may account for a reduced impact.

3 EFFECTIVENESS OF ACTIVE LABOUR MARKET PROGRAMS IN OTHER DEVELOPED COUNTRIES

3.1 Overview of Employment Programs in Developed Economies

In this chapter of the report, we review evidence on the effectiveness of the generic categories of Active Labour Market Programs (ALMP) as they are delivered by public agencies in other developed economies. This review will serve as a context for comparative evidence on EBSM effectiveness.

3.2 Subgroups, Program Features, Sequencing of Services, and Labour Market Context

The specific aspects of program evaluation we will focus on in the cross-country survey are these: participant subgroups, program features, bundling or sequencing of services, and labour market context. Participant characteristics are the most commonly reported subgroup effects. We report estimates of program features, sequencing, and labour market context when available.

3.3 Methodological Standards

We limit our review of results to evidence from comparison group–designed evaluations of programs. We consider evidence from both experimental and non-experimental evaluations. At the back of this report we attempt to provide an exhaustive bibliography of relevant prior studies. In this report we reference only a subset of the studies listed. For each evaluation study referenced we will briefly summarize the identification strategy for producing reliable estimates of program effects. For example, we clearly distinguish evidence from studies where

experimental designs with randomization were used to identify exogenous program treatments. Similarly, for nonexperimental evaluations we briefly summarize the conditioning variables for dealing with nonrandom selection into program assignment. Our review of evidence focuses on statistically significant program-effect estimates on employment, earnings, and conservation of cash EI assistance. Where available, our discussion of results includes program effect estimates by the following areas: demographic subgroup, program features, services bundles and sequences, and the labour market context. Summary tables of the key research evidence for each program are presented in the paper. The factors summarized in the tables are as follows: author (year), country, sample and time frame, identification strategy, estimates by subgroup, program feature, labour market context, and service bundle or sequence.

3.4 Findings by Intervention Type

3.4.1 Skills Development

Studies of the effectiveness of publicly funded Skills Development (SD) have been conducted in a number of countries, although only a limited number of these studies present results for subgroups of the population. While there is considerable variation across the studies, in the main, SD has been found to have positive impacts on employment or earnings, although there is considerable uncertainty about whether the positive impacts offset the social and private costs.

The purpose of SD is to enhance individuals' human capital, which may be defined as the set of skills and knowledge that an individual possesses that may be applied in a job in order to be productive. The skills may be general or specific, meaning that they may be applicable in many jobs, or they may be applicable only to a specific occupation or job. Skills are sometimes referred to as "soft" or "hard." Soft skills are personal attributes or characteristics that tend to affect job performance through interpersonal interactions in the workplace. They include characteristics such as personality traits, communication skills, motivation, friendliness, and optimism. Hard skills are the abilities to perform a certain type of task or activity.

Public funding of SD may be warranted for unemployed individuals and may be warranted for incumbent workers as well. Unemployed individuals may be emerging workers, meaning that they have very limited labour market experience, if any at all, or they may be dislocated workers, meaning that they have become involuntarily unemployed after many years of tenure. In either case, workers' skills do not match the skill requirements of jobs because useful skills have not been acquired or because existing skills have become obsolete. Public funding of SD is appropriate on the basis of 1) the efficiency gains and positive externalities that are associated with employment, 2) imperfect capital markets that do not generally support investments in human capital, and 3) potential informational diseconomies to the extent that individuals may not be aware of or learn about the payoffs to training or educational opportunities. Public funding of SD for incumbent workers may be warranted to prevent such workers from becoming unemployed.

In any case, skills development is an investment that is made in order to gain future benefits in the form of employment and earnings, as well as productivity. Table 1 provides information about two thorough and rigorous meta-analytical studies that summarize results from several dozen studies of training.

Table 1 Characteristics of Meta-Analyses of Skill Development Impacts

Study	Number of underlying studies	Geographical coverage	Dates of coverage	Dependent variable	Main covariates	Main results
Greenberg, Michalopoulos, and Robins (2003)	37	U.S.	1975–2000	Annual earnings	Training type, gender, age, race, unemployment rate, program cost, experimental dummy	Men: inconclusive; on-the-job training (OJT) significant in most specifications; experimental dummy strongly positive; nonwhite positive Women: all training types except basic ed strongly positive; no other covariates significant Youth: classroom skills training strongly positive; nonwhite positive; unemployment rate negative
Card, Kluve, and Weber (2009)	97	26 countries; primarily European	1995–2007	Sign/significance of short-term impacts; sign/significance of medium-term impacts	ALMP type; age; gender; country group; experimental dummy; duration in program	Short-term impact: training not effective Medium-term impact: training (classroom or OJT) effective Outcome for men and women similar

SOURCE: Studies comprising rows of the table.

In the United States, a summary article by Friedlander, Greenberg, and Robins (FGR) (1997, p. 1810) notes,

> The broadest generalization about the current knowledge of government training programs for the disadvantaged is that they have produced modest positive effects on employment and earnings for adult men and women that are roughly commensurate with the modest amounts of resources expended on them. . . . Moreover, they have failed to produce positive effects for youth.

Barnow and Smith (2009, p. 173) conclude,

> Most employment and training programs have either no impact or modest positive impacts. Many do not pass careful social cost-benefit tests, though some that fail may be worth doing on equity grounds.

In perhaps the most exhaustive, recent review of the evidence, Card, Kluve, and Weber (2009, p. 20) found, in their meta-analysis of active labour market policy evaluations representing 26 countries,

> In the short run [< 12 months], job search assistance programs appear to have a relatively positive impact, while training programs seem to have a bigger advantage in the medium run [12–24 months]. . . . Programs for youth appear to be relatively unsuccessful in the short or medium run. (bracketed phrases added for explanation)

An interesting methodological finding in this meta-analysis is the suggestion that nonexperimental research designs are likely unbiased—a conclusion drawn by the authors after finding that experimental and nonexperimental studies have very similar outcomes (positive and negative) controlling for outcome measure, type of program, and type of participants.

In his summary of EBSM under the LMDA in Canada, Nicholson (2010, p. 11) reports as follows:

> The results in Table 3 for active claimants are encouraging. . . . The typical estimated gain in annual earnings was about $2,000–$4,800. . . . In proportional terms, the earnings gains are quite large by international standards—about 10–20 percentage points. . . . Estimated impacts of SD on former claimants are also shown in Table 3. In general, these estimates were more variable than were the ones for active claimants.

Oftentimes, studies treat SD—i.e., training—as if it were a "black box" and do not attempt to disentangle what characteristics of the training might be responsible for its impacts. SD interventions are quite heterogeneous; among other things, they differ in terms of content, modality of delivery, pedagogy, and length. Some studies have attempted to examine some of these aspects. In the National JTPA Study (NJS), a rigorous random-assignment evaluation of training, Orr et al. (1994) were able to estimate impacts on the earnings of three service strategies: classroom training (either occupational skill training or basic education), on-the-job-training in combination with job search assistance (OJT/JSA), and all other service combinations. Their results were as follows:

> Both women and men in the *classroom training* subgroup experience earnings losses during the in-program period with earnings gains in the post-program period. None of these estimated impacts are statistically significant when adult women and adult men are analyzed separately, but when the two target groups are

> combined statistically significant impacts on earnings are found in both post-program years. . . . Women in the *OJT/JSA strategy* had immediate, statistically significant earnings gains that persisted throughout the follow-up period; men had estimated impacts that were quite similar in magnitude, but were only statistically significant in the second post-program year. (pp. 132–133, emphasis in original)

Interestingly, despite using the phrase "modest" in their summary of results, FGR (1997) report rather hefty rates of return to training. They use the NJS data to show real rates of return, assuming that the mean effect lasts 3 years or lasts 10 years. These rates are over 80 percent for OJT and, in the case of men, they are over 70 percent for classroom training. For women, classroom training has rates of return of < 0 percent and 15 percent, depending on whether the mean effect is assumed to last 3 or 10 years, respectively.

Greenberg, Michalopoulos, and Robins (2003) report that, in the U.S. experience, classroom skills training has apparently been effective in increasing earnings, but basic education has not. This meta-analysis also finds that OJT significantly increases earnings for adults. Ostensibly, this study intended to test the hypotheses that 1) publicly-funded training would become more effective over time as administrators learned what worked and what didn't work and that 2) training outcomes should be correlated with the cost of training. The authors conclude that both of these hypotheses are false—over the 25-year time frame of studies, there was no positive trend in effectiveness, and the most expensive training modalities were not superior for adults.

Biewen et al. (2007) investigate the issue of the length of training using German data. For programmatic purposes, Germany categorizes training programs into short-term training (lasting 2–12 weeks), further training (several months to 1 year), and retraining (2–3 years or more). Further training could be purely classroom or practical (involving OJT, internships, or other stints at a firm). This study uses propensity score matching to estimate the net impacts of the various training modalities. The authors argue that a rich set of preprogram variables as well as a case worker assessment of participant motivation and regional unemployment rates satisfy the conditional independence assumption (CIA) necessary to identify findings. The study finds that all training modalities increase employment likelihoods, but surprisingly, short-term training and classroom further training had the most beneficial outcomes.

Osikominu (2009) also investigates the issue of training length and notes that long-term training has a tradition in Germany even though many studies have noted the lock-in effect of such training. This analysis examines spells of unemployment and employment using a competing risks hazard model. The author maintains that variation in the timing of treatment is adequate to identify treatment effects. The study finds that short-term training reduces unemployment spells, especially if it is introduced early in the spell. Long-term training, on the other hand, increases unemployment spells, especially if it is used early in the spell; however, long-term training pays off with longer employment spells.

3.4.1.1 Subgroups

Obviously, an important characteristic of any analysis of the impacts of training on employment or earnings is the distribution of those impacts across subgroups of the population. Subgroup analyses may identify subpopulations for whom training seems to be systematically effective or ineffective. To the extent that disparate outcomes across subgroups are caused by unintentional aspects of the program, then it may be possible for administrators to alter policies or practices to alleviate any inequities. On the other hand, if there is a structural or programmatic reason for the disparate outcomes, then alternative programs or policies may need to be developed, if possible and if desired, by policymakers. The subgroup effects that have been analyzed in studies include the following: age (youth versus adults), sex, disability status, geographic region, educational background, and circumstance of training participation (dislocation or not). Table 2 summarizes the studies that examine impacts for subgroups.

Age. A number of studies have found consistently only modest, if any, positive impacts from training youth (usually defined as less than 21) through the public workforce development system. Kluve et al. (2007) discuss evaluation findings for youth programs in Europe. They report that one study (Brodaty et al. 2002) that looked at workplace training programs for two cohorts of youth in France (1986–1988 and 1995–1998) found positive results in the earlier cohort, but the results for the second cohort turned negative. This nonexperimental study, which uses a matching technique that relies on a competing-risk duration model to identify the propensity score, indicates that private subsidies yield better results for the short-term unemployed, whereas training works better for long-term unemployed youth. Kluve et al. (2007) further cite a Finnish study that found positive employment and earnings outcomes from "labour market training" but slightly negative outcomes from "youth practical training," which is the largest but least expensive program for unemployed youth in Finland.

The Kluve et al. (2007) literature review goes on to cite no or negative effects of youth training programs in Sweden, Portugal, Norway, and Italy.

In the United States, the NJS essentially found no earnings impact of classroom training or OJT/JSA for youth. Orr et al. (1994) state,

> The only significant impacts occurred during the first six months of the follow-up period—a significant earnings *loss* for female youths in the classroom training subgroup and a significant earnings *gain* for those in the OJT/JSA service strategy. It is likely, however, that . . . this many estimates would be statistically significant by chance. On the basis of these results, then, there is no evidence that any of the JTPA service strategies improved the earnings of either female youth or male youth non-arrestees. (p. 150, emphasis in original)

Table 2 Summary of Studies That Provide Skill Development Impacts by Subgroup

Study/subgroup	Country(ies)	Impact
Youth		
Brodaty et al. (2002)	France	Positive in 1986–88; negative in 1995–98
Kluve et al. (2007)	Finland	Positive for "labour market training" Negative for "youth practical training"
Kluve et al. (2007)	Sweden, Portugal, Norway, Italy	Negative or insignificant
Orr et al. (1994)	United States	Negative for female youth; insignificant for all others
Hollenbeck and Huang (2003)	United States	Insignificant earnings; negative short-term employment; positive long-term employment
Hollenbeck and Huang (2006)	United States	Negative short-term earnings; insignificant short-term employment; positive long-term earnings and employment
Gender		
Nicholson (2010)	Canada	Impacts for men often exceeded those of women
Card, Kluve, and Weber (2009)	26 countries	Program outcomes similar for women and men
Orr et al. (1994)	United States	Impacts for adult women substantially larger than for adult men
Greenberg, Michalopoulos, and Robins (2003)	United States	Earnings effect largest for women and quite modest for men
Heinrich, Mueser, and Troske (2008)	United States	Women's employment and earnings impacts larger than men's
Race/ethnicity		
Orr et al. (1994)	United States	Earnings impacts for Hispanic women less than for whites or blacks; no racial/ethnic differences for men
Greenberg, Michalopoulos, and Robins (2003)	United States	Nonwhite men and youth have higher earnings impacts than whites; no racial difference for women
Disability		
Aakvik (2003)	Norway	Positive, but not significant, employment impacts
Hoglund and Holm (2002)	Denmark	No impact
Hollenbeck and Huang (2006)	United States	Positive employment and earnings impacts, but uncertain because of potential selection bias
Geographic region		
Biewen et al. (2007)	Germany	Positive impacts in western Germany; no impact in eastern Germany
Nicholson (2010)	Canada	Earnings and employment variation across provinces and territories
Educational background		
Orr et al. (1994)	United States	No difference in impact between high school graduate and GED equivalence

SOURCE: Various.

For readers unfamiliar with the NJS, it should be pointed out that Orr et al. disaggregated the results that they found for youth into individuals with and without an arrest record. The overall impacts for youth were estimated to be negative and significant, but the impacts for nonarrestees were insignificant.

Hollenbeck and Huang (2003, 2006) estimated the short-term (three full quarters after program exit) and longer-term (9–12 full quarters after exit) net impacts of the JTPA Title II-C Youth program and the Workforce Investment Act (WIA) Title I-B Youth program, respectively, using data from the state of Washington. Unfortunately, no analyses were done looking strictly at those youth who received training. In the earlier study examining the JTPA program, these authors find no significant earnings gain and a (significant) short-term employment loss and longer-term employment gain. In the later study examining the WIA program, the short-term earnings and employment impacts were negative, although the employment impact was not significant. Notably, the longer-term earnings and employment impacts of the WIA Youth program turned positive. Again, it is not known the extent to which these impacts can be attributed to skill development. Both of these studies used a propensity score matching technique with a rich specification of preprogram education and labour market experience measures.

Sex. Many studies examine the differential impacts from training for women and men. In Canada, Nicholson (2010) reports that even though his review of the literature suggests that SD-type programs appear to help adult women but are not especially beneficial for adult men, the evaluations of the EBSM delivered under the LMDA that were able to estimate gender-specific impacts for SD participants found gains for men often exceeding those for women.

If the conventional wisdom is that training helps adult women but not men, then Card, Kluve, and Weber (2009) also seem to contradict that convention. They note,

> This feature allows us to perform a simple but powerful "within-study" comparison of program effectiveness by gender: we simply compare the sign/significance of the program estimates for women and men. For the 28 studies from which we can extract both a short term estimate for women and a short term estimate for men, we found that the estimates were the same . . . in 14 cases (50%); the women had a <u>more</u> positive outcome in 8 cases (29%); and the women had a <u>less</u> positive outcome in 6 cases (21%). This comparison provides further evidence that the program outcomes tend to be similar for women and men. (pp. 20–21)

This citation is mainly summarizing European evidence. In the United States, as noted in the citation above, the NJS found that classroom training was ineffective at raising earnings for either men or women; however, on-the-job training contracts significantly increased earnings for women in both the first and second follow-up periods, and increased earnings for men in the second follow-up period. The effects for women were substantially larger than for men. (See Orr et al. 1994.)

In a meta-analysis of the findings from 31 evaluations of 15 voluntary U.S. government-funded training programs that operated between 1964 and 1998, Greenberg, Michalopoulos, and Robins (2003) report,

> On average, the earnings effects of the evaluated programs seem to have been largest for women, quite modest for men, and negligible for youths. For men and women, the earnings effects of training appear to have persisted for at least several years after the training was complete. (p. 31)

It has been suggested that since the United States provides custodial single parents (mostly women) with income maintenance support through the Temporary Assistance to Needy Families (TANF) program (formerly Aid to Families with Dependent Children, or AFDC), women may benefit more from training because of the regular financial support.

Heinrich, Mueser, and Troske (2008) report significant earnings and employment impacts of training for the WIA adult program. In this nonexperimental evaluation, the results for women are larger than for men, as in the NJS. In the third year after program entrance, the impact of training on women's quarterly earningswas approximately $850 per quarter, compared to about $420 for men. The employment rate net impact estimates for the same time period were 7.5 percent for women and 2.0 percent for men. This study, in general, found insignificant employment or earnings impacts for training for dislocated workers in the WIA program; however, it might be noted that the point estimates for women are more positive (or less negative) than for men.

Race/ethnicity. U.S. studies have examined the effect of training on earnings or employment by race/ethnicity. Exhibit 5.8 of Orr et al. (1994, p. 135) displays impacts of the JTPA, as estimated in the NJS, on women by ethnicity subgroups. White and black non-Hispanic adult women have significant 30-month earnings impacts, whereas the impact for Hispanic women is insignificant. However, the authors of the study warn that the F-test of whether the impacts are different across the three groups is not significant. Exhibit 5.9 (p. 137) shows that there are no significant differences for men by ethnicity.

Greenberg, Michalopoulos, and Robins (2003) find contrary results. The meta-analyses presented in this report show that nonwhite men and youth receive higher payoffs from training, whereas there is no difference by race/ethnicity for women. They conjecture,

> Perhaps, surprisingly, government-funded training seems to have been less effective for white men and white youths than for nonwhite and racially mixed groups of men and youth. One possible explanation for this finding, which did not occur for women, is that white workers faced fewer employment barriers than nonwhite workers did and could more readily find jobs on their own without the aid of training. (pp. 50–51)

Disability status. Estimating the returns to skill development for individuals with disabilities is difficult because most programs are targeted solely to such individuals, and so it is difficult to isolate the impact of the training. Nevertheless, an extensive literature on vocational rehabilitation programs has arisen. Annex IV in HRSDC (2011) has an extensive bibliography for this literature.

In programs that serve individuals with and without disabilities, the problem of unobserved heterogeneity arises. Aakvik's study of the employment effects of education on disabled workers in Norway (2003) uses econometric selection models and finds that the employment impacts of education are substantial (8 percentage points); however, they are not statistically significant.

Kluve et al. (2007) summarize a study by Hoglund and Holm (2002) that found that education had no significance in explaining whether Danish workers participating in vocational rehabilitation programs for long-term sick-listed workers returned to employment at either their former or a new firm.

Hollenbeck and Huang (2006) found quite large and significant employment and earnings impacts for vocational rehabilitation programs provided to disabled individuals in the state of Washington in a nonexperimental study that compared participants to individuals who applied for services but did not participate. However, without detail on the makeup of the comparison group, these results should be considered tentative.

Geographic region. Within a country, or within a political jurisdiction such as a province or state, skill development interventions may vary in their effectiveness if there are substantial differences in access to the interventions or in resources used to provide the interventions. For example, Biewen et al. (2007) used German administrative data in their study of training program effectiveness and found positive employment impacts of short-term and medium-term programs in West Germany, but no positive treatment effects in East Germany.

Not surprisingly, Nicholson (2010) found earnings and employment variation across provinces and territories in his summary of the SD impacts of EBSM under the LMDA. However, that summary report did not attempt to find a systematic explanation for the substantial provincial variation.

Educational background. Presumably, a trainee's preprogram educational experiences will interact with the training that is provided, so it is difficult to identify the training effect because of its endogeneity. In our review of the literature, the only study that seemed to provide direct evidence was the NJS, and its findings suggest that whether or not a participant had their high school diploma or GED equivalence did not make a statistically significant difference in earnings or employment impacts (Orr et al. 1994).

Dislocated workers. Cavaco et al. (2005) analyzed the impact of retraining for displaced workers in France using a competing risks duration model. They found an increased likelihood of reemployment of 8 percentage points for participants in the program. However, they also

found that the program was not well targeted. Impacts would have been much larger—perhaps as great as 28 percentage points—if the program had been taken up by those who would have benefited the most, which according to their analyses would have been younger, more highly educated displaced workers.

In a careful nonexperimental analysis using administrative data from 12 states in the United States, Heinrich et al. (2008) estimate that retraining dislocated workers through the WIA program has no impact on earnings or employment. Sixteen quarters after entering WIA as dislocated workers, the difference in earnings and employment rates between individuals who went through training and those who did not is essentially zero for both men and women.

In a study using Washington data, Hollenbeck and Huang (2006) find results that are somewhat more sanguine. Dislocated workers who received training had a 4-percentage-point employment advantage and an approximately $300 quarterly earnings advantage over a comparison group that did not receive training. Hollenbeck (2009a) posits four hypotheses as to why the results from both Hollenbeck and Huang (2006) and Heinrich et al. (2008) differ for dislocated workers, whereas they are broadly consistent for training disadvantaged adults. These hypotheses include 1) treatment point, 2) source of comparison group data, 3) matching technique, and 4) estimator differences.

3.4.1.2 Conclusions

Many studies have been undertaken that provide estimates of the impacts of publicly provided skills development (i.e., training) programs. The studies use data from many different countries and pertain to many different types of programs offered under differing labour market contexts. Abstracting from the heterogeneity, the studies generally suggest that skills development delivers positive labour market impacts for adults. Training or education tends to improve the likelihood of employment and to increase earnings, if employed. However, in general, very little support exists in studies for similar positive impacts of skills development for youth.

While impacts on the labour market are positive (for adults), authors who have attempted to calculate the social and private costs of training question whether the benefits of skills development activities exceed their costs. The general consensus here is that positive net impacts on employment and earnings must last several years in order for the benefits to exceed the costs.

Skills development activities vary widely in their characteristics—content, instructional modality, duration, use of technology, and so forth. Very few studies have rigorously analyzed these characteristics, although some evidence exists about content and modality. This evidence suggests that classroom skills training and on-the-job training activities have better labour market impacts for participants than short-run classroom training, basic skills training, or longer-run (greater than two-year) educational programs.

The broad conclusion that skills development is found to have generally favorable impacts for adults begs the question of how those impacts might vary across subgroups of the population. Results for training youth through the workforce development system tend to be discouragingly low, although there are a few studies that find positive outcomes. All but three of the studies reviewed here that examined programs in European countries and the United States find no positive or negative impacts for youth. The three studies that report positive impacts for youth have important qualifications to them to discount those findings.

The general convention, particularly based on studies conducted before the year 2000, is that SD has its strongest impacts on adult women, and that it has weaker but usually positive impacts on adult men. However, this convention seems to be somewhat challenged by more recent studies. The meta-analysis by Card, Kluve, and Weber (2009) suggests that outcomes are quite similar for men and women. Nicholson (2010) notes that the evaluation results for EBSM delivered in Canada under the LMDA in which gender-specific results were reported found gains for men that often exceeded those for women.

A couple of studies relying on U.S. data examine the impacts of skills development by race/ethnicity. Orr et al. (1994) report very weak evidence that Hispanic women may not have benefited as greatly from the JTPA program as other women, but there was no difference by race/ethnicity among men. Greenberg, Michalopoulos, and Robins (2003) report somewhat more solid evidence that nonwhite men and youth receive higher payoffs from training than white males, whereas there is no difference by race/ethnic group for women. In short, there is no recent evidence about differences across these subgroups, and the evidence that does exist is somewhat conflicting.

In terms of other subgroups, the evidence presented here suggests at best weak support for positive labour market impacts for individuals with disabilities, no impacts by educational background, and no systematic variation by geographic area.

Even though there have been many empirical studies of the impacts of training, there are still areas where little seems to be known. Most of the studies treat training as a “black box”; observations either participated or not. Little is known about the impacts of characteristics such as seriousness of purpose, engagement, attendance, or even completion of the training intervention. Furthermore, inadequate sample sizes seem to have precluded much analyses of personal characteristics, such as age, race, educational background, disability status, or place of residence (urban versus rural).

3.4.2 Targeted Wage Subsidies

Wage subsidy and wage supplement programs offer tax credits or other financial incentives to businesses or individuals to improve earnings and employment prospects. These programs are seen as more flexible and less expensive alternatives to direct income support or direct government employment. The private sector bears a share of the cost by paying that portion of the wage bill that businesses consider equals the value of the marginal product of the

subsidized workers they hire. In theory a subsidy to a business or to an individual yields identical employment and earnings results; in practice that has proven not to be the case. The equivalence of employer- and employee-based programs requires that the existing labour supply (those already employed when the subsidy is implemented) and the new labour supply (those who were not previously employed) be the same, in that they respond in the same way to market incentives and are otherwise interchangeable (Dickert-Conlin and Holtz-Eakin 2000). However, programmatic and behavioral issues arise with respect to participation rates, program costs, efficiency, deadweight loss, and unintended stigma effects on targeted populations.

Most programs that incentivize employment focus on wage subsidies to employers. Subsidies in this case affect the demand for labour, expanding an employer's demand for labour by reducing the cost of employing a worker. Many wage subsidy programs target subpopulations that have difficulty obtaining employment. If the subsidy is large enough to compensate employers for hiring someone they would not otherwise have employed, the subsidy increases employment for that specific subgroup. It may also be the case that wage subsidies could benefit a subgroup by providing incentives that move individuals in the targeted group from part-time to full-time status. Furthermore, employment and/or earnings gains for a targeted subgroup of workers may be achieved at the expense of another subgroup of workers. Consequently, evaluations of targeted wage subsidies should consider the displacement effects.

Wage subsidies can be in the form of cash paid directly to the firm or through a voucher to the worker. Other subsidy schemes involve a tax credit to firms or a reduction in payroll taxes. Many programs provide a wage subsidy only, but others provide the subsidy in conjunction with training, such as on-the-job training (OJT), apprenticeship programs, or internships. In rare cases, wage subsidies are used to encourage qualified workers to enter an occupation in short supply, such as Illinois's Great START (Strategy to Attract and Retain Teachers) program for early childhood providers and educators. Other examples are Nova Scotia's Film Industry Tax Credit, which subsidizes wages paid to Nova Scotia residents working on film or TV productions, and New York City's subsidy of green jobs.

In contrast to wage subsidy programs, wage supplement programs provide financial incentives directly to employees. They typically are targeted at economically disadvantaged populations, who because of a low level of skills or lack of work experience have difficulty finding jobs that pay above a worker's reservation wage. The wage supplement fills the gap between the wage rate an employer would be willing to pay to hire that person and the wage the worker believes he or she must receive to make working worthwhile. When the take-home wage of the employee increases, the labour supply increases, in theory. Wage supplements are typically paid to individuals through the income tax system, as a credit or reduction in their taxes. Although the number of wage supplement programs offered is small compared with wage subsidy programs, in some countries wage supplements reach more participants and distribute more money than wage subsidies. In the United States, for example, the Earned Income Tax Credit (EITC) distributes more than $35 billion annually to low- and moderate-income workers.

3.4.2.1 Subpopulations targeted by wage subsidies

Wage subsidy programs target a wide variety of subpopulation groups. In Canada, for example, Canada Business's Web site lists 72 separate wage subsidy programs. While many programs focus on economically disadvantaged and marginalized populations, there are some, albeit far fewer, programs that seek to encourage the expansion of employment in targeted occupations and industries, such as green jobs or in high-tech sectors. We, however, will concentrate primarily on programs that help to improve employment prospects for the harder-to-employ populations.

Table 3 provides examples of the various target groups included in the more prevalent past and present wage subsidy programs. Some of the programs listed offer wage subsidies to a long list of eligible groups. For example, the Work Opportunity Tax Credit (WOTC), a wage subsidy program currently operating in the United States, subsidizes employers for wages paid to 12 groups of job seekers. The groups include long-term TANF recipients, other TANF recipients, veterans, 18- to 39-year-old SNAP (food stamp) recipients, 18- to 39-year-old designated community residents living in disadvantaged areas, 16- to 17-year-old summer youth, vocational rehabilitation referrals, ex-felons, SSI recipients, Hurricane Katrina employees, unemployed veterans, and disconnected youths.

Additional programs target other subgroups. For example, Australia's Indigenous Wage Subsidy program targets unemployed Aboriginals or Torres Strait Islanders. The subsidy is worth up to AUS$6,600 over 26 weeks for a full-time ongoing job of at least 25 hours per week, or up to AUS$3,300 over 26 weeks for a part-time job of at least 15 hours per week. For employers that provide career development assistance, an additional reimbursement of up to AUS$550 is available.

Some wage subsidy programs combine subsidies with additional services. On-the-job training programs, which are offered in several countries, provide wage subsidies for the explicit purpose of reimbursing firms for providing on-the-job training. The wage subsidy compensates employers not only for direct training costs but also for the lower productivity of workers while they receive training. The U.S. On-the-Job training program (the portion funded through Recovery Act dollars) uses a sliding scale for determining the wage subsidy, depending upon the size of the firm, with the consideration that training is a relatively larger burden for smaller firms than for larger firms. Another example is the United Kingdom's New Deal for Lone Parents, which combines an earnings subsidy provided by the Working Families Tax Credit (WFTC) with case management, information, referrals, and other limited financial support.

Although several of these programs have been evaluated, these evaluations rarely estimate the effect of subsidies on each of the subgroups served. In several cases, the number of participants in a particular group is simply too small for an accurate assessment. Evaluations of the WOTC and of other wage subsidy programs typically differentiate participants only by gender and race.

Table 3 Selected Wage Subsidy Programs that Target Specific Subgroups

Subgroups	Programs	Country	Description	Evaluation
Disadvantaged youth	TJTC, WOTC	U. S.	TJTC: employers claim a tax credit for up to 2 years (50% subsidy first year, 25% second year) on the first $6,000 earned annually by newly hired employees	Katz (1996)
Summer youth 16–17-year-olds	WOTC	U.S.	WOTC: firm receives a tax credit up to a maximum subsidy of 40%, depending upon hours worked, up to $6,000 in earnings for a max. subsidy of $2,400	Evaluated in Hollenbeck (1986)
Public assistance	TJTC, WOTC, WtW	U.S.	WtW: firm receives tax credit (35% first year, 50% second year) up to the first $10,000 in wages each year, w/ max. subsidy of $8,500 over two years	Hamersma (2008)
SNAP recipients, 18–39-year-olds	WOTC	U.S.	See above	
Ex-felons	WOTC	U.S.	See above	
Disabled	WOTC, TJTC	U.S.	See above	Evaluated in Hollenbeck (1986)
Disabled	Flexjob	Denmark	Subsidy based on severity of disability	Gupta & Larsen (2010)
Disabled	Ticket-to-Work	U.S.	SSDI recipients receive voucher to exchange for job or support services	
Disabled	Austrian Employment Act for Disabled	Austria	Subsidizes employment of severely disabled and grants employment protection	Humer et al. (2007)
Disabled	FAS Wage Subsidy Scheme	Ireland	General subsidy for a perceived productivity shortfall in excess of 20%, maximum of €10,000/yr.	
Indigenous	Indigenous Wage Subsidy	Australia		Hunter et al. (2003)
Older workers	Reemployment Trade Adjustment Assistance	U.S.		None
Single mother welfare Recipients	TJTC	U.S.		Katz (1996)
Long-term unemployed	JobStart	Australia		None
Dislocated workers	OJT	U.S.		New Jersey Experiment
Long-term unemployed	Danish Wage Subsidy Scheme	Denmark	Subsidizes 50% of minimum wage for a max. of one year, with restrictions to minimize displacement	Rotger & Arendt (2008)
Low-income working families	Working Families Tax Credit	UK	Low-income working families receive reduction in welfare payments the more they work	Brewer et al. (2005)
Hard-to-place persons	German Wage Subsidy Program	Germany	As much as 50% of the monthly salary for at most 12 months	Stephan (2009)
General population under age 65	Swedish Regional Payroll Tax Reduction	Sweden	Firms in northern part of Sweden are allowed to cut payroll taxes by 10 ppts.	Bohm & Lind (1993); Bennmarker, Mellander, & Ockert (2008)

3.4.2.2 Factors in evaluating wage subsidy and supplement programs

The efficacy of wage subsidy programs depends upon several factors. One is the ability to target only those populations that are considered in need of a subsidy. Providing a general subsidy to all workers greatly inflates the cost of the program and reduces its efficiency. Another related issue pertaining to employer subsidies is whether the subsidy should cover the entire workforce or merely new hires. Marginal employment subsidies are provided only for a firm's additional employment over some baseline level of employment. Included within marginal employment subsidies are subsidies to new hires. They are less costly per job created, but they may provide some incentive for firms to increase turnover. Some firms, however, may not find such subsidies attractive unless the subsidy is more than enough to cover training costs. Also, hiring subsidies do not completely eliminate deadweight loss, since it is still difficult to disentangle those people whom the firm would have hired only with a subsidy from those whom the firm would have hired anyway. And among all these concerns, one cannot forget the issue of stigma, which increases the more narrowly a population group is defined.

Evaluations need to take into account these behavioral and programmatic differences. Five criteria are typically used to assess the effectiveness of these programs:

1) *Job creation*: Did more people from the targeted group get jobs?
2) *Earnings*: Were earnings the same or higher compared with similar unsubsidized jobs?
3) *Displacement*: Did the new hires crowd out or displace others from getting a job?
4) *Deadweight loss*: Would firms have hired these individuals without the subsidy?
5) *Participation:* Do firms find the subsidy adequate to compensate them for the additional costs of hiring a subsidized worker?

Most evaluations of wage subsidies and supplements are based on nonexperimental design. We highlight below several evaluations that use various matching techniques to construct comparison groups. We include the Canadian Self-Sufficiency Project as an example of an evaluation based on random assignment design.[6]

Table 4 summarizes the results from evaluations of the programs listed in Table 3. These evaluations are selected because they, more so than most evaluations, consider the net impact of the programs on subgroups of the population and because they use rigorous methodologies to

[6] A few evaluations of the UK's Working Families Tax Credit have used structural labour demand and supply models which, when calibrated, simulate the effects of the program (e.g., Brewer et al. 2005).

Table 4 Examples of Evaluations of Wage Subsidy and Wage Supplement Programs

Program/evaluator	Targeted group	Evaluation type	Employment effect	Deadweight loss	Displacement effect	Participa-tion rate
TJTC (U.S.) Katz (1996)	Disadvantaged youth	Comparison group constructed from change in program	7% in the short run	40–52% of TJTC certifications created jobs for group targeted.	-	8% of TJTC certificates hired because of their TJTC status
TJTC (U.S.) Hollenbeck et al. (1986)	Disadvantaged youth; welfare; veterans; handicapped	Comparison group of eligible but noncertified workers	Youth and handi-capped generally gained, with real earnings gain of $462 (1998$) and $1,940 for handi-capped. Most earnings gains came from an increase in employment. White females gained most .	-	Displacement greatest among black male youth, black males, black females, and white females on welfare. Also black veterans.	-
JTPA OJT (U.S.) Orr et al. (1996)	Displaced workers	Random assignment	-	-	60%	-
WOTC (U.S.) Hamersma (2008)	Welfare recipients	Comparison group constructed from change in program	5.9% higher probability of employment in 2nd quarter; no difference in probability in 4th quarter and into second year.	38% of tax credit is passed through to workers in higher wages.	-	< 10%
Flexjob (Denmark) Gupta and Larsen (2010)	Disabled	Comparison group of closely matched ineligibles	Probability with subsidy is raised 33ppts from base of 44%; does not reduce exit to disability pension.	-	-	-
Austrian Employment Act for the Disabled (Austria) Humer et al. (2007)	Disabled	Fixed-effect regression	Employment protection has greater impact on employment than subsidy.	-	-	-
Indigenous Wage Subsidy (Australia) Hunter et al. (2003)	Aboriginals and Torres Straits Islanders	Comparison group of those who did not complete program	Longer duration of employment; shorter duration of unemployment	-	-	-

Program/evaluator	Targeted group	Evaluation type	Employment effect	Deadweight loss	Displacement effect	Participa-tion rate
Danish Wage Subsidy Scheme (Denmark) Rotger aand Arendt (2008)	Long-term unemployed	Firm-based evaluation; comparison group includes eligible but not participating firms; firms with < 10 employees	Subsidy contributed 0.71 jobs per subsidized firm; net creation is 0.26 jobs per subsidized firm after taking into account separation rate	-	Separation rate of 0.45 per subsidized firm	< 7%
Finland Wage Subsidy Scheme (Finland) Venetoklis (2004); Kangasharju (2005)	Difficult-to-employ workers	Comparison group of eligible firms that do not participate	6–9% increase in payroll	No evidence of displacement	-	-
Swedish Regional Payroll Tax Reduction (Sweden) Bohm and Lind (1993)	Disadvantaged region	Compared employment changes in disadvantaged region and nearby counties	No evidence of statistically significant employment effects	-	-	-
Finnish Regional Payroll Tax Exemption Experiment (Finland) Korkeamaki and Uusitalo (2008)	Disadvantaged region in northern Finland	Matched comparison group by similar region and similar firms within region	No evidence of statistically significant employment effects	-	-	-
German Wage Subsidy Program (Germany) Stephan (2009)	Hard-to-employ persons	Matched comparison of subsidized and unsubsidized workers	Initially subsidized workers have higher employment rates, but wage rates were not statistically different from comparison group	-	-	-
Self-Sufficiency Program Experiment (Canada) SDRC	Long-term welfare recipients	Random assignment experiment	Those receiving supplement were twice as likely as control group to work full-time; increased earnings by > 20%; no diff. when supplement ran out.	-	-	33%

Program/evaluator	Targeted group	Evaluation type	Employment effect	Deadweight loss	Displacement effect	Participa-tion rate
EITC (U.S.) Eissa and Liebman (1996)	Long-term welfare recipients; women with and without children	Constructed comparison group from change in program	LFP rate increase between 2.4 ppts and 6.1 ppts for least-educated women with children; no evidence that it increases hours of those already working.	-	-	-
Working Families Tax Credit (UK) Brewer and others (2005)	Low-income working families	Microdata used to estimate structural labour supply model	Compared with previous program: increased labour supply of lone mothers by 5 ppts.; slightly reduced labour supply of mothers in couples; slightly increased labour supply of fathers in couples.	-	-	-

construct counterfactuals. Most use propensity score matching techniques to construct comparison groups, but a few utilize random assignment and structural modeling techniques.

The evaluations reviewed above found generally modest gains in employment and labour force participation of participants in wage subsidy programs. The net effects range from 3 to 9 percentage point increases in employment compared with their counterfactuals. For those evaluations that consider earnings effects, none found a decline in earnings associated with the employment gains, which suggests that participants are not sacrificing earnings for an increase in the prospect of finding employment. Only a few studies reported deadweight loss estimates and participation rates. Those that did found deadweight loss to be greater than 50 percent, suggesting that at least half—and probably more than half—of the money spent on subsidies goes to workers that would have been hired without the program. Participation rates are low, with evaluations showing less than 10 percent of eligible hires typically claimed by employers.

Wage supplement programs, on the other hand, appear to have higher take-up rates than wage subsidy programs. Evaluations also show that their employment effects are comparable to, if not greater than, many of the wage subsidy programs. The Canadian and U.S. programs show positive effects on labour force participation rates. Evaluation results of Canada's Self-Sufficiency Project experiment found that those receiving the supplement were twice as likely as the control group to work full-time, and their earnings increased by 20 percent.[7] The U.S. results were not as large but were still positive: researchers found a 2.4 percentage point increase overall and a 6.1 percentage point increase for less-educated women with children.

[7] Because the earnings supplement applied only to full-time jobs, part of the increase in full-time employment came from workers changing from part-time to full-time jobs in order to obtain the subsidy.

3.4.2.3 Summary of wage subsidy and wage supplement evaluation results

Subgroup Effects. These positive employment effects are shared by various subgroups of participants: disadvantaged youth, particularly white females; disadvantaged adults, particularly lone mothers and hard-to-place individuals; the disabled; dislocated workers; and indigenous populations. However, it is difficult from the evaluations to surmise whether wage subsidy and wage supplement programs are more effective for one group than for another. The estimated net impacts of these programs on employment are similar across evaluations for different subgroups. For example, Katz (1996) finds a 7 percentage point increase for disadvantaged youth for the Targeted Job Tax Credit (TJTC) program. Hamersma (2008) finds a 5.9 percentage point increase for welfare recipients in the U.S. WOTC program. Venetoklis (2004) also finds around a 6 percentage point increase in employing hard-to-employ workers in Finland. However, Hollenbeck et al. (1986) find no statistically significant effects of the TJTC program for female youths or for black and Hispanic veterans. They also find greater displacement effects for black male youth, black adult males and females, black veterans, and white female welfare recipients.

The ability to compare the relative effects of wage subsidy and wage supplement programs on population subgroups is complicated by the fact that specific programs target different population subgroups and that these programs differ in design and external context. By external context, we mean that the programs are administered across different countries and their impacts are influenced by different cultures and various types of other social and workforce programs and different economic environments.[8]

To offer a richer perspective on the types of programs provided and the design and findings of evaluations conducted on these programs, we offer in the next several sections brief descriptions of evaluations of programs in Australia, Canada, Denmark, Finland, and the United States. We selected these programs and evaluations because they offer examples of programs that target different population subgroups. The evaluation of the U.S. Targeted Job Tax Credit is particularly noteworthy because the program targeted several different subgroups and the evaluation was designed to estimate the relative effects of the program on these groups.

3.4.2.4 Examples of wage subsidy programs in Finland, Denmark and the Unitred States

Finland. Venetoklis (2004) evaluates Finland's wage subsidy program by using propensity score matching to construct the comparison group of firms and by using difference-in-differences to estimate the net impact of the wage subsidy program, drawing from a data set of 18,000 firms. The Finnish legislation provides subsidies to profitable firms that take on unemployed workers who find it difficult to find unsubsidized jobs. Typical subsidized jobs are

[8] The use of comparison groups helps to mitigate possible external contextual effects, since both the treatment and comparison groups are supposedly subject to the same "environment." However, even when programs are evaluated using random assignment experiments, it is unclear how generalizable the results of those evaluations might be when placed in different contexts.

for janitorial workers, clerks, secretaries, and unskilled manufacturing workers. The purpose of wage subsidies is to improve the human resources development of the unemployed and to encourage firms to increase employment. Wage subsidies are based on an amount of up to approximately 770 Euros per month for up to 10 months (in 2002). Workers in subsidized jobs are usually paid according to the prevailing wage rate. The subsidy fills the gap between the prevailing wage rate and what the firm would be willing to pay to hire that person.

Venetoklis analyzes the financial statements of business firms that submitted tax returns to the Finnish tax authority from 1995 to 2001. These data were linked to information on the annual amount of wage subsidies participating firms received during the same seven-year period. The data set covered nearly all firms in Finland. A comparison group was constructed using propensity score matching, with five financial variables and the industrial classification of the firm as the identifying variables. The results, derived under three matching techniques and regression adjustment and for various lengths of time, show that wage subsidies are associated with a 6 percent increase in payroll. In another study of the Finnish wage subsidy program, which used the same data set and similar methodology, Kangasharju (2005) estimated a 9 percent increase in payroll as a result of the wage subsidy program. He also finds no evidence of a displacement effect.

Denmark. Rotger and Arendt (2008) used a similar approach to evaluate the Danish wage subsidy program. The Danish program targets long-term unemployed and subsidizes 50 percent of the minimum wage for a maximum of one year, with restrictions to minimize displacement. Rotger and Arendt considered only small firms (1–10 employees) in their analysis. Using matching techniques to form a comparison group of similar firms, their analysis finds that after the completion of the subsidized period, the subsidy contributed 0.71 jobs per subsidized firm. Taking into account a separation of 0.45 per subsidized firm, the net job creation attributed to the subsidy is 0.26 jobs per subsidized firm. Obviously, with respect to total employment creation, the take-up rate is important. They find that less than 7 percent of the eligible firms in this size range have a subsidized employee.

United States. Hamersma (2008) examines the effects of WOTC/WtW wage subsidies on employment, wages, and job tenure. She uses unique administrative data from Wisconsin to identify subsidy-eligible and subsidy-certified workers. Her evaluation focuses on one of the targeted populations under WOTC—short-term welfare recipients. Hamersma finds a positive effect of WOTC on employment near the time eligibility occurs (a 5.9 percent higher probability of employment over the comparison group in the second quarter after eligibility), but this effects does not extend to the fourth quarter after eligibility. Hamersma attributes the negligible long-run effects to low rates of participation in the WOTC/WtW programs. Fewer than 10 percent of eligible individuals were claimed by their firms in order to collect the subsidy. Hamersma does find a positive earnings effect, with subsidized workers earning $105 (or 9 percent of the average quarterly earnings for a relevant job). However, she does not find any evidence that subsidy-certified workers have longer job tenure than comparable uncertified workers. Therefore, it appears that the primary earnings gains due to WOTC came through the subsidized job itself and not through changes in the worker's broader future employment following the subsidized jobs.

3.4.2.5 Evaluating subgroups served by U.S. wage subsidy programs

The U.S. federal government has implemented two major wage subsidy programs. The Targeted Job Tax Credit was authorized in 1978 and was in effect until 1996, when it was replaced by the Work Opportunity Tax Credit, which was augmented in 1998 by the Welfare-to-Work Tax Credit. Several evaluations of these programs were conducted; none, however, used random assignment. The Hollenbeck et al. (1986) study, conducted for the U.S. Department of Labor, is worth highlighting because it provides separate analyses of four subgroups. The evaluation uses administrative data, including UI wage records and program data, to construct the treatment and comparison groups. The treatment group in this case is TJTC-certified workers, and the comparison group is other noncertified workers. Instead of using matching techniques, Hollenbeck et al. used regression analysis and selectivity-corrected regression to control for demographic factors. They used two methods to correct for selectivity bias: The first was to exclude all post-hiring vouchers from the sample, since those who were admitted to the program after receiving a positive outcome—that of being hired—obviously had a smaller likelihood of having been randomly selected into the program. The second method was to use the Barnow, Cain, Goldberger (1980) approach of including in the outcome equation a predicted variable of program participation based on a subset of explanatory variables not included in the outcome equation.[9] The general findings of this evaluation are that subsidies increased the length of employment but reduced the mean wages relative to comparison groups.

Table 5 summarizes the results for various subgroups. Following Hollenbeck et al.'s (1986) notation, "+" denotes positive and statistically significant, "0" denotes an effect of nearly zero, and "–" denotes negative and statistically significant. A second entry of "0" indicates that the coefficient was not statistically significant. According to the table, welfare recipients benefit from subsidies by being employed more quarters than their comparison group, and the greater number of quarters results in higher average wages throughout the period studied. These benefits accrue without a significant decline in average wages during employment or displacement. The results are similar for males and females and for white and nonwhites, with white females (and to some extent white males) exhibiting slightly stronger results with respect to average wages during employment. Gender differences were noticeable for youth and the handicapped. For youth, females fared better than males; for the handicapped, males did better than females. The most typical impact of TJTC is to increase quarters of employment but to have negative effects on mean wages, conditioned on employment relative to comparison groups, which suggests that wages are relatively lower than in the comparison group. Furthermore, while the results are not shown in the table, the authors find displacement effects in many of the subgroups, with the magnitude being no more than a one-to-one substitution effect.

[9] Including the selection-correction equation in the outcome equation suggested nonrandom selection. The authors conclude that in general these results suggest that white males who are vouchered tend to be the least employable, while "creaming" is exhibited for white females and nonwhites (p. V-1). The authors comment that several of the estimates are implausibly large.

3.4.2.6 Comparing the effect of wage subsidy programs on indigenous people with the effect of other programs

A report commissioned by Australia's Department of Employment and Workplace Relations (now called the Department of Education, Employment and Workplace Relations) evaluated the effectiveness of wage subsidies on Australian indigenous job seekers relative to other programs. This report, Hunter et al. (2003), used a longitudinal survey to follow the employment outcomes of job seekers who participated in five different programs: 1) employment support, 2) training, 3) job creation, 4) wage subsidy (including apprenticeship and traineeships), and 5) job search training. To control to some extent for selection bias, Hunter et al. compared the outcomes of those who completed the program with those who did not complete the program. Using this methodology for each of the five program types, they found that wage subsidies stood out as the most effective ALMP treatment. Wage subsidies were associated with a longer duration of employment, a shorter duration of unemployment, and a greater number of spells of employment than the other programs. The authors, however, point out that comparing program completers with those who did not complete the program can introduce bias into the estimates because of differences between the two groups, and it is unclear whether the bias overstates or understates the effect of the program. For example, if noncompleters are less motivated than completers, then the comparison may overstate the effects of the program. On the other hand, as the authors point out, if the noncompleters who did not indicate a reason are more likely to leave a program because they secured employment or reentered education, then the difference between completers and noncompleters may understate the effect of a program (p. 66). The evaluators were not able to account for displacement or substitution effects (Hunter, Gray, and Chapman 2000).

3.4.2.7 Evaluation of two wage supplement programs: Canada's SSP and the U.S.'s EITC

Wage supplement programs have a similar purpose as wage subsidy programs: to encourage low-income workers to find jobs and expand their hours of work. The difference is that wage supplement programs provide tax credits (or other income assistance) directly to employees, whereas wage subsidy programs provide tax credits directly to employers. Theoretically, the difference should not matter in encouraging employment, but evaluations have shown that differences do exist, particularly the possible stigma effect when workers are identified as low-income and possibly low-productivity workers. Canada conducted and evaluated an experiment, the Self-Sufficiency Project (SSP), which provided wage supplements

Table 5 Estimated Effects of TJTC Subsidies on Selected Subgroups

Race/Sex	Outcome				
	Avg. wages during employment	Avg. quarters of employment	Avg. wages	Avg. quarters per employer	Displacement
Youth					
White males	**+/0**	+	+	-	-
Nonwhite males	**-/0**	+	+	-	**-/0**
White females	+	**+/0**	+	-	**-/0**
Nonwhite females	+	**0**	+	**-/0**	**-/0**
Welfare					
White males	**+/0**	+	+	**-/0**	**0**
Nonwhite males	**-/0**	+	+	**+/0**	-
White females	+	+	+	**+/0**	**-/0**
Nonwhite females	**-/0**	+	+	**+/0**	**-/0**
Veterans					
Whites	**-/0**	+	**+/0**	-	-
Blacks/Hispanics	**+/0**	**+/0**	**0**	**-/0**	-
Handicapped					
Males	+	+	+	**-/0**	-
Females	**-/0**	+	**+/0**	**-/0**	**-/0**

Source: Hollenbeck and others (1986).

to long-term welfare recipients. It was evaluated using a random assignment research design. The United States enacted a major wage supplement program, the Earned Income Tax Credit (EITC), which was evaluated using a quasi-experimental research design. Whereas the SSP was never implemented into a large scale program; the EITC has evolved into the United States' largest welfare assistance program.

The results from the SSP evaluation show a relatively large take-up rate, increasing employment and earnings and reducing welfare use and poverty. One-third of the long-term welfare recipients who were offered the SSP earnings supplement took up the supplement and worked full-time. Results also showed that those receiving the supplement were twice as likely as control group members to be working full-time. As a result, SSP increased earnings by $3,400 per person, more than 20 percent over the earnings of the control group. However, the effects of SSP were not long-lasting. After the supplement ran out, there was no difference in employment outcomes between the control and treatment groups.

Since no provision was made to evaluate the United States' EITC program using random assignment, researchers have had to construct comparison groups using various methods. Most have looked at the effect of changes in program design at various times in its history by comparing the effects on those individuals affected by the change with those not affected.

Examples of this approach include the evaluation by Eissa and Liebman (1996). To construct a comparison group, they used the increase in subsidy rate in 1986 from 11 percent to 14 percent and an increase in the maximum income to which the subsidy rate was applied. The change affected single women with children only. Therefore, the study compares the change in the labour supply of single women with children (the treatment group) to the change in the labour supply of single women without children (the comparison group).

Results from difference-in-differences estimation shows that the labour force participation rate of the treatment group increased from 0.729 to 0.753, a statistically significant increase of 2.4 percentage points. In contrast, there was no change in the participation rate of the control group. For the least educated women with children, the participation rate increased 6.1 percentage points. However, there is no evidence that EITC induces those already in the labour market to work more hours. Moreover, there is no strong evidence that the EITC depressed wage rates significantly. Eissa and Liebman's results are consistent with those of other researchers who have examined the various effects of EITC.

3.4.2.8 Summary of evaluation effects on population subgroups

The evaluations reviewed above found modest gains in the employment and labour force participation of participants in wage subsidy programs. The net employment effects range from around 3 to 9 percent increases. Wage supplement programs appear to have larger take-up rates and induce significant increases in participation rates. Many wage subsidy programs target specific populations. The programs for which we selected evaluations targeted upwards of 15 population subgroups. The net employment effects across these population subgroups are mostly positive, and for those that obtained statistically significant estimates, the magnitudes of the net impact estimates are quite similar. This assessment of wage subsidy and wage supplement evaluations is similar to that provided by Kluve (2010) in his meta-analysis study of European programs. Of the 22 European wage subsidy programs in the study, 17 found positive and statistically significant employment effects.[10] Nine of these studies evaluated programs that targeted youth, and of these nine evaluations, six found positive and statistically significant results. Kluve concludes from his meta-analysis that wage subsidy programs can be "effective in increasing participants' employment probabilities." Betcherman, Olivas, and Dar (2004) are less favorable in their critique of 17 evaluations of wage subsidy programs in developed countries. Their conclusion is that the results are mixed, with only six showing significant long-run employment effects. This is consistent with our assessment with respect to long-term effects, but we find that several more estimate short-term employment effects. Our list includes more recent programs or more recent evaluations.

Bartik (2001) offers a favorable assessment of wage subsidies in the United States. His critique of two dozen or so evaluations of U.S. programs leads to the conclusion that properly designed programs can be effective in increasing the employment and earnings of the disadvantaged. This applies to both wage subsidy and wage supplement programs.

[10]Not all of the 22 programs are wage subsidy programs, since Kluve combines wage subsidy programs with self-employment programs in his analysis.

Evaluations also found significant inefficiencies in the programs, although not all evaluations included estimates of deadweight loss. Bartik (2001) concludes from his critique of the various studies that the windfall wastage is somewhere within the range of 50 to 92 percent, with a midpoint of around 70 percent. This means that seven out of every ten dollars spent on subsidies goes to subsidize hires that would have occurred anyway. The wastage inflates the per-participant cost beyond the amount of the wage subsidy. Also, it appears that programs that target a narrow population subgroup are less inefficient. One study found significant displacement effects for specific subgroups such as black male youth and black adult males and females.

Another issue with wage subsidies paid directly to employers is the low take-up rates. According to Bartik, this may be the biggest limitation. Evaluations show that less than 10 percent of eligible hires are typically claimed on employers' tax returns. Employers complained of high compliance costs, even when much of the paperwork was handled by consultants. Some evaluations surmised that employers did not want to be encumbered by government regulation and oversight and that the benefits of the subsidies did not exceed the perceived costs of such intrusion. However, even when the required paperwork associated with the programs is handled by other organizations, firms are still reluctant to participate. It is not clear whether larger subsidies to cover the fixed cost of compliance would make a difference.

Yet another unintended effect of wages subsidies that concerns some evaluators is their negative effect on skill formation. They argue that wage subsidies (to both employers and employees) targeted at the economically disadvantaged discourage the acquisition of skills, because higher skills lead to higher wages, which in turn reduces the likelihood that such employees will be entitled to wage subsidies. One study shows that wage subsidies actually reduce the accumulation of skills (Heckman, Lochner, and Cossa 2003).

Subsidies paid directly to employees have proven more effective. Dickert-Conlin and Holtz-Eakin (2000) concur that employer-based programs have been characterized by low participation rates and relatively little success. An employee-based approach, such as the EITC, appears relatively more successful in targeting the desired population, inducing additional labour force participation, and raising earnings (p. 263).

3.4.3 Self-Employment (SE)

Self-employment assistance is an active labour market program (ALMP) to promote reemployment for a small proportion of UI recipients. Unlike most other ALMP, which operate through the supply side of the labour market by increasing the quantity or value of job seeker skills, self-employment assistance is designed to boost labour demand through direct job creation for unemployed workers. Before discussing the effectiveness of SEA as a reemployment program option, we start by reviewing related literature on the effects of self-employment spells on future labour market experiences.

3.4.3.1 Background on self-employment

In 2009 there were 32.5 million self-employed in the 27-nation European Union. These workers accounted for nearly 15 percent of total employment, or more than one job in seven (European Commission 2010a). In the United States in 2009 about 15.3 million people were self-employed, constituting nearly one out of every nine jobs (Hipple 2010). The self-employed are a heterogenous group including building trades workers, home-based craft and piece workers, proprietors of small farms, cosmetologists, and professional services providers like physicians, attorneys, and accountants. A summary of self-employment studies is given in Table 6. Information on self-employment patterns for subgroups is given in Table 7.

Branchflower (2000) presented a survey of self-employment in OECD countries at a Burlington, Ontario, conference sponsored in 1999 by the Canadian Employment Research Forum. He presented evidence that since 1966 the trend in self-employment has been down in most OECD countries (except Portugal, New Zealand, and the United Kingdom).[11] In most of the countries he studied, Branchflower (2000) finds that the rate of self-employment declines as the unemployment rate rises; that the self-employed are more likely to be male and older; that both the least-educated and the most-educated have higher probabilities of being self-employed; and that the self-employed are less willing to move from their neighborhoods, towns, and regions than are wage and salary employees.[12] Branchflower (2000) attributes this last result to the pull on the self-employed of their customers.

There is a wide range of estimates concerning the effect of self-employment on subsequent income and employment stability. Some transitions to self-employment are permanent and result in secondary job creation through hiring within microenterprises. For many others, self-employment is transitory and is followed by lower future employment rates and earnings in wage and salary jobs.

Bruce and Shuetze (2004) examine the effect of self-employment experiences in the United States using data from the Panel Study of Income Dynamics dating back to 1979. They estimate regression models on earnings controlling for observable characteristics.[13] They find that, relative to continued wage employment, brief spells in self-employment do not increase—and probably actually reduce—average hourly earnings upon return to wage employment.

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[11] While net self-employment has been down, Branchflower (2000, p. 488) estimates a U-shaped time trend with a minimum toward the end of the 1980s and a gradual recovery in self-employment rates since that time.

[12] Branchflower's (2000, p. 489) analysis of determinants in Canada suggested higher self-employment rates for those who are less educated (but not higher educated), older, males, and have dependent children. He also found a slight upward time trend in Canadian self-employment from 1975-1996.

[13] Control variables include months of job tenure, union membership, local area (county) unemployment rate, educational attainment, race, age, marital status, number of children under age 18, household asset income, residence in a metropolitan statistical area, and geographic region of residence (Northeast, South, and West, where North-Central is the reference category). To control for the potential endogeneity associated with self-employment and unemployment experience, they add the log of hourly wage sector earnings recorded at the start of the five-year period as an independent variable.

Table 6 Summary of Studies of Self-Employment Experience on Subsequent Earnings

Authors (year)	Countries	Method	Sample	Intervention	Earnings
Bruce and Schuetze (2004)	U.S.; SE rates: males 6%, females 3%	Control for differences in individual, household, and occupational characteristics in OLS models of outcomes.	Panel Study of Income Dynamics: 7 cohorts each with 800 men, 300 women. Inflow: 1979 to 1985; Outflow: 1984 to 1990	Observational study of labour force transitions. Any SE or unemployment in 5-year period vs. steady employment	Neither SE nor WS earn > SE earn > unemployed earnings
Moore and Mueller (2002)	Canada	Control for observable characteristics in probit models of transition from WS to SE.	Canadian Labour Market Activity Survey, 1988 to 1990. Transitions within a year from WS to either WS or SE. SE: 650; WS: 9182	Observational study of labour force transitions	IF UI receipt less earnings from SE
Zissimopoulos and Karoly (2007)	U.S.	Control for observable characteristics in multinomial logit models of transition from WS to SE or other labour market states.	Health and Retirement Study: Five survey waves of persons over 51 years of age in years 1992 to 2000	Observational study of labour force transitions from WS to SE, retire, or out of LF	-

NOTE: SE = self-employment, WS = wage and salary employment, LF = labour force

Table 7 Summary of Studies of the Effect of Self-Employment Experience on Earnings: Subgroups

Authors (year)	Subgroups	Subgroups	Features	Other	Net benefits
Bruce and Schuetze (2004)	More males in SE, similar effects	Males higher avg. wages, similar percentage effects	SE also increased prob. of PT vs. FT at terminal	1984 to 1990 expansion	-
Moore and Mueller (2002)	Longer-run increases SE	Involuntary sep. more likely SE	Pers. rea. sep. most likely SE	Vol. sep. less likely SE	-
Zissimopoulos and Karoly (2007)	Pension coverage reduces movement in SE	Wealthier older workers moved into self-employment	Flexible earnings arrangements lead to SE	No diff. M or F	-

Furthermore, those who experience self-employment have difficulty returning to the wage sector. However, these consequences are small compared to similar experiences in unemployment. Bruce and Schuetze (2004) examined seven cohorts over five-year periods with inflows between 1979 and 1985 and outflows between 1984 and 1990. In Bruce and Shuetze's samples, about 6 percent of men and 3 percent of women experienced self-employment. They found that on average those who experienced a spell of self-employment had subsequent income that was above those who experienced unemployment during the five years, but below those who were neither unemployed nor self-employed. Additionally, self-employment increased the likelihood

that future work would involve part-time jobs. Overall men fared better following a spell of self-employment than did women.

Moore and Mueller (2002) studied transitions from wage and salary employment into self-employment in Canada. They hypothesize that some workers may be pushed into self-employment because of inadequate demand for wage and salary workers. Examining transitions from paid work to self-employment using data from the Labour Market Activity Survey for the years 1988 to 1990, Moore and Mueller find evidence of their self-employment push hypothesis. They estimate probit models of the change from wage and salary employment to self-employment, controlling for observable characteristics.[14] They find that the likelihood of self-employment is typically influenced by five factors. It is 1) increased by longer spells of joblessness(and this effect is stronger for women), 2) reduced by the receipt of cash unemployment benefits between jobs, 3) higher for workers involuntarily laid off because of lack of work than for workers who undergo voluntary separations, 4) lower for job separations due to personal reasons, and 5) unaffected by labour market conditions. Overall, Moore and Mueller (2002, p. 800) conclude that "few of the individuals in the sample, however, appear to be pushed into self-employment."

Zissimopoulos and Karoly (2007) studied self-employment among older workers in the United States. They use five waves of data from the Health and Retirement Study (HRS) panel—every other year from 1992 to 2000—and estimate multinomial logit models of transition from wage and salary employment to self-employment, retirement, or not working, while controlling for observable characteristics. This HRS survey sample of older Americans has higher average rates of self-employment than a sample of younger Americans surveyed in the Current Population Survey. Indeed, Zissimopoulos and Karoly (2007, p. 275) compute that on the HRS survey date, 39 percent of male respondents aged 66 who reported working full-time were in self-employment. In estimating models of transition to self-employment, Zissimopoulos and Karoly (2007) control for four categories of observable factors: 1) demographic characteristics; 2) wealth, pensions, and inheritance; 3) health status; and 4) job characteristics. They find that among older workers, movement into self-employment is more likely to occur among those with the following characteristics: pension coverage, higher levels of personal assets, and more flexible employment arrangements. Their evidence also suggests that poor health—as measured by the presence of a work-limiting health condition—appears to push older workers into self-employment. Since health-limiting factors do not increase movement into self-employment for younger workers, they argue that older workers with a work-limiting health condition are better able to accommodate their condition and continue working if they are self-employed, as compared to having wage or salary employment.

[14] Control variables include the regional unemployment rate, weeks between jobs, UI benefit receipt, involuntary job separation, voluntary job separation, separation for personal reasons, gender, region (Atlantic, Quebec, Prairies, British Columbia), number of family members in labour force, marital status, number of children, age, visible minority, immigrant, educational attainment, union membership, pension coverage, months of job tenure.

3.4.3.2 Background on self-employment assistance (SEA)

Through the self-employment assistance (SEA) program, unemployed Canadian workers are provided assistance in setting up their own businesses. Those eligible for EI cash payments through Part I, if granted SEA assistance, are not required to search for a new job. Instead they are able to collect their remaining cash EI entitlements though Part I, and may receive additional Part II benefits. Earnings while in SE are not insurable under EI and therefore do not renew EI eligibility (Nicholson 2010, p. 4). Total spending on SEA in Canada amounted to about $145 million in 2005/2006, with approximately 12,000 individuals starting this intervention during the year. Hence, on a per-participant basis, this is an expensive intervention—averaging over $12,000 per client (Nicholson 2010, p. 14).

France provided the first self-employment assistance to the unemployed in 1979. By the early 1990s there were SE programs in 17 member countries of the Organisation for Economic Co-operation and Development (OECD). The French and UK programs are examples of the two main alternative SE assistance designs. The French program provides clients with a lump sum payment for business start-up, whereas in the UK, clients receive weekly "Jobseeker's Allowance" payments but are granted a work-search waiver while self-employment is pursued. In addition to France, the lump sum approach is used in Luxembourg, Norway, Portugal, Spain, and Sweden. The periodic payments approach is used in Australia, Belgium, Canada, Denmark, Finland, Germany, Greece, Ireland, Italy, the Netherlands, and the United Kingdom. Self-employment programs are used by a small fraction of the unemployed, but they can account for a large share of new business formation. In France, SE constituted nearly 25 percent of new businesses in the early 1980s, and in the UK, it constituted about 20 percent in 1987 (Messenger and Wandner 1994).

Various forms of SEA programs operate in most OECD countries, although many of these are relatively small. Overall, OECD countries devoted only about 2 percent of total active labour market spending to self employment, so, by this standard the Canadian program is quite large (Nicholson 2010). Among the 27 European Union (EU) countries, 19 have SEA programs. The numbers include 10 of the 15 countries that were EU members before 1996 and 9 of 12 who joined after 1996. SEA spending constitutes on average 2.5 percent of ALMP spending for the older EU members and 2.0 percent for the newer members (European Commission 2010b).

In the United States, an SEA program was established in 1993 on a temporary basis as part of the North American Free Trade Agreement (NAFTA) and was made a permanent part of federal UI legislation in 1998. Federal legislation followed directly from the design of a successful Massachusetts demonstration project. To participate, UI claimants must be determined to be likely to exhaust their UI benefits, based on the worker profiling mechanism that is used for the Worker Profiling and Reemployment Services system. The SEA program has remained very small, with only seven states choosing to have active programs. Only five states typically have more than 100 participants per year, and no state has had as many as 1,000 participants in any year. Participation has been mainly in the states of New Jersey, New York, and Pennsylvania, with fewer participants in Maine and Oregon. The small size of SEA can be seen in various

types of measures. The number of workers entering SEA has only risen as high as 7,300 per year, and annual payments have amounted to no more than 50,000 weeks compensated and $17 million in benefits paid. These are very small numbers compared to the regular UI program, which paid $32.7 billion to 7.5 million beneficiaries in 2007 (Wandner 2010). Participation in SEA rose sharply during the recent severe recession: for example, Oregon SEA cases rose from 319 in 2008 to 716 in 2009. Still, no states even come close to approaching the federal cap, set at 5 percent of a state's pool of regular UI beneficiaries (Haislip 2011).

3.4.3.3 Effectiveness of self-employment assistance

The LMDA evaluations in Canada suggest that SEA increased hours worked after participation, but there was no evidence about whether the added hours were in self-employment or wage and salary employment. The Canadian SEA was also found to decrease EI receipt, but estimates of impacts on earnings were mostly negative. The negative impacts on earnings and the decreased use of EI may have reduced total annual income; however, this was not assessed in the evaluations. Nicholson (2010) suggests the deadweight loss for the Canadian SEA program may be high, because offers are typically made to older people with more work experience and to those with above-average educational attainment. That is, people with a high propensity to become self-employed anyway are more likely to receive SEA. He also notes that the take-up rate is low, and most employment is in low-skill, low-wage sectors.

Our review of self-employment assistance and microenterprise evaluations includes studies of programs in Canada, Germany, Hungary, Poland, Sweden, and the United States. A summary of these SEA evaluations is given in Table 8. Evaluation results for SEA subgroups, program features, and labour market conditions are given in Table 9.

Canada. Other evaluations of SEA in Canada were done by Graves and Gauthier (1995); Wong, Henson, and Riddell (1998); and Finseth (1999). The latter was not a formal comparison group design evaluation, but rather a survey of self-employment activity, prior evaluations of SEA, and the adequacy of financing systems to support microenterprise establishment and development in Canada.

Graves and Gauthier (1995) performed a nonexperimental evaluation of SEA in Canada with a sample of 1,479 participants and 2,700 comparison-group subjects contacted in telephone surveys. The participant sample resulted from a 62 percent response rate when attempting to interview the census of SEA participants in program year 1992/93. The comparison group resulted from strategic selection on observable variables from the register of UI beneficiaries and their 28 percent response rate to the survey. The comparison group was selected to match the SEA participant survey respondent sample on the dates of beginning UI benefit receipt and province. When contacted as part of the survey, the comparison group was asked to rate their interest in self-employment, in the same way that SEA participants are asked during program screening. The final comparison group was culled to yield a pattern of interest in self-employment similar to the SEA participant sample. With these participant and comparison samples, program effects were estimated using regression adjustment on observables. The

evaluation noted that because of the similar dates of beginning UI receipt and the time spent in SEA by participants, there was a longer mean post-program period for the comparison group (65 weeks) than for the participant group (40 weeks). The net result of matching and response rates yielded a comparison group that was younger, less likely to be married, less likely to own a home, and less likely to have educational attainment beyond secondary school. Compared to the general population of UI recipients, SEA participants tend to be older, with more labour market experience, more education, and higher levels of personal financial assets.

Graves and Gauthier (1995) attempted to overcome sample selection issues by computing program effects in regression models, controlling for observable characteristics. They estimated logit models on binary outcomes and OLS models on continuous outcomes, controlling for observable variables such as age, sex, education, geographic region, marital status, and attitudes toward risk-taking. They assert that efforts to correct for selection bias by using Heckman two-step procedures suggested that selection bias was not an issue. The report would have benefited from a fuller exposition of these results, since some of the results suggest that selection was a key problem in the evaluation. In particular, the SEA program effect estimates were much larger when contrasted to the full comparison group than when compared to those who actually started self-employment. About 83 per cent of SEA clients were still operating their SEA venture at the time of their evaluation interview, an advantage of 7 percentage points over the average of those in the comparison group who started their own businesses. SEA participants experienced an increase in earnings between the pre- and post-program periods, but in contrast to the comparison group had lower earnings in the first year after SEA and significantly higher earnings in years two and three after SEA. SEA participants were 4 percentage points less likely to receive cash social assistance than those in the comparison group, and, measured on average eight months after completing the program, they also had drawn $2,632 less from the UI account.

Graves and Gauthier (1995) estimated that, from a governmental perspective, UI benefit savings do not outweigh the substantial $13,000 of additional UI resources which were invested in workers' SEA participation. The savings from SEA were estimated to pay back the government costs within an average of three years and nine months. Considering all values and costs in a full social accounting framework, the average break-even point could be reached within 18 months. However, it was estimated that about half of program participants would have entered self-employment even without government SEA. This deadweight estimate doubles the average payback costs to around 7 years for the government and 3 years for society. Participant versus nonparticipant contrasts among subgroups, controlling for characteristics, revealed that SEA impacts for social assistance recipients (SARs) were positive but smaller. SAR participants had smaller investments in SE and used public instead of private financing. However, the business survival rate for SAR participants was about 85 percent on the survey data, which is on par with the full participant group. Compared to other SEA participants, SAR participants had higher reported earnings, business profits at the same level, were less likely to have paid employees, and made less use of EI after SEA. There were no statistically significant differences in outcomes across age groups. Canadian equity groups, including visible minorities, disabled, and aboriginals, did not show significant differences in self-employment survival, earnings, income, EI use, or hiring. There were no differences across regions in survival rates, but self-

employment start-ups in Quebec and Atlantic Canada had more favorable outcomes compared to SEA start-ups in Ontario and Western Canada. These included higher values of business revenues and profits and more hiring of paid employees. Participant versus nonparticipant contrasts among subgroups, controlling for characteristics, revealed that SEA impacts for social assistance recipients (SARs) were positive but smaller. SAR participants had smaller investments in SE and used public instead of private financing. The business survival rate for SAR participants was about 85 percent. Compared to other SEA participants, SAR participants had higher reported earnings, business profits at the same level, were less likely to have wage or salaried employees, and received less EI after SEA. There were no statistically significant differences in outcomes across age groups. Canadian equity groups, including visible minorities, the disabled, and aboriginals, did not show significant differences in self-employment survival, earnings, income, EI use, or hiring. There were no differences across regions in survival rates, but self-employment start-ups in Quebec and Atlantic Canada had more favorable outcomes compared to SEA in Ontario and Western Canada. These included higher values of business revenues and profits and more hiring of paid employees.

Wong, Henson, and Riddell (1998) examined whether self-employment assistance in Canada increased the earnings of program participants during the years 1987 to 1996. The self-employment incentive (SEI) program operated under the Community Futures program from 1987 to 1991 in about 200 nonmetropolitan areas experiencing labour market adjustment difficulties. The SEI was funded from general revenues and provided a weekly taxable allowance of $180 (rising to $230 in 1991) for up to 52 weeks to successful applicants in lieu of UI or social assistance while they attempted to start self-employment. The SEI also provided access to free business counseling. From 1992 to 1995 the self-employment assistance (SEA) program operated as part of the UI program. Entrepreneurship training remained free but became mandatory under SEA. The SEA start-up allowance was equivalent to the weekly UI benefit amount and was paid from the UI fund. Additionally, the SEA required a personal investment of at least 25 percent of start-up costs, up to a maximum of $4,000.

The evaluation relied on program administrative records maintained by Human Resources Development Canada and earnings records maintained by Revenue Canada. From the Status Vector of the social insurance benefits master file, detailed information was drawn on work history, insurable earnings, and UI claim history. The Canadian Jobs Strategy (CJS) file provided records of program interventions financed from general revenues. The T1 tax filer administrative data gives annual individual income by source.

More than 66,000 persons participated in self-employment between 1987 and 1998, amounting to about 2 percent of regular UI claimants. An initial comparison group of 160,205 individuals was randomly selected from the UI inflow, evenly distributed across years and amounting to one-half of one percent of regular UI claimants. About one-quarter of the initial comparison sample was randomly selected for analysis. Relative to the SEI period, the SEA program enrolled a significantly higher share of UI beneficiaries, topping out at around 11,000 annual participants at the end of its period, or three times the level of SEI.

Table 8 Summary of Self-Employment Assistance Program Evaluation Mean Effects Estimates

Authors (year)	Method	Country/sample	Intervention	Self-empl.	Any empl.	EI/UI Receipt
Benus et al. (2008)	RA: participants got 13 hours more SE training than comparison group	USA/Inflow at one-stop centers: 2003 to 2005; 7 sites: Pennsylvania (2), Minnesota (2), Maine (3)	Assessment, classroom training, 1-on-1 business counseling, help with loan applications	At 3 quarters +6%; at 6 quarters +3%	No significant effect, but more in SE than in WS	+1 week, +2 weeks if on UI first
Benus et al. (1995)	RA: among profiled high-exhaustion-probability UI beneficiaries	USA/Massachusetts Inflow 1990–93, treatments: 614, controls: 608	British style SEA: weekly UI pay with work search waiver	at 19, 31 mos. +11%, +5%	at 19, 31 mos. +1%, −4%	−1.8 weeks
Benus et al. (1995)	RA: among profiled high-exhaustion-probability UI beneficiaries	USA/Washington State Inflow 1989–90, treatments: 755 controls: 752	French-style SEA: Lump sum balance of UI entitlement	at 21, 33 mos. +16%, +12%	at 21, 33 mos. –9%, −8%	−7.6 weeks
Carling and Gustafson (1999)	NE : controlling for observable characteristics in hazard models	Sweden/Inflow census 6/95–12/96 follow-up at 27 months, se: 9,043, ws:14,142	Policy to target se to women and foreign-born	reduces unempl. after SE vs. subsidized job	SE double WS for male and female natives of Sweden	—
Graves and Gauthier (1995)	NE: matching on observables plus regression adjustment	Canada/Telephone survey, se participants: 1,479, comparison: 2,700	UI-type payments continued an extra 52 weeks once SEA starts, and participant finances 25% of project	8% higher rate vs. those in comparison group who started SE	18.3% vs. those in comparison group who started SE (regression-adjusted)	−8.6% v full comp group (reg adj) −7.3 weeks v comp gp started SE (reg adj)
O'Leary (1999)	NE: matching on observables plus regression adjustment	Hungary/Inflow in 1996, participants: 1067, comparison: 3,338	British style: monthly UI-type payments for 18 mos.	—	WS employment 0.16 percentage point increase 9 months after SE	−1.6 months
O'Leary (1999)	NE: matching on observables plus regression adjustment	Poland/Inflow in 1993–94, participants: 709, comparison: 10,000	French style: lump sum, but must repay	—	WS employment 0.27 percentage point increase about 36 months after SE	+3.65 months
Pfeiffer and Reize (1998)	NE: instruments for SEA in probit model	Germany, Oct. 1993 to August 1995 census	UK-type after 4 weeks ui; 6 to 24 months	Survival gain, East 0.0197 West 0.0080	—	—
Wong, Henson, and Riddell (1998)	NE: regression adjustment for observable differences, diff-in-diff models	Canada, 1987 to 1995 census SEI and SEA; comparison random sample UI inflow	SEI 1987–91: $180/wk. to $230 not UI 52 wks.; SEA 92–95 = UI from UI account 52 to 156 weeks	Increased SE+WS earnings	—	—

NOTE: RA= random assignment experiment, NE= nonexperimental, SE = self-employment, WS = wage and salary employment, FE = fixed-effects estimator.

Table 9 Summary of Self-Employment Assistance Program Evaluation Subgroup Effects Estimates

Authors (year)	Earnings	Subgroups	Subgroups	Features	Labour markets	Net benefits
Benus et al. (2008)	−$200 WS in the year after SEA	No impact on public assistance receipt	—	—	Economic recovery period	No rise in SE earnings, fall in WS earnings, bigger effects on UI recipients
Benus et al. (1995)	SE $1,219, WS $3,053	Males less likely in WS employment	Males less likely in SE Males gain in total earnings (SE+WS)	Add jobs at 19, 31 mos. minus 0.4, 0.9	UN89–93: 4.1, 6.0, 8.2, 7.6, 6.2	$13,843 to individuals, $2,089 to government
Benus et al. (1995)	SE $2,157, WS –$1,744	Males less likely in SE	Whites, males, expect recall: SE+WS earnings up	Add jobs at 21, 33 mos. plus 0.1, 0.3	UN89–93: 5.5, 4.4, 6.0, 7.3, 6.9	$696 to individuals, −$1,246 to government
Carling and Gustafson (1999)	—	No diff. in effects by sex for Swedish-born	Not effective for foreign-born unemployed	Increase in supply of skilled unemployed	Rising unemployment rate 1990–2000	—
Graves and Gauthier (1995)	−1,139 1 yr. + 3,911 2 yr. 3 yr.	Social assistance recipients' effects on par or stronger than average participant	No diff. by age; equity groups 5% lower survival & $300 less earnings	Added jobs higher by 6.5 percent vs. comp. group	Quebec and Atlantic more likely to hire others	6 years to recover SEA costs per participant
O'Leary (1999)	−$26/mo.	+Older	+Vocational education +School graduate	—	+High unemployment areas	—
O'Leary (1999)	$69/mo.	+Females +work experience less than 3 yrs.	+Blue-collar occupation +Not long-term unempl.	Effect increases with size of own investment	—	—
Pfeiffer and Reize (1998)	—	Employment growth east Ger 0.02, west Ger 0.03	—	—	—	—
Wong, Henson, and Riddell (1998)	SEI 19K to 27K per yr. SE+WS	Prime age males SE higher earn. than comp.	Women had higher SE income; older and Quebec worse (lower)	SE superior to UI alone; SE earnings effect declines yr. 2 and yr. 3	Recession 1991–92	—

NOTE: SE = self-employment, WS = wage and salary employment.

The Wong, Henson, Riddell (1998) research design boils down to regression correction for differences in observable characteristics. The first step is to compute differences in earnings before and after the program period separately for program participants and the comparison group. These computations are intended to remove effects on earnings due to characteristics that do not change over time, including unobservable factors such as innate ability and motivation. These first difference earnings data are then used as dependent variables in regression models with indicators for program participation and other variables to correct for differences across the two groups in observable characteristics. In addition to a dummy variable for participation, the explanatory variables include gender, age, experience (and experience squared), and regional labour market controls.

The empirical evidence provides support for the hypothesis that combined earnings from market work and self-employment are increased by adding a self-employment experience through either SEI or SEA. The basic fixed effect estimates of the SEI effect on gross earnings from self-employment are large and positive, ranging from $19,000 to $27,000 per year, depending upon the cohort. These mean estimates increase somewhat when other controls are included. Control variables suggested that women had higher self-employment income, while older persons and those in Quebec had lower income. The impacts are much stronger in the first year after the program, with self-employment earnings estimates tapering off in the second and third postprogram years. These estimates of earnings effects range from $1,000 to $2,000 per month and are large compared to results found in other countries. This study would have benefited from matching participants with nonparticipants in the very large random comparison group before impact estimates were computed by difference in differences, with controls for observable characteristics.

Finseth (1999), with support from the International Labour Office, conducted a survey of SEA and microenterprise/microfinance programs in Canada. For evaluation results he relied on Canadian performance measurement and prior evaluation studies. He documented sources of business start-up assistance and asserted that the Community Futures model is the best channel. He stated that at the end of the 1990s about 1,200 borrowers were served by microenterprise loan funds, but that these constituted only 3.8 percent of such loans. The vast majority borrow from private financial institutions while relying on government loan guarantees. Finseth (1999) asserts that the SEA program has a number of positive impacts on the target clientele and on government, including a greater level of business success, reduced reliance on unemployment benefits, increased savings for social assistance, increased income for SEA participants, greater job satisfaction, increased levels of induced employment, and increased tax revenue contributions. From the point of view of cost-effectiveness, the SEA Program is not appealing. Finseth argues that the institutional framework for self-employment assistance delivery is inefficient. Nonetheless, he asserts that Canada's SEA program is a good thing. It is achieving its purpose and making a difference to a significant group of people, turning them into net producers and thereby reducing the number of people who act as a continuous draw on the resources of Canadian society.

Germany. Pffeifer and Reize (1999) compare firm survival and employment growth in subsidized start-ups by unemployed persons with firm survival and employment growth in subsidized start-ups by nonunemployed persons. The study was done using data from four länder in eastern Germany and eleven länder in western Germany soon after reunification. The start-up subsidy for the unemployed was provided by the Work Support Act. The empirical analysis is based on a sample of new start-ups established between 1993 and 1995. Data on German start-ups was compiled from administrative records by the research institute of the German public employment service (IAB/BA) and included 9,613 start-ups, of which 618 received a start-up allowance for the unemployed; of these, 4,311 and 395, respectively, were in the eastern länder. The model for empirical estimation is a survival equation including an endogenous indicator variable for receipt of a start-up subsidy. The auxiliary equation predicting receipt of a start-up subsidy includes the following as instruments: legal form of ownership; industry type; and sex, age, and age squared of the business leader. In eastern Germany, firm survival is longer among subsidized start-ups, and receipt of the subsidy is correlated with a slightly higher mean hiring rate. In the western part of Germany, subsidized start-ups survive at a rate similar to unsubsidized start-ups but have a slightly higher mean employment growth rate.

Hungary. O'Leary (1998) evaluated the effectiveness of self-employment assistance to the unemployed in Hungary based on a census of more than 1,000 SEA recipients during the first three calendar quarters of 1996 in half of the twenty counties in Hungary. Opposite this population was a comparison group created as matched pairs from a larger random sample of more than 3,000 unemployed persons registered as job seekers with the public employment service at the same time.[15] Matching was done without replacement by the minimum sum of squared distance on characteristics. The variables used for matching were age, sex, educational attainment, job separation reason, occupation, and county of residence. The SEA in Hungary was provided as a series of up to 18 monthly payments in amount and procedure similar to unemployment compensation, like in the British model for SEA. Follow-up surveys were conducted about 24 months after inflow to UI and about 12 months after completion of SEA. Impact estimates computed in regression models adjusting for observed characteristics suggest that SEA raised the probability of being employed on the survey date by 21 percentage points and shortened UI receipt by 1.6 months, but lowered earnings by an equivalent of $26 per month relative to the comparison group mean of $142. In Hungary, SEA was more effective for those who were older, were vocational or high school graduates, or were working in high unemployment areas.

Poland. O'Leary (1998) evaluated the effectiveness of self-employment assistance to the unemployed in Poland based on a census of 709 SEA recipients in 8 of the 49 voivodships in the country. These eight included four of the most populous voivodships and one-quarter of the nation's population. SEA participation occurred during 1993 and 1994. Interviews were conducted with participant and comparison samples between February and April 1997, so the results reflect a longer term follow-up than observed in other evaluations. The comparison group was created as matched pairs from the inflow to the public register of unemployed at the same

[15] The nation of Hungary includes a total of 20 counties (megye) as political administrative districts. Samples were drawn from 10 counties chosen to span the range of economic conditions in the country.

time. Additional variables used for matching were age, sex, educational attainment, job separation reason, occupation, and county of residence.[16] The SEA in Poland was provided as a lump sum as in the French SEA model, except the sum is a loan that must be repaid to the national labour office. Impact estimates computed in regression models adjusting for observed characteristics suggest that SEA raised the probability of being employed on the survey date by 29 percentage points and lengthened UI receipt by 3.65 months, but raised monthly earnings by an equivalent of $69 per month relative to the comparison group mean of $193. In Poland SEA was more effective for females, those with less than three years of work experience, blue collar workers, and those who were unemployed fewer than six months. In addition to having to repay the loan, the personal contribution to start-up funds was tracked, and outcomes were found to be improved for those contributing more to the self-employment project.

Sweden. Carling and Gustafson (1999) estimated the effects of SEA relative to subsidized jobs in Sweden. They used a census of data from administrative records on all participants in SEA and subsidized work for an 18-month period starting in June 1995. Controlling for participant characteristics, they estimated that SEA participants had a significantly lower risk of reunemployment compared to those who got subsidized jobs. The 1990s rise in unemployment in Sweden induced the government to increase enrollment in SEA relative to subsidized work, on the premise that the newly unemployed included a large share of skilled workers. Furthermore, SEA was targeted at women and immigrants, in hopes of helping those particular groups. Carling and Gustafson (1999) studied the census of 18-month program inflows for SEA and subsidized jobs starting in June 1995. To identify the effect parameter, Carling and Gustafson (1999, p. 21) first estimated a simple model with the endogenous program indicator for self-employment relative to subsidized work. They estimated a huge effect for self-employment to reduce the hazard of unemployment (–0.927),; the effect estimate gets smaller in magnitude as the effect is incrementally identified by sequentially adding covariates to reach a final estimate at (–0.752). The control variables included individual demographic characteristics, geographic region of Sweden, labour market conditions, and local employment office indicators. They tracked outcomes through March 1999 and found the beneficial effects of SEA to be similar for men and women who were native Swedes, but they found no reliable evidence of beneficial effects of SE for unemployed immigrants in Sweden.

United States. A recent microenterprise evaluation study, Growing America through Entrepreneurship (GATE), studied the value of helping new entrepreneurs in rural and urban communities start and expand their own small businesses (Benus et al. 2008). Evidence from the GATE evaluation added to information about providing self-employment assistance through the UI program that was tested through field experiments in Massachusetts and Washington State during the late 1980s (Benus et al. 1995).

GATE enrollment was done between fall 2003 and summer 2005 in seven urban and rural sites in the states of Minnesota, Pennsylvania, and Maine. GATE was available to any person

[16] Matching was done without replacement by the minimum sum of the squared distance measure. The characteristics used for matching were age, educational attainment level, gender, months of work experience, date of registration with the employment office as unemployed, and local labour office where registered as unemployed.

over age 18 who was lawfully able to work in the United States, whether employed or unemployed, and who was interested in starting or expanding a small business. GATE participation required three things: 1) registration either at a GATE kiosk at a One-Stop Career Center, at the GATE Web site, by mailing a postcard, or by calling a toll-free number; 2) attendance at an orientation session at a One-Stop Career Center; and 3) completion of an application package mailed to the evaluation contractor. GATE offered three services: 1) an individual assessment session; 2) training in a variety of relevant topics, including general business, legal and personnel issues, and business accounting computer software; and 3) individual meetings with business counselors about business plans and loan applications. Each participant could decide to receive any or all of the three services.

A total of 4,198 GATE applicants were randomly assigned to either the treatment or the control group. Control group members were not prevented from receiving other self-employment services available in the community. Therefore, this evaluation addresses the policy-relevant question: What is the effect of adding GATE to the array of self-employment services already offered in the community? Outcomes were measured through telephone follow-up surveys conducted at 6 and 18 months after random assignment. A total of 3,450 interviews were completed after 6 months (with an 82 percent response rate); of these, 3,039 were interviewed 18 months after enrollment. For the evaluation, the follow-up survey data were merged with administrative records on UI payments and quarterly wage records covering the 12 months before and after random assignment.

Project GATE generated a small but significant impact on business ownership in the 18-month follow-up. By the third quarter after random assignment, 43 percent of the program group reported owning a business, a statistically significant 6 percentage points more than the comparison group. The advantage for participants dwindled to 3 percentage points (statistically significant at the 10 percent level) at the 18-month follow-up. There were no significant effects on total employment (self-employment plus wage and salary employment), with rates at about 70 percent in the first quarter after random assignment and 85 percent 18 months after random assignment. However, compared to the control group, GATE participants were more likely to be self-employed and less likely to be employed in wage and salary jobs.

Control group members earned slightly more than participants from wage and salary jobs and about the same through self-employment, so that GATE participants earned somewhat less during the 18-month follow-up. The wage and salary difference was $1,800 based on the survey, but only $200 less based on UI quarterly wage records (not statistically significant). Both groups earned about $6,000 over the 18-month follow-up period. GATE increased receipt of UI benefits by about one week, or about $340 per person for all participants and $605 for those already receiving UI benefits when they applied to GATE. There were no program impacts on the receipt of public assistance or other income.

The GATE design in Minnesota was similar in many respects to Self-Employment Assistance (SEA) provided in some states through the UI program. While the Minnesota sample size was quite small (n = 459), GATE dramatically increased the probability of owning a

business during the 18-month period after random assignment. There was also a strong positive effect on total employment. During the second half of the follow-up period (Q4–Q6), recent UI claimants in Minnesota experienced strong and statistically significant employment gains relative to the control group (7 to 9 percentage points). Control group members earned more than participants from wage and salary jobs, with an average quarterly difference of $350 in the first three quarters but no difference in the next three quarters. There was no impact on self-employment earnings, but there was increased receipt of UI benefits.

In Minnesota, GATE increased UI receipt by three weeks over the follow-up period and increased the amount of UI benefits received by about $1,240 per person. GATE had no impacts on the receipt of public assistance or other income. Furthermore, GATE had no impacts on the likelihood of receiving public assistance, the amount of public assistance benefits received, household income, or the earnings of the entrepreneur's spouse. In total, the results for recent UI claimants in Minnesota indicate positive impacts on business formation and employment but negative effects on earnings and UI receipt.

Overall, the GATE study suggests the following: self-employment services can be effectively offered at One-Stop Career Centers; self-employment services are readily available even in the absence of GATE; increased business ownership might not lead to increased self-employment earnings in the short run; self-employment can lead to a loss of earnings from wage and salary jobs in the short run; self-employment programs have larger impacts on UI recipients; and 18 months is too short for a follow-up to determine the long-term effectiveness of GATE.

The Massachusetts UI Self-Employment Demonstration evaluated the effectiveness of SEA for unemployed workers who are likely to exhaust their UI benefits (Benus et al. 1995). The evaluation involved random assignment, with half the eligible UI claimants receiving self-employment services (the treatment group) and the other half receiving regular UI services (the control group). Enrollment in the experiment started in May 1990 and ended in May 1993. Only about 2 percent of targeted UI claimants met the initial demonstration requirements of attending an orientation and submitting an application. A total of 614 UI claimants were assigned treatment, and 608 assigned to the control group. Following the British model, the Massachusetts experiment provided weekly cash payments at the level of regular UI for the remainder of the entitled duration if self-employment efforts were continued. Follow-up telephone surveys were conducted at an average of 19 and 31 months after random assignment. Compared to the control group, the treatment group was more likely to have the following results: at least one self-employment experience during the observation period; about 1.5 months more in self-employment; no significant difference in earnings from self-employment; no significant difference in the likelihood of having a wage and salary job during the observation period; approximately one month more of having worked than controls in wage and salary employment; significantly more earnings than controls from wage and salary employment; positive impacts on the likelihood of employment (in either wage and salary or self-employment) during the follow-up period; positive impacts on the likelihood of employment (in either wage and salary or self-employment) at the time of the follow-up survey; positive impacts on total time employed (in either wage and salary or self-employment) during the observation period;

positive impacts on combined wage and salary and self-employment earnings; positive impacts on employment; reduced length of the first unemployment spell; reduced UI benefit receipt during the benefit year; increased likelihood of self-employment and accelerated entry into employment; and positive effects on total earnings, largely driven by large, positive impacts on wage and salary earnings, rather than by impacts on self-employment earnings.

Self-employment assistance in the Massachusetts experiment increased business start-ups among project participants, reduced the length of their unemployment, and increased their total time in employment—which includes self-employment plus wage and salary jobs. The experiment also had a substantial positive impact on participants' earnings. When placed into a benefit-cost framework, the self-employment assistance program model proved to be cost-effective for project participants and society as a whole. It proved cost-effective to the government sector as well. Overall, the self-employment assistance provided in the demonstration significantly increased participants' total time in employment (i.e., the combination of self-employment and wage and salary employment) after having been randomly assigned to the project. Including time spent both on self-employment and wage and salary employment, participants were employed 1.9 months longer than the control group. Total earnings from all sources for the comparison since random assignment averaged $10,056. Controlling for differences in observable characteristics, the self-employment treatment increased average earnings for participants by $5,940 over the thirty-one month follow-up period.[17]

The Washington UI Self-Employment Demonstration (SEED) involved random-assignment enrollment to treatment and control groups in Washington State between September 1989 and September 1990, with business services available to participants through March 1991 (Benus et al. 1995). A total of 755 new claimants were enrolled in SEED at the six sites and were offered demonstration services; 752 new claimants who applied to SEED were assigned to the control group. The SEED treatment followed the French-style lump sum, with the offer being the remainder of a UI beneficiary's entitlement at the start of self-employment efforts along with business start-up and development efforts. The first Washington telephone survey was conducted, on average, 21 months after random assignment.

Only about 4 percent of targeted Washington UI claimants met the initial eligibility requirements of attending an orientation and submitting an application. Compared to the control group, treatment subjects had the following results: they spent about 4.0 months more in self-employment; earned more than controls from self-employment during the follow-up period; had reduced likelihood of wage and salary employment; spent about one month less in wage and salary employment.; earned significantly less from wage and salary employment; had similar earnings from wage and salary and self-employment during the observation period; had higher rates of employment; reduced the length of the first unemployment spell; excluding the lump-

[17] Impacts were estimated in regression models controlling for observable characteristics: age, race, sex, educational attainment, on employer stand-by awaiting job recall, prior earnings quartile, prof/tech/mgr occupation, service industry, site location, quarter of UI benefit year begin (BYB) date, local unemployment rate at BYB (Benus 1995, p. B-214).

sum payment, had reduced UI benefit receipt during the first benefit year; and including the lump-sum payment, had higher total UI payments during the first benefit year.

3.4.3.4 Summary of Self-Employment Assistance

Self-employment accounts for about 15 percent of total employment in Europe and 10 percent in the United States. Some transitions to self-employment are permanent and result in secondary job creation through hiring within microenterprises. For others, self-employment is transitory and is followed by lower future employment rates and earnings in wage and salary jobs. In the United States, relative to continued wage employment, brief spells in self-employment do not increase average hourly earnings, reduce the chances of returning to employment in the wage sector, and increase the likelihood of future part-time work. However, these consequences are small compared to similar experiences in unemployment. Overall, men fared better following a spell of self-employment than did women.

Self-Employment Assistance (SEA) programs operate in most OECD countries. However, only about 2 percent of total active labour market budgets are spent on self-employment. In the United States, only seven states have active programs, and no state has had more than 1,000 participants in any year. Several countries conducted evaluations of SEA operated as an alternative to continued receipt of UI benefits. Some evaluations identified the program treatment by random assignment, while others were observational studies that used either or both, matching on characteristics and regression adjustment for observables. Nearly all evaluations found positive impacts of SEA. These included a higher rate of engagement in self-employment activity and higher earnings from self-employment. Many studies also found evidence that SEA was associated with higher total income after participation; this was often due to wage and salary income. This finding suggests that SEA had benefits for regular employment in the job market. The SEA intervention was not always associated with lower UI benefit payment costs, but targeting to those expected to be long-term unemployed improves this outcome. SEA tended to be most successful for its typical target population: older, experienced, more educated, and having higher levels of personal assets. Indeed, programs requiring personal contributions for partial costs of business start-up tended to be successful.

A main shortcoming of existing evaluations is the short duration after intervention when outcomes were measured. Just as for measuring small business start-up success, business survival, income, and hiring should be checked annually for at least five years. Proprietorships lasting at least five years have a high probability of achieving long-term success.

3.4.4 Job Creation Partnerships (JCPs)

Other countries have tried direct job creation efforts in various forms. Generally, the programs operated as public works or public service employment have income transfer as the main objective. Corollary aims may be to develop workplace behavior skills, arrest the deterioration of such skills, and contribute to social capital or public welfare. Program participants are commonly either long-term unemployed or at high risk of long-term

unemployment, these include youths and minorities potentially facing discrimination. A key question for measuring outcomes of direct job creation is whether the work experience can qualify one for unemployment compensation.

Public service employment (PSE) programs provide a fixed amount of funding to a government agency or not-for-profit organization to employ unemployed individuals, typically disadvantaged persons who have difficulty finding unsubsidized employment. These programs are also referred to as direct job creation programs. There are two primary differences between PSE programs and wage subsidy or supplement programs. The first is that PSE programs are typically for government or not-for-profit jobs, although there are exceptions, whereas wage subsidy programs target primarily private-sector jobs. The second is that PSE programs provide funding to support the entire employment cost of an individual, whereas wage subsidy programs provide funding for only a portion of it. It should also be mentioned that in many cases PSE-supported workers are paid minimum wage and usually do not receive benefits.

Some countries place a greater emphasis on PSE programs than others. For example, of the 27 OECD countries for which data are available for 2007, four countries—the Netherlands, Ireland, Belgium, and France—spent the highest percentage of GDP on direct job creation (OECD, 2009). Belgium emphasizes direct job creation much more than the others, spending 0.34 percent of GDP on PSE programs. In contrast, in 2007 the United States and Canada spent only 0.02 percent of GDP on these programs.

There are typically three types of PSE programs. The first is designed to provide employment during severe economic downturns or in severely economically distressed areas. The purpose in both cases is to reduce unemployment and provide workers with the means to support their families. Improving long-term earnings or employment prospects is not the primary goal. In its purest form, this category of PSE program is generally open to anyone having difficulty finding employment. However, rarely does this type of program have such an open-door eligibility policy; more typically, it is restricted to hard-to-employ populations. One example of a program that falls into this category is the Summer Youth Employment Program in the United States, which provides minimum-wage, subsidized jobs to economically disadvantaged youth throughout the country. The program began in 1964 as part of the War on Poverty initiative and continues today under the Workforce Investment Act. Another example is the German public employment program (PEP), which was instituted to help reduce the high unemployment rate in the former East Germany after reunification occurred in 1990. PEP participants were employed by government agencies as well as private-sector establishments in various industries.

A second type of PSE program is targeted to specific population subgroups who have great difficulty finding unsubsidized employment. Here the goals are to provide work experience and perhaps skills training, along with short-term aid and income, through subsidized employment. An example that combines this category with the previous one is the New Hope Project in Milwaukee, which guarantees work to anyone living in a very poor section of the city.

The third category includes mandatory work programs. These programs are usually associated with welfare programs, which offer cash benefits to employable recipients over a long period of time. In response to public concern that some able-bodied persons are living off the taxpayer's dollar, some states have required beneficiaries to participate in mandatory work programs while they receive benefits. The goal is to ensure that aid recipients meet their social obligation to earn their income, while still providing aid to those who need it. An example of such a program is New York City's Work Experience Program, which assigns welfare recipients to subsidized positions in sponsoring New York City agencies (Ellwood and Welty, 1999).

The advantage of PSE programs is that they are simple and direct. The limitation is that by their very nature, they do not necessarily increase the attractiveness of employing disadvantaged persons. Consequently, the likelihood of PSE participants transitioning into unsubsidized jobs after they leave PSE programs is minimal, unless the work experience and perhaps training (if offered) enhances their employability. Another disadvantage is the potential worker substitution, or displacement effect. Government agencies or not-for-profit organizations have an incentive to use the PSE funds to hire disadvantaged workers they may have hired anyway. PSE workers also have the potential to crowd out (displace) private-sector workers. While having no direct effect on private-sector employers, PSE programs may have an indirect effect by reducing the availability of disadvantaged workers for private-sector jobs and by increasing their wages as the pool of unemployed persons shrinks. In addition, if PSE programs are financed through taxes, higher tax rates may indirectly affect firm behavior. Another indirect effect of PSE is the low output and productivity of the workers filling these positions, since the primary purpose of PSE jobs is to employ those who are not likely to be employed in an unsubsidized job. In some cases, the PSE job positions are designed to do work that government agencies or not-for-profit organizations need done, and thus place value on that work. Ideally, the person filling the position can meet the qualifications of the job—maybe not immediately, but quickly enough to be of value to the organization. At the other extreme, the job is a make-work job, which has little value to the company but which gives the position holder some experience in a workplace setting and occupies his or her idle time.

3.4.4.1 Program design

PSE program design differs considerably across programs. Some PSE programs are confined to creating jobs in the government and not-for-profit sectors, while others include the private, for-profit sector (e.g., Germany's PEP and Belgium's Progression to Work program). Some programs include training and labour market support along with the subsidized employment (e.g., France's New Start program and the Netherlands' "Offer for All" strategy). PSE programs also differ in the population groups they target. Some programs, particularly those that are implemented in recessions or in economically distressed areas, include a broad range of participants. Others target narrow subpopulation groups, such as disadvantaged youth in the case of the United States' Youth Summer Employment Program. Another dimension of program design is the time in a worker's unemployment spell in which he or she enrolls in a PSE program. Typically programs target the longer-term unemployed. Evaluation results offer

insight into the design of PSE programs that improves the effectiveness of the programs and minimizes displacement effects.

3.4.4.2 Employment effects of PSE

Since the primary purpose of most PSE programs is to find employment for the unemployed, the outcome of interest for PSE programs is the employment of participants. Most evaluations consider two types of employment effects on participants: in-program and after-program. There is no doubt that a participant of a PSE program is employed during his or her period of participation. The issue is whether PSE programs contribute to employment of participants after they exit the program. There is also a macro or general equilibrium dimension to the employment effects of PSE programs, and that is the possibility of positive or negative interactions between program participants and other employed or unemployed workers. These were described earlier as displacement or substitution effects. However, the focus of most evaluations has been on estimating the short-term employment effects of PSE programs for the treated populations, disregarding the possibility of general equilibrium effects.

Employment effects on the treated. Results from more than two dozen evaluations of the PSE programs, mostly within European countries, suggest that participation in PSE programs reduces the likelihood of unsubsidized employment after exiting the program.[18] Of the 25 evaluations of PSE programs in 12 European countries, nearly four-fifths find negative or statistically insignificant results. Roughly half of these 25 studies used matching techniques to construct comparison groups. The other half used either duration models or OLS regression adjustment. None of the European studies used experimental design. However, the evaluation methodology did not appear to influence the results. For each method, about three-quarters of the evaluations yielded negative or statistically insignificant results. Kluve (2010), in conducting a meta-analysis of 139 evaluations of European active labour market programs, concludes that "evaluations of direct employment programs are significantly less likely to estimate significant positive impacts, and more likely to estimate significant negative results," relative to training programs (p. 913). A World Bank study came to a similar conclusion, particularly with respect to after-program effects, when considering PSE programs in developed countries: "The majority of evaluations show that participants are no better off, and may be worse off, in terms of finding employment after the program" (Betcherman, Olivas, and Dar 2004, p. 48). Their critique of 20 studies of programs in developed countries found that only 7 of the studies showed positive impacts on employment and none showed positive impacts on earnings.

German Reunification Work Support. Probably one of the more studied PSE programs is the attempt by the German government to reduce high unemployment in eastern Germany after reunification.[19] Disparities between the labour markets in the western and eastern

[18] One explanation for the negative postprogram effect is the stigma placed on workers for participating in a direct employment program. This is likely similar to the stigma effect associated with participating in wage subsidy programs.

[19] At least eight evaluations have been conducted to date, including Steiner and Kraus (1995), Hubler (1997), Bergemann et al. (2000), Kraus, Puhani, and Steiner (2000), Eichler and Lechner (2002), Cliendo, Hujer,

regions of Germany prompted the government to provide public service employment in eastern Germany through the reform of the Work Support Act. Potential participants in the public employment program (PEP) have to be long-term unemployed (more than one year) or unemployed for at least six months within the last twelve months and have to be eligible for unemployment compensation. Participants in PEP are not directly employed as a regular employee nor paid by the labour office. Rather they have an employment contract with a program-supporting employer, which can be a public institution, a private non-profit organization, or a firm. Normally, between 50 and 75 percent of the wages are covered, with 100 percent covered in special cases. Such special cases are common in eastern Germany, and because of the high or complete subsidies, the PEPs in this case look more like PSEs than wage subsidies.

Participation in a PEP is typically limited to 12 months, although both shorter and longer (up to 24 months and, under special conditions, up to 36 months) durations are observed. During their time in the program, participants are paid the going wage for that particular position, which in nearly all cases is higher than unemployment benefits. PEP also pays social security contributions, which could create new rights for unemployment benefits. An individual leaves a PEP when it ends, and it is possible, and desired, that the firm subsequently employs that person without a subsidy. Furthermore, an individual must leave a PEP immediately to take up a job offer in an unsubsidized job if one is proposed by the labour office.

The evaluations use a variety of methods, including discrete hazard rate models, matching, and hybrid approaches. Most studies look at the employment status of participants after they have left the program. The length of time the evaluations follow their employment status varies but can be as long as 36 months after the treatment.

Results vary across evaluations. Steiner and Kraus (1995) find that relative to nonparticipants, male PEP participants have a higher employment probability 12 months after PEP participation. Eichler and Lechner (2002) also find positive results: their findings suggest that individuals participating in PEP substantially reduce their risk of unemployment in both the short run and long run and for both men and women. For men, the majority of the reduced unemployment risk results from an increased employment probability; for women, it is because women drop out of the labour market. The other studies find negative or insignificant results. For example, Kraus, Puhani, and Steiner (2000), in a later paper than Steiner and Kraus's above, and using a different methodology, find a significant negative effect to PEP participation. Hubler (1997) concludes that PEP participation does not create the expected positive effects, although results differ according to the methodologies and specifications used. Hujer, in several papers coauthored with Caliendo and Thomsen, generally finds negative or statistically insignificant effects for PEPs.

U.S. National Supported Work Demonstration. Random assignment experiments involving PSE programs are very limited and are basically confined to programs in the United States. The three programs that were evaluated using this methodology—1) the Supported Work

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and Thomsen (2004), Hujer, Caliendo, and Thomsen (2004), and Hujer and Thomsen (2006).

Program, 2) the AFDC Homemaker Home Health Aide Program, and 3) the Community Work Experience Programs—were conducted in the 1970s and 1980s and are no longer in operation. The results of the evaluations suggest that PSE programs are generally more effective for adult women than for adult men and for youth.

To highlight the nature of supported work programs and their impacts, let us consider the National Supported Work Demonstration in more detail. The supported work program provided individuals who had severe employment problems with work experience of a year or so under close supervision in work groups. Four hard-to-employ groups were included in the national demonstration: 1) women on Aid to Families with Dependent Children (the U.S. welfare program, now called Temporary Assistance for Needy Families), 2) ex-addicts, 3) ex-offenders, and 4) young school dropouts, often with criminal records or histories of delinquency. The goal of the program was to provide work experience to these groups so that they would be better prepared to join the labour force and do productive, meaningful work, cease engaging in socially destructive or dependent behavior, and become self-supporting members of society. The program was a five-year demonstration operated by nonprofit organizations at 15 sites beginning in 1975. During the period of operation, 10,000 participants went through the program.

To evaluate whether this program accomplished these goals, a random assignment experiment was performed. A total of 6,616 individuals were included in the random assignment experiment: 3,214 were participants and 3,402 were controls. Each person in the sample received a baseline interview and up to four successive interviews at nine-month intervals. The results are reported by the four subgroups listed in the preceding paragraph:

AFDC Group. To be eligible for this program, a person had to be female, on AFDC for 30 of the last 36 months, and have no children under 6 years old. The average age of AFDC participants was 34, all but 5 percent were black or Hispanic, and fewer than one-third were high school graduates. The evaluation showed that in months 25–27 the experimental group's employment rate was 20 percent above that of the control group, hours worked by participants in the experimental group were 35 percent higher, and their earnings exceeded those of the controls by almost 50 percent. This differential persisted for wage rates even during the time the experimentals were employed in supported work programs. After month 12, when increasing numbers left the program, the difference between the wage rates of the experimentals and the controls began to rise.

Ex-Addict Group. Both the participants and the control group were selected from those participating in drug treatment programs. Almost all had used heroin, and the majority were young black or Hispanic males who had dropped out of high school. The evaluation found that during and after their time in the supported work program, the experimentals were substantially less involved in criminal activity than were members of the control group. Employment and earnings of the experimental group improved perceptibly over those of the control group after the 27th month. The reason that this improvement did not occur earlier may be related to the difficulty and, hence, delay that many ex-addicts experienced in finding employment after leaving the supported work program.

Ex-Offender Group. Eligibility for the ex-offender group required an individual to have been incarcerated as a result of a conviction within the past six months. Participants were predominantly young black or Hispanic males, nearly half of whom had not worked in a full-time job in the previous 24 months. The evaluation showed that the program had little effect on the participant group over the control group. Participants had a somewhat better employment and earnings record after 27 months, but the difference was not statistically significant. Unlike the ex-addicts, the ex-offenders who had participated in the program did not show any reduction in criminal behavior. Furthermore, ex-offenders dropped out of the program at a relatively high rate; their average length of stay was 5.2 months.

Youth Group. Eligibility was limited to young people between the ages of 17 and 20 years who had dropped out of school. Half of the enrollees had a record of delinquency or crime. For the youth group, supported work had no long-term impact on the earnings, employment, criminal activity, or drug abuse of the participant group over the control group. The evaluation results did suggest that the program was more beneficial for younger youth than for older youth. One reason for the lack of success was the difficulty in engaging youth in the program. Many youths found it difficult to decide what kind of work to do, and they seemed to quickly tire of their jobs and left the program.

Effects by subgroups from other studies. Several studies of PSEs have examined their effect on subgroups, mostly on youth and on men and women. Five of the 25 European studies looked at the effects of PSE programs that focus on youth. Only one, a program in France in the 1980s, showed positive effects. The other four programs yielded negative or statistically insignificant results. Several U.S. PSE programs focused on youth, but also with mixed results. Participants of the Summer Youth Employment Program were slightly more likely to work the following year, but no long-term gains were observed (Crane and Ellwood, 1984). Evaluations of the Youth Corps found no statistically significant postprogram earnings or employment impact. However, there was some evidence of positive effects for African American, Hispanic, and white female participants (Jastrzab, Masker, Blomquist and Orr, 1997).

Effects of PSE by gender are generally more favorable for women than for men. For all four evaluations of European programs, listed in Table 10, the estimated effects for men never exceed those for women. However, only one study, by Eichler and Lechner (2002), actually showed positive results for women, in that their unemployment rates declined after leaving the program. Yet in the long run this was due more to their leaving the workforce than to their increasing employment, and this result is hardly the program's intent. The other three studies found that the negative effects were not as strong for women as for men. Evaluations of U.S. programs in general show stronger results for women than for men, in terms of both youth and adults. For example, adult and young women in the CETA PSE program had higher earnings after leaving the program, but adult and young men had lower earnings. For the Support Work Program, AFDC recipients, who were nearly all women, experienced much higher postprogram earnings impacts than did ex-offenders and ex-addicts, who are mostly men, although obviously the latter two groups have additional employment barriers to overcome.

Table 10 Summary of Evaluation Results of European PSE Programs

Country	Evaluation study	Target group	Observation period	Evaluation method	Program effect	
					Overall	Subgroup
Denmark	Graversen (2004)		1994–1998	Duration	Negative	
Denmark	Rosholm (1999)		1983–1990	Duration	Insignificant	
Denmark	Rosholm & Svarer (2004)		1998–2002	Duration	Negative	
Finland	Natti et al. (2000)		1990–1995	OLS/selection	Negative	
France	Bonnal et al. (1997)	Youth	1986–1988	Duration	Insignificant	
France	Brodaty et al. (2002)	Youth	1995–1998	Matching	Negative	
France	Brodaty et al. (2002)	Youth	1986–1988	Matching	Positive	
Germany	Bergemann et al. (2000)		1990–1998	Matching	Negative	F > M
Germany	Bergemann (2005)		1990–1999	Matching	Positive	
Germany	Caliendo et al. (2004)		2000–2002	Matching	Negative	
Germany	Eichler & Lechner (2002)		1992–1997	Matching	Positive	F > M
Germany	Hujer, Caliendo, & Radi (2004)		1995–1999	Matching	Insignificant	F > M
Germany	Hujer, Caliendo & Thomsen (2004)		2000–2002	Matching	Negative	F > M
Germany	Steiner and Kraus (1995)		1990–1994	Matching	Positive	
Germany	Kraus, Puhani, & Steiner (2000)		1990–1994	Duration	Negative	
Germany	Hubler (1997)		1990–1994	Matching	Negative	
Ireland	O'Connell & McGinnity (1997)	Youth	Early '90s	OLS/selection	Insignificant	
Netherlands	Ridder (1986)		Early '80s	Duration	Positive	
Norway	Hardoy (2001)	Youth	1989–1993	OLS/selection	Insignificant	
Norway	Lorentzen & Dahl (2005)		1992–1999	Matching	Insignificant	
Norway	Zhang (2003)		1990–2000	Duration	Insignificant	
Poland	Kluve et al. (1999)		1992–1996	Matching	Negative	
Slovak Rep.	Van Ours (2001)		1993–1998	Duration	Insignificant	
Sweden	Frederiksson & Johansson (2003)		1993–1997	Matching	Negative	
Sweden	Sacklén (2002)		1991–1997	OLS/selection	Positive	
Switzerland	Gerfin & Lechner (2000)		1997–1999	Matching	Negative	
UK	Payne et al. (1996)		Early '90s	Matching, early	Insignificant	

NOTE: Most studies cited in Kluve (2010). The notation "F>M" denotes that treatment is more effective (or less detrimental) for females than for males.

3.4.4.3 Substitution and displacement effects

As previously mentioned, PSE participants may displace workers who are not in the program. The extent of displacement depends upon the design of the program, particularly the degree to which the program targets specific subgroups. The importance of the size of the displacement effect depends upon the intent of the program. If the program is intended to effect a countercyclical employment policy or one that targets workers in distressed areas for the purpose of reducing unemployment, such as the German PEP program, then the size of the displacement effect matters. A one-for-one displacement would negate any impact of the program of putting more people to work. If the program is viewed as enacting an antipoverty policy or one that targets hard-to-serve individuals, such as the National Supported Work program, then as long as the targeted group is employed through the program, the displacement

effect is of small consequence. However, after the individual leaves the latter type of program, the displacement effect becomes a larger concern. If the program helps a hard-to-serve person find employment after leaving the program, and the firm hiring the program leaver hires that person instead of another hard-to-serve person who never went through the program, the displacement reduces the long-run benefits of the program. On the other hand, if the firm hires a hard-to-serve "graduate" of the program instead of hiring a higher-skilled person who did not go through the program, then the intent of the program is served even though a more highly skilled person is displaced.

Two methods have been used to estimate substitution. One involves asking questions of program operators and supervisors and the other involves using econometric analysis. The more rigorous and accepted approach is the latter. Unfortunately, only a relatively few studies have examined substitution rates, far fewer than those that have evaluated participant outcomes. Johnson and Tomola (1977) was one of the first studies to examine substitution rates using an econometric approach. They examined possible displacement caused by the CETA PSE program from 1966 through 1975. They found that for every 100 PSE slots, 31 existing workers were displaced by the third quarter and 97 by the sixth quarter, which results in full displacement after 18 months. Borus and Hamermesh (1978) disputed these results, showing that they were unstable and sensitive to functional form. Yet the basic finding of high displacement remained. After CETA was revised in 1976 to narrow the eligibility rules to include only low income and longer-term unemployed, subsequent studies found somewhat lower substitution rates (Adams, Cook, and Maurice 1983). A field study, chaired by Richard Nathan, concluded that substitution rates were between 10 and 20 percent (Nathan et al. 1981). These estimates were based on conversations with providers and examinations of fiscal patterns, not on econometric analysis.

The other program that has received considerable scrutiny with respect to substitution effects is the Youth Incentive Entitlement Pilot Projects (YIEPP), which offered high school students a guaranteed part-time job during the school year and a full-time job during the summer. The demonstration project was administered in the late 1970s. Two approaches were used to estimate substitution rates: field research and econometric methods (Unicon 1982). Both yielded consistent results. The field research found substitution rates of 57 percent in the private sector, 28 percent in the nonprofit sector, and 21 percent in the public sector. The econometric analysis found substitution rates of 47 percent in the private sector, 45 percent in the nonprofit sector, and negligible displacement in the public sector.

A few studies outside the United States also estimate the displacement effects of PSE programs. The two highlighted here examine Swedish PSE programs from the 1960s through the 1980s, and both find displacement effects. Gramlich and Ysander (1981) focus on the two largest categories of public relief expenditures and employment: 1) health and welfare workers and 2) road construction workers. They find evidence of considerable displacement in road construction, but not in health and welfare. Forslund and Krueger (1994) investigate the displacement effects of public relief workers from 1976 to 1991 for construction workers, and from 1982 to 1990 for health and welfare workers. Results for construction workers show a negative and statistically significant effect: for every additional 100 public relief workers hired,

69 fewer private construction workers are employed. However, the results for health and welfare workers are much less clear.

3.4.4.4 Total displacement and private crowd-out effects

Most studies do not consider the other form of displacement, which is private crowd-out, particularly for programs that provide subsidized jobs in only government or not-for-profit sectors. One reason may be that PSE programs focus primarily on unemployed workers, who are predominantly economically disadvantaged and are unlikely to be in demand in the private sector. The private crowd-out effect is difficult to estimate. It requires estimates of the economic parameters of supply and demand, which in turn depend on the target group and the economic conditions. MDRC attempted to estimate the total displacement effect of the YIEPP program for youth. They compared the employment and unemployment rates of youth in the program with those for similar youth in comparison sites where the program was not instituted. Before the program began, youth employment rates were comparable in the treatment and control sites. MDRC found the total displacement rate averaged about 40 percent over the course of the program. The rates differed by site. MDRC decomposed the total displacement effect by sector and estimated a 55 percent displacement rate for the private sector and a 29 percent one for the public sector.

3.4.4.5 Value of output

For PSE programs that focus on creating jobs for individuals that firms are reluctant to hire, either because of poor economic conditions or the participant's lack of qualifications, the issue is whether the program is simply paying for make-work jobs, or does it add value in the products or services produced by the program participant? This issue is more important from a benefit-cost standpoint, but it has been a consideration in gaining support for implementing a PSE program and perhaps in providing relevant work, and thus work experience, for program participants. Estimating the value of work is difficult when an outside entity, a government program, is paying the wage bill. For public works programs, such as the Works Progress Administration (WPA) of the 1930s which used the funds to build roads and bridges, as well as the New York City Work Experience of the 1990s which used the funds to maintain city parks, the answer is fairly simple. Many miles of roads were built under the WPA, and tangible measures of cleaner and better-maintained parks were recorded as a result of the New York program (Ellwood and Welty 1999). Other attempts at estimating the value of output involved asking employers, assigning quantitative value to output produced, and using the retail value of the good or service produced, if it is actually sold. Estimates have also been based on comparing wages of regular employers with those of the program participants,. when it can be determined that they perform similar tasks. Studies that have used this last method, such as the evaluation of Youth Corps, came up with rather implausible results, as they found that the wages paid to youth through the program were almost double the wages paid by the private sector.

3.4.4.6 Summary

Public Service Employment programs provide subsidized employment to individuals who have difficulty finding work in unsubsidized jobs. Europe emphasizes this form of active labour market intervention more than the United States or Canada. Evaluations generally find that PSE programs are not effective in helping program participants find unsubsidized jobs after they leave the program. In addition, there are sizable displacement effects with many of the programs. However, the program appears to be more effective for adult women than for adult men and for youth. There are some features of the program that work better than others. In some studies, participants with less work experience tend to have greater postprogram benefits. Shortening the length of time participants are enrolled in the program appears to increase their likelihood of finding unsubsidized employment. PSE programs tend to get better results when they place participants with regular employers in jobs similar to unsubsidized ones. Evaluation results also indicate that displacement effects can be mitigated by narrowing the eligible group to those who are less likely to find regular employment, which may decrease the chances of those persons to find unsubsidized employment after completing the program.

3.4.5 Employment Assistance Services (EAS)

In Canada, Employment Assistance Services (EAS) are normally used by job-ready clients who need information about labour markets, help with job search, and access to tools such as resume writing software and the Internet. These and other services are commonly offered through public employment services in most developed countries. Access to such services is normally unrestricted for any citizen legally entitled to work in a country. In particular, access is not limited to eligible unemployment insurance (UI) beneficiaries. Nonetheless, some evaluations of employment services (ES) do measure effectiveness in conserving UI funds. This section provides a survey of results from studies evaluating the effectiveness of widely accessible public employment services. The magnitudes of the net impact are typically small, however—but then so too are the costs, making many of these interventions socially beneficial.

Our survey of public employment services divides evaluation studies into the following ten topic groups: 1) Job Interview Referrals and Job Placements, 2) Counseling, 3) Job Search Assistance, 4) UI Work Test, 5) Mandatory Services, 6) Sanctions, 7) Targeted Job Search Assistance, 8) Reemployment Incentives–Bonuses, 9) Reemployment Services to UI Beneficiaries, and 10) Youth Employment Services. A summary of selected research evidence on these topics is presented in Table 11.

Table 11. Summary of Evidence on the Effectiveness of Employment Services

Service	Evaluation method	Impact estimates	Study location	Authors (year)
Employment Service (ES) job interview referrals	NE: Controlled for observable characteristics in regression models.	Increased earnings	Sites in 27 U.S. states	Johnson et al. (1983, 1985)
ES job search assistance (JSA)	NE: Controlled for observable characteristics in regression models.	Acts as a backstop against long term U	Pennsylvania	Katz (1991)
ES referrals	NE: Controlled for observable characteristics in regression models.	−2.1 weeks UI	Washington	Jacobson & Petta (2000)
ES referrals	NE: Controlled for observable characteristics in regression models.	−1.1 weeks UI	Oregon	Jacobson & Petta (2000)
Job search counseling	RA: Among ES customers judged in need of counseling, some were randomly assigned to it	No impacts	Florida, Minnesota, Utah	Benus et al. (1977)
Job search counseling	NE: Controlled for observable characteristics in regression models.	No impacts	Sites in 27 U.S. states	Johnson et al. (1981)
Job search counseling	NE: Controlled for observable characteristics in regression models of unemployment duration.	7-percentage-point reduction in 1-year return to unemployment	France	Crepon et al. (2005)
Job search counseling	RA: ES registrants referred or not, with tests for homogeneity. Impacts in duration models with control variables.	Increased search intensity and rate of job finding	Netherlands	Gorter and Kalb (1996)
Job search assistance	RA: ES registrants referred or not, with tests for homogeneity. Impacts in duration models with control variables.	No impacts on exit to employment, no differences between subgroups	Netherlands (only Amsterdam and Rotterdam)	Van den Berg and van der Klaauw (2006)
Stronger work test	RA: Regressions with controls.	−0.55 weeks UI	Charleston, SC	Corson et al.(1985)
Stronger work test plus placement	RA: Regressions with control variables for observables.	−0.61 weeks UI	Charleston, SC	Corson et al.(1985)
Stronger work test plus placement and JSW	RA: Regressions with control variables for observables.	−0.76 weeks UI	Charleston, SC	Corson et al.(1985)
Report 4 employer contacts	RA: Regressions with control variables for observables.	−0.70 weeks UI	Maryland	Klepinger et al. (1998, 2002)
Make 2 employer contacts but no reporting	RA: Regressions with control variables for observables.	0.40 weeks UI	Maryland	Klepinger et al. (1998, 2002)
Make 2 employer contacts plus JSW	RA: Regressions with control variables for observables.	−0.60 weeks UI	Maryland	Klepinger et al. (1998, 2002)
Make 2 employer contacts, both verified	RA: Regressions with control variables for observables.	−0.90 weeks UI	Maryland	Klepinger et al. (1998, 2002)

Table 11. Summary of Evidence on the Effectiveness of Employment Services

Service	Evaluation method	Impact estimates	Study location	Authors (year)
Remove the work test	RA: Regressions with control variables for observables.	3.30 weeks UI	Tacoma, WA	Johnson & Klepinger (1991, 1994)
Remove the work test	Natural experiment, temporary closure of employment offices, suspended reporting requirement.	5.28-week increase in duration of JSA	Northern Ireland—Job seekers allowance (JSA) replaced "the dole"	McVicar (2010)
Delay job search assistance call-in	RA: Regressions with control variables for observables.	Earlier call-in had slightly shorter unemployment spells, and higher earnings after five years.	U.K. national sample in the new "restart program"	Dolton & O'Neill (1996, 2002)
Mandatory services	NE: Matching on observable characteristics in similar labour market areas.	5.0-percentage-point increase in job finding	U.K. new deal for young people	Blundell et al. (2004)
Sanctions	NE: Selection bias correction for endogenous sanction on exit rate from employment	58% male, 67% female increase in job finding	Netherlands response to 5–30% reduction in UI benefits	Abbring et al. (2005)
Job search assistance (JSA)	RA: Regressions controlling for individual characteristics local offices, timing of enrollment.	−0.47 weeks UI	New Jersey	Corson et al. (1989)
JSA plus training	RA: Regressions controlling for observables, offices, timing.	−0.48 weeks UI	New Jersey	Corson et al. (1989)
JSA plus reemployment bonus	RA: Regressions controlling for observables, offices, timing.	−0.97 weeks UI	New Jersey	Corson et al. (1989)
Structured job search	RA: Regressions controlling for observables, offices, timing.	−1.13 weeks UI	D.C.	Decker et al. (2000)
Individual job search	RA: Regressions controlling for observables, offices, timing.	−0.47 weeks UI	D.C.	Decker et al. (2000)
Individual job search plus training	RA: Regressions controlling for observables, offices, timing.	−0.61 weeks UI	D.C.	Decker et al. (2000)
Structured job search	RA: Regressions controlling for observables, offices, timing.	−0.41 weeks UI	Florida	Decker et al. (2000)
Individual job search	RA: Regressions controlling for observables, offices, timing.	−0.59 weeks UI	Florida	Decker et al. (2000)
Individual job search plus training	RA: Regressions controlling for observables, offices, timing.	−0.52 weeks UI	Florida	Decker et al. (2000)
Connecticut WPRS	NE: Regressions controlling for observable characteristics.	−0.25 weeks UI	Connecticut	Dickinson et al. (1999)
Illinois WPRS	NE: Regressions controlling for observable characteristics.	−0.41 weeks UI	Illinois	Dickinson et al. (1999)
Kentucky WPRS	NE: Regressions controlling for observable characteristics.	−0.21 weeks UI	Kentucky	Dickinson et al. (1999)
Kentucky WPRS	RA: With matching on observables.	−2.20 weeks UI	Kentucky	Black et al. (2003)

Table 11. Summary of Evidence on the Effectiveness of Employment Services

Service	Evaluation method	Impact estimates	Study location	Authors (year)
New Jersey WPRS	NE: Regressions controlling for observable characteristics.	−0.29 weeks UI	New Jersey	Dickinson et al. (1999)
Maine WPRS	NE: Regressions controlling for observable characteristics.	−0.98 weeks UI	Maine	Dickinson et al. (1999)
Illinois UI bonus	RA: Regressions controlling for observable characteristics.	−1.15 weeks UI	Illinois	Woodbury & Spiegelman (1987)
New Jersey UI bonus	RA: Regressions controlling for observable characteristics.	−0.50 weeks UI	New Jersey	Corson et al. (1989)
Pennsylvania UI bonus	RA: Regressions controlling for observable characteristics.	−0.69 weeks UI	Pennsylvania	Corson et al. (1992)
Washington UI bonus	RA: Regressions controlling for observable characteristics.	−0.47 weeks UI	Washington	Spiegelman et al. (1992)
Targeted UI bonus	RA: Regressions controlling for observables, office, timing.	−0.75 weeks UI	Pennsylvania and Washington	O'Leary et al. (2005)
Reemployment and eligibility assessments (REA)	RA: Matching and regressions controlling for observable characteristics.	−1.2 weeks UI	Minnesota	Benus et al. (2008)
UI and ES in one-stop centers	NE: Matching on observables and regression adjustment using other relevant variables.	−1.8 weeks UI	Wisconsin	Almandsmith et al. (2006)
Active labour market programs for younger and older unemployed	NE: A natural experiment plus regression adjustment on observables (after difference over time of dependent variable).	Smaller effects for youth, older, low education	Portugal	Centeno et al. (2009)

NOTE: RA= random assignment experiment, NE= nonexperimental , JSW means job search workshop. WPRS means Worker Profiling and Reemployment Services.

Job Interview Referrals and Job Placements. In the early 1980s, the U.S. Department of Labor sponsored a nationwide evaluation of the effectiveness of the Employment Service: Johnson et al. (1983, 1985) evaluatedES referrals to job interviews. The authors conducted interviews with 8,000 new applicants in 30 ES offices in 27 states between July 1980 and May 1981, and did in-home follow-up interviews six to nine months later. Their strategy was to compare those referred to an ES job interview with other ES registrants. They focused on the following five outcomes: 1) time from ES application to first job, 2) earnings during the six months after ES contact, 3) employment at the time of follow-up interview, 4) usual work hours per week on the new job, and 5) wage rates.

Johnson and his colleagues recognized that receipt of an ES job interview referral is likely a consequence of both individual and ES agency actions. They suggested that individual motivation may distinguish who seeks out a service, and efforts by agency staff to achieve high reemployment rates may amount to cream-skimming. Both these actions may cause the participant samples to differ from nonparticipant samples. They asked baseline survey questions

to permit construction of three indices: work ethic, lack of confidence, and efficacy in carrying out plans. They used local office factors to create controls for staff referral practices.

Preliminary analysis comparing recipients of an ES job interview referral with nonrecipients suggested differences among men as to prior earnings patterns, but no significant differences in prior earnings among women. Those referred by the ES to job interviews also scored higher on the baseline test of individual motivation, while those not referred were slightly more advantaged in terms of having higher pre-ES earnings. Impact estimates were computed in regression models controlling for the influence of demographics, financial resources, work history, motivation, local office characteristics, and local labour market conditions.[20]

Large positive earnings gains were estimated for unemployed women, including all subgroups of women. Among women an ES job referral was estimated to speed return to work by 2.8 weeks, with an 11.1-percentage-point increase in the probability of employment on the survey date six months after ES referral. Women with a referral were 7.4 percentage points less likely to have dropped out of the labour force, and their earnings in six months were $325 more than the $1,400 mean for the nonreferred. Among men, an ES job interview referral was estimated to speed return to employment by three days, but the estimate was not statistically significant. Men over age 45 and men in urban areas also had positive but statistically insignificant effects. There were no estimated impacts for either sex on reemployment wage rates. About the effects for women, the authors speculate that "part of the reason may be that women have less labor market experience and less access to the traditional network of job finding methods and that an ES referral constitutes more of a service for women" (Johnson et al. 1985, p. 136).

The National Commission for Employment Policy sponsored research that exploited an uncommon feature of UI to estimate the effectiveness of ES for dislocated workers in Pennsylvania (Katz 1991). The study used data on UI recipients in Pennsylvania during the period from 1979 to 1987. Unlike in most states, in those years Pennsylvania claimants were not required to register for job search with the ES. Program effects were estimated by comparing labour market outcomes of ES users against those of nonusers of ES, so that program effects were subject to selection biases. This study examined job search assistance (resume assistance, job search workshops, job finding clubs, labour market information, and job search planning) and job placements. Net impact estimates were computed in regression models controlling for differences in observed characteristics.[21] Job search assistance was most effective right after the start of a spell of joblessness. Both job placements and referrals were found to be most effective two or three calendar quarters after commencement of joblessness. The key insight gained from this study has to do with the response to interventions at differing times in the jobless spell. The

[20] They also tried Heckman selection bias correction procedures, but lacked instruments to predict receipt of an ES service that were not also correlated with outcomes.

[21] **Error! Main Document Only.**In addition to the indicator for ES service receipt, regression models included control variables for age, sex, quarters of prior work experience, prior work in manufacturing, SMSA unemployment rate, weekly UI benefit amount, exhausted UI benefits, number of weeks of UI benefits, and used ES before joblessness.

pattern that emerged led the study's author to describe the ES as a "backstop," or a job finding path followed when others have yielded no appealing prospects: "The effectiveness of the ES appeared to be much less a function of the characteristics of individual workers than the overall length of their joblessness." (Katz 1991, p. 21.)

Jacobson and Petta (2000) did an evaluation in Washington and Oregon that found employment service job placements most effective for those with a strong record of job attachment, affording evidence that job search assistance would be an appropriate intervention for dislocated workers. The data for Washington consisted of survey data on 587 job seekers who used the public labour exchange in the first half of 1998, plus administrative data on 328,815 jobless spells that occurred between 1987 and the middle of 1995. The Oregon data were based on administrative records for 138,280 jobless spells in 1995. An analysis of job placements using the Washington survey data suggested differences in impacts across ES customers depending on their recent pattern of job attachment. For job seekers characterized as having a spotty work record, the impact of a job placement was estimated to be large, while the impact estimate was even larger for those with a strong work record. The analysis of administrative data was done in regression models controlling for observable variables including demographic characteristics, work history, history of recent UI claims duration and exhaustion, and local labour market conditions. The impact of job referrals based on the Washington administrative data was estimated to be −2.1 weeks, and the estimate based on the Oregon data was −1.1 weeks. Concern about the degree to which the Washington and Oregon ES evaluations were externally valid regarding displacement led to a related study. Davidson and Woodbury (1993, 2000) used a computerized simulation model of the labour market called a general equilibrium search and matching model. They calibrated the model with labour market data from Washington state and with impact estimates of Washington public labour exchange (PLX) services (Jacobson and Petta 2000). "The crowding-out effects of PLX referral and placement activities are small both absolutely and relative to the increases in employment that result from PLX activities . . . the benefits generated by PLX referral and placement activities outweigh the costs. The benefits, again, are twofold: shorter unemployment spells for PLX users and general improvements in the labour market that result from PLX activities" (Davidson and Woodbury 2000, pp. 19–20).

Counseling. Benus et al. (1977) conducted the first scientific evaluation of employment counseling in the United States as a small field experiment in which individuals judged to be in need of counseling were randomly assigned to receive it or not; both groups received normal placement services, and the treatment group received additional counseling. Study sites (and sample sizes) were Minneapolis (215), Salt Lake City (278), and West Palm Beach (204). The study authors concluded that counseling had no significant impact on duration of unemployment, employment earnings, job search effectiveness, job satisfaction, or occupational prestige.

The results from Benus et al. (1977) are consistent with evidence from a national survey of the counseling program conducted by Johnson et al. (1981). Terry Johnson and colleagues surveyed ES staff in 30 offices around the country in 27 different states. Counseling supervisors reported that counselors were primarily evaluated on the quality of the counseling records kept,

the size of their caseloads, and their communication and relationship skills. Many fewer supervisors indicated that counselors were evaluated on ES services provided to job seekers or the results of those services. Johnson et al. (1983) reported impact estimates from their national evaluation of several employment services in the United States. Impact estimates were computed in regression models controlling for the influence of demographic, financial, work history, motivation, local office characteristics, and local labour market conditions.[22] They concluded that employment counseling produced no significant impact on duration of unemployment, earnings, or job satisfaction.

Crépon, Dejemeppe, and Gurgand (2005) evaluated the effects of intensive counseling schemes provided to about 20 percent of the unemployed in France since the 2001 unemployment policy reform (PARE). Several of the counseling schemes amount to a kind of qualitative profiling, intended to improve the quality of assignment of workers to jobs. The authors use a nationally representative one-in-twelve sample of newly unemployed persons for all new spells of UI receipt between July 2001 and September 2003. They only analyze the first observed spell so as to avoid correlation of unobservable variables over consecutive spells. Data end in June 2004, and unemployment spells are arbitrarily truncated at 900 days. In estimating duration models they control for a large number of individual characteristics and unemployment history traced back to 1993. In particular they control for gender, nationality, children, marital status, educational level, age, region of residence, reason of entry into unemployment, unemployment history (cumulative unemployment duration since July 1993 and since July 1999), unemployment recurrence (number of spells since July 1999), welfare transfer (RMI), and type of unemployment benefit eligibility. They find significant favorable effects of counseling on both unemployment duration and cycling back to unemployment. In particular, the program reduces the likelihood of new unemployment from 33 to 26 percent. This suggests that screening customers by subjective factors, using professional employment counselors, can reduce cycling back to unemployment.

Counseling-and-monitoring is a service offered in the Netherlands that consists of monthly meetings with an employee of the local UI agency for a period of six months, starting immediately after inflow into UI. During these meetings, recent job search activities are evaluated and a plan for the next period's job search activities is made. A distinguishing feature of this program is the random assignment nature of its operational design. Unemployed job seekers (who are entitled to receive unemployment benefits) are randomly divided into two groups (treatment and control). Moreover, the program is implemented as part of the regular contact that unemployed people receiving benefits are obliged to maintain with the Joint Administration Office (JAO). At these meetings, the control group is dealt with in the "traditional" way: progress in finding a job is discussed (for example, the number of applications made and whether invitations are obtained for job interviews), and occasionally the information an unemployed person provides is checked. The basic approach of having regular meetings and the exchange of information is exactly the same for the treatment group. The difference is, however, in spending more time with each unemployed person in the treatment group than is

[22] They also tried Heckman selection bias correction procedures, but lacked instruments to predict receipt of an ES service that were not also correlated with outcomes.

common in the “traditional” approach. The following two paragraphs summarize two separate evaluations that found differing results for the Dutch program’s counseling and monitoring.

Gorter and Kalb (1996) estimated the effects of counseling and monitoring in the Netherlands on unemployment using random assignment, in a sample drawn from the registered inflow to unemployment from November 1989 through January 1990 at seven regional administrative offices (located in Haarlem, Maastricht, Arnhem, Vlaardingen, Apeldoorn, Venlo, and Rijswijk). After necessary restrictions, the usable analytic sample included 722 observations. The evaluation focused on outcomes for the job finding rate, application intensity, and the matching probability. In the authors’ job search model, the job finding rate is equal to the product of the application intensity (frequency of applications in a period) and the matching probability. The assumption is that ES counseling and monitoring boost the application intensity, thereby increasing the likelihood of finding a job. A sample of the inflow into unemployment was randomly assigned to treatment and control groups. The search activities and labour market events of the sample were recorded through interviews every four weeks for a total period of about one year. For analysis Gorter and Kalb retained only those observations that provided full information on the "search history" of the individual—that is, 743 of 1,631. Furthermore, they restricted the period between becoming unemployed and being assigned to CM to less than 60 days. This reduced the sample to 722 cases. The homogeneity of participant and comparison groups was confirmed by simple tests which show that the sample means and standard errors of the "labour market history" variables for those receiving and those not receiving CM are not significantly different. Impacts were estimated in duration models controlling for observable variables such as age, educational attainment, sex, prior job—permanent or temporary, years of work experience, years unemployed, days unemployed before registration as unemployed, occupation, and indicator variables for local area of residence. The empirical results suggest that both counseling and monitoring reduce the time required to find a job because of increased job search intensity—i.e., people given these services make more job applications than the comparison group.

Van den Berg and van der Klaauw (2006) also examined the Netherlands program for counseling and monitoring of unemployed workers. They theoretically analyze these policies in a job search model with two search channels and endogenous search effort. Their empirical analysis uses administrative data generated by random assignment plus follow-up survey data on new UI registrations in Amsterdam and Rotterdam between August 24 and December 2, 1998, at two local branches of the public employment service. The experiment ended on February 8, 1999. The randomization process in the experiment prevented crossover between treatment and control groups in assignment or afterwards. The participants in the experiment were not informed in advance about the fact that the experiment was going on. None of the individuals in either group complained about their status. Since treatment assignment was compulsory, there was no noncompliance, and therefore no self-selection into or out of the assigned treatment or control group. The authors estimated average treatment effects across the population of UI entrants in duration models of exit from unemployment to work. In estimation they controlled for observable characteristics including age, sex, prior UI receipt, UI benefit amount, marital status, and city of residence. They find no evidence that counseling and monitoring affect the exit rate

from unemployment to work. They find instead that monitoring causes a shift from informal to formal job search. They assert that low-intensity job search assistance programs, such as the counseling component of the Dutch counseling and monitoring program, have small effects at best. They suggest that high-intensity job search assistance programs may have a more positive effect on the exit rate to work, and that monitoring of relatively well-qualified individuals in favorable macroeconomic conditions leads to inefficient substitution of search methods or channels. Individuals with worse prospects may have less scope for substitution, and monitoring of their search activity may lead to an increase in the exit rate to work. The null hypothesis of a zero treatment effect is never rejected, not even for specific subgroups or specific time intervals. However, the results suggest that transition rates to employment were somewhat higher for younger individuals and those who collected UI benefits before, those who did not vary at all by gender or marital status, and those who differed slightly across the two cities; and that transition rates were lower for those who had lost part-time jobs.

Comparing the random assignment evaluations and results from the similar studies in the Netherlands, the differences can be partly explained by the fact that the intervention examined by Gorter and Kalb (1996) was more intensive, and labour market conditions were generally more favorable, than was the case for the Van den Berg and van der Klaauw (2006) study. In particular, the Gorter and Kalb treatment may be considered an add-on to the Van den Berg and van der Klaauw treatment, hence the stronger positive impacts found by Gorter and Kalb.

UI Work Test. The U.S. program for UI has a strong focus on reemployment. The UI work test is a critical program feature for promoting reemployment. The work test normally requires both registering with the public employment service and contacting potential employers on a weekly basis. Once initially eligible, claimants must be able to work, available for work, and actively seeking work in order to continue collecting weekly UI benefits. Nearly all states waive the work search requirement for workers on temporary layoff with a definite recall date in the near future. Workers who find their jobs through union hiring halls also are commonly excluded from the work search requirement. These workers are not expected to search for work independently, as long as they are registered with the placement service of their union hiring hall. Finally, workers are excluded from the work search requirement for those weeks during which they are enrolled in training approved by the state UI agency.

A work test field experiment in Charleston, South Carolina, involved random assignment of 5,675 new initial UI claimants to three treatment groups and a control group between February and December 1983 (Corson, Long, and Nicholson 1985). The experiment evaluated three treatments representing successively larger bundles of services. Claimants assigned to the control group were given the customary work test, which involved informing claimants that ES registration was required. However, there was no systematic monitoring of this requirement. The three treatments were 1) a work test requiring that an ES registration notice be sent after the first UI benefit check was paid, with payment of the second check suspended for failure to register with the ES; 2) a work test plus enhanced placement services, a personal placement interview within one week of the first UI check, a job referral or an outreach attempt to contact a prospective employer (job development), training in using the job vacancy listings, and special

additional services again once the claimant drew nine weeks of benefits; and 3) enhanced placement services, plus job search workshops: a three-hour JSW, and after four weeks of UI benefits a JSW on labour market information. Treatment impacts on earnings and weeks of UI benefits were computed in regression models controlling for observable characteristics: age, race, sex, educational attainment, potential duration of UI benefits, UI wage replacement ratio, and base period earnings. The first treatment had the largest impact. It alone shortened the duration of compensated joblessness by more than half a week. The additional services did not lead to significantly larger impacts. Impacts of the treatments were concentrated among men who averaged impacts of greater than 1.0 weeks for all treatments, and among workers in the construction industry, who had impacts of more than 4.0 weeks.

A field experiment on the work test conducted enrollment between July 1986 and August 1987 in Tacoma, Washington, job service centers. It involved a total of 6,763 UI claimants assigned to one of three treatments; 2,871 claimants were assigned to the control group (Johnson and Klepinger 1994). The control group faced the standard work search rule requiring three employer contacts per week, plus an eligibility review interview (ERI) 13 to 15 weeks after the initial claim was filed. The three treatments were as follows: 1) exception reporting—a complete relaxation of the work test, whereby claimants were not required to file the standard biweekly continued UI claim form and were told that UI payments would continue until the claimant reported a change in circumstances such as return to work or an increased level of earnings; (2) new work search policy—individualized work search requirements including a group ERI followed by an intensive one-on-one follow-up interview; and 3) intensive services—individualized work search requirements (Treatment 2), plus a two-day JSW after four weeks (two days of classroom instruction plus 10 hours of phone canvassing), plus a group ERI after 12 weeks with a focus on employability development, plus individual follow-up. Treatment impacts on UI receipt and reemployment were estimated using regression and probit models including the following variables for individual and regional characteristics: a set of age dummies, male dummy, race/ethnicity dummies, a set of education dummies, veteran status dummy, earnings and hours worked in each of the three years before filing for benefits, a set of occupation and industry dummies for the person's most recent job, WBA, maximum number of weeks of UI benefits payable, a set of dummies for type of UI claim, union member dummy, a temporary layoff dummy, and the unemployment rate in the county three months after the person filed the claim. That last variable aimed to approximate labour market conditions at about the mean duration of the UI spell. Impacts on reemployment earnings were estimated in regression models controlling for the above listed variables, plus each model included an additional Heckman variable to correct for selection bias due to estimation on samples of only those gaining reemployment. Suspension of enrollment into the first treatment was done earlier than planned because the larger-than-expected response could easily be detected with a sample much smaller than designed. Claimants relieved of the work test and the continued claim filing increased their period of UI benefits drawn by a statistically significant 3.3 weeks. This impact was bigger for women with children and men without children, and for married women and unmarried men. Treatment 2 had an effect on UI benefit receipt of +0.17 weeks and was statistically indistinguishable from the existing standard work search rule applied uniformly to all claimants. Treatment number 3, which was customized and featured a JSW after four weeks and an ERI

after 12 weeks, had a statistically significant impact of 0.47 weeks. Impacts were bigger for women without children and unmarried women. An analysis of the timing of the components of this treatment and claimant response (at 4 and 12 weeks), combined with an analysis of the timing of the standard treatment given the control group (at 13 to 15 weeks) and the response to that, provided new insight into claimant behavior. In both cases exit from UI benefit receipt appeared to be more likely right before a scheduled intervention, rather than after the service was provided. Such a response might be termed an "invitation effect." This led to the conclusion that the timed elements of the work test—JSW and ERI—acted more like a stick, prodding return to work, than a carrot, providing nourishment for achieving that end.

Enrollment into the Maryland UI work search experiment was conducted in six public labour exchange offices around the state throughout the entire calendar year of 1994 (Klepinger et al. 1998, 2002). A combined sample of 23,758 new monetarily eligible UI claimants were enrolled in the experiment. The standard work search policy was given to the control group. This required two job search contacts per week, which had to be reported on the biweekly UI continued claim form but were not verified. The four alternative treatments tested were 1) report four weekly employer contacts, which are not verified; 2) contact two employers per week, with no need to report the two contacted; 3) report two weekly employer contacts, which are not verified, plus attend a four-day JSW early in the unemployment spell; and 4) report two weekly employer contacts, which will be verified. Impact estimates were computed in regression and logit models controlling for age, sex, race/ethnicity, employment by a federal agency or the military, U.S. citizenship, earnings in each of the four quarters preceding the quarter a claimant applied for benefits, local employment office indicators, entry quarter, and maximum weekly benefit amount. Estimates were computed on the full sample of all monetarily eligible new claimants enrolled in the demonstration. The estimates can be interpreted as average effects over all eligible claimants, regardless of whether they actually adhered to the work search requirements in the experimental design. Requiring four job search employer contacts per week reduced the average duration of UI benefit receipt by 0.7 weeks. This reduction in duration resulted even when the employer contacts were not verified. Telling beneficiaries to make two employer contacts per week, but removing the requirement to report the two contacts, resulted in a statistically significant increase in average UI benefit durations of 0.4 weeks. Telling beneficiaries to make two employer contacts per week, plus requiring attendance at a four-day JSW early in the unemployment spell, reduced the average duration of UI receipt by 0.6 weeks. Requiring two employer contacts to be reported, plus telling claimants that their two contacts would be verified, shortened the average duration of UI benefit receipt by 0.9 weeks. A verification rate of 10 percent appeared to be an adequate level of threat to generate a statistically significant response. Notably, the bulk of the response to the fourth treatment occurred during the first spell of joblessness in the UI benefit year. Similarly, the first treatment generated the bulk of its response during the first spell of joblessness in the benefit year. The effects of treatments 1, 3, and 4 were not associated with lower reemployment earnings. However, eliminating the work search reporting requirement, as in Treatment 2, raised reemployment earnings by a statistically significant 4 percent. The estimated effects of the treatments are not significantly different among specific demographic subgroups. In particular, there were no

differences across subgroups in the effects of the treatments on total UI benefits paid and the length of the first spell of UI by local office, race, age, sex, or prior earnings.

In the United Kingdom (UK), UI is administered by that country's public employment service and has a uniform initial duration of entitlement of 12 months. In 1987, a new program called Restart was introduced nationally. Under Restart, UI beneficiaries nearing six continuous months of receiving benefits were called in for an appointment at their local public employment service office and were provided with intensive job search assistance (Dolton and O'Neill 2002). An evaluation of the UK Restart program estimated short-term effects similar to those observed in the Tacoma alternative work search experiment (Dolton and O'Neill 1996). Both evaluations suggested that there was a modest shortening in the duration of compensated unemployment and that the invitation for intensive job search assistance acted more as a prod than as a support for reemployment. In a subsequent random-assignment field experiment, the treatment group received the standard UK Restart services when it was nearing six continuous months of claiming UI, while the randomly selected control group was given the same services when it was approaching 12 continuous months of receiving benefits. The researchers found evidence that, over the short term, required job search assistance prodded both groups of UI beneficiaries to go back to work but that, over a longer, five-year term, the group that received such support earlier in its jobless spell had measurably higher earnings.[23] This finding suggests that job search assistance can be a valuable service for job seekers. Long-term impacts by gender were large and positive for males but were not measurable for females.

Prior to 1996, unemployment benefits in the UK were often called "the dole" because of the lack of a work search requirement and the low wage replacement rate. The benefit was renamed Jobseekers Allowance (JSA) because of the strong requirements for active work search. One requirement for continued benefits under the JSA is for the claimant to report in person every two weeks to a public benefits office to validate that he or she is making an active search for work. A problem with this strategy was that public jobs offices were in a separate location from benefits offices. Starting with a pilot program in 1999, Jobs and Benefits Offices (JBO) were created to strengthen the link between receipt of JSA and job search in Northern Ireland. These new institutions would strengthen the fortnightly monitoring interview, providing additional time and new elements of job search assistance, with advisers now able to suggest—and submit electronic applications to—suitable registered vacancies during the interview. Northern Ireland is divided into 35 administrative areas. Jobs and Benefits Offices (JBO) were established in 25 of these 35 areas by the end of 2008. Refurbishment of each office took between 6 and 18 months, during which time the reporting requirement was suspended for continued JSA benefit receipt. T-tests on the sample means suggest no significant difference between the treatment and control areas in terms of JSA stocks per capita, outflow and inflow rates, and redundancies and vacancies. In other words, treatments have not been differentially

[23] Short-term estimates (Dolton and O'Neill 1996) were computed in competing risks models controlling for the observable characteristics age, sex, race, educational attainment, marital status, children, young children, driver's license, home ownership, urban area, working spouse, and local unemployment rate. The long term estimates (Dolton and O'Neill 2002) were estimated in linear probability models controlling for age, sex, urban area, past unemployment experience, and change in local unemployment rate.

imposed on high- or low-unemployment areas, on faster- or slower-falling unemployment areas, or on high- or low-vacancy or redundancy areas. During the sample inflow period, the aggregate JSA stock in Northern Ireland fell from over 60,000 in any given month to under 30,000. All areas of Northern Ireland experienced declining JSA stocks over this period, so labour markets were improving throughout the region. Identification of the program change was done by the natural experiment of rolling remodeling efforts to create unified JOB offices in Northern Ireland. McVicar (2010, p. 311) estimates that "withdrawal of job search monitoring on its own significantly increases the JSA stock, whether based on simple DID estimates, estimates from regressions of the JSA stock with various lag structures, or simulations of the JSA stock based on estimated flow effects." The magnitude of the effect estimated "—on the order of a 15% increase in registered unemployment—is similar to that found by Klepinger et al. (2002) when their zero monitoring and tough monitoring regimes are compared and contrasted" (McVicar 2010, p. 311).

Mandatory Services. Mandatory reemployment services are part of policies for many public employment services. Frequently the penalty for not participating is a temporary suspension or reduction in UI benefit entitlement. Blundell et al. (2004) examined the effect of mandatory reemployment services under the New Deal for Young People in the UK. The program provides extensive job assistance and wage subsidies to employers. Prior to this program, young people in the UK could, in principle, claim unemployment benefits indefinitely. Now, after six months of unemployment, young people enter the "Gateway," which is the first period of intensive job search assistance. The program is mandatory, including the subsidized options part. Blundell et al. (2004) focused on the job assistance and wage subsidy elements. They used data drawn from administrative records on a pilot test conducted in selected areas before nationwide program implementation. The comparison group was selected from slightly older people of similar unemployment duration who lived in areas not involved in the pilot. The authors applied a number of different econometric techniques, all exploiting the longitudinal nature of the data set being used but making different assumptions about the structure of the disturbances. All estimators amounted to difference-in-difference estimators with matching. They find that the outflow rate to jobs was increased by about 20 percent for young men as a result of the New Deal—that is, the number of men finding jobs in the first four months of the New Deal rises by 5 percentage points above a preprogram level of 26 percentage points.

Sanctions. Abbring, van den Berg, and van Ours (2005) used administrative data on new UI spells in the Netherlands in 1992 to assess the effect of sanctions for failure to comply with job search requirements. In 1987 a new Dutch law on UI was introduced providing for a tightening of the eligibility rules and a system of benefit sanctions. The law requires three things of claimants: 1) avoidance of unnecessary job loss, 2) positive actions to prevent staying unemployed, including active job search and participation in active employment and training programs, and 3) efforts to keep the administrative organization informed about everything that is relevant to the payment of the unemployment insurance benefits. Failure to comply with these requirements can result in benefit sanctions. The sanctions can be either a temporary or a permanent reduction—full or partial—of the benefit level. In practice, the temporary partial reduction of benefits ranges from 5 percent for four weeks to 25 or 30 percent for thirteen weeks.

The UI administrative agencies are free to choose the sanctions they think are appropriate. Administrative data was provided on 182,239 unemployment spells starting in 1992. A sanction was imposed in 2.9 percent of the cases, with the majority, 53 percent, being imposed between 8 and 24 weeks of unemployment. The effect of one random event on another, in the presence of selectivity, was estimated by a simple graphical procedure that extracts information on the effect of sanctions from the UI data, and that requires neither multiple spells nor regressors. The estimator relies on the intuition that, "if a sanction increases the reemployment rate . . . then a relatively large fraction of those who make this transition [out of unemployment] have been given a sanction shortly before [they exit unemployment]" (Abbring et al. 2005, p. 618). They estimate that reemployment rates are significantly increased by imposition of a sanction. Individual reemployment rates of males increase by 61 percent in the metal industry and by 36 percent in the banking sector. For females, these effects are 98 percent for the metal industry and 85 percent for banking. Estimates on data in which the metal and banking industries are pooled with other industries suggest economy-wide sanction effects of 58 percent for males and 67 percent for females.

Targeted Job Search Assistance. Targeting of JSA surfaced as a policy option during the 1990s, following the massive economic restructuring and worker dislocation of the previous decade. Earlier research had identified JSA as a cost-effective tool for promoting return to work. The question of whether JSA would be effective for those at risk of long-term unemployment was evaluated in the context of a major field experiment in New Jersey (Corson et al. 1989). Together with earlier evidence on JSA cost-effectiveness, results from the New Jersey experiment supported establishment of the Worker Profiling and Reemployment Services (WPRS) system, which required targeted JSA (Wandner 1994).

Two subsequent experiments have evaluated the effectiveness of targeted JSA. The first was undertaken around the time of WPRS start-up, with special accommodations made to ensure experimental integrity (Decker et al. 2000). The second evaluation was done using data from after WPRS implementation (Black et al. 2003). In this section, we briefly review the design and findings of these studies.

New Jersey UI Reemployment Experiment. Enrollment into the New Jersey UI Reemployment Experiment was done between July 1986 and June 1987 (Corson et al. 1989). The sampling frame for random assignment was set to target the evaluation to dislocated workers claiming UI benefits. Characteristics screens were set to construct the sampling frame. These screens required that a claimant meet five conditions. The claimant 1) must receive a first UI payment, and that payment must occur within five weeks of applying for benefits; 2) must be at least 25 years of age; 3) must have worked for the pre–UI claim employer for at least three years; 4) may not be on standby awaiting return to the claimant's previous job with a specific recall date; and 5) may not be a union hiring hall member.

The first three of these eligibility conditions permitted the offer of an intervention early in the jobless spell, and of these three, the second and third ensured that subjects of the experiment were well established labour force members separated from a long job attachment. The fourth

and fifth conditions provided the potential for interventions to affect job search plans. Claimants who are awaiting recall to their prior jobs and members of union hiring halls are not required by the UI system to engage in active job search.

Random assignment sent 2,385 claimants to the control group and 8,675 to one of three treatment groups. All three treatments included JSA, the first being JSA alone. The second treatment added job training to JSA; the third treatment added a cash reemployment bonus to JSA. The bonus was for reemployment within 11 weeks of the claim and was a cash payment of half the remaining UI entitlement, with the initial offer good for two weeks and then declining by 10 percent per week. The bonus was not paid if return to work was a recall, or if the job was temporary, seasonal, part-time, or with a relative. For all three treatments, at five weeks into the claim, all claimants were given JSA orientation, skills and aptitude testing, a JSA workshop, and an assessment or counseling interview.

During the benefit year, the impacts on weeks of UI benefit receipt were −0.47, −0.48, and −0.97 for the three treatments, respectively. All of these impacts were estimated to have statistical significance. The cumulative impacts on weeks of UI benefit receipt over the six years after the initial benefit claim were −0.76, −0.93, and −1.72 for the three treatments, respectively, with the impact from the third treatment estimated to have statistical significance (Corson and Haimson 1995).

The New Jersey UI Reemployment Experiment demonstrated that JSA targeted to claimants likely to be long-term unemployed had the same cost-effective impact as that found for other groups of UI claimants—about half-a-week shorter UI receipt. The encouraging results for the bonus treatment led the U.S. Department of Labour to further investigate the ideal design for a reemployment bonus offer (Decker and O'Leary 1995).

Job Search Assistance Experiment. The Emergency Unemployment Compensation Act of 1991 authorized the U.S. Department of Labour to conduct the Job Search Assistance Experiment. The experiment was designed to evaluate whether providing early JSA to claimants identified by statistical models as being likely to exhaust their UI benefit entitlement would be cost-effective (Decker et al. 2000). During the planning stages of the evaluation, which was scheduled to be run in the District of Columbia and the state of Florida, federal legislation leapfrogged public policy analysis.

In 1993 President Clinton signed Public Law 103-152, which required state employment security agencies to establish and use a system of profiling all new claimants for regular UI benefits. The Worker Profiling and Reemployment Services (WPRS) system was intended to identify UI claimants who were most likely to exhaust their regular benefits, so that they might be provided with early reemployment services to make a faster transition to new employment.

The WPRS established a two-stage process. First, UI recipients who are expecting recall or who are members of a union hall are dropped. These groups are excluded because they are not expected to undertake an active independent job search. Second, remaining UI recipients are

ranked by their likelihood of exhausting regular unemployment insurance benefits. Beneficiaries are then referred to early reemployment services in order of their ranking until the capacity of local agencies to serve them is exhausted. The early assistance comprises at least eight hours of job search assistance, which usually includes an orientation to self-help facilities available at the public labour exchange and a JSW.

In Florida, the JSA experiment proceeded with enrollment between March 1995 and March 1996 at 10 sites around the state where regular WPRS operations were temporarily delayed. Random assignment in Florida involved 8,071 claimants. In Washington, D.C., the experiment counted as the federal district's WPRS implementation. Random assignment enrollment for the JSA experiment was done in all public labour exchange offices throughout the District between June 1995 and June 1996 and involved 12,042 claimants.

The JSA experiment established an eligible pool of claimants using a two-stage process: 1) exclude job-attached and union hiring hall members, then 2) evaluate claimants' probability of exhausting UI entitlement and target those with the highest probabilities for the evaluation. These claimants were randomly assigned to a control group or to one of three treatments. The treatments were as follows: 1) structured job search assistance (SJSA): orientation, testing, JSW, and one-on-one assessment interview. Failure to participate could result in denial of UI benefits. Two additional visits with staff to report job search progress; 2) individualized job search assistance (IJSA): orientation and one-on-one assessment interview. Individual plan developed, which may include additional mandatory services; 3) individualized job search assistance with training (IJSA+): identical to IJSA, plus a coordinated effort with EDWAA staff to enroll the customer in training.

The impacts of the three treatments on weeks of UI compensation in the benefit year in Washington, DC, were −1.13, −0.47, and −0.61, respectively, all estimated to have statistical significance. Estimates of the same parameters in Florida were −0.41, −0.59, and −0.52, all of which, again, were statistically significant. Both evaluations indicated that reemployment occurred at wage rates similar to prior levels. The treatments had generally positive and significant effects on earnings in Washington, DC, but no impact on participant earnings in Florida.

Structured JSA emerged as the most cost-effective intervention examined. The authors of the evaluation report attributed the generally larger impacts observed in Washington, DC, to stricter enforcement of JSA participation requirements. They recommend making particular JSA services mandatory and maintaining clear linkages between UI and ES in the new one-stop environment under the Workforce Investment Act (WIA).

Dickinson, Decker, and Kreutzer (1999) conducted a six-state evaluation of the Worker Profiling and Reemployment Services (WPRS) system implementation and effectiveness. Reliable data was available for only five of the six states (Connecticut, Illinois, Kentucky, New Jersey, and Maine). Samples were drawn from the top of each state's profiling score distribution, with UI beneficiaries near the top compared to those just below the WPRS referral

threshold. Program impacts were estimated in regression models controlling for observable characteristics. "The independent variables included in the regression models differed somewhat across states, but the models generally included variables for personal characteristics (including age, race, sex, and education), employment characteristics (including earnings, job tenure, industry, and occupation at previous job), UI entitlement (including weekly benefit amount and potential duration of UI benefits), and the probability of UI exhaustion calculated using the state's statistical model. When possible we included all variables in the state's statistical model as independent variables in our model. For some states, we were also able to control for local unemployment rates. Finally, all models included indicators for local office of initial claim and the quarter of the first payment" (Dickinson, Decker, Kreutzer, 1999, p. III-11). Estimated average impacts on weeks of insured unemployment were –0.25, –0.41, –0.21, –0.29, and –0.98 for Connecticut, Illinois, Kentucky, New Jersey, and Maine, respectively.

Evaluation of Worker Profiling and Reemployment Services in Kentucky. While Kentucky was included among the states studied in the national evaluation of WPRS, an independent assessment of WPRS in Kentucky based on an experimental design arrived at a much different conclusion. The profiling model used in Kentucky was developed by economists at the Center for Business and Economic Research at the University of Kentucky (Berger et al. 1997). In working with the Kentucky Department for Employment Services on the WPRS system, these economists advocated a methodology for assignment to the WPRS that provided ready data for an experimental evaluation of WPRS effectiveness.

Kentucky divides the predicted UI exhaustion distribution into 20 groups spanning 5 percentile points each. Every week the local WPRS capacity is hit within one of the 20 groups. That group is referred to as a profiling tie group (PTG). In Kentucky, profiled WPRS customers within PTGs are randomly assigned to WPRS, or not. This is viewed as an appropriate rule for referral to the WPRS from a group of UI claimants having scores that are not statistically significantly different. It also provides the basis for evaluation of the WPRS based on randomized trials.

From the PTGs, experimental and control groups were formed by the randomized trials to conduct an evaluation of the WPRS in Kentucky (Black et al. 2003). Data was collected starting with the very beginning of WPRS implementation in Kentucky, in October 1994, and lasting through June 1996. The PTGs yielded a total sample of 1,981 claimants, and 1,236 of these were assigned to mandatory WPRS JSA. Compared to the total population of 48,002 profiled and referred Kentucky claimants during that period, means of observable characteristics (age, schooling, gender, race, prior earnings, and weekly benefit amount) for the experimental treatment group were not statistically significantly different from those in the control group.

The impact estimates for WPRS in Kentucky were dramatic. On three outcomes of interest, the estimated impacts were −2.2 weeks of UI, −$143 in UI benefits, and a $1,054 increase in earnings during the UI benefit year. The difference in these estimates from the national WPRS evaluation were most likely due to the fact that Black et al. (2003) essentially confined their contrasts within PTGs, thereby achieving a closer counterfactual. Dickinson et al.

(1999, 2002) compared those assigned to WPRS who had the highest probability of benefit exhaustion against all those profiled but not referred, including many with very low exhaustion probabilities. This meant the comparison group in the national evaluation was likely to have shorter mean benefit duration than program participants even in the absence of WPRS services. Essentially Black et al (2003) estimated local average treatment effects while Dickinson et al. (2002) estimated average treatment effects for the program. When finely targeted, the WPRS program appears to be more effective.

The willingness of the Kentucky Department of Employment Services to accept the recommendation of Professor Mark Berger and his colleagues regarding WPRS model design and system implementation was a key to producing reliable WPRS impact estimates (Berger et al. 1997). Professor Berger and his colleagues at the Center for Business and Economic Research (CBER) at the University of Kentucky recommended randomization on the margin in assignment to WPRS. This should be standard WPRS practice for all state and local employment service delivery agencies. In setting up WPRS administrative rules, the Kentucky agency realized the value of evaluation research and used that orientation to help resolve the resource allocation problem. When resources are limited, randomization in program assignment can always be viewed as an equitable mechanism. It has the added benefit of providing for very strong evaluation evidence.

Reemployment Incentives–Bonuses. Between 1984 and 1989, four reemployment bonus experiments targeted at unemployment insurance (UI) recipients were conducted in the United States. These experiments provided various levels of lump-sum payments to UI recipients who took new, full-time jobs within 6 to 12 weeks of their benefit application and held those jobs for at least three to four months. Empirical UI research had produced evidence that UI payments might lengthen jobless durations beyond what they would be in the absence of UI. The purpose of these interventions was to learn more about the behavioral response of UI recipients to changes in the UI program. Reemployment bonuses were intended to speed the return to work in a manner that would benefit employees, employers, and the government, and would be cost-effective. UI claimants would be better off if they returned to work sooner and found jobs that were similar and paid similar wages to the jobs they would have taken in the absence of a bonus offer. Employers would be better off if they experienced lower UI payroll taxes. The government would be better off if the cost of the bonus was offset by a decrease in UI benefit payments to unemployed workers and an increase in income and other tax contributions by workers during their longer period of employment.

Illinois UI Incentive Experiment. The first bonus experiment was conducted in Illinois during 1984–1985 and was sponsored by the Illinois Department of Employment Security. Its goal was to examine the theoretical and empirical economic implications of a reemployment bonus offer to UI claimants and the potential for developing a cost-effective bonus program. The Illinois design provided a $500 bonus amount, equivalent to about four weeks of UI benefit payment—i.e., four times the UI weekly benefit amount (WBA). To collect a bonus payment, treatment group members needed to become reemployed within 11 weeks of filing their UI claims. The estimated impact of the Illinois reemployment bonus offer to UI claimants was a

reduction in the duration of UI compensated unemployment by 1.15 weeks (Woodbury and Spiegelman 1987). This reduction was so great that the reemployment bonus was cost-effective to the UI Trust Fund, generating a benefit cost ratio of 2.32. At the same time, participants suffered no reduction in postunemployment wages, which indicated that the bonus offer did not reduce job quality.

New Jersey UI Reemployment Demonstration. Independent of the Illinois experiment, the U.S. Department of Labour (USDOL) sponsored a New Jersey UI experiment that included a reemployment bonus treatment group. This project was designed and became operational in 1985 and 1986, before the results from the Illinois experiment became available. As such, the New Jersey experiment was not designed to replicate or validate the Illinois experiment. The New Jersey bonus offer was designed so that the amount of the offer was tied to a claimant's remaining UI benefit entitlement and the amount paid was larger in cases of more rapid reemployment. The initial bonus offer was one-half of the claimant's remaining entitlement at the time of the offer. This offer amount remained constant for the first two full weeks after the initial offer. Thereafter the amount of the bonus offer declined by 10 percent of the original amount per week, falling to zero by the end of the eleventh full week of the bonus offer. Initial bonus offers in New Jersey averaged $1,644, which was about nine times the UI weekly benefit amount. The evaluation of the New Jersey experiment suggested that the reemployment bonus, as it was implemented in New Jersey, generated modest savings in UI. Since the cost of offering and paying the bonuses exceeded the modest UI savings, the New Jersey bonus was not cost-effective from the perspective of the UI system.

Pennsylvania and Washington Reemployment Bonus Experiments. In 1987, with the evaluation of the Illinois experiment completed and the New Jersey experiment operations over, the USDOL sponsored two additional reemployment bonus experiments, one in Pennsylvania and the other in Washington State. In contrast to the Illinois experiment, these later trials generated much more modest results. In the Pennsylvania and Washington experiments the bonus offers were set as multiples of the worker's weekly benefit level. This design was adopted because in the Illinois experiment claimants receiving less than the UI maximum weekly benefit responded more strongly to bonus offers than those constrained by the maximum (O'Leary, Spiegelman, and Kline 1995, p. 267). The Pennsylvania and Washington experiments tested benefit levels that bracketed the Illinois bonus amount (4 × the weekly benefit allowance, or WBA) and tested qualifications both similar to the earlier offers and about half as great. The resulting designs provided for four treatment groups in Pennsylvania and six in Washington. The dimensions of each design were the level of the bonus (high and low in Pennsylvania; high, medium, and low in Washington) and the qualification period or duration of the bonus offer (short and long in both states). While half of the 10 treatments in Pennsylvania and Washington were cost-effective to claimants, society, and the government sector as a whole, only two of the treatments were cost-effective for the UI system (Decker and O'Leary 1992, 1995).

The relatively weak response to the bonus offer in Pennsylvania and Washington led to a reexamination of the powerful Illinois results. It was discovered that within the designed experiment, a second experiment had unintentionally taken place. In 1984, as Illinois was

recovering from a major recession, the availability of Federal Supplemental Compensation (FSC) was terminated. This resulted in about half of the claimants studied having 38 weeks of UI benefit eligibility, with the remainder being eligible for only 26 weeks of regular UI benefits. It turns out that the mean bonus response of −1.15 weeks in Illinois was made up of a response of −1.78 weeks for those eligible for FSC and −0.54 weeks for those not eligible. The average response of −0.54 for the non-FSC sample in Illinois is close to the response observed in Pennsylvania and Washington, where the entitled duration of benefits was also similar.

Among the individual treatments, the impact on weeks of UI benefits ranged from −0.05 for the low bonus amount/short qualification period offer in Washington to −1.78 for the bonus offer to FSC-eligible claimants in Illinois. Impacts for Pennsylvania tended to fall between those for Illinois and those for Washington. Overall, a cash bonus can be expected to modestly shorten spells of insured unemployment—the mean effect of the offers made in the three states yielded about a one-half week reduction in weeks of UI benefits.

The degree of response to the bonus offer was also examined for important subgroups within the sample. Results from Pennsylvania and Washington suggest that UI claimants in low-unemployment areas and claimants whose prior employment was in manufacturing tended to respond more strongly to the bonus. However, close inspection of subgroup results reveals one main finding: there is no difference between any pair of subgroups shown that is both statistically significant at conventional confidence levels and consistent across the three experiments. The implication of this finding is quite striking—the reemployment bonus has a remarkably even impact on various subgroups of workers, whether delineated by gender, age, race, industrial sector of employment, level of local unemployment, or level of the weekly benefit amount.

O'Leary, Decker, and Wandner (2005) investigate whether targeting reemployment bonus offers to UI claimants identified as most likely to exhaust benefits would reduce benefit payments. They show that targeting bonus offers with profiling models similar to those in state WPRS systems can improve cost-effectiveness. However, estimated average benefit payments do not steadily decline as the eligibility screen for targeting is gradually tightened. The authors find that narrow targeting is not optimal. The best candidate to emerge is a low bonus amount with a long qualification period, targeted to the half of profiled claimants most likely to exhaust their UI benefit entitlement.

Two potential behavioral effects might reduce cost-effectiveness for an operational program (Meyer 1995): First, an actual bonus program could have a displacement effect. Displacement occurs if UI claimants who are offered a bonus increase their rate of reemployment at the expense of other job seekers not offered a bonus. Second, there is also the risk that an operational bonus offer program could induce an entry effect. That is, the availability of a reemployment bonus might result in a larger proportion of unemployed job seekers entering the UI system.

If entry and displacement effects are sizable, actual program cost-effectiveness will be lowered. However, targeting low bonus amount/long qualification period offers to only those most likely to exhaust UI should reduce both these risks. Targeting would introduce uncertainty that a bonus offer would be forthcoming upon filing a UI claim, which should reduce the chance of a large entry effect. Also, targeting should reduce any potential for displacement, since a smaller proportion of claimants would receive the bonus offer.

Reemployment Services to UI Beneficiaries. The high unemployment levels and long durations of UI receipt occurring in the 1975 recession led the U.S. Department of Labor (USDOL) to renew its emphasis on active job search by UI beneficiaries. Guidelines for an eligibility review program (ERP) were issued by USDOL to all state employment security agencies in 1976, and beginning in 1977 states were allotted funds for operating ERPs (Walker 1982). The ERPs required states to do two things: 1) continuously review ability, availability, and the efforts of beneficiaries to gain reemployment and 2) promote reemployment by supporting an active job search by UI beneficiaries. Over time the use of ERPs dwindled in many states, along with federal funding for staff to provide services. Employment Service (ES) funding fell by half in real terms between 1984 and 2007 (O'Leary and Eberts 2008). In 2005 the USDOL renewed and expanded the ERP concept by dividing $30 million in funding among 18 states to provide reemployment and eligibility assessment (REA) grants. The REA requires two conditions. First, UI beneficiaries must be required to report in person to a One-Stop Career Center for staff-assisted services as a part of the REA. Second, assessments must include four steps: 1) a review of continued eligibility and referral to adjudication, as appropriate, when a potential issue is identified; 2) the provision of labour market information; 3) development or review of a work search plan; and 4) a referral to employment services (e.g., job search assistance workshops or job placement services) or to occupational or skills training, when appropriate. Funding to states for REA rose to $50 million in 2009 (Small 2009).

Two recent evaluation studies provide additional evidence that work search requirements and JSA affect the duration of insured unemployment. These studies looked at the Reemployment and Eligibility Assessment (REA) program and the Wisconsin reemployment demonstration in One-Stop Career Centers. Both studies found beneficial effects of strengthened work search enforcement and linkages to reemployment services.

The REA initiative was a U.S. Department of Labor demonstration project with a budget of $20 million to provide assistance to states establishing new or significantly revamped REA programs. REAs are an eligibility review program, run within the UI program without the participation of One-Stop center staff. REA efforts were implemented in 21 states in 2005. Federal funds for REAs were appropriated with the proviso that research would be conducted in the pilot states to learn whether REAs could be a model for shortening jobless durations and reducing insured unemployment. Evidence from a random assignment evaluation in Minnesota suggests that REAs reduced the duration of UI benefit receipt by 1.2 weeks (Benus et al. 2008). Estimation of the REA effects for Minnesota involved matching and regression adjustment on observable characteristics. Recent federal initiatives have pumped millions of dollars into states to broaden the use of REA programs for UI beneficiaries.

The Wisconsin demonstration project was also sponsored by the Employment and Training Administration (ETA) of the U.S. Department of Labor. It brings UI and One-Stop center staff together to provide reemployment services and eligibility reviews in the One-Stop center. In this cooperative operations model, UI staff are out-stationed in the One-Stop centers. The Wisconsin demonstration, with its nonexperimental evaluation design, provides further information about the cost-effectiveness of ES cooperation in the UI work test. The project matched demonstration group claimants with up to three claimants from the comparison sites using an algorithm that linked individuals based on postal zip codes, their propensity to return to work (as predicted by their WPRS profiling scores), their prior employment history, and other individual background characteristics. Impacts were estimated on outcomes of interest using regression adjustment including the following variables: had a disability; limited English proficiency; single parent; and number of previous UI claims. Those receiving additional attention for the work test in One-Stop centers shortened UI durations by 1.8 weeks and lowered benefit year compensation by $468 (Almandsmith 2006, p. 7).

Youth Employment Services. Centeno, Centeno, and Novo (2009) estimated average treatment effects on unemployment duration of active labour market programs in Portugal addressing long term unemployment of younger and older workers. The Portuguese labour market is characterized by extremely high employment protection, long unemployment spells and generous unemployment insurance, and a low arrival rate of job offers—a rate that is low even in comparison to other European countries. The youth program (Inserjovem) targets all young people (less than 25 years old) before they have been registered for six months; the other program (Reage) attempts to serve all adults before they reach 12 months of unemployment. Both programs provide job search assistance, including vocational guidance, counseling, monitoring, and training or retraining options. Potential sanctions—including loss of unemployment insurance and fee exemption to access the public health services—can result from failure to comply with the directions of the Employment Office (EO) placement team. The programs were first introduced in a subset of EOs beginning in June 1998. They were later rolled out sequentially to the other EOs, fully covering the country by January 2001. The pilot EOs were chosen for logistical reasons unrelated with the programs' goals in terms of labour market outcomes. Centeno, Centeno, and Novo (2009) apply a difference-in-difference methodology using a natural experiment, resulting in treatment and control groups originating from the sequential program implementation across the country. They try to achieve identification by assuming that the average outcomes for treated subjects and controls would have followed parallel paths over time. They claim that results are robust to a wide variety of quasi-experimental designs and estimators. They attempt to estimate the effects of the program compared to the outcome for the individual had he or she continued to search for a job in the absence of the support provided. Impact estimates were computed while including a vector of covariates (predetermined with respect to the introduction of the program) that was included to correct for differences in observed characteristics between individuals in treatment and control groups. The control variables include age, sex, nationality, schooling, place of residence, and some variables related to previous labour market experience (e.g., reason for job displacement). Results suggest the adult program leads to a modest reduction in the unemployment duration of

workers finding a job upon participation, whereas the impact for youth is generally negative (extended durations). These results are robust to a wide variety of constructions of nonexperimental settings and estimators. On the other hand, the results were mixed, and thus less satisfactory, for young workers, for those over 40, and for the less educated. Women also benefited less from the programs. The results seem to improve slightly for young workers in the second semester of implementation, but they deteriorate in the medium term. The lack of wage subsidies in the Portuguese programs may explain the minor impacts obtained when compared to similar programs. Gender, age, and schooling seem to play an important role in determining the programs' impact. In transitions into employment, the impact is larger for older men (a reduction of unemployment close to one month). In terms of age, the largest impact is observed for individuals aged between 30 and 40. Workers with a higher degree of education seem to benefit more.

Jensen, Rosholm, and Svarer (2003) study a Danish program associated with a dramatic decline youth unemployment. In 1996, Denmark established the Youth Unemployment Programme (YUP) for low-educated unemployed youth. The dual aims of YUP are to increase the employability of low-educated unemployed youth, and to motivate them to undertake further education. The European Commission has labeled this program a "best practice." Young persons under the age of 25 without any formal education beyond secondary school, and who have been unemployed for 6 months during the last 9 months, are offered 18 months of specially designed vocational education. Participants in a YUP education program, or other approved education program, receive UI at 50 percent the normal rate. Refusal to participate in an approved education program results in loss of unemployment benefit entitlement. Jensen, Rosholm, and Svarer investigated the impact of YUP on the duration of unemployment spells and the transition rates from unemployment to schooling and employment. They analyzed three effects at the very beginning of the YUP: an announcement effect, a direct programme effect, and a sanction effect; and found evidence that the YUP caused some of the observed reduction in youth unemployment. Data was collected by interviewing approximately 3,500 individuals aged 16–24 from April 1996 to December 1996. For each individual, they have information about labor market transitions occurring between the time of selection and the time of the interview, and about personal characteristics (age, gender, number of children, education, ethnic status). They also know whether and when the individuals have received an offer from the labor market office to participate in the YUP. This information was used to construct treatment and a control groups. They provide homogeniety tests showing the control group was not meaningfully contaminated, and they argue this is because the sample was drawn early in the YUP implementation. They estimate impacts on the risk of leaving unemployment for either skill improvement (education or training), or employment in a competing risks model controlling for observed and unobserved characteristics.[24]

The main result is that the transition rate from unemployment to schooling is significantly raised by the YUP. This results mainly from a direct program effect and partially from the threat of sanction. These effects are estimated after correcting for the strong seasonality in the transition rate from unemployment to schooling. In addition, they find somewhat weaker effects

[24] They impose a restriction in estimation that unobserved errors in the training equation are perfectly correlated with errors in the employment equation.

on the transition rate from unemployment to employment. Their estimates of YUP effects suggest that aggregate youth unemployment may have been reduced by YUP through shifting young people away from ‘‘waiting on the dole’’ to ‘‘waiting in the classroom.’’ “Clearly, a significant increase in the transition to employment would have been more satisfactory. However, the possible ‘‘scarring’’ effect of unemployment suggests that it could be welfare-improving to move the youth out of unemployment and into the classroom. Thereby, the long-term consequences of unemployment may be mitigated.” (Jensen, Rosholm, and Svarer 2003, p. 314)

3.4.5.1 Summary of EAS-related evaluations

Our review of the effectiveness of support measures offered by public employment services around the world that are or could be available in Canada under the category of Employment Assistance Services (EAS) includes ten subcategories. Evidence from evaluations in the U.S. suggest that job interviews and referrals can improve return-to-work rates and earnings, particularly for women and men in urban areas. Evidence from France, the Netherlands, and the United States found small positive effects from job search counseling. Several studies in the United States and the United Kingdom testing active work search requirements for current UI beneficiaries found significantly shorter durations of UI receipt when the work test is stronger, and much longer durations when the work test is removed. Mandatory services tested in the United Kingdom for unemployed youth had modest effects similar to the work test, while sanctions curtailing UI benefit entitlement in the Netherlands produced large and significant reductions in compensated durations. Targeted job search assistance in the United States to those at risk of long term UI receipt is found to significantly shorten benefit durations. Cash incentives for early return to work paid as bonuses after reemployment had modest effects in the United States, but the cost-effectiveness of such offers was improved when they were made to those most likely to have long durations of UI receipt. Reemployment services provided on a regular schedule during a continuous spell of UI benefit receipt was found to shorten unemployment durations in the United States. Similar favorable results were found when improving linkages between UI and ES services in One-Stop centers in the United States. Despite the threat of sanctions, employment services targeted to unemployed youths were not particularly effective in Portugal, but results were more encouraging in Denmark where outcomes include further education or training as well as employment.

4 KNOWLEDGE GAPS IN THE EVALUATION LITERATURE

This chapter aims to lay a foundation for future evaluations of active labour market programs in Canada by identifying gaps in knowledge. Filling these knowledge gaps would help to identify useful improvements in public employment policy. The chapter is divided into three sections. The first section summarizes gaps in the international evaluation literature reviewed in this paper. The second chapter lists knowledge gaps in Canadian evaluations of public employment programs. The third section lists items missing from the Canadian evaluations that are not reliably provided by the international literature.

4.1 Knowledge Gaps in the International Literature

This section provides an overview of informational items not found in the international literature concerning program impacts by participant subgroup, program features, bundling or sequencing of services, and labour market context. The five categories of active labour market programs reviewed in Chapter 3 are discussed in sequence: skills development, targeted wage subsidies, self-employment assistance, job creation partnerships, and employment assistance services. The remaining subsections of this chapter each follow the same sequence.

Skills Development. Publicly funded job skill training has been a feature of active labour market policies in developed countries throughout the world for decades. Nevertheless, the literature seems to suggest that policymakers and administrators of programs have not been able to improve outcomes over that time period. While there has been considerable variation in evaluation findings, the majority of studies find modest positive labour market outcomes for adults and insignificant results for youth.

However, virtually no evaluation study has dissected the characteristics of the training that has been provided. Training can be offered in formal educational institutions, on-line, at a work site, or in some combination of these. Its content can be quite specific in nature or can integrate several domains of information. It can be offered in a concentrated period of time or spread out over several months or years. Furthermore, the effectiveness of these various modalities of training can interact with characteristics of the trainees and with characteristics of the trainers. With quite limited exceptions, the international literature has not pried into the "black box" of training.

Given that the evidence to date suggests that the social payoff to skills development, if any, is derived only after several years, another gap in the literature is that it contains virtually no papers on the depreciation rates of skills that are acquired through training. This, of course, would require long-term follow-up. Fortunately, longitudinal databases have been constructed in a number of European countries. Unfortunately, these data have not been fully exploited for this type of analysis.

Much of the international evidence stems from European countries that are, relative to the United States or Canada, densely populated. Skills development traditionally has been offered in classroom settings, and so it may be hypothesized that rural populations have had access problems. The literature provides little evidence on differences in training impacts across urban and rural subgroups.

Finally, the impact of the business cycle on training is not well-known. Theoretical arguments can be made either way. When unemployment is relatively high, the pool of unemployed includes relatively higher-skilled and more motivated individuals, which suggests that more positive outcomes may occur. On the other hand, a soft labour market makes it more difficult to find employment after being trained.

Targeted Wage Subsidies. The most notable gap in the international evaluation literature on targeted wage subsidies has to do with understanding the effectiveness of the design of the program. Most evaluations treat a targeted wage subsidy program as a "black box," in the sense that it is known that the program provides a wage subsidy, but little attempt is made to evaluate the effectiveness of different aspects of the program's design. An evaluation typically describes the program it is studying, but rarely does it assess the effectiveness of variations in the design of the program. Since program design varies significantly across targeted wage subsidy programs, it is difficult to generalize about the effects of such program features. It would be useful for policymakers if evaluation reports included more details about the treatment examined. Consequently, it is hard to piece together results from various studies to try to understand the marginal effects of program design changes, because of the confounding factors among them. For instance, the United States' Earned Income Tax Credit (EITC) program provides a tax credit of up to 40 percent on the first $8,890 of earned income. The subsidy decreases as earned income increases, until the subsidy is phased out. The exact amount depends upon family circumstances. The United Kingdom's Working Families' Tax Credit (WFTC) provides a flat amount of roughly £145 for up to £80 of earned income before tax and benefits. For earned income greater than £80, the person's income after benefits and taxes jumps to £225, and then increases proportionally with increases in earned income thereafter. The WFTC also includes a generous child tax credit for child care; the EITC does not. Clearly, the EITC and the WFTC are different in design, but the literature does little to determine how much the difference in generosity affects the outcomes. Furthermore, the frequency of the subsidy payment to recipients differs between the two programs, depending upon whether the EITC recipient elects to receive advance payment. The impact of the WFTC on labour force participation is shown to be around 5.0 percentage points for lone mothers, and the impact of the EITC on the labour force participation of single mothers is shown to be roughly 2.4 percentage points. Yet a study of the EITC finds that the program raises the labour force participation rate of least-educated mothers by 6.2 percentage points (Eissa and Liebman 1996).

It is difficult to know how to think about these results with respect to the design of the program. Comparing two evaluations done by the black box approach, there are too many variables changing to attribute an effect estimate to one factor. For example comparing WFTC and EITC, does a more generous tax credit raise the labour force participation rate from 2.4 percentage points to 5.0 percentage points (to take the average results of the evaluations of the two programs)? Or could it be that the participants of the WFTC are much less educated on average than the participants of the EITC, and a more apt comparison would be the average results of the WFTC of 5.0 percentage points with the results for the least-educated recipients of the EITC of 6.2 percentage points? Perhaps reimbursement of child care under the WFTC accounts for the difference? What is missing is the ability to assess how variations in particular program program features affect outcomes. Such assessments should be done within specific program evaluations.

Another gap in the literature is that very few programs have been evaluated using random assignment. The majority of evaluations rely on comparison groups constructed using propensity score matching. The gap is obviously not the fault of researchers but rather can be

blamed on the lack of attention or funds from policymakers to conduct demonstration projects or on their failure to set aside funds from a portion of an existing program to conduct random assignment evaluation. Obviously, some programs, particularly ongoing programs, are difficult to evaluate using experimental methods, whether for ethical reasons or because the services are considered entitlements or essential to the well-being of the individual. Therefore, nonexperimental methods will always be relied upon to evaluate most of the programs that are in place. Nonetheless, questions do emerge as to the validity of nonexperimental approaches. For example, consider that the only recent random assignment evaluation of a wage supplement program was conducted for the Canadian Self-Sufficiency Project (SSP), even though the United States' EITC and the United Kingdom's WTFC are extensive and costly programs. The trial Canadian program is different from the U.S. and U.K. programs in that it has a three-year limit for receiving the subsidy, whereas the latter two are indefinite as long as applicants qualify according to their earnings. For any given year, the Canadian and U.K programs are more generous in the subsidies provided than the U.S. program. The United Kingdom covers child care, while the other two do not. Despite these differences in program design, it is interesting to compare the evaluation results of the three programs: one based on random assignment methodology and the other two based on matching techniques. For lone mothers, the Canadian random assignment evaluation found an increase of between 6.1 and 12.6 percentage points in full-time employment (at least 30 hours per week) during the first four years. Two evaluations of the U.K. program found an impact of between 2.9 percentage points (Leigh 2005) and 5.5 percentage points (Francesconi and Van der Klaauw 2004). The question is whether the Canadian SSP, since it is an experiment based on random assignment, should be used as the benchmark for evaluations of the other programs. Obviously, it might be an interesting starting point for comparison, but the program design features differ so much as to render such a comparison problematic.

Another issue is the interaction of programs that exist concurrently with those under study. Most evaluations have a narrow focus, but their participants may participate or have options to participate simultaneously or sequentially in other programs. The evaluation results may be influenced by such interactions, even though the evaluation methodology tries to control for such factors. On the other hand, it is important to understand how programs interact, and by not explicitly taking this interaction into account, the evaluations leave a gap in our understanding of those relationships.

Self-Employment Assistance. International evidence on self-employment assistance is generally positive. Controlling for other factors, evaluations suggest that SEA participants experience higher rates of self-employment, higher levels of income (from self-employment and other earnings), and draw less UI compensation. Most evidence comes from programs providing a weekly or biweekly stipend at the level of UI benefits during the SEA start-up period. The Canadian SEA program provides periodic payments, but it also requires personal contributions to the start-up costs. There is little or no international evidence on this last feature. While only the United States targets SEA offers to those at high risk of long-term UI benefit receipt, compared to other unemployed job seekers most SEA participants worldwide are older and have more education, more labour market experience, and more personal assets. This profile probably also

fits Canadian SEA participants, and these factors are probably positively correlated with SEA success. Statistical evidence is lacking about SEA success in the dimensions of demographic characteristics, industrial and occupational settings, and self-funding requirements. Encouraging evidence suggests that SEA can be a successful intervention in areas of high unemployment. Since most SEA evaluations have been relatively short term, and business survival is best judged over the longer term, future evaluations done five or more years after SEA assistance would be very valuable.

Job Creation Partnerships. Gaps in the evaluation literature of direct job creation using government funds are similar to those identified for the targeted wage subsidies in Section 3.4.2. The lack of evaluations that estimate the marginal effects of different program designs leaves policymakers without a clear road map as to which features of a direct employment program they may prefer to adopt. Since these programs may be used for different reasons, ranging from a countercyclical program to a program for employing the hard-to-employ, understanding these features is important.

In addition, very few programs are evaluated using random assignment methodology. The only program identified in this critique is the United States' Supported Work Demonstration, which was conducted more than three decades ago. The relevance of the findings of that program for reforming current programs or fashioning future ones could be questioned, since the labour environment and general attitudes of the groups covered in that demonstration may be different today, which could affect their response to the services and incentives integrated into that demonstration.

Employment Assistance Services. Our review of the international literature on interventions that could fall under the heading of Employment Assistance Services (EAS) includes ten categories: 1) Job Interview Referrals and Job Placements, 2) Counseling, 3) Job Search Assistance, 4) UI Work Test, 5) Mandatory Services, 6) Sanctions, 7) Targeted Job Search Assistance, 8) Reemployment Incentives/Bonuses, 9) Reemployment Services to UI Beneficiaries, and 10) Youth Employment Services. Evidence from several countries suggest that general public employment services, including job interview referrals and placements, counseling, and job search assistance, improve rates of reemployment and often modestly improve reemployment earnings. These services are uniformly identified as being inexpensive to provide. Employment service measures taken to assure an active job search by UI beneficiaries have been shown to be effective in several countries. These studies include experimental removal of the work test in the United States and the United Kingdom, requiring mandatory services participation in the U.K., and the imposition of sanctions for failure to participate in the Netherlands. Early targeting of reemployment services to those with the highest risk of long-term joblessness is also effective, as are targeted cash incentive payments for speedy return to work. For unemployed youth who are ill-prepared for the job market, a Danish study found that withholding or reducing cash unemployment assistance is an effective lever for motivating return to work or learning. In Portugal, however, requiring participation in reemployment services combined, with the threat of losing UI for nonparticipation, worked as an incentive for older workers but not for younger workers.

4.2 Knowledge Gaps in the Canadian LMDA Evaluations

This section overviews estimates not provided in the Canadian LMDA evaluations of EBSMs by participant subgroup, program features, bundling or sequencing of services, and labour market context.

Skills Development. The evaluation evidence from the Canadian LMDA evaluations of EBSMs mirrors the evidence from the extant literature from other countries. That evidence shows relatively modest positive employment and earnings impacts, but it has very little to say about how those impacts vary by training or trainer characteristics. Sample size constraints furthermore limit the Canadian findings about subgroup impacts, and the follow-up periods have been too short to estimate depreciation rates.

An advantage of the Canadian evidence vis-à-vis the international evidence is that it is able to exploit provincial variation in labour market conditions. Thus there is tighter evidence about the effect of the business cycle on labour market outcomes. Furthermore, the population densities across the provinces differ considerably, and so Canadian evidence of the effectiveness of skills development for rural populations should be available.

Targeted Wage Subsidies. Most of the LMDA evaluations estimated significant employment and earnings gains from targeted wage subsidies, with gains more pronounced for younger and harder-to-employ customers. The evidence on EI savings was less encouraging, and most certainly resulted from the definition of insurable earnings under EI that permitted EI receipt after separation from employment supported by a targeted wage subsidy. The Targeted Wage Subsidy program is the third largest active labour program in Canada. There is adequate sample size among participants to do a deeper analysis of subgroup impacts. The collection of LMDA evaluations has been constrained by budgets for follow-up surveys. Better use of program administrative data could provide deeper insights into the workings of the program. Important evidence for Canada is also available from the self-sufficiency experiments. As for targeted wage subsidies, results suggest that intervention effects are more positive for former claimants than for active claimants. No evidence is available in the Canadian context for wage subsidies to employers through the income tax system; this could be a rich area for future policy and research. The Canadian earnings supplement project (ESP) tested wage subsidies paid to workers. Such supplements are appealing since they can avoid the stigma resulting from paying employers for hiring the unemployed. Unfortunately, the very low take-up rate for the ESP did not yield reliable evidence of effects. Such an intervention could be retried in Canada as an integral part of EI or the tax system. Either avenue may yield higher take-up than the adjunct payments system tried in ESP.

Self-Employment Assistance. The Canadian SEA provides weekly income support payments at the rate of EI benefits, and requires personal contribution of at least 25 percent of start-up costs. Evidence from the LMDA evaluations suggest that SEA increases annual working hours by 20 to 30 percent, but that it also results in reduced annual income for SEA participants relative to proper comparison groups. Evidence from the Wong, Henson, and Riddell (1998)

evaluation of earlier versions of self-employment assistance was more favorable. Perhaps a reconsideration of program design features could be undertaken. In particular, the role of the personal contribution could be evaluated in a random assignment experiment. Furthermore, it would be useful to have evidence on SEAs in different economic contexts, by demographic characteristics, by occupational and industrial categories of activity, and with long-term follow-up.

Job Creation Partnerships. The LMDA evaluations yielded inconsistent and weak evidence on the effects of job creation partnerships. There were insignificant main effects, and in most cases reliable impact estimates were not provided for subgroups delineated by participant characteristics, provider characteristics, labour market conditions, or ownership settings. The former is not surprising; the latter is disappointing. The main aims of direct job creation programs are normally to provide income transfer to a disadvantaged population while preventing further decline in employability skills through work experience. Transition to regular nonsubsidized employment is not typically the main focus. However, in any evaluation there are aspects of the intervention with positive outcomes, and detailed subgroup analyses should be done to reveal such results.

Employment Assistance Services. Evidence about effects of EAS from the LMDA evaluations is mixed and mostly insignificant. Nicholson (2010) writes that a main challenge to evaluators is the fact that EAS is usually combined in an action plan with other interventions. Therefore, evaluations focused on “EAS only" claimants have little statistical precision or power. These are the most widely used active labour market interventions in Canada, and the least is known about them. Evaluation studies could estimate incremental effects of services by comparing bundles delivered in action plans.

4.3 Questions to Be Answered: Overlap in Knowledge Gaps

This section summarizes categories of evidence not provided in either the international studies or the Canadian LMDA evaluations of EBSMs by participant subgroup, program features, bundling or sequencing of services, and labour market context.

Skills Development. In the next round of EBSM evaluations, a major contribution could be made by carefully collecting data on training characteristics. These would include total hours, hours per day, days per week, trainer(s) background and characteristics, trainee attendance, certification, technology and equipment used, characteristics of the location(s) of training, and so forth. If feasible, randomized controlled experiments around some of these features would add value to the evaluation.

Furthermore, evaluations should plan for long-term follow-ups to track outcomes over time. While this may be cost-prohibitive for entire cohorts, there may be subsamples of participants and controls for which longer-term follow-ups could be conducted.

Targeted Wage Subsidies. Larger sample sizes achieved through the use of

administrative data could support deeper analysis of subgroup effects in the Canadian context on impacts by participant characteristics, location characteristics, provider characteristics, or source of support. Detailed records on program features should be kept in administrative data, and these should be used in evaluation. Important program features include the duration of subsidy and the amount of subsidy. Other mechanisms for wage subsidies could also be tried in controlled evaluations. For example, subsidies paid to employers through the tax system for hiring from targeted groups, or wage supplements paid to job seekers through the EI or tax system. Additionally, limiting EI insurable earnings to non-subsidized work is an important question for policy debate; however, current entitlements make evaluation a challenging proposition.

Self-Employment Assistance. Participation in SEA is quite small in Canada. The full administrative file on all participants should be used for evaluation. Comparison groups could be drawn from incomplete SEA applications, or from matching on characteristics among EI beneficiaries or reachbacks in EBSM records. It would be useful to have evidence on SEAs in different economic contexts, by demographic characteristics, by occupational and industrial categories of activity, and with long-term follow-up. Much of this analysis could be done with larger sample sizes available through use of administrative data. A random assignment experiment could evaluate the role of the 25 percent cost contribution to SEA participation and success.

Job Creation Partnerships. There is very little reliable evidence about job creation partnerships (JCP) from the LMDA evaluations. Worldwide there is little evidence that direct job creation programs like JCP lead to high rates of unsubsidized work. There is some international evidence that when direct job creation programs are operated by private sector enterprises, the transition to unsubsidized work is higher. This program design feature could be tried and evaluated. The main aims of direct job creation programs are normally to provide income transfer to a disadvantaged population while preventing further decline in employability skills through work experience. Different metrics for JCP success could be devised—for example, preventing a rise in social assistance participation and program costs. Such analysis would require linkage of administrative records across programs. Evaluation of other EBSMs could benefit from linked program administrative data.

Employment Assistance Services. Evaluations of a wide range of active labour market programs (ALMPs) across a variety of countries have produced three essential findings: 1) job search assistance programs are the most cost-effective, 2) large-scale public service employment programs are the least cost-effective and most costly, and 3) job training programs and employment subsidies fall somewhere in between, with the degree of cost-effectiveness dependent on proper targeting of assistance. EAS is the most widely used active labour market intervention in Canada, and it might be the most cost-effective. However, reliable knowledge on program effects is lacking. The bundling of services in the action plan has confounded efforts to identify the effects of separate services. International evaluations have found some services in the EAS menu more effective for certain categories of customers. Compilation and analysis of EAS administrative data is invaluable for revealing the Canadian patterns of effectiveness for services by subgroup, location, and economic context.

5 IMPROVING THE NEXT ROUND OF LMDA EVALUATIONS

5.1 Increasing Effective Sample Size

Previous evaluations of EBSM under LMDA suffered from insufficient sample sizes. Larger sample sizes would support more extensive analysis of effects by participant subgroups, program features, services bundling and sequencing, and labour market context. The main constraint on sample size was the cost of follow-up surveys conducted by third party survey agencies. Sample sizes for all EBSM evaluations can be increased by more use of available administrative data on program participants. When samples are chosen properly, these data can also be a source of information on very large comparison groups. Supplementary follow-up surveys can be justified in certain cases. This was requirement in recent years when administrative data could not be linked to Revenue Canada tax records for outcome measures of reemployment. Linking to Revenue Canada data for outcome measurement greatly increases the research potential for program administrative data in Canada.

5.2 Advantages and Drawbacks of Using Program Administrative Data

Use of program administrative data in evaluations has the potential to increase the effective sample sizes for evaluations at relatively low cost. This section focuses on the drawbacks of using administrative data for evaluations. There are a number of issues when working with administrative data records that make use of such data for policy analysis quite challenging. These include but are not limited to: 1) the volume of data to be extracted, 2) unique identification of individuals, 3) documentation (or mainly lack thereof), and 4) missing and altered data.

A census of micro records for policy analysis requires intensive use of computing resources and should be accomplished on statistical server(s) and input/output (IO) sub-system(s) dedicated to that purpose. Not only is considerable physical, data storage required but the IO sub-system attached to statistical server(s) must have the throughput capabilities to deliver the data and receive it again when modified by statistical processes. Random access memory (RAM) must also be very large depending upon the statistical software used to process the data. As an example, the Upjohn Institute has completed many projects over the past 11 years for one state that involve use of all, state-wide quarterly wage records dating back to 1993. The expected addition of data in 2011 will result in raw wage record data in excess of 330,000,000 records. Given that our statistical software (Stata) relies heavily on RAM for processing, the definition of new variables, data sorts and the balancing of the wage record data require over 128 GB of RAM. Furthermore, given a census of data from this state for unemployment insurance (UI) and the employment service (ES), the compilation all the wage and program data from the mid 1990s to the present currently requires 45 hours of continuous processing to produce all the analysis-ready data sets needed.

Another important consideration for data extraction is the question of who will extract the data and compile it for statistical analysis? The experience of the Upjohn Institute is that data

programmers for many government agencies are very capable at writing the code needed to extract data but often do not have the software and statistical knowledge needed to thoroughly examine the extract to make sure the results are in fact a "census" and that the results produced "make sense." Also, it is best practice to rely on experienced statistical programmers to merge data across data files and programs. What this means is that unless the statistical analyst requests and receives complete data dumps of all agency data, extracts will often need to be repeated after the data are examined by experienced analysts and underlying errors are found.

One of the most sensitive and unfortunate issues with regard to administrative data concerns the unique identification of individuals. Here in the United States that is best accomplished using social security numbers. However, pervasive fear and multiple headlines over the past decade concerning "identity theft" have made this a very difficult issue. Another state with which the Upjohn Institute has completed several projects over the past decade refuses to release the social security numbers with the micro data, and relies on a system that generates unique identifiers for each individual. While that is typically not a problem for projects that are one-time events, data extracts that are repeated several times over a period of years for updates of the same project or new projects that often involve other agencies, introduce the problem of consistent identification over time. Currently, the Upjohn Institute is facing this issue with the state just mentioned. An update of certain data that involve "bogus" identifiers has a period of overlap from 2006-2008 with the same type of data received previously. A merge of the "unique" identifiers from both data sources resulted in a successful match rate of just 0.1 percent upon initial receipt of the data. An agency error was uncovered, the data re-written, and our successful match rate is currently 63 percent. Obviously, more errors need to be resolved.

An obvious piece of information needed to understand administrative data sets and what might be possible from a research perspective is documentation. In actual practice, that documentation is often just a COBOL program or a listing of variable names within a relational database. Also, on more than one occasion as we have approached agencies to formulate a data request, we have asked for documentation only to receive the reply, "You tell us what you want and we'll tell you if we have it." The important part of this aspect of acquiring agency data is developing relationships with the right people who have authority and fully support the project. Until that happens, be prepared to be greeted with suspicion.

Missing values for many observations of important research variables is common within administrative data sets. For a recent project with yet another state involving unemployment insurance data, upon initial examination of the data, we found that 28 percent of all UI records were missing values for race/ethnicity, 29 percent missing for education and 35 percent missing data for the length of tenure on the most recent job from which they had separated. Fortunately, the proper research pool from which we ultimately estimated statistical models was a sub-set of all UI applicants and this pool had much lower rates of missing data. Nevertheless, we chose to include binary indicator variables for missing data within certain classes of variables because the systematic reasons those data were missing were resulting in certain groups failing to receive agency services. By including the indicator variables in the models that were to be used by the agency, these persons were re-included in the pool for service receipt.

Finally, an issue related to missing data is that because agencies use data primarily for program administration instead of planning for research, things are done that compromise the research value. For example employment service transactions are commonly over written each time a customer visits an employment center so that individual transactions histories cannot be recovered. The Upjohn Institute has also encountered administrative coding of data fields intended to have other meanings. We worked with two state UI agencies that use the individual's prior occupation code data field to instead record other administrative data. One agency modifies the occupation code to track whether the person is exempt from job search requirements and whether the client ultimately returns to past employment. Another agency modifies the occupation code to indicate whether the client had previously registered with the employment service. Obviously, these modifications transform an important variable for economic research into something useless, or worse–misleading.

5.3 Filling Knowledge Gaps in Evaluations

The LMDA evaluations produced limited information on differences in program effects across participant subgroups, labour market conditions, program delivery methods, program features, program combinations, program sequences, and labour market context. In Chapter 4, these subgroup impact estimates were the main gaps identified in the Canadian evaluations of EBSM. Also in Chapter 5, the gaps review of the international literature suggests some important areas where future Canadian evaluations and policies could focus. For example, requiring active job search while receiving cash EI benefits, closely monitoring and supporting active job search, and enforcing consequences when found deficient. A common theme emerging from all the studies is to provide high quality services, while at the same time expecting a high level of effort from customers.

5.4 Next Steps: Suggestions for Improving the Draft Cycle II Evaluation Plan

We endorse the plan to use administrative data as the main information source for round two of EBSM evaluations under LMDA. We endorse the national strategy for pooling data across provinces to evaluate particularly small active measures, and for programs with larger participant counts, to facilitate extensive analysis of impacts by participant subgroups, labour market characteristics, and program features.

Participation of the federal partner should yield economies of scale in evaluation design and operations. Program administrative data linked to Revenue Canada records should be the main sources of information for evaluations. A national perspective in evaluation could also help adjust for economic conditions and generate labour market adjusted estimates to facilitate comparison of effectiveness across areas.

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